magnitude, and the other a blue 9. The contrast of colours was very striking, but there was little difference in size. In a letter recently received from the Rev. Mr. Webb, I find that it was previously observed by him, and it appears as one of his own discoveries in the second edition of "Celestial Objects," published in 1868. The red star is there classed as 6·5 mag., and the blue as 8. The two stars, therefore, appeared to differ very considerably in magnitude when seen by Mr. Webb, while to me, eleven years subsequently, they seemed quite nearly equal. Hence I conclude that the red is a variable, and I wish to call the attention of observers to it while it still remains in view. By a rough measurement I make out its position for 1881 = α 7h. 10m. 44s., and δ – 23° 6′·6. JOHN BIRMINGHAM.

Millbrook, Tuam, April 9

Concealed Bridging Convolutions in a Human Brain

IN his work on the "Convolutions of the Human Brain" Ecker denies explicitly that the first and second external bridging convolutions of Gratiolet, as seen in Cercopithecus, Inuus, &c., are ever concealed, either in the higher apes or in fœtal or adult man. I have however in my possession an adult human brain in which *a* convolution nearly corresponding in position to the external bridging one of Gratiolet is concealed, while another slightly external to it is nearly so. The brain was hardened in nitric acid with the membranes on (a much preferable method, by the by, to that of first removing the membranes; as these, by absorbing the acid and swelling, serve, like so many wedges, to keep the convolutions apart, and prevent the shrinkage that otherwise takes place). There was no indication of any concealed convolution until the membranes, just moistened for the purpose with water, were being removed. Then, owing to the opening out of the sulcus occipitalis transversus of Ecker, the tip of one became visible, and this tip, even now that the edges of the sulcus are widely separated, is from one-eighth to one-sixth of an inch beneath the general surface.

Its position relative to the great longitudinal fissure and to the posterior border of the gyrus supramarginalis seems to me pretty accurately to correspond to that of the external bridging convolution to those parts in the brain of an Indian pig-tailed baboon of undetermined species with which I have compared it; but in the latter the sulcus occipitalis transversus does not exist, while in this human brain, as is very common, the lateral or horizontal portion of the fissura parieto occipitalis, beneath the bevelled edge of which in the baboon the convolution lies concealed, has a very short course indeed.

The only difference then is that in the one specimen (the human) the concealed convolution lies in the transverse occipital fissure, there being no lateral extension of the parieto-occipital fissure, while in the other it lies in the parieto-occipital fissure, the transverse fissure not existing WILLIAM CARTER

Liverpool, March 26

Sound of the Aurora

IF I had consulted Franklin's account of his Polar researches before I sent you my extract from Tacitus, I should not have revived the question of sounds being heard with the aurora borealis. Franklin and his companions watched the aurora 343 times in two successive winter seasons; and never once, he says, did they observe a sound. Were, then, the experiences quoted by your other correspondent and myself mere illusions? Perhaps not. Franklin made his observations at and about the southern shore of Bear Lake, in latitudes varying from 67° to 69° north; might not the greater volume of air through which the phenomenon had to pass in reaching our island have caused the electric fluid to work up a sound? Surely that is possible The attractive force of the aurora is—we learn from Franklin himself—increased within a certain limit as its rays proceed southwards; for whereas Parry and his party at Port Bowen in latitude 73° 15′ noticed no deflection of the compass-needle under the influence of coruscations, Franklin and his party on the shores of the Bear Lake, six degrees further south, constantly observed this effect. And the attractive force is strongly felt here—hindering telegraphic communication at all events. Might not the vibratory force not sensible at within so short a distance from its origin as the attraction be increased within a greater limit? M. L. ROUSE

Sunnymead, Chislehurst Common

PERIODIC OSCILLATIONS OF BAROMETRIC PRESSURE

THE MSS. of the accompanying article, which was left unfinished by the late Mr. John Allan Broun, F.R.S., were handed over to me some time ago by Prof. Balfour Stewart, with a request that I would put them into shape for publication.

I have not found it necessary to make many alterations in, or additions to the original, and where made they are mostly indicated in initialed foot-notes.

