and this product by the price of sterling exchange, in United States money (E), or $S \times d \times E$, and uses the computed denominator 106.560. The value of a legal-tender dollar and of other silver coins is obtained by other denominators given, — thus, for the dollar, $\frac{d \times E}{387.372}$. On the 6th of August, with silver worth 42d. per ounce in London, our silver dollar was worth in gold bullion 71.21 cents, our trade dollar (full weight), 75.505 cents, and our subsidiary coin, 68.7 cents to the dollar.

'Recent results in the sorghum sugar industry' was the title of a paper by Dr. Peter Collier, of Washington. Numerous comparisons were made between tests of sugar-cane and sorghum, favorable to the latter as a sugar-producing plant. As an illustration, 72 approved varieties of sugar-cane grown upon Governor Warmouth's plantation in Louisiana being examined, averaged 185 pounds of available sugar to the ton of cane. Similar examinations of sorghums by Dr. Collier and Professor Wiley, at the U. S. department of agriculture, including over one hundred varieties, showed the available sugar, per ton of cane, ranging from 177 to 199 pounds. The sorghum also, on the average, produced a lower per cent of glucose and of rejected solids than the sugar-cane, this being also in its favor. As a rule, sorghum yields a less product per acre than cane, but the cost of cultivation per acre is enough less to more than compensate. The great cost of an acre of cane is well known, while sorghum costs not over ten per cent more than a crop of Indian corn of the same area. Chemical results and the manufacture of sorghum sugar, both on an experimental scale and commercially, in Kansas and New Jersey, are such, to date, as to offer every encouragement to this industry. Dr. Collier thinks the record justifies his prediction of the production of sorghum sugar in this country, in the near future, at a cost not exceeding one cent a pound. Dr. Collier also presented, in the form of graphical charts, with brief verbal explanations, 'Statistics relating to the dairy industry.' Compiled from official figures, these charts conclusively disprove the claim that agricultural land and labor, live stock and products, including butter, have suffered depreciation at all disproportioned to the recent general skrinkage in values, because of the introduction of oleomargarine and other butter substitutes and imitations. On the contrary, the number and value of milch cows in this country, and of their pure products, are steadily increasing; and there is now more and better butter made and consumed in America than ever before, while its price, compared with most food products, has been strikingly well sustained.

'The theory of rent, and its practical bearings,' was discussed by Edward T. Peters of Washington, and with such communistic leanings as to meet little approval.

Mrs. John Lucas, of New Jersey, entered a paper upon silk culture, which was received and assigned a place on the programme, but the author failing to appear at the appointed time, the paper was read by title only.

PROCEEDINGS OF THE SECTION OF MATHEMATICS AND ASTRONOMY.

SO MANY important papers were presented in this section, that we cannot even mention them all. Professor Rogers presented two papers, one on the best form of chronograph, and the other, with Anna Winlock, on 'The limitations in the use of Taylor's theorem for the computation of the precessions of close polar stars.'

The next paper was by Professor Doolittle, of Lehigh university, upon a 'Change in the latitude of the Sayre observatory.' In 1877 Professor Doolittle made a zenith-telescope determination of the latitude of this observatory. Nine years later, he now brings forward a new determination of the same latitude, from the same pairs of stars (fifty-seven in number), with about the same number of observations, the two pieces of work being done with the same instrument, by the same observer, and as nearly as possible under exactly the same conditions. No two equally thorough and equally comparable pieces of work with the zenith-telescope have ever been offered as evidence for or against a change in latitude, and the result is interesting. The difference of the two latitudes comes out

$$\phi_1 - \phi_2 = +0''.393 \pm 0''.063,$$

when the probable error of the declinations is used in the weight-coefficients in each case. Or, since the results may be assumed practically free from the errors of declinations, the result is

$$\phi_1 - \phi_2 = +0''.393 \pm 0''.045.$$

In the remarks that followed, Professor Newcomb stated that to him it only meant that in one or both of these series of observations there was — as with every observer and every instrument — some source of small systematic error which 'no fellow could find out.' Mr. Woodward, of the geological survey, an expert with the zenith-telescope, and also in questions of probable error, stated that in the absence of further observations he should hesitate to say that the observations themselves really indicated a real change of latitude.

