

# In the Grip of the Python: Conflicts at the University-Industry Interface

David Healy

North Wales Dept of Psychological Medicine, Bangor, United Kingdom

---

**Keywords:** hazards of drug treatment, ghostwriting, academic freedom.

**ABSTRACT:** *When the University of Toronto withdrew a contract it held with me in December 2000, it initiated a sequence of events that led to a public letter to the University from senior figures in the world psychopharmacology community protesting against the infringement of academic freedom involved and a first ever legal action, undertaken by this author, seeking redress for a violation of academic freedom. The issues of academic freedom surrounding this case have been intertwined with a debate about the possibility that the selective serotonin reuptake inhibitor (SSRI) group of antidepressants have the potential to trigger suicidality in a subgroup of patients. Whether the SSRIs do trigger suicidality or not, exploration of this issue has given rise to a number of worrying sets of observations. First, in my view, there is evidence that pharmaceutical companies have miscoded raw data on suicidal acts and suicidal ideation. Second, this author also maintains that there is a growing body of examples of ghostwriting of articles in the therapeutics domain. Many of the tensions evident in this case, therefore, can be linked to company abilities to keep clinical trial data out of the public domain – this is the point at which the pharmaceutical python gets a grip on academia.*

## Introduction

Many latent conflicts of interest in the relationship between academia and industry have become manifest in recent years, following the Olivieri affair.<sup>1</sup> Some of the issues involved were outlined by Marcia Angell in an editorial: 'Is Academic Medicine for Sale?'<sup>2</sup> Others were hinted at in an article called 'Dancing with the Porcupine'.<sup>3</sup> The title imagery of this latter article conveys the complexities of relations between academia and industry in a powerfully suggestive rather than an explicit form.

---

**Address for correspondence:** Dr. David Healy, North Wales Dept of Psychological Medicine, University of Wales College of Medicine, Hergest Unit, Bangor LL57 2PW, United Kingdom; Healy\_Hergest@compuserve.com (email).

1353-3452 © 2002 Opragen Publications, POB 54, Guildford GU1 2YF, UK. <http://www.opragen.co.uk>

D. Healy

David Weatherall, the Emeritus Regius Professor of Medicine at Oxford, in a recent meeting with Nancy Olivieri, Brenda Gallie, Peter Durie, John Dick, Helen Chang and Allison Hutchins, the Toronto group at the heart of the Olivieri affair, offered a more explicit image. In a trip some years previously to Malaya, Weatherall had seen a stage performance of a woman dancing with a Python. This had a peculiar fascination. Part of the dancer's skill lay in making sure the Python's tail could not hook on to any object, as that would put it in a situation to squeeze the dancer to death.

### **SSRIs and Suicide**

The first link between the selective serotonin reuptake inhibitors (SSRIs) and suicide was noted in 1990 with an article by Teicher, Glod and Cole.<sup>4</sup> Following this article I am told by Teicher that he received a call from his head of department asking him to explain what was involved in what had quickly become a 'Prozac controversy'. It seems that the department head had received a phone call indicating that other data might not support the Teicher position and that it might be irresponsible to have the topic aired. But, on being more fully briefed, the head indicated that he would not block further efforts to publicise the problems.

Before seeing the Teicher paper, I had treated two men who had become suicidal within the first weeks of treatment with SSRIs. These cases were presented at meetings and written up in 1991.<sup>5</sup> A subsequent review discussed the field in the light of other clinical data.<sup>6</sup> Early drafts of both articles had gone to Eli Lilly for comment. The company offered me a consultancy that involved no consultation on the issue of Prozac and suicide. I had in addition one meeting with a company lawyer just before the most famous Prozac case, the Wesbecker case took place in 1994.<sup>7</sup> In the meantime, a small series of medico-legal cases came my way. My opinion in all instances was that Prozac had not been involved in injury to the claimants.

In 1997, I was approached by an American legal firm about the case of William Forsyth, a man who had no prior history of mental illness. In the early 1990s, Forsyth had retired to Maui a wealthy man, but he found retirement difficult. There were strains with his wife. He saw a physician who prescribed an antidepressant and tranquilliser, which seemed not to help. Marital counselling appeared to help more. His physician nevertheless suggested trying Prozac. A day after going on Prozac Mr. Forsyth checked himself into a mental hospital much to the astonishment of his physician, his family and others. He appeared not to settle there and after a week discharged himself. The treating team who had initially been surprised at his admission were now concerned at his discharge but unclear what was prompting their anxiety. The night after his discharge, in March 1993, William Forsyth stabbed his wife June 15 times before impaling himself on a set of serrated kitchen knives. The policeman at the scene later said he had never seen so much blood in his life.

