

Through the kindness of the Hydrographer of the Admiralty, I have been favoured with all the observations made in the *Challenger* of the specific gravities of the Atlantic at intermediate depths between surface and bottom. From these observations it will be seen that there is scarcely any sensible difference between the mean specific gravity of the equatorial and the two Atlantic columns.

The following Table shows the mean specific gravities of the three columns:—

ATLANTIC.			EQUATORIAL.		
Depth in Fathoms.	I.	II.	Depth in Fathoms.	III.	IV.
	Lat. 38° 3' N. Long. 39° 19' W. Specific gravity at 60°.	Lat. 26° 58' N. Long. 22° 57' W. Specific gravity at 60°.		Lat. 1° 22' N. Long. 26° 36' W. Specific gravity at 60°.	Lat. 3° 8' N. Long. 14° 49' W. Specific gravity at 60°.
Surface	1'02684	1'02685	Surface	1'02616	1'02591
100	—	1'02732	50	1'02630	1'02598
150	1'02677	1'02658	90	1'02627	—
250	1'02641	1'02642	100	—	1'02643
400	—	1'02609	200	1'02607	1'02620
500	1'02608	1'02600	300	1'02618	1'02610
1500	1'02607	1'02620	400	—	1'02629
			1500	1'02618	1'02613
	1'026211 = mean of Column A.	1'02623 = mean of Column B.	Mean specific gravity of columns.	1'026181	1'026223
			Mean of the two.	1'026202 = Mean of Equatorial Column C.	

The mean specific gravity of the equatorial column as proved by the two soundings III. and IV. in the Table is 1'026202; and 1'026211 of sounding I. of the Table may be regarded as the mean specific gravity of the North Atlantic Column A, for the observations were made at a place on the same latitude, and only about two degrees to the east of that column. Consequently the specific gravity of Column A exceeds that of the equatorial by only '000009, a quantity which does not amount to one inch in 1,500 fathoms! Sounding No. II. of the Table, made at a place a few degrees to the east of Column B of the section, gives 1'02623, which may be regarded as the mean specific gravity of that column, and the more so as another sounding made in this region gives identically the same mean value. The difference between the Equatorial Column and Atlantic Column B in lat. 23° N. therefore amounts to only '000028, or 3 inches in 1,500 fathoms. It must of course be observed that as the specific gravities in the table are not taken at equal intervals the mean of the figures does not represent the mean specific gravity of a column. The number of fathoms represented by each separate value must be taken into account in determining the mean value of a column.

My result is, therefore, not materially affected, even after I have thus taken into account difference of salinity, and computed the amount of expansion according to Prof. Hubbard's Table. The surface of the North Atlantic in lat. 38° to be in static equilibrium must be 3 feet 3 inches above that of the equator, and in lat 23°, 2 feet 3 inches above it.

It is perfectly true that according to the gravitation theory the ocean is never in a state of static equilibrium, but it must be observed that as the surface-flow according to this theory is from the equator polewards, it is the equatorial column that is kept constantly below the level necessary to static equilibrium; hence, were I to make allowance for want of static equilibrium, I should make the slope greater than 3 feet 6 inches. Dr. Carpenter's objection that the force of my argument rests on the assumption that the sea is in equilibrium is based on a misapprehension of the problem, for in reality, by not making allowance for want of equilibrium, I give his theory an advantage which it does not deserve. Were the surface-flow from the North Atlantic to the equator, there would then be some force in his objection, for by leaving out of account want of equilibrium I would be making the slope greater than it should be. Dr. Carpenter states that his objection met the approval of General Strachey

and Sir William Thomson at the British Association meeting. If it did, it shows that they must either have misapprehended my argument or his objection to it.

I have again to remind Dr. Carpenter that "viscosity" can have nothing to do with the question at issue. The water has to flow up the "gradient," and that by means of gravity. This is mechanically impossible, whether water be viscous or not.

It is needless to quote the opinions of Lenz, Arago, and Pouillet. They were not in possession of sufficient data to enable them to determine the question with certainty. The question, be it observed, is not "Can difference of temperature produce circulation?" Everyone will admit that were there no other agencies at work but equatorial heat and polar cold, a difference of temperature would soon arise which would induce and sustain a system of circulation; but this condition of things is prevented by the equatorial waters being swept away by the winds as rapidly as they are heated. I submit that I have proved that this is the case in reference to the Atlantic. If I am wrong, let it be shown where my error lies.

JAMES CROLL

Edinburgh, Nov. 10

Refraction of Light and Sound through the Atmosphere

THERE is in Upper Thibet a plateau called the "Kyan Chu Plain," on which phenomena of mirage are frequently seen. The plain is at a height varying from 15,000 to 16,000 feet. A cold wind comes down from the surrounding mountains, while an exceedingly hot sun heats the ground. While marching through this plain on Aug. 19 I saw the mirage in perfection. A mountain in front of us, at a distance of about five miles, appeared to be situated on the border of a lake of a deep and rich blue. A shepherd with a flock of sheep seemed to wade through the water, and the reflection of each sheep was most distinct and sharp. The effect was so complete that one of my companions proposed to leave the pool of water at the side of which we had encamped for breakfast, in order to go to the borders of the lake.

