SCIENCE AT PORTLAND.

MEETING OF THE AMERICAN ASSOCIATION.

HOW THE ANNUAL MILITIA MUSTER AFFECTED THE SCIENTIFIC ASSEMBLY—PLENTY OF HARD WORK ON A RAINY DAY—SAFETY IN NAVIGATION, AND PROF. PEIRCE'S VIEWS THEREON—THE DARWINIAN THEORY—SUDDEN INTERRUPTION OF AN EXCITING DEBATE.

[FROM THE SPECIAL CORRESPONDENT OF THE TRIBUNE.]

PORTLAND, Aug. 22.—This was a rainy day; raw and disagreeable. But there was no lack of interest among the members of the Association, nor, I may add, among the citizens of Portland. The latter in fact did themselves special credit, for the ladies of Portland provided a bountiful lunch for the Association and waited on the tables themselves. The town is full of country people, drawn together by the annual muster of the Maine militia. Old-fashioned wagons, drawn by horses that have seen service at the plow, carry through the streets bevies of brown-cheeked lasses, with a goodly show of older members of families. Sometimes a home-spun clad figure looks in upon the scientific folk, but for the most part the strangers who linger about the doors of the sections, and not understanding what is said saunter away, are dressed in their Sunday clothes.

Those who had studied the programme of the day and who recollected the faculty which Prof. Swallow of Missouri had on a previous occasion exhibited for "setting people by the ears," recognized in the announcement of his paper on the Origin of Species the nucleus to a lively debate. But as the day wore away with the reading of papers it became evident that this one was to be the last. And while there was a good attendance all day in both sections it was very evident that the culmination of popular interest was toward the close of the afternoon meeting.

One of the most valuable papers read in yesterday's meeting was that of Gen. J. G. Barnard. I am unable by a days delay to present a better abstract of it than would have been possible yesterday.

ON THE RELATION OF INTERNAL FLUIDITY TO THE PRECESSION OF THE EQUINOXES—BY GEN. J. G. BARNARD.

Prior to the investigations by Sir Wm. Thomson concerning "the earth's rigidity," and his determination of the degree of rigidity needed in order that the actual precession should correspond so nearly as it does to the calculated precession for a perfectly rigid homogeneous spheroid, the earth, or even the "thin solid crust" of the geologists, had in researches concerning precession always been regarded as perfectly rigid or rather it had been taken for granted that the solid earth, or the solid crust, however thin, was practically rigid enough to be considered perfectly rigid. Hence the famous investigation of the late Prof. Wm. Hopkins of Cambridge, in which treating the solidified portion of the earth as rigid, it was attempted to find in the phenomenon of "precession" a test of the question of internal fluidity, and a determination of a minimum thickness admissible. In the treatment of this difficult problem by Prof. Hopkins there is a mastery of the higher analysis and a skill in its application to physical problems which excorts admiration; and yet there is in the fallacy of the final result and the general acceptance with which it has been received that which gives the investigation a claim to be ranked among the "curiosities" of modern mathematics. Although the discrepancy (one-seventh) between the observed and computed precession, which really forms the basis of his practical application, is now, however, believed to be very minute (owing to the less mass now attributed to the moon), and although the demand originally required by Sir Wm. Thomson puts "out of court" the question of a "thin crust," yet so late as 1868 the eminent French astronomer (then President of the Academy of Sciences) has attempted a mechanical confirmation of it by an experiment with a glass globe ten inches in diameter, containing water, by which he would demonstrate that the neglected property of "viscosity" of a fluid by Prof. Hopkins vitiates his results. The late Archdeacon Pratt, on the other hand, an authority on the "Figure of the Earth," attempts to vindicate Prof. Hopkins. Although neither the refutation nor the vindication are believed to be one or the other, they show that the problem has not by any means lost its interest. Papers read before the American Academy of Sciences, and published among the Smithsonian contributions to knowledge, are referred to for the proof given by the author that the centrifugal force of diurnal rotation of the solid portion of a fluid developed in a wholly fluid spheroid exactly neutralizes the precession—producing couple due to the solar or lunar attraction when the causes the protuberance; hence that in such a spheroid no precession can exist. Precession, to the extent protuberances are developed in a solid spheroid, or in the thin crust (in the latter case nearly as great as if wholly fluid), will the precession be neutralized. A much greater difference is not developed as Wm. Thomson demonstrates that the rigidity of the earth's substances must greatly exceed the actual rigidity of iron to depths of 1,000 or 2,000 miles. On the other hand, all known gaseous and liquids in congealing separate. Observation shows that this law obtains in volcanic lavas. Hence, regarding the earth as a congealed spheroid once liquid, there would be large subterranean cavities, still containing uncongealed liquid. By reference to these, Sir Wm. Thomson explains the geological phenomena of volcanoes, earthquakes, upheavals, etc., may be explained.

