Pillars of Social Psychology

Stories and Retrospectives

Edited by Saul Kassin

From Ideomotor Theory to the IAT in Just 35 Years

Anthony G. Greenwald

8

The title's 35 years are from June 1963, when I received my PhD, to June 1998, when the article that introduced the Implicit Association Test was published (Greenwald et al., 1998). This chapter will be replete with digressions from the path between ideomotor theory and the IAT. I've italicized the digressions to make them obvious.

Place, Manhattan. Time, early September, 1949, first day of junior high school. I had been "skipped" twice in earlier years, completing Grades 1–4 in two years. In New York's public schools, this occurred commonly for students who progressed rapidly in reading and arithmetic. At age 10, I was two years younger than most of my new 7th grade classmates. I went to my assigned home room, where I was surprised to learn that, with no advance indication to me or my parents, the school had assigned me to an accelerated program that would complete Grades 7–9 in just two years. When the bell rang for students to go to their first classes of the day, I could not follow my homeroom classmates to their first class. I needed to go elsewhere for the accelerated program's first class, but no one had informed me where I should go.

I soon found myself in the emptying hallway of the totally unfamiliar school building, not knowing what to do. When I was at last alone, I did the most reasonable thing – I cried. I was discovered soon by an adult to whom I (sobbing, I'm sure) explained my plight. I was escorted to an administrative office, where I received the additional news that the compressed academic schedule of the accelerated program could not include time for an orchestra class that I was very much looking forward to – I wanted to start playing trumpet. I have no memory of the rest of the school day, but I did learn that I would be allowed to choose between the orchestra class and the accelerated program. When I got home I discussed the choice with my parents. They were as uncertain as I was, and they left it to me. The next day, I started in the orchestra class. This was a decision that likely affected everything significant in the rest of my life.

I discovered ideomotor theory during the most nerdish period of my career. (Not to mislead, I remain a nerd, but not quite so much as when I was a PhD student.) Early on at Harvard, I undertook to learn everything I could about learning-behavior theory, the dominant theoretical paradigm of experimental psychology at that time. I worked

Anthony G. Greenwald, University of Washington, USA.

in the laboratory of a learning theorist, Richard Solomon, in my first year. I was helping him and Lucy Turner, his chief lab associate, with experiments in which the subjects were dogs who were injected with curare (i.e., paralyzed) as part of the protocol for traumatic avoidance learning experiments. A decade or so later, these experiments were discontinued, being regarded as animal cruelty. I confess that I did not question or object to participating in these experiments. It was a different time.

In addition to the standard program for first year social psychology PhD students, my first year of graduate work included a seminar on learning theory taught by Solomon, a lecture course on learning taught by B. F. Skinner, and a following summer spent working in New Haven at the laboratories of Frank Logan, Allan Wagner, and Neal Miller in Yale's Institute of Human Relations Building. In their laboratories, the experimental subjects were rats, mice, and rabbits, and I worked directly with the animals. (In Solomon's lab, my only interaction with the dogs was to feed them, which I'm sure they enjoyed. My main responsibility was to learn about the topics being investigated.) My nerd tendencies got greatest exercise in my course work. I read everything that was assigned. Solomon's seminar assigned a lot. Weekly, the syllabus included perhaps twenty articles, most of them published recently in top-tier empirical and theory journals, in addition to lengthy chapters in prominent method and theory texts by Osgood, Woodworth, and Schlosberg, the large handbook by S. S. Stevens, and Ernest Hilgard's learning theory text. Not knowing enough to be selective, I read the assigned readings in entirety for the weekly seminar assignment of writing a 500-word synthesis of conclusions warranted by what I had read. I got interested in the disagreements among the major learning theorists (especially Edwin Guthrie, Clark Hull, and Kenneth Spence), but I was also attracted to Edward Tolman's cognitive learning theory, which was at intellectual war with Hull and Spence.

I also started reading philosophers on the topic of volition. Strangely, the topic of volition was treated not at all by the learning-behavior theorists whose work my courses focused on. Skinner's lecture course was of great interest, but I soon discovered that he shunned theory (I read his 1950 article in *Psychological Review* titled "Are theories of learning necessary?"). Skinner understood that "instrumental responding" (what occurs in Skinner boxes) was a product of "schedules of reinforcement." He felt no need for further conceptual understanding. Apparently, the prior half-century of American behaviorism had made volition an alien topic.

