mals in all parts of the United States and in foreign lands. In particular he seeks information as to (1) the terms used to start, hasten, haw, gee, back, and stop horses, oxen, camels, and other animals in harness; (2) terms used for calling in the field cattle, horses, mules, asses, camels, sheep, goats, swine, poultry, and other animals; (3) exclamations used in driving from the person domestic animals; (4) any expressions and inarticulate sounds used in addressing domestic animals for any purpose whatever (dogs and cats). References to information in works of travel and general literature will be very welcome. Persons willing to collect and forward the above-mentioned data will confer great obligations on Mr. Bolton. He is already indebted to many correspondents for kind replies to his appeal for the 'Counting-out Rhymes of Children,' the results of which have been published in a volume with that title (London, Elliot Stock). To indicate the value of vowels in English, please use the vowels-signs of Webster's Unabridged, and in cases of difficulty spell phonetically. All correspondence will be gratefully received, and materials used will be credited to the contributors. Address Mr. H. Carrington Bolton, University Club, New York City.

LETTERS TO THE EDITOR.

*** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

Twenty copies of the number containing his communication will be furnished free to any correspondent on request.

The editor will be glad to publish any queries consonant with the character of the journal.

Weather-Predictions.

MR. CLAYTON'S letter on weather-predictions, in the last *Science*, furnishes a very interesting comparison. I find in the Bulletin of the New England Meteorological Society for October, 1887, Mr. Clayton's interpretation and verification by his own rules of the government predictions. These are made generally for the whole of New England, but it is to be presumed that he has made a fair estimate so as to give a comparison with his own predictions for south-east New England. He gives the Signal Service 58 per cent, and himself 85 per cent. It now appears (see *Science*, Jan. 27) that precisely the same predictions, stripped of all ambiguity and narrowed down to a definite locality (Boston), give, by an application of the same rules, 96 and 80 per cent respectively. This striking difference of 43 per cent, in the application of the same rules of verification, shows the absolute need of a fair comparison in weather-predictions, and that, too, between similar things. X.

The Snow-Snake and its Name.

AS my notes on the snow-snake were written partly to elicit information, and partly to point out an anachronism, I am glad to receive so early a reply. I objected, by implication, to the use of misleading terms for what is probably an old game. I am also aware that a Southern Iroquois nation, for over one hundred and seventy years past resident in New York, now has the snow-snake and a name for it; but I did not and do not think the Southern winters appropriate for the game. The description to which I referred was in every way erroneous, and yet was made to have an historic air. But I wished also to learn the extent to which the game was played, North and South, East and West, and it is pleasant to be assured that it "was a favorite out-door sport of the Carolinian and Virginian tribes of Iroquois." I would esteem it a personal favor if Mr. Hewitt will kindly furnish quotations descriptive of its early use south of the James River. They will be prized by me and others, having escaped our attention.

A more important question is raised by Mr. Hewitt. My orthography of the word *ka-wher-tah* needs no correction, as spelling and pronunciation were given me by living Onondagas, not taken from lifeless books. But the point, rather incorrectly stated by Mr. Hewitt, is worthy of attention. It is not the case, as he says, that the letter *r* "does not occur in the speech of the Onondagas of the present time," but it certainly has become obscure and rare. In all our early records the letter is frequent: Zeisberger employed it largely in his Onondaga dictionary; in Schoolcraft's vocabulary I think it is found only in the numerals; among the present Onondagas it occurs but sparingly in proper names and other words. Some time ago my Onondaga friend, Sa-go-na-qua-der, sent me a version of the Lord's Prayer in that language. He was not sure of his spelling, and wished me to revise it with him when next at his house. The letter in question frequently occurred, but the sound was obscure. I went over the version with him syllable by syllable, to get the exact sound, and retained the letter four times as clearly enunciated.

It is probable that some Onondagas have given up the letter altogether, while others retain it, and this would account for variations in orthography. My work for many years has been mainly on the early history and customs of the Onondagas, and notes on their language have been but incidental. I am now offered assistance by them in this, and, if I can carry out a contemplated plan will pay especial attention to the question brought up by Mr. Hewitt. Until I have more original data, it would be out of place, for me to do more than justify my present use. The point is debatable, in a sense, but will require some critical research if we are to know the exact extent which the change has reached.

