that Diplodus and Xenacanthus were generically identical.

In 1883 Professor Cope (*Proc. acad. nat. sc. Philad.*, p. 108) substituted the name Didymodus for Diplodus, because the latter name had been given in 1810 to Sargus by Rafinesque. The distinguished naturalist was evidently unacquainted with the researches of his predecessors.

There is much variation in the dentition of Pleuracanthus (as we shall now call Diplodus, or Didymodus), but it is rather a variation consequent on position in the jaws than specific or generic; and not only 'the species,' but one and the same species, may 'possess two, three, or four denticles,' but not teeth at all like Chlamydoselachus. However, somewhat analogous teeth are those of the type named Diplodus incurvus by Professors Newberry and Worthen (*Pal. Ill.*, vol. ii. p. 62, pl. 4, f. 4). These were very different from Diplodus, and belonged to a genus called Thrinacodus by St. John and Worthen (*Pal. Ill.*, vol. iii. p. 289, pl. 5, f. 1, 2). But whether the animals armed with such teeth resembled Chlamydoselachus may well be doubted.

In fine, the order called Ichthyotomi by Professor Cope appears to be demanded; but it has nothing whatever to do with the Pternodonta or Selachophichthyoidi, and it may not even belong to the selachians (some of its characters are very peculiar, and resemble those of protodipnoans). Further, the order had already been recognized, defined, and named by Lütken. Didymodus, or Diplodus, and Triodus, can be co-ordinated with the spines, Pleuracanthus, Orthacanthus (pt.), and Xenacanthus. All these names are referable to a single *family* (Pleuracanthidae) of the order Xenacanthini of Lütken. The proposed memoir of Professor Cope will, however, be a great boon to science; and to enable him to co-ordinate his data with those of the earlier paleichthyologists, and thus render it still more valuable, is the object of this communication. Apparently two genera, distinguished by their spines, exhibit the Didymodus, or Diplodus, dentition, — Pleuracanthus and Xenacanthus. Information is especially desirable respecting the character of their branchial apertures.

As to Chlamydoselachus, the anatomy will probably reveal a structure most like that of the Opistharthri (Notidanidae), but of a somewhat more primitive type. Mr. Garman's memoir will unquestionably be of great value, for probably no one is better acquainted with the selachians than that gentleman.

THEO. GILL.

The 'unit of time' controversy.

Upon reading your editorial comments in *Science*, No. 58, upon the 'change in the unit of time' controversy, which close with the words "Unless, then, this matter admits of speedy and permanent decision, the one way or the other, with the entire agreement of all parties to the controversy, astronomy would appear to run the serious risk of forfeiting her claim to a place among the exact sciences," it strikes me, that unless the whole thing is intended as a sarcastic criticism of Mr. Stone, of which there is no evidence, it is about time to call a halt upon some one for loose writing.

If Mr. Stone maintains that a mean solar day, instead of depending upon the actual time of rotation of the earth on its axis and the actual time of its revolution round the sun (and hence capable of determination by actual observation), is an arbitrary interval of time fixed by the dictum (of Bessel, Leverrier, or any other human being) that in that time the earth shall move so far in its journey round the sun (and that is exactly what his theory amounts to), and if he says,[1] "Professor Adams's argument, that 'mean solar time is measured, not by the sun's mean motion in longitude, as Mr. Stone's theory supposes, but by the motion of the sun in hour-angle,' is one that I do not profess to understand," and if he persists in maintaining these absurd positions, then astronomers will simply leave him to himself, for argument in such a case is useless.