E. DOUGLAS ARCHIBALD

In an article which appeared in NATURE (vol. xix. p. 6) a remarkable relation was shown to exist between the annual ranges of the atmospheric pressure and of the temperature of the air, as derived from the monthly means obtained from several years' observations of the barometer and thermometer at certain stations in India. The results and the conclusions from them do not appear to have been always understood, and as they bear on some of the most interesting questions on meteorology, I shall now examine them anew with the aid of observations at some other stations, under different local conditions.

For this end it is desirable to employ some elementary considerations. Let us, first of all, consider the action of varying temperature on a vertical column of the atmosphere. Let us consider a column of air reaching from the soil at B to the upper limit of the atmosphere at A; and suppose that the pressure shown by a barometer at B is 30 inches, while at a higher station, C, it is only 20 inches. If, now, the column of air is heated so that the temperature of the part B C is increased by 10° F. we know from laboratory experiments that the air will expand, so that a part of that which was below C will be pushed above it, and while the barometer at B will continue to show 30 inches, that at C will show 20·2 inches, the mercury at C will have risen two-tenths of an inch.

If, now, we suppose that the mass of air remains constant throughout the year, there will be an annual variation of the barometer at C, where its height will be greatest in the warmest month and lowest in the coldest month. For the same reason the difference of the barometric heights at B and C will be least in the warmest month and greatest in the coldest.

It has been supposed that the mass of the atmosphere remains constant throughout the year; if this is not the case the variations of pressure at C will not depend on temperature alone, but also on the other causes which produce variations at B.

In NATURE, vol. xx. p. 55, Mr. Douglas Archibald has given a series of differences of barometric heights at high and low stations in India for the months from October to April. The month of lowest mean temperature, January, shows always, as in the case just supposed, the greatest difference of pressures. As the high and low station is never in the same vertical, the one being sometimes 300 miles horizontally distant from the other, it is difficult to eliminate the part of the variation due to temperature at the higher station, but if we take as an approximation, however rude, the mean of the temperatures at the two stations as that of the vertical column, we can see that a considerable part of the variation at the upper station may be due to the expansion of the column with temperature.

Thus for Leh and Lahore the mean temperatures and difference of barometric heights are[1]:—

[1] The numbers are taken from the work cited by Mr. Archibald, "The Indian Meteorologist's *Vade Mecum*," by Mr. H. F. Blanford, Pt. ii. pp. 176, 178.

	Mean temp.	Diff. of Pressures.
	°	in.
January	35·0 Fahr.	... 9·690
July	74·1 ,,	... 9·105

Now as the difference of the mean temperature of these two months is nearly 40°, according to what has been said before, the expansion of the air from January to July should have increased the pressure at the upper station by about 0·8 inch (eight-tenths of an inch of mercury) and have diminished the difference of pressures by that amount. The observed diminution is, however, less than 0·6 inch, a deficiency of two-tenths, which has to be accounted for in some other way.

In the case of the annual variation of atmospheric pressure near the sea-level, we have to consider only the atmosphere above the station; but in that for a high station we have a much more complicated problem in which the atmosphere below the station plays an important part, a part also which may vary greatly, according as the upper station is on a mountain top, on a gradually ascending height, or on high table land.

It was for these reasons that in the article (NATURE, vol. xix. p. 6) under consideration, nothing was said of the annual variation on mountains or at great heights; the results were obtained from observations at stations little above the sea level, and from them it appeared that for every degree Fahrenheit on the range of monthly mean temperature, there was a variation of from ·021 inch at Singapore, to ·025 inch at Bombay and Madras, in the range of the monthly mean barometric height; while at Pekin the amount was diminished to ·015 inch. The conclusion that this ratio was nearly constant in India referred necessarily to stations under similar conditions. Mr. Archibald's letter has brought to notice a series of Indian observations made at stations farther from the sea and in North India, which are given in an interesting and useful work by Mr. H. F. Blanford, Meteorological Reporter to the Government of India, and which merit consideration. The means employed by me previously were derived from observations at standard observatories during five to twenty-six years; and it was mentioned that, as single years gave somewhat variable results, the monthly means from several years' observations were necessary for accurate values. In the cases to be found in Mr. Blanford's work, several are of observations during only two years (or even less), and we have no exact idea of the weight which in any case can be accorded to the observations at the stations. As, however, the results for different stations in general confirm each other (the exceptional cases will be noticed apart), I shall here form a series of groups of stations, each group under nearly the same local conditions as regards latitude, height above, and distance from, the sea. The value of k, the variation in thousandths of an inch, of pressure-range for 1° F. of the temperature-range, is the same for all stations in each group, with the exceptions referred to below.

