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Rosenhan revisited: successful scientific fraud

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Abstract
The publication of David Rosenhan's 'On being sane in insane places' in *Science* in 1973 played a crucial role in persuading the American Psychiatric Association to revise its diagnostic manual. The third edition of the *Diagnostic and Statistical Manual of Mental Disorders* (*DSM-III*) in its turn launched a revolution in American psychiatry whose reverberations continue to this day. Rosenhan's paper continues to be cited hundreds of times a year, and its alleged findings are seen as crucial evidence of psychiatry's failings. Yet based on the findings of an investigative journalist, Susannah Cahalan, and on records she shared with the author, we now know that this research is a spectacularly successful case of scientific fraud.

Keywords
American psychiatry, David Rosenhan, *DSM-III*, scientific fraud, Susannah Cahalan

On 19 January 1973, *Science* (alongside *Nature*, the most influential general science journal in the world) published an article that instantly captured major media attention. In itself, that is not unusual, for science journalists often use *Science* as a source of copy, but what *was* somewhat unusual was that the author of this paper was a social scientist, not someone from the biological or natural sciences. What is even more unusual is that this particular paper has enjoyed an extraordinarily long half-life. Nearly a half-century after its appearance in print, it continues to attract hundreds of citations a year, and to be a staple of undergraduate textbooks in both psychology and sociology.[1] More remarkably still, one can plausibly argue that its findings had an extraordinary real-world impact, playing a major role in transforming a major sub-specialty in medicine in ways that continue to resonate all the way down to the present. Few academic papers in the social sciences can claim to have had a comparable impact: academically, among the broader public, and on the orientation and practices of a major profession.

The paper was by David Rosenhan, a Stanford Professor of Psychology and Law, who had had a fitful academic career during the 1960s – a string of temporary teaching appointments along the East Coast, before landing a more promising job in 1968 at Swarthmore College (Pennsylvania).

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Two years later, he was invited to visit Stanford for a year, which in turn led to the offer of a permanent post there.[2] At Stanford, he co-authored a textbook on abnormal psychology with Martin Seligman (Rosenhan and Seligman, 1984) – a textbook that went through four editions between 1984 and 2000 – and set himself up as an expert advising on jury selection. His paper in *Science* was his only significant contribution to the social psychological literature, albeit one that made him famous for decades. He never revisited the topic in any academic journal, and published nothing of comparable impact for the rest of his career.

We now know why, thanks to the work of a remarkable investigative journalist, Susannah Cahalan, on whose work (with her permission) I draw extensively later in this paper. David Rosenhan, as she meticulously shows, perpetrated one of the most egregious and successful academic frauds of the twentieth century. In what follows, I shall: spell out the context of Rosenhan's study; document its massive impact on American psychiatry; and (drawing upon the materials Ms Cahalan has generously shared with me) show why we must reject his findings, almost a half-century after they appeared in print.

'On being sane in insane places' (Rosenhan, 1973) purported to report the results of an experiment involving eight subjects, one of whom was Rosenhan himself. (Rosenhan stated that there had been a ninth participant, but he had been dismissed from the study for violating the experimental protocol.) These volunteers, who had been screened to eliminate anyone with mental health issues, were instructed to show up at a variety of mental hospitals claiming that they were hearing voices, and to seek admission. Those were the sole symptoms they were to report, and they were strictly enjoined to behave normally post-admission and to inform their doctors that they were no longer symptomatic. Together, the volunteers had approached a total of 12 mental hospitals (some participants engaged in the charade more than once). The hospitals, Rosenhan reported, were spread across five states, and represented a wide spectrum of facilities, from isolated, run-down rural state hospitals through public facilities that were modern and relatively well-staffed, as well as a single private mental hospital linked to an academic department of psychiatry. Uniformly, however, whatever institution they approached, the subjects of Rosenhan's experiment were admitted as in-patients, and spent anywhere from 7 to 52 days in hospital before they were discharged (an average of 19 days). The private mental hospital diagnosed its lone patient as manic-depressive, a relatively favourable diagnosis. By contrast, all of those admitted to public facilities were given the label of schizophrenia, and Rosenhan reported that, upon discharge, they were noted to be 'schizophrenics in remission'.

On the basis of these results, he claimed that 'we cannot distinguish the sane from the insane in psychiatric hospitals'. As he began to give presentations of his findings prior to publication, staff at a local teaching and research hospital 'doubted that such an error could occur in their hospital'. Rosenhan's response was to inform them that he would send along pseudo-patients over the following three-month period, and see whether they could detect the imposters. It was a trap. 'Forty-one patients were alleged, with high confidence, to be pseudo patients by at least one member of staff. Twenty-three were considered suspect by at least one psychiatrist.' Rosenhan (1973: 252) rather gleefully reported that he had sent not a single pseudo-patient.

A furious correspondence ensued.[3] Psychiatrists from all over the country lined up to criticize the study and to reject its findings. A number of correspondents were incensed at Rosenhan's use of the terms sanity and insanity, objecting that these were legal, not medical terms. (In reality, psychiatrists made use of 'insanity' as a medical term well into the twentieth century, and terminological disputes were in any event irrelevant to the question at hand: the damage Rosenhan's findings had created for the public's view of the profession's (in)competence.) What these complaints also missed was that, by adopting these vernacular terms, Rosenhan (doubtless deliberately) had invited greater lay attention to his findings.

That psychiatric diagnoses were highly unreliable was not exactly news to the profession. During the 1960s, a series of sober academic studies conducted by leading lights in the profession had repeatedly documented the problem. As early as 1959, Benjamin Pasamanick and his associates had drawn attention to the fact that:

> commonly promulgated definitions of mental . . . illness are still so vague that they are frequently meaningless in practice . . . [It is] an even stronger indictment of the present state of psychiatry, that equally competent clinicians as often as not are unable to agree on the specific diagnosis of psychiatric impairment . . . Any number of studies have indicated that psychiatric diagnosis is at present so unreliable as to merit very serious question when classifying, treating and studying patient behavior and outcome. (Pasamanick, Dinitz and Lefton, 1959: 127).

