EPILOGUE

Nearly seven years have passed since that enlightening episode with Stephen Hawking's soup. Now I'm watching another scientist eat – and again, it's rather distracting. This time I am in the cafeteria of the Medical Research Centre at Cambridge University, and newly crowned Nobel laureate Venkatraman Ramakrishnan has just sat down at the next table. He is tucking heartily into a banana, and I'm wondering whether I should grab the opportunity to interview him before he wanders back to his lab.

With a little effort, I pull my focus back to Michael Fuller, the man sitting at the table with me. Fuller is the technician who built Crick and Watson's famous model of DNA. He is telling me about the years he spent working with Francis Crick, but he can see that I'm not quite giving him my full attention. Fuller, a warm, generous, walking smile of a man, sees my predicament. 'Do you want to go and talk to Venki?' he asks.

I think about it, then dismiss the idea. 'I might catch him later,' I say. But I know that I probably won't bother. Ramakrishnan won't be able to tell me the secret of how to win a Nobel Prize. The scientists themselves rarely can. Anyway, it's not that secret that I

am really here to uncover. Ten days after Crick's death in 2004, a British journalist called Alun Rees published a 'scoop' that he had been sitting on for years. Crick, Rees reported, had been high on LSD at the moment when he and James Watson had discovered the structure of DNA.

It is certainly not impossible: Crick was not shy of drugs. In 1967 he signed a letter to the London *Times* – other signatories included Paul McCartney and Graham Greene – that called for a reform of the drug laws. Around the same time he helped found a pressure group seeking to get cannabis legalised. The group was called Soma, after the socially acceptable mind-altering drug that featured in Aldous Huxley's *Brave New World*. Huxley was a great advocate of the use of LSD and mescaline, among other pharmaceuticals.

Rees said in his article that it was while moving in this circle that Crick met Richard Kemp, a young biochemist who went on to develop a new and extremely efficient way to manufacture LSD. According to Rees, Crick had been Kemp's inspiration: Crick had told Kemp that LSD had enabled him to see the structure of DNA, and that all the academics in Cambridge were using it to free their minds. Unfortunately, Rees got this story from a 'friend' of Kemp's. When he asked Crick directly about his LSD use, Rees reported, Crick 'listened with rapt, amused attention … He gave no intimation of surprise. When I had finished, he said: "Print a word of it and I'll sue."'

The truth is hard to uncover now. Rees's evidence that Crick used LSD to discover the secret of life comes third hand, from unreliable sources, and entirely uncorroborated. Neither Crick nor Watson ever made any reference to it. In his biography of Crick, Matt Ridley gives the idea that Crick used LSD to open his mind to the structure of DNA a summary dismissal. According to Ridley, both Crick's widow and the man who supplied the couple with LSD assured him that their first encounter with the hallucinogen came in 1967. What's more, Ridley says, the drug was 'barely available' in the UK in 1953. The notion that the 'then impoverished and conventional Crick would have had access to LSD when it was newly invented in the early 1950s' is implausible, Ridley argues: 'there is simply no evidence for it at all'.

Something about this doesn't quite ring true, though. LSD wasn't 'newly invented' in the 1950s; it was first synthesised in 1938. By 1947 the pharmaceutical firm Sandoz was marketing LSD under the trade name Delysid as a useful drug for psychotherapy. According to David Nichols of Purdue University, who has researched the history of LSD for the Royal Society of Chemistry, Sandoz made it 'readily available to scientific and clinical investigators for medical research' until the early 1960s.

The idea that Crick, a signatory to a public call for the legalisation of cannabis, was 'conventional' is somewhat laughable – he was anything but. When the Queen came to Cambridge to open the new Medical Research Centre building in 1962, Crick stayed away in protest: he was staunchly against the monarchy, and some years later he refused a knighthood. Crick's house parties were legendary for their wild drunkenness. And he was an inveterate womaniser. One secretary tells of being chased round the laboratory benches by a randy Crick; when he caught her she had to stab the stiletto heel of her shoe into his foot in order to escape. Then there's the fact that the 'conventional' Crick regularly smoked pot and used LSD later in life. Ridley reports that Crick found LSD's effects 'fascinating'.

