The Lambda Limit: the incompletability of science

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The idea that science is nearing completion assumes that science is completable. I argue that it is incompletable *in principle*. This needs to be recognized if science is to be fully deployed for human welfare, addressing critical global problems of the age. Nonrecognition of incompletability leads not only to the diversion of human, intellectual, material and energy resources away from critical human problems but exacerbates the neglect, misidentification and misconceived prioritization of human problems and the goals of science. The case for incompletability is outlined in terms of three dimensions of scientific development: theoretical, technological and economic. There appear to be insurmountable theoretical barriers (for example, Gödel's Theorem and the possible inconstancy of "laws of nature"), growing technological uncertainties (for example, in pharmaceuticals and nanotechnology) and an economic trend to diminishing marginal returns on investment (for example, the Large Hadron Collider and the International Thermonuclear Experimental Reactor). These dimensions may converge at a limit, the *Lambda Limit*. Dire human problems that are resolvable with current science and technology are neglected, such as water supplies and the eradication of malaria. The author argues that one reason for this is that science is misconceived as the ideology of scientism, a universal project in which "everything" can eventually be explained and predicted.

Keywords: diminishing marginal returns, inconstant constants, fusion, Gödel's Theorem, Large Hadron Collider, nanotechnology, scientism, sustainability

1. INTRODUCTION

Is basic science nearly complete in the sense that it has almost achieved a complete theoretical picture of the physical word? An affirmative answer would assume that science is completable; that is, that it *can* reach a point at which "everything in the physical world" is explained. I propose that this assumption is wrong and that completability is impossible. Furthermore, it is harmful for science to proceed on the assumption of the validity of this kind of linear completability, an assumption which is a tenet of what may be called "scientific fundamentalism" or "scientism".

However, if science is incompletable it does not mean there is nothing left for it to do. Indeed, everything of *real* importance for humanity is left to do. A recognition of this incompletability would help us to focus on the achievable, and that would in turn help us to focus on what *ought* to be achieved for human welfare on the basis of the scientific and technological principles we already have. Within these recognized *external* limits of explanation, prediction and control there is no *internal* limit to what science can do to sustain and improve human life, if the political and social will were to be mustered. When science recognizes and rebounds from certain transcendent limits, then it will find itself in a realistic and creative position for confronting actual human problems on a global scale.

The attempt to complete science in the face of transcendent incompletability manifests itself in contemporary difficulties in progressing in (a) theoretical science, (b) technology (applied science), and (c) the economy of science, as I shall show with contemporary examples. I think these interdependent difficulties will converge in a historical crisis in this century, which I call (for convenience) the *Lambda Limit* (Λ Limit). It is far better to change our understanding and our science policy before the Λ Limit is reached, at which the collapse of modern industrial civilization is a real possibility.

2. CONTEMPORARY VIEWPOINTS

The idea that scientific progress is reaching some kind of impasse was explored by John Horgan in *The End of Science*, first published in 1996 [1]. Horgan surveyed the idea of scientific progress and its critics, but did not clearly address the question of incompletability.

I begin with noting how curious the current dominant view of science is, especially among many scientists themselves. One rather unthinking view is that it goes on forever towards some nebulous scientific paradise. Another view held by quite a few modern scientists and science writers, but not of course all of them, is that basic science could be completed, in the sense that it could—probably quite soon—provide a complete, general,

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Received 4 July 2012; *accepted* 30 September 2012

explanatory and predictive picture of the physical universe and, perhaps more than that, even a complete picture that includes the social and mental domains. By this they mean that all the basic laws and constants of nature will be unified, consistent, coherent and complete. In one respect this view is not very new. In the closing decades of the 19th century we heard something very similar from scientists. They believed at that time that they finally knew what was possible and what was impossible—all had been explained, or just about. I emphasize here that my thesis is not about *completion*, but incomplet*ability*.

The conception I am criticizing is that science made a slow start, accelerated in growth through the 17th to 19th centuries, and there will be a final push to complete the picture, and the graph of growth in knowledge will then be flat. There will be no more growth, not because science is incompletable but because it is complete. All that remains in the science of physics lies "in the sixth place of decimals" (i.e., in fine details), said Albert Michelson, famous for his study of the velocity of light [2].