As to the relation of astronomy to the exact sciences, let us see how much is the point in dispute. The increasing discrepancy between the formulae of Bessel and Leverrier for the annual mean motion of the sun in longitude is $0''.0602$ per year; that is, six-hundredths of a second of arc while the sun moves 1,296,028 seconds. This amounts to eight-hundredths of a second of time ($0^s.08$) in *twenty years*. Expressed as a ratio to the whole constant, it is .000,000,046, or about 1 part in 21,500,000. The discrepancy between the two best modern determinations — those of Hansen and Leverrier — is only $0''.0043$ per year, or about one-fourteenth of the above; and perhaps it will be admitted by even the most enthusiastic devotees of the 'exact sciences' that this is a fairly well determined astronomical constant. The proper theme for exciting astonishment should be, that Bessel, with the data available in his day, should have been able to determine this, and a dozen other constants, so wonderfully near their true values as modern observations show them to be. Only an intellectual giant of his wonderful skill and indomitable energy could have accomplished such results.

H. M. PAUL.

Washington.

[*Caeteris paribus*, loose writing is much less probable than loose reading. We counsel our correspondent to re-read, and with circumspection. *Science* hopes to present the views of all parties when so expressed as to merit a hearing, and, least of all, takes occasion to espouse the cause of a partisan. The controversy on 'the unit of time' is regrettable; but foreign astronomers are abundantly competent to conduct the discussion, as they have done heretofore, without additions to the literature of the subject on the part of any one here.]

The use of the method of limits in mathematical teaching.

Science for March 14 contains a letter by Professor Safford on methods of teaching the calculus, in which he refers to the 'new method of rates' by the writers, in comparison with the method of limits. The phrase, 'new method of rates,' is quoted from a list of subjects for discussion by the M. P. club, Boston, and was probably intended as an abbreviation of the title of a pamphlet, "On a new method of obtaining the differentials of functions, with especial reference to the Newtonian conception of rates or velocities."

We have more recently published a treatise on the differential calculus, founded upon the method of rates or fluxions, in which the method published in the pamphlet is employed in obtaining the differentials of functions, but which has nothing in common with the methods used by Maclaurin, except the employment of the conception of velocity in the fundamental definitions.

Professor Safford regards the doctrine of 'the survival of the fittest' as having pronounced against the method of fluxions, and in favor of the method of limits. It seems to us that it is rather the *geometrical methods* of Maclaurin and the immediate followers of Newton that have thus been condemned, as com-

[1] *Monthly notices*, January, 1884, p. 81.

pared with the analytical methods and more flexible notation adopted by the followers of Leibnitz.

The Leibnitzian notation, although originally connected with the doctrine of infinitesimals, has now been universally accepted; so that we must inevitably denote an absolute velocity by $\frac{dx}{dt}$, and a relative velocity by $\frac{dy}{dx}$. The question which is still, as it seems to us, debatable, is whether these symbols shall be defined (1°) by the conception of a velocity, (2°) as limits of finite differences, or (3°) as the ratios of infinitesimal differences. The second course arose as a protest against the logical difficulties involved in the conception of infinitesimals: it labors under the disadvantage of attaching no separate meanings to the symbols dx, dy, and dt, and thereby loses much of the advantage of the Leibnitzian notation. This method is best exemplified in the excellent treatise of the late Dr. Todhunter. On the other hand, the employment of the notion of rates in the fundamental definitions enables us to give to the detached symbols dx, dy, and dt, definite meanings which are not of necessity infinitesimal.

It appears to us that this method of presenting the subject is better adapted than that of limits to the purposes of elementary instruction. We do not attempt or desire to dispense with the use of limits, as the following quotation from our preface will show: —

"The distinction between the view of the differential calculus here presented, and that found in most of the standard works on the subject hitherto published, may be stated thus: the derivative $\frac{dy}{dx}$ is usually defined as the limit which the ratio of the finite quantities Δy and Δx approaches when these quantities are indefinitely diminished. When this definition is employed, it is necessary, before proceeding to kinematical applications, to prove that this limit is the measure of the relative rates of x and y. In this work the order is reversed; that is, dx and dy are so defined that their ratio is equal to the ratio of the relative rates of x and y: and in chapter xi., by applying the usual method of evaluating indeterminate forms, it is shown that the limit of $\frac{\Delta y}{\Delta x}$, when Δx is diminished indefinitely, is equal to the ratio $\frac{dy}{dx}$. Thus the employment of limits is put off until we are prepared to show that the limit has a definite value, capable of expression in a language already familiar to the student."

