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overseas. They could converse amicably about theatre, books and politics, but Wilkins found her peremptory, off-handed and 'spiky'. For whatever reason, they failed to communicate about science, and neither trusted the other.

At 23 years of age, the immature and intellectually overconfident Watson could not handle this situation. Wilkins showed him Franklin's results, but Watson was unable to deal straightforwardly with her — he says he was actually afraid of her, although Maddox thinks that "the male fear of the female has always been absurd". Assured of his own intellectual superiority, Watson believed that Franklin's approach was too rigorous, and that she lacked the mental adventurousness to transform the diffraction photographs into a structure. In fact, her insight led closer to the truth than Crick and Watson's first uninformed guess in November 1951, or Pauling and Corey's hasty proposal in early 1953.

Maddox's book opens a window into the minds of the central players. She has spent considerable effort interviewing the protagonists and everyone around them. Less partisan than Sayre, she avoids general philosophical questions by keeping more closely to the story. Her weakness is in presenting the scientific background. The oversimplified chapter introducing the role of DNA in biology contains confusing errors, and descriptions of diffraction techniques have distracting inaccuracies. She gives no insight into the differences between the A and B forms of DNA, the process of going from X-ray-diffraction observations to a structural interpretation, or the significance of the twofold axis in DNA crystals.

These points can easily be ignored by a scientifically literate reader, and are perhaps irrelevant to the wider audience that the book will attract. For those who do not need the scientific issues explained, the book provides a thrilling commentary on the unusual characters of Franklin, Wilkins and Watson. ■

David Blow is in the Biophysics Group, Blackett Laboratory, Imperial College of Science, Technology and Medicine, London SW7 2BW, UK.

Record of a revolution

An Annotated Census of Copernicus' De revolutionibus: (Nuremberg, 1543 and Basel, 1566)
by Owen Gingerich
Brill: 2002. 404 pp. $132

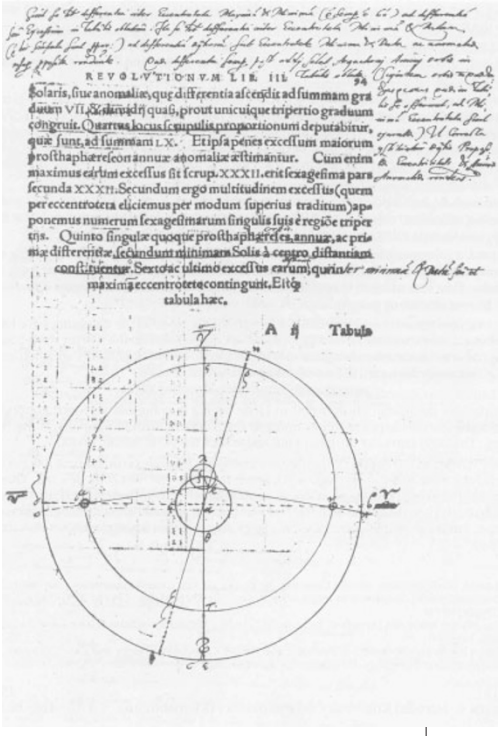
Adrian Johns

Arthur Koestler once described Copernicus' *On the Revolutions* as "the book that nobody read". Unlike landmark publications such as Darwin's *Origin of Species*, the volume that launched the astronomical revolution of the Renaissance had been "an all-time worst seller". In Koestler's view, the great miracle of that revolution was not so much that Copernicus destroyed geocentrism, but that anyone noticed.

Owen Gingerich's census of *De revolutionibus* lays Koestler's verdict to rest, but in an intriguing and unexpected way. Gingerich has spent 30 years pursuing every single known copy. His has been a monumental enterprise, without parallel in the history of science. The result shows that, contra Koestler, Copernicus' book was not particularly rare — its print-run of 400–500 was quite usual for the period — and astronomers, at least, did indeed read it. The depredations of Catholic censorship, we can now tell, were real in Italy but virtually non-existent everywhere else. Most interestingly, however, Gingerich's survey begins to reveal *how* astronomers read the book — and how their readings coalesced into a copernican consensus.

Gingerich's careful recording of marginalia — including many photographs — permits us to see the readers of *De revolutionibus* emerging as a community. Notes and calculations originating with a handful of early devotees — Wittenberg professor Erasmus Reinhold, itinerant mathematician Paul Wittich, Tübingen scholar Michael Maestlin and Rhenish astronomer Jofrancus Offusius — were read, copied and recopied by successive generations of students. Their readings thus spread across Europe, establishing canonical methods, examples and critiques. Only the kind of detective work that Gingerich has now done could possibly have revealed these patterns of inheritance and emulation, out of which copernicanism itself grew.

