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 homespun 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 prelude 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 extorts 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 observed and calculated precession, which really forms the basis of his practical application, is however, believed to be very minute (owing to the less mass now attributed to the moon), and although the demand for rigidity demonstrated 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 formal "refutation," founded on 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 all 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 tidal protuberances developed in a wholly fluid spheroid exactly neutralize the precession—producing couple due to the solar or lunar attraction which causes the protuberance; hence that in such a spheroid there would be no precession. Pari passu, 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. That such protuberances be not developed Sir 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 heterogeneous mixed 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 believes that the geological phenomena of volcanoes, earthquakes, upheavals, &c., 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 result of the latter, that for homogeneousness of shell a fluid, precession will be the same as if the whole were solid. It shows that 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 manner of computing the pressure of a heterogeneous enveloped 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 enveloped 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 one and 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 heterogenous 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:

making an independent determination of that longitude through another cable.

The following is a comparison of results:

	Nights.	m.	sec.
Brest—Greenwich	6	17	57.13
Greenwich—Paris	5	9	21.11
Brest—Paris (sum)		27	18.24
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 0.46 seconds less than the value now found. It is a singular combination of circumstances which led to the correction of the longitude between the two principal observatories of Europe by American astronomers.

For the longitude 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:

LONGITUDE HARVARD OBSERVATORY FROM GREENWICH OBSERVATORY.

	H.	M.	S.		H.	M.	S.
1867	4	44	31.00	1872	4	44	30.99
1870	4	44	31.05				
Mean					4	44	31.01

The close agreement of these results indicates a very near approximation to the truth, and this fundamental longitude may now be considered as settled. Adding to the above value the difference in time between the Washington and Harvard observatories, 23 minutes 12.12 seconds, we get for Washington 5h. 8m. 12.12s.

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 inappreciation 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 the native artists represented 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 bottoms at great depths, it is supposable that the fishermen at Enoserma were entirely ignorant of the matter. Their theory, however, as represented by the native artists has wrongly represented the Hyalonema. These 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 any structural evidence that Polython owed anything for food to the object which had given it local support. It, however, "chums" 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 zoöphyte 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 combatted the view that Hyalonema was sunk in the mud up to the neck, arguing that the polyps surrounding the stem could not so live; that it could not use its tentacles to obtain food, and that the position of the egg-cases of the deep-sea shark, the oldest egg 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 essayist spoke of the deep-sea sharks 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 general 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 as 50°, and containing five or six of the longest rivers in the world, draining a region of 3,000,000 or 4,000,000 of square miles. In reference to the Arctic Ocean, Mr. Wheildon controverted the statements made by the late Prof. Rogers of Edinburgh in his physical atlas, of a converging and rotating ocean, and discussed the somewhat confused question of outlets as defined by that gentleman, 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 revolve around that imaginary object which some explorers seem to have regarded as a post, 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 not long since declared that "Capt. Hall would set his foot upon the pole itself."

The subject of Arctic currents was then fully discussed, and the great Polar current from the Siberian rivers toward Spitzbergen and Greenland described. This great current appears to run south-west, loaded with ice and driftwood, nearly or quite in the track of what is assumed to be a prolongation of the Gulf Stream, so that on the Gulf Stream theory these streams must cross each other. The latest intelligence furnished by Dr. Peterman locates the polar current far to the east and nearly in the middle of the Spitzbergen 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 Straits as one 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 of the remarkable 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 as well as 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. Capt. 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. If the Gulf Stream keeps an open sea, as alleged, no doubt it could keep an open gateway into it; and so far as that may go, Capt. Silas Bent'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 Spitzbergen, Greenland, Jan Mayen, Kennedy Channel, and probably on more 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 moun-

is no opportunity to take observations in the whole voyage. The Sumner method of taking observations upon any visible star would be of great value, and should be understood by all navigators. Professor Peirce went out with an excellent captain, who was not instructed in this method; a pupil 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 weight 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 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 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 our subsidiary 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. No modification of the 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 and 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 in progress, 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 as to weight with 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 gold and simple metric basis 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 of London, the eminent 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 nine-tenths fine, the object being to secure recognition of the last-mentioned unit. In a report to that Congress by Dr. Farr it was proposed that the system of Great Britain be so changed as to substitute the ten-gramme piece with certain decimal subdivisions for use in place of the existing British unit of account. This decagramme would be the exact equivalent of six of the proposed United States metric dollars or of six Japanese yens (or dollars). The corresponding United States three-dollars piece being consequently precisely one half of such proposed British unit. It is to be hoped that the system will be adopted in England, as thereby a simple correlation between the two proposed metric systems will be secured.

