visible to us, whether we compare direct measures of the Solar disk or observed periods of axial rotation.

The contraction, therefore, is so small, that as much allowance can be made for the assumptions introduced above as may be thought necessary, without altering our general conclusion in the slightest degree, namely, that the source of the Solar heat and light is connected with the mechanical theory of heat by means of the contraction of the composing mass.

The application of this theory to other bodies is almost without limit; the Earth has contracted, and has stored up a corresponding amount of heat in the non-conducting rocks and soils; the stars, by their intrinsic brilliancy, indicate the operation of the force of gravity upon contracting matter; the nebulæ afford examples of the commencement of this operation; and periodical variations in light now become perturbations, the effect of disturbing masses in motion, producing endless changes subject to the great principle known as the conservation of energy.

January 1872.

On the Insufficiency of Existing National Observatories.
By Lieut.-Col. A. Strange, F.R.S.

The Astronomer Royal, in a recent paper* on the "Proposed devotion of an Observatory to observation of the phenomena of Jupiter's Satellites," introduced his remarks in these words:— "The position which the Royal Astronomical Society holds in the astronomical world may well justify it in employing its judgment and its influence in the direction of astronomical enterprise exterior to its own body."

Adopting Mr. Airy's plea, I now propose to invite the attention of the Society to some further consideration of the question he has raised, as to the sufficiency of existing National Observatories.

These establishments were organised at a time when the objects of astronomical research were comparatively few, readily enumerated, and easily defined. The Royal Observatory was founded in the interests of navigation, and though that purpose has been very liberally interpreted, and much of the work done has had far higher aims, still, on the whole, it has been, for the most part, of a routine, observational character.

But within a comparatively recent period, the science of astronomy has made enormous advances in directions not contemplated when Greenwich was founded. And it has been for some time clear, that Greenwich cannot, as at present constituted, be expected to contribute systematically to the advancement of the new branch of astronomy to which I allude, namely, what is

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now generally recognised as the Physics of Astronomy. I have spoken of Greenwich as the type, and the highest type, of National Observatories; but if we turn to the Report of the Council of the present year, which gives us the latest account of the operations of all such observatories as can be considered in any sense official, we shall find the same characteristic prevailing, namely, a nearly total exclusion of physical work.

I do not state this in order to reflect disparagingly on the proceedings in question. With respect to the Royal Observatory of Greenwich, indeed, there can be no doubt that its strict adherence for so long a period to a well-marked system of almost purely meridional observation, has conferred incalculable benefits on astronomy, and I wish to express my humble tribute of admiration of the wise self-denial which has characterised its administration, and which has been proof against the allurements of researches, the pursuit of which must have militated against the primary functions of the Institution, namely, that of laying, extending, and conserving the very foundations of the science.

Still the fact must be plainly recognised, that whilst one branch of astronomy is provided for in the most efficient and permanent manner, the other branch is left literally to chance. And the question I wish now to ask is, whether this is a satisfactory state of things; and if not satisfactory, whether the first astronomical body in the world cannot do something to amend it? I know that this question is very likely to raise a controversy as to the value of private exertions, and as to the risk of damping individual enterprise by the establishment of new and powerful official institutions. I therefore, at the outset, beg to express my great respect for the labours of those who have devoted their means and time, gratuitously, to the cultivation of science in whatever department. I should be very sorry to see the spirit of individual research, in which Englishmen exceed all other people, discouraged by the action of the community; but I have no fear of such a result. I believe that by a proper classification of objects, there will be left an ample field for private observers, even should official astronomy be very much extended.

It appears to me that this classification should be based on the principle that researches which involve long-continued, systematic labour, should be undertaken by permanent establishments. I would give, as a simple example of such researches, the study of the Sun—a study which, in all probability, will never come to an end. In such a study, well-considered measures, strictly and uninterruptedly persevered in for a long period of years, are absolutely indispensable. It is certain that we cannot count upon such extended continuity from private energy, which however supreme it may be, must die with its possessor.

On the other hand, the work which may be expected from volunteer astronomers consists chiefly in opening out new fields, and in devising new processes of inquiry. I may here again cite the same example of the Sun, the systematic study of which has been rendered possible by the personal exertion and genius of men too well known to us all to need enumeration. But the work is now fast getting too heavy for private resources. The mapping of the Moon is another example of work which individuals have shown us how to do; but which individuals can never complete within any reasonable period.