The paper sets forth the "rationale" of the action and reaction between an internal fluid and an enveloping shell (or crust), and it is very elegantly treated by Prof. Hopkins, and thence declares the results of the latter, that for homogeneousness of shell a fluid, precession will be the same as if the whole were solid. It shows also this same result applies equally to the case of heterogeneousness of shell and fluid—that is, to the case of the actual earth; that Prof. Hopkins's famous result that there must be a solid crust to a depth of at least 800 to 1,000 miles is founded upon his fallacious method of computing the pressure of a heterogeneous envelope fluid upon its envelope; that this determination containing all the difficulties of the transcendental problem of the "figure" of a heterogeneous earth is made by Prof. Hopkins as extraordinary for its fallaciousness as for its simplicity—*i. e.*, by integration of the attractive forces on the elementary particles of the fluid as free forces.

Gen. Barnard solves the question by this proposition, and asserts that its denial would, carried to its legitimate consequences, involve violation of the law of "conversation of energy." Given a heterogeneous fluid wholly developed by a rigid shell, and a state of static equilibrium assumed in relation to internal forces and a foreign attraction, the pressure-couple of the fluid on the shell due to this attraction is precisely the same as that which would be exerted on the fluid solidified.

According to Prof. Hopkins, this identity exists only in the case of homogeneousness; and hence, and hence only, his famous and fallacious result that precession is different for a heterogeneous shell and fluid from that which would belong to the spheroid wholly solidified, and that there must be a crust of 800 or 1,000 miles thickness.

Gen. Barnard shows that this simple correction converts Prof. Hopkins's formula into one exhibiting identity of precession in the two cases; he shows moreover that his actual formula as it stands expresses exactly the reverse from what he supposes; that it really gives greater (instead of less, what he needs) precession by thickening the crust.

The following "conclusions" quoted from Gen. Barnard's Smithsonian paper are designed:

making an independent determination of that longitude through photographing.

The following is a comparison of results:

	Nights.	H.	sec.
Brest—Greenwich	9	27	37.19
Greenwich—Paris	9	0	37.11
Brest—Paris (sum)	9	27	18.94
Brest—Paris, direct	9	27	18.18

showing a difference of only 0.06 seconds, which is to be equally distributed between the several values. The French determination, by telegraph, of Brest—Paris, made in 1863, is within 0.01 second the same as above while that of Paris—Greenwich, made earlier, is 0.48 seconds less than the value now found. It is a singular combination of circumstances that led to the determination of the longitude between the two principal observatories of Europe by American astronomers.

Of the longitudes in Harvard-Greenwich we have now three independent results, obtained in different years, by different observers, and through different cables, for in 1870 the two cables were joined at St. Pierre, and the signals exchanged directly between Brest and Duxbury, while in 1872 an intermediate station was made at St. Pierre, whence the signals were sent via Sydney and the Provinces to Cambridge. The results are:

LONGITUDES HARVARD OBSERVATORY FROM GREENWICH OBSERVATORY.

	H.	M.	S.		H.	M.	S.
1867	4	44	31.00	1872	4	44	30.99
1870	4	44	31.08				
Mean	4	44	31.01				

The close agreement of these results indicates a very near approximation to the truth, and for the present the longitude may now be considered as settled. Adding to the above value the difference in time between the Washington and Harvard observatories, 25 minutes 12.12 seconds, we get for Washington as the "

CONCERNING HYALONEMA—BY DR. SAMUEL LOCKWOOD.

The recent deep sea dredgings have done much toward clearing up the singularly anomalous history of the Japanese glass-rope sponge. Prof. Lockwood, however, thought that, either from imappreciation or otherwise, the knowledge thus obtained has not been applied to the elucidation of certain mooted points connected with Hyalonema. With regard to the mistakes in representing Hyalonema "wrong end up," my opinion is that the error was led off by the Japanese themselves. The drawings by native artists represent these curious objects as attached to the sea bottom by the sponge mass, thus making the fascicle to be erect and uppermost. Obtained by the net, or some such means, from the bottom of the sea, the Japanese, the same as Norfolk fishermen at Enoserma were entirely ignorant of the matter. Their theory, however, as represented by the native artists has wrongly represented the Hyalonema. Those ropes attached to the sponge and sand are some distance from the main or upper portion encrusted with parasites, after removing portions of the encrusting case from the fascicle, he could not detect a defect. We therefore that Polython owed anything for food to the object which had given it local support. It, however, "churns" with the sponge for a purpose of its own. Prof. Lockwood thinks that it draws sustenance from the fishing process of its radiating tentacles.

