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Officers 2003

President

J. Bruce Overmier
Professor of Psychology
University of Minnesota
N-218 Elliott Hall
75 E River Rd.
Minneapolis, MN 55455\
Email: psyjbo@umn.edu
Tel: 612-625-1835

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University of Colorado
Boulder, CO 80302
Email: wert@psych.colorado.edu
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Bethesda, MD 20816
aboneau@gmu.edu

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Donald Dewsbury

Editor: *Review of General Psychology*

Douglas Candland
Department of Psychology
Bucknell University
Lewisburg, PA 17837
dcandland@bucknell.edu

Inside

Stick around long enough and things that keep coming around keep coming around and around. The annual APA convention and the Society's contribution to its program is an example. Here it is again, this time in Toronto, but this time a convention that may suffer from Kahneman and Tversky's "availability bias," the tendency to overvalue recent salient events.

The SARS outbreak had APA leaders debating whether or not to cancel the convention because of a few hundred deaths around the world in the past several months. To put this into perspective, I have heard that 400,000 people die each day, most, probably, from disease of one kind or another. That seems to be a reasonable ballpark figure if you do the calculations. What is going on here? Are the media really warping reality that much?

Despite the distraction, the Society is presenting its usual stellar array of events thanks to the efforts of Program Chairs David Lubinski and Nancy Segal. See inside for a schedule



Messages from the Society President

On APA in Toronto

September 11, 2001, was a traumatizing event for us all. TV makes even distant things very immediate. And those images frighten us still. Add the reminder of our personal vulnerability when we are searched at airports with armed guards (I call it the Homeland Insecurity act with its orange warning), and our anxiety is almost palpable. As psychologist we know that anxiety sensitizes us to all possible additional threats—a kind of hyper-vigilance. The result is that the perceived threat of SARS in Toronto becomes exaggerated (and the hype by media adds even more). As a result, some individuals who really want to attend APA's annual convention and all the good things there—friends sharing scholarship—are considering not going. "Not worth the risk" they might say. But what is the risk? The real risk? Risk assessment is not a strength of normal cognitive processes, we have learned from our colleagues who do study this. We overestimate small but novel risks and underestimate large but common risks.

I was in Washington DC last week. I didn't worry about it. Seemed normal thing to do. My taxi driver's sense of urgency did remind me that I could be in an accident! But I never thought about being shot... But in fact, the chance of my being shot in Washington DC are greater than my chances of getting SARS in Toronto. Really. Now risks are additive, so if SARS shows up in DC, I really should stay away. But in Toronto, Canada, there is virtually no risk of getting shot.

All this is to suggest that in deciding on whether to go to the APA annual convention—and of course each individual must decide—use your psychology to make a rational, true risk-based choice. I will be there. I hope to see you, too.

On Awards

The Society of General Psychology gives several awards annually for lifetime achievement, for an outstanding book, for a critically important seminal paper, and, occasionally, for special service.

Organizations commonly make awards to persons (most commonly members). This is not altruism but rather serves serious purposes. Of course the awards

honor and bring recognition to the individual recipient and 'reward' the individual for their achievements and contributions. And, as we all know, rewards strengthen the behavior that leads to them. Such awards also call attention not only to the individual awardee but to the awarding organization and its values and goals. They promote the organization as well, plain and simply. Additionally, few of us can know all that is going on, so awards can help us to focus on important achievements and identify work that we should probably read up on; in that sense, awards function as a guide.

But because we are a diverse organization that spans all of psychology and because we—as individuals or even small committees—cannot read or know about everything that is important, the award process is critically dependent upon nominations from members who as a group do span all of psychology. This simple fact eludes most of our members, it seems. I say this because, truthfully, only a few of our members bother to make nominations for awards—any awards, division or APA. It is a problem.

