Most Underappreciated

50 Prominent Social Psychologists Describe Their Most Unloved Work

Edited by

Robert M. Arkin

OXFORD

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Bluma Zeigarnik, circa 1921. Born in 1901, Zeigarnik studied in the 1920s at the University of Berlin under the mentorship of Kurt Lewin (widely regarded as the father of modern social psychology) on essential aspects of his field theory, leading to her dissertation on what was later to become known as the "Zeigarnik Effect" (where she found that tasks interrupted are recalled approximately 90% better than those fully completed). In 1925, she graduated from university, and in 1927 was awarded the PhD. She returned to Moscow in 1931 and continued work as a neuropsychologist for 56 years, ultimately holding a chair in Neuropsychology at Moscow State University. Photo courtesy of her grandson Andrey Zeigarnik with permission of the Zeigarnik family.

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Printed in the United States of America
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To my teachers, friends, and cheerleaders: Jerry and Phil

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PREFACE

Okay, so there are actually 55 essays, not 50, and 56 authors in all, as one essay is co-authored. Mea culpa, I suppose. I invited only a handful of others, early on, but those scholars either thought all their work was overappreciated, or they were just too busy to contribute something at the time. With 55 final invitations, I felt sure that a few people would have writer's block, get carpal tunnel syndrome, rethink their commitment, or miss a deadline. No one did.

The reason for this record-setting perfect attendance, I think, is that the idea for this book hit a chord with virtually everyone who stopped, even briefly, to think about the idea. This book is unique. Each essay is brief and to the point, and each essay serves a purpose—not merely for the reader, but for the author, as well.

For the author: This is a collection of reflections written by some of the most eminent social psychologists of this era. Each author was asked to describe some work she or he has published that just didn't hit the mark, didn't get the kind of attention it "should have," was misunderstood or misconstrued—what I described to them as their "most underappreciated" work. For some, it would be a matter of timing, publishing something before its time; for others a problem in the framing of the hypothesis or of the findings; for still others the publication outlet, the audience, and so forth. For some time, I have been asking visitors to Ohio State University informally, "What is your most underappreciated work?" and nearly without exception, people perk up and have a story to tell. As I asked this question, each conversation led to a dramatic change in my conversation partner's face, moving from a blank sort of "start," to a faint smile of recognition, a look into the middle distance, followed by a response that took a latency of only, perhaps, 10 seconds in all. Every such conversation (at least with senior scholars) led to a story, an illustration, a recounting of a project or idea—and a great story. As often as not, the story concerned a "monkey" that people had "on their backs." And so, this book offered the chance to right the ship of scholarship, to explain again more clearly, to correct a misapprehension, a misunderstanding, a mis-citation, and so forth. In short, writing a brief essay, for some, was an opportunity of a lifetime. The chance to get a monkey off one's back doesn't present itself every day.

My conversations with visitors often reminded me of the "Zeigarnik effect," which was first reported in the doctoral research of Bluma Zeigarnik (1927), a disciple of the Berlin Gestalt psychologists (Kohler, Wertheimer, and Lewin). She was an early PhD student of Kurt Lewin, who is generally regarded as the father of the discipline of social psychology. Zeigarnik found that people typically remember uncompleted tasks far better than completed tasks. Perhaps apocryphal, it has been said that the hypothesis emerged over a dinnertime conversation about servers in restaurants, who at the time were expected to recall patrons' orders at least until the bill was presented. My guess was that even these eminent psychologists had feelings of "unfinished business" about at least some of their work, and that I was hitting that "minor chord" with my invitation.

For the reader: I think the essays turned out to be even more delightful for the reader than they were unburdening for the authors. Some are "laugh out loud" funny and charming. Despite being written by some of the most eminent psychologists, one writer revealed that he has a published paper (in the flagship journal) that has *never* been cited—not even by himself! Another wrote that he has been trying to become a *social* psychologist for years and years, only to get the cold shoulder, even on the dance floor during after-meeting parties at conferences (that almost made me want to teach him the secret handshake). One writer mentioned a study inspired in part by The Who song "Pinball Wizard," and mentioned the dependent variable name "balls." One author, one of the nicest (and shortest) people you could ever meet, wrote about intellectual "sparring" and said she left meetings "bloodied, metaphorical sword still in hand… but jubilant that someone had engaged with the ideas."

