

Chapter 12 Epilogue Irrational Science

In 1944, John Whitehorn was asked to review the therapeutic scene within psychiatry for the American Psychiatric Association's one hundredth anniversary. ECT had just exploded onto the scene and shock treatments dominated the sessions on therapy at the APA meetings. Nevertheless Whitehorn had difficulties writing his review. ECT was not derived from a theoretical framework; it simply worked and no one knew exactly why. "The shock treatments remain essentially empirical. We are without adequate rational understanding of their mode of helpfulness: the empiricists have posed a formidable problem for rational research. In addition the shock therapies have stimulated further researches regarding prognosis in the more severe type of psychosis, with the result that there has been a better validation .. regarding the personality assets long known by the experienced psychiatrist to be important in individual prognosis."¹

Two decades later, during the 1960s, when the practice of ECT had begun to fall under a cloud in the West, a series of epidemiological studies looking at the incidence and outcome of schizophrenia in both the developed and the developing world was carried out. The study reported that outcomes were significantly better in places such as India than they were in the West. The most common explanation was the role of cultural factors, such as extended family networks supporting patients returning to their community after treatment.

None of these studies commented on the fact that ECT continued to be used in India much more freely than it was in the West at that time. The standard practice in many hospitals and units in India was for patients who had been hospitalized for more than two weeks to receive ECT almost regardless of diagnosis.² As of the 1980s, three-quarters of Indian patients given ECT had diagnoses of schizophrenia, and up to twenty percent of those admitted were treated with ECT. The impression of many Indian psychiatrists and observers was that shock treatment facilitated the discharge of many patients who would not otherwise have been discharged. In Western psychiatric facilities, practice had meanwhile moved to a world in which patients might be treated with multiple, different psychotropic drugs or combinations of drugs before ECT was ever considered.

This history of ECT has focused very heavily on its use in the West, and in particular on the United States. The story, however, needs to be seen in a global context. In recent years, the use of ECT in India has declined. There are no good figures on this but Indian psychiatrists talk about a substantial decline. When questioned about the reasons for this, they note that when Indian movies, made in Bollywood, tackle psychiatric issues, they all too often resort to ECT in unmodified form to convey the horrors of psychiatry. In addition to the influence of the movies, spiritual leaders and others, concerned about the encroachment of psychiatry on their domains, have been critical of the use of treatments like ECT. Finally, driven by psychopharmacology, Indian psychiatry has become industrialized and commercialized, and there is increasing pressure on psychiatrists to opt for drug treatments rather than ECT and to conform their clinical practice to algorithms

and protocols drawn up in the West, which typically place ECT as a final option on any treatment hierarchy.³

Thus our story, while situated in the West, is replaying itself systematically other cultures. Does it matter if the story repeats itself?

In 1999, I (DH) was involved in a project comparing the prevalence of catatonic features in India and in the West, as part of an effort to replicate the work of Max Fink and colleagues, who using a catatonia rating scale had found a prevalence of five to ten percent of such features among patients admitted to psychiatric facilities in the United States.⁴ A number of other studies reported similar findings from the same period.⁵ Such a figure seemed extraordinarily high, given the prevailing wisdom that catatonia as a diagnostic category had all but died out in the West.⁶ Cases just didn't seem to happen any more. The supposed disappearance of catatonia has typically been attributed to improvements in the nutrition of patients since the 1950s and 60s. It also seemed possible that better health in general and the prevalent use of antibiotics in medicine made a difference. The advent of psychopharmacology led to claims that treatment with psychotropic drugs aborted the development of a full-blown syndrome.⁷

It seemed possible, however, that catatonia might still be present in India at much the same frequency as before the advent of the pharmacological era. Older hospital records pointed to frequencies of ten percent or more among patients. This led to a project aimed at comparing catatonic features in a hundred consecutively admitted patients in Wales and in Hyderabad. It turned out that up to ten percent of both Welsh and Indian patients had catatonic features.⁸ The implications of this are startling. Highly trained Western physicians, it seems, are systematically missing very clear clinical presentations. If they are missing such dramatic aspects of the mental state of their patients, how can anyone have confidence in the theories or treatments that are being put forward at this point in psychiatry? It's difficult to accept that we are making progress, and we may even have gone backwards.

The curious history of catatonia gives some insight to why this might happen. In retrospective, it is clear that when Ladislav Meduna initially induced convulsions with metrazol, it was a fortuitous coincidence that many of his first patients were catatonic. This was at a time when catatonia was a hallmark diagnostic feature of schizophrenia. Had Meduna tried metrazol therapy on non-catatonic patients, the initial results might have been less clear cut.