Our experience has been, that the student trained by this method finds no difficulty in passing to the employment of infinitesimals, in obtaining the differentials which are required in the mechanical applications of the integral calculus; for example, those required in the determination of moments of inertia, resultant attractions, etc.

J. M. RICE.
W. W. JOHNSON.

U. S. naval academy.

Silk-culture in the colonies.

In your review of my census report on silk-manufacture in the United States, your critic takes issue with me as to the amount of silk raised in the colonies. He declares that there is a tendency on my part "to depreciate the quantity and quality of silk produced, — a tendency which is natural, and doubtless unconscious in an agent of manufacturers." In support of this grave imputation, your critic adduces two points on which he disputes my proof that certain estimates, hitherto accepted as relating to raw silk, really refer to cocoons, and probably to fresh cocoons. He says, first, that I by no means make it clear that the term 'raw silk balls' really meant cocoons, "as it might apply to the twisted hanks of reeled silk, and the term 'cocoons' was in use at that time." To this it need only be said, that, in the literature of the colonial period, cocoons are frequently designated by the term 'balls,' or 'silk balls.' For instance: —

"Removing your branches from the tables, and your silke-balls or bottomes from the branches 5 dayes after the worke is perfected, the balls are then to be made election of for such seed as you will preserve for the year following. Bonoeill and De Serres do both agree that there should be proportioned 200 balls for one ounce of seed, the balls male and female."

On the other hand, in a widely extended reading on the subject, I have never met with the term 'balls' as signifying reeled silk in any form; and I have no reason to believe that reeled silk was made into balls.

Your critic remarks, secondly, "It is certainly not justifiable to assume that the cocoons were necessarily fresh, as they are not thus handled and marketed." They are so handled and marketed at the present day. Statistics of production in European countries and districts are compiled, based on the weight of fresh cocoons. The commerce in them is very large. Quotations of their market-prices appear, during the season, in trade reports and journals. For instance: in the *Moniteur des soies* of June 30, 1883, under the headings 'Prix des cocons Français' and 'Marchés des cocons Italiens,' there are pages of this sort of information; and it is so well understood as referring to fresh cocoons, that no special designation is used for them: they are simply 'cocons.' Indeed, I am assured, on good authority, that it is only fresh cocoons that go from the producers to the filatures: even if 'choked,' they are accounted fresh.

Is it not justifiable to believe that estimates of the weight of cocoons produced in Georgia, and of what was sent to the filature there, were similarly made; that is, that they referred to fresh cocoons? This view of the case came to me only after months of research and final good fortune in tracing the origin of an historical error. Until then, I had accepted without question the current histories in their accounts of silk production in the colonies. My explanation reconciles their strange discrepancies: before refusing it, should not some other solution be offered?

While differing wholly from the conclusions of your article as to the causes of failure and cessation of silk-culture in this country, I should not have troubled you with a reply to criticisms on my work, had they not contained the imputation to which, with great regret, I have deemed it necessary to refer.

WM. C. WYCKOFF.

Rainfall at Amherst, Mass.

The month of February, 1884, stands alone upon the meteorological record of Amherst college in showing an average cloudiness of seventy-seven per cent of the sky. During the forty-two years which this record covers, in no previous case has the cloudiness of a month averaged more than seventy-four per cent; in only five cases has it reached seventy; the range generally being between forty and sixty, and the mean almost exactly fifty.

The fact that clouds and fog gather only in air containing particles of dust, which has been scientifically demonstrated, suggests the question, whether the volcanic dust from Krakatoa, which in higher air gave to us the brilliant evening skies of December last, may not, in its gradual descent toward the earth, have reached in February the lower level, in which our clouds are formed, and have been the cause of this unprecedented accumulation of them.

S. C. SNELL.

Amherst, Mass.