

## Wessely S (2009). Surgery for the treatment of psychiatric illness: the need to test untested theories.



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I run a clinic for sufferers with chronic fatigue syndrome (CFS), sometimes also called myalgic encephalomyelitis (ME), and known to a previous generation of neurologists as 'neurasthenia' or 'nerve weakness'. It is my practice to record all the previous treatments that any patient has received. Perhaps three or four times a year someone will tell me about their trip to a private clinic where they received "colonic lavage". This was to "remove the toxins" that they have been told are contributing to their fatigue, debility and exhaustion. The fact that they have still come to our clinic suggests that the treatment has not been successful. Despite that, very few express any retrospective scepticism about the procedure, nor regrets for the often considerable money with which they have parted for this and other "detoxifications". The procedure seems logical – numerous media articles remind us that we are "overloaded" with toxins, especially given our modern polluted environment and our poor diets, rich in carbohydrates, awash with food additives, and swimming in chemicals. One popular book on ME is called "Overload" (Steincamp 1989), numerous other self-help books, and countless magazine and newspaper articles continue to promote the idea that ME is a very modern illness, linked to the increasing assault on our bodies by a wide range of contaminants and hazards – not just food additives, but also dental amalgam, candida infection, mercury in vaccines, pesticides, electromagnetic radiation, and chemicals of almost any description. Those who see patients complaining of multiple chemical sensitivity (MCS) will recognise the above, and more. It all seems very modern, and anyone reading this literature would probably conclude that this is a condition unique to our age, a hubristic response to our despoilation of our planet.

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But it is not new. A hundred years ago such theories were espoused by at least some of the most conventional, establishment and orthodox doctors of the period. And whilst I might regret the fact that my patients seem to have parted with their money for such little benefit, at the beginning of the twentieth century many patients paid with their lives as a result of action based on the same theories.

### The theory

Autointoxication was the name of the theory (Bynum 2001). According to Bynum its intellectual origins can be traced to Charles Bouchard, a French physician who, in the 1890s, showed that if you inject faeces or urine into experimental animals, the results could be fatal. From that came the more general idea that the gastrointestinal tract was the source of many toxins, and that if and when these "leaked" beyond the colon and into the body, a number of different illnesses could result. In particular malaise, depression, insomnia, dizziness, fatigue and the general feeling of being "toxic" could be explained by this phenomenon, which occurred across the life span. Elie Metchnikoff was a famous advocate, as was the American entrepreneur and inventor of the cornflake, JD Kellogg, another particularly eccentric pioneer. At King's College Hospital, where I now work, surgeon Arbuthnot ("Willie") Lane was a strong British champion of autointoxication as a cause of unexplained symptoms in young women (Lane 1921), and is often cited as the inspiration for Cutler Walpole in Shaw's *Doctor's Dilemma*. It did Willie no harm – he rose to become Sir Arbuthnot Lane, surgeon to the King, and is as far as I know the only person to have wards named after him in two separate London teaching hospitals. But what about Willie's patients? Some were not so fortunate. Because Willie was a surgeon, and doing what comes naturally to a surgeon is operating, his solution was to remove the colons of many of those referred to him with these non-specific symptoms and ill health. Ten per cent of them died. That was actually relatively good - Willie was technically a very good surgeon – the usual death rate for colectomies was higher.

Willie Lane was not alone. Autointoxication was but one part of the general theory of focal infection (Bynum 2002). This held that our bodies contained numerous foci of micro-organisms, which, when concentrated together, could cause untold misery. The only solution was to remove them. And whilst the vogue for removing colons passed not long after the passing of Willie Lane, the vogue for removing tonsils, teeth and appendixes only really went into decline about the time I qualified as a doctor, in the 1970s. Our current generation of medical students hears little about chronic appendicitis or chronic tonsillitis, but there was a time when few of us, myself included, made it to adolescence without either one or the other being removed.