Although Richard Solomon was my formal advisor, informally I was working with Elliot Aronson and Walter Mischel, who were both assistant professors in the Social Relations Department, where the PhD program in social psychology was located. After they left, Merrill Carlsmith, a PhD student one year ahead of me, became not only a good friend, but my informal mentor. I also had very helpful support from graduate students in other departments who were, like me (from my second year on), resident tutors in Harvard's Leverett House. Merrill outdid me in just about everything, except on the squash courts. I wrote about this part of my career in the first several pages of my chapter in Elliot Aronson's 2010 festschrift.

What happened with the junior high school orchestra class? After three years (end of 9th grade) I was a decent trumpet player, enabling me to gain competitive

admission to New York's High School of Music & Art (M&A). In my second (junior) year at M&A, two classmates took me under their wing, having concluded it was essential that I learn to play Jazz. Their strategy was to sit me down and play LP records of the major Jazz artists for me. New York was the place to learn about Jazz. The local Jazz clubs (especially Birdland, Basin Street, and Five Spot) would admit under-drinking-age students (I was way under age, in addition to which I was short). As a consequence, I got to repeatedly see and hear the Jazz greats of the Bebop era. Charlie Parker had already died, but he had influenced everyone. I had numerous opportunities to hear live performances by Clifford Brown (my all-time favorite Jazz trumpet player, who tragically died just a few years later, in his late 20s, in a car crash) and also Miles Davis and Dizzy Gillespie, also saxophonists John Coltrane and Sonny Rollins, drummer Max Roach, pianist Thelonious Monk, the Modern Jazz Quartet, and the big bands of Count Basie and Maynard Ferguson.

In my senior year at M&A, I applied to and was turned down for admission by both Yale and Harvard, despite my having the highest grades in my graduating class of close to 500 students. But I did get onto Yale's waiting list. (Much later, I learned that Yale then had a 10 percent cap on Jewish admissions. The quota apparently started in the 1920s and lasted into the 1960s. I graduated in 1959 from Yale in the top 1 percent of my class.) A trombone playing friend of mine at M&A had been admitted to Yale's School of Music and did me the great favor of telling Yale's Director of Bands that there was an excellent trumpet player available to be plucked from Yale's waiting list. That worked. By the time I was a senior at Yale, I was playing solo first trumpet in the University Concert Band (also marching with the Yale Band at all football games). In my last two years at Yale, I also held the 3rd trumpet chair in the New Haven Symphony Orchestra (the first two chairs were held by trumpet majors in the Music School) and I played regularly with a campus Jazz sextet. My trumpet career continued for another 25 years after Yale, into the early 1980s, when I decided that the ten practice hours per week it was taking to keep my lip in shape were taking too much time away from work.

The first scholarly treatment of volition that I read was William James's chapter on "Will" in his two-volume 1890 opus, *Principles of Psychology*. James started a section titled "ideo-motor action" (Vol. 2, pp. 522–528) with a question: "Is the bare idea of a movement's sensible effects [a] sufficient mental cue ... before the movement can follow?" James concluded "yes" and credited that conclusion to the earlier (1852) work by German philosopher-psychologist Hermann Lotze. James's statement of the central thesis of ideomotor theory was that "Every representation of a movement awakens in some degree the actual movement which is its object" (1890, Vol. 2, p. 526).

That last statement is remarkable for expressing what appears to be an empirically testable proposition. When James was writing in 1890, however, there were no methods available to test this Lotze–James ideomotor hypothesis. Seventy years later, when I read James's chapter, methods were available, but no one was using them to conduct tests of ideomotor theory (again, perhaps a residual consequence of behaviorism).

My first ideomotor study used an idea suggested by Lotze's observation that "The spectator accompanies the throwing of a billiard-ball, or the thrust of the swordsman, with a slight movement of his arm" (James's translation from the German original). An experiment done with PhD student Stuart Albert at Ohio State indirectly confirmed Lotze's observation by showing that observers learned more from observing over the shoulder of another subject performing at a discrimination task than when viewing that subject from the side. (The over-the-shoulder subject had superior sensory exposure to the arm and hand movements required by the discrimination task.) This experiment, published in 1968, provided only a minor confirmation of the Lotze-James hypothesis – but it was a start (Greenwald & Albert, 1968).