Both Polyps and sponge provide for themselves. In his view the zoophyte is what we must call a compensal, could not exist without that sort of support from Hyalonema which the oak affords the vine, and Hyalonema, too, is a compensal; for how long would it endure without the support of Polython. The stem, without this support, would not be able to hold the stem erect. Other varieties are supported by stems consisting of sheaves of short spicules, bound together by bony cement. These have and need no supporting Polython. He considered that the Hyalonema was sunk in the mud up to the necks, arguing that the spines surrounding the stem could not so live; that it could not use its tentacles to obtain food, and that the position of the organs of sense, deep-seated, the oldest ones being attached, and the most recent at the bottom. Sustaining this view some account was then given of the material structure of the encoating Polython. The examples given were from the waters off Setubol, making that place their feeding-ground, because of this facility afforded them to secure these egg cases by the abundance of the Hyalonemas there.

THE ARCTIC REGIONS—BY PROF. WM. W. WHEILDON.

Mr. Wheildon's paper was on the phenomena of the Arctic regions considered in reference to the atmospheric theory of an ameliorated climate and an open polar sea, a subject which he has presented in no former papers. He spoke of the geography of the region and the great water-sheds in the Arctic Ocean in both countries, and extending generally as far north at 80°, and containing five or six of the longest rivers in the world, draining a region of 3,000,000 square miles. In reference to the Arctic Ocean, Mr. Wheildon controverted the statements made by the late Prof. Rogers of Pittsburgh in his physical atlas, of a converging and rotating ocean, and discussed somewhat confused question of outlets as defined by Rogers and Hall, and also his views with regard to the movements of the waters of the Gulf Stream. Capt. Barrow considers the Polar Sea as defined by a circle in the latitude of 70° of 2,400 geographical miles in diameter and 7,200 miles in circumference. It seems to have been supposed that because it surrounds the position of the theoretic pole it must necessarily involve around that imaginary object which some explorers have sought to reach, and find, and one of them speaks of it as a "pivot or point of the axis on which the globe of the earth turns." An American pilot boat captain declared that "Capt. Hall would set his foot upon the pole finally."

The subject of Arctic currents was then fully discussed, and the great Polar current from the Siberian rivers toward Spitsbergen and Greenland described. The great mass of ice, and the cold current, loaded with ice and drift wood, nearly or quite in the form of what is assumed to be a prolongation of the Gulf Stream, so that on the Gulf Stream theory these streams must meet each other. The latest intelligence furnished by Dr. Peterman locates the polar current far to the east and nearly in the middle of the Spitsbergen Sea. The surveys and explorations of Admiral John Rogers were referred to and quoted against the idea of a simply rotating current around the pole.

The subject of outlets and inlets, as defined by Prof. Rogers and others, was next considered. Prof. Rogers speaks of Behring's Straits as "out of the really but two inlets," but regards it as null on account of its shallowness; and subsequently, in the same paper, makes it the special inlet of the Japanese current, which he uses to enforce the Gulf stream, neither of which, as the speaker believes, reach the Arctic Ocean at all. Capt. Kerlett's views of the current in Behring Straits are quoted as those of Admiral Rogers.

The several temperatures of the Arctic Ocean, and the Poles of cold, are considered as ascertained facts, stated that the position of the poles of cold is nearly in the central latitude of the great ice belt which surrounds the polar sea, and the inference is that there is a milder temperature north of the south of it, a fact confirmed by all explorers, and recently by Capt. Hall and Capt. Tyson of the Polaris expedition. The subject of frozen ice blink one of the most constant Arctic phenomena, it is believed can only be explained on the atmospheric theory. Capt. Rogers said he was satisfied no man could explain the phenomena as he had seen it; there is no difficulty in doing so in the present light on the subject. Capts. Beechey, McClintock, Dr. Kane and others are quoted. The ice-barrier is next considered. It has been enumerated at all points, and no gateway, as claimed, has ever been found through it. The Gulf Stream keeps an open sea, as alleged, no doubt it could keep a gateway into it; and so far as that may go, Capt. Silas Beal's theory is reasonable. The general climate of the Arctic region is considered, and a large amount of evidence as to its character furnished. Capt. Parry found fresh water ponds on the ice north of the latitude of the poles of cold. Capt. Hall and Capt. Tyson describe the climate as mild beyond their expectations, as shown by the absence of snow and ice and a rising thermometer. Animal life was abundant, and is so in the Arctic regions, on the mountain ranges of Spitsbergen, Greenland, Jan Mayen, Kennedy Channel, and probably on more northerly shores, all Woden. There are many well-established facts which appear to authorize the belief that there is beyond the ice-barrier a climate equally as mild, if not much more so, than that to the south of it. The same line in the Arctic regions in the valleys and extends for a considerable distance up the sides of the mountains of no opportunity to take observations in the whole voyage. The Bunker method of taking observations upon any vessel that was not of great steadiness, could be understood by all navigators. Professor Peirce went out with an excellent assistant, who was not instructed in the method, a result of the Professor taught it to the captain during the voyage, and it happened that observations thus taken were the only ones that they could obtain.

