

Most Underappreciated

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In science, not all great ideas see the light of day. But now, thanks to *Most Unappreciated*, a new book edited by APS Fellow Robert Arkin, Ohio State University, we learn what a number of eminent social psychologists widely known for significantly advancing the field, think is their least appreciated – or in some cases, most misunderstood – work; hidden gems that didn't have the anticipated impact, or in some cases were influential for the wrong reasons.

“For some time, I have been asking colleagues ‘What is your most underappreciated work?’” said Arkin. “Nearly every conversation included a story, a recounting of a project or idea – often connected to some obvious emotion. Most were great stories, and some had all the earmarks of a therapeutic unburdening! I figured I'd better pull this together as a book, as a service to these tortured souls, the prospective authors, as much as for the benefit of any reader.” Many psychological scientists gladly accepted the opportunity to share their unique experience. “It was touching, to see these distinguished scientists take the time and space to offer advice, mentoring, ideas to young investigators just entering the field, using their own “unfinished business” and still-hidden gems to help others steer a better course.

The contributors are generous in sharing their stories, and the result is a fascinating collection of personal and professional insights about the processes and even politics of research. Presented here are excerpts from some of the essays featured in “Most Unappreciated.”

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Susan T. Fiske

Princeton University

Whatever Happened to Schema-Triggered Affect?

My very own first new idea as a fresh PhD was to swim upstream against the cognitive tide sweeping social psychology, to make a case for affect. My hunch: Social expectations invariably import affective baggage. Schema-triggered affect was born. The theory was simple. Categorization not only cues cognitive associations, but also triggers affective responses. Schema triggered affect, what's more, relied on instant categorization, not on memory for the data that originally generated the affective tag. My little lab had a great time designing schema-triggered –affect studies, some on close relationships, others on political person perception, and still others on stereotyping. These studies were not flawless, and we caved before obstacles and objections. I was swimming against the tide without a guide or a life preserver. I simply did not appreciate the importance of relentlessly persevering on a good idea, calibrating methods, fine-tuning analyses, until we had it right. I wish we had possessed the self-confidence, the wit, and perhaps the life preserver to have kept swimming. Still...who can complain, when other people did both the theory and the research much better than we did or would have done? The field has appreciated and absorbed those people's remarkable insights, to the benefit of all concerned. And now we don't have to do those studies.



Roy F. Baumeister

Imagined and Genuine Opposition to New Ideas on Sexuality

I spent much of the decade of the 1990's reading and writing about sex. One theme was female erotic plasticity: male sexual behavior tends to be more natural than cultural, whereas female sexuality is the opposite. Another examined the cultural suppression of female sexuality [which] found...that women [not men, contrary to evolutionary and feminist theorists' beliefs] are the main sources of influence restraining each other's sexuality. [This] led to sexual economics theory. The gist is that men want sex from women and therefore give women other resources (money, respect, attention, love, commitment) in exchange for it. How much they give (the "price" of sex) fluctuates with market conditions, such as supply and demand. We [also] found that men have a higher sex drive [than women]. [Unfortunately] The resistance came early. Sex researchers did not want to hear [any of] this. [One] factor, I fear, is that gender is inevitably part of sexuality, and these days gender is a heavily politicized topic. Some tried to discourage me from talking about these ideas. [Although] what I was saying was apparently not offensive or even controversial [,] over and over, people assumed it would be. [I learned that] It is not always the direct stifling of an idea [that suppresses it]; rather, well-meaning colleagues and friends warn you away from ideas and even from lines of inquiry.



James W. Pennebaker

University of Texas at Austin

The Idea, the Audience, and Me

I have had many good – Ok, brilliant, really – ideas slammed by grant and journal reviewers, editors, friends, and even family members. As a graduate student, I borrowed a fellow student's begonia plant to see it was possible to apply principles of temporal conditioning on the leaves of the plant [, only to learn that] pursuing interesting but possibly crackpot ideas prior to tenure is a very bad idea. My still-unpublished doctoral dissertation found that people's beliefs about their control over bodily functions could influence their actual control [, but] the average reviewer simply could not relate to the theory or phenomenon. This one seemed like a no-brainer...women and men use words differently, even when writing about the same topics. [Still,] the paper (Newman et al., 2008) was rejected by every major journal in psychology and social psychology-for years. [So] what makes for an idea that is appreciated by others? A truly appreciated idea – one that resonates with people – is one that is simple, directly

relevant to the real world as people experience it, and, ideally, makes people see that world differently, in new ways.



