Remarks at the Launch of Ross Honeywill's, *Lamarck's Evolution*

Glebebooks, Sydney

Wednesday 6 August 2008

By

John A. Schuster[1]

Ross Honeywill, Ted Steele, academic colleagues, ladies, gentlemen, including ladies and gentlemen of the media. Thank you for coming tonight. I hope to encourage you to explore this important book. I do this as a practising historian of science, interested in the dynamics of scientific change. I'm also here as a long time friend, former colleague and intellectual sparring partner of one of the book's two central characters, Ted Steele. Finally, I'm here to broadcast my admiration for Ross Honeywill's exceptional effort, and vision, in putting this book together, a remarkable piece of history of science popularisation. You all know that Ross is an innovator in his own professional patch— a guru of the study of consumer behaviour—I've read and appreciated large swathes of *Neo-Power*. But, it's Ross the historian of science I come to praise:

We academics often take a dim view of popularisations; but this is wrongheaded. The new breed of popular history of science widens awareness of science and of its history…some readers are future students of ours. If their interest is kindled—great; if we think some additional education is needed, so be it—at least these students have arrived and arrived motivated. But even more is going on with Ross. In this book Ross is the knowledgeable, humane, amateur of science, the enthused man of letters interested in awakening the wider reading public to some big moves in the world of science, and doing it, as many earlier disseminators did, through the story of a particular hero: as Huxley to Darwin; and Playfair to Hutton; Gamow to Einstein, so, perhaps Honeywill to Steele.

This book is about the trajectory of the idea, or …the dream, of the inheritance of acquired characteristics, by which we mean the passing into subsequent generations of changes acquired by individuals during their brief lifetimes—whether these changes involve form, function or behaviour. The book runs from the best early statement of the idea by Jean-Baptiste Lamarck in 1809 down to its possible resurrection and acceptance as a real mechanism in molecular genetics, due to the career long struggle of Ted Steele, an iconic and controversial Australian scientist, who is the latest in a distinguished lineage of Australian molecular immunologists, containing a few Nobel prizes and maybe more to come.

Lamarck it was who coined the term biology. And modern biology, like its sister discipline modern geology, came of age, when at the turn of the 19th century it became historical—not just meaning 'aware of long spans of time'—rather aware of, problematising, change—a dynamic of causes and effects through time. Ideas about evolution or transformation were inscribed in that space, in many variants before and after Darwin. Within that field of possibilities one variant has haunted scientific thought and popular imagination, inheritance of acquired characteristics. In the late Enlightenment, this shaping of future generations by the experience of ancestors was thought of in positive, progressive, ameliorative ways. If inheritance of acquired characteristics occurs in the wild, what might rational men achieve, by

[1] Prof. John A. Schuster <j.a.schuster@unsw.edu.au>
Program in History & Philosophy of Science
School of History and Philosophy
University of New South Wales

Unit for History & Philosophy of Science
Faculty of Science
University of Sydney

re-designing social experience, education, and institutions? The idea has had both technical scientific and wider cultural and ideological valencies. These have interacted and entangled over the past two centuries, climaxing, it now appears, in the technical scientific struggles of Ted Steele.

When we speak of Darwinism, we must distinguish three things. First, the Darwinism espoused by Charlie Darwin himself. This included inheritance of acquired characteristics as a subsidiary mechanism, and a theory of inheritance, pangenesis, which explained it. Secondly, there is the neo-Darwinian synthesis, established from the 1930s. Here classical genetics, emergent early in the 20th century met with Darwin and population studies, excluding both inheritance of acquired characteristics and Darwin's own pangenesis theory. Finally, there's the wider cultural penumbra of modern public Darwinism. Embodied, for example, in Richard Dawkins, this isn't a scientific discipline, but a species of modern, highly structured, reasonable, and convincing legitimatory ideology. It provides defence against creationism, and other cultural services. Public Darwinists oppose Steele on principle: On this ideological level they assert that the Darwinian synthesis, the science, is correct, frozen and unimpeachable. If Steele is right, much of their talk is going to have to change. But, science ain't frozen like that, and, Ted Steele is about scientific technicalities, not ideology, so let's bracket off public Darwinism tonight.