Why was an American legal firm approaching me? It appeared that they had great difficulties getting expert witnesses within the US. The reasons touted about, rightly or wrongly, were that many US psychopharmacologists were afraid to get involved in

these cases. Many it seems may have been worried about possibly compromising further research funding for their own work.<sup>8,9</sup>

I consulted colleagues before getting involved. The views that were probably decisive were those of two colleagues within the pharmaceutical industry, one with an interest in the history of psychopharmacology, the other who seemed concerned that the industry should operate to a high ethical standard. Their view was that involvement of this type was something that people more safely did at the end of their careers, but that if the case was sound on clinical grounds I should get involved. Implicit in their formulations was the notion that taking a stand might harm the standing of someone not close to the end of their career. Neither of them however was clear on what this might mean for me. Neither they nor I were aware that a window was closing on the capacity for plaintiffs to pursue cases like the SSRI cases.

Expert witnesses in cases like these, it seems, typically pass through a period of wondering whether they might be assassinated or whether injuries of other sorts might befall them.<sup>10</sup> A range of things happened that might or might not have been related to my involvement. There can be many possibly real, possibly imagined pressures. It was difficult to tell for instance whether invitations from pharmaceutical company field managers to present material on the antidepressants to groups of psychiatrists in the United States were a trap or not. The greatest problem for a clinician probably comes from ordinary clinical practice, which of its nature inevitably leads to some unfortunate outcomes. These outcomes can easily be portrayed as mistakes. To judge by the histories of the Olivieri group, the likelihood of these “mistakes” being scrutinised seems greatly increased and the risk of that scrutiny ending up as material used in Court exerts a very real pressure on the clinical scientist to stay out of or to bow out of issues.

The Forsyth case led on to the Miller case against Pfizer, after Matthew Miller, a 13-year old boy with no prior history of nervous problems committed suicide one week into a course of Zoloft (sertraline), that was given as much for diagnostic as for therapeutic purposes.<sup>8,9</sup> One of Pfizer’s experts suggested that this was a case of autoerotic asphyxiation. The Miller case led to the Tobin case. The plaintiff here was Timothy Tobin, the son-in-law of Don Schell, a man who had had several relatively brief and mild nervous episodes from the mid 1980s through to 1998. In the course of one of these, Schell had been exposed to Prozac, which had caused clear adverse reactions in the early phase of treatment. In early 1998, Schell sought help for a further nervous episode from his primary care physician who, unaware of Mr Schell’s prior adverse reaction, put him on Paxil (paroxetine). Two days later Donald Schell put three bullets through the heads of Tim Tobin’s 9-month old daughter Alyssa and his wife Deborah, as well as his mother in law, Don’s wife, Rita, before killing himself. This led to a court case in Cheyenne Wyoming on May 21<sup>st</sup> 2001.<sup>11</sup>

All the while, my personal contacts with members of the pharmaceutical companies on the ‘other side’ of legal actions that I was involved in remained cordial and indeed at times almost supportive. It was impossible to connect the company people I was meeting with some of the other things that began to happen to me.

## No Right to Present These Data

In late 1999, we conducted a healthy volunteer study aimed at looking at mechanisms of action of antidepressants. There was good evidence that an individual's personality/constitution shapes how he or she will respond to a selective psychotropic drug. This is probably what leads to the 'better than well' responses described with drugs like Prozac that have so shaped public perception of the SSRIs. The corollary is that this is probably also where the risk for suicide may stem from. Given that there have been about 100 million people exposed to SSRIs and that these drugs generate \$10 billion worth of business per year, it is extraordinary that the companies have not done the kinds of study that would enable physicians to direct patients toward the best rather than a proprietary treatment option for the individual patient.<sup>12</sup>

The implications of our study were in fact that anyone going to their primary care physician for an antidepressant stands a 50/50 chance of getting the wrong drug for them.<sup>13</sup> The epidemiology of antidepressant prescribing indicates that no more than 40% of people continue to take SSRIs for longer than a month. This is completely consistent with our results. In the course of this particular study two of our volunteers became suicidal. The qualitative data on what happened to these volunteers was written up rapidly and published in a peer reviewed paper in *Primary Care Psychiatry*.<sup>14</sup>

My first presentation of these results at an academic meeting was at the British Association for Psychopharmacology meeting in July 2000. At this meeting the guest lecturer apparently broke off in the middle of his lecture, which was not on the topic of suicide, and gave the opinion that there was material being presented at this scientific meeting that he didn't agree should be presented. Being warned of this, I expected that this senior researcher would visit the poster presenting my data in the course of the afternoon. He did, and as I remember it, gave the opinion that I had no right to present data like this. Even when it was pointed out that these data were consistent with other data in pharmaceutical company files, he still insisted I had no right to present the data. He said it would ruin my career. Both a witness and I found the conversation disturbing.

