real. Of the 6000 nebulæ hitherto discovered we know nothing of the spectrum of more than 300 or 400, while the observation of all the others with a large horizontal telescope would not be a very formidable undertaking. It would also be interesting to observe the spectra of all the clusters. It is possible that some may consist of stars having singular spectra, or even of disconnected nebulous masses, in fact forming clusters of planetary nebulæ. The interesting discovery by Dr. Copeland that Burnham's double nebula in Cygnus is gaseous, shows the same tendency to aggregation in these bodies as in stars. Observations of the spectra of all the red stars and variables would also probably lead to interesting results.

4. Photometry. Should the instrument be devoted to photometry numerous problems suggest themselves. Variable stars could be observed near their minimum when too faint to be identified with an equatorial without great loss of time. Faint stars in zones or faint companions to bright stars could be measured very rapidly. The relative light of all the asteroids would be an interesting problem. Many coarse clusters appear to consist of stars of nearly equal brightness. Their light compared with their distances apart might aid our study of their formation. Another useful investigation would be to measure the brightness of all the nebulæ.

In the application of physics to astronomy doubtless many other problems will suggest themselves. Thus no satisfactory results have been obtained in the attempt to measure the heat of the stars with the tasimeter. The use of this instrument would be vastly simplified if it was placed on a solid pier near the ground, was not moved during the observation, and could be perfectly protected from other changes of temperature than those which it was intended to measure.

As either of the problems proposed above would occupy the time of a telescope for at least one year, it is obvious that there could be no difficulty in keeping such an instrument occupied indefinitely.

The horizontal mounting is especially adapted to an elevated position, and would permit the use of a telescope where an equatorial mounting would be quite impracticable. On the other hand, to an amateur, or for purposes of instruction, an instrument which could be set quickly from one object to another, and where the observers need not be exposed to the cold, would offer many advantages. The impossibility of observing far from the meridian would be less important with a large instrument, where the number of objects to select from is very great.

There are certain purposes to which this mounting could not be advantageously applied. The study of close double stars and other objects requiring long examination and very perfect definition could be better left to other instruments. The sun, moon, and planets can also generally be better observed off the meridian. If, however, the entire time of an instrument can be employed to advantage, and it can collect several times as much material as an instrument of the usual form, it is no evidence against its trial that there are certain problems to which it cannot be advantageously applied.

The working force required for such an instrument should consist of at least one observer, an assistant to record, and a number of copyists and computers to prepare the working lists, reduce the observations, prepare them for the press, and read and check the proof-sheets. A large volume of valuable observations could thus be produced every year, which would require at least double the time and money to produce by the same telescope mounted equatorially. The difference in the amount of work will be evident when we compare the number of objects observed with a transit instrument per night, with those observed with an equatorial. A hundred objects in various declinations might be examined in a single evening, while it is seldom that the same number could be identified and measured by an equatorial in a week.

ON MAXIMUM SYNCHRONOUS GLACIATION.*

By W. J. McGee.

In the development of knowledge of the cosmos, the tendency has ever been to look at first upon all phenomena as mystical and incomprehensible; and only after repeated observation and much study has it been decided that any class of phenomena may be the result of the operation of the identical laws whose existence is established by every-day observation. Thus, in geology, catastrophism prevailed long, but finally yielded to a rational uniformitarianism; in general biology the idea of special creation has only given way to that of derivation within the memory of a child; and in anthropology the mystical view yet generally prevails. The narrow domain of glacial physics, as embodied in the glacial theory, is still in the transitional stage. When that theory was first acceptably propounded by Agassiz, the details were so varied, the recognized relations so unique, and the whole conception so grand and startling, that even the more conservative of those who early became its advocates, forgot for the time the necessity for keeping all assumed data within the bounds of actual observation or legitimate induction; and hence not the least valuable of the later contributions to the theory are those which bring out its relation to established laws. Such is the aim of the memoirs bearing the above title; the particular phase of the subject discussed being that known as the "ice-cap theory."

