# Oliver Smithies

# Science brick by brick

*The Nobel Prize in Physiology or Medicine 2007 was won by Mario R. Capecchi, Martin J. Evans and Oliver Smithies for discoveries that led to the development of knockout mice.*

**PROFILE**

- Excellence Professor of Pathology and Laboratory Medicine. University of North Carolina at Chapel Hill
- Oliver and fraternal twin, Roger, were born prematurely on 23 June 1925, in England.
- An early childhood infection left him with a mitral valve murmur, and doctors forbade him playing sports until his mid-teens.
- Was inspired to become a scientist by a comic strip about an inventor
- Studied for undergraduate degree and PhD at the University of Oxford
- Can make useful devices from 'junk' – oddments in his graduate lab would be labelled NBGBOKFO: No Bloody Good, But Okay For Oliver.
- Invented gel electrophoresis in 1955
- Moved to University of North Carolina at Chapel Hill, with wife Nobuyo Maeda, in 1988

**How important is an interdisciplinary approach in addressing urgent scientific questions, and how can we foster such collaborations?**

I don't believe interdisciplinary approaches need further fostering. There has been a good deal of such effort already, perhaps even too much in the United States. Emphasizing research proposals that have an interdisciplinary approach leaves less money for more individual projects that tend to be better at generating completely new ideas and so are very important.

Of course, many scientific questions of the day are broad, and therefore need to be addressed from multiple angles. But this should not be forced onto scientists by funding agencies, although it is an important mission for funding agencies to facilitate researchers from different fields talking to each other. Because that's what science is all about: building and communicating knowledge. You may have a beautiful experiment in your lab notebook, or in your head, but it isn't science until you make it available to others so that they can build on it.

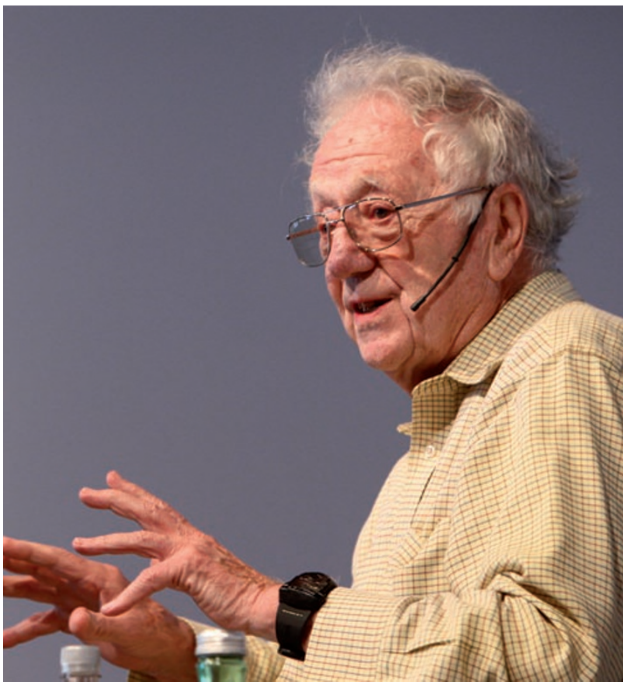
**Bell Labs and other corporate research sites, which led to many Nobel prizes, are on the decline or have closed. Is corporate, basic research critically needed, or is research in academia sufficient?**

I think all people in the science community who observed what happened to Bell Labs feel regret. I think it will be a shame and real loss if the corporate community fails to see the value of having basic research in their own labs. Very often, basic research will interact with more practical research in quite surprising and beneficial ways when the two are carried out in close proximity.

There is still quite a lot of one type of corporate research going on in the biological sector in the United States, because historically it was relatively easy to obtain venture capital in this field. So, if scientists had a good idea for a new drug or a new product, quite a number succeeded in founding their own nascent pharmaceutical companies. Very likely, this type of research will eventually thrive again despite the current lack of investment capital.

**How can the public be convinced of the importance of fundamental research with no applications in sight?**

The Hubble telescope is a good example of a project that has helped people to see what basic research is about. The beautiful images it produces don't have any immediate application for most of us, but from them we learn what the Universe looks like and how it works. Such endeavours satisfy the curiosity that is part of human nature.



FLEMMING, C./LINDAU LAUREATE MEETINGS

We humans have been able to develop because we have learned to communicate what our predecessors have found out about the world, and use this knowledge as the basis of new ideas. But we shouldn't forget that people in the Stone Age did not only make tools, they painted on cave walls for heritage.

**Many people consider the peer-review system broken. Do you share their view, and do you have a solution?**

In general, I think that scientific reviews are fair. Certainly most scientists try hard to be fair when they review. Nevertheless, sometimes personal views intrude, but that's just human nature. We try ourselves and teach our students to look at things fairly whether the result is in our interest or not.

What bothers me, though, is that today so much effort and so much time is spent by scientists trying to have their work approved. Researchers get together for dinner and instead of discussing science, they talk about funding. If we were able to take away some of the economic pressures that most scientists experience today, it would probably improve the output of many.

**You must have experienced a lull at some point in your research career. What kept you going?**

The usual suspect for a lull in a researcher's life is boredom; what you are doing no longer seems very interesting. And the way out of this situation is simple in principle but not always in practice: think of something new to do. Just before I began my work on gene targeting, I was getting increasingly bored with the work I was doing, and it was not going very well either. I needed some new inspiration.

Eventually the trigger came to me in the form of a paper published in 1982 by Mitchell Goldfarb *et al*. It described a complex gene-rescue procedure, which I realized could be used to test whether it is possible to modify a gene by homologous recombination. In that moment I had regained my excitement for science. It took three years to make the experiment work, which 20 years later led to my receiving a share in a Nobel prize.