## The theory as a basis for treating mental illness

It is sad but true that knowledge about the nature of psychiatric disorders lags behind that of the conditions seen in what I consider to be the 'easier' medical specialities, or at least those whose target organs are more accessible. Hence historians eager to pinpoint medical hubris have often found some of the choicest examples in psychiatry. And there are few better exemplars than Dr Henry Cotton, the subject of a major study by Andrew Scull, one of the foremost contemporary historians of psychiatry (Scull 2005). Cotton was the superintendent of the Trenton State Hospital, and perhaps the most enthusiastic proponent of the notion that focal infection was the cause of the major psychiatric illnesses, most particularly dementia praecox (which we now call schizophrenia) and manic depression (our bipolar disorder).

Henry Cotton arrived in Trenton, New Jersey, in 1907, to take over the main mental hospital. He was no maverick figure – he was a protégé of Adolf Meyer at Johns Hopkins, the most influential American psychiatrist of the day. Cotton was young, energetic and ambitious. In his early years at the Trenton Hospital he removed mechanical restraints, installed fire alarms, opened a nursing school, introduced occupational therapy and hired social workers (Ramchandani 2007). He was, however, particularly dismayed by the general pessimism that surrounded the care of the seriously mentally ill, partly because of the prevailing quasi-genetic doctrines of degeneration, which, for example, were associated with the name of Henry Maudsley, the founder of the other hospital in which I work.

A hallmark of what we now call junk/pseudo-science is that it reflects or parodies what is truly exciting and innovative in the science of the day. And in Cotton's day this was bacteriology. Germ theory was transforming medicine. It was only in 1913 that spirochaetes had been discovered in the brains of patients who had died from general paralysis of the insane (GPI), which, exceptionally, justified the cliché 'major breakthrough'. And it was not just the presence of micro-organisms themselves; just as important were the toxins they were now known to produce. Cotton enthusiastically embraced these new ideas, and transferred them to his own discipline. As Scull describes:

*The key, Cotton argued, was germs. Germs and pus. For years, conservative medical men had resisted the implications of the work of such scientists as Louis Pasteur and Robert Koch, and the warnings about the perils of pus in the practice of surgery that emanated from the apostle of antiseptic surgery, Joseph Lister. But by the dawn of the 20th century, the gospel of "germs" was sweeping all before it. Medicine embraced the laboratory as a source of cultural authority. Bacteriological models of disease brought gains in etiological understanding and, to a more limited degree, in therapeutic efficacy. The upshot was that physicians and surgeons, donning the mantle of the new science, found their prestige and their prospects soaring. And yet there were diseases and disorders that remained recalcitrant, resistant to the new paradigm, and frustratingly beyond the reach of modern therapeutics.*

So Cotton was neither a maverick nor an isolated voice. No less a figure than Emil Kraepelin, whose descriptions and classification of dementia praecox (schizophrenia) heralded the modern era of psychiatry, wrote about the importance of "Selbstvergiftung" (self poisoning) in the aetiology of psychosis (Noll 2007). Wagner-Jauregg, one of only two psychiatrists ever to win the Nobel Prize (for his malarial treatment of neurosyphilis), thought much the same (Noll 2007).

Cotton appears to have been converted to the cause of focal infection in 1915. He then brought all the technological advances of modern bacteriology into the mental hospital, and set about proving that nearly all of those admitted with psychotic disorders were actually suffering from the effects of focal infection lurking somewhere in their bodies. This in itself was not revolutionary, but it was the zeal that Cotton brought to the cause that would eventually be the cause of his downfall. And zeal it was – here is Scull's account of Cotton's 1921 Vanuxem lecture at nearby Princeton University:

*As early as 1916, Cotton had begun to attack and remove the most obvious site of infection, the teeth: unerupted and impacted teeth; teeth with infected roots and abscesses, decayed or carious teeth, apparently healthy teeth with periodontitis, poorly filled teeth, sclerotic teeth, teeth with crowns. When many of his patients stubbornly refused to recover, he was undeterred, redoubling his efforts to locate the underlying focal sepsis he felt certain was there. Tonsils and sinuses were soon joined by spleens and stomachs, colons and cervixes, as he ruthlessly pursued his goal of a thorough cleansing of his patients' bodies. And the results, he informed his rapt Princeton audience, were little short of astonishing. In his final lecture, he reviewed case after case of patients seemingly condemned to a lifetime of mental darkness who, once relieved of their infected teeth, tonsils, stomachs, or colons, made near-miraculous recoveries.*

