The White Bear Story

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It is dangerous to comment on your own article. There just isn't a good way to do it. At one extreme is the specter of the gushing award-winner ("You really *like* me") who goes on too long, turning what should be a happy moment into an unseemly orgy of self-congratulation. At the other extreme is the self-effacing commentator who spends so much time in exaggerated thankfulness to all the little people, or in remarks about standing on the shoulders of giants, that the accomplishment looks like a big mistake. On the assumption that there is a fine line between narcissism and toadyism, and that we might somehow tightrope on it for just a few pages, we have agreed to write an article about an article of ours.

So, let's take a quick look at the origins of the white bear studies (Wegner, Schneider, Carter, & White, 1987). We do so with a glimpse at what we think might have been the beginnings of the idea, a glance at where it seems to have led, and a peek at the nature of "classic" articles in general. Already we're feeling slightly off balance, so let's get on with it.

Getting Our Bearings

There are several stories that could be told about where the white bear article (Wegner et al., 1987) came from. The obvious ones are that (a) Freud (e.g., 1924) thought of all this long ago and developed it into a grand theory that informed an entire century of psychology and (b) both Dostoyevsky (1955) and Tolstoy (see Simmons, 1949) wrote about how difficult it is to avoid thinking about a white bear. A healthy shot of Freud mixed with a dash of Russian literature, all poured over the crushed ice of the cognitive revolution, and you have the white bear cocktail.

Here's another story. Two social psychologists at different universities in San Antonio get together to chat. Both are interested in person perception and self-perception, and one had read a quote from Dostoyevsky about white bears years before (unfortunately, in a *Playboy* magazine in college). The other had been thinking about people's inabilities to control strong emotions. They had talked a lot about how diffi-

cult it is to disregard false information (as in impression perseverance; Ross, Lepper, & Hubbard, 1975), and one of them had even done some studies of how readers could not avoid drawing inferences from information that was denied in headlines (e.g., "Talbert Not Linked With Mafia"; Wegner, Wenzlaff, Kerker, & Beattie, 1981). Perhaps the study was an escalation of the war against unwanted information. If people cannot overcome information when they are informed it is false, what happens when they earnestly try not to think about it?

Another story could also be told. Two bright, starry-eyed undergraduates at Trinity University—Sam Carter and Teri White—each decided to do senior honors thesis work with a psychologist. They picked Wegner, and first Carter and later White got involved in an oddly unnerving project. It seems that Wegner had this colleague Schneider from across town at the University of Texas at San Antonio, and Schneider was making fun of the plan and saying how Wegner ought to straighten up and do research on normal things. The students laughed tensely in these meetings, taking minor solace in the fact that Schneider seemed jovial enough that he might possibly be kidding. Both Carter and White responded by working earnestly and carefully in the lab and creating masterful thesis research. Carter chalked the data from the first study on the board in Wegner's office, and it stayed there for 2 years while Wegner rhapsodized about it to most everyone who walked in. Schneider turned out to have a soft spot for bizarre phenomena and soon succumbed, and they all tried to publish it in *Science*, where they were coldly turned away. Fortunately, Editor Jim Sherman and a band of merry reviewers at *Journal of Personality and Social Psychology* brought the article (Wegner et al., 1987) in from the cold.

And then there is the true story. It was a conspiracy of white bears. Of course, there were the original Russian bears who started things by eating people and getting the novelists to mention how much everyone was hoping to keep such dangers out of mind. But there were also stuffed animal bears, dozens it seems, who wanted to be sent to the Wegner family's first child,

Kelsey, when she was born. There was a smiling one on a sign outside the White Bear Pub in Bristol, England, who wanted to have his picture taken with Wegner for some reason. There were others who volunteered to appear in photos to be sent to Wegner and to Schneider from Los Angeles, Tucson, Austin, and even Bearsville, NY, and still others who wished to appear in psychology textbooks to give readers visual relief and something to try not to think about. Yes, white bears were behind the whole thing.

Bearing Ahead

Where has the white bear article (Wegner et al., 1987) led? This is not a good spot for a big review, so we can just point out that there are already several reviews that suggest white bears have been loping off in many directions (Abramowitz, Tolin, & Street, 2000; Beevers, Wenzlaff, Hayes, & Scott, 1999; Bodenhausen & Macrae, 1996; Monteith, Sherman, & Devine, 1998; Wegner, 1989, 1992, 1994; Wenzlaff & Wegner, 2000). Some of the main bear trails are worth noting.

One happy circumstance for the article (Wegner et al., 1987) was its relevance to clinical concerns. Thought suppression is, of course, a standard bandage people use when they are sad, anxious, worried, or obsessed, and, as we ultimately discovered, streams of research in clinical psychology were very close to the topic of thought suppression when the article surfaced (e.g., Rachman & De Silva, 1978; Salkovskis, 1985). Fortunately for us, Rich Wenzlaff had been our student in San Antonio, and his clinical take on thought suppression, along with his knack for research, quickly lifted the white bear phenomenon into view in this literature (e.g., Wenzlaff, Wegner, & Roper, 1988). And fortunately again, Jamie Pennebaker had started to uncover the remarkable health effects of disclosure at just this time (Pennebaker, 1985) and so provided a therapeutic alternative to suppression that captured the clinical imagination as well.

The white bear article (Wegner et al., 1987) also appeared at about the same time as a major breakthrough in the study of prejudice—Trish Devine's (1989) own classic article analyzing the automatic and controlled components of stereotyping. This discovery was unceremoniously dragged into church and married forever to the white bear by Neil Macrae and his team (Macrae, Bodenhausen, Milne, & Jetten, 1994), prompting a rash of research on the self-control of prejudiced thinking. The further connections that were discovered between thought suppression effects and accessibility effects (Wegner & Erber, 1992) then linked the white bear to people walking slowly (Bargh, Chen, & Burrows, 1996), and the development of the ironic process model of thought suppression (Wegner,

1994) further implicated the bear in problems of cognitive busyness (Gilbert, 1991). Needless to say, this pattern of kindred developments could not have all been happenstance. There must have been a zeitgeist. The white bear was part of a movement in social psychology to understand the unconscious underpinnings of social behavior and consciousness.