My thesis, I emphasize, is quite different. It is not that science is or is nearly complete but that it is *incompletable*, which is very different. It is an important matter for scientists and policy-makers to recognize this and to recognize it earlier rather than later.

The idea of the completability of science, as well as its comprehensiveness, persists to this day. Cambridge physicist Stephen Hawking proposed in his 1988 book *A Brief History of Time* (which sold over 10 million copies) that the completion of physics is in sight. In Chapter 10 on 'The Unification of Physics' he declares: "… I still believe there are grounds for cautious optimism that we may now be near the end of the search for the ultimate laws of nature". No doubt emboldened by his own words, he ends the chapter like this: "A complete, consistent, unified theory is only the first step: our goal is a complete *understanding* [his emphasis] of the events around us, and of our own existence" [3].

To give just one other example from the many possible: Francis Crick, Nobel prize-winner of DNA fame, persevered until his death in 2004 in trying to find a reductionist explanation even for consciousness. He tried to reduce consciousness to the physical activity of the particular neurons that supposedly underlie the ability of attention. In his 1994 book, *The Astonishing Hypothesis*, he writes that,

> "… 'You', your joys and sorrows, your memories and your ambitions, your sense of personal identity and free will, are in fact *no more than* [GH's emphasis] the behaviour of a vast assembly of nerve cells and their associated molecules" [4].

I am indeed astonished to read this. I am of the view that while it is true that if there were no such "vast assembly" in a person's head then there would be no joy and sorrow, or even a "person" to have joy and sorrow, but joy and sorrow in no way derive their significance from this "vast assembly" but entirely from contexts such as that my wife has given birth or my grandfather has died. Neither physics nor neuroscience have the slightest relevance for understanding what is joyful or sorrowful about these contexts.

Admittedly, many scientists are also uncomfortable with the ideas of the fundamentalists like Hawking, Crick and Richard Dawkins. In the last decade or so leading scientists are expressing doubts and many have started to come to terms with the internal limits of science, but not in a systematic way, and not in a way that has yet had much impact on scientific thinking or policy. Take, for example, Professor Paul Davies, a British physicist who was awarded the Royal Society's Faraday Prize in 2002 and the Templeton Prize in 1995. In his popular physics book *The Mind of God* he seriously doubts whether we could obtain final knowledge through science, because of internal theoretical and mathematical limits [5]. However, it seems to me that these doubts have not had any impact on science and technology policy.

Instead of the model of a flatline graph it is more fruitful to think of an asymptotic one. An asymptote is a line that a graph approaches, but cannot possibly intersect. In the simplest case, a graph of $y = 1/x$, the line approaches the x-axis ($y = 0$), but never touches it. I suggest that the growth of science on its current basis is asymptotic, and therefore unsustainable. It grew very slowly for a long period, accelerated to exponential growth, is now tailing off and will tend asymptotically to the horizontal (see Figure 1). In this asymptotic schema basic science would be—despite some counter-trends—slowing down, and every decade that passes sees some growth but less than in the previous decade. Growth tends to zero but in principle it never actually stops, it just becomes unsustainable because the resources required to make the next small step are vast. But it is not just a matter of resources. I would speculate that things are this way because human knowledge itself has a limit.

Expecting too much of science results in two kinds of illusion: that it can go on growing on the same basis for ever, or that it will flatline when it is complete. This "expecting too much" is what I call "scientism", a multipurpose framework that explains not just the world of matter and energy but the human world of love, hope, war, race hatred, religion, poetry and art. The sciences and what they have achieved and what they can still achieve to enhance the human condition are certainly not in doubt, but scientism is quite another matter. It is a

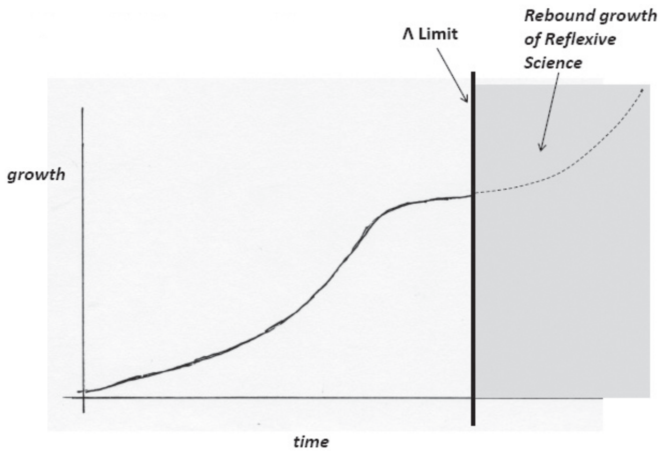

Figure 1. The rebound of science.

species of fundamentalism, and fundamentalism of every kind always ends up doing harm.

