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Q & A

Enrico Coen

Enrico Coen did a PhD in genetics with Gabriel Dover at the University of Cambridge, UK. After a one-year postdoc at the Plant Breeding Institute, Cambridge, he then took up a research group leader position at the John Innes Centre in Norwich, UK.

What drew you to your specific field of research? After graduating in genetics in 1979, I did a PhD on *Drosophila* genome evolution in Cambridge. I developed a passion for research but also realised that scientists were not as objective as I had imagined: they could become attached to ideas and disparage those who disagreed with them. I hoped that perhaps I could find an obscure research area free from such attitudes. I applied for a fellowship to isolate genes underlying a well-known but understudied developmental and evolutionary trait: heteromorphic self-incompatibility in primroses. This proved an intractable problem with the technology available then — it took 30 years before the genes could be cloned — but the work started my lifelong interest in plant genetics. A year or so into my fellowship, a position came up at the John Innes Centre to study transposons in snapdragons. Over the next few years I worked with Rosemary Carpenter, an experienced snapdragon geneticist, to isolate and characterise several developmental genes, taking me into mainstream plant research. Did I find a research field untrammelled by personal attitudes? No, I came to realise that science is a human activity with all the passions and foibles that this brings, including my own.

What is the most memorable scientific advice that you've been given? Just before taking up my position at the John Innes Centre, I planned a trip to the US to visit key people studying plant transposons. Among those on my list was Barbara McClintock at Cold Spring Harbor. I rang her up towards the end of 1983 to finalise arrangements but was told that she was in Stockholm: she had been awarded the Nobel Prize, at the age of 81, for her work on discovering transposons in maize.

I was awestruck by Barbara once I finally met her — three times my age, yet she left me standing with the rapidity of her thoughts. She spent the best part of a day with me, but throughout that time, as she quizzed me with her sharp eyes, I felt that, if I gave a wrong or stupid answer, she would activate a trap door to open beneath my feet. My plan to study snapdragon transposons was met with scepticism. What new insights could I possibly hope to arrive at in a system that was so poorly developed compared with maize? I left inspired by her ageless mind but discouraged by her advice to forget snapdragons. Seven years later I was invited to give a talk in Cold Spring Harbor on the ABC model of flower development. Barbara was in the audience. After the talk, I mentioned to her that we'd met a while ago but assumed she wouldn't remember. "Sure, I remember," she said, "I gave you some bum advice". Then she said, "do you like Stubby?" I eventually realised that she was referring to Hans Stubbe, a German pioneer in snapdragon genetics. She led me to her office and fished out a stack of reprints that he'd sent to her over the years, including several on unstable genes. After I admitted that I couldn't read German, she proceeded to peruse the abstracts and select the reprints that she judged would be useful to me, stroking each as though bidding it farewell. "I'll sort out a bag for you later," she kept reminding me as the pile mounted. After she'd finished, she opened a cabinet containing a motley collection of neatly folded paper bags. She picked one out of the right size, placed her selection in it and handed it over. She died the following year.

What do you think makes a good scientist? In 1936, the statistician Ronald Fisher published a paper showing that Mendel's genetic results were closer to his predicted ratios than could be reasonably expected by chance. Fisher's paper spawned a series of responses, from "it doesn't matter because Mendel was right anyway" to Arthur Koestler writing that "Mendel's statistics in that classic paper were faked". Koestler wrote this in his 1971 bestseller *The Case of the Midwife Toad*, which was aimed at exonerating Paul Kammerer, an early twentieth-century proponent of the inheritance of acquired characteristics. Kammerer had been exposed as a fraud after ink had been found injected into one of his key specimens that had been purported to demonstrate inheritance of acquired pigmentation. However, Koestler argued that Kammerer had been unfairly accused by a vindictive Mendelian establishment: the ink was most likely injected by a jealous colleague. Koestler knew what it was like to feel unfairly attacked by the establishment: a leading scientific authority, Peter Medawar, had written a brutal review of Koestler's previous book *The Act of Creation*.

So who was the fraud, Kammerer or Mendel? After Mendel published his paper on peas, he was heavily criticised by botanist Carl Nägeli, who encouraged him to repeat his crossing experiments with hawkweeds. Mendel bred hawkweeds for five years but failed to replicate the pea results and became disheartened. We now know that hawkweeds are exceptional in reproducing by apomixis. By contrast, Kammerer reported in his book *The Inheritance of Acquired Characteristics* that he found his ideas confirmed wherever he looked. Disbelieving critics were blinkered, even though others failed to replicate his findings.

