

FOR





# WHEN

# THE REVOLUTION CAME

# AMY CUDDY

*As a young social psychologist, she played by the rules and won big: an influential study, a viral TED talk, a prestigious job at Harvard. Then, suddenly, the rules changed.*

*By Susan Dominus  
Photograph by Alec Soth*

# I AMY

# FIRST MET CUDDY

in January, soon after she moved into a new office at the Harvard School of Public Health. Cuddy was, at the time, officially on the faculty at Harvard Business School, but she was taking a temporary leave, her small box of an office filled with boxes. As she talked about her life in recent years, my attention kept drifting to her left arm, which she had trapped underneath her right leg, which crossed the left. She was slightly hunched over, and yet her right arm, long and lean — she danced for many years — gesticulated freely and expressively, so that the contrast gave the impression of someone in a conflicted emotional state, someone both wanting to tell her story and unsure about doing so.

That visual might have escaped me altogether, except that Cuddy, a social psychologist, is best known to the public for her work on body language. And hers seemed to embody a divide that had characterized her life in the last couple of years, a sense of two selves, one highly sensitive, the other more confident, even skilled in the art of conveying that confidence.

Cuddy became famous in her field for a 2010 study about the effects of “power poses.” The

study found that subjects who were directed to stand or sit in certain positions — legs astride, or feet up on a desk — reported stronger “feelings of power” after posing than they did before. Even more compelling than that, to many of her peers, was that the research measured actual physiological change as a result of the poses: The subjects’ testosterone levels went up, and their cortisol levels, which are associated with stress, went down.

The study impressed not only Cuddy’s colleagues — it was published in the prestigious journal *Psychological Science* — but also CNN, Oprah magazine and, inevitably, someone at the TED conference, which invited Cuddy to speak in 2012. In the talk, Cuddy was commanding; she was also confessional, telegenic, empathetic. She really wanted the audience members to ace their job interviews, to find confidence in the face of nerves, and she had a plan, a science-supported life hack, for how to do it: the power pose. “Don’t fake it till you make it — fake it till you become it,” she told the audience, before urging them to share the science of power posing with others who might need that boost: “It can significantly change the outcomes of their life.”

The video is now TED’s second-most popular, having been seen, to date, by some 43 million viewers. In the years after the talk, Cuddy became a sought-after speaker, a quasi celebrity and, eventually, the author of a best-selling book, “Presence,” in 2015. The power pose became the sun salutation for the professional woman on the cusp of leaning in. Countless hopefuls, male and female, locked themselves in bathroom stalls before job interviews to make victory V’s with their arms; media trainers had their speakers dutifully practice the pose before approaching the stage. Cuddy has gone on to give talks on power and the body (including power posing) and stereotyping to women’s groups in Australia, at youth homeless shelters, to skin-care workers by the thousands, to employees at Target and agents at State Farm Insurance. Cuddy’s fans approach her in airports, on ski slopes in Telluride, in long lines after her talks, to hug or to thank her, filled with their own power-posing stories — sharing how bold body language helped them get their jobs or win some match or confront a bully at work.

But since 2015, even as she continued to stride onstage and tell the audiences to face

down their fears, Cuddy has been fighting her own anxieties, as fellow academics have subjected her research to exceptionally high levels of public scrutiny. She is far from alone in facing challenges to her work: Since 2011, a methodological reform movement has been rattling the field, raising the possibility that vast amounts of research, even entire subfields, might be unreliable. Up-and-coming social psychologists, armed with new statistical sophistication, picked up the cause of replications, openly questioning the work their colleagues conducted under a now-outdated set of assumptions. The culture in the field, once cordial and collaborative, became openly combative, as scientists adjusted to new norms of public critique while still struggling to adjust to new standards of evidence.

Cuddy, in particular, has emerged from this upheaval as a unique object of social psychology's new, enthusiastic spirit of self-flagellation — as if only in punishing one of its most public stars could it fully break from its past. At conferences, in classrooms and on social media, fellow academics (or commenters on their sites) have savaged not just Cuddy's work but also her career, her income, her ambition, even her intelligence, sometimes with evident malice. Last spring, she quietly left her tenure-track job at Harvard.

Some say that she has gained fame with an excess of confidence in fragile results, that she prized her platform over scientific certainty. But many of her colleagues, and even some who are critical of her choices, believe that the attacks on her have been excessive and overly personal. What seems undeniable is that the rancor of the critiques reflects the emotional



Cuddy during her TED talk in 2012.

at Indiana University, found that when he asked children to execute a simple task (winding line on a fishing rod), they performed better in the company of other children than they did when alone in a room. Over the following decades, a new discipline grew up within psychology to further interrogate group dynamics: how social groups react in certain circumstances, how the many can affect the one.

way group pressures or authority figures could influence human behavior. In one simple study on conformity in 1951, the social psychologist Solomon Asch found that people would agree that one drawn line matched the length of another — even if it clearly did not — if others around them all agreed that it did. In subsequent years, researchers like Stanley Milgram (who tested how people weighed their consciences against the demands of authority) and Philip Zimbardo (who observed the effect of power on students assigned as either prison guards or prisoners) rejected the traditional confines of the lab for more theatrical displays of human nature. "They felt the urgency of history," says Rebecca Lemov, a professor of the history of science at Harvard. "They really wanted to make people look."

Since the late 1960s, the field's psychologists have tried to elevate the scientific rigor of their work, introducing controls and carefully designed experiments like the ones found in medicine. Increasingly complex ideas about the workings of the unconscious yielded research with the charm of mesmerists' shows, revealing unlikely forces that seem to guide our behavior: that simply having people wash their hands could change their sense of culpability; that people's evaluations of risk could easily be rendered irrational; that once people have made a decision, they curiously give more weight to information in its favor. Humans, the research often suggested,

***She worried about asking peers to collaborate, suspecting that they would not want to set themselves up for intense scrutiny.***

toll among scientists forced to confront the fear that what they were doing all those years may not have been entirely scientific.