INTERNATIONAL COINAGE—ITS PROGRESS—BY PROF. E. B. ELLIOTT OF THE TREASURY DEPARTMENT, WASHINGTON.

It is generally conceded that the establishment of a simple and correlated system of international coinage is in a high degree desirable, and that such system should be upon a gold and a metric basis; gold to be a legal tender of payment in all amounts, while silver should be subsidiary, an overvalued token and legal tender only in sums of limited amount; and the relation of the standard coins to be referable to the metrical unit of weight, the gramme. Partial progress toward the establishment of such an international system has of late been made in Europe (Germany and the Scandinavian States, in the Netherlands, and also in the United States. Japan has adopted a complete system of coinage on a gold and a simple metric basis, and is the only country of the world of which this can be said. The basis adopted by Japan is that recommended to Congress for adoption by this Association at its session at Salem in 1870.

The action referred to above as lately taken by the United States in the direction of a metric system of international coinage, is the establishment by the late Congress of a unit of silver coinage upon a metric basis. The half dollar hereafter coined to weigh just 12½ grammes and the quarter dollar and dime in proportion. Silver pieces of three and five cents are not hereafter to be coined, and the amount of gold coinage was made in the late Legislature, action in this direction being reserved for sum future session. The States now comprising the German Empire, Scandinavian States, Sweden, Norway, Denmark and the Netherlands have up to the present time, or within a very late period, had systems of coinage based upon an exclusively civil standard. In these several States legislation has been just completed or is now pending to change their standard units of account from silver to either a gold basis or to that of a double standard. It is to be regretted that the new standard of gold coinage will not have simple relations to the metric unit (the gramme), although expressed in terms of the gramme. Concerted action will doubtless ere long be had by which for exchange and other international purposes a common unit of account upon a metric basis (the gramme) will be adopted by the different commercial nations of the world.

At a meeting of the International Statistical Congress at the Hague in 1870 it was recommended on motion of Dr. Farr and Prof. Soetbeer, a statistician, that in statistical publications values should be given not only in the monetary units of the country to which the statistical values pertain, but also in francs and in decagrammes of gold (napoleons or 20-franc pieces), doubtless ere long be had by which for exchange and other international purposes a common unit of account upon a metric basis (the gramme) will be adopted by the different commercial nations of the world.

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The following table compares the four standards of certain existing and proposed systems of coinage:

One kilogramme of standard gold of nine-tenths fineness (or 900 grammes of pure gold) may be divided into	Each piece weighing grammes.
100 proposed decagrammes	10.
903,153 United States eagles	1.6718+
600 Japanese yens	1.6666
310 ten-franc pieces (French)	3.2258+
250 ten-mark pieces (German)	3.9935
223.2 proposed 10-cent pieces	4.4833—
147.62 proposed 10-florin pieces (Netherlands)	6.7750+

NOTE.—The British gold sovereign is of a fineness of eleven-twelfths and weighs 7.98805 grammes, containing of pure gold 7.32338 grammes, and consequently equivalent in value to 8.13596 grammes of the fineness of nine-tenths.

The audience in Section B rapidly increased in the afternoon as the hour approached for the delivery of Prof. Swallow's paper.

ON THE ORIGIN OF SPECIES—BY PROF. G. C. SWALLOW.

A large number of the cultivators of science have believed in the integrity of species. They have based their investigations on the hypothesis that certain organic beings are alike in all their essential characteristics; that each has a well-defined beginning and will produce its kind; and that each series will maintain its identity indefinitely to the end. They have at the same time admitted that individuals may be somewhat changed by various causes, such as climate, food, &c., and those changes may be fostered and transmitted, especially by man's care; that varieties with marked characteristics may be produced by special causes, but that when these causes cease and the original conditions are restored, the variations will rapidly disappear and the varying series will return more or less perfectly to the original stock.

