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features

A Pill for Every Mood: An Interview with Christopher Lane

In Oren Rudavsky's recent film, *The Treatment*, a wealthy Manhattan widow is baffled that a schoolteacher might be so anxious about speaking in public that he can't eat and suffers from stomach cramps and diarrhea. After all, surely he must speak in front of others every day. His reply is, in effect -- well, yes, but only in front of students. The schoolteacher probably wouldn't take any comfort from the popularity of his fear: According to one notorious statistic, Americans are more afraid of public speaking than of death.

I've begun with this example it points up a real enigma about our minds: How can a purely cultural experience such as public speaking translate into brain chemistry? After all, neither term in "public speaking" is straightforward. How big a group counts as "public"? Are they friends, colleagues, strangers, or a mix? Am I drunk or sober? Am I reading a prepared speech? fielding questions? participating in a judicial, civic, or religious ritual? How is it that our serotonin levels are able to make such finegrained judgments? Even if one focuses just on physical responses -- mild sweat, an elevated heart rate -- people may well attribute different meanings to those responses. (I was scared / I was in the zone / I was angry.) Despite these difficulties, some psychiatrists have proposed that "public speaking phobia" ought to receive its own diagnostic classification.

By focusing on the intersection between culture and chemistry, Christopher Lane's wonderful new book, *Shyness: How Normal Behavior Became a Sickness* (Yale UP, 2007) shows why we ought to be more skeptical of the rush to medicate "social phobias" -- *Psychology Today*'s "disorder of the decade"! -- with powerful drugs, especially in children and adolescents. Despite the alleged precision of recent editions of the *Diagnostic and Statistical Manual of Mental Disorders*, "social phobia, the most enigmatic and poorly-defined anxiety disorder, became *the* psychosocial problem of our age." It is as if the very vagueness of the definition allows its meaning

to expand, until the "unavoidable conclusion is that we've narrowed healthy behavior so dramatically that our quirks and eccentricities -- the *normal* emotional range of adolescence and adulthood -- have become problems we fear and expect drugs to fix." What's worse, he suggests, the drugs we expect to fix our problems all too frequently fail to do so, and in many cases actually make matters worse.

Shyness's argument has three main parts: first, Lane exposes the shaky conceptual foundations of such modern diagnoses as "social anxiety disorder," which simultaneously purport to replace vague psychoanalytic categories ("anxiety neurosis") with more precise, evidence-based ones -- and yet which cover a spectrum of behaviors ranging from discomfort over speaking in public to a genuinely crippling aversion to others. This portion of the book, drawing as it does on archival documents from the American Psychiatric Association as well as interviews and published research, ought to worry anyone hoping to find coherence or rigor in the diagnosis of mental illness. The second part of the book focuses on the tight fit between the turn to neuropsychiatric models of diagnosis and the marketing demands of big pharma. The side effects of these powerful drugs make a mockery of the word "selective" in selective serotonin-reuptake inhibitor (SSRI). As we are beginning to understand more fully, SSRIs are a decidedly mixed blessing, and their consumer-orientated marketing is unseemly at best. The final part of the book looks at novels and movies resistant to the dominant psychopharmacological perspective in our culture.

Throughout the book, Lane suggests that the conceptual problems of the *DSM* arise in part from its weird eagerness to break decisively with Freud. Lane has vividly reconstructed the decision-making process of the *DSM-III* in the 1970s, showing how scoring points over rival theoretical schools frequently trumped logic or consistency. Insisting on the biochemical nature of all mental suffering leads psychiatrists to turn away from the vicissitudes of the mind -- what Lane calls "the strange, unusual turns of consciousness, themselves in thrall to vivid memories, irrational fantasies, persistent associations, and sometimes-inexplicable impulses." By reducing the complexity of these "turns" into "disorders" -- no matter how "multiaxial" -- modern psychiatry seems to drain the life out of the mind. *Shyness* is passionately and compellingly argued, in clear prose that is in turn scathing, hilarious, and sympathetic.

In the interview below, Lane discusses the origins of the book, the implications of shifting from a "reaction-based" to a "disorder-based" model of diagnosis, the differences between psychoanalysis and neuropsychiatry, and the problem of emotional blunting.