I measured the temperature of the air at various heights from the ground. The following readings were obtained:—

Height above ground.	Dry Bulb.		Wet Bulb.	
5 feet	49°	...	32°	...
4 inches	55°	...	38°	...
1 inch	56°	...	39°	...

The ground at that place was stony, and no accurate measurement of its temperature could be taken. A few miles further on, however, a sandy ground was found to have a temperature of 90°.

The difference between the temperature of the ground and that of the air was painfully striking to me, as, owing to blisters, I had to walk with bare feet. My feet felt burning hot, while the remainder of the body was unpleasantly cold. The mirage was seen in its greatest perfection at about 9 o'clock A.M.

Such a condition of the atmosphere must, according to Prof. Reynolds, prevent any sound from being heard at a great distance, owing to its refraction upwards. Such was really the case. A rifle fired by the above-mentioned companion at a short distance remained almost unheard.

With regard to the question whether our better hearing at night is due to the absence of disturbing noises, or to the cause suggested by Prof. Reynolds, I wish to remark that the Upper Himalayas are particularly free from any disturbing noises, yet the increase in our power of hearing at night is most marked.

Sunnyside, Upper Avenue
Road, N.W., Nov. 20

ARTHUR SCHUSTER

Evidences of Ancient Glacier Action in Central France

HAVING read with much interest Dr. Hooker's contribution to NATURE on "Evidences of Ancient Glaciers in Central France," I am tempted to send you a few remarks which may interest those who look out for glacial phenomena wherever they travel.

When travelling in Auvergne with Sir William Guise in 1866, we unfortunately missed the transported erratics in the Tranteine Valley, described by Dr. Hooker. We saw, however, examples of what we believed to be ice-borne erratics, on more than one occasion, and consulted M. Lecoq on the subject at his residence at Clermont Ferrand. He had observed travelled boulders in certain localities, but, as mentioned in the note-book of Sir William Guise, "attributed to transport by snow many of the effects generally assigned to glacial action."

I would also ask attention to a subject which appears to me of considerable interest with regard to the age of the most modern

of the lava currents of Auvergne. M. Lecoq had in his museum some fossil remains of the Marmot, the Mammoth, and the tichorhine Rhinoceros, and he distinctly told us that these relics of northern mammalia, which geologists are accustomed to associate with glacial times, were mostly found in cracks and fissures in the lava-streams near Clermont Ferrand. From this it would appear that the latest lava streams of Auvergne had become cold, consolidated, and fissured before the introduction of the bones and teeth of the northern quadrupeds into the fissures.

But if this prove to be true, on further investigation, I do not wish to imply that there have been no volcanic eruptions in Central France since the last outpour of lava currents, or the days of the Mammoth. On the contrary, I think the evidence is the other way. I have just returned from a visit to the extinct volcanos of the Haute Loire and the Ardèche, where I was accompanied by my friends Sir William Guise, Capt. Price, and Mr. Lucy; and I believe there is evidence of a certain amount of volcanic action in the Ardèche since the outpouring of the later lava-streams. There are outbursts of volcanic ash and scorix which form what are termed "chimneys," and which are blown right through the most recent lava-currents. Both near Montpezat, so admirably depicted by Mr. Scrope, and near the bridge at La Beaume, there are outbursts and eruptions through the basalts, which dislocate and throw off the basaltic columns. It is not improbable that some of these attempts at forming a volcano happened in the Ardèche during the fifth century, when the Archbishop of Vienne, Alcinus Avitus, in his homily on the "Rogation Days," speaks of "frequent shocks of earthquakes," and "fires often blazing," and "piled up mounds of ashes." Gregory of Tours also speaks of stags and wolves wandering about Vienne. These wild animals may have been driven from the forests of the Ardèche, by these last volcanic eruptions, as far as Vienne.

W. S. SYMONDS

Communication of Information among Bees

SOME two or three years ago a swarm of bees entered a very small hole under the slates near the eaves of the roof of my house in the Highlands, and established themselves for the summer but died out in the subsequent winter. I infer that there were no survivors among the bees to remember the circumstance (see Appendix to Kirby and Spence's "Entomology") and to account in any degree for what occurred this summer.

