

Angry Birds

By John A. Bargh, Ph.D.

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The discussion sparked by my previous post has now far transcended the remarks I made in the post itself, in defense of our lab in the face of the "Clever Hans" charge. That was a slur on our lab that had to be responded to in order to set the record straight. Insults like that typically make people angry, and so a lot of heat was generated, but too much heat produces smoke, and smoke obscures clear vision. Let's see if we can continue the discussion without anger and hostility clouding the real issues.

Based on the more thoughtful replies to the last post, there are 3 main points I'd like to make now. The first concerns the valuable role of replication in science, the second concerns the "art" versus "science" of social psychology (the "insider knowledge" issue), and the third concerns what this all means for the nature of the unconscious and for the unconscious as an object of scientific inquiry—which after all, is the reason for this blog.

On replication and so-called "failures to replicate"

Back to the elderly priming study. As mentioned last time, the results were predicted in advance by theory—mainly Wolfgang Prinz's pioneering work on the overlap between perceptual and actional representations. The elderly priming studies were conducted in 1991. The graduate students in charge of the project were surprised that it worked, they certainly did not expect it. When they came to me with the results of what was eventually published as Study 2a in that paper, and expressed their surprise, I immediately said then let's run it again and make sure. So we did, and obtained exactly the same results, and published the replication as well as Study 2b in the article.

Even then, however, in 1991, we did not rush to publish it. We ran a further study (Experiment 3 in that article) in order to conceptually replicate the basic stereotype-behavior effect. This conceptual replication was designed to be as different as possible to the elderly priming study in operationalizations and concrete details while retaining the hypothesis that activating a stereotype in the perceiver without the perceiver's knowledge would increase the perceiver's own behavioral tendencies to act in line with the content of that

stereotype. By changing as many of the concrete details of the study while retaining the more abstract conceptual hypothesis, we were striving to test the generalizability of the basic stereotype-behavior principle, beyond the particular elderly stereotype and walking speed measure of Study 2, to a different stereotype, a different method of activating it, and a different behavioral dependent variable. If we obtained the same basic effect then we could be more confident that the effect did not depend on the particular way we ran the elderly study and was not for some fluky reason limited to the elderly stereotype or our walking speed measure.

Accordingly we replicated the classic Devine (1989) Study 2 but with a behavioral rather than a judgmental dependent variable. For our nonconscious primes we showed African-American or European-American male faces subliminally, at very brief durations and immediately pattern masked, during an intentionally boring focal experimental task. The behavior we measured was hostility, not walking speed. Again the experimenter was blind to the participant's priming condition. The hostility measurement was made by other people unaware of the study hypotheses and unaware of the participants' experimental conditions, as we videotaped the participants' reaction to the experimenter's request to do the boring task all over again because of a computer error that had lost their original data. And we did obtain the stereotype-behavior effect again, activating this different stereotype once again produced stereotype-consistent behavior, without the participants' awareness of this influence on them. [This particular experiment was replicated and extended in Chen & Bargh (1997)].

And even then we did not submit the package of studies, until we learned that the same conceptual effect had been obtained in other labs, unaffiliated with our own. Most notably, Steele and Aronson's (1994) demonstration of 'stereotype threat' effect was a clear conceptual replication. It relied on a very similar indirect priming of the group stereotype (an ethnic-identity checklist at the top of the main dependent measure). They found that activating the stereotype produced decrements in academic performance, as predicted from the content of the stereotype under study—that this social group was believed to be (in the wider culture) poor at academics. At around the same time, we learned that Dijksterhuis and van Knippenberg (1998) had run two further stereotype-behavior studies, with entirely different stereotypes than anyone else had yet studied, and had also replicated the basic stereotype-behavior effect as well.

Only at this point did we submit our set of studies, and we submitted it to the top journal in our field, the one with the highest rejection rate of any American Psychological Association journal, and it was published in 1996.

So in response to the current criticisms and 'failure to replicate' charge, I do not see what more we could have done as far as being cautious, even conservative, in making sure our basic conceptual effect replicated, before we published it. We were aware from the beginning that the effect would be surprising to many (apparently, it still is, 15 years later), and so we immediately set out to replicate it ourselves. We then ran a further conceptual replication with all of the concrete details changed in order to make sure that the results were due to the hypothesized stereotype-behavior effect and not to the particulars of the elderly priming study per se. We even waited to see if other labs not affiliated with our own obtained conceptually similar results. And we submitted our studies to rigorous peer review, to the journal with the highest rejection rate, with an editor knowledgeable in the history and methods of social psychology. (The kind of editorial oversight, by the way, that the journal PLoS-ONE says that they do not believe in.)

