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In Praise of Mistakes

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Mistakes are a key driver of scientific progress. We should do all we can to eliminate them, partly to keep the literature record clean, but also because expunging what is wrong often leads us to understand what is right.

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"I HAVE NOT FAILED, NOT ONCE. I HAVE DISCOVERED TEN THOUSAND WAYS THAT DON'T WORK"

This is one of great many reported wordings of a quote attributed to Thomas Edison about how he developed the light bulb into a viable and efficient device. It chimes with an insight that came to me in the most unlikely of locations and ways. One evening in Longyearbyen on Svalbard, the world's northernmost permanent human settlement, after a day teaching space physics at UNIS, the world's northernmost university, I happened to share the hotel sauna (and a crate of beer therein) with a surgeon from Oslo who had been flown in to perform a specialist operation at Longyearbyen hospital. We got talking about humans that we admired and when asked to select three, I chose Richard Feynman, Marie Curie and Ernest Shackleton for, respectively, charisma, humanity and leadership. More of Feynman and Curie later, but the surgeon was particularly delighted by my choice of Shackleton, saying how much he admired his determination and skill in rescuing every single member of his ill-fated expedition and how little respect he had for the other famous British Antarctic explorer, Robert Falcon Scott, whom he considered to have been arrogant and foolhardy. Why this conversation was so revealing to me was that it made clear something I have come to believe to be an important distinction in science—the difference between *heroic failures* and *dismal failures*. Heroic failures are to be cherished and celebrated because they often reveal more than do successes—it is unedifying dismal failures that must be avoided.

My point is that we should not be overly fearful of mistakes or failure in any area of science or we will not even try to push boundaries. Of course, we should check everything as deeply and carefully as we possibly can to avoid dismal failure, but it is vital that we do not allow that to cause excessive delay in publishing. Charles Darwin made his seminal observations on the voyage of the Beagle between 1836 and 1838 yet he did not publish them in full until 1859. His friends, botanist Joseph Hooker and geologist Charles Lyell, both warned him in 1856 to pause refining and developing his ideas and publish because other scientists were starting to think along the same lines. In the end, Darwin had to rush to publish one of the most important books in all science (Darwin, 1859) at a time of great personal trauma with the death of his son from scarlet fever and when his daughters were seriously ill with diphtheria. The reason for the rush was a letter he received from the Malay Archipelago from Alfred Russel Wallace that laid out the same themes as were in Darwin's now famous, but then still unpublished, book. The ideas of Darwin and Wallace were jointly announced to the world at the Linnean Society of London in 1858 (Darwin and Wallace, 1858).

I remember a few rare evenings riding my bicycle home, filled with the elation of a scientific epiphany. Somewhat ludicrously, I would ride with extra care on such special evenings—thinking I was the first human to ever realize this, albeit small, contribution to science and fearing it could be lost or delayed if something happened to me. Later, of course, it usually transpired that there were space scientists in other parts of the world who were pretty much there or thereabouts in coming to the same conclusion. That means that if I had waited, even for just one more round of checking, I risked being written out of the story as told by the literature record. For that is, the way of it: a great deal of the real development of science goes unreported. The notebooks of many famous scientists tell a very different story to their publications: the publications give the impression of a logical progression towards an infallible and inevitable conclusion, whereas the notebooks reveal a series of mistakes and failures with b backward steps and f forward ones (which is fine as long as $f > b$). Fortune favors the brave, and if we are right and have really moved things forward we will win the acclaim of our peers in the world of science if we publish–and if we are wrong they will surely tell us.

PEER REVIEW

Peer review is science's way of correcting its mistakes. In my view, it is by far the greatest British contribution to science. I say "British," but such things are never so simple as crude nationalism pretends, and peer review has incrementally evolved over the past 350 years in many places and in both procedure and aims (Moxham & Fyfe, 2018). In addition, although first introduced in 1665 by the founding Editor of Philosophical Transactions of the Royal Society of London (Henry Oldenburg), he was actually an immigrant to Britain who was born and educated as Heinrich Oldenburg in Bremen, Germany (Rix, 1893).

Peer review is repeatedly accused of being "no longer fit for purpose," often by those with a vested interest in overturning a part of the scientific consensus that it has generated. And that is, the key point—peer review generates and documents scientific consensus. Surely it can be fallible and/or inefficient and/or slow—but it is the only method that we have ever had of organizing scientific findings into a consensus. You just need to look at the information wild west of the internet to see what can happen without it—misinformation abounds and swamps reliable information. So next time you are left angry by an unfair and poor review of your paper (it happens!) you have to just accept it as the price that we all pay for generating scientific consensus and find a way around your unhelpful reviewer.

