II., p. 37, Dr. W. B. Scott established the genus *Mesocyon*, basing it on the same species *coryphæus*. Dr. Scott seems to have afterwards forgotten his genus, since he employed Eyerman's name. Indeed, all paleontologists who have had occasion to mention the genus have called it *Hypotemnodon*. It is obvious, however, that it must yield to *Mesocyon*.

In 1865, in Proceed. Acad. Nat. Sciences of Philadelphia, p. 90, Dr. Leidy described, from the Eocene of South Dakota, a carnivore which he called *Amphicyon gracilis*. Unfortunately for his species, Pomel had, as early as 1847, employed the same name for a fossil carnivore found in Europe. Cope in 1884, in his Vertebrata of the Tertiary Formations of the West, p. 916, made Leidy's name a synonym of *Galecynus gregarius*. Scott and Osborn in 1887, in a paper in the *Bulletin of the Museum Comp. Zoology*, Harvard, Vol. XIII., p. 152, speak of it as a distinct species under the name *Cynodictis gracilis*. Matthew recently, in *Bulletin of the American Museum*, Vol. XII., p. 54, records it as an 'invalid species' and apparently as a synonym of *Cynodictis lippincottianus*. When those disagree who have access to the type specimens and to abundant materials belonging to related forms, it is evident that the last word has not been said. Until it can be determined with some degree of unanimlty where Leidy's specimens belong, it will be better to keep them to themselves under a distinct name. Furthermore, the possibility exists that the discovery of additional materials will prove Leidy's form to be a good species. Pending this settlement of the question I propose to call the *Amphicyon gracilis* of Leidy *Cynodictis hylactor*. The specific name is that of one of Actæon's dogs.

O. P. Hay.

U. S. National Museum, July 27, 1899.

THE PROPER NAME OF THE POLAR BEAR.

The technical name of the Polar Bear as usually mentioned is *Thalarctos maritimus* (Linn.), the reference being Systema Natura, X., 1758, p. 47. In looking up this reference I find it is simply mentioned under *Ursus arctos*, as follows: 'Ursus maritimus albus major arcticus'; with a reference to Martën's Spitzbergen, and concluding with a note doubting the specific distinctness of this bear. A question as to the value of this reference was referred to several noted authorities on the Mammalia, whose answer did not sustain the reference, and induced me to examine the case closer. The next date when any mention of the Polar Bear was made was 1776, when Müller and Pallas each gave it a name. Müller in his Zoologiæ Danicæ Prodromus, etc., p. 3, refers to it as a variety of *U. arctos*, calling it *U. albus*, but giving only a reference to Martën's Spitzbergen, and a short note on its habitat. Pallas, in his Reise, III., bh. 2, p. 691, describes this species as *U. marinus*, with a good diagnosis, which proves he knew the animal very well. As the name of Pallas is undoubtedly the best, being accompanied by a good description, therefore the name of the Polar Bear should be *Thalarctos marinus* (Pallas). The reference is Reise, III., bh. 2, p. 691, 1776.

James A. G. Rehn.

Academy of Natural Sciences, Philadelphia, August 7, 1899.

THE INTERNATIONAL CATALOGUE OF SCIENTIFIC LITERATURE.

To the Editor of Science: A few days after contributing to your esteemed journal my remarks upon the bibliographical methods proposed for the Catalogue of Scientific Papers I received a report of a committee of Dutch scientists, whose conclusions are diametrically opposed in certain points to the opinions which I expressed. Impartiality requires that I should not pass this criticism unnoticed.

Let me translate from the French text: "The adoption of the Dewey Decimal Classification having been favored by certain persons, we wish to state our opinion in regard to this system. This opinion is very unfavorable. In our opinion the adoption of the system would lead to the failure of the enterprise.

"Our conviction in this matter is based upon the faulty manner in which the system has been worked out for various sciences in the 'Decimal Classification and Relative Index' of Mr. Dewey (1894, Library Bureau, 146 Franklin Street, Boston; 21 Bloomsbury Street, London).

It is possible that a classification such as is here proposed may serve for arranging a library containing principally popular science works or pedagogy, but for the end proposed by the International Catalogue it seems to us inapplicable. In regard to certain sciences, notably mathematics, physics, astronomy, it seems to us scarcely possible that there can exist in this regard a serious difference of opinion among persons really competent to judge.

"Furthermore, we reproach the system as worked out by Dewey with being inelastic. In view of the very restricted number of places left vacant, the addition of new subjects of a fundamental character can soon be accomplished only by very artificial means, and, moreover, it would require the use of a disproportinate number of the former figures."

On reading this report one can hardly fail to be struck with the emphasis that has been laid upon the book that has been used as an authority, pains being taken to give even the street number of the firm selling the work in Boston and in London. This emphasis is, of course, in part, due to the fact that the committee wished to prove that its criticism was not made without examining the 'Decimal Classification' of Dewey, a neglect which has been admitted by certain other critics. But to those who have followed the matter closely it will be apparent that this assertion has a deeper meaning. It is a frank declaration that the committee declines to examine the application of the decimal system to card bibliographies. Dewey, as is well known, never proposed the use of his system for bibliographies. It is an application which I believe was first made by Mr. Pickford Mann, but which has since found wide extension largely in consequence of the effort of the International Institute of Bibliography in Brussels. Now, this statement is intended as a disavowal of these applications. Such a proceeding is manifestly unfair. What should we say of a person who should insist upon using a work dealing with electric lighting as an authority for judging the possibility of utilizing electricity for telegraphy? The Brussels Institute took the decimal system, expanded certain parts according to the principles expounded by Dewey, added a few distinctive signs, such as the colon and the parenthesis, and at once the system attained the extreme pliability requisite for bibliographic purposes. For library purposes pliability is a fault, a work on the Locusts of Mexico can not be duplicated under Locusts and under Mexico; but for bibliography this is a *sine qua non*. Moreover, the success or failure of the system in libraries is no valid argument respecting its use for card bibliographies. In library organization the question is whether or not a methodical arrangement of the books according to subject-matter be possible and practical. Where the decimal system has failed, it will be found to have been the strict methodical arrangement that has been found impractical. But for bibliography the arrangement by subject-matter, however difficult to attain, is essential, and *for cards* this presupposes some system such as the decimal system.*