A year previously I had been interviewed for a post in the University of Toronto as a Professor in the Mood Disorders Programme. In early 2000, I had accepted this post. It typically takes a year to register medically in the case of a move from the UK to North America or vice versa. While this was proceeding, in August 2000 I was invited to be a speaker on November 30<sup>th</sup> at a symposium called 'Looking Back Looking Ahead' to mark the 75<sup>th</sup> anniversary of the University Department of Psychiatry, the 150<sup>th</sup> anniversary of the Queen Street Mental Health Service and a number of other anniversaries within Toronto Mental Health Care.

I had just finished writing a book.<sup>15</sup> I had begun to prepare lectures based on this material and had given the first related lecture at an Astra Zeneca meeting in June. A few days after I gave the Toronto lecture, I gave the same lecture almost word for word at Cornell on the 5<sup>th</sup> and 6<sup>th</sup> of December as part of a distinguished Guest Lecture series. An identical lecture was later given in Paris, Minneapolis and Cambridge. In all places it was received well. After the lecture in Cornell, Jack Barchas, the Head of the

University Psychiatry Department, mentioned that my work on the history of psychopharmacology would be remembered 100 years from now. In Toronto, my lecture was rated the highest for content.

Following the lecture at Cornell there was a meal at which the Dean of the Medical School was present. His first question to me was what had happened in Toronto. I was surprised that he might have thought anything had happened in Toronto. What I didn't know, and what emails from Cornell later were to make clear, was that there had been phone calls to senior people within Cornell in the few days before my lecture, trying, it would seem, to persuade them not to go ahead with the lecture. I was also to find out later, through copies of correspondence, that at a series of council meetings for the American Foundation for Suicide Prevention on December 1<sup>st</sup>, my work was mentioned unfavourably by a council member. At this stage the Dean at Cornell was almost certainly unaware that I knew far less than he did.

## **What had happened in Toronto?**

The day before the 'Looking Back Looking Ahead' meeting I had been interviewing people who would be working with me in the Mood Disorders Programme. I had been consulted on the decor of my office, had met with the University Head of Department, to discuss future research possibilities as well as with the Physician-in-Chief at the Centre for Addiction and Mental Health (CAMH) to discuss removal expenses. The only issue from the point of view of either the University Department or the Clinical Service appeared to be to get me there as soon as possible.

There had also been a meeting with a representative from a medical communications company who was helping to organise a meeting on antidepressants for Wyeth to be held at Laguna Beach with CAMH input. The meeting was due to be chaired by Dr Charles Nemeroff, Chair of Psychiatry at Emory University. The centrepiece of the meeting was a meta-analysis, which subsequently appeared in the *British Journal of Psychiatry*,<sup>16</sup> where its appearance has caused some controversy.<sup>17,18</sup> There had been pressure for some months for me to participate in this meeting. The medical communications company was offering to write my article for me for the meeting and do anything else that might facilitate my attendance.

The lecture at the Toronto Symposium had always had the potential to be interesting; Dr Nemeroff was one of the other speakers in the programme. A lawyer acting on his behalf later confirmed that he had been consulted by members of the psychiatry department on that day, that he had given his views and that in his opinion a decision of some sorts concerning me had been made following the lecture.

During the course of the afternoon I learned for the first time that one or two people, as it was put to me, were unhappy with some aspects of my lecture, which ranged from the serendipitous discovery of chlorpromazine to the horizon of a possible future cosmetic psychopharmacology. I was told that there were concerns about claims that Prozac can make you suicidal, that Lilly knew about it (a claim never made), and about the notion that we are now treating more patients than ever before and that this was not a good thing.<sup>19</sup> I was surprised as these themes were incidental to the main

thrust of the lecture. After the lecture, a senior staff member at CAMH said to me that there were only three things that anyone remembers from any lecture and in this case they were that Prozac makes you suicidal, that Lilly knew about it and that high dose antipsychotics could cause problems. A further point subsequently made by representatives of CAMH was that I had claimed that an increasing proportion of the therapeutics literature is ghost written.