Now this problem could be because our members don't actually read anything, or they don't make evaluative judgments, or they don't care, or that there is really nothing out there of merit. I cannot believe that any of the four are true! Alternatively, they may believe that they are too busy, or that nobody attends to these nominations, or that there are already too many nominations for theirs to matter. None of these alternatives is true either. We cannot be too busy to find a few minutes to share our excitement about some work in our field, Nominations are taken very seriously. And, there are not too many nominations. In fact, this last is the reason for this note.

This is a call for you to take each request you see for a nomination seriously—and to respond. Tell the world what contribution or who's scholarship you think is important and why. Your nomination may not always be selected—but sometimes it will be and that is itself a contribution to our field which will make you feel very good. So, when the Society for General Psychology calls for nominations next Autumn, contribute your nominations. Recognizing others can be a fun part of you life. And, someone and our society will be helped.

Bruce Overmier



About Primatologist (and erstwhile “rodentologist”) Harry Frederick Harlow, Other Experimental and Monkey Psychologists, and a potpourri of Simian Anecdotes

Robert Perloff
University of Pittsburgh

Prefatory Remarks: The question arises, what am I, an untidy and undisciplined applied psychologist who has mucked around in industrial/organizational psychology, consumer psychology, and program evaluation—not to mention several distracting excursions into bureaucratic leadership endeavors and, more recently, into the mesmerizing corridors of general psychology and the history of psychology, doing (indeed how dare I, what right have I, what credentials have I) sticking my neck into the lofty environs of comparative psychology, experimental psychology, animal psychology? Indeed, I can envision experimental psychologists giving me the same kind of cold shoulder that industrial/organizational psychologists do when they look down their noses upon unscientific and atheoretical clinical psychologists, psychotherapists, and other assorted poseurs who are infesting an APA that is, at least in the minds of many, sinking into the swamp of “professional psychology.”

The answer is, I suppose, that I am a closet experimental psychologist. The most interesting and valuable psychology courses I had as an undergraduate at Temple University were my nine credit hours in laboratory psychology and my six hours in history (E. G. Boring) and systems psychology (Edna Heidbredder). As a doctoral student at Ohio State—and this should not be viewed as a disparagement of my excellent training there in measurement, quantitative psychology, and in applied psychology—my most valuable courses, courses that gave me an enduringly solid foundation in the underpinnings of psychology, were my courses with D. D. Wickens in comparative psychology (for which I received at Ohio State the only grade below “A”—a “B”), in experimental design with Paul Fitts, in social psychology with Don Campbell, and my exposure to Horace B. English and Sidney Pressey. My major professors, Herbert A. Toops and Robert J. Wherry, along with Harold Burr and Cal Shartle, were magnificent and exemplary mentors, I need to mention here with limitless emphasis, but still the foregoing intellectual experiences were closer to the core of

fundamental, basic, “true” psychology

My first postdoctoral job was as a research psychologist with the Army Research Institute (then called the Personnel Research Section) with the Department of the Army in Washington. One of my early experiences there were one or two engaging and memorable parts of afternoons I had dialoguing with Harry Harlow, who was then (1950-51) an advisor at G-One, the personnel arm of Army headquarters at the Pentagon. J. E. Uhlaner (then the Research Manager at PRS) recently (2003) refreshed my memory with regard to the huge impact that Harry Harlow had on military psychology and the applications of psychology that informed policy for the Army, for applied psychology in general, and for the armed forces even beyond the Army. Jay Uhlaner reminded me that perhaps without the influence of basic scientist Harry Harlow the famed HumRRO (Human Resources Research Organization), led for many years by the unflappable Meredith Crawford might never have seen the light of day. (And Jay, with uncharacteristic modesty, failed to remind me of the powerful influence he, Jay, had on the establishment of HumRRO.) So this is to say that I, as a dustbowl empiricist engaged in applied psychology, was immeasurably influenced by the effect that Harry Harlow—and, later, Kenneth Spence had on the fortunes and activities in applied psychology in general and in military psychology in particular.

