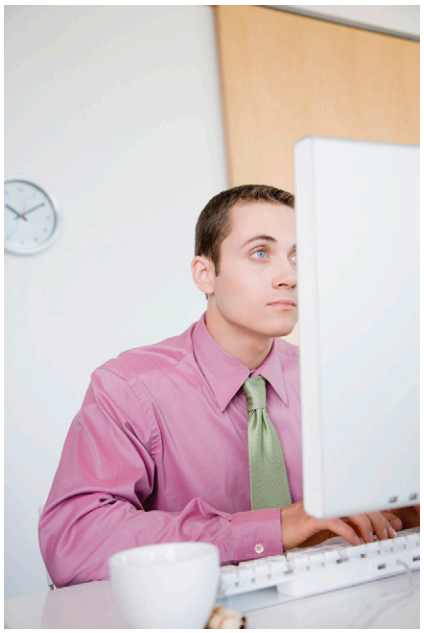


Online: A new database may soon contain details of all known species. (Picture: Photos.com)

(EOL), has now become available online. The project, coordinated by a secretariat at the Smithsonian Institute in Washington DC, plans to create pages containing the estimated 1.8 million known species of organism on the planet, when completed.

The plan is to include data on distribution, behaviours and endangered status, to further help researchers. The mass of data is being drawn from a variety of sources, including several existing specialist databases such as AmphibiaWeb and Fishbase.

The team hope the project will be completed by 2017, and will bring together a vast amount of information about wildlife gathered over the past 250 years. They hope that the initiative will provide a 'macroscope' — a microscope in reverse — which will allow users to discern large-scale biological patterns and provide information such as what insects could be used to pollinate plants in areas where honeybees are threatened.

Other potential uses could be to trace the relationship between changes in wildlife populations and climate, assisting work to conserve species already known and providing a reference source to help identify new plants and animals that have not yet been described.

Sandra Knapp, a plant taxonomist at the Natural History Museum in London, who contributed the pages on the tomato and the potato, said the encyclopaedia would be an exciting and valuable tool, which would illustrate the bigger picture and allow a conversation between scientists and the public.

Another stated aim is to raise awareness of biodiversity when so many species are threatened. On every page there is information provided by the World Conservation Union on the status of the species, showing if it is threatened, endangered or extinct, says James Edwards, executive director of the EOL. "We think it is important to serve information on organisms that are doing OK, but also those that have been recently extinct."

The EOL's creators also aim to get information online as soon as possible when new species are identified. The project will solicit the help of users to submit photographs and information for assessment by the authentication team.

The team hope the project will bring together a vast amount of information about wildlife gathered over the past 250 years

The idea of such a catalogue has been around for some time, but this present project can be traced back to an influential pleas by Harvard University biologist E.O. Wilson. In 2006, Wilson wrote a letter to the McArthur Foundation in Chicago, outlining his idea, which helped him secure preliminary funding for the project.

"If someone were to sit down and start writing from scratch, an encyclopaedia of life, it would take them about 100 years to complete. But we think we'll be able to do it in one-tenth of that time," says Edwards.

While Victorian taxidermy collections remain a unique, if vulnerable, resource and insight into the flora and fauna at that time, the EOL promises a vastly enhanced archive of living forms.

Q & A

Nicholas Swindale

Nicholas Swindale is a Professor of Ophthalmology and Visual Sciences at the University of British Columbia. He completed his PhD in invertebrate neurobiology with Paul Benjamin at Sussex University in 1976, then went on to do post-doctoral research on the visual cortex with Colin Blakemore and Horace Barlow in the Physiological Laboratory at Cambridge University. He uses experimental and theoretical approaches to understand the development and organisation of feature maps in visual cortex.

What turned you on to biology in the first place? As a child I wanted to understand how everything worked, but I wasn't primarily interested in biology. When I was about 10 years old I wanted to be a chemist. Then I got interested in radios and digital electronic circuits. This was before the era of integrated circuits and I had fun making logic devices out of transistors and capacitors and resistors, including a thing that flouted the logical aspect and behaved randomly. At the same time I got interested in physics and I went to Cambridge University with the idea of doing theoretical physics and cosmology. In my first year, I had to choose four courses and I decided to include physiology in addition to the standard courses in maths, physics and crystallography. My very first physiology tutorial was on the ionic basis of the action potential and I was fascinated by the resemblance to the digital electronic circuits I had played with. Not long after, I decided, in a moment of inspiration in my college library, that it might be more interesting and just as profound to look down the telescope from the wrong end and to study the thing that was looking up it. I was able to switch the main direction of my course work over to biological subjects (though I never studied biology) while still doing a modest amount of physics. An amazing amount of what I learnt at Cambridge has turned out to be useful, although I found doing exams very frustrating because I could not see much connection between the ability to write answers to questions on paper and what is required to do good research.

