Hypotheses and Humility: ideas do not have to be right to be useful.

Editorial published in Bioscience Hypotheses, Volume 2(1), 2009, Pages 1-2

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Abstract

Hypotheses need not be completely right to be useful. A hypothesis that explains an important aspect of the world, but not all of the world, can stimulate new ideas and new experiments which take science forward. Phlogiston is a typical case study. While not universally accepted at the time, and subsequently proved to be wrong, the hypothesis that there was a 'fire-giving' substance in inflammable materials prompted the development of quantitative chemistry, and the subsequent discovery of oxygen. Hypotheses today do not have to explain all aspects of the world perfectly. But they should be honest about those aspects they do not explain, and demonstrate some humility in their limitations. It is very unlikely that a single idea explains everything in a biological system. Theorists should recognise this: if they do not, their readers will, and will discount their ideas because of their lack of awareness of their context. Humility may get you a hearing.

TEXT

Does a hypothesis have to be right? Obviously we cannot require a hypothesis to be proven by robust experiment beyond reasonable doubt. That is not what a hypothesis is. But should we put a hypothesis into the public domain that we know has one, or two, or even more obvious flaws, and not waste our readers' time and subscription?

I believe we might, for two reasons.

Firstly, as I have mentioned before, the object of publishing a hypothesis is not to add another stone to a dead pyramid of fact, an Objectivist logical structure that we work out from first principles and then stick to for all time. It is to stimulate science with a new idea, from which others will arise as our knowledge of the world develops.

As scientists we tend to view past incorrect hypotheses, such as phlogiston or the luminiferous aether, as being historical oddities, dead-ends in research that were 'wrong' and would have better been avoided. But this is simplistic. Without a specific hypothesis to test, would Pasteur have devised the experiments that showed that organic matter could be sterilised, and remain sterile in the absence of contaminated air? If no-one had postulated a luminiferous aether through which the Earth was travelling, would Lodge, Michelson, Morely and others tried to measure the velocity of the Earth, and by failing lay the groundwork for relativity? In general it is easier to answer the question 'is this right' than the question 'what is right?'. So a reasonable hypothesis has value even if it is wrong, even if parts of it are suspected of being wrong at the time, because it provides a specific idea to test.

Phlogiston theory illustrates this. Phlogiston was the substance that was combined with ores to make metals. Normal air was lacking in phlogiston – when a metal burned, its phlogiston was released into the air, leaving the calx, the dephlogistinated 'essence' of the metal. When the air's ability to accept phlogiston was saturated the air could not support any more combustion, and burning ceased. Phlogiston theory was consistent with eighteenth Century knowledge of chemistry (and biology), made testable predictions, and was consistent with everyday observation – watching something burn, you could *see* its substance being reduced as something flowed from the burning matter into the air.

Even at the height of acceptance of the theory, a minority of chemists thought Phlogiston theory was at best poorly supported and at worst wrong. In the 1740s they did experiments to probe the theory, and found that not only did metals gain mass when they burned (it was not clear if phlogiston had mass, but it could not have negative mass in any reasonable variant of the theory), non-metals such as sulphur, phosphorus and carbon did as well. There was a fraction of air that was removed when substances burned (some confusion about what fraction that was arose because the standard assay for 'dephlogistonated air' was quantitatively ambiguous). Eventually it became clear that only careful *quantitative* measurements would resolve what was going on, and when these were done by Lavoisier and Priestly the Phlogiston theory collapsed, oxygen was discovered, and the importance of quantification in chemistry was confirmed (see reference (1) for a review of the history of Phlogiston's rise and demise). Thereafter new hypotheses had to conform quantitatively as well as qualitatively to the facts (something I would appreciate authors considering when they submit papers to this Journal). Phlogiston was an important stepping stone to that understanding, not because it was right or wrong, but because it provided something specific to test.

Secondly, as I and others have commented, biomedical science is becoming increasingly conservative and risk-averse in the Western world. So even ideas that are almost certainly right find it hard to get an audience. Publishing one that has some remaining flaws helps to counter this.

So, yes, I believe that we can publish papers that are interesting, intriguing, but not yet fully worked out and quite possibly wrong. I have stated this quite explicitly (2-3). The publishers agree. Readers seem to agree: papers are being downloaded from the Journal's web site, so someone is reading some of them. But most authors do not agree. Too often papers are cast in language that seeks to explain the entire of a field of biology in terms of absolute certainty, as if any doubt, any areas left unexplained are a fatal flaw in the hypothesis. And I admit that my editorial process sometimes encourages this, as I respond to a paper with a list of things that I consider clearly wrong. But I do so because what the paper *does* explain is not sufficiently interesting in light of what it omits, and because I believe the informed reader will trip over the flaws before they see the opportunity that the new ideas represent, and so stop reading. A paper that starts out with "We know that the sky is green ..." will not get anyone to read on about the author's ideas on colour perception or aerial algae or oxygen photochemistry, because the reader will say "No it is not" and move on to the next paper.

So papers that state a hypothesis in a way that admits it is not complete, or may even be wrong, are to be welcomed. They lead the reader past the problems that the reader would have seen anyway to the new ideas the paper presents.

In this vein I am particularly pleased to be able to publish George Parris's paper in this issue (4). I think it is probably wrong. So does Dr. Parris, and he says so:

> The analysis that led to the "internal guidance hypothesis" (as summarized below) did not consider known biology, and was only restricted to mechanisms that seem chemically plausible. Nonetheless, a model that seems biologically plausible resulted from the work and it is presented here for consideration by the evolutionary developmental biology community as a hypothesis.

and even more bluntly

> This might be a useful idea even if the mechanism proposed here is completely wrong.

But there are some exciting, testable ideas here that provide an explanation for important, poorly explained aspects of the evolution of complex body plans. Experienced geneticists will look at this and think of a dozen reasons why it is wrong (those with experience in the study of the evolution of repetitive DNA will probably be quite annoyed). Experienced developmental biologists will think of a dozen experiments that provide evidence against aspects of the ideas here. Of course! They can probably find a dozen experiments to provide evidence against each others' ideas as well. This is not meant to be the last word in Evo Devo, but a new look at a hard, old problem which I hope will stimulate new ideas. If they do think of those experiments, and then do one or two of them, then we will have achieved our goal.

Parris is not alone. Even in Bioscience Hypotheses' short career, other papers have come out that are quite explicit in their limitations (I mention references 5-7, but just as examples: there are others). Too many submissions, though, do not say this, and rather make global claims of certainty that are not justified by the evidence. Be warned! Your readers will not be convinced. Your paper does not have to explain everything. A little humility in the face of biology can be a good thing. If you reader gets to the end of the paper and says 'I am going to go and test that', then you will get a citation, right or wrong, and have added real value to science.

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