is important, but I have seen no convincing evidence that it has been done or, for that matter, that it can be done.

In their last paragraph Hardy, Wolff, and Goodell appear to make the astonishing suggestion that the reason we failed to use their method successfully was because of the well-known effectiveness of a placebo in treating pain. We have dwelt upon this fascinating fact time and again, but if their method does not distinguish between a large dose of morphine and a placebo, where can one depend on it?

I should like to close by commenting on a matter that seems to me to be fundamental. We are firmly convinced that dependable information free from bias in this difficult field can be obtained only when *neither the subject nor the observer* knows what has been used. Now, when others have failed to confirm their work (as with aspirin, for example), Hardy, Wolff, and Goodell have for years insisted that their success where others failed was due to the fact that they used a few highly trained, experienced subjects over long periods. As everyone knows, the analgesic drugs produce a number of subjective responses which may or may not be related to pain relief—euphoria, for example, "giddiness," and so on. In other words, highly experienced subjects who know in general that they are involved in studies on analgesics are certainly not unaware when a narcotic is used and when a placebo is used. They know all too well what responses are expected. In short, I do not believe it is possible to fulfill the essential requirement of the unknown technique with such drug-wise, sophisticated subjects. This is not by any means to impute dishonesty to them. I would not trust data obtained by myself or by any other observer who knew what was used. Dr. Hardy, Dr. Wolff, and Miss Goodell evidently now also subscribe to the importance of the "double-blind" experiment (see their last paragraph). I doubt if they can achieve it with their highly trained, drug-wise subjects. The investigator I referred to above got good elevation of threshold when he knew morphine was used. He got no dependable data when he did not know what was used.

The highly trained subjects used by Hardy, Wolff, and Goodell came to have a vested interest in the outcome. To be sure, learning on the part of the subjects is always a hazard, but far more so with the group just mentioned than with a group that does not know what to expect, who have no interest in how the data come out, and who are discontinued as subjects after a few doses of the drugs.

It is absolutely essential that unconscious bias be eliminated here. Subjects who know how to recognize the subjective sensations of analgesics and who have an interest in the outcome cannot be considered as unbiased. I am obliged to continue to question the validity of any such experiments when either the subject or the observer is aware of what was used.

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# *Book Reviews*

***Science and the Social Order.*** Bernard Barber. Glencoe, Ill.: Free Press, 1952. 288 pp. $4.50.

It is one of the anomalies of our time that in a period of history when science is markedly influencing the social order this book is probably the first complete study of the subject. Robert K. Merton discusses in his foreword the reasons for the strange neglect of this field. The natural scientists are still, by and large, uninterested in the social sciences, and the social scientist has been afraid to speak of the natural sciences in which he has not been trained. A third factor that may have retarded this study is the fact that for some time the subject was almost monopolized by writers who accepted the Marxian analysis of history.

The book collects under one cover, and in a very readable style, the bulk of what is known on the mutual interaction of science and society. It attempts to correlate the material on the basis of techniques and concepts that have proved of value in other areas of sociological thought.

Barber's conclusions are very tentative—as they should be on the basis of so little established data. Only at the end, where he discusses the role and feasibility of the social sciences, does he become eloquent in defense of his field. His defense is convincing except on one point. He seems to underestimate the possibility of misuse of the social sciences on the basis of his statement that human beings will not let themselves be made into utter automatons. But great suffering can be caused long before this stage is reached, and it requires continual vigilance and a knowledge of what science can and cannot do, if we are not to acquiesce in programs labeled "scientifically established," when in fact scientific knowledge is applied to inhuman ends.

The book begins with a discussion of the nature of science and devotes an all too brief chapter to the sociological basis underlying the historical development of science. The major part of the book then deals with contemporary science both in liberal and authoritarian societies, and specifically with the social organization of science in America in the universities, in industry, and in government. A chapter on the respective roles of the individual and society in the progress of science makes fascinating reading, followed by the most provocative part of the book, deal-

ing with the social consequences of science, the social responsibility of science, and the question of whether science can be planned.

The criticism of Norbert Wiener's position is not well founded, for the author approves Bridgman's barring Nazi scientists from his laboratory, implying that he would also approve the refusal of scientists to work for the Nazis. But such a stand is in essence no different from Wiener's refusal to publish data in peacetime that would immediately be applied to guided-missile research. If the principle is accepted that the scientist must draw the line at some point as to the exploitation of his knowledge by society, and if, as is also done in this book, the stand is taken that respect for individual conscience is a sociological factor favoring the development of science, then Wiener's stand must be accepted as a valid exercise of social responsibility, even if the author cannot agree with the point where the line is drawn. The studies of J. U. Nef summarized in his *War and Human Progress* show that Wiener's position was widely held in the early period of modern science.

