

Did psychologists believe themselves in the science that they professed?

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Abstract : Bertrand Russel asked in a newspaper column (1932) of astrologers: “Do they believe themselves in the sciences that they profess?” This paper asks whether academic psychologists believed the science that they professed. The replication crisis—researchers do not obtain results comparable to the original when repeating that study—has, in the past decade, seriously challenged psychology's status as a science. Most of the social and behavioral sciences have been invalidated by the replication crisis, but I focus on psychology because of my personal involvement in this field. I draw on the work of Žižek and Zupančič on denial and disavowal to approach the question of what we believed. I will argue, contra Zupančič, that disavowal is not necessarily a phenomenon of mass individualism. As doctoral students, we learned to transgress scientific norms, and then to act as if this transgression had never happened. To enter the community of scientists, we had to disavow that psychology is not a science. After running enough studies—falling on our knees and praying—some of us came to believe that psychology is a science, whilst others held on to a more cynical position. Subjectively some of us believed, some did not, objectively we all believed. I also pursue the related questions of why we did what we did—why was it, for psychology as a field, necessary to break scientific norms and what enjoyment did we, as subjects, derive from what we were doing?

Keywords: Belief; Disavowal; Denial; Enjoyment; Žižek; Zupančič

Alenka Zupančič (2024, p. 1) starts her new book *Disavowal* by citing a joke also cited by Freud: 'A man says to his wife: 'If one of us dies, I shall move to Paris.' She uses this joke to illustrate the template of disavowal, most concisely formulated by Octave Mannoni (2003): 'I know well, but all the same....'. Despite rationally acknowledging that we will die, we behave as if it were otherwise. This is not the same as denying the fact of death. In disavowal one knows all about the facts, and then goes on as before. Zupančič goes on to argue that disavowal has become a predominant feature of our social and political life. Denial, often associated with conspiracy theories, and disavowal, often associated with the business-as-usual mainstream, are, she contends, the two predominant modes of dealing with the recent onslaught of serial crises (e.g., climate crisis, Covid pandemic, Ukraine war, Middle East crisis...).

The present paper focuses on a crisis of smaller scale—the replication crisis in psychological science. Or more specifically, the crisis of disillusionment that the doctoral student—the psychological scientist in training—faces when learning that to become a psychological scientist, one has to break with the scientific norms one has been taught and which one has taken for granted. The student realizes that psychology is not a science, and is required to disavow that knowledge in order to continue in academia. Alternatively, the paper could have been framed around the replication crisis in social sciences, behavioral sciences, or behavioral economics. Or more generally around the replication crisis in sciences that employ statistics to test hypotheses, including fields as diverse as medicine, ecology, and cancer biology. However, I focus on psychology because of my personal involvement with the field—I have been part of the community of psychological scientists for two decades. I sometimes use the colloquial “we” to refer to our community, because we, as I will argue, truly were a community, and to emphasize that I am not blaming the particular individuals whom I take as examples, I am also implicated.

Bertrand Russel asked in a newspaper column (28 September 1932) of astrologers: “Do they believe themselves in the sciences that they profess?” He acknowledged that this is a very hard question. Having in the last two decades published around a hundred papers, many of them in top-tier personality and social psychology journals, I am, to paraphrase Russel, haunted by the question of what we believed—did we ourselves believe in the science we professed? I draw on the work of Žižek and Zupančič on denial and disavowal to approach the question of what we believed. I will argue, contra Zupančič, that disavowal is not necessarily a phenomenon of mass individualism. Doctoral students, upon entering the

field, quickly came to learn that they were required to break with scientific norms and codes of conduct; essentially unlearn what they had learned through their education up that point—methodological norms were to be broken and statistical analyses were to be abused. I.e., prospective psychological scientists learned that psychology is not a science. To enter the community of scientists, one had to disavow this knowledge, and behave as if psychology were a science. The alternative was the abandonment of an academic career. Throughout the paper, I will also pursue the question of why we did what we did—what enjoyment did we derive from whatever it was that we were doing? I will also introduce the reform movement, born in the aftermath of the replication crisis. The reform movement believes that psychology can become a real science by paying rigorous attention to scientific norms.

