

Surely Levy must have asked him why. Other travellers are not like that. Now I'll never know the answer.

Bok was clearly a man of great character but we are not told what drove him. Levy concentrates on his life more than his science. I would have liked to know more about Bok's writings, and why he wrote what he wrote, and how it influenced the advancement of astronomy. The reader is told indirectly a great deal about what Bok thought of Bok, and what Levy thought of Bok, but I would have liked to have learnt more about what other astronomers thought of Bok. □

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## Propaganda of symbiogenesis

T. Cavalier-Smith

**Concepts of Symbiogenesis: A Historical and Critical Study of the Research of Russian Botanists.** By L. N. Khakhina. Edited by L. Margulis and M. McMenamin. Translated by S. Merkl and R. Coalson. Yale University Press: 1992. Pp. 177. \$37, £22.50.

THE idea that new organisms could arise as chimaeras by the fusion of separate organisms goes back at least to the ancient Greeks. It began to be taken seriously by scientists only with the discovery by the Russian Andrie S. Famintsin (1835–1918) and the German S. Schwendener (1869) that lichens are composites of a fungus and an alga. This led Heinrich A. de Bary to adopt in 1879 the word 'symbiosis' for such relationships. Another Russian, K. S. Mereschkowsky (1851–1921), coined the term 'symbiogenesis' for the idea that symbiosis could lead to the permanent fusion of two distantly related organisms. Famintsin and Mereschkowsky both believed that symbiogenesis was an important and widespread evolutionary mechanism, as did a third Russian, Boris M. Kozo-Polyansky (1890–1957), and the American Ivan E. Wallin (1883–1969), who even thought it was the basis for all speciation in eukaryotes. The ideas of these scientists are the focus of this book.

The evolutionary importance of symbiogenesis in rare instances is now well established, with the acceptance of Mereschkowsky's theory of the symbiotic origin of chloroplasts from cyanobacteria (then called blue-green algae); of the theory of the symbiotic origin of mitochondria from bacteria, developed independently by Wallin and Kozo-Polyansky; and of the

theory of the symbiotic origin of the kingdom Chromista (cryptomonads, chlorarachniophytes, haptophytes and heterokonts — for example, brown algae, diatoms, chryomonads) — from two separate eukaryotes. The symbiotic origin of peroxisomes and glycosomes is, however, still only hypothetical.

This translation of L. N. Khakhina's historical work published in Russian in 1979 is at first sight most welcome particularly as US researchers often wrongly attribute Mereschkowsky's theory of the multiple symbiotic origin of chloroplasts to Raven and Margulis. In the past dozen years, three US researchers have independently proposed theories of the symbiotic origin of the nucleus as if they were novelties, not realizing that this erroneous idea goes back at least to Boveri in 1904, and probably well into the nineteenth century; it was espoused particularly strongly by Mereschkowsky and Kozo-Polyansky. In their lengthy introduction, the editors attribute this ignorance, overcharitably, to ignorance of Russian in the United States. But Mereschkowsky's seminal 1905 and 1910 papers were in German and his long review in 1920 was in French. All have been widely cited in English language publications.

Unfortunately, the book — especially its introduction — is riddled with propaganda and error, and the longest chapter on 'contemporary concepts' is 15 years out of date. For a start, Alexander Vacinich in his foreword badly misrepresents Mereschkowsky's views by stating that he thought that all cells are symbiotic combinations. He did not: he excluded both bacterial and fungal cells from the idea of a dual origin.

Margulis has vigorously promoted two major confusions that permeate the book. The first confuses the symbiotic origin of mitochondria (true) with the symbiotic origin of the eukaryotic cell (probably false). Khakhina's book is neither as critical nor as historically accurate as it should be. She often refers to "the origin of the cell by symbiosis" when she means either the origin of nuclei or the origin of chloroplasts.

In her introduction, Margulis states that Khakhina's main point is that "the early symbiogeneticists believed Darwinism and natural selection to be useless or irrelevant to the concept of evolutionary change". On the contrary, Khakhina emphasizes that Famintsin thought that "natural selection provided a singularly plausible and completely satisfactory explanation of the causes of the formation of adaptations", that Kozo-Polyansky consi-



Newton Wonder — one of over 2,000 apple varieties surveyed in *The Book of Apples* by Joan Morgan and Alison Richards with paintings by Elisabeth Dowle. Ebury Press in association with the Brogdale Horticultural Trust, £19.99.

dered "natural selection as the primary mechanism of the development of the organic world", while Mereschkowsky "thought it necessary to correlate it [symbiogenesis] with the teaching of Darwin. This he did with the utmost brevity." Although she states that Mereschkowsky thought earlier theories (those of Darwin, Haeckel and Naegeli) were "unsuccessful", "outmoded" and "did not agree with" symbiogenesis, she does not make his exact criticisms clear.

Margulis's second major confusion has been to muddle the pervasive role in evolution of symbiosis as a factor influencing the mutual coadaptation of symbiotic partners with the extremely rare permanent merging of such partners to form a single organism — that is, symbiogenesis in Mereschkowsky's original sense. Unfortunately, Khakhina makes the same mistake and uses the term symbiogenesis for both roles. Her claims that "symbiosis is emerging . . . in the world of prokaryotes as a major mechanism of evolution" is totally misleading. There is not a single known example of cellular endosymbiosis in prokaryotes, and no good reason to think that two different prokaryotes have ever merged together to form a single new organism.

