

## The Naive Epistemology of a Working Social Psychologist (Or the Working Epistemology of a Naive Social Psychologist): The Value Of Taking “Temporary Givens” Seriously

Mark P. Zanna

Department of Psychology  
University of Waterloo

*In this article I first describe the origin of 6 recent lines of inquiry. In describing these 6 case studies, I attempt to determine the inspirations for my ideas. I also attempt to determine (a) whether these specific inspirations (or, perhaps, strategies) have occurred in my past research and (b) whether, in conclusion, some general principle can begin to account for how I generate my ideas.*

When Arie Kruglanski and Tory Higgins first asked me to contribute an article to this special issue of *Personality and Social Psychology Review*, I thought, “no”—primarily because I did not need another project to add to a crowded agenda. Then, of course, I said, “yes”—primarily because (as usual) these guys had a good idea.

In this article I describe (with the “brilliance of hindsight” and with, perhaps, a bit of poetic license) the origin of my six most recent projects. In doing so, I try to determine the inspirations I use to generate ideas for research. I also try to determine whether these inspirations (or, perhaps, strategies) are recurring ones by briefly looking back to see whether each was employed in an earlier line of research. Finally, I try to determine whether there is a more general principle that can begin to account for how I generate my ideas. But, before I begin, I need to set the context.

### Graduate School Fears Versus the Reality

In graduate school one of my greatest fears was that when on my own I would never be able to come up with

an original idea. Shortly after becoming an assistant professor, however, I discovered the problem was not coming up with ideas, but rather identifying those “good enough” to pursue. Put simply, there is no such thing as an easy experiment and, therefore, there are always opportunity costs to pursuing any given idea; pursuing one line of research means you may not be able to pursue another. Thus, early on I had to learn to inhibit the impulse to pursue every idea. Instead, I learned that it was critical to try out new ideas with colleagues. So, I pestered colleagues at my own university as well as relevant researchers (whether I knew them or not) at conferences. (By the way, in the old days, the Society of Experimental Social Psychology was a particularly good venue to talk to folks about research in areas that were near and dear to their hearts.) I did this not only to get technical feedback and suggestions for further reading, but more importantly to discover whether I could hold the attention of rather busy (and often egocentric) social psychologists for at least 2 minutes! If I could, I concluded that I might have an idea worth pursuing.

I also need to state at the outset that although I consider my research to be theoretical in nature, I have not tended to develop and test grand theories, but instead have tended to test and apply existing theoretical notions in an effort to increase our understanding of basic social psychological processes and phenomena. Finally, I need to acknowledge that my research career has been entirely collaborative with graduate students and, more often than not, with colleagues. Therefore, in reality, ideas that have passed the initial try-out (and certainly the experiments that have tested these ideas) have often been shaped and refined (a lot) by my students and colleagues.

With this preamble completed, let me now turn to six recent projects that (in retrospect) have involved, at least, five sorts of inspiration for theory-generated re-

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Requests for reprints should be sent to Mark P. Zanna, Department of Psychology, University of Waterloo, 200 University Avenue West, Waterloo, Ontario N2L 3G1, Canada. E-mail: mzanna@watarts.uwaterloo.ca

search: *solving puzzles* (e.g., concerning attitude ambivalence), *acting on conviction* (e.g., concerning culture and dissonance), *applying phenomena* (e.g., concerning subliminal priming and persuasion), *extending phenomena* (e.g., concerning [a] the interaction between explicit and implicit attitudes and [b] the misapplication of mediation analysis), and, finally, *integrating one’s own lines of research* (e.g., concerning overcoming resistance to persuasion). To foreshadow, I discovered that I seem to start each new project by taking something (such as, for example, a theory or a finding) seriously and, in effect, asking myself if the theory or finding is true, then what interesting implications might follow. Thus, I take the theory or finding as a “temporary given.” That is, I decide not to challenge the theory or finding (thus, avoiding an infinite regress), but instead to rely temporarily, at least, on the “received wisdom” of the field, assuming the theory or finding is good enough for now to warrant moving on to study the interesting implication. Although the decision to take something as a temporary given probably has more to do with the art (rather than the logic) of science, if the research inspired by the temporary given works out, more support will have been obtained for whatever was taken seriously at the outset.