Peer review is harsh and sometimes cruel—mistakes will be commented on, and quite rightly so because the literature record must be kept clean, clear and correct. One of the most brutal aspects about being a scientist, and therefore subject to constant peer review and comment, is the imperative that it drives to be ruthlessly honest with yourself. You may feel your idea may be more elegant than reality, but if it doesn't fit all the facts then it is, to some level, wrong and you must concede that. It is vital to face up to a mistake as early as possible—we are all fallible but we do great damage to both our reputation and our effectiveness as a scientist by persisting in defending something that is, wrong. Science as a whole, like the individuals who progress it, learns from its mistakes.

MUTATIONS

Let me use an analogy. In nature, mutations are the key element of Darwinian evolution and they are nothing more than DNA replication mistakes (e.g., Pray, 2008) and look what evolution has achieved in turning single-cell organisms into the rich variety of life on Earth today. This has more than a trivial parallel to the development of science. In evolution, the principle of "survival of the fittest" decides which mutations thrive and which fail. Similarly in science, if a concept, theory or equation survives peer scrutiny and is adopted then it thrives otherwise, like an unsuccessful mutation, it dies away.

If one can recognize a mistake, it is often a path to unexpected progress. This can come about in a number of ways. An erroneous result causes scientists to conduct further experiments and often identify a previously unsuspected truth. Mistakes often bring to your attention new areas, techniques and theories that you had not realized were relevant and so drive lateral thinking and serendipitous discovery. Trial-and-error is a very common path to progress. A more subtle point is that making a mistake and then coming to understand why it is wrong is invaluable in helping you identify what is right.

The truth is that mistakes and failure are embedded in the very fabric of science. If we do not test our own ideas to destruction, somebody else will. I note that Marie Curie once said "There are sadistic scientists who hurry to hunt down errors instead of establishing the truth." Sadly, that is, a valid comment as there are individuals who use the excuse of the progress of science, when their real motivation is one-upmanship. As scientists we must rise above that, by not indulging in it ourselves and not being perturbed or deflected (into either agreement or disagreement) if it is targeted at us. It is rare, but scientists are, after all, human beings. The best scientists praise progress wherever it comes from and feel empathy with those whose ideas fall by the wayside. In other words, they try to class failures as heroic rather than dismal. My experience has been that the positivity and shared goals of the global space science community massively outweigh destructive petty rivalries. Maybe I have just been lucky, but I think not. I genuinely believe that the ethos of science drives better behavior.

LEARNING HOW TO HANDLE AND EXPLOIT MISTAKES

We should try to avoid mistakes but we should not fear them. After all, the greatest minds in science have all made mistakes. Stuart Firestein is a Professor in Biological Sciences at Columbia University and his excellent 2015 book "Failure: Why Science Is So Successful" reviews a great many historical and contemporary examples. I will give just one example here—a mistake by arguably the most famous, influential and astonishing scientists of all time, Albert Einstein. In 1916 he published his general theory of relativity, a truly astounding intellectual achievement, the equations of which included the “cosmological constant,” to make the universe static by counteracting contraction under gravity—something Einstein regarded as necessary at the time. Then in 1929 Edwin Hubble showed that the universe is expanding, and Einstein removed the cosmological constant from his equations, reputedly calling it his “greatest blunder.” Ironically, his blunder has turned out to be removing it as we now know that the universe is not only expanding, but the expansion is accelerating. To describe why that is, happening, scientists have effectively re-introduced the cosmological constant into general relativity and reinterpreted it as the energy density of space, or vacuum energy, that arises in quantum mechanics, and the related concept of dark energy (e.g., O’Raifeartaigh et al., 2018), which means it is negative where Einstein saw it as positive. Einstein’s mistake showed us the way forward when it turned out not to be a mistake even though he had proposed the very opposite of what is needed!

Serendipity—and maintaining a memory of a mistake—played a large part in my career. It was a mistake that involved Richard Feynman. In 1979 he came out to New Zealand to give a series of evening lectures on quantum electrodynamics at Auckland University while I was a post-doc there. They were brilliant lectures—entertaining, informative, clear, funny and fascinating—and he was, quite simply, the most charismatic speaker that I have ever seen. The Wednesday lecture posed a problem for me. It was a must-see event, but it was also our third wedding anniversary. The agreed solution was an early-evening meal at our favorite restaurant, followed by a quick dash over Albert Park to the University for the lecture (never let it be said I didn’t know how to show a girl a good time!). I was researching the spatial pattern of field-aligned flows of ions in the polar ionosphere at the time (the “polar wind”) and that day I had come across a review paper entitled “Ion velocity distributions in the high-latitude ionosphere” (St-Maurice and Schunk, 1979). I was already running late, so without looking at it, I hurriedly ran off a photocopy (all 36 pages of it) and rushed home. While I was waiting for Celia to get ready, I had a look at it and immediately realized I’d made a mistake: it was about bizarre ion distribution functions driven by collisions between ions and atoms and not about the spatial distributions of flows as I had hoped. I remember that when she emerged ready for her half-a-night out on the town, I dropped the paper in the waste bin, rather theatrically, saying “that was a waste of time and paper, I will never work on that!” I was wrong. Eight years later we were studying ion flows in the cusp using the EISCAT radar, searching for the ionospheric signatures of flux transfer events—bursts of solar wind magnetosphere coupling (we did find them eventually: Lockwood et al., 1993) and the derived ion temperatures were puzzling me. The radar measured the line-of sight component of the ion velocity and I was trying to use the ion temperature (derived assuming a Maxwellian distribution) to infer the magnitude of the velocity to get the vector flow, but that inferred velocity was often far too high making it look like fast flows were always nearly perpendicular to the radar beam! Joe Doupnik from Utah had joined us as a consultant when we were setting up the UK EISCAT activity and I remembered his wise mantra “always check your raw data—look at the radar spectra.” I did, and I immediately recognized a characteristic form that I had very briefly glimpsed that evening 8 years before in St-Maurice and Schunk’s brilliantly predictive paper. My mistake had led to the discovery of radar echoes from non-thermal ions in the auroral ionosphere, which generated a whole series of papers for both me and my colleagues. It showed me how you search for one thing and you often find something quite different and the key that unlocked that particular door was remembering a mistake I had made 8 years earlier!

