into the hands of students who have but little time to spare and may not intend to become professional chemists, a very wide analytical field is got over; indeed a little too much is attempted in the space, and sacrifices have in nearly all cases to be made where "shortness and simplicity" is the combined ruling idea.

We fully agree with what the author says as to the educational value of quantitative analysis. It is indeed high time that our more elementary students should have the long courses of qualitative analysis shortened, and some more *exact* exercises substituted.

In the course of the 127 pages of this book, including six for tables, we are introduced to the balance, and it is much to be regretted that more has not been said about it. What is said is purely practical—how to turn up the handle and put on the weights.

The first exercises are the determination of water in a carbonate and the ash in several substances, after which a couple of specific gravity methods are given, and then we pass to "simple gravimetric analysis," iron, silver, barium, lead, &c. In the silver exercise the factor 0·75276 is introduced to get the actual silver from the weight of chloride found, and this "factor" is given in all other analyses. It is not of much use any way, and for beginners it is not advisable, as it binds them down to the book, and no appreciable time is saved for ordinary analysis calculations.

The directions for volumetric analysis are very good, and the exercises are well arranged in order of difficulty. The separation exercises and miscellaneous examples will need some attention from the teacher.

In the description of organic analysis—combustion of carbon compounds—the closed-tube process is well described, and a student might be able to do a combustion from the description only; but we are not informed, when the open tube is spoken of, whether the same length, viz. 18 inches, will be sufficient or not. By inference it will. We venture to say that a very doubtful analysis, especially of a volatile body, would result from the use of an open tube only 18 inches long. The description here is much too slight to work by.

The tables at the end are sensible—only just those wanted in the course of the work in the book itself.

Qualitative Chemical Analysis. By Dr. C. Remigius Fresenius. Tenth Edition. Translated and edited by Charles E. Groves, F.R.S. (London: J. and A. Churchill, 1887.)

THE fifteenth German edition of this well-known book contains many emendations and additions, especially in the concluding portions devoted to the reactions of the alkaloids and the systematic methods of detecting them. Of this edition of the original work the present edition of the English translation is as nearly as possible an exact reproduction, and much credit is due to the translator and editor for the care with which he has accomplished a very difficult task. Various styles of type and other typographical improvements have been introduced, in the hope, as Mr. Groves explains, that the book may thereby be rendered more handy and useful to students.

Melting and Boiling Point Tables. Vol. II. By Thomas Carnelley, D.Sc., and Professor of Chemistry in University College, Dundee. (Harrison and Sons, 1887.)

THE issue of vol. ii. of this important work completes it. It is not too much to say that these two volumes will be found in every laboratory. Their compilation represents an amount of patient work from which most men would have recoiled; and the total result, which has cost ten years of effort, reflects the highest credit upon Prof. Carnelley.

Part II., dealing with organic compounds, brings the data down to 1885.

Part III. deals with vapour tensions and boiling points of simple substances, and freezing and melting points of cryohydrates, including facts recorded in 1886.

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.*]

The Law of Error.

EVERYONE interested in the theory of statistics is aware how strongly Quetelet was under the conviction that there is only one law of error (or curve of facility, to use the corresponding expression for the graphical representation of the law) prevalent for the departure from the mean of a number of magnitudes or measurements of any natural phenomenon. I have done what I can to protest against this doctrine as a theoretic assumption; and recently Mr. F. Galton and Mr. F. Y. Edgeworth have shown in some very interesting and valuable papers in the *Philosophical Magazine* and elsewhere how untenable it is, and how great is the importance of studying the properties of other laws of error than the symmetrical binomial, and its limiting form the exponential.

