Having the three sides of the spherical triangle ⌇ Ca, the angle at C can be calculated, and it proves to be 53° 15′ 26″ for the 1st January, 1755.

Owing to the Second Rotation the Pole P is carried round C as a centre, at the annual rate of 40.9″. Between 1755 and 1850 there are 95 years, which multiplied by 40.9″=1° 4′ 45.5″ for the increase of the angle at C, which becomes 54° 20′ 11.5″ for 1850, when the Pole has reached P′.

We then have P′C=29° 25′ 47″ Ca=26° 37′ 3″ and the included angle P′Ca=54° 20′ 11.5″ to calculate P′a.

By calculation P′a=24° 54′ 21.2″ and found by observation, 24° 54′ 21.4″.

For 1st January, 1890, the angle C becomes 54° 47′ 27.5″ and by calculation, as before, P′a=25° 5′ 55″, and by the Nautical Almanac 1890, 1 January=25° 5′ 54.8″.

Hence the polar distance can be calculated for 135 years to within one second; and, considering the uncertainty of refraction, it is probable that the calculation is more correct than observation.

Such a result speaks for itself, and may well excite admiration of General Drayson's perseverance during many years of tedious calculation, until his labors have at last been rewarded by the splendid discovery of the radius of the circle described by the Pole of the Heavens, and the centre of that circle.

Had Newton with his marvellous intellect known, as we do *now*, that an almost tropical climate existed in what are now Arctic regions, and an Arctic one as low as 54° of latitude; that the axis of the earth varied its inclination to the plane of the Ecliptic; and that vast elevations and depressions had occurred upon the surface of the Globe causing its centre of gravity to vary its position by the consequences of these movements, as in transferring enormous quantities of the waters of the sea from one locality to another; who can doubt that he would have discovered the manner in which the Pole of the Heavens would have moved in obedience to the law of gyration? And with such catalogues as we now possess, he might have achieved the same results as have been obtained by Drayson in discovering, as he has done, the details of the Second Rotation. At all events he would certainly have attributed the Precession of the Equinoxes to the true cause of this, and not to the assumed joint action of the sun and moon on the protuberant Equatorial Zone.

A SEGREGATION OF FRESH-WATER FISHES.

BY THEODORE GILL, M. D., PH. D., WASHINGTON, D. C.

One of the most remarkable facts in zoögeography is the segregation of the greater part of fresh-water fishes represented by the ostariophysal orders, that is, the families *Characinidae, Cyprinidae, Siluridae* and their subdivisions. These are all genetically related, and must have developed from a common stock early accommodated to the fresh water and subsequently differentiated into many families and a host of genera with many hundreds of species. The few marine representatives of that host are the *Ariinae*, or *Tachisurinae*, and the *Plotosidae*, and these must have diverged from primitive fresh-water types.

Another case of segregation of a widely distinct series of families has never been recognized, and attention should be directed to it. It is that of the haplomous fishes.

The *Haplomi* are teleocephalous fishes with a pneumatic duct and abdominal ventrals, and were considered by Prof. Cope to be an order of physostomous fishes, including *Esocidae, Umbridae, Cyprinodontidae* and *Hypsaeidae*. These are evidently related to each other, although not very closely, and are mostly fresh-water forms. There are two other families which have hitherto found no satisfactory resting place which I am disposed to associate with the typical haplomes—*Percopsidae* and *Aphredoderidae*.

If the six families thus associated are really genetically related, we would have another series of families segregated as a fresh-water group, and which must have been long established. The only one of these six families with marine representatives is *Cypronodontidae*, and this seems to be the most generalized and most nearly related to the Synentognathous fishes, on one hand, and the Perciform, on the other. Whether the salt-water Cyprinodontids are the descendents of primitive salt-water fishes or have reverted to the sea in later times, is now an open question. This I do not propose to discuss at present, reserving it for future consideration, as well as numerous collateral questions which may suggest themselves. My only object at present is to draw attention to the zoögeographical fact mentioned and the morphological problem involved.

