tense look of cunning as he set about to obtain the cane.

First agreeing that the terms 'reflex action,' 'instinct,' and 'reason' shall be defined according to the definitions of Dr. Romanes ('Animal intelligence,' p. 17), the action, or series of actions, executed by No. 2 must have been, wholly or in part, either reflex, instinctive, or rational. If reflex, there must have been: —

(*a*) Particular and often recurring stimuli, to have given rise to the acts of No. 2; and also,

(*b*) The acts must have been adaptive, although not intentional.

Manifestly, these two prime conditions did not obtain, and therefore the acts of No. 2 were not reflex, either in whole or in part. If instinctive, then the acts of No. 2 must have been performed "without necessary knowledge of the relation between means employed and ends attained, but similarly performed under similar and frequently recurring circumstances by all the individuals of the same species."

1° Had No. 2 ceased his current-making when he obtained the piece of wood, his act might possibly have been in part instinctive; but having obtained one object by this means, he seems to set the same cause in action to gain another object, which he conceives to be similarly conditioned, and when, apparently by new observations (data), finding that this second object is confined by a force greater than that which he can command by his water-current, he desists immediately from his exertions, it is evident that three several mental processes have occurred, to wit: —

(*a*) The employment of like causes to produce like effects.

(*b*) The exercise of a certain amount of memory (individual education by experience).

(*c*) The correct estimation of the difference in force, exerted upon the cane, between the water-current of his making and the confining power of his mate's paw, e. g., judgment.

2° By the conditions of our definition, it would be necessary, in order that these acts of No. 2 might be instinctive, that the same should be observed of the majority of polar bears when similarly conditioned. An appeal to facts shows that these acts are rarely executed by bears. Hence it follows that the said acts of No. 2 were not, either in part or in whole, instinctive. Finally, by the conditions of the proposition, these acts, being neither reflex nor instinctive, must be rational, or else did not take place, e. g., either reason must exist in certain bears of the polar species, or the mind of man must refuse to think of the acts of said bears. The only attempt at the vitiation of the foregoing argument is conceived to exist in the fact that it rests upon but one observation.

JAMES P. MARSH.

The eccentricity theory of the glacial period.

Croll's eccentricity theory of the glacial period is certainly an attractive theory. The ingenuity and learning of its author have merited and received universal respect. The proposal thus to link together by one additional tie the sciences of astronomy and geology, is in harmony with that profound sense of the unity of nature, which is a dominant sentiment in modern science. In utter despair of the possibility of constructing any reliable time estimates by measuring the amount of erosion or deposition, every geologist would gladly welcome the opportunity of importing into his science something of the chronological definiteness which has been the boast of the astronomer. And it must, I think, be conceded that no very satisfactory explanation of a glacial period by means of purely terrestrial conditions has been proposed.

Nevertheless, there has always been a considerable degree of skepticism in regard to the fundamental conception of the eccentricity theory. The question whether the conditions of aphelion winter and perihelion summer, in an epoch of great eccentricity, would tend to accumulate snow and ice, and produce a glacial period in the hemisphere so conditioned, has never been so answered as to command universal assent. Indeed, J. J. Murphy has argued, with much plausibility, that the glaciated hemisphere would be the one with perihelion winter and aphelion summer.[1] Others have believed that there would be no appreciable effect in the direction of glaciation in either hemisphere. I desire to call attention to a class of well-known facts whose bearing upon the question has not, I think, been adequately regarded. A very brief preliminary discussion will suffice to show the bearing of the facts referred to.

There would evidently be two marked contrasts in the character of the seasons between the two hemispheres at an epoch of high eccentricity. The hemisphere with aphelion winter would have a long winter and a short summer, while the other hemisphere would have a short winter and a long summer. Again, the hemisphere with aphelion winter would have extremes of heat and cold, its summer being very hot and its winter very cold, while the climate of the other hemisphere would approximate a mean throughout the year. It is by no means certain that the effects of these two contrasts upon the matter of glaciation would be in the same direction. As regards the difference in the length of the seasons, I suppose there can be no doubt that increased length of winter would tend to glaciation. Other things being equal, the longer the winter, the larger would be the proportion of precipitation in the form of snow, and the smaller the proportion in the form of rain. And increased snow-fall would certainly tend to accumulation of snow and ice.

But what would be the effect of the difference in the intensity of the seasons? Would glaciation be favored by cold winters and hot summers, or by mild winters and mild summers — by a climate of extremes, or by a climate of means? It seems to me that a comparison of the northern and southern hemispheres at present, in the matter of glaciation, will suggest an answer to this question. The present value of the eccentricity of the earth's orbit is so small that its climatic effects are completely masked by geographical conditions. The northern hemisphere now has the perihelion winter, and the southern hemisphere the aphelion winter. So far, therefore, as astronomical conditions control climate, the northern hemisphere should have a climate of means, and the southern hemisphere of extremes. But this relation is completely reversed by geographical conditions. The great amount of land in the northern hemisphere gives that hemisphere a climate of extremes, while the vast expanse of water in the southern hemisphere produces a climate of means. This

[1] *Quarterly journ. of geolog. soc.*, xxv. 350, 1869; *Amer. journ. science*, [2] xlix. 115, 1870.

contrast will appear very striking to any one who will compare the maps of January and July isotherms, respectively, for the globe. The two maps will be seen to differ but slightly in the southern hemisphere, immensely in the northern. I know no reason why a contrast between extremes and means in climate, produced by geographical conditions, should have a materially different effect, as regards glaciation, from a like contrast produced by astronomical conditions. It appears, then, that a comparison of the northern and southern hemispheres may show us whether a climate of means or a climate of extremes is favorable to glaciation.