How did you get started on your PhD research? In my final year at Cambridge (1972) I read about Eric Kandel's work on *Aplysia* and also an article in *Scientific American* by A.O.D. Willows on command neurons in *Tritonia*. It seemed to me that mollusc brains should be simple enough to understand, and one could then move on to more complex ones. So I decided I wanted to work on invertebrates. However, I got a lower second in my finals, which was not good enough to get into research. After a period in the wilderness applying unsuccessfully for jobs (which in retrospect I am glad I never got) I was very lucky to be rescued by Paul Benjamin in the Ethology and Neurophysiology Group at Sussex University. He took me on as a research assistant — with much better pay than I would have got as a graduate student! — and arranged for me to register as a part-time PhD student. Paul was a wonderful supervisor and Sussex was refreshingly egalitarian — I mixed on equal terms with the faculty and exams were no longer a criterion for success. There were very good people around from whom I learnt a lot — Mike Land, Tom Collett and Ian Russell had offices and labs just down the corridor; John Maynard Smith was somewhere on the floor above and other colourful characters included Christopher Longuet-Higgins and Stuart Sutherland in the next-door department of experimental psychology.

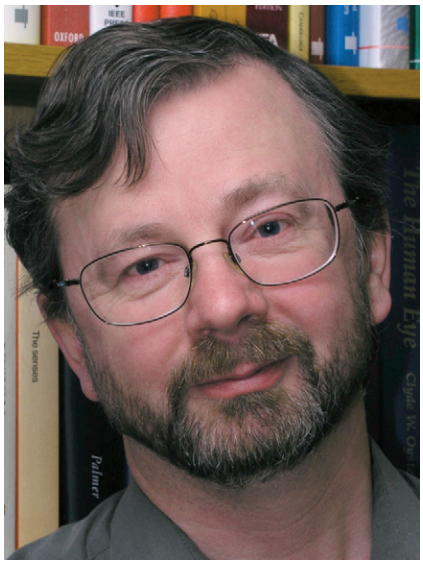
And after that? I came to the conclusion that it might be quite difficult to understand how mollusc and other invertebrate brains work, and also that that knowledge might not necessarily tell us much about how the human brain works. The cerebral cortex, although vastly larger and presumably more complex than any invertebrate brain, has a very uniform structure and the idea, which is still with me, that a full understanding of a small part of it might generalize in a powerful way to all of it seemed very appealing. I was also very lucky to be offered, shortly before I finished my PhD, a post-doc position with Colin Blakemore back in the Physiology Department at Cambridge. At that time Cambridge was a hotbed of vision research. I arrived there knowing almost nothing about vision and people kept asking me what I wanted to do — I had very little idea and it was quite embarrassing. But Colin very tolerantly let me dither around for what now seems like a very long time before I started producing any papers.

Do you have a favourite paper? It would have to be 'A quantitative description of membrane current and its application to conduction and excitation in nerve' (J. Physiol. *117*, 500–544, 1952) by Hodgkin and Huxley— the fourth of the series of papers in which they proposed their mathematical description of the action potential. It is an example of mathematical modelling in biology at its best. Not only did they devise the model, but they did the equally innovative experiments that provided the data on which the model is based; and their hypothetical gating particles have turned out to have their counterparts in reality — an amazing achievement.

What is the best advice you've been given? My college tutor at Cambridge, Denis Haydon, advised me that I would be better off studying maths and physics, even though I was planning to go into biology because it is easier to learn those hard areas when you are young, whereas biological details can be learnt at any time. I am not sure how well this philosophy translates to those areas of biology where there is now a great deal of specialised knowledge which is not at all trivial to learn, but I would still advise anyone planning a career in systems neuroscience to do an undergraduate degree with plenty of maths and physics in it. The reason for that is that physicists understand how to make and interpret measurements, and to make models — at least they have a longer history of doing this successfully than the other branches of science — and those things are fundamental.

What is your favourite/least favourite conference? The Society for Neuroscience, in both categories. It is wonderful because it is so big and comprehensive, but appalling at the same time because its size (25,000 or more attendees) makes it impossible to more than skim the surface of what is there, or to spend more than a brief period talking to colleagues whom you may not have seen for several years.

Do you have a scientific hero? I have many — a brief list would include physicists like Einstein, Bohr, Dirac and Schrödinger, mathematicians like von Neumann and Turing, biologists like



Darwin, Crick and Watson. Hodgkin, Huxley and Cajal would have to be at the top of my list of neuroscience heroes. More local heroes would include Hubel and Wiesel, who laid the foundation for my own field of visual cortex research. If I had to pick one of these, it would be Darwin though.