Replication crisis in psychology

What is commonly known as the 'replication crisis in psychology' (other alternatives are 'replicability crisis' and 'reproducibility crisis') refers to the now widespread recognition that the results of many scientific studies within psychology cannot be reproduced. Important research once thought unassailable has failed to replicate. Even the most well-known and respected findings have not been immune. Scientific progress and the accumulation of knowledge depend on reliable past findings that can generate new ideas. Pervasive problems with replicability hinder scientist from building on previous work to generate cumulative knowledge. This very basic philosophy of science carries the implication that psychology may not be a science.

Two articles are often mentioned as triggers in narrations of the crisis. One was authored by one of the most prominent social psychologists of the past five decades, Daryl Bem (2011). In his 2011 paper 'Feeling the future: experimental evidence for anomalous retroactive influences on cognition and affect,' he used state-of-the-art experimental-statistical methods to show that humans are able to perceive the future. This paper, which in essence demonstrated the existence of a paranormal or psi phenomena, will be discussed in more detail below. Another article—'False-positive psychology: undisclosed flexibility in data collection and analysis allows presenting anything as significant'—published in 2011 by Simmons, Nelson and Simonsohn, who all belonged to a new generation, demonstrated the simplicity with which researchers could gather statistically significant evidence for a false hypothesis. Employing a series of experiments and simulations they showed that "undisclosed flexibility in data collection and analysis allows presenting anything as significant." Specifically, they collected real data on the effects that certain songs have on

listeners' age and employing the standard practices that, although commonly employed conflicted with scientific norms and everything we were taught in methods-statistics textbooks (more on these below), showed that listening to the Beatles both subjectively and factually made people younger. Both papers attracted much attention and gave rise to more detailed investigations into the extent of the crisis. However, it is important to note that we have been aware of the problems of statistical testing not since 2011, but since the 1960s (e.g., Bakan 1966; Meehl 1967). Reviewing four decades of severe criticism of the statistical tests that we employ, Cohen titled his 1994 paper in the journal *American Psychologist* "The Earth Is Round ($p < .05$)". We knew all along what we were doing, what changed in the early 2010s was that the challenges gained more traction.

The 2011 articles were followed by several large-sample multi-study projects that attempted to replicate many major psychological findings. For instance, in an attempt to replicate 100 experimental and correlational studies published in three of the very best psychology journals, only 36% of the results proved replicable (Open Science Collaboration 2015). Other large-sample multi-study collaborations have produced similar results (e.g., the "Many Labs" projects 1 to 5). There are many reasons for why a result will not replicate. From the present perspective, the most interesting reason is the flexibility that we enjoyed in our analytic decisions and reporting, as demonstrated in the above mentioned paper on false positives (Simmons, et al. 2011). Journals tended, and still tend, to publish only paper that report statistically significant results. Few are interested in studies in which nothing has happened. Statistical significance meant, and often still means, that we needed to publish results with a probability smaller than 5% of being caused by chance. When the results of our initial analyses were statistically non-significant, we turned to Questionable Research Practices (QRPs).

A 2012 anonymous survey of over 2,000 psychologists about QRPs showed that many of these practices constitute the prevailing research norm (John et al., 2012). Based on self-admission rates and prevalence estimates that the participants provided, the authors concluded that the majority of respondents had engaged in several QRPs. Among the most common was selective omission of observations, experimental conditions, and studies—in other words, we deleted from analysis those participants, subsamples, samples, experimental conditions, experiments, and studies that ruined the statistical significance of the results we wanted to present. The majority of respondents directly admitted to these particular QRPs, and respondents' estimates of their prevalence among their colleagues—likely to be a much more honest estimate than self-admission rates—were even higher.

