also apply to man, the intermarriage of congenital deaf-mutes through a number of successive generations should result in the formation of a deaf variety of the human race." For example: let some of the congenitally deaf marry congenital deaf-mutes; then let some of their deaf children marry congenital deaf-mutes, and some of *their* deaf children marry congenital deaf-mutes, etc., then the percentage of deaf children born of such marriages will increase from generation to generation, until finally all, or nearly all, of the children will be born deaf. The families of which this would be true would then constitute a variety of the human race in which deafness would be the rule instead of the exception.

Now, Mr. Jenkins is greatly exercised over the fact that all the distinguished scientific men whose opinions are quoted in the little pamphlet entitled "Facts and Opinions relating to the Deaf," admit this theory to be true. He gets over the difficulty, however, when he discovers that these gentlemen all belong to a scientific association of which I also am a member; and he says, "A member of their own fraternity has asked them their opinion on a theory of his own formulating; and, in complimentary deference to a great name, they have indorsed the theory."

I need make no further comment upon this than to say that the "fraternity" refers to no less a body than the National Academy of Sciences; and that the gentlemen who are so willing to subordinate their real opinions out of complimentary deference to me are Professor Edward D. Cope, Professor Alpheus Hyatt, Dr. H. P. Bowditch, Professor William H. Brewer, Professor Simon Newcomb, and Professor W. K. Brooks.

But to all his numerous mistakes Mr. Jenkins puts a climax when he credits the above theory to me. Such an error might be pardonable in one not connected with the Hartford School for the Deaf; but it is surely unpardonable that Mr. Jenkins should not know the author of the theory to have been the principal of the very school in which Mr. Jenkins himself is an instructor.

In my "Memoir" (p. 196) I quote the words of the late Rev. W. W. Turner, as follows: "It is a well-known fact that among domestic animals certain unusual variations of form or color which sometimes occur among their offspring, may, by a careful selection of others similar and by a continued breeding of like with like, be rendered permanent, so as to constitute a distinct variety. The same course adopted and pursued in the human race would undoubtedly lead to the same result. . . . Early consideration of philanthropy, as well as the interests of congenitally deaf persons themselves, should induce their teachers and friends to urge upon them the impropriety of intermarriage" (from a paper upon "Hereditary Deafness," published in 1868; for further references see my *Memoir*, p. 196).

The above is the theory for which I have so often been denounced. But the statistics of the "Memoir," to which alone I can lay claim, and which have led me to fear that a deaf variety of the human race is actually in process of formation in America, have never been seriously questioned.

Many statistics have since been collected by deaf-mutes themselves, and by their teachers, to show that there is no cause for alarm; but their figures all demonstrate that the percentage of deaf offspring born of deaf-mute intermarriages is many times greater than the percentage of deaf offspring born of the marriages of those who hear.

The testimony of the present principal of the Hartford School, Mr. Job Williams, is specially strong upon this point, although it is adduced to sustain the opposite contention (see *Facts and Opinions*, pp. 42–50).

In view of these facts, we cannot but note with alarm that many of the most prominent teachers of the deaf in America advocate the intermarriage of deaf-mutes. Dr. Philip G. Gillett, superintendent of the Illinois Institution for the Education of the Deaf, says (*Facts and Opinions*, p. 53), "I do not discourage the intermarriages of the deaf, as they are usually more happily mated thus than where one of the parties only is deaf. The deaf need the companionship of married life more than those who hear, and it is a gross wrong to discourage it."

Dr. Gillett is probably the oldest teacher of the deaf in America, —not oldest in years, but oldest in service, —and he is looked up to as a guide by very many in the profession.

Much good might arise from a comparison of views between Dr. Gillett and those scientific gentlemen who have given most attention to the subject of heredity. May I ask him, through the columns of *Science*, what would be his advice in such a case as the following?—

A young man (not a deaf-mute) became deaf in childhood while attending public school. He has one brother who is a deaf-mute, and another who can hear. Two others of the family (believed to be hearing) died young.

