Feedback

Letters and comments that appear here may have been edited.

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In defence of fusion science

In reply to "ITER's woes" (Feedback, July p22) about the practicality of controlled nuclear fusion.

It is widely acknowledged that science thrives and progresses in an environment where constructive criticism flourishes and encourages research. It is therefore sad to see in *Physics World* anonymous comments from your website masquerading as legitimate, serious scientific critiques.

We cannot allow nonsense like this to be perpetrated in a public forum without a response. We have devoted, between us, more than 50 years to achieving the objective of controlled fusion power produced in magnetically confined plasmas. It is insulting to be told by one commenter, cmfluteguy, that our work amounts to no more than a welfare project and that we do not have the intellectual integrity to admit its impracticality. Fusion works in the stars: all the renewable energy that will ever be harnessed on Earth was ultimately produced by fusion in a place less than 10 light-minutes away. Some may regret the existence of the hydrogen bomb, but we know that this also works.

We are fully aware that controlling this enormous, practically inexhaustible and relatively safe source of power in an economical way in a furnace at 200 million degrees on Earth is a tall order, requiring as it does the solution of challenging problems in plasma turbulence, transport, stability, heating, ash-removal and even greater materials and engineering challenges. It is, however, completely incorrect to say, as cmfluteguy does, that Halliday and Resnick's well-known text "demonstrates the impracticality of doing this on a large scale". Statements like this are reminiscent of the mathematical "proof" of the impossibility of heavier-than-air powered flight just before the Wright brothers' famous demonstration at Kitty Hawk, or Lord Kelvin's claim that all of physics was understood just as quantum theory was starting to emerge.

In a separate comment, protogonus compares the tokamak approach to fusion used in the ITER project to cold fusion. This is like comparing astronomy to astrology. The Joint European Torus (JET) at Culham, Oxfordshire, demonstrated in 1997 that controlled thermonuclear fusion of deuterium and tritium is scientifically feasible; a few years before that, cold fusion was shown (at many centres in the world) to be experimentally and theoretically impossible. There is every reason to expect that ITER will establish conclusively that fusion can produce electrical power in usable amounts.

Fusion power is certainly proving to be a long haul, and requires serious investment to make it a reality, but the price tag of ITER and other large fusion projects should be viewed in the context of the enormous benefits that fusion offers. The aggregate cost of ITER over its lifetime will be less than 1% of the annual worldwide energy budget. We have no objection to comments such as the one from adivita, which raise legitimate questions concerning the feasibility of the ITER approach, but we urge *Physics World* to refrain from publishing ill-informed, unfounded, pseudonymous criticism, as it has no place in a professional scientific publication.

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Bacon's predecessors

In reply to Brian Clegg's feature article "Debunking Bacon" (June pp37–41), which examined the life and career of the medieval scholar Roger Bacon.

It is pleasing that Clegg seeks to cast a more positive light on Bacon and his legacy. Although Clegg states that Bacon cannot truly be called "the first scientist", he was certainly a scientist in the sense that science deals with knowledge rather than wisdom. A similar case can be made for a number of other important medieval thinkers, including Robert Grosseteste (ca 1175–1253), who is the subject of an inter-disciplinary project at Durham University (http://ordered-universe.com) that aims to reassess his scientific works and to rekindle interest in this most mathematical and intriguing of minds.

Grosseteste may or may not have been Bacon's tutor but they were in Oxford at the same time (Grosseteste became tutor to the Oxford Franciscans in 1228/9) and his work had a strong influence on Bacon. The source of Grosseteste's originality lay in the way he thought about unity and order, and in his search for causal explanations of natural phenomena. As we have demonstrated recently (Bower *et al.* 2014 *Proc. R. Soc. A* **470** 20140025), his search for unity in his explanations was underpinned by a belief in an ordered universe and based on observation. In his treatise on colour (*De Colore*), for example, he stated "What is understood in this way about the essence of colours and their multiplication, becomes apparent not only by reason but also by experience to those who thoroughly understand the depth of the principles of natural science and optics."

The question of whether the principle of the telescope was properly understood at the time of Bacon and Grosseteste is an intriguing one. Clegg refers to Bacon's interest in optics, but Bacon was not the first to discuss optical devices "contrived so that distant objects appear near at hand". In Grosseteste's discussion of geometrical optics in his treatise on the rainbow (*De Iride*), written between about 1228 and 1232, we find the statement: "For if known perfectly, this part of perspective shows us the way in which we may make things placed very far away appear as though placed close by." Certainly, Bacon was not the first to realize that telescopic viewing based on refraction was possible. Whether or not such a device was actually constructed is an entirely different matter.

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Telescopic hindsight

In reply to the *physicsworld.com* blog post "Cosmic blunders that have held back science" (2 June, http://ow.ly/xwC7C), about an essay by the astronomer Avi Loeb in which he criticized, among others, his Harvard University predecessor Edward Pickering, who claimed in 1909 that telescopes had reached their optimal size.

I think Loeb's article is slightly unfair on Pickering. We all know with hindsight that he was wrong, but that doesn't mean that his statement was unreasonable based on what he and others knew at the time. Faced with the cloudy skies and poor "seeing" in New England, Pickering may well have considered that there would be little advantage in building larger telescopes there.

