have come into contact with it during only one part of the year, and are unaware of its seasonal fluctuations.

A permanent marine laboratory, adequately located, engaged actively in research during the whole year, I should suppose to be a cardinal necessity for biological development. As Mayor himself realized, the need is so obvious as to require frequent restatement; his reports, and letters from him, show that the realization of such a laboratory was for him a great hope.

In such a laboratory inquiries become possible which in other situations can hardly be undertaken at all. Temporary social isolation would perhaps have to be faced by resident investigators, and partial loss of contact with libraries; but there are compensations. Time to "sit still and think things over," on the ground, is of tremendous value in itself. The zoologist's business, I take it, is to provide an account of animals, in terms, ultimately, of the properties of materials and of their relations. An enormous segment of this task remains relatively unexplored. A truly scientific natural history of animals, prerequisite for the stability of biological theory, is still for the future. There is here a possibility of huge reward. To grasp it requires intensive work of a character which existing agencies for zoological inquiry do not make possible, for the work can not be done by means of visits to the seashore in summertime. A permanent laboratory in semitropical waters, moderately equipped, with a stationary staff, not cursed with a "program," could justify itself in this necessary work, and that without great expense.

That the only American institution for research in a position to fill this need may fail to do so, seems to me the most serious aspect of the case, rather than the possibility that another summer laboratory may be closed.

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ON TRANSLATING EINSTEIN

TO THE EDITOR OF SCIENCE: Generally I am well pleased with whatever Dr. W. J. Humphreys writes but I can't say I like so much his pleasantly written criticism in *Science* of November 24. He says that he very much dislikes my little article on relativity in *The Scientific Monthly* of November, 1922.

Because, giving the words used the only meanings recognized by layman and scientists alike, save a few specialists, several of the assertions are sheer nonsense. Certainly no system of equations, however clever, can prove to one of common sense, the existence of a real fourth dimension; that time and space are not wholly independent; that just because we and the Martians may be unable to synchronize our clocks there is no "now"; that time is "curved"; that a phenomenon may be seen before it happens; that the mere inclusion of gravitation in a more comprehensive expression eliminates it from nature; and so forth, and so on, through a long list of absurdities—absurd, that is, if their customary meanings be given to the words used.

It is my custom, whenever I get a new scientific book to pick out the most perplexing passage and try to put it into ordinary language. It is more fun, to my mind, than trying to solve the problem of three bodies on a billiard table and pays better. The book I had in hand was the English version of "Time—Space—Matter" by Weyl, the leading exponent of *Einsteinismus* in Germany. The paragraph I selected for translation into the vernacular was the following: (p. 274.)

Every world-point is the origin of the double-cone of the active future and the passive past. Whereas in the special theory of relativity these two portions are separated by an intervening region, it is certainly possible in the present case for the cone of the active future to overlap with that of the passive past; so that, in principle, it is possible to experience events now that will in part be an effect of my future resolves and actions. Moreover, it is not impossible for a world-line (in particular, that of my body), although it has a time-like direction at every point, to return to the neighborhood of a point which it has already once passed through. The result would be a spectral image of the world more fearful than anything the weird fantasy of E. T. A. Hoffman has ever conjured up. In actual fact the very considerable fluctuations of the g_{ik}'s that would be necessary to produce this effect do not occur in the region of world in which we live. Nevertheless there is a certain amount of interest in

speculating on these possibilities inasmuch as they shed light on the philosophical problem of cosmic and phenomenal time. Although paradoxes of this kind appear, nowhere do we find any real contradiction to the facts directly presented to us in experience.

Now I have two favors to ask:

First, that any reader who is interested compare my little skit on "Tangling Up the Time Line" with this and see whether I have made any serious misuse of the text.

Second, that Dr. Humphreys put this same idea into five hundred words so that mathematicians would approve of it and editors accept it. I am proposing this, not because I think that Dr. Humphreys can't do it, but because I know he can. I greatly admire, and have often benefited by, his power of clear exposition and I want him to apply it in this case. I will not only thank him for it but I will pay him for it.

Somebody must do this job of translating Einstein and it ought to be done by thorough mathematicians like Dr. Humphreys rather than by outsiders like myself. I realize that translating mathematics is like translating music. Still I suppose that even the most complicated equation could be put into ordinary language though it would be so wordy and involved that nobody would read it. All that can be done is to give by illustrations and analogies some notion of the conception. I may say that, according to my custom, I submitted my version to a professor of mathematics in one of our leading universities, who specializes in Einstein and I reworked the wording twice in accordance with his suggestions although I will not incriminate him by mentioning his name.

Most of the "long list of absurdities" that Dr. Humphreys mentions are not in the article he criticizes; for instance, gravitation. I know that Einstein has not eliminated gravitation from the universe, for if he had I should have felt a sense of relief amounting to 187 pounds. What he has done is well expressed by Lord Haldane, in his "Reign of Relativity," when he says that Einstein's doctrine "has banished out of physics the necessity of attributing an objective character to gravitation," and he adds "a time may arrive when even the good old name gravitation will not be discoverable in any respectable textbook." The way Weyl puts it is: (p. 226).