Context transcending Harry Harlow. In her *The Monkey Wars* brilliant science writer Deborah Blum (1994) surveys broadly and with scholarly depth the panoply of primate research before, during, and subsequent to Harry Harlow’s celebrated contributions explicitly in the animal domain and, of even more significance and impact, implicitly for human behavior. Her “Monkey Wars” cover the animal rights battles and battlegrounds, the profoundly influential research using chimpanzees and monkeys, and the scientific compass inherent in these animal research endeavors which led to enormous insights into human behavior and lifesaving ideas, concepts, and hypotheses affecting countless men, women, and children ravaged by AIDS; Alzheimer’s disease; brain injury; Ebola, Herpes, and other viruses; heart disease; leprosy; malaria; osteoporosis; and other

maladies (Harry would probably have said, prompted by the popular song of that era, "A pretty girl is like a malady).

The research exploits using monkeys as subjects that Blum describes show the huge similarities between humans and infrahuman organisms and, as such, is an exemplary popular text in comparative psychology.

To be sure, Blum's account is not confined to psychological research. For example, the trailblazing work in transplant surgery pioneered by surgeon Thomas Starzl at my University of Pittsburgh campus is depicted more than perfunctorily. I daresay that there is probably no better source for portraying the excitement and significance of animal research carried on by psychologists the likes of Roger Fouts, Frans de Waal, Seymour Levine, the incredibly productive and innovative Duane Rumbaugh and Sue Savage-Rumbaugh, Larry Squire, and, of course, Harry Harlow. Harlow's engagements with animal rights activists, along with the pros and cons of the animal rights movement are responsibly and, in my judgment, fairly treated by Blum.

Blum's amazing skills as a painter of word pictures is no more graphic and instructive than her metaphor of the brain "as an enormous kitchen pantry occupied by a hyperactive octopus..." and "the scientist as a kind of kitchen spy, on his knees, peering through the keyhole...trying to understand what's happening inside" (p. 63).

I can conceive of no better magnet for attracting young men and women into careers in psychological and biological science than Blum's "Monkey Wars." It is no wonder, then, that she was awarded a 1992 Pulitzer Prize for her series of articles that segued into her nonpareil *The Monkey Wars*.

The Main Course: *Love at Goon Park: Harry Harlow and the Science of Affection*. Deborah Blum's (2002) tour de force is, like television's Fox News, "fair and balanced." Her praise for Harlow's research, along with his successful rebellion against psychology's inexplicable and irresponsible disengagement with love, affection, and attachment, do not discourage her from chronicling Harlow's flaws—which he had more than his share of. I venture the belief that had she been a doctoral student in one of our elite universities, including the University of Wisconsin in Madison where Harlow did most of his groundbreaking thinking, research, writing, teaching, and mentoring, this book, this elegant biography of one of psychology's all-time superstars, would have been accepted as her doctoral dissertation,

and more.

Lest, in my zeal to sing her praise for this primus inter pares biography I overlook two glaring omissions in her otherwise impeccably researched tome, let me mention these now: Harlow's superb service as an advisor to the Army, as mentioned earlier in this account, and his concept of "learning to learn"; I will skip the former because it was treated above and will move, therefore, squarely into the brilliant notion of "learning to learn." (It may well be that she has covered this in *Love at Goon Park*, and so if I missed it I hereby apologize to Ms. Blum and her Perseus editors.)

"Learning to Learn." Lindzey, Hall, and Thompson (2d edition, 1978) provide a succinct account of this concept, which widely acknowledges Harlow's assertion of the idea, an idea referred to as a "learning set." The idea is, as the learned readers of this newsletter know far better than I, that the learner develops a learning set for a particular kind of learning and when this set has been acquired the learner negotiates the task like U. S. Defense Secretary Donald Rumsfeld briskly weaves in and out of the challenging questions put to him by the press corps. If you want a reference out of the horse's (or monkey's) mouth on learning sets, see Harlow (1949).