Now, strictly speaking, as Ross shows, Steele doesn't purport to falsify scientific Darwinism; rather, he bids to add a hugely significant modification to it. His core claim is that transfer of genetic information can occur from somatic cells to germ line cells—using mechanisms of RNA reverse transcription, but only alterations of genes in somatic cells in the immune system. This is a precise, technical type of inheritance of acquired characteristics. And, it does indeed bid to refute something in the scientific realm: Not natural selection, but Weismann's doctrine of the hermetic separation of germ line cells and somatic cells—the "Weisman barrier".

In the 1880s August Weismann spent more time than he should have cutting the tails off generations of laboratory mice. He observed, correctly, that the mice never passed on their shortened tails through reproduction. He, concluded, grandly, that Lamarckian inheritance of acquired characteristics is not a fact of nature. (Perhaps he should just have reflected a moment on the history of circumcision.) Weismann worked before the emergence of classical genetics. But, his claim about the barrier was later mapped onto classical genetics, becoming a fundamental principle in the neo-Darwinian synthesis. The Weismann barrier is a big scientific fact.

But what is a big scientific fact? Time for some history and philosophy of science: Large scientific facts are, in fact, rather complicated things—more complicated in modern science than ever they are in common sense and everyday life. In one of Dorothy L Sayer's stories, Lord Peter Whimsy is told *"My Lord, facts are like cows. If you look them in the face hard enough, they generally run away"*. Scientific facts have histories, they are not little mirrors held up to nature. Deep facts in modern science are elaborate tissues of theory, theory-loaded experimental hardware, and skilfully shaped and selected evidence. A fact asserts claims about some objects or processes in nature. Facts evolve, they are renegotiated, they can change relevance, and they can cease to exist, all due to the unfolding best practice dynamics of science.

Before Ted Steele others have stood on theoretical ground from which Weismann's fact looked shaky. Paul Kammerer and his brilliant biographer Arthur Koestler stood there, and so did mid 20th century advocates of cellular inheritance. But, prior to Steele, nobody has done to Weismann what displacement of big facts requires—an alternative claim has to take some hold in the expert community—new problems have to be envisioned and solved, new predictions made and adequately confirmed; plausible links made to other accepted bodies of theory and theory-loaded fact. And, as a claim begins to take hold as fact in the weave of scientific practice—it too necessarily begins to evolve—it's embedded in other arguments for other claims, perhaps embedded in instruments and techniques and in textbooks.

Historians of science call all this the "micro politics of fact–making within expert traditions". Such communities consist of humans—so their fact–making and fact–breaking is always contested and

always in flux. This—and not some mythical, universal method–is the very genius of the modern sciences. Ross's book reflects all this at every stage: in the three way dynamics amongst Lamarck, his nemesis, the brilliant and very considerably right Cuvier, and Lamarck's brilliant early devotee and intellectual heir, Étienne Geoffroy Sainte–Hillaire; or in Ross's description of Weissman's shifting views; Kammerer's dreams, rebuffs and tragic end; and above all in Steele's incredible journey within modern molecular immunology, the insider–as–outsider and outsider–as–insider.

Ross shows that Steele's early attempts to establish the inheritance of acquired characteristics at the molecular level in the immune system were controversial, and met with legitimate, as well as somewhat political and personal resistance. But, in recent years Ted's adoption of new techniques of investigation, new collaborators, as well as new tactics, have led to a quickening flow of evidence in favour of his claims. About this more in a moment.

First, we need to reflect a bit on Ross's moving account of the wider biography of Steele, the relation between the man and his science. Let's focus on the Steele Affair at the University of Wollongong, the greatest academic scandal in Australia since the Orr case two generations ago and one with much wider system resonances.

In the middle of a national debate about soft marking of fee paying students, Ted was summarily fired by Wollongong Vice Chancellor Gerard Sutton for publically reporting two such cases involving honors theses. Sutton asserted he'd fully investigated, finding no basis for Steele's claims. But, even if true, that couldn't legalise Sutton's breach of the Enterprise Agreement, and Wollongong lost the case, and the appeal, some judges having choice words for the University along the way. All this is well known and Ross tells it well. But there's more, little known, yet on the public record, fully documented at the time and well canvassed by Ross.