### A Puzzle (Concerning a Result)

The first case study concerns a puzzling result. In the mid-nineties, Megan Thompson, Dale Griffin, and I (Thompson, Griffin, & Zanna, 1995) reported that the correlation between two measures of attitudinal ambivalence—a structural measure that independently assessed positive and negative evaluations and a self-report measure of experienced conflict—was, at best, .40 (Thompson, et al., 1995). Needless to say, I was ambivalent about this result. I was happy that two measures of the same construct were significantly correlated, but I was unhappy that two measures of the same construct only correlated at .40. As I pondered this conundrum the obvious occurred to me. The key to solving this puzzle was to reconstrue these two measures of ambivalence as measures of two different constructs—specifically, the potential to experience ambivalence (i.e., having both positive and negative evaluations of an attitude object) and felt ambivalence (i.e., the actual experience of ambivalence)—and then to ask what might moderate the “potential ambivalence–felt ambivalence” relation. The key insight to answering this new question was suggested by the work of John Bassili (1996) demonstrating that people experience greater conflict on political issues when two relevant and conflicting values are simultaneously accessible. Of course, there was plenty of research on attitude accessibility per se, but, as far as I knew, no one had yet talked about the simultaneous accessibility of two

knowledge structures. So, Bassili’s notion of simultaneous accessibility was a revelation to me. In any event, this led to a program of research designed to test the notion that the potential-felt ambivalence relation was moderated by the simultaneous accessibility of positive and negative evaluations of the attitude object. In other words, we (Newby-Clark, McGregor, & Zanna, 2002) tested the notion that conflicting evaluations are necessary but not sufficient to experience ambivalence. To experience ambivalence (or cognitive dissonance, for that matter; cf. Zanna, Lepper, & Abelson, 1973), the conflicting evaluations or cognitions must come to mind quickly and equally quickly, that is, they must be simultaneously accessible (McGregor, Newby-Clark, & Zanna, 1999). In the course of this research, Newby-Clark hypothesized an additional moderating variable. Specifically, he proposed that to experience ambivalence one must not only have conflicting evaluations that are simultaneously accessible; one must also care about being consistent in the first place (Newby-Clark et al., 2002). Thus, as in virtually all research programs, a new idea emerged along the way that went beyond the original hypotheses and that, I believe, increased our understanding of the phenomenon.

### Two Earlier Puzzles (Concerning Undergraduates’ Questions)

Solving puzzles of one sort or another appears to be a recurring theme. An earlier example that comes to mind is research on the effects of intoxication on decision making. The impetus for this line of research came about when a prospective honors thesis student asked me why her friends, who had strong attitudes against drinking and driving, often drove when intoxicated! After pondering this apparent attitude–behavior “disconnect” for a while it occurred to me that no one had ever bothered to study attitudes toward drinking and driving (and, for that matter, attitudes toward other behaviors that often occur when individuals are intoxicated, such as unsafe sex) when people were, in fact, intoxicated. So, we (MacDonald, Zanna, & Fong, 1995, 1996) decided to do so, applying Claude Steele’s theory of alcohol myopia (the notion that intoxication reduces cognitive capacity so that individuals are primarily influenced by salient cues in their environment) to solve the puzzle of why people who are against drinking and driving do so when intoxicated and why people who claim virtually always to use condoms do not always do so when intoxicated (MacDonald et al., 1995, 1996). Our most recent work in this line of research actually supports the alcohol myopia hypothesis that alcohol does not always have a disinhibiting effect. In fact, when the environmental cues are restraining (rather than impelling, as they typically are in the real world), individuals appear to be more prudent when in-

toxicated (MacDonald, Fong, Zanna, & Martineau, 2000).