> We shall find actually that the planets pursue the courses mapped out for them by the guiding field, and that we need not have recourse to a special "force of gravitation," as did Newton, to account for the influence which diverts the planets from their paths as prescribed by Galilei's Principle (or Newton's first law of motion).

Is not Weyl to be taken literally when he makes such a statement as the following: (p. 278)?

> We conclude that space is closed and hence finite. If this were not the case, it would scarcely be possible to imagine how a state of statistical equilibrium could come about. If the world is closed, spatially, it becomes possible for an observer to see several pictures of one and the same star. These depict the star at epochs separated by enormous intervals of time (during which light travels once entirely round the world).

Professor Eddington of Cambridge, who started the Einstein boom by his report of the British eclipse expeditions of 1919, puts this point still more plainly and literally in "Space, Time and Gravitation": (p. 161)

> Perhaps one or more of the many spiral nebulæ are really phantoms of our own stellar system. Or it may be that only a proportion of the stars are substantial bodies; the remainder are optical ghosts revisiting their old haunts. It is, however, unlikely that the light rays after their long journey would converge with the accuracy which this theory would require.

Both Weyl and Eddington are careful to state that what is theoretically possible may be a practical impossibility and I imitated their caution when I said:

> Such a thing (as the influence of the future on the present) is conceivable in the generalized theory of relativity, though, like most conceivable things, it does not occur, or is never known to occur, in reality.

I submit that this is a fair warning to the reader as to the speculative nature of these deductions and a fair translation of Weyl's words:

> In actual fact the very considerable fluctuations

of the g_{ik}'s that would be necessary to produce his effect do not occur in the region of the world in which we live.

I did not invent Einstein. I am not responsible for the theory of relativity or the deductions made from it by physicists and mathematicians. It seems to me that Dr. Humphreys' criticism should be directed toward them rather than toward their humble interpreter.

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ON THE FORMATION OF FAMILY NAMES LIKE TINGIDÆ

IT is astonishing to observe how great a display of erudition may be made in vain, the net result being error. In recent numbers of SCIENCE Dr. Holland, Mr. A. C. Baker and I have issued manifestoes on how to construct family names based on third declension *i*-stems not increasing in the genitive, and in each case the argument has been vitiated by at least one mistake. However, each author has contributed an item of truth, and it is now possible to settle the matter for good and all.

As Dr. Holland says, the stem of the Latin word *Tinge* is undoubtedly *Tingit-;* but, as Mr. Baker points out, Fabricius did not adopt this word, rather he introduced into the neo-Latin language the word *Tingis,* genitive *Tingis,* stem *Tingi-*. This brings us to my contribution, *i. e.,* that Fabricius considered *Tingis* "his own and indicated what its declension should be"—perhaps a somewhat misleading statement of the idea clearly formulated by Mr. Baker. My argument, however, had the merit of reaching the right conclusion, namely, that *Tingidæ* is the correct form for this family name, and I have no hesitation in diagnosing as pathological the form *Tingitidæ* in this particular case and *Tingiidæ* or its like in all similar cases.

I have always had a vague notion, founded chiefly on unconscious observation, that in forming patronymics from *i*-stems (not increasing in the genitive) the final *i* of the stem is to be dropped; and, indeed, who ever heard of such terms as Apiidæ, Aphiidæ, Feliidæ or Caniidæ, until the publication of the last number of the Proceedings of the Entomological Society of Washington?[1] To confirm or disprove this belief and so to settle the matter beyond question, I lately addressed an appropriate question to Mr. Henry Pennypacker, now of Harvard University and formerly Greek teacher and headmaster of the Boston Latin School. In reply I received the following statement of the grammatical principle concerned, as the joint opinion of my old teacher and of Professor Clifford H. Moore, head of the department of the classics at Harvard:

> Rules regarding the formation of family names which may be described as patronymics are subject to modification not only in the interest of convenience but also of euphony, and in spite of the fact that the stems of the nouns you mention [Nabis, Apis, Tingis, Coris, Aphis] in Latin end in "i" and that the termination "idæ" is conventional in such cases there seems to be no doubt that the spelling with a single "i" carries universal authority and the penultimate "i" is short in quantity.

The authors of the International Code, of course, were fully conversant with this principle and expected it to be applied in connection with Article 4, as it had been in the past.

Lest my acquaintance with the unexpressed expectations of the members of the commission be questioned, I should say that it is founded on three considerations: (1) They were and are educated men; (2) their own works contain no such monstrosities as "Feliidæ" or "Anguiidæ"; (3) authors and editors of standing throughout the world have unanimously acted upon the assumption which I have expressed above.

There remains the widely but not universally accepted belief that priority should obtain in family names, but the Code is not clear on this point (*i. e.,* What determines the type genus of a family?); however this may be settled in future, we arrive in the present instance at the following conclusions: (1) that *Tingidæ* is nomenclaturally and philologically correct, as Westwood was well aware when he proposed the name in 1840; and (2) that it will not be necessary to make the change in hundreds of

[1] The editor, Mr. A. C. Baker, substitutes the term "Aphiidæ" for the term "Aphididæ" used by the author of an article.