While in New York preparing for the lectures at Cornell and consulting Pfizer's healthy volunteer data in their archives I checked my emails out of hours and had an email from my future employer at Toronto, who wanted to talk. As this was logistically impossible until I returned home, he had little option therefore but to continue the conversation by email. Given the timing of things I knew therefore there would be an email waiting for me when I arrived home. There were two. One was from University of Toronto rescinding the contract I had with the university, stating that "Essentially, we believe that it is not a good fit between you and the role as leader of an academic program in mood and anxiety disorders at the Centre. While you are held in high regard as a scholar of the history of modern psychiatry, we do not feel your approach is compatible with the goals for development of the academic and clinical resource that we have. This view was solidified by your recent appearance at the Centre in the context of an academic lecture."

Almost everyone to whom I have shown this email has commented that the contents appear to indicate that the University was worried about the risk to the financial inflows to the department from pharmaceutical company sources. My personal reactions focussed on the acknowledgement of regard for work on the history of modern psychiatry. This seemed to mean that my analysis of the situation was correct but I was being let go nonetheless. It suggested a host of analogies from being sacked from a German university department in the late 1930s for good work right up to the present.

I took a considerable period of time to think through the implications of the Toronto email. My first instinct was to lie low and let no one know what had happened. However it was not possible to pass the situation off simply as a decision on my part not to move to Toronto as it seemed very probable that one of the first questions on the witness stand in the upcoming Tobin case would have to do with why I had been sacked by the University of Toronto.

On February 15<sup>th</sup> I wrote to the Chair of the Ethics Board at the Centre for Addiction and Mental Health (CAMH) copying the letter to the Dean of the Medical School, the Head of CAMH and others indicating that there were problems that they may not be aware of. I hoped they would not wish to have their actions mixed up in an important medico-legal case. I also indicated to them that half a year before I had given the lecture I had had an article in the Hastings Center Report, one of the premier bioethical journals in the world, called 'Good Business Good Science'.<sup>20</sup> This was an article on Prozac, which sat beside four other articles written by philosophers with an interest in psychiatry or psychiatrists with an interest in philosophy discussing the issues of Prozac and alienation.<sup>21-24</sup> Views were expressed for and against the issue that Prozac was being used to treat alienation rather than depressive disease and that this

was a good thing. My article suggested that in fact the primary issues had to do with money and marketing rather than the treatment of alienation. The article made most of the claims that half a year later became contentious in Toronto, as had previous articles.<sup>25-27</sup> The Hastings article specifically mentioned the risk of suicide and Prozac, the question of ghostwriting and issues to do with the increasing treatment of ever-larger numbers of the population. Following publication, Eli Lilly, who had been one of the biggest private supporters of the Hastings Center, withdrew their support.<sup>a</sup> There was, therefore, clearly a context in which the public was likely to read the events surrounding my lecture and the withdrawal of my job.

I was due to present a lecture at a History of Psychiatry Conference in Toronto in April. I asked whether I could meet up with key people in CAMH and the University. The reply from CAMH was dismissive. I approached the Canadian Association of University of Teachers who began to write to the President of the University of Toronto, on my behalf. His replies were noncommittal. The Healy affair hit the media in mid-April, especially in the *Globe and Mail* and on the Canadian Broadcasting Corporation. I began to hear reports that I was a bad clinician, that I was racist and that I was a Scientologist.

While I was in Toronto in April, a public lecture was organised for the Centre of Bioethics in the University of Toronto on the question of SSRIs and Suicide. An invitation was sent to Glaxo SmithKline, Pfizer and Eli Lilly to attend or send any number of experts to present the opposite side of the case. None did so. The affair continued to be explored in the media. The Tobin case was scheduled for a month later in Wyoming. SmithKline filed a motion suggesting that the Court, in the Tobin versus SmithKline proceedings, should bar any reference to the Healy affair.

Meanwhile, I received an email from the medical communications company that had planned the Laguna Beach meeting with an article attached. The covering note suggested that this article could be altered in any way that I or any co-authors might see fit. The plans were to publish it along with others presented at the Laguna Beach meeting in the *Journal of Psychiatry and Clinical Neuroscience*. Along with a colleague, I sent a version back with two revisions – one favourably mentioning a competitor compound, mirtazapine, and the other referencing our healthy volunteer study and its implication that many antidepressants, including Wyeth's venlafaxine, could be dangerous without a sensitive matching of patient to drug. There was an immediate objection to the mention of mirtazapine.

