

# THE BIOLOGY OF MENTAL ILLNESS

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*Jonathan Leo*

*One of the greatest marketing feats of the past 20 years is the use of pharmaceutical companies' dollars to convince the mass media that psychiatrists who prescribe these companies' drugs are basing their treatment on anything resembling science.*

-Bruce Levine, psychologist and author of *Commonsense Rebellion*

No doubt, the majority of the 28 million Americans taking an antidepressant, or similar drug, were told by a doctor that they have a genetic defect resulting in a shortage of a chemical, and that to rectify this chemical imbalance they need to take a pill. The basic tenet of biological psychiatry is that mental illness is an "organic" disease, meaning that the patient has too much or too little of a neurotransmitter, too much or too little of a receptor, or an overactive or underactive neuronal circuit. Whatever the problem might be, it is "biological" and biological problems are best treated with drugs. As everyone now knows, clinical depression is just like diabetes; one patient is short of insulin, another is short of serotonin; one patient needs insulin, another needs Prozac-and so the story goes. Never has a theory with so little scientific evidence been so well accepted by the American public-three of the seven most commonly prescribed drugs are now mood elevators. While the pharmaceutical companies certainly deserve much of the credit for this, unfortunately these companies are not alone. Most of us are sufficiently jaded by the shenanigans of big business to really place too much blame on these companies for turning an unproven theory into a marketing slogan; skepticism of big business is part of our national psyche. The trickier question is deciding how much of a role academicians in the American medical community, particularly those in medical school psychiatry departments, have contributed to the problem.

Times have changed so dramatically that psychiatrists no longer see depressed patients as people with complicated issues that might need to be worked out and discussed. Instead, patients are bundles of neu-

rotransmitters in need of a tune up; out comes the prescription pad and only if the patient is lucky enough to have adequate insurance will they get to see a psychologist or a therapist. A recent study found that it took three minutes for a patient to get a prescription for an anti-depressant medication-about as much time as the typical person spends talking to a grocery store check-out clerk.

According to Nathaniel Lehrman, the former clinical director of the Kingsboro Psychiatric Center, "We find ourselves in the increasingly difficult position because Psychiatry has badly mishandled depression in its all-consuming reliance on drugs as the first line of treatment." As just one example of how far we have come, in one of her recent columns, Dr. Joyce Brothers diagnoses a woman with depression and recommends therapy *and* medication-all based on a one-paragraph letter. Like most people who are depressed, there is a reason for her depression-it didn't just appear out of nowhere. Consider her circumstances; the woman is getting divorced from her chronically unfaithful husband, her boss just died, and she is about to lose her job. No one is going to argue that people have psychological problems, but does this woman have a disease? Is the anti-depressant recommended by Dr. Brothers treating her serotonin shortage or is it just a little something to take the edge off during a crisis? The only way to say that this person has a "disease" is to ignore her as an individual.

## **The Chemical Theory of Mental Illness**

The scientific basis of this theory can be summed up like this: first, in laboratory preparations, drugs like

Prozac increase the availability of serotonin—thus the term selective serotonin reuptake inhibitors (SSRI). Second, some patients report that they feel better after taking Prozac, so, putting these two factoids together, then depression must be due to low serotonin levels. That's it, as far as blood tests, brain scans, or any other quantitative tests forget it. Yet, because of the diabetes analogy, much of the general public has the mistaken belief that doctors can measure serotonin levels with a blood test in the same way insulin is measured, but scientists have never documented low serotonin levels in humans diagnosed with depression. If a psychiatrist says you have a shortage of a chemical ask for a blood test and watch the psychiatrist's reaction. The number of people who believe that scientists have proven that depressed people have low serotonin is a glorious testament to the power of marketing.

Elliot Valenstein, Professor Emeritus at the University of Michigan, has spent his life studying the brain and behavior, so when he started to write a book on the chemical theories of mental illness he expected to find significant evidence in support of these theories. To his surprise, as he proceeded it became clear that the evidence for the biological basis of mental illness was weak—much weaker than we are commonly told. In his book *Blaming the Brain: The Truth about Drugs and Mental Health*, he documents that there are major flaws in the theory that depression is due to a shortage of serotonin, or schizophrenia to an excess of dopamine, or attention deficit disorder to a shortage of dopamine. These flaws, while often subtly acknowledged in professional journals by psychiatry researchers, are simply glossed over in presentations to the general public. In his words, "What physicians and the public are reading about drugs and what causes mental disorders is by no means a reflection of all the information that is available."