Dr. Gould read a very interesting historical account of the early attempts at astronomical photography, showing that it originated in this coun-

try, and was for a time most actively pursued here, culminating in those beautiful photographs of the moon taken by Rutherfurd, as well as photographs of several double and multiple stars, and of the clusters Praesepe and the Pleiades. He told how Rutherfurd constructed a micrometer measuring engine, and obtained the first measures of the distances and position-angles of stars upon photographic plates, and how the work was received with considerable skepticism abroad. The speaker then described his own continuation of this same kind of work at Cordoba, and stated that he had brought home plates whose measurement would take a lifetime. Dr. Gould thought that he had the records of many 11th magnitude stars on his plates, the first photographs of such faint stars. Few of the plates were yet measured, and he was becoming solicitous about obtaining the necessary funds to proceed as rapidly as possible with this measurement, as he had detected a tendency, in some of the plates, of the collodion film to become detached from the plates.

A paper by Mr. E. F. Sawyer, entitled 'Some account of a new catalogue of the magnitudes of southern stars,' was presented. Mr. Sawyer has been observing the relative magnitudes of all the stars between the equator and — 30°, using an opera-glass with the stars slightly out of focus, and employing Argelander's method. Dr. Gould paid a high compliment to Mr. Sawyer's work, as did also Mr. Chandler.

A paper by Dr. Elkin, of the Yale college observatory, upon 'A comparison of the places of the Pleiades as determined by the Konigsberg and Yale college heliometers,' was presented by Professor Newton. The results given were provisional; but they show unquestioned change of position with reference to η Tauri since 1840. Most of the brighter stars of the group, as shown by Newcomb in his catalogue of 'standard stars' go with η Tauri, but among the smaller stars there are unquestioned departures from this community of proper motion.

In Monday's session a paper by Professor Abbe created some discussion. The point of the paper was, that, as the force of gravity varied from the equator to the poles, thirty inches of mercury in the barometer indicated a less gaseous pressure, and consequently less density of the atmosphere, at the equator than thirty inches at the poles, and hence a correction for latitude should be introduced in allowing for refraction. He showed that, for the difference of latitude of Pulkowa and Washington, it would make 0″.1 difference in the refraction at 45° of zenith-distance, and might be sufficient partly to account for differences in systems of star declinations which depended upon observations at great zenith-distances.

The most important paper in the section, and the one that attracted the most attention and discussion, was by Mr. Chandler, of Cambridge, upon 'A comparative estimate of methods and results in stellar photometry.' We have not space to do justice to this valuable and rather revolutionary paper, but we will try briefly to give its gist. Prefacing his remarks with the statement that it had long been known that small differences of stellar magnitude could be determined very accurately by Argelander's method of steps, by naked-eye estimates, but that it had been generally supposed that large differences could not be accurately so determined, and that the general idea had been that, as soon as photometry came generally into use, and so-called measurement took the place of estimation, a much more accurate scale of magnitudes, depending upon a true geometric light-ratio, would at once take the place of the old, the latter becoming obsolete, Mr. Chandler took for his text the general statement that instrumental photometry had thus far proved a failure; that is, it had not developed a more uniform scale of magnitudes than Argelander's, nor had the accuracy of individual determinations been increased, but they were, on the contrary, far more uncertain than the old differential naked-eye estimates. These statements he proceeded to back up with a convincing array of well-digested results, of which we can only give the briefest summary: 1°. For stars of Argelander's scale between magnitudes 2 and 6, the photometric catalogues of Seidel, Peirce, Wolf, Pickering, and Pritchard differed among themselves as much (or more) in their measures of what Argelander called a difference of one magnitude, as they did in their measures of his successive magnitudes. 2°. Their average values of the logarithm of the light-ratio (we will call it simply light-ratio hereafter, for brevity) for one of Argelander's magnitudes between 2 and 6, ranged between .30 and .38, about .35 for the mean of all the above-mentioned catalogues. 3°. Between magnitudes 6 and 9 of Argelander, the catalogues of Rosén and Ceraski averaged about .35 for the light-ratio, while Pickering's late results with his large meridian-photometer gave (between magnitudes 6 and 8.5) .48 instead of .35 for this ratio. 4°. To show the discrepancies in another way, assume a common light-ratio of .35 for all the photometers, and that their scales agree at magnitude 6. Then, for stars of the second magnitude, they will differ by 0.8 of a magnitude. That is, at a distance of four magnitudes away from where they agree, one photometer will say that the same star is twice as bright as another will. 5°. To test the uniformity of the different scales, all were referred to the average scale of all the photometers, and it was