Several months later, a copy of the final draft of this article arrived in my office. It had apparently already been sent to the journal. There were numerous changes, none of which my co-author or I had an opportunity to scrutinise. The mention of our healthy volunteer research was missing and a statement that "current data suggests that venlafaxine and amitriptyline may induce full remission in a greater number of patients" was included, literally as a bottom line. This was something that I would

---

a. For further information see <http://www.health.bcit.ca/CRP/snub.htm>, 'Snub to Prozac critics upsets teachers...' by Ann McIlroy *Globe and Mail* 2000; see also, <http://www.thejournalnews.com/rtc/27part7.htm>, 'Manufacturers push their psychotropics'



D. Healy

never have agreed to. It appeared that an article that was already heavily biased towards Wyeth's position had had an even more blatantly pro-company message included by a senior CAMH figure. I withdrew my name from the article.<sup>28</sup>

### **The Tobin v SmithKline Trial**

The Tobin v SmithKline trial<sup>11</sup> lasted two weeks during which the jury heard me speak about a series of healthy volunteer studies undertaken by SmithKline which revealed agitation following Paxil and dependence on Paxil. I also reported on clinical trial results comparing Paxil to placebo in suicidal patients that had never been published and evidence of ghost writing, sealed trials and other material.<sup>29</sup> They also had a detailed breakdown of Don Schell's suicide risk from **another key expert**, Terry Maltsberger. Following a 2½ week trial, on June 8<sup>th</sup> 2001, the jury returned within hours, finding against Glaxo SmithKline on issues of failure to warn and test, and awarded \$ 6.4 million to the plaintiffs. The verdict was a first against a pharmaceutical company for an adverse psychiatric effect of a drug.

Even this verdict did nothing to persuade CAMH or the University of Toronto to revisit the issues, even though the main plank of their defence was that Healy's position on SSRIs posed a threat to patient care. A CBC programme exploring the issues in the wake of the Tobin verdict, faced with the CEO of the Centre for Addiction and Mental Health stating that he would be concerned if the level of pharmaceutical company funding to CAMH ever rose above 20%, produced evidence that the previous year 52% of the funding for the Mood Disorders Programme came from company sources.<sup>b</sup>

It is unclear what the more general impact of the Tobin verdict will be. There have only been two other SSRI cases that have gone to Court, the Fentress (Wesbecker) and Forsyth cases. In the United States, following the settlement in the Fentress case in 1994, a large number of other cases against Lilly settled. These settlements left only 2 plaintiffs firms in the field. Taking legal action in the US involves expenses of approximately \$200,000 before a case comes to Court for a minimal slate of experts and the overheads incurred in responding to a range of motions filed by the defendants aimed at eliminating the plaintiffs' experts or obtaining summary judgements. It typically requires several attempts at such a case before a verdict can be achieved and therefore effectively requires half a million dollars to "get into the game". The capacity of the two remaining firms to keep "playing", when the appeals process can delay any payment from a successful verdict as in the Tobin case, remains uncertain.

In the United Kingdom, the plaintiffs' legal teams, if they lose, normally have to cover both their own and the defendants costs. This means that a case against a major pharmaceutical company may require millions of pounds of insurance. As several cases need to be taken in order for lawyers involved to learn how to handle these actions so as to have a reasonable prospect of success, and as it is now clear that actions against

---

b. See <http://www.pharmapolitics.com/cbc.national.htm> for a transcript of the interview.



pharmaceutical companies will not be legally aided, it is perhaps not surprising that there have been no legal cases involving SSRIs in the United Kingdom.

Thus the avenues of challenge are closing just when the scientific evidence is becoming overwhelming. In April 2000, Khan *et al*<sup>30</sup> published a table of suicides and suicidal acts in placebo controlled trials of sertraline (Zoloft), paroxetine (Paxil), nefazodone (Serzone), and mirtazapine (Remeron) compared to placebo. Freedom of information requests in ongoing SSRI cases had made it clear that a substantial proportion of suicidal acts coded under placebo for SSRIs had occurred during the washout phase of clinical trials.<sup>31,32</sup> Even without making adjustments for this, the original Khan *et al* data showed an excess of suicidal acts on new antidepressants compared to placebo. Adjusting for placebo washouts, there were statistically significant increases in the relative risk of suicidal acts in randomised controlled trials for sertraline and paroxetine over placebo with relative risks for these SSRIs extending up from 3.0 upwards to over 10 in the trials of fluoxetine, in which the same coding practice for suicidal acts had taken place.<sup>33</sup>

