when I was examining all the investigations I could find on the subject, after looking through Lagrange's memoir (and reading carefully Todhunter's *résumé* of it), I came to the conclusion that it contained nothing that conld, properly speaking, be regarded as an anticipation of the later investigations of Gauss, Laplace, &c., and I contented myself therefore with merely a passing reference.

Lagrange's paper, as its title implies, gives a mathematical justification of the choice of the mean of a series of discordant observations, and a determination of the chance that the resulting error lies between certain limits, with developments, &c.; but the method of Least Squares may be described as an extension of the principle of the arithmetic mean to the combination of linear equations, involving more than one unknown; the problem being to obtain the best values of the unknowns from a series of discordant *linear simultaneous equations.*

The method of Least Squares was first proposed in print by Legendre in his "Orbites des Comètes" (Paris, 1805), as a convenient way of treating observations without reference to the Theory of Chance. Legendre's words are "la méthode qui me paroît la plus simple et la plus générale, consiste à rendre minimum la somme des quarrés des erreurs . . . et que j'appelle Méthode des moindres quarrés. The method, regarded from a practical point of view, is a very natural one; we shall clearly get a good result by determining the quantities to be found so as to make the sum of the $2n$th powers of the errors a minimum, and in order that the resulting equations may be linear (and therefore manageable), we must take n equal to unity.

Though first published by Legendre, the rule was applied by Gauss, as he himself states, as early as 1795, and the method is explained and the usual law of facility for the first time found in the "Theoria Motus Corporum Cœlestium, Hamburgh, 1809 (not 1808, as in Prof. Hall's letter). The principle on which Gauss proceeds may fairly, I think, be stated as follows:—If there are given a number of discordant observations V_1, V_2, &c., of a quantity x, so that we have the equations $x - V_1 = 0$, $x - V_2 = 0$, &c., then it is known that a very good result is obtained by giving to x the arithmetic mean of its observed values, and writing $x = \frac{1}{n}(V + \ . \ . \ . + V_n)$; and it is required to find an equally good rule for determining x, y, z, &c., from a number of discordant equations of the form $a_1x + b_1y + c_1z + \ . \ . \ . = V_1$, $a_2x + b_2y + c_2z + \ . \ . \ . = V_2$, &c. Assume therefore that $x = \frac{1}{n}(V_1 + \ . \ . \ . \ V_n)$ is the most probable value of x derived from the first system of equations, and find the law of facility of error that this may be the case; then, having this law, the most probable values of x, y, z, &c., can be found for the second system.

The law of facility Gauss finds to be represented by $\frac{h}{\sqrt{\pi}} e^{-h^2x^2}dx$, viz., this is the chance of an error of magnitude intermediate to x and $x + dx$; and thence it follows that the most probable values of x, y, z, &c., are found by making $(a_1x + b_1y + c_1z + \ . \ . \ . - V_1)^2 + (a_2x + b_2y + c_2z + \ . \ . \ . - V_2)^2 +$, &c., a minimum. Gauss then proceeds to determine h in the manner still generally adopted.

Subsequent writers, Laplace, Poisson, &c., have in consequence investigated how far the arithmetic mean is the most probable result, &c., and in one sense Lagrange (and *a fortiori* Simpson) may be said to have very slightly anticipated a portion of the analysis required in these researches, although, as far as the method of Least Squares is concerned, there is no anticipation. A slight examination will show how greatly superior Laplace's analysis is to Lagrange's on the same subject.