The house is of four stories, and stands in the garden, in which, about fifty yards from the house, on the other side of a hedge, are my beehives. For a few days, during which there were the usual indications of swarming being imminent in one of the hives, a great many bees found their way into the lower rooms of the house; there was a constant hum of bees in one of the chimneys, at the top of which there was always a group flying about. The top of this chimney is about thirty feet horizontally from the settlement of the old swarm, and fifteen feet above it; there was also occasionally a cluster of bees on the roof of a "semi-detached" lower building (the kitchen) on the other side of the house from the old settlement, but as far as we saw no bees visited the old settlement, and nothing indicated any intention of the swarm to go there, though we expected it to make for the house and probably for the chimney I have mentioned. In due time the swarm came off and rose unusually high, and I immediately made some smoke in the chimney to prevent their entering it. Presently the swarm settled on a low apple-tree and was snugly hived in the usual way in a straw "skep" about noon. Next day, however, about 10 A.M., the swarm left its "skep" and made for the old settlement without any hesitation, and there they established themselves in spite of all we could do.

Of course the whole proceedings may have been disconnected, but the impression left on my mind was that the queen, or her counsellors, had previously "prospected," and resolved to go to the old settlement as an eligible "location," and that the common bees learned somehow that "the house" was to be their destination, but that some of them fancied the chimney, others the roof of the kitchen, and others wandered vaguely in at various open windows, while the queen knew exactly where she wanted to go, but got confused the first day.

The manner in which the bees learned that the house was to be their destination may have been that the queen in her investigations had left strong traces of herself at the chimney and on the roof of the kitchen, which attracted the bees to these places, and a general odour of royalty about the house which induced the bees to come in at the windows; but it may have been that there was some "talk" in the hive about it. In connection with

Sir John Lubbock's papers, the incidents may be worth your notice.

There has also been some question as to the distance bees go in search of "pasture." It may be worth noting that at Arisaig House, I am told, bees are to be found in the peach-house every spring at the time of the blossom, while, so far as I can learn, there are no hives within ten miles but my own, which are separated from it by an arm of the sea (Loch Ailort), a mile wide with islands, and a second arm of the sea (Loch-na-Nuadh), two miles wide without islands, the whole distance being about four miles from the hives to the peach-house.

University of Glasgow, Nov. 13

HUGH BLACKBURN

A New Palmistry

I HAVE lately consulted two standard works upon the proportions of the human figure to which Prof. Ecker does not refer in the suggestive paper of which I gave an abstract in NATURE (vol. xiii. p. 8), in the hope of finding some definite information as to the relative lengths of the "index" and "ring" fingers. In the first of these two works, Quetelet's "Anthropométrie" (Bruxelles, 1870), no mention whatever is made of the proportions of the several digits, whether of hand or of foot; while from the second authority, the "Proportions-lehre" of Carl Gustav Carus (Leipzig, 1854), all the information that can be derived, meagre as it is, is purely inferential. In the skeleton of a hand represented at Fig. 4, Taf. iii. of this fine folio work, the "index" is considerably longer than the "ring" finger; and in the letter-press explanatory of this plate, a table is given of the lengths of the various factors of the digits, e.g. the metacarpals and the three phalanges, in "modulimines," constant lengths, each of which is equivalent to about seven millimetres. Now the length of the "index" is twenty-three, while that of the "ring" finger is only twenty "modulimines," the former thus exceeding the latter digit by about twenty-one millimetres, a difference much greater than any which has been recorded by Prof. Ecker. In the extended left hand of an *ideal* (sexless) figure, at Taf. iv. (*ibid.*), the "ring" and "index" digits are of the same length, the former being perhaps a shade longer.

Regiments and large asylums would be a fertile field for the further investigation of this interesting and highly suggestive subject.

J. C. GALTON

IN Mr. J. C. Galton's interesting article bearing the above title, in NATURE, vol. xiii. p. 8, no mention is made of the position of the hand at the time of making the observation as to the comparative length of the fingers. Perhaps Mr. Galton will kindly make it known whether Dr. Ecker has specified the position which he adopted. That the position makes some difference may be clearly seen in the following manner:—

Place the hand, back upwards, horizontally across the front of the chest, and observe the comparative length of the "index" and "ring" fingers. Then, by a motion of the wrist, moving the arm as little as possible, turn the hand outwards in the same plane, until the fingers stand at right angles to their first position, and again observe the two fingers. Naturally the "index" will appear to be longer in the first position than in the second, on account of the different condition of the muscles. Neither of these positions is likely to be adopted by anyone investigating the subject, but in any comparison of results *one and the same* position should be referred to as a standard, and this standard should specify whether the hand is held with the back or the face upwards. Dissimilarity between the two hands, as mentioned by Mr. Pryor, appears to be common. F. T. MOTT

Leicester, Nov. 19

I HAVE made a collection of over fifty outlines of the fingers of European hands (right and left). At present I find that the tendency in the female hand is to a proportionately longer third than index, in *both* hands, than in the male. In all the hands I have examined, the third finger of the left hand (when longer than the index) is also proportionately longer than the same finger of the right. In this series I have found only one case of an index longer than the third, and only one in which they were equal (both males). These are all carefully drawn into a pocket-book, care being taken that the hand is perfectly free from any muscular strain, which alters the result very appreciably; and the race, sex, and general physical characteristics are noted on the sheet. The list at present includes some eminent classical