The SmithKline defence in the Tobin case included an appeal to articles,<sup>34,35</sup> in which suicidal acts during the washout period were coded as placebo suicidal acts suggesting that these authors had not seen the raw data. SmithKline's leading expert John Mann conceded at the trial that he had not seen the raw data. This raises in the most profound form possible the issue of authorship of key texts in the current psychopharmacological literature. There has been considerable concern expressed recently about the issue of ghostwriting of the therapeutics literature with suggestions that authors formally declare their role in the authorship of a paper.<sup>36</sup> The discrepancy between the raw data on the issue of SSRI-linked suicide attempts and the published data suggests that more is needed – namely that the only safeguard is to make the raw untabulated data available.<sup>37</sup>

In early September 2001, a letter signed by 29 senior figures in the field including two Nobel Prize Winners, former Presidents of the American Psychiatric Association, the American College of Neuropsychopharmacology and a range of other psychiatric and psychopharmacological organisations world-wide was sent to the President of the University of Toronto protesting against the violation of academic freedom involved in “the Healy case”. There was input from Europe, North America, South America, Asia, including both Japan and China, and Australia. The President's response on behalf of the University suggested these signatories were not fully aware of the issues in the case.

There seemed to be only one way to find out more about the issues of the case. Two weeks later I filed a legal action against the university, which at present I believe to be the first legal action involving a claim for violation of academic freedom. There were further claims for breach of contract, and for libel.<sup>c</sup> The libel arose when in the

---

c. In April 2002, the University of Toronto and the Centre for Addiction and Mental Health agreed to a settlement with this author. In a joint statement released by the parties on 26 April 2002, the university stated it “underscores the support for free expression of critical views and acknowledges Dr. Healy's scholarship by confirming it will appoint him as a visiting professor in the faculty of medicine.”

course of responding to the issues, representatives of CAMH and the University had appeared to make it clear that clinicians could not be allowed to talk about the hazards of pharmacotherapies.<sup>d</sup>

## The Tail of the Python

It has been argued eloquently that the giving of a gift creates the presumption of a bias,<sup>38,39</sup> and that dancing with the porcupine is a tricky business.

Another way to express this is in terms of an academic bias multiplier. If SSRIs cause suicide at a rate of 1 per 1,000, this may seem like a small figure but if 50 or 100 million people get SSRIs the actual number of suicides will become a major public health problem. In the same way the giving of research sponsorship, speakers' fees or other emoluments biases the recipients. We each hope that our individual bias will be small. A small individual bias applied across all academicians in all medical departments in all universities in the Western world, however, may well produce a significant problem.<sup>40</sup> At this stage, what may be a small bias in individual cases would appear to extend to the point where plaintiffs find it all but impossible to recruit an expert to testify on their behalf in legal cases involving the adverse effects of psychotropic drugs. These experts, many of whom have been educated at the public's expense, have effectively been 'body snatched' by the pharmaceutical industry.

There are even more serious issues of legal jeopardy produced by the current clinical trial process. In psychiatry up until approximately 1980, independent clinical researchers designed the protocols for the therapeutic trials, collected the patients, analyzed the results and wrote up the articles that appeared in the scientific literature and could stand behind those articles.<sup>12</sup> However now the vast majority of therapeutic trials are run according to protocols produced by pharmaceutical companies. It is my observation that not all the patients in these trials actually exist.<sup>41-43</sup> The results of the studies are analyzed in-house and are often written up within communications agencies, with, it seems on some occasions, the names of prestigious authors attached afterwards.<sup>44</sup> Major journals in the field are successfully targeted for publication. The speakers on company platforms are chosen for impact factor rather than familiarity with the drug.. In my view, this new state of affairs means that few clinicians know much about the true nature of the new drugs or their adverse effects.

I contend that the investigation or non-investigation of these adverse effects is now entirely in company hands. A key revelation from the SSRI story is that adverse effects such as agitation, emergent suicidality and suicide have not been investigated properly. This means that patients voluntarily engaging in clinical trials out of a sense of civic duty are *de facto* providing the experiences that, inappropriately coded, produce 'data' that companies then argue in court reveal that no adverse effect has occurred. Any of us engaging in clinical trials are, therefore, *de facto* putting our relatives, friends and others in a state of legal jeopardy, as the non-collection and non-exploration of adverse

---

d. See [www.pharmapolitics.com](http://www.pharmapolitics.com) for the statement of claim.

effects that occur to us are allowing pharmaceutical companies to make arguments in Court that lead to plaintiffs' claims being dismissed.