All of this is enough to make me slightly suspicious of Cristof Koch's testimony. Koch, a University of California neuroscientist who describes Crick as his 'mentor', says that although their conversations were very wide-ranging, Crick never mentioned taking LSD. 'He told me about a lot of private things, including his parties. But not once about any serious drug use,' Koch says.

I did say they were *secret* anarchists. Which is why I am hoping that Fuller might be able to shed some light on the situation. He worked with Crick and Watson for years, attended some of those parties, and went out to buy the champagne on the day of the Nobel Prize announcement. He saw them in almost every situation they faced – does he know about any LSD use? He shakes his head. 'But,' he says, 'Knowing Francis ... I imagine he would have experimented if he'd had the chance.'

I imagine so too. We have seen that scientists will do anything in their pursuit of discovery, and Crick and Watson were certainly getting desperate. The American Linus Pauling was closing in on the structure of DNA. Rosalind Franklin and Maurice Wilkins were bickering, and being slow to come forward with the data Crick and Watson wanted – so slow, in fact, that Crick and Watson stole what they needed from Wilkins' lab. When Wilkins complained, Crick told him to 'cheer up and take it from us that even if we kicked you in the pants it was between friends. We hope our burglary will at least produce a united front in your group.'

The evidence of anarchy piles up. Crick and Watson weren't even meant to be working on DNA: their boss at Cambridge had told them to stop. They gave a shrug, went underground and carried on in secret. And Crick's attitude to scientific propriety is clear from his pronouncements years later. In 1979, amid many accusations that Franklin wasn't accorded enough credit, Crick declared that she didn't have what it takes to be a top-class scientist. She was 'too determined to be scientifically sound and to avoid shortcuts', he wrote in 1979. Soon after that, he repeated his belief in the merits of scientific anarchy. 'First-class scientists take risks,' he said. 'Rosalind, it seems to me, was too cautious.' One more piece of anarchy, a little hallucinogenic help in visualising the structure of DNA, would hardly have made a difference.

It is not as if this would have made Crick a one-off. As we have seen, LSD seeded Kary Mullis's Nobel Prize. Another Nobel laureate, the physicist Richard Feynman, enjoyed marijuana and LSD (but had already done his best work before he tried them). The cosmologist Carl Sagan was also a regular user of cannabis, and describes many experiences of seeing things in a new way when stoned. His insights were so profound to him that he made tape recordings to try to persuade his 'down' self to take them seriously the next day:

> If I find in the morning a message from myself the night before informing me that there is a world around us which we barely sense, or that we can become one with the universe, or even that certain politicians are desperately frightened men, I may tend to disbelieve; but when I'm high I know about this disbelief. And so I have a tape in which I exhort myself to take such remarks seriously. I say 'Listen closely, you sonofabitch of the morning! This stuff is real!'

Sagan was open to the idea that drug-induced experiences would help his research. His best friend was Lester Grinspoon, a professor of psychiatry at Harvard. They used to get high together, and Grinspoon remembers Sagan asking him for his last 'bud' to help him with the following day's research. 'Lester, I know you've only got one left, but could I have it?' Sagan said. 'I've got serious work to do tomorrow and I could really use it.'

It has to be admitted, though, that while Sagan said that pot improved his appreciation of many things – including, oddly enough, potatoes – there is little evidence that his cannabis use had any impact on his scientific work. He describes one occasion when he was able to recall seemingly irreconcilable experimental facts when stoned, and coming up with something that might pull them together, but admitted that it was 'a very bizarre possibility'. He wrote a paper that mentioned the idea. 'I think it's very unlikely to be true,' he later wrote, 'but it has consequences which are experimentally testable, which is the hallmark of an acceptable theory.'

Hardly a moment of blinding revelation, then. But neither do we have any blinding revelation when it comes to Francis Crick's use of LSD in the 1950s. There is no solid evidence, just conflicting testimony and the presence of a personality that would almost certainly have used anything he could to steal a march on his competitors. We are back to that mantra again: anything goes. When I started this project, Feyerabend's idea that 'anything goes' in science seemed like a glimpse of its dark side. Now, having explored the lengths to which scientists will go in the pursuit of discovery, it has become apparent that 'anything goes' is a virtue – the secret of science's success.

