

symmetry. This advance has not been without marked influence on the methods of determining the physical properties of crystals. The old idea to consider a crystal as a solid bounded by plane faces, the relative positions of which harmonised with Haüy's law of rational intercepts, is giving way to the more logical principle that a crystal consists of a homogeneous arrangement of discrete particles in space. Indeed, as has been frequently pointed out, a theory which ignores the internal structure cannot avoid the difficulty presented by a peculiar case of pseudo-trigonal symmetry. To the new school, which is typified most completely by Schönflies's well-known treatise, the present work belongs.

Dr. Sommerfeldt devotes a considerable portion of his book to the determination of the thirty-two classes of crystal symmetry. He establishes the four possible types of axes of symmetry in the usual way, and proceeds to evolve the classes in the following order:—the holohedral groups; the merohedral groups, comprising those possessing centres of inversion, those without such centres, but having mirror-image symmetry; and, lastly, those without such centres, and enantiomorphous. In the discussion a modification of the "Fundamentalebene" of Schönflies is introduced. It is the smallest spherical triangle defined by the elements of symmetry. The symmetry pertaining to each class and the shape of typical simple forms are clearly illustrated by means of the admirable plates, of which there is one for each class except that devoid of symmetry. After a brief discussion of the zonal law and the linear and stereographic projections, the author proceeds to what he considers not the least interesting portion of the book, namely, the application of the methods of vector analysis to crystallography. This form of mathematical analysis is undoubtedly graced by elegance, and presents the generalised formulæ in neat guise, but its unfamiliarity to the ordinary student of crystallography seriously militates against the general utility of the book. The formulæ in question—some of which, by the way, do not lend themselves readily to arithmetical computation, and are, therefore, not of immediate practical use—could be established without greater difficulty by means of ordinary analytical geometry. Nevertheless, to the advanced student who may be versed in mathematics it would be interesting and stimulating to study a different method. The book concludes with a very complete bibliography and a good index.

*Untersuchungen über künstlichen Parthenogenese und das Wesen des Befruchtungsvorgangs.* By Prof. Jacques Loeb. German edition, issued with the author's cooperation, by Prof. E. Schwalbe. Pp. viii+532. (Leipzig: J. A. Barth, 1906.) Price 7.50 marks.

THE greater part of this remarkable book appeared in English dress in the Decennial Publications of the University of Chicago, and has been already noticed in our columns. As is well known, Prof. Loeb set himself some years ago the task of discovering chemical or physical methods of stimulating development in unfertilised eggs. Taking every precaution which he could conceive of, he has been able to induce artificial parthenogenesis in the ova of sea-urchins, of the annelid *Chaetopterus*, and of the gasteropod *Lottia gigantea*. He thinks that the list will be added to as our mastery of the technique increases, for he does not believe that there is any essential peculiarity in those ova which develop in response to the artificial stimulation. As to the nature of the stimulation, Loeb is more and more convinced that it depends on setting-up or increasing

oxidation processes in the ovum, and also on the synthesis of nuclein substances from the protoplasmic materials. It is possible, he says, that the two processes are interdependent, and that oxidative syntheses take place. Everyone will wish more power to this ingenious experimenter's elbow in his untiring efforts to gain control of life.

*Handbook of Metallurgy.* By Dr. Carl Schnabel. Translated by Henry Louis. Vol. ii. Second edition. Pp. xvi+867; illustrated. (London: Macmillan and Co., Ltd., 1907.) Price 21s. net.

PROF. LOUIS is to be congratulated on the completion of the translation of the second edition of Dr. Schnabel's great work. Little delay has been experienced in placing it in the hands of English metallurgists, as the corresponding German edition was not published until 1904. The volume which has just been issued contains the metallurgy of zinc, and shorter sections on cadmium, mercury, bismuth, tin, antimony, arsenic, nickel, cobalt, platinum, and aluminium. As the first edition appeared nine years ago, there have been great advances in the metallurgy of some of these metals since it was written, and these have caused many alterations and a considerable enlargement in the present volume. The changes are distributed throughout, the whole text having been carefully revised, but some of the most striking changes occur in the sections devoted to the production of aluminium on a large scale and to the electrolytic treatment of zinc. Electrolytic methods generally are fully treated, the author expressing his indebtedness to the works of Dr. Borchers for much of this part of the book.

