JAMES ROBERT BROWN

MONEY, METHOD AND MEDICAL RESEARCH

Pure vs Applied

It's sometimes useful to start with a quiz, even if it seems irrelevant to the issues at hand. Suppose you have to organize a tennis tournament with, say, 1025 players. Match winners will go on to the next round while losers bow out until all have been eliminated except, of course, the final champion. Your problem is this: How many matches must you book for this tournament?

Perhaps you started to calculate like this: The first player plays the second, the third plays the forth. That's two matches. The winners of these two matches play one another. That's three. And so you carry on calculating the number of matches this way. This is very tedious, but you could eventually get the right answer. There's another method that is much better. Don't focus on matches with winners going on to the next round; instead focus on losers. Every match has exactly one. And every player, with the exception of the final champion, is eventually a loser. That means there will be exactly 1024 matches (i.e., 1025 – 1). Indeed, the general solution is now completely obvious: if there are n players, then there are n – 1 matches.[1]

This little puzzle comes from Paul Halmos (1985) who uses it to criticize the standard distinction between pure and applied mathematics. The normal distinction says, for example, that the statement "7 is a prime number" is piece of pure mathematics, while "7 apples are in the basket" is applied. Figuring out how many matches in a tennis tournament is straightforwardly then a case of applied mathematics. But Halmos dismisses this and says instead that you are doing applied mathematics when you calculate the number of tennis matches the first, clumsy way, and that you are doing pure mathematics when you do it the second, elegant way. What this comes down to is that if it's tedious, boring, or ugly, then it's applied. But if it's beautiful, elegant, or clever, then it's pure. Indeed, Halmos solemnly declares that general relativity and quantum mechanics are part of pure mathematics, while all applied mathematics is hopelessly ugly.

Halmos is on to something, but it can't be exactly what he claims. For one thing, we should uphold the existing pure/applied distinction, since, among other things, it captures the presumed necessity and a priority of mathematics. "7 is a prime number" is an a priori and necessary truth, while "7 apples are in the basket" is neither. Einstein's field equation and the Schrödinger equation, if true at all, tell us something contingent about the empirical world, neither is a necessary truth.

The true moral of the tennis example concerns what is and what is not of genuine mathematical interest and importance. That, I think, is the real lesson to be learned, the lesson that Halmos, I suspect, really wanted to convey. Often we cannot tell by considering a question, whether it will or won't be of mathematical interest in its own right. It is the answer to the question rather than the question itself that will sometimes surprise, delight, or mystify us, even if it comes out of a lowly problem such as calculating the number of tennis matches.

When it comes to science, a similar moral can be drawn. Being a pure or applied problem will give us no indication of the epistemic interest and importance of a particular piece of research. Details — the well-known domicile of the Devil — are often unique and always important. Details are what I want to discuss here, in particular, some details involving the privatization of medical research. The issues I wish to take up here are not conceptually deep but are challenging, nevertheless. I hope and expect, however, that those with an interest in pure epistemology will, like Halmos's pure mathematician, find the

https://doi.org/10.3366/epi.2004.1.1.49 Published online by Cambridge University Press

issues wonderfully intriguing. They are, in any case, challenging and of vital importance, since they effect the quality of our lives.

Details

Details and ideals are often at odds. The ideals of science are easier to describe and endorse than they are to observe. Open and free inquiry, shared results, an absence of taboo subjects, and so on, are part of the package. We often hear that "political correctness" is undermining these laudable aims by hampering research into sensitive subjects, such as race and IQ or gender and cognition. Though I'm sceptical, there may be a grain of truth to this, but this is as naught when compared to the amount of intellectual corruption brought about by commercial factors. Indeed, to be concerned with the former while ignoring the latter betrays a colossal ignorance of what is genuinely important both to science and to society.

Much has been written about the influence of commercial interests on science. There are a variety of ways in which science can be debased. Let me briefly review some of the corrupting influences that the increase in commercialization has brought. I'll begin with a case that is likely to confirm many of our a priori suspicions.