A second earlier puzzle was presented to me by another prospective honors thesis student, who was curious to know whether he was necessarily prejudiced if he objected to certain forms of affirmative action. Because it seemed possible (and probable) that not all those who oppose affirmative action are bigots, this question led me and others (Bobocel, Son Hing, Davey, Stanley, & Zanna, 1998; Son Hing, Bobocel, & Zanna, 2002b) to design a series of high-impact survey experiments that attempted to answer the question of whether prejudice is, indeed, the whole story when it comes to opposing affirmative action, especially affirmative action programs that appear to violate the merit principle (i.e., the principle that the most qualified individual deserves to be hired or promoted). Interestingly, to solve this puzzle we first had to figure out how to design experiments that would enable us to rule out the possibility that other potential determinants of opposition to affirmative action (such as strong beliefs in the merit principle) were not mere rationalizations for prejudice. So far, our studies have demonstrated that although prejudice is, indeed, a big part of the story, it is clearly not the entire story—justice beliefs (such as a belief in the merit principle) are genuine and do relate to opposition independent of prejudice (Bobocel et al., 1998; Son Hing et al., 2002b).

### A Conviction (Concerning a Theory)

The second case study concerns a result that was hard (for me, at least) to accept. I have to admit that I have been a card-carrying dissonance theorist since graduate school. So imagine my reaction to the suggestion that dissonance was merely a Western phenomenon—that Easterners (i.e., those from collectivistic cultures with interdependent self-construals) do not experience dissonance (Heine & Lehman, 1997). Also, imagine my reaction to the following anecdote:

That decisions are relatively less important to the Japanese than North Americans is a point frequently observed by cross-cultural scholars. For example, the Japanese psychiatrist Takeo Doi (1971), on his first trip to the United States, was surprised how hosts entertaining their guests asked them a barrage of “trivial” questions about their preferences—for example, whether they would like coffee or tea, with or without cream and sugar, and so forth—questions to which Doi’s own reaction was “I couldn’t care less” (p. 12). . . . For Japanese, Doi felt that hosts were more likely to attempt to size up the situation and to help their guests by taking care of things themselves, thus not burdening their guests. (Heine & Lehman, 1997, p. 398)

Well, because of my conviction that dissonance ought to be universal, rather than being automatically persuaded that Asians wouldn’t be motivated to rationalize their decisions, I wondered instead about the Asian host. If Asians (or, more generally, people from collectivistic cultures) have interdependent self-concepts (as cultural psychologists have suggested—and that I take as a temporary given), then wouldn’t hosts be worried about making suboptimal decisions for their guests? In other words, wouldn’t Asians need to rationalize decisions that threatened their interdependent selves?

We (Hoshino-Browne, A. S. Zanna, Spencer, & M. P. Zanna, in press) tried to “bottle” the psychology of the host in a program of research designed to test the notion that, whereas Asians (in contrast to North Americans) would not rationalize a decision for themselves, they would (in contrast to North Americans) rationalize a decision made for their best friend. In our free-choice paradigm, participants rate and rank luncheon entrées for a Chinese restaurant that is ostensibly opening near the campus in the not-too-distant future. Then they are given a choice of a gift certificate between their fifth- and sixth-ranked entrée for either themselves or their best friend and, finally, they rate the entire set of entrées a second time. The results indicated that when making a decision for themselves, North American students rationalized their decision (by “spreading” their evaluation of the two entrées following their decision), whereas Asian students did not. In contrast, when making a decision for their best friend, Asian students rationalized their decision, but North American students did not (Hoshino-Browne et al., in press).

In follow-up research we continued to take cultural psychologists seriously by proposing (and demonstrating) that to “take the sting” out of dissonance for Asians one must give them an opportunity to affirm their interdependent (not their independent) selves. And, of course, because an interdependent self-affirmation manipulation did not exist, Hoshino-Browne had to invent one.<sup>1</sup> At this point in our research program, then, it appears that dissonance is universal after all, though it clearly plays out differently as a function of culture. In fact, in future research we plan to test the notion, originally proposed by Adam Zanna, that the phenomenological experience of dissonance will differ in Eastern versus Western cultures (by employing culturally sensitive “dissonance thermometers”) and, as a consequence, can be eliminated by culturally specific misattribution opportunities.