Their own study of the issues demonstrated that the clinical and theoretical commitments of the treating psychiatrist were of greater importance than the symptoms presented by the patients. In particular, 'the greater the commitment to an analytic orientation, the less the inclination toward diagnosing patients as schizophrenic' (p. 131).

Aaron T Beck's subsequent review of systematic studies of reliability, undertaken some three years later, essentially confirmed these findings. Setting aside organic cases, where inter-rater reliability might reach 80 per cent or more, in functional cases of mental disorder, psychiatrists at best agreed with one another just over 50 per cent of the time, and often agreement was far less than this. Diagnosis was, he acknowledged, vital for research, treatment and teaching, and yet the highest agreement on specific diagnoses that he found in these studies was only 42 per cent. Perhaps, he suggested, though this situation was a major problem for research and epidemiological work, it mattered less in clinical settings, since most psychiatrists were 'seldom bound by the actual diagnosis . . . [and may] simply regard the clinical diagnosis as an additional bit of information (unreliable as it may be) which may support the therapeutic decisions made on the basis of other factors' (Beck, 1962: 213; see also: Beck, Ward, Mendelson, Mock and Erbaugh, 1962; Spitzer, Cohen, Fleiss and Endicott, 1967).

Beck was gesturing here to one of the more extraordinary features of American psychiatry in the 1960s (and indeed the decade before). In the immediate aftermath of World War II, American psychiatry was divided into three largely separate worlds: an academic psychiatry whose existence had been extensively nurtured by the Rockefeller Foundation over the preceding decade and a half (Scull, 2018); an isolated and increasingly beleaguered group of state hospital psychiatrists, poorly paid and working in thankless conditions with the most severely disturbed patients; and – the most rapidly expanding segment of the profession – those committed to out-patient practice, most of whom either employed classical psychoanalysis, or a bowdlerized and Americanized version of Freud's approach that they called psychodynamic psychiatry. With few exceptions, most notably the psychiatrists at Washington University, St. Louis, academic departments were dominated by analysts, and those sympathetic to psychoanalysis. Psychoanalysts disdained diagnostic labels, treating them as essentially an irrelevance. Indeed, Karl Menninger, whose best-selling books had made him perhaps the most famous psychodynamic psychiatrist of the post-war era, had argued that diagnostic labels should be abandoned altogether (Menninger, 1963). Not only was practising in this fashion a charade, since the labels had no discrete meaning, but affixing a diagnosis actively harmed patients, reifying and stigmatizing them while providing nothing useful for the clinician. Small wonder, then, as Donald Klein reported, that 'For the psychoanalysts, to be interested in descriptive diagnosis was to be superficial and a little bit stupid' (quoted in Lieberman, 2015: 142).

American psychiatry had produced a volume called a *Diagnostic and Statistical Manual of Mental Disorders* in 1952 (American Psychiatric Association [APA] 1952), and a second edition was published in 1968 (APA 1968). They were slight documents, barely over 100 pages long.

Although the broad distinction the manuals made between neuroses and psychoses was uncontroversial, their contents were otherwise lightly regarded and little consulted. For those who saw the treatment process as involving an inquiry into the precise psychodynamics of the individual case, diagnostic labels were seen as essentially irrelevant, artificial creations that added nothing of substance to the understanding of a patient's problems or to the treatment process. That the primary forms of psychosis, schizophrenia and manic-depressive psychosis, derived from the work of Freud's *bête noire*, Emil Kraepelin, probably further alienated the analysts from the whole process.

Just a year before Rosenhan produced his paper, a systematic cross-national study of the diagnostic process in Britain and the USA was published by Oxford University Press. John Cooper and his colleagues presented results which laid bare just how uncertain the ontological status of psychiatric diagnoses was, how variable and subject to the whims of local culture. They looked at the two most serious forms of mental breakdown, schizophrenia and manic-depressive illness, and measured the cross-national differences in the ways these conditions were diagnosed. No-one would expect large cross-national variations in the diagnosis of, say, tuberculosis and pneumonia. Scientific knowledge is meant to be universal, and to travel easily across national boundaries. It proved to be quite otherwise in the psychiatric realm. Schizophrenia, it turned out, was diagnosed far more frequently in the USA than in Britain. Contrariwise, the diagnosis of manic-depressive illness was embraced far more frequently by British psychiatrists. The contradictions were massive. New York psychiatrists diagnosed nearly 62 per cent of their patients as schizophrenic, while in London only 34 per cent received this diagnosis; and while less than 5 per cent of the New York patients were diagnosed with depressive psychoses, the corresponding figure in London was 24 per cent. Detailed re-examination of the patients suggested that these differences were not rooted in differences in their symptoms, but were the by-product of the preferences and prejudices of each group of psychiatrists. Yet these differences had real-world consequences, producing major differences in the treatments the patients received (Cooper, Kendell and Gurland, 1972).

These and other studies were, however, couched in dry academic prose. They were of concern to a sub-set of psychiatrists who worried about their implications, but laymen did not read the psychiatric journals, or dry academic monographs that were written for a handful of specialists and priced accordingly. It was quite otherwise with David Rosenhan's findings, which had appeared in the most prestigious of places, presumably after strict peer review, so who could doubt their integrity? Their implications were profound. If psychiatry could be so easily duped, and would assign the most devastating of diagnoses – schizophrenia – on the basis of such superficial grounds, it was surely an emperor with no clothes.

None of the earlier critiques of psychiatry's problems with diagnosis had attracted any attention outside the profession, and most within it had treated the problem as trivial. 'On being sane in insane places' altered the landscape at once, and quite fundamentally. Rosenhan's findings attracted massive media attention all across the USA. At least 70 newspapers, both regional and national, gave prominent attention to his study. Television and radio shows interviewed Rosenhan. A major commercial publisher offered him a lucrative contract for a book based on his research, an offer Rosenhan accepted with alacrity. Harvard University even sent out feelers about a possible appointment to its faculty. Rosenhan's exposure of psychiatry's flaws caused a sensation. No wonder so many practitioners rushed to register their objections in the pages of *Science*, which, quite extraordinarily, devoted nine pages of a subsequent issue to their howls of protest, and to Rosenhan's response. That in itself was a measure of how powerfully this exposé resonated outside the cloistered world of academia.