3. THEORETICAL SCIENCE

We now look at just two cases of theoretical difficulties that science has in its attempt to make new breakthroughs of principle and unification. These difficulties are profound and may ultimately be of a logical or epistemological nature. First I have a few words about the impact of Gödel's Theorem, and recent radical questions about the laws of nature and constants of nature.

3.1 Gödel's Theorem

Until 1930 mathematicians generally assumed that there must be a procedure that could demonstrate the truth of all the mathematical propositions in an axiomatic system. After all, if there were no such guaranteed procedure then how could we be sure that there weren't contradictions in that system? In 1930 Kurt Gödel showed that this assumption was false [6]. There is no such procedure of complete decidability. It meant that the consistency (or freedom from contradiction) of arithmetic cannot be proved by arithmetic itself. This was an enormous shock to scientists as well as mathematicians, for physics and other disciplines depend on mathematics. The idea that science rested on solid mathematical foundations underwent an earthquake.

The arguments and disagreements among scientists, mathematicians, logicians, computer scientists and philosophers continue to this day. Does this mean the great scientific project is incompletable? Some eminent thinkers believe so, and others do not. Even the experts cannot decide [7].

3.2 Inconstant constants

The words heading this section formed the title of a revolutionary article in *Scientific American* in 2005 [8]. The idea that there are physical constants (i.e., physical quantities that are universal and fixed in time on which the laws of nature depend) is the bedrock of the great scientific enterprise. For science to be possible it has to be assumed that some things stay the same. If constants are not constant after all, then the whole edifice starts to shake. Science would be revealed to be partial, piecemeal or provisional. Such constants include the speed of light in a vacuum (roughly 186 000 mph), the gravitational constant, Planck's constant, and the fine structure constant that characterizes the strength of the electromagnetic interaction.

But have constants always been the same? Some scientists are beginning to think not. And then there is the thorny question of why constants have the particular quantity that they do. They are not derived from theory, they are just peculiar numbers obtained from observation or experiment and parachuted into the theories to make them work—and work they generally do, up to a point. Some of these constants are really important. The tiniest variation in just one of these constants and we humans or even matter itself could not be here at all.

Take the fine structure constant: it has a currently accepted value of about 1/137.035999074 [9]. One argument for the fine structure constant being what it is really opens the door to incompletability. It is that the constant is what it is because we human observers exist. This is the *Anthropic Principle*, which has caused great controversy among scientists as to whether it is a scientific way of approaching matters at all. According to that principle, if this constant were different from what it is then there would be no carbon and therefore no life and no humans and no scientists to ponder the fine structure constant [10]. Therefore, there is no reason to assume that this constant is an absolute feature of the "universe" or "reality"; it is what it is because we humans are here as observers.

Richard Feynman, possibly the greatest physicist of recent times (a Nobel Prize-winner, who died in 1988) said of this constant, in exasperation: "It's one of the greatest damn mysteries of physics: a magic number that comes to us with no understanding by man. You might say the 'hand of God' wrote that number, and we don't know how He pushed the pencil" [11].

Could it be that the unanswerability of "Why 1/137?" is somehow connected with the incompletability of theoretical science?

3.3 Is theoretical science limited?

What the Gödel Theorem and the nature of physical constants may show, when taken together with a wide range of other theoretical difficulties, is that the more the computational complexity of theoretical science increases the smaller and smaller are the increments in real knowledge and principle. That is, there are diminishing marginal knowledge returns on the increasing complexity of its theoretical basis.

Where does that take us? Nobel laureate Steven Weinberg put it this way: "The more the universe seems comprehensible, the more it seems pointless" [12]. However, Weinberg, a brilliant scientist who belongs in the camp of scientism, insists on believing it is becoming more comprehensible when—as it seems to me and in fact to many leading scientists—this strains credibility. Take Niels Bohr, one of the founders of quantum mechanics, who said of quantum theory that if you are not bewildered by it you have not understood it [13].

