atomistics may be useful; and a middle field when the usefulness of atomistics is doubtful, and to this last field only does Boltzmann's contention apply that both methods should be developed together.

Whether or no we may agree with Boltzmann as to the atomistic basis of the calculus, or the propriety of Volkmann's classification, we shall be inclined to hold his main contention as sound and conservative, albeit on utilitarian grounds. Both the language of mathematics, the medium of expression of the phenomenologist, and that of the atomisticist are but methods, after all, human instruments, ingeniously devised and beautifully developed, but merely instruments.

Whether we shall obtain a theory for observed phenomena which shall be as comprehensive as the atomic hypotheses without its adherent drawbacks, as flexible, as labor saving, as suggestive as the calculus, without its complexity in certain desired applications, by the absorption of one method by the other, it is not possible to say as yet. Quite possibly the ideal theory is to come from an entirely different direction. But for the present we must use those instruments which are at hand, and as long as they prove useful each in itself is worth the highest development we can give it. The idea expressed by one of the previous speakers this evening, that the coming generation will shake off the fetters of 'mechanism' and concern itself with 'parameters' may be true, but it seems to me a rather bold prophecy. More likely is it that notions of the 'atoms' and 'parameters' will develop side by side, the distinguishing feature of their study being a clearer view, a *realizing sense* of their exact relationship to phenomena. And with this it seems fair to assume will come those new principles which Ostwald has prophesied to account for phenomena, where the 'energistics' has failed and whose form it is futile to predicate at present.

To this end the discussion was probably necessary and should prove most useful. It is to be regretted that the time allotted me will not suffice to call to your attention the views of other thinkers on this most interesting subject. I trust that what I have been able to bring before you will at least indicate the status and importance of the subject.

FRANK K. CAMERON.

ON ARTIFICIAL PARTHENOGENESIS IN SEA URCHINS.

IN the last October number of the *American Journal of Physiology* I published a preliminary note on the artificial production of larvæ from the unfertilized eggs of the sea urchin. I mentioned that unfertilized eggs were able to develop into normal plutei after having been in a solution of equal parts of a 20/8 n $MgCl_2$ solution and sea water for about two hours. The control experiments by which the possibility of a fertilization of these eggs through spermatozoa had been excluded were briefly mentioned. In the April number of the same journal a full description of my experiments was published which I believe puts an end to any doubt concerning the possibility of an error. Nevertheless I decided to repeat these same experiments with the additional precaution of using *sterilized* sea water. Through the kindness of the board of trustees of the Elizabeth Thompson Fund I was enabled to make further experiments on artificial parthenogenesis at the Pacific Coast. These experiments have led to a number of new results which will be published in the *American Journal of Physiology*. Here I will confine myself to a description of the precautions which were taken in these experiments to exclude the possibility of a fertilization of the eggs through spermatozoa.

The sea water used for these experiments was heated the day before, very slowly, to a temperature of from 50 to 70° C. and was kept at that temperature for about ten minutes and allowed to cool very slowly. The control experiments proved that, as was to be expected, the spermatozoa are killed by this treatment. During the time the water was heated no sea urchin was opened in the laboratory or was even kept there. The sterilized sea water was kept in special flasks and covered jars which were utilized for this purpose only. Before we started an experiment we disinfected our hands thoroughly with soap and brush in the same way as is customary in a surgical operation. Every sea urchin before it was opened was exposed for from two to five minutes to a powerful stream of fresh water and care was taken to wash the whole surface of the animal as thoroughly as possible with fresh water. The mouth of the sea urchin was then cut out with scissors that had been sterilized the day before in the flame and had been kept dry since. Through the excision of the mouth the sexual glands were exposed and their color allowed to decide whether the animal was a male or a female. If the first animal that was opened was a female the intestine was removed with a sterilized forceps and care was taken not to bring the forceps in contact with the ovaries or with the outside surface of the animal. After the intestine had been removed and nothing left except the ovaries, the inside of the animal was repeatedly filled with fresh water and washed out. Then each of the five ovaries were taken out *in toto* with a sterilized section lifter and special pains were taken that the ovaries did not come in contact with the surface of the sea urchin or with the hands of the experimenter. The ovaries were first put into a dish of fresh water, were washed off carefully and then put into sterilized sea water.

