Baubree has produced artificially one variety (lherzolite) by dry fusion, but this appears to be the first clear case of a peridotite volcano with peridotite ash.

Perhaps an analogous case is in Elliot county, Kentucky, where Mr. J. S. Diller has recently described an eruptive peridotite which contains the same accessory minerals as the peridotite of Kimberley, and also penetrates and encloses fragments of carboniferous shale, thus suggesting interesting possibilities.

H. CARVILL LEWIS.

The eccentricity theory of the glacial period.

I desire to add a supplementary note to my letter of Aug. 16, published in the issue of *Science* for Aug. 27.

In that letter I called attention to the contrast between the northern and the southern hemisphere in respect of glaciation, as tending to show, that, other things being equal, a climate of means (mild winters and cool summers) is more favorable to the accumulation of snow and ice than a climate of extremes (cold winters and hot summers). The bearing of this proposition upon the eccentricity theory is pointed out in my letter.

I now wish to call attention to another well-known geographical fact, which seems to confirm the conclusion that glaciation is favored by a climate of means rather than by a climate of extremes. I refer to the altitude of the snow-line in torrid, temperate, and frigid zones respectively. At the equator the snow-line falls below the annual isothermal plane of 32° F.; while, as we recede from the equator, the snow-line rises above the plane of 32°. So far does the snow-line rise above the isothermal plane of 32°, as we go polewards, that, while the latter plane reaches the sea-level not far from 60° latitude, it has been doubted whether in the northern hemisphere the snow-line anywhere reaches the level of the sea. According to J. D. Forbes, "the mean temperature at the snow-line near the equator is 34.7°; in the temperate zone it is 25.3°; in the arctic regions, about 21°" (Johnston, *Physical atlas of natural phenomena*, Edinburgh and London, 1856, p. 33). While all such numerical statements of the temperature of the snow-line in different latitudes can be considered only rough approximations, there can be no doubt of the general law that (apart from local abnormalities) the temperature of the snow-line falls as we go from the equator towards the poles. Now, it is also true that the annual range of temperature increases from the equator to the poles. At the snow-line near the equator, the extreme summer temperature is but little above the freezing-point; while at the snow-line in the arctic regions, though the mean temperature for the year falls several degrees below freezing-point, the extreme summer temperature rises far above it. The comparison of the zones of climate leads, accordingly, to the same conclusion as the comparison of the northern and southern hemispheres. The existence of perpetual snow is shown by both comparisons to depend less upon cold winters than upon cool summers.

A very simple *a priori* consideration suggests the probability of the same conclusion which we have drawn from geographical facts. It seems probable, *a priori*, that extreme winter cold cannot greatly increase the amount of snow-fall. So long as the temperature of any place keeps below 32°, the precipitation will be all in the form of snow; and this will be the case when the temperature is but little below 32°, as truly as when it falls far below zero.

Cooling a mass of air from 32° to a lower temperature can produce but little additional precipitation, since the maximum vapor tension at 32° is very little, and the change of maximum vapor tension corresponding to changes of temperature in the lower part of the thermometric scale is very slight. The influx of warm and moist air bearing supplies of vapor is not favored by extreme winter cold, since such extreme cold tends to increase barometric pressure over the area affected. On the other hand, every degree that the summer temperature rises above 32° shows an effective increment of the melting-power of the summer sun. The inference would seem to be justified, that, in any place where the annual mean temperature is below or not much above 32°, the more nearly the extreme summer and winter temperatures approach the annual mean, the greater will be the tendency (other things being equal) to the accumulation of perpetual snow. This *a priori* inference seems to be in exact accord with the geographical facts referred to in this and in my former letter.

WILLIAM NORTH RICE.

Wesleyan university, Oct. 8.

The theory of utility.

In connection with the suggestive article in *Science* of Oct. 1, on 'Launhardt's Mathematical economics,' I would like to offer a new theory of utility, or, rather, to discuss it from a new standpoint, and indicate what I consider to be the error in Jevons's main premise.

Utility, or usefulness, is the satisfying of desires. Desires are always in the present, though many, perhaps the most of them, have a prospective nature. Usefulness is not the capacity or capability of being useful: it is the state or quality of *being* useful. It involves, not a possible, but an actual satisfying of desires: e.g., on a certain day a loaf of bread would have possessed utility for Robinson Crusoe in satisfying his hunger; a second loaf would have possessed utility, not in satisfying the hunger of the morrow, but in satisfying his desire to have the possible wants of the future provided for.

If utility be defined as a capacity to serve man or to satisfy his desires, and by this is meant something quite different from the actual satisfying, it serves no purpose of distinction, for with this definition, when affirming utility to be an attribute of any thing, we must always add, 'under certain circumstances;' and there is probably not a thing in existence but what, under certain circumstances, possesses this capacity.

The confusion prevailing as to the nature of utility has arisen from the fact, that, in discussions upon the subject, the provident trait in man's character has been entirely neglected; for from this trait spring desires which are, indeed, of a prospective nature, but whose satisfaction involves utility as indubitably as does the satisfaction of his physical needs.

Utility being of the present moment, time is not one of its dimensions, as the theory of 'final degree of utility' necessarily presupposes. When Jevons ('Theory of political economy,' p. 51) declares that "utility may be treated as a *quantity of two dimensions*, — one dimension consisting in the quantity, and another in the intensity of the effect produced upon the consumer," — it is clear that the supposed dimension of quantity does not have reference to the

mass simply, but to the duration of the commodity, to the time elapsing while it is being consumed.

