

book reviews

Like any good hero myth, Kalb's story pits a virtuous outsider-neophyte-innocent (himself) against evil giants (Johanson, the National Science Foundation, "Berkeley types" and everyone's favourite bogeymen — peer reviewers). But whatever Kalb may have intended, it is the geologist Maurice Taieb who emerges as the true hero in this book. It was Taieb who first explored the entire Awash Valley, and found Hadar and most of the other major study areas of the Afar region. He recognized their palaeo-anthropological significance, brought them to the attention of the world, shared them with large teams of professional scientists, and changed palaeoanthropology for ever.

Kalb's unrelentingly negative characterizations of his one-time friend and partner Donald Johanson are both amusing and revealing. Readers who enjoyed Johanson's popular accounts of the Lucy discovery and its aftermath may be surprised by Kalb's warts-and-all descriptions of the same exciting days. Despite his current avowed dislike of Johanson, Kalb does admit to enjoying his company in the early years. He attributes much of this to field time spent on recreational drugs. Kalb is silent about whether his idiosyncratic ideas on Afar tectonics and lake migration originated from this source. In contrast, he makes it perfectly clear that once the Hadar hominids were found, the camaraderie with Johanson rapidly turned to one-upmanship, and worse. The reader should thus 'consider the source' when judging the contents of Kalb's book. For example, knowledgeable readers will recognize his portraits of Desmond Clark and Berhanu Abebe, among other scholars of Ethiopia, as unwarrantedly negative.

Throughout the book, Kalb calls attention to the need for continued development of personnel and infrastructure in Ethiopian palaeoanthropology. This is an important consideration too often ignored in this type of account. Kalb's most serious contention is that Western palaeoanthropologists have exploited Africa's fossil fields without giving back. In far too many instances, he is correct. The academic community too often forgets that these fossils, so critical for understanding human evolution, are ultimately Ethiopian antiquities whose study involves Ethiopian scholars.

Today, Ethiopian scholars contribute regularly and directly to advances in palaeo-anthropological knowledge and have achieved international status as professionals. A second generation is currently developing. But even now, three decades after the events chronicled by Kalb, the 'Bone Wars' rage on. Attitudes decried by Kalb remain fossilized in some palaeoanthropologists. The sad legacy of Western colonial exploitation of African palaeontological resources is not merely a historical phenomenon.

Kalb best captures only the early years of

"the race to discover human ancestors in Ethiopia's Afar Depression". As he so clearly documents, the lure of ancient human fossils can evoke the worst in modern human behaviour. Have things improved? Last year a highly qualified young Ethiopian scholar was displaced from a new hominid site he had discovered in the Afar. He had worked there for three seasons, and had even published. But without his consent or initial knowledge, and despite his protests, foreign researchers have now managed to collect and export fossils (including a hominid) from his localities. The foreign researchers claim that their acquisition of his site was entirely legal, but I consider the ethics of their approach to be questionable.

In my opinion, this kind of behaviour, all too common among modern palaeoanthropologists and government officials, spells serious trouble for human evolutionary studies. Many Western palaeoanthropologists turn a blind eye to the ethics of such cases, dismissing them as internal African bureaucratic disputes, while eagerly awaiting invitations to study the exported fossils. Kalb's will not be the final dispatch from Afar. ■

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Citation gold standard

The Web of Knowledge: A Festschrift in Honor of Eugene Garfield

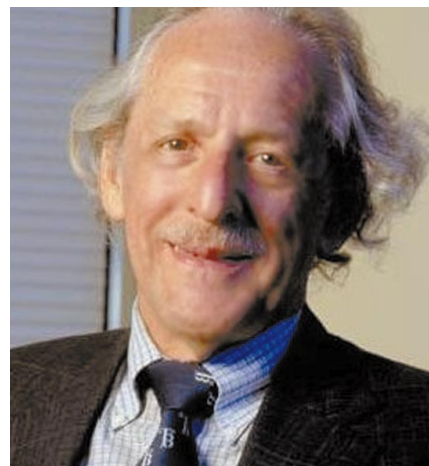
edited by Blaise Cronin & Helen Barsky Atkins

Information Today, Inc.: 2000. 544 pp. \$49.50

John Ziman

Until the 1950s, only librarians, journal subeditors and other information technicians were interested in 'the communication system' of science. True, we knew about abstract journals, where creative scrutiny of an idiosyncratic subject index might unearth something of more than passing interest. Then came Eugene Garfield, a restless postdoc wandering in the ill-charted wilderness of biomedicine. Garfield invented and then used his entrepreneurial skills to promote the *Science Citation Index (SCI)*.

Here at last was a systematic procedure for searching the literature. First, start with any paper relevant to your problem and list all the earlier sources it cites. Next, locate in the *SCI* all the more recent papers that cite the items on your list. List the most vaguely



Insightful: Garfield's *SCI* continues to prove its worth as a general catalogue of scientific activity.

relevant of these, along with the sources they cite. Iterate, until nothing more of interest turns up. Thus, surely, you will discover most of what is scientifically known, or conjectured, on the subject.