Beyond the charm, the engaging stories, there is an intellectual objective as well, actually more than just one. The book is organized into five chapters:

- Big Science, Big Theory, Big Ideas
- Middle-Range Theories
- Methods and Innovations
- Phenomena and Findings
- Application: Making Science Useful

This organization means that the book covers the waterfront of social psychology. The essays span the same range of theories, methods, findings, and application as found in the typical social psychology textbook. The book is brief enough to be used as a supplement to a conventional textbook. And, for well-prepared students and those with some background, it could even serve as the core of a course. The theoretical, methodological, and practical matters raised make so many useful observations and touch on so much of the field that the book could serve profitably as the backbone of a "professional problems" course and be accompanied by readings.

To enhance the pedagogy, and to put a human face on the scholarly enterprise, each author was asked to address one of the four questions:

- Who were your mentors, or influential figures in psychology, that led you to study this particular question?

- What is important or useful about the theoretical framework that drove this research question?
- What advice would you offer for a new, young investigator, just entering the field, based on this work and your experience?
- What was the impact of this research on your own future research agenda?

This enabled people to write, with great affection, about their intellectual "North Stars" in a way that isn't usually available. There are fond recollections of Hal Kelley, Ned Jones, Don Campbell, Jud Mills, Jack Brehm, Michael Argyle, Jos Jaspers, Bob Zajonc, Fritz Heider, Stanley Schachter, and many more. And, of course, the Table of Contents reveals that the authors in this book are no less "North Stars" themselves; they are the luminaries of today. Many give advice to young scholars, the sort of guidance one could only get in casual conversation. The advice ranges from how to prepare for, and make the most of, a professorship in a liberal arts college context (where teaching is highly valued, perhaps more than research) to how to frame a research question, title an article, handle a controversy, pursue a passion, devise a method, think about a meta-analysis, write persuasively, and more.

Finally, and perhaps most engagingly, these eminent psychologists to a person made their professional lives a much more "human" and "social" enterprise than anyone usually knows or can see. Their stories are personal; they touch on relationships, people's passion about ideas, the emotional highs and lows of academic life, the parts of the "life of the mind" that get neglected in the sometimes dry, scientific prose that is the coin of the realm. These authors are all people who have enjoyed immense success. They are the sorts of scholars who typically do not let anything "go to press" unless they view it as a gem. But some of these expected gems are received in underwhelming ways, and it seems that even the leaders in our field don't quite get over that. This turns out to be a good thing, as their unfinished business (Zeigarnik, 1927) presents this chance to peer through the window and see how even the best and brightest are occasionally misunderstood, underappreciated, mis-cited, and, at least occasionally, missed entirely.

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I thank my graduate school advisor and friend for more than three decades, Jerry Jellison, whose enthusiasm is contagious; his support is a continuous source of strength to me and his friendship through the years is an immeasurable gift. Both Jerry and Phil "Zim" Zimbardo, friend and teacher for two decades, were cheerleaders throughout this project, providing advice and an occasional push (sometimes followed by shove) to get it done.

I owe a huge debt of gratitude to Lori Handelman, Senior Editor at Oxford University Press, who saw the value in this project from the start. Her advice and counsel at every stage, start to finish, and her willingness to field incessant questions and lend her critical eye to all made her the best sounding board anyone could hope to have. Lori is not only a gifted thinker and writer generally, but her PhD in social psychology meant she knew all the "usual suspects" and she understands the field intimately, and I ultimately adopted the salutation Jedi Lori when writing her (as in "Dear JL").