However, this was not the first discovery of a cure for catatonia. The catatonic patients who responded favorably to metrazol were, in fact, patients left over from an earlier and forgotten breakthrough. From the late 1920s it was recognized that catatonic patients, if treated with barbiturates before symptoms had progressed too far, could respond fully and be discharged shortly afterwards.⁹ A subgroup of patients failed to respond but nevertheless, these patients also showed a dramatic response to barbiturate. After injection, these patients woke up from their stupor and were able to converse normally, read, draw, and engage in other activities, before they slipped back into a stupor as the

barbiturate wore off. Following the advent of the antipsychotics, these earlier pharmacological dramas became crystallized in the psychiatry of the late 1950s and 60s as evidence that barbiturates were not a cure for catatonia. They produced only brief responses in schizophrenia compared with the outcomes that more specific antipsychotic drugs delivered.

But there was little reason to think that the antipsychotics would be a treatment for catatonic schizophrenia, or to believe that the early use of antipsychotics in the 1950s would lead to a demise of catatonic syndromes. In those days, catatonia was a syndrome that featured heavily in psychiatric theorizing and research. And from the perspective of the key researchers in this area such as Henri Baruk, the exciting thing about the new phenothiazine antipsychotics was that, along with bulbocapnine and a limited number of other drugs, they could produce an experimental catatonia in animals.¹⁰ By the end of the 1950s, experimental catatonia was widely used by pharmaceutical companies as a screening method to identify potential antipsychotics. Clearly, substances that induce catatonia would not ordinarily act as effective agents against it.

In 1960 a new side effect of antipsychotics was described by Jean Delay and colleagues in their first clinical trial of haloperidol¹¹, which they later termed “syndrome malin des neuroleptiques” because it could be lethal. A smattering of case reports in English-language journals appeared in the following two decades, until in 1980, Stanley Caroff wrote the first systematic paper on neuroleptic malignant syndrome (NMS), an end-stage condition resulting from the use of antipsychotics in certain patients.¹² This registered widely and struck fear into the heart of psychiatrists in the United States, probably because from the mid-1970s these clinicians had found themselves the subjects of lawsuits for antipsychotic-induced tardive dyskinesia¹³. Along with ECT, tardive dyskinesia had become a lightning rod for antipsychiatry. Putting patients afflicted with this highly visible condition on the witness stand or in front of television cameras was almost as potent a weapon as Jack Nicholson playing the patient Randle McMurphy receiving unmodified ECT. Tardive dyskinesia spread a chill over the pharmacotherapy of severe mental illness. But at least patients with tardive dyskinesia stayed alive. Neuroleptic Malignant Syndrome killed. Up to fifty percent of the patients affected were at risk of fatality, and there were claims that as many as one percent of patients prescribed antipsychotics were at risk for NMS.

This was the background in 1983, when a young patient at Massachusetts General Hospital was given haloperidol postoperatively for a confusional state and appeared to develop NMS. The patient was the son of a wealthy foreigner. The attending doctor on call, Gregory Fricchione, called in the head of department, Edward (Ned) Cassem, the professor of psychiatry, to help. Cassem, a drinking, smoking, Catholic priest, popular with the nursing staff, faced a patient rigid in his bed; he searched the medicine cabinet, fished out a drug, and gave it to the patient who responded dramatically. Emerging from his mute stupor, the patient was able to look after himself. After a further dose of the “magic” medicine the following day, the patient was restored and his care continued uneventfully thereafter. When the nursing staff asked Cassem what he had given, they were told “Holy Water.” In fact it was lorazepam, a benzodiazepine from the same class

as diazepam (Valium), and Cassem and Fricchione subsequently gave lorazepam with benefit to a number of other patients with NMS and reported the results in 1985.¹⁴

Fricchione later worked with Max Fink in New York. There, as they became more aware of the history of catatonia and similarities between malignant catatonia and NMS, Fink wondered whether ECT might be a helpful treatment for NMS. Faced with a patient unresponsive to lorazepam, they tried ECT, which produced a complete recovery.¹⁵ In the course of the following ten years, the great majority of patients given ECT for NMS showed a positive response.¹⁶ This points to a number of possibilities. One is that NMS is a variant of catatonia. Another is the possibility that the primary effects of ECT are on the motor system, given that it works for NMS and Parkinson's disease as well as catatonia.