I have been making some calculations recently, principally in the field of meteorology, and I should be extremely glad of the

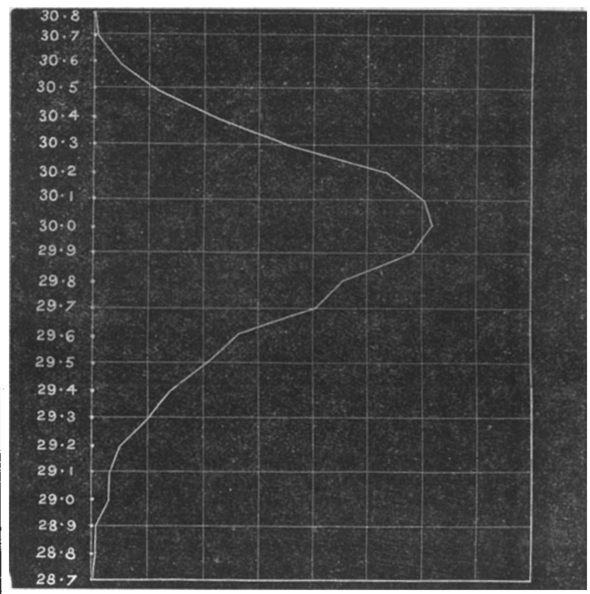

FIG. 1.

judgment and criticism of any of your readers who may be better versed in this science than myself. It must be carefully understood that the questions here raised are solely these:—(1) Do the magnitudes, when arranged in order of their departure from the mean, display a *symmetrical* arrangement? (2) If so, is this arrangement in accordance with the binomial or exponential law?

The first diagram represents the grouping, in respect of relative frequency, of 4857 successive barometric heights. They are from the observations of Mr. W. E. Pain, of Cambridge, and show the readings at 9 a.m. on successive mornings for about thirteen years from January 1, 1865. They are the results of the same instrument, which has required no correction or alteration during that period. They are given to the first decimal place.

The second diagram refers to a similar set of 4380 thermometric observations (1) of the maximum, (2) of the minimum temperature on successive days[1] from January 1, 1873.

In regard to the first diagram the asymmetry is obvious. I have tested the conclusion in the usual way. For instance, the total of 4857 observations was composed of seven batches of a little less than two years each. Precisely the same asymmetry, in varying degrees, is displayed by each of these batches. The asymmetry is of course obvious to the eye in the diagram, but various numerical tests may be proposed. For instance, we may compare (1) the position of the mean value (in this case 29·91) between the extreme values, (2) the relative positions of the maximum ordinate and the mean ordinate, (3) the comparative magnitudes of the "mean errors" to the right and the left of the mean ordinate. They all yield a result in the same direction.

I should be very glad if any of your readers could confirm (or correct) these results by those of more extended observations, or by results taken from other districts. That something of this kind should be displayed where, as here, we are dealing with a one-ended phenomenon—*i.e.* with one in which unlimited variation was conceivable in one direction but not in the other—seems to me in itself reasonable. But I was certainly surprised to find it so marked, considering how small is the fluctuation in relation to the actual magnitude of the variable phenomenon.

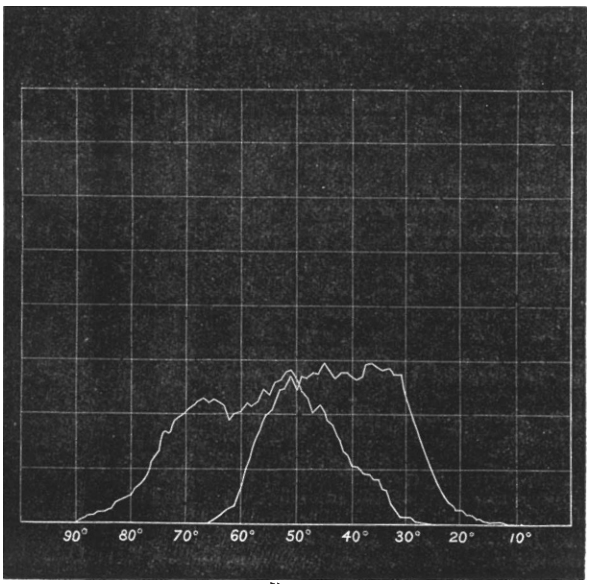

FIG. 2.

It seems to suggest that the common theoretic assumption of a sort of fixed mean or type which is swayed about by a large number of equal and opposite independent disturbing causes, does not hold good in this case.

As regards the second diagram, the two curves are (especially that of the minima) tolerably symmetrical, but they depart widely from anything approaching to Quetelet's supposed fixed type.