Now, there can be no doubt that at present the southern hemisphere is suffering a greater degree of glaciation than the northern. As the facts are so well known, it is only necessary to allude to them. New Zealand, with a mean temperature about the same as that of Switzerland, has glaciers extending as nearly to the sea-level as those of Norway.[1] Nor is this due to any exceptionally large snow-fall in New Zealand, for the precipitation there is no greater than in Norway, and considerably less than in Switzerland. Tierra del Fuega, with a mean temperature about equal to that of southern Norway, and with a winter temperature no colder than that of Switzerland, has glaciers extending to the sea.[2] The same is true of the island of South Georgia, if, indeed, perpetual snow does not descend to the level of the ocean (as reported by Captain Cook).[3]

It may, I think, fairly be concluded that glaciation depends less upon the coldness of the winter than upon the coolness of the summer. Not a climate of extremes, but a climate of means, tends to produce glaciation. It appears, accordingly, that the two characteristics of the seasons, in an epoch of high eccentricity would tend in precisely opposite directions, as regards glaciation. In one hemisphere, the length of the winter would tend to glaciation, while the intensity of extremes of temperature would oppose glaciation. In the other hemisphere, the shortness of the winter would oppose glaciation, while the approximation to a mean temperature would favor glaciation. The actual tendency to glaciation would be, then, the algebraic sum of two values of opposite signs. In which hemisphere would the tendency to glaciation predominate? And would the absolute value of the algebraic sum of the two tendencies in either hemisphere be sufficient to have any appreciable influence? I simply suggest these questions, making no attempt to answer them.

I may remark incidentally that there is something aparently unsound in the argumentation by which the advocates of the eccentricity theory seek to show that the hot perihelion summer would not melt the snow and ice. They virtually deny that the perihelion summer would be hot, urging that the temperature could not rise above the freezing-point until the ice was all melted.[4] It may well be conceded that the summer temperature could not rise much above the freezing point in the centre of a polar ice-cap, or at the apex of a snow-capped peak. But at the margin of a snow-field, polar or alpine, the climatic conditions would be very different. The ice-fields of a glacial period would not be created instantaneously in their maximum extent, but would be the results of a slow accumulation for many centuries. As each hemisphere in turn gradually approached the condition in which the climax of its winter would fall in aphelion, the snow-fields would be at first of very small extent. Outside the boundaries of those snow-fields, the land would be heated to a temperature increasingly hot, as year by year the climax of the summer approached the perihelion; and that high temperature of the surrounding areas would produce rapid melting at the margins of the snow-fields. Moreover, even at the extreme of glaciation, the area covered by ice would form but a small part of the surface of a hemisphere. Cold aphelion winters must be accompanied by perihelion summers not only potentially but actually hot.

WILLIAM NORTH RICE.
Wesleyan University, Middletown, Conn., Aug. 16.

[1] *Science*, iv. 426, 1884.

[2] Darwin, 'Journ. of researches during voyage of H. M. S. Beagle,' p. 224. N. Y., 1875.

[3] Lyell, 'Principles of geology,' vol. i. p. 242. N. Y., 1872.

[4] Croll, 'Climate and time,' pp. 58–67. New York, 1875.

The causation of pneumonia.

In *Science* for Aug. 13, 1886, p. 133, I notice a paragraph relative to results of observations by Dr. Seibert of seven hundred and sixty-eight cases of pneumonia, wherein it appears that pneumonia prevails to its greatest extent "whenever there exists a low or falling temperature, with excessive and increasing humidity, and high winds." This reminds me that readers of *Science* may be interested to know that facts respecting a very much larger number of cases, and respecting pneumonia in different parts of the United States, in England, and in India, — that is to say, in several climates and under different conditions, — confirm to some extent the conclusions reached by Dr. Seibert, as mentioned by *Science*. Such statistics, presented by abstract at the last meeting of the American climatological association, demonstrate, I think, that the sickness from pneumonia is absolutely controlled by the temperature of the atmosphere. The higher the temperature, the less the sickness from pneumonia; and the lower the temperature, the more the sickness from pneumonia. This is equivalent to saying that that part of the conclusion of Dr. Seibert which relates to humidity is an error; because the absolute humidity of the atmosphere is, speaking roughly, inversely as its temperature, and there is most sickness from pneumonia when, or soon after, the air is driest absolutely; and there is least sickness from pneumonia when, or soon after, the air contains the most vapor of water, that is, when the temperature is highest. The error of many who have written on this subject, and probably the error of Dr. Seibert, consists partly in calling the 'per cent of saturation of the air' (technically known as 'the relative humidity,' the humidity of the atmosphere. But the curve for 'relative humidity' is not, when inverted, the same as the curve for pneumonia, as you may see by comparing such curves, on the diagrams I published, based upon over twenty-seven thousand weekly reports of sickness in Michigan, by observers in different parts of the state, and upon over one hundred and twenty thousand observations of the psychrometer during the same time, namely, the seven years, 1878–84. Relative humidity seems to have an opposite relation in the warm months to what it has in the cold months. The fact, which I think I have completely demonstrated, is, that pneumonia is quantitatively proportional to the coldness and dryness of the atmosphere;