In some research that I was involved in many years ago, the notion of learning sets came in very handy. We wanted to see if an incentive for learning was more or less, or equally, efficacious for white high school students as for black high school students. Our design was as follows: we compared test scores for one form of an intelligence or aptitude test with another form. Before administering the second form we instructed the subjects to try as hard as they could and for every point scored higher on the second form the subject would be given a dime. Well, as it turned out, while both the white and the black subjects increased their scores, the increase was greater for the white students than for the black students, leading us to conclude inferentially that the white students did better under the incentive condition because they were accustomed to performing under motivational conditions and that the black students weren't. We concluded, in a word, that the white students had "learned how to be motivated," but that the black students hadn't. That is, a motivational set had been acquired by the white students, enabling them to readily perform under the motivational scheme we designed, and that the black students who in their lives had had fewer opportunities to negotiate tasks under motivational stimulation did not do as well as the white students.

Goon Park. The word "goon" denotes either a stupid person or someone who is hired to terrorize others as, for example, labor goons who are hired by unenlightened management to pummel striking workers picketing by dock sites or by the entrance to factories, coal mines, or wherever laborers are expressing their grievances against management. In the case of Wisconsin and Harlow, however, the moniker of "Goon Park" arose out of a typographical ambiguity. The departmental address was 600 N. Park, which, if written hurriedly or, famously, by a physician writing illegibly an Rx, turned out to look like Goon Park, and "love" at Goon Park was not a coed smooching with a football hero but, rather, a reference to Harlow's famous experiments on affection and love.

Harry Frederick Israel. Because of his surname, Israel, Harry, who was not Jewish, was widely suspected as being Jewish and in those days in the opening decades of the 20th century, Jews were not welcome as college professors. Hence Harry's advisor, Lewis Terman, suggested that Harry change his name to one that would be more Christianly oriented. Harry selected his father's middle name, Harlow. [As far as I know Harry's father, Alonzo Harlow Israel (who went by the name of "Lon"), was not related to the Hollywood screen siren, Jean Harlow, or at least Deborah Blum did not connect up the two, Lon and Jean.] Although the Israels were gentiles for generations, the name Israel was traced back to 1753, when an ancestor was buried in a Jewish cemetery; and so it may have been that Harry was 1/64th Jewish, a tortured cross—forgive the expression—to bear.

Harry was born on Halloween evening, October 31, 1905, which was a dirty "trick," I suppose, in the eyes of belligerent animal advocates but a "treat" for psychology and science. He died December 6, 1981, at age 76, and unlike many lesser psychologists who were a legend in their own minds, Harry Harlow was arguably a towering legend in his own time, and beyond.

His Stanford mentors were Calvin Stone, Walter Miles, and Lewis Terman. His dissertation was on feeding habits of baby rats. Harry grew to dislike rats even though rats were in those days the gold standard in experimental psychology. He referred to rat research as "rodentology" and despaired of being a "rodentologist," morphing him, instead, to work with monkeys.

Harry had a troubling and embarrassing speech defect, leading him to mangle the letter "r" in words, and this sometimes gave his speech "...a cartoonish quality in the 'silly wabbit' style of Elmer Fudd (p. 27) Because of his shyness and that speech defect "...he could not have entered Terman's gifted study. And

Terman made that clear to him" (p. 27).

Basically, the Blum book is about Harlow's conviction that everyone needs a foundation of affection, a concept that was amply and persuasively demonstrated by his experiments on monkey attachments and affection. Given the universal acceptance of Harlow's work and views on affection and love it is strange that there is no division in APA on love. Is anyone for, say, a "Division of Psychologists Studying the Psychology of Love" or a "Division of Psychologists who are in Love with Love"? Maybe what we need to heal psychology's wounds is a "Division of Scientific Psychologists Who Love Professional Psychologists and Professional Psychologists Who Love Scientific or Academic or Research Psychologists" or perhaps a "Division of Professional Psychologists Who Love the Scientific Basis of Therapy as Much as they Love Money.."

It is interesting, is it not, that Burrhus Frederick Skinner, who disavowed the existence of feelings in animals, and Harry Frederick Harlow, who trumpeted feelings in animals, had the same middle name, Frederick. It may well be that that was the paramount thing that BF and Harry had in common!