By 1998 slightly fewer than two thirds of published articles on the effects of secondhand smoking said that it is indeed harmful, a bit more than third claimed that it is not. Barnes and Bero (1998) wondered why review articles on the health effects of passive smoking reach different conclusions. They examined a large number of articles on this subject published in medical journals and tested a number of hypotheses. For instance, does time of publication of the article make a difference? Perhaps earlier pieces were less likely to find effects from second hand smoking, while more recent articles would be more likely to find a connection. No, this didn't seem to matter. Does being published in a peer-reviewed journal make a difference? No, that didn't seem to matter either. Perhaps the quality, the care, and extensiveness of the research as measured in various different ways made a difference? No, this didn't seem to matter, either.

According to Barnes and Bero "the only factor that predicted a review article's conclusion was whether its author was affiliated with the tobacco industry." (1998, 1599) They observe that "although 37% (39/106) of reviews in our study concluded that passive smoking is not harmful, 74% (29/39) of these were written by authors with tobacco industry affiliations." (ibid.) They further noted that near the time of their work a rash of tobacco sponsored articles had been published, prompting them to remark: "These findings suggest that the tobacco industry may be attempting to influence scientific opinion by flooding the scientific literature with large numbers of review articles supporting its position that passive smoking is not harmful to health." (ibid.)

The tobacco industry already has such a bad press that we are shocked but not surprised to discover that they are influencing research. Let me now turn to examples where the conclusion isn't so easily anticipated.

When a random drug X produced by company A is compared with drug Y produced by company B, we would expect X to prove better than Y about half the time in treating some specific health condition. That is, with no other information, we naturally expect that the results of testing could go either way. These may indeed be the actual results of serious scientific study — but they are not the results that get published. Remarkably, when the study in question is funded by one of the pharmaceutical companies, the sponsor's drug invariably does better. Richard Davidson, for instance, found that in his study of 107 published papers which compared rival drugs, the drug produced by the sponsor of the research was found to be superior in every single case. (Davidson 1986) When we think about the statistical chance of this, the mind boggles.

The Davidson study is typical; there are many others that come to similar conclusions. For instance, Friedberg et al (1999) found that only 5% of published reports on new drugs that were

https://doi.org/10.3366/epi.2004.1.1.49 Published online by Cambridge University Press

sponsored by the developing company gave unfavourable assessments. By contrast, 38% of published reports were unfavourable when the investigation of the same drugs were sponsored by an independent source.

Stelfox et al (1998) studied 70 articles on calcium-channel blockers, which are used to treat high blood pressure. They divided the articles into favourable, neutral, or critical in their evaluations of the products. The finding was that 96% of the authors of favourable articles had financial ties with a manufacturer of calcium-channel blockers; 60% of the authors of neutral articles had such ties; and only 37% of authors of unfavourable articles had financial ties. Incidently, in only two of the 70 published articles was the financial connection revealed.

With the details of these cases in mind, we rightly become worried about who is funding the research and what effects it is having, effects that are far from the spirit of ideal science and far from what the public thinks it is getting.

Liberal Regulators

Naturally, the potential range of sentiments on corporate involvement in research is extensive. We could imagine some people defending the view that there should be no private or corporate funding of research whatsoever, since it corrupts that research to such an extent. And we could imagine others who say all funding should be private, since scientific objectivity is independent of the source of funding. In defending this latter view, they might appeal to, and even take for granted, the well-known philosophical distinction between "discovery" (subjective source of ideas) and "justification" (objective testing). Corporate interests, this line of thinking goes may have some initial influence but will get filtered out in the process of objective, empirical theory testing.

Many others would defend something in between, happy to accept private funding for research so long as suitable safeguards are in place to ensure that objective evaluation is indeed objective. Such a view is held by Robert M. Berdahl, Chancellor of the University of California at Berkeley. Since Berkeley is one of the great research universities of the world, the sentiments of its leading policy maker are of paramount interest.