4. TECHNOLOGY (APPLIED SCIENCE)

In applied science we now encounter great difficulties in making breakthroughs with any significant applications that could solve major current problems. My two examples here are pharmaceutical medicine and nanotechnology.

4.1 Pharmaceutical technology

The pharmaceutical industry is facing unprecedented problems. A 2010 graph based on an expert data analysis of the number of new drugs approved per billion dollars spent over a 60 year period shows a steady, inexorable decline [14]. As James Le Fanu explains in his book *The Rise & Fall of Modern Medicine* (1999), this decline was not just due to the tightening of regulations because of toxicity concerns in the wake of the Thalidomide calamity [15] (the development costs of each new drug sky-rocketed not just ten times but as much as 40 times). Another cause is that it turned out that human physiology and the disease process are much more complex and difficult to understand than had been assumed. Despite some important new drugs, such as those for AIDS, most of the apparently new 'blockbusters' are just improvements on existing drugs. This points to the need to design drugs scientifically (i.e., biologically) rather than adopt a hit-and-miss approach.

One might think that the 3000 million dollar Human Genome Project has revitalized pharmaceuticals and medicine; in the year 2000 Bill Clinton asserted it would "revolutionize the diagnosis, prevention and treatment of most, if not all, human diseases" [16]. The fact is that the connexion between the genome and disease is very complex, not to say fuzzy, and the benefits have been very few indeed, as recent studies have shown. To take one: A Boston medical research team gathered 101 genetic variants that showed statistical links to cardiac disease in genome-scanning studies. The variants proved to have *no value at all* in predicting disease among the 19,000 women who had been followed over a 12 year period. In fact, the research group reported that the old-fashioned method of obtaining a family history was more useful [17].

Gradually the pharmaceutical industry has turned for profits to so-called life-style drugs like *Regaine* for baldness and *Viagra* for impotence. It is also casting an eye at nanotechnology.

4.2 Nanotechnology

I have been involved in social research in three EU-funded nanotechnology projects for a number of years [18]. It is said to be an exciting new field. However, nanotechnology is easily misunderstood. It is not a specific technology but rather a significant shift in the *scale* at which existing technologies can operate. The ability to manipulate matter at the nanoscale (i.e., the scale of viruses and small bacteria) is breathtakingly complex and clever, and promises to be very useful [19]. Familiar kinds of matter (say, carbon, gold or titanium dioxide) behave quite differently at such a small scale and this is what holds out a promise of innovation for new applications and—it has to be said—new toxicity risks. We shall be seeing many more nanotechnology applications in electronics, energy, new materials, agriculture, food industry and in medicine. But there is nothing particularly *new* here so far as basic *science* is concerned. It is not a theoretical breakthrough like Newtonian mechanics, quantum theory or the DNA helix.

The cost of nanotechnology research facilities (e.g., for nanometrology and the characterization of nanomaterials) is very high. A superlative, multifunctional atomic force microscope can cost over £1M, and it has to be run and maintained by experts. The development of efficient and effective nanoscale processes for the industrial *manufacture* of nanomaterials and devices is also expensive. For example, one will need new complex machinery and clean rooms of an exceptional standard, and newly skilled technicians. Many companies are not convinced that the returns are worth the investment in developing new applications, despite a lot of financial support being given by the EU and other governments. The American research and development budget for nanotechnology runs into billions of dollars [20].

In the case of EU support—which I know well—just the logistical support required for getting the experts in different countries to know each other and share knowledge and facilities runs into hundreds of millions of Euros per year—before the real laboratory research has even started. There are risks. The investment required to really understand and manage the toxicity implications of nanotechnology in the realm of occupational, public and environmental health and potential ecological damage is prohibitive. The resultant uncertainty about risk and the fear of a regulatory clampdown inhibit industry. In my published research in 2010 with Dr Michael Riediker, an occupational health expert in Switzerland, I found that the European experts working in occupational and public health, or in human and environmental toxicology, believe that we still do not sufficiently understand the impact of manufactured nanomaterials on living systems [21]. This is a source of concern for them, especially since there are already over 1,000 different nanotechnology consumer products on the market.