Some of the stories about science in this book might be shocking, but hopefully it is now clear that science often progresses in ways that defy our usual notions of what scientists get up to. And the anarchists have made important discoveries. Einstein might never have proved without doubt that $E = mc^2$, but that doesn't mean it isn't true. What's more, our understanding of the interplay between mass and energy helped to bring about the end of the Second World War. Even more important were fortuitous discoveries that led to the Allies' ability to engineer the atomic bomb ahead of the Nazis. The scientists involved can't exactly take credit for those discoveries – they don't know quite how some of them happened – but they grabbed them with both hands and used them to make the world a better place.

Anything goes; science does what it needs to do. Barry Marshall infected himself with a hazardous dose of bacteria because he was frustrated at the suffering of others – his identification of the cause of stomach ulcers was ultimately selfless. Werner Forssmann tricked and lied his way into a hospital operating theatre because he suspected that, if he had access to its equipment, he could improve our understanding of the heart and find ways to treat otherwise untreatable conditions. Stanley Prusiner couldn't prove that prions existed, but he was convinced that the concept would help researchers to fight the ravages of a swathe of brain diseases. If his colleagues objected to the way he went about it, then that was a small price to pay for new insights that might lead to the prospect of cures for Alzheimer's or Parkinson's disease.

Even the fights, the injuries and the injustices have their purpose. If you want to rise to the top, you and your scientific insight have to be bomb-proof. Any big new idea and its proponent both have to survive so much violence, and unseat such strongly rooted predecessors, that, if they make it through to widespread acceptance, we can be as sure as is possible that they are correct. Most of us are the unwitting beneficiaries of this gladiatorial process. That is why we unhesitatingly board aeroplanes or take aspirin: science is trustworthy. But few of us are aware of the cost at which that trust is achieved. The strange thing is that the scientists would rather you remained in the dark.

When James Watson published an autobiography in 1968, Francis Crick and Maurice Wilkins, his co-recipients of the 1962 Nobel Prize, were furious. According to Matt Ridley, that was because *The Double Helix* took readers into 'the messy, competitive, error-strewn, naughty, human business of grappling with ignorance, rather than to describe science as a stately march towards discovery by paragons'.

This same issue was explored by Peter Medawar throughout his writings. 'It is a layman's illusion,' he said, 'that scientists caper from pinnacle to pinnacle of achievement and that we exercise a Method that preserves us from error.' But, for all his brand-busting honesty, Medawar was in no doubt about science's ability to reach those pinnacles:

> In terms of the fulfilment of declared intentions, science is incomparably the most successful enterprise human beings have ever engaged upon. Visit and land on the moon? A fait accompli. Abolish smallpox? A pleasure. Extend our human lifespan by at least a quarter? Yes, assuredly, but that will take a little bit longer.

Medawar was being unduly conservative with that last statement. Over the past two hundred years, human life expectancy has doubled in the developed world, thanks to advances in our understanding of healthcare and nutrition. Can we prolong it still further? Quite possibly. Cambridge University's Richard Smith, an expert on population dynamics, points out that each time a natural limit has been suggested, it has been exceeded. In the 1920s, life expectancy in the United States was around fifty-seven years, and the best estimates were that this could be extended by around seven years at most. In 1990, the experts said that, without major breakthroughs in slowing down the rate of ageing, average life expectancy could not exceed eighty-five years. Only six years later, Japanese women left that upper limit behind. Smith wryly notes that the United Nations has now abandoned the practice of estimating upper limits on life expectancy.

Not that these successes of science are problem-free. In a world with a population of seven billion and rising, all kinds of issues, such as food production, housing and healthcare, pose unprecedented challenges. Nonetheless, this is the world that science has created – the world we asked science to create – and the secret anarchists have risen to the occasion. Whether these successes can continue, whether science can solve the next set of problems, will depend on whether we are willing to let the anarchy come out into the open. Science has achieved much during its period of covert action. But can we now, in the light of what we have learned about how science really works and how it has been so misguidedly rebranded, set up a better system?

