The digestion experiments offer nothing of special interest in this connection, and we pass at once to the experiments upon the nutritive value of the digested cellulose. These were so arranged as to compare the effect of the latter with that of an equal weight of sugar in two respects: 1°, as to its influence upon the proteid metabolism of the body; and, 2°, as to its influence upon the gain or loss of fat.

The influence of carbhydrates in the food, as is well known, is to decrease the proteid metabolism, as is shown by the diminished excretion of nitrogen in the urine. In v. Knieriem's experiments, 22 grams of crude fibre, of which 11.02 grams were digested, decreased the proteid metabolism by 22%, while 11 grams of cane-sugar decreased it 15.3%: in other words, the digestible crude fibre showed itself more effective in this respect than an equal weight of sugar.

As regards the gain or loss of fat, the advantage is on the side of the sugar; the latter diminishing the daily loss from the body by 2.5 grams, while the cellulose decreased it by 1.7 grams.

These are the results of a single experiment, and, as regards exact numerical values, are of course subject to correction by future investigations. They certainly show, however, that the nutritive value of cellulose is by no means insignificant, and probably not very much below that of other carbhydrates. If, as in the former article, we assume that the heat evolved by the fermentation of the cellulose in the alimentary canal is of use to the organism, then the sole loss by the fermentation is the latent energy carried off in the marsh-gas evolved. In that paper the amount of that loss was estimated on the basis of Henneberg and Stohmann's determinations of the amount of marsh-gas excreted in their respiration experiments. If, instead of this, the amount of marsh-gas evolved in the fermentation of one gram of cellulose be made the basis of the calculation, a somewhat lower value for the cellulose results. According to Tappeiner, one gram of cellulose yields 0.335 grams CO_2, 0.047 grams CH_4, and 0.618 grams of organic acids. One gram of cellulose yields 4,452 cal.; 0.047 grams CH_4, 614 cal.: leaving 3,838 cal. to represent the available heat-value of the cellulose. One gram of cane-sugar yields 4,173 cal.; one gram of starch, 4,479 cal.: consequently, if our fundamental assumption is correct, the value of one gram of cellulose is about 92% of that of cane-sugar, and about 86% of that of starch. These results agree well with those of v. Knieriem's experiments; and the two together appear to justify the conclusion, previously stated, that the nutritive value of cellulose is not greatly inferior to that of other carbhydrates.

H. P. ARMSBY.

The naval observatory publications.

Referring to your criticism in *Science* for April 3, on the delay in printing annual volumes of 'Astronomical and meteorological observations' made at the U. S. naval observatory, I am glad to be able to say that the cause for complaint in this direction has been, at least temporarily, removed; and in future we hope to have our volumes printed as fast as the limited number of computers will permit the proof-sheets to be sent to the printer.

During the closing days of congress, the following resolution was introduced and concurred in: "That the annual volume of the 'Astronomical and meteorological observations' of the naval observatory for the years 1881 and 1882 be printed, and that 2,000 additional copies of each volume be printed, of which 400 copies will be for the use of the senate, 800 for the use of the house, and 800 for the use of the navy department, or for sale at the cost of paper and printing."

The manuscript sheets of the volume for 1881 are now in the hands of the printer, to be followed immediately by those for 1882; so that both of these volumes will be distributed this year, and it is hoped that we will continue to be able to have (as you very pertinently suggest) all annual volumes printed independently of the regular appropriation for the navy department.

ALLAN D. BROWN,
Commander, U. S. navy.

U. S. naval observatory, Washington, D.C.,
April 6.

An attempt to photograph the corona.

Mr. Pickering's interesting experiments described in *Science* for April 3 would seem to be practically conclusive as to the unreality of the coronal forms which appear upon the plates of Dr. Huggins and Mr. Woods, if it were evident that he had observed all the conditions which they indicate as essential.

His letter, however, is silent in respect to one important point. It is not stated whether or not the plates were 'backed' with any light-absorbing substance, in order to prevent the so-called 'halation' produced by reflection from the back surface of the plate under a strong light. The English observers insist urgently upon the necessity of this precaution, and use for the purpose, I believe, a coat of asphalt varnish, colored with Brunswick black. It is possible that even this expedient would not wholly prevent a streaky scattering of light at the edge of the sun's image, because minute particles of foreign matter embedded in the glass itself would have their influence; but it is obvious, that, if the experiment was tried without the precaution, it cannot be looked upon as any way decisive.

Perhaps Mr. Pickering would kindly supplement his communication by a brief statement regarding this point.

C. A. YOUNG.

Princeton, N.J., April 8.