The theory of varying degrees of utility seems to have its origin in the fact, that, assuming the provident trait to be perfectly developed, the intensity of our desires of a prospective nature varies with our estimate of probable utility, the probability decreasing as the length of time estimated to ensue before the anticipated satisfaction increases; Jevons's chapter on the 'Theory of utility,' with the necessary changes in phraseology, would furnish an excellent discussion on the subject of desires of a prospective nature, which *do* have two dimensions, — one the estimated intensity of the anticipated satisfaction, the other its probability as affected by the length of time to elapse before its estimated occurrence.

But when we enter upon discussion as to the sources of desires, and how desires may be modified, we must say, with Pascal, "C'est un cercle infini, dont le centre est partout la circonference nulle part."

A. E. ROGERS.

Orono, Me., Oct. 5.

Earthquake sounds.

Does any one attempt to offer an explanation for the sound that preceded and accompanied the late earthquake, or earthquakes in general, where the sounds are noticeable? I supposed it was presumable that they were due to the commotion in the earth's crust caused by the radiating waves. But how can that be, when the earth-waves move six to eight times faster than sound-waves? If that be so, would it not appear as if the sound-waves ought in part to come up after the shock has passed? I was asleep when the first and heaviest shock first reached this place (six miles west of Greensborough, in Guilford county), so I cannot tell to what extent the sound preceded the shock. There were two subsequent shocks which were preceded by low roaring and rumbling, so that we predicted the coming of the earth-waves. I said to my wife, 'Now we will have another shake;' and we waited probably three seconds after I had spoken, when the house began to rock. I do not expect you to write me personally, as you will not likely have time, but, if my question should be worthy of note, perhaps some of the geologists of your company could give us a line through *Science*. JOSEPH MOORE.

New Garden, N.C., Oct. 6.

Unexplained noises.

Your comment on mysterious noises in *Science* for Oct. 1 recalls to my memory a very remarkable instance of the transmission of sound and motion.

On the 14th of February, 1862, I was working with my father in his sugar orchard ten miles west of Madison, Ind., and five miles north of the Ohio River. During the entire morning, which was warm, cloudy, and calm, we heard most distinctly the discharges of heavy artillery. The reports would often follow in quick succession. I, as most lads would have been in similar circumstances, became thoroughly alarmed. I felt quite sure that the whole confederate army was close upon us, since the source of the cannonading seemed to be no farther south than the river.

I finally prevailed upon my father to go home, where we found the inmates of the house greatly alarmed at the noises and the rattling of the windows. The shocks, as I remember them, were much like the slight earthquake disturbances experienced lately in different parts of the country. For several miles along the river these noises were heard and the shocks felt. Nevertheless the day passed, and no invading foe appeared. The morrow brought the news of the bombardment of Fort Donelson.

When it is remembered that Fort Donelson is more than two hundred miles from the locality just described, it is certain that these concussions could not have been carried through the air.

I have been told that the limestone formation coming to the surface along the right bank of the river in Jefferson county, Ind., is the same as that on which Fort Donelson rests. The cannonading which was heard so distinctly that day by hundreds of people in Indiana occurred at Fort Donelson, and the sound-waves were conveyed entirely across Kentucky, and probably at a considerable depth below the surface, by a continuous ledge of limestone. I have thought the phenomena above described worthy of record in your columns. H. W. WILEY.

Fort Scott, Kan., Oct. 8.

How astronomers may work.

In your editorial of Sept. 24, referring to Professor Pickering's plan for making the Harvard college observatory useful to all other observatories, and to astronomers all over the world, you also notice a plan of my own, which I formulate as follows: —

"We mean to put the large telescope (of the Lick observatory) at the disposition of the world by inviting its most distinguished astronomers to visit us one at a time, and by giving to them the use of the instrument during certain specific hours of the twenty-four. In this way we hope to make the gift of Mr. Lick one which is truly a gift to science, and not merely one to California and to its university."

Your comment on this plan is that you suspect that Professor Holden 'was hard-pressed to devise it.'

I trust that your impression will not be shared by Professor Young, if he remembers the discomforts of his expedition to Sherman; or by Professor Langley, if he recalls the hardships of his own to Mount Whitney; or by Dr. Huggins, when he recollects the hundreds of failures which have come in his delicate researches in spectroscopy and photography from the London climate; or by Mr. Burnham, when he remembers how many of the double stars which he discovered at Mount Hamilton with a six-inch telescope were 'difficult' in Chicago with one of eighteen inches. Not to mention any other names, I am sure that these astronomers will feel a sense of gratitude when the facilities of the Lick observatory and the opportunities of its climate are put at their disposition, and will attribute the offer to a generous desire to forward science, and not to a scheme to eke out a scanty income. As a matter of fact, I have directed the policy of the observatory since 1874, and it is a pleasure to me to be able in 1886 to announce a plan which has been constantly in my thoughts for more than ten years, and which seems to me to be a long step in the true direction. I trust it will also seem to be such to my fellow-astronomers. It would have been natural to have looked for the same view from the editor of *Science;* but, as long as the plan commands their respect and my own, it will be carried out. You will have to look to its results to see if it may not eventually command your own also.

EDWARD S. HOLDEN.

Berkeley, Cal., Oct. 2.