Brief Account of How Nicholas Maxwell Came to Argue for the Urgent Need for a Revolution in Universities

We need urgently to bring about a revolution in universities around the world, wherever possible, so that they take their fundamental task to be, not to acquire and apply knowledge, but rather to help humanity learn how to resolve conflicts and problems of living in increasingly cooperatively rational ways, so that we may make progress towards a good, genuinely civilized, wise world. The pursuit of knowledge would be a vital but subsidiary task. I have argued for the urgent need for such an academic revolution for nearly 50 years, ever since the publication of two books: *What's Wrong With Science? Towards a People's Rational Science of Delight and Compassion*, 1976, and *From Knowledge to Wisdom: A Revolution for Science and the Humanities*, 1984, both available free online. This article, written in preparation for a talk given to Pittsburgh University on the 15th September 2023, gives an outline of my baffled struggles with problems of philosophy, science, literature and life that led up to the discovery of the urgent need for an intellectual and institutional revolution in universities around the world, needed in order to galvanize the social revolution required to enable us to make progress to a world in which peace, justice, democracy, individual freedom, sustainable prosperity, the possibility of a good life, are available to all, insofar as that is attainable.

The quickest and perhaps best way to find out what I have been up to during the last 50 years or so is to have a look at an article I published in 2021 called 'How Universities Have Betrayed Reason and Humanity—And What's to Be Done About It', *Frontiers in Sustainability*, 2021. (Click the title.) This article gives what is, I hope, a vivid and readable account of the triumphs and disasters of my research work during the period in question.

The key events are, I suppose, the following. Long ago, in the Summer of 1961, after a year as an undergraduate at Manchester University, I made what I thought to be an absolutely astonishing discovery. I wrote down at the time what I thought I had discovered in the form of the following enigmatic sentence: "The riddle of the universe is the riddle of our desires".

What I meant at the time by this enigmatic sentence was, roughly, the following: "The fundamental problem that confronts us in life is not Einstein's problem – what kind of universe is this? (the fundamental problem of science). It is rather the problem: what ultimately is desirable and of value in life? How are we to live given that what ultimately is desirable and of value in life is so profoundly and inherently mysterious, so bafflingly problematic that, whatever decisions we make in life we are almost bound to get it wrong?" Since almost all our desires, our aims, in life are almost bound to be wrong, not in our best interests, how do we solve the all-important question: How can we best go about improving our desires, our aims, as we live? Philosophy, I realized, as a consequence of what I had discovered (or thought I had discovered) urgently needs to undergo a revolution. Instead of being what academic philosophy was at that time, the sort of absurd nonsense that philosophers did at Oxford in those days, what philosophy needs to do is to help tackle this fundamental problem: Given that only a few decades, at best, are allotted to us, before eternal non-existence ensues as our ultimate fate, what do we do? How do we live in this brief life? What ultimately is of value in life, and how is it to be realized, given especially that life is so brief?. I decided that, when I got back to Manchester University that Autumn, I would explain to the Philosophy Department what I had discovered and, as a result, the revolution in philosophy we required would occur. (For an account of events associated with this "great discovery" of mine, in the Summer of 1961, see an article I published decades later called 'Arguing for Wisdom in the University: An Intellectual Autobiography', *Philosophia*, 2012, pp. 663-704.)

I returned to Manchester University as a second year undergraduate, and found I could not open my mouth about what I had discovered. I felt I had lost my great discovery, and for some years almost lost myself in black despair. In the third year at Manchester, the philosophy course was devoted to Oxford philosophy. I stopped going to lectures entirely. I was terrified I might be brainwashed into believing in the intellectual rubbish I knew Oxford philosophy to be. When Professor Prior handed me my final result, an upper second, he apologized for not giving me a first. I felt that an upper second, in the circumstances, was quite generous. In all the final exams, I wrote, not what I was supposed to write, but what I actually thought. Prior always thought well of me. In my second year I thought hard about the brain and consciousness, and wrote an undergraduate essay on the subject. I knew nothing of J.J.C. Smart's slightly earlier work on the subject. Prior was impressed by my essay and suggested I send it to Smart (Prior knew him personally). Back came the comment: "very ingenious". I didn't think that quite did justice to the situation: my version of the so-called brain process theory of consciousness was clearly much better than Smart's. Nevertheless, I have always had an enormous admiration for Smart's *Philosophy and Scientific Realism.*