But the essential historical point here is that Fink and Fricchione had rediscovered something known for a long time: catatonia responds to pharmacotherapy and convulsive therapy¹⁷. That this information had been disacknowledged to the point of forgetting indicates that, far from current psychiatric practice being evidence-based and rational, it is as ideological as it has ever been, with most clinicians cut off from vast swathes of data and knowledge that do not suit the interests of the dominant paradigm.

Part of the problem here lies in the fact that the drug companies were no longer promoting benzodiazepines, as these were all patent, and by the 1980s without the kind of support that comes with pharmaceutical company interest it was nearly impossible to raise the profile of catatonia. From the large pool of signs and symptoms that patients present to clinicians, pharmaceutical company promotion emphasizes those that lead to drug-selling diagnoses or profiles. In other words, the pill names the illness.

The psychiatric and psychopharmacological marketplace is now structured to sell SSRI antidepressants and atypical antipsychotics; each specific brand in these drug categories essentially duplicates a compound held by a competitor company. Far from there being a plethora of agents on the market, as the profusion of brand names might suggest, there are only a limited number of truly distinct drugs—fewer in fact than there were in the 1960s. Without diversity in drug-treatment options, companies have little incentive to support different constructions of psychiatric illness or to emphasize the problems following the use of rival treatments. Although commercially motivated, this might at least stimulate thinking, which in turn, would benefit the consumer. But where all companies are essentially trying to achieve the same end, their combined marketing weight drowns out the possibility of noticing discrepant observations. Psychiatric thought, far from having developed since the 1960s, has arguably atrophied. The number of ideas in play is increasingly limited. The only treatment modality that challenges the dominant paradigm is ECT.

Does this matter? There are several issues here. In the first place, there is probably no other branch of medicine where the outcomes for a core disease are steadily worsening. Bacteriologists eliminate diseases. Duodenal ulcers are a thing of the past. The life expectancy of cancer patients is steadily improving. Fatal heart attacks are much less common than they once were. But in the West, patients with schizophrenia are dying

younger than they were in previous decades, and furthermore their mortality can be correlated with the number of antipsychotic drugs prescribed.¹⁸ Where is the radical assessment of modern practice that this scandal calls for?

There are good grounds for considering that some of these patients might have benefited from a course of ECT. Patients with schizophrenia display distinct motor features, such as mannerisms, perseveration, or stereotypies, and many cases of thought disorder can be reframed as motor problems. Given the direct motor effects of ECT visible in the response of depression with psychomotor retardation, catatonia, NMS, Parkinson's disease, and mania, there is a therapeutic foundation for thinking ECT may help such patients.

Another basis stems from the response of catatonia, which may well be best described as a disorder where there is a split between will and action. In this case the efficacy of ECT on motor functions might be seen as a form of cerebroversion¹⁹, aimed at restoring normal signaling sequences in the brain, in just the way that cardioversion resets comparable disturbances in signal sequencing that give rise to heart block or fibrillation. The optics of cardioversion are not pleasant, and yet placing paddles on the chest is celebrated as heroic and life-saving in television's medical dramas, in contrast to ECT, which is still shown in its unmodified form whenever it is portrayed in film.

Antipsychotic drugs can also in their own right trigger profound motor problems from Parkinsonism to NMS and tardive dyskinesia. Would these patients benefit from ECT? No one asks that question because when patients fail to respond to one set of drugs, clinicians proceed down a checklist to the next combination of drugs without stopping to examine the specific profile of a drug's effect. Looking for the magic bullet that will clear thought and produce calm, no one notices the onset or offset of motor symptoms inherent in the individual's presentation or as part of a drug-treatment response. The possibility of piecing together the jigsaw that is schizophrenia has been all but obliterated by the removal of key pieces from the clinical board. We are left hoping that the hunt for a gene in a haystack will turn up some answers, and in the meantime the gears of psychiatric theorizing have been shifted into neutral.