The seriousness of the crisis the profession faced was immediately recognized by psychiatry's elite. Within weeks of the article's appearance, the Board of Trustees of the American Psychiatric Association (APA) called an emergency meeting in Atlanta on 1 February. How could they respond to 'the rampant criticism' that enveloped the profession, not least to the perception (or rather, the reality) that its practitioners could not reliably make diagnoses of the mental illnesses they claimed to be expert at treating (Decker, 2013: 141–2)?

The preceding decade and a half had seen psychiatry come under withering assault from other quarters as well, worsening the sense of panic that enveloped the profession's leaders. These critics had inflicted great damage on psychiatry's legitimacy even before Rosenhan's work brought things to a head. Although a movement away from traditional mental hospitals had begun, and though only a small fraction of the profession (and not its leading members) made their living from institutional psychiatry, the asylum was still where several hundred thousand of the most seriously mentally ill were to be found. It was the arena that had given birth to psychiatry, and the setting most prominently associated with its practice. Yet a whole series of studies of the mental hospital had painted an overwhelmingly bleak portrait of their impact on the mentally ill. Captured most vividly in the Canadian-American sociologist Erving Goffman's (1961) best-selling book *Asylums*, which called them 'total institutions' (a category that also included prisons and concentration camps), mental hospitals were denounced as inherently anti-therapeutic institutions that damaged and deformed those they purported to treat. In the words of another sociologist, Ivan Belknap (1956: 212), 'The abandonment of the state hospitals might be one of the greatest humanitarian reforms and the greatest financial economy ever achieved'. Not a few psychiatrists were beginning to express similar sentiments (e.g. Barton, 1959; Wing and Brown, 1970).

Sociologists were one thing: lay critics whom psychiatry could dismiss as biased and clinically ignorant outsiders. But the public heard even more damaging attacks from within the profession's own ranks, and though most psychiatrists also dismissed these critiques, and ostracized those who uttered them, they received a wide and respectful hearing in many quarters. The Scottish psychiatrist RD Laing (1967) made the heretical suggestion that it was society, not the mental patient, which was deranged, and that schizophrenia was a voyage of discovery that should be indulged and encouraged. For a time, he secured a considerable following among intellectuals. The renegade New York psychiatrist Thomas Szasz had published a best-selling book, *The Myth of Mental Illness*, in 1961. He spent the rest of the decade and beyond repeating the theme that 'mental illness' was nothing more than a bad metaphor with no legitimate status in medical science, and accusing psychiatrists of systematically violating their patients' rights, acting not as therapists, but as jailers and agents of social control on behalf of society at large. Those who had adopted such views saw Rosenhan's 'findings' as further evidence that psychiatry was a profession unworthy of the name.

As the APA's Board of Trustees recognized, Rosenhan's paper, and the immense amount of publicity it had already received, had created a focal point for psychiatry's critics, and amounted to an existential crisis for the profession. Indeed, barely a year later, a prominent member of an emerging mental health bar – lawyers who were suing psychiatry on multiple fronts – authored a long law review article dismissing psychiatrists' claims to be experts in the diagnosis and treatment of mental illness as fraudulent and scientifically indefensible. Advised behind the scenes by Rosenhan himself, Bruce Ennis alleged that psychiatrists who weighed in on questions of sanity fared no better than a trained monkey flipping a coin (Ennis and Litwack, 1974).

In context, then, it should come as no surprise to learn that, after prolonged discussion, the trustees of the APA came to a decision: the association would set up a task force charged with evaluating and reworking the *Diagnostic and Statistical Manual* (*DSM*) that had proved so grievously inadequate. Before that task force could be assembled, however, another controversy arose. For decades, psychiatry had held that homosexuality was a form of mental illness – a claim that had deep roots in Freudian doctrine, and that reinforced strongly held prejudices among the public at large. Now, however, prompted by the civil rights revolution, gay activists, including closeted gay psychiatrists, revolted, and demanded that the profession reverse its previous position. After much internal debate and discussion, the APA resolved the issue by means of a postal ballot – an approach that solved a wrenching political issue, but one that invited public ridicule, and provoked further commentary about the reliability and scientific standing of psychiatric diagnoses (see Bayer, 1987).

The Columbia psychiatrist Robert Spitzer had played a large role in brokering the ‘solution’ that settled the controversy over the status of homosexuality. After some behind the scenes lobbying, a grateful association soon appointed him to head the task force charged with revising the *Diagnostic and Statistical Manual*. Most psychoanalysts continued to regard the whole project as silly and unworthy of their time, which allowed Spitzer great leeway in determining the working group’s make-up, and the one psychoanalyst in their midst, finding himself marginalized and ignored, soon ceased attending its sessions.

One of the first tasks Spitzer set himself as the task force began its work was to rebut David Rosenhan’s study. He engaged in extensive correspondence with Rosenhan, seeking to pierce the veil of secrecy and anonymity that had been a central feature of the published paper. Who were the pseudo-patients, he asked, and which hospitals had they been admitted to? Rosenhan deflected and refused to answer. ‘I am’, he wrote, ‘obliged to protect these sources.’[4] Spitzer’s response was to write two papers attacking the methodology, the logic and the conclusions of Rosenhan’s study, one of them forming part of a symposium that he organized to rebut Rosenhan’s claims (Spitzer, 1975, 1976). But while Rosenhan’s (1973) *Science* paper reached an enormous audience across the scientific community and, via the mass media, an even larger lay public, Spitzer here spoke only to his professional colleagues. There is, moreover, a deep irony at play here. It was paradoxically Rosenhan’s study, and the extraordinary publicity which had accompanied its publication, that had prompted the APA to set up a task force to revise its diagnostic system. Also it decided to appoint Spitzer to direct the creation of the new *DSM*, which would propel him to the forefront of the profession, and make him one of the most influential psychiatrists of the second half of the twentieth century.

The transformational impact that the third edition of the *Manual, DSM-III*, would have was not clear at all when Spitzer obtained his appointment. Most of the profession’s elite disdained what they saw as the dull, intellectually uninteresting task of constructing a new nosology for the field. They had, as they saw it, far more interesting intellectual puzzles to pursue. Spitzer demonstrated an extraordinary far-sightedness, and great political skill in putting together the membership of the task force, guiding its members toward consensus, and then in persuading a sceptical profession to adopt its work product. But in the absence of the impetus provided by Rosenhan’s study, one wonders how eager psychiatry would have been to revise its diagnostic procedures.