What ethical issues do you see being raised by recent trends in neuroscience research? A major issue, with difficult consequences for society, has to do with understanding population variations in human behaviour and the implications of the fact that these often have associated genetic and measurable neurobiological correlates. Correlates are surfacing because of cheaper genetic testing and improved imaging technologies for studying the brain. Structural and neurochemical correlations are to be expected because *all* behaviour has *some* kind of structural and neurochemical basis in the brain. Likewise, although structural differences can be the result of behaviours as well as causes of them, genes have such a pervasive role that genetic variations seem very likely to underlie many behavioural and structural variations as well. So the finding, to take a pertinent example with ethical implications, of possible genetic differences plus subtle differences in the brain neurochemistry of children with the set of behaviours defined by psychiatrists as attention-deficit hyperactivity disorder (ADHD) confers no extra status on that condition in my view — differences

are to be expected, and it should remain a socially defined condition. Probably many behavioural variations, major and minor (sexual preferences, IQ, preference for Coke versus Pepsi, maybe even being a psychiatrist) will turn out to have correlates in brain structure and neurochemistry, and sometimes a genetic basis as well. This will not mean that being a psychiatrist (for example) is something that should be treated. But I am hopeful that the list will become long (and absurd) enough that the surprise factor in finding that human behaviour has structural and genetic correlates will go away.

Any comments on the increasing interest in computational biology?
Many biologists and neuroscientists feel that mathematics and theory have little to offer and there is a sense in which they are right — there will never be a 'theory of everything' in biology as there may be in physics, or even a theory of reasonably large bits of it, because that would require predicting the directions taken by evolution which seems impossible given its accidental course. So predicting the kinds of solution the brain might have come up with in the face of specific computational problems is fraught with difficulty and empirical investigation is usually the best option. However, there is still plenty of room for computational approaches. The best example I can give is that you may have what you think is a complete reductionist description of the behaviours of the components of a system, but it may be beyond your ability at that point to account for its behaviour when all the components are put together. You will have to resort to a computational model in all probability: if the model works, the chances are your reductionist description is correct; if not, you may have to go back to the lab to find out what you have missed — and the model will likely help suggest what to look for. The Hodgkin and Huxley model is a perfect example of that — it was necessary to show that the empirical description of the ionic events underlying the action potential was sufficient to account for its shape and mathematical modelling was the only way to be sure that it was.

Department of Ophthalmology and Visual Sciences, University of British Columbia, 2550 Willow St., Vancouver, V5Z 3N9, British Columbia, Canada.
E-mail: swindale@interchange.ubc.ca

Essay

The foundations of molecular biology: A 50th anniversary

John Cairns

Science tends to advance by small redundant steps. But sometimes it suddenly enjoys a giant leap forward — heliocentric Galileo, Newton and gravity, Lavoisier and atoms, Maxwell and electromagnetism, Einstein's relativity, and so on. Unusually, the giant leap that occurred in the biological sciences in the middle of the last century seems in retrospect to have had a strangely inevitable quality about it. Perhaps that was because physics, which had played such a large part in ending World War II, was being purged by the move of physicists into the innocent pursuit of biology, and they were bringing with them all those useful isotopes plus a happy ignorance of, even distaste for, classical biochemistry and genetics. If Newton had not existed, his analysis of the forces of physics might have been delayed for another century. If Watson, Crick and the X-ray crystallographers of King's College London had not published the structure of DNA when they did, Pauling would surely have worked it out correctly within a few more months (though he probably would have presented it with less bravura).

These matters have been discussed by some of the protagonists and by the historians of science, but most practising scientists these days are too hard pressed to be much interested in history. (I remember hunting through textbooks of physics some years ago to find out who first estimated Avogadro's number and being surprised to find that, although the actual number was given, publishers and authors had apparently decided that few students would want to know who worked it out or how they did it.) At this time, another history of the origins of the molecular biological revolution would hardly interest anyone, especially as an entire book has already been written about one of the crucial experiments. But it is now the 50th anniversary of that experiment, and so this may be the right occasion to discuss what was the mindset in the years before the coming of molecular biology and why it was so difficult to make the jump into the present way of looking at biology.

Anyone trained in the biology of the 1940s could learn the fine details of glycolysis, but not until the discovery that myosin is an ATPase was there any link between the breakdown of glucose and the real business of living. The job of the biochemist, it seemed, was to work out the pathways of intermediary metabolism and the steps of catabolism and to purify the enzymes that carried out those steps and not to spend too much time wondering how these clever enzymes were created. Enzymes were known to be proteins but it was not clear what feature enabled a protein to act as an enzyme, still less what mechanism could ensure that antibodies were shaped in exactly the right way so that they bind specifically to particular antigens. Indeed, the notion of specification of exact three-dimensional shape seemed to imply that the way proteins were created would forever be beyond human understanding.

Geneticists seem to have been less pessimistic, perhaps because theirs was a subject that rejoiced in a multitude of essentially abstract words (dominant, recessive, epistatic and so on) — the kind of words that are designed to avoid the need for further thought. The processes underlying genetics were, of course, just as obscure as those of biochemistry but somehow this did not seem as worrying, perhaps because few geneticists tried to link what they were studying to the underlying chemistry of genes and chromosomes. Indeed, one sophisticated hypothesis, announced shortly before the start of the molecular biological revolution, was that genes should not be thought of as actual physical entities.

I remember, when an undergraduate, finding these two great disciplines equally unattractive. I had to read (in German) Warburg's magisterial account of *in vitro* glycolysis by enzymes extracted from *Escherichia coli*, but the whole thing seemed boring because of its remoteness from the real world of living creatures. And genetic