which they do not regularly receive reprints from their colleagues.

Is the situation in the colleges to be considered significant or critical, when admittedly the greatest volume of published research in the biological sciences comes out of the universities? It can be shown, I think, that it is of vital importance. A glance at the "American Men of Science" will show that the American scientists have begun their education in two sorts of institutions, roughly classifiable into small colleges and large universities. A count shows the proportion starting in the smaller schools to be about two for every three starting in the larger ones. But a survey of the total enrolment of students found in those of the two types of schools listed in the above publication shows about four times as many for the larger institutions as for the smaller. (In the *New York Times* of November 16, last year, a list shows approximately as many students in some 600 small colleges as in the larger universities. One can conclude that not all the small colleges have contributed to the "American Men of Science.") Thus, upon consideration of the institutions which any contribution at all to make to productivity as noted in the "American Men of Science," it appears that any given college undergraduate has two and two thirds greater chance of ultimate productivity than any given university undergraduate. Furthermore, the readers of Science can think of their own early training, and that of their colleagues, to see where a large proportion of them got their first start. An analysis, too, of the present graduate students of promise in any of the universities would indicate a large number coming previously from small colleges. Obviously, if they started in a small college, it must have been there that their interest was stimulated along their chosen line before they sought advanced study in the larger university. Unless we would kill the goose that laid the golden egg, we must not overlook the small schools, even though they be relatively unproductive of important finished research. Let our colleagues in the universities with access to the complete files of the chief journals remember the close relation existing between the colleges and the graduate schools, and help us in feeding to them the student with the inquiring mind.

To take the place of the commercial and industrial concerns standing back of the *Abstracts* of our colleagues, the chemists, we shall probably have to depend always on some sort of philanthropic support outside our circle of scientific investigators and teachers. This is the more true since the number of articles to be abstracted greatly exceeds that in the field of chemistry, and this is a correspondingly greater undertaking. But a united front on the part of the biologists of the country in fully approving the continuance and the continued improvement of *Biological Abstracts,* and their tangible support of the undertaking by subscriptions, will do much. In that direction an unmistakable evidence of support has already been indicated in the well-known fact that the publication in question has more subscribers than any other technical biological journal.

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THALLIUM POISONING

Dr. Brooks' recent note in Science for January 22, 1932, page 105, is unusually interesting. Until about a year ago, I was scarcely aware that such a toxic element as thallium existed. In the *Journal of the American Medical Association,* May 30, 1931, pages 1866–1868, appeared three different articles on the toxic effects of thallium, as used in a cosmetic preparation for removing hair. In the same journal for September 19, 1931, page 851, appeared an account of two fatal cases of thallium poisoning, due to the administration of the acetate as a depilatory in scalp ringworm. The same journal contains various other notes on the toxicity of the element. On January 30, of this year, page 406, is an editorial comment on fatal human cases due to eating poisoned grain. In the issue of February 20, 1932, pages 618–620, is a very complete note on thallium poisoning from a depilatory cream. A discussion of the element, from its discovery to its dangerous therapeutic use and toxic properties, is given. Its toxicity and pathologic effects on higher animals appear to be well known.

It is quite a surprise to learn that it seems to be equally toxic for vegetation. The amount of thallium distributed in poisoned grain for destroying rodents and other forms of life is quite appalling. In its use in destroying ground squirrels, it would be interesting to know if there is any likelihood of the thallium treated grain being placed in the same situation year after year. That is, are the old burrows occupied by incoming ground squirrels? If such is the case, there would seem to be danger of causing patches of soil sterility. All in all, thallium apparently is a dangerous poison, and it would be well to restrict its use in human medicine and wholesale poisoning activities for lower forms of life, until more is known about its action and the habits of the animals against which it is used.

It is interesting to note that the *Journal of the American Medical Association* for February 27, 1932, page 741, contains a news item that the poisonous depilatory cream has been prohibited for sale in San

Francisco. However, the State Department of Agriculture of California (Serial Publication No. 109) uses thousands of pounds of thallium coated grain in rodent control work. In spite of careful supervision, several fatal cases[1] of thallium poisoning in man have already occurred, as well as some losses to domestic animals, according to press dispatches.