It's probably important to know that Berdahl is a liberal-minded administrator, the sort that typical faculty members want as a leader and in most major universities actually have as a leader. It's an appealing picture he paints, almost romantic. In his survey of the history of American universities he speaks warmly of their growing democratization, especially because of such things as the land-grant system of the 19th Century and the G.I. Bill after WWII. He even speaks warmly of the Free Speech Movement of the 1960s in his own university and he expresses dismay at the right-wing attacks on universities in recent years, as represented by such books as Alan Blum's The Closing of the American Mind, Roger Kimball's Tenured Radicals, and Dinish de Sousa's Illiberal Education. These and several similar works charged that American academics were dangerous leftists pushing their "politically correct" and dogmatically liberal views on innocent students. Another right-wing assault involved the creation of conservative think tanks and foundations to fund conservative academic projects. The Olin Foundation, the Free Enterprise Institute, and the Heritage Society, are just a few. Berdahl remarks: "In what has been an extremely clever and effective political campaign, these conservatives have preached a conservative doctrine that is relatively doctrinaire, while managing to persuade the public that universities are themselves held captive by liberal ideologues who countenance no other points of view. And they have succeeded in broadening the curriculum of business schools and other professional schools with a less liberal curriculum." (Berdahl 2000)

From these concerns, Berdahl turns to the problems of commercialization and privatization of university life. The industry-university "partnership," as he and so many others like to call it, has grown enormously in the past few years. In the 1980s the Reagan and Thatcher governments cut support for public services, including universities. Desperate to stay afloat, universities turned to private sources of funding. And conservative governments

https://doi.org/10.3366/epi.2004.1.1.49 Published online by Cambridge University Press

eagerly encouraged them to do so. This has led to numerous problems. Berdahl acknowledges the controversy that has arisen, especially in cases such as that involving Novartis. He could hardly avoid it, since the Novartis case is one of the most notorious and involves his own university.

Berkeley's Plant and Microbial Biology Department signed a deal with the large agricultural company, Novartis, in which the department received $5 million per year for five years to support research. In return Novartis is given the right to licence any patents held by the university up to one third of the patentable intellectual property developed by that department. One of the most contentious points is that the five person committee that will review the applications for the $5 million in annual grant money will have two members appointed by Novartis. Berhahl stresses that Novartis does not have majority control. Others who are less sanguine quite rightly stress the significant surrender of autonomy. The ways in which this autonomy has been surrendered are many and complex. They need to be explored and exposed, but not here.

Berdahl defends this agreement on the grounds that "the Novartis arrangement is not qualitatively different from other arrangements into which faculty are permitted to enter with private companies." (ibid.) As justifications go, this is surprisingly weak. The sad truth, however, is that the Novartis deal is all too typical and represents a trend that is growing with alarming speed. Berdahl realizes there are dangers that accompany private funding and for-profit research, but he thinks "there is no going back." Instead, he offers some mild cautions. He notes, for instance, that the humanities have been seriously undermined by this process. This has not only hurt people in the humanities as far as their wages go, but it has undermined the importance of the humanities, and this, he rightly stresses, is a particularly bad thing at a time when value issues are of paramount concern with, for example, the rise of various biotechnologies that raise important ethical issues.

To put things this way, however, is to miss the point, at least in part. There certainly are important moral concerns. But there are epistemic concerns, too, and they may be considerably more pressing. We may well be getting second-rate science, thanks to the influence of market forces on research. Indeed, I'm sure we are. To be fair to Berdahl, however, he does worry explicitly about the direction and objectivity of research. Can privately funded research be trusted to be objective, or will it invariably arrive at results that suit the sponsor's commercial interests? Will research goals be those involving the general public, or only those goals that are profitable? Berdahl is perfectly right to worry about such things. My quarrel with him is over the extent and the seriousness of these problems. He would allow existing arrangements to persist but in a more regulated environment. I would not, but rather would put an end to the profit-driven structure of research.

He says "there is no going back." I don't know how seriously to take this remark. Critics of any problematic situation are often accused of "utopianism" or of "living in the past." Such hollow defences are merely evasive. If, as he says, "there is no going back," then let us at least strike out in an entirely new direction. We need very much more than hand-wringing by a liberal-minded man of good will. For our universities to continue on the current course is significantly to abandon any hope of uniformly high quality research, which along with teaching is our very reason for being.

When Did the Past End?

It may seem strange to ask when the past ended. But there is a definite answer. It ended in 1980 when the US Congress passed the Bayh-Dole Act. It's impact has been enormous, greater even than the Reagan-Thatcher assault. This act allows private corporations and universities to reap the rewards of publically funded research. Before Bayh-Dole there were only a couple of hundred patents annually stemming from university research in the US. Now the annual number is in the several thousands. As all who live outside the US know,

https://doi.org/10.3366/epi.2004.1.1.49 Published online by Cambridge University Press

American ideas and practices spread quickly. Good ideas are copied. Not so good ideas are adopted, too, often courtesy of the World Trade Organization, the World Bank, or the International Monetary Fund, perhaps in the name of "free trade" or a "level playing field." In short, a pattern of commercialized and profit-driven research is being forced on us all.