<sup>1</sup>Our interdependent self-affirmation manipulation asks participants to choose one value from a list of six (e.g., business/economics, social life/relationships, religion/spirituality, and so on) that is most important to them and their family, and then to write about why the value is so important to them and their family.



### **An Earlier Conviction (Concerning a Hole in the Literature)**

Acting on conviction also appears to be a recurring theme. On December 7, 1989, the day following the Montreal Massacre, when 14 female engineering students were killed at Montréal’s *École Polytechnique* by a feminist-hating gunman, it became clear to me that mainstream social cognition research on stereotypes (and stereotyping) was not capturing the essence of prejudice. That is, it became clear to me that the evaluative implications of stereotypic traits (such as effeminate, emotional, and artistic in the case of gays or lazy, athletic, and unintelligent in the case of Blacks) could not plausibly result in the hatred and hostility that was so apparent in the real world. This conviction was the impetus for research (Haddock, Esses, & Zanna, 1993) that attempted to demonstrate that prejudice (i.e., negative attitudes toward outgroups) is based on more than stereotypic beliefs—and in the process to bottle hatred in the laboratory. Our research did demonstrate that there is more to prejudice than stereotypic beliefs, that the relatively more negative (or prejudicial) attitudes held by individuals high in authoritarianism are best predicted by symbolic beliefs (beliefs that typical members of the outgroup violate cherished traditions, customs, and values), and that prejudice based on symbolic beliefs is, in fact, related to greater hostility<sup>2</sup> (Esses et al., 1993; Haddock et al., 1993; Zanna, 1994).

### **An Application (of a Phenomenon to a Classic Problem)**

Throughout my career I have often found myself applying a concept or a finding from one area to another related, or even seemingly unrelated, area. The third case study concerns applying a relatively new phenomenon to a classic problem. Although I had no reason to question the conventional wisdom that subliminal persuasion does not and cannot occur (especially after interacting with Anthony Pratkanis when I was a visiting professor at the University of California, Santa Cruz), it did occur to me that it would be perfectly reasonable to suggest that subliminal priming (which the literature does suggest is a real phenomenon) could be used to create a psychological state that a persuasive communicator could take advantage of. Thus, subliminal priming might enhance persua-

<sup>2</sup>Hostility was assessed by a budget-reduction ballot, in which participants voted the extent to which the budgets of various clubs on campus should be reduced. In one experiment, for example, participants whose attitudes toward gays were primarily based on symbolic beliefs voted to reduce the budget of the gay and lesbian club on campus by 52%, thus virtually voting the club out of existence (Haddock et al., 1993).

sion! Given that I may have been the only social psychologist in the known world never to have conducted a priming study, this notion was attractive for another reason (i.e., after all, one doesn’t want to be accused of being an old fogie who’s out of touch).

In any event, this insight/application led to a program of research that has, indeed, demonstrated that subliminally priming thirst enhances the effectiveness of an ad for a new sports drink, Super-Quencher, that claims to be the best thirst-quenching sports drink to ever hit the market and that subliminally priming sadness enhances the effectiveness of an ad for Tweed Monkeys, a new rock band whose debut CD, according to an ad, will “put you in a good mood.” Interestingly, following a prediction that Steve Spencer derived from Tory Higgins’ notion that for priming to be effective the primed concept must not only be activated, it must also be applicable (Higgins, 1996), we predicted (and found) that priming enhanced persuasion only when people were motivated to begin with (e.g., when they were thirsty to begin with, in our Super Quencher studies; Strahan, Spencer, & Zanna, 2002).

### **An Earlier Application (of Using One Theory to Test Another Theory)**

The application theme has appeared in several of my past programs of research. The one that comes to mind most readily is my research (Fazio, Zanna, & Cooper, 1977) on the so-called dissonance versus self-perception controversy. I have already admitted that I am a card-carrying dissonance theorist. I should also admit that I have been a card-carrying attribution theorist since graduate school as well.