### **The Biological Theory of Mental Illness**

For students interested in learning how to become critical readers, there is nothing quite like the psychiatry literature. A recent brochure about clinical depression from the National Institute of Mental Health (NIMH) states: "Substantial evidence from neuroscience, genetics, and clinical investigation shows that depressive illnesses are disorders of the brain. However, the *precise* causes of these illnesses continue to be a matter of intense research." Statements like this, where the theory is affirmed while simultaneously acknowledging that there is no specific evidence to support it, are common in the psychiatry literature. Rather than a straightforward statement that the chemical theory of mental illness is a theory in search of evidence, instead,

these authors try to put a good face, or a good spin, on a theory whose usefulness as a marketing tool has far exceeded its scientific validity.

Psychiatrists have written a tremendous amount of material about the chemical theories of mental illness, but it is very difficult to pin them down on what exactly they think is the best evidence supporting their theories. When you pick up a magazine article on mental illness, or even a review article from a psychiatry journal, you need to consciously weed thru the "implications" and ask yourself "Where is the proof?" The following examples are the "proof" from three prominent leaders of the psychiatry community. To be fair, these three authors have written a huge amount of material about mental health, and to sum up their work in a short paper is difficult, but another way of looking at these examples is: Given the chance to defend their theories is this the best these scientists can do?

Dennis Charney, Chief of the Mood and Anxiety Disorders Research Program at NIMH, is one of the most influential biological psychiatrists in the country, and the author of the textbook *Neurobiology of Mental Illness*. A recent issue of the journal *Cerebrum* contains a debate in which Charney takes on the task of defending the biological theory of mental illness; on the opposing side is Elliot Valenstein.

Valenstein starts off the debate by pointing out problems with the theory that depression results from too little serotonin. Charney, like many biological psychiatrists, does not say that the serotonin theory is discredited. Instead he talks about how the theory has become more "refined" to include other neurotransmitters, for instance substance P. Valenstein points out that while these other transmitters might become future avenues of research, at this point these studies are still preliminary. A problem with Charney's logic is that because of the virtually unlimited number of transmitters the theory can live on for eternity, moving from one transmitter to another. If scientists hit a blank wall with substance P, of course there will be another transmitter on the horizon; the theory can never die but at the same time it is never proven correct.

As the two debaters go back and forth, you can sense Charney's frustration, because for every bit of evidence that he cites in support of his theories Valenstein points out the problems and limitations. Charney claims there is "clear experimental evidence" that chronic administration of antidepressants increase serotonin activity in the brain. But again Valenstein points out the limitations, namely, that these experiments were done in rats, not humans, and that some studies with rats have shown

completely opposite results. Furthermore, all drugs affect the brain (more than most psychiatrists concede) but what do these effects have to do with telling us about biological causation of depression?

Charney claims that Valenstein has a "superficial understanding of current research into the neurobiology of psychiatric disorders" and cites recent studies examining the effect of stress on the hippocampus. But Valenstein points out that the principal investigator of these studies has said that his theory could only account for a small subgroup of depressed patients. By the end of the debate Charney becomes so frustrated that he refers to Valenstein and the *Cerebrum* readers to his text-book, and one wonders if his frustration really stems from the fact that he cannot marshal enough solid evidence to support his theory.

In a similar debate Rodrigo Munoz, former President of the American Psychiatry Association, in response to criticisms, claimed that neuroimaging, biochemical studies, and genetic research "may help us to learn which neurotransmitter, cellular membrane, and cellular metabolism factors are important in triggering depression and causing its persistence. Until we have a better understanding, we will have to use these tools" (*Psychiatric Times*, Vol. 27). This is hardly a ringing endorsement of the theory and most importantly Munoz's defense is very different from the usual assurances about the theory that are so common in the media.

When reading the psychiatry literature it is also essential to look at what the experts do not say. In his debate Munoz did not cite confirmed and replicated experiments; instead he talked about future successes leading the observant reader to ask: If there is good experimental evidence to support the serotonin theory of depression why did he not cite it?

When psychiatrists defend the idea that "distress" is due to an underlying biological and genetic defect they invariably mention schizophrenia because of all the conditions that psychiatrists treat, schizophrenia supposedly has the strongest biological basis. But have psychiatrists really found a biological basis for schizophrenia? Nancy Andreasen, Editor of the *American Journal of Psychiatry*, who has written numerous articles for scientific journals and has authored two books for the general public, is considered one of the leading experts on the topic. Andreasen says that brain scans provide perhaps the strongest evidence that people with schizophrenia have an underlying biological disease. Yet in the next paragraph later she says that "A diligent search by any talented neuroscientists has not identified any such specific regional abnormalities or nerve cell l

sions" (*San Diego Reader*, January 2003). On the one hand she says that the imaging research is the most convincing research that schizophrenia is a disease, but on the other hand she acknowledges that scientists have not identified any pathological marker for the condition.