Christopher Lane is the author of three additional books: *Hatred and Civility: The Antisocial Life in Victorian England* (Columbia UP, 2003); *The Burdens of Intimacy: Psychoanalysis and Victorian Masculinity* (U of Chicago P, 1998); and *The Ruling Passion: British Colonial Allegory and the Paradox of Homosexual Desire* (Duke UP, 1995).He also is the editor of *The Psychoanalysis of Race* (Columbia UP, 1998) and, with Tim Dean, co-editor of *Homosexuality and Psychoanalysis* (U of Chicago P, 2001). He is currently the Herman and Beulah Pearce Miller Research Professor at Northwestern University. (In the interests of full disclosure, I should say that Chris Lane directed my doctoral dissertation, and that I have an essay in *Homosexuality and Psychoanalysis*.)

Let's begin with your most serious claims. You argue that social anxiety disorder is conceptually overbroad and overdiagnosed, while Paxil and the other drugs prescribed for this disorder are ineffective, if not outright dangerous, for many patients. But you also argue that the role of serotonin in mental illness is vastly overstated, and in fact has no direct causal relation. At the risk of sounding naïve, how did the mythology around serotonin take hold?

I think a lot of the mythology about low serotonin sprang up when neuropsychiatrists in the 1970s and '80s championed biological explanations for mental illness. Their goal was really to help us think that such distress stemmed from the brain rather than the mind. In June 1976, for example, Robert Spitzer, then chair of the task force overseeing major revisions to *DSM-III* (the third edition of the *Diagnostic and Statistical Manual of Mental Disorders*), tried to get approved a very bold claim: “A mental (psychiatric) disorder is a medical

disorder." He wasn't successful, because so many pointed out the enormous influence of psychological and social factors in shaping mental distress. But Spitzer's argument has since gained momentum because it's appealingly straightforward and has so much financial support. If we can say that the cause of distress is a "chemical imbalance," then the solution points logically to drug treatments rather than therapists focusing on the mind by, say, encouraging a shift in perception.

But just one of many problems with the "chemical imbalance" argument is that it oversimplifies so much. No one can establish conclusively what a chemical *balance* is because it varies so much from one person to the next and, indeed, from one day to the next.

Another thesis you advance is that modern psychiatry has essentially "rebooted," deliberately going back to a kind of pre-Freudian indifference to subjective experience. Has the purging of subjectivity yielded significant fruit in treating mental illnesses?

The "rebooting" of modern psychiatry stemmed, I think, from a widespread effort to eliminate all trace of psychology from American psychiatry. This was not without serious intellectual and clinical consequences. Some of the psychiatrists responsible for this wholesale debunking of Freud, in particular, later twigged that they had thrown out the baby (in this case, interest in consciousness) with the bathwater. So in some cases they needed to start again almost from scratch: They had to find new ways to discuss perception that wouldn't at the same time sound Freudian.

In my opinion, the whole exercise was immensely self-defeating for psychiatry. Experts pointed out at the time that it was rather arrogant to believe that one could simply trash 70 years of carefully argued analysis, itself tied to clinical experience, but few at the time were willing to listen. They were on a mission to turn psychiatry into a study of the brain, and thus a hard-boiled science. It's unfortunate, then, that Ted Millon, one of their consultants, came forward in 2005 and told the *New Yorker,* "There was very little systematic research [informing *DSM-III*], and much of the research that existed was really a hodgepodge -- scattered, inconsistent, ambiguous."

I'd say the "purging of subjectivity" from discussions about anxiety ended up impoverishing what we do know about anxiety, and have known for a very long time, which is that it *crosses* biology and perception, rather than being reducible to one or the other. Put another way, while the effects of anxiety are obviously biological -- a racing heart, sweaty palms, shortness of breath, and so on -- what *triggers* those effects is necessarily tied to consciousness.

What does it matter if we call something a "disorder" as opposed to a "neurosis"?

This partly connects with your opening question. To call something a "disorder" is to say that the malady is biological, even genetic. If anxiety were still known as a "neurosis," by contrast, then doctors and psychiatrists would be signaling that the problem is chiefly one of psychological conflict and should be addressed accordingly.

Until 1980, the language in the *DSM* was very much about calling mental distress "reactive" and situational. And, actually, this included even the psychoses, which were known then as "paranoid reactions" and "schizophrenic reactions." This makes perfect sense for anxiety, as well: people may be anxious about speaking in public, but rarely or never feel so on other occasions. Yet the word "reaction" was deleted from all later editions of the *DSM,* in a way that totally changed the way we think about illness. Instead of being able to say, "you had an anxious reaction" to a particular event, we moved almost overnight into implying, "You have social phobia" or even, "You are socially phobic." That suggests that the conflict is life-long and essentially beyond the patient's control. So whether you agree or not with the outcome, it's definitely a serious shift in approach that needs acknowledging.