Before Bayh-Dole, an entirely different attitude prevailed. Yale University, for instance, once proclaimed: "It is, in general, undesirable and contrary to the best interests of medicine and the public to patent any discovery or invention applicable in the fields of public health or medicine." That was in 1948, not so long ago as any baby-boomer can attest. Now Yale holds lots of patents including one on an anti-AIDS drug. It's a patent that they share with Bristol Myers and enforce to the disadvantage of the 6000 people each day who die in Africa from AIDS because they can't pay the royalties.

Journal Reaction

The editors of several leading biomedical journals had similar worries about corporate influence, so they got together recently to forge a common editorial policy that was published simultaneously in several journals.[2] The editors wished to protect their journals from being a "party to potential misrepresentation" and were especially worried about contract research where participating academics do not have access to the full range of data that play a role in the final version of the submitted article. The new guidelines are part of a revised document known as "Uniform Requirements for Manuscripts Submitted to Biomedical Journals"[3], a compendium of guidelines used by many leading biomedical serial publications. Here in outline are some main points:

- Authors must disclose any financial relations they have that might bias their work. For example: are they shareholders in the company that funded the study or manufactures the product, are they paid consultants, etc.? At the journal editor's discretion, this information would be published along with the report.
- Researchers should not enter into agreements that restrict in any way their access to the full data, nor should they be restricted in contributing to the interpretation and analysis of that data.
- Journal editors and reviewers should similarly avoid conflicts of interest in the peer review process.

These guidelines, if rigorously enforced, should go a long way to helping improve the situation. But notice how little journal policy will matter when it comes to problems such as self-censorship. The new regulations will — at best — ensure that any work that does get done and reported meets a certain threshold of scientific legitimacy. But the problem with research self-censorship is that some types of work won't get done at all or, if done, won't be reported. And there is nothing that any journal can do about that. I'll return to this point below.

There is another epistemic problem that arises from a lack of alternative theories that are serious rivals. It is to this type of problem that I know wish to turn. The point is so obvious, it hardly needs mentioning, yet, it is of prime importance. Corporations understandably want a return on their investments. The pay-off for research comes from the royalties generated by patents. This means corporations will tend only to fund research on things that could in principle result in something protected by intellectual property rights. Other kinds of information are financially useless and might even be financially harmful.

Imagine two ways of approaching a health problem. One way involves the development of a new drug. The other way focusses on, say, diet and exercise. The second could well be a far superior treatment, both cheaper and more beneficial. But obviously it will not be funded by corporate sponsors, since there is not a penny to be made from the unpatentable research results. It should be just as obvious that a source of funding that does not have a stake in the outcome but simply wants to know how best to treat a human ailment, would happily fund both approaches. Public funding is clearly the answer to this problem.

https://doi.org/10.3366/epi.2004.1.1.49 Published online by Cambridge University Press

Even within patentable research, some areas will be less profitable than others. Consequently, diseases of the poor and the third-world are going relatively unexplored, since the poor cannot afford to pay high royalties. Research is skewed; privately funded researchers choose to avoid some medical problems, however scientifically interesting and socially important, because they are financially unrewarding.

Deliberate Ignorance

There is considerable evidence that the tobacco industry took legal advice to the effect that they should not do any research into the possible harmful effects of tobacco. Had they come to know of any harmful effects, this would have greatly increased their liability. When such information came into their hands, they naturally tried to suppress it. But obviously, from their viewpoint, it is better not to know about it in the first place. Given the potential law suits over liability, ignorance is bliss. Interestingly, many of the lawyers who advised the tobacco industry also advise pharmaceutical companies. The legal firm of Shook, Hardy and Bacon, for instance, advises both the tobacco industry and Eli Lilly. This legal strategy assumes a distinction philosophers know well — the distinction between discovery and justification, which I had occasion to mention above. The thinking seems to be that vague suggestions that tobacco causes lung cancer or that SSRIs (I'll explain below) sometimes induce suicide are just that — vague. And it does not constitute evidence. For that, they claim, we would need extensive clinical trials. Since these trials have not been done, we simply have no evidence at all. There is no justification, according to them, for making these claims about the harmfulness of tobacco or SSRIs.