**One of the seminal** social-psychology studies, at the turn of the 20th century, asked a question that at the time was a novel one: How does the presence of other people change an individual's behavior? Norman Triplett, a psychologist

The questions grew even more profound, using experiments to tease out universal susceptibilities, raising the possibility that behavior was more easily swayed by outside forces than personality researchers previously believed. The field reached a moment of unusual visibility in the mid-20th century, as practitioners, many of them Jewish refugees or first-generation immigrants from Europe, explored, post-World War II, the

were reliably mercurial, highly suggestible, profoundly irrational, tricksters better at fooling ourselves than anyone else.

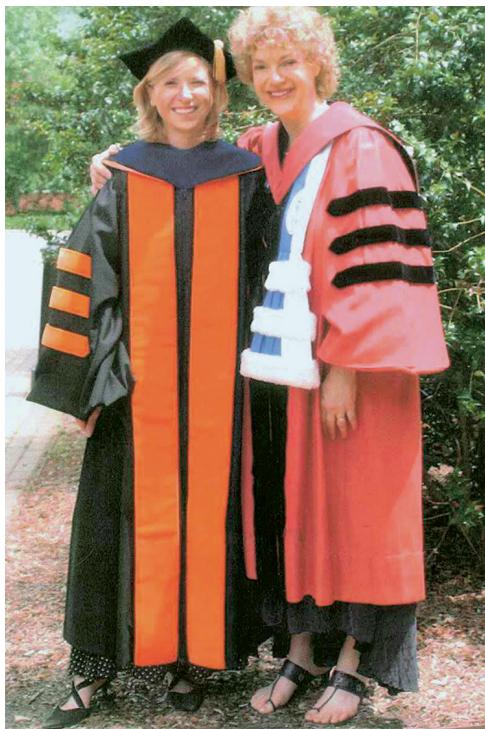
Already relatively accessible to the public, the field became even more influential with the rise of behavioral economics in the 1980s and 1990s, as visionaries like Richard Thaler, (who won the Nobel Prize in economics this month) found applications for counterintuitive social-psychology insights that could be used to guide policy. In 2000, Malcolm Gladwell, the author of the best-selling “Tipping Point,” applied irresistible storytelling to the science, sending countless journalists to investigate similar terrain and inspiring social psychologists to write books of their own. In 2006, Daniel Gilbert, a professor of psychology at Harvard, published the best seller “Stumbling on Happiness” — a book that tried to explain why we plan so poorly for our own future. That same year, TED started airing its first videos, offering a new stage for social psychologists with compelling findings, ideally surprising ones. The field was experiencing a visibility unknown since the midcentury; business schools, eager for social psychologists’ insights into leadership and decision-making, started pursuing social psychologists, with better pay and more funding than psychology graduate schools could offer.

This moment of fizziness for the discipline was already underway when Cuddy arrived at Princeton’s graduate program in 2000, transferring there to follow her adviser, Susan Fiske, with whom she first worked at the University of Massachusetts Amherst. She moved to Princeton with her husband at the time — they later divorced — and in her second year there, she had a child. Her academic work continued to thrive as she collaborated with Fiske on research on stereotyping, which found that groups of people (for example of a particular ethnicity) who were judged as nice were assumed to be less competent and vice versa. (“Just Because I’m Nice Don’t Assume I’m Dumb,” was the headline of a Harvard Business Review by Cuddy.) Fiske and Cuddy’s resulting papers are still heavily cited, formulating a framework for stereotyping that proved hugely influential on the field.

And yet, especially early on at Princeton, Cuddy felt uncertain of her place there. She feared that her brain simply could not function at a high-enough level to power her through the program. Cuddy suffered a traumatic brain injury in a car accident the summer after her sophomore year in college, when a friend of hers fell asleep at the wheel while Cuddy was asleep in the back seat. In the months after the accident, Cuddy was told she should not expect to finish college; her fog was so deep that she remembers being retaught how to shop for groceries. Even

after Cuddy recovered, her friends told her that she had changed, that she was not the same person — but she could not remember who she had been before. It took her four years and multiple false starts before she could return to college. Even after she was accepted to graduate school at University of Massachusetts, she confessed to Fiske that she feared she would not be able to keep up with the work.

Cuddy was trained as a ballet dancer — in between her stints at college, she danced as an apprentice with the Colorado Ballet — but her interest in studying the body and its relationship to power did not begin until 2009, her first year



Cuddy with her adviser, Susan Fiske, at Princeton.

as a teacher at Harvard Business School. At the invitation of her department chair, she joined a small circle of academics meeting with Joe Navarro, a former F.B.I. agent who had written a book about body language. The invited parties, which included Dana Carney, a professor at Columbia University who studied body language and power, all spoke briefly about their work. In the conversation that followed, Navarro pointed out that Cuddy’s own body language, during her presentation, signaled insecurity: She was fiddling with her necklace, wrapping her arms around her torso.

Carney and Cuddy had been noticing the

same kinds of body language in their female students, who performed well on written materials but lost points, compared with their male counterparts, on class participation. Their body language was constricted; they raised their hands with their elbows cradled in their other hands; they made themselves physically small; they spoke less often.

The two researchers wondered whether people whose physical cues looked like their female students’ — self-protective, insecure — would feel more powerful or even change their behavior if they simply adopted more expansive body positions.

Carney and Cuddy brainstormed a research project to test this question. At Columbia, Carney recruited students, telling them that they were part of a study intended to measure the effects of placing an electrocardiograph’s electrodes either above or below the heart. In the study of 42 subjects that they eventually published, experimenters arranged half the students into positions associated with high power (leaning back in a chair with feet crossed on a desk, for example) and half the students into positions associated with low power (like crossing arms in front of the body). Before and after the poses, experimenters took saliva swabs from the students to measure how the positions affected cortisol and testosterone levels. They also asked the students to report (before and after the poses) how in charge and powerful they felt on a scale of one to four (a measurement known as “self-reported feelings of power”). And they measured whether that feeling had what researchers call a “downstream effect” — a resulting behavior. People who feel power, the literature suggests, are more likely to engage in a range of certain behaviors, including risk-taking; so the experiment also measured the subjects’ willingness to bet on a roll of the dice.

“I remember how happy we were when Dana called me with the results,” Cuddy says. “Everything went in the direction it was supposed to.” The abstract that they eventually wrote — that their editors approved — reflects the incautious enthusiasm that characterized the era: “That a person can, by assuming two simple 1-min poses, embody power and instantly become more powerful, has real-world, actionable implications.”