We are now at the tail of the python. This is the bit that, when it grips on to something, enables the python to squeeze the life out of its prey. The pharmaceutical corporations have become among the largest and most profitable corporations on the planet. They have become so because patients encouraged by clinicians voluntarily participate in clinical trials. This voluntary and free participation in clinical trials, which involves the donation of bodily fluids, personal data and other data, is translated into the enormous market capitalization of these companies. Both patients and clinicians are persuaded that engagement in clinical trials is a good thing. That without such trials the development of new and breakthrough agents would not happen. This simply isn't the case. In my view, the majority of clinical trials are either for marketing studies or are studies on agents that never reach the market. Pharmaceutical companies find out about the inefficacy or deleterious effects of these agents by giving them to us. All the while the actual flow of new breakthrough clinical entities, despite market hype, is falling.

A further feature of the recent evolution of clinical trials is that clinical trial data has become proprietary in a way that was not the case before. The Olivieri and other cases have given rise to a debate about academic freedom in which it has been suggested that traditional university and scientific values on one side have to be trumped by the clinical values of concern for patient welfare on the other. The proponents of the University of Toronto position have compared the speaking out about the hazard of drugs to someone screaming fire in a crowded theatre. The implication is that some people may be hurt in the stampede to the exit. But what if there is a fire in the theatre?

In fact the supposed contrast between the values of science and the values of clinical care is an artificial one. In both the Olivieri and Healy cases what is involved is a contrast between the values of science and the values of business. The clinical trial data that results from pharmaceutical company trials today is proprietary data. As long as this data remains unavailable to public scrutiny, it cannot be called scientific.

Furthermore it is quite possible that ultimately the confidentiality clauses in clinical trials run by pharmaceutical companies will be found to be illegal. One of the explicit reasons for conferring 'prescription-only' status on therapeutic drugs was so that medical prescribers, who it was thought were in a better position than lay people to bring potential hazards of the drugs to light, would in fact do so. Were this not to happen, prescription only arrangements, a system designed for addicts in the first instance, would become a mechanism whereby companies could deliver adverse medical outcomes with near legal impunity. In addition to confidentiality clauses running against the grain of science, a good case can be made that where clinical trials throw up adverse events, the gagging of clinicians is fundamentally at odds with the mechanisms that make these drugs available under prescription-only arrangements.

It seems that the woman David Weatherall saw dancing with a python sometime later made a mistake and was killed by the python. This need not happen in the dance between academia and the pharmaceutical industry as, in fact, the industry needs the public and the public's academics far more than we need the pharmaceutical industry.