One part of the eggs was put into sterilized sea water to serve as control material. A second portion was put into a mixture of equal parts of sterilized sea water and a 20/8 n $MgCl_2$ solution. An hour or two later these eggs were taken out of this mixture and put into sterilized sea water. While of the latter eggs as many as 25 per cent. developed into blastulæ and swam around the next day, not an egg of the control material even segmented. We spent hours searching the control material for segmented eggs but were never able to find a single one.

In addition to these control experiments we made several others. It was necessary to apply the mixture of equal parts of the 20/8 n $MgCl_2$ solution and sea water for from one to two hours in order to bring about the development of the unfertilized eggs. We made it a rule to take out one portion of eggs from this solution much earlier—in some cases after ten minutes. In no case did one of these eggs segment or develop.

A third series of control experiments was applied. Solutions with less $MgCl_2$ and more sea water were tried. In solutions of 30 cc. 20/8 n $MgCl_2$ and 70 cc. sea water not an egg was able to develop.

If the first animal opened in these experiments happened to be a male the instruments were at once laid aside for disinfection and the next animal was opened by another experimenter with the same precautions.

In some experiments we used sea water that had been filtered through a new Pasteur filter. Although no spermatozoa are able to pass through such a filter, the eggs treated with a mixture of equal parts of a 20/8 n $MgCl_2$ solution and filtered sea water developed while none of the control eggs were able to develop.

In one of the former papers I mentioned the fact that the mixture used for artificial fertilization killed the spermatozoa in a comparatively short time and injured many of the eggs. Contrary to the common prejudice, it is a fact that spermatozoa are much more sensitive and are killed much sooner than the egg.

My experiments at Pacific Grove were carried on with *Strongylocentrotus franciscanus* and *purpuratus*. In both animals artificial parthenogenesis can easily be accomplished.

In the experiments at Pacific Grove I enjoyed the valuable assistance of Mr. W. E. Garrey.

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April 3, 1900.

A CURIOUS PHASE OF INTER-STREAM EROSION IN SOUTHERN OREGON.

THE 'Rogue River Valley,' in southwestern Oregon, is one of the five great depressed areas of the Pacific Coast country, which separate the Klamath and Coast ranges from the Sierra Nevada and Cascade Mountain systems. Its main stream, the Rogue River, issues from deep cañons in the Cascade Range, and flows thence, in its middle course, through a broad valley whose floor is a flat plain, two to five or more miles in width. In crossing the basin, the stream found soft strata to work upon, and not being obliged to cut deep, it eroded a broad valley, strongly contrasting with the cañons above and the narrow, rocky gorge in which the river makes its passage through the Klamath Mountains near the sea. All the tributary streams within the area of the basin have eroded similarly broad, flat-bottomed valleys, and between them they have reduced to a local base-level, the greater portion of an area forty or fifty miles in length by twenty to thirty miles in width. Within these limits there are many hills and low mountains, remnants of the Tertiary strata in which the broad valleys are excavated, but they are quite insignificant in comparison with the high mountains which enclose the basin, of which the Siskiyou range on the southwest rises to 7000 feet and over, and the Cascades on the east to 6000 feet on the average, surmounted by the beautiful volcanic, snow-clad cone of Mt. Pitt.

Since the partial base-leveling of the 'Rogue River Valley,' which doubtless was accomplished nearly at sea-level, the territory has been elevated and the basin tilted, mainly toward the northwest. The valley plain descends from an altitude of about 1900 feet at Ashland to less than 1300 feet where the C. & O. R. R. approaches the Rogue River. In ascending along the river, the gradual rise in the plain is everywhere quite perceptible, and it has attained an altitude of approximately 2000 feet where the main stream issues from the foot-hills proper of the Cascade Mountains. This tilting has increased the gradient of the streams, causing them to cut below the old level, and all the principal ones now flow in comparatively narrow, sharp-cut, cañon-shaped troughs, excavated from 30 to 75 feet below the valley plain. These cañons are few and widely separated, telling of the youthfulness of this new cycle of erosion.

The inter-stream tracts are broad plains, undissected by deep gulleys. Some portions of them are without timber or even chaparral, although generally supplied with a sparse growth of grass, and in the vernacular of the country are known as deserts. It is on these 'deserts,' some of which are four or five miles in length and one to three miles in width, that is developed the peculiar type of surface erosion which has given rise to this paper.

When viewed from a distance, the surface of the 'deserts' appears to be remarkably even, suggesting an absolutely uneroded, water-laid deposit such as might result from the complete filling of a broad, shallow lake basin. But, upon endeavoring to cross these barren plains in the rainy