In 2014, on a podcast called “Story Collider,” Cuddy connects the study that made her so famous with the accident that subtly shifted her identity. After the crash, she recalled, she felt as if she were merely passing for herself, an inauthentic version of who she used to be. It made sense, then, that she ended up “studying how people can become their aspirational selves,” she said. “How can you become a self that you are not now?”

**The year that** Amy Cuddy published her power-pushing paper, Joseph Simmons, who attended graduate school at Princeton with Cuddy, was starting to think about his own seminal paper, one that would, unknown to either of them, have as much influence on her life as it would on his own; it would, in fact, change not just their lives but the field as they knew it, with wildly differing consequences for each of them.

Cuddy and Simmons, each of whom came from working-class backgrounds, had been fond of each other at Princeton, even if they did not socialize often: Cuddy was a new mother, and Simmons was five years younger and heavily committed to his softball team. Simmons considered Cuddy a friend, someone he was always happy to see at a party, despite their obvious differences: Cuddy, who used to follow the Grateful Dead, would have been the one dancing at the party, while Simmons would have been the one laughing with his close friend, a fellow graduate student named Leif Nelson, about the latest buzzy journal article that seemed, to them, ridiculous.

Having arrived at Princeton wide-eyed, straight from Mount St. Mary's College in Maryland, Simmons, within a few years, appeared to some of his classmates to have lost some of his

professor of psychology at Cornell, Daryl Bem, who claimed that he had strong evidence for the existence of extrasensory perception. The paper struck them as the ultimate in bad-faith science. "How can something not be possible to cause something else?" Nelson says. "Oh, you reverse time, then it can't." And yet the methodology was supposedly sound. After years of debating among themselves, the three of them resolved to figure out how so many researchers were coming up with such unlikely results.

Over the course of several months of conference calls and computer simulations, the three researchers eventually determined that the enemy of science — subjectivity — had burrowed its way into the field's methodology more deeply than had been recognized. Typically, when researchers analyzed data, they were free to make various decisions, based on their judgment, about what data to maintain: whether it was wise, for example, to include experimental subjects whose results were really unusual or whether to exclude them; to add subjects to the sample or exclude additional subjects because of some experimental glitch. More often than not, those decisions — always seemingly justified as a way of eliminating noise — conveniently strengthened

researchers would happen to get the results they achieved — or even more extreme ones — if there were no phenomena, in truth, to observe? (And no systematic error.) For decades, the standard of so-called statistical significance — also the hurdle to considering a study publishable — has been a P-value of less than 5 percent.

To examine how easily the science could be manipulated, Simmons and Simonsohn ran a study in which they asked 20 participants their ages (and their fathers' birthdays). Half the group listened to the Beatles song "When I'm Sixty-Four"; the other listened to a control (the instrumental music "Kalimba"). Using totally standard methodology common to the field, they were able to prove that the participants who listened to the Beatles song were magically a year and a half younger than they were before they had heard the music. The subject heading of the explanation: "How Bad Can It Be? A Demonstration of Chronological Rejuvenation." It was witty, it was relatable — everyone understood that it was a critique of the fundamental soundness of the field.

"We realized entire literatures could be false positives," Simmons says. They had collaborated with enough other researchers to recognize that the practice was widespread and counted themselves among the guilty. "I P-hacked like crazy all through my time at Princeton, and I still couldn't get interesting results," Simmons says.

The paper generated its fair share of attention, but it was not until January 2012, at a tense conference of the Society of Personality and Social Psychology in San Diego, that social psychologists began to glimpse the iceberg looming ahead — the sliding furniture, the recriminations, the crises of conscience and finger-pointing and side-taking that would follow. At the conference, several hundred academics crowded into the room to hear Simmons and his colleagues challenge the methodology of their field. First, Leslie John, then a graduate student, now an associate professor at the Harvard School of Business, presented a survey of 2,000 social psychologists that suggested that P-hacking, as well as other questionable research practices, were common. In his presentation, Simonsohn introduced a new concept, a graph that could be used to evaluate bodies of research, using the P-values of those studies (the lower the overall P-values, the better). He called it a P-curve and suggested that it could be used, for example, to evaluate the research that a prospective job candidate submitted. To some, the implication of the combined presentations seemed clear: The field was rotten with the practice, and egregious P-hackers should not get away with it.

(Continued on Page 50)

**'The reformers were annoyed, because they felt like they had to come in after the fact and clean up after us. And it was true.'**

idealism about academia; maybe he exhibited his idealism about science in a way that could be mistaken for cynicism. Simmons had an unusual interest in statistics, the way its airtight logic could neatly prove, or disprove, the worth of an extravagant idea. He and Nelson were endlessly critical of other studies' findings, an intellectual exercise they enjoyed and considered essential. The two of them, Nelson says, were "into thinking about subtleties in data collection and analysis."

After finishing a postdoctoral program at Princeton, Simmons lost touch with Cuddy, who was by then teaching at Northwestern. He remained close to Nelson, who had befriended a behavioral scientist, also a skeptic, Uri Simonsohn. Nelson and Simonsohn kept up an email correspondence for years. They, along with Simmons, took particular umbrage when a prestigious journal accepted a paper from an emeritus

the findings' results. The field (hardly unique in this regard) had approved those kinds of tinkering for years, underappreciating just how powerfully they skewed the results in favor of false positives, particularly if two or three analyses were underway at the same time. The three eventually wrote about this phenomenon in a paper called "False-Positive Psychology," published in 2011. "Everyone knew it was wrong, but they thought it was wrong the way it's wrong to jaywalk," Simmons recently wrote in a paper taking stock of the field. "We decided to write 'False-Positive Psychology' when simulations revealed it was wrong the way it's wrong to rob a bank."

