Early birds: Volunteers in Malta on dawn patrol to stop illegal hunting of birds. (Photo: Grahame Madge (rspb-images.com).)

habitats directives. It is the first such report to include EU-wide data on bird populations and the second one on habitat quality.

While just over half of European bird species are listed as "secure", the report finds that one in three European bird species is now threatened, including formerly common species such as the skylark and the turtle dove, whose populations shrank by 50 and 80%, respectively, according to PECBMS figures. By contrast, many species of wintering birds, including water birds, appear to have benefited from the conservation measures resulting from the birds and habitats directives.

Further highlighting the plight of Europe's birds, Birdlife International officially launched a European Red List for birds based on IUCN methodology on June 3rd. This document is the result of a three-year project supported by the European Commission and closes a gap, as Europe-wide lists already existed for all other groups of terrestrial vertebrates.

The State of Nature report mainly paints a bleak picture of the state of Europe's habitats and biodiversity, with more than half the habitats assessed found in an "unfavourable state". Unlike the bird census, which took all species into account, the habitats analysis focused on areas and species believed to be in need of attention, so this result does not necessarily reflect a more negative actual situation than the results on bird populations. Favourable assessments were more frequent in the Alps and in the Black Sea area, while northern areas and the Atlantic coastal areas were assessed less favourably.

One chapter of the report is dedicated to the impact of the Natura 2000 network, which is the world's largest coordinated network of protected areas. Currently, it covers 18% of the EU's land area and 4% of its ocean surface. It has grown considerably in recent years mainly through the implementation of the EU directives in new member states such as Croatia, Bulgaria and Romania. The network is now considered "almost complete" on land, but the report identifies the need for further marine protected areas in EU waters. A literature review has demonstrated the value of the network for conservation of bird species, but no equivalent information is available for other species or for habitats.

Thus, as the lengthy process of testing and reviewing the EU directives gets rolling, both the Exeter study and the EEA report provide strong arguments in favour of the existing directives. As the man-made mass extinction continues, Europe's birds and other species depend on strong protective legislation for their very survival.

Michael Gross is a science writer based at Oxford. He can be contacted via his web page at www.michaelgross.co.uk

Q & A

William Earnshaw

William Earnshaw grew up in Stockbridge, Massachusetts, home of the painter Norman Rockwell (he features in the 1968 Rockwell painting "The Right To Know"). Undergraduate work at Colby College was followed by a Ph.D. at MIT and postdocs with Tony Crowther, Ron Laskey and Ulrich Laemmli. After thirteen years at the Johns Hopkins School of Medicine, he moved to Edinburgh as a Wellcome Trust Principal Research Fellow in 1996. Bill's primary goal throughout his career has been to understand how chromosomes are compacted and segregated when cells divide. The achievements of his research team and their collaborators include identification of the first kinetochore proteins, identification of the chromosomal passenger complex, design of the first synthetic human artificial chromosome and studies of the role of non-histone proteins in mitotic chromosome structure. He has been elected to EMBO, the Royal Society of Edinburgh, the Academy of Medical Sciences and the Royal Society of London.

How did you get into science? When I was in grade school, my mother, Ruth, took me to the science museum in Pittsfield, Massachusetts on Saturday mornings for the Nature Hour, a programme of scientific films and short presentations. What a brilliant way for her to get some free time and for me (and lots of other kids) to be inspired! A few years later, she announced one day that I would be attending summer camp at the local Audubon Bird Sanctuary. That was actually quite an intense learning experience and gave me a huge appreciation for the outdoors. I briefly thought that I might become an ecologist, but I soon realized that you can't do that if you are crap at math. Any statistics you see in papers from our lab is led by the students and postdocs, not me!

Another person who deserves mention is my college biochemistry teacher Douglas Mayer. He taught us that science was based on research, and that it was a living subject, not just a collection of facts. I still remember his exams, which were open book and

happened at night with no particular time limit. Thinking, and not just spouting facts, was what got you through there.

So you always knew you would do lab science? Not exactly! When I was in university, the lab courses were probably my least favourite classes. On those interminable afternoons, I never identified the unknown, dissected the right nerve or got the correct number of grams of a white flocculent precipitate. If it hadn't been for the inspiration of Doug Mayer and several other teachers, I would have never gone to MIT. But I did (after three months in the Air Force — but that is another story), and there I was introduced to science in the real world. Jon King presented me with a scientific problem where no one knew the answer, and actually no one knew how to even GET the answer. It all came alive for me then! I was hooked from the first week in Jon's lab.