Thus the most northerly group near latitude 30° N. and 600 miles from the sea, viz., Lahore, Delhi, Dera Ismail Khan, Mooltan, and Roorkee, gives the following result:—

Mean latitude of group (λ).	Mean height above the sea (h).	Mean annual temperature (t).	Mean ranges of temperature and pressure.		Mean barometric oscillation for 1° Fahr. $\left(k = \frac{\Delta p}{\Delta t}\right)$.
			Δt	Δp	
°	feet.	°	°	inch.	
30½	660	75·2	39·0	0·620	0·016

This group extends about 450 miles from Delhi to Dera Ismail Khan, and the heights vary from 440 to 890 feet. The value of k is the some for each station.

The next group comprises Bareilly, Lucknow, Jhansi, and Benares, for which the following are the values of the different elements:—

λ	h	t	Δt	Δp	k
°	feet.	°	°	in.	
26	520	77·7	32·4	0·554	0·017

The distance of the extreme stations is not 300 miles; the heights vary from 270 feet at Benares to 860 feet at Jhansi. For each station k = 0·017.[1]

The third group includes Patua and Gya, and the values are—

λ	h	t	Δt	Δp	k
°	feet.	°	°	in.	
25½	260	78·5	28·0	0·537	0·019

The distance between the two stations is about 70 miles; the heights are 179 and 347 feet respectively, and the values of k are within half a thousandth of each other.

The fourth group includes Berhampore, Burdwan, Dacca, Goalpara, Chittagong, Calcutta, and Saugor Island, for which the values are—

λ	h	t	Δt	Δp	k
°	feet.	°	°	in.	
23⅓	100	78·0	18·7	0·471	0·025

With the exception of Goalpara and Chittagong, these stations all lie in the Gangetic delta, at nearly the same elevation, and with an extreme variation in the value of k amounting to 0·004 inch.[2]

The fifth group includes Bombay alone, the values for which are—

λ	h	t	Δt	Δp	k
°	feet.	°	°	in.	
18½	37	78·8	11·6	0·287	0·024

The sixth, Madras, Negapatam, Trichinopoly, and Madura, with the values—

λ	h	t	Δt	Δp	k
°	feet.	°	°	in.	
11⅑	190	81·7	11·1	0·243	0·021

The seventh, Colombo, with the values—

λ	h	t	Δt	Δp	k
°	feet.	°	°	in.	
6½	42	81·3	3·6	0·069	0·019

It will be seen from this table that the value of k is a maximum (= 0·025) nearly under the tropic of Cancer, that it diminishes thence both north and south. This diminution cannot, however, be attributed altogether at least to the latitude, since the value is nearly the same at Pekin and at Delhi.

The variation of the value in Northern India from the maximum near Calcutta has an apparent relation to the height above the sea and the distance from it,[3] as well as to the amount of the temperature oscillation; the value of k diminishing as the others increase. We cannot, however, relate the value to any one of the other variables. No doubt height above the sea may have some influence, as has already been shown, but here mountain stations have been avoided; and while in the Lahore group the heights vary from 420 feet (Mooltan) to 890 feet (Roorkee), the value of k remains constant.

It should be observed that the temperature ranges are those of the stratum of air a few feet above the soil, while the ranges of pressure include the whole atmosphere above each station. We see that the value of k dimi-

[1] There appears to be some difference in the values of Δp as got by Mr. Broun and those derived from my edition of the "Vade Mecum" for these two groups. Thus from my edition (1877), the value of Δp for Group I. is 0·614, and therefore k = 0·015; there are also slight differences in the value of k at each station. In Group II. k is 0·017 only for Bareilly and Lucknow, the value for Jhansi and Benares being 0·016.—E. D. A.

[2] The figures for the fourth group have been supplied, and the fifth, sixth, and seventh groups have been added as the text seemed to imply their intended formation by the author.—E. D. A.