You see, when Ted won, he was reinstated, only to be met by a trumped up threat of disciplinary action (this abuse of industrial process is a regular feature of Wollongong's personnel 'management'). At this point, and this point only, Ted played the mother of all trump cards; the university suddenly decided to negotiate a settlement and the rest is history. The shock and awe trump card was sworn testimony and related correspondence from the outside examiner of the honors theses in question, the distinguished ANU molecular immunologist Professor Bob Blanden. Blanden's evidence proved that he had intended to fail the work, told them so and had not supplied numerical marks. The marks had come from somewhere else! Moreover, at no time had the University done the routine, real world, thing—called up Blanden, discussed the problem, gotten his view, and with large discrepancies looming, brought in a new panel of examiners. What, then, of Sutton's inquiry? Well, a reasonable person might well conclude either that the inquiry had been completely incompetent, or that it perhaps never happened. (I too was subjected at Wollongong to such a non-existent inquiry and miraculous public announcement of its magically obtained 'results'.) Vice Chancellor's have been sacked for <u>much</u> less. Perhaps the University Council should have taken an interest? They had all the evidence; but, as Bobby Zimmerman says, "they went along for the ride…" Stacked with New South Wales labor machine flunkeys and mickey mouse academic toadies, they never batted an eye, and never have since.

There are still some brilliant academics at Wollongong, and some of their humanities stars are with us tonight. May God, or destiny, protect them until the place is investigated, and reformed *"root and branch"*, as Ollie Cromwell was wont to say.

Enough! … Let's return to Steelian molecular immunology. The book shows, and I can further confirm, that Ted has landed on his feet. In recent years there's been measurable progress in hardening his claims toward "fact–hood". Ted is now located at the ANU; to be precise in the Genomic Interactions Group of the Research School of Biological Sciences at the Institute of Advanced Studies. There he works with a biostatistician, but he also works at home. He has no courses to teach, no work load model to complete, no committee meetings to attend. Getting up in the morning he can turn on his computer and explore the shifting state of his field.

He enjoys each day a panoptical vision of the changing field of contestation in which he works: it metaphorically parallels his panoramic view of the Illawarra coast, north toward Thirroul, Austinmer and beyond, from his architect designed perch at the cliff edge facing north at Sandon Point, Bulli. There, like some new fangled type of scientific day–trader, he keeps daily watch on the Tasman sea and the moment to moment global state of molecular genetics, and related fields. This facilitates interventions, invitations, fine tuning of papers, micro positioning of himself and his claims. He spends long days, even months, analysing computerised genome sequencing data, looking for signs of his model at work, and evidence for or against the few remaining conventional competitors.

In recent years Steele has published frequently in the top specialist journals, customarily taken as second only in pecking order the peak generic channels of *Nature* or *Science*…journals such as *DNA Repair* and *Molecular Immunology.* He has had remarkable speaking invitations, amongst them to a Royal Society theory conference and to a Gordon Conference. The latter are ultra elite, ultra specialised, by invitation only cutting edge occasions. Ted was in the 'hot topics' section of his.

Ted has been shrewdly courting, and significantly receiving, attention from younger members of neighbouring specialties; for example, that exotic area known as 'RNA repair" These folks are more willing to listen and take claims on board for their own uses, thus, as I said, embedding them toward fact hood. Additionally there's important, perhaps crucial, experimental work afoot. Ted is loathe to discuss it, and so neither will I.

So to conclude, let me reiterate how much I admire this book: Ross Honeywill's account of the entire 200 year story is breathtaking—even the second time I read it I was enthralled and enthused, as I almost never am by popular history of science writing. The material is well informed by history of science perspectives, and a wise use of interviews with scientific and historical experts. In particular Ross has nailed the Steele story. This is a tremendous service to Ted and reputational windfall for him. I can see now just how good popular history of science writing can be. Glebe is maybe not the galactic epicentre of earnestness and confession, but that is genuinely what I think and I confess to greatly admiring this work marked by passion, energy, detail and amazing speed and address.

So, with that, may I propose a toast to the perspicacious, efficient, humane and literate Ross Honeywill, and also to his chief two subjects, the great, politically progressive and biologically visionary Jean-Baptiste Lamarck (not with us tonight), and a great Australian man of science, in particular a great man of the great Australian specialty, molecular immunology…our living, breathing scientific guest of honour this evening, Ted Steele.