experiments recorded in No. 89 of the 'Proceedings of the Royal Society' (vol. xv. p. 339).

The effect on the free acidity of the urine was inconsiderable. The free acidity may have been a little increased in the brandy period, but the change was slight.

The effect on the chlorine was not certain, as its ingress was not sufficiently constant, but it seems to be lessened in the exercise period.

As the action of alcohol in dietetic doses on the elimination of nitrogen and on the bodily temperature is so entirely negative, it seems reasonable to doubt if alcohol can have the depressing effect on the excretion of pulmonary carbon which is commonly attributed to it. It can hardly depress, one would think, the metamorphosis of tissues, or substances furnishing carbon, without affecting either the changes of the nitrogenous structures or bodily heat. It seems most important that fresh experiments should be made with respect to its effect on carbon elimination, as without a perfect knowledge on that point the use of alcohol as an article of diet in health cannot be fairly discussed.

IV. "Report on Scientific Researches carried on during the Months of August, September, and October, 1871, in H.M. Surveying-Ship 'Shearwater.'" By WILLIAM CARPENTER, LL.D., M.D., F.R.S. Received June 13, 1872.

[This paper will appear in full in a future Number of the 'Proceedings.']

June 20, 1872.

Sir JAMES PAGET, Bart., D.C.L., Vice-President, in the Chair.

Prof. William Grylls Adams, Dr. Andrew Leith Adams, Dr. John Cleland, Dr. Michael Foster, Prof. William Stanley Jevons, and Dr. William James Russell were admitted into the Society.

The following communications were read:—

I. "Preliminary Note on the Reproduction of Diffraction-gratings by means of Photography." By the Hon. J. W. STRUTT, M.A. Communicated by Prof. G. G. STOKES, Sec. R. S. Received May 23, 1872.

During the last autumn and winter I was much engaged with experiments on the reproduction of gratings by means of photography, and met with a considerable degree of success. A severe illness has prevented my pursuing the subject for some months, and my results are in consequence still far from complete; but as I may not be able immediately to resume my experiments, I think it desirable to lay this preliminary note before the

Royal Society, reserving the details and some theoretical work connected with the subject for another opportunity.

It is some years since the idea first occurred to me of taking advantage of the minute delineating power of photography to reproduce with facility the work of so much time and trouble. I thought of constructing a grating on a comparatively large scale, and afterwards reducing by the lens and camera to the required fineness. I am now rather inclined to think that nothing would be gained by this course, that the construction of a grating of a given number of lines and with a given accuracy would not be greatly facilitated by enlarging the scale, and that it is doubtful whether photographic or other lenses are capable of the work that would be required of them.

However this may be, the method that I adopted is better in every respect, except perhaps one. Having provided myself with a grating by Nobert, with 3000 lines ruled over a square inch, I printed from it on sensitive dry plates in the same way as transparencies for the lantern are usually printed from negatives.

In order to give myself the best chance of success, I took as a source of light the image of the sun formed by a lens placed in the shutter of a dark room. I hoped in this way that, even if there should be a small interval between the lines of the grating and the sensitive surface, still a *shadow* of the lines would be thrown across it. Results of great promise were at once obtained, and after a little practice I found it possible to produce copies comparing not unfavourably with the original. A source of uncertainty lay in the imperfect flatness of the glass on which the sensitive film was prepared, though care was taken to choose the flattest pieces of patent plate. The remedy is, of course, to use worked glass, which is required in any case if the magnifying-power of a telescope is to be made available.

Almost any of the dry processes known to photographers may be used. I have tried plain albumen, albumen on plain collodion, and Taupenot plates. The requirements of the case differ materially from those of ordinary photography, sensitiveness being no object, and hardness rather than softness desirable in the results. After partial development, I have found a treatment with iodine, in order to clear the transparent parts, very useful. In proceeding with the intensifying, the deposit falls wholly on the parts that are to be opaque. It is more essential that the transparent parts should be quite clear than the dark parts should be very opaque.

The performance of these gratings is very satisfactory. In examining the solar spectrum, I have not been able to detect any decided inferiority in the defining-power of the copies. With them, as with the original, the nickel line between the D's is easily seen in the third spectrum. I work in a dark room, setting up the grating at a distance from the slit fastened in the shutter, and using no collimator. The telescope is made up of a single lens of about thirty inches focus for object-glass, and an ordinary eyepiece held independently. I believe this arrangement to be more efficient than a common spectroscope, with collimator and telescope all on one stand; at any rate, the magnifying-power is considerably greater, and it seems to be well borne.