In the early part of the present century Lamarck, De Hillaire and others advanced the theory that inorganic matter is developed into the lower orders of organisms, and these in turn are developed into higher organisms, until man is reached. This theory never became of profound sensation, but Cuvier, Linnæus, and others successfully defended the immutability of species. Near the middle of this century the "Vestiges of Creation" appeared, and a new era began. Facts, and more facts, extending the theory of development far beyond any limits indicated by previous thinkers. Some ten years later, Mr. Charles Darwin and Mr. Alfred Wallace proposed a theory accounting for the origin of species in various ways, but more especially by "natural selection." Mr. Darwin has sustained his theory by many able works, but he wisely limits his theory by admitting the creation of primordial beings, thus avoiding the weakest parts of the old theories. The latter become more conspicuous when Drs. Bastian and Child and Prof. Haeckel and Mr. Herbert Spencer declare that the evidences of evolution covers the whole space from inorganic matter to man.

But these opinions generally lead not to a definite conclusion but to a balancing of possibilities. Changes involving the addition of new bones or new organs have no reference to changes in the nervous centres, additions or enlargements of either. The same is true in regard to the introduction of new reasoning powers. Between the lowest organism and man there seem to be two very serious barriers, and all efforts seem to be utterly insuperable. We have facts to show that we can change things, qualities, and principles, but none to show that we can develop new ones. It is as easy to give life to motes as to give new powers to a plant. Also the fact that you can change one animal into another only supports a change of animals and not of plants into animals.

The other, to me, insuperable barrier is the change of animals into man. So far as the physical nature is concerned, it might be possible, but in the highest degree improbable. But when you say the intellectual powers of a monkey are developed into the mind of man the facts adduced have no reference to the moral powers of the, money will give the moral powers of a man.

Prof. Swallow made the point that the religious elements could never be developed out of the monkey. Superstition was more prevalent in earlier races of men than in later; hence it was not a process of development. But the monkey could not be taught to reverence the unknown. There was nothing in the direction which in the monkey could be developed. He instanced that man could have lived in the tertiary period. He described excavations into an Indian mound in Missouri, where there were remains of the mammoth found in strata around the mound at a higher level than the excavation made. It was his conviction that the mammoth-bearing strata were a subsequent deposit. A pot was taken out of the bottom of the excavation containing probable that human, perhaps 500,000 years, is too much in the case of Europe and America. The discovery of a more anthropoid type of animals, if ever made, will be in tropical Africa, a region of earth little known.

In the matter of the tail there is not as much of a fall in the highest anthropoid apes as in man. This was remarked by Darwin, comparing the terminal vertebræ of the simia and the human. The latter was the latter were the least anchylosed. It is almost incredible to one who has not attempted it to see how accurately bone for bone, muscle for muscle, nerve for nerve, the anthropoid ape bears comparison with man. The difficulty is not to perceive the similarity but to find the difference. The evidences adduced against Darwinism were given to those of a negative character, such as the absence of intermediate forms. In the matter of variation—while many of the altered forms would fail to exist, some would certainly remain. Compare all such forms as have had long domiciliation in one locality, and their definite features seem to be fixed. He instanced the case of a naturalist of eminence, who would make a generic distinction between the fox and the dog, though no one doubted the derivation of the latter from the former. As to variation in a state of nature, he defines, what constitutes a species. A few years ago there were 130 supposed species of birds. But now the leaders of distinguished ornithologists, among whom were Prof. Allen of Cambridge, Prof. Baird, Dr. Coues, and others, they were reduced to 90, and it was shown that what had been supposed different species was simply variation in a vast number of instances.

In respect to hybrids we have innumerable instances of unnatural hybridizing in this country. He gave several instances of hybrids that are exceedingly common and persistent, referring to well-known Western birds. As to degeneration, he thought it was a case of misapprehension. The man of the present day was certainly not inferior to the man of the past. If we admit our derivation from the savages that resisted the landing of Cæsar in Great Britain we may admit a derivation from a still lower period. The evolution theory gave us the prospect of a vast and magnificent improvement in the human race. In paleontology every year and every month brought changes. It was but yesterday that we listened to the announcement of the missing reptilian feature, the perissodactyla and horned ruminants. Even the eminent paleontologists had been at war respecting an animal whose mixed characters rendered it difficult to refer it to any one group of mammals.