It's not important that this cerebroversion hypothesis is correct; the point is to highlight modern psychiatry's failure of imagination. At the start of the twenty-first century, thinking has been dominated by "bio-babble", a discourse characterized by jargon and an emphasis on the monoamines, dopamine, serotonin, and norepinephrine. Within a few years, this will almost certainly seem as vacuous as Freudian notions about libido. The problem, in the meantime, is that just as psychoanalysis once inhibited a generation from making progress in understanding what mental disorders are, so too psychopharmacology has held back development in theoretical aspects of psychiatry, at the expense of patients. What incentives are there to work out how clinical features and syndromes relate when such efforts are unlikely to be recognized, publicized, or funded in a field so beholden to the pharmaceutical industry. We have reached a situation in psychiatry that is almost the diametric opposite to Whitehorn's 1944 jibe about the shock therapies being entirely empirical. ECT, and its related procedures, rTMS, VNS, DBS and MST, are the only

therapeutic approaches that keep alive the possibility that clinicians might someday understand how the major psychological syndromes cohere .

There have been enormous benefits from research on basic psychopathology, and ECT and the other physical treatments have helped keep this window open. But these benefits have come at some cost. A generation of NIMH funding has been devoted to research that has helped sharpen the scientific questions. Yet this has diverted funding from clinical studies that might have led to an earlier establishment of the efficacy of ECT for psychotic depression, NMS, Parkinson's disease, or resistant mania. A clinical trial program interested in therapeutic outcomes would surely by now have produced some progress in delineating the schizophrenic syndromes that might be ECT responsive.

ECT poses a vibrant challenge in areas besides psychopathology. It has been central to the genesis of informed consent in medicine. The patients likely to receive this treatment now are very often deluded, and as such they pose acute questions to our understanding of what informed consent means. But informed consent has changed in recent years from a formulation that emphasizes the disclosure of information into something closer to a risk assessment. Good clinical practice involves patients and their caregivers working with nursing staff and physicians to examine how the risks stack up in ECT and other procedures. A gut feeling or common sense may suggest a weak or inappropriate course of drugs carries greater risk to the patient than modified ECT ever would. It can be difficult to know the answer; the bottom line will often be whether patients or their relatives are convinced that psychiatric staff would have ECT themselves if in the same position. On this point, lots of mental health professionals working with severely ill patients make informal living wills alerting their colleagues to the fact that they would wish to have ECT if they ever became this ill, where they would be far less likely to have DBS or VNS.

The complexity is layered. Was giving your consent a good idea? It depends on when and how the question is asked. In chapter 9, we saw that physicians' and patients' assessments of benefit and harm can differ dramatically. This difference is something to celebrate in that nowhere else in psychiatry is there such systematic research available from multiple different viewpoints. One of the features of research undertaken by patient groups on ECT and other physical therapies is that assessments are made months after treatment has ended. This is in contrast to clinician-led research or drug trials, which typically is undertaken much closer to the treatment.

But the differences in results are not simply a matter of timeframes: self-assessment is a problematic tool in psychiatry. It is not uncommon for patients, who are clearly improving in the course of ECT treatment to report that "everyone tells me this is helping, but I can't see it." Marked differences like this between points of view are phenomena that should challenge anyone with a real interest in the mysteries of psychiatry and consent. What does it mean if a treatment produces benefits readily apparent to disinterested observers but not apparent to the patient, and what are the implications of this for informed consent?

A history like this does not seek to answer questions about science, symptoms, or consent. It seeks instead to show how certain aspects of the mysteries involved in a domain like that of mental health come into and slip out of view at different points in time. What has happened in the case of ECT does not seem to sit comfortably beside either the dominant philosophies of science, which appeal to a steady accumulation of knowledge, nor with the business philosophies of modern clinical practice, which assume an ever more rational marketplace, in which it's almost inconceivable that a therapy of such importance could have been kicked aside for such trivial reasons as its image in a film. It's hard in fact to think of anywhere where the mismatch between rhetoric and reality is as great as it has been in the history of ECT. Medicine is clearly not vacuum-sealed against irrationality.

¹ John C. Whitehorn, in *One Hundred Years of American Psychiatry. 1844-1944* (New York: Columbia University Press, 1944), p. 188.

² J. S. Neki, "Psychiatry in South East Asia," *British Journal of Psychiatry* 123 (1973), pp. 257-261; N. S. Vahia, D. R. Doongaji, Dilip V. Jeste, "Twenty-five Years of Psychiatry in a Teaching General Hospital in India," *Indian Journal of Psychiatry* 16 (1974), pp. 221-228; G. D. Shukla, "Electroconvulsive Therapy in a Rural Teaching General Hospital in India," *British Journal of Psychiatry* 139 (1981), pp. 569-571.

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⁴ George Bush, Max Fink, George Petrides, F. Dowling, Alan Francis, "Catatonia: Rating Scale and Standardised Examination," *Acta Psychiatrica Scandinavica*, 93 (1996), pp. 129-136.

