The next 20 years of ecology and evolution

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This, the July issue of *TREE*, is the second of two issues commissioned to celebrate the 20th birthday of the journal. The people shown on the cover of the issue are a sample of those who will shape our science in the future. They are a non-random sample: we posted a call on two widely distributed e-circulation lists for pictures of graduate students and young post-docs working in ecology and evolution today. Several established figures accused us of ageism and others tried to slip themselves in (including a C. Darwin, who submitted the only painted portrait). In the end, we received ~375 pictures of people who looked like they had the chronological potential to be driving our field in 20 years. Not all could appear, so we left the final cut to the graphic designer. We hope the cover will at least generate some mirth in 20 years time.

But what will ecologists and evolutionary biologists be doing 20 years from now? Predicting science is for the foolhardy, but current opportunities are easier to spot. There is clearly much scope for new work in all areas of current activity: it is hard to identify an area active now that might be solved and abandoned in two decades. To the openings discussed in many of the birthday articles, we add the following more general observations.

Destruction, degradation and disasters all provide scientific opportunities. Two decades ago, ecologists actively debated the relative merits of observations versus small-scale experiments, and the need for simple models. The various approaches are still with us, but these are already being enriched by new attention to natural and unintended human-caused 'experiments' that provide insights at relevant scales. Invasive species enable us to observe species interactions in the context of climate and soil variation and *in situ* food webs. Changes in atmospheric CO_2 concentrations have provided canopy-scale context for insights gleaned from small chambers, revealing why simplistic interpretations might not apply to productivity on a rapidly changing Earth. Climate change will soon be revealing how fast species can migrate. Chernobyl makes it possible to look at the importance of genetic perturbations in real ecosystems (and whether human occupancy of an area is worse for wildlife than is radiation). Vaccination enables an analysis of the selective removal of part of a biota. And, sadly, habitat loss is already enriching our understanding of demography and genetics at low population densities.

People calling themselves systems biologists are emerging in all branches of biology, excited about the prospect of iterative dialogue between mathematical models and huge amounts of molecular and physiological data. Ecologists and evolutionary biologists have been systems biologists for at least 40 years, albeit with different data. Established systems biologists have tremendous opportunities for contributing to the study of suborganismal systems, not least because they understand that mastering complexity is not simply a matter of more data or bigger computers. Inevitably, the new systems biologists will also manage to merge new models and vast data streams in novel ways that analysts of ecological systems will surely be able to exploit. And, ultimately, suborganismal mechanistic function is of interest because of what it does at an organismal or even ecosystem level. Yet the analysis of genotype × environment interactions, something that many *TREE* readers specialise in, is experimentally demanding, and beyond the experience (and even current interest) of most molecular and cell biologists.

Biologists working at suborganismal scales continue to reveal a swathe of natural history that receives far less attention from evolutionary biologists and ecologists than is deserved. For instance, the natural history of mammalian immunology continues to be understood in ever more intricate detail. Yet there is no quantitative understanding of the population biology of interacting cell types and no predictive explanation for why the immune system is designed as it is. Much human misery is caused by immunopathology [1], yet we have no understanding of why our immune genes often harm us and, indeed, whether immune self-harm occurs in anything other than us and laboratory mice. Analogous questions apply to most areas of suborganismal physiology and cell biology.

Indeed, it is striking that the attention that ecological and evolutionary biologists pay to something seems to be inversely related to the amount of mechanistic detail known about it. Is this because we have been slow to wade into the jargon-laden arena of biomedicine? Or is it because our theories do not work well when there are lots of facts? Becoming involved in biomedicine requires a detailed understanding of the experimental techniques and the jargon involved, a genuine respect for biological reality, and an ability to sort signal from noise. For ecologists and evolutionary biologists who can do this, enormous potential exists.

A tension in some areas of organismal biology, which is reflected in several of the birthday articles, concerns the use of model organisms. Model organisms that are well understood by mechanistic biologists (the odd bacterial, yeast, fly, worm, rodent or weed species) have, in some cases, been used with great success to address issues in ecology and evolution, and there is undoubtedly considerably more mileage to be gained that way. However, model organisms are not typically studied in their ecological context (indeed, most have essentially no known ecology outside a laboratory container). By contrast, little

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Available online 24 May 2006

mechanistic detail is known about organisms that are well studied in the wild. With technological advances, it will be increasingly possible to carry out detailed mechanistic and genetic work on non-model organisms in nature, ultimately weakening the case for studying ecology and evolution in jars. For instance, it should be soon feasible to study bacterial evolution in great detail in contexts where they cause disease. And interesting genetic polymorphisms identified in mouse and human studies could be subjected to detailed ecological genetic analyses in field populations of wild animals with known genealogies. A detailed understanding of selection on such polymorphisms has to move out of the lab or away from the constraints of a human study. However, one could argue that some areas of our work could make less use of wild, non-model organisms, and work more on laboratory models. It would be good to resolve quantitatively the evolution of sex for at least one organism.

Need-driven research offers huge opportunities, not least because ecologists and particularly evolutionary biologists have been slow to fully address real world problems. For instance, had SARs persisted in the human population, would it have evolved to be more or less virulent for humans? We have no quantitatively successful explanations of virulence change for any disease. Over one million people die each year of diseases transmitted by insects but most *TREE*-reading entomologists do not work on these species. And, with a few notable exceptions, we have almost entirely left the evolution of drug resistance (one of the basic challenges of 21st-century medicine, and one of the prime examples of evolution in real time) in the hands of people who have no formal training in either evolution or population biology.

Finally, there are enormous opportunities for communicating beyond our community. Perhaps more than ever, there is a need to get our science across in an accessible, jargon-free way that society can use. Climate change and evolution are now household words, no longer viewed as hypothetical possibilities, irrelevant to ordinary lives. Society often has strong views on our science, or wants to know about it, or has no views about things that it ought to. More than ever in the history of our subject, there are important opportunities in teaching, popularising and policy.

Our subject is now so large that this monthly review journal flourishes. But a danger with large disciplines is that those in them keep busy talking only with each other. We hope that a substantial proportion of the cohort represented on the cover of this issue will consider looking outward as their careers develop. For those prepared to get out a bit, there are very real opportunities to make a difference to our discipline, to other sciences, and to society as a whole.

Reference

1 Graham, A.L. *et al*. (2005) Evolutionary causes and consequences of immunopathology. *Annu. Rev. Ecol. Evol. Syst.* 36, 337–397

doi:10.1016/j.tree.2006.05.003