jects. Besides several excellent series of examples, a very useful summary is added to each chapter, which will be serviceable for revision purposes. The adoption of different sizes and kinds of type, when it is, as here, carefully done, is also a very great boon to beginners.

The Slide-Rule. A Practical Manual. By Charles N. Pickworth, Wh.Sc. (London : Emmott and Co., Ltd., 1894.)

THE most modern form of slide-rule is of the Mannheim or Tavernier-Gravêt type, and undoubtedly surpasses its predecessors in many ways. At the present time this instrument is in general use on the continent, principally in France and Germany, and it is now becoming more popular in England.

The slide-rule may be defined as an instrument for mechanically effecting calculations by logarithmic computation. By its aid arithmetical, algebraical, and trignometrical processes may be performed much more quickly and with greater ease than by the ordinary methods, while the accuracy of the results are quite within the limits of error for all practical purposes. There is no doubt that when the instrument is better known, and its labour-saving property recognised, it will be more commonly seen in the laboratory and workshop than it is now. So many manipulations can be done with it that, without some guide, its full value cannot be appreciated. In the present little manual the author brings these all together, and in such a form that the reader can, by paying attention to the mechanical and mathematical principles, obtain an intelligent interest in the manipulations, and have confidence in the results.

W. J. L.

I Fondamenti Matematici per la Critica dei Risultati Sperimentali. Del Prof. P. Pizzetti. (Genova, 1892.)

AN elaborate memoir, of the nature of a complete treatise on the Method of Least Squares, in its application to the reduction to order of a long-continued series of experiments and of their numerical results.

It contains a valuable bibliography of writings on the subject, arranged alphabetically according to authors' names.

Hitherto the astronomer has made most use of this theory ; but the artillerist is now finding it important for his purposes, in calculating from the number of hits to effect a desired amount of destruction the amount of ammunition required. G.

Teppich-erzeugung im Orient. By various Contributors Pp. 204. (Wien : K. K. Österr.Handels-Museum, 1895.)

THIS work consists of a series of monographs on important antique tapestries contained in European museums and private collections, contributed by Sir George Birdwood, Mr. C. Purdon-Clarke, Mr. Vincent J. Robinson, Mr. S. J. A. Churchill, Dr. W. Bode, M. Gerspach, and M. O. M. Stoeckel. In addition to the history of antique tapestries, the work contains descriptions of a number of the more important types of modern tapestries of the Levant, Persia, and India. The illustrations are numerous and of high quality.

A Laboratory Manual. By W. R. Orndorff, A.B., Ph.D. (Boston : D. C. Heath. London : Isbister and Co., 1894.)

A COURSE of experiments in organic chemistry, systematically arranged as an adjunct to Prof. Ira Remsen's work on the "Compounds of Carbon." As Dr. Orndorff has had a large experience in the laboratory work to which the book refers, the conditions of the experiments described can be depended upon, which is the highest recommendation that can be given to a manual of this kind.

LETTERS TO THE EDITOR.

Finger-Prints.

I HAVE been quite unable, since I saw Mr. Faulds' letter in your issue of October 4, to take the matter of it in hand hitherto ; and I do so now only because I think Mr. Faulds is entitled to raise the question if he pleases. To the best of my knowledge, Mr. Faulds' letter of 1880 was, what he says it was, the first notice in the public papers, in your columns, of the value of finger-prints for the purpose of identification. His statement that he came upon it independently in 1879 (? 1878) commands acceptance as a matter of course. At the same time I scarcely think that such short experience as that justified his announcing that the finger-furrows were "for-ever unchanging."

How I chanced upon the thing myself in 1858, and followed it up afterwards, has been very kindly stated on my authority by Mr. Galton, at whose disposal I gladly placed all my materials on his request. Those published by him are only a part of what were available. (See his "Finger-Prints," page 27, and his "Blurred Finger-Prints.") To what is there stated I need now only add, at Mr. Faulds' request, a copy of the demi-official letter which I addressed in 1877 to the then Inspector-General of Jails in Bengal. That the reply I received appeared to me altogether discouraging was simply the result of my very depressed state of health at the time. The position into which the subject has now been lifted is therefore wholly due to Mr. Galton through his large development of the study, and his exquisite and costly methods of demonstrating in print the many new and important conclusions he has reached.