## REFERENCES

1. Thompson, J., Baird, P., Downie, J. (2001). *The Olivieri Report*, James Lorimer & Co., Toronto.
2. Angell, M. (2000). Is academic medicine for sale? *New England Journal of Medicine* **342**: 1516-1518.
3. Lewis, S., Baird, P., Evans, R.G., Ghalli, W., Wright, C.J., Gibson, E., Bayliss, F. (2001) Dancing with the Porcupine. Rules for governing the university-industry relationship, *Canadian Medical Assoc. Journal* **165**: 783 – 785.
4. Teicher, M.H., Glod, C., Cole, J.O. (1990) Emergence of intense suicidal preoccupation during fluoxetine treatment, *Am J Psychiatry* **147**: 207-210.
5. Creaney, W., Murray, I. and Healy, D. (1991) Antidepressant induced suicidal ideation, *Human Psychopharmacology* **6**: 329-332
6. Healy, D. (1994) The fluoxetine and suicide controversy, *CNS Drugs* **1**: 223-231
7. Cornwell, J. (1996) *The Power to Harm*, Viking Penguin, New York.
8. Cole, J. O. (1999) Affidavit in Miller vs Pfizer.
9. Davis, J. (2001) Expert testimony in Miller vs Pfizer, Kansas November 19<sup>th</sup>.
10. Oswald, I. (2000) The Halcion Affair. *The Psychopharmacologists* Volume 3, Arnold, London.
11. *Tobin versus SmithKline Beecham. Case No. 00-CV-0025-BEA.*
12. *Healy, D. (1997) The Antidepressant Era, Harvard University Press, Cambridge MA, USA.*
13. Tranter, R., Healy, H., Cattell, D. and Healy, D. (2002) Functional effects of agents differentially selective to noradrenergic or serotonergic systems, *Psychological Medicine* **32**: 517-524.
14. Healy, D. (2000) Antidepressant induced suicidality, *Primary Care Psychiatry* **6**: 23-28.
15. Healy, D. (2002) *The Creation of Psychopharmacology*, Harvard University Press, Cambridge MA. USA.
16. Thase, M.E., Entsuah, A.R. and Rudolph, R.L. (2001) Remission rates during treatment with venlafaxine and selective serotonin reuptake inhibitors, *British Journal of Psychiatry* **178**, 234-141
17. Wright, I.C. (2002) Conflict of interest and the???, *British Journal of Psychiatry* **180**, 82-83
18. Editorial (2002) Just how tainted has medicine become? *Lancet* **359**: 1167-1168.
19. Healy, D., Savage, M., Michael, P., Harris, M., Hirst, D. Carter, M., Cattell, D., McMonagle, T., Sohler, N. and Susser, E. (2001) Psychiatric bed utilisation: 1896 and 1996 compared, *Psychological Medicine* **31**, 779-790
20. Healy, D. (2000) Good Science or Good Business? *Hastings Center Report* **30**: 19-22.
21. Elliott, C. (2000) Pursued by Happiness and Beaten Senseless, *Hastings Center Report* **30**: 7-12.
22. Kramer, P. (2000) The valorisation of sadness, *Hastings Center Report* **30**: 13-18.
23. DeGrazia, D. (2000) Prozac, enhancement and self-creation, *Hastings Center Report* **30**: 34-40.
24. Edwards, J.C. (2000) Passion, activity and “The Care of the Self”, *Hastings Center Report* **30**: 31-33.
25. Healy, D. (1999) Guest Editorial: A Failure to Warn, *Int J Risk & Safety in Medicine* **12**: 151-156.
26. Healy, D. (1999) The Three Faces of the Antidepressants. Critical Comments on the Clinical-Economic Framework of Diagnosis, *Journal of Nervous & Mental Disease* **187**: 174-180
27. Healy, D., Langmaak, C. and Savage, M. (1999) Suicide in the Course of the Treatment of Depression, *Journal of Psychopharmacology* **13**: 94-99.
28. Tranter, R, C. O'Donovan, Chandarana, P, and S. Kennedy (2002) Prevalence and outcome of partial remission in depression, *Journal of Psychiatry and Clinical Neuroscience* **27**: 241-247.
29. Healy, D. (2001c.) Trial testimony in Tobin vs SmithKline, Cheyenne, Wyoming, May 22<sup>nd</sup> 2001.
30. Khan, A, Warner, H.A. and Brown, W.A. (2000) Symptom reduction and suicide risk in patients treated with placebo in antidepressant clinical trials, *Archives of General Psychiatry* **57**: 311-317.
31. Brecher, M. (1991) FDA Review & Evaluation of Clinical Data Original NDA 20-021, Paroxetine Safety Review June 19<sup>th</sup> 1991.
32. Lee, H. (1990) Statistical reviews on Sertraline for FDA, August 14<sup>th</sup> 1990 and January 31<sup>st</sup> 1991.

33. Healy, D. (2003). Lines of Evidence on the Risks of Suicide with Selective Serotonin Reuptake Inhibitors. *Psychotherapy and Psychosomatics*. (Scheduled for publication March 2003).
34. Lopez-Ibor, J.J. (1993) Reduced suicidality on paroxetine, *European Psychiatry* **1** (Suppl 8): 17s-19s.
35. Montgomery, S.A., Dunner, D.L., Dunbar, G. (1995) Reduction of suicidal thoughts with paroxetine in comparison to reference antidepressants and placebo. *European Neuropsychopharmacology* **5**: 5-13.
36. Davidoff, F., DeAngelis, C.D., Drazen, J.M., et al (2001) Sponsorship, authorship, and accountability, *New England Journal of Medicine* **345**: 825-827.
37. Healy, D. (in press). Is academic psychiatry for sale? *British Journal of Psychiatry*.
38. Schafer, A. (subm). Medicine, morals and money. Dancing with porcupines or sleeping beside elephants.
39. Lewis, S., Baird, P., Evans, R.G., Ghali, W., Wright, C.J., Gibson, E., Bayliss, F. (2001) Dancing with the Porcupine. Rules for governing the university-industry relationship. *Canadian Medical Assoc. Journal* **165**: 783 – 785.
40. Healy, D. (2001) When Interests Conflict. *Pharmaceutical Executive*, August p. 98.
41. Stecklow S, Johannes L. (1997) "Questions arise on new drug testing. Drug makers relied on clinical researchers who now await trial," *Wall Street Journal*, August 15<sup>th</sup>
42. Eichenwald K, Kolata G. (1999) "Drug trials hide conflict for doctors," *New York Times* May 16<sup>th</sup> pages, 1, 28, 29; and "A doctor's drug studies turn into fraud," *New York Times* May 17<sup>th</sup> pages 1, 16, 17.
43. Boseley S. (1999) "Trial and error puts patients at risk," *Guardian (London)* July 27<sup>th</sup>, p 8.
44. Healy D. (2003/in press). *Let them Eat Prozac*, Lorimer, Toronto.