The father of this young man was born deaf in one ear, and lost the hearing of the other subsequently from illness. He had a congenitally deaf brother who married a congenital deaf-mute and had four children (three of them congenital deaf-mutes).

The mother of the young man was a congenital deaf-mute, and she also had a brother born deaf.

The paternal grandmother of the young man was a congenital deaf-mute, and she had a brother who was born deaf. This brother married a congenital deaf-mute, and had one son born deaf.

The great-grandfather of this young man (father of his paternal grandmother) was a congenital deaf-mute; and he was, so far as known, the first deaf-mute in the family.

Thus deafness has come down to this young man through four successive generations, and he now wants to marry a congenital deaf-mute.

The young lady has seven hearing brothers and sisters, and there was no deafness in her ancestry, but she herself is believed by her family to have been born deaf.

Dr. Gillett must not think that this is a purely hypothetical case, for it is not. The parties are engaged, but the marriage has not yet been consummated, and I know that Dr. Gillett's advice would have weight with the young people.

The teacher of the young lady has been consulted, and she feels her responsibility deeply. Her heart is with the young couple, and she desires their happiness, and yet her judgment is opposed to the union.

Will Dr. Gillett tell us what his advice would be in such a case?

ALEXANDER GRAHAM BELL.

Washington, D.C., Sept. 1.

Treatment of Snake-Bites.

IN *Science* of Aug. 22, 1890 (p. 107), it is stated that Professor Kaufmann strongly condemns the use of large quantities of alcohol in the treatment of snake-bites, as he thinks it paralyzes and depresses the nervous system.

Now, this paralyzing and consequent depressing effect of alcohol in snake-bites is just wherein its medicinal or remedial value lies; for by this paralyzing effect, tissue change and general metamorphoses of both the solids and fluids of the body are retarded, and the reactionary susceptibility of the system is blunted and benumbed; so that the venom is more slowly fed into the system, which is, by the paralyzing effects of the alcohol, rendered less susceptible to disturbing influences. Thus the *vis medicatrix naturæ* is given more time in which to eliminate, and in smaller quantities, the venom from the system.

This is another striking proof of the truth of the ancient aphorism, "Do not allow your theories to interfere with your practice."

Q. C. SMITH.

Austin, Tex., Aug. 26.

Temperature in Storms and High Areas.

ONE of the first practical discussions of this question was published in 1886 by M. Dechevrens, of Zikawei, China, and a translation of this paper will be found in the *American Meteorological Journal* for August, 1886. An independent investigation of this same question was carried on in this country before the above publication, the results of which will be found in the journal quoted above for October, 1887. The latter study showed that the temperature fluctuations were almost exactly the same, and had the same phases, both at the base and summit of high mountains, which was exactly opposite to the results obtained by M. Dechevrens. Dr. Hann of Vienna espoused the cause of M. Dechevrens, and tried to show that the observations at Sonnblick indicated the

same effect. Inasmuch as these two foreign discussions reached conclusions directly contradictory to all the teachings of meteorology, the importance of determining where the error lies, and of establishing the truth, will readily be seen.

A careful study of the question will show that the entire difficulty, and apparent contradiction, has arisen from a neglect of the consideration that at considerable heights in the atmosphere a lower temperature has a tendency to contract the air and cause a diminution of pressure, and a higher temperature just the reverse. The best example of this is to be found at Pike's Peak (14,134 feet), where the lowest pressure ever recorded was 16.88 inches, on Jan. 20, 1883, while the temperature at the summit was −34°, and while a high area of great magnitude was passing at the base. This shows that we must ignore fluctuations in pressure at the high station, and consider only those below. When this is done, the whole difficulty vanishes at most stations. Dr. Hann seems to have found a few cases where an increase of pressure at the lower stations near Sonnblick has been coincident with an increase of temperature on the mountain 8,700 feet above. It may be well to pursue this discussion under a slightly different form, and unite the results as obtained at Mount Washington (6,279 feet) with those in Austria.