Famintsin did not think, as we do now (and as Khakhina sometimes implies), that chloroplasts were organelles that had evolved from symbionts, but that they are symbionts: he spent years vainly trying to cultivate them, as did Wallin for mitochondria. The idea that chloroplasts and mitochondria were permanent organelles that never arose *de novo*

evolved only gradually: though some people thought this a century ago, it was established only in the 1960s.

Khakhina is loth to make historical judgements, writing that Elenkin's view that Famintsin conceived symbiogenesis long before his discovery of the dual nature of lichens, and P. Borodin's totally contradictory view that the lichen discovery led to the idea of symbiogenesis, both "seem just": clearly at least one must be wrong. A reluctance to take sides or to be critical of dogma is perhaps understandable for someone writing in the Soviet Union of the 1970s. Equally understandable is her praise of symbiogenesis as approaching "a truly dialectical understanding of the factors of organic evolution" and her nationalistic conclusion that "Russian scientists played a leading role in developing the concept of symbiogenesis, particularly in the early stages"; this exaggeration appears plausible only because the book largely ignores non-Russians, except for Margulis, whose contributions are overstated.

One day, there will be a decent history of research on the origin of cell organelles by symbiogenesis. This one, I am afraid, is too distorted by propaganda. □

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## La dolce vita?

Giovanni F. Bignami

**L'università dei Tre Tradimenti.** By Raffaele Simone. Laterza: 1993. Pp. 151. L13,000.

FULL professors in Italian universities, some of them unfondly known as *baroni*, all lust for power. Some have it but none of them really control the powers that rule over their own university and which could (if properly directed) make it work. But how does one become a full professor in Italy? Easy, just follow the few written and many unwritten rules clearly given in the chapter "How to get in". You'll learn about the laws of "primordial affiliation", of "exchange of academic gifts", of "rewarded loyalty" and more. And if you think that these are self-explanatory, beware: Italian academia has more facets than you or I can hope to fathom. Even in his courageous and brilliant pamphlet, Simone, lest he bore the reader, shies away from describing precisely how to get a chair in an Italian university. You actually get it by winning a *concorso*, a national competition judged by ministerially appointed committees where the needs of individual universities are diluted if not lost to academic bargaining at best.

Some time ago, D. Burr explained the process (*Nature* 357, 273; 1992); he used a whole page and still only covered part of the subject. He also gave up on translating the Italian word *concorso*, the heart of the professorship procedure. (It is actually best grasped from its obvious Latin origin: *cum correre*, to run together ....) That exposé was indeed necessary in the wake of a flurry of letters, obscure, I suspect, to the average *Nature* reader, from Italian academics unhappy about the outcome of the last round of *concorsi*. By necessity, most of those letters were grouped together under 'academic promotion'. To an Italian ear, however, 'promotion' sounds a totally inadequate rendering for the radical change in official and social status that comes with winning a *concorso*.

It will be a challenging, and very useful, task to translate into English this book by Simone, himself a linguist. At the mo-

Dei. (Simone, however, makes a point in his opening quotation of Karl Kraus, that "it is not always appropriate to name names ....") J. LaPalombara, a student of things Italian and a personal friend of a former prime minister (also, of course, a full professor), calls the system one "of strict cooptation" (*Democracy Italian Style*, Yale University Press; 1987).

The professors' *nòstos* is bad for the students, towards whom the most important of the three treasons is directed. It may, by contrast, be negligible, considering Simone's postulate on indifference towards students: "no one cares about students in Italian universities". A concept that does no justice to many a hard-working and available colleague, but which is, on average, sadly true, and especially so in the small, "provincial" universities. And it is not just the professors who are at fault. The whole ministerial machinery wastes human and practical resources in local mini-campuses, underdeveloped and underattended, frequently only created for local, quasi-political interests. This blatant waste of limited state resources is the second of the three treasons, not always easy to appreciate for outsiders, and one which escaped the poignant analysis, now a few years old, of B. R. Clark (*Academic Power in Italy*, Chicago University Press; 1977).

Now for the third treason: that towards research, by law one of the *raisons d'être* of the Italian university system. It may, in selected cases, be of some relevance, but it is more frequently "irrelevant" and, paradoxically, is by and large impossible, owing to lack of funds and the abundance of red tape. Simone, a humanist bordering on science, draws here a

fine line between humanities and science. In the former, irrelevant research is more frequent than in the latter, and not just because of the keen international competitiveness of science. Physicists in particular and scientists in general, we learn, are "more attached to their trade", for reasons which, Simone says, defy explanation. (How about "because it's there"?)

A delightful and important book, not about corruption but about reality in the Italian university system. It is easy to predict that it will become a bestseller at least in Italy though, of course, not among our students, the logical target for the book. Rather among professors, keenly bent on discovering the various treacheries of their own colleagues. □

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### The 'old-boy network' can come in handy.

ment, you not only need to be proficient in Italian, but also be conversant with classical Greek, for example to appreciate the concept of "*nòstos*", one of the plagues, according to Simone, of our universities. Heroes in classical mythology had to spend a large part of their life wandering in desolate and hostile places doing, by definition, heroic deeds. Eventually they felt they had earned their right to and started longing for, their *nòstos*, a triumphal and touching homecoming. Not that Italian university professors have much in common with Greek heroes but, after winning their *concorso* and serving out their prescribed time in a small university, they all want to come back to their parent institution, usually one of the big ten, out of the 60 or so in the country. But positions in big universities are few and coveted, so here is where the 'old-boy' network comes in handy, or better still, membership of the freemasons or Opus

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