There was a buzz of excitement through the audience, and Prof. Gill's remarks closed amid great applause. Prof. Morse sprang to his feet, stating that though he had been absent during the earlier portion of Prof. Swallow's communication, the latter [illegible] of reptiles. Speaking of intermediate forms, revealed by the study of life in past ages, Prof. Morse said that birds had always been regarded, until recently, by naturalists as a closed type; but now we had to deal with such creatures as the Archeopteryx, a fossil bird with a tail longer than his body, in which the characters were so closely related to reptilians as to show no appreciable difference. In other words, while at the present day birds and reptiles stand widely apart, in past geological times their characters were so intermixed that it is impossible to refer certain forms to either of those classes. He instanced as one of the missing reptilian features still persisting in birds, the possession of teeth in the embryo, first discovered by Goethe in the parrot and confirmed by Prof. Morse in the hawk swallow. So general is this mixture of character in the earlier groups of animals that long ago Dana and Agassis recognized them as comprehensive or sympathetic types of animals. At this point Prof. Morse seized the chalk and was rapidly proceeding to demonstrate the truth of his views, by drawings on the black-board, when an unlooked for interruption took place. The presiding officer announced that the General and his staff were about to be saluted by the State Militia, which was then under review in front of the hall; and the clamor of the crowd below and consequent confusion would render the remarks of the speaker unintelligible to the audience. Amid expressions of extreme disappointment the assemblage slowly dispersed, the chairman announcing that the discussion would be continued on the morrow.

SATURDAY'S PROCEEDINGS.

PORTLAND, Aug. 23.—At the morning session of the American Association for the Advancement of Science the following new members were elected: Robt. H. Richards, Boston; Mrs. Elizabeth Whittier, Plymouth; N. B. Whitaker, Providence; Lorenzo Watson, Providence; J. C. Monroe, Lexington, Mass.; S. H. McAllister, Akron, Ohio; Miss Chadeagne, Jersey City; J. W. Chickering, Washington; Dr. Geo. F. French, Portland, and Josiah Curtis, Washington. Ten new papers were received and referred. Section B was divided into two sub-sections, one devoted to biology and the other to geology. Asa Gray, Wm. B. Hough, C. B. Emerson, J. D. Whitney, B. W. Hillyard, J. B. Mayberry, L. H. Morgan, Chas. Whittelsey, and W. H. Brewer were appointed a committee to memorialize Congress in regard to the preservation of forests. A committee was appointed consisting of the four highest officers of the Association, to wait on Mrs. Elizabeth Thompson of New-York and thank her for her gift of $1,000. The Association then broke up into sections.

The discussion on Prof. Swallow's anti-evolution paper was continued in Section A by Dr. Dawson and Prof. Morse. The discussion was brief, and no new points were brought out. Dr. Dawson then gave a description of a specimen of sigularia, showing marks of fructification. In the geological section Prof. Hall of Albany read an admirable descriptive paper on the Geology of Portland and vicinity. The Association adjourned at 12½ o'clock, when members to go on an excursion down the bay in the steamer Forest City. The attendance is continually increasing, and a large number of papers will be submitted.

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only in the case of homogeneousness; and hence, only, his famous and fallacious result that precession is different for a heterogeneous shell and fluid from that which would belong to the spheroid wholly solidified, and that there must be a crust of 800 or 1,000 miles thickness.

Gen. Barnard shows that this simple correction converts Prof. Hopkins's formula into one exhibiting identity of precession in the two cases; he shows moreover that his actual formula as it stands expresses exactly the reverse from what he supposes; that it really gives greater (instead of less, what he needs) precession by thickening the crust.

The following "conclusions" quoted from Gen. Barnard's Smithsonian paper are deduced:

First: The analysis of Prof. Hopkins, in its application to a homogeneous fluid and shell, seems to establish (and the result is confirmed by the harmony with tidal phenomena as developed in p. 43) that the rotation imparts to the fluid a practical rigidity by which it reacts upon the shell as if it were a solid mass, while its pressure imparts to the shell the requisite couple to preserve the precession unchanged.

Second: The same practical rigidity is, with entire reason, attributed to the heterogeneous fluid by which (leaving out of view minute relative oscillations which do not affect the mean resultant in other natural phenomena and should not in this) the shell and fluid take a common expression.

Third: The two masses retaining their configurations, mutual relations, and rotary velocities, essentially unaltered by the hypothesis of internal fluidity, there is no violation of fundamental mechanical principles where the resulting precession not identical with that when the entire mass considered as solid.

Fourth: The common and identical precession of fluid and shell resulting from the analysis, is indispensable to any conception of precession of the earth as composed of thin shell and fluid; for otherwise internal equilibrium would be destroyed and the "figure of the earth" cease to have any assignable expression. The entire mass, fluid and solid, must (without invoking the aid of viscosity) be carried along in the precessional motion of the earth. Prof. Hopkins has not examined demonstrates the possibility and exhibits the existence of such a community of precession, but fails in the attempt to exhibit a test of the existence or absence of internal powerful pressures that would be exerted. [illegible] rigid crust would probably produce in it [illegible] sustaining movements; while if the crust is not of a rigidity far surpassing that of the constituents of the cognizable crust, the precessional motion of the earth would, owing to the neutralizing effect of tidal protuberance, scarcely be observable.