My present object is to draw attention to the general question of the insufficiency of existing astronomical establishments, in order to elicit opinions on it. The details of any new scheme will be matters for future consideration. At present, I shall content myself with stating my opinion that permanent national provision for the cultivation of the Physics of Astronomy, is urgently needed. If the study of the Sun only were in question, that alone would, I think, justify such a measure; for there can hardly be a doubt that almost every natural phenomenon connected with climate can be distinctly traced to the Sun as the great dominating force, and the inference is unavoidable, that the changes, and what we now call the uncertainties of climate, are connected with the constant fluctuations which we know to be perpetually occurring in the Sun itself. The bearing of climatic changes on a vast array of problems connected with navigation, agriculture, and health, need but be mentioned to show the importance of seeking in the Sun, where they doubtless reside, for the causes that govern these changes. It is indeed my conviction, that of all the fields now open for scientific cultivation, there is not one which, quite apart from its transcendent philosophical interest, promises results of such high utilitarian value as the exhaustive, systematic study of the Sun.

The present seems to me an opportune time for agitating this question, for two reasons. In the first place, it has been recently announced that the famous series of Solar photographic researches conducted by Messrs. De La Rue, Balfour Stewart, and Loewy, have been brought by circumstances to a close. The changes daily in progress on the Sun's surface are now therefore no longer systematically depicted by photography anywhere in the British empire. The results obtained, as recorded by the three above-named astronomers in the *Transactions* of the Royal Society, are of such striking interest and importance, that any prolonged loss of continuity in the series will be a most deplorable circumstance. No time, therefore, should be lost in averting such an evil.

In the second place, it is, no doubt, generally known, that a Royal Commission, of which the Duke of Devonshire is Chairman, has been for about two years engaged in a twofold inquiry into "Scientific Instruction and the Advancement of Science." The first branch of their inquiry is completed, and they are about to enter on the second, namely, the "Advancement of Science." We may feel very sure that the present generation will not see so comprehensive an inquiry undertaken a second time, and that any conclusions this Commission may now come to will govern the principles of State action with regard to science for many years to come. The opportunity, therefore, which is now open to the Astronomical Society to make any recommendations which in the present state of the science may seem to it advisable, is one which for a long period may not again be presented to it.

On the Law of Facility of Errors of Observations, and on the Method of Least Squares. By J. W. L. Glaisher, B.A., F.R.A.S., F.C.P.S., Fellow of Trinity College, Cambridge. (Abstract.)

The *American Journal of Science and Arts* for June 1871, contains a note by Professor Cleveland Abbe, the object of which is to point out that Professor Robert Adrain, of New Brunswick, published the method of Least Squares in 1808, having been independently led to its discovery. It is well known that all the proofs that have been given of the method of Least Squares, contain, to say the least, some points of difficulty, and on this account any new investigation of the result is necessarily a matter of much interest. Although some of the investigations of the law $e^{-h^2x^2}$ are far from rigorous, still there is not one that is not of some importance, as throwing additional light on the properties of this law, so that a fresh investigation, and one moreover, by which the law, not previously known to the author, was discovered, might be suspected to be a real addition, or at all events, confirmation of the known processes. Dr. Adrain's investigation is therefore examined at some length; but such is not found to be the case; the reasoning being of so slight and inconclusive a nature as to lead one to believe that, like Legendre, Dr. Adrain first remarked the convenience of testing equations by the method of Least Squares, and subsequently endeavoured to justify this rule by the Theory of Probabilities. After noticing the way in which Legendre originally presented the method, the other investigations that have been given are examined in detail, being grouped as follows:—

1. Gauss's original investigation, including Encke's, De Morgan's, and Leslie Ellis's remarks on the principle of the Arithmetic mean.
2. Laplace's Method, including Poisson's and Ellis's Simplifications, and Ivory's Criticisms.
3. Gauss's Second Demonstration, and its connexion with Laplace's.
4. Sir John Herschel's proof, with Ellis's and Boole's Criticisms thereon.
5. Professor Tait's, and similar proofs (Quetelet's, Hagen's, &c.)
6. Donkin's Proof.

Besides the above, four investigations were given by Ivory of the method, of which only one is referred to in detail, the others having been discussed by Ellis. Memoirs by Bessel and Mr. Crofton are also referred to.

