

classified as a sequence. Passing to the question of functional relation we take up independent and dependent variables, and show that these names correspond to at least three separate distinctions, a fact not ordinarily recognized by mathematicians. We give a somewhat elaborate discussion of functional relations, showing that what is ordinarily put forward as the Dirichlet definition of function does not adequately characterize a functional relation, and moreover is not really the definition given by Dirichlet. We lay down what we deem to be the conditions under which two or more variables may be said to be in functional relation with each other; and show that previous authors, in their treatment of functions, have not attained to a clear and precise view of the essential characteristics of a functional relation.

To these salient features of our work Professor Miller gives no heed whatsoever, though assuredly they comprise topics of fundamental importance in mathematics. He is content to dismiss our inquiries by stigmatizing them as relating chiefly "to definitions and the choice of words." We plead that our work is concerned chiefly with the unfolding of the conceptions which words should awaken in our minds, and not with the words themselves. To purely verbal questions we give scant attention. In our endeavors to attain to distinct and exact conceptions of what is fundamental to the inquiries of mathematics, we have found that the portrayal of these conceptions, as set forth by mathematicians of the highest eminence, are not free from great imperfections. We have spared no labor in obtaining and in stating in full "the definitions given by those who have made important advances in the fields" into which we go; and when unable to assent to these definitions, we have carefully set down our reasons for holding that they do not truly depict the lineaments of the conceptions which they purport to unfold. And Professor Miller, though manifesting his disapproval of our criticisms, makes not the slightest attempt to show that our charges of error are baseless, and that Baire, Pringsheim, Riemann, Russell, Weber, and the other

authorities whom we controvert are not guilty of the errors we ascribe to them.

Three passages of our work are specifically condemned by Professor Miller. All of these are trivial and could be removed from the work without affecting any of its doctrines or any major or minor argument put forward in defense of them. One fault that is imputed to us is that "on page 177 and elsewhere, the common erroneous assumption according to which the word function was used as synonymous with power is repeated." We merely say that "the word function *is said* to have been used by the older analysts as synonymous with power." We took care to insert the qualifying phrase *is said*, and so worded our remark is neither an assumption nor an error. And no reference to this usage occurs elsewhere. We are also rebuked for saying that "The only mathematician that we recall as making a specific distinction between quotient and ratio is Hamilton." We must acknowledge that such a distinction has been made by others, but we deny that the distinction we endeavor to enforce is as common as Professor Miller would have it appear. Finally we are chided for applying to imaginary and complex quantities the distinction between positives and negatives. Yet, if precedent is to be a guide, we can plead that both Gauss and Weierstrass used the two adjectives with respect to imaginary quantities.

ROBERT P. RICHARDSON,
EDWARD H. LANDIS

QUOTATIONS

SCIENTIFIC SNOBBERY

ONE reason for the neglect of science is that scientific men themselves frequently misrepresent the objects for which they work. For example, they often pretend that they perform their labors merely for their own amusement. We once heard it wittily said of such a man that he takes out his watch before dinner and exclaims, "Ha! I have half an hour before I must dress for dinner; I will just step over to my laboratory and make a discovery." But the public is not so easily deceived and therefore thinks in its dull way that the man of

science really labors in the hope of making some enormous fortune or obtaining some great honor. On another occasion, we heard it said of a man who has been toiling for years in the tropics for nothing, that "nobody knows why he does it, but we all believe that he wants a knighthood." When we timidly suggested that he was guided merely by a sense of duty, we were met by a stare of astonishment. Certainly this worker has never received a penny for his work as yet and never will, and we fear that not even a knighthood will come his way. No, men of science do not work either for amusement or to make fortunes. Like artists and musicians, they often find their labors fascinating because nature imbues them with an instinct in the directions chosen by them; but they are also conscious that their work will bring them no personal profit—not so much as that which a tithe of the ability shown by them in science would have yielded them in politics, law or grocery. Their ultimate object is to benefit humanity by adding to the store of knowledge which lifts the civilized man so far above the savage of the jungles. And that is the greatest object which any man can keep before his eyes.

Another form of scientific snobbery is the pretense that science has no practical object in view—it is so lofty a pursuit that the man of science should live among the stars and not soil his fingers with the common earth of everyday life. Even Lord Kelvin said "that no great law in natural philosophy has ever been discovered *for* its practical application"—though no one based more patents on his own researches than did Lord Kelvin. He may have been right in one sense, but certainly not in all (and he can not be accused of any form of snobbery). Thus geometry was really founded for the purposes of architecture and navigation. Mechanics was created to assist the engineer, and the theories of heat and of the conservation of energy were probably generated by the steam-engine; while the entire science of pathology has simply been created for its practical application as regards the prevention and cure of disease. Certainly investigations which were apparently useless at the

outset have often led to valuable practical applications; but they were usually undertaken because the worker knew that he must first solve general problems before applying the solution to specific cases. We believe that all the great theorists had practical applications before them like a distant light even in the greatest darkness of their efforts. Is it likely that Newton, or Harvey, or Faraday did not prophetically see that their work would some day benefit humanity? Nature is infinite, and it is therefore wise to toil in immediate contact with human needs and not to lose oneself entirely far away from the remotest utilitarian objects. In most cases those who lose all touch of the useful in their investigations end by becoming useless themselves. They are above the practical, and therefore become unpractical, and finally impossible.