There has been a plethora of research investigating the structural underpinnings of the replication crisis. Academic precarity, the ‘publish or perish’ culture, journals’ unwillingness to publish null results, the media attention given to sexy and surprising results have all been mentioned as explanations. However, there has been very little attention to what was happening at the level of the individual researcher.

Method-statistics courses are a major part of virtually all psychology programs. Everyone doing a major in psychology will need to take some classes specifically on statistics. Many other classes, particularly those that involve experimental design or research methods, will inevitably involve advanced statistics. Without a foundation of statistical knowledge, it is not possible to make sense of the research papers upon which all domains of psychology depend. PhD students are, of course, required to learn far more statistics. They need to collect data, analyze data, turn data into statistics, and present results in their dissertation. The knowledge of methods-statistics that all of us had, and that we both admitted to engagement with QRPs and observed others in the field do the same. begs the question of what we believed we were doing and the related question of why we were doing it. What motivated us in our day-to-day research? Where we in it for the money or other career concerns? Or for the status and attention? Where was the enjoyment?

I will approach the above questions by applying the framework of disavowal and denial to analyze two case examples: the above-mentioned Daryl Bem and Brian Wansink. I in no way want to discredit these two professors, suggest that they were “bad apples,” or that any one person could or should be blamed. The only reason to focus on Bem and Wansink is that their immense success and status made their cases a matter of public record. They also illustrate two different belief structures, disavowal and denial, respectively.

Daryl Bem displaced belief on the wheel of statistics

Professor Daryl Bem—one of the best known, most merited and respected social psychologists, whose career spanned more than five decades—published in 2011 the above mentioned paper on feeling the future in our field’s flagship journal, *Journal of Personality and Social Psychology*. In that paper he presented results from nine experiments, involving more than 1,000 participants. Bot the number of experiments and the number of participants were extremely impressive—these participants did not respond to some easy to complete online questionnaire, but came in person to Bem’s research center in order to take part in on-site experiments. The results of this series of experiments consistently demonstrated the

existence of paranormal psi phenomena—more specifically that precognition, the ability to see future events, is actually real (Bem, 2011). This proved an explosive notion that attracted the both the mainstream medias and critics' attention.

The nine experiment that Bem (2011) ran, despite screaming “p-hacked” all over the methods and results sections (engaging with QRPs to arrive at p-values smaller than the employed threshold for statistical significance, is commonly referred to as p-hacking), more than fulfilled the standard criteria for publication. This highly labor-intense undertaking was most likely not a Sokel-inspired hoax that Bem ran to demonstrate problems with our current system of statistical/scientific research and publishing. Rather, Bem has consistently published research supporting Psi (e.g., Bem & Honoron, 1994; Bem et al., 2016; see also Real, 2016). In a positive review of a handbook on paranormal phenomena, Bem (2016) explained that the “conceptual model of physical reality” that psychologists “still take for granted” is outdated among physicists, and that quantum theory can be reconciled with “precognition and retrocausation”.

In an interview for Slate, several years later, when confronted about p-hacking, Bem was somewhat dismissive of the issue: “I’m all for rigor,” he noted, “but I prefer other people do it. I see its importance—it’s fun for some people—but I don’t have the patience for it.” It’s been hard for him, he said, to move into a field where the data count for so much. “If you looked at all my past experiments, they were always rhetorical devices. I gathered data to show how my point would be made. I used data as a point of persuasion, and I never really worried about, ‘Will this replicate or will this not?’” (Engber, 2017).

Bem’s intellectual disregard and affective contempt towards empirical investigation as merely “a point of persuasion” (Engber, 2017) may appear striking. However, despite this contempt, Bem performed the experiments and ran the analyses. This distinguished him from a figure like Diedrik Stapel—an academic rockstar and author of several well-regarded studies on human attitudes and behavior—who, shortly after being caught, admitted to outright inventing results and fabricating and faking data to more than fifty highly published studies (Bhattacharjee, 2013). Why did Bem not do the same? In Bem, the split between knowledge and belief, characteristic of disavowal, is at work. The classic formulation of Mannoni (2003): “I know well, but all the same...” can, for Bem, be written as “I know well that this is not science, but all the same I will act as if it were”.