We do not live in that counter-factual universe, however. The revision of the *DSM did* take place, and the publication of the new third edition, 494 pages long compared with the 134 pages of its predecessor, transformed American psychiatry irrevocably. It accelerated the rapid decline of psychoanalysis, and its replacement by a biologically oriented psychiatry claiming that the ‘diseases’ the manual identified and listed were akin to those that mainstream medicine diagnosed and treated. It provided an almost mechanical approach to the diagnostic process, one that, at least in theory, sharply raised the odds that psychiatrists in Topeka or Walla Walla or San Francisco or New York would attach the same label when confronted by the same patient (though it did so by ignoring issues about the validity of the diagnosis that was reached). As supporters of the new approach boasted, it thus created a ‘Psychiatry Reborn’ (Lieberman, 2015: Part 3).

If the *DSM-III* miraculously created a reincarnated medical psychiatry, one of the midwives of the rebirth was the insurance industry. Insurers immediately embraced the new manual, since it provided a diagnostic checklist to which the industry could attach new treatment profiles of a limited sort, something greatly preferable in their eyes to the almost interminable treatments offered by psychoanalysts. The other midwife of psychiatry’s rebirth in radically changed form was the pharmaceutical industry. For these corporations, the existence of stable diagnostic categories could play a vital role in the testing of new drugs that needed FDA approval. One of the most crucial and consequential legacies of the *DSM-III* was thus the creation of ever-closer links between psychiatry and pharmaceutical corporations, and the money that flowed from that connection has greatly improved psychiatry's standing among medical school deans. A final source of validation for the new *DSM* was the decision of the National Institute of Mental Health to embrace the new system. That endorsement ensured the triumph of the approach Spitzer had championed for decades. For once, the Columbia psychiatrist Jeffrey Lieberman (2015: 148) was not exaggerating when he proclaimed, 'Never before in the history of medicine had a single document changed so much and affected so many'.

Rosenhan's paper thus served as the catalyst for a revolution in the orientation and practice of psychiatry, a revolution whose effects have dominated our approach to mental illness for almost four decades now. Yet for all its influence, it is important to remember that this study, however highly regarded the journal in which it appeared, rested upon what were alleged to be the experiences of a mere eight pseudo-patients and a dozen purported admissions to a series of unknown psychiatric hospitals. When the investigative journalist Susannah Cahalan raised private doubts about the integrity of the study, one obvious initial response was to wonder about the care with which the peer review process at *Science* had been done, and I suggested she should investigate this. After all, the journal's imprimatur and prominence had been vital to dissemination of the study, and to its credibility.

Ms Cahalan sought her own clarification from the current editor of *Science*, Monica Bradford. One of her assistants, Meagan Phelan, responded in an email: 'Unfortunately the peer review process of research articles like the one you cite below is confidential, so I'm afraid I cannot provide answers to your questions.'[5] Asked subsequently whether she could share the identity of the editor of the section in which the paper ran, Ms. Phelan wrote 'It was edited by one of Science's editors on staff at that time, but we do not share information about the editorial process – including editor names.'[6]

In an attempt to use my standing as an academic and historian of psychiatry, I then wrote asking for the same data. My request read in part:

> there is much to worry about in reading [Rosenhan's] paper 40 odd years on. The pseudo-patients who are the key to the paper's explosive findings are anonymized, as are the institutions where they apparently sought admission. While understandable on some levels, this means that the author's fundamental honesty has to be taken on trust – a trust that was greatly bolstered by the prestige of the journal where these results were published – yours. As someone deeply committed to the peer review process, I both understand its limits and also the need for anonymity to protect referees. But in this instance in particular, it seems to me to be of great importance to understand the basis on which *Science* accepted the paper for publication, and what the peer reviewers saw and requested to see. Did the journal check on the identity, even the very existence of the pseudo-patients? Were they made aware of which mental hospitals had supposedly admitted them? Did they verify the account Professor Rosenhan provided of how long they remained patients or what the attending psychiatrists had to say in their case notes? These are all matters of great moment, and at present all one has to go on are the apparently uncorroborated statements of an academic whose entire career was built on this single foundation.
>
> Would it be possible for you to share redacted versions of the referees' reports with me, removing anything that might identify them? Could you or someone else on your staff also enlighten me on what steps, if any, *Science* took to ensure the validity of Dr. Rosenhan's work? Given the extraordinary real-world impact of this paper, these are questions that really need to be answered satisfactorily. Dr. Rosenhan published no other work of comparable importance in his career, and I find what appears in the public record less than

> fully convincing. Given all the current controversy swirling around the replicability of findings in psychology and economics, these issues have even greater salience, and it seems to me, as one of the leading journals for the sciences, *Science* itself has a vested interest, indeed an obligation, to look closely into the verifiability of these reported findings.[7]

Two weeks after receiving that enquiry, Monica Bradford supplied a different but equally negative response to my request: ‘we do not have manuscript/peer review records from that time period’.[8] In my view, that leaves us with serious doubts about the truth of Rosenhan’s claims. It would not be the first time *Science* (like other journals) has been deceived (see e.g. Kolata, 2005). Such queries decades after the fact are irrelevant, of course, to the question of the paper’s impact at the time, and for many years following its appearance. That does not detract from the importance of clarifying the historical record.

Still, what more could be said after all this time? Some of us might have developed concerns about whether Rosenhan’s study was fraudulent, in whole or in part. But the attempt to see whether the contemporary peer reviewers had exercised due diligence when assessing the paper’s suitability for publication had reached a dead end, and nearly a half century after the study appeared in print, the easiest course of action would have been to call off the investigation.