There is little to be said in criticism of Dr. Schnabel's book. The description of alloys is usually rather meagre, with curiously slight regard to the work of the last twenty years. Then, again, the rapidity with which the Silesian zinc furnace is giving place to the Belgo-Silesian furnace does not seem to be realised by the author. In general, however, the information is full, accurate, and up to date, and is conveyed in a pleasant, readable manner.

#### LETTERS TO THE EDITOR.

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#### The Inoculation Accident at Mulkowal.

I SHOULD like to direct the attention of your readers to this matter. The evidence regarding the unfortunate Mulkowal accident, as given in the *Lancet* and the *British Medical Journal* for February 2, and in the *Journal of Tropical Medicine* for February 1, shows that on October 30, 1902, nineteen persons were inoculated from a single bottle of Haffkine's prophylactic labelled 53N, while numerous other persons were inoculated from other bottles. A week later all the nineteen inoculated from bottle 53N developed tetanus, and subsequently died, while none of the others suffered at all. This gives a strong argument in favour of the view that the poison was associated with the contents of that particular bottle; but the evidence is clearly not mathematically absolute even on this point, while it gives no indication whatever as to when the tetanus bacillus entered the bottle. It might possibly have entered during the processes of manufacture and bottling, or later through a loosened cork, or in several ways during the opening of the bottle and the inoculation of the contents. But the commission that was appointed to consider the subject seems to have somewhat hurriedly adopted the conclusion that it actually entered during preparation, and not later. Mr. Haffkine, as head of the laboratory, was

blamed, especially because he had omitted, for good reasons, to add carbolic acid to the prophylactic. Great alarm was produced. The idea that the poisoning was due, not to local accident, but to carelessness at the laboratory, caused, I have been told, a sudden and wholesale rejection of the invaluable vaccine by the people, with the probable result that thousands of lives may have been lost from plague.

Now it appears that the tetanus bacillus could not have entered the bottle at the laboratory at all! I agree with Prof. Simpson (*British Medical Journal*, February 9) in thinking that the arguments on this point are extremely strong. Had the contents of the bottle been polluted at the outset, they would have had a very offensive smell when used some time later, and would have produced a very rapid infection in the inoculated. As a matter of fact they had no smell, and produced a slow infection, while bottles filled simultaneously were quite sound. Moreover, evidence has been given tending to show that the prophylactic was polluted during the opening of the bottle. On what grounds, then, were the laboratory and its director indicted? Even if the bacillus had entered during the complicated process of manufacture, the blame can hardly be attached to the director, who cannot himself superintend the preparation of each bottle. As for the omission of the carbolic acid, the inventor of the prophylactic was himself surely the best judge of how it was to be made.

The serious part of the affair seems to lie, not so much in the loss of life due to the accident itself, considerable as that was, but in the much greater loss which probably followed the suspicion thrown upon the prophylactic by the apparently erroneous judgment of the commission, and, more even than this, in a certain ingratitude shown in India to a man who is one of the very greatest benefactors it has ever had. Haffkine not only elaborated the method of immunisation by dead culture, but, where many a man of science would have contented himself with merely writing an article on the subject, he addressed himself, on the contrary, to the much more difficult practical verification. I well remember when he arrived in India with his anti-cholera vaccine and by his energy and perseverance gradually forced his ideas upon the people and the Government. When the frightful calamity of the plague overtook the country in 1896, largely, in my opinion, owing to the inadequacy of the sanitary organisation and to want of firmness and resolution in the authorities, when measure after measure failed and the people were dying by hundreds of thousands, then Haffkine was the only one who made any successful stand at all against the storm. Quickly inventing his anti-plague prophylactic and forcing the authorities along with him, though he could not control the disaster, he at least checked it by saving thousands, if not hundreds of thousands, of human beings, who now owe their lives solely to him. The fact that more than six million doses of the prophylactic have been issued in India alone attests the success and magnitude of his work. Yet he has received for it less than nothing. For services which compared with his are really of a trifling nature, all kinds of officials receive in many cases pensions, promotion, and decorations. As for him, not only has he received no adequate recognition for his immense service, but he has been blamed for an accident which could not have been due to his fault, and it is doubtful whether he will ever return to a country which has treated him—I can only say—so ungratefully. Contemplating this history, we cannot help being filled with indignation at it. India seems to be becoming quite notorious for its treatment of scientific workers, suggesting ignorance both of science and of the importance of science. I remember the persecution suffered by Colonel King as the result of his work on vaccination, the complete want of gratitude shown to Mr. Hankin for his great work on the prevention of cholera, and several similar cases. While all kinds of people climb easily into the seats of honour, it seems that the men of real merit are fortunate if only they can escape without censure.