Simmons called those questionable research practices P-hacking, because researchers used them to lower a crucial measure of statistical significance known as the P-value. The P stands for probable, as in: How probable is it that

Journal-paper presentations on statistics are usually unremarkable affairs, but this one precipitated a sequence of exchanges so public that the field had to take notice. The first took place during that event, with Norbert Schwarz, an eminent social psychologist, in the audience. Schwarz, as he listened, grew furious: He believed that the methodology of the survey was flawed, and he indignantly objected to the idea of the P-curve as a kind of litmus test aimed at individuals. He would not let these ideas go uncontested; he interrupted loudly from the front row, violating standard academic etiquette. “The whole room was like, ‘Oh, my God, what just happened?’” recalls Brian Nosek, a social psychologist who now runs the Center for Open Science, intended to encourage and normalize replications. Others quietly thanked Schwarz for bravely speaking up.

Not content to stop there, Schwarz followed up four days later with an open letter to 5,000 members of the society’s listserv, explaining in further detail, and with some condescension, his reservations. Although Simonsohn was angry, he still hoped to cool down the conversation. He emailed Schwarz asking if they could talk, so that they could come to a sort of understanding, in the name of science, and release a joint statement. Schwarz agreed but told Simonsohn, over the course of several email exchanges, that he needed more time. Simonsohn lost patience after three weeks: He posted large parts of the email exchange on his personal website, then posted a blistering attack on Schwarz on the society’s listserv, filled with bold caps and underlines, in which he said, among other things, that he knew firsthand that Schwarz had engaged in P-hacking.

“I regret it,” Simonsohn says now about posting the emails. Since then, Simmons, Simonsohn and Nelson say they have given a lot of thought to codes of conduct for communicating responsibly when conveying concerns about a scientist’s work. But the academic blowup between Simonsohn, then a relative unknown in social psychology, and Schwarz, the standard-bearer, signaled from the beginning that leaders on each side would ignore the norms of scientific discourse in an effort to discredit the other. One imminent shift in methods would bring another shift — one of tone — that would affect the field almost as drastically.

**After 2012**, questions of methodology started dominating every social-psychology conference, as did the topic of replications. Across disciplines, a basic scientific principle is that multiple teams should independently verify a result before it is accepted as true. But for the majority of social-psychology results, even the most influential ones, this hadn’t happened. Bryan Nosek,

who started the Reproducibility Project (now called the Center for Open Science), an effort to test 100 important social-psychology papers, said that recognition of potential flawed methodology only fueled interest in his project. “The paper shone a light on how easily things could go wrong,” Nosek says. “Knowing that possibility in concept made the Reproducibility Project a test case in some people’s mind of ‘Does it?’”

For years, researchers treated journal articles, and their authors, with a genteel respect; even in the rare cases where a new study explicitly contradicted an old one, the community assumed that a lab error must account for the discrepancy. There was no incentive to replicate, in any case: Journals were largely not interested in studies that had already been done, and failed replications made people (maybe even your adviser) uncomfortable.

But in the years after that Society of Personality and Social Psychology conference, a sense of urgency propelled a generation of researchers, most of them under 40, to re-examine the work of other, more established researchers. And politeness was no longer a priority. “All of a sudden you have people emailing other people, asking for their data and then writing blog posts accusing them of shoddy practices,” says Eli Finkel, a social psychologist at Northwestern. “That was unheard-of. Now it was happening all the time.” Some blog posts took on the impact of journal articles, as interested parties weighed in with an impromptu peer review. In 2014, *Psychological Science* started giving electronic badges, an extra seal of approval, to studies that made their data and methodologies publicly available and preregistered their design and analysis ahead of time, so that researchers could not fish around for a new hypothesis if they turned up some unexpected findings.

Not surprisingly, replicators sometimes encountered the kind of outraged resistance that Simmons and Simonsohn initially did. The same month that Simmons and Simonsohn gave their talk, Stéphane Doyen, a social psychologist in Belgium, published a paper challenging a classic study in the field of priming, which holds that small cues, like exposure to certain words, can subconsciously trigger behaviors. The original study found that research subjects walked more slowly after being exposed to words associated with old age; the replicators found no such effect and titled their journal article “Behavioral Priming: It’s All in the Mind; but Whose Mind?” John Bargh, a professor at Yale, a luminary who published the original study, responded with a combative post on *Psychology Today*’s blog, claiming that discrepancies in the experiment design accounted for the difference and calling the researchers “incompetent or ill informed.” When other priming studies failed to replicate later that year, the Nobel laureate Daniel Kahneman, who discussed priming in his book “Thinking Fast and Slow,” wrote a letter

to social psychologists who studied the effect, urging them to turn their attitude around. “To deal effectively with the doubts, you should acknowledge their existence and confront them straight on,” he wrote.

The intention of replicators was now unassailably noble — who could argue with better science? — but there was also, for the first time, status to be found in topplings, as journals started publishing more replications, many of which received lavish press attention. It was also inevitable that those being challenged would read envy into their attackers’ motivations. (In a tweet, Gilbert described those he deemed the worst offenders as “shameless little bullies.”)

Jay Van Bavel, a social psychologist at New York University, has tweeted openly about a published nonreplication of one of his studies and believes, as any scientist would, that replications are an essential part of the process; nonetheless, he found the experience of being replicated painful. “It is terrifying, even if it’s fair and within normal scientific bounds,” he says. “Because of social media and how it travels — you get pile-ons when the critique comes out, and 50 people share it in the view of thousands. That’s horrifying for anyone who’s critiqued, even if it’s legitimate.”

The field, clearly, was not moving forward as one. “In the beginning, I thought it was all ridiculous,” says Finkel, who told me it took him a few years before he appreciated the importance of what became known as the replication movement. “It was like we had been having a big party — what big, new, fun, cool stuff can we discover? And we forgot to double-check ourselves. And then the reformers were annoyed, because they felt like they had to come in after the fact and clean up after us. And it was true.”

**In August 2014**, the day before her second marriage, Amy Cuddy learned that a replication of her 2010 study led by a 34-year-old economist at the University of Zurich named Eva Ranehill had failed to yield the same results. “I remember thinking, Oh, bummer,” Cuddy says. But she was not distraught; often there was some perfectly good reason for a discrepancy in two studies of the same concept.

There were several key differences — Ranehill’s sample size, at 200, was much bigger, and she had designed a double-blind setup. Ranehill had her subjects hold two poses for three minutes each. She did not find an increase in either risk-taking behavior or the expected hormone changes.