There exists, however, a mass of additional documentation about the origins and conduct of Rosenhan’s study: David Rosenhan’s private papers, including his notes, correspondence and a diary; a draft manuscript of the book he was commissioned to write about his study; Rosenhan’s own medical records of his admission to Haverford State Hospital in Pennsylvania (the hospital stay that launched the pseudo-patient study); and an almost contemporaneous published paper by the someone who participated in the study but who was then excluded from the results published in *Science*. Susannah Cahalan obtained all these materials in the course of research for a book on Rosenhan, and supplemented this extensive set of written records with interviews with Rosenhan’s colleagues and friends, and with extensive efforts to track down the pseudo-patients. The search extended over many months and involved interviews and correspondence tracking potential leads across the USA, and even extended to the engagement of a private investigator. As I indicated earlier, Ms Cahalan has shared these materials with me, and given me permission to use them in this paper.

It is curious that, nearly a half-century after the publication of a study as famous and with such far-reaching effects as ‘On being sane in insane places’, only a single person has publicly acknowledged being part of the experiment. This person, ironically, is a patient dismissed in a footnote as someone who had failed to follow the study protocol, and whose data had therefore been excluded from the published findings. So, besides Rosenhan himself, which other pseudo-patients have been identified? Only one: someone who at the time was a graduate student in the psychology department at Stanford, and was recruited by him to take part.

In *Science*, Rosenhan had described the make-up of his pseudo-patient population as follows:

> One was a psychology graduate student in his 20s. The remaining seven were older and ‘established.’ Among them were three psychologists, a pediatrician, a psychiatrist, a painter, and a housewife. Three of the pseudo-patients were women, five were men. (Rosenhan, 1973: 251).

They were, he asserted, admitted to 12 different mental hospitals across five different states on the East and West coasts.

Despite two years of diligent effort, the only pseudo-patient Ms Cahalan was able to locate was the psychology graduate student, whose case is examined below. In the course of his extensive correspondence with Rosenhan in 1974 and 1975, Robert Spitzer had repeatedly sought access to the pseudo-patients' admission records, 'with all the safeguards for preserving confidentiality of persons and institutions'. Those redacted records, he pointed out, would demonstrate whether these volunteers 'were able to follow protocol and limit their histories to monosymptomatic illness'. 'Many psychiatrists', he noted, 'doubt that the patients only complained of hallucinations. It would be good to set this issue to rest.'[9] Although Rosenhan had earlier indicated that he would be 'delighted to send you the admissions notes', he never did so. Instead, he informed Spitzer that, like others, he would have to wait: 'I have asked others who desire raw data on our observations and/or others' observations of us to wait until I have completed analyzing for the book I am preparing [on the study].'[10]

In 1974, Rosenhan had indeed signed a contract with Doubleday to write a book about his study, receiving a first instalment of $11,000 of a promised $44,000 advance. He had begun to produce a manuscript (originally to be called *Odyssey into Lunacy* and later re-titled *Locked Up*), writing more than 200 pages that survive in his files. But nowhere in this draft, or anywhere else in his files, can one find any materials bearing in any significant way upon the identities and experiences of the supposed pseudo-patients: not their names, not their admission documents, not their own observations about what occurred at admission or during their hospitalization; not the names of the institutions to which it is claimed they sought admission. The published paper in *Science* included all sorts of detail about the patients' time in the hospital, including quantitative data allegedly recording the amounts of time psychiatrists and staff spent with the patients, but there was no trace of the observations that underpinned these numbers. Rosenhan's files, while they are wildly disorganized, are replete with other information about the study: fan letters from those who endorsed its findings, to whom he religiously replied; criticism from psychiatrists; copies of commentaries on the study; and so on. But the crucial raw materials, which should have been collected as the study was done and which formed the basis for its claims, are nowhere to be found.

'Curiouser and curiouser', cried Alice' (Lewis Carroll, *Alice's Adventures in Wonderland*, 1866: Ch. 2) – and so might we be. But perhaps, Ms Cahalan thought, a rigorous search, speaking with Rosenhan's colleagues and friends and with the pseudo-patient from the Stanford graduate programme, whom careful research had located, might produce other names? Unfortunately not. There is a file labelled 'Pseudo-patients' in Rosenhan's papers, but all it contains is a list of pseudonyms, and some brief biographical data. Patient 1 is 'David Lurie', a 39-year-old psychologist who claimed to work in advertising (in reality David Rosenhan himself). The others are: 'John and Sara Beasley', a recently retired psychiatrist and psychologist respectively (John was listed as being admitted to two hospitals); John's sister 'Martha Coates', who posed as a housewife; 'Laura and Bob Martin', described as a famous abstract painter and a paediatrician respectively (Laura was the only patient admitted to a private mental hospital and the only patient to receive a diagnosis of manic-depressive illness, rather than schizophrenia); 'Carl Wend', who had recently finished a PhD, and was described as going undercover four times; and 'Bill Dixon', graduate student (who turned out to be Rosenhan's own student, whom Ms Calahan subsequently located and interviewed).

Rosenhan's book manuscript elaborated a bit further on the biographies of these people, providing the basic information that was used in an effort to find the real people who corresponded to these pseudonyms. In a further attempt to track down the pseudo-patients, Susannah Cahalan identified and found someone described by former colleagues as Rosenhan's long-time 'research assistant', Nancy Horn, a woman who had been an undergraduate at Swarthmore and had then followed him to Stanford. She was very cooperative and acknowledged that she had worked very closely with Rosenhan (whom she still admired) in a variety of roles for several years. She had, it turned out, been in contact with two Stanford graduate students who had taken part in the study. She had visited them while they were in mental hospitals, and had even taken the notes of the medical records of the second student, who read them to her over the pay phone. Crucially, she remembered their names.

Tracking these two men down turned out to be difficult, but not impossible. The first former student was Wilburn Underwood, who had, as a graduate student, co-authored two papers with Rosenhan on affect and altruism in children. Wilburn (Bill) Underwood (aka Bill Dixon) was living in Austin, Texas, and agreed to be interviewed. He had been admitted to Agnews State Hospital in California just months before it ceased to function as a mental hospital and was turned into a facility for people with developmental disabilities. Contrary to Rosenhan's claim that he had spent weeks preparing each pseudo-patient, rehearsing their stories, teaching data collection methods, and discussing what life would be like on the ward, Underwood experienced none of that. He arrived, in his own words 'cold turkey'. The only preparation he recalls was Rosenhan showing him how to 'cheek' whatever psychotropic medication he was given, so he could later dispose of it without swallowing it. Yet in his *Science* paper (1973: 256), Rosenhan specifically claims that, before his admission, Underwood 'had trained for quite some time to get into the hospital'.

