With the advent of newer and newer computer generations, most of the former tools of practical arithmetics and statistics gradually became antique relics of the pre-computer age, let alone the dramatic fall of the slide rule. Some might have thought that graphical methods would soon share this ignominious fate, since their very limited accuracy could be far surpassed by computer tabulations of myriads of data. However, the most competent experts of computer application have clearly seen even twenty years ago that “there are still good reasons for retaining a strong interest in graphical techniques”. Namely, “an immense amount of information can often be compressed into a single graph and in a few graphs the economist or engineer may have the essence of a whole problem at his fingertips”.¹

A similar situation came about in mathematical statistics. While formerly the use of statistics had been an absolute necessity to represent otherwise unmanageable records of data, computers could readily store and evaluate practically any amount of information in full details. The main role of statistics has turned to be rather a tool of exploratory data analysis; to uncover the hidden patterns in data sets.

Both graphical and statistical methods of data analysis could attain survival only by adapting themselves to the new requirement: to provide a deeper insight into the intrinsic structure of quantitative information. The new statistical view has been soon codified in a flood of papers on jackknife, bootstrap, robust estimations and related topics in the 60’s and 70’s, which was crowned by Tukey’s benchmark book.² Development of graphical methods took place much more behind the scenes; only a few papers were directly devoted to the problems of graphical display of data. (E.g. the otherwise excellent book of Schmid and Schmid³ was more technical than conceptual in nature.) That’s why Tufte’s book acted as a real revelation.

Graphics reveal data – that is one of the main lessons of the book. Tufte’s book reveals graphics – that is how the reviewer can summarize his opinion.

The book is abundant with historical examples, which are, however, not just decorative illustrations. Tufte revives the classic works of Playfair, Marey, Minard, and others as requisite tools in setting the aesthetic and functional standards for statistical graphics. The standards are those of clarity, integrity, precision, efficiency, and truth. They are formulated in laconic principles like “Graphical excellence is that which gives to the viewer the greatest number of ideas in the shortest time with the least ink in the smallest space” or “Graphical elegance is often found in simplicity of design and complexity of data” and illustrated by a vast number of examples and counter-examples. Among them one may find several ingenious novel ideas (such as the quartile plot or the range frame); in one or another of the examples of misrepresentation one will unavoidably recognize Fig. xx. of his own latest paper (or, at least, realizes that “(Fig.)”, often used to refer to graphics, is an ugly abbreviation and is not worth the two spaces saved”).

Readers of scientometric inclination will surely find amazing that Tufte has built and boldly used several quantitative indicators for assessing graphical eminence of individual displays as well as for ranking journals.

The Lie Factor (=size of effect shown in graphic/size of effect in data) was found 14.8 in a New York Times illustration (values greater than 1.05 or less than 0.95 indicate substantial distortion).

The highest level of Graphical Sophistication (percentage of statistical graphics based on more than one variable, but not time-series or a map) was reached by two Japanese newspapers Akahata and Asahi Shimbun (9.3% and 7.6%, respectively); among newspapers with 0.0% one may find Frankfurter Allgemeine, Pravda, The Times, and Wall Street Journal.

Median value of Data Density was found highest in Nature (48.0 numbers per square inch). Values for some other journals are: Science 21.0, Journal of the American Statistical Association 17.0, Scientific American 5.0, Pravda 0.2.

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Maybe, some of these indicators can be refined but one can hardly-imagine any reasonable scale of excellence, on which Tufte's book would not reach the highest possible score. It offers the unprecedented opportunity to learn with delight what has been done wrong and how to improve it.

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Max F. PERUTZ, Ging's ohne Forschung besser? — Der Einfluss der Naturwissenschaften auf die Gesellschaft, Wissenschaftliche Verlagsgesellschaft mbH Stuttgart 1982 (pp. 54)

The genesis of this thin paperback (the English title of which might be “Would we be better off without research? — The influence of the natural sciences on society”) is not unlike many other publications on science and society nowadays. A publisher approached the Nobel-prize winning author to produce a booklet on the impact of molecular biology on society, and the author then chose to make the subject somewhat broader. The commission offered an opportunity for the author to go beyond the technical aspects of his scientific specialty, and to articulate his ideas on the broader aspects of the context of science. Practically all who work in science develop such ideas just in the course of daily activities, even though few of us are “experts” on them in the sense of having devoted a considerable amount of time to them in a systematic way over a long period. A commission, however, offers an opportunity to organize the casual ideas and to do a bit more background study.

In such a study there are two distinct elements. One is to gather the relevant material as well as the opinions of others, and to understand them. This can be readily achieved by most scientifically trained people, since the technical aspects of these issues, along the broadest lines, are certainly within what such people can handle. The second element, however, is much more difficult to attain: It is a judgement acquired by longstanding experience, a weighting of the various components of the problem, a balanced synthesis of the pros and cons into a conclusion. Yet, in the final analysis, it is this second element that enables a person to form a stand on a given issue, that determines whether the stand will turn out to be realistic and wise in the course of subsequent events.

When asking Nobel-winners to write about broad questions of science and society, the implicit assumption is made that a person who was able to make a significant contribution to a technical aspect of science will also score better on taking a wise and, a posteriori, realistic stand on broad issues even if he is not an "expert" in the above mentioned sense. I am dubious about the validity of this assumption. Nevertheless, in practice, Nobel laureates command greater attention from the public when they make statements about public issues, and hence it is interesting to see what they say and how they argue it.
All this I can say in this case without trying to be critical, since in fact I found Perutz's book on the whole quite sensible. It covers a truly broad spectrum: The humanizing influence of science; the demands of science; science and food production; the shortage of raw materials; damaging sideeffects; science and health; science and energy; energy consumption, food production, and the population problem; and science and politics gives a brief summary of the topics. Perutz’s approach is a level-headed and, on the whole, quite optimistic one. He presents the problems, looks at the available solutions and those likely to be available in the future, decries the prevalence of present-day fear-mongering, affirms his conclusion that the adverse effects of science are very small compared to its beneficial ones, and thus answers the title of the book in the negative. His documentation is quite systematic, and yet he does not claim omniscience on his part. Indeed, one of the very refreshing aspects of the book is Perutz’s modest and relaxed tone, giving the impression that even if the author is not a specialist expert on many of the topics he discusses, he will at least be unlikely to be carried away by pretentiousness, by emotions, by an abuse of the platform for venting personal biases.