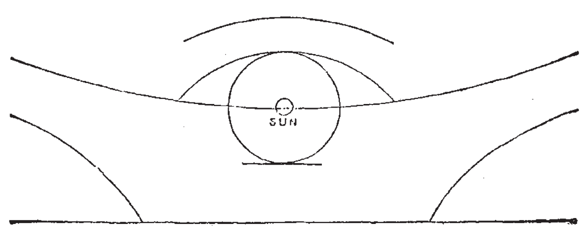
With reference to the independent discovery of the method of Least Squares by Dr. Adrain of New Brunswick, U.S. (see Prof. Abbe's note in the *American Journal of Science*, June 1871), I may remark that if for distinction we call the introduction of the merely practical use of the rule its "invention," and its philosophical deduction by the Theory of Probabilities its "discovery" (so that Legendre invented the method and Gauss discovered it), then Dr. Adrain can only be credited with the independent invention of the rule, viz., he only did what Legendre had done two years previously. This is worth noticing, as from the occurence of the function e^{-x^2} in Dr. Adrain's paper, it might be supposed that it contained some anticipation of Gauss' investigation; but such is not the case, and Dr. Adrain's reasons for the adoption of the law are of so trivial a nature that it is incredible that any mathematician should have been led to the discovery of the method by means of them. I imagine that he had noticed the practical convenience of the rule, and subsequently endeavoured to justify it analytically; it may be noted that it is possible that Dr. Adrain may have seen or heard of Legendre's memoir published two years before; his silence on the matter, however, renders it unlikely that this was so. On the whole, by far the greater part of the merit of the introduction of the method is due to Gauss; while the credit of the first suggestion of the practical rule must be assigned to Legendre, Dr. Adrain having, in all probability independently, also suggested the same rule subsequently. It is necessary to be thus particular, as Gauss' publication having taken place in 1809 and Adrain's in 1808, it might be thought that the latter had anticipated the former to some extent, which is in no wise the case.

In writing the history of the Theory of Errors or the Theory of the Treatment of Observations, there are several memoirs anterior to Legendre's that would have to be included, and notably Thomas Simpson's "Miscellaneous Tracts," 1757 (which is the work Prof. Hall doubtless refers to), Daniel Bernoulli's "Dijudicatio maxime probabilis plurium observationum discrepantium," &c. Acta. Petrop. 1777, Trembley's paper in the "Berlin Memoirs," 1801, "Observations sur la méthode de prendre le milieu entre les observations," &c. For the above references I was indebted to Todhunter's "History of the Mathematical Theory of Probability from the time of Pascal to that of Laplace" (London, 1865), which contains a notice of every work or memoir on the subject to the commencement of the present century (there is a *résumé* of Lagrange's memoir occupying 13 pages), so that no one need have any fear of passing over any writings published previously to 1800. Having had occasion to make much use of the work, I may be permitted to say that its value, both as regards accuracy and completeness, cannot be over-estimated. J. W. L. GLAISHER

Trinity College, Cambridge, June 8

Solar Halos

A BEAUTIFUL combination of solar halos was visible here during the morning of March 2. At 10·45 the sun having an altitude of about 40° was surrounded by a complete rainbow-tinted circle of some 18° or 20° radius, red inside and blue outside. An arc of a larger circle coloured in the same way touched the complete circle at its highest point, rendering the point of contact dazzlingly bright. A short arc touched the lowest point



of the circle in the same manner. A white halo passed through the sun's position parallel to the horizon, and two fainter white arcs intersected it obliquely in the point opposite to the sun, forming a conspicuous sun-dog. There were also two rainbow-arcs having their convexities toward the sun. These were blue inside and red outside, and their centres appeared to be about 90° from the sun, and some 15° below the horizon. Later an arc concentric with that touching the complete circle appeared above it, having the colours reversed, namely, blue inside and red outside. These appearances lasted about an hour and a half before beginning to fade away. W. W. J.

Gambier, Ohio, March 5

The Volcanoes of Central France

THE Auvergne volcanoes threaten to be as periodic a subject of controversy as the authorship of the letters of Junius. It is only seven years since the last eruption of letters. At that time I contributed apaper to the *Geological Magazine* (vol. ii. p. 241), in which I collected, printed, and translated all that I could find on the subject, and came to the conclusion that it was very probable there had been some local outbreak of volcanic action. Thus I agree with Mr. Garbett, but it appears to me that he has not