Bem’s experiments and analyses play for him the role the Tibetan prayer wheel. One of Žižek’s most famous examples of displaced belief is that of the Tibetan prayer wheel: “you write a prayer on a paper, put the rolled paper into a wheel, and turn it automatically, without

thinking (or, if you want to proceed according to the Hegelian 'cunning of reason', you attach it to a windmill, so that it is moved around by the wind). In this way, the wheel itself is praying for me, instead of me — or, more precisely, I myself am praying through the medium of the wheel. The beauty of it all is that in my psychological inferiority I can think about whatever I want, I can yield to the most dirty and obscene fantasies, and it does not matter because — to use a good old Stalinist expression — whatever I am thinking, objectively I am praying" (Žižek, 1989, 31–2). The experiment and the analyses allowed Bem, in his psychological inferiority, to think obscene thoughts about quantum physics and paranormal phenomena. However, objectively he was doing top-notch psychological science. As an aside, the counterpart of the wind that moves the prayer wheel could be a piece of code that runs all possible combinations of methodological choices and statistical analyses—that is, engages which QRPs for us—and highlights the permutations that give statistically significant results, which we can then choose from, depending on for instance news value (this selection of the most surprising and counter-intuitive results could most likely also be done by a piece of code).

Ironically, Bem, who believed in Psi, is similar to the figure of the "primitive" to whom Žižek claims that certain "superstitious beliefs" are attributed. The anthropological evidence, suggest, according to Žižek (1998), that "the "primitives" to whom certain "superstitious beliefs" were attributed, when directly asked about them, answered that "some people believe", immediately displacing their belief, transferring it onto another. Bem, similarly, went through the rituals of empirical investigation because some other people (the community of psychologists that he needed to persuade) believed in them. And Žižek continues: "And again, are we not doing the same with our children: we go through the ritual of Santa Claus, since our children (are supposed to) believe in it and we do not want to disappoint them." Bem went through the rituals of empirical investigation in order not to disappoint his colleagues, those who still clung on to an outdated worldview.

With regards to the questions that motivated this paper, it may seem that Bem did not believe in the science that he professed. He knew psychology was not a science, whilst belief was displaced on empirical investigation and statistical analysis, a psychological scientist's version of the 'Tibetan prayer wheel' or the 'someone else.' However, Žižek's (1989) analysis of the contemporary subject of ideology suggests another interpretation. Žižek takes the Marxist notion of ideology as starting point: "the most elementary definition of ideology is probably the well-known phrase from Marx's Capital: 'Sie wissen das nicht, aber sie tun es' — 'they do not know it, but they are doing it.'" (p. 24). This Marxist concept of

ideology rests on people not knowing what they are doing; that is, false consciousness. However, Žižek considers the Marxist notion outdated—in contemporary societies, ideology’s dominant mode of functioning is cynical. Cynical subjects “know very well..., but still they are doing” (p. 26). Bem was cynical towards psychological science, but this alleged cynicism functioned only to conceal that he was still, in his behavior, committed to psychological science. That he believed himself not to be duped by psychological science, to be in the know, gave it an even greater hold on him. Given that it was all make-believe anyway, Bem had no scruples about transgressing methodological-statistical norms and could continue to contribute to the flourishing of the science. Knowing that it was all pretend may have been essential not only for academic success, but also for academic survival, as illustrated by the self-destruction of Brian Wansink.

Brian Wansink kneels to psychological science

“The external must be joined to the internal to obtain anything from God, that is to say, we must kneel, pray with the lips, etc., in order that proud man, who would not submit himself to God, may be now subject to the creature. To expect help from these externals is superstition; to refuse to join them to the internal is pride.” Pascal (*Pensées*: 250)

Žižek (1989) interprets Pascal’s advice—that doubting subjects should get down on their knees and pray, and then they will believe—as suggesting that once subjects have come to believe through praying, they will also retrospectively see that they got down on their knees because they always believed, without knowing it. In psychological science, by conducting experiments and running analysis the subject could come to believe that they are doing science, despite consistent engagement with QRPs. Moreover, if they have always believed, perhaps others also believe. The initial realization that all of us had when entering the field—this is not science—becomes denied. This process and its potentially dire consequences illustrated by Professor Brian Wansink from the University of Cornell.

