Q & A

Andrew Murray

Andrew Murray is Professor of Molecular and Cellular Biology and Director of the Bauer Centre for Genomics Research at Harvard University. He grew up in Britain as an expatriate American, received his BA from Cambridge University and then moved to the US to do a PhD at Harvard before spending 13 years at the University of California, San Francisco. He has worked on the cell division cycle, with a particular emphasis on chromosome segregation in mitosis and meiosis, and has recently begun experimental studies on evolution.

What turned you on to biology in the first place? Embarrassingly, I don't know. As a child, I was a reader and tinkerer rather than a bug collector. Because I grew up in England, I had to choose between the sciences and the arts at 16, and did so with some difficulty. I then encountered an extraordinary chemistry teacher, David Powell, who quoted Shakespeare while he explained the Schrodinger equation. His influence and some intrinsic attraction to the mysteries of biology convinced me that I wanted to become a biochemist.

Do you have a favourite paper? Yes, Bruce Nicklas' 1969 paper (*J. Cell Biol.* 43, 40-50) showing that chromosomes could only be stably attached to the meiotic spindle if they were under tension. It's an example of framing a question clearly, collecting data, using them to make the simplest possible hypothesis, and then doing an elegant experiment to 'prove' (apologies to Karl Popper) it. To me, this paper is as beautiful as a great painting.

What is the best advice you've been given? As a brand new graduate student, I was sent to ask Arg Efstradiadis advice about a lunatic scheme I had cooked up to clone genes. He said "This is a very clever idea, but it will never, ever work". I ignored him and plunged into 6 months of unrelentingly failed experiments. Now, with a better sense about what makes experiments feasible, I know he was absolutely right. My advice is: think independently, challenge dogma, but get, listen to, and consider advice. Feel free to ignore it, but be able to defend why you're doing so.

If you knew what you know now earlier on, would you still pursue the same research path? My big mouth, magpie mind and unrelenting optimism would always lead me to be involved in too many things, run a lab that's too big, and work in too many areas. I would like to be able to compare this style with that of a different me, who would have been a lone scientist who did their own experiments on a single topic whose grasp I couldn't escape. I think it sad that it's much harder to pursue the monastic option than it used to be.

What has been your biggest mistake? Adhering too strictly to the NIH's holy grail of 'hypothesis driven research'. I've sent students off on some terrible pursuits of hypotheses that seemed so cute they had to be right, whereas the most interesting things I've found have been the result of surprises or "I wonder if…" experiments.

What is your favourite conference? A small one dealing with a wide range of topics. I'm disappointed that as the barriers between different biological fields erode, these meetings are paradoxically dying and being replaced by more and more specialized conferences.

Do you have a scientific hero? I wrote a piece for *Current Biology* on the subject a few years ago, and named three: Lee Hartwell, Tim Hunt and Bruce Nicklas; Lee for the majesty of his mind, Tim for his generosity, and Bruce for the elegance of his work.

Any views on the state of science publishing? I'm very unhappy that papers are less and less accessible to people outside a very narrow audience. The worst thing is the undefined abbreviations which now litter papers. For example, how many people understand this title, "Cooperation of GGAs and AP-1 in packaging MPRs at the trans-Golgi network" (*Science* 297, 1700-1703). This journal has gone further, by axing the Materials and Methods from their papers, meaning that only the tiny fraction of readers who download the Supplemental Materials can really judge the paper.

What is your greatest ambition? To learn about evolution. In the last four years, we've begun to do experiments on evolution, both to see how specific interesting traits such as sex, cross-talk and multicellularity evolve, and also to ask more general questions about how the different factors — sex, mutation rate, interactions amongst mutations — control the rate and trajectory of evolutionary adaptation.

What do you think are the big questions to be answered next in your field? Understanding how cells work. I find microbes especially impressive, because they're tiny, metabolically complex and they face wildly variable environments. How do they coordinate all the different, and often mutually incompatible reactions they perform? How do they manage to make the right response to what are likely to be thousands of different combinations of stimuli that populations encounter on time scales that are short enough that adaptation must be physiological rather than evolutionary? I think progress here will depend on two things: picking a small number of problems to use as examples and interactions between biologists and others (I'm partial to physicists).

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