Now began my second phase of creativity, sober, conventional and unremarkable, from 1964 to 1969. I decided to do an MA thesis in philosophy, rediscover what I had found and lost in 1961, and then escape the madhouse that I thought the university to be. I discovered Karl Popper, became an occasional student at the LSE to attend his Seminars, was immensely impressed, and decided Popper had done what I attempted to do in 1961. I breathed a sigh of relief. I wrote my MA thesis, which passed even though condemned with faint praise by the external examiner, Peter Alexander with the words "shows signs of originality". I got a temporary, one year job lecturing in Philosophy of Science at Manchester University, which I enjoyed. That year, 12 jobs came up in philosophy of science, an unprecedented number, due to the creation of "New Universities". I was turned down by all of them, but accepted by the best of what was on offer, Philosophy of Science in the Department of History and Philosophy of Science at University College London. I accepted. Paul Feyerabend would be a colleague. I read everything I could discover that Feyerabend had written. I was immensely impressed. This was before Feyerabend was driven mad by his non-discovery of incommensurability, became a Dadaist, the Court Jester of World Philosophy, and descended into nonsense. My new Department, I discovered, was so old it had invented the discipline it represented, as were almost some of its members, I was to discover, when I arrived on the scene. In those days London was the world capital of Philosophy of Science, beyond any question, and that was my destination. I bought Feynman's marvellous *Lectures in Physics*, in Manchester, before I left for London, in the hope that they would save me from exposure as a charlatan (I knew almost nothing of what I was supposed to know). I arrived in London, and met my new colleague, Larry Laudan, who knew everything I did not know. We debated realism versus instrumentalism endlessly, and became good friends. I extracted three papers from my MA thesis, polished them up and published them with the titles 'Physics and Common Sense', BJPS, 1966; 'Can there be Necessary Connections between Successive Events?', BJPS, 1968; and 'Understanding Sensations', Aust. J. Phil., 1968. I knew these three papers would provoke a revolution in philosophy, in part because they were about the world, something that, in those days, was not possible, because philosophy was not an empirical, factual discipline, but one exclusively about concepts, in that respect a bit like pure mathematics. I felt publishing these three articles was the equivalent of tossing a firework with a very loud BANG into a genteel tea party.

As it was, nothing happened. I concluded that publication was not done to communicate ideas and results; it was done to advance careers. I decided to stop publishing and concentrate on the joy of discovery.

Then, years later, my first three articles had an immense impact, not just on philosophy, but on other disciplines too, but via the articles of others, not directly via my articles. Eight years after my first article, Thomas Nagel published 'What is it Like to be a Bat?', the basic contents of which were anticipated by my first and third article. Then, 20 years after the publication of my first article, Frank Jackson published 'What Mary didn't know', the contents of which were anticipated by my earlier work. Decades later, during my second visit to Pittsburgh as a visiting research fellow in the Spring of 1999, I wrote to Nagel, pointing out what had happened. With great generosity, Nagel replied "There is no justice. No, I was unaware of your papers, which made the central point before anyone else". Around that time, I met Frank Jackson at Oxford. He said he knew of my 1968 paper, 'Understanding Sensations', which anticipated his. There was no refence, however, to my paper, so in this case straightforward plagiarism is involved, of idea, thesis and argument, not words. An entire army of other philosophers published work on physical essentialism, natural necessity, non-Humean accounts of causality, almost all of which devolved from my 1968 paper on necessary connections between successive states of affairs, most of this work dependent on my earlier work without acknowledgement, reproducing elements of my work in a degraded form, thus ensuring that my earlier work remains even now unknown. As one not entirely friendly friend said of my Hume paper "It belongs to the pre-history of the subject" which, if true, ensures the paper remains unknown for ever! Subsequently, another very busy research industry started up in philosophy of science called "The Metaphysics of Science". This would never have taken the form it did take if my earlier work on the subject had been better known. My attempts to point this out to participants in this busy research enterprise got nowhere.