Much to her credit, Blum lays bare Harry's rough edges, his coarse and biting lapses in public which earned him the scorn of many people. He was an inveterate punster, more often than not using puns to convey politically incorrect ideas. For example, he oftentimes showed pictures of two monkeys copulating, calling the talk "The Sermon on the Mount." He also said that "I'm glad humans aren't the only animals to lose their heads over a piece of tail" (p. 240).

A splendid summary of "Goon Park" was written by Lisa Warren, Director of Publicity for the publisher, Perseus Books, in a promotional blurb for the book: "Harry Harlow...altered our understanding of love. At the time in which Harlow was working, affection between parents and children was very much discouraged—psychologists thought it would create needy and demanding offspring; the medical community was convinced it would spread infectious disease. But Harlow's groundbreaking experiments with primates proved that a loving touch not only didn't harm babies but in fact ensured their emotional and intellectual growth. His conclusions about attachment sparked a profound cultural shift and a revolution in psychology that overturned the Freudian notion that the mother-child bond is exclusively about food. His results ...showed how early love and affection directly affect our intelligence as well as our adult relationships. Love, it turns out, makes us smarter."

An insightful observation comparing animals to humans was passed on to me recently by University of Kentucky animal psychologist Thomas R. Zentall (2003), reinforcing and even validating moderately the work of Harry Harlow and his cohorts: "...as humans we assume that we are always aware of our emotional state, when in fact I expect that that assumption is not always correct. Sometimes emotional states that we hide from ourselves, such as anger, are more apparent to others through nonverbal cues we provide. Thus, one of the most interesting aspects of animal research is the insights it provides about human behavior."

To me it is monumentally ironic that, in spite of Harlow's research which shows that literal touching is beneficial, there is a strong and pervasive movement within the psychotherapy community and the psychotherapist members of APA who are so scared silly of being sued for malpractice and of behaving unethically when they touch patients—and as an enemy of the medical model I prefer to call the psychotherapist's customer, a "client"—that all touching, reassuring and comforting and reinforcing and trusting touching and not only blatantly sexual touching or fondling is forbidden. They seek to throw out touching with the dirty bath water.

A potpourri of Harlow and other simian anecdotes. Here are a couple of Harlow anecdotes supplied by Lewis Paeff Lipsitt (2002). First, "When Harry got some high award from APA, he said as he approached the microphone to acknowledge the award, 'It's about time;'"

Next at a raucous (are there any other kind) of party at an MPA convention at the old Roosevelt Hotel in Chicago, Harlow was babbling and drunk and they had a hard time getting him to leave the room, at which point Kenneth Spence picked Harlow up and dumped him unceremoniously in a laundry basket in the hallway. As the others walked away, Harry had his eyes closed and Spence said, "That's all right, leave him there for a while. He'll be all right."

Victor Hugo Denenberg (2000) told me that at a University of Wisconsin banquet honoring Harry Harlow, Harry was identified as Wisconsin's youngest full professor. After the sumptuous feast Harry said that he wasn't sure that he was the university's youngest full professor but that he sure as hell was the university's fullest young professor, mouthing, I am confident unbeknownst to him, a chiasmus. A chiasmus is a reversal in the order of words in two otherwise parallel phrases. A delightful book on chiasmi was written by Grothe (1999), *Never let a fool kiss you or a kiss fool you*. Here are some other clever

chiasmi, found in Grothe: Mae West: "It's not the men in my life but the life in my men"; John F. Kennedy: "Ask not what your country can do for you, ask what you can do for your country"; Anonymous: "A magician pulls rabbits out of a hat, and the experimental psychologist pulls habits out of a rat"; Dorothy Parker, when asked by an irate editor at *The New Yorker* about her delinquent book review and why she was late with it, replied "I've been too fucking busy and vice versa." And, not to be outdone, Bob Perloff, in referring a few years ago to former vice president Dan Quayle, said that the former veep was "forgotten, but not gone."

And, no, the monkey wrench has nothing to do with monkeys, but everything to do with that tool's inventor, Charles Moncke.