The report states that for various sciences the system has been worked out in such a faulty manner that it seems scarcely possible for a divergence of opinion to exist. The sciences that are selected as examples differ from those mentioned in the memorandum of the Royal Society's Committee. According to the Dutch report, mathematics, astronomy and physics can not be dealt with in this way. To deny the possibility of a divergence of opinion in this regard is certainly too strong. I have laid the matter before the representatives of these sciences in Zürich, and two of them declare themselves pronounced advocates of the decimal system; the third believes it perfectly applicable. For mathematics, Professor Rudio, who has been watching the movement for a year past, feels that certain changes are necessary and pointed out the modifications necessary to bring the scheme into harmony with the Jahrbuch. It is, indeed, my conviction that the objections raised relate to the fact that the classification is conventional, not scientific. But it is easy to show that this is no valid ob-

* It is important to note that, out of over one thousand divisions used by Dewey in the part worked out by the Concilium Bibliographicum, only three have been modified. This is a sufficient answer to those who claim that the system must be totally remodeled.

jection; on the contrary, a classification embodying the latest scientific conceptions is seldom fit for bibliographical work. In the Dutch Academy of Sciences ridicule was cast upon the decimal system because physiology was made a sub-division of medicine. Scientifically it is absurd; bibliographically it is the only wise course. The literature of the past century passes insensibly over into medicine, and a system disregarding this historical fact would be extremely faulty. All attempts at a strictly scientific classification must be personal and liable to change. Most zoologists place *Limulus* with the Arachnids; bibliographically this would be folly. Arachnidologists, collecting the spiders of the various countries of the world, have not yet, at least, become so impressed with this kinship that they seek the seas for *Limulus*; while the malacologists persistently add *Limulus* to their lists of captures. The bibliographical system should correspond with the customs of authors; it is not intended to teach taxonomy.

The assertion that the decimal system is inelastic scarcely needs comment. The system was first published in 1876, with 1,000 divisons, requiring 12 pages of print; to-day by simple expansion nearly 50,000 divisions, filling 400 pages, having been added. For certain sciences the expansion has been continued still further. Indeed, there are now far more divisions in our simple zoological tables than in the entire original work. In certain trials leading up to the establishment of the final system used by the Concilium Bibliographicum the attempt was made to proceed by successive sub-divisions down to families and sub-families. In this experiment as many as a thousand new divisions were introduced at a single point in the series; it is needless to say that no inconvenience was experienced.

It is a pity to see cautious men of science make assertions like this, which have not the slightest foundation in fact. They are so plainly based upon gross misconception that one might well pass them by in silence were it not that they are liable to have weight in deciding one of the most vital questions now before the scientific world.

Herbert Haviland Field.

NOTES ON INORGANIC CHEMISTRY.

The paper read by Collie and Tickel before the Chemical Society (London) on the quadrivalence of oxygen, as shown by the probable constitution of dimethylpyrone, 'an oxygen base' has been recently noticed in this column. In this paper the authors mention that in 1888 J. F. Heyes advocated a similar view to account for such peroxides as MnO_2 and BaO_2. In the last *Chemical News* C. T. Kingzett calls attention to the fact that in a paper before Section B at the Southampton meeting of the British Association, in 1882, he reviewed the modes of formation of ozone and hydrogen peroxid, arguing for the variable valence of oxygen, and adds: "I am not aware that anyone had previously represented oxygen as a tetrad." Being present at the Southampton meeting, I remember Mr. Kingzett's paper very well; indeed, I was so much impressed with it that I have since used the formulæ $O=O^{iv}=O$ and $H_2O^{iv}=O$ in my teaching. I recall, however, that after the session one of the members remarked to me: "Kingzett is right, but there is nothing new in it; I have been teaching that for a number of years." It has long seemed strange to me that the idea of the variability of oxygen's valence has had so few advocates, especially when its position in the periodic system is considered.

In a recent number of the *Archiv der Pharmacie* a new method of detecting arsenic in fabrics is given by O. Rössler. A small piece of the goods is burned in the upper part of a Bunsen flame in a fine platinum spiral, and the arsenious oxid formed collected on the outside of a porcelain dish filled with cold water. The deposit, which is hardly visible, is moistened with silver nitrate. On subsequent fuming with ammonia the yellow precipitate of silver arsenite becomes visible, and then disappears by solution in more ammonia. No data are given as to the delicacy of the reaction, but it must be vastly inferior to Reinsch's test, except for such compounds of arsenic as are wholly insoluble in hydrochloric acid. In the case of the sulfids of arsenic Rössler's test might have a considerable value, as the quantity of arsenic present in such a yellow pigment is not small.