Once this book was in production, Abby Gross took over where Lori left off and she was also a delight to work with; Ashley Polikoff was terrific in overseeing all of the project through production, and Joanna Ng and Anisha Shankar were both amazing in their detailed, professional work turning the manuscript in to a book. In every way, Oxford University Press proved a great organization populated by hardworking and really nice people! Thanks to all.

My oldest son, JD, and Uncle Bill, a Thanksgiving fixture in our home, listened first to my own story of being underappreciated, and didn't mind when I dashed off to get scratch paper and write out the idea for this book. The rest of the family didn't hear much from me during the busy times with deadlines, but they greeted incessant updates the rest of the time with good cheer, and even occasional interest.

The expectation that collecting these tales from colleagues would work stemmed, I think, from my many years of serving as a journal editor. I have routinely been amazed by the generosity and the thoughtful, clever, insightful things (yes, sometimes devastating) my colleagues have shared in their reviews of one another's work. This generous spirit of sharing what they know is what led me to believe such top-caliber scholars would contribute to this set of stories. So, I thank all those amazing scholars over the years for convincing me that social psychology is

populated both by exceedingly bright and also very nice people who recognize that the whole to which they contribute is far greater than the mere sum of its parts.

Social psychologists are a talented, smart, sometimes smart-alecky, clever, and fun group of scholars who have remarkable insight into everyday life. I thank the authors of these essays, who rose to the occasion and who made these essays distinctly "human" and "social" and revealed something usually hidden in the scientific enterprise: a window onto how top-flight scholars think, act, and feel about their work.

Bob Arkin
Columbus, Ohio
June 2010

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INTRODUCTION

MY OWN MOST UNDERAPPRECIATED RESEARCH

On Thanksgiving Day last year, Uncle Bill turned the conversation toward a topic at the natural intersection of the mind–body question that dogs philosophers and psychologists alike. He cited the statistics for air travel, that as many as 25% of flyers catch a cold from the shared, recirculated air in the plane. He then left the room briefly to get his Airborne, that relatively new product supposedly concocted by a teacher—those folks who are exposed to all manner of disease in their classrooms, and are therefore likely to be sick all the time. On his return he talked about his friends who insisted he use it to fend off a cold during all travel by air. My oldest son, on his first visit back home during his first quarter away at college (and consequently, knowing utterly everything, at least in his own mind), pointed to his head… symbolizing that he guessed this was more about mind (that is, Uncle Bill's mind) than body. Really, I think he might have meant that Uncle Bill's mind was full of hot air.

Because my son, JD, was about to take Introductory Psychology his next quarter, and we rarely talk about my work or my spouse's work (she's a psychologist, too; poor kids) at home, I launched into a description of a set of studies that I noted was among my most underappreciated work.

Not that any of my work has changed the face of social psychology, or anything else, mind you, but I have had an eclectic set of interests over these 30 years, and I've done okay, even serving as a dean at Ohio State for 8 of those years, a journal editor for fully 14 of those years. The impact of my research has been variable, from getting some notice to receiving a little less.

Some of the least-noticed efforts are among the ones I am most proud of and pleased with, and this irony has been a source of amusement to me (and, I suppose, frustration) over the years. The idea for this book stems from that very moment and that conversation with my family and from my own nagging feeling that sometimes one's least appreciated work may be among what a scholar might see as his or her most "underappreciated" work. It isn't easy in the context of an article or chapter in a book always to say clearly why you think some idea, or set of data, is special, or huge, because there is not a lot of room to wax eloquent on that point. And, as they say, timing is everything. Sometimes one's work is out of time, or out of place. It may be before its time. It may be in a journal where the right set of scholars' eyes simply never see one's work, or it could be buried in a messy literature or read in the context of a literature that obscures the big "take-home point" that you see as most special.