W. M. BEAUCHAMP.

Baldwinsville, N. Y., Jan. 30.

The Occipito-Temporal Region in the Crania of Carnivora.

IN the Proceedings of the Academy of Natural Sciences for 1886, p. 36, I briefly described, under the name of the post-tympanic bone, an ossicle which lies over the squamosal and opisthotic bones in *Ursus*. I have since examined *Amphicyon*, *Dinictis*, and *Archælurus*. I find that the inferior surfaces of the conjoined bones above named exhibit appearances which resemble those seen in *Ursus*, and make it probable that a post-tympanic bone of larger size than the ursine ossicle was present in these genera. Apart from the bone itself, it is noteworthy that the details in the structure and proportions of the squamosal and opisthotic, as they unite to form the post-tympanic process, afford characters by which these genera can be identified.

I have also found that the species of extant *Felidæ* can also be separated by characters of the tympanic bone, especially by the shape of the tympanic ring, i.e., the part of the tympanic bone in advance of the septum. HARRISON ALLEN.

Philadelphia, Feb. 7.

Monocular versus Binocular Vision.

THERE is an interesting phenomenon which is new to the writer, and which very beautifully illustrates the prevalence of monocular over binocular localization. This explanation which we suggest may or may not be true, but it will certainly lead the way to a better comprehension of the fact in case it cannot be accepted as we explain it. We mention the phenomenon as much to ascertain whether it can be verified by others as to point the way to its explanation. It certainly has an interest in the question regarding the perception of distance and the localization of images in stereoscopic combination.

Take two circles, as in Fig. 1, and combine them by crossing the eyes in the ordinary way. We shall see, as is well known, three circles in the field of view, the central one the combined result of two images, and apparently nearer to us than the other and exterior circles, and nearer also than the sheet of paper upon which they are drawn. It is possible that to some experimenters the central circle does not seem nearer than the other two: to the writer it always does. If we combine them by fixating the eyes beyond the plane on which they are drawn, the central circle will appear larger and farther off than the other two. So much, however, is not new, but it is a necessary preliminary to the singular phenomenon which we have not noticed in any investigation of binocular vision. It is also known that the observer can place a pencil or pin point at the apparent location of the central circle, and it will seem to coincide with it, and there is no hesitation in placing it at a point between the sheet of paper and the eyes.

But now, if we take a fine piece of wire, a knife-blade, a needle, or a sharp pencil-point, such objects being used in order to get double images more easily, and place it a short distance farther off than the apparent position of the central circle while we keep the attention upon some point of the circumference of the circle, at a very short distance beyond the point of fixation the needle or piece of wire will appear double, and represents the ordinary homonymous images, which are the images localized beyond the horopter. We may increase this distance of the needle from the point of convergence, and the distance widens as usual between the images. There is perhaps nothing new in this fact. But if we keep the convergence of the eyes perfectly fixed for the combination of the two circles to form the central one, and turn the attention to the two homonymous images apparently beyond the point of convergence, and without allowing the convergence to change so as to combine the images of the needle, we shall find, by very close attention, that they will instantaneously spring into the position of heteronymous

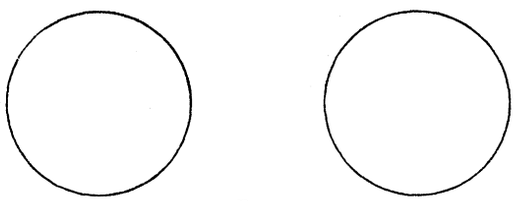

FIG. 1.

images, nearer the eyes than the circle, and without either becoming really heteronymous, or in the least approaching each other. Rivalry often takes place between the two positions, so that the images of the needle will alternately seem nearer and farther than the central circle at the point of convergence.