There is a great deal of naivety about so-called scientific method. It often seems like willful ignorance. Some researchers claim that full randomized clinical trials are necessary and sufficient for definite knowledge, and that anything short of such trials is useless. This all or nothing attitude is ridiculous. After all, we choose which clinical trials to run on the basis of plausibility, where circumstantial and anecdotal considerations play a role. This too is evidence, though obviously not as strong. Often, though, these plausibility considerations are enough — or should be enough — to launch a serious study. Refusing to take action on the grounds that there is nothing but "anecdotal evidence" is not only bad philosophy of science, it can also be criminal. Even Nazi scientists established a strong smoking-lung cancer link in the 1930s. (Proctor 1999) Commercial interests elsewhere stood in the way for decades, during which millions died.

Above I mentioned SSRIs. Selective serotonin re-uptake inhibitors, as they are called, are widely used for depression. Prozac is the best known of these. SSRIs have been linked to suicide. People seem most susceptible to suicidal impulses when beginning treatment. In some cases it may be that SSRIs actually improve depressed people's condition to the point of suicide. This sounds paradoxical. What may happen is that extremely depressed people are sometimes in a near "vegetative" state. The SSRI will improve their condition to the point where they have the energy and the wherewithal to commit suicide. In any case, the link to suicide is something a profit-seeking corporation would prefer not to know about and is very reluctant to investigate.

David Healy is a British psychiatrist, currently the Director of the North Wales Department of Psychological Medicine at the University of Wales. He has spoken and written extensively on mental illness, especially on pharmaceuticals and their history. (See, for example, Healy 2001and 2003.) In September 2000, he was offered a position as Director of the Mood and Anxiety Disorders Program in the Centre for Addiction and Mental Health. CAMH, as it is know, is affiliated with the University of Toronto. As part of the deal, he was also appointed as Professor in the University's Department of Psychiatry. Healy accepted this appointment. Before he actually moved permanently to Canada, he took part in a Toronto conference in November, 2000. He gave a talk that was quite critical of the pharmaceutical industry.

https://doi.org/10.3366/epi.2004.1.1.49 Published online by Cambridge University Press

Among other things, he claimed that Prozac and other SSRIs can cause suicides. Within days of his talk at Toronto, CAMH withdrew the appointment. Healy was, in effect, fired before he started. Needless to say, this led to quite a scandal. What happened? There are, of course, rival views. The official view coming from CAMH is that they realized they had made a mistake, that Healy would not be a suitable appointment for purely academic reasons. Another view suggests that pressure from Ely Lilly did him in. Yet another opinion is that Lilly did not put any pressure on, but that self-censorship was at play. Lilly contributes financially to CAMH, a fact that lends some credence to the latter speculations.

But we needn't worry about the details of the Healy case. The crucial thing is that it is not in Ely Lilly's interest to "know" that Prozac causes suicide, since that would increase their potential liability. If work is to be done on this issue, it will have to be publicly funded.[4]

Creation of a New Medical Condition

Eli Lilly is currently promoting a product called Sarafem for those who suffer from PMDD. What, you may ask, is PMDD? What, indeed! Premenstrual dysphoric disorder is an updated version of PMS, premenstrual syndrom. This mental disorder is alleged to affect some women in the so-called luteal phase of the menstrual cycle, just before the onset of menses. The American Psychiatric Association has not yet accepted this disorder, but does list it in the appendix of the bible of this field, Diagnostic and Statistical Manual of Mental Disorders, known in its latest version as DSM-IV. To be diagnosed as having PMDD a woman must have five or more of the following eleven symptoms which characterize the disorder.

- markedly depressed mood
- marked anxiety
- marked affectivity
- decreased interest in activities
- feeling sad, hopeless, self-deprecating
- feeling tense, anxious, or "on edge"
- persistent irritability, anger, and increased interpersonal conflicts
- feeling fatigued, lethargic or lacking in energy
- marked changes in appetite
- a subjective feeling of being overwhelmed or out of control
- physical symptoms such as breast tenderness, swelling or bloating.