Patrick McGrath (2005) takes up the story:

*Within a few years of taking over Trenton State Hospital, he [Cotton] was removing the infected teeth and tonsils of dozens of his patients, not to mention their stomachs, gallbladders, colons, testicles and ovaries -- with special emphasis on the right side of the hindgut, which, he declared, had particularly "decadent tendencies."*

*When Cotton reported cure rates as high as 85 percent, many in his profession were hugely impressed and, of course, eager to embrace such an apparently efficacious approach. Simply stated, Cotton's treatment was based on the assumption that all manifestations of madness were merely symptoms of a deeper underlying pathology, that of bacterial infection. Remove the infected part and you cure the madness.*

News of Cotton's ideas, and his apparent success, soon spread. As Scull writes (Scull 2005, p 81):

*Desperate for relief from the demons that tormented them (or their nearest and dearest) and dazzled by the seemingly authoritative reports emanating from Trenton about the extraordinary breakthroughs associated with a bacteriological model of madness, patients (or their families) urgently sought to share in the new miracle cures.*

Cotton became famous, whilst the State of New Jersey, which ran the asylum, also benefited financially from the influx of fee-paying patients. Then as now, journalists also seized upon the story, the *New York Times* heralding Cotton as a scientific genius whose investigations gave "high hope" for the future.

But there was a price to pay, and for some it was a heavy one.

*Cotton's fame in the United States and Europe spread rapidly, but as he continued to cut out his patients' insides, postoperative deaths increased alarmingly, mostly from peritonitis. Soon the death rate was 30 percent and higher (McGrath 2005).*

Cotton's surgeons were not the equal of Willie Lane, although, ironically, Cotton took advice from Willie during a visit to the United Kingdom, and his mortality figures did improve in consequence.

### **The theory put to the test**

But not everyone was convinced by Cotton's theories. This was not because of revulsion at the carnage that Cotton and his disciples were causing (although that did play some part), but rather because of a refusal to believe that the major psychoses really could be laid at the door of focal infection. Several of the leading lights of American psychiatry expressed both public and private scepticism, sometimes in trenchant language. Cotton's bitterest critic was George Kirby, also a protégé of Adolf Meyer, who had taken charge of the New York Psychiatric Institute. He employed two junior doctors, Clarence Cheney, a psychiatrist, and Nicolas Kopeloff, a bacteriologist, who combined forces to study Cotton's theories, methods, and results. They presented their findings at the 1922 annual meeting of the American Psychiatric Association, in the most popular and the most controversial session, and then again in 1923. They subsequently published their results in two papers in the *American Journal of Psychiatry* (Kopeloff and Cheney 1922; [Kopeloff and Kirby 1923](#)).

Thirty-six years later, Jonathan Cole, a pioneer of modern psychopharmacology, referred to Kopeloff's, Cheney's and Kirby's research as "the first scientific experimental evaluation of psychiatric therapy of which I am aware" (Cole 1959). The experimental element to which Cole referred, and the reason that this research is featured in the *James Lind Library*, is that experimental and control groups were generated prospectively using an unbiased method of allocation. To quote from Kopeloff's and Kirby's 1923 account:

*The procedure followed therefore, after the diagnostic classification had been made and the infections determined, was to designate automatically that alternate patients be operated.*

Use of alternation to achieve unbiased generation of comparison groups in clinical trials was still rare in the 1920s, but New York happened to be the city in which this important component of controlled trials was being applied in assessing treatments for pneumonia (Podolsky 2008).