Take peer review, for example, today's gold standard for scientific publishing. This procedure, where ideas and results are examined by suitably qualified scientists before being published, was not always a standard route to publication. The modern publication system evolved from exchanges of letters between scientists. If one scientist had something to say to another – it never used to be about letting *everyone* know – they would write them a letter. Eventually, as science grew and the letters needed to be distributed to more and more people, the practice of publishing letters for everyone to read was born.

At first, peer review was not part of this system. Einstein was certainly unused to peer review – in the latter part of his career, when peer review had started to become fashionable, he railed against having to modify a paper to meet the objections of his colleagues before he could publish it. 'I see no reason to address the – in any case erroneous – comments of your anonymous expert,' he wrote to the editor of *The Physical Review*, who had sent one of Einstein's papers to an expert on general relativity. Einstein's objection included a declaration that he had not expected it to be shown to anyone: he had sent the paper for *publication*. 'On the basis of this incident I prefer to publish the paper elsewhere,' he told the editor. And he did. The paper, 'Do Gravitational Waves Exist?', was unquestioningly accepted by another journal, and published complete with the mistake that the reviewer (but not Einstein) had spotted.

The famous Crick and Watson paper on the structure of DNA was also not peer-reviewed prior to publication; the editor of *Nature*, John Maddox, declared its correctness to be 'self-evident'. At the time, the only kind of peer review carried out by *Nature* was done by a member of staff who would take submitted papers

with him to the Athenaeum club and, over coffee or luncheon, ask other scientifically qualified members whether the ideas contained in the papers had any merit.

It was the growth in the numbers of professional scientists seeking publication that led to formal peer review becoming the norm. Faced with a barrage of submissions, the journal proprietors simply had to impose a filter. Today, journal editors receive papers from scientists, decide which ones look interesting, and send them out to two or three experts in the field. These experts decide – anonymously, to avoid unpleasantness – whether the papers merit publication. It seems like a sensible system, but only if you believe the misinformation about who scientists are, and how they behave. The fact is, peer review isn't working too well – precisely because scientists are far too human.

Imagine that you are submitting a scientific paper for publication. It will be reviewed by the experts in your field: your competitors. They are not going to reject it just because it's not their work; that would be far too obvious. But the temptations are there. If you have completed work that they are only halfway through, they will be tempted to delay your acceptance – perhaps subconsciously. If your work makes theirs redundant, it will be difficult for them to fall on their sword and admit defeat. If they just don't like your approach, they will be tempted to pick holes in it – or create some. I have heard researchers moan, for instance, about a reviewer who couldn't find flaws in their work, but told the journal editor that the work should be published only if accompanied by this disclaimer: 'The most plausible explanation of these results is that they are somehow wrong.'

Even if reviewers are unbiased and objective, for the system to be effective they need to have the time and the inclination to examine papers thoroughly. Reviewers are humans under enormous pressure. They all know that they can't just refuse to review their peers' work – the journal editors know who they are, after all, and could reciprocate by refusing to even look at papers the reviewers might submit themselves. As scientists will occasionally admit off the record, the over-busy reviewer frequently offers little more than a cursory examination. Present-day peer review does not mimic Michael Faraday's rigorous recreating of every experiment. Some papers are rigorously examined – especially if they make great claims – but not all. Far from it.

The traditional form of peer review is an archaic system. Many scientists admit – privately – that it just doesn't work. Occasionally, they come out and say it in public. Martin Rees, for example, a former President of the Royal Society, has conceded that reviewing by learned journals 'is not the only way to ensure quality control in science'. Electronic publication accessible to anyone, he suggests, would allow scientists to weigh claims, attempt replication and point out – perhaps endorse – papers that merit attention.

Rees is a fan of the preprint archive arXiv.org, an online repository operated by Cornell University. It provides a showcase for new papers in physics and related fields, and most scientists are able to tell at a glance whether a paper merits their attention. If someone were to add to it a recommendation system for registered scientists, something like the review system on the Amazon website, a rival to the decades-old standard peer review system would be born. Yes, it is still open to bullying, but the removal of anonymity would soon halt that in its tracks.