Carol S. Dweck

Stanford University

Buried Treasures: Depression, Murder, Praise, and Intelligence

The field of psychology, of the world beyond, did not embrace our work [on implicit theories] in the way we had hoped...we have identified the consequences of believing that people have fixed traits versus malleable qualities [on depression and moral judgment]. [Since] these findings have interesting implications for the judicial process [we thought] surely these findings would cause a stir, but not even a ripple has been detected as yet. I have come to realize that much of the public does not understand why research is special. They do not quite grasp why someone with good evidence should be heeded more than a charismatic guru who simply has conviction.



Philip G. Zimbardo

Stanford University

Saga of My Stealth Bomber Chapter: Can't Miss, but Vanished Without a Trace

I needed to resurrect my career, which had been stuck in a basement dungeon of the Stanford Prison...so I would start with a new theory that generates a range of testable hypotheses, present a series of original

experimental tests using novel and traditional methodologies, [and include] pithy quotes and hundreds of references that cut across many disciplines. Wait, I almost forgot the special ingredient that should generate a heated exchange in our field: Do a series of unethical experiments. In this case, the point of some of the research was to predict the onset of psychiatric symptoms of paranoia, phobia, and somatic disorders – by making normal student participants mad, for a short time. Last but not least was a provocative title: “Discontinuity Theory: Cognitive and Social Searches for Rationality and Normality – May Lead to Madness.” We [demonstrated] that the most ordinary process of trying to make sense of a disharmony in one’s experience can get side-railed into a biased search for a meaning or the biased search to appear normal that generates irrational thinking and abnormal reactions. In every society, madness equals irrational plus abnormal in a given person. [Sadly, this] can’t miss *numero uno* top-of-the-hit-parade chart buster vanished somewhere between Stanford and the Bermuda Triangle. My take on this abysmal failure [is that] it was too cognitive for social psychologists at that time, and too social for cognitive psychologists, and too clinical for both groups. The title made it seem like it was all about “madness,” which isn’t scientific, and it was much too long. I end my sad saga of this crash-and-burn flight of fancy with the hope that somebody out there will read it and tell me how to repackage it to revive it, and make it work as intended – or proclaim: Let it Rest In Peace.



Constantine Sedikides

University of Southampton

The Causal Structure of Person Types and Stereotypes

The fruits of our [Craig Anderson and I] joint research on implicit personality theory were most rewarding for me, both professionally and personally. One of these fruits, however, the Sedikides and Anderson (1994) article ...has been more forbidden than sweet. This article capitalized on the so-called typological view of person perception. People, this view has it, think about others’ personalities in terms of types, [which] consists of several traits. Knowing one trait within a given type allows one to predict other traits in the same type. More importantly, traits within types have a rather mysterious form of interconnectedness [that we wished to study]. We reasoned that traits within each person type are seen as causing each other. Take, for instance, the type “depressed.” Being *lonely* may cause someone to be *gloomy* and *pessimistic*, which in turn causes that person to be *unhappy* and subsequently *fearful*. We put this idea to the test [and found] the hypothesis that causal connections glue together traits within

person types received empirical backing. [Despite the various] implications relevant to both basic and applied research as well as real-world interventions, [especially] the compelling explanation for why stereotypes are resistant to change, this little-engine-that-could has not exactly taken the field by storm. Perhaps because I did not follow it up and did not pursue the implications of these findings. The field (like any vibrant and diverse scholarly field) needs to hear a certain message again and again, and then once more, before it begins to assimilate it. Perhaps...[it] has to do with the methodology: It was rather involved and cumbersome. Or, perhaps the idea (horror of horrors) was not the revolutionary. We shall never know. Regardless, I [still] felt I benefited a lot from this work.



Paula M. Niedenthal

CNRS and University of Clermont-Ferrand

Some Things Get Better With Age

It all started one day when Bob [Robert B. Zajonc, one of my graduate school advisors] announced...that if facial expression had a causal relationship to emotional state, then the mimicry of another person's facial expression (because it produces corresponding use of facial musculature) should be a fundamental mechanism by which we know another person's emotion; one basis of empathic understanding. If this is so, then people who are married for a long time should grow to resemble each other. People who are married are [often] trying to understand each other's feelings. If they have to mimic each other in order to really empathize, then they should use their facial muscles in a very similar way over years...[leading to a] particular development of the many facial muscles that participate in facial expression, [and therefore] an increased resemblance from before the time of their wedding. So we started the research [and found that] compared to randomly created pairs, individuals who were married to each other grew to look more alike over the course of their marriage. Furthermore, and important for the empathy and facial feedback account, this relation was even stronger for those couples who reported that they were happy, held the same attitudes, and were basically well adjusted. We were very excited about these results, [but] the work had absolutely no scientific impact whatsoever. No one in the social psychology community knew what to do with the findings... To my complete surprise [however], influential neurophysiologists were fascinated by the results. Later [on], a famous neurophysiologist...described our findings on National Public Radio. It took all those years of embarrassment, discouragement, and sometimes hilarity, but the findings were finally appreciated by scientists.