shown that Argelander's scale in the 'Durchmusterung' was just as close to this as that of any single one of the photometers. 6°. Coming to accidental errors, Mr. Chandler showed that, from a full discussion of the naked-eye estimates of Gould, Sawyer, and himself, the probable error of a single estimate was a little over ±.06 of a magnitude when the stars were at considerable distances from each other, and about ±.05 of a magnitude when near; while the probable error of a single measure in the 'Harvard photometry' was ±.17 of a magnitude, and in the 'Uranometria Oxoniensis' about ±.10 of a magnitude, thus showing that the eye-estimates were from two to three times as accurate as the photometric. 7°. Discussing the cause of the large residuals in the 'Harvard photometry,' Mr. Chandler showed the strong probability of wrong identification of stars in many cases, citing one case where no bright star existed in or near the place called for by the observing-list, on account of a misprint in the 'Durchmusterung,' and yet some neighboring star was observed on several nights for it. 8°. Also the method of applying a correction for the mean value of the atmospheric absorption was very questionable, since overwhelming evidence pointed to an enormous difference in this absorption from night to night. 9°. The author pointed out that we must obtain better results from photometers if we ever expect to use their results for the detection or measurement of variable stars, since several variables have been detected, and their periods and light-curves well determined, by careful eye-estimates, whose whole range of brightness is no greater, or even less than, the range of error in the photometric observations upon a single star with the meridian photometer.

In a discussion of a paper by Mr. Barnard upon 'Telescopic observations of meteor-trains,' Professor Newton pointed out that the study of their drift was the only method we have of studying the upper currents of our atmosphere, except such rare catastrophes as the Krakatoa explosion.

The closing paper was by Mr. Chandler, 'On the use of the zenith-telescope for latitude.'

PROCEEDINGS OF THE SECTION OF BIOLOGY.

THE regular work of the biological section began on Thursday, and a partial classification of the papers into botanical and zoölogical added considerably to the interest and convenience of those present. Some have proposed a divison of the section of biology into botanical and zoölogical sections, but this, with a small meeting, seems hardly desirable, as there are apt to be only enough papers to occupy the time.

Among the first of the botanical papers was one by Prof. W. J. Beal, giving a comparison between the hygroscopic cells of grasses and sedges. In both grasses and sedges, as has long been known, there are one or more longitudinal rows of cells on each leaf, the function of which is to fold or close the blade in times of drought, and thus prevent too rapid evaporation of moisture from the surface. These rows of cells, as well as the cells themselves, vary in shape, size, and distribution in the different genera and species, and may have some value in the discrimination of critical species. The most interesting point brought out was, that many parallels exist between the genera of grasses and sedges in the arrangement of these hygroscopic, or, — as Professor Beal chooses to term them, — bulliform cells.

The paper of Messrs. J. M. Coulter and J. N. Rose, giving a synopsis of the North American pines, based on leaf-structure, had some points in common with the one just mentioned, and was of especial value from a systematic stand-point, from the fact that any species in this somewhat difficult group can at once be distinguished by the peculiarities of its minute leaf-structure; and the results of the author's observations are shown to be worthy of attention from the fact that a classification based on these characters is, in its broader features, closely like that of the late Dr. Engelmann, which, as is well known, took into consideration the whole tree.

The relations of germs to disease naturally occupied a prominent place in the proceedings of the section, and the presence of over half a dozen investigators in this line made the discussions interesting. Dr. D. E. Salmon read two papers bearing on the causes of immunity from a second attact of germ diseases. There are three possible explanations: 1°, something is deposited in the body during the attack which is unfavorable to the germ; 2°, something has been withdrawn which is necessary to its development; 3°, the tissues have acquired such a tolerance for the germ or for an accompanying poison that they are no longer affected by it. Dr. Salmon favored the last view, and gave details of a large number of experiments to substantiate his opinion. He said that Metchinkoff's phagocyte theory was not wholly satisfactory, and that large doses of the germs were more powerful than small ones. He attributed their action to a poison which was a result of their growth, and thought that a large dose had a greater effect because the poisons benumbed or killed the cells, thus giving the bacteria a better chance to grow and to thus produce more poison.

Dr. Joseph Jastrow gave an account of some