The principle of the arithmetic mean, regarded either as an axiom or a result of demonstration, is examined, and the author believes them to be untenable. Laplace's method, as extended by Ellis, is restated with some alterations, and Poisson's general theorem given in the *Connaisance des Tems* for 1827, is proved, starting at once from a multiple integral, which is evaluated by means of Lejeune-Dirichlet's principle. The awkward transition from finite to infinitesimal errors in the course of the analysis is thus avoided. The exact sense in which Laplace's system of factors gives the most probable values of the quantities to be determined, is examined in some detail, and attention is drawn to the case taken by Laplace, when the facilities $\phi_1(x)$, $\phi_2(x)$ are supposed known, and the most probable values of the unknowns are to be found; and the actual case when in effect $\phi_1(x)$, $\phi_2(x)$ as well as the most probable values, are to be found. The one case is, given the observations and weights, find the values; the other, given the observations, find the weights and values.

The method of Least Squares gives in fact only the first approximation to the true values, which, however, enables us to weigh the observations and so obtain a second approximation, and so on. A criticism like Professor Pierce's is false in principle: it may certainly be better to reject an observation entirely, than to give it a weight equal to that of the best; but a proper extension of the method of Least Squares would give an abnormal observation a very small weight. Reasons are given why Laplace's first view of the subject seems much preferable to his second (the disadvantages in which are considered), and therefore preferable to Gauss's second demonstration also. The two views of the matter taken by Laplace (in his first investigation), and by Gauss, lead to a theorem in multiple integrals, which is verified. The final result of the discussion of all the proofs of the method that have been given is, that the only correct and philosophical view of the subject is that which regards an error as formed by the aggregation of a great number of smaller errors, due to independent sources, and subject to arbitrary laws of facility, whence by Laplace's analysis (though it was not so applied by its author), we have generally the law $e^{-h^2x^2}$ for individual actual errors, and the rule of Least Squares follows at once from this law, so that if the number of observations be large, we have, as it were, a double reason for the method. The paper concludes with one or two curious conclusions that would follow if the law of facility were $e^{-m\sqrt{x^2}}$, a form which not only seems very natural, but which was actually assumed to be the true one by Laplace in one of his earlier memoirs.

The Solar Eclipse of December 12th, 1871.
By Mr. Tebbutt.

The delicate state of my health prevented me from joining the expedition for observing the total eclipse at Cape Sidmouth; but I availed myself of the instrumental appliances at Windsor, for the observation of the partial phase. It unfortunately happened that the evening of the 11th was cloudy, so that no observations could then be taken for the determination of the mean time at the Observatory. The following morning, however, broke clear and fine, and I succeeded some hours after sunrise in observing the transits of two stars of the first magnitude. These observations in connexion with others taken in the evenings of the same and the following day, served for the determination of the chronometer errors and rate. A strong N.W. wind prevailed throughout the day of the eclipse, and broken clouds passed rapidly across the sky during the forenoon. With the exception of two or three small fleecy clouds, which passed almost instantaneously over the disc, and that without appreciably obscuring it, the Sun was bright and unclouded throughout the phenomenon. There were several spots on the Sun, but they were not of a very remarkable character. A group of small spots was to be seen on the eastern limb, involved in an immense tract of faculæ. The first contact appeared to take place at $2^h\ 18^m\ 39^s{\cdot}1$, owing, however, to a momentary removal of the eye from the telescope, this phase of the eclipse must have been observed somewhat late, probably to the extent of two or three seconds. The Sun's limb was remarkably steady and sharply defined. A very small sun-spot was observed to disappear at the Moon's edge, at $2^h\ 40^m\ 44^s{\cdot}5$, and at $2^h\ 57^m$, the same spot was again seen immediately after it had emerged from the Moon's limb. The most conspicuous spot on the disc was bisected by the Moon's limb at $3^h\ 28^m\ 41^s{\cdot}9$. Up to $3^h\ 30^m$, the images were very steady, and the cusps sharply defined; there being no rounding-off of the latter at the extreme points. At this time, however, the Sun's limb began to boil, which disturbance increased rapidly at $3^h\ 36^m$. The large conspicuous spot was again bisected at $3^h\ 37^m\ 24^s{\cdot}4$, while emerging from the Moon's limb. At $3^h\ 43^m$, the limb approached very near to the large tract of faculæ, but did not pass over it. During the early progress of the eclipse, the mountain-peaks and ranges along the edge of the Moon were beautifully distinct against the solar background. I believe I never before saw them to such advantage. Towards the end of the eclipse the images became more steady. The last contact took place at $4^h\ 4^m\ 38^s{\cdot}3$. The most interesting circumstance perhaps in connexion with the eclipse was the effect which the phenomenon had on the vacuum solar thermometer, suspended freely in the Sun's rays. From the beginning of the eclipse the thermometer gradually sank till about the time of greatest obscuration, when it began to rise, and continued to do so till the end of the phenomenon. The