Or take Andreasen's textbook chapter on schizophrenia (*Textbook of Clinical Psychiatry*). From a superficial reading you might conclude that scientists are doing well on finding a biological basis for schizophrenia. However, a more critical reading reveals that Andreasen has implicated the following regions of the brain: pre-frontal cortex, temporal cortex, cingulate cortex, limbic cortex, thalamus, hippocampus, cerebellum, and the basal ganglia. In fact, her list includes almost all the major regions of the brain. She also mentions GABA, glutamate, and serotonin as transmitters that might be involved. The language of biological psychiatry is filled with "implications," "maybes," and "possibilities" but short on documentation.

In *The Broken Brain* Andreasen summarized the dopamine theory of schizophrenia with the statement: "We know that schizophrenics may have a chemical imbalance in their brains" (emphasis added). This wonderfully vague sentence, from a 278-page book supposedly documenting the biological revolution in psychiatry, exemplifies how important it is to read the psychiatry literature with a critical eye and to notice that *volume* can never take the place of *specifics*.

It is hard to fault these authors for acknowledging that the chemical theories of mental illness have not been proven; after all, they are scientists and there is simply not enough evidence to say they have been proven at least in scientific journals. However, a more straightforward definition of the chemical theory of mental illness might be worded like this: "We (the experts) think that mental illness *may* be due to a chemical imbalance, but at this point based on the available data we do not *know* if this hypothesis is valid." Theories are essential for scientists but deserve to be called just that—theories. It would be interesting to know if these authors are upset about the simplistic advertising slogan in a recent brochure for Paxil that says, "This medicine works by bringing the levels of serotonin back to normal." If they are upset, then what about the fact that there is ample anecdotal evidence that this sort of slogan is precisely what many patients hear directly from their doctor's mouth as the prescription is being written?

### Science or Marketing

According to the editors of the *LA Times*. "Drug company funding is corrupting medical research" and

they call on the National Institute of Health "to counter the influence of private funding on science" (2/24/03). An example of how the chemical theory of mental illness can be used as a marketing pitch is the web site *mental-health-matters.com* which has pharmaceutical advertising right alongside articles such as "The Chemical Imbalance in Mental Health Problems" by Joseph Carver. Readers of Carver's article, with no medical background, would assume that scientists have proven that the cause of every mental disorder is nothing more than out of whack neurotransmitter levels which can easily be fixed with a pill-and naturally the companies making the pills are just a mouse click away. The article contains statements such as, "As research in neurotransmitters continued, studies between neurotransmitters and mental conditions revealed a strong connection between certain neurotransmitters in the brain and the presence of psychiatric conditions," or "Research also tells us that several neurotransmitters are related to mental health problems-dopamine, serotonin, norepinephrine, and GABA. Too much or too little of these neurotransmitters are now felt to produce psychiatric conditions such as schizophrenia, depression, bipolar disorder, obsessive-compulsive disorder and ADHD." Carver has provided a nice side-by-side listing of transmitters and disorders, but no evidence that any of these disorders can be linked to any specific transmitter-but this is only the beginning.

Indeed, he goes on to state that it is possible to detect altered transmitter levels in psychiatric patients, but that the average psychiatrist cannot conduct these tests in a typical office setting simply because machines, like PET scanners, are too expensive. But the reason these tests are not used for diagnosis is much more straightforward. *They do not work.* The problem is that a psychiatrist, given the brain scans of 100 psychiatric patients and 100 "normals," cannot differentiate between the two groups let alone identify a single one of the patients' brains as showing evidence of a psychiatric disorder. PET scans and other such machines are used in research, not in clinical psychiatry. Even the experts acknowledge this in scientific journals. Daniel Weinberger, Chief of the Clinical Brain Disorders Branch of NIMH, recently stated: "At this time, the only clinical reason to do a neuroimaging study in psychiatry is to rule out a neurological disease masquerading as a psychiatric illness" (*Neurology Today*, June 2002) Or take Joseph Callicott's remarks about functional neuroimaging of schizophrenia in *Current Opinion in Neurobiology* "the identification of pathognomonic physiological features of the illness or even a consensus regarding the interpretation of reported findings remain unfulfilled goals." Besides a patient his-

tory, at this point in time, psychiatrists have no biological or objective test to diagnosis mental illness, and it has nothing to do with cost as Carver suggests.

