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John Lawton's view from the Park 7

(Modest) advice for graduate students

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I had more or less finished drafting this essay when I realised that I may be developing IS. In its full-blown form this is extremely unpleasant, at least for everybody except the afflicted, and probably incurable. I should explain that IS is Infallibility Syndrome, a condition identified and defined by one of my colleagues at Imperial College, Felix Weinberg. Felix recently won first prize in an essay competition organised by *Physics World* for his seminal article characterising the affliction.

IS typically attacks senior people in all walks of life, including politicians (particularly prime ministers), captains of industry, and professors. Having *carte-blanche* to write essays for *Oikos*, and then discovering that people actually read them and take notice is exactly the sort of dangerous dabbling with potentially addictive activities that can lead to full-blown IS. It is but one of a range of endeavours, identified by Felix Weinberg, that favour the development of the condition. They include being paid to know (consultancy); too much grant and paper refereeing to read any of it properly, but being asked for an opinion anyway, because you are the expert; the need to make rapid decisions on everything from how to cure the university's mounting cash deficit, promotions, and firing an incompetent member of staff, to whether or not it is environmentally sound to have plastic disposable coffee cups in the departmental seminar room; lots of "important" committees; and last, but by no means least, the pressures of trying to do science (so that the post-docs do not regard you as totally past it) without actually having any time to think. IS then develops precisely because you are very busy and very senior. For both reasons nobody has the time, let alone the inclination (after all tenure may depend on not offending the boss) to tell you that you got it wrong. After a few years of never being told about your mistakes, you naturally assume you never make any, and full-blown IS develops.

Victims are usually unaware of their condition, but early symptoms include spectacular displays of unwarranted and unreasonable self-assurance, combined with dogmatic pronouncements on all manner of things. Terminal cases may even speak of themselves in the royal plural, as in Mrs (as she was then) Thatcher's memorable line: "We are a grandmother".

Greatly alarmed by the possibility that writing *Views from the Park* may contribute to my developing IS, I took comfort from the fact that I couldn't be falling victim, because I realised I might be; you cannot have real IS and know about it. But just to be on the safe side I inserted "modest" in parentheses in the title of my draft manuscript, and felt much better. There are, after all, some quite serious points I want to make and it would be a pity to abandon the article just because I happened to read Felix Weinberg's essay.

Being a graduate student is not easy in any subject, including ecology. Lousy pay and thirty-six hour days are two of the best things to look forward to. But there are several things that it would help all aspiring graduate students in ecology (and their advisers!) to know or think about. In no particular order they include the following.

Avoid re-inventing wheels. Too many students have an appallingly poor grasp of the literature, particularly anything published before they were born, and on anything other than their study organisms and related taxa. There is an awful lot of literature out there and you are going to have to devote a great deal of time to reading it. It may be more fun to devote most of your time to reading current papers and to doing experiments; it is also dangerous. Take advice from older colleagues and colleagues who work on different ecosystems and on different kinds of organisms about relevant literature. If you think you have a really new idea, pause and ask about; it is probably in something published as long ago as 1965, or even more obscurely, in the *Origin of Species*. One example will suffice. At a recent international meeting I attended, one newly graduated Ph.D told me perfectly seriously that there was very little evidence for density dependent population regulation. Such appalling ignorance is not dissimilar to believing that the world is flat, or that evolution is a myth, both beliefs that are easy to sustain if you don't actually know anything.

Do not, however, be put off by all this literature. The best way through it is not to try and read it all (an impossible task!), but to take advice, preferably from supervisors and colleagues who do not have IS, and be selective. And remember, if anybody is going to have a really good, new idea, it is you. Too much reading can kill creativity; too little leads to illusions of creativity. It is a difficult balance to strike, but you are going to have to try.

Avoid picking a species to work on because it is "interesting", cute and cuddly, or because nothing is known about it. Species-oriented studies lead to papers that start with: "Very little/nothing is known about the (some activity, say mating habits, or migration, or digestive physiology) of the (species name), particularly in (a place, say Wyoming) during (some particular time e.g. winter, or wet Tuesdays)". There may be ten million species on earth; picking one at random to work on is not interesting and is unlikely to get you a job. It *is* important to be familiar with the biology of a species or group before embarking on tests of some general theory; if the biology is wrong, so the answer to the question will be wrong. Moreover, there are clearly some species, charismatic vertebrates or beautiful plants for example, with threatened or endangered populations, where species-oriented work is justified in order to conserve them. For this tiny fraction of species that the public care about, money may be available to work on them for their own sake. But even here, keep your eye on the general principles. Study processes and general problems, not Latin binomials.

Think very hard before you set out to study niche-differences between closely related species, or co-existence, or inter-specific competition, or some combination of these in yet another pair of species or group of organisms. The literature is awash with such studies. There are still some good problems to investigate here, but if you cannot write them down on the back of a postcard, in simple, plain English, don't do it. ("Nothing is known about how species X, Y and Z coexist" is not a good reason for studying them).

There are still vast areas of ecology where very little is known, and where there are very few model field studies to get us thinking, or to guide theoretical questions. They include (still) mutualistic interactions, and (more so) the dynamics of infectious diseases in wild populations. Numerous questions at the interface of population biology and ecosystem research remain wide open, not least the role of biodiversity in key ecosystem processes. If I had my time over again, I think I would become a microbial ecologist. Microbes may be less exciting to look at than mammals, but the planet will probably continue to function without wild mammals; it is unlikely to remain habitable without many of its micro-organisms, most of which are still undescribed, and about which we know virtually nothing.

Do not travel solely on the current bandwagon of field-orientated manipulation experiments. Hypotheses can be tested very powerfully in laboratory systems (what Peter Kareiva calls "bottle experiments"), and there are many large-scale ecological processes that cannot be tested at all by manipulative experiments. But that does not mean that they are immune to rigorous investigation. Some of the most important current problems in ecology are in the interplay of local and regional processes, the determinants of geographic ranges, and so on.

Learn to love theory; but be prepared to discard even the most elegant and pleasing mathematical models, if the data say so. For example, the core-satellite hypothesis and some food-web theory have probably both served their useful lives and should quietly be retired as explanations for what happens in nature.

When you start to publish, avoid salami science, otherwise known as the minimum publishable bit. Publish substantial papers, not weekly comic strips that reveal the full story in thirty-six parts. Incidentally, supervisors, major professors and advisers do not have a God-given right to put their names on all your papers. They have to earn it. Authorship is tricky, of course, because job references are provided by the same people who want to put their names on your papers. One useful argument, in difficult situations, is that simply putting the head of a lab's name on a paper can lead to considerable embarrassment for that person, if the paper is subsequently shown to be mistaken, or wrong.

I could go on, but that will do for now. Personally I find giving advice very rewarding, and rarely find that people ignore it, or question it. We will develop some of these ideas in a later article, when we feel moved, once more, to offer pearls of wisdom. We do however, have one final thought: Take as much advice as you can about your work, but take it all with a pinch of salt, particularly when it is offered by somebody with IS. The evidence suggests that the best and most creative work in science is done by young people. Don't let us stop you!

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