Why did you pick MIT for graduate school? I was lucky to get a National Science Foundation fellowship to attend graduate school. Probably there wasn't a lot of competition from other applicants in Maine, where I went to university. This meant that I got a number of offers, including one from Yale, which had rejected me as an undergraduate. I chose MIT because I was not certain if I wanted to pursue science or art, and on the MIT faculty in the school of architecture was the inspirational photographer Minor White. I thought that if the science gig didn't work out, I would go try to work with him. I did meet with one of his disciples and was offered the chance to work with them, but in the meantime I had discovered that designing experiments gave me the same creative rush. Although I occasionally try small photographic projects, I have never regretted the choice.

What was your worst-tasting experiment? I had grown up 15 liters of phage-infected *Salmonella*, and spun the cells down for lysis and harvesting the phage. I had about five milliliters of goopy cell pellet, which I was trying to resuspend by mouth pipetting. Oops! I swallowed about two milliliters when the pipette caught an air bubble. No problem — not even a stomach ache. Not an experiment that I would want to repeat — before dinner at any rate.

Well THAT sounds like a health and safety disaster. Was that your worst? Well, there was the time I stabbed my thumb full of five or six drawn-out glass Pasteur pipettes. I am one of a select few who have had a CAT scan of their thumb! ('Softer' x-rays were supposed to see the glass. They didn't.) When the hand surgeon operated on my thumb a couple of years later to remove one bit of glass that kept hitting a nerve, his first words when I was on the table and he was about to cut were "I had a dream about this operation last night." Seriously! I could not believe it when what seemed like an hour later he pulled out a 1 cm long wisp of glass that was basically invisible even in his forceps. Hmmm… These days, of course, we take health and safety issues very seriously.

What was your most satisfying paper-writing experience? I have had a few, but writing the discussion of my *Nature* paper on the structure of DNA packaged in phage heads with Steve Harrison was really something special. We sat side-by-side at his desk and worked through what we wanted to say together. Steve is such a massive intellect, it only occurred to me later on in my career how unselfish he had been to let a lowly graduate student have an equal say in that conversation. He must have been holding himself back deliberately to let me talk. A piece of good advice for supervisors — when a student comes into your office with an exciting piece of data, instead of jumping in with "Great! You know that this means…blah, blah". Sit back and say "Great! What do you think that means?" It's hard when you want to just run with the intellectual follow-up, but restrain yourself and you will get the pleasure of watching them work it out.

Want to share any other great moments? My student Becky Bernat had been microinjecting anti-centromere antibodies into cells and saw a confusing range of phenotypes. Sometimes the chromosomes lined up on a metaphase plate and cells arrested there. Sometimes, the chromosomes never seemed to move at all after the nuclear envelope broke down. We had no idea why we were seeing these two really different things until at one group meeting my postdoc Eddie Wood asked "Does this phenotype correlate with the cell cycle?" It turns out that Becky had written down the time every colony was injected and also the time when each cell entered mitosis. This meant she could back-calculate when in the cell cycle every cell had been injected. That was the answer! Cells injected in G1 and S showed the more severe phenotype, while cells injected in G2 were able to line up their chromosomes. Great experimental documentation by Becky and great suggestion by Eddie! And of course there was the moment when Yuri Lazebnik came into my office and informed me that he had developed a cell-free system to study cell death. Now, *that* was not entirely obvious from the outset!

What is the best thing about being a scientist? It's not possible to give a single answer to this — there are too many right answers. I wouldn't change this life for anything. Let me share two.

Firstly, I enjoy meeting young scientists, sharing their enthusiasm, and watching their careers develop. In this, I am not referring to my own lab members — it goes without saying that working with them has been a privilege, and if they hadn't been a pretty special bunch, I would not be in a position to be asked to write this. I am talking here about young people who I have met, usually at meetings and on seminar visits. I won't single out names. It is a real kick to meet people who are

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in those initial stages where they are brimming with passion and excitement and are struggling to find their special individual scientific 'voice'. Then, over time when you write letters that help them to advance, you feel like in a small way you helped them to go on to open completely new fields, get recognition, become mentors for others, and (in some cases) go on to fame.

Secondly, I enjoy traveling and collecting music. One of the things I like to do when I go to meetings is to escape and try to find a good music store. This is getting harder of course, now that the world is being turned into a homogenous cloud-based bland musical consensus. But in some countries, there are still the quirky individuals and no Amazon. I have a pretty good collection of Swiss, Italian and Finnish jazz, Greek, Serbian and Portuguese guitar bands, and obscure Czech classical music. Much of my music library is not in the iTunes store, and I am always pursuing interesting sounds.