Underwood had first been examined at a community mental health facility in San Jose, which then recommended his admission to Agnews. He told the first psychiatrist that he was a graduate student who had started hearing voices and, by his account, this plus his nervousness was sufficient to secure a recommendation for in-patient treatment. There, a foreign-born, heavily accented psychiatrist interviewed him, asking questions about his sex life and his drug use, and admitted him as a paranoid schizophrenic. Thus far, with the exception of the failure to prepare him for the experience, what Underwood recalled largely matched Rosenhan's account in the *Science* paper, though no detailed notes survive to demonstrate whether or not Underwood elaborated on the aural hallucinations during his intake interview. So we have, perforce, to rely on his vague memories nearly 50 years later about what occurred on that occasion.

Not completely, however. Underwood stayed at Agnews for only eight days, during which he found himself in a large ward in an overcrowded and badly under-staffed facility. Rosenhan claims that all his pseudo-patients recorded the percentage of time staff and psychiatrists spent in direct contact with patients, rather than in cubicles and offices. Underwood had not been trained to make such observations and did not do so. Nor was he always successful in 'cheeking' his medication, and on at least one occasion he was dosed with Thorazine, producing somnolence and mental symptoms that disturbed his visiting wife.[11] Rosenhan also claimed that, with the sole exception of the female treated in a private mental hospital, all of the study's subjects were released with the diagnosis of 'schizophrenia in remission' (p. 252). No such notation exists in Underwood's hospital chart, which is strikingly devoid of entries. The date of his discharge was recorded, but the space labelled 'reason for discharge' was left completely blank. Underwood recounts that when he wanted to be released, he simply told the staff that he wanted to go to attend a motor-cross tournament, and they readily acceded to his request.

Bill Underwood remembered the first name of another graduate student who had taken part in the study: Harry; a list of Stanford graduate students at the time included a Harry Lando. Harry's biography, however, did not correspond to the brief descriptions outlined in Rosenhan's paper. There was, however, a small footnote where Rosenhan acknowledged that there had originally been a ninth pseudo-patient, whose data, he asserted, had been omitted from the published study, because 'although his sanity went undetected, he falsified aspects of his personal history, including his marital status and parental relationships. His experimental behaviors therefore were not identical to those of the other pseudo-patients' (p. 258, n. 6).

Few readers seem to have paid much attention to this passing remark, but in fact, once examined, it is rather peculiar. Every pseudo-patient in Rosenhan's account falsified his or her name and various biographical details about their careers and lives, and no-one more so than David Rosenhan himself (as I shall show). Bill Underwood, for example, renamed himself Bill Dixon, provided a work history to avoid identifying himself as a graduate student, and informed his psychiatrists that he was single. What else might explain Harry Lando's exclusion from the study?

It turned out that, unlike Underwood, Lando had subsequently made a career as a psychologist, and was working in the School of Public Health at the University of Minnesota. Three years after Rosenhan's study had appeared, he had published a paper recounting his experiences as a pseudo-patient (Lando, 1976). Written by an unknown assistant professor then at Iowa State University and appearing in a minor professional journal, Lando's paper was ignored. Its contents, however, are sharply at variance with the central findings of Rosenhan's study, and Ms Cahalan and I believe this explains the exclusion of the 'ninth' pseudo-patient from the study.[12]

On one point, Lando's report of his experiences is largely consonant with Rosenhan's paper. He reports that staff did indeed tend to interpret his behaviour through a pathological lens. For example, his extensive note-taking 'was viewed as withdrawal from personal contact, despite the fact that I spent the preponderance of my time interacting with both patients and staff. My concern for other patients was seen as a defense against dealing with my own problems'. He was seen as 'lacking affect' and using his notes 'to maintain a hold on reality' (p. 50).

In other respects, however, Lando's experiences directly contradicted Rosenhan's findings. He had originally sought admission to Langley-Porter, a facility affiliated with the University of California San Francisco, but was denied admission because the address he reported was not in its catchment area. Instead, 'Walter Abrams' was admitted to the psychiatric ward of a large general hospital. He had a 45-minute admissions interview, supplemented by neurological, physical and psychological examinations. We do not know what the examinations revealed, but it is unlikely, for example, that a 45-minute diagnostic examination was simply taken up with reporting auditory hallucinations of a rather crude sort. The upshot of all these examinations was a diagnosis of chronic undifferentiated schizophrenia.

The ward to which Lando was admitted was attractively furnished with books and magazines, a games room, television, and so forth, and was left unlocked. It was heavily staffed, and for fewer than 18 patients, there were seven full-time psychiatric nurses, a social worker, a clinical psychologist and two psychiatrists. Staff contact with patients was extensive, with 'average daily contact with doctoral staff [of] well over an hour' and constant interaction with nursing staff (p. 48). 'Staff showed little tendency to isolate themselves in the nurses' station' (p. 51) – another direct contrast with Rosenhan's claims. So, far from being ignored, patients were treated with respect and care. 'The powerlessness and depersonalization of patients emphasized by Rosenhan simply did not exist in this setting. On no occasion did I observe a patient ignored by staff.' Small wonder that, at the end of a 19-day stay, Lando reported that 'My overall impressions of the hospital were overwhelmingly positive' (p. 51).

It is not surprising, I suggest, that Rosenhan wanted to exclude these findings from his paper. But when it suited his purpose to extract something from Lando's notes and use it to bolster the portrait he sought to draw of mental hospitals, Rosenhan did not hesitate. Readers of the *Science* paper learned that: 'Another pseudo-patient attempted a romance with a nurse . . . The same person began to engage in psychotherapy with other patients – all this as a way of becoming a person in an impersonal environment' (Rosenhan, 1973: 256). They were given no inkling that these details came from the excluded pseudo-patient, Walter Abrams.

The misconduct did not end there. In Rosenhan's files, Ms Cahalan discovered an earlier draft of the *Science* paper that he had pre-circulated to the psychologist Walter Mischel for review. This version contained nine pseudo-patients, not eight. Harry Lando had not yet been excluded. Both papers reported what purported to be hard data about the lack of contact between staff and patients, presented in tabular form and elaborated upon in the text. The numbers were identical even after the ninth patient was removed in the published version. On the face of it, we know that this is a statistical impossibility. In this instance, given that Lando reported hours of contact with staff on a daily basis, both sets of 'data' are transparently falsified.