I have also experimented on the reproduction of gratings by a very different kind of photography. It will be remembered that a mixture of gelatine with bichromate of potash is sensitive to the action of light, becoming insoluble, even in hot water, after exposure. In ordinary carbon printing the colouring-matter is mixed with the gelatine and the print developed with warm water, having been first transferred so as to expose to the action of the water what was during the operation of the light the hind surface. In my experiments the colouring-matter was omitted, and the bichromated gelatine poured on the glass like collodion and then allowed to dry in the dark. A few minutes' exposure to the direct rays of the sun then sufficed to produce such a modification under the lines of the gratings that, on treatment with warm water, a copy of the original was produced capable of giving brilliant spectra. In these gelatine-gratings all parts are alike transparent, so that the cause of the peculiar effect must lie in an alternate elevation and depression of the surface. That this is the case may be proved by pressing soft sealing-wax on the grating, when an impression appears on the wax, giving it an effect like that of mother of pearl. It is known that the effect of water on a gelatine print is to make the protected parts project in consequence of their greater absorption, but it might have been expected that on drying the whole would have come flat again. It is difficult to say exactly what does happen; and I am not even sure whether the part protected by the scratch on the original is raised or sunk. Gelatine can scarcely be actually dissolved away, because the uppermost layer must have become insoluble under the influence of the light. I do not at present see my way to working by transfer, as in ordinary carbon printing.

I have not yet been able to reduce the production of these gelatine-gratings to a certainty, but can hardly doubt the possibility of doing so. One or two of considerable perfection have been made, capable of showing the nickel line between the D's, and giving spectra of greater brightness than the common photographs. Not only so, but the gelatine copy surpasses even the original in respect of brightness. The reason is that, on account of the broadening of the shadow of the scratch, a more favourable ratio is established between the breadths of the alternate parts.

Theory shows that with gratings composed of alternate transparent and opaque parts the utmost fraction of the original light that can be concentrated in one spectrum is only about $\frac{1}{10}$, and that this happens in the first spectrum when the dark and bright parts are equal. But if instead of an opaque bar stopping the light, a transparent bar capable of retarding the light by half an undulation can be substituted, there would be a fourfold increase in the light of the first spectrum. I accordingly anticipate that the gelatine-gratings are likely to prove ultimately the best, if the conditions of their production can be sufficiently mastered.

With regard to the application of the photographs, I need not say much at present; it is evident that the use of gratings would become more general if the cost were reduced in the proportion, say, of 20 to 1, more particularly if there were no accompanying inferiority of performance.

The specimens sent with this paper are both capable of showing the nickel line and give fairly bright spectra, but they must not be supposed to be the limit of what is possible. From their appearance under the microscope I see no reason to doubt that lines 6000 to the inch can be copied by the same method, a point which I hope shortly to put to the test of experiment.

II. "On the 26-day Period of the Earth's Magnetic Force." By J. A. Broun, F.R.S. Received June 3, 1872.

The Astronomer Royal's communication to the Royal Society on this subject (*suprà*, p. 308) has drawn my attention to the investigation made by the Director of the Prague Observatory.

Dr. Hornstein having remarked the uncertainty of the result for the time of the sun's rotation as deduced from the movement of the spots in different zones on its surface, thought it would be desirable to consider other phenomena associated with the sun's rotation; and the apparent connexion of the frequency of the solar spots with the amount of the magnetic oscillations induced him to seek for a period in the daily mean values of the magnetic elements. For this end he grouped the daily means of observations made at Prague and Vienna in 1870 in periods varying from 16 to 28 days; and subjecting the resulting means to calculation for the term

$$a \sin(\theta + c)$$

in the usual formula of sines, he considered the most probable of the periods to be that for which a had the greatest value.

The most carefully calculated result from the declination at Prague gave 26·7 days nearly, but a graphic interpolation from the same observations indicated 26·2 days; the declination for the same year at Vienna giving by calculation 26·4 days, while the inclination at Prague showed 26 days.

Dr. Hornstein concludes that the mean of these four values, "26·33 days, may be considered provisionally as the most probable value [of the period], and as the result of the first experiment to determine the time of the sun's synodical rotation by means of the magnetic needle. The true time of the sun's rotation derived from this = 24·55 days, almost exactly coinciding with the value found by Spoerer from astronomical observations for the time of rotation of the sun's spots in the equatoreal zone" *.

In a letter to the late Sir David Brewster, published in the 'Philosophical

* Sitzungsb. der k. Akad. d. Wissensch. zu Wien, Band lxiv. S. 73.