readers to say whether this shows a spirit of fairness or a desire to suppress discussion. Does it even indicate an inclination to refuse 'articles which give a true account of what has been said against the American system?'

"So much the editor of the *Journal* could have inferred from the action of the *Gazette*. It is enough to raise at least a presumption that his correspondent's statement was untrue. But he prefers to assume that what the *Gazette* has rejected has been rejected for the purpose of suppressing the truth.

"As a mattor of fact the *Gazette* has rejected but one article on the subject of nomenclature. The article 'suppressed' by SCIENCE was rejected by us because it contained numerous objectionable personalities. In returning the MS. we took pains to inform the author that we objected only to the personalities, *not* to his opinion on nomenclature, and that if the personalities were eliminated the paper would be accepted. When the MS. was returned to the editor, however, it had been so greatly amplified that it would have filled at least thirteen pages of the *Gazette*. It was therefore returned to the writer with a request to condense it, and he was offered any space up to five pages (about the space required by the original paper), but he declined to alter the MS., and finally withdrew it.

"It is difficult to believe that a wish to be fair to what he is pleased to call 'the arbitrary dicta of certain American botanists' animates the utterances of the editor of the *Journal of Botany*. If it does it is at least curious that two scientific men should come to such opposite conconclusions upon the same facts as do Mr. James Britten and a strenuous but gentlemanly opponent whose name we withhold but whose voluntary words we are permitted to quote:

"'I have greatly regretted the ill-natured statements of J. Britten, especially those in which he implies that there has been any unfair suppression of opinion by the *Gazette*. I am confident that whatever has been rejected by the *Gazette* has been refused for the best reasons and for the sake of harmony and the best good of all concerned.'"

It would seem certain from the above that no attempt has been made either by SCIENCE or by the *Botanical Gazette* to suppress discussion of botanical nomenclature. Probably no American journal wishes to suppress discussion, but it is evidently impossible to accept everything presented, and but few journals would care to print an article such as that contained in the July number of the *Journal of Botany*.

J. MCKEEN CATTELL.

BLOOD EXAMINATION IN DISEASE.

THE suggestion of Prof. Le Conte that some notice be taken of articles in which statements are made that are liable to mislead, or that are absolutely erroneous, calls to mind an article in the Scientific American Supplement for May 4, 1895 (p. 16, 126), by Prof. John Michels, entitled "Does a nucleus exist in the red corpuscles of mammalian blood?" In it the following assertion is made:

"It is a remarkable fact that although a knowledge of blood is of such importance and probably the key to a perfect knowledge of the treatment of disease, little or next to nothing is known relating to its physiological properties, its constituents or its effects on the human economy in health or disease. No physician ever makes a microscopical examination of blood in making his diagnosis, and if he did, he would be unable to interpret the appearances he would notice, for there is no guide to the subject, the medical profession remaining under a cloud of ignorance in regard to this matter, and they appear to be content to wait to have this knowledge forced upon them by chemists and biologists, rather than make any effort on their own part to relieve their condition of disgraceful ignorance."

That there still remains much to be learned regarding the blood is undeniable. But that the medical profession is in a state of ignorance in regard to it, or that no one ever makes a microscopical examination of blood in making his diagnosis, is absolutely false. Since the discovery of the hematozoa of malaria by Laveran, in 1880, thousands of cases of malarial fever have been diagnosed absolutely by blood examination. All late books on the practice of medicine refer to this as a valuable aid to diagnosis in this disease. Dr. Wm. Osler, of John Hopkins University, who has made a special study of malarial diseases, can, perhaps, give Prof. Michel some information on this point.

So, too, in cases of anæmia. An examination of the blood will infallibly diagnose the

case, and all physicians in cases of doubt make this examination or have it made. Special instruments like the hæmacytometer of Gowers or Thoma, or the hæmaglobinometer of Gowers, have been made for this purpose and can be purchased from all dealers in microscopical instruments

The disease known as Filariasis can be and is diagnosed by blood examination. The parasites causing this disease occur in the immature state in the blood, passing, as they mature, into the lymphatics. These parasites are truly remarkable from the fact that they are found in the blood only at night, being almost or entirely absent in the daytime; if, however, the patient sleep during the day this is reversed, thus showing that the condition of sleep is an important factor in determining the presence the organisms.

From these facts it would seem that the medical profession is not in quite as 'dense' a state of ignorance regarding the blood as Prof. Michels would have his readers believe, and that they do make use of blood examination in the diagnosis of disease.

JOSEPH F. JAMES.

WASHINGTON, D. C., Sept. 4, 1895.

SCIENTIFIC LITERATURE.

The Science of Mechanics. A Critical and Historical Exposition of its Principles. By DR. ERNST MACH, Professor of Physics in the University of Prague. Translated from the Second German Edition by Thomas J. McCormack. The Open Court Publishing Co., Chicago.

The Science of Mechanics is an English translation of the German treatise by Professor Ernst Mach, on The Development of Mechanics; a work whose ability and importance entitle it to critical attention. While not a complete history of the science, it deals with the subject by the historical method and purports to be a philosophical discussion of the nature, origin and relations of those ideas and principles in mechanics which, when thus linked together, give an intelligible and comprehensive view of the science as it now is, and of the sometimes tortuous way by which it reached its present state. The book as a whole is unique, and is a valuable addition to any library of science or philosophy.

The author's well-known psychological bent is here directed to getting rid of metaphysical obscurities that befog the discussions of the seventeenth and eighteenth century physicists. He presents mechanics as a physical rather than a mathematical science, employing mathematics to some extent, necessarily, but with care not to make of a proposition in mechanics a mere peg on which to hang mathematical formulæ.

After a brief introduction, the work is arranged in a historical view of the development of the principles of statics, to which a hundred and twenty pages are devoted; then about an equal space is given in the same manner to dynamics, this being the order in which the science actually grew up. These divisions overlap somewhat, the former being carried well into the eighteenth century, while the latter begins with Galileo in the seventeenth century, but the order is, on the whole, very satisfactory.

Although the subject-matter of the first chapter may be of less immediate interest than that of the next, yet the author's treatment of it and his philosophical discussion of the early investigators' work and methods of working is most interesting, while the manner in which he shows how a principle has been employed *in essence* by one and another such investigator in its application to special and apparently unrelated questions, before some one makes the happy generalization that gives it the force of a law, is admirable. As one example among others, it is shown how the principle of virtual velocities was made use of by Stevinus in the sixteenth century, and later by Galileo, Torricelli and others before 'the universal applicability of it to all cases of equilibrium was perceived by John Bernoulli,' early in the eighteenth century.

"They that know the entire course of the development of science will, as a matter of course, judge more freely and more correctly of the significance of any present scientific movement than they who, limited in their views to the age in which their own lives have been spent, contemplate merely the momentary trend