This census is a great achievement. By



Read revolution: annotated copies of Copernicus' *De revolutionibus* reveal his readers' responses.

bringing us as close as we'll ever get to seeing Copernicus' readers at work, struggling to understand and use his demanding text, it reveals a scientific revolution in the making. ■

Adrian Johns is in the Department of History, University of Chicago, 1126 East 59th Street, Chicago, Illinois 60637, USA.

Eco-physiology comes of age

Physiological Ecology of Vertebrates: A View from Energetics
by Brian K. McNab
Comstock: 2002. 575 pp. $75

John Speakman

D.A. NORTHCOTT/CORBIS

When left high and dry, the tree frog *Phyllomedusa sauvagei* secretes a waxy coat to reduce water loss.

Physiological ecology has been an emerging subject for the past 50 years, since its gestation in the seminal work of physiologists such as Knut Schmidt Nielsen, Per Scholander and Laurence Irving, who first looked at the physiology of animals in the wild. Brian McNab published his first paper in this area in 1963, and so has been on the scene since the field was in its infancy. He was involved in its development during the 1960s and 1970s, when it consisted of little more than than compiling stories of wonderful animals living in harsh places. He played an active part in the sometimes heated debates during the difficult 'adolescent' years, when the field was trying to define itself — exemplified by *New Directions in Ecological Physiology* (Cambridge University Press, 1988), edited by Martin Feder *et al.* — and its final emergence as a mature field during the late 1990s.

The timing of *Physiological Ecology of Vertebrates* really could not have been better. McNab has apparently been on the cusp of writing it for the past 30 years, but I can see why he has never consolidated his ideas into a final volume until now. The field has really been in too great a flux over the past decade to allow any definitive summary to be produced, and before that it would have been little more than a catalogue with no unifying thread.

This is a book about physiology in wild vertebrates that contains no molecular biology. If we pursue the analogy between the development of the field and the development of a person, then this is a field in its confident early 20s, able to take stock of the issues that dogged its teenage past. This is the time to make a defining statement about where we are, because looming on the horizon is a mid-life crisis for this field, to do with how physiological ecology will accommodate the genomics revolution. So the timing seems appropriate, and I am glad McNab didn't wait another 10 or 15 years to write this book because by then it would have been unpublishable.

The book serves as a defining statement of where physiological ecology is right now. It is encyclopedic in its coverage, and there are almost as many references from the first 20 years of McNab's career as the past 20. I think that this will turn out to be a major strength of the book because for graduate students who think that science started in 1980 it will be a revelation to find that a lot of good science was done in the 1960s and 1970s that for all intents and purposes is lost because it is not included in modern computerized literature databases. If the book serves only to open the eyes of some students to this rich data field it will have achieved something. But I think this book will achieve much more than that.

It is a book that graduate students and established scientists alike will revel in. Undergraduates, too, will like the straightforward style of most of McNab's writing. Although it may be too expensive and probably a bit too detailed to work as an undergraduate course text, as a source of additional reading, library copies will be well used. I am certain that if I take this back to my office one of my postdocs or students will borrow it within days.

I enjoyed dipping into the book to read snippets about fields with which I am much less familiar than my own. Everyone will find favourite bits about which they were previously unaware. For me it was the 'waterproof frogs' that cover themselves with a layer of lipids but then have to remain motionless so that the waterproof veneer doesn't crack. Perhaps more importantly, I never felt that the coverage of my own field was inadequate.

McNab has a reputation among ecophysiologists for taking an unconventional stance on issues in the field. During the 1980s and early 1990s he engaged in a protracted series of debates about the virtues of using phylogenetic contrasts to correct for the lack of independence in comparative studies. More recently he has been vocal on the merits and demerits of using mass-specific or non-mass-specific units to express basal metabolism. So I had expected a rather one-sided view of these issues to emerge in the book, but instead the treatment of these subjects is very well balanced — although McNab's own view is clear on most of the topics.

The book shows that physiological ecology has come of age. It contains over 3,000 references and, although it omits lots of individual studies, it still provides comprehensive overall cover. As a defining text covering 40–50 years of research it is unsurpassed and will become established as a defining reference.

Will this be the text that defines where the field goes in the coming decades, as promised in the foreword by James Brown? I actually don't think it will. The biggest thing that will happen in physiological ecology in the next 40 years is its integration with genomics, and the book doesn't touch on this issue at all. But to be honest, I don't think that matters. After all, Julian Huxley's *Evolution: The Modern Synthesis*, which came at a similar stage in the field of evolution, is still a classic. It doesn't really matter that because it was written in 1942, long before we knew the structure of DNA and the mechanisms by which the whole thing works, it didn't mention these topics. So the fact that McNab's book doesn't touch on genomics shouldn't stop it becoming a classic text in the field. ■

John Speakman is in the Department of Zoology, University of Aberdeen, Aberdeen AB24 3TZ, and the Division of Appetite and Energy Balance, Rowett Research Institute, Aberdeen AB21 9SB, UK.