and the cortex as equipotential are equally dead. Obviously such studies are going to throw much light on questions relating to human behavior.

"Communication" is considered in a very broad way in the book—even the possibilities that sneezing, coughing, and vomiting may be forms of communication are touched on. A fundamental confusion arises where the organized social group is regarded as being identical with the system of communication; I believe that it is much more useful to view the communication system as one of the essential mechanisms by which group behavior is organized, a point of view which leads to more emphasis on the function of the message and less on the particular channel. This is particularly important in the study of nonhuman primates, where, as is pointed out by Altmann, the message may have no auditory component or, if it does, this component may be unimportant. The long discussion of design features common in primate communication leaves the reader with no clear notion as to why human language appears to be so different. Emphasis on the message clarifies this point, as the nonhuman primates can convey very little information, whereas human language by virtue of using names for environmental reference can convey a vast number of messages. Probably the greatest difference between man and the nonhuman primates is language and the anatomical structure which makes it possible. Study of the communication systems of the nonhuman primates can reveal both the fundamental differences and the importance of human language.

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Theoretical Battles Waged and Won

Electrons, Ions, and Waves. Selected works of WILLIAM PHELPS ALLIS. SANBORN C. BROWN, Ed. M.I.T. Press, Cambridge, Mass., 1967. 452 pp., illus. $20.

In this volume Sanborn C. Brown has collected some of W. P. Allis's theoretical papers to commemorate his colleague's new appointment as professor emeritus at M.I.T. The grapevine has it that this task was accomplished in the greatest secrecy just in time for formal presentation on a most festive evening when wine, women, and song abounded.

Allis battled the Boltzmann equation long before the modern era of plasma physics dawned on us, and most of those armchair generals who like to relive battles at their desks will find, upon reading *Electrons, Ions, and Waves,* an easy answer to the question of how they would have fared in this intellectual struggle: The Boltzmann equation would have won. Allis, more courageous and talented than many, has managed to fight the inscrutable equation to a draw: Involved partial differential equations, in anywhere from one to seven independent variables, are handled by extensions of the perturbation kinetic method that Lorentz applied to electron motion in metals. The list of problems solved is impressive, and includes, among others, a determination of the velocity distribution of the electrons drifting and diffusing in a Townsend apparatus and a theoretical treatment of high-frequency electrical breakdown in gases. The Townsend problem involves an approximate solution of the space-dependent Boltzmann equation, a boundary-value problem which has not been exactly solved to this day. Evaluation of breakdown potentials requires a precise determination of the high-energy tail of the electron velocity distribution when both elastic and inelastic collisions with real neutral atoms play an important role. It is no surprise then that this collection includes also some of the most detailed modern calculations made on the scattering loss of charged particles from magnetic-mirror confinement systems. These are all challenging problems in kinetic theory.

During the 1950's Allis tackled the ambipolar diffusion problem by a mixture of analytical and numerical techniques. Anyone who has ever tried to subtract slightly differing plasma electron and ion charge densities without ending up with 10^6 volt/cm will appreciate that his paper on "The Transition from Free to Ambipolar Diffusion" is a classic. According to the editor's explanatory remarks such subtraction continues to be a problem in this field, and led to the inclusion of a recent unpublished paper, "On the Ambipolar Transition."

Many have been dismayed by the jungle of misnomers, semantic confusion, and redundancy in the literature of wave propagation in plasmas. Toward the end of the 1950's Allis decided to do something about it. In the period that followed, the notions extraordinary wave, $m/M = 0$, right-hand polarized, left-hand circular, ordinary, cut-off, $M/m >> 1$, resonance, and the like went into the grinder and reappeared as principal lines and as phase velocity curves on what has come to be called the CMA (Clemmow-Mullaley-Allis) diagram. Some approximations had to be made: Collisionless damping fell by the side, and most other thermal effects were ignored. This upset some of the purists, but when all is said and done Allis's classification of waves has proved its worth, especially to the experimentalists who now frequently interpret wave propagation through nonuniform plasmas with the help of such diagrams. About one-fourth of this volume is devoted to these subjects in wave propagation. Here too, in his paper "Electron Plasma Oscillations," we find Allis (1958) deriving a Landau damping rate, correct apart from a factor $16\pi^2$. Present-day readers may be upset to find that his derivation is based on a calculation of the work electrostatic waves do on the trapped electrons, a nonlinear effect, when the Landau derivation was based on linear phenomena. The resolution of this difficulty is left to the reader, who is encouraged to remember the emotions Landau damping generated ten years ago.

These selected works mirror the characteristic style with which one man has influenced many colleagues and students over the past four decades. For his editorial efforts in collecting these works Brown deserves the thanks of all who wish Allis well.

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Why Science Succeeds

The Organization of Inquiry. GORDON TULLOCK. Duke University Press, Durham, N.C., 1966. 242 pp., illus. $5.50.