[3] Mr. Archibald has considered height not to be an element, and he cites in evidence Lucknow and Sibsagar, very nearly at the same height; the former having k = 0·017 and the latter k = 0·028 (NATURE, vol. xx., footnote p. 54. The latter value, however, is a mistake, for at Sibsagar k = 0·018, very nearly as at Lucknow. Mr. Archibald's conclusion is that it is not height, but distance from the sea, as the stations are at widely different distances from the sea. We should, owing to this error, be obliged to reverse the conclusion, and say it is not distance from the sea, but height, that is in question; but, in reality, Sibsagar though not so far from the sea, is still as far as Patua or Gya, with nearly the same value of k.

[See my note on this point, NATURE, vol. xxi. p. 131.—E. D. A.]

nishes as the range of the surface-temperature increases, and it is certain that the ranges of the temperature at all the stations as we ascend above *the soil*, will approach always nearer to each other the higher we go; so that if we knew in what way the relation between the oscillations is produced, and thence, in all probability, the part of the atmosphere which is chiefly in question, we might find a more constant relation between Δt and Δp at all the stations.

I will now proceed to make some remarks which may aid in the study of these questions. I would first observe with relation to the stations in the preceding table that the value of k is only 0·024 at Chittagong, while it is nearly 0·029 at Saugor Island. I have no doubt, however, that this difference is due to some local or instrumental cause.

In the first article I have, as already stated, used only observations made in first-class observatories. Every one acquainted with the difficulties of obtaining good observations in India, especially at out-stations, will understand that the mere printing of their results cannot give them any certain weight. Thus from the tables of means given in Mr. Blanford's work, it appears that the value of k at Vizagapatam is 0·032, while at Cuttack less than 3° to the north, it is 0·025. This great difference would lead to the conclusion that there must be some remarkable local cause, or that there is some error in the observations; if the former, then farther research as to this cause would be of the highest importance; the latter seems to me the most probable explanation. The following example, that of a station so well known as Madras, will show some reason for this belief. The results for Madras, given in the table, are those employed in the first article, and deduced from observations made in the Magnetic and Meteorological Observatory from 1841 till 1855. In Mr. Blanford's tables, however, means are given from observations for nine years. These means give results markedly different from the others. The following summary for the months of December, January, and June, will prove this :—

	Height of Barometer.		
Group of Years.	December.	January.	June.
1841–45	29·954	29·998	29·691
1846–50	·957	·986	·693
1851–55	·992	·998	·702
Means	·967	·994	·695
1868–71	·978	·967	·671
1872–76	·955	·926	·802
Mr. Blanford's means for nine years	·965	·944	·744

Now the three means for January, each deduced from five years' observations, do not deviate from the mean of the whole more than ·008 inch, while the corresponding extreme deviation for June is ·0 7 inch. Whereas the means from nine years, according to Mr. Blanford's tables, differ by 0·050 inch from the means of fifteen years, and the annual range, which is 0·299 by the fifteen years' observations, becomes only 0·200 by the last series. Judging from the means for four years—given by Mr. Blanford in his valuable memoir in the *Phil. Trans.* on the winds of North India—which I believe to be part of the nine years, the means for five years, 1872–76, give a range of only 0·124 inch from January to June; but even the mean pressure for the place and the annual law of variation is altered by the nine-year series, the maximum pressure occurring in December instead of January. It is essential in such investigations that some confidence can be placed in the observations, and if this can be done anywhere it is certainly in observations made at such a station as Madras. I have employed previously means deduced from fifteen years' observations in the Magnetic and Meteorological Observatory; but these means differ in a manner so extraordinary (for a South Indian station) from the means given by Mr. Blanford from nine years' observations, that this demands attention and explanation.[1]

We see that the annual oscillation of pressure increases with that of temperature, and as shown in the article already cited, that this relation holds for two places on opposite sides of the Ghâts, nearly at the same height within sixty miles of each other, for which the annual oscillations at one are twice those for the other.

Let us now see what our knowledge from laboratory experiments of the laws of expansion and equilibrium of gases would induce us to conclude with reference to the stations in question.

In the first place, let us remember that the yearly mean temperature at Pallamcottah is about 7° Fahr. greater than at Trevandrum, while the yearly mean pressure is the same. We conclude at once from this result that the mean pressure does not depend on the mean temperature.