I take the opportunity, in reference to a late article on Anthropometry (in the *Nineteenth Century* of September 1894, p. 365), to deprecate, as being to the best of my knowledge wholly unproved, the assertion that the use of finger-marks in this way was "originally invented by the Chinese." I have met no evidence which goes anywhere near substantiating this. As a matter of fact, I exhibited the system to many passengers and officers of the P. and O. steamship *Mongolia* in the Indian Ocean, during her outward voyage in February 1877 ; and I have the finger-prints of her captain, and of all those persons, with their names. It is likely enough that the idea, which caught on rapidly among the passengers, may have found a settlement in some Chinese port by this route, and have there taken a practical form ; but whether that be so or not, I must protest against the vague claim made on behalf of the Chinese, until satisfactory evidence of antiquity is produced.

Littlemore, November 7. W. J. HERSCHEL.

(TRUE COPY OF OFFICE COPY.)

Hooghly, August 15, 1877.

MY DEAR B——.—I enclose a paper which looks unusual, but which I hope has some value. It exhibits a method of identification of persons, which, with ordinary care in execution, and with judicial care in the scrutiny, is, I can now say, for all practical purposes far more infallible than photography. It consists in taking a seal-like impression, in common seal ink, of the markings on the skin of the two forefingers of the right hand (these two being taken for convenience only).

I am able to say that these marks do not (bar accidents) change in the course of ten or fifteen years so much as to affect the utility of the test.

The process of taking the impression is hardly more difficult than that of making a fair stamp of an office seal. I have been trying it in the Jail and in the Registering Office and among pensioners here for some months past. I have purposely taken no particular pains in explaining the process, beyond once showing how it is done, and once or twice visiting the office, inspecting the signatures, and asking the *omlah*[1] to be a little more careful. The articles necessary are such as the *daftari*[2] can prepare on a mere verbal explanation.

Every person who now registers a document at Hooghly has to sign his "sign-manual." None has offered the smallest objection, and I believe that the practice, if generally adopted, will put an end to all attempts at personation.

[1] Clerks. [2] Man in charge of stationery.

The cogency of the evidence is admitted by every one who takes the trouble to compare a few signatures together, and to try making a few himself. I have taken thousands now in the course of the last twenty years, and (bar smudges and accidents, which are rarely bad enough to be fatal) I am prepared to answer for the identity of every person whose "sign-manual" I can now produce if I am confronted with him.

As an instance of the value of the thing, I might suggest that if Roger Tichborne had given his "sign-manual" on entering the Army on any register, the whole Orton case would have been knocked on the head in ten minutes by requiring Orton to make his sign-manual alongside it for comparison.

I send this specimen to you because I believe that identification is by no means the unnecessary thing in jails which one might presume it should be. I don't think I need dilate on that point. Here is the means of verifying the identity of every man in jail with the man sentenced by the court, at any moment, day or night. Call the number up and make him sign. If it is he, it is he ; if not, he is exposed on the spot. Is No. 1302 really dead, and is that his corpse or a sham one? The corpse has two fingers that will answer the question at once. Is this man brought into jail the real Simon Pure sentenced by the magistrate? The sign-manual on the back of the magistrate's warrant is there to testify, &c.

For uses in other departments and transactions, especially among illiterate people, it is available with such ease that I quite think its general use would be a substantial contribution towards public morality. Now that it is pretty well known here, I do not believe the man lives who would dare to attempt personation before the registrar here. The *mukhtears* [1] all know the potency of the evidence too well.

Will you kindly give the matter a little patient attention, and then let me ask whether you would let me try it in other jails?

The impressions will, I doubt not, explain themselves to you without more words. I will say that perhaps in a small proportion of the cases that might come to question the study of the seals by an expert might be advisable, but that in most cases any man of judgment giving his attention to it cannot fail to pronounce right. I have never seen any two signatures about which I remained in doubt after sufficient care.

Kindly keep the specimens carefully.

Yours sincerely,
W. HERSCHEL.

Boltzmann's Minimum Function.

MR. CULVERWELL, in his letter to NATURE of October 25, questions the existence of Boltzmann's minimum function, and asks will somebody tell us what the H-theorem really proves?