Where Carver got his information is unclear, because even a superficial reading of the psychiatric literature will confirm that PET scans cannot be used to diagnose disorders such as schizophrenia, bipolar, ADHD, or depression. In a review article in the journal *Lancet*, even Nancy Andreasen stated, "diagnosis of schizophrenia relies on observation-based criteria," and this from a scientist whose expertise is the use of PET scans to study schizophrenia. If PET scans could be used to diagnose a mental disorder Andreasen would certainly have mentioned it. If you are wondering why scientists can be honest and direct in their journals while at the same time massaging the truth for the general public, Carver supplies one clue: "Technical aspects of neurotransmitter levels, the psychiatric symptoms they produce, and how medications have been developed to raise or lower the brain levels of these neurotransmitters can be very complicated." He goes on and explains this is why psychiatrists use simple analogies like comparing the brain and its chemicals to the oil in your car which can be measured with a dipstick. Many critics have said that the chemical theory of mental illness is a grand oversimplification of a very complex issue, but this is the first example that I know of where a professional has answered the critics by saying that the general public cannot understand the "technical aspects." Nevertheless, Carver is correct about the issue of neurotransmitter levels being more complicated than he and his colleagues have stated; lost on Carver is the fact that the issue is more complicated simply because scientists have never correlated altered transmitter levels with any DSM-IV diagnosis.

Most disconcerting is that by juxtaposing an article by a professional in neurotransmitter fantasy land with pharmaceutical company advertising, these drugs are promoted in a way that the companies themselves could never do. If they did they would be accused of false advertising. Carver's statements are completely at odds with what Charney, Andreasen, and Munoz say in professional journals, his statements contradict what scientists say about objective tests, his statements could never be made by the pharmaceutical companies, yet they appear on the web page of an influential voice for the treatment of people with psychological distress.

### **Medication, Placebo, and Therapy**

Even if the biochemical theory of mental illness has never been proven correct many psychiatrists will say, "Theories aside, Prozac works." Again, it is not that simple. At this point I can hear my critics sharpening

their pencils and getting ready to declare that any doctor who withholds these medications is negligent and not following the best practice, but there is a large body of research, essentially ignored by American Psychiatry, suggesting otherwise.

A recent study in the *Journal of the American Medical Association* examined the effect of St. John's Wort on depression (2002, Vol. 287, p. 1807). The authors found that on most measures it was no better than a placebo. This was blared out over the radio waves, prominently reported in the papers, and told to all Americans on the nightly news. For critics of alternative medicine this was a loaded gun with an unlimited supply of ammo. Yet this entire story exemplifies how the media learns about these topics. Reporters do not read the studies; they read the pharmaceutical company press releases. If they had read the original study they would have known there were three treatment groups in this study. Zoloft, one of the most common antidepressants, was also included in this study, and most of the press failed to report that Zoloft was no better than placebo. The alternative medicine critics were more than happy to unload all over Zoloft; but, at least according to this study, there is as much scientific justification for prescribing St John's Wort as there is for prescribing Prozac.

Even more interesting is the work of Irving Kirsch and his associates at the University of Connecticut. In 1998, Kirsch compiled and analyzed data from numerous published studies that had compared anti-depressants to placebo. Kirsch's meta-analysis found that the standard medications were only slightly better than placebo, and the difference was virtually insignificant. Not surprisingly the critics were harsh-Kirsch's study calls into question the entire rationale and justification for the millions of anti-depressant prescriptions written every year-and claimed that Kirsch's study was biased because of the studies he included in his analysis. So, in a stroke of genius, Kirsch, Scoboria, and Moore used the freedom of information of act to gain access to all the studies that the drug companies had submitted to the FDA for the purpose of getting these drugs approved. They redid the analysis with the new data, and again found that the placebo response was responsible for the majority of the effect. In the case of Prozac, the placebo response duplicated 89% of the drug response.