One might wish that every scientist would ponder the implications of this study. It is not by any means a subject of purely academic interest, for the interaction of science and society is molding our future, and much can be done to guide the interaction wisely.

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***Thermodynamics and Statistical Mechanics.*** William P. Allis and Melvin A. Herlin. New York–London: McGraw-Hill, 1952. 239 pp. $6.00.

This book has been designed as a text for a half-year course for college seniors majoring in physics. Within the confines of such a course, the authors have endeavored to present both the fundamental ideas of thermodynamics, and enough statistical mechanics to give the student some idea of the statistical basis of thermodynamic laws. This has made it necessary to limit the presentation of both fields rather stringently.

The treatment of heat and thermodynamics has been simplified by rigid restriction of the illustrative materials—only pure substances are considered, and the only force discussed is hydrostatic pressure. Later, in the section on statistical mechanics, magnetization is considered, and the student sees the formalism in terms of variables other than P, V, T.

Statistical mechanics is treated from the Boltzmann-Planck point of view. The microstates of systems are described in terms of cells in phase space; the existence of a natural cell size, $h^3$ in three-dimensional phase space, is deduced from the third law of thermodynamics and the Sackur tetrode equation. In this part of the book the illustrative material is abundant and varied. It includes discussion of gases, magnetism, specific heat theory, Brownian movement, thermal noise and radiation, He II, and electrons in metals. Here, space limitations are met by extreme simplifications of the discussion, occasionally to the point of misleading superficiality. College seniors are likely to find the range of illustrative material intriguing, but the reviewer is inclined to wish that the authors had followed the pattern established in their treatment of thermodynamics, with fewer illustrations more completely developed.

On the whole, this is an attractive text, with many illustrative problems and a good index.

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## *Scientific Book Register*

***Phosphorus Metabolism: A Symposium on the Role of Phosphorus in the Metabolism of Plants and Animals,*** Vol. II. Sponsored by the McCollum-Pratt Institute of The Johns Hopkins University. William D. McElroy and Bentley Glass, Eds. Baltimore: Johns Hopkins Press, 1952. 930 pp. Illus. $11.00.

***Water: Miracle of Nature.*** Thomson King. New York: Macmillan, 1953. 238 pp. $3.50.

***Races of Maize in Mexico: Their Origin, Characteristics and Distribution.*** English ed. of *Razas de Maiz en Mexico.* E. J. Wellhausen, L. M. Roberts and E. Hernandez X., with collab. of Paul C. Mangelsdorf. Cambridge, Mass.: Bussey Inst., Harvard University, 1952. 223 pp. Illus.

***Léon Fredericq: Un Pionnier de la Physiologie.*** Volume publié à l'occasion du Centenaire de sa naissance. Z. M. Bacq and M. Florkin, Eds. Liège: Sciences et Lettres, 1953. 232 pp. Illus. 210 Belg. fr.

***Kinetics and Mechanism: A Study of Homogeneous Chemical Reactions.*** Arthur A. Frost and Ralph G. Pearson. New York: Wiley; London: Chapman & Hall, 1953. 343 pp. Illus. $6.00.

***Lillie's Development of the Chick: An Introduction to Embryology.*** 3rd ed. Revised by Howard L. Hamilton. New York: Holt, 1952. 624 pp. + plates. $8.50.

***Possums.*** Carl G. Hartman. Austin: Univ. Texas Press, 1952. 174 pp. Illus. $6.00.

***Principles of Organic Evolution.*** Arthur Ward Lindsey. St. Louis: Mosby, 1952. 375 pp. $5.75.

***Applied Physiology.*** 9th ed. Samson Wright with collab. of Montague Maizels and John B. Jepson. London–New York: Oxford Univ. Press, 1952. 1190 pp. Illus. $9.00.

***Third Inter-American Congress on Brucellosis.*** Held in Washington, D. C., November 6–10, 1950, under the joint auspices of the Inter-American Committee on Brucellosis, the U. S. Committee on Brucellosis of the National Research Council, and the Pan American Sanitary Bureau. (In English and Spanish.) Washington, D. C.: National Research Council, 1952. 302 pp. $5.00; $3.25 paper-bound.

***The Statistics of Bioassay.*** Reprinted, with additions, from *Vitamin Methods,* Vol. II. C. I. Bliss. New York: Academic Press, 1952. 184 pp. Illus. $3.50.

***Encyclopédie Biogéographique et Écologique.*** Vol. IX, *Le Tapis Végétal en Basse-Provence.* G. Kuhnholtz-Lordat. Paris: Paul Lechevalier, 1952. 208 pp. Illus. 3500 fr.

***Thermionic Vacuum Tubes and Their Applications.*** 6th ed. W. H. Aldous and Edward Appleton. London: Methuen; New York: Wiley, 1952. 160 pp. Illus. $2.00.