The book is easy reading and can be covered in a matter of an hour or two. Yet it should be both pleasant and educational reading for all who are interested in the contextual aspects of science.

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Norman STOCKMAN, Antipositivist Theories of the Sciences, D. Reidel: Dordrecht/Boston/Lancaster, 1983. 284 pp.

The aims of this study are (1) to examine the bases for the confusion that characterizes the attacks on “positivism” by critical rationalists (notably Popper), critical theorists (the Frankfurt School), and scientific realists (notably Harre); (2) to clarify the nature of the debates about positivism among these three “schools”, and especially to consider the least developed side of this triangular relationship, the one linking critical theory and scientific realism; and (3) to extract what is useful from these debates for facilitating reflection and self-reflection in the natural and social sciences. The author, a sociologist at the University of Aberdeen, uses the method of “comparative exposition” to achieve these aims. He describes his work as an account of scientific method that is partly philosophical and partly sociological. Specifically, he tries to establish a basis for mutual understanding by translating the three traditions he analyzes into comparable epistemological terms. The final goal of this exposition is to provide support for the theses that (1) the antinaturalism of critical theory is a better basis for social science methodology than the naturalism of scientific realism; and (2) there may be some substance to the idea that natural science plays an ideological role in contemporary society.

The rationale Stockman offers for focusing on critical rationalism, critical theory, and scientific realism is threefold: (1) the Positivismusstreit dispute in German sociology that pits critical theory against critical rationalism; (2) the more diffuse debate between critical rationalism and scientific realism; and (3) Stockman’s contention that the positivism-antipositivism debate can be clarified by bringing critical theory and scientific realism into contact with each other. In Part I, Stockman discusses the problem of “identifying positivism”. Part II sketches the philosophies of the natural sciences in the three traditions; Part III deals with philosophies of the social sciences. Stockman outlines the well-known views of his protagonists in a style that is more a textbook summary than a critical interpretation. It is clear from statements at the beginning and conclusion of this book that...
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Stockman belongs in or somewhere near the critical theory camp. But he spends most of his time role-playing his protagonists, and it is easy to lose track of his presence in the triangular conversations he reports.

It is not surprising that Stockman has difficulty achieving his goal of “mutual understanding” among critical rationalism, critical theory, and scientific realism. Clifford Hooker has persuasively argued that philosophies of science (and, by implication, theories of science in general) must be construed as world views if they are going to be usefully compared. (I assume Stockman is familiar with Hooker’s argument since he cites the relevant paper). Hooker’s approach makes sense from a sociology of science and knowledge perspective; and in fact Hooker’s ideas are compatible with the so-called “constructivist” perspective in contemporary science studies. By ignoring both Hooker and contemporary science studies, Stockman fails to avail himself of crucial conceptual tools for sociological analyses of theories of science. Being marginal to these approaches could, of course, be an asset. But in this case, marginality leads Stockman to re-play old and worn battles instead of to intellectual innovation.

This is a frustrating book in part because there is in the closing paragraphs a hint of the innovative textbook (if not treatise) Stockman might have written. Imagine a study in the sociology of science that begins by pointing out the use of epistemology as a political weapon in the 1976 West German Bundestag election campaign. This situation is related to the debate between the “Finalisierung” project directed by Habermas and von Weizsäcker, and the “Wissenschaftsforschung in der Bundesrepublik” study group. This struggle between, respectively, defenders of “Alternatives in Science” and the “autonomy of science” is then identified as an imperative for analyzing the ideological function of the natural sciences. It is also viewed as a stimulus for exploring the possibility of developing alternatives to the conception of scientific progress as a function of internal deductive systematization. Stockman makes all of these points. The study would go on, as this one does not, to discuss the world view and constructivist arguments against the Habermasian distinctions between instrumental (natural science) and communicative (social science) action, and between monologic (natural science) and dialogic (social science) activity. These distinctions might have been recognized as examples of the fallacy of heavenly discourse — comparing an allegedly pure substance or entity with an allegedly adulterated one. So too the scientific realist’s distinction between “structure of a mode of production” and “objects studied by the natural sciences” might have been recognized as an example of the fallacy of the incommensurable comparative.

Given the above approach, Stockman would have had to consider that the idea of “what is or is not natural science” is not fixed forever; and he would have had to deal with the fact that humans are natural entities as well as social beings; and furthermore, that perceptions as well as knowledge are social constructs. Stockman might have considered the evidence for the views that nature is a social projection (even from a realist perspective!), that we do engage in symbolic communication (dialogue) with our projected social relations (or that we can and do create dialogues when, for example, we ask what we would do if we were that electron and we entered that field). More attention would have been given to the idea that scientific practice is an exercise in hermeneutics. This is the study we get a glimpse of in the closing twenty-five pages or so of this book of nearly three hundred pages. If the book had begun where it ended, Stockman might have recognized the inevitability of intellectual conflict (granting a distinction between constructive and destructive forms of conflict), and jumped into the fray on the side of the critical theorists (with whom his shadowy allegiances seem to lie), or on behalf of his own program. Instead, he is a ghostly presence on an ill-fated mission of good will.

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