In the next four years I and grad student co-authors reported four studies showing both that performance was faster when stimuli approximated sensory feedback from required responses (e.g., subjects would say letter names more rapidly in response to hearing them spoken than in response to seeing them printed or, alternately, would write letters faster in response to seeing them in print than to hearing them spoken). Even more convincing experiments came in the next few years, showing that two 2-choice decisions could be made simultaneously without mutual interference, but only when the stimulus for each decision was "ideomotor-compatible" with its required response - for example, moving a lever to the left or right in response to a left- or right-pointing arrow was ideomotor-compatible - while simultaneously speaking the name of a letter heard at the same time the arrow was seen. The apparatus and data-recording requirements for these experiments were challenging in the 1970s. About thirty years later I created a demonstration procedure involving presentations of a visual arrow pointing left or right simultaneously with hearing "left" or "right" in earphones. It is totally simple to make simultaneous rapid decisions of pressing a key with left or right forefinger in response to the arrow stimulus while simultaneously saying "left" or "right" in response to the heard words. In contrast, performance slows greatly (and errors are made) if the spoken words must be given in response to the arrows, with the keypresses made in response to the simultaneously heard words.

The ideomotor compatibility phenomena demonstrated in these experiments revealed automatisms that played roles in voluntary performance. Depending on the assignments of responses to stimuli, these automatisms could either (a) enable perfect timesharing of two decision tasks or (b) cause large interference between the two tasks, greatly slowing performances. In addition to supporting ideomotor theory, this finding provided a significant exception to the (still) widely accepted cognitive psychological principle that choice decision tasks require a limited capacity response selection process that can make only one decision at a time.

My trumpet playing at Yale and my continued development of Jazz "chops" (i.e., technique) had welcome side effects. In the summers after my sophomore and junior years, my job was not in a laboratory, but playing daily and nightly in the band of a Holland-America Line student ship that took eight days for a trip in late June to Amsterdam and likewise to return to Hoboken in late August. The bands were a fivepiece Dixieland band ("The Ivy Five") in 1957 and a six-piece Bebop band ("Ivy Five Plus One") in 1958. The eight weeks between trips were spent driving a VW bus around Western Europe with my bandmates, sightseeing in the major capitals, and working as the house band for extended stays in Anzio (at a resort hotel mostly for seniors) in 1957 and at a strip bar frequented by American soldiers in Frankfurt am Main in 1958.

In the summer of 1959, after my Yale graduation, my trumpet playing included a two-week tour of Western Europe with the Yale Concert Band and a scholarship to the two-week session of the Lenox (MA) School of Jazz, where the faculty consisted of major Jazz instrumentalists, composers, and writers. I had the opportunity to perform alongside fellow student, Ornette Coleman, who had just recorded his first album (titled "Something Else"), also to have trumpet lessons with Kenny Dorham, to play with fellow scholarship winners in a sextet led by saxophonist Jimmy Giuffre, and to be lead trumpet player in the school's big band, led by trumpet player Herb Pomeroy. When I moved shortly thereafter to Boston for grad school, Pomeroy "hired" me as fourth trumpet player in his sixteen-piece band that performed two nights a week at The Stables, a bar in downtown Boston. (The sub-minimum-wage pay was based on bar earnings.) I loved every minute of it, playing alongside both veteran musicians and students at Berklee School of Music, three of whom later became well-known Jazz professionals.

The work inspired by ideomotor theory was being done while my primary line of work was on attitudes and persuasion. I decided early in my career that it was desirable to maintain an active line of cognitive research alongside my social psychological research. In the 1970s, I added a third line of research, on methodological topics. My earliest methodological work grew out of an experience of repeated failure in attempting to reproduce others' findings. Although it got some attention, it wasn't very satisfying to publish failed replications. In the 1980s I adopted two plans that I thought were more effective than publishing failed replications. First, I concluded that I (not others) should be responsible for assuring that any results I produced were replicable. Also, when I figured out that a result of p = .05 meant that there was only a 50 percent chance of an exact replication obtaining a result for which $p \le .05$, I concluded that I should never base confident conclusions on a finding for which the reported p value was not substantially smaller than .05¹ I am, alas, aware of one subsequent publication for which I was a co-author in which a non-replicated finding with p = .05 was used as the basis for a confidently expressed conclusion. It was quite a few more years before that finding was shown not to be reproducible.