The same issue of *Prevention and Treatment* that published Kirsch's analysis of the pharmaceutical company data also published several commentaries-both pro and con. Surprisingly, Kirsch's critics did not argue with his findings, instead they quibbled about the interpretation of the results. In their reply, Kirsch and his colleagues stated: "We are very heartened by the

thoughtful responses to our article. Unlike some of the responses to a previous meta-analysis of antidepressant drug effects, there is now unanimous agreement among commentators that the mean difference between response to antidepressant drugs and response to inert placebo is very small." They go on to explain that this miniscule difference between placebo and medication is commonly referred to by researchers, FDA reviewers, and a small group of critics as the "dirty little secret."

It remains to be seen how the mainstream press explains this "dirty little secret" to the American public, but so far it has received little airtime. Just for a moment imagine the reverse, and that Kirsch's article had found a significant difference between placebo and medication-the pharmaceutical company marketing forces certainly would have been working overtime to explain to reporters and the general public the importance of *these* findings. If you think the press has done an adequate job reporting these issues then ask yourself this: What is more significant, the failure of a drug that millions of people take or the failure of St. John's Wort? Without a doubt, the failure of Zoloft is much more significant than the failure of St. John's Wort. It would be hard to find a better example of the media's failure, or the pharmaceutical industry's success.

In many studies comparing drugs to therapy, investigators will record the observations of both the patients and the doctors at several different points, such as 4-weeks, 8-weeks, 16-weeks, or longer. From a scientific point of view this is exactly what should be done, but it should be pointed out that a sales pitch is almost sure to emerge from this type of protocol. For instance, many anti-anxiety drug advertisements often mention a study finding a virtual cure of the attacks at 4-weeks. Left unsaid in these same ads, is that *at* 8-weeks the advantage of drug *over* placebo had disappeared and that beyond 8-weeks the patients on the medication were having more panic attacks than when they had started the treatment. Or that a subsequent study found that *from* the eighth-week onwards therapy was better than placebo (*See Duncan and Sparks, The Heroic Client* for a more extensive discussion). Again, it is hard to blame the pharmaceutical companies for putting a twisted slant on these studies, but the medical community could certainly take a more active role in how these studies are portrayed to the public.

A recent review on the topic of psychotherapy versus medication by Antonuccio, Danton, and DeNelsky discusses an often cited National Institute of Mental Health 18-month collaborative study that compared medications to psychotherapy for the treatment of depression. This multi-site study compared imipramine,

a common antidepressant, to several different types of therapy. Antonuccio and his associates point out that the study is often mistakenly cited as evidence of the superiority of medications over therapy. While clinicians reported that imipramine did slightly better than therapy, the patients reported that the therapy was better. Furthermore, even for the subgroups of *severely depressed patients* the drugs were no better on any measures; most interestingly at the 18 month follow-up, Antonuccio and his associates point out that "Al-though the differences were not significant, the psychotherapies outperformed imipramine on almost every measure." On the other hand the patients taking Imipramine "were the most likely to seek treatment during the follow-up period, had the highest probability of relapse, and had the fewest weeks of minimal or no symptoms."

### **Hollywood Distorts the Truth**

Like many adaptations of true stories, when it comes to treating mental illness, no one will be surprised to learn that Hollywood has altered the truth. When Ron Howard, the director of *A Beautiful Mind* (2001), accepted the Academy Award for Best Picture the smiles on the drug company executives could not have been wider. After all, here was the heroic story of a mathematician struck down by a disease in the prime of his life, and because of the wonders of modern medication was able to continue on with his work and even receive a Noble Prize. In the movie version of John Nash's life when he goes up to receive his award he leans over and says, "I take the newer medications. They don't cure me but they help." However, Sylvia Nasar, the author of the book, made it clear that John Nash stopped taking medications in the early 1970's. The screenwriters, who must have been aware of the truth, apparently made a conscious decision to alter the story. It is unclear why they did this but I imagine that they thought Nash's story was an exception and did not want to be sending a false message to millions of people diagnosed with schizophrenia. How valid is the message that the only way people diagnosed with schizophrenia can recover is with medications?

Leaving anecdotes aside let us turn to the research. Robert Whitaker is an independent reporter who has written numerous features for various newspapers such as the *Boston Globe*. His recent book, *Mad in America: Bad Science, Bad Medicine and the Enduring Mistreatment of the Mentally Ill*, is about how the American psychiatric profession has treated people diagnosed with schizophrenia. Much of his book focuses on a study conducted by the World Health Organization (WHO), which examined the outcomes for people diagnosed with