I have had that experience more than once during my three-decades-long career. The one I described to my son that Thanksgiving Day was from the early 1980s. It was a "one-shot" effort that stemmed from long conversations between one of my most accomplished PhD students and me. Jerry Burger (now a professor at the Santa Clara University) was deeply invested in studying the psychology of control. He had published a scale on the Desire for Control with my friend and colleague Harris Cooper, and was working on a first book on the psychology of control. As time passed, literally dozens of articles on the topic came out of Jerry's lab at Santa Clara.

Our conversation was about learned helplessness. We talked at length about what would produce it, what might preclude it. Ultimately, the conversation landed on the question of what might inoculate people against succumbing to learned helplessness. Given our training, and the "Festinger tradition" that was our heritage, Jerry and I landed on a 2 × 2 conceptualization that independently explored the experience of predictability (present vs. absent) versus the experience of feelings of control (present vs. absent).

To that point, learned helplessness had been characterized as a response to loss of control, and the behavioral syndrome was (loosely) characterized as giving up, cowering, "throwing in the towel." Originally studied with animals, the absence of control would lead to passivity, cowering in the corner, and in people—to clinical depression.

Jerry and I noticed that, in all the research, the perception of predictability of events had been confounded with perceived control over those events. We explored the *independent* influence of perceived control and perceived predictability of an aversive event on participants' performance on a memory task, and their depressive affect that would result. Our guiding hypothesis was that, at least for people, predictability of aversive events might well be enough to provide inoculation against the nastiest effects of lack of control.

Participants received noise blasts that were both unpredictable and uncontrollable in one condition, and these individuals displayed performance deficits—and depressive affect about it—relative to a no-noise control group. However, participants who were able either to exert control over the noise blasts *or to have a measure of predictability* about the noise blasts did not show the same losses or depression. In short, either the sense of perceived control, or of perceived predictability, was sufficient to mitigate learned helplessness! Functionally, perceived control and perceived predictability were the same. Each inoculated our participants against the most maladaptive effects of learned helplessness.

In short, you don't necessarily have to enjoy complete control over events to avoid being a learned helpless basket case. For instance, when we go to the dentist, at least my dentist, I am always told something like "This is going to feel like a pinch, and might give you some discomfort" just before he injects a needle in my gums (a needle meant more for a Budweiser Clydesdale than for my gums, it seems to me). So, I clench my hands around the arm rests with my death grip. It's much the same when I am in a jetliner, taking off, and while I cannot control things any longer, I can at least grab my seat and tense up effectively. I am not fully learned helpless. I can prepare for aversive events. And while I don't have control, I enjoy the benefits of predictability.

We had a memorable exchange with Chuck Carver, who was then the editor of the personality section of the *Journal of Personality and Social Psychology*, the flagship journal for social psychology of the American Psychological Association. He really liked our distinction between predictability and control, and our idea that the two could be experimentally distinguished. He also liked our findings, and saw the results we uncovered as compelling support for distinguishing these two concepts and noting their independent impact, at least for humans.

However, the editor also had one (the one that we struggled the most with, and that I remember best) concern that nearly stumped us. He noted that it wasn't easy, and might not be possible, to think of a situation in daily life that reflected the presence of feelings of control (or the sensation of exerting control) but that was absent any sense of feelings of predictability. Chuck felt that publication of our work should probably depend at least in part on whether such a thing was possible (and, if not, then predictability and control were not truly orthogonal, or independent, in so-called real life).

Jerry and I talked endlessly about this, and eventually came up with our illustration. The editor was satisfied, the paper was published, and it … well … it seemed to land on deaf ears, despite being published in the flagship journal.

Our illustration was a person taking preventative or palliative cold medicine, which would convey a sense of exerting control over the viral infection known as the common cold. But taking the cold preventative/palliative at the outset of a cold doesn't convey anything about predictability. If you read on the label that something will cut the length of your cold in half, or cut your symptoms by a third, it is still entirely unclear what you can expect about the duration or debilitation of your cold. You get little in the way of predictability, and it's only the next morning when you wake that you can say with much confidence what you are likely to face for this particular cold—and then you're not certain.