So it should come as no surprise that although I took self-perception theory as a temporary given, I did not believe that self-perception processes provided a good account of dissonance phenomena. It should also come as no surprise that we used the logic of attribution theory (especially the misattribution logic) to demonstrate that dissonance induction procedures produced an aversive psychological state—and, thus, that attribution theory in general and self-perception theory in particular could not account for dissonance phenomena (Zanna & Cooper, 1974). Incidentally, this would be particularly unsurprising if you knew that my office in graduate school was next door to Mike Storms’—and that I have always thought of the Zanna and Cooper experiment as simply applying the Storms and Nisbett (1970) misattribution manipulations to an induced-compliance paradigm. And, finally, it should come as no surprise that as a dissonance theorist on Monday, Wednesday, and Friday and as an attribution theorist on Tuesday and Thursday, I decided (perhaps, on the weekend) that each theory had its own domain of applicability. Because we believed that dissonance was a theory of attitude change whereas self-percep-

tion was a theory of attitude formation, we expected dissonance processes were more likely to be aroused when the induced compliance was clearly counterattitudinal, whereas self-perception processes were more likely to be engaged when the induced compliance was generally consistent with one's attitude (cf. Fazio et al., 1977).

### **Two Extensions (Concerning Moving Toward a Third Generation of Research and the Misapplication of Mediation Analysis)**

The fourth case study concerns employing implicit attitudes (i.e., relatively automatic evaluations of attitude objects) to study two classic problems. Although I have been interested in the concept of implicit attitudes for the past decade, I have sat on the sidelines and did not contribute to this literature—primarily because I didn't have any interesting ideas. My perhaps naive (and certainly superficial) view of this literature is that it has gone through two generations of research. The first generation of research adapted the idea from cognitive psychology, proposed various techniques to measure implicit attitudes, discovered that implicit attitudes were often only loosely connected to explicit attitudes, and tended to claim that implicit attitudes (because they weren't contaminated by various response biases) were the "real deal." The second generation of research demonstrated that explicit attitudes (if appropriately measured) and implicit attitudes were both authentic but that each tended to influence (or at least predict) different sorts of behavior. Explicit attitudes seemed to predict more thoughtful, controlled behaviors (such as verbal friendliness), whereas implicit attitudes seemed to predict less thoughtful, automatic behaviors (such as nonverbal friendliness).

My entry into this literature came when my student, Leanne Son Hing, and I decided to supervise an undergraduate honors thesis student, Winnie Li, on a project designed to test the hypothesis that hypocrisy (induced by "rubbing the noses" of participants in the fact that they do not always practice what they preach) would reduce prejudice and discrimination (Son Hing, Li, & Zanna, 2002). At some point in designing this experiment it occurred to Leanne and me that it would be interesting if we were able to identify those individuals who would be most influenced by a hypocrisy induction procedure. Conceptually speaking, we decided that aversive racists (or those individuals who—according to Gaertner & Dovidio, 1986—are hypothesized to consciously believe themselves to be nonprejudiced while being prejudiced at an unconscious level) ought to be the most influenced. That is, reminding aversive racists that they do not always practice what they preach ought to make them feel

guilty, and, as a consequence, they ought to bend over backwards not to discriminate in the future. Fine, but at the time we were designing this study, the problem was that there was no existing self-report measure of aversive racism *per se*. And how could there be, given the fact that the prejudice of aversive racists is supposedly unconscious.

We solved this problem by inventing a measure of aversive racism—or, at least, we proposed that by taking advantage of the loose connection between explicit and implicit prejudice we would hypothesize that aversive racists are those individuals with low levels of explicit prejudice but high levels of implicit prejudice, whereas truly nonprejudiced individuals are those with low levels of both explicit and implicit prejudice. In our study, we found that in our control condition (in which participants only wrote a proattitudinal, antiprejudice essay), aversive racists did discriminate more against the Asian Student Association on a budget-reduction ballot (cf. Haddock et al., 1993). That is, less than 5 min after writing an essay stating that University of Waterloo students should not discriminate against Asian students on campus—an essay that participants believed would be used in the next freshman orientation week—aversive racists essentially voted to "screw" Asian students on campus by voting to massively cut the budget of their student association. In contrast, in our hypocrisy condition (in which participants were induced to recall two prior instances of prejudice or discrimination after writing their proattitudinal, antiprejudice essay), aversive racists did, indeed, experience more guilt and did bend over backwards by barely cutting the budget of the Asian Student Association—in fact, nearly half the aversive racists voted not to cut the budget at all (Son Hing, Li, & Zanna, 2002). Thus, we found strong evidence that persons low in explicit prejudice but high in implicit prejudice toward Asians acted like aversive racists. In follow-up research, Leanne Son Hing and her students (Son Hing, Chung-Yan, Grunfeld, Robichaud, & Zanna, *in press*) have demonstrated that the classic aversive racism finding (that aversive racists discriminate only when there is an excuse to "lower the boom" on an outgroup member) holds primarily for those individuals low on explicit but high on implicit prejudice.