The plan proposed is simply to compare side by side the temperature fluctuations at both base and summit. If we had balloon observations at 10,000 feet and others at sea-level on the earth's surface, such comparisons could be made readily and accurately; but it should be noted that when we use mountain observations, especially those on ranges and not isolated peaks, we cannot hope for an absolute comparison. The difficulty will be enhanced if our base station lies at some distance from the mountain, though in the case of an extended range we may obviate some of this source of error by taking stations on both sides of the range. It will be universally admitted that, north of the equator, the usual fluctuations of temperature at sea-level on the passage of storms and high areas are perfectly well known, though these may at times be masked or even reversed, as, for example, when the centre of the storm passes just a little south of the station. In general, as a storm comes up, there is a southerly breeze and a great increase of temperature. This increase of temperature is observed even though there be a calm and the sky be clouded, hiding the sun's direct rays. We naturally conclude that this heat condition is an accompaniment of the storm, and is largely independent of the sun's direct influence in raising the temperature. Exactly the reverse of this is experienced when a high area or a clearing condition approaches a station. Here the sky is perfectly clear, and though the sun has apparently a much better opportunity to heat up the earth and air, still we find a marked lowering of temperature. This is the normal condition, but suppose we find that with the increased pressure there is increased temperature at sea-level, or that the clouds come up and there is rain, then we must conclude that the conditions are abnormal, and in any general discussion or comparison of temperature conditions at the base and summit of a mountain we must give such cases a separate study and not unite them with the normal fluctuations. It seems quite plain that we may draw curves showing the observed temperatures at base and summit and compare them directly. There is a slight difficulty, however, which must first be overcome, and it is this. At the earth's surface there is a marked daily effect from the sun's direct heat which generally causes a steady rise of temperature from sunrise to about 3 P M., and this would mask the other conditions. It would be a great advantage if we could use observations more than once a day, as the maximum point in the passage of a storm and a minimum point in a high area might occur at any hour of the twenty-four. One way of eliminating this diurnal range would be to apply the difference between the monthly mean and the mean for any hour to each daily observation of that hour; for example, we would have to add a little to nearly every sunrise observation and subtract from nearly every maximum observed; but a better way still would be to take the mean of the hour which agrees most closely with the mean for the day and apply the difference between that and the hourly mean to each observation, as this would save one-third of the labor when we are studying three observations each day. In the latter case the 9 P.M. observation, for example, would be projected without modification. In projecting the temperature curves it was found most convenient to use the night observation rigidly and to interpolate the morning and afternoon observations if either or both differed widely from that. For Mount Washington the station at Burlington was chosen for the base until it was closed in 1883, and after that Portland. After projecting the curves there was found to be a most extraordinary similarity between the changes at the base and summit. To illustrate this I have drawn Fig. 1, which gives the fluctuations for January, 1876. I think this will be recognized as a perfect accordance. The slight hitch on the 16th on the summit has only a very slight bending at the base, but it is noticeable. Such slight coincidences were ignored in the summing-up. In the 78 colder months there were 1,128 accordances out of 1,240 cases, or 91 per cent; and in the warmer months, April, 1873, to September, 1879, 42 months, there were 553 accordances in 601 cases, or 92 per cent. It would seem that even if there were no explanation for these few discordances, the evidence is conclusive that whatever fluctuations of temperature take place at the base, they are faithfully repeated at the summit. The comparison will seem all the more effective when we reflect that this thin strip between these curves represents a difference in vertical height of over 6,000 feet.