In reply to Professor Young's communication, I would say that the precaution to which he refers was carefully attended to, and that all the plates employed were backed the day before the eclipse with asphalt varnish. It would be very interesting to know how far the corona, as photographed by Dr. Huggins, extends from the sun: for a very long exposure would probably mask the real phenomenon; one that was very short would be insufficient to obtain an impression of it. My exposures were so timed, that, by a long development, the darkening could be traced to a distance of .8 of the sun's diameter, while, with a short development, the darkening only reached to .2. But in no case could any particular rays be identified in the different photographs.

WM. H. PICKERING.

Sir William Thomson's Molecular dynamics.

As it is possible that some of your readers may have obtained copies of the papyrograph report of my lectures on molecular dynamics, delivered at Baltimore during October, 1884, I should be obliged by your giving publicity to the following corrections: —

P. 34, lines 18 and 19, delete 'We may call it a dynamax but not a paradox.' I have no recollection of, nor can I imagine, what the word was that I suggested as more logical than 'paradox.'

P. 59, line 14, for 'distortional' substitute 'condensational.'

P. 296, in the two expressions for ψ, given in equation (17), insert 'tan i' before $\frac{(\mu^2-1)^2}{\mu^2+1}$; also in the expression for 'tan e' and 'tan e_1,' of equation (20), insert 'tan i' before $\frac{(\mu^2-1)^2}{\mu^2+1}$. The formula from which these expressions are deduced is correctly given at the foot of p. 295.

P. 296, in line 13 from top of the page, and in the left-hand members of equations (20) and (21), for 'w' and 'w_1,' read 'ω' and 'ω_1' respectively.

William Thomson.

The university, Glasgow, March 26.

The cold weather of February and March.

During the past two months the cold weather has been of unusually long duration; so much so, that in many places in and about the city the water and gas pipes, which are placed about four feet under the ground, have been frozen. This being the case, I have thought that it would be interesting to see, from the records of Draper's continuous self-recording thermometer of this observatory, what was the difference in the duration of the cold in this year, as compared with last. The following table shows the comparison of temperature every ten degrees, from the lowest to the highest, for the years 1884 and 1885, during the months of February and March, and also the number of times or hours the temperature was below or above 30°, which has been taken as a temperature of neither freezing nor thawing.

Degrees.	1884. Hours' duration.		1885. Hours' duration.	
	February.	March.	February.	March.
−10 to 0	–	–	2	–
0 to 10	14	11	44	5
10 to 20	30	71	191	139
20 to 30	97	105	250	157
Hours of cold	141	187	487	301
30 to 40	375	223	155	362
40 to 50	152	225	30	62
50 to 60	28	102	–	19
60 to 70	–	7	–	–
Hours of heat	555	557	185	443

Hours of cold, in 1885, for February	487	
Hours of cold, in 1885, for March	301	788
Hours of cold, in 1884, for February	141	
Hours of cold, in 1884, for March	187	328
Difference of hours of cold between the two years		460

There were therefore, during these two months, 460 hours more of cold in 1885 than in 1884.

Daniel Draper, Ph.D.,
Director.

CIVIL AND ASTRONOMICAL TIME.

There seems to be a good deal of doubt whether the recommendations of the Prime-meridian conference are going to be very generally accepted. France, and the nations under French influence, certainly will not adopt the new anti-Greenwich meridian for many years, if ever. The matter is really one of comparatively little importance; that is to say, it will make no very great practical difference to any one if different nations continue to use different meridians: still there can be no question that there would be a real and considerable convenience in the establishment of a single meridian, and consequently of a time-system, which, like our present railroad-time in the United States, would be identical as to minutes and seconds all over the earth. It is probable that the gentle pressure of this convenience will, after a while, bring about the desirable concurrence, especially as the increasing extent and rapidity of travel and communication will all the time bring out more forcibly the inconveniences of the present state of affairs, and tend to weaken mere local feeling and prejudice, which, after all, is the main obstacle at present to the universal adoption of the meridian proposed.

The recommendation that astronomers should come into agreement with other folks, and begin their day at midnight instead of the following noon, as at present, seems especially likely to fail. The Greenwich observatory, indeed, adopted the new plan on Jan. 1; but, so far as we know, no other important astronomical establishment has yet done so. Commodore Franklin, of the U. S. naval observatory, proposed to follow the example of Greenwich, and issued an order to that effect; but it excited so much opposition from certain eminent and influential astronomers, that the order was suspended before the time came for it to go into operation.

The objections of Professor Newcomb, who has formulated more fully and forcibly than any one else the reasons why the change should not be made, relate not so much to the fact that astronomers would find it inconvenient to change the date of their observations at midnight, as to the confusion that would be likely to result in the combination and comparison of observations taken before the introduc-