There were three bad outcomes of this sorry saga. First, my early work got nothing like the attention it deserved. Second, as a consequence of this work being communicated via the work of others, what I had to say only became known to most philosophers in an appallingly fragmentary, distorted and degraded form. Third, when, a few years later, I had something to communicate which seemed to me to be of profound significance for the future of humanity, this work was ignored and misunderstood by academic philosophy because, instead of being well-known, I was almost entirely unknown. This state of affairs continues right up to today, as I shall attempt to explain in a moment,

Finally, around 2017 I think, the wonderful Professor Otávio Bueno, then editor of Synthese Library, invited me to write a book about what had happened. I did so, and the book was duly published with the title *The Metaphysics of Science and Aim-Oriented Empiricism: A Revolution for Science and Philosophy*, 2018. The book got two terrific reviews that entirely endorsed the account of events I have just given,[1] but the book itself sank without trace, and has never been seen since. It is nevertheless worth reading; it gives a lucid, readable, detailed and devastating account of work going on that passes these days for philosophy of science, and in effect provides a survey of what is wrong with philosophy today.

We come now to the third, really big phase of creativity in my life, from about 1971 to some kind of frenzy of creative energy in 1976, to a time of dedicated hard work, from 1977 to 1983, to a gradual decline in creative energy from 1998 to some time last year, in 2022, when it all petered out through sheer exhaustion. There is a final phase of creative energy, a creative explosion of joy that happened a month ago, a quite astonishing occurrence for a person aged 86. I will tell you something about that right at the end, if there is time.

The third phase of creative energy began around, 1971, as I have said, when I started to write a book called '*The Aims of Science*'. Suddenly I began to rediscover what I had first discovered in the Summer of 1961, and then had lost again, which led to a decade of inner black desperation. Suddenly, as if by magic, it all began to emerge from the tip of my biro as

I wrote that title, apparently so banal, but actually vital to release my ancient discoveries. What I had discovered in 1961, after all, was just that most aims are, somewhere, profoundly problematic, and thus, almost inevitably, somehow, the wrong aim to pursue, the right aim, genuinely in our very best interests, some kind of as-yet not considered modified version of what we at present so energetically attempt to achieve.

In writing *The Aims of Science* I discovered that the aims of science, generally acknowledged to be the actual aims of science, namely to improve knowledge of truth, are not the real aims of science at all. The real aim of science is the profoundly problematic one of *improving knowledge of truth presupposed to be unified or explanatory*. And that is just a special case of a more general aim of science of *improving knowledge of truth of value* in some way or other. That in turn is sought in order to fulfil the ultimate aim of science, even more profoundly problematic, of making available to people what science discovered to promote what is of value in human life. And then I discovered, as I wrote *The Aims of Science*, that it is not just the aims of science that are misconstrued and problematic; far worse, the aims of academic inquiry are misconstrued, and are, in reality, fundamentally problematic. We need, I began to realise, a new conception and kind of science, and a new conception and kind of academic inquiry. And it went further. The aims of life, personal, social, political and in every other way, are permanently and profoundly problematic. We need a new kind of academic enterprise that helps us improve aims and methods of life, as we live. I finished the book, typed it out, and sent it to Macmillans for consideration for publication, The relevant editor, a somewhat elderly man, agreed to see me about the manuscript. He became excited about the book, but before anything could be agreed, he retired. An ambitious young woman replaced him. We met, and discussed the book. She too became quite excited about the book. But she started to complain to me about the glass ceiling she found installed at Macmillans. Before we could reach any agreement about *The Aims of Science*, she left for greener pastures because she sensed her career would be frustrated at Macmillans. Next, a new editor was appointed, a Marxist I was told, which seemed to me somewhat strange for a publisher called Macmillan. I never met him. He just turned down *The Aims of Science* flat, without any discussion. That was that. Something like a year, or even two or three, had been wasted. *The Aims of Science* was never published. It is lost somewhere in the attic of my home.