I stated that the mean temperature in January at Pallamcottah, on the east side of the Ghâts, was 4° Fahr. greater than at Trevandrum, sixty miles distant on the west side of the mountains, and that the mean pressure was 0·065 greater at the former than at the latter. Sir John Herschel has shown that according to our knowledge of the expansion of gases, there should be a single diurnal atmospheric tide due to one side of the earth being warmer than the other, and this due to the expansion of the gases in the warmer half causing the centre of gravity of the air to be higher than in the other, should produce a sliding motion of the warmer air towards the colder in twelve hours.

If such a result is what may be supposed to occur in the period of a day, there cannot be the smallest doubt that if a mass of the atmosphere has a lower pressure and a lower temperature than another mass at a short distance, the former should flow towards the latter, and produce equilibrium within twenty-four hours. Here, however, we find that there is a difference of pressure of 0·065 inch, which remains during a whole month at a distance of sixty miles, and this continued difference of pressures is not to be explained by any known property of gases.

We see, indeed, the remarkable fact that the oscillation of the monthly mean pressure at one station proceeds quite independently of that at the other, and both in a constant ratio with the variation of the temperature of the atmosphere in the lowest stratum.[2] All this is inexplicable by any heating action alone.

I have omitted all notice of the aqueous vapour in the atmosphere. It is obvious, however, that this cannot explain the annual oscillation of pressure, since it is just when the tension of vapour is greatest, that the barometric height is least. The introduction of more vapour into the atmosphere does not make the whole lighter, but heavier; and when we adopt the arithmetical operation of Dove and subduct the vapour pressure from the barometric height, we find the oscillation to be greater, not less, than before.

It is not easy to determine the variation of vapour pressure from the indications of the dry- and wet-bulb thermometers, and the exact result of the total vapour above the barometer cannot as yet be determined, but its amount will not explain the annual law, nor will it explain the independent oscillations in question. I have, however, long ago suggested that the humidity of the air may be in question, and as the oscillations of dryness or humidity of the lower mass are probably related to the temperature, the oscillations of pressure may also be related to them.

Sedgwick has said, in his inaugural discourse on the

[1] Mr. Broun asked me if I knew what years made up the nine, but I know of no one except Mr. Blanford himself who can give a satisfactory answer to this question, and he is in India. The "Vade Mecum" being an official work, ought certainly to be trustworthy.—E. D. A.

[2] And probably of the mean temperature of a layer of some considerable height.

studies of his University: "To explain difficulties in these questions, the atmospheric strata have been shuffled in accordance with laboratory experiments." Thus, for example, the mean pressure of the atmosphere remains, on the average of the whole year, 0·038 inch lower for every 100 miles we proceed north in this country, a difference which is called a *gradient*, as if it were a fall on a railway line, though it is really the position of equilibrium like that of the watery ocean, which has also a gradient of nearly thirty miles from the equator to the poles.

I have previously remarked that at Bombay the maximum pressure precedes nearly by a month the minimum temperature, while the minimum pressure is a month later than the maximum temperature. This is also true for all the stations in North India. At Madras, however, and Trevandrum, January becomes the month of maximum pressure. I do not, therefore, place much weight on this fact as showing that the two oscillations are not cause and effect. The month of maximum pressure at Pekin agrees most nearly with that of minimum temperature.

I have stated in the first article on this subject that I did not admit that the oscillation of pressure was due to that of temperature, and therefore could not allow that a higher annual mean temperature [would in any case cause a lower annual mean pressure] From the fact that the annual variation of pressure and temperature in Central Asia is greater than in any other portion of the globe, the greatest pressure coinciding nearly with the lowest temperature, and the least pressure with the highest temperature, it was concluded by Mr. Chambers that years of greatest mean pressure should also be years of least mean temperature. Now if we assume that the pressure depends only on the mass of the air and watery vapour in it, as the former is constant, and the latter, the only variable part, is greatest when the temperature is highest, it would follow that years of greatest heat should be years of greatest pressure, which is just the reverse of the conclusion deduced by the analogy from the annual variations.

Indeed, it is one of the great difficulties in the hypotheses which have been proposed, to explain the annual variation of pressure of the mixed atmosphere, that when we subduct the vapour pressure, as far as our means of calculating this exist, we have a much larger dry air oscillation than before.