schizophrenia throughout the world and compared the outcomes between poor countries such as Nigeria, India, and Colombia to the outcomes in the United States and four other developed countries. Surprisingly, on just about every measure, schizophrenics did better in the poor countries than they did in the rich countries. For instance, compared to the patients in the rich countries, the patients in the poor countries were less likely to become chronically sick; they were more likely to be fully recovered and faring well in society; and they were less likely to relapse. Why countries like Nigeria have a better success rate in treating people diagnosed with schizophrenia than countries like the United States is unclear, but it could be due to the fact that psychiatrists in the rich countries prescribe drugs much more freely than their colleagues in the poorer countries. As Whitaker points out, only 16% of the patients in the poor countries were maintained on neuroleptics, while in the rich countries 61% of the patients were kept on medications. The problem is that even if a patient recovers from schizophrenia, it is unlikely that the drugs will ever give up their hold so that once a patient starts down the medication road there is little hope, for any kind of recovery. The WHO study is not the only research documenting the problem. Whitaker also discusses a study by Courtenay Harding analyzing the long-term outcomes of chronic schizophrenics. In a group of chronic, "back ward" schizophrenics released from the Vermont State hospital Harding found that one third of them had recovered, and furthermore, that *all* these recovered patients had weaned themselves from neuroleptic medications.

Thus, the true story of John Nash's life is not an isolated case study, but is actually an excellent example of the research that Whitaker cites, documenting that people diagnosed with schizophrenia can recover without medications. On the other hand, Nash's fictitious story is more in line with the myth-propagated by American Psychiatry-that Schizophrenics need to stay on medication all their lives. It should not go unnoticed that the American Psychiatry Association would have had a very difficult time explaining John Nash's true story to the American public.

In the last chapter of his book Whitaker discusses one way to remedy the situation: "Stop telling those diagnosed with schizophrenia that they suffer from too much dopamine or serotonin activity and that the drugs put these chemical back into "balance." That whole spiel is a form of medical fraud, and it is impossible to imagine any other group of patients-ill, say, with cancer or cardiovascular disease-being deceived in this way." Evidently, the experts at the NIMH do not agree with Whitaker's advice.

The editors of the journal *Lancet* recently posed the following question, "Just how tainted has medicine become?" Their answer: "Heavily, and damagingly so. A more important question arises: do those doctors who support this culture for the best of intentions-e.g., to undertake important *research* that would otherwise remain unfunded-have the courage to oppose practices that bring the whole of medicine into disrepute." Not surprisingly in the previous paragraph the editors documented a case where the editor of the British *Journal of Psychiatry* was recently questioned about receiving money from a drug company while simultaneously publishing a favorable review of one of its drugs.

### Is Science for Sale?

The excesses of modern day psychiatry became all too obvious in November 2000, when David Healy, the author of numerous books on the history of psychiatry, including *The Antidepressant Era*, delivered a lecture at the University of Toronto. Besides being a historian, Healy is also a psychopharmacologist, and had been offered a job as a psychiatry professor and Head of the Mood and Anxiety Disorders Program at the University of Toronto's Center for Addiction and Mental Health (CAMH). He was in the process of finding a house in Toronto when he delivered a lecture on the history of psychopharmacology at CAMH-his soon to be work place. But during the talk Healy did the unthinkable; he mentioned that Prozac might make some people more likely to commit suicide. Not surprisingly, Healy's job offer was rescinded within the week. Yet Healy did exactly what scientists are supposed to do: he had a hypothesis, he tested it, and he presented the results in an academic setting--and he lose his job. The CAMH administrators stated that rescinding the job offer had nothing to do with the fact that CAMH had recently received a gift of \$1.5 million dollars from Eli Lilly, the manufacturer of Prozac. As the Healy affair demonstrates, no one should underestimate the extent to which pharmaceutical companies have made inroads into academia. During the next year or two the American medical community will face a test in how it handles. not only the fact that randomized clinical trials pointing to a link between Prozac and suicide have come to light, but also that this evidence has been around for years.

While the issue of suicide and Prozac raises serious concerns about the integrity of American Science-even major medical journals such as *Lancet* and *The British Medical Journal* refused to publish papers by Healy suggesting that there might be a problem with Prozac, while the same journals had no problem publishing papers by pharmaceutical companies downplaying prob-

lems with Prozac-there is no doubt that the drug companies are superb at taking results and spinning them into whatever message they desire. Even professional skeptics get taken in. For example, Stephen Barrett's website, *Quackwatch. coin*, is a well-known source for information on alternative medicine marketing strategies. Acupuncture, hair analysis, and questionable treatments for cancer all come into the range of Barrett's radar, and he pounces, and rightly so, on companies selling products based on testimonials and not randomized clinical trials. If you didn't use a placebo Stephen Barrett will expose you. Yet, if you are a psychiatrist you are safe. Barrett, a psychiatrist himself, seems to have one standard for alternative, medicine and another standard for his own profession.