Rosenhan's files contained further notes on 'Walter Abrams'. They were riddled with errors. Lando's diagnosis was mis-recorded as paranoid schizophrenia. His hospital stay was given as 26 days (it was actually 19 days). He was not turned away for 'three days' before securing admission, and the ward was not 'full'. Finally, he was released with medical advice, not against it, and was not given the diagnosis of 'schizophrenia in remission'. All these statements were fabrications.

Although Rosenhan's files contained no information about the real identities of the pseudo-patients or the institutions to which they had allegedly been admitted, Susannah Cahalan spent two years attempting to track them down. Even Nancy Horn, who was Rosenhan's closest confidant during the relevant period, and who had been in contact with both pseudo-patients that had surfaced, had no idea about who the other pseudo-patients were. Nor did Rosenhan's colleagues and friends.

The artist 'Laura Martin', for example, was described in Rosenhan's unpublished book manuscript as having paintings in major museums across the country, and as having been institutionalized 'in one of the top five [private mental hospitals] in the country, where she remained for 52 days'. Clues like these formed the basis of a nationwide search for someone who matched this description. A number of potential leads were followed up, but none led anywhere. Art dealers and museums were surveyed in search of an artist matching the profile Rosenhan provided. Rosenhan's assistant, Nancy Horn, thought that perhaps the elite private institution she had managed to get admitted to was Chestnut Lodge in Maryland, but this suggestion was wrong: no patient matching Laura Martin's profile had been an in-patient at Chestnut Lodge. Eventually, there were no more avenues to explore, and the search for this elusive pseudo-patient had to be abandoned.

That experience was repeated with all the other pseudo-patients except the two graduate students.[13] The pseudo-patients had vanished or, more likely, they had never existed at all, save in Rosenhan's imagination. There is, of course, a remote possibility that they will still surface. Perhaps publication of this paper will draw one or more of them out, but I am not sanguine. After all, in the three years since Ms Cahalan's book appeared, none has. It is remarkable, surely, that in 50 years, no one besides Harry Lando has (unbidden) acknowledged being a participant in this famous (perhaps one should now say infamous) study. Although some of Rosenhan's colleagues were unwilling to accept that his work could be fraudulent when the suggestion was made to them, others have reacted to the idea by indicating that either they suspected as much, or that the idea did not surprise them.

We have already documented Rosenhan's willingness to fabricate and distort evidence, but there remains still more damning evidence of his chicanery. Rosenhan's files contain his own medical records, documenting his admission to Haverford State Hospital, as well as his notes on his institutionalization. It is revealing to compare the account he provides there with what the contemporaneous records show.

A central tenet of Rosenhan's paper was that reporting simple auditory hallucinations – voices saying 'Hollow', 'Empty' and 'Thud' – was sufficient, in the absence of any other symptomatology, to secure admission to mental hospitals and to obtain a diagnosis of schizophrenia (or, in one case, manic-depressive psychosis). Psychiatrists at the time expressed extreme scepticism that such a ploy would work, but their protests were dismissed as self-interested, and the prestige of the journal where Rosenhan's study appeared sufficed to give it credibility.

What do we learn about Rosenhan's admission to the mental hospital from his files? These contain copies of his medical records as well as his diary entries about his in-patient experience.

'David Lurie' was admitted to Haverford State Hospital on 6 February 1969. He had telephoned the hospital the day before seeking admission, and the person he spoke with made notes on the conversation. She indicated that: 'It was difficult for him to express himself as his speech was retarded, and he was very emotional.' He reported he was suffering from auditory hallucinations 'that were noisy and bothersome to him'. He had tried escaping them by moving to a friend's house, but to no avail. 'He is not sleeping and feels cold all over' and had been unable to work for an extended period. The patient had consulted two physicians and taken the medication they had prescribed, but 'they did not help either'. He was advised to go into the hospital the following day.

When Lurie arrived there, he was examined by an experienced staff psychiatrist, FL Bartlett. It was a lengthy examination, and Dr Bartlett kept extensive notes. Lurie told him that he had been hallucinating for three to four months. At first, he heard unexplained noises. More recently, 'he has been able to discern that the voices say, "It's empty", "nothing inside", "It's hollow, it makes an empty noise"'. As the interview proceeded, Lurie added to these symptoms. 'He has felt that he is "sensitive to radio signals and hear[s] what people are thinking". . . . He has tried to shut out the noises by putting "copper over my ears." One reason for coming to the hospital was because things "are better insulated in a hospital".' Lurie then confessed that he also 'had suicidal thoughts'. He had worked as a freelancer in advertising for eleven years, but six months earlier, he had ceased working, creating great financial strain for his family. 'As his finances deteriorated it became necessary for his wife to do part time typing and they have had to borrow (?) [*sic*] money from his wife's mother. "This has been very, very embarrassing."'

Bartlett probed further. He noted that 'When first spoken to [Mr. Lurie] responded slowly, somewhat absently and appeared perplexed. He grimaced frequently, twitched in his chair and spoke in a low voice'. Though oriented to time, place and person, 'his mood was depressed. He felt his wife really did not know how disturbed and helpless and useless he was and that "everybody would be better off without me"'. Six months previously, his employer and friend 'told him that his work was not good and they had an angry parting. "Since then I have not been able to concentrate or do anything"'. Bartlett found himself in the presence of an intelligent man who 'has tended to get lost in unproductive fantasies' and had failed to fulfil his potential. 'He is', he concluded, 'very frightened and depressed". Bartlett ruled out any organic disorder and diagnosed Lurie as 'Schizophrenic, schizo-affective type, depressed'.

