The Studies C interview: Nicholas Jardine

Gregory Radick, Editor-in-Chief

For many in and around the history and philosophy of science, Nick Jardine (b. 1943) will need no introduction. For some, he is a major historian of early modern astronomy, renowned especially for his work on Kepler. Others know him principally as a philosopher of the sciences in general, interested in how questions come to seem real only in certain times and places. Running throughout his career, however, has been a distinctive ‘hpbio’ thread, to do with classification in natural history. He has treated this theme philosophically and historically but also – notably as someone skilled in the hunting of field mushrooms – scientifically. And he was, of course, a founding editor of this journal.1

Our interview took place in May 2012 in his office in Cambridge University's Department of History and Philosophy of Science. By way of beginning, I reminded him of a passage in David Hull’s Science as a Process (Chicago, 1990, p. 128) recalling the moment in 1967 when, in Hull’s words, ‘a bright young philosopher showed up on the British [systematics] scene, Nick Jardine,’ with a paper on homology in the British Journal for the Philosophy of Science that, Hull explained, updated the work of J.H. Woodger in an exciting way. What, I asked, had led to that debut at the formally demanding end of the 1960s debates on systematics?

NJ: I sort of dropped out into it. I came up [to Cambridge] as a medic; but I was rather squeamish, and switched almost immediately – within a week or so of arriving – to natural sciences. I then went botanizing in Iran, came back rather unwell and missed a chunk of a year. As a result I was allowed to spend my third year redoing Part I [of the Cambridge Natural Sciences Tripos] so as to get a decent result, and that was when I switched over to mathematics. I didn't actually know any mathematics at that time, and I didn't really know what I'd do next. I do remember going to an extremely wild party given by [the historian of science] Robert Young, then the graduate tutor at King's College, and apparently I described what he considered a wonderful research project in mathematics and taxonomy, for which he somehow found a grant for me later that summer.

I was very lucky because, as I developed the project at King's, I worked with my brother [C.J. Jardine] and Robin Sibson, who were both trained mathematicians. I ended up registered for the PhD in History and Philosophy of Science only because it was then a sort of dumping ground for things that didn't fit elsewhere. My stuff didn't fit into applied mathematics, which was then very much into engineering – there was little interest in the stability of information-processing algorithms and so on. Although I was formally supervised by Mary Hesse and Hugh Mellor, informally, on the more technical side of things, my supervisor was the mathematician Peter Swinnerton-Dyer. I'm one of a whole generation of students that look back rather guiltily at how much we owe to him.

Our group's interest in the debate about phenetic methods in classification was in the algorithms used. There were plenty of algorithms around then for doing automatic classification. We were interested in setting up a theoretical framework for analysing their properties – basic things like whether an algorithm is well-defined, because some didn't necessarily give the same result each time for the same data (it could depend on the order in which you fed it in), or whether they're stable and how you measure stability, or whether they're optimal and how you measure optimality. Basically what we did was to define methods for classification that had these various properties. And we did actually apply it to the classification of plants, among other problems.

The work was highly collaborative; but for me it brought together my recent enthusiasm for mathematics with an older interest in natural history, especially mushrooms, going back to when I was 10 or 11. As for the BJPS paper: it's hard to recapture now, but there was a whole movement to provide analyses of key concepts, and analysing homology in terms of relation-preserving mappings was just the kind of stuff that analytic philosophers fancied in those days. For such purposes, Woodger was indeed rather visionary; it's just that his command of the technicalities of logic was wobbly. There were all sorts of things that he struggled to do which are actually quite easily done. But his programme was rather splendid.

At any rate, the sort of work I was doing was very badly affected by the Lighthill Report [of 1973], which condemned huge areas of artificial intelligence in British universities as being worthless, and especially if associated with speculations about how the brain worked. It was very damaging to the funding situation, and the King's group disbanded. In my own case I couldn't look ahead to a career teaching mathematics, because, apart from the bit of mathematics that our group had made up, I had no general knowledge of mathematics at all. I suppose I could do a little bit of elementary trigonometry; but I hadn't even done A-levels in maths. In 1974 I went off to Cornell, came back and fortunately got a job here [in Cambridge HPS]. But at that stage, without a team to work

1On his collaboration with Marina Frasca-Spada in establishing and editing the journal, see the note at the beginning of the previous issue.
with, there really wasn’t any prospect of me continuing with the
work in mathematical taxonomy. 2

GR: Even so, at that point, in the mid-70s, and despite the
attention you gave to the history of homology concepts in your
BJPS paper, from the Cuvier-Geoffroy debate forward, you seem to have
been moving in a much more narrowly philosophical direction than
would characterize your later publications.

NJ: I wasn’t sure what I wanted to do when I returned to
Cambridge – maybe more theory of classification. What came to
occupy me more and more was a difficult Kepler manuscript that
Tony Grafton had launched me on, though nothing much came of
that work in print until the 80s. What I didn’t do in any way was
carry on with philosophy of biology, in my research or my teaching.
I’ve supervised or advised sixty PhD students over the years, and
only two of them, Tim Lewens and Angela Breitenbach, are in
any sense philosophers of biology. Indeed, part of the reason I got into
the history of natural history was simply that, before Jim Secord
arrived here, there were lots of students who wanted to study in that
area and there was no one to teach them, so I sort of took it on. With
the discussion group that eventually gave rise to Cultures of Natural
History [a 1996 volume that Nick coedited], the Cabinet of Natural
History, which was set up 1988, the idea was that people like
Michael Dettblech and Shelley Innes and Myles Jackson and Emma
Spary would, as it were, supervise each other, in lieu of having much
in the way of input from expert supervisors.