One of the problems the administrators of science have with such a system is that they have built their assessments upon peer-reviewed journals, weighing up a scientist's worth on the basis of how many publications they produce, and in what standard of journal. Here again, we find the cart before the horse: instead of publishing to alert their colleagues to interesting new findings, they are publishing to survive the system and make sure they get enough funding to continue in their line of work.

The fact is, scientists have been the architects of their own problem. For decades they have posed as dependable, trustworthy, non-radicals, and now they wonder why they have a management system that treats them like docile workers on a production line, rather than what they know themselves to be: creative and curious minds, pursuing lines of inquiry that could lead anywhere – or nowhere. Of course, having taken on board the idea that scientists shouldn't cause a fuss in any sphere – part of the tacit agreement of the post-war period – all that scientists can do is moan to one another about the deadening hand of their administrators.

Which brings us to another undesirable consequence of the cover-up. Scientists don't mobilise. They don't agitate. They don't kick up a stink. Through decades of conditioning, rather like wolves who have allowed themselves to be domesticated and slowly bred into yappy chihuahuas, they have been tamed. Scientists, to put it bluntly, have lost their will to bite and snap at anything that lies outside their immediate sphere.

As a consequence, they are a politically inert group who have become convinced that they should advise (if asked), but never seek to influence, the political agenda. 'Scientists should be on tap, but not on top' is how Winston Churchill saw it in – again – the era immediately after the Second World War. It is an ideology that scientists have wholeheartedly, and somewhat cravenly, accepted throughout the decades since. Scientists involved with advising governments have laboured under the self-delusion that they do so on behalf of the wider population. The truth is likely to be a little more self-serving than that: the primary aim has been to appear useful without appearing troublesome (or worse, meddlesome) to those who control science's funding.

If scientists didn't have such a crucial role to play in building and safeguarding our future, that might be acceptable, if all too human, behaviour. The problem is that, for reasons everyone has forgotten, our society now gains next to no input from some of the finest minds in our midst. As Michael Nelson and John Vutevich put it in the *Chronicle of Higher Education*, 'It is a perversion of democracy to muffle the voice of the most knowledgeable among us and consequently amplify the voice of those with the greatest ignorance.'

Thanks to the post-war whitewash, a cloud also hangs over our ethics boards. The very committees that were meant to avoid a repeat of atrocities and murder have, in some cases, caused thousands of deaths through bureaucratic delays. Ethics committees were set up at the same time as scientists and governments were seeking to dispel public fears about the scientist's sense of responsibility, so they were always going to be overcautious. The acts that precipitated the Nuremberg Code took place in extraordinary circumstances that no longer apply (and no amount of regulation on ethics would prevent anyone intent on such acts from carrying them out). Just as 'the Sabbath was made for man, and not man for the Sabbath', ethics committees are meant to serve science. Science is not meant to be enslaved to their ever-widening remit.

The medical literature contains many studies of the performance of ethical review panels that highlight the problems they can cause. A study in Scotland, for instance, approached nineteen committees and found that fifteen of them had designed their own application forms, creating an inconsistent, time-consuming system where applications were subject to the whims and particular interests of those on the committee. Some committees required researchers to submit twenty copies of the documentation, and the time taken for final approval varied from 39 to 182 days – on average, it took three months for researchers to get the go-ahead. Perhaps the most worrying finding was that the final decisions

tended to depend on the personal moral stance of the committee members.

It is easy to see why scientists seek ways to sidestep ethics committees. Lives have been lost because of the administrative burden imposed: when one committee delayed a trial designed to test new drugs for heart attacks, around 10,000 people died unnecessarily. As a 2004 editorial in the *British Medical Journal* pointed out, 'The burdens imposed by ethics review might be justified if it could be shown that, on balance, it does more good than harm to patients' interests. Delays may, however, have important consequences and sometimes jeopardise the interests of patients.'