The very next day, there was another seminal moment in my life. I met Feynman himself at lunch in the senior common room. He was charming and kind to me, a young researcher who had completed his PhD under a year earlier, and having a world-famous Nobel prize winner take a real interest in my work was wonderful for my confidence. I told him it was all coming together rather nicely except one aspect and he said “follow the bit that doesn’t work, young man, that’s the good bit”—advice that later led me to quite a few realizations (including the non-thermal ion distributions) and that I still give to my PhD students.

ONE MISTAKE WE MUST NEVER MAKE

My third choice was Maria Salomea Skłodowska (Marie Curie). For me she is not only an inspiration, but also a lesson in why equality of opportunity is so vitally important. Such extraordinary talent is so rare that it must never be ignored and I would invoke her as a perfect example of what could have been lost to humankind by gender discrimination—just as Srinivasa Ramanujan shows what we would lose to ethnic discrimination, John Dalton to religious discrimination, Michael Faraday to class discrimination, Linus Pauling to economic discrimination, Albert Einstein to racial discrimination, Alan Turing to sexuality discrimination and Stephen Hawking to disability discrimination. Talent for science can and does come from anywhere and we must never make the mistake of failing to recognize and nurture it because of a prejudice. What I love about Marie Curie is her skill, her total dedication to, and passion for, her research, and also how determined she was to use it for the good of humanity. She once wrote one of the most inspiring quotes that I know: “You cannot hope to build a better world without improving the individuals. To that end, each of us must work for his own improvement and, at the same time, share a general responsibility for all humanity, our particular duty being to aid those to whom we think we can be most useful” (Curie, 1923).

In 1903 Marie Curie gained a PhD from the Sorbonne, becoming the first woman ever to receive a doctorate in France. In the same year, she was the first women to be awarded a Nobel prize, despite the French nomination only citing her husband Pierre and Henri Becquerel for their studies of radioactivity: great credit goes to the Swedish mathematician Magnus Mittag–Leffler who made sure Marie was included in the award even though she had not been proposed for it. Of that incident Marie Curie's friend, the British engineer, mathematician, physicist, inventor, and suffragette Hertha Ayrton quipped "Errors are notoriously hard to kill, but an error that ascribes to a man what was actually the work of a woman has more lives than a cat." In 1911, Curie won her second Nobel prize, becoming the first person ever to do so. Being an entirely new area of study, neither of the Curies could have had any idea just how dangerous ionizing radiations from radioactivity are to human health. Famously, Marie's notebooks and personal effects are still so radioactive they are kept in lead containers and still only handled with extensive precautions. She died aged 66 in 1934, of bone marrow failure but probably not induced by her work with radioactive materials, as is commonly thought. A study of her exhumed body in 1995 found that it was almost certainly over-exposure to X-rays that killed her (Butler, 1995). The reason for that is, that she also developed, funded and operated mobile battlefield X-ray units to save the lives of thousands of wounded French soldiers in World War 1. She operated these with her daughter Irène Joliot-Curie who went on to also win a Nobel prize (for Chemistry), the only mother-and-daughter pair to ever do so. Sadly Irène, like her mother, died of lukemia aged 59 and almost certainly for the same reason. The cause of death of Marie and her daughter makes the way that she was repeatedly demonized and hounded by the hypocritical, racist and misogynistic press in her adopted country even more disgraceful and shameful.

I partly mention Marie Curie here in the interest of balance in discussing mistakes. In 2003 some class notes, written in 1907 by a 13-year-old student called Isabelle Chavannes, were saved from destruction by her great-nephew. They tuned out to be verbatim descriptions of lessons given by Marie Curie. From them we learn that Curie taught her students to avoid mistakes, saying "The secret is not to work too quickly" (Chavannes, 1907). Whilst noting all that I have said above about speed of publication being vital, Marie Curie was, of course, absolutely correct.

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