In a second line of research (Jordan, Spencer, & Zanna, 2002), we proposed that individuals with high explicit but low implicit self-esteem have fragile or defensive self-esteem, whereas those with high explicit and high implicit self-esteem have a secure sense of self-worth. So far, we have discovered that those high in explicit but low in implicit self-esteem are particularly high in narcissism (a personality trait related to defensiveness) and reduce dissonance in our Chinese restaurant paradigm (a social psychological phenomenon related to defensiveness) more than any other group (Jordan et al., 2002; Jordan, Spencer, Zanna,

Hoshino-Browne, & Correll, in press). In his doctoral thesis project, Christian has also discovered that following a direct threat to their self-worth, individuals with defensive self-esteem stereotype and discriminate the most (cf. Fein & Spencer, 1997).

Thus, although we do not claim that measures of implicit attitudes assess unconscious attitudes, it is clear that the interaction between explicit attitudes (or more thoughtful evaluations) and implicit attitudes (or more automatic evaluations) may often lead to a greater understanding of classic phenomena in social and personality psychology. And, in any event, this research clearly foreshadows (I believe) a third generation of research on implicit attitudes, in which theorists hypothesize that interactions between explicit and implicit attitudes (i.e., specific explicit-implicit mismatches) are likely to test old, previously intractable theories and lead to novel predictions.

The fifth case study concerns the failure of mediation analysis that I fully expected to work out. In the last section, I described a study with Leanne Son Hing that demonstrated that an hypocrisy induction reduced discrimination in aversive racists, presumably (we thought) because it made them feel guilty (Son Hing et al., 2002). And, in fact, the guiltier they felt following the hypocrisy induction, the more they bent over backwards. So, again, imagine my reaction when we discovered that a traditional Baron and Kenny type mediation analysis did not work. Well, first I was confused because I thought mediation analysis had every reason to work. An induction that was predicted to create a psychological state in aversive racists did so. And the more the psychological state (of guilt) was produced, the less aversive racists discriminated. Second, precisely because I was dumbfounded, I actually had to think hard about why mediation analysis (as currently practiced) could not confirm what seemed obvious to me: that the hypocrisy induction made aversive racists feel guilty, which in turn made them bend over backwards not to discriminate against Asians in the future. In thinking through the logic of mediation analysis (apparently for the first time!), it became clear to me that the Baron and Kenny type mediation analysis is not always appropriate in the context of experimental research. First, everyone would agree that it would be inappropriate (i.e., it wouldn't work) for the hypothetically perfect experiment in which all the variance in the outcome variable (e.g., discrimination) and the presumed mediating variable (e.g., guilt) is between-condition. Once condition is taken into account, there is simply no variance left in the mediator to relate to the outcome—which has no variance to which to relate in any event. Of course, this case would never occur in reality. However, it occurred to me that our aversive racism study was prototypical of many experiments in social psychology in which participants in a control (or baseline) condition that are not in the psy-

chological state under investigation are compared to participants in an experimental condition in which the psychological state is induced, typically to varying degrees. Because the critical psychological state to be induced in the experimental condition does not exist in the control condition, there is no reason to expect that the measure of the presumed mediator (e.g., guilt about discriminating against Asians) will relate to the outcome variable (e.g., discriminating against Asians) in the control condition—either because there is very little variance in the measure or, more important, because it really is not a measure of the mediating variable (but instead, for example, a measure of guilt about something other than discriminating against Asians)! And, if there is no relation between the presumed mediating variable and the outcome variable in the control condition, there is very little variance left in the mediator to relate to the outcome once condition is taken into account in the mediation analysis—and, thus, mediation analysis will not work.