Too Much to Bear?

As representative authors of the white bear article (Wegner et al., 1987), we are delighted that several respected colleagues have called it a "modern classic." We hope that this appellation allows us the brief luxury of talking for a moment about what it takes to make a classic. Before we reveal ourselves to those who don't know us as the pompous windbags we are, we should note two key facts. First, we have published a lot of other articles that can't even be mentioned in the same room with the word *classic*, so our formula for making a classic is not something we can call the least bit reliable. Second, we reach our observations on the nature of classics in social psychology by studying what we consider to be some real classics: Milgram (1963), Asch (1952), Festinger and Carlsmith (1959), Schachter and Singer (1962), and Darley and Latané (1968).

What does it take to make a classic? We agree with Fiske (this issue) that classics often make people uncomfortable. Most of the social psychological studies that have moved into the classic column make people look morally bankrupt or intellectually challenged. It is not just that classic studies are counterintuitive—although they usually are—it is that they are pejorative. They make people look bad. The bubble is popped, the illusion shattered. Good research in any science does this, but the expectations broken in social psychology are particularly noteworthy because they are expectations about ourselves and the people we need to trust who are all around us.

Beyond this basic minimum, there are other special features of classics that are worth noting. A classic article must have *subjective appeal*, for instance, in that anyone can immediately empathize with the participant in the study. Fiske is right that these studies make us uncomfortable, but they do this because we can rapidly slip into the participant's mind-set—seeing all too quickly how easy it would be to press those shock buttons or to look away when someone needs help. We become the research participant—just as when we read a good novel—and when the participant in the classic study does wrong, we feel all of the participant's inner pangs and squirmings. The classic study is gripping, a "good read," because it engages empathy with the experimental participant (Wegner & Gilbert, 2000).

Classics also share a *simplicity* that is almost embarrassing. The basic study can usually be described in a sentence (this is a good test for a graduate student, by the way—if the student can't describe a planned study in a sentence, perhaps it shouldn't be done!). The classic study also can be summarized in a single comparison, at most two, and never, ever goes into the kinds of factorial designs that make analysis of variance teachers so proud. This simplicity promotes another feature of classic studies—what might be called *fertility,* a set of methods that make them easy to replicate and extend. The classic study becomes classic not just because it is a good study (most of them are certainly not experimental design classics), but because it survives the evolutionary winnowing pressures that occur in the process of scientific consensual validation. The only way for a study to be validated properly is if just about any team in any lab can carry it off successfully and finds the procedure straightforward enough that this is not daunting. Each time we get inveigled in a seriously complicated method that will produce "just the right evidence," we reflect on whether this is something anyone else would ever care to do or be able to do again.

Classic studies curiously share the absence of several features that we all have come to expect should be part of good psychological research. One of the missing elements, for example, is theory. Although some classics do focus on theory (Festinger & Carlsmith, 1959; Schachter & Singer, 1962), this does not seem to be a prerequisite. In fact, even the classic study that features theory often comes with a Teflon theory-proof slipcover, an inexplicable quality that keeps most any theory from sticking for long and leaves multiple theorists and researchers striving to attach their favored formulation. On the assumption that the white bear article (Wegner et al., 1987) echoes some characteristics of the real classics, it is worth noting that we did not have much of a theory of thought suppression—just a rebound effect. Although a theory later developed (Wegner, 1994), the theorizing is by no means over, and there are still those that aspire to explain the effect in new ways. It is unfortunate in this light that major social psychological journals now emphasize the importance of evidence on processes rather than just observations of effects—sadly, this seemingly well-reasoned advice may undermine the production of classics in the future. We think theories are wonderful and are eager to have them bless our every empirical enterprise. However, the blessing is mixed when the theory always has to be there from the outset. The classic studies teach us that not every interesting finding can be fully explained in the same breath with which it is reported.

Another missing element in the classics is the need for a control group. Yes, all the classics contained control groups of one sort or another—comparisons that made the experimental manipulation stand out. But often these weren't really needed. The findings stood out anyway: People obeyed too much, conformed too much, justified their actions too much, did not help enough, or changed their emotional state too much. The comparisons in the classics were implicit contrasts with intuitive expectations. The classics had control groups as a kind of window dressing that made them scientific exercises rather than simple demonstrations—they didn't need control groups because these groups were absolutely necessary to make the point. Again, this observation seems to hold for the white bear effect. People in the study thought about a suppressed thought more than they should have—because one would hope somehow that suppression would just stop a thought completely. We certainly couldn't publish an article in which the bar graph contains only a single bar. But perhaps there is a lesson to be learned: Social psychological research is best when it just clears that single bar—to produce effects that simply should not happen.

A final missing element in the classics is an emphasis on application. Yes, we know what Lewin (1951, p. 169) said—that there is nothing so practical as a good theory—and we are fully in favor of applications of social psychology. However, applied social research does not seem to grow to classic status. Although there are lots of applied implications of the classics, studies that start out in applied domains do not create the kinds of general principles that characterize classics. The study of pure science has its place, then, in producing classic nodes in the network of knowledge that can reach out to inspire application.

Conclusion

An article about an article is, as we have said, a hard thing to write. And if you think it was also hard to read—what with all the insufferable bear puns—imagine what it must be like being us and having everyone you know think that they have to mention white bears in some cute way each time they see you. It's enough to create an obsession.

Note

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