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EDITORIAL

A Recipe for Success in Science: The Hamming Prescription

In academic settings casual conservations often turn to the subject of how can the committed scientist do significant research. Is there a recipe for success in science? Will all of one's work, carefully done over a lifetime, disappear unremembered and unsung even by colleagues in the same discipline? What distinguishes those who achieve the highest peer recognition from their faceless compatriots in science? How does a researcher at the start of a career choose an area of work and find problems whose solutions are of some importance? Is it wise to set one's sights on the 'grand challenge' problems, to the exclusion of all else or is such overweaning ambition likely to lead a researcher into the quixotic exercise of tilting at windmills? In a recent conversation, with a deceptively laid back but thoughtful colleague, my attention was drawn to a talk given by Richard Hamming over two decades ago. The talk in its entirety, including the discussion that followed, is available on the worldwide web with an annotation by J. F. Kaiser. Entitled *You and Your Research*, Hamming's talk forms part of a Bell Communications Research Colloquium Series and was delivered on 7 March 1986. His prescriptions are listed as '*Ten Simple Rules for Doing Your Best Research, According to Hamming*' in a recent editorial in the journal, *PLoS Computational Biology* (Erren, T. C. *et al.*, 2007, **3**, e213). Richard Hamming (1915–1998) was a mathematician and computer scientist (at a time when the discipline was in its infancy), who did much of his work at Bell Labs in the period 1946–76. This was the time when the electronics revolution took place and the age of information was born. Hamming began his career at Los Alamos, where he had a ringside view of the Manhattan project. In his own words, he says that he was 'brought in to run the computing machines that other people had got going, so those scientists and physicists could get back to business. I saw I was a stooge. I saw that although physically I was the same, they were different. And to put the thing bluntly, I was envious . . . I saw Feynmann up close. I saw Fermi and Teller. I saw Oppenheimer. I saw Hans Bethe: he was my boss. I saw quite a few very capable people. I became very interested in the difference between those who do and those who might have done'. Hamming entered Bell Labs in 1946 to become an important contributor to the scientific revolution that would take shape in this unique institution.

Bell Laboratories has been in the news recently as the parent company Alcatel-Lucent has begun the process of winding down basic research, in an institution that was once the undisputed leader of fundamental work in an industrial environment. The Bell Labs, whose ambience Hamming describes, has passed into history. But for a time it must have been science's equivalent of Camelot. Claude Shannon (1961–2001) wrote the paper, which marks the birth of information theory in the in-house journal *Bell System Tech.* in 1948. William Shockley, John Bardeen and Walter Brattain did the work on the transistor in 1946–47 and by 1950, semiconductors were beginning to be understood. John Tukey (1915–2000) the statistician, information scientist and generalist, whom Shannon credited as the coiner of the term 'bit' (binary digit) was there. It was these men whom Hamming observed and whom he describes in his remarkable lecture. He shared an office with Shannon who worked at that time on information theory. Hamming notes that he did his own famous work on coding theory at the same time. How did this happen? As Hamming says: 'it was in the atmosphere'. An obituary of Shannon notes that he 'must rank near the top of the list of major figures of twentieth century science, although his name is relatively unknown to the general public. His influence on everyday life which is already tremendous can only increase with the passage of time' (Colderbank, R. and Sloane, N. J. A., *Nature*, 2001, **410**, 768). In Hamming's assessment 'Shannon's scientific career' (and here he refers to extraordinarily creative activity) ended 'when he left Bell Labs'. Hamming recognizes that there can be no ideal environment for doing science. Managements and systems can often appear to be obstructive and inhibitory. His advice is to learn to subvert the system to serve your own scientific goals. Many scientists in India would do well to heed his words: 'If you choose to assert your ego in any number of ways . . . you pay a small price throughout the whole of your professional career. And this over a whole lifetime adds up to an enormous amount of needless trouble . . . By realizing you have to use the system and studying how to get the system to do your work, you learn how to adapt the system to your desires. . . . Many a second rate fellow gets caught up in some little twitting of the system and carries it through to warfare. He expends his energy in a foolish project. Now you are going to tell me that somebody has to change the system. I agree; somebody has to. Which do you want to be? The person who changes the system or the person who does first class science? . . . Be clear when you fight the system and struggle with it, what you are doing, how far to go out of amusement and how much to waste your effort fighting the system. My advice is to let somebody else do it and you get on with becoming a first class scientist. Very few . . . have the ability to both reform the system and become a first-class scientist'. Hamming's advice is especially relevant in surroundings where the environment and management structure are far from ideal for the practice of science. It is sometimes too easy to come up with alibis for non-performance, citing a long list of systemic hurdles. Hamming has sage counsel: 'Don't try and kid yourself. You can tell other people all the alibis you want. I don't mind. But to yourself try to be honest'.