In our experiment, aversive racists reported hardly any guilt in the control (or baseline) condition and did so with very little variance—and whatever variance in guilt they reported presumably had more to do with cheating on their girlfriends or boyfriends or failing to call home to mom or dad than with feeling guilty about discriminating against Asians, given the fact that they had just written a strong, forceful, proattitudinal essay urging their fellow students not to discriminate against Asians.

Thus, when the relation between the presumed mediating variable and the outcome variable theoretically ought to be moderated by condition (such that there should not be a relation in the control condition and there should be a relation in the experimental condition), a Baron and Kenny type mediation analysis should not be expected to work and, thus, is inappropriate. This insight has inspired an article (Spencer, Fong, & Zanna, 2004) about the general strategies of mediation analysis, focusing on the conditions when it does versus does not make sense to do a Baron and Kenny type analysis (Spencer et al., 2004). Among other points, we suggest that it only makes sense to conduct a standard mediation analysis when there is theoretically relevant variance on both the presumed mediating variable and the outcome variable in both the baseline and experimental conditions. It is our hope that this article will eliminate the seemingly mindless current practice of mediation analysis in the experimental context—in ourselves and, hopefully, the field.

#### **An Earlier Extension (of a Phenomenon to a New Domain)**

Extending a line of research also appears to be a recurring theme. An older example that immediately comes to mind is my work on self-fulfilling prophecies in



the context of between-group interactions (Word, Zanna, & Cooper, 1974; Zanna & Pack, 1975). In the early 1970s, virtually all the work on self-fulfilling prophecies investigated either experimenter- or teacher-expectancy effects. Because I have always construed stereotypes as expectations (cf. Olson, Roese, & Zanna, 1996), I simply thought it made sense to take the notion of self-fulfilling prophecy as a temporary given and extend research on self-fulfilling prophecies into the domain of intergroup relations. The experience of designing the Word et al. (1974) studies also taught me early on (a) how to study mediation processes across studies and (b) how important it is to have a clear and provocative “take-home” message: in this instance, that if White applicants were treated like Blacks, they wouldn’t do very well!

### **Integrating One’s Own Lines of Research**

My sixth (and last) case study concerns integrating my own lines of research. Although it is relatively easy to come up with ideas for follow-up studies in between sabbaticals, in my experience many of my ideas for new lines of research have occurred to me while on sabbatical. My strategy for sabbaticals is simple. First, I always go away to an intellectually interesting place. Second, I try to tie up loose ends to clear my mind. Third, I try to get into a playful, curiosity-based mindset. For me the best way to create this sort of mindset is simply to immerse myself into the various, ongoing research programs of the faculty and graduate students at the university I am visiting. Hopefully, if and when I have cleared my mind and created a playful mindset, some new ideas will occur to me. And, then, of course, I pester my new colleagues with my ideas to determine whether any of them are any good. Because I have given these new colleagues feedback on their own work, they usually reciprocate by providing excellent feedback and free associations to my ideas. And, interestingly, the fact that they are new colleagues increases the probability that I am likely to hear something that I would be unlikely to hear at home. (Of course, once I return to Waterloo, I pester my longtime colleagues soon enough!)

Interestingly, on the sabbatical that I just completed (at the University of Arizona), my new ideas were astonishingly surprising—at least to me. Specifically, I discovered that what I had considered to be separate lines of my own research (separate but primarily, I guess, because I am conducting these lines of research with different graduate students) might very well enrich each other in interesting and novel ways. To cite just one example, I have a variety of research projects that focus on overcoming resistance to persuasion, including the program of research on subliminal priming

and persuasion previously discussed (Strahan, et al., 2002); a program of research on using self-affirmation to create a more open-minded, less defensive message recipient (Correll, Spencer, & Zanna, in press); and a program of research on narrative persuasion (Dal Cin, Zanna, & Fong, 2004).