Perhaps the worst form of scientific snobbery is the pretense that the man of science is absolutely above cash in any form. Let us distinguish. To effect discovery, a man must concentrate all his energies upon a single point; he has no time to watch the share market, or to promote companies in connection with his findings; and it will be lucky if he succeeds in making any advance at all even with all his energies bent upon the point of issue. In that sense, therefore, he must ignore cash. But even here various circumstances should influence him. If he is a bachelor, he may do as he pleases, and may live as a recluse upon brown bread and water in a monk's cell if he wish. But if he has children or other dependants, is he justified in allowing them to be brought up uneducated in poverty? Such a thing would not be meritorious in him but a crime; for we have our duties not only to science but to our families. The scientist who pretends his indifference to money is, therefore, often only a snob. Moreover, although he himself may have no children, or may possess independent means, this need not necessarily be the case with others. His quixotic attitude merely lowers the price of science in the world and causes other and probably better men to suffer. Still further, for the most obvious economical reasons, it causes science

in general to suffer, because when young men see the poverty of the most successful investigators they hesitate to enter such fields of labor and the recruiting of the voluntary army of science is naturally reduced. Certainly no scientific man has the smallest desire to be a millionaire; but moderate competence is useful to him as to others. A certain amount of money gives him a proper influence for good in society, and enables him to devote himself to those investigations which his nature tells him he is most capable of conducting. On the other hand, keep him in poverty and he soon loses his enthusiasm; he becomes a fakir sitting in rags by the roadside, and the ripest years of his life are often wasted. Is there any intrinsic reason why the greatest efforts of the best minds in the most fertile of fields should lead to poverty? Yet the history of the world proves that they generally do so—to the loss not only of science but of the world. And why, pray? Because when science asks for her dole, the world replies, "But those great men, Smith and Jones, are proud to labor for nothing; why then should I pay you?" Alas, poor ignorant world does not know that if Smith and Jones are genuine workers they are probably too much engrossed in their toil to bestir themselves for payment; while if, as more often happens, they are merely purveyors of others' labors, then their lofty and popular pose is adopted for a purpose. And, indeed, snobbery is often a paying cult, and those who labor for nothing do little but frequently get much!

In science as in other things, the proper and honest procedure is to pay for work done; and, to be frank, the encouragement of science, of which we hear so much nowadays, must in the end come to this—or to nothing. And in science as in other things snobbery is a false pose which brings only contempt upon those who adopt it.—*Science Progress.*

PROCEEDINGS OF THE NATIONAL ACADEMY OF SCIENCES

THE third number of Volume 3 of the *Proceedings of the National Academy of Sciences* contains the following articles:

The Condensation and Evaporation of Gas Molecules: Irving Langmuir, research laboratory, General Electric Company, Schenectady, N. Y. A discussion of the evaporation vs. the reflection theory with conclusions favoring the former.

The Ninth Satellite of Jupiter: Seth B. Nicholson, Mount Wilson Solar Observatory, Carnegie Institution of Washington. Comparison of the orbits of the Eighth and Ninth Satellites. The mean period of the Ninth is 745 days and its diameter is probably about 15 miles.

Aortic Cell Clusters in Vertebrate Embryos: H. E. Jordan, department of anatomy, University of Virginia. The hemogenic activity of embryonic endothelium is a normal function at a certain stage of embryonic development.

Rheotropism of Epinephelus Striatus Bloch: Hovey Jordan, Bermuda Biological Station for Research, Agar's Island, Bermuda. The lip region is the most sensitive part of the body surface. The end organs of tactile sensitivity serve also as organs of rheotropic sensitivity.

Studies of the Genus Phytophthora: J. Rosenbaum, Bureau of Plant Industry, Washington, D. C. A search for determining characters of diagnostic values in testing the different species.

A Possible Function of the Ions in the Electric Conductivity of Metals: Edwin H. Hall, Jefferson Physical Laboratory, Harvard University. A discussion of the number of ions necessary to maintain currents of great density, and of the temperature relations of conductivity if due to ions.

The Gravimetric Survey of the United States: William Bowie, Division of Geodesy, U. S. Coast and Geodetic Survey. A summary of the present status of the subject.

The Magnetization of Iron, Nickel, and Cobalt by Rotation and the Nature of the Magnetic Molecule: S. J. Barnett, department of physics, Ohio State University. A confirmation of the assumption that only electrons are in orbital revolution in all the substances investigated.