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.