The conclusion of Tyndall that such a supply of heat as may be necessary to produce large quantities of aqueous vapor, and an area of sufficiently low temperature to not only condense but congeal the vapor brought to it, are the first requisites for glaciation, is adopted at the outset; but it is shown that while the regions which furnish and those which congeal the vapor may be contiguous, they must be quite distinct. There is no other substance than water in the solid state which will abstract heat from the superfluitant vapor with such facility as to form, when spread over the surface, a condenser of sufficient power to meet the requirement of glaciation; and such a condenser must so far exceed in capacity any tax that may ever be placed upon it, that it will *immediately* condense and congeal all moisture that may be brought to it by aërial currents; for if the vapor is not immediately condensed it will cut off radiation from the ice below, and thus accelerate melting; and if the vapor is only condensed but not congealed it will fall as rain, and every pound of it will melt 143 pounds of ice before it is itself frozen. *The integrity of the condenser hence depends on its capacity being far in excess of the work it may be called upon to perform.* Now if a condenser formed of an ice-sheet 1,200 or 1,400 miles in diameter on any part of the globe be assumed, it is manifest that the tendency of the accumulating ice will be to form an annular belt of maximum thickness, gradually attenuating toward the center of the area; for if the vapor-laden air were not immediately robbed of its moisture in sweeping over the condenser, the marginal portions of the ice would soon be destroyed. But no matter how perfect the condenser, glaciation can never occur unless there are ample quantities of vapor supplied to it; and the greatest possible accumulation of ice at any latitude may accordingly be regarded as proportional to the moisture conveyed thither. It follows that the greatest possible accumulation of ice in polar regions can never have been nearly as vast as that at lower latitudes during the quaternary; and indeed it was probably never much greater than at present. Geological evidence, so far as accessible, corroborates this view.

Similar conclusions are reached by an independent line of investigation. Within an extensive area covered by ice or snow, both aërial and aqueous currents would be either stopped or so modified as to be practically inopera-

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tive as distributors of heat. The temperature would hence become approximately proportional to the solar accession, which has been computed by Much for the various latitudes, and may be roughly reduced to thermometrical degrees by means of an easily determined constant. Moreover the presence of the ice would greatly facilitate both radiation and direct reflection of solar energy. The general diminution of temperature produced in this manner is calculated for each latitude; and from a comparison of these figures with actual temperatures, as recorded by Dove, the temperature of the whole hemisphere when the ice-sheet extends to any latitude is also computed. From the several figures obtained it appears that if the globe were encrusted with ice, the crust would probably (and indeed almost certainly) never be melted unless by proper terrestrial heat; while the temperature in polar regions, as well as over much of the ice-covered hemisphere, would sink so low as to practically eliminate all aqueous vapor and effectually prevent the further accumulation of ice. The annual variations in solar intensity would not materially affect the values obtained.

Since the results reached in the manner indicated embody values widely different from those of existing temperatures, and are hence *a priori* improbable, a detailed investigation of certain meteorological phenomena is undertaken in order to verify these results. The observed and computed temperatures of the northern hemisphere are first compared, and are found to indicate that the temperature-equalizing agencies are 1.5 times as effective in summer as in winter. The effect of atmospheric dryness in diminishing the effectiveness of these agencies is then found to be still greater. The values developed in the investigation of this subject demonstrate that the climatal perturbations previously pointed out as the necessary result of the considerable extension of a polar ice-sheet do not differ in kind, but only in degree, from those whose constant occurrence is a matter of authoritative record; and analogy with observed phenomena moreover indicates that the calculated extent of these vicissitudes is in perfect harmony with the magnitude of the formulated course.