Finally, Saint Bonaventure (in his "Conference on the Gospel of John") said this of the monkey: "The higher it climbs the more you see of its behind." Which is why I'm resolved not to climb any higher in this smorgasbord about monkeys, men, and Harry Frederick Harlow.

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Bob Perloff's email address is
rperloff@katz.pitt.edu



Whatever Happened to Cognitive Dissonance Theory?

**Eddie Harmon-Jones
Cindy Harmon-Jones
University of Wisconsin — Madison**

Research and theoretical developments concerning the theory of cognitive dissonance are reviewed. Review focuses primarily on theoretical revisions that propose different underlying motivations for cognitive dissonance reduction. After reviewing the self-consistency, self-affirmation, and aversive consequences revisions, the authors review recent research that has challenged each of the revisions and that supports the original version of the theory. In the end, the authors review the action-based model of dissonance, which accepts the original theory's proposal that a sufficient cognitive inconsistency causes dissonance and extends the original theory by proposing why cognitive inconsistency prompts dissonance.

Cognitive dissonance theory is concerned with how perception and cognition influence and are influenced by motivation and emotion. Cognitive dissonance research dominated social psychology from the 1950s until the 1970s. Hundreds of experiments have tested dissonance processes. For the most part, these experiments have explored the ways that the experience of cognitive dissonance causes attitude and behavior changes. In recent years, there has been renewed interest in this theory (e.g., Beauvois & Joule, 1996; Harmon-Jones & Mills, 1999).

In addition to the large volume of research on cognitive dissonance theory itself, the theory has held a wide influence on the psychological theory and research. Aronson (1992) identified a number of social psychological theories that could be thought of as dissonance in other guises, including self-affirmation theory (Steele, 1988), symbolic self-completion theory (Wicklund & Gollwitzer, 1982), self-evaluation maintenance theory (Tesser, 1988), self-discrepancy theory (Higgins, 1989) and action identification theory (Vallacher & Wegner, 1987).

Leon Festinger formulated the original theory of cognitive dissonance in the mid-1950s. Festinger theorized that, when an individual holds two or more elements of knowledge that are relevant to each other but inconsistent with one another, a state of

discomfort is created. He called this unpleasant state "dissonance."

Festinger theorized that the degree of dissonance in relation to a cognition = $D/D + C$, where D is the sum of cognitions dissonant with a particular cognition and C is the sum of cognitions consonant with that same particular cognition, with each cognition weighted for importance (see Sakai, 1999, and Shultz & Lepper, 1999, for more precise mathematical models).

Festinger (1957) theorized that persons are motivated by the unpleasant state of dissonance to engage in cognitive work so as to reduce the inconsistency. To reduce the dissonance, individuals could add consonant cognitions, subtract dissonant cognitions, increase the importance of consonant cognitions, or decrease the importance of dissonant cognitions. One of the most often assessed ways of reducing dissonance is change in attitudes. Attitude change in response to a state of dissonance is expected to be in the direction of the cognition that is most resistant to change. In tests of the theory, it is often assumed that the knowledge about recent behavior is usually most resistant to change, because if a person behaved in a certain way, it is often very difficult to undo that behavior.

Experimental Paradigms Used to Test the Theory

Free choice. In 1956, Brehm examined dissonance theory's predictions for post-decision processing. According to the theory, after a decision, all of the cognitions that favor the chosen alternative are consonant with the decision, while all the cognitions that favor the rejected alternative are dissonant. The greater the number and importance of dissonant cognitions and the lesser the number and importance of consonant cognitions, the greater the degree of dissonance experienced by the individual. In a decision-situation, dissonance is typically greater the closer the alternatives are in attractiveness (as long as each alternative has several distinguishing characteristics). Dissonance caused by a decision can be reduced by viewing the chosen alternative as more attractive and/or viewing the rejected alternative as less attractive. Brehm conducted an experiment in which participants made either an easy or a

difficult decision between two alternatives. The difficult decision was one in which the alternatives were close in attractiveness, whereas the easy decision was one in which one alternative was much more attractive than the other. Participants were asked to evaluate the decision options before and after the decision. Brehm found that, when persons made a difficult decision, they changed their attitudes to become more negative toward the rejected alternative. After an easy decision, participants did not change their attitudes.