Cuddy thought it was likely that the difference in time — six minutes of standing versus two — was a crucial one and probably accounted for the disparity in the results. “It’s not a crazy thing to test,” Cuddy says. “I guess under the theory that more is better? But it could go the other way — three minutes is a really, really long time to be holding a pose like that. It seems likely to me that it would be really *(Continued on Page 52)*

## Cuddy

(Continued from Page 50)

uncomfortable, but sure, study it, and let's see."

She was relieved to see that the "feelings of power" finding had replicated. But Ranehill used language in her write-up that played down that finding's importance. Although in one study of her own, Cuddy also played down the finding, she has otherwise consistently, in interviews, been enthusiastic about the idea that a body posture could change someone's feelings. "We're psychologists," she says. "We try to change how people feel." She also, at the time of the Ranehill replication, still anticipated that other research would probably show downstream effects — more risk taking, or more competitiveness, or better performance in job interviews.

By the time Cuddy got word of Ranehill's replication, she had given her TED talk, developed a significant speaking career and was writing a book. Simmons had received tenure at Wharton and was writing, with Simonsohn, a blog called Data Colada, in which they sometimes tried to replicate other people's work. By 2014, there was near-unanimous agreement the Data Colada team had profoundly changed the field's research techniques for the better. But for the average researcher, an email from someone at Data Colada signaled unpleasantness ahead. "It's like the police knocking on your door in the middle of the night," one psychologist said.

In the wake of Ranehill's failed replication, Cuddy and Carney set to work on a response. Carney, who is now a tenured associate professor of management at the University of California, Berkeley, tried to chart a P-curve of all 33 studies they were mentioning in their paper (which was already under review). Carney sent the paper and the P-curve to Nelson for some feedback, but he sent it on to Simmons and Simonsohn, as they were the experts.

The letter Simmons wrote back to Carney was polite, but he argued that her P-curve had not been executed correctly. He and Simonsohn had each executed P-curves of the 33 studies, and each found that it was flat, suggesting that the body of literature it reflected did not count as strong evidence. He did write that "conceptual points raised before that section are useful and contribute to the debate" but that they should take the P-curve out. "Everybody wins in that case." According to Cuddy, she and Carney thought the P-curve science was not as settled as Simmons believed it to be. But afraid of public recrimination, they did exactly as he said — they took out the P-curve.

A few weeks after the paper was published, Cuddy learned from Simmons and Simonsohn that they were writing a blog post on the paper. A mutual friend of Cuddy and Simmons's from graduate school, Kenworthey Bilz, a professor of law at the University of Illinois, tried to reassure Cuddy. "He'll say his piece, and you'll say yours,

and that will be the end of it," Bilz told Cuddy. "It's not like you're going to become the poster girl for this kind of thing."

Cuddy was at her home office in Boston when she received an email from Simmons and Simonsohn. They showed her a draft of the post they planned to put online criticizing the paper; they invited feedback on anything the authors felt was incorrect or unfair. "We embrace trying to be as civil as possible but understanding that at some point you are going to say, 'You were wrong,'" Simonsohn said. "People won't like that, no matter how much you dress it up. It's not something people want to hear."

The post criticized the new paper, as well as the 2010 study. It showed Simmons and Simonsohn's own unfavorable P-curve and essentially argued that the original published findings on hormones and risk-taking were probably a result of chance. They did not include a feelings-of-power measure in the P-curve they showed. But the blog post did mention in its last footnote that there was a significant effect of power posing on "self-reported power," although the language made it clear that it didn't count for much, Simmons believes that self-reports of power generally reflect what is called a demand effect — a result that occurs when subjects intuit the point of the study. Cuddy believes that studies can be constructed to minimize that risk and that demand effects are often nuanced.

Cuddy responded to Simonsohn with a few points that they incorporated into the post but said she preferred to write a longer response in a context in which she felt more comfortable.

Cuddy felt ill when Simmons and Simonsohn published the post with the headline: "Reassessing the Evidence Behind the Most Popular TED Talk." As illustration, they used a picture of Wonder Woman. Cuddy felt as if Simmons had set them up; that they included her TED talk in the headline made it feel personal, as if they were going after her rather than the work.

The post, which Simonsohn distributed to his email list of hundreds, quickly made the rounds. "People were sending me emails like I was dying of cancer," Cuddy says. "It was like, 'We send our condolences,' 'Holy crap, this is terrible' and 'God bless you; we wish we could do something, but obviously we can't.' She also knew what was coming, a series of events that did, in fact, transpire over time: subsequent scrutiny of other studies she had published, insulting commentary about her work on the field's Facebook groups, disdainful headlines about the flimsiness of her research. She paced around, distraught, afraid to look at her email, afraid not to. She had just put together a tenure package and worried that the dust-up would be a continuing distraction.

Cuddy did not like seeing her work criticized in a non-peer-reviewed format, but she wrote a bland statement saying, essentially, that she disagreed with their findings and "looked forward to

more research on this important topic." Carney reassured Cuddy in the months after the Data Colada post that their paper would eventually be vindicated — of course the effects were real; someone would prove it eventually.

**Eventually, the Data** Colada post caught the eye of another influential blogger, Andrew Gelman, a professor of statistics and political science at Columbia University, whose interest in Cuddy's work would prove durable, exacting and possibly career-changing for Cuddy. Gelman wields his sizable influence on the field from afar, on his popular blog [andrewgelman.com](http://andrewgelman.com), where he posts his thoughts on best statistical practices in the sciences, with a frequent emphasis on what he sees as the absurd and unscientific. Gelman, who studied math and physics at M.I.T. before turning to statistics, does not believe that social psychology is any more guilty of P-hacking than, say, biology or economics. But he has devoted extensive attention to the field, especially in more recent years, in part because of the way the media has glorified social-psychology research. He is respected enough that his posts are well read; he is cutting enough that many of his critiques are enjoyed with a strong sense of schadenfreude.