Also on the methodological front, in a paper published with Anthony Pratkanis, Michael Leippe, and Michael Baumgardner (1986), I advocated the challenging strategy of demonstrating within a single publication that a finding of uncertain reproducibility can be reliably obtained under one set of conditions and reliably *not* obtained under a different set of conditions. As difficult as this may sound, it can be done and has been done in at least four publications in which I was involved

¹ Greenwald et al. (1996) described why a result of $p \le .005$ could be accepted confidently as a basis for confidence in reproducibility by an exact replication. Subsequent Bayesian treatments have offered similarly strengthened criteria for concluding that a published finding is likely to be reproducible.

(Pratkanis et al., 1988 for the sleeper effect in persuasion; Pratkanis et al., 1994 for effects of subliminal self-help audiotapes; Draine & Greenwald, 1998 for subliminal semantic priming; and Greenwald & De Houwer, 2017 for unconscious classical conditioning).

Two of the four findings just cited were from research in which I investigated cognitive automatisms in a variety of ways. Those accumulating results convinced me that the human mind often makes judgments without awareness of the mental processes that produce those judgments. This was added to in the 1980s by research on what was described by its major innovator, Larry Jacoby, as "remembering without awareness." Larry lost the branding war on this topic to Daniel Schacter who, along with Peter Graf, had come up with the more readily adopted label, "implicit memory" (Graf & Schacter, 1985). Mahzarin Banaji (who started her PhD training at Ohio State in 1980) and I were both very interested in these developments in understanding the operations of memory outside of awareness. That shared interest evolved into an active and long-continuing collaboration that sought to determine when and how those developments could be applied to social cognition. Part of my motivation for this was a long-standing dissatisfaction with social and personality psychologists' near-exclusive reliance on self-report measures for investigating the major social/personality constructs of attitude, stereotype, self-concept, and selfesteem. The problem, at least as I saw it, was evident from well-known findings, mostly obtained in the 1960s, that established artifacts of demand characteristics and self-presentation as contaminants of many self-report measures.

Marzu and I started our collaboration by building on the "false fame" effect described by Jacoby and colleagues in 1989. (Mahzarin became "Marzu" due to my error in pronouncing her preexisting nickname [Mahzu] when she told me of it at our first meeting in 1980 - she was too shy to correct me then; that shyness did not last long.) We wondered if Jacoby's implicit memory effect might be understood as (what we later came to call) an implicit gender stereotype - one of associating male more than female with fame-producing achievement (Banaji & Greenwald, 1995). Our first thought was that this effect might occur primarily for just male names that had acquired familiarity by being encountered on a list that subjects had examined on the preceding day as part of a memory study. When we wrote to Larry asking him whether he had tested for this, he informed us that, actually, almost all of the names used in his false-fame studies were male. When Marzu and I did the obvious experiment of replicating the false-fame study with lists containing equal numbers of male and female names, we found in four replications that the effect was consistently stronger for male than female names. While continuing with other experiments on implicit stereotypes, we put our major effort into producing a theoretically oriented literature review, eventually published (1995) in Psychological Review, titled "Implicit social cognition: Attitudes, self-esteem, and stereotypes."

The final sentence of our 1995 article was, "Perhaps the most significant remaining challenge is to adapt these [indirect-measure] methods for efficient assessment of individual differences in implicit social cognition." That sentence was drafted in early December of 1993. In January, 1994 we submitted a proposal to NSF. The first two

sections of our proposal described planned research on implicit stereotyping that would be conducted in Marzu's lab at Yale. The third section described research planned for University of Washington on assessment of individual differences in implicit social cognition.

That third section of our NSF proposal had four paragraphs, each describing a distinct procedure based on the methods I had been investigating in my cognitive work of the previous decade. Each offered a possibility to assess cognitive content that might function with conscious cognitive control. The first two procedures (subliminal priming and supraliminal priming) were already known to produce priming effects, although the subliminal effects were weaker and less consistently obtained. Supraliminal priming (i.e., priming by clearly visible stimuli) had been used in research initiated by a 1986 publication by Russ Fazio and colleagues. However, it had not been evaluated as an individual differences measure. These were tested at the UW lab and were judged not to have strong enough potential as individual difference measures. The third ("mixed judgment") method showed itself to be so amazingly effective that it put an end to the search. Its description in the proposal (stated in terms of a measure of gender-attitude associations) was:

[T]wo categories of words are assigned to each of two response keys. Subjects are asked to rapidly press one key whenever the stimulus word is *either* female-associated or pleasant in meaning, and the other key for words either male-associated or unpleasant in meaning. Through the course of a session, pairings of the male and female categories with keys are switched (while left and right are left consistently paired with unpleasant and pleasant, respectively).