Hamming's entertaining talk has been summarized into a set of ten rules by Erren *et al.* in 2007. Many are self evident, while a few are deserving of serious thought. Like others before him, Hamming endorses the 'prepared mind'; a phrase traced to Pasteur's famous saying that 'fortune favours the prepared mind'. Luck is an important element in success, but opportunities must be quickly recognized by an alert and searching mind. Like Edison before him, Hamming advocates hard and effective work. Genius, as the oft quoted adage goes, is after all 'ninety nine per cent perspiration and one per cent inspiration'. I have noticed that many aspiring researchers do not realize how much effort is expended by their more successful counterparts. Productivity requires very hard work and total committment. The image of casual geniuses who solve problems by sudden flights of imagination is romantic, but misleading. Deep insights occur only to those who think deeply and to those who are obsessed with the problems they are tackling. Hamming quotes Newton: 'If others would think as hard as I did they would get similar results'. He goes on to emphasizing the importance of hard work, in language that is characteristically his own: 'Knowledge and productivity are like compound interest. Given two people with exactly the same ability, the one person who manages day in and day out to get in one more hour of thinking will be tremendously more productive over a lifetime'. Hamming acknowledges that age is important, although youth is not an asset in many fields. He advises aspiring researchers to be self-confident ('Yes I would like to do something significant') and to be courageous. Self-confidence and courage in tandem can be a powerful combination. If hard work and intelligence are added the recipe for success is nearly complete.

There are two pieces of advice that Hamming gives, which merit special emphasis. He argues that blaming 'working conditions' for poor performance may be a common, but inappropriate excuse: 'What most people think are the best working conditions, are not. Very clearly they are not because people are often most productive when working conditions are bad. One of the better times of the Cambridge Physical Laboratories was when they had practically shacks – they did some of the best physics ever'. Hamming strikes a chord here, to which most older readers will respond. The constant refrain that we hear of 'poor conditions for research' maybe an excuse that is wearing thin. He is a strong advocate for transforming 'a defect into an asset' by arguing: 'It is a poor workman who blames his tools – the good man gets on with the job, given what he's got, and gets the best answer he can'. Of all Hamming's pieces of advice I liked his 'open door' the best: 'Another trait, it took me a while to notice. I noticed the following facts about people who work with the door open or the door closed. I notice that if you have the door to your office closed, you get more work done today and tomorrow, and you are more productive than most. But 10 years later somehow you don't quite know what problems are worth working on; all the hard work you do is sort of tangential in importance. He who works with the door open gets all kinds of interruptions, but occasionally gets clues as to what the world is and what might be important. Now I cannot prove the cause and effect sequence because you might say, "The closed door is symbolic of a closed mind". I don't know. But I can say there is a pretty good correlation between those who work with the doors open and those who ultimately do important things, although people who work with doors closed often work harder'. Hamming's 'open doors' principle seems to operate in some places at some times, with extraordinary science emerging as a consequence of 'group creativity'. The Bell Laboratories of the 1940s and 1950s and Cambridge for physics in the 1920s and molecular biology in the 1950s are often cited as examples (Erren, T. C., *Medical Hypotheses*, 2008, **70**, 473–477).

Hamming observed his subjects closely. He notes perceptively that 'great scientists tolerate ambiguity well. They believe the theory enough to go ahead; they doubt it enough to notice the errors and faults so they can step forward and create the new replacement theory. If you believe too much you'll never notice the flaws; if you doubt too much you won't get started. It requires a lovely balance'. But Hamming reserves some of his most penetrating insights for the immortals. 'When you are famous it is hard to work on small problems. This is what did Shannon in. After information theory what do you do for an encore?' He argues that 'when you get early recognition it seems to sterilize you'. In surroundings, where the bar for success is placed rather low, the phenomenon of early over-recognition may serve to inhibit rather than promote good science.

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