Wansink was an academic superstar in the 2010s. His research topic—he was an expert on the psychology of eating—and his exciting research findings—for instance, children eat more vegetables when they have colorful names like “power peas”, large plates lead people to eat more food—had attracted huge media coverage. For instance, he had appeared on *The Oprah Winfrey Show* and *The Dr. Oz Show*, as well as having written several best-sellers.

Moreover, his results had impacted US government policies, such as the redesign of school cafeterias, and US army programs encouraging healthy eating (Bright, 2021). Wansink presented psychology as a rigorous science that provided the powers-that-be access to small experimental manipulations or nudges; for instance, how the food is positioned in the service line, or providing labels with nutritional information. These nudges were supposed to empower the masses of their own free will to make better and healthier decisions with regards to their eating—no need for government regulation.

Wansink had it all. This having it all included disciplines.... a public blog:

“When she arrived, I gave her a data set of a self-funded, failed study which had null results . I said, “This cost us a lot of time and our own money to collect. There's got to be something here we can salvage because it's a cool (rich & unique) data set.” I had three ideas for potential Plan B, C, & D directions (since Plan A had failed)...

Every day she came back with puzzling new results, and every day we would scratch our heads, ask “Why,” and come up with another way to reanalyze the data with yet another set of plausible hypotheses. Eventually we started discovering solutions.”

Wansink (2016) is in the above blog post describing what initially appeared to be a "failed study" with "null results", until a grad student who he spurred on kept going back over the data until she began finding "solutions that held up". Wansink was advising young researchers on how to do research—i.e., recommending they keep analyzing the data until they found something that seemed feasible. In further e-mail correspondence with the grad student Wansink suggests that she break up the diners into all kinds of groups: “males, females, lunch goers, dinner goers, people sitting alone, people eating with groups of 2” and so on (Lee, 2018).

Wansink (2016) went on in the blog post to contrast the “doggish” Turkish woman with one of his paid post-docs:

“At about this same time, I had a second data set that I thought was really cool that I had offered up to one of my paid post-docs (again, the woman from Turkey was an unpaid visitor). In the same way this same post-doc had originally declined to

analyze the buffet data because they weren't sure where it would be published, they also declined this second data set. They said it would have been a "side project" for them they didn't have the personal time to do it. Boundaries. I get it.

Six months after arriving, the Turkish woman had one paper accepted, two papers with revision requests, and two others that were submitted (and were eventually accepted -- see below). In comparison, the post-doc left after a year (and also left academia) with 1/4 as much published (per month) as the Turkish woman. I think the person was also resentful of the Turkish woman."

The blog post attracted the attention of fellow researcher, of whom some appeared concerned. One comment suggested that the blog "perfectly sums up the perverse incentives that create bad science" and that "what you describe Brian does sound like p-hacking and HARKing" (McCook 2017). HARK:ing is defined as presenting a post hoc hypothesis (i.e., one based on or informed by one's results) in one's research report as if it were, in fact, an a priori hypotheses. P-hacking and HARK:ing are standard QRPs (John et al. 2012). The critical comments were warranted; Wansink's blog post is a prototypical example of misuse of data analysis to find patterns in data that can be presented as statistically significant—by rearranging, testing, breaking down and putting together over and over, one can always find something that comes out looking statistically significant (often referred to as 'torturing the data').

The blog post turned out to be a remarkable feat of self-destruction. A committee investigative faculty committee was quickly assembled, and they found that Wansink had committed a litany of academic breaches, such as misreporting of research data, problematic statistical techniques, failure to properly document and preserve research results, and he resigned shortly afterwards. The downfall and resignation of professor Wansink was covered in all major news outlets (e.g., NYT, WSJ, CNN).