Sarafem's active ingredient is fluoxetine hydrochloride, exactly the same active ingredients as Prozac, the anti-depressant, also made by Eli Lilly. However, Lilly is definitely marketing Sarafem in a different way. Speaking for the company, marketing associate Laura Miller said: "We asked women and physicians about the treatment of PMDD, and they told us they wanted a treatment option with its own identity that would differentiate PMDD from depression. PMDD is not depression. As you know, Prozac is one of the best-known trade marks in the pharmaceutical industry and is closely associated with depression. They wanted a treatment option with its own identity."[5] What then, we might wonder, is the difference between taking a dose of Prozac and a dose of Sarafem? It's 20 mg of fluoxetine hydrochloride either way — though the pills have changed colour from green to a shade of pink (were you surprised?). "The difference," according to Miller, "is that PMDD is a distinct clinical condition different than depression. PMDD is not depression. PMDD is cyclical — women suffer from PMDD up to two weeks before their menses, and the other two weeks of the month they don't have the symptoms of PMDD." (ibid.)

The promotion of Sarafem comes at a time when Lilly is about to lose much of its patent protection on Prozac. It's hard to resist speculation on the connection. Patent laws will protect a discovery if it's a distinct new use of an already existing entity. For example, if I were to discover that fluoxetine hydrochloride is very good for unclogging blocked pipes, then I might be able to secure a patent and start selling Jim's Miracle Toilet Cleaner. For patent protection, then, it's crucial that Lilly find a new use for fluoxetine hydrochloride. If PMDD is depression, they are out of luck.

https://doi.org/10.3366/epi.2004.1.1.49 Published online by Cambridge University Press

Once again, the philosophical moral is evident. Through clever marketing, advertizing, and public relations, Lilly is creating a disease. How this takes place is an enormously curious and philosophically interesting thing. I think it is safe to say that PMDD did not exist in the past. But I fear that it might start to exist in the near future. Let me clarify what I mean.

The tame sense of saying Lilly is "creating" a disease is the sense in which they are merely getting us to believe that such a disease exists. There is no doubt that they are doing this. But there may be a deeper sense in which they are creating the disease. Ian Hacking (1995, 1998) has investigated so-called "transient mental illness." These diseases are not just transient in an individual, but in a society. Multiple personalities, mad travellers, anorexia are likely examples (though debatable, of course). The mental disease comes rather suddenly and spontaneously into existence at a specific time and place, and just as quickly disappears.

In brief, there are three interesting possibilities.

1. The disease PMDD has always existed; Lilly is merely bringing this recently discovered fact to light (and promoting Sarafem as a way of treating it).
2. The disease does not exist, but Lilly is trying to get us to believe that it does, anyway, (since that will lead to sales of Sarafem).
3. The disease has not existed in the past, but the public relations activities of Lilly will create the disease (and that will lead to sales of Sarafem).

In the first case we could be thankful to Lilly. A problem will have been brought to our attention and a remedy provided. Of course, there is no need to be too thankful, since publicly funded medicine could also have made such a discovery and provided such a remedy — without our having to pay steep royalties. If (2) or (3) is the case, then the dangers of private funding of research are manifestly clear.

What Is To Be Done?

We should note that examples such as the one I've discussed here are grist for the mill of any social constructivist. Though I think we should have little sympathy for the anti-realism and epistemic relativism of serious social constructivists, there can be no doubt that cases of highly commercialized research support their case. The constructivist outlook may be wrong in general, but there are important instances where it provides the right analysis.[6] These are often cases where we, as concerned citizens, need to take some sort of political action to set things right.

Broadly speaking, I see three ways of dealing with the epistemic problems stemming from the commercialization of research:

1. Deny it, ignore it, or just live with it.
2. Introduce regulations to cope with it.
3. Introduce full public funding and eliminate commercial interests for all medical research.

The first of these does not even merit comment. The other two should both be taken seriously.

The regulation route has much to recommend it. Many of the problems with the commercialization of research can be mitigated by carefully constructed rules that are rigorously enforced. The biomedical journal policy that I described above, for instance, will go a long way toward catching and preventing many of the more blatant abuses that have arisen. But there are problems that such regulations cannot solve and it is crucially important to make this known.