The scientists' attempts to paint their field whiter than white, putting on their 'anxious to please' face, as Jacob Bronowski described it, have contributed to this over-cautiousness. But, in the light of what we now know about the way scientists work, ethics committees would do well to focus on their own flaws. There is very little evidence that scientists are prone to conducting unethical experiments by choice – they have to publish, discuss and defend these results before their peers and the wider public. They also have to look to the next round of grant applications: when the focus is your own career, your own rise to the top of the field, performing experiments that put others in danger is akin to shooting yourself in the foot.

Then there is the issue of science education. How do we inspire the next generation of scientists? Since the 1950s, the public face of science has been dull, spiritless and cautious. Scientists have taken a back seat in society and culture, allowing rock stars, sportsmen and -women, and fame-hungry TV celebrities to win the attention of our children. And we wonder why these naturally curious children, who displayed a delight with science in primary school, show disaffection and lack of interest by the time their eleventh birthdays come around. Once they become aware of what is valued, what is deemed exciting, in the wider world, science loses its lustre. If the high-school students of today were permitted to learn – perhaps through scientists taking a more honest approach with the media – what science and scientists are *really* like, the days of a career in science being the dull, dismal road less travelled would be behind us.

There is also the problem of methodology: science teaching methods and curricula have also been a victim of the cover-up. Children have, by and large, been taught the letter but not the spirit of science. As the philosopher Rousseau suggested, we should not teach children the sciences, but give them an appetite for them.

It is open to question, for instance, whether students really need to learn all of the scientific information on the science curriculum. For most, it is an experience that seems to destroy any interest in science. And anyone who has done a school science practical will know how hard it can be to get results that the textbooks say they should. Why is this seen as a failing? Imagine if teachers were then allowed to use this experience to explain the challenges and rewards involved in making breakthroughs and discoveries, rather than having to press on to the point where the student's notebook contains the 'right' answer. Science teachers have been unwittingly co-opted into the effort to conceal the true nature and spirit of science.

Setting the anarchists free will certainly be difficult while such mindsets prevail. One result has been that many of those who survive their education with sufficient interest in science to pursue it further are of a personality type that perpetuate the problem. They are drawn to science as it has been portrayed: staid and comfortable.

This is a problem that Stanford Ovshinsky, the scientist without a college degree, understands better than most. Traditional forms

of education, he says, can hamper scientific creativity in students: 'All the time they're being treated in a "giving of information to you" kind of way, and then when they get out of school they say, "Okay, now you're on your own, think, be creative." After all those years of trying to kill it.' Kary Mullis also worries about the scientists that the post-war growth of the science establishment is continuing to produce. Out of the investment 'came a lot of scientists who were in it for the money because it was suddenly available', he says. These scientists, he observes, were not, like him, 'the curious little boys that liked to put frogs up in the air'.

An interesting question to consider is what kind of science such scientists produce. Surely, if we flood the universities with visionless scientists, it is inevitable that much of science will become boring. Take a 2008 paper by the GEM particle physics collaboration as an arbitrary example. It stretches over twenty pages, has thirty-one authors, and relates to the minutiae of whether a particular kind of subatomic particle called a meson forms a 'bound state' within an atomic nucleus – two decades previously, a couple of physicists had suggested that it might. Unfortunately, the data presented proved nothing; as the last line of the paper states, 'Further data are clearly needed.' It is difficult to tell who would care even if further data *weren't* needed. It is an example of the over-specialised result, a natural inclination of science, and one that has to be resisted where at all possible.

The problem was identified as long ago as 1930, by a Spanish philosopher, José Ortega y Gasset. In order to make progress, science demands that its workers become ever more specialised. The result, Ortega said, is that the majority of scientists are 'shut up in the narrow cell of their laboratory, like the bee in the cell of its hive'. This type of scientist 'is familiar only with one particular science … in which he himself is engaged in research'. The result, according to Ortega, is a succession of mediocre, tedious advances, not Nobel Prize-winning breakthroughs. And that was pre-war; in the post-war environment things got worse.