I think I shall be excused for writing somewhat strongly on a subject on which I have long felt still more strongly, and on which I have reason to know many others feel as strongly as myself without being as free as I am to express

their opinions. It appears to me a foolish thing for a nation to treat great men as we have sometimes treated ours, and the case of Mr. Haffkine—to whom, as he is a foreigner, we are doubly bound to show national gratitude—seems to be a glaring example of such treatment. I hope that steps will be taken to press upon the India Office the need for a reconsideration of the affair; the reputation of the whole country is concerned in it.

March 19.

RONALD ROSS.

#### Mean or Median.

THE article by Mr. Francis Galton in your issue of March 7, entitled "Vox Populi," is exceedingly interesting, and the variations in the estimates of individual competitors afford an admirable instance of the advantage to be derived from the use of the weighbridge at live-stock markets in preference to buyers and sellers relying on their own judgments; but the letter raises several interesting points as to the theoretical treatment of statistical data, to two of which I should like to allude.

In the first place, as to bias. No doubt, in estimating carcass weights in such a competition as that referred to by Mr. Galton, each competitor judges as truly as he can. But has a butcher (buyer) had his judgment to any extent warped in the course of years through having constantly had to judge of the weight of a beast (when buying) so as to be on the safe side, and secure himself from loss in the event of its not cutting up so well as he anticipated? If so, it might be expected that buyers would have an instinctive tendency to under-estimate the weights of animals; and similarly farmers (sellers) might be expected to over-estimate. This tendency, on either side, should, of course, not be large, as constant intercourse between buyers and sellers has raised such transactions almost to the point of a fine art. I should therefore like to ask Mr. Galton whether he has any information showing the proportion of these 787 competitors who were farmers and butchers respectively. It is very interesting to observe, from the figures given, that the estimated weights at each decile are throughout the whole series invariably below the weights which might be anticipated from the normal law of error. This rather looks as if buyers were in a majority in this competition: a not impossible suggestion, since, although farmers doubtless attend such exhibitions in larger numbers than butchers, yet the latter would, in a weight-judging competition, probably be more numerous than the former, at least relatively, if not actually.

The second, and more important, point to which I desire to direct attention is the use of the median in this connection, and I could wish that Mr. Galton had also calculated the arithmetic mean of the 787 observations. I should, in fact, like to strike a note of hesitation in regard to the too general use of the median in preference to the mean. The former has several advantages, one of which is that it is a form of "average" which can be very readily calculated. It is also very useful in cases such as those referred to in Mr. Galton's letter in *NATURE* of the preceding week, where it is desirable to eliminate one or two "cranks" whose opinion might have undue weight among a relatively small number of other opinions—in cases, in fact, where the distribution of opinions is known to be very erratic. But is this the case here? I am not sure that Mr. Galton is quite right in regarding the present instance as a case of "vox populi" at all. It is to be remembered that the great bulk of the trade in English cattle—and consequently the determination of the price of our native beef—is the result of transactions such as the competition in question is intended to test. Cattle are practically sold by inspection, and the judgment of buyer and seller as to how much beef there is in a given ox is really much more a matter of skill than of popular judgment; their livelihood depends upon the accuracy of such judgments. In such circumstances, is the median a nearer approximation to the truth than the mean? Here the question could be answered by calculating the arithmetic mean. I have not the actual figures, but judging from the data in Mr. Galton's article, the mean would seem to be approximately 1196 lb., which is much closer to the ascertained weight (1198 lb.) than the median (1207 lb.).