I have made a careful study of the discordances, and find that they can all be explained under the following heads: (1) Often the curve turns at the summit before it does at the base, or, in other words, a lagging at the base causes the summit curve to cross that at the base. (2) At other times there is an abnormal condition of the upper atmosphere, a fall of rain, for example, in the centre of a high area, which shows a remarkable disturbance of temperature conditions. (3) There is sometimes in a high area a perfectly clear sky, which promotes intense radiation from the soil at the base, but which has no counterpart at the summit. For example, during the progress of a high area which culminated at 4 P.M., Nov. 16, 1874, the following temperatures were observed:—

	15			16			17		
	7 A.M.	4 P.M.	11P.M.	7 A.M.	4 P.M.	11P.M.	7 A.M.	4 P.M.	11P.M.
Burlington.....	28°	35	37	40	36	31	36	44	46
Mount Washington..	13°	19	18	15	15	22	23	27	30

Here the minimum temperature at the summit was reached during the afternoon of the 16th, but the great radiation after dark at the base gave a minimum at that point at night, though it is also probable that a portion of this was due to a lagging at the base. (4) There are occasions in the centre of a high area, which has only a very slight onward motion, when the sun's heat appears to have an abnormal effect upon the air column, causing an increase in temperature at the summit above that at the base. In all the cases examined there were only two under this last head. There is no difficulty at all in explaining all the exceptions, and these may fairly be said to *prove* the rule. The evidence is overwhelming that if the principles just laid down are accepted there is a marked increase in temperature at 6,000 feet height in our storms, and a decrease in our high areas.

It is a very interesting fact that the distance between the curves at the base and summit of Mount Washington during the passage of storms and high areas seems to be nearly constant. The slight crowding on the right-hand side of the curves between Nov. 6 and 11, and 26 and 30, is due to the fact that the base is to the northwestward of the summit, and hence the latter lags behind a little as a storm moves to the east. Does not this similarity prove that there is no uprush of air in the storm, nor a downrush in the high area?

To complete this investigation it is necessary to make a similar study of the observations used by Dr. Hann in arriving at his conclusions. It should be borne in mind that Sonnblick, the mountain station used by Dr. Hann, is almost entirely outside of the

track of storms and high areas. Mount Washington, that we have just studied, lies almost directly in the path of storms that cross the United States, and a little to the north of the ordinary path of high areas. Sonnblick is also on a long range of mountains, and not an isolated peak. The nearest base station on the north side is Salzburg (53 miles), and on the south side Görz (100 miles). The difference in height between Sonnblick and Salzburg is 8,722 feet, which is not quite one-half greater than Mount Washington above Burlington. On projecting the temperature curves at these Austrian stations we are struck at once with the

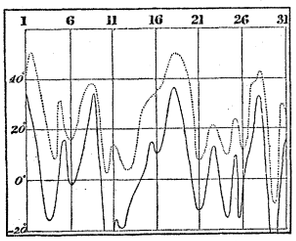


FIG. 1.

Full curve, Mount Washington; dotted, Burlington. Vertical lines are at intervals of five days, horizontal lines at each twenty degrees Fahrenheit.

enormous difference in the character of the curves. I have shown, as compared with those in this country, the curves for the month of March, 1888 (Fig. 2). We see at once that there is a marked similarity in the bendings of the curves; but the fluctuations are very moderate, and do not have sharp points, as was to be expected from what has already been said. One of the more marked discrepancies in Fig. 2 occurs on the 26th, which shows a deep depression at Salzburg, and none at Sonnblick. On projecting the temperature curve at Görz (shown broken in Fig. 2), we see that the curve for Sonnblick coincides exactly with that at Görz. This is a very significant fact, and shows that the mountain range is a serious drawback to a study of this question from these observations. Taking out all the coincidences, we may say

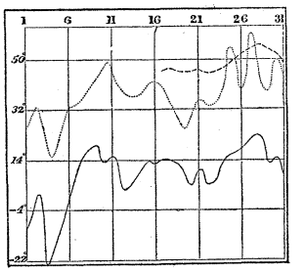
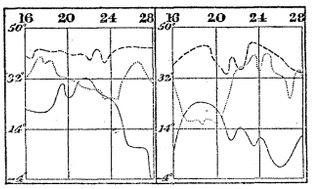


FIG. 2.

