

if the topics were selected more to spark an immediate response in that audience than out of the author's own intellectual interest in them. The way topics are presented, too, suggests an anxiety to keep courting the reader's flagging attention: though the academic content is substantial, it is larded at frequent intervals with turns of phrase that are highly coloured or seek a (contrived) intimacy with the reader — "like the merits of Mom and apple pie . . .", "hormonal hurricanes", "math and sex is a hot topic" and so on. If it is a university audience that Fausto-Sterling has in mind, this surely implies a depressing (but accurate?) estimation of students' stamina for unalloyed academic argument.

What is clear about the intended audience is that it is American. That emerges from the style, the references to "this country" (the United States) and the overwhelmingly American literature that Fausto-Sterling draws on. Indeed, in one sense her book is more revealing about the state of contemporary American society than about the fundamental biology of the sexes, especially in focusing critical attention on some strongly hereditarian and perhaps even social-Darwinist (if that misnomer may be excused) strands surviving in American political life. Fausto-Sterling is greatly and understandably concerned about the presentation of biological theories in the media, and about the tenor of public debates on their social implications, for example on whether arguments about the biological basis of female or male actions should be accepted in the courts as diminishing responsibility for them. This results in frequent references to what *Time*, *Newsweek*, *Mainliner* (apparently an American in-flight magazine) and many others have to say. Up to a point, this is right and proper, since a socially responsible scientist should undoubtedly take notice of what the public at large is reading about science: but it seems to me that some overly superficial treatments are here allowed to determine the agenda and set the tone of the debate.

In short, Elaine Morgan is the more natural and more successful writer of the two, and what she has to say commands attention if not conviction. Stimulating though it was at first publication, however, *The Descent of Woman* is outdated. Anne Fausto-Sterling's *Myths of Gender* is substantial and useful, and has considerable intellectual merits: but, for my money, the author (not uniquely) strives too hard for commercial appeal, and might have been more successful had she had more confidence in the intrinsic interest of her material. □

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At the information exchange

Eugene Garfield, Henry Small and George Vladutz

Scientific Communications and Informatics. By A. I. Mikhailov, A. I. Chernyi and R. S. Giliarevskii. Translated by Robert H. Burger. *Information Resources Press, Suite 700, 1700 North Moore Street, Arlington, Virginia 22209, USA: 1984. Pp.402. \$55.50.*

THE East Europeans have often excelled as theoreticians out of necessity. Lacking a technological infrastructure that allows them the luxury of developing practical solutions to many problems, they lag in applied science but have the time to view developments from the outside. While there are many good theoreticians in the Western world, the distractions of applied research are frequent and often compelling.

The re-publication in English of a leading Soviet book, about eight years after its appearance in Russian, speaks of some strange priorities in the Soviet Union. There the scientist is deprived of the immediacy and convenience of personal access to foreign journals and books; nor are the authorities willing to truly join in international communication by allowing their own journals and books to be published first in English — the lingua franca of international science. Without this misplaced linguistic pride, the seminal research of such as the biochemist Shemyakin would have been known to the rest of the world much earlier. Instead, dozens of notable works by Soviet scientists are doomed to delayed recognition.

How does one properly appraise the translation of a book that itself was not very current? Ten years ago we would have had to say it is a very important work, remarkable as a thorough compilation and analysis of materials on information science (or informatics, as Mikhailov named it many years ago). Today it is still interesting for some of its statistical tables, but there is little in it that is new to the serious student of the subject.

One of the book's most appealing features (although others may find it annoying) is the frequent allusion to historical, sociological and philosophical work which serves to place the field of information science historically and intellectually in the social sciences. Much of what the authors say on the topic of contrasting national styles in information science remains valid, showing how little the field has progressed on the international front. Also, even now the succinct discussion of refereeing — the subject of a vast literature — makes interesting reading.

Here the authors quote John Ziman; it will amuse him to learn that he is an American.

One wonders if the book review editor of *Nature* wasn't a bit mischievous in asking staff of the Institute for Scientific Information to review a work by leaders of its Soviet counterpart. There is, for instance, only a cursory remark (p. 159) about ISI's *Science Citation Index*, in connection with Derek de Solla Price's "half-life" index for literature obsolescence. Price leaned heavily on the *Index* for support for his theoretical notions — as, incidentally, did V. V. Nalimov, who coined the term "scientometrics" ("naukometriia") for the "science of science". But although the ageing effect is illustrated by the interesting discussion on pp. 152–153 of "low-calibre" articles, and Price is quoted, the term "uncited" has been ignored. Similarly, had the book been translated by an information scientist, the term "substantive" might have been replaced by "citation classic". The translator, the authors and Norbert Wiener, whom they quote on the cumulative effects of scientific work, all seem to have been unaware of Ortega y Gasset or the so-called Ortega hypothesis.

More generally, the authors' claim that our failure in the West to read Russian puts us at a disadvantage. This is no more significant than our inability to read Japanese or Chinese; the importance of each "exotic" language varies from field to field and from time to time. The unfortunate effect of the language barrier in the past was in delayed recognition. In the case of Shemyakin we could only have known about his work a year earlier, because it was abstracted. But its publication in Russian, rather than in English, was the principal factor in its relatively low citation compared to the work of David E. Metzler.

There can be little doubt that this book should be in every school of library or information science, though the publisher would have been wiser to urge (or allow) the authors to provide an up-to-date bibliography and a decent index, especially to cited authors. Writing a completely revised second edition will be a considerable task, but no doubt it will occur. We must hope that when it does, the English version will appear simultaneously with the Russian. The publication delay for the translation of the present book, which is by no means uncommon, makes communicating with our Soviet colleagues much like communicating with Mars. □

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• Springer-Verlag has published Part I in Series A (original scientific publications) of *Heisenberg: Collected Works*. Price is DM 180. Part IB was reviewed by Nicholas Kemmer in *Nature* 313, 826 (1985).