Nowhere on Barrett's site will you find a discussion about the "dirty little secret," or that many studies submitted to the FDA for getting psychotropic drugs approved simply bypass the complications caused by the placebo effect. If Barrett ever analyzed psychotropic drugs with the same lens he uses to analyze alternative medicines the SSRIs would never meet his standards. For instance, the four studies that formed the basis for the FDA's approval of Prozac included a placebo wash-out period, where all the patients in the study were put on placebo for a week and anyone who got better was excluded from the study. Yet Barrett, and many other skeptics, are more concerned with the latest hair analysis scam instead of studies showing that Prozac barely beats placebo, or studies showing that Prozac makes some people more susceptible to suicidal thoughts.

### The National Tragedy

The money flowing forth from pharmaceutical companies combined with the complete abandonment of reason and logic from the halls of academic psychiatry has wreaked havoc on children. With every new epidemiological study examining the use of these drugs in children the public shakes its head in disbelief; yet, the prescriptions keep right on coming. In January of 2000, one study found a 300% increase in the use of Ritalin in preschoolers. The same study mentioned that in 1994, doctors wrote 3000 prescriptions For Prozac for children less than 12 months old (*JAMA*, 283, p. 1025). Yet, the typical response from those writing the prescriptions is to call for more money to investigate the safety and efficacy of these drugs; meanwhile, the travesty goes on, and on, and on. Prozac was recently approved for children aged 7-17 based on two studies by a group of psychiatry researchers from the University of Texas Southwestern Medical Center in Dallas, with Graham Emslie as the lead author. The first study was funded by NIMH, but the second study was funded by

Eli Lilly. Curious readers should know that all eight authors of the second study, including Dr. Emslie, were either paid consultants or employees of Eli Lilly. In the *second* Ernsly study the authors report that 65% of the Prozac treated children met the prospectively defined response criteria but 53% of the placebo group also met the same criteria, which was not statistically significant. Regardless of these findings, the authors did find that on some criteria the Prozac group did better than the placebo group, yet on the single patient-rated scale there was no significant difference between the two groups. Conflicts of interest do not invalidate studies but in the case of Prozac for children the science is hardly overwhelming.

In the summer of 2003, the FDA and MI-IRA (the British equivalent of the FDA) issued statements that Paxil, another common antidepressant, should not be used in children because of the risk of suicide. The warnings were based on unpublished studies, which finally came to light, apparently showing that children taking Paxil were more likely than those on placebo to think about suicide. Yet, NIMH-the most important mental health organization in the world-had no official statement concerning this problem. Two months later Wyeth Pharmaceuticals issued a similar warning about their drug Effexor-and still no word from NIMH. It is not NIMH's job to approve these drugs, or to pull them off the market, but it is their job to investigate the effects of these drugs on behavior. While the NIMH scientists deserve much of the credit for the wide-spread cultural acceptance of dealing with children's behavior by medicating them, apparently they feel that increased thoughts of suicide among children taking these medications does not fall under their jurisdiction. It would be nice if NIMH could take the moral high road and at least acknowledge the problem, but heightened public awareness might lead to some difficult questions for NIMH. For instance: Why was NIMH so willing to assist in the promotion of these medications, but so reluctant to heed the warning signs, or to conduct the necessary studies?

One unanswered question swirling around this affair is the history of the unpublished studies implicating Paxil. It is still unclear when these studies were conducted, how long they sat in the file drawers at the FDA, who had access to these studies, why they were ignored at first, and who exerted the pressure on the FDA to finally take action. Graham Emslie, the lead scientist for the studies responsible for getting Prozac approved for children, was also an author on four of the unpublished Paxil studies. Emslie was quoted in *The New York Times* as saying that "Some of these studies were finished a couple of years ago but negative trials

tend not to get published." However, he refused to comment on the specifics because of signed secrecy contracts with the pharmaceutical companies (NYT, 8/17/03). It is certainly Dr. Emslie's right to sign a secrecy contract with a drug company, but when a professor cannot answer questions about his own work-done at a public university-can this professor still be considered an unbiased scientist in search of the truth? Yet, these conflicts did not stop Dr. Emslie from chairing *The Texas Consensus Conference Panel on Medication Treatment of Childhood Major Depressive Disorder*, which received funding from both NIMH and the Texas Legislature. The Conference, held in 1998, three-years before Prozac was approved for children by the FDA, provided a forum for the pharmaceutical companies to get a quasi-official statement promoting the use of their medications, prior to FDA approval; and, in what can only be considered a phenomenal marketing coup for the pharmaceutical companies, the chairman of the conference was apparently on their payroll.