Full curve, Sonnblick; dotted, Salzburg; broken, Görz.

there are about 75 per cent fairly satisfactory, though hardly more than 50 per cent, perhaps, as marked as at Mount Washington. I think these discrepancies are due to the causes already set



November, 1889. December, 1889.

FIGS. 3 AND 4.

Full line, Sonnblick; broken, Görz; dotted, Salzburg.

forth, and certainly sink into utter insignificance when compared with the coincidences at Mount Washington. There are two quite interesting discordances in the whole set of curves, and

these are of enough importance to merit a separate discussion. Figs. 3 and 4 exhibit these cases. It will be clearly understood that these are the most serious discordances in the records for more than three years and a half. If it is possible to explain or elucidate these cases. we have practically attained the same result in Austria that we found in this country. During both these periods a high area of great magnitude and persistency settled over this region. We have no similar phenomena in this country as this of high areas (30.7 inches) remaining over one spot for ten days or a fortnight. In Fig. 3 there is no marked fall in temperature at Salzburg; and at Görz, on the south, the curve is almost flat. If we could shift the Sonnblick curve five days later, we would have an almost exact accordance between that and Görz, though I do not insist that that is a necessary view to take. When we look at Fig. 4 we see that there is an exact accordance between the Sonnblick and the Görz curve for a part of the way, and with Salzburg for the rest, so that here all the difficulty disappears at once. We may well believe that in this mountain region there will be great irregularities in the effect of the sun upon the earth and atmosphere. During the prevalence of such high areas the air becomes almost calm and stagnant, and it is an open question whether under these conditions the sun may not have a strong effect at the higher station during the day-time, while the radiation at night would be very much less than at the base; and hence there may be a steady accumulation of heat at the upper station, while at the lower the curve would be nearly horizontal or slightly depressed. It should be noted that while with the progress of ordinary high areas there may be a fall of 20°, and even 40°, at the base, yet in these cases it was very much less, amounting to less than 10° in November.

It is realized that this is merely a beginning in this discussion, and it is hoped that others will take it up, for it is all-important that this whole question be settled. H. A. HAZEN.

Washington, D.C., Aug. 26.

AMONG THE PUBLISHERS.

THE D. Van Nostrand Company have issued, as No. 98 of their Science Series, "Practical Dynamo-Building for Amateurs," by Frederick Walker. In this little volume the construction of a dynamo is described in detail so carefully and clearly that any intelligent amateur, skilled in the use of tools, will have no difficulty in producing an efficient machine wound for any desirable output. The book is the first American edition of the work, carefully revised from the second English edition.

—Dr. J. M. Mills of New York has been for several years studying the relation of eye-strain to headaches, etc., among children, and publishes a summary of his findings in an illustrated article in *Babyhood* for September. There appears to be no doubt that cases of short sight, far sight, and irregular sight often go unrecognized until the continued eye-strain results in a chronic headache and lassitude, or even serious nervous disorder. Other articles in the same issue are "Malaria," "Helps for the Fretful Baby," "Occupations and Amusements," and questions and answers upon subjects connected with the diet and clothing of children.

—A work in two quarto volumes, on "The Fossil Insects of North America," by Dr. Samuel H. Scudder of Cambridge, will be issued early in October by Macmillan & Co. The two volumes, of which only one hundred copies will be issued, not only contain, with some slight exceptions, a description of all the species of fossil insects of all American strata so far as known, but practically include the entire body of literature on this topic. The work will be illustrated by about sixty full-page plates, and occasional figures in the text. An English translation by Dr. George McGowan, of Professor Ernest von Meyers's "History of Chemistry," is announced for early publication by the same firm; also (in September) an illustrated work by Dr. R. W. Shufeldt of the Smithsonian Institute, entitled "The Myology of the Raven, a Guide to the Muscular System of Birds."