I have quoted extensively from these documents (to which I had access through the kindness of Ms Cahalan) to demonstrate just how sharply what happened in the intake examination strayed from what Rosenhan (1973) represented as Lurie's behaviour in the *Science* paper. Far from confining himself to reporting three discrete aural hallucinations and otherwise behaving perfectly normally, Rosenhan gave ample evidence of deep intellectual and emotional disturbance. Besides the grimacing and twitching, and the dull, halting speech pattern, he indicated that the radio was broadcasting to him, and that he could 'hear' other people's thoughts. He was depressed and frightened, and had been unable to work for months. Outpatient treatment with drugs had failed to improve matters. Visibly 'tense and anxious', he thought he was worthless and had contemplated 'suicide as everyone would be better off if he was not around'. These constitute an infinitely more serious and extensive set of pathological symptoms. Had Rosenhan told the truth about his presentation of self at the hospital, no one would have been surprised that a psychiatrist would decide to admit him, or to diagnose the patient before him as he did.

In his *Science* paper, Rosenhan further claimed that, once admitted, the pseudo-patients, himself included, immediately stopped displaying symptoms and behaved normally. Again, the surviving medical records show that in his case this is quite false. In the days after his admission, two other psychiatrists examined him at some length. Both documented the depths of the pathology Lurie was showing.

To Robert C Browning, who examined him the day after his admission, he told a story that closely mirrored what he had told Bartlett. Browning probed to see whether he had experienced any seizures or blackouts. No, he had not. Lurie elaborated further on his auditory hallucinations. They had begun six months earlier, and had gradually become more severe.

> They began as a lot of undifferentiated noise and followed by music, recently voices began. . . . They had become more severe this past month and he thought they were coming from a radio. He placed a copper pot up to his ear to differentiate noises that he was hearing and he tried to interfere with this signal he thought he was hearing.

During the examination, he 'admits to ideas of reference, [and] delusions of persecution [are] evident'. Once again, 'suicidal ideation admitted to over the past two months, but he states, "I do not have the guts"'. Though Lurie informed Browning he was at present not hearing voices, this recital, if accurate, suggested a very disturbed individual.

Four days later, Lurie sat for another interview with a Dr Philip Katz. The history remained much as before, although Lurie claimed his illness had probably begun a decade ago. Katz noted, however, that 'there was no evidence of psychosis at time of interview'. Lurie informed him that he was hoping to resume an earlier career in economics, and had secured a job interview for the following Thursday. Three days later, the hospital discharged him to the care of his wife, urging him to seek 'psychotherapy on a [*sic*] out-patient basis. The diagnosis, therefore, is acute paranoid schizophrenia in remission'.

Conclusion

'On being sane in insane places' has to rank as one of the most influential social science papers published since World War II. Besides its canonical status in the disciplines of psychology and sociology, it had an immense impact on the public at large. The striking demonstration of the fallibility of psychiatric claims to expertise found (and often continues to find) a ready audience. Psychiatry as a profession has, since its origins, encountered great public suspicion and even among the medical profession at large has had great difficulty in securing recognition of its legitimacy. Unsurprisingly, the appearance of Rosenhan's study was widely publicized in the mass media, and provoked an instant crisis in the halls of the profession. That crisis in its turn led to the establishment of a task force to establish a reliable system of psychiatric diagnosis and, within a few years, had led to the adoption of the third edition of the APA's *Diagnostic and Statistical Manual of Mental Disorders* (1980), a best-selling volume whose influence remains intact and all-pervasive to this day.

Indirectly, therefore, Rosenhan's study played a major role in the complete re-orientation of American psychiatry. Very early in the 1980s, the psychoanalysis that had dominated it since World War II lost its hold over the profession, and soon withered away to almost nothing. In its place, biology and neuroscience came to rule the roost, and both profession and public came to adopt a radically different perspective on mental illness. These were momentous changes. Remarkably, I suggest, the study that greatly helped to smooth the way to their acceptance was thoroughly dishonest, a scientific scam that stood largely unchallenged for nearly a half century. It is time for it to be revealed for what it is: a successful scientific fraud.

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Notes

1. A recent survey of 12 leading textbooks on abnormal psychology, for example, found that half of them still gave extensive attention to Rosenhan's paper, summarizing its design and endorsing its basic findings. Only two of them acknowledged any criticisms of the study, although methodological critiques have emerged over the years since the article's initial publication (see Bartels and Peters, 2017). As another example of the study's extraordinary half-life, as recently as 1 January 2018, the *Washington Post* ran a wholly uncritical article summarizing Rosenhan's findings (Morris, 2018).
2. Rosenhan had held a two-year appointment at Haverford College from 1960 to 1962, and thereafter had been employed as a part-time lecturer at the University of Pennsylvania and then at Princeton University.
3. No fewer than 15 letters from psychiatrists were published in April 1973 in *Science* (180: 356–369), along with a rejoinder from Rosenhan. More extended critiques continued to appear over the next two years, as I will discuss below.
4. Rosenhan to Spitzer, 15 January 1975; David L Rosenhan Papers, Special Collections, Stanford University.
5. Meagan Phelan to Susannah Cahalan, March 14, 2016.
6. Meagan Phelan to Susannah Cahalan, March 14, 2016 (a second email).
7. Andrew Scull to Monica Bradford, March 16, 2016.
8. Monica Bradford to Andrew Scull, March 30, 2016.
9. Spitzer to Rosenhan, April 30, 1975; David L Rosenhan Papers, Special Collections, Stanford University.
10. Rosenhan to Spitzer, January 15, 1975; David L Rosenhan Papers, Special Collections, Stanford University.
11. I do not analyse here the risks that any pseudo-patient might run. Underwood had signed a form on admission giving permission for the hospital administration to administer both medication and electroconvulsive therapy, both treatments with potentially serious side-effects. One of the staff psychiatrists at Agnews was well-known for his cavalier use of ECT (so much so that his nickname among the staff was 'Dr. Sparky'), though Underwood avoided treatment by this physician. The ethical problems of putting study participants at risk in this fashion are obvious. But throughout, Rosenhan seems to have been oblivious to these and other ethical issues.
12. This is also Lando's own explanation for the exclusion of his admission from Rosenhan's paper, and 'the uncomfortable distance' Rosenhan kept from him during his remaining time in the Stanford doctoral programme. He did not discover that he had been excluded from the study until he read the paper in *Science*, and commented: 'I felt like I kind of had the rug pulled out from under me' (Interview of Harry Lando by Susannah Cahalan, Minneapolis, Minnesota).
13. The details of these lengthy searches can be found in Cahalan, 2019.

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