It is noteworthy that all the families enumerated are represented in the United States, and half of them (*Hypaeidœ*, or *Amblyopsidae, Percopidae* and *Aphredoderidae*) are found nowhere else. The *Esocidae* and *Umbridae* are represented in Europe as well as America. The *Cyprinodontidae*, or *Poeciliidae*, are generally distributed. All the families are remarkably well defined. Finally, it may be suggested that the unwonted position of the anus (jugular or thoracic) of two (*Amblyopsidae* and *Aphredoderidae*) is possibly more than a mere coincidence, and may be an inheritance from common ancestors.

BIOLOGICAL INVESTIGATION IN BOTANY.

BY J. CHRISTIAN BAY, BACTERIOLOGIST OF THE IOWA STATE BOARD OF HEALTH, AMES, IOWA.

A couple of smaller notes on the biological question, as far as botany is concerned, were published by me in this journal. To the first of these, What is biology? this little note is to be regarded as an appendix. My first paper contained, originally, a number of notes on the modern methods of biological investigation in botany; I kept them back in order that they should not be misunderstood.

A short time ago I received Professor N. Wille's inauguration speech in taking the chair of botany at Christiania, Norway. Professor Wille has said, in a few words, what I wished to say on the occasion above referred to. Therefore, I shall quote him:

"The so-called plant-biology is a child of the Darwinian theory of selection. It should be called, more correctly, *oecology*. This branch of investigation should embrace, as nearly as possible, the science of all life-phenomena of plants, *minus* physiology: in other words, oecology is the science of the mutual relationship between the plant and the surrounding nature, when this relationship does not rest upon physical and chemical causes.

"Oecology has still retained many reminiscences from the teleological conception of earlier days, when nature as a whole was thought of as created for the sake of being principally of use to, or a plaything for, the human race. Plant oecologists, or as they like to call themselves, plant biologists, have the idea that everything must be useful or developed in a certain way in order to be of use for certain purposes.

"We shall give an example of one of the typical representatives of this line of study. He placed an ant on the leaf of *Sonchus*, and found that the ant tore the cuticula, so that the milk juice from the leaf came out. The resin

of this juice stuck to the ant, which became so affected by it that it rolled down from the leaf. The conclusion drawn from this experiment was that milk juice is, wherever it is found, protective against ants, and keeps them away from the plants.

"It is easily understood that it is unallowable to draw such general conclusions from facts so uncertain and which prove so little. Before such a conclusion could be drawn, we ought to find answers to the following questions:

"1. Are the ants kept away from the plant by the milk juice?

"2. How much damage would the ants make, and how would they eventually make it?

"3. Is this damage so extensive that it would be in proportion to the energy used in producing the milk juice?

"4. Is the milk juice produced for a certain purpose, or is it only an inevitable by-product of metabolism?

"5. Does the milk juice of Sonchus serve for other purposes?

"6. Is the milk juice not serving for different purposes in the different plants?

"To give an answer to these questions would take years of study; therefore, it is easier to draw conclusions from the observations made in a few minutes, by means of imagination. The importance of imagination to the investigator is not to be underestimated, but critical consideration must separate out the chaff. However, it occurs to me that he who looks round, at present, in the science of plant biology, will find more chaff than grains."

This is another reason why biology should not replace physiology. It is pleasing to know that excellent biological theories have been established by Darwin, Bütschli, Schimper, Schwendener, Haberlandt, Mueller, Moeller, Lundström, Warming, Delpino and many others, and the most important facts put on record by such men as Trelease, Robertson, and many Europeans; but outside of flower-biology a great deal of the work done—especially when the facts have been arranged in order to prove a theory made beforehand—cannot stand close inspection.

LETTERS TO THE EDITOR.

***Correspondents are requested to be as brief as possible. The writer's name is in all cases required as a proof of good faith.

On request in advance, one hundred copies of the number containing his communication will be furnished free to any correspondent.

The editor will be glad to publish any queries consonant with the character of the journal.

THE IMAGINARY RACE OF CANSTADT OR NEANDERTHAL.