While on sabbatical, it became clear to me that to demonstrate that one has overcome resistance to persuasion, one must first more clearly demonstrate that message recipients have, in fact, resisted persuasion. And, given the fact that I now had the time and space to think, it occurred to me that defensive individuals (i.e., those high on explicit but low on implicit self-esteem) might be (a) the most likely to resist persuasion and (b) the most likely to be influenced by a self-affirmation opportunity preceding persuasion. Further, once I was “on a roll,” it even occurred to me that self-affirmation might be especially effective in overcoming resistance to persuasion for defensive individuals if the affirmation opportunity were to be subliminal.

At this point I do not even know whether I will pursue these ideas. What I do know is that it is unlikely that I would have had these ideas had I not been on sabbatical. (By the way, please don’t tell my chair, and especially my dean, that the “big insights” of my last sabbatical were that various lines of my own research could, and probably should, inform each other!)

### **Conclusion: The Value of Taking Temporary Givens Seriously**

On reflection, it seems that a general principle that may characterize or underlie much of my search for ideas good enough to pursue follows from taking something in which I believe strongly (e.g., a theory, some data, an experience, or even an intuition) as a temporary given and, in effect, assuming that if  $X$  (i.e., the temporary given) is true, then some interesting,  $Y$ , should follow—and when  $Y$  does not follow, trying to figure out why.

In the case of ambivalence, once introduced to the notion of simultaneous accessibility by John Bassili, I took this concept as a temporary given and thought it might moderate the potential ambivalence–felt ambivalence relation. In the case of culture and dissonance, there were two temporary givens: first, my conviction that dissonance was a universal phenomenon, and second, the finding from cultural psychology that individuals from collectivistic cultures have interdependent self-concepts. Together these assumptions led to the notion that Asians ought to experience dissonance, but only when their interdependent selves were threatened, and that to take the sting out of dissonance for Asians one would have to affirm an interdependent self. In the case of subliminal priming and persuasion, the temporary given was that although subliminal persuasion per

se is not possible, subliminal priming is. Given this assumption, the notion that subliminal priming might enhance persuasion, especially if the primed concepts are functional or applicable, seemed to follow. In the case of implicit attitudes, the temporary givens were the beliefs that both explicit and implicit attitudes were authentic and that the mismatch between them might be, at least, diagnostic of interesting psychological states. Given these assumptions, the notion that aversive racists might very well be characterized as those low in explicit but high in implicit prejudice and the notion that defensive individuals might very well be characterized as those high in explicit but low in implicit self-esteem both seemed to follow. In the case of my "mindless" use of mediation analysis, the temporary given was the conviction that our mediation hypothesis had to be correct, even given our failure to confirm it. Because this temporary given led to a disconfirmation, I had to become "mindful" to seek a solution to this puzzle. Finally, in the case of overcoming resistance to change the temporary givens (or insights) were, first, the notion that to demonstrate overcoming resistance to persuasion one must first clearly demonstrate that a communication has, in fact, been resisted, and second, that our ongoing research on defensive self-esteem suggested precisely who might be most likely to resist.

I know many other ideas that I have pursued over the years may not be able to be characterized in these (undoubtedly oversimplified) ways.<sup>3</sup> Nevertheless, the exercise of introspecting about how I have come up with my most recent ideas (and reflecting back on how I came up with older ideas) was fun and illuminating (at least for me). I hope this exercise (along with all the other articles in this special issue) might also be helpful to others who are engaged in the research enterprise, especially those who are just beginning to discover the joy of doing research in social psychology on their own.

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<sup>3</sup>For example, recently we simply tested Bob Altemeyer's (1998) insightful (and chilling) lethal union hypothesis that, in our version, metaphorically demonstrated that social dominance oriented leaders would, indeed, "sell their grandmothers down the river" for profit, happily aided and abetted by right-wing authoritarian followers (Son Hing, Bobocel, & Zanna, 2002a).



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