Anyone familiar with the IAT will recognize this as a description of the IAT's procedure. A very astute reader may also have noticed the similarity between the last sentence of the above description of the "mixed judgment" task and my description of an experiment involving two simultaneous ideomotor-compatible decisions, which used the contrast between (a) a very easy dual-task setup of pressing a key with the left or right forefinger when seeing a left- or right-pointing arrow and saying "left" or "right" when hearing one of those words and (b) the greatly more difficult task in which spoken "left" and "right" were to be given in response to the arrows and the left or right keypresses in response to the heard words. Both depend on the contrast of an easy combination of tasks with a very difficult combination involving (re-paired) the same stimuli and responses. For the perfect timesharing experiment the two tasks were performed simultaneously, whereas the two tasks were performed singly in the IAT.

The connection between the timesharing experiment and the IAT is more than the structural similarity of their respective tasks. It is also that both involve the automatic activation of mental associations (at least, that's the current best theory). For ideomotor compatible tasks, the associations are links between mental representations of sensory consequences of actions and motor representations theorized to initiate those actions. In IAT experiments, the associations are of mental categories with valences (attitudes), traits (stereotypes), or self (identities). What I see as most important, however, is that both of these paradigms allow observation of automatic operation of associations, meaning that they operate without mental effort or awareness. In the

difficult task combinations of both paradigms, the automatically active associations interfere with instructed performance, rather than facilitating it.

In an article we published in 2017, Marzu and I stated that the body of work we had been building was transforming understanding of the relation between conscious and unconscious mental functioning by making much clearer how automatically activated mental associations can shape conscious judgments outside of awareness. We had borrowed that idea from another nineteenth-century German philosopher-psychologist (also a physicist and physiologist), Hermann von Helmholtz (1925), who called it *unbewusster Schluss* (translated: unconscious inference). We used two metaphors to try to capture this: "associations might be understood as mental pigments that operate in combination to construct rich mental images and judgments. A more psychological metaphor is that a mass of associative knowledge acts as a *cultural filter* that elaborates perception and judgment" (Greenwald & Banaji, 2017, p. 868).

Had I not taken the orchestra class in 1949, I would have graduated high school a year earlier, I would not have gone to M&A, and I would not have become a trumpet player. Would I have gotten into Yale? Without the Yale influences, I might have ended up in a discipline other than psychology. I would have gotten my PhD a year later, and I could not conceivably have started a postdoctoral position with Sam Messick's Personality Research Group at ETS in fall of 1963. That last observation is the most significant one. Also arriving at ETS to work as a research assistant in fall 1963 was Jean Alexander, who had graduated from Oberlin as a psychology major two years earlier. Jean and I married six months after our first date. I am crying as I write this, remembering the last fifty-seven years with Jean, who died of leukemia in 2021. Unlike my crying in desperation in 1949, my crying as I write about Jean has a large component of joy. It was Jean's nature for others (not just me) to be happy in her presence. I brought her along to all professional occasions I could persuade her to attend, including quite a few conferences away from home. Early on, I understood that not only was I happier when Jean was with me - my colleagues also seemed much happier to see me when Jean was with me.

Jean influenced a great many things that I did in the past fifty-seven years. Her influence continues. When she and I disagreed, I might try to persuade her of my view. That no longer works. I frequently described Jean to others as "my better 80%." Her death has not changed that. I can't imagine how my choice in 1949 to learn to play trumpet could have turned out any better.

Suggested Reading

- Banaji, M. R., & Greenwald, A. G. (1995). Implicit gender stereotyping in judgments of fame. Journal of Personality and Social Psychology, 68, 181–198.
- Draine, S. C., & Greenwald, A. G. (1998). Replicable unconscious semantic priming. Journal of Experimental Psychology: General, 127, 286–303.
- Fazio, R. H., Sanbonmatsu, D. M., Powell, M. C., & Kardes, F. R. (1986). On the automatic activation of attitudes. *Journal of Personality and Social Psychology*, 50, 229–238.