As I have made use of the theorem on several occasions, I may be permitted to say a word in its defence. I will endeavour to answer Mr. Culverwell's question what the theorem proves for the simple case of equal elastic spheres. If I can do that, it will probably not be difficult to generalise the result.
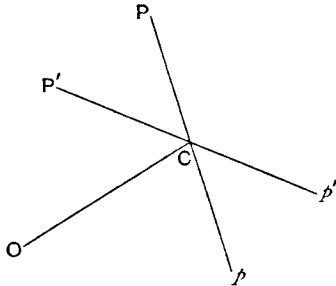
Let then V, or OC in the figure, denote the velocity of the common centre of gravity of two elastic spheres, each having diameter c. Let R be their half relative velocity. If we describe a spherical surface with radius R about centre C, and if Pp be any diameter of it, the actual velocities of two spheres are OP and Op.

Let the number per unit of volume of spheres whose velocities are represented by lines drawn from O to points within the element of surface dS at P be denoted by FdS. Let fdS denote the corresponding number for the element dS at p. Then FfdS is the number of pairs whose relative velocity R falls within the cone

[1] Attorneys.

described with solid angle dS about PCp as axis. Let P'Cp' be any other diameter, and let F'dS', $f'dS'$ be the corresponding number of spheres with velocities OP' and Op'.

If a pair of spheres collide the relative velocity assumes, as the result of collision, a new position only, and what that position shall be depends on the coordinates of the collision, *i.e.* the point in which a line parallel to the relative velocity through the centre of one sphere cuts a circular area of radius c, drawn through the centre of the other sphere at right angles to that line. If the collision coordinates be taken at random, then the following condition holds, viz. :—For any given direction of R before collision, all directions after collision are equally probable. Call that condition A.

Now assume condition A to be fulfilled, and consider all collisions which take place between pairs of the V R spheres.

The number which after the collisions belong to the class FfdS will be on the above assumption $\dfrac{dS}{4\pi}\int\int F'f'dS'$.

But before the collisions it is FfdS. Therefore, as the result of collisions it is increased by $dS\left(\dfrac{\int\int F'f'dS'}{4\pi} - Ff\right)$. That is by $\dfrac{dS}{4\pi}\int\int (F'f' - Ff)dS'$, F$f$ being treated in the integration as constant.

Therefore

$$\frac{dF}{dt} = \frac{df}{dt} = \frac{\pi c^2 R}{4\pi}\int\int (F'f' - Ff)dS'$$

and if

$$H = \int\int f(\log f - 1)dS,$$

$$\frac{dH}{dt} = \frac{\pi c^2 R}{4\pi}\int\int dS \int\int (F'f' - Ff)\log(Ff)dS'$$

$$= \frac{\pi c^2 R}{4\pi}\int\int\int\int dSdS'(F'f' - Ff)\log(Ff)$$

$$= \frac{\pi c^2 R}{4\pi}\int\int\int\int dSdS'(Ff - F'f')\log F'f' \text{ by symmetry}$$

$$= \tfrac{1}{2}\frac{\pi c^2 R}{4\pi}\int\int (F'f' - Ff)\log\frac{Ff}{F'f'}dSdS',$$

which is necessarily negative if not zero. The above is true for all values of V and R, and therefore for the whole system.

Thus we have proved that if condition A be satisfied, then if all directions of the relative velocity for given V are not now equally likely, the effect of collisions is to make H diminish.

The objection that I understand to be made is that if you reverse all the velocities after collisions, the system will retrace its course with H increasing—which is supposed to be contrary to the thing proved.

The objection is wrong because in your reverse motion condition A is not fulfilled. The proof (is not wrong but) ceases to be applicable by failure of the condition on which it is based.

Somebody may perhaps say that by this explanation I save the mathematics only by sacrificing the importance of the theorem, because I must (it will be said) admit that there are, after all, as many cases in which H increases as in which it diminishes. I think the answer to this would be that any actual material system receives disturbances from without, the effect of which, coming at haphazard, is to produce that very distribution of coordinates which is required to make H diminish. So there is a general tendency for H to diminish, although it may conceivably increase in particular cases. Just as in matters political, change for the better is possible, but the tendency is for all change to be from bad to worse. S. H. BURBURY.

1 New Square, Lincoln's Inn, November 12.

The Kinetic Theory of Gases.

I CANNOT quite agree in Mr. Bryan's remembrance of what took place in the discussion of the Thermodynamics Report at Oxford. As far as I recollect, Prof. Boltzmann's reply was not in special reference to such a point as the specific heats of gases, but was in answer to a very vigorous, if somewhat general, onslaught of Prof Fitzgerald, who simply stated that it appeared evident from the spectra of gases and other considerations, that the energy could not be equally divided among all the degrees of freedom of the coordinates, and said what he wanted to know from Prof. Boltzmann was *when the theory*