Four months after the Data Colada post, Gelman, with a co-author, published an article in *Slate* about Carney and Cuddy's 2010 study, calling it "tabloid fodder." Eventually, Cuddy's name began appearing regularly in the blog, both in his posts and in comments. Gelman's writing on Cuddy's study was coolly dismissive; it bothered him that Cuddy remained fairly silent on the replication and the Data Colada post. For all he knew, Cuddy was still selling the hormone effect in her speaking gigs and in her best-selling book, "Presence," which he had not read. Had he looked, he would have been annoyed to see that Cuddy did not include a mention of the Ranehill replication. But he might have been surprised to see how little of the book focused on power posing (just a few pages).

On his site, Cuddy's name, far from the only one he repeatedly invoked, became a go-to synecdoche for faulty science writ large. When he saw that Cuddy had been invited to speak at a conference, he wondered why the organizers had not invited a bunch of other famous figures he clearly considered bad for science, including Diederik Stapel, who had been accused of outright fraud.

His site became a home for frequently hostile comments from his followers. "She has no serious conception of 'science,'" one posted. Another compared Cuddy to Elizabeth Holmes, the Theranos chief executive under investigation for misleading investors. Though Gelman did encourage his readers to stick to the science, he rarely reined anyone in. In one exchange in July 2016, a commenter wrote, "I've wondered whether some of Amy Cuddy's mistakes are due to the fact that

she suffered severe head trauma as the result of a car accident some years ago." Gelman replied, "A head injury hardly seems necessary to explain these mistakes," pointing out that her adviser, Fiske, whom he has also criticized, had no such injury but made similar errors.

Gelman, whom I met in his office in late June, is not scathing in person; he is rather mild, soft-spoken even. Gelman was vague when asked if he felt there was anything unusual about the frequency of his comments on Cuddy ("People send me things, and I respond," he said). He said it was Cuddy who was unrelenting. He later emailed me to make sure I was aware that she attacked him and Simmons and Simonsohn on a private Facebook page, without backing up her accusations with evidence; he was still waiting for a clear renunciation of the original 2010 paper on the hormonal effects of power posing. "I would like her to say: 'Jeez, I didn't know any better. I was doing what they told me to do. I don't think I'm a bad person, and it didn't get replicated' — rather than salvaging as much as she can."

Gelman considers himself someone who is doing others the favor of pointing out their errors, a service for which he would be grateful, he says. Cuddy considers him a bully, someone who does not believe that she is entitled to her own interpretation of the research that is her field of expertise.

Gelman has asked herself what motivates Cuddy. "Why not help social psychologists instead of attacking them on your blog?" she wondered aloud to me. "Why not come to a conference or hold a seminar?" When I asked Gelman if he would ever consider meeting with Cuddy to hash out their differences, he seemed put off by the idea of trying to persuade her, in person, that there were flaws in her work.

"I don't like interpersonal conflict," he said.

**On Sept. 26, 2016,** Amy Cuddy woke up and checked her phone to find a chilling text from a friend. "I'm so sorry," it said. "Are you O.K.?" She felt a familiar dread, something closer to panic. For the past year, she had mostly stopped going to social-psychology conferences, feeling a chill from her community. Another social psychologist had told her that a graduate student asked if she really was friends with Cuddy. When she responded, "Yes," the young woman asked, "Why?"

It was the kind of information Cuddy wished she did not have; her closest friends were told to stop passing on or commenting about that kind of thing, but acquaintances still did it. She felt adrift in her field. She worried about asking peers to collaborate, suspecting that they would not want to set themselves up for intense scrutiny. And she felt betrayed, not just by those who cut her down on social media, in blog posts, even in reviews (one reviewer called her "a profiteer," not hiding his contempt), but also by some of those who did not publicly defend her. She was not wrong to think that at least in some cases,

it was fear, rather than lack of support for her, that kept people from speaking up. Two tenured psychology professors at Ivy League universities acknowledged to me that they would have publicly defended some of Cuddy's positions were they not worried about making themselves targets on Data Colada and elsewhere.

Two days before Cuddy received that text from a friend, Gelman once again posted about the power-posing research, but this time he issued a challenge to Dana Carney. "When people screw up or cheat in their research, what do their collaborators say?" he wondered in the post. For Carney, he wrote, "it was not too late." Unknown to Cuddy and Gelman, Carney had already linked, in her C.V., to Simmon and Simonsohn's critique of that first, influential 2010 study, but she hadn't made the kind of statement or gesture that Gelman expected from Cuddy.

That morning of the troubling text, Cuddy logged onto her computer and discovered that Carney had posted on her website a document (then quickly published on New York magazine's site) that seemed intended to distance its author forever, in every way, from power posing. "I do not believe that 'power pose' effects are real," she said. Not only had she stopped studying power poses, "I discourage others from studying power poses."

She listed a number of methodological concerns she had, in retrospect, about the 2010 paper, most

of which, Cuddy says, Carney had never raised with her. In an email a few months earlier, Carney had clearly told Cuddy that she thought the study's data was flimsy, the sample was tiny, the effects were barely there. But Cuddy said she had never received notice that this kind of renunciation was coming. Carney declined to comment for this article, but Nelson, who is in her department, said she was clearly in a tough position, saddled with all the negatives of the work — the hit to her reputation — with none of the upside: the speaking fees and the positive feedback from teary fans that no doubt fuel Cuddy's conviction in the research.

For much of the scientific world, Carney's statement was an act of integrity and bravery. "Whoa! This is how to do it!" tweeted Michael Inzlicht, a professor of psychology and neuroscience at the University of Toronto who had eloquently written about his own crisis of confidence about his field of research, ego depletion.

To Cuddy, Carney's post seemed so sweeping as to be vague, self-abnegating. Even Simonsohn, who made clear his support for Carney's decision, thought the letter had a strangely unscientific vehemence to it. "If I do a bad job proving there's a ninth planet, I probably shouldn't say there's a ninth planet," he says. "But I shouldn't say there is no ninth planet, either. You should just ignore the bad study and go back to base line."

*(Continued on Page 55)*

## ROSS + SIMONS

celebrating our 65th anniversary of fabulous jewelry



### Sterling Silver Bolo Bracelet from Italy

Sliding bead adjusts bracelet to fit most wrists.

1/8"-wide diamond-cut cylinder beads.

Shown slightly larger for detail.