By coincidence, I happened to teach *Mrs. Dalloway* this past week, and was struck by the way Virginia

Woolf's critique of Sir William Bradshaw's "Proportion" and its cruel sister, "Conversion," anticipates many of the concerns you raise here about how quickly good intentions tip into a ferocious coercion: "If you won't take these pills..."

Yes, that's a powerful moment in the novel. And, sadly, it's the very effort on Dr. Bradshaw's part to *make* Septimus rest that intensifies the latter's sense of being persecuted. But, ironically, Dr. Bradshaw firmly believes he's acting in Septimus's best interests. And that would remain so today: everyone's fundamentally trying to act in the best interests of the people they treat.

But that doesn't mean the treatments aren't occasionally harmful, because full of so many side effects. Nor does it mean that every diagnosis is accurate and every remedy necessary or appropriate.

What concerns me most is that the alleged cure for social anxiety disorder is often a great deal worse than the disease. Honestly, what good does it do people who dislike speaking in public if they take an anti-anxiety pill such as Paxil and, as the drug maker forewarns, one of the drug's side effects is... anxiety?

You're a professor of English, with three books out on Victorian and Edwardian literature, as well as edited collections on psychoanalytic theory. What drove you to this more explicitly medical project?

My last book, *Hatred and Civility: The Antisocial Life in Victorian England*, tried to cross disciplines by putting literature in dialogue with psychology and psychiatry. I was (and remain) fascinated by how differently the Victorians thought about antisocial behavior. Above all, I wanted to find out what had happened to misanthropes in the 20th and 21st centuries. When I raised this issue with one of the leading psychiatrists I interviewed for *Shyness*, he responded, a bit matter-of-factly, "I suppose they all got medicated." That response revealed a certain truth, I think, but also a worrying one. We tend to think that we can medicate away extreme emotions and states because our tolerance for them, as a culture, almost certainly has diminished. I wanted to find out why.

Is there a connection between *Hatred and Civility* -- which in part defends misanthropes -- and *Shyness*?

One of the threads uniting the two books, I'd say, is that individuals aren't adequately represented by the cultural categories and diagnostic terms that try to sum them up. Shyness and anxiety are of course very complex terms that have different shades of meaning from one culture and generation to the next. The *DSM* in my opinion is quite incapable of capturing those inflections, because its rule-bound criteria try to slot people into pre-existing diagnostic grooves. Humanity -- and human suffering -- is far more complicated than that.

One of the striking features of *Shyness* is its rich engagement with the APA archives, and its extensive interviews with the framers of DSM-III and DSM-IV, especially Robert Spitzer. How did you convince them to grant you such access?

Well, when I first approached the APA and Robert Spitzer, each of them said that the papers probably had been lost when the APA moved from downtown Washington, D.C., to its present location nearby in Arlington, Virginia. That was worrying for several reasons: the documents are really vital to understanding what happened to American psychiatry in those crucial years.

In the meantime, I managed to track down Mitchell Wilson, author of a wonderful essay on the history of the *DSM*, which quoted several *DSM-III* memos. I was intrigued. Spitzer apparently had given him copies of the papers when he'd pegged Wilson to be the man who would write his biography. But things hadn't worked out that way. Mitchell kindly said that I could copy his papers if I flew out to Berkeley, so I got on a plane. When I returned to Chicago and began reading the memos, I contacted the APA and Spitzer again, saying I had many of the papers, but that I really wanted the book to be exhaustive and complete. At that point, the papers turned up -- it was great to know they hadn't been lost -- and the APA's librarian kindly told me I could access them because the statute of limitations on them had expired. Shortly after that, Spitzer graciously invited me to his house just north of New York City, where I interviewed him intensively one afternoon. It was really a most pleasurable

afternoon -- very focused on events in psychiatry that had occurred almost three decades earlier, but Spitzer was incredibly sharp and had amazing recollection. I tried to capture what he relayed to me that afternoon.

At the same time, a significant portion of your argument -- especially the part about the marketing of Paxil -- has been "hiding in plain sight." Why haven't we faced these questions openly before?

