studied in detail, presents results that may not be without interest to this Society, even in an Annual Address.

The observations I refer to were made with a view to discovering whether it was possible by change of environment, in minute life-forms, whose life-cycle was relatively soon completed, to superinduce changes of an adaptive character, if the observations extended over a sufficiently long period.

For such observations it is manifest that the lowest forms of the infusoria offer suitable subjects.

In themselves and taken by themselves, these organisms, under such experiment must afford instruction, if we can obtain results. But it is also of interest to remember that the inference that the higher and more complex animals and plants are vast aggregations of cells differently endowed in different parts of the organism, but all functionally united and correlated to secure the life of the living thing they compose, is an admitted fact in biology. This must add, indirectly, a further interest to the subject.

Few biologists need any direct demonstration to convince them of the truth of Darwin's great law of the origin of species. It underlies as a necessity all our widest and deepest biological knowledge. Concurrent adaptation to concurrent changes of environment is in fact so apparent now, that we wonder, often, why it was not earlier seen.

Nevertheless, if it be possible to look upon the progress of changes in minute living organisms, superinduced by elected changes of environment, however simple, and which results in morphological and physiological adaptations and survivals, it cannot be other than a gain both to philosophical and practical biology.

Before actually setting up a definite line of procedure, I spent a year and a half in tentative experiment; and very soon found that the best subjects for my research would be the monad forms I had become so familiar with, and the phases of whose life-history I knew; and that the best and most amenable agent I could use for altering slowly and cumulatively the environment, was heat.

After the year and a half of trial I obtained certain very definite results, which it appeared to me pointed to the possibility of obtaining others of a far higher meaning and value, if the methods of conducting the inquiry were carefully devised, and for an indefinite time continuously operative.

At this time I was closely tied to a provincial town, and had little opportunity for consultation with leading men of science; but amongst the few who influenced my determination was the late Chas. Darwin. He had shown great interest, and given me great encouragement in prosecuting the life-histories; and in correspondence, amongst other things, I gave him details of the imperfect but still interesting results I had obtained by thermal experiments on these forms, and the preparations I was making for systematic inquiry in that direction. After words all too generous, he said in his reply, which was dated July 2nd, 1878, "I did not know that you were attending to the mutation of the lower organisms under changed conditions of life; and your results, I have no doubt, will be extremely curious and valuable. The fact

which you mention about their being adapted to certain temperatures, but becoming gradually accustomed to much higher ones, is very remarkable. It explains the existence of algæ in hot springs. How extremely interesting an examination under high powers on the spot, of the mud of such springs would be."

Shortly after this I brought my tentative and experimental work to a close, and commenced the course of observations I shall here detail.

I need not remind you that all biological changes must be slow. Variations are constant, of that there can be no doubt; and under domestication they are very palpably increased and conserved.

But the smallness of every variation, as a rule, and the relative fewness of the generations that come into existence, even of prolific animals and plants during the working life of an observer, to say nothing of the difficulties that would present themselves in other ways, make anything like individual observation on visible forms under experiment almost impossible and hopeless.

Save for the useful and remarkable modifications effected in animals and plants under domestication, the great process of biological progression, made clear to us by Darwin, is essentially a secular one, and is comparable to the vast secular processes of astronomy, such as the precession of the equinoxes; which, although we have observed but a minute fraction of the complete cycle of movement, leaves us as certain of what that cycle is as though we had traversed its immense circumference under continuous observation.

But this very fact, as in astronomy, so in biology, makes any observed facts that may come within our reach, or be possible to our laboratories, of even enhanced value.

Now in the Infusoria—say the septic organisms—the cycle of life is so relatively short, and the generations succeed each other so rapidly, while the successive progenies can be so easily observed, that if we can devise apparatus and conditions which will enable us to institute slow changes of environment, we should be able to observe critically how far changes in the organisms led to responsive adaptations and successive survival. At the same time it would be possible to closely investigate the condition and appearance of the organisms themselves.

To know the living forms under experiment, through all the changes of their life-cycle, was therefore important; and I chose such of the monads whose life-history I had worked out, as were most easily obtainable and most abundant.

Every ingenious mind will have its own suggestion for the modification of the environment of such organisms; but after the tentative work I had already done, and in view of the fact that at that time the question of the influence of heat on this whole series of putrefactive organisms was being eagerly discussed, I determined finally to make cumulative increments of heat the means of adverse environment; I wanted therefore a delicate thermostat, that should be capable of alteration at will to the temperature at which it should again become static.

After I had made a considerable number of preliminary experiments, and had been aided by suggestions, especially from my friend Mr. Joseph