Norbert L. Kerr

Michigan State University

HARK! A Herald Sings...But Who's Listening?

I occasionally teach a graduate research methods course, and on one occasion I used Judd et al.'s (1991) excellent textbook. [Two] chapters are full of very good advice on how to write a clear, engaging, and publishable research report. But... one piece of advice that bothered me [was that it seemed] to legitimize taking a genuinely *post hoc* hypothesis, one not foreseen or credited when the study was conceived and designed, and presenting it in the introduction of one's write-up as if it were a genuine *a priori* hypothesis, one that justified and guided the research. I came to call this practice HARKing (*Hypothesizing After the Results are Known*). This bother[ed] me so much [that] I gradually moved from fretting and grouching about HARKing to studying it in somewhat more systematic fashions. I did a survey of social scientists to get some descriptive data (e.g., Does HARKing occur? How much? Why?) and some prescriptive information (i.e., Should HARKing occur?). [I found that while] (at least some) forms of HARKing were widely seen as improper, [it was still] a widespread. The reactions [to my findings] were usually strong, ranging from outrage that I was airing "dirty linen" in a way that might damage the discipline of social psychology to outrage that anybody might ever engage in HARKing to relief that finally somebody was talking about a dilemma that authors constantly face. I seemed to be hitting a nerve. [Although my paper] was published in an excellent journal [and] has been cited respectably, I remain deeply disappointed in the paper's (lack of) impact. Most bothersome is the possibility that HARKing really is a serious problem – one worth discussing, debating, and researching – but that I simply failed to convince others of this. The biggest disappointments are felt when you have tried your best to excite others about an idea or a problem that has really excited you, but failed.



Judith Harackiewicz

University of Wisconsin

I Can't Explain

I can still remember how excited my graduate students were when they came to show me the results of our first pinball experiment, and how much fun it was to title a paper – “Rewarding Pinball Wizardry.” We had found an old mechanical pinball machine and rewired it so that we could control participants’ scores in a naturalistic manner. We were interested in... performance-contingent rewards. Whenever you reward someone for doing well relative others, there is the possibility of achieving excellence, symbolized by that reward, but there is also the possibility of feeling pressured or threatened by the performance evaluation implicit in that reward structure. We argued that rewards for competence might have negative effects due to the stress of performance evaluation, but that they might also have positive effects because they provide positive feedback upon receipt; even more important, they inspire engagement and involvement from the outset as people strive for rewards that symbolize the competence they attain. [With our supporting results], we thought that we had isolated different reward properties with this design, and could explain *why* performance-contingent reward can have both positive and negative effects. In hindsight, the results were more complicated than we realized. We [also] introduced the term “symbolic cue value” to account for the power of a reward to inspire positive striving toward competence, but failed to convey the importance of this idea, which went against prevailing notions about the detrimental effects of rewards. Paralyzed by a desire not to offend the theorists who had inspired us, or held back by political correctness, we seemed unwilling or reluctant to simply state that rewards can be good when they inspire people to strive for excellence. The pinball paper never attracted much attention, [but] I still think it’s probably the best experiment I’ve ever done, even if I can’t explain the results clearly 25 years later.



Joseph P. Forgas

University of New South Wales

Episodes in the Mind: Or, Beware When the Paradigm Shifts...!

What I consider to be my most consistently ignored or underestimated work happened rather early in my career – actually, it was my doctoral research! I [was] intrigued by the possibility of using this [multidimensional scaling] to analyze and represent the way common, everyday social episodes are mentally represented by people in their daily lives. Within a couple years, I ran something like eight empirical studies mapping people's mental representations of the social episodes that occurred in their daily lives. I thought that I was onto a good thing with this line of research. It was solidly empirical, told us something inherently interesting and fascinating about the way people implicitly think about and mentally represent real and recurring social situations, and made a useful contribution to what even to this day is missing in social psychology: a reliable empirical taxonomy of social situations. [Although] the [early] reception was universally positive, the [social psychology fads, or] paradigm shifted under my feet. By the early 1980's social cognition emerged as the new way of doing social psychology, [which involved] studying isolated individuals sequestered in a cubicle having an interaction with a computer screen. What was *not* happening was any renewed or enduring interest in the ecologically valid study of everyday social situations and episodes. My previously ever-so-promising...work on social episodes, even though published in the best journals, was fast sinking into oblivion.