Anyone looking at the curve of maxima would say at once that it mingled the results of two distinct means (in Quetelet's phrase), as if we were to group together the observed statures of a great many Scotchmen and Frenchmen. That we are mingling results of distinct means seems true enough, but not of *two* such, and I cannot account for the two peaks in the curve. What I should have expected would have been something of this kind: Each day has its own appropriate mean maximum (subject to the usual fluctuation), and these mean maxima are themselves grouped about *their* mean, hence the true mean of all ought to be decidedly the commonest result, *i.e.* the curve should have a single vertex.

The facts are quite otherwise. The depression towards the centre is far too deep to be accidental, and the final mean (*i.e.* about 57°) is very far from being the commonest value.

Somewhat similar remarks may be made about the curve of minima. There is some evidence (though not conclusive) of a depression towards the centre in this case also, and the curve is very fairly symmetrical. But the true mean of all the minima cannot claim any numerical preponderance over any other value between 32° and 52°.

I am far too deeply conscious of the numerous pitfalls which lurk about the statistician's path to offer these results with any great confidence. But considering how large is the number of observations included, it certainly seems to me that they call for some explanation. There may of course be some blunder in the calculations, but I have done my best to guard against this. What I trust is that these results may be the means of calling forth some discussion by practised experts in this branch of statistical inquiry, which may serve to confirm or correct my results, and in the former case to offer some explanation of the causes of the phenomena. Very likely this practical inquiry has been already undertaken elsewhere, but the statistics of meteorology are so vastly extensive that it is impossible for any but a professional student of the subject to be acquainted with what goes on in it.

J. VENN.

Cambridge.

[1] In this case, as the lengths of the successive ordinates from the original data were very irregular, I have smoothed the curve out by taking the mean of three successive heights. For instance, to take the actual figures, the number of occasions on which the maxima were 58°, 59°, and 60°, were respectively 108, 99, and 124; I have assigned the number 110 to 59°, and so on.

The Sense of Smell in Dogs.

WILL Mr. Russell (whose letter in NATURE of August 4 I have just read) be so good as to make another experiment with his pug bitch? He says that she had been "taught to hunt" for biscuit; probably she was also enjoined to "*find it*," or something similar, when she came into the room. Can he manage to try her powers without awakening her expectation?

I ask it because it seems to me that in this case (and many others) we have something different to observe than mere quickness or keenness of sense, and something well worthy of observation; namely, exclusive direction of the attention of a sense—if I may so term it.

We may note this mysterious power in ourselves to a certain extent. In the case of a dog or bird, or any other in which there is little brain work going on to cause distraction, it may be much greater, and account for many wonderful things. It may be said that this is trying to explain the unknown by the even less known; nevertheless, by gathering together many and varied instances of the action of any power some light must be thrown upon it. The mesmerizer seems to deal with this one when he closes all avenues to the senses of his subject except the one he wishes to keep open.

The sense of hearing in some birds seems as wonderful and discriminating as that of smell in dogs. I have watched with astonishment a thrush listening for worms—as their manner is—and very evidently hearing them too, within two yards of a noisy lawn-mower on the other side of a small hedge of roses. Probably the worms came nearer to the surface in consequence of the vibration caused by the machine—they are said to do so—but that the thrush *heard* and did not *see* them was evident. Robins appear to be able to distinguish the voices of their own offspring and parents from a number of others, and at a great distance. I say *appear*, for in such a case one cannot be quite sure, still less can one give all the small details of long-continued observation that make up the evidence in favour of it.

All these cases have a common and mysterious element. It is as if a window were opened in one direction and all others closed; or a chord set vibrating that answers, as a struck glass answers, only to one note; or as if all the available energy were directed along one narrow path. At any rate there is something more than mere keenness of sense.

J. M. H.

Sidmouth.

Electricity of Contact of Gases with Liquids.

WILL you allow me to ask Mr. Enright (NATURE, p. 365) how he proved that the "charge of the escaping hydrogen was positive" or negative, as the case may be? That the escaping *spray* was electrified by friction, after the manner of the steam spray in Armstrong's old hydro-electric machine, is a natural explanation of these capricious effects; but that *gas* should be thus electrified, and that this electrification should have any relation whatever to the subject of "atomic charge," are propositions which strike one as improbable.

OLIVER J. LODGE.