which we find treated at considerable length. As to the arrangement of the chemical part, the method adopted in "Miller's Chemistry" of arranging the elements under the terms metals of the alkaline earths, &c., has been adopted, which is a very excellent method of arrangement for teaching purposes, as it allows of elements with similar properties being compared. There is evidently throughout the whole of the book a tendency to condense far too much into a small space. It would be an exceedingly difficult book indeed to be put before an *absolute* beginner. The explanatory part is reduced apparently as much as possible, although a great many facts are crammed in, certainly in good order; but still a beginner requires very much more explanation of facts than is to be found in this book. On that account, and being more an epitome of facts than explanations, especially in the chemical portion, it is scarcely possible to criticise it. The arrangement is very excellent and the details are well up to date. We notice that ozone has been put in in the form of an addendum: surely its position is closely in connection with oxygen. It is very liable in this position to be overlooked, or at any rate neglected, by a student. As there is such a considerable amount of attention given to the rare metals, especially vanadium, many of its compounds being detailed, it is somewhat surprising that davyum, though perhaps not yet absolutely settled, is not mentioned along with them. On looking carefully through the book, a number of points occur in which more explanation, or even an explanation of formulæ, would be very advantageous; but on the whole Mr. Watts is to be complimented on having produced a very complete, though certainly not quite elementary, manual on the science.

Arithmetical Chemistry. By C. J. Woodward, Birmingham and Midland Institute. (London: Simpkin, Marshall, and Co., 1884.)

THIS is almost a book of questions selected from the Cambridge and Oxford Local, University of London, Science and Art Department, and other examination papers. It is divided into headings on laboratory calculations, where, after an example of a volumetric or gravimetric analysis, a number of exercises and questions follows, and gas analysis with corrections of gases for pressure, &c., and determinations of vapour densities, specific and atomic volume, specific heat, calorific power, calorific intensity, kinetic theory of gases, and diffusion. The explanations are in most cases short and to the point, but the immense number of examples and exercises given tend to make it a "getting-up" book for examinations rather than a book to work with in the laboratory.

Experimental Chemistry. By J. Emerson Reynolds. Part III. (London: Longmans, Green, and Co., 1884.)

PROF. REYNOLDS, in the first and second parts of this little work, has departed somewhat from the usual method adopted in chemical books for junior students. The first and second parts deal entirely with the non-metals and their compounds, acids, &c.; while the third part is devoted to metals. It is divided into numbered experiments for the student to perform in rotation, and should be exceedingly valuable to medical and other students who have only a short time at disposal for practical chemistry. There is no attempt at systematic analysis, but the experiments are sufficiently logically arranged to enable a student who gives his attention to them to be able to perform any simple qualitative analysis. At the same time each experiment is very fully explained, and the reactions expressed in most cases with equations. Part III. is supplemented by a series of analytical tables at the end, which, however, are not very clear. They are certainly somewhat too complex for the class of student for which the book is intended. On the whole, however, it is a very excellent work.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Reply to Mr. Grubb's Criticisms on the Equatorial Coudé of the Paris Observatory

I HAVE just received the last number of the *Scientific Transactions of the Royal Society of Dublin*, containing a description of an instrument analogous to the equatorial constructed on a principle which I published in 1871 in the *Comptes Rendus*. Since it seems to me indispensable after I have studied Mr. Grubb's memoir to reply to it publicly, I beg you to insert this note in NATURE. In order to give greater importance to the modifications imagined by himself, the author, Mr. Grubb, submits the instrument imagined by me to a very severe criticism, and attributes very severe defects to it. There is no doubt that there is a considerable difference between the instruments in question. One of them, in fact, as experiments carried on during two years have proved, lends itself to the execution of all possible astronomical researches under the best possible conditions for securing precision, while the other, according to the description given in the publication above cited, renders impossible a very great part of the researches to which an equatorial is specially destined. Further on I shall insist on this difference, but I wish first of all to reply to Mr. Grubb's objections and to show that they are in all points contrary to the fact.

To give weight to his argument Mr. Grubb examines a case of the construction of an instrument of 27 inches aperture, and he anticipates in the construction the following difficulties, which he considers insurmountable:—(1) The optical difficulty of constructing a large plane mirror. (2) The practical difficulty of procuring a disk of the necessary dimensions. Mr. Grubb affirms that there is no glass-works capable of making a disk of glass so large. (3) The difficulty of moving a mirror of which the weight, according to Mr. Grubb's calculations, will be very nearly half a ton. (4) The dearness of the instrument, which would cost more than an ordinary equatorial, plus dome and observatory.

I will discuss these points one by one. (1) The construction of plane mirrors is a settled question nowadays. Many astronomers have been able to convince themselves that in the equatorial coudé of 11 inches aperture the introduction of a plane mirror of 16 inches in no way marred the definition. The brothers Henry, who constructed this mirror, have just made another of 40 inches diameter, which leaves absolutely nothing to be desired in this respect. This difficulty, moreover, is so little felt or feared by our opticians that the price of a similar mirror for an instrument coudé is only about a quarter of the price of an object-glass, in spite of the great difference of their respective surfaces. Thus, for an object-glass of 27 inches, the price of which is 70,000 francs, that of a plane mirror of 38 inches is 18,000 francs. For an object-glass of 40 inches, price 200,000 francs, the corresponding mirror of 58 inches only costs 40,000 francs. I admit, nevertheless, that in this respect Mr. Grubb may have had apprehensions. In the past, in fact, serious difficulties have been met with in the production of plane mirrors, but my own personal experience has enabled me to realise the real cause of this want of success. Up to the present time, to satisfy preconceived ideas it was believed that to establish rapidly an equilibrium of temperature it was necessary that the thickness of the mirror should be small. Then, under the influence of a tightening, however slight, or only a flexion, the mirrors were deformed unequally, and consequently produced an obvious alteration in the beauty of the images. The brothers Henry, studying the same question by different processes, have arrived at the same conclusions. In giving to the disk a sufficient thickness, the production of a plane surface is not more difficult than any optical surface whatever. The means of verification are so delicate that in a mirror of 40 inches diameter an error of 1/50,000 of a millimetre can easily be determined and eliminated. So if there be a sphericity in the mirror, the radius of curvature will have at least 1600 leagues, that is to say, about