Nanotechnology most certainly has a positive rôle. The cost and environmental damage of many *standard* (non-nanotechnology) industrial processes can be substantially reduced by introducing nanotechnology (such as nanocatalysts, filters and strong, lightweight materials). But without a radical change in our very conception of the associated economics and science (i.e., its limits, rôles and priorities), it may not attract sufficient public and private investment to bring the kinds of benefits that society so desperately needs. Too much nanotechnology is currently going into enhancing consumer goods such as cosmetics, sun-block creams, sports drinks, go-faster skis and car polish and not enough into the super-strong super-lightweight composites and energy-related industries that we need to confront the environmental and energy crises. Industries may need public financing to make their transition to a higher level of sustainability; it is not yet forthcoming and in the present economic climate may never be.

Finally, there is a serious issue of complexity and interaction at the nanoscale. Can nanotechnology R&D overcome the technological blockage of inadequate nanomaterial *characterization* (i.e., understanding the physical properties of substances at the nanoscale) in what is a theoretical environment of unprecedented uncertainty and indeterminacy? Without adequate characterization, nanotoxicology, too, remains largely on a rudimentary, case-by-case, *post hoc* basis.

4.3 Technology limited?

Again, what these and similar examples may well indicate is that increases in technological complexity are resulting in smaller and smaller increments in important technological applications. In other words, there may be growing evidence for diminishing marginal returns of significant technological applications from the increasing scale and complexity of emerging technologies such as nanotechnology and biotechnology.

5. THE ECONOMY OF SCIENCE

We also find difficulties in the economy of science; that is, how to continue to provide the funding, skills and infrastructures for the research and development returns needed. Is there evidence of a growing crisis of the same general form as the two described above, namely, an asymptotic approach to a limit?

5.1 Atomic physics and particle colliders

The Large Hadron Collider (LHC) is a European particle collider, and is the world's largest and most powerful. It lies in a tunnel 17 miles in circumference near Geneva and took 10 years to build. It was switched on recently, promptly blew a gasket at a cost of at least £24M to repair, and we now await an interpretation of the accumulating scientific findings [22]. Its purpose is to search for a new family of atomic particles including the so-called *Higgs Boson*. Some scientists think that the findings may enable them to build that elusive, complete *ultimate theory* of the physical universe.

Does the cost justify the information we shall gain? Who knows? If the collider does find new and smaller particles then will that actually complete the physical picture? Will scientists be satisfied, or will they require an even larger collider to find out something even more refined and abstruse at a cruder and grosser price?

The Americans were one step ahead for a while, trying to build an even bigger collider, but then they thought better of it. The American Congress in 1993 decided to stop the $8 billion Superconducting Supercollider Project to discover entities beyond quarks. $2 billion had already been spent on digging a 15 mile-long tunnel in Texas [23]. Was this cancellation the wisdom of limits or the economic reality of the limits of Big Science experiments; or in some way both?

The question, in short, is whether the cost is worth it in terms of the projected returns to society, returns that make a difference in *sustaining* that society.

5.2 Fusion energy

The International Thermonuclear Experimental Reactor (I.T.E.R.) is the scientific and technological project to end all projects, a gamble of enormous proportions. It promises a solution to modern civilization's ever-growing demand for energy in the face of dwindling oil resources and the management of the output of global warming gases. The idea is not to split atoms, but to fuse atoms, which releases the same kind of energy as that emanating from the Sun.

This project is well under way in Provence on a massive building site, with an expected total cost of 15 thousand million euros, but already suffering overspends.

Recently, Brussels approved another overspend of 1.3 thousand million euros, despite the grave EU financial crisis [24].

It must be remembered that I.T.E.R. is not a project to *produce* energy and plug it into national grids. It is an *experiment* to find out if we *can* produce energy in this way. In other words, it may not work, and so far fusion-based energy has spectacularly failed to work. There are two technological questions of great complexity requiring solutions: firstly, can we produce the energy in worthwhile amounts and, secondly, having produced it can we control it and keep it in its container? Then, of course, it has to be commercially viable. Pierre-Gilles de Gennes is reported as saying of this project: "We say that we will put the Sun into a box. The idea is pretty. The problem is, we don't know how to make the box" [25]. The direct benefits of fusion so far are probably zero. Not a few voices are saying that the project smacks of desperation and that there is a range of more promising, simpler and more economically and technologically distributed energy alternatives in which science and technology funders should invest.