A beautiful way of testing the same result is to place the knife-point or needle upon the sheet of paper, and coinciding with any point in the circumference, but always allowing the length of the object to lie in, or parallel to, the vertical meridian. If the attention is fixed strongly upon the knife-blade or needle while convergence combines the two circles, the two images of the needle or blade seem to coincide with two of the circles, the central and combined circle, and one of the outer circles. But the central and combined circle seems in the same plane with the sheet of paper and the other two circles. This may vary, however, with rivalry, as experience will show. But if now we begin to move the object toward the eyes, and therefore toward the point of convergence, without altering the latter, and without changing the attention, the two images of the needle or knife-blade will appear nearer than the central circle, and also seem to approach the eyes until they reach a certain point, where they instantaneously assume the homonymous position beyond the central circle. The feeling of surprise is very marked at this sudden appearance of the images at a greater distance than they had just seemed.

If, again, we draw the circles upon a plate of glass in order to combine them by fixating beyond it, and try the experiment as we have described it, the images at first appearing beyond the central circle and homonymous, by close attention will suddenly appear in the heteronymous position, nearer than the central circle, as before. It must be remembered, however, in both cases, that the images do not become really heteronymous, as can be proved by suddenly closing and opening one of the eyes. The same image vanishes in both apparent positions of the double images. The single interesting fact, both when we combine by convergence and when we combine by fixating beyond, is that the two images of the object really beyond the point of fixation will appear at times to be nearer, and will not assume a fixed homonymous position until the attention upon them is relaxed. Now for the explanation.

It is clear that the double images of the needle or knife-blade are simply the ordinary homonymous images, and hence are localized beyond the horopter, or point of fixation. So far the phenomenon only accords with the ordinary law. The anomaly appears when their relative position is changed and they seem translocated into the heteronymous position. But if we revert to the influence of attention in all sensory processes, we may discover a cause for the effect we have described. It is known that we may so absorb our attention as to be unconscious of a severe pain in the tactual sense. Or in vision we may be so occupied with a particular object as not to notice the presence or approach of another. We may even lose entire sight of all objects except the one in which we are interested. Again, it is a universal fact that attention directed to any object in the field of view, at once and automatically sets the eyes into the proper movement for adjustment to produce single vision. At the same time the visual tension of the eyes is relaxed for the object from which the attention is turned. With these simple facts, we may turn to the experiments we have described. Here, when we keep the adjustment for combination constant, but direct the attention to the two homonymous images, the tension for binocular localization is relaxed by the change, and we are left to monocular principles for the localization of the images of the needle as well as that of the central and combined circle. The latter appears in the same plane as the sheet of paper, or approximates it in proportion to the relaxation of binocular tension, and thus introduces monocular influences into the localization of combined images, while only monocular functions are left to localize the homonymous images of the needle or knife-blade. Hence it appears as it really is; namely, nearer than the central circle. We may test whether it is due to the prevalence of monocular over binocular innervation by moving the needle far enough off to make its images coincide, or nearly coincide, with the circumference of the combined circle at the termini of the diameter, and, while they seem in the heteronymous position, suddenly close and open one of the eyes. We shall see the remaining image of the needle apparently nearer than the circle, and in the same position, without change, which it occupied before closing the other eye. The eye must be closed and opened as quickly as possible, so that the other eye will not have time to resume the parallel position, and hence there will be no apparent motion of the circles. This will enable us to determine more accurately the monocular character of the localization of the homonymous images. We see the image of the needle and the circle in the same relative positions as before closing the eye; and, since this can be only monocular, we can best suppose that the translocation we have described is due to the prevalence of monocular functions over the binocular by the withdrawal of attention from the latter.

It is a still more interesting fact that the writer has been able, by considerable practice, to localize one of the images of the needle homonymously under the circumstances described, and the other heteronymously. I have been able to alternate them to some ex-

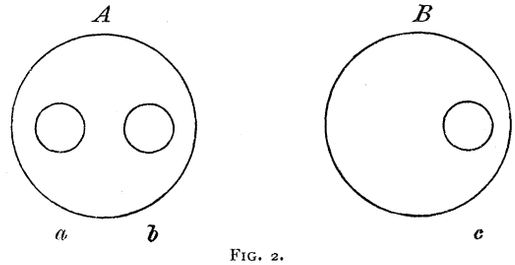

FIG. 2.