- Graf, P., & Schacter, D. (1985). Implicit and explicit memory for new associations in normal and amnesic subjects. Journal of Experimental Psychology: Learning, Memory, and Cognition, 11, 501-518.
- Greenwald, A. G. (2010). Under what conditions does intergroup contact improve intergroup harmony? In M. H. Gonzales, C. Tavris, & J. Aronson (Eds.), The Scientist and the Humanist: A Festschrift in Honor of Elliot Aronson (pp. 267-281). New York: Psychology Press.
- Greenwald, A. G., & Albert, S. M. (1968). Observational learning: A technique for elucidating S-R mediation processes. Journal of Experimental Psychology, 76, 267–272.
- Greenwald, A. G., & Banaji, M. R. (1995). Implicit social cognition: Attitudes, self-esteem, and stereotypes. *Psychological Review*, 102, 4–27.
 - (2017). The implicit revolution: Reconceiving the relation between conscious and unconscious. American Psychologist, 72, 861-871.
- Greenwald, A. G., & De Houwer, J. (2017). Unconscious conditioning: Demonstration of existence and difference from conscious conditioning. Journal of Experimental Psychology: General, 146, 1705–1721.
- Greenwald, A. G., Gonzalez, R., Guthrie, D. G., & Harris, R. J. (1996). Effect sizes and p-values: What should be reported and what should be replicated? *Psychophysiology*, 33, 175–183.
- Greenwald, A. G., McGhee, D. E., & Schwartz, J. L. K. (1998). Measuring individual differences in implicit cognition: The Implicit Association Test. Journal of Personality and Social Psychology, 74, 1464–1480.
- Greenwald, A. G., Pratkanis, A. R., Leippe, M. R., & Baumgardner, M. H. (1986). Under what conditions does theory obstruct research progress? *Psychological Review*, 93, 216–229.
- Jacoby, L. L., & Whitehouse, K. (1989). An illusion of memory: False recognition influenced by unconscious perception. *Journal of Experimental Psychology: General*, 118(2), 126-135.
- James, W. (1890). The Principles of Psychology, 2 vols. New York: Holt.
- Lotze, R. H. (1852) Medicinische psychologie. Leipzig: Weidmann.
- Pratkanis, A. R., Eskenazi, J., & Greenwald, A. G. (1994). What you expect is what you believe (but not necessarily what you get): A test of the effectiveness of subliminal self-help audiotapes. *Basic and Applied Social Psychology*, 15, 251–276.
- Pratkanis, A. R., Greenwald, A. G., Leippe, M. R., & Baumgardner, M. H. (1988). In search of reliable persuasion effects: III. The sleeper effect is dead: Long live the sleeper effect. *Journal of Personality and Social Psychology*, 54, 203-218.
- Skinner, B. F. (1950). Are theories of learning necessary? *Psychological Review*, 57(4), 193-216.
- von Helmholtz, H. (1925). *Handbook of Physiological Optics* (Vol. 3; J. P. C. Southall, Trans.). New York: Optical Society of America (Original work published 1867).

"This is a masterpiece of an introduction to social psychology. The greatest living scientists from the first three generations tell their stories about how they discovered, fell in love with, and, in many cases, transformed the field of their dreams with now classic studies of social experience and influence. As social psychologists will tell you, learning ideas through stories about people is the most effective way to learn a subject. Because it's also the most enjoyable way, I'd recommend this book to any teacher of psychology who wants to both inform and inspire their students with these stories from the 'golden age' of social psychology."

"Prominent social psychologists reflect candidly on their professional journeys, highlighting interconnections among theories, research questions, and people. By illustrating the way personal abilities, social circumstances, and mere chance combine to drive successful careers, these stories can inform and motivate future researchers and all those curious about human behavior." Susan Clayton, The College of Wooster, USA

"I *love* Saul Kassin's collection of the life stories told by social psychology's legends – and their stories of the legends who mentored them. For any social psychologist or student, these captivating accounts of serendipitous encounters, disheartening failures, creative moments, fruitful collaborations, and surmounted self-doubts will inform and inspire." David G. Myers, Hope College, USA, and co-author (with Jean Twenge) of *Social Psychology*, 14th Edition

"With the care of a master architect, Saul Kassin has assembled the personal stories of 48 leading 'pillars' of social psychology to build a monumental edifice with secret doorways connecting many of the rooms. At once thought-provoking and fun, this is a terrific book for anyone interested in psychology."

Scott Plous, Wesleyan University, USA, and Executive Director of the Social Psychology Network

"This book is a treasure trove of wisdom and inspiration. Any one story provides a behind-the-scenes account of one scholar's journey, but the collection reveals an immersive and interconnected history of how science unfolds." Toni Schmader, The University of British Columbia, Canada

Cover illustration: 3 Pillars - Sanctuary Print, by Yoram Raanan. © 2022 www.raanan.art



Designed by EMC Design Ltd