I'm not sure about Jerry, but I thought this was one of the best insights linking "real life" with my work that I'd ever had. I had been taught that ecological validity was nice, but not a necessary precondition for doing quality thinking or excellent, important research. But there was something really energizing about solving this puzzle, and noting a pretty common experience of daily life that was a neat example of the conceptual distinction we made in the research.

Few readers saw its beauty. Not many cited our work. It was a "one-shot" study, which nearly everyone agrees is not the way to make you famous as a scholar, but gee… it was such an elegant study, and such a neat idea.

It was still a thrill to me to tell my son and Uncle Bill about the study, the idea, the totally fascinating original learned helplessness work of Seligman's that first stimulated our thinking. I had gotten my wife to tell about the original learned helplessness work, as her doctoral dissertation was on a cognitive aspect of the original work, extended to clinical psychology. It also gave me the opportunity to talk with my son about positive psychology, and Seligman's role in that, and then talk about my friends and colleagues and neighbors in our community here in Columbus, Ohio, and their influence, and how much my son was going to love psychology when he got started next quarter. In short, a side benefit of the 25 years of underappreciation that had stuck in my memory was an opening to talk with my son about the mind–body problem in general (he also took a beginning philosophy course that next quarter) and to connect with my son's educational experience and my own joy in learning. I'm sure it would be much nicer to have had the study Jerry and I published have a bigger impact, but this side benefit was not an all-bad substitute.

And during the conversation, I jotted down the idea for this book, so… things have a way of righting themselves, in the end.

Most Underappreciated

PART I

Big Science, Big Theory, Big Ideas

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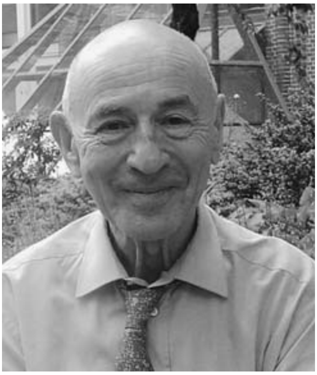
WALTER MISCHEL, Columbia University

Most Cited, Least Read?



The greatest irony of my professional life is that my 1968 monograph *Personality and Assessment,* which brought me quick fame and even more infamy, is credited with causing an endless debate I found absurd from the start, splitting the two fields I hoped it would unite. It remains widely cited (more than 2500 citations), is still republished, and has been praised and hated for decades, generally for the wrong reasons, I suspect mostly by people who never read it, but keep discussing it in their textbooks and lectures, almost never quoting from it, not even paraphrasing. It would take a historian of science to figure out why it became the Rorschach card for so many colleagues on both sides of the social-personality hyphen, and still may serve that function, I hope less often. Perhaps my remarks here might encourage a few curious newcomers to psychological science to actually read it. But given its track record for 40 years, that's not likely—so at least take a look at the concluding paragraph from the book reputedly written to "kill personality" and undermine the role of individual differences:

> Global traits and states are excessively crude, gross units to encompass adequately the extraordinary complexity and subtlety of the discriminations that people constantly

> make. . . . The traditional trait-state conceptualizations of personality, while often paying lip service to [people's] complexity and to the uniqueness of each person, in fact lead to a grossly oversimplified view that misses both the richness and the uniqueness of individual lives . . . [and their] extraordinary adaptiveness and capacities for discrimination, awareness, and self-regulation. (Mischel, 1968, p. 301)

Still strikes me as something that shouldn't have been too upsetting for personality psychology even 40 years ago, and I'd expect most everybody to quietly, even sleepily, nod and say "Sure, why not." Then why all the controversies with much sound and fury for so many years?