GR: Was it through these students that you encountered Lorenz
Oken’s Elements of Physiophilosophy (1847), and more generally
German natural history in the naturphilosophische mode, in the way you set out in opening
pages of The Scenes of Inquiry? 3

NJ: No. In the late 1980s Andrew Cunningham and I got into a fit
of enthusiasm about German Romantic matters. I can’t quite
reconstruct now how it came about… a whole wall of my house is
still full of Schelling and Fichte and von Baader and so on… One of
the things I came to realize is that a lot of what’s said about the
supposed incomprehensibility of this sort of stuff is just wrong.
There is not that much of a problem – provided you’ve got the
languages and so on – in ‘going native’. It’s coming back and
communicating meaningfully to others that’s the problem! Ian
Hacking has said something similar about reading Paracelsus. With
Oken, I got the feeling that I sometimes get when reading Heidegger
and Heideggerians – that I could keep going like that for a little bit,
and maybe even answer questions about, in Oken’s case, how many
primordial forms there are and so on – but there’s not much that I
can bring home from it. Kant, by contrast, is completely different; it’s
very alien, but you come back with lots of goodies for now.

GR: How would you characterize the philosophical agenda you
brought to Oken’s book?

NJ: I was particularly concerned with the question of what
Hacking called ‘positivity’ – of what makes questions that are
radically different from ours real for the people who posed them –
and I found it philosophically quite instructive in that way.

GR: What did you conclude?

NJ: My sort of philosophizing doesn’t yield much by way of
general conclusions, apart from repeatedly discovering that the past
is a foreign country. That said, I did have one conclusion, and even
thought about writing a book about it, though I never got around to
it, and others in the meantime have written on it. And that is that the
picture we have of the empirical sciences as coming into their first

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2 This work is summarized in Jardine and Sibson (1971) for reflections on it in the light of later, wider questioning of the status of taxonomic categories in the sciences, see Jardine (2000a).

3 Jardine (1991/2000), p. 1. Although it doesn’t spring in the same way from a work in
the history of the life sciences, Jardine’s previous philosophical book, The Fortunes of
Inquiry (1986), makes use of a range of examples from that history.

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4 Full disclosure: I commissioned this essay, as Reviews Editor for the BJHS.
also based in Cambridge, produced beautiful botanical paintings, as well as a vast dictionary of Greek botany. There was a sort of memorial volume after Raven died, in which I published an essay about his botanical work, alongside the lectures and some of Lindsell’s images.

GR: Do any of your current projects bear on the history of natural history?

NJ: Natural history’s material past comes within a heritage project that I’ve been working on at Cambridge with Lydia Wilson. Part of it has been concerned with how scientists view their own material heritage, how it might be used more productively in their teaching and so on. Another part of it aims to compare various policies and approaches to heritage in order to prepare advice for Cambridge University as a whole. It’s quite a challenging project, not least because things are changing so fast. There’s the whole culture of museums and how they’re being transformed by digitization. And one quickly realizes that the sort of standard practice that tends to govern the fate of documents in institutions has no counterpart in the world of scientific objects and instruments, where, in a place like Cambridge, lots of things survive just because some old prof has saved them from being thrown out or because they’re in a cupboard that no-one’s yet cleared, and the records are all so patchy. Until recently the vast volume of natural history things which once, in the age of taxonomy, served both as display and as teaching items, are now more or less completely orphaned. So much of it has been thrown away. But suddenly they're being furiously documented and looked at.

GR: Turning finally to Studies C: what was the motivation for starting it? And what changes did you perceive in ‘hpbio’ over the course of your editorship?

NJ: By the mid-90s the surge in interest in the life sciences was noticeable at every level, but especially in the students coming to the department to work on history of the life sciences and the growing participation in the Cabinet of Natural History, which started out as a tiny group, and by then would have packed-out seminars. More generally this focus on the life sciences and away from the so-called exact sciences has swept the field. I think it has all sorts of different causes. In Britain there’ve been large sums of Wellcome Trust money. There have also been concerns with biodiversity and climate change. On the historical side, the work has become steadily less history-of-ideas-y and more devoted to local practices and so on. More recently still there’s been much concern with ‘knowledge in transit’ (to use Jim Secord’s handy expression), with many studies focussing on the movement of natural historical specimens and knowledge between cultures. On the philosophical side, it seems to me that, of the various philosophies of the various sciences, philosophy of biology is liveliest for collaborations between philosophers and scientists. A lot of philosophy of biology is quite evidently cutting-edge. An upshot is that Studies C is more and more likely to receive submissions where it’s not always clear whether it’s philosophical or not – which is maybe a sign that this isn’t the right question to be asking.

**Selected writings by Nick Jardine:**