Like Bem, a figure of disavowal, Wansink ran the experiments, analyzed the data, and published well-structured narratives around the results. However, Wansink, in contrast to Bem appears not to be cynical. Through running enough experiments and analyses, the realization that all of us, including Bem, must have had—this is not science—is denied. Returning to the initial question—did we believe that science that we professed—one could argue that in contrast to Bem, who believed objectively, or in his behavior, Wansink believed

both objectively and subjectively. Besides these two figures, there were those who did not believe objectively—a good example is the above mentioned Diederik Stapel, who did not go through the motions of experimenting, but instead invented results and fabricated data. The unsatisfying but in retrospect not very surprising response to the question of belief would thus be that it depends not only on whether one refers to objective or subjective belief, but also that it varied from subject to subject.

The question of belief can perhaps only be resolved by accepting a split between knowledge and belief, a split that Russell and psychological science would perhaps not accept. However, the question of why still insists. Why didn't we all, like the post-doc whom Wansink criticized, all have quit upon learning what it takes to be a psychological scientist? Noble ideas about pursuing truth or knowledge, sometimes said to motivate researchers, evaporate when one is, and one always was, instructed by those in the know to start torturing the data. What enjoyment did pursuing a career built on disavowal offer?

Enjoying the dark side of psychological science

Similarly to any science, there is an explicit set of norms for how to do psychological science. These norms are written down in methodological handbooks, they are taught in all methods and statistics classes, and are upheld by institutions such as journals. Overtly recognized instructions, methods, and practices serve to explicitly regulate how we should conduct our research. These norms allow us to claim a basis for our knowledge. Good scientific practices guarantee the accumulation of knowledge and prohibit us from doing and claiming whatever we want. However, for the abstract methodological rules and norms to pass into real research practices, their contextualization or adaption to real world circumstances is necessary. This adaption is illustrated above by Wansink's blog post and email exchanges. The abstract experimental-statistical norms do not really matter. What matters is the way in which they are adapted in everyday research. And this adaption into everyday research practices involved much transgression of scientific norms.

Žižek argues that all communities are built around enjoyment that is organized around cultural practices that involve transgression of the publicly sanctioned laws and norms. These transgressive practices allow subjects some experience of what is usually prohibited. Žižek's famous example is that of the military community: "at one level, you have a set of explicit rules (hierarchy, procedure, discipline, etc.), but in order for these explicit rules to function they need an obscene supplement: that is, all the obscene unwritten rules that

sustain a military community — dirty sexist jokes, sadistic rituals, rites of passage and so on. Anyone who has served in the military knows how the whole military discipline is sustained ultimately by this obscene underbelly” (Žižek & Daly 2013: p. 128). The transgressions in Žižek’s examples often revolve around sex, death, drugs, defecation, festivals, or violence. Such experiences involve what Žižek calls *jouissance*. This is a term from Lacanian psychoanalysis that is usually translated from the French as “enjoyment.” *Jouissance* is always transgressive, always at the limits of what one can admit publicly. Wansink crossed this limit by publicly reveling in his transgressions, his source of *jouissance*.

To become a member of a social group, it is not enough to follow the explicit rules. In fact, Wansink talks disparagingly of the post-doc who did cling on to good scientific practices—refused to engage with QRPs—and could thus neither produce results nor stay in the field. In order to become a psychological scientist, become a member of the academic community, one had to transgress the publicly sanctioned standards and practices of good science. To quote Žižek, “the subject who closely follows the explicit rules of a community will never be accepted by its members as “one of us”: he or she does not participate in the transgressive rituals which actually keep this community together” (Žižek 1989: p. lxi).