Yes, that's partly true, though several pieces of fine investigative journalism have certainly helped document the Paxil story: Brendan Koerner wrote a superb article about it for *Mother Jones* ("Disorders Made to Order"), July/August 2002; and Beth Hawkins followed up a couple of months later, in *City Pages,* with "Paxil Is Forever." Both focused on the marketing of social anxiety disorder as a prelude to representing Paxil as its remedy, but neither had access, I believe, to the poor early trials of Paxil, and doubtless hadn't space to write about how the marketing interfaced so cleanly with the *DSM* revisions.

Overall, though, I think there are several explanations for our slowness to piece together these complex stories: First, the drug companies are quite canny in how they release new information about side effects. They add the details to healthcare providers over the course of several weeks, even months, so the revelations don't come all at once, as a shock. I guess we just learn to say, "Oh, okay, now they've put in bold that one side effect of Paxil is renal failure." Next week it's platelet aggregation problems, and so on.

Plus, despite what the drug companies say, it remains incredibly difficult for the general public to find out all it needs to about the drugs themselves because, as the *New York Times* reported on May 31, 2005, Eliot Spitzer, then Attorney General of New York, may have succeeded in getting the drug companies to settle over his class-action lawsuit, but, as the article's title put it, "Despite Vow, Drug Makers Still Withhold Data."

What about the DSM-III task force itself? Did it face any self-imposed conceptual dilemmas?

I'd say so, yeah, because Spitzer selected only "kindred spirits" to join him -- friends and colleagues whom he knew shared his interest in "criteria-based diagnoses." There were two consequences to this that are worth noting. First, the DSM-III task force met for *four years* before it even occurred to a participant that their perspective might be a bit, you know, slanted toward neuropsychiatry and thus a fraction unlikely to factor in other approaches. Second, most of those involved recall their discussions are stringent and completely fair. Spitzer actually said in another interview some years ago, "We didn't want anybody to feel that their diagnostic concepts were being excluded." Unfortunately, that generosity extended only to those who'd already been invited to participate. It rather magnificently overlooked those who'd been shut out of the process for four years. So there were very few checks and balances.

My "favorite" -- if that's the right word -- potential diagnosis that you reveal is "chronic undifferentiated unhappiness disorder," where "kvetching" and saying "Oy vay, don't ask" were proposed as signs of sickness. Were there particular discoveries that stand out to you as "favorites"?

That was certainly one of them! Another was that *DSM-II* actually included a code (318.00) for those who should be *diagnosed* as having "No Mental Disorder." Imagine the illogic! Eavesdropping on the correspondence between Spitzer and Don Klein, his sometime ally and occasional nemesis at Columbia, was also quite a revelation. They would fire back-and-forth these extremely aggressive memos, trading diagnostic barbs as a way of insulting each other under the guise of completing their work. So, for example, Spitzer wrote at one particularly tense moment over the criteria for avoidant personality disorder, "Does the reference to 'hypersensitivity to rejection' get too close to Hysteroid Dysphoria for your personal comfort?" That's got to be a classic!

You have a great deal of fun with the psychiatrists for their penchant for wildly ahistorical diagnoses, such as Samson's antisocial personality disorder. How does this differ from Freud's use of figures such as Oedipus or Moses, or, on a less rarefied plane, Jones's interpretation of Hamlet?

I'd say there are major differences and, alas, painful similarities here. First the differences: The literal-mindedness of many neuropsychiatrists today really doesn't equate with the willingness of psychoanalysts and literary scholars to cite Oedipus, Moses, or Hamlet as *analogies*, to form metaphorical comparisons. When neuropsychiatrists try to diagnose a Biblical figure like Samson as suffering from ASPD, by contrast, they're neither joking nor have much sense of irony about their assertions: they're trying to shore up the prevalence of a disorder by saying it recedes far into antiquity, though people just didn't have the tools to recognize it then.

But there's definitely some similarity, too. There's still a tendency among some psychoanalysts and psychoanalytic literary critics to treat fictional characters as if they were patients awaiting a diagnosis. I don't personally find that approach persuasive or appealing, but I recognize it's been a strong current of the complex, varied history of psychoanalysis, going back through Ernest Jones's work to Freud's own. After all, Freud's own essays on literary criticism are very much about asserting the validity of his theories through fiction and myth. Nowadays, by contrast, psychoanalytic critics tend to be more interested in signaling how literature fails to sustain meaningful diagnoses of characters, not least because that approach is in the end far more psychoanalytic (it's truer to a theory of the unconscious).