What do you see as the most serious issues in science today? Oh? Time to get serious? Let me throw a question back at you — why do most older scientists decide to hang up the pipettes and give up the life? Well, there are a myriad of reasons here. Funding issues and translational versus 'pure' basic research are definitely big factors. However, one that I have heard time and again from my friends is that they are just sick and tired of dealing with the third referee. I am convinced that the appalling quality of anonymous scientific refereeing is one of the most serious problems impeding scientific progress today. Why?

Firstly, lost time. You submit a paper when you know the story that you want to tell, you have done experiments that you think document what you want to claim, and you have agonized over writing a coherent discussion of your results. If you submit to a top journal and spend 6–12 months or more on revising the work for referees, what do you gain? *Very* occasionally the story is significantly improved, but far more often at the end of this time, a few i's have been dotted and a few t's crossed. How often do we see that the new data gotten to please the referees ends up in Supplementary Materials? What is *actually* happening here is that students and postdocs are losing precious time that they could be using to develop their next story.

Secondly, lost focus. Whose paper is it anyhow, yours or the referees'? Often it becomes impossible to tell. When referees impose their prejudices, they hijack the paper and frequently papers end up not really being about anything in particular — the focused message is lost.

And finally, the course of science gets diverted. I once had a senior editor at one of the top journals complain to me that it was difficult dealing with papers from a certain community, because the people were too nice and supportive of one another, and it was difficult to reject papers! Oh, how I wish that was true for the cell cycle/mitosis/kinetochore community! Why are the top journals filled with so much samey-samey research? (Sorry, but this is the way I see it.) This is because that is what gets accepted! If you want to see what I mean, just try to work on something totally novel. When we submitted the first description of a cell-free system for apoptosis to the same journal mentioned before, we were told: "There is nothing that can be done to render this MS suitable for publication here." Isn't that great? I mean, like *nothing*? That comment wasn't totally worthless, because I still get laughs from it all these years later, but it did hurt when the next two or three studies basically following up on what we had done all DID come out in that same journal. I was told by one of the referees of my paper that he was asked by an editor: "How do we know if this is really right?" His answer – "You don't", which actually was fair enough, but not necessarily justification to reject the paper.

You talk a good complaint. What should we do about it? I'd like to propose a serious suggestion and a more light-hearted one.

Firstly, I think that all Ph.D. students should be required to take courses on how to referee manuscripts as part of their core training, along with courses on how to read papers defensively. Such courses could aim to help the students to determine whether authors actually prove what they claim to prove, and, if not, what must reasonably be done to make their proofs scientifically correct. We need to cure referees of the notion that 'good refereeing' means thinking up a clever experiment that you might do if this was *your* study and you had six free months on your hands. Because, of course, the critical point is that the *authors* own the manuscript, and not the *referees*.

This can already start for all of us in our journal clubs. How many journal clubs that go into papers in depth end up by concluding that the work was really well conceived and executed and should have been published without further experiments? This is pretty rare in my experience. Most journal clubs seem to end with the conclusion that, unlike work from our lab – which is faultless, of course – the journal club paper needs many more experiments. My mantra is "Just because you *can* do an experiment does not mean that you *should* do it!"

Now for the more subversive suggestion. Scientists should publish all of their reviews on their web sites. I recommend creating a page called 'reviews'. This might resemble the initiatives taken by *EMBO* and *eLife* to begin to open the review process up to the scientific community. However, an interesting next step could be that people who enjoy puzzles can take the reviews and run textual analysis software on them to see what they come up with. Personally, I think that it would be great if anonymous referees could be 'outed' like this.

There would be no need for anonymous refereeing if *all* refereeing was attributed. If you criticize my paper and sign the report, I am not going to do something petty to get even with you if I also have to sign my referee report. Moving to attributed refereeing would have to be a move taken by all, but I think that it would have a significant impact on scientific productivity, as postdocs and students would lose less time pursuing experiments that ultimately end up in the dusty attic (otherwise known as Supplementary Material online).

Wellcome Trust Centre for Cell Biology, University of Edinburgh, ICB, Michael Swann Building, King's Buildings, Mayfield Road, Edinburgh EH9 3JR, Scotland, UK.
E-mail: bill.earnshaw@ed.ac.uk