I mentioned one problem above with the new journal policy requiring financial disclosure and full availability of all data to the researchers involved; I will reiterate it now. The new regulations will ensure that any work that does get published meets a certain threshold of scientific legitimacy. But the epistemic problem stemming from commercialization is that some types of work simply won't get done, or if done, won't get reported. I stress that this is quite different than the problem of questionable or even fraudulent reporting that the journal policy

https://doi.org/10.3366/epi.2004.1.1.49 Published online by Cambridge University Press

is designed to combat. Regulation might be able to control that. But there is nothing that any journal or any regulatory body can do about self-censorship. They can only check what is submitted. They cannot oversee or referee work that isn't done or isn't submitted, no matter what the reason. Valuable information is being suppressed.

I've been hinting along the way at the solution to these very serious epistemic problems — a return to public funding for all medical research. Let me know be specific. I have two recommendations.

- Eliminate all patents and other intellectual property rights in the area of medical research.
- Increase government funding to cover the shortfall that will result from the withdrawal of private funding.

These are, I should expect, both rather clear and obvious points, so there is no need for me to spell out what they mean. Nor is there any need for additional justification, since the foregoing examples and analysis has, in effect, been an argument for them. I should, however, address the question of feasability. Given the size and influence of private involvement in medical research, calling for an exclusively public science may seem a radical step. Perhaps it is, but there is an obvious analogy that makes it seem a little less extremist. Any sensible person agrees (I hope) that socialized medicine is unquestionably better than its free-enterprise alternative. And even though it would be a very radical change for some countries — the US in particular — to switch to socialized medicine, knowledgeable people who have not been corrupted by the private medical industry think the overhaul would be a very good thing. In calling for the socialization of medical research I am calling for something less radical than a complete overhaul. In fact, my proposal should be seen as a mere extension of existing national health services. Socialized research is the only way to solve the spectrum of problems associated with privately funded research. From a methodological view it is, quite possibly, the only proper way to proceed in applied health research.

Scientific Socialism

Questions such as "What is to be done?" invoke the memory of Lenin. This was not intended, but since the thought has arisen, I might as well dwell on a related matter for a moment. It may seem that my insistence on being involved in policy decisions and the particular policy advocated is a mere reflection of various values that I happen to hold. Perhaps. But there is another way to consider this epistemic issue, a way in which values don't play a particularly important role at all.

The expression "scientific socialism" might be taken as a fair description of the view of medical research I have been urging. But those who have some acquaintance with Marxism may conjure up a different image. Marx and Engels famously distinguished their outlook from utopian socialism. The difference is quite important. The motivation for utopian socialism is primarily moral. It is promoted by those who are outraged — as they should be — by poverty and social injustice. Social change, if it comes, would be an ethical response to the horrors of the prevailing situation. The scientific socialism of Marx and Engels takes quite a different view. Capitalism, according to them, contains the seeds of its own destruction. Capitalist competition leads inevitably to the impoverishment of the working class and to the creation of monopolies. Socialism will grow out of this state of affairs in a perfectly natural and inevitable way. Though morally superior, socialism, according to any traditional Marxist, is not the result of moral choice, but rather the result of something akin to a law-like process. I see the problems involving medical research in a somewhat similar way.

Scientific method is not fixed for all time, but rather seems to evolve, often under the influence of scientific discoveries. Not all of it, perhaps, but parts at least. For instance, the discovery of placebo effects led to the introduction of blind tests. That is, the discovery of a fact led to the institution of a norm: In situations of such and such a type, one ought to use blind tests. Though this is a norm, it is not what ordinarily would be called a value. That is, it is not a social or moral value, though it is certainly an epistemic one. The practice of blind testing, as

https://doi.org/10.3366/epi.2004.1.1.49 Published online by Cambridge University Press

even one who holds a conservative view of science, such as Larry Laudan, has repeatedly stressed[7], is best seen as science simply adopting what it has itself established as appropriate methodology. An ought cannot be derived form an is, but facts and norms are not wholly disconnected, and sometimes they are intimately tied.

We can view the current situation in medical research in a similar light. We have learned empirically that research sponsored by commercial interests leads to serious problems, so serious that the quality of that research is severely degraded. The elimination of patents and the switch to public funding solves many, if not all, of these epistemic problems. So, as an epistemic norm we advocate public funding for medical research. This chain of inference is no different than, first, discovering the placebo effect, next, discovering that blind testing can overcome the difficulties that the placebo effect involves, and then as a result of all this, adopting the methodological norm of employing blind tests.