The following were among the papers read to-day:

DETERMINING LONGITUDES DETERMINED BY THE U. S. COAST SURVEY—BY PROF. J. R. HILGARD OF WASHINGTON.

The exact determination of the longitude of some point in the Coast Survey from the principal observatories of Europe is a matter of the first importance. The [illegible] of moon culminations, of lunar conditions, and [illegible] of chronometer carriage, formerly employed, [illegible] a certain degree of accordance still left [illegible] The success of the transatlantic [illegible] it possible to use the telegraphic method, [illegible] promptly done in 1867 when the longitude [illegible] Greenwich was found. When the [illegible] from Brest to St. Pierre and Cape Race [illegible] were laid, a second determination was made, [illegible] charge of Assistant C. W. Dean, in 1870, in [illegible] the contract of the method; but as no [illegible] could be had between Brest and England, the [illegible] connect with Greenwich was still wanting, and [illegible] the telegraphic latitudes in Paris and [illegible] Greenwich, an indifferent [illegible] of value was obtained. The missing link was supplied by Prof. J. R. Hilgard, who at the [illegible] of the transatlantic determination, [illegible] was corrective at Brest simultaneously [illegible] from Greenwich, and [illegible] retarding the American station at once [illegible] difference of longitude between the latter and Greenwich successively visited, with his own instrument, all the other stations in order to compare [illegible] equations, and while at Greenwich he exchanged [illegible] with the Paris observers, thus [illegible] to its character furnished. Capt. Parry found fresh water ponds on the ice north of the latitude of the poles of cold. Capt. Hall and Capt. Tyson describe the climate as mild beyond their expectations, as shown by the absence of snow and ice and a rising thermometer. Animal life was abundant, and is so in the Arctic regions, on the mountain ranges of Spitzbergen, Greenland, Jan Mayen, Kennedy Channel, and probably on many northerly shores, all Winter. There are many well-established facts which appear to authorize the belief that there is beyond the ice-barrier a climate equally as mild, if not much more so, than that to the south of it. The snow line in the Arctic regions is in the valleys and extends to a greater or less distance up the sides of the mountains, while the tops are often bare and comparatively free of snow and subject to frequent falls of rain. The deer and other animals find their feeding grounds above the snow-line, and there is abundant evidence that they remain there through the Winter. Capt. McClure, in his famous passage around the North American Continent, says: "The greater part migration descended from this high ground to the sea ridges, so that a supply of game was kept up during the Winter." Record is made of a great variableness of climate generally in the Arctic regions, but beyond the ice belt it may be nearly as equable as in the temperate zones, "as under the tropics there are only the rainy and dry seasons; the North there are only Summer and Winter." The most recent intelligence from the Arctic regions confirms, as we have said, the supposition of an ameliorated climate beyond the most northerly latitude yet reached. Capt. Hall's last written records in latitude 80° 30', were: "We find this a much warmer country than we expected. We have found the country abounds with live seals, game, geese, ducks, musk oxen, rabbits, wolves, foxes, bears, partridges, lemmings, &c.

THE CO-EFFICIENT OF SAFETY IN NAVIGATION—AN ATTEMPT TO ASCERTAIN WITHIN WHAT LIMITS A SHIP-CAN BE LOCATED AT SEA BY ASTRONOMICAL OBSERVATIONS—BY PROF. WM. A ROGERS.

This was an attempt to ascertain mathematically the average number of miles that a ship may be out of her reckoning. It was a paper of length, indicating long and careful research. It stated that in the case of British vessels there is a continual increase in the proportion of wrecks, as shown in the following:

British vessels.	Wrecks.
Ins., 1866 over 1849.....33 pr. ct.	Ins., 1862 over 1859.....62 pr. ct.
Ins., 1868 over 1858.....44 pr. ct.	Ins., 1867 over 1867.....67 pr. ct.

For 1869 we have a decrease in the number of vessels 4 per cent and an increase in the number of wrecks of 21 per cent. The confidence in navigation was thus lessened and increased the danger. He considered separately (1), wrecks by causes beyond control; (2), wrecks to obtain insurance; (3), wrecks by deviation of compass; (4), wrecks by errors of observation. He concluded that 70 per cent of wrecks were from preventible causes. There were 1,000 vessels annually totally wrecked as uninsured. The ratio of errors in chronometers was contrasted in an elaborate series of tables showing that the navigator must expect from this source an error of 3.6 miles, must be on the look-out for one of 11.6, and must not be surprised at one of 21 miles, all on the supposition that he has an average chronometer. One serious source of error is the unaccounted currents during a voyage. The conclusion was that the navigator who assumes that he can get the place of his ship certainly within five miles, or probably within fifteen, exhibits an overconfidence which may lead him to ruin.