In the case of fusion energy we cannot plot a graph of investment against marginal returns because at present all we have is investment and *no* returns. A series of smaller experiments over the past several decades has been rather disappointing—more power was put in than one gets out. If one day fusion power does work it would have the advantage of yielding massive amounts of energy with very little long-term nuclear waste. But will it work? No one knows. Some experts are very sceptical. The USA has cut back on its fusion budget. Meanwhile, in Europe the head of research at the French National Council for Scientific Research, Sebastien Balibar, thinks that the technology is inadequate for the task and that taking the cost from the public budget for science is unacceptable [26].

In this area too, then, we may see diminishing marginal returns or even, dare I say it, a continuation of zero marginal returns.

5.3 Scientific development economically limited?

Financial investment in techno-economic complexity is becoming a very risky business, which even governments and very large corporations hesitate to embrace. I realize that in the case of all the examples I have given there are counter-trends; the difficulty is discerning the overall trend, and I venture the hypothesis that the overall trend is tending to the Λ Limit some time this century.

It would certainly support my incompletability thesis if we could establish the nature of long-term historical trends in expenditure on scientific research and development. I browsed through some of the literature on this and found it to be very fragmentary, incomplete, hard to compare and difficult to draw conclusions from. This is hardly surprising, I suppose. In one interesting graph we see that over a period of about half a century, for the USA, there has been a great increase overall, as a proportion of GDP, which has arrived at a plateau in recent years [27]. It would be most revealing for researchers into the "economics of science" to look further into the data underlying this crucial issue. The two big questions here are (1) what is the rate of *return* to society on these increases? and (2) when does it become *politically* impossible to increase the proportion of GDP going to R&D?

We can now perhaps begin to hope for some deeper grasp of the multidimensional problem from which we started: whether science has a limit and, if so, of what nature?

6. COMPLEXITY

Joseph Tainter has thrown some light on the contemporary impasse of science and technology. He is a professor of anthropology and in his book *The Collapse of Complex Societies* (1988) he puts forward a thesis about the cost of increasing *complexity* in industrial society [28]. He looks at the collapse of previous civilizations such as the Roman and concludes that civilizations grow because they solve layers of problems with increasing complexity. However a point is reached when collapse becomes a real possibility because their investments in social complexity and their energy subsidies reach a point of diminishing marginal returns. We see impending collapse when a civilization starts rapidly disposing of significant layers of its complexity, such as cancelling huge projects, delayering bureaucracy and dumping outlying areas. We might add to this list: severe environmental damage, widespread financial instability, deepening inequality and popular uprisings and riots.

Contemporary evidence for diminishing marginal returns, says Tainter, is to be found not only in R&D but in agriculture; minerals and energy production; investment in health; education; government, military and industrial management; the productivity of GNP for producing new growth; and some elements of technical design improvement. Tainter concludes: "It is clear that some industrial societies are now experiencing declining marginal returns in several crucial and costly spheres of investment" [29].

I would hazard a guess that these difficulties, because they are *interdependent* in complexity, will converge and intersect in a point of *unsustainability*, a crisis or tipping-point that I have called the Λ Limit. It is far better to change our understanding and our science policy before the Λ Limit; for then it will provide us with the opportunity to rebound to a liberating reflexive science, in which the difficulties are on a path of resolution. There are several ways to go—slow decline, catastrophic decline (collapse), an unstable plateau (which cannot last indefinitely), or a new upward curve (Figure 1). An upward curve (i.e., a measure of human welfare returns on investment in science) requires a different conception of the scientific enterprise. Further linear progression of the enterprise we have had for the last century could be self-destructive.

7. REFLEXIVE SCIENCE

I think it is possible to rebound from the Limit and defeat diminishing marginal returns. Dare I say that in reflexive science, with the underpinning reductionist model abandoned, scientism would no longer hold sway either intellectually or in terms of the consumption of research resources. Instead, at the centre of scientific endeavour we might choose to put *sustainability* within the limits of science, technology and economy—with a new materials science, green energy alternatives, green chemistry and chemical engineering, clean water for all, agriculture without toxins and manmade genetic risk, housing for all, education for all, productive waste management, as well as expanded sciences of ecology, anthropology and political ethics.