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.
598,153	United States dollars	1.6718+
600	Japanese yens	1.666
310	ten-franc pieces (French)	3.2258+
250	ten-mark pieces (German)	3.9825—
223.2	proposed 10-crown pieces (Swedish)	4.4803—
147.62	proposed 10-florin pieces (Netherlands	6.774

NOTE.—The British gold sovereign is of a fineness of eleven-twelfths and weighs 7.98805 grammes, containing of pure gold 7.32238 grammes, and consequently is 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 time 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 beginning 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 produced. This theory occasioned a 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 presented a formidable array of facts, extending the theory of development far beyond the limits indicated by previous thinkers. Some ten years later, Mr. Charles Darwin and Mr. Alfred Wallace produced 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 matter becomes of more serious import when Drs. Bastian and Child and Prof. Haeckel and Mr. Herbert Spencer declare that the evidence 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 which involve merely modifications 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 one of them seems 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 molecules as to give mental 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 case. What powers of the money will give the moral powers of a man?

Prof. Swallow made the point that the religious element 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 contain-

probable that 100,000, perhaps 200,000 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 part of the earth least known.

In the matter of the tail there is not as much of a tail in the highest anthropoid apes as in man. This was restated anatomically, comparing the terminal vertebra of the simia and the gorilla with man, showing that 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 generally 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 hog and the wild boar, though no one doubted the derivation of the latter from the former. As to variation in a state of nature, he defines what constitutes species. A few years ago there were 750 supposed species of birds. But by the labors of distinguished ornithologists, among whom were Prof. Allen of Cambridge, Prof. Baird, Dr. Coues, and others, they were reduced to 500, 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 natural hybridization 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 form. The evolution theory gave us the prospect of a vast and magnificent improvement for the human race. In palæontology every year and every month brought changes. It was but yesterday that we listened to the announcement of an intermediate form between the perissodactyla and horned ruminants. In fact two eminent palæontologists 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 part needed reply. 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 Archeopterix, 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 many reptilian features still persisting in birds, the possession of teeth in the embryo, first discovered by Goethe in the parroquet and confirmed by Prof. Morse in the bank swallow. So general is this mixture of character in the earlier groups of animals that long ago Dana and Agassiz 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 Governor and his staff were about to be saluted by the State Militia, who were passing in 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. Munroe, 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, W. B. Hough, G. B. Emerson, J. D. Whitney, E. W. Hillyard, J. S. Newbury, 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 sigillaria, showing marks of fructification. In the geological sub-section, Prof. Hitchcock read an admirable descriptive paper on the Geology of Portland and vicinity. The Association adjourned at 12 o'clock, to allow the members to go on an excursion down the harbor in the Revenue-cutter McCulloch. The attendance is continually increasing, and a large number of papers will be submitted.

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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 prelude 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.

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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 actua 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 extorts 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 observed and calculated precession, which really forms the basis of his practical application, is however, believed to be very minute (owing to the less mass now attributed to the moon), and although the demand for rigidity demonstrated 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 formal "refutation," founded on 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 all 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 tidal protuberances developed in a wholly fluid spheroid exactly neutralize the precession—producing couple due to the solar or lunar attraction which causes the protuberance; hence that in such a spheroid there would be no precession. *Pari ratione*, 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. That such protuberances be not developed Sir 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 heterogeneous mixed liquids in congeal-

ing 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 believes that the geological phenomena of volcanoes, earthquakes, upheavals, &c., 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 result of the latter, that for homogeneousness of shell a fluid, precession will be the same as if the whole were solid. It shows that 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 manner of computing the pressure of a heterogeneous enveloped 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 enveloped 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 one and 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 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 its 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, it would be a violation of fundamental mechanical principles were the resulting precession not identical with that due to 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 for 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. The analysis I have examined demonstrates the possibility and exhibits the *rationale* of such a community of precession, but fails in the attempt to exhibit a test of the existence or absence of internal fluidity.

Fifth: The powerful pressures that would be exerted upon a thin and rigid shell would probably produce in it noticeable mutational movements; while if the shell be 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 protuberances, scarcely be observable.