Kopeloff and his colleagues begin the description of their trial with an elegant rationale for the controlled trial ([Kopeloff and Kirby 1923](#)):

*Because of the difficulties of interpretation inherent in an investigation of this nature, it seemed desirable to reduce the study as nearly as possible to the terms of an experiment. Consequently, all patients were divided into two groups as nearly identical as possible. All members of one group receive operative treatment....while members of the other group has no surgical interference and could therefore be regarded as controls. In this way, operative treatment might be considered to be the crucial factor in this experiment and an evaluation of its influence on the course of the psychoses might thus be readily established.*

But why the need for a new control group? They continue :

*It might be argued that there was ample control material in the patients of the past and present years who had never been treated from the viewpoint of focal infection. Since it is well-known that any special attention services to improve the status of a hospital population at any given time, it was considered much more satisfactory to have such a control group made up of patients observed at the same time by the same physicians and under the same living conditions as the patients to be actively treated.*

Patients assigned to surgery had been subjected to a full-blooded effort to remove any possible source of focal infection. In their first series this was largely a matter of removing tonsils and teeth, but later they also operated on sinuses and cervixes as well.

What they did not do was remove any colons. The authors note this apparent anomaly, but state that despite their extensive investigations of the gastrointestinal track, Dr Lynch, the surgeon who advised them, had not suggested any colectomies were indicated. They added that he had previously performed such operations at the Trenton State Hospital under Dr Cotton between 1918 and 1919, presumably to demonstrate his particular expertise, although one wonders if Dr Lynch was by now getting cold feet.

The results were clear cut. There was no difference in outcome, neither in the first report, nor the second, larger sample. The results were presented only as percentages recovered or improved, without statistical analysis (although it is clear that the proportions between the two groups were so similar in the first group of 60 cases and controls and the second of 120 that no significant differences existed). However, they did also present the results stratified by diagnosis. The reason for this was that when they looked at those who had done well, in all cases this had been predicted before on the basis of clinical history - "in every case that recovered a recovery had been forecast before treatment was started". This was because the good prognosis group consisted largely of what we now call bipolar disorders, and the poor prognosis group was largely made up of those with schizophrenia. But there was still no difference in the proportions recovered or improved between the cases and controls stratified by diagnosis. So focal infection had nothing to do with it - "we have no evidence on which to base a conclusion that the removal of focal infection has of itself brought about recovery".

The paper concluded with a section examining problems with the bacteriological rationale for the concept of focal infection, and its relationship to psychosis.

The first, preliminary report (Kopeloff and Cheney 1922) had been measured and circumspect, but in the extended experience reported the following year (Kopeloff and Kirby 1923) they went on the attack. Cotton now received a serious working over - "the task has been made more difficult", Kopeloff and Kirby noted, "because of Cotton's failure to publish complete data on either the psychological or laboratory side". Cotton's descriptions of his techniques were inadequate, his results hard to credit, and his conclusions unsupported. Just because one can demonstrate, for example, the presence of bacteria in the teeth or the stomach does not mean that this is associated causally with psychosis - "the mere presence of bacteria we contend does not necessarily imply infection". Cotton himself was blind to this because of his "failure to have controls of either a psychiatric or a laboratory nature".

In an interesting aside, however, Kopeloff and Kirby did not imply that psychiatrists should abandon the search for focal infection. "On the contrary", they wrote, "psychiatry owes Cotton a lasting debt for directing attention to the neglect of certain physical conditions in those suffering from mental diseases". In other words, there is no objection to aggressively pursuing the treatment of physical illness in those with major psychiatric disorders, provided that, as they put it, "it is not to be expected that this will, *per se*, clear up the psychosis". Even now psychiatrists still need to be reminded about the importance of managing physical health care problems in psychiatric patients (Semele et al. 2007).

As the discussion proceeded it was clear that the writing was on the wall for Cotton. In discussion published at the end of Kopeloff's and Kirby's report (1923), Edward Strecker, who had made his name in war neuroses and now

worked at the prestigious Pennsylvania Hospital, could not hide his scorn for Cotton. Kopeloff's work was "timely, scientific and very practical", because as Strecker relates, "not a day has passed during the past year when neuropsychiatrists.....have not been approached as to the desirability and advisability of ....removing alleged foci of infections in the attempt to cure patients who had psychoses"... "the method had acquired considerable publicity so that it was no longer sufficient to say simply that one did not believe in it. People wanted to know why, if it was successful in the hands of some men, it should not be tried as logical treatment".