I then tried desperately to find some other way of publishing the extraordinary discoveries I felt and thought I had made. I began to write and write, in a kind of mad frenzy of lunatic activity. I remember a pile of manuscripts that grew and grew on my desk at UCL, rejected by me again and again because I knew they would not of themselves bring about the changes to science, and to universities, that I sought to make happen. I did not appreciate that no piece of writing, however powerful, can of itself just effect magical changes in the august institutions of science, and academia, so that they melt and mould themselves in instant obedience to the validity of an argument. The world just does not work like that, however valid an argument might be. Some years later, I looked at this pile of manuscripts. I was amazed at how powerful and vivid the arguments were. But I did not think they would just convince the Royal Society of the need for change, and it would just happen like that.

I took a holiday, visited my friend Larry Briskman in Edinburgh, and recovered from my bout of near writing madness. Then suddenly all my problems seemed at an end.

A friend, Richard Kirby, said he had a friend who had a Printing Press in the front living room of his house. If I could write a book in the next 6 weeks, he would print and publish it. I decided to write a new book from scratch. I thought about it for 3 weeks. Suddenly I had left myself only 3 weeks to write the book. It was in the middle of the Summer vacation. I was living in Teddington at the time, a place on the southwest fringe of London, on the Thames. Most of the book I planned would be a fierce argument between a Scientist and a

Philosopher. The former would represent the best of scientific orthodoxy, the latter would represent me. I arranged for the secretary of the Philosophy Department, not my Department, to type up the manuscript. Computers did not really exist in those days, the Summer of 1976.

This was the first of two books that are the products of this period of extraordinary creativity in my life, from around 1970 to 1985 or so.

The first book, the one I have been describing so far, written entirely in 3 weeks, is called *What's Wrong With Science? Towards a People's Rational Science of Delight and Compassion*, Bran's Head Books, 1976; 2nd ed. Pentire Press, 2007, https://philpapers.org/archive/MAXWWW-6.doc .

The second one which I shall say something about in a moment, took something like 4 years of dedicated hard work to write. It is, on the face of it, an entirely different kind of book. It is called *From Knowledge to Wisdom: A Revolution in the Aims & Methods of Science*, Blackwell, 1984. The second edition has a better subtitle: *From Knowledge to Wisdom: A Revolution for Science and the Humanities*, Pentire Press, 2007: https://philpapers.org/archive/MAXFKT-3.pdf

These two books, that together capture the very essence of what my entire research work has been dedicated to for the last 50 years or so, appear to be quite different sorts of books. It is important to appreciate, however, that they say, essentially, the same thing. When I came to work on producing the second edition of *What's Wrong With Science?*, and I read the book for the first time for years, I realized with utter dismay, that all that I had ever wanted to say, all those subsequent publications, were here, in this first book of mine, dashed off in 3 weeks.

I did not really write *What's Wrong With Science?* Most of the book, as I have said, is a furious argument between a scientists and a philosopher. I wanted the argument to go as real arguments go in real life, not as arguments go in Plato's dialogues in which, however silly Soccrates's arguments are, his companion seems always to say something like Yes, O Socrates. How true, O Socrates. In real arguments in real life, no one convinces anyone of anything. No one wins, and no one loses. Afterwards, someone may think again about some point that was made, but nothing gets conceded at the time. My argument was going to go like that. Even though the Philosopher's arguments were going to be so much better than the Scientist's, nevertheless the Scientist would concede nothing. (It took me a long time to realize that the Philosopher had to be called "Philosopher". I did not have much time for Philosophy, although I had a great admiration for science and some scientists: Einstein, Faraday, Darwin, Kepler, Galileo, and a few others, scientists I later came to call natural philosophers.

I did not, as I have said, really write the book. What happened was this. I went for a walk each morning in Bushy Park, near where I was living. As I walked I eavesdropped on the furious argument going on between Scientist and Philosopher. I then rushed home and transcribed what I had overheard, or heard going on in my head onto paper. I then drove in to UCL and brought the scribbled dialogue to the Philosophy Department Secretary, actually a graduate student in the Philosophy Department who was furious at the indignity of typing out these ridiculous scribblings, as she saw it. In her frustrated anger, she made lots of mistakes. Somehow the entire book got finished in the allotted three weeks. The manuscript was rushed to Richard Kirby's friend. The book was printed and published by some miracle. I floated on air in a state of bliss. At last I had done, I was convinced, what I had been trying to do for the last decade or so. A fellow Natural Philosopher, Andrew Matus and I, took a train to Exeter, I think, where the annual conference of the British Society for the Philosophy of Science was being held that year. We did a joint performance at the conference, with conviction, dash, delight, and a touch, perhaps, of arrogance. I told all my colleagues, especially those from the LSE, exactly what I thought of them, one night over a shared bottle of whisky, to their utter incredulity and laughter, and thus, without realizing what I had done,