I first stumbled towards this book when I was a beginner teaching at Harvard in the Social Relations Department in 1960, preparing a survey course for graduate students in the personality program on the state of personality psychology and assessment. The deeper I got into the personality and assessment literature, most of which I had managed to avoid as a graduate student, the more I was surprised by the discrepancies between what the personality theories assumed and what the data showed. The theories assumed broad consistency in the individual's trait-relevant behaviors across diverse situations. But the gist of the data indicated that the aggressive child at home, for example, may turn out to be less aggressive than most when in school; the man exceptionally hostile when rejected in love may calmly accept criticism of his work; the one who dissolves anxiously in the dentist's office may be also be a courageous deep-sea diver; the bold risk-taking entrepreneur may shrink at his own cocktail parties. Research articles and doctoral dissertations often were reaching the same conclusions, but the disappointed investigators blamed themselves for the failure of their personality tests and their studies to yield the expected correlations. There was lots of "mea culpa" about poor methods and unreliable measures, but nobody questioned the key theoretical assumptions that guided them.

WHY THE TRAUMATIC FALLOUT FROM 1968?

The 1968 monograph traumatized many personality psychologists, I think, not because it called attention to the disappointing results of global trait-based personality assessment research that was already beginning to become clear. It was distressing because it asked: What if the problem is not just with bad methods and poor studies but also with wrong core assumptions? And I concluded that for a half-century researchers had been looking for personality guided by untenable assumptions, and therefore could not find the consistencies they expected. The fallout was that it left most personality psychologists with their paradigm down. Not a great way to make friends.

Upon publication the 1968 book was dismissed on a back page of *Contemporary Psychology,* in a short review titled "Personality Unvanquished," but within a year the "person versus the situation debate" exploded and dominated much of the agenda in personality and social psychology. This heated confrontation filled the journals' pages and the field's national and international meetings for more than 15 years, and deepened what to me was the absurd conceptual split between person and situation and between personality psychology and social psychology. Most personality psychologists reacted to the 1968 book as trivializing the importance of personality and overblowing the causal power of situations, and took it as a rejection of the "existence of personality" and the "power of the person." Most social psychologists cited it as proof for the "power of the situation" and the relative insignificance of individual differences in personality. In their debate, the two sides pitted the "power of the person" versus "the power of the situation," to argue about which was the bigger causal agent, which one accounted for more variance.

I thought both sides equally missed the point and the intended 1968 message. For years in subsequent papers I tried to make clear that I had always refused to ask "Is information about individuals more important than information about situations?" because phrased that way it is unanswerable and can only serve to stimulate futile polemics, in which "situations" are erroneously invoked as entities that supposedly exert either major or only minor control over behavior. The debaters kept on debating. The dispute took on its own life, further splitting social and personality psychology at exactly the most unnatural joints, severing the study of persons from the situations in which they functioned rather than focusing on their links and reciprocal interactions.

The result on one side was a "situationist" extremism that indeed trivialized the role of individual differences, and treated personality coherence as an illusion and an attribution error. On the other side, many personality psychologists renewed even more intensely their efforts to retain the traditional paradigm. They argued that global dispositions as traditionally conceptualized were "alive and well" if one simply aggregated multiple observations and measures across different situations. Thereby they again eliminated the role of the situation by averaging it out. This strategy now acknowledged that specific behaviors across different types of situations could not be predicted by such a model and simply continued to treat the situation as a source of noise by removing it as before.

As the debate escalated, so did the distance between what was said about the book that ostensibly caused it and the book's contents. In the l980s I was not infrequently described by personality psychologists as the devil of the field who tried to destroy it, and "Mischel, 1968" was stuck into parentheses as the cited evidence. A multiple choice test item on a major state examination for many years was particularly upsetting to some of my students by asking them to identify the psychologist who "did not believe in personality," and making Mischel the right answer. It required short-term therapy from their mentor.

WHAT WAS IN THE 1968 BOOK? NOT *SAID* ABOUT IT, BUT *IN* IT?