There is little doubt that what had most irritated Cotton's colleagues was not the ethics of his research, nor even the consequences, but Cotton's habit of presenting his results in the newspapers rather than in medical journals. Strecker concluded with what one hopes was an ironic understatement that "any operation which is attended by a mortality rate of 30% should not be undertaken unless the indications are clear-cut and definite".

At the close of the 1923 meeting Dr Englander announced that they had been conducting similar research at his asylum and had come to similar conclusions. He also added that, during the period over which they had failed to improve the prognosis of their patients by removing their focal infections, they had "also been having a campaign to get the patients out of hospital and keep them out", again familiar sentiments to most modern psychiatrists. However, they had done so not by removing colons, but because they had "increased our social service department. Further efforts in our already large occupational therapy department were in evidence and I feel that the combined efforts of these helped considerably to increase the number of patients who left the hospital" (published at the end of Kopeloff and Cotton 1923).

The second half of Cotton's career saw him under increasing attack – the papers by Kopeloff, Cheney and Kirby had damaged him professionally, but not finished him off; indeed, he went on to make a highly successful tour of Great Britain. It is not entirely clear why his former mentor, Adolf Meyer, continued to protect Cotton. Meyer himself was no supporter of auto-intoxication or focal infection, and instead laid part of the groundwork for the subsequent virtual take-over of American psychiatry by the psycho-analysts, and the near vanishing of biological psychiatry for several decades. In 1896 he had critically reviewed the 5th edition of Kraepelin's influential textbook, and specifically criticised auto-intoxication (Noll 2007).

It was probably Adolf Meyer who managed to suppress a damning piece of whistleblowing by Phyllis Greenacre, another rising star from the Johns Hopkins' stable who had written a paper calling into question most of Cotton's data. Greenacre herself would end her career as one of the leading lights of the New York psycho-analytic movement (how Cotton would have hated that). Cotton also survived an investigation by the New Jersey Senate in 1925 and managed to keep out of the newspapers statistics showing that his alleged 85 per cent cure rate was also associated with a 30 to 40 per cent mortality. However, Cotton's aggressive surgical management of the psychoses eventually fell out of favour as psycho-analysis gained in strength. Colectomy as a treatment for psychosis continued only at the Trenton asylum, and the dismal procession of surgical procedures and associated mortality did not end there until Cotton suddenly died of a heart attack in 1933.

## Conclusions

What does this tale tell us? Somatic explanations for psychiatric disorders can be seductive. Andrew Scull writes about one patient of Dr Cotton's, a 20 year old girl called Margaret Fisher. In 1918 she experienced a psychotic breakdown and was admitted to the Bloomingdale Asylum. However, the doctors were unable to do much for her, and so the following year she was transferred to the Trenton State Hospital and Dr Cotton. Her father, Yale economist Irving Fisher, was a friend of the wildly eccentric John Harvey Kellogg, and thus was, as Scull writes "eager to embrace this somatic explanation of their daughter's disorder. It provided an etiological account that was in close accord with their own beliefs about human health, and a far more hopeful prognosis than the one the doctors at the Bloomingdale Asylum had delivered" (Scull 2005 p 83). After he had demonstrated the presence of "pure colon bacillus", Cotton convinced them (and they did not need much convincing) that Margaret's cervix needed to be removed. She was also given "antistreptococcus" serum and vaccine. Unfortunately her condition then worsened, she developed septicaemia and died. There is a strong, albeit circumstantial, case to be made that the cause of her fatal septicaemia was Cotton's treatment. Despite that, Margaret's father continued to be a firm advocate of Cotton's theories.

