When scientists disagree

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Stephen Blundell ponders the history of disagreements between scientists — from the ancient Greeks to questions about room-temperature superconductivity — and what they show about how to disagree well.

Journalists would never begin an article with a statement such as "Politicians claim that Brexit is an unrivalled success" or "Politicians conclude that Brexit is an unmitigated disaster", but instead be clear that in each case it was a particular group of politicians making the claim. Although science news stories are often written as if all scientists speak with a single voice, most of them originate from a small group of individual scientists who have completed a single study and are promoting it. Scientists do not necessarily all agree with each other, and sometimes those disagreements can become extremely bitter. What can historical scientific disputes tell us about how to do physics today?

[H1] Disputes over priority

Disputes can break over matters of priority: who came up with the idea first? Isaac Newton claimed to be above such disputes, writing to Edmund Halley that "Philosophy is such an impertinently litigious Lady that a man had as good be engaged in Law suits as have to do with her" [1]. In truth, his secretive way of working, publishing only when pressed, and refusing to grant that any of his rivals could have made an important contribution to his thinking, made such disputes almost inevitable. He was justly proud that his inverse-square law of gravity explained Johannes Kepler's laws and the elliptical motion of the planets around the Sun; he was however unwilling to admit that the inverse-square law had been suggested years earlier by Christopher Wren and Robert Hooke, albeit on the much lower-level (but nevertheless highly intuitive) notion that gravitational force should, like light from a point source, somehow spread out on a spherical surface of radius $4\pi r^2$, and thus should decay in strength as $1/r^2$. His refusal to include even anything more than a somewhat grudging acknowledgment to either scientist in his *Principia*, which would scarcely have detracted from the magnitude of his achievement, fuelled the simmering feud with Hooke. Likewise, his dispute with Gottfried Wilhelm Leibniz over which of them had invented calculus (spoiler alert: they both did) was extremely acrimonious and as a result we are left today with two separate notations for the time derivative, $\mathrm{d}y/\mathrm{d}t$ (Leibniz) and $\dot{y}$ (Newton).

[H1] Disputes over principle

It is perhaps more edifying when disputes break out over matters of principle. Cambridge astronomers Fred Hoyle and Martin Ryle did not get on, for many reasons associated with their own respective backgrounds and personalities, but they were at least in dispute over a matter of real significance: is the Universe eternal (Hoyle) or did it have a beginning (Ryle)? Ryle was, of course, eventually proved right, although what didn't help was that he initially discounted Hoyle's steady-state model by relying on his own catalogue of radio sources (the so-called 2C, or Second Cambridge, catalogue) which had inadvertently miscounted some of the radio-emitting objects. Various weaker sources had blended together in the interferometer, creating the effect of a single stronger source, and this had happened multiple times so that the source count (which assesses the number of sources as a function of their power) was skewed. Ryle had approximately the right conclusion, but for the wrong reason.

This problem came to light through a related, but unflawed, study by the Mills Cross Telescope in Australia [2].

Other disputes on matters of principle have included whether or not light was a particle (Newton) or a wave (Hooke); Newton's corpuscular view held sway for decades, until Thomas Young's experiments pointed very much in the wave direction. The final irony was that quantum mechanics showed that light was both particle and wave, with the wave–particle duality melting away the distinction.

The question of whether everything is made of atoms, bound up with the question of whether the vacuum exists, had been controversial right back to the time of the ancient Greek philosophers, with the Aristotelian viewpoint (that denied atomism and the vacuum) prevailing over the atomists such as Democritus, Leucippus, and Lucretius into the early modern period. Leibniz, who was right about lots of things, got these two calls spectacularly wrong, writing near the end of his life: "All those who maintain a vacuum are more influenced by imagination than by reason. When I was a young man, I also gave in to the notion of a vacuum and atoms, but reason brought me into the right way" [3]. These "fictions of superficial philosophy", as he called them, turned out to be only too real, though as late as 1902, and despite the advances made by John Dalton, Pierre Duhem could write that "modern chemistry does not plead in favour of the Epicurean doctrines" [4].

[H1] The value of collaboration

Though many factors can bring scientists to dispute with each other, there is so much more to be gained by working together. One of the first people to really capitalise on this was Heike Kammerlingh Onnes, who was in a race to be liquefy the last of the so-called "persistent gases": helium [5]. By the turn of the twentieth century, nearly all the known gases had been cooled to sufficiently low temperatures to demonstrate the gas to liquid phase transition, but helium had stubbornly resisted all attempts (the attraction between helium atoms is extremely weak, making them extremely reluctant to group together). Kammerlingh Onnes' main competitor was James Dewar, working largely by himself in a laboratory in London, but over in Leiden the working methods were rather different. Kammerlingh Onnes had realised that the experiments were sufficiently complex that he needed a small team of highly-skilled technicians to construct the glassware, maintain the pumps, operate and repair the equipment and keep his laboratory in tip-top condition. It was perhaps not surprising then that in 1908 his laboratory was the first to liquefy helium, earning him the Nobel Prize for Physics five years later and providing him with the experimental capability of making serendipitous discoveries in low-temperature physics, which led to the unexpected discovery of superconductivity.

This reliance on teamwork and mutual cooperation was vital for large-scale endeavours, such as the Manhattan project, and has led to the enormous teams that now work on major particle physics experiments. The individual scientist is barely visible in these vast groups of workers, detecting rare decays or producing extremely short-lived exotic particles. In such collaborations, the challenges include managerial ones: keeping the scientists focused on their common goal, sharing information, critiquing new results, handling the PR message, and maintaining the funding stream to fuel the entire operation. Even here though, there can be internal disputes between competing teams analysing data from different parts of the same experiment [6].

Small-scale science still thrives, and here disputes can be resolved by the work of multiple teams. Earlier this year, a report appeared of the discovery a possible ambient-pressure room temperature superconductor, known as LK-99 [7]. This would be a major breakthrough in superconductivity research, and the announcement came on the back of controversy over recent claims of new room-temperature superconductors stabilised at high pressure which have been dogged by questions about their reliability [8]. However, doubts were quickly raised over the quality of the LK-99 work, and other groups quickly repeated the synthesis of the new material (a copper-doped lead phosphate) but failed to replicate the superconducting behaviour. Then it turned out that the claimed 104°C superconducting transition temperature closely matches the critical temperature of a structural phase transition in Cu_2S, a compound that shows up as a major impurity phase in LK-99 [9]. Moreover, the resistivity of Cu_2S drops like a stone on cooling through this transition, which could easily be misinterpreted as a superconducting transition in an experiment that was a little cavalier with accurately probing low-resistance states. Thus, a potentially exciting result has quickly melted away, but thereby demonstrating the intrinsically self-correcting nature of the scientific endeavour.

Scientists disagree about all kinds of issues, as long as there are interesting and unexpected experimental results appearing, and intriguing and puzzling questions remaining. The trick is to make such disagreements amicable, and to find ways of resolving them. The scientists who set up the Royal Society were of a generation that had lived through the English Civil War, and were painfully aware that disagreements over matters of politics and confession could easily spiral into violence [10]. The scientific meetings they instigated were designed to provide a forum where scientific issues could be discussed, rationally and calmly, without impugning each other's honour to the point where the only resolution was pistols at dawn. These lofty principles of rational discussion were tested to the extreme by disputatious characters such as Newton, but in the main they worked. In these fractious times, it is worth remembering that scientific disagreements can be intellectually fruitful, illuminating the key questions in science and refining our ideas, but in the final analysis, cooperation is best of all.

Acknowledgements

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