The transgression of scientific norms—engagement with QRPs—has recently been challenged by the reform movement. This movement aspires to make psychology a real science—new infrastructures need to be put in play that change the way we do science. Processes such as preregistration and registered reports—paper submission formats which entail the registration and disclosure of study plans and details before data collection—are intended to shift the standards of how we do science. These more advanced approaches are hoped to eliminate QRPs and make psychological science credible and trustworthy. The reforms have been interpreted through the lens of bureaucratization—a continuously increasing routinization of tasks, involving division of labor, hierarchy, standardization, and new bureaucratic tools that allows us to show how strictly we now adhere to methodological principles, how committed we are to “safeguard the process of science, to pursue or protect objectivity” (Penders 2022: 110). A new community has been forged around promoting the use of bureaucratic instruments that elevate the integrity and credibility of the scientific process, distinguishing reformed science from the old, messy science that was unable or unwilling to follow its own rules, a weakness now portrayed as shameful and disgusting (Penders 2022). The old community, formed around cynical disavowal (“I know psychology is not a science, but...”) and transgressing scientific norms has been argued not only to be wrong, but a manner of manners and morals—either one accepts the now emerging new

scientific etiquette, or one is an embarrassing, shameful, even disgusting failure (Penders 2022). The increasing routinization of tasks has been argued to undermine science as a vocation—bureaucratization as a process of modernization will bring with it not only efficiency and optimization, but also alienation and disenchantment (Lee & Walsh 2021). The core of modern science can be understood as the removing of the body and the self from the scientific proves, “an ethos of self-annihilation as the price of knowledge” (Daston 2002: 16). The central theme in these accounts of alienation and self-annihilation is that science, due to increased bureaucratization will no longer be enjoyable—no more transgression, no more enjoyment.

From a Lacanian perspective, however, this modernization of science will not bring with it an end to enjoyment: “For Lacan, the trouble with *jouissance* is not only that it is unattainable, always-already lost, that it forever eludes our grasp, but, even more, that *one can never get rid of it*, that its stain drags on for ever – that is the point of Lacan’s concept of surplus-enjoyment: the very renunciation of *jouissance* brings about a remainder/surplus of *jouissance*.” (Žižek, 1999: 351). Excessive obedience to the letter of the law, the morally superior view from nowhere voice that the objective scientist can embody after disposing of the self, may replace transgression of the law as the point at which the psychology scientist enjoys and at which a new community is formed.

One final question regarding our motivations—why did we not follow scientific norms from the get-go? Similarly to how we in capitalism as consumers “derive satisfaction from the failure to obtain a perfect commodity” (McGowan 2016: 14) we could, by doing science properly, derive satisfaction from the failure to obtain the perfect result. Research findings and the theories that build on those findings are almost by definition false. New evidence challenges old results and theories, and paradigms shift. Why did psychological science not allow its practitioners this mode of satisfaction, the satisfaction of not arriving at the final truth, but always finding new challenges and new questions? That the reform movement has encountered much resistance could provide some clues on this. Several of the most experienced and prominent scientists in our field have mainly ignored evidence that QRPs were used to produce significant results and dismissed the critics as unscientific (for a review of common responses and arguments that claim that there is no crisis, see Schimmack 2022). Besides the more obvious reasons, such as the challenge to old power and authority that reform constitutes, one reason the reform movement has met with much resistance is that it may threaten to reveal something even more horrifying than that we employed QRPs to produce results. Perhaps we will have no “results” at all, we will have

nothing, or at least nothing interesting to show, if we employ methods and statistics correctly. This is something that some critics of the reform movement may be aware of (recall that we have known about these problems since the 1960s, so there would have been time to change).

Engagement with QRPs may have been necessary for psychological science, or at least many subfields of the discipline, if we were to claim any knowledge that was beyond what common sense would suggest. Not only experts but also laypeople are remarkably good at guessing which of our research results are replicable; i.e., the surprising and counter-intuitive results (Hoogeeven et al. 2020). Which implies that when we attempt to move “beyond” what common sense would suggest, we fail. An intriguing question for future research is why laypeople, especially if they more or less know which results are fake and which are not, are nevertheless intrigued by psychological science, especially by the fake results—the counterintuitive, surprising results that did not replicate were the ones that drew the media attention (YouYou et al. 2023). Perhaps laypeople enjoyed the stupidity of “experts”—hate-reading “experts” results may have validated laypeople’s reliance on common sense. On the other hand, laypeople are often also intrigued by horoscopes without hating them. Perhaps disavowal is at work also among laypeople who read popularized psychological science, not only media articles popularizing the newest results, but also self-help books.