Induced Compliance. Festinger and Carlsmith (1959) hypothesized that dissonance should be aroused when a person acts in a way that is contrary to his or her attitudes. To test this prediction, they brought participants into the laboratory and asked them to perform a boring task. Then, participants were paid either \$1 or \$20 to tell "another participant" that the task was interesting. According to dissonance theory, lying for a payment of \$20 should not arouse much dissonance, because \$20 provides sufficient justification for the counter-attitudinal behavior (i.e., it adds 20 cognitions consonant with the behavior). However, being paid \$1 for performing the same behavior should arouse much dissonance, because \$1 was just enough justification for the behavior (i.e., it adds only one consonant cognition). As expected, participants in the \$1 (low-justification) condition changed their attitudes to be more positive toward the task, whereas participants in the \$20 (high-justification) condition did not change their attitudes.

Challenges to the Research and Original Theory

After these and other dissonance results appeared in the literature, some theorists began to question whether the results were due to motivation. Some theorists hypothesized that the effects were due to non-motivational, cognitive processes (e.g., Bem, 1972) or impression management concerns (Tedeschi, Schlenker, & Bonoma, 1971). However, subsequent research confirmed that dissonance is a motivated process (Harmon-Jones, 2000a, 2000b).

Beginning in the late 1960s, researchers began to propose motivational explanations for dissonance effects that differed from Festinger's originally proposed theory. Four revisions of dissonance theory have been proposed, and their originators have provided evidence to support the new conceptions. These include Aronson's (1968, 1969, 1999) self-consistency theory, Steele's (1988) self-affirmation theory, Cooper and Fazio's (1984) new look at dissonance and Harmon-Jones' (1999, 2000c) action-

based model.

Self-Consistency. In his self-consistency theory, Aronson proposed that dissonance is not due merely to an inconsistency between cognitions. Instead, he posited that dissonance occurs when a person acts in a way that violates his or her self-concept, that is, when a person performs a behavior inconsistent with his or her sense of self. Since most persons have a positive self-concept, dissonance is most often experienced when a person behaves negatively, behaving in an incompetent, irrational, or immoral manner. One of the primary predictions derived from this revision is that low and high self-esteem individuals should respond with less and more dissonance reduction (e.g., attitude change), respectively, because in dissonance experiments high self-esteem individuals are induced to act in ways that are more discrepant from their positive self-views. Experiments testing this prediction have produced mixed results. Beauvois and Joule (1996, 1999) have also obtained results that are difficult to explain with this revision.

Self-Affirmation. Steele (1988) proposed a different alternative to Festinger's dissonance theory. He proposed that persons possess a motive to maintain an overall self-image of moral and adaptive adequacy. He stated that dissonance-induced attitude change occurs because dissonance threatens this positive self-image. While Festinger's dissonance theory posited that individuals are motivated to reconcile inconsistent cognitions, Steele proposed that, instead, individuals are merely motivated to affirm the integrity of the self. In support of this idea, Steele presented experiments, where, following a dissonance induction, participants either were, or were not, presented with an opportunity to affirm an important value. When participants were allowed to affirm an important value, dissonance-related attitude change did not occur.

However, in 1995, Simon, Greenberg and Brehm presented data supporting an alternative explanation for Steele's findings that was in line with the original theory of dissonance. Festinger's original theory proposed that the degree of dissonance experienced depended upon the importance of the dissonant and consonant cognitions. Simon, Greenberg and Brehm hypothesized that making an important value salient could reduce dissonance by reducing the individual's perception of the importance of the dissonant act. They conducted an experiment in which participants who opposed a tuition increase were given high choice to write essays in support of a tuition increase (a counter-attitudinal statement).