ensured I had no hope of a future reasonable career in academia – quite apart from the book I had just finished, the book of my life which, from an academic point of view, was, and still is, entirely unacceptable. At this conference I met an extraordinary woman called Esther Salaman, a novelist, from Russia, actually from Ukraine I learnt the other day, who expressed her horror when she heard I did not know whether I was related to *the* Maxwell or not. "Vot" she exclaimed in horror "You don't know?" She and her husband subsequently became very close friends of both me and my wife, even though they were so much older than we were then. Esther told me all about her astonishing life, her friendship with Einstein and Dirac, and she knew in her Russian, or rather Ukrainian bones, what my work was all about, whereas her husband, who had done vital scientific research into the role of viruses in cancer at Cambridge, and had written a book about Popper, never had the faintest idea. A standard attitude from many of my scientist friends, but by no means all.

My book was published. It received three reviews, and sank without trace. Even though I had thought I had achieved everything I needed to achieve, in reality I had achieved nothing.

Then, an intelligent young man from Blackwell approached me, or the other way round, about the possibility of publishing a book with them. I agreed. Earlier, a friend I never met, a Popperian, called Dr Horrobin, creator and editor of a journal called Medical Hypotheses, thoroughly Popperian in spirit, which still exists I believe, persuaded me that I needed to write my "Logic of Scientific Discovery", a very academic book in style, which very few people will actually read; but the mere general awareness of its existence will enormously help my argument, my case, for the urgent need for a revolution in universities around the world, so that they become devoted to helping humanity achieve what is of value in life, and make progress towards a civilized world. Dr Horrobin convinced me. When the Blackwell offer came up, I took it very, very seriously, Here, at last was the opportunity I had been waiting for. With absolute dedication and seriousness, I began to write *From Knowledge to Wisdom*. I began in the morning, and wrote, carefully and slowly, every sentence considered and reconsidered until I got it just right, and stopped in the evening. I remember thinking I felt like a spider who was pulling this web material from his very being, but only had so much available on any day, and then he would have to stop. I knew I was saying something that no one had ever said before. I was amazed that the English language could be bent and stretched and used to say something it was never intended to say whatsoever. I was proud of the first sentence of the book: "Our planet earth carries all too heavy a burden of killing, torture, enslavement, poverty, suffering and death." I felt it, I knew it, and I wanted it to stop.

After some four years of slow, dedicated writing, every fibre of my being concentrating on the task in hand, the book was finished. An initial second chapter got too long, and had to be discarded. It was published in *Inquiry* as an article called 'Science, Reason, Knowledge and Wisdom: A Critique of Specialism', *Inquiry 23*, 1980, pp. 19-81: https://philpapers.org/archive/MAXSRK.

The book was published some time in March or April I think, of 1984. Absolutely nothing happened. If you are an actor, as some members of my extended family were and are, the response to your performance is instantaneous; there are boos or cheering and clapping straight away. But if you are an author, nothing happens immediately at all, unless you arrange yourself for something to happen, or you are so famous that your publisher does it for you. In my case, nothing happened for some months. Then, one morning I went to the Haldane Room, a place one goes as an academic for a morning coffee. A distinguished Professor of Anthropology, who I knew of, but did not really know in person, got up off the chair where he had been sipping his coffee, approached me with outstretched hand, and said very seriously, and very definitely not as some joke, "I must shake the hand of the author of a book that has just got the best review in *Nature* that I have ever seen". I pretended to sip my coffee, and surreptitiously gulped it down. I then ambled out of the room as if I had all the

