

The Prime Directive for scientists should be to tell the truth as best they understand it. Unfortunately, experience shows that when science intersects with politics or the opportunity to get funding, truth often becomes forgotten and hype takes over. I give some examples.

Laser Fusion

My readers will be aware of the recent experiment at the National Ignition Facility which is part of the Lawrence Livermore lab, The claim was that fusion was induced generating more energy than was input. This was hailed as a breakthrough, showing that practical fusion was on the way. What's the truth? Fusion was indeed induced by a giant array of lasers, and the energy of the laser pulses was less than the energy produced by fusion. But this ignores that powering the lasers is extremely inefficient, and so the true input energy was perhaps 100 times greater than the energy created.

In effect the lasers induced a small thermonuclear explosion (which you can think of as a micro hydrogen bomb). Radioactive gunk is spread throughout the chamber in which the fusion takes place, and currently only one such pulse at most can be done in a day. There is absolutely no chance that laser fusion can be scaled up to produce steady electric power at any quantity.

Even the physics here is rather dull. It is well understood that if you heat the appropriate materials to an extreme temperature, then fusion will occur. So this experiment is extremely expensive, not very interesting and irrelevant to the prospects for practical fusion power. There are reasons to suspect that the true motive is to study thermonuclear fusion so as to build better hydrogen bombs. I don't believe that the world (or indeed the U.S.A.) will benefit from 'better' bombs but if this is the true motive for the experiment the public should be told.

I'm not against fusion research, the payoff for success would be huge. The most promising approach is to contain very hot plasma with magnetic fields, and the favored design is the 'Tokamak' which which originated in the Soviet Union more than 60 years ago. There are massive difficulties in making the idea work but progress is being made, albeit very slowly. I don't expect practical fusion power anytime soon, and advise my readers to treat claims of imminent success with great skepticism.

Quantum Computing

In the 1980's Richard Feynman was advocating using a quantum device to simulate physical systems, but the idea attracted little funding or interest. This changed in 1994 when Peter Shor, then of Bell Labs, found an algorithm for a quantum device that could rapidly factor integers, a problem that appears very hard on a classical computer. Shor's algorithm is of great theoretical and perhaps practical significance, as much of internet security would be broken if

it could be implemented at scale. Over the last 30 years there have been innumerable papers on quantum computing claiming improvements in methodology, both algorithmic and in hardware implementation, and IBM have announced ‘CONDOR’, a device with 1000 qubits. [A qubit is the basic quantum unit here – and a quantum computer will have numerous qubits that are ‘entangled’ which implies quantum weirdness, where there is at any time a complicated joint distribution of the states of the system.] I was impressed at this announcement and decided to look up the largest integer that has actually been factored using Shor’s algorithm. The answer is that no such factoring has ever happened! A version of the algorithm did factor 21 (but required a side calculation needing the information that $21 = 3 \times 7$. This is obviously a cheat – as the authors of this work freely admit. The cheat is carefully explained in the wonderfully titled *Pretending to factor large numbers on a quantum computer* (<https://arxiv.org/abs/1301.7007>), which was also republished in Nature with a disappointingly softened title.

Quantum technology is a perfectly valid field and there have been real achievements in secure communication. I was especially impressed by a paper in which a Chinese satellite (<https://www.nature.com/articles/nature23655>) used quantum technology to distribute cryptographic keys with absolute security. But the hype in the field is huge. One sometimes gets the impression that quantum computing will be a kind of ‘universal solvent’ (which the old alchemists were searching for) and will be able to solve any kind of computational problem fast, and further this is going to happen soon. None of this is true.

So here is an objective metric to measure advance in the field. To evaluate real progress in quantum computing one should ask what is the largest integer one can factor using Shor’s method. How well will CONDOR do here? When Shor’s algorithm was published I told my friends that this was a technology for the 22nd century. Nothing has happened since then to change my view.

Hype in science is nothing new. I remember in the 1970’s the new idea in physics was the possible existence of free magnetic monopoles. Around that time I heard an interview on BBC radio with a monopole expert:

” Magnetic monopoles might lead to a cure for cancer, and a mole of monopoles would meet the country’s needs for a year. ” I remember thinking at the time, this is a person working hard to get his next big grant.

One reason that hype is common in Academia is that there is no real downside to making promises that are not fulfilled. If you promise commercial fusion power within a decade you don’t get fired if in ten years time actual progress has been negligible. I have worked in government (cryptography), finance (predicting and trading the markets) and the academy. Of these three it is absolutely clear that the academy is the least interested in what is true. The reason is simple: if you are code breaking you either read the secret message or you don’t. If you

decrypt a message that says ‘Attack at dawn’ you likely have got it right. In my trading job, you either make money or you don’t and if you have a new idea your colleagues are very interested in whether you are right. Their bank balance depends on this. In the academy whether your ideas are right is much less obvious. A really good paper may be ignored and a really bad paper make you famous. Overall, progress is made, but it’s a slow process and there’s lots of opportunities for hype and even fraud and grift.

There is a major related problem in the administration of big science, akin to regulatory capture. If you want to know if gain-of-function research on human pathogens should be funded, and you set up a committee of gain-of-function experts, the answer will be to fund it. The committee members benefit from generous funding for their field. Exactly the same applies to laser fusion and quantum computing. To decide (for example) the best approaches to quantum computing one does need specific expertise, but general scientific competence should be enough to judge the likely value in the near term. Similarly one should not need deep expertise in nuclear weapon technology to form a judgment about the value of upgrading U.S. nuclear weapons at a total cost of around two trillion dollars. In a democracy it should be possible to outline to the public why this is a good idea. Instead these kinds of very important decisions get made by a small group of experts who will all benefit from the program, and the political decision makers, who usually have no scientific competence, have little choice but to go along.

What is to be done? I don’t know, but I do think that the National Academy of Sciences should take the lead in exposing obvious hype, and policy makers who may not have much expertise should be skeptical of advice which seems self-interested.