The figures obtained incidentally demonstrate the existence of an empirical meteorological law, which may be stated as follows: *Any increase in thermometrical range is accompanied by a diminution in mean temperature.* Since the law strongly corroborates the results reached by the second line of investigation, it is quite fully considered, especially in its application to the present eondition of the two hemispheres. That hemisphere whose winters occur in aphelion experiences a greater variation in solar accession and consequently in temperature than the opposite one, and hence, according to the law, ought to have a lower mean annual temperature. The southern hemisphere is so situated at present; and accordingly, notwithstanding more favorable geographical and other conditions, its temperature is lower than that of the northern hemisphere. The bearing of the law on Croll's celebrated theory of secular variations in terrestrial climate is manifest.

Since it is developed in both lines of investigation that the accumulation of glacier ice is dependent upon, and in a general way proportional with, precipitation, the maximum accumulation at any latitude may be roughly computed. The final determination is as follows:

Latitude 40°	18,594 feet.
" 50	9,777 "
" 60	5,728 "
" 70	2,800 "
" 80	1,799 "
" 90	1,440 "

It may accordingly be concluded that a sufficient accumulation of polar ice to displace seriously the earth's center of gravity, or to influence the motion of middle-latitude glaciers, never can have taken place.

The nature and course of ice motion are discussed at some length; and the phenomenon is shown to be analogous to those exhibited by all classes of substances, though generally in a less striking degree. The "viscous theory" of Forbes is adopted with some modifications; and the principal objections thereto are considered. It is also pointed out that ice-streams are necessarily in tension, and hence that the central mass of an ice-field can exercise an influence on the motion of its peripheral portions. The assumption of a vast polar ice-cap to explain the motion of the quaternary glaciers accordingly appears to be not only unnecessary but incompetent.

GLUCOSE.

BY ALBERT E. EBERT, PEORIA, ILL.

The process of making glucose, or grape sugar, is as follows: corn, after being shelled, is placed in large tubs and soaked in hot water from a day and a half to five days, or even longer, the time depending on the hardness of the grain. If fermentation is not wished, the water is changed when the substance begins to sour. It is then ground, while wet, with ordinary burr stones, and with a stream of water running into the hopper with the corn. The meal, or "chop," is then run on vibrating sieves, made of fine silk bolting cloth, also fed with streams of water. By this treatment the starch, which washes through the sieves, is separated from the gluten and cellular matter, which waste portions go over the tail of the sieves, and after passing through rollers which squeeze the mass, and return the water to the sieves, is sold for feed. The portion which went through the sieves is run into tanks and settled, the water drawn off and the sediment again mixed with clean water and treated with alkali, about one pound of caustic soda, (more or less, according to the hardness of the water), being used for each bushel of corn. This treatment separates any traces of gluten from the starch, which is then run into metal-lined troughs or gutters about eight inches deep, from fifteen to thirty-six inches wide, and usually from one hundred to one hundred and fifty feet long. These are inclined slightly, and the water runs off at the lower end, leaving the starch as a sediment at the bottom. In some factories this starch mixture goes direct from the sieves to the gutters or "tables," as they are usually called. It is left to dry somewhat in the tables, and is then shoveled out. At this stage of the process it is known as "green starch." It is quite solid, but moist, containing about fifty per cent. of water.

Up to this point the process is the same as starch-making. Starchmakers take the green starch and wash it, some several times, by mixing it with clean water and allowing it to settle, then drawing off the water, and repeating the process. It is then sometimes bleached by chloride of lime or sulphurous acid, and after washing, settled, made into blocks about eight inches square, when it is dried in a kiln. For the finer grades, about half an inch of each side of the cake is shaved off when partially dry, the rest of the cake being wrapped in paper and put back into the kiln until it forms into little sticks or pipes.

For glucose, however, the green starch is made quite thin with water, and run into converters, usually after several additional washings. The converters are large wood tanks or tubs, where it is treated with acids, sulphuric being usually used, although muriatic, nitric, or even oxalic may be substituted. Sulphuric is preferred, as it is cheap and easily gotten rid of in an after stage of the process, when it has performed its work. The acid does not combine with the starch, but merely exerts a catalytic action; therefore the necessity of providing for its removal. While under the acid treatment the contents of the converters are heated to the boiling point by