You sometimes seem to argue that the faux-medicalization of Freudianism in the United States contained within it the seeds of its own destruction at the hands of someone like Spitzer. Is that a fair characterization?

Partly so, yes, but psychoanalysts at the time also were on the horns of a real dilemma. To some, it must have seemed logical that the narrower the divide between them and neuropsychiatrists, the closer they would be to resolving a standoff over diagnosis and rising health costs. They could, in theory at least, present a united front before the HMOs and insurance companies. But these, in turn, were understandably concerned about costs and looked for the most efficient way of cutting them. They latched onto psychoanalysis as a culprit and made it an unfair target, I'd say, because in-patient hospital costs are always *the* leading factor in healthcare costs. Beyond this, the HMOs and insurance companies weren't interested in psychoanalysts and neuropsychiatrists getting along; they soon saw that they'd get furthest, in terms of cuts, if they kept the two sides at odds, because that way the battling psychiatrists would be more likely to compromise independently.

And that's in effect what happened. The neuropsychiatrists knew that their lists of criteria, statistics, numbered axes, and commitment to rapid empirical results put them at an apparent advantage relative to psychoanalysts who refused to play ball. Among psychoanalysts I have sympathy for the pragmatists *and* the idealists. It's easy to heroize the idealists for refusing to compromise, but in the end I'm not sure their approach would have been more successful.

A subtle moral claim emerges near the end of the book, when you cite Satel and Sommer's point that "suffering can be edifying," and note that this view is "anathema to the psychiatric literature." If we really could medicate away suffering -- if the pharmacological dream really could somehow be realized -- why wouldn't that be ok?

This complex issue doubtless belongs in the realm of ethics rather than morals. I'm trying to talk about something that's very easy to misinterpret, so I should also try to say what I'm not arguing. I'm talking here about the experience and the confidence that are gained from working through a problem. With a discussion very particularly about anxiety, I'm also arguing -- as I do throughout the book -- that medication can be an ersatz solution to the problem, because it frequently does not address the underlying causes of anxiety.

The issue, for me, is mostly that psychiatric medication for mild disorders can't deliver on its promise. For starters, with its litany of side effects it can in fact create *more* anxiety and suffering than less. It also can lull us into the belief that there's a quick-fix solution to our problems, when frequently, alas, there is not. When the drug companies hold out the promise of so many treatment options, they're doing much more than hinting that we can end suffering, a worthy and very understandable goal. They're also holding up an ideal of contentment, even bliss, that's unrealistic, and that can in turn create yet more distress, because we can't possibly measure up to

such perfection. As one *New Yorker* cartoon put it, in an exchange between a doctor and patient that I reproduced in the book, "I think the dosage needs adjusting. I'm not nearly as happy as the people in the ads." I think that's so true.

The APA was scheduled to start laying the groundwork for *DSM-V* this year. Have you heard anything about those plans?

I gather from Spitzer and from the writings of key figures (for instance, Ronald Kessler at Harvard) that the trend of including more-and-more mild problems in the *DSM* is almost certain to continue. There's talk of including apathy disorder in *DSM-V*. There's also a lot of momentum behind including, as disorders, overuse of the Internet and excessive shopping. Finally, there's a strong desire to formalize "Premenstrual Dysphoric Disorder," which right now exists only in the appendix to *DSM-IV,* though, even so, it strangely was still given that all-important diagnostic code to authorize drug treatment. What Eli Lilly did, incidentally, after the code was created, was simply to repackage Prozac as Sarafem. As one of the people involved in the new marketing campaign said quite openly, Lilly took the green-and-yellow pills that make Prozac so distinctive and decided they should henceforth be lavender, while "promoted with images of sunflowers and smart women." I think he caught the message of that campaign very well.

At various points in the book, you lament the potential passing of certain emotions/attitudes. Introspection is one; love is another that you touch on repeatedly. Why shouldn't we simply see these deep, turbulent emotions as atavistic?

Fortunately, introspection hasn't completely disappeared! Nor, indeed, has love. The major issue here is "emotional blunting," an increasingly recognized and well-described side effect of antidepressant and anti-anxiety medication, whereby people can feel as if they're living in a fog, largely numb to their reality. That's a very undesirable place to be; it's another example of the suffering I was alluding to above. I can only speak personally about such things, but I'd certainly prefer the emotion to the risk of its being distorted by medicine.