these fundamental assumptions. To define a quantity as a vector, and then conclude that the parallelogram law holds begs the whole question. The logical way to proceed would be to first *prove* that the quantity is a vector, that is, that the parallelogram law holds and then (advantageously) apply the principles of vector analysis. We can not prove, however, that a *force* is a vector. We must depend upon experience for our justification in assuming a force to be a vector.

We do not know what a force is. To say that "force is an action" explains nothing, and to define it as a vector begs the whole question. Experience and experience alone can justify us in dealing with forces as vectors of a certain kind. In other words, the "parallelogram law of forces" is nothing more than an assumption and is *not* a purely "geometric principle." If we *assume* that a force can be measured by the motion it produces, and if we *assume* that the effect of each force is independent of the effect of the other forces acting, then it follows that the parallelogram law holds also for forces, since we know that this law, as a consequence of the principle of independence, does hold for the motions (accelerations) produced. This argument, however, makes two assumptions. First, it assumes that a force can be measured by the acceleration it produces (in its own line of action), and, secondly, it assumes "the principle of independence" for forces. Now these two assumptions are involved in Newton's Second Law of Motion. In other words, the parallelogram law of forces is a consequence of Newton's Second Law of Motion, and, therefore, in its last analysis is an assumption. If, however, the parallelogram law is once assumed for forces, then it can be proved for moments and other (vector) qualities involving force. It is, therefore, sufficient to *assume* the law to hold for *forces*.

It is a question whether we have a right to assume the parallelogram law even for velocities and accelerations without proving it, and to assume it for forces is equivalent, as we have seen, to assuming Newton's Second Law of Motion.

In my criticism it was stated:

On page 102 he assumes that a force is proportional to the accelerations produced. This assumes Newton's Second Law.

In reply he says:

This statement is not quite right. The relation between force and acceleration which I have called *force-equation* is derived on page 106 from the fundamental principle which I have postulated. In this derivation I have made use of the definition of kinetic reaction which is stated and illustrated on pages 102 to 105, but this is not equivalent to assuming a new principle.

This is true as far as it goes, but he fails to add that the form of this "force-equation" depends upon the actual value of this "kinetic reaction" which he finds as the result of experiments to be equal to the mass times the acceleration produced, that is,

$$\text{Kinetic reaction} = mf.$$

He seems to me to be making a "distinction without a difference." At least he is making an assumption here that is equivalent to assuming Newton's Second Law of Motion.

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ACCESSORY CHROMOSOMES OF MAN

IN reply to Professor T. H. Morgan's statement in SCIENCE, June 5, 1914, I wish merely to request the reader who may be interested to read my note of May 15[1] and my paper, "Accessory Chromosomes in Man,"[2] and then Professor Montgomery's paper,[3] that he may decide for himself whether Montgomery and I have not agreed in the main regarding the accessory chromosomes of man. This was the only point at issue in my former communication, which was meant not as a "complaint," but as a correction to a misleading inference.

As to the material on which Montgomery and I came to different conclusions regarding a second pairing of the ordinary chromosomes, Professor Morgan is mistaken in stating that

1 SCIENCE.

2 *Biol. Bull.*, XIX., 4; September, 1910.

3 *Jour. Acad. Nat. Sci. Phila.*, XV., second series, 1912.

we obtained our results from "the same identical preparations." Montgomery never saw my preparations, nor I his. For a minor part of his work he used some material from the same individual I had worked on, but this material had been standing in alcohol some two years before he obtained it from me, so that it is to be expected that he would not get as clear-cut preparations as from freshly fixed material, to say nothing of the fact that fixation may have been unequal in different bits of the tissue.

Concerning the question of sex chromosomes in fowls, I may say that in my opinion the final word has by no means yet been said. I hope in the near future to contribute some further evidence in the matter.

M. F. GUYER

SCIENTIFIC BOOKS

Chemistry in America. Chapters from the History of the Science in the United States. By EDGAR F. SMITH, Blanchard Professor of Chemistry, University of Pennsylvania. Illustrated. New York and London, D. Appleton and Company. 1914. Pp. xiii + 354. Price $2.50.

In his preface the author says: "The writer has lectured for several years to his graduate students on the development of chemistry in the United States. A mass of material has been collected, most of which is not only interesting but valuable. Repeated requests have been made for the publication of these facts as a history of chemistry in the United States. To the writer's mind the information in his possession is not sufficiently complete to warrant such an important undertaking. The earliest endeavors of our country's scientists require even more careful and extended research."

The earliest contribution to chemistry from this country appeared September 10, 1767, in the *Transactions of the American Philosophical Society.* The title is "An Analysis of the Chalybeate Waters of Bristol in Pennsylvania." The author is Dr. John de Normandie. Liberal quotations from the article are given which show that the author used the balance. Then follow quotations from an article by James Madison, who was professor of chemistry and natural philosophy at William and Mary College as early as 1774, and from an article by Dr. Robert McCauslin. The author of the book thereupon remarks: "These communications testify to a spirit of inquiry, at least, on the part of our early devotees to science. They are, further, interesting in that they show the use of the balance as early as 1768 and indicate the steps of analysis."

In 1792 the Chemical Society of Philadelphia was founded by James Woodhouse. The fact is noted that the members of this society favored Lavoisier's doctrine of combustion.

According to Dr. Smith "the arrival of Joseph Priestley in America in 1794, and his frequent presence among the men of science of that day, greatly stimulated scientific studies." But Priestley's thoughts appear to have been on theological subjects fully as much as on scientific in these latter years of his life. He was elected professor of chemistry in the University of Pennsylvania in 1794 but felt obliged to decline the honor. In a letter to Dr. Rush in regard to this he says: "Nothing could have been so pleasing to me as the employment, and I should have been happy in your society, and that of other friends in the capital, and, what I have much at heart, I should have an opportunity of forming an Unitarian congregation in Philadelphia."

Thomas Cooper, professor at Dickinson College and afterwards at the University of Pennsylvania, was the first one to make metallic potassium in this country. He was also the editor of Thomas Thomson's "System of Chemistry." From 1820 to 1834 he was president of the College of South Carolina, "attaining distinction as an extreme advocate of the States' Rights doctrine during the nullification period."

Robert Hare, who was born in Philadelphia in 1781, was without doubt the most influential chemist of his time in America. In 1801, when he was only 20 years old, he communicated to the Chemical Society of Philadelphia a description of the oxy-hydrogen blowpipe which