Dr. D. G. BRINTON, in his "Current Notes on Anthropology"—XXII (*Science*, Feb. 10, 1893), has given a brief summary of what has been said about the skulls of Canstadt and Neanderthal at the twenty-third meeting of the German Anthropological Association at Ulm (August, 1892). According to this summary, many facts allied by von Holder, Virchow, Kollman and Fraas, show that the skull of Canstadt, in all probability, belongs to the fourth or fifth century, A.D., and that the Neanderthal skull is hardly more ancient. In short, the human race of the quaternary period, described by de Quatrefages and Hamy, has never existed,—it is an "imaginary race," and "it should be recognized, once for all, that there is no sort of foundation for these scientific dreams."

Mr. Henry W. Haynes has answered to two points of Dr. Brinton's article (*Science*, Feb. 24, 1893). This answer was followed by Dr. Brinton's reply (*Science*, March 10, 1893). Finally, Mr. E. W. Claypole (*Science*, April 7, 1893) has sent a short note in answer to Dr. Brinton.

In their answers, Mr. Henry W. Haynes and Mr. E. W. Claypole have discussed the historical aspect of the question, but the main point has not been handled. This will be my aim.

According to the explanations given in 1867, 1872 and 1892, by Dr. von Holder, Dr. Fraas of Stuttgard, and Virchow, it is stated that the Canstadt skull has no date.

Be it so, I do not object.

According to the statements of the same German anthropologists, Dr. Fullroth's relation concerning the skulls of Neanderthal discovery is false, and it is by no means demonstrated that this celebrated skull is a fossil one, but, on the contrary, it has probably belonged to a Frank.

Be it so, if you like; I can agree with it.

But I cannot agree with Dr. von Holder concluding: "Die Rasse von Canstadt ist also meiner Ansicht nach ein Phantasiegebilde wenn ich so sagen darf, in vielleicht eben so hohen Maasse wie die schönen Gedanken es sind, die über den Neanderthaler Fund in die Oeffentlichkeit gedrungen sind"—and I must protest against Dr. Fraas's like conclusions: "Wir dürfen füglich die Cannstatter Rasse für immer zur Ruhe legen, und hoffen dass sie nicht mehr auferstehe, die Geister zu beunruhigen."

I may forsake to the anthropologists of the Congress at Ulm the skull of Canstadt, and, perhaps, the skull of Neanderthal; but the fossil human race of Europe—which we are speaking about—has not been established *over those two documents only.* There are, further, the fossil bones or skulls of Staegenaes (Sweden); of La Denise (France); of L'Olmo (Italy); of Eguisheim (Germany); of Clichy (France); of Brüx (Bohemia); of Schipka (Moravia); of Tilbury docks (London); of Arcy (France); of Gourdan (France); of Malarmand (France); of Goyet (Belgium); of La Naulette (Belgium); of Spy (Belgium).

The Congress of Ulm has forgotten all those, and discussed the skulls of Canstadt and Neanderthal only, *as if the fossil race of our ancient European ancestors were personified in these two skulls.*

People certainly know that de Quatrefages and Hamy have given to every one of the pre-historic races they established a name recalling the most ancient or the most celebrated locality where were found human remains reported to one of those types. The names "race of Canstadt," "race of Cro-Magnon," "race of Furfooz," have no other meaning for those anthropologists, and must not have any other signification for ourselves

Logically, therefore, M. Virchow, von Holder and Fraas could only conclude "that de Quatrefages and Hamy had been unlucky by choosing precisely Neanderthal and Canstadt in order to christen that race." They could affirm nothing more.

Before being empowered to conclude that there is no fossil human race presenting the type of the Canstadt's or Neanderthal's skull, they ought to have examined every other discovery and demonstrated that those discoveries were of no more value than the one of Canstadt or Neanderthal. Then only they could rightly call that race a "Phantasiegebilde." But they did not.

I do not wish to examine by myself every one of the discoveries I have quoted, and to discuss their value. I will only examine the human remains of Spy—having been an actor by their discovery and author of their description. For seven years I have been now busy with the study of these remains.

One of the discoverers, Professor Max Lohest, will show in a forthcoming issue of *Science* the geological value of the human remains found at Spy; and I myself will endeavor, in my following letters, to show the anthropological signification of those remains.

American readers will then be able to decide if this ancient race, established by de Quatrefages and Hamy, is an "imaginary" one and a "Phantasiegebilde" or not.

JULIEN FRAIPONT.

Liege, Belgium, Dec. 1, 1893.