Did we have "inside knowledge," not shared with others, that was necessary to produce our effects? As for the idea that there is 'insider knowledge' about how to produce these effects, or that there is an 'art' to these studies that is not shared and widely communicated to anyone who wants it, here again I need to set the record straight:

1. We were aware of this issue in the mid-1990s and so set out immediately to write down and make available to all everything that we knew and had learned by experience about how to conduct priming and automaticity research. Our opportunity came when Harry Reis and Chick Judd asked us to write a priming and automaticity methods chapter for their Handbook of Research Methods book published by Cambridge University Press in 2000. Tanya Chartrand and I compiled all that we knew on the subject and this chapter has been freely and easily available on our lab's website as well for over 5 years. Thus we were sensitive to the 'inside knowledge' issue long ago and made every effort to share what we had learned about doing this kind of research, with anyone who took the time and effort to read it.

2. Believe it or not, folks, a PhD in social psychology actually means something; the four or five years of training actually matters. It seems as if those from other fields believe that they (and anyone else, for that matter) can run social psychology studies without any training or scholarship in the history of the ideas being tested, and without making any effort to read the relevant literature on those ideas. What these researchers believe is special 'inside knowledge' is actually what graduate students in social psychology typically learn, at least in labs working in the area of automaticity and unconscious processes. The attitude that just anyone can have the expertise to conduct research in our area strikes me as more than a bit arrogant and condescending, as if designing the conducting these studies were mere child's play. Perhaps the representativeness heuristic is at work here, with the use of basic, mundane dependent variables such as walking speed down a hallway being mistaken for only basic, mundane thought being required to design the experiment in the first place.

No article in any field can or should be expected to recapitulate all of the hard-won knowledge of that field—journal space (except, perhaps, in pay-as-you go journals) is too precious and limited for that, and at least some degree of training and scholarship—reading the relevant published research in that field so as not to be ignorant of it when designing one's own studies—is expected of those who want to perform quality research, as free as possible from obvious design flaws. For example, brain imaging or fMRI journal articles do not each repeat that participants were screened for the presence of metal in their bodies, that care was taken not to bring anything metallic into the chamber while the magnet was on, and so forth. Such standard procedures are assumed to be known by anyone performing such research, including anyone who is attempting to replicate the research described in that particular article.

Yet some have said that—getting back to the particulars of the elderly priming study—knowing not to call the participant's explicit attention to the dependent variable being collected, as when Doyen et al. called their participants' attention to how they were supposed to walk down the hallway leaving the experiment, is somehow 'inside knowledge' that they could not have been expected to know. There are two rather obvious responses to this: First, calling their participants' attention to how they walked down the hall was a change, an addition to our 1996 procedure, and so already violated the standard scientific principle that when replicating a previous study you follow the

procedure as closely as possible, and you do not make changes to it if (as Doyen et al. did) you want to later argue that you 'failed to replicate' the prior study. Second, and more germane to the 'insider knowledge' charge, if you had been trained in social psychology, or at least had read the main journals on your own then you would have been aware of the many demonstrations over the years that conscious attention and thought directed to the automatic or unconscious process under study is likely to interfere with the expression and manifestation of those automatic processes. We have known this not only in social psychology but in cognitive psychology itself since the classic work and model of Posner and Snyder (1975), and Jim Neely's programmatic 1977 tests of that model. Neely's study showed that conscious expectancies concerning what kinds of targets followed what kinds of primes could override automatic spreading activation effects (if the participant were given more than a half second to respond).

In social psychology we have known this principle since the classic Langer, Blank, & Chanowitz (1977) 'xerox machine' study, in which participants 'mindlessly' agreed to bogus requests by a person to jump in front of them in a queue - except when the cost of doing so was too high (triggering conscious attention or 'mindful' processing to the request). Abraham Tesser (1978) and later Wilson & Schooler (1991) showed how conscious thought moved one's attitudes away from their 'natural' position and thus lowered later attitude-behavior consistency. Shelly Chaiken and I (1992, 1996) showed that removing strategic, conscious thought components from the automatic attitude paradigm had the unexpected effect of increasing the generality of the effect - the less conscious, strategic involvement there was in the experimental task (as when participants pronounced, rather than evaluated the target stimulus), the more easily the automatic attitude effect could be detected (and for a wider range of stimuli). Thus, the Schwarz & Clore 1983 study mentioned in my previous post, which also showed that making a causal factor attentionally salient (as Doyen et al. did with their instructions to the participant as to how to walk down the hallway) attenuated the automatic, unconscious effect was just one example of a well-known and widely demonstrated principle, in both cognitive as well as in social psychology. It is not special inside information on our part.

Now, beyond the 'insider' issue, does the fact that conscious attention directed to an automatic process can interfere with it place a limit on the stereotype-behavior effect? Yes, it certainly does. Just like thinking too much about how to move your racket when serving

in tennis, or swing your golf club, can mess up what you normally and naturally do. Yogi Berra was once asked what he thought about when up at the plate; he answered "How can you think and hit at the same time?" The name of this blog is The Natural Unconscious for a reason.