We can also look at a similar decision that took place a generation or so later, when the Royal Greenwich Observatory decided to site its 2.5 m-class Isaac Newton Telescope at Herstmonceux in Sussex. Within a few years of that

decision, the advent of transatlantic air travel made it practical for astronomers to go almost anywhere in the world, and thus to use the best sites for telescopes. For an astronomer in Pickering's day, in contrast, locating Harvard's telescope on a mountaintop in California would have required observers to travel for several days on the train, followed by hours in the saddle to get up the mountain.

We also need to remember that although Edwin Hubble got the credit for demonstrating the expansion of the universe using the Hooker Telescope at Mount Wilson (then the largest in the world), the first measurements showing redshifts of spiral galaxies were made by Vesto Slipher approximately a decade earlier, in 1912, using a much smaller telescope. Slipher's results subsequently informed Willem de Sitter's ideas of an expanding universe based on his solution to Einstein's equations of general relativity.

Laurence Cox
via *physicsworld.com*

Listening exercises

In reply to Robert P Crease's article "Why don't they listen?" (May p19, http://ow.ly/x6xUr) about the reasons politicians alter or ignore scientific advice.

Crease's analysis is insightful and I think it could apply equally well to the general public. It does seem that many people don't appreciate the difference between an experimental result (that is, a fact about nature) and a mere opinion.

markhodges
via *physicsworld.com*

The public has had some bad experiences with scientists – think of the problems linked to lead in gasoline, cigarettes and innumerable diets, just to pick a few. I can buy scientific representation just like legal representation if I have the money. Also, science is a lot harder to understand now. When the atom was like a little solar system, people didn't think it was as hopelessly intimidating.

SUSYSPIN
via *physicsworld.com*

I think the article started getting at the main issue in the last section. Here, Crease implies that while the US science adviser John Marburger did a decent analysis of the problem based on Max Weber's three "categories of authority", his list of categories may have been incomplete. It certainly seems that "ability to predict" could become a major component of authority in the future. When (or if) a majority of the citizens in a society, as well as their political leaders, learn to place authority in those who make the best predictions, scientists should gain authority. Flawed as current scientific practice can be, I don't think there's much doubt (at least among those who seriously examine the evidence) that science, as a discipline, really has no competitors in terms of the quality of its predictions – although that is different from saying that all, or even most, of those predictions will turn out to be correct or accurate.

kyoung21b
via *physicsworld.com*

Science doesn't seem to be popular, and if it is not popular, how can politicians take it seriously? That's the problem with science. It can't stop being a specialist field.

msafwan
via *physicsworld.com*

Bad practice in exams

In reply to Matin Durrani's article "Experimental mistake" (May p15, see also http://ow.ly/vDYIM) and the discussion that followed (Feedback, July p20 and June p21–22).

As a recently retired teacher, I have witnessed many examples of staff "bending rules" or "coaching" pupils (and worse) through internal assessments, and not only in practical exams, either. Prior sight of questions, dictated answers, crib sheets, marking scheme checklists, answers displayed on the exam room blackboard, redrafting of failed papers – I have seen them all, in both private and state schools.

None of this mattered much, of course, when the final grade was almost wholly dependent on an external exam, but the age of "turn up to pass" modular courses is upon us. Contrary to some reports, head teachers, exam boards and politicians all welcome grade inflation, and staff feel pressured to "get the pupils through their assessments". Even the deserved failure of weak, lazy, truanting or disruptive pupils is deemed to reflect badly upon the professionalism of staff members. The very idea that occasional "verification" inspections will expose or deter these practices is absurd.

Name and address supplied

Purposeful reports

In reply to the comment "The rest is silence" (Feedback, June p21) by bbdalzell.

Having spent more 40 years in physics, bbdalzell decries the need for writing progress reports. But after 60 years in physics, I have come to the point where I write progress reports to *myself*. After a lapse of time and a million thoughts, I often forget what I was thinking when, or how I did what I did. These reports can be short notes, but they serve a real purpose.

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Another magical expression

In reply to comments on "Beautiful equations" (Feedback, July p22).

We often hear about Euler's identity and $e^{i\pi}$, but rarely about e^{π}, which has magical properties of its own. If you raise it to the power $-x^2$ and integrate from $-\infty$ to ∞, the result is 1. If the ubiquitous Gaussian distribution were written in terms of e^{π}, we would eliminate the factors of $\sqrt{\pi}$ needed to normalize it. Like this:

$$\int_{-\infty}^{\infty} (e^{\pi})^{-x^2}\,dx = \int_{-\infty}^{\infty} e^{-y^2}\frac{dy}{\sqrt{\pi}} = 1$$

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Quantum or not?

In reply to the *physicsworld.com* news story "Is D-Wave's quantum computer actually a quantum computer?" (20 June, http://ow.ly/ye1b3).

I cannot say if the computer built by D-Wave Systems works or not, as I have neither seen nor examined the system. But I also cannot say definitively that a practical, effective, pure quantum or quantum annealing system won't ever be built. That kind of prediction seems rash. After all, working quantum computers have been around for years now. They have shown the expected increase in computation speed for specific problems, such as Shor's algorithm for factoring integers. The practical performance of such systems, however, is worthy of discussion. Now that we can perform quantum calculations on real machines, and those machines are beginning to scale up, we are encountering performance limits in translating the outputs to usable/observable form. Quantum computing is here. It's just a matter of time before it becomes ubiquitous.

funakoshi
via *physicsworld.com*

The answer is a yes/no superposition.

Giovanni Spataro (@GiovanniSpataro)
via *Twitter*

Only when no-one observes it.

Janoxley (@joxo72)
via *Twitter*