**Only \$49 plus free shipping**

Ross-Simons Item #864709

To receive this special offer, use offer code: **MAGIC45**

1.800.556.7376 or visit [www.ross-simons.com/MAGIC](http://www.ross-simons.com/MAGIC)

## Cuddy

(Continued from Page 53)

Cuddy wrote a lengthy response to Carney that New York magazine published. (New York, Slate and The Atlantic have closely reported on the replication movement.) Then she stopped taking phone calls and went almost completely offline. She found that she couldn't eat; at 5-foot-5, Cuddy went down to 100 pounds.

Less than two weeks after Carney's disavowal, Cuddy got on a plane so she could meet her commitment to speak to a crowd of 10,000 in Las Vegas. As frail as she had been since her accident, she headed to an arena in Las Vegas and roused the crowd, a tiny woman on a giant stage, taking up space, making herself big, feeling the relief of feeling powerful.

**When I emailed** Joe Simmons in July and asked to meet with him, he readily agreed but warned me that he does not check his email often. "I had to take email off my phone," he explained when we met at a coffee shop across the river from Wharton. A lot of his work these days was stressful — sometimes the emailer was angry, sometimes he was — so if he looked at his phone before bed, "that was it — I wouldn't sleep all night."

When Simmons and I met, I asked him why he eventually wrote such a damning blog post, when his initial correspondence with Carney did not seem particularly discouraging. He and Simonsohn, he told me, had clearly explained to Cuddy and Carney that the supporting studies they cited were problematic as a body of work — and yet all the researchers did was drop the visual graph, as if deliberately sidestepping the issue. They left in the body of literature that Simmons and Simonsohn's P-curve discredited. That apparent disregard for contrary evidence was, Simmons said, partly what prompted them to publish the harsh blog post in the first place.

But the email that Simmons and Simonsohn had sent was, in fact, ambiguous: They had explicitly told her to drop the P-curve and yet left the impression that the paper was otherwise sound. At my request, Simmons looked back at his original email. I watched as he read it over. "Oh, yeah," he said quietly. He had a pained look on his face. "We did say to drop the graph, didn't we?" He read it over again, then sat back. "I didn't remember that. This may be a big misunderstanding about — that email is too polite."

Cuddy and Carney had taken their advice literally. Simmons stood by his analysis but recognized that there was confusion at play in how they interpreted the events that transpired. Simmons says he harbored no ill will toward Cuddy before criticizing her paper; if anything, he remembered her warmly. "She was great," he said, smiling at the memory. "We published the blog post despite my history with Amy. Because

I realized that once we pulled the trigger on this. . ." He did not finish the sentence. Cuddy had, in fact, become the poster girl for this kind of work, which even he thought was not fair. "The original study wasn't particularly egregious," he said. "It was published in 2010 before anyone was thinking about this."

For a moment, the scientist allowed the human element to factor into how he felt about his email response to that paper. "I wish," he said, "I'd had the presence of mind to pick up the phone and call Amy."

**The public nature** of the attacks against Cuddy have reverberated among social psychologists, raising questions about the effects of harsh discourse on the field and particularly on women. Earlier this year at the conference of the Society of Personality and Social Psychology, there was a presentation of a 2016 survey of 700 social psychologists, assessing their perceptions of the influence of social media on their careers. The subsequent conversation on popular Facebook groups was so combative that Alison Ledgerwood, a social psychologist at the University of California, Davis, felt the need to respond in a blog post. In it, she argued that if scientists keep having hostile conversations on social media, women are more likely to be driven away from the field. (Women in the profession, the survey presented at the conference reported, participated less than their male colleagues in social-media discussions.)

Even people who believe that the methodological reforms are essential say its costs to science are real. "It's become like politics — we've created two camps of people who shouldn't be in two camps in the first place," says Jay Van Bavel, the social psychologist at N.Y.U. "It's perceived slights and defensiveness, and everybody has some history or grievance — and it will never end because there is that history of perceived grievances, of one of your colleagues who has been put through it, or criticized your friend in a public forum. It's terrible for science. It's not good."

If Amy Cuddy is a victim, she may not seem an obvious one: She has real power, a best-selling book, a thriving speaking career. She did not own up fully to problems in her research or try to replicate her own study. (She says there were real hurdles to doing so, not least of which was finding a collaborator to take that on.) But many of her peers told me that she did not deserve the level of widespread and sometimes vicious criticism she has endured. "Amy has been the target of mockery and meanness on Facebook, on Twitter, in blog posts — I feel like, Wow, I have never seen that in science," Van Bavel says. "I've only been in it for 15 years, but I've never seen public humiliation like that."

As a result, the breadth of the accusations — how diffuse they are — could easily be mistaken for the depth of her scientific missteps, which

at the outset were no different from those of so many of her peers. "We were all being trained to simplify, to get our message out there — there were conferences and panels on how to do it," says Richard Petty, a social psychologist at Ohio State. "One of the ironies is that Amy just did it more successfully."

I was surprised to find that some of the leaders in the replication movement were not Cuddy's harshest critics but spoke of her right to defend her work in more measured tones. "Why does everyone care so much about what Amy says?" Brian Nosek says. "Science isn't about consensus." Cuddy was entitled to her position; the evidence in favor or against power posing would speak for itself. Leif Nelson, one of the three pioneers of the movement, says Cuddy is no different from most other scientists in her loyalty to her data. "Authors love their findings," he says. "And you can defend almost anything — that's the norm of science, not just in psychology." He still considers Cuddy a "very serious psychologist"; he also believes the 2010 paper "is a bunch of nonsense." But he says, "It does not strike me as at all notable that Amy would defend her work. Most people do."

Every researcher has a threshold at which he or she is convinced of the evidence; in social psychology, especially, there is no such thing as absolute proof, only measures of probability. In recent months, Cuddy reached the threshold needed to alter her thinking on the effect of hormones. She mentioned, at a psychology conference where she was presenting her work, that a study had recently been conducted on power posing. "They found no hormonal effects," she said before taking a breath. "That study is done very well, and I trust those results." Although 11 new papers have recently been published that do not show the downstream effects of power posing on behaviors, Cuddy is still fighting for power posing. The research, she says, still shows its effect on feelings of power: At the conference, she presented a comprehensive meta-analysis, a version of which, she says, she will soon publish, with a strong P-curve supporting that, and she also presented a P-curve suggesting that power posing had a robust effect on self-evaluations, emotions and moods.