This is quite different than advocating public funding for moral reasons. The policy I urge is motivated by epistemology. The contrast is not unlike the contrast between utopian and scientific socialism. It is not moral outrage — though I cheerfully admit its presence — but rather the internal logic of capitalism or of current medical research that drives things along.

There is, however, one glaring disanalogy. According to Marx's scientific socialism, the internal logic of capitalism leads inevitably to socialism. However, the internal logic of current medical research does not lead inevitably to socialized research. It still requires political action to bring it about. And that is anything but inevitable. It is at this point that philosophical argument in the public domain becomes essential. I am optimistic about the long haul, since good medical research is in the interest of far too many people, though I would expect a long, hard battle.

Let me conclude by repeating that one's advocating public funding for research in the health sciences might appear to stem from one's socialist ideology. This is so in part, but it is much more than that. The need for patent-free public funding in medical research is like the need for blind testing. It is, to put it simply, an epistemic discovery. The fact that scientific socialism, as I am here calling it, harmonizes so well with one's moral sense is merely a very happy accident.

---

https://doi.org/10.3366/epi.2004.1.1.49 Published online by Cambridge University Press

Bibliography

Barnes, Deborah E. & Lisa A. Bero (1998) "Why Review Articles on the Health Effects of Passive Smoking Reach Different Conclusions", Journal of the American Medical Association, 279 (19), 1566-70

Berdahl, RM (2000) "The Privatization of the Public Universities", http://cio.chance.berkeley.edu/chancellor/sp/privatization.htm [accessed 26.6.2001]

Brown, James Robert (2000) "Privatizing the University — The New Tragedy of the Commons", Science, Dec 7

Brown, James Robert (2001) Who Rules In Science: An Opinionated Guide to the Wars, Cambridge, MA: Harvard University Press

Davidson, R (1986) "Sources of Funding and Outcome of Clinical Trials", Journal of General Internal Medicine, vol 12, no 3, 155-58.

Friedberg, et al (1999) "Evaluation of Conflict of Interest in New Drugs Used in Oncology", JAMA 282, 1453-7.

Hacking, I (1995) Rewriting the Soul, Princeton: Princeton University Press

Hacking, I (1998) Mad Travelers, University of Virginia Press

Halmos, P (1986) I Want To Be A Mathematician, New York: Springer

Healy, D (2001) The Creation of Psychopharmacology, Cambridge, MA: Harvard University Press

Healy, D (2003) Let Them Eat Prozac, Toronto: Lorimer

O'Meara, K (2001) "Misleading Medicine", http://insightmag.com/archive/200104301.shtml [accessed 1.6.2001]

Proctor, R (1999) The Nazi War on Cancer, Princeton: Princeton University Press

Stelfox, HT (1998) "Conflict of Interest in the Debate over Calcium-Channel Antagonists," New England Journal of Medicine, vol 338, no 2 (Jan.8), 101-06.

Notes

[1] The mathematically trained might have noticed that 1025 – 1 = 210 and focussed on this as relevant to solving the problem. You were deliberately fooled.

[2] For instance, in Lancet, vol 358, September 15, 2001, 854-856.

[3] Available on line at http://www.icmje.org/index.html

[4] At the time of writing (early 2004), the connection between SSRIs and suicide is becoming a public issue. A televised report on the Canadian Broadcasting Corporation in March 2004 went so far as to say Healy was "vindicated."

[5] Quoted in O'Meara (2001)

[6] For a discussion of social constructivism, see Brown (2001).

[7] For example, Laudan, Science and Values.

James Robert Brown is Professor of Philosophy at the University of Toronto. His research interests include philosophy of science, philosophy of mathematics, foundations of physics, and the social relations of science. His publications include *Who Rules in Science? An Opinionated Guide to the Wars* (2001); *Philosophy of Mathematics: An Introduction to the World of Proofs and Pictures* (1999); *Smoke and Mirrors: How Science Reflects Reality* (1994); *The Laboratory of the Mind: Thought Experiments in the Natural Sciences* (1991); and *The Rational and the Social* (1989).

https://doi.org/10.3366/epi.2004.1.1.49 Published online by Cambridge University Press