Another reason for resistance towards reform may be the focus on effect sizes. Shifting focus on effect sizes rather than hypothesis testing—a shift required by the reform movement—may bring attention to the fact that our effect sizes in psychology are overall so small that they mean nothing, especially not for individual people (Möttus 2022). Interest in psychology would be much dampened if this knowledge spread.

Conclusions

Russell acknowledged that it is hard to tell whether astrologers believed or not. The same is true of psychological scientists. The reason that the question may be perplexing is that psychology, and many other sciences, lack the notion of a split subject. This makes both believing and not believing difficult to understand. The concept of the Freudian unconscious allows one to both believe and not believe. However, the Freudian unconscious is banned from psychology. One reason could be that so much of our research builds on the idea that we are transparent to ourselves—the idea that we know who we are, what we do, and why

we do it. This is necessary in order for us to be able to give accurate, often quantitative, accounts of our identity, motives and behavior to the psychology scientist who ask us to report on them (psychology is very much a science of self-reports). The current focus on statistical analysis may mask another crisis—our methods and our research may lack all theoretical grounding and all validity. Statistical methods may be standing in for theoretical rigor—our most important constructs, like “intelligence” or “extraversion” are as real as Santa Claus (Borsboom et al., 2003), and we do not even know where to begin to construct theories (Borsboom et al. 2021).

The theoretical work that I hope this paper and the example of psychological science can do is to challenge Zupančič's (2024) notion that disavowal is “an individual mass phenomenon” (122) and a “pinnacle of individualism” (122). In our field, the successful doctoral student was forced to develop a cynical distance towards psychological science. The most elementary experimental-statistical norms had to be routinely transgressed; one was required to do so by one's supervisors and future colleagues. Failing to do so meant that one did not belong and had to leave academia. At this late stage of study, the student may already be financially committed and perhaps also emotionally attached to becoming a psychological scientist. To continue this path, to join the community, one needed to disavow that one now knew that psychology was not a science. Although one knew this, one needed to behave as if one did not.

Zupančič's (2024: 122) example, in the context of collective-forming, is that of climate change; she argues that the denial of climate change and the conspiracy theories that stem from this denial have a collective-forming capacity. If one denies climate change, one needs a conspiracy to explain who is responsible for creating and spreading false evidence of climate change. By contrast, the disavowal of climate change—the business elite knows all about it but continues with business as usual as if they didn't believe in climate change—does not create community. However, in the case of psychological science, disavowal of knowledge (the knowledge that psychology is not a science) was an initiation ritual for the community of psychological science. If one clung on to the belief that psychology was a science—refused to break empirical-statistical norms, refused to enjoy in the proscribed way—one had no results, did not publish, and could not become a member of the community. Denial could play a role later in one's career, as in the case of Wansink—by running enough experiments and analyses he may have come to believe that psychology was a science, shifting from disavowal to denial.

Other communities within academia (e.g., social sciences, behavioral economics, medicine), perhaps also outside of it (Wall Street brokers playing a game of musical chairs?), are likely to have formed around disavowal. This could characterize not only the old community of psychological scientist (a new, rival community has formed around the reform movement), but also other pseudoscientific communities. Those pseudoscientific communities that have established themselves within academia may differ from other pseudoscientific communities in that they have the institutional power to produce generally accepted knowledge. They need not deny scientific knowledge and provide explanations for why the generally accepted knowledge is false—for instance, by resorting to conspiracies, often involving academia (perhaps we actually were the conspiracy?). Rather, pseudoscientific communities within academia, or at least some of them, like psychology, would know that what they are doing is not science. The successful psychological scientists disavowed this knowledge and built community around the enjoyment that transgressing scientific norms offered.

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