time in the world. Once outside, I bolted as fast as I could to the Science Library, where I knew I would find this week's *Nature*. In it, I found the following review of my book. For goodness sake, *Nature*, vol 312, Nov 1984, p. 204., by Christopher Longuet-Higgins.
He didn't get everything right, I noted, but in the circumstances, it was a pretty good review. Some of it, as you can imagine, went straight to my heart. I seem to remember that I wrote him a very short letter, saying simply "Thank you". I didn't expect a reply, and got none. I was very moved when I learnt of his death, in 2004. We never met, although I now realize I knew someone who knew him well. There were many favourable reviews of the book after this first one, although philosophers, as I have already said, mostly wrote idiotically critical reviews that just idiotically missed the point. One such philosopher was from the USA, I am somewhat abashed to have to admit. What he said in his review of my book was so bad I was forced to reply: see 'The Fate of the Enlightenment: Reply to Kekes', *Inquiry 29*, 1986, pp. 79-82. https://philpapers.org/rec/NICTFO . But during my first visit to Pittsburgh as a visiting Research Fellow to the Center for the Philosophy of Science from September 1987 to April 1988, I discovered I was only there because Professor John Kekes, a previous visiting fellow, had recommended me. So my inextricable connection with Pittsburgh University owes its existence to John Kekes, and to my book *From Knowledge to Wisdom*. And there is more. The first bit of what I discovered which led to the two books, the publication of which I have just described, was first announced in a University context, at Pittsburgh University. Some time in 1973, I believe, Larry Laudan, who had once been my colleague at UCL, then left to take up an appointment in the newly formed Department of History and Philosophy of Science at Pittsburgh University. Larry invited me to stay in I think the winter of 1973, and asked me to take a seminar during my stay. This I gladly did. I had a lot to say. Various giants of those days said they were looking forward to my talk, but they would have to leave before I finished. They had another appointment they could not get out of, Adolf Grünbaum said it; Nicholas Rescher said it; and Ted McGuire said it. We began. Gradually, these very important persons left, one by one, nodding in a friendly way as they left. After I had been speaking for a while, but a good half hour before I was due to stop, Larry gently intervened and said we would have to move to another room as another lecture was due to begin where we were ensconced. I was a bit surprised, but we all duly trooped out Larry, the chair, me the speaker, and the audience. I started up again. There was still half an hour to go by my watch, and I still had a lot to say. Finally, Larry gently suggested I had perhaps spoken long enough, and it might be time for some questions. I conceded, again a bit surprised. There was still half an hour to go by my watch. Then an awful thought occurred. Yes, just what you have guessed. My watch had stopped. I had been speaking, with scarcely an interruption, apart from the change of venue, for a solid 3 hours. That was how aim-oriented empiricism was first launched to the world: to an audience of devoted, or at least compliant students and staff at Pittsburgh University. What I said was eventually published as 'The Rationality of Scientific Discovery, Part I: The Traditional Rationality Problem', *Philosophy of Science 41*, 1974, pp. 123-53: https://philarchive.org/rec/MAXTRO-5 . The very first statement of the argument for aim-oriented empiricism, which should have carried an acknowledgement that the paper had first been given as a talk at a seminar at Pittsburgh University. Thus my work, especially my book *From Knowledge to Wisdom,* is inextricably implicated and entangled in life at Pittsburgh University, and those of you fortunate enough to live in that magical place have an absolute obligation to read my two books, easy to do as they are both available for free online, and then shout from the rooftops to everyone else about their supreme significance for the future of science, the university, and, far more important, the future of humanity.

Notes

[1] One review says “Maxwell's views deserve more attention and his contributions should be recognized by the philosophical community. Overall, I recommend the book to anyone interested in the recent debates in the metaphysics of science and Maxwell's take on these issues.” (Ali Barzegar, Metapsychology Online Reviews, Vol. 23 (39), September 24, 2019)

The other review says “*The Metaphysics of Science and Aim-Oriented Empiricism* … is a solid and persuasive exposition of the main elements that have marked this author’s philosophical career: an original, remarkable philosophical doctrine, and a wide-encompassing proposal for academic reform. The main strength of this book, from the philosophical point of view, is its cogency in presenting a well-developed, appealing, and rigorous philosophical system pertaining to the metaphysics and the epistemology of science, something that not many philosophers even attempt to do these days. Damian Beanato, *Principia,* 23(9), 2019, pp. 529–533.