In 1950, the German physicist Erwin Schrödinger reprised Ortega's lament. He worried that specialisation creates a societal ennui that could eventually kill the scientific endeavour. 'Never lose sight of the role your particular subject has within the great performance of the tragic-comedy of human life,' Schrödinger warned his fellow scientists. 'If you cannot – in the long run – tell everyone what you have been doing, your doing has been worthless.'

Is such overspecialisation avoidable? Yes, but it demands effort and courage – exactly the kinds of qualities not possessed by those who went into science because it offered an unchallenging route to a secure existence. Andre Geim, who won the 2010 Nobel Prize in Physics, has advice for those wanting to do truly groundbreaking research: don't work where others are working – go off the beaten track. 'If you follow the herd, all the grass is gone,' he says. For a truly significant breakthrough, you 'have to do things no one else is doing. Unless you happen to be in the right place at the right time, or you have facilities that no one else has, the only way is to be more adventurous.'

It seems that, here again, the cover-up may have hurt the progress of science. In the first few decades after the Second World War, scientists opened their field to cohorts of researchers who didn't share their anarchic curiosity. The result was a grudging acceptance of mediocre, tedious advances as equally valid contributions to the scientific endeavour, and a consequent public blindness to science as a vital and fascinating facet of human culture.

Finding a way to keep science outward-looking, relevant, energetic and accessible is vital, Schrödinger said. With uncanny prescience, he pointed out that the 'masses' outside science decide issues such as what gets included in school curricula. According to Ortega and Schrödinger, public disengagement from science through tedium is the first step in a journey that includes, for example, allowing Creationism into the science classroom, and ends with the disappearance of science from popular culture.

The scientists' pose as grey, faceless, unthreatening ushers of a brighter future has had a significant impact. The world is a worse place because of it, and unless something changes, it could get much worse still. That is why we need to set the secret anarchists free.

Here at the Medical Research Centre, I have resigned myself to the fact that I am not going to discover the truth about Francis Crick and his use of LSD. His secret – if there is one – is safe. But Crick and Watson are not the only Nobel laureates Michael Fuller has worked with. He arrived at the MRC, aged sixteen, in January 1952 to work as a technician. In the fifty-eight years he has spent here, the centre has won twenty-six Nobel Prizes (it had won three, including Alexander Fleming's, before Fuller arrived). In all his time at the MRC, Fuller has been charged with going out and getting the champagne for each Nobel celebration. Sometimes, he says, Cambridge has almost run dry of bubbly.

I have to ask: what is their secret? Fuller pauses for a long time before he responds. 'Single-mindedness,' he says. 'They can't be deterred by anything or anyone.' There is another pause; he seems unsure whether to make his next remark. 'And big egos,' he adds eventually. 'Incredible egos. They just know, somehow, despite what anyone says, that they are right.'

I am reminded of Albert Szent-Györgyi's comment that scientists are egotists, selfish beings who get their kicks solving the puzzles of nature. It occurs to me, as Venki Ramakrishnan throws his banana peel into the bin and heads back to his lab, that he doesn't look like much of an egotist. But then, as I have said many times, these are *secret* anarchists.

Now that their secret is out, they are not diminished. Having discovered the true depth of their resourcefulness, I am filled with a renewed admiration for the anarchists of science. They make discoveries not despite their humanity, but precisely because of it. If we want more scientific progress, we need to release more rebels, more outlaws, more anarchists. The time has come to celebrate the anarchy, not conceal it.

Science is something we should hold in the highest regard. In the words of Bronowski, 'These are the marks of science: that it is open for all to hear, and all are free to speak their minds in it. They are marks of the world at its best, and the human spirit at its most challenging.' It is challenging because, as he also noted, this reckless, relentless pursuit is made at great personal cost. Claude Bernard once said that the joy of discovery is available only to those who have felt the 'torment of the unknown'. This is science: torment, dreams, visions, restlessness, lying, cheating, despair, brawling, bullying, desperation and – in the end, when everything works out – a moment of euphoria that makes it all worthwhile. Typically, Bronowski found a way to put it more simply. 'Science is the acceptance of what works and the rejection of what does not,' he said. 'That needs more courage than we might think.'