The Texas Conference set the stage for medicating thousands of children, yet nowhere in the Consensus Conference Report-which has a tremendous influence on clinicians-is there any mention of Dr. Emslie's conflicts of interest. In retrospect, the Texas Conference was a triumph for the marketing departments but a sad day for science. At the very least we are left with a situation where the doctor responsible for getting Prozac approved for children has now essentially removed himself from any kind of legitimate national discussion about these issues because of such an enormous conflict of interest.

This latest chapter in the history of psychiatry goes far beyond the specific issue of whether there is a link between SSRIs and suicide. The entire affair calls into question the behavior of the profession over the past decade; years from now historians will be writing about how during the past decade academic psychiatry sold itself to the pharmaceutical companies. Ghostwritten papers, company written papers, editors refusing to publish articles critical of the "science", NIMH press releases announcing the latest new research findings while ignoring contrary data, and psychiatrists calling their critics "quacks" all point to major problems with the profession-not just a minor glitch. Nowadays, it is hard to even know how to approach scientific papers in the major psychiatry journals. As just one example, an internal document, prepared by SmithKline Beecham, providing guidance on how to manage the results of two clinical trials examining the efficacy of Paxil, stated that the findings were "insufficiently robust" to support an application to regulatory authorities to get Paxil

approved for children. According to the document, Paxil was no better than placebo but that, "It would be commercially unacceptable to include a statement that efficacy had not been demonstrated, as this would undermine the profile of Paroxetine (Paxil)." Yet, in *The Journal of American Academy of Child and Adolescent Psychiatry*, a paper about the very same study, authored by some of the most prominent academicians from the field of child psychiatry, including Dr. Emslie, stated that Paxil is, "generally well tolerated and effective for major depression in adolescents."

I hesitate to even mention a case study, because it is almost too easy for critics to jump all over me for presenting a one-sided story but these types of scenarios are becoming so common that I imagine many people know at least one similar story. While anecdotes cannot take the place of science, it seems fairly obvious that with all the money flowing out of pharmaceutical coffers that negative case studies will not get the airing they deserve. An occupational therapist friend, who is skeptical of my view on these drugs, called me with this one because even she was shocked by this scenario. My friend was in a meeting to discuss a four-year old girl named Caroline; also present were the school nurse, the teacher, and Caroline's mother when the nurse pointed out that Caroline was taking the same dose of Prozac that an adult would take and that the mother might want to check with her doctor. "No-No" said the mother-she wouldn't think of questioning her doctor. After all, the Ritalin he prescribed for her two older children was working wonderfully.

What are we to make of a mother with three children, all of whom are taking psychotropic drugs-two on Ritalin and one on Prozac? Are we dealing with a gene that has affected all her offspring or a medical profession that has some serious issues to work out? Fixing the current psychotropic drug problem is daunting because there are numerous reasons, such as questionable ethics, political pandering, health care financing, and persistent advertising, that have all contributed to the widespread use of psychotropic medications. However, while all these issues are important to acknowledge they are not the central problem; the heart of the matter is the faulty science that forms the basis of modern day psychiatry.

Psychiatrists like to label, and they typically label their critics as conservative ideologues, who want everyone to pull themselves up by the boot straps; dreamy liberals, who believe that all the problems of the world can be cured with a hug; hustlers, who want to sell books; or flat-earthers, who are ignorant about science. My favorite tactic is the reflex-like reply by psychia-

trists to label anyone who questions their theories as a "scientologist." It is an easy way to dismiss critics as just sort of "out there," and an easy excuse for the lack of an intelligent reply, and, not surprisingly, a tactic drummed up by the pharmaceutical companies. Earlier I mentioned that critical analysis of the psychiatric literature requires wading thru the excess wordage, stories, analogies, speculations, and labels, and looking for facts. I urge all readers of any responses to this article to approach them with a critical eye and ask: "Where is the evidence that mental illnesses are caused by biological deficits?" Of course people suffer, and people sometimes need help, but to say that emotional distress is due to an underlying biological defect that can be "cured" by taking a pill is a grand oversimplification of human nature.

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#### SUGGESTED FURTHER READINGS

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