Reflexive science is based on the reasoned rejection of scientism (scientific fundamentalism). It puts the identification of the *purpose* of science nearer to the people who should benefit from it, and further away from corporations, the military establishment, and even the science establishment. In a globalized world, this would mean nearer to the needs of the world population. This in turns entails the identification of the long term welfare *priorities* of scientific endeavour. Only then would policy-makers be well placed to consider the *content* of scientific projects. I have argued the case elsewhere for this approach with nanotechnology [30].

In science itself we need regional and global cooperation on new energy sources, pollution remediation, revival of biodiversity and forests, more equitable food and water industries, the prevention of epidemic diseases, affordable housing, better sanitation, the mitigation of flood and drought risk, waste disposal (especially of toxic materials), a clean atmosphere, addressing the environmental causes of cancer and neurological disorder, understanding the human psychology of prejudice and denial, authoritarianism and war, understanding biodiversity and ecology, monitoring climate change, proposing sustainable people-centred alternatives to the capitalist growth economy—an endless list. Is this not enough for science and technology to get on with?

The Nobel Prizes of the future will perhaps be for those who contribute to solving these problems. If not, then a new global prize needs to be created, and I suggest a *Noble Prize* rather than a *Nobel Prize*. If we can do this, then big speculative science will be subdued and well-reasoned, evidence-based, testable and verifiable, purposeful science will flourish. What I have said does not entail the end of science, but the end of a particular conception of science.

To give some concrete reality to the question of priorities in science and technology expenditure I wish to mention the growing global need for water and the scourge of malaria. I spent 12 years of my life teaching in Africa, so my experience of these issues is perhaps more direct than that of many people in the developed world.

7.1 Malaria and water

About two-thirds of a million people die of malaria every year, most of them children. One analysis indicates that full prevention and treatment measures in the worst-hit African countries would only cost about \$2.2 billion per year for 5 years [31].

The lack of clean water kills and debilitates millions of people every year. The World Bank estimates the cost of reaching "basic levels of coverage … in water and sanitation" to be at least \$9 billion, or \$30 billion a year for "achieving universal coverage" for water and sanitation [32].

Now compare these figures with those I gave earlier for scientific projects. It might be said that science has already given us the tools to deal with these issues, so the real problem is the political will. That is, the problem does not lie with the scientists, technologists and engineers. I consider this to be evasive thinking. Science, at the end of the day, especially publicly funded science is nothing but our own decision-making and is everyone's responsibility. With a wholehearted scientific and technological assault on the major problems of humanity—as was the case in putting a man on the Moon—they could probably be solved much more quickly and cheaply. This assumes, of course, that existing knowledge is sufficient and that the challenge is of an engineering nature. We have to keep in mind that *not* solving the water crisis will eventually cost more than this, and possibly more than the global economy can bear.

My resolution of our encounter with the Λ Limit, then, is that a new upward curve requires more than just more science and technology on the same basis as before—it needs a new conception of science, *reflexive science*.

8. CONCLUSION

A substantial part of our current human problem is a failure to recognize limits and the arrogance that goes with that. So, I would like to conclude with a sobering and very simple fact about human ignorance provided for us by science itself. And that is: we cannot possibly know what is outside the humanly observable universe. No improvement in instruments or techniques will allow us to overcome this problem. This is due to the fact that our observable universe is constrained by the speed of light [33]. We cannot know anything at all about what is beyond our observational horizon. Is our visible universe a finite part of a whole, and if so how big a part? Is it instead an infinitesimal part of an infinite universe? Is it typical of the rest of the whole, or not? We do not know the answers to these questions, and *we can never, ever know*. It also means we cannot *ever* be certain we understand the origin of the *whole* universe, despite the impression given by some promoters of scientism.

Perhaps, then, it is time for us to start deploying science to put our own small and still rather lovely house in order.

ACKNOWLEDGMENTS

This paper was first presented as a public lecture at St Mary's University College, London on 21 February 2012. I am grateful to Prof. Philip Esler for supporting this occasion and to Prof. Richard Leach of the National Physical Laboratory (UK) and Prof. Ugur Tüzün of Chemical Engineering, University of Surrey for their questions on the day. Responsibility for this speculative thesis is entirely mine.

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