I gave, however, different reasons for concluding that the range of temperature was not itself the cause of the diminished pressure, although the two go nearly together. One was that the observations of Bombay showed the greatest pressure to precede the lowest temperature by a month, and this is true for all the stations in the groups of North India already given. I also pointed out that were the two directly related, the mean pressure at Trevandrum should be greater than at Pallamcottah by nearly one-tenth of an inch, which is not the case, the isobars and isotherms having no relation to each other.

If we suppose that we have the same atmosphere over each station as over the whole earth, there is no possibility of explaining the variation of pressure by that of temperature. The only known property of heat which affects the mass has no doubt been employed to cause the hotter air to flow away somewhere, and surely in that case it should flow to the nearest colder station, where the pressure is less; but we have seen that this is not so in the case of Trevandrum and Pallamcottah, nor is it so in the valleys of the Ganges and Indus, where the oscillation increases as we ascend from the sea. These oscillations proceed with the greatest regularity, approximately in proportion to the temperature variation from month to month, and without the slightest regard to the hypothesis which should cause equilibrium in twenty-four hours, by the sliding of the most expanded masses over those least so. In what way, then, can we associate the two oscillations if one is not the cause of the other? I have long ago suggested that the varying humidity of the air may be in question; this is only a suggestion. I do not mean the mere tension of vapour—as already stated when we try to get rid of that from the total atmospheric pressure, the subject becomes more difficult, the dry air oscillation being greater not less than that of the whole—but if we suppose that the attraction of gravity is not the only attraction which affects the pressure of the atmosphere, but that this pressure varies through some other attracting force such as an electric attraction of the sun depending upon the varying humidity of the air, and this again depending on its temperature; we should find a method of relating the two variations which does not exist if gravitation alone is employed.

It is quite certain that many physicists will not admit the idea of an electric attraction on our atmosphere in the present state of our knowledge, hence the efforts to make expansion, and a shuffling of the atmospheric strata suffice. We must not, however, in our ignorance, attempt to force conclusions in opposition to facts, and if these can be satisfied more easily and with greater probabilities in its favour by the aid of the hypothesis of an electric attraction of the sun, that hypothesis will have a better claim to acceptation than the other.

I shall here note a few facts which cannot be explained by thermic actions.

1. I have shown that on the average of many years observation in our latitudes the mean pressure diminishes at the rate of 0·038 inch of mercury for every one hundred geographical miles we proceed towards the north. This has been called a gradient from the similar term used in railway slopes; but it is no slope, it is a level of a surface of equilibrium like that of the sea. It is the mean heights of the barometer at the sea-level which indicate the form, if we may say so, of the equilibrating atmosphere.

2. In India we have seen that the atmospheric pressure oscillates at each station even when these are quite near to each other, independently of the known laws of equilibrium of pressure of gases.

When we turn to the semi-diurnal oscillation of the barometer we are only amused at the attempts made to explain it by shuffling the atmospheric strata. Nothing can be more certain than that the theories of expansion, or resistance to expansion and overflow, are the vain efforts to make the laws of nature agree with a theory. Over the great ocean within the tropics, where the diurnal variations of temperature are small and the air is absolutely without perceptible currents for days together, the barometer rises and falls a tenth of an inch twice in twenty-four hours with the regularity of the solar clock. The action of the sun on the whole atmosphere which produces this movement varies chiefly during the day hours at inland stations with the temperature oscillation, so that, as in the case of the annual variation, the fall of the barometer at 4 P.M. is greater in the same latitude as the temperature is higher. This variation occurs during the most complete calms; the smoke rises vertically from the plains of Tinnevelly; no current is visible in the motion of the clouds; yet the barometer falls at four in the afternoon as it did at four in the morning, only it falls farther.

THE ETNA OBSERVATORY

THE accompanying illustration of the Observatory on Mount Etna is reproduced from the *Memoirs* of the Italian Spectroscopic Society. It shows that the building is so far complete, and surmounted by its revolving dome for the protection of the large Merz equatorial of thirty-five centimetres aperture. In the engraving the volcanic cone appears much nearer the Observatory than it really is. The work of building was suspended during the stormy weather of 1879, but was completed in the summer of last year. But it cannot be said that the