tent, although generally it is the left image that appears nearer, and the right image farther, than the point of binocular fixation. In such cases evidently one eye can keep up the binocular innervation, while the other becomes monocular in it. Astonishing and presumptuous as such a supposition may seem, it is entirely confirmed by the following second experiment, which also illustrates the rivalry between binocular and monocular functions, as in Fig. 1. Take the circles *A* and *B*, with the smaller circles *a*, *b*, and *c*, as we have drawn them, and combine them by convergence. It is plain that the fusion of *b* and *c* will take place at the same time with that of *A* and *B*. But *a* has no corresponding circle in *B* with which to fuse. If *b* were absent, the binocular effort at convergence would automatically tend to combine *a* and *c*, so that they would appear nearer than the fused image of *A* and *B* in the precise ratio of the convergence required for their combination. We have elsewhere worked out the explanation of all such localization

in accordance with the reflex innervation, if we may call it such, for adjustment. But we shall not enter upon this in our present problem. We have mainly to notice that *a* and *c* will not fuse while the latter, *c*, can fuse with *b*. Now, as no greater degree of convergence is required for the combination of *b* and *c* than for *A* and *B*, their combined image will appear in the same plane as that of *A* and *B*. This is of course relatively a monocular localization. But, singularly enough, there is a binocular effort, as it were, in one eye, to combine *a* with *c*; and the result is that *a* appears nearer the observer than the combined image of *A* and *B*, without in the least translocating the fused image of *b* and *c* from their position in the plane of *A* and *B*, and without separating them to produce any fusion of *a* and *c*, although the latter can be effected if we will. Rivalry will at times suppress the translocated image of *a*, so that it appears monocularly located in the same plane with *b* and *c*, or *A* and *B*. The alternations may be very distinctly observed. But here we have a very evident case of binocular innervation in one eye, and localization of *a* in accordance with it nearer the observer; while no such binocular translocation and innervation take place for the fused image of *b* and *c*, because it preserves a constant relation to that of *A* and *B*. *b* and *c* sustain the same relations of distance to the median plane, and hence will be monocularly localized in the same position of the third dimension as *A* and *B*, although binocularly combined. Whatever of tension or innervation there may be in the left eye for binocular combination of *c* with *a* is counteracted by the opposite tension to retain the fusion of *b* and *c*, which remains located in the plane of *A* and *B*, or, better, of their fused image. Thus there is left only the binocular innervation of the right eye to translocate the image of *a* to a position nearer the observer than the other images, except when this tension is suppressed by rivalry. Then *a* is located at the same distance as the others. The incident is interesting as showing that there may be rivalry between binocular and monocular functions for localization in the *third* dimension as well as the ordinary rivalry between colors in plane dimension. It confirms also the results of the first experiment we have described.

We have presented these phenomena to suggest the possibility that monocular influences, apparent in the instances noted, may account for many irregularities and illusions in binocular vision as practised by the experimenter to investigate localization. Why may not rivalry between them suppress certain impressions, so that the effect may appear to be different from what it really is? Why may it not account for the failure of stereoscopic combination of two real objects to translocate their fused image to the point of fixation? We do not insist that our explanation must be correct: nor will too great stress be laid upon our conjectures without some verification from the experience of others. To our experience there seems no other way of looking at the matter.

J. H. HYSLOP.

Baltimore, Md., Jan. 31.

Transcontinental Railroads.

IN treating the subject of transcontinental railways, *Science* (x. No. 241) uses language to the effect that the Cascade Range of Oregon and Washington is known to be a continuation of the Sierra Nevada, and mentioning as a striking and all-important structural difference that the Cascades are volcanic, while the Sierra is granitic, therein assuming as facts two propositions which have been much debated, but which, in the present state of geological knowledge, can hardly be demonstrated. In order to learn the progress of opinion respecting the connection of the two ranges, readers of *Science* should consult the *American Journal of Sciences*, third series, vol. vii. p. 177, wherein Prof. Joseph LeConte suggests the idea, original with himself I believe, of the unity of the two ranges in age and cause.