Our lab strives to learn how the unconscious naturally operates in the real world. Our laboratory studies are designed in order that the conditions under which we test our hypotheses are as similar as possible to the real world outside the laboratory to which we want to generalize our results. This is known as ecological validity. Very few if any psychology studies prior to Doyen et al. end with the experimenter telling the participant how to walk down the hall when leaving the study. If you want to generalize to the real world in which a person encounters an elderly person or some other stimulus related to the elderly, in that real world setting no one is telling the person how to walk. Doing so as part of the laboratory study is thus un-natural. Anyone genuinely interested in how the unconscious naturally operates in normal life would not have included such an instruction in their study.

What is really going on here? During my commute into work this morning I was listening to the sports-talk station, and the general manager of the Houston Texans football team was being interviewed. He was asked about a controversial draft decision he had made several years ago, and answered to the effect that 'you think hard and long about a decision before you make it; it may not turn out to be the right decision, but a lot of thought always goes into it'. In the case of discovering and publishing potentially surprising effects, all one can do is make sure the finding is grounded in theory, replicates in the particulars as well as on the conceptual level, submit the research to rigorous peer review, and if possible see if other labs can and do replicate the effect, at least conceptually. If all this is done, as it was, in good faith, I do not see what else a researcher could be expected to do. If in the long, for whatever reason, the effect does not turn out to be replicable (again, I do believe the stereotype-priming effect has been), then that is the self-correcting nature of science. There is nothing here, at least on the surface, meriting the current Sturm und Drang.

So then what is this really all about? Why all of the attention to the elderly-priming study and why the persistent beating of the "failure to replicate" drum? I do not claim to have knowledge of the motivations or agenda of the researchers who are engaging in this, but I do want to point out for those of you unaware of it that it does

seem that only certain types of studies are being singled out, and that the claim of 'failure to replicate' has been (misleadingly) applied over several years now to other studies demonstrating unconscious effects, prior to its most recent application to the elderly priming study. Moreover, the researchers seeking to widely disseminate the charge of 'failure to replicate' these particular studies seem to come from one specific domain of psychological research. You can draw your own conclusions.

In an article published last December, solicited by the editors of a special issue of the journal *Social Cognition*, I reviewed published research criticizing the Dijksterhuis & Nordgren 1996 "Unconscious Thought Theory" (UTT). Specifically, the studies I reviewed focused on the UTT claim that decisions were qualitatively (and objectively) better if made after a period of distraction (preventing conscious thought about the decision) than after the same amount of time consciously deliberating about the right choice. For that review I read over a dozen articles, nearly all in the journal *Judgment and Decision Making*, including one by co-authors of the Doyen et al. article, which claimed that they had "failed to replicate" the UTT finding. But this was misleading. All but one of these 'failures' had actually shown the equivalent quality of decisions made after a period of distraction, compared to the same period devoted to conscious deliberation. What they had failed to find was superiority of unconscious compared to conscious decisions. To be fair, UTT had claimed this superiority. Yet it went absolutely unremarked across this set of studies that unconscious thought had even in their own studies produced the same quality of decisions than did conscious thought. If the researchers were truly interested in the issue of whether unconscious thought could operate successfully in the judgment and decision making domain, one would have expected them to remark and comment on this important outcome of their studies. Yet all focused instead on the 'failure to replicate' UTT.

Thus the elderly priming study is another demonstration of an unconscious process that members of this same group have now, they claim, 'failed to replicate'. I won't repeat everything I've already said to the effect that this is again a misleading conclusion, especially at the broader conceptual level. I would like to instead ask the question of whence the apparent antipathy towards unconscious processes? Why are not studies of conscious processes ever the focus of these attempts to replicate?

There is a long history of such antipathy to unconscious processes, going back at least to Freud in *The psychopathology of everyday life* and most likely earlier than that. Since at least the 1950s people were genuinely afraid of brainwashing and mind control, and there is published evidence (Wilson & Brekke, 1994 *Psychological Bulletin*) that people are inordinately fearful of subliminal advertising (and not at all afraid of the powers of regular advertising, which is a much stronger influence on us). I understand all that; I just don't understand the scientific animosity towards unconscious processes among (some, many?) judgment and decision-making researchers. Shouldn't we, as scientists, be open-minded about the influences of both conscious and unconscious forms of thought? Isn't this an open, objective, scientific question, and not a matter of personal preference or ideology (though even there, I'm not sure why there would be a preference for promoting one type of process over the other)?

I want to be very clear: I do not know or presume to know what the underlying reason or motivation might be for the present situation, though I could certainly speculate (let's see: wishful denial of our animal nature, overgeneralization of one's own specific research domain to human life in general - something Freud himself was criticized for, generalizing from observations of abnormal case studies to a general model of normal human functioning). Scientists are human too, they engage in motivated cognition, and they are prone to biases and overgeneralizations, especially when these processes operate outside of their conscious awareness. You would think, then, that for the sake of furthering scientific progress and the (I would hope) search for objective truth that scientists themselves would want to know about the potential operation of these unconscious processes, and to not be closed-minded and resistant to the idea of their existence.

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