Garfield's idea was not entirely novel. In the legal profession, where precedent is holy writ, citations had been indexed semi-officially since 1821. What now made it feasible in science was the electronic computer. In 1963, with the encouragement of a few far-sighted senior scientists — especially the Nobel-prizewinning geneticist Joshua Lederberg — he published the first volume of the *SCI*. Its scope was already large enough to be of real use, and it quickly became a standard component of the scientific information system, found in every institutional library.

How well does it serve its original purpose now? Is it commercially viable as a search tool? The pharmaceutical industry must find it indispensable. I imagine that all industrial research and development organizations rely heavily on it to check for prior discovery claims relevant to their patents. But this book is suspiciously mute on the subject. Perhaps the *SCI* is already being superseded by more powerful Internet search engines.

Yet the *SCI* continues to prove its worth in other ways, especially as a general catalogue of scientific activity. Each year it lists a vast number of papers in a variety of disciplines, tagged by author(s), journals and institutions. It is much easier to retrieve, count and compare these numerically, category by category, than it ever was in conventional abstract journals. Weight these figures with 'impact factors' derived from citation rates, and you have a wonderful database for quasi-quantitative research. In other words, the *SCI* allows the systematic practice of 'scientometrics', which provides the substance of much of this book.

But there are some serious pitfalls. For example, the *SCI* doesn't cover all the

scientific literature, especially that outside the United States. Thus, as Jane Russell points out in her essay, the index seriously undervalues the work of the many excellent researchers in developing countries such as Brazil, who mainly publish and are cited in their national journals. Again, as Jonathan Cole admits, individual citations are so variable in meaning and weight that it is absurd to take notice of small numerical differences. And can Anthony van Raan really believe that the distinction between 'top' and 'not-quite-top' research performance might be represented by a numerical indicator calculated from such wonky data?

The trouble is that the supposed quantities in this assumed relationship are statistically completely abnormal. The distribution of citation rates is extremely skew. Just a few scientific papers are cited ubiquitously and endlessly: but the great majority are cited only by their proud authors. And as Lev Landau once remarked, research performance ought to be measured logarithmically: first-class scientists are extremely rare, but they achieve 10 times as much as their numerous second-class colleagues, and so on.

In such circumstances, the arithmetical mean is not a stable statistic. Thus, it is perfectly natural mathematically for van Raan to find that impact factors seem to decline when averaged over larger and larger groups. The more elaborate indicators sometimes derived from such data are even more suspect.

Again, consider the scientometric analysis of co-citation linkages—that is, the number of times that two authors cite, or are cited by, a third one. Clearly, this is an indication of some sort of epistemic and/or social connection. The network of connections can be pruned into 'trees' (Henry Small), or scanned for 'clusters'. Experts claim (although without compelling proof) that this is a helpful way of surveying a scientific domain and getting early warning of nascent relationships. But as Tony Cawkell points out, there are many different ways of presenting these connections visually. What he fails to observe is that a scientific document has no numerical coordinates other than its date, so that even the dimensionality of this 'map' is notional. The usual choice of just two dimensions can produce geometrical contradictions. Thus, for example, Howard White's 'point-line-plane' strategy for mapping co-citational 'neighbours' of a given author is not always feasible. On the other hand, a three-dimensional map is

difficult to read, and mere humans can't grasp four-dimensional clusters. The complex multiple connectivity of the scientific literature undoubtedly says something about some of its cognitive and social features. But exploration of these is not, strictly speaking, a 'scientific' enterprise. For if there really is an underlying invariant 'structure', it is a topological abstraction that is not necessarily capable of being mapped consistently in a limited number of space-like dimensions.

Scientific citation is a thoroughly institutionalized, social practice. It functions effectively as a two-way token of interpersonal trust (Elisabeth Davenport and Blaise Cronin). In essence, it is an exchange process, where instrumental legitimacy is traded for symbolic recognition (Robert Merton). Closer attention could well be given to its changing norms. But, as Charles Oppenheim says of patent citations, "it is remarkable how much work has been undertaken based upon unproved or shaky foundations". Even after 40 years of devoted research, the foundations of Eugene Garfield's enterprising idea are still not that much more secure. ■

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Crescent Sun: a partial eclipse over the Hagia Sofia, or Church of the Holy Wisdom, in Istanbul, Turkey.

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Darkness at noon

Glorious Eclipses: Their Past, Present and Future

by Serge Brunier & Jean-Pierre Luminet
[translated by Storm Dunlop from *Eclipses, Les Rendez-vous Célestes*; Larousse-Bordas: 1999]
Cambridge University Press: 2000. 192 pp.
£25, \$39.95

Jay M. Pasachoff

At first sight, this is the ultimate eclipse book. Oversize, well-produced photographs in a large-format book are accompanied by interesting text covering a wide variety of eclipse phenomena. The partnership of an astronomer writer/editor and a professional astronomer, albeit a cosmologist, successfully brings the beauty of total solar eclipses to the fore.

Although the writers alternate, the difference in tone is not jarring, perhaps thanks to the expert translator, Storm Dunlop. In a chapter entitled "The great cosmic clockwork", Serge Brunier, long-time editor of the French popular astronomy journal *Ciel et Espace*, discusses the many kinds of eclipses and occultations (when one astronomical body occults, or hides, another). He even includes the eagerly awaited 2004 transit of Venus — the passage of Venus across the face