⁵ Sergio Starkstein, Gustavo Petracca, Alejandra Teson, Eran Chemerinski, Marcello Merello, Ricardo Migliorelli, Ramon Leiguarda, "Catatonia in Depression: Prevalence, Clinical Correlates and Validation of a Scale," *Journal of Neurology, Neurosurgery and Psychiatry*, 60 (1996), pp. 326-332; Patricia Rosebush, Anne Hillebrand, Brian Furlong, Michael Mazurek, "Catatonic Syndrome in a General Psychiatric Inpatient Population: Frequency, Clinical Presentation, and Response to Lorazepam," *Journal of Clinical Psychiatry*, 51(year), pp. 357-362.

⁶ B. Mahendra, "Where Have All the Catatonics Gone," *Psychological Medicine*, 11 (1981), pp. 669-671.

⁷ Max Fink, Michael Taylor *Catatonia* (Cambridge, Eng.: Cambridge University Press, 2003); David Healy, *The Creation of Psychopharmacology* (Cambridge, MA: Harvard University Press, 2002).

⁸ David Healy, Margaret Harris, Dinah Cattell, Pam Michael, Padmaja Chalasanni, David Hirst, "Service Utilisation in 1896 and 1996: Morbidity and Mortality," *History of Psychiatry* (2005); Padmaja Chalassani, David Healy, Richard Morris, "Incidence and Presentation of Catatonia and Related Motor Syndromes in Two Acute Psychiatric Admission Units in India and the United Kingdom" (subm).

⁹ William J. Bleckwenn, "Production of Sleep and Rest in Psychotic Cases," *Archives of Neurology and Psychiatry*, 24 (1930), pp. 365-375.

¹⁰ Henri Baruk, J. Launay, J Berges, "Experimental Catatonia and Psychopathology of Neuroleptics," *Journal of Clinical & Experimental Psychopathology*, 19 (1958), pp. 277-291.

¹¹ Jean Delay, Pierre Pichot, Therese Lemperiere. "Un neuroleptique majeur non-phenothiazine et non reserpinique; L'haloperidol dans le traitement des psychoses," *Annales Médico-Psychologiques*, 118 (1960), pp. 145-152.

¹² Stanley N. Caroff, "Neuroleptic Malignant Syndrome," *Journal of Clinical Psychiatry*, 41 (1980), pp. 79-83.

¹³ Sheldon Gelman. *Medicating Schizophrenia. A History.* Rutgers University Press, New Brunswick (1999).

¹⁴ Gregory L Fricchione, Ned Cassem, Daniel Hooberman, Douglas Hobson, "Intravenous Lorazepam in Neuroleptic Induced Catatonia," *Journal of Clinical Psychopharmacology*, 3 (1983), pp. 338-342.

¹⁵ Fink and Taylor, *Catatonia*, p. 46; Gregory Fricchione, "Neuroleptic Catatonia and its Relationship to Psychogenic Catatonia," *Biological Psychiatry*, 20 (year), pp. 304-313.

¹⁶ John Davis, Stanley Caroff, Stephen Mann, "Treatment of Neuroleptic Malignant Syndrome," *Psychiatric Annals*, 30 (2000), pp. 325-332.

¹⁷ The rediscovery of catatonia as a clinical entity had begun in the 1970s. In 1991 Fink and Michael Alan Taylor urged that it be included in DSM-IV. See their "Catatonia: A Separate Category for DSM-IV?" *Integrative Psychiatry*, 7 (1991), pp. 2-10.

¹⁸ U. Osby, N. Correia, L. Brandt et al., "Mortality and Causes of Death in Schizophrenia in Stockholm County, Sweden," *Schizophrenia Research*, 45 (2000), pp. 21-28; Matti Joukamaa, Markku Heliovaara, Paul Knekt, Arpo Aromaa, Raimo Raitasalo, Ville Lehtinen, "Schizophrenia, Neuroleptic Medication and Mortality," *British Journal of Psychiatry* (forthcoming).

¹⁹ The term "cerebroversion" may first have been proposed by Conrad Schwartz. Its first use in print was as a synonym for ECT by Jeffrey H. Morse and colleagues in Salem, Virginia. See their letter, *AJP*, 148 (1991), p. 1764. But this word and variants on it such as neuroversion have probably occurred to many. In this case, the use of the word is to describe a process rather than ECT as such, and its first use that DH is aware of was by P. Chalassani.