Forty years after *Personality and Assessment* was published it was therefore a happy surprise to see that Orom and Cervone (2009) did something that almost never happens for the 1968 book: They did a scholarly review and systematic, quantitative content analysis of what's in it, not what gets said about it. Their analysis showed that the book consists of two halves: the first documents the challenges facing the field and some of the main limitations in the concepts and methods regnant at that time; the second:

> concerns psychological dynamics, cognitive processes, subjective meaning, and individual idiosyncrasy. In these pages, the book has little coverage of personality "traits" or "consistency"—topics commonly thought to have dominated Mischel's work. Our analysis indicates that . . . the point of his book was to advance a personality psychology that centered on psychological dynamics of meaning construction and that simultaneously was sensitive to the idiosyncrasies of the individual. (Orom & Cervone, 2009)

Orom and Cervone then underline that a key message of the 1968 book was that the assessor's focus needs to be on the particular meanings that stimuli and situations have acquired for the individual. They conclude their paper by making a point I have long hoped to see in the personality literature: "Whether you liked it or not, the first half of Mischel's famed volume did not argue that cross-situational consistency in personality functioning is low. It argued that cross-situational consistency in personality functioning is low when one searches for consistency through the lens of global, nomothetic trait constructs. When one tries on different lenses, things clear up" (Orom & Cervone, 2009).

FROM PARADIGM CHALLENGE TO PARADIGM ALTERNATIVE

Beginning in the late 1970s and early 1980s, the factor analytic approach was rediscovered to resuscitate the classic trait paradigm. It was reborn with an agreement (far from unanimous) among researchers about the major traits, dubbed the "Big Five," needed for a comprehensive taxonomy of personality, based on trait ratings. To me it looked like a 20-year regression supported by popular vote and acclamation, not by convincing new evidence, to return to business as usual. For many personality psychologists it soon became synonymous with the construct of personality itself. It was hard for me to believe that a model like the five-factor theory, a conception like the Big Five, and a measurement tool like the NEO-R, was really going to become equated with the very definition of personality. Was this field ready to have a view of the human being confined to such characterizations with trait adjectives that categorize people so simplistically? Was personality going to be split from the study of the self, of individual differences in how people think, feel, and process information about the social world? Was it going to be divorced from how what we think, feel, and do connects to what is around us, and links to how our brains work, even to how our genes play out? Put simply, I feared that the view of human personality in our science was in danger of becoming headless, brainless, self-less, de-contextualized from the social world, lacking an unconscious, and missing an emotional/motivational system.

THE MORAL OF THE STORY

The resurgence of the traditional trait approach in the form of the Big Five further spurred my desire, shared with Yuichi Shoda and many others, to go from challenging the paradigm to seeking a better alternative, and getting to the locus for the intuition that there surely is consistency or at least coherence in personality, but not where it had been assumed to be. Over many years, it was found by incorporating the situation into the assessment and conception of the individual, rather than by treating it as the error term. By including the situation as it is perceived by the person, and by analyzing behavior in this situational context, the consistencies that characterize the person, far from disappearing as had been assumed, began to be identified. We discovered that these individual differences are expressed not in consistent cross-situational behavior; instead, consistency is found in distinctive but stable patterns of *if* … *then* …, situation–behavior relations that form contextualized, psychologically meaningful cognitive-affective-behavioral signatures (e.g., "she does, thinks, feels A when X, but B when Y"). And these signatures of personality in turn begin to open windows into the underlying relatively stable processing system that generates them—the Cognitive Affective Processing System or CAPS that Yuichi Shoda and I outlined in our 1995 *Psychological Review* piece.

If there's a lesson to be learned from this story, perhaps it's that a paradigm challenge will either be ignored, or create a lot of noise and strife, but will change little until a better alternative emerges and gets a chance. Perhaps the 1968 challenge had some value, even for those who never read it, maybe by leading to a polarization that sharpened the issues, insisted they be confronted, and even pointed to the needed next steps. But to have a chance of changing anything, you need a paradigm alternative, lots of luck, great students and colleagues, dog-like persistence, and above all longevity. Yes, and tenure at a good university helps a lot. And then it takes another 40 years to see if any of it mattered or is remembered. But no matter how it plays out, it's still the best serious game in town.

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