In *The Organization of Inquiry* Gordon Tullock attempts to explain how the apparently nebulous structure of scientific activity has enabled scientists to produce a coherent and cumulative body of knowledge. He begins his treatment with an analysis of the social organization of science and goes on to consider the question Why in-

quire?, the subjects and the methods of inquiry, data collection, the problems of induction and of verification and dissemination, and the backwardness of the social sciences. He concludes with a chapter of "practical suggestions."

Tullock offers three reasons why scientists inquire: practicality, induced curiosity, and curiosity. Practicality and curiosity are rather traditional concepts normally subsumed under the rubrics of applied and basic interests. Induced curiosity refers to the process by which researchers are motivated to engage in pure research through extraneous rewards. The author recognizes that reasons for undertaking research are complex and that these distinctions are difficult to maintain in actual cases, but he stresses their analytic usefulness. His discussion of the relationship of pure to applied science and the limitations of induced research is well done.

The relationship between applied and pure research is discussed in some detail, with particular emphasis given the part played by the practical application of pure research findings in the verification process of science. Indeed, according to Tullock, it is the principle of replicability and verifiability that unifies scientific inquiry. The effectiveness of science as a cumulative endeavor results primarily from the reliance on verification and replication that prevents the perpetuation of results based on fraud, unconscious bias, or accidental error. Here, perhaps, in the manner in which science makes visible the relationships that exist within a given system, lies the heart of what differentiates science from other kinds of human activity. Tullock correctly draws a sharp distinction between the formulation of a hypothesis and its verification. The scientific method applies to the latter; the former appears to be more or less an art, including accident, serendipity, and logical progression among its sources.

Tullock criticizes the university system, which he says induces curiosity through extraneous rewards, one result of which is the writing of a multitude of minor or even meaningless papers. On a number of grounds he questions whether the university is an optimal setting for pure research. Particular victims of induced curiosity are the social sciences, which Tullock labels "backward." While his reasoning in spots is tortuous, and his ascriptions of causality not fully convincing, his underlying point about the banality of much social science work is self-evident. I suspect, however, that the ratio of banality to important work in the social sciences approximates that in other sciences, though perhaps social scientists are more apt to get their banalities published. In any case, more documentation of Tullock's assertions is in order.

Indeed, the book, despite its readability and the incisiveness of many of its author's comments, must be faulted because the author does not provide systematic evidence in support of his position or consider the systematic evidence of others. For instance, Tullock says that separation of teaching and research would probably not harm research and might improve teaching. Contradicting this are results of a study by Pelz and Andrews, who found that the best researchers were those whose work was diversified, that is, who were engaged in teaching or some other activity in addition to research. Studies by Marcson and Ben David also are relevant to Tullock's discussion of the university. The social organization of science has been dealt with systematically by Marquis, Pelz, and myself, among others, and Glaser and Krohn have dealt with the motivation of researchers. The scope and conclusions of these and other studies that could have been cited may not be as broad as Tullock's speculations, but the studies are systematic and they do throw doubt on some of his conclusions.

If the history of science tells us anything, it is that logical, speculative structures are not reliable in and of themselves. If this were not so then there would be little need for verification. This is as true for the study of science as for science itself. In a review of the literature dealing with organizational aspects of science, Ann Folger Decker and I compared speculative discussions with systematic observations and found that in two-thirds of the comparisons the systematic observations contradicted in part or totally the conclusions based upon speculation. Speculation and unsystematic observation are starting points in any given area of inquiry, but under no circumstances can speculation replace systematic investigation. In too many books and articles dealing with the organization of science this is precisely what happens. Systematic evidence is overlooked or neglected by authors who would sharply criticize researchers in other areas who rely on speculation where systematic evidence is available.

If *The Organization of Inquiry* had been written a decade ago my review would have been much more sanguine. But, in the last ten years a number of systematic investigations of direct relevance to the subjects Tullock discusses have been undertaken, and his failure to consider them leads me to feel that on balance the work obscures more issues than it clarifies.

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Ancient Nubia

Lost Land Emerging. WALTER B. EMERY. Scribner, New York, 1967. 350 pp., illus. $7.95.

This is a book for the general reader with an intelligent interest in archeology. Emery is one of the leading figures in the field and is prominent in the special area of Egypt. The book thus commands more than a little attention. Emery presents a straightforward account of the role archeology has played in the discovery of ancient Nubia, a role which the High Dam at Aswan has made especially important in view of the fact that the Nile waters now cover most of the sites which marked the Nubian past.

It is something of a historical quirk that the gradual submergence of the Nubian heartland which began with the Aswan dam early in the century and is now completed by the new dam motivated important developments in the field techniques of modern archeology. Reisner's seriation approach, the sensitive salvage of human and other organic remains, and the full realization of interdisciplinary methods are some of these developments. Emery does full justice to these achievements, so that the book contains a valuable description of an important chapter in the annals of Egyptology.

The greater part of the book is concerned with the efforts made by archeology to salvage the Nubian monuments and a historical account of Nubia with the pertinent archeological discoveries highlighted. The writing is excellent, but the real glories of the book are the author's illustrations, which are probably without peer in archeological publication.

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