Second, Clarence King, in 'Geology of the 40th Parallel,' pp. 441–454, extending theory far beyond the support of adequate observation, held that the Cascades were separated in age from the older Sierra by a vast time-interval (to wit, the whole of the cretaceous period), and that the Blue Mountains of eastern Oregon were the real continuation of the Sierra.

Third, Dr. Becker of the United States Geological Survey, basing his opinion on the finding of granitic and metamorphic rocks in the cañon of the Umpqua River in the southern Cascades, remarks (see Bulletin 19, United States Geological Survey) that that portion of the range has a foundation similar to the California ranges, and is probably due to the same upheaval. He thus maintains a proper reserve as to the general question.

Lastly, Mr. Diller (Bulletin 33, United States Geological Survey), after examining the stratigraphical relations of the Cascades, Sierra, and Coast Range at their presumed point of divergence in northern California, while quoting Dr. Becker's discovery and opinion, sums up his own conclusions thus: "As far as is definitely known, the Cascade Range was not represented by a ridge of older metamorphic rocks which were folded and upheaved at the same time with the Sierra and the older portion of the Coast Range, and *is entirely distinct from them in structure and origin.*" In another connection he says, "Such rocks [granitic and metamorphic] make up the Coast Range west of Mount Shasta, and it may be that they form an elevated foundation for the Cascades between Rogue River and Mount Hood; but this is rendered less probable by the complete section along the Columbia River, where the range is cut across nearly to sea-level, showing, according to Professor LeConte, that it is made up almost wholly of recent lavas resting on undisturbed miocene strata." Mr. Diller, of his own observation, announces that the Cascades, where intersected by the Klamath River, are also composed exclusively of recent eruptive rocks. Thus the matter stands to-day, and it is doubtful if the question of a simultaneous origin is to be settled on other than paleontological grounds, by a careful and minute comparison of fossil evidences.

The second assumption, that the two ranges differ in the one being granitic, the other volcanic, I dare say, is but the reflection of the common belief which took its rise from the circumstance of the only known or visited section, that of the Columbia gorge, being entirely volcanic, but is nevertheless a most indiscriminating and erroneous opinion, as I will endeavor to show.

I find that the drift brought down by the ancient glaciers of the Cascades, and deposited in the valleys below, invariably contains a proportion, though very variable, of granitic and sedimentary bowlders. In some cases, particularly of certain ice-streams which flowed into the Willamette valley (which, by the way, is covered for the most part with glacial *débris* to a great depth), the granite and metamorphic bowlders and gravel predominate immensely; sometimes, indeed, to the exclusion of volcanic sorts. The prevailing types in most other drift localities, however, are volcanic. The significance is that a part of the rock-masses eroded by the ancient glaciers were granitic and metamorphic beyond a doubt; and, in the cases where transported bowlders prevail, the parent granitic and metamorphic rock-masses from which they were derived must have preponderated over the volcanic masses. I leave the question of the comparative erodibility of the various rock-masses, as well as the considerations arising from their relative positions, all of which must have had influence on the proportions of granitic, metamorphic, and volcanic glaciated bowlders.

But we need not depend upon the accidental evidences of extinct glacial action to prove the composition of the Cascades, for examinations of the range at different points have shown me that it is not exclusively volcanic by any means; indeed, I doubt much if the granitic and metamorphic rocks do not preponderate over the volcanic rocks, viewing the range as a whole. Judging by the evidence of formations *in situ*, I should say, notwithstanding the existence of exclusively volcanic sections, that the foundation of the range in general is not unlike that of the Sierra, excepting that I see no indication of the great orographic blocks which, according to Mr. Diller, compose the northern Sierra.

Judging from what has been published concerning the range, the prevailing idea of its structure seems to regard it as composed of a single anticlinal ridge composed wholly of basalt, and crowned with snow-covered conical peaks set at regular distances along the range. Geologists who have this idea will be surprised to learn that granite appears in the range at an altitude of two thousand feet, within eight miles of the Columbia. This is on the north side of the river; while on the south, towards Mount Hood, it is said to be found at five thousand feet. I cite only the former instance as observed by myself. I also find granite on the Santiam River at a height of five thousand feet above sea-level, and on the McKenzie