A hallmark of a good scientist is how they respond to criticism — are they prepared to question their own ideas and findings, or do they become defensive and attack their critics? Given Mendel's response to Nägeli's criticism, I doubt that he consciously manipulated his results. He may have been selective about which results he presented, just as scientists commonly publish their best image or exclude data that they consider unreliable, but that is very far from Koestler's accusation of fakery. Kammerer's attitude, by contrast, betrays one of a fraudulent or self-deluded scientist. Koestler's defence may have more to do with his own anger at the scientific establishment for its criticism of him than his having a real case to argue. There are many ingredients that go into making a good scientist, but being self-critical and taking the criticism of others on board are surely important ones.

What do you think are the problems science as a whole is facing today?
On the morning of 9th November 2016 I was due to give a talk at University College London. I'd just heard that Donald Trump had been elected US President. How could so many people have voted for a blatant liar? I'm usually nervous about giving talks, but in this case giving a presentation to a scientific audience was the best therapy that I could have had. I was surrounded by people who evaluated ideas based on evidence and logical consistency, not rants and hearsay. I felt incredibly privileged to belong to a community insulated from the post-truth era.

Science, particularly the peer-review system, is often criticised for its lack of transparency. Surely there is a better way, modelled on social media, with everyone having access to who says what. That assumes, however, that scientists don't take criticism personally. Early on in my career I was discussing a scientist's work with him at a conference. He began to suspect that I had reviewed his paper, which had been recently rejected from *EMBO Journal*. When he asked me outright whether I was a reviewer, I saw no harm in admitting that I was and explaining the reasons for my decision. He was still on my case three hours later.

We invest so much time, effort and emotion into producing a paper that it is understandable that we take it personally if it is rejected or criticised. I get a Pavlovian wrench in my stomach when I see an e-mail from a journal in my inbox, informing me of its decision. A day or so later, I am better placed to understand the reasoning behind the comments, whether it is a lack of clarity on my part or a flaw in the work. The paper invariably ends up improved.

The impact of rejection causes some scientists to blame the system: there are vindictive ignoramuses out there who are hiding behind the blanket of anonymity. Take away the blanket and all will be solved. I doubt it. Worse, it may engender dishonesty for fear of offending influential peers or provide an incentive to flatter them. The more funding that depends on peer review, the more acute the problem is likely to be. I am not saying that the current system is perfect or cannot be improved. Nor am I saying that all reviewers are completely fair. I'm saying that, for all its imperfections, science is a jewel that we should treasure, and we need to tread very carefully when trying to introduce improvements, lest we inadvertently cause untold damage.

DECLARATION OF INTERESTS

The author declares no competing interests.

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Active vision gates ocular dominance plasticity in human adults

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Primary visual cortex (V1) retains a form of plasticity in adult humans: a brief period of monocular deprivation induces an enhanced response to the deprived eye, which can stabilize into a consolidated plastic change[1,2] despite unaltered thalamic input[3]. This form of homeostatic plasticity in adults is thought to act through neuronal competition between the representations of the two eyes, which are still separate in primary visual cortex[4,5]. During monocular occlusion, neurons of the deprived eye are thought to increase response gain given the absence of visual input, leading to the post-deprivation enhancement. If the decrease of reliability of the monocular response is crucial to establish homeostatic plasticity, this could be induced in several different ways. There is increasing evidence that V1 processing is affected by voluntary action, allowing it to take into account the visual effects of self-motion[6], important for efficient active vision[7]. Here we asked whether ocular dominance homeostatic plasticity could be elicited without degrading the quality of monocular visual images but simply by altering their role in visuomotor control by introducing a visual delay in one eye while participants actively performed a visuomotor task; this causes a discrepancy between what the subject sees and what he/she expects to see. Our results show that homeostatic plasticity is gated by the consistency between the monocular visual inputs and a person's actions, suggesting that action not only shapes visual processing but may also be essential for plasticity in adults.

We used a purpose-built altered-reality system that projected the outside world onto two independent monocular screens, with the image for