Cuddy now seems ready to move on to a new phase. We met near her home in Newton, Mass., in August. Cuddy, smiling, fresh from physical therapy for a torn ACL, was in a tennis skirt, looking young and more lighthearted than I had ever seen her. She had abandoned the dream of tenure. She was planning a new project, a new book, she told me. It was coming together in her mind: "Bullies, Bystanders and Bravehearts." It would be personal; there would be research; she would write, and she would talk, and she would interview people who had suffered fates worse than her own and bounced back. She would tell their stories and hers, and because she is a good talker, people would listen. ♦



Photographed by Kathy Ryan at The New York Times  
on Oct. 11, 2017, at 9:31 a.m.

**David Rees***Letter of Recommendation,  
Page 16*

David Rees is a writer and former cartoonist. His 2012 book "How to Sharpen Pencils" led to a nationwide pencil-sharpening tour, as well as a brief stint demonstrating pencil-sharpening techniques for American Air Force personnel stationed in Africa. He grew up listening to punk rock rather than heavy metal, but middle age has changed his musical tastes. "Now that I'm in my mid-40s, the weight and tempo of doom, sludge, stoner and other slow metal subgenres make more sense," he says. "I grew up in North Carolina, and I miss those really humid afternoons when everything is forced to a crawl. Now I get that same slow-metabolism kick from bands like Sleep and Pallbearer."

**Susan Dominus***"When the Revolution Came for Amy Cuddy,"  
Page 28*

Susan Dominus is a staff writer for the magazine. Her last feature was about open marriage.

**Michael Erard***"The Content of Their Characters,"  
Page 34*

Michael Erard is writer in residence at the Max Planck Institute for Psycholinguistics and the author of "Babel No More: The Search for the World's Most Extraordinary Language Learners."

**Dan Kois***"Superweirdo,"  
Page 38*

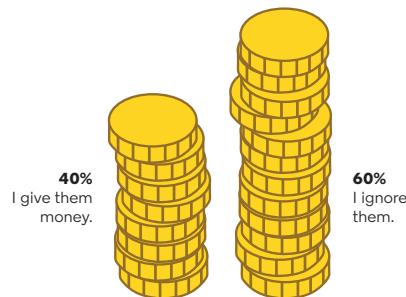
Dan Kois is a frequent contributor to the magazine and an editor at Slate. His book "The World Only Spins Forward: The Ascent of 'Angels in America,'" written with Isaac Butler, comes out in February.

**Jason Zengerle***"State of Chaos,"  
Page 42*

Jason Zengerle is a contributing writer for the magazine and the political correspondent for GQ. His last feature was about North Carolina politics.

## Dear Reader: How Do You Treat The Dispossessed?

Every week the magazine publishes the results of a study conducted online in June by The New York Times's research-and-analytics department, reflecting the opinions of 2,903 subscribers who chose to participate. This week's question: *Do you give money to the homeless, or do you ignore them?*



Editor in Chief

Deputy Editors

**JAKE SILVERSTEIN****JESSICA LUSTIG,****BILL WASIK****ERIKA SOMMER****GAIL BICHLER****KATHY RYAN****MATT WILLEY****ILENA SILVERMAN****CHARLES HOMANS****CAITLIN ROPER****NITSUH ABEBE,****MICHAEL BENOIST,****SHEILA GLASER,****CLAIRE GUTIERREZ,****LUKE MITCHELL,****DEAN ROBINSON,****WILLY STALEY,****SASHA WEISS****JEANNIE CHOI,****JAZMINE HUGHES****MARK LEIBOVICH****SAM ANDERSON,****EMILY BAZELON,****TAFFY BRODESSER-AKNER,****SUSAN DOMINUS,****MAUREEN DOWD,****NIKOLE HANNAH-JONES,****JONATHAN MAHLER,****WESLEY MORRIS,****JENNA WORTHAM****C. J. CHIVERS,****PAMELA COLLOFF,****NICHOLAS CONFESSORE,****JIM RUTENBERG**

Writers at Large

**JOHN HERRMAN****BEN GRANDGENETT****RODRIGO DE BENITO SANZ****DEB BISHOP****JESSICA DIMSON****STACEY BAKER,****AMY KELLNER,****CHRISTINE WALSH****JENNA PIROG****ROB HOERBURGER****HARVEY DICKSON,****DANIEL FROMSON,****MARGARET PREBULA,****ANDREW WILLETT****NANDI RODRIGO****ROBERT LIGUORI,****RENÉ MICHAEL,****LIA MILLER,****STEVEN STERN,****MARK VAN DE WALLE****ANICK PLEVEN****PATTY RUSH,****HILARY SHANAHAN****LIZ GERECITANO BRINN**

**Publisher:** ANDY WRIGHT **Advertising Directors:** MARIA ELIASON (Luxury and Retail) • MICHAEL GILBRIDE (Fashion, Luxury, Beauty and Home) • SHARI KAPLAN (Live Entertainment and Books) • NANCY KARPF (Fine Arts and Education) • MAGGIE KISELICK (Automotive, Technology and Telecom) • SCOTT M. KUNZ (International Fashion) • JOHN RIGGIO (Recruitment) • JOSH SCHANEN (Media, Studios and Travel) • ROBERT SCUDDER (Advocacy) • SARAH THORPE (Corporate, Health Care, Liquor and Packaged Goods) • BRENDAN WALSH (Finance and Real Estate) **National Sales Office Advertising Directors:** KYLE AMICK (Atlanta/Southeast) • LAUREN FUNKE (Florida/Southeast) • MAGGIE KISELICK (Detroit) • CHRISTOPHER REAM (Los Angeles/San Francisco/Northwest) • JIMMY SAUNDERS (Chicago/Midwest) • ROBERT SCUDDER (Boston/Northeast/Washington) • KAREN FARINA (Magazine Director) • MARILYN McCUALEY (Managing Director, Specialty Printing) • THOMAS GILLESPIE (Manager, Magazine Layout). To advertise, email karen.farina@nytimes.com.

Reproduced with permission of copyright owner. Further reproduction prohibited without permission.