A brief discussion of very great interest followed, in which Prof. Peirce spoke of the question of the life of a ship, and doubted whether the United States can be held back much longer from the building of ships by the transient popularity of iron vessels. It does not pay to use iron, when we consider the increased number and proportion of accidents. It is cheaper for a merchant now to build of wood than of iron, and more especially of a combination of wood and iron than of iron alone. In respect to the use of instruments our sailors have yet much to learn. A good navigator will always, during a voyage, that he did not trust his position within 20 miles. A similar remark was made by the commander of the United States steamer Bache. No captain should be permitted to come within 20 miles of land without casting lead. If he does, and has passengers, he ought to be hung. The rule of casting lead when approaching land should be absolutely compulsory by law. The most rigid construction of such a law should be insisted upon. This rule of taking soundings was the reason of the absence of loss on the part of the Coast Steamers. It happens again and again that there

Prof. Swallow made the point that the improvement could never be developed out of the monkey, superstition was more prevalent in earlier races of men than in later; hence it was not a process of development. But the monkey could not be taught to reverence the unknown. There was nothing in this direction which in the monkey could be developed. He denied that man could have lived in the tertiary period. He described excavations into an Indian mound in Missouri, where there were remains of the mammoth found in strata around the mound of a higher level than the excavation made. It was his conviction that the mammoth-bearing strata were a subsequent deposit. A pot was taken out of the bottom of the excavation containing a skull of a woman. It was of a fine texture and of elegant form. There was no evidence that this belonged to a race in any wise inferior to man of the present day. These two objections to the development of the monkey into man—first, the absence of moral sense in the monkey; second, the utter want of evidence of the physical modification of the monkey into man—Prof. Swallow considered insuperable. The usual arguments of the anti-Darwinian school were brought to bear in addition. The tendency of animals to return to their original types was adverted to, and an instance was related where in a Western district hogs of good breed, domesticated, being left without owners in the woods, became wild in appearance and long tusks grew upon them, so that a European seeing them could not be made to believe that the creatures were not native wild boars. Similar instances were adduced in regard to many other domestic animals. The speaker was not impressed by the arguments of Darwin in respect to hybrids. He mentioned the case of a hybrid between the willow and the oak which had become extinct.

His time having elapsed, the motion was made that the time be extended. Prof. Swallow declared that he was not anxious to take up the time of the section. He was allowed time to finish his paper. He proceeded to set forth, first, that species were usually fruitful; second, that the tendency of the offspring of fruitful hybrids was to return to one or other of the parent species. He alluded to the extreme rarity in the case of cases of natural hybridisation. He mentioned the case of a trillium which he had discovered—a "sport" of unusual size and color—but the efforts of the most careful cultivators failed to perpetuate it.

In regard to fossils, he thought there was no evidence whatever that any one species had ever been changed into another. We had merely discovered that there were more varieties than we had before known. He instanced the cystis as exhibiting the widest variety, but no development into anything else. In the case of the insectorial [illegible] of the honey-combing better, with every advantage of climate and locality, they died out entirely. He considered that the case of man was similar; that in different ages nations had for a time improved, but afterward degenerated, and he sketched the rise and fall of past civilization.

The audience had not to wait long for the reply. Prof. Theodore Gill, the ichthyologist, was the first to gain the floor.

Prof. Gill said we were familiar with most of the arguments advanced by the speaker. He took up the arguments in their order. In respect to Linnæus's repudiation of the doctrine of development theory, he said the doctrine was not advanced at that time. Perhaps nineteen in twenty of the naturalists in Europe of the naturalists were evolutionists and many of them Darwinians. The distinction has not been made between Darwinism and the evolution theory. The latter is a belief in progressive developments by growth from the simple to the complex through successive differentiations; Darwinism adds to this theory by showing its factors in [illegible] selection, natural selection, protective coloring, mimicry, &c.

The arguments are not perhaps sufficient to prove spontaneous generation, but we cannot yet refuse the evidence of our senses in reference to some of the experiments of Bastian. The questions of homology formed a principal part of the science of the day. As to these [illegible] can be no doubt. We believe that the fishes from the very lowest form to the highest. No definition has yet been established between animals and plants in their lowest forms. In fact it has been necessary to form a third group to include the forms intermediate between plants and animals. As to moral sense in animals, the psychological evidence in this case cannot be directly perceived. We only know as to dogs that they dream, and there is abundant evidence to show a rudimentary moral sense in these and other domestic animals.

In respect to the antiquity of man, it is not of course

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