After writing the essay, participants either were given an opportunity to affirm an important value (self-affirmation condition), were asked to write about a value that was not important to them personally but was of general importance (value salient condition, e.g., world hunger), or neither (control condition). Participants were then asked to rate whether or not they supported a tuition increase. Participants in the control condition changed their attitudes to be more favorable toward a tuition increase, as expected. Participants in both the self-affirmation and value salient conditions did not change their attitudes. They had trivialized, or reduced the importance of, the tuition increase issue by thinking about other important values, even when these values were not personally important and thus not self-affirming.

New Look. Cooper and Fazio (1984) proposed the idea that the discomfort experienced in dissonance experiments was not due to an inconsistency between the individual's cognitions, but rather to feeling personally responsible for producing an aversive consequence. They stated, "Dissonance has precious little to do with the inconsistency among cognitions per se, but rather with the production of a consequence that is unwanted" (Cooper & Fazio, 1984). In support of this idea, Cooper and Worchel (1970) replicated and extended Festinger and Carlsmith's (1959) classic experiment. In addition to the conditions of the original experiment, Cooper and Worchel added conditions in which, when the participant tells the confederate that the boring task is interesting, the confederate is not convinced by the lie. They found that attitude change occurred only in the low-justification condition where the confederate believed the lie. Cooper, Worchel, Fazio, and others interpreted this result as indicating that dissonance-related attitude change only occurred in the condition in which an aversive consequence was produced. A number of other experiments produced similar results. The new look, or aversive consequences, revision of cognitive dissonance theory was widely accepted.

However, concerns regarding the aversive consequences revision persisted among some dissonance theorists. According to Eagly and Chaiken, the aversive consequences revision "transformed the quite general theory that Festinger (1957) had envisioned into a mini-theory that delineates a particular set of circumstances that produce a particular type of attitudinal adjustment within the induced compliance paradigm (Eagly & Chaiken 1993, p. 520)." Berkowitz and Devine (1989) also lamented the rise of this model, saying, "Gone was the theory's broad sweep"

(p. 499).

In addition, the results obtained in paradigms other than the counter-attitudinal action paradigm are not consistent with the aversive consequences model. Dissonance research using a selective-exposure paradigm has demonstrated that persons are more willing to examine materials that confirm their beliefs than materials that dispute their beliefs (Brock & Balloun, 1967; Frey, 1986). Research using a belief disconfirmation paradigm has shown that, when persons are exposed to information that challenges their beliefs, they often strengthen their original belief (Batson, 1975; Burris, Harmon-Jones, & Tarpley, 1997). Research using a hypocrisy paradigm has shown that persons change their behavior to be more in line with their beliefs when they are reminded of times when they did not live up to their beliefs (Aronson, Fried, & Stone, 1991; Stone et al., 1994). It is difficult to reconcile any of these lines of dissonance research with a conception of dissonance theory in which the production of an aversive consequence is the only motivator of dissonance-related attitude change.

Certainly, according to the original theory of cognitive dissonance, the production of aversive consequences would be expected increase the amount of dissonance produced (see Harmon-Jones, 1999). However, the original theory would deny that an aversive consequence is necessary to produce dissonance.

So why did the new look research find that, in the induced-compliance paradigm, attitude change only occurred when the participant caused an aversive consequence? First of all, the lack of attitude change in the no-aversive-consequences conditions is a null effect. Null effects are notoriously difficult to explain and subject to multiple alternatives. Attitude change may have been produced, but may have been too slight to be detected with the small sample size of these experiments. It is also possible that not enough dissonance was aroused in these experiments to produce attitude change without the additional help of an aversive consequence. For example, too much justification for the counter-attitudinal behavior may have been provided. It is also possible that, in these experiments, dissonance was produced, but it was not detected or was reduced by a route other than attitude change.

Beginning in 1996, Harmon-Jones, Brehm, Greenberg, Simon, and Nelson conducted experiments that demonstrated that dissonance-related attitude change can occur without the production of aversive consequences. Under the guise of an ex-