The drive to find somatic explanations for psychiatric disorders remains as strong as ever for two reasons. First, because many people, including me, believe that they will eventually bear fruit. Few modern psychiatrists doubt that the causes of schizophrenia, for example, lie within the brain, nor that bipolar disorder is essentially genetic. And even if we do not embrace with quite such enthusiasm solely somatic explanations for disorders such as depression or the anxiety disorders, nor do we discount the role of genetics in determining pre-disposition, or biology influencing treatment response. Of course we know from sad experience that claims of the latest breakthrough tend to lead to disappointment, but perhaps no more so in psychiatry than in other areas of medicine. Overall many of us expect that just as neurology was transformed by new developments in neurophysiology at the end of the 19th century, neuroscience will transform psychiatry during the coming decades.

But there is also a second and more disturbing explanation for the alacrity and uncritical nature with which somatic explanations are endorsed on often the flimsiest of evidence. Psychiatry, its patients and its practitioners, continue to be stigmatised like no other branch of medicine. In 2008 I spoke at a meeting on chronic fatigue syndrome organised by the Royal Society of Medicine (RSM). There was a concentrated effort by some campaigners to have my invitation and that of my two psychiatric colleagues rescinded. Others wrote to the President of the RSM asking why psychiatrists could be permitted to attend a meeting at a society with the name "medicine" in its title, and seemed perplexed to learn that psychiatrists were actually permitted not just to attend, but to be members of the organisation. If one reads the angry responses to any article that mentions chronic fatigue syndrome and psychiatry in the same breath, it is clear that the drive to find a somatic biomarker for chronic fatigue syndrome is driven not so much by a dispassionate thirst for knowledge but more by an overwhelming desire to get rid of the psychiatrists. That it is psychiatry that to date has made the most progress in treating chronic fatigue syndrome is at best an irrelevance, and at worst just a further insult. Henry Cotton achieved his brief moment of fame by his rejection of anything other than a narrow somatic basis for psychiatric illness. He was intolerant of any other approach to the subject, and his views on psycho-analysis were close to unprintable.

For Andrew Scull the story of Henry Cotton is a medical morality tale – his chosen title "A tragic tale of megalomania and modern medicine" makes that clear. It is also a story of hubris and then nemesis. It was a nemesis that did not even cease with Cotton's death – his two sons would both go on to take their own lives, even though their father had insisted that their own teeth be removed as a prophylactic measure against focal sepsis; indeed, the search for infective causes and triggers for psychiatric disorders has never ceased. Scull ends his tale with a bleak look at what little remains of both Cotton's reputation and the Trenton Asylum.

Does anything survive? Cotton, autointoxication and focal infection have largely vanished from mainstream and academic circles, but it is a mistake to think that they have vanished all together. Far from it. Of course, no contemporary surgeon would contemplate removal of the colon as a cure for any psychiatric disorder, although I doubt that the removal of "grumbling appendices" or healthy gall bladders has ceased entirely. But in the world of popular culture Cotton's theories are alive and well. Every time we hear of a new dietary regimen which claims to promote "detoxification" or a new spa or clinic offering their services to "cleanse the body", we should be reminded of Henry Cotton. And even if colons are now safe from surgical resection, colonic irrigation continues to flourish, if that is the word. Some of those who appear in the pages of *Hello* magazine can also be seen in the private clinics around Harley Street where colonic irrigation is part of the regimen. A recent review in an alternative medicine journal began with the statement that "colonic irrigations enjoy widespread popularity among alternative medicine practitioners" (Richards et al. 2006). According to the authors, colonic irrigation fell from popularity in mainstream medicine not because it was based on a discredited theory, but as part of the general move in the 1920s and 1930s to distinguish orthodox medicine from "quacks and charlatans". Their sympathies with what they regard as a safe procedure are clear (Henry Cotton is not mentioned), but remain unlikely to convince the medical profession (Ernst 1997). Kopeloff, Cheney and Kirby's early controlled trial did play a part in ending one false and damaging theory; but the fact that Cotton and others continued their dangerous obsession for many years afterwards, together with the continuing survival of autointoxication ideas in popular culture and treatment, remind us that evidence alone is rarely enough.

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