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THE EXISTENCE OF AN EXTRA-PANCREATIC (OR CELLULAR) INSULIN IN THE DOG AFTER PANCREATECTOMY?

A RECENT article by Tuttle[1] offers a reinterpretation of some results published by the present writer in the paper entitled "The Utilization of Carbohydrate by Totally Depancreatized Dogs Receiving no Insulin."[2] While granting that the results in question undoubtedly show the utilization of large amounts of carbohydrate by such animals, Tuttle interprets this as being evidence for "the existence of an extra-pancreatic (or cellular) insulin in the dog after pancreatectomy."

I should like to point out that this hypothesis was considered in my paper, but rejected in favor of the "Overproduction Theory of Diabetes," for the following reasons:

(1) The animals did not survive indefinitely, as they do with insulin injections. Nor can the death of these animals be ascribed to the withdrawal of raw pancreas from their diet, as Tuttle suggests. Reference to a later paper by Hershey and the writer[3] will readily show the difference in the time periods involved and the entirely different pictures presented by these two syndromes.

(2) Dogs 1 and 14 (Tables I and IX), which according to the results obtained should have acquired a fairly adequate extra-pancreatic insulin supply, were as "diabetic" as ever when insulin injections were resumed and then discontinued for the second time.

But perhaps a greater objection to Tuttle's contentions is based upon what the writer believes to be a fundamental error in the logic employed. Tuttle's major premise is that "insulin is necessary for the oxidation of glucose." It must be pointed out, however, that this belief is largely based upon the supposedly fixed and peculiar nature of the D:N ratio and the respiratory quotient in the diabetic organism. Since the validity of these phenomena is the very thing which my results have disproven, Tuttle has, in effect, assumed as true the very thing he set out to prove.

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[1] A preliminary report of these cases appears in the *Journal of the American Medical Association* for March 26, 1932, page 1076.

[1] *New Eng. Jour. Med.*, 206: 8, Jan. 7, 1932.

[2] *Jour. of Nutrition*, iii: 99, Sept., 1930.

[3] *Am. Jour. of Physiol.*, 98: 74, Aug., 1931.

THE MUSKRAT, A NEW HOST FOR PARAGONIMUS

DR. E. W. PRICE[1] has recently described four new species of trematodes from the muskrat, *Ondatra zibethica,* and in the same paper included a key to all the known trematodes of this animal, but the mammalian lung fluke, *Paragonimus,* was not listed. I pointed out in a recent paper[2] that I had been able to infect muskrats with *Paragonimus* in the laboratory, but that I had not found this parasite in a collection of 249 wild muskrats from Michigan, chiefly from the southeastern part of the state. Since that paper appeared, this parasite has been found in wild muskrats from two localities in western Michigan. During November, 1931, four out of 34 muskrat carcasses secured near Manistee for the use of the class in parasitology were found to be infected with *Paragonimus.* Later, two out of 19 carcasses from the same source and four out of 26 from Muskegon examined by me were infected. Three of them bore unusually heavy infections. Thus, during the fall of 1931, 79 muskrats were examined, of which 10 (12 per cent.) were infected.

In the same paper indicated above, I expressed an opinion that the mink is the normal definitive host of *Paragonimus* since a 17 per cent. infection was found in a collection of 563 carcasses of minks from Michigan and northern Ohio. *Paragonimus* was not found in 308 raccoons, 109 opossums, 22 weasels and one badger from the same region examined during the same period. Wallace[3] reported a 7 per cent. infection in 234 minks which he examined at fur farms in Minnesota.

The discovery of *Paragonimus* in such a high percentage of wild muskrats from a limited locality warrants the assumption that, under certain conditions, these animals serve as well as the mink in the capacity of definitive host.

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[1] E. W. Price, "Four New Species of Trematode Worms from the Muskrat, *Ondatra zibethica,* with a Key to the Trematode Parasites of the Muskrat," Proc. U. S. Nat. Mus., 79: art. 4, 1–13, 1931.

[2] D. J. Ameel, "More Data on the Lung Fluke, Paragonimus, in North America," SCIENCE, n. s., 74: 493–494, 1931.

[3] F. G. Wallace, "Lung Flukes of the Genus Paragonimus in American Mink," *Jour. Am. Vet. Med. Asso.*, 31: 225–234, 1931.