Butyl series.	At. wt.	Sp. gr.	Condensation.
C ₄ H ₁₀	29	0.600	231.7
C ₅ H ₁₂	36	0.628	192.3
C ₆ H ₁₄	43	0.669	174.2
C ₇ H ₁₆	50	0.699	156.6
C ₈ H ₁₈	57	0.726	142.6
C ₉ H ₂₀	64	0.741	132.9
C ₁₀ H ₂₂	71	0.757	119.4
C ₁₁ H ₂₄	78	0.766	110.0
C ₁₂ H ₂₆	85	0.778	102.5
C ₁₃ H ₂₈	92	0.796	97.0
C ₁₄ H ₃₀	99	0.809	91.5
C ₁₅ H ₃₂	106	0.825	87.1

From this it will be seen that the amount of condensation of the gas, as it passes into the liquid state, becomes less and less as the molecular composition becomes more complex, and moreover, that the difference in amount of condensation of any two adjacent members of the series becomes diminished at the same time; thus the difference in condensation on passing from butyl hydride to amyl hydride is 39.4; from myristyl to benyl only 4.4 c.c. If the fact that the sp. gr. of these substances has been taken at a common temperature, instead of at their boiling points be considered, it will be seen that the difference is really less marked than it otherwise would be.

From the fact that an increase in the number of atoms in the molecules is accompanied by a decrease in condensation, it would appear that a substance might be found which should possess the same, or nearly the same, specific gravity in the state of gas and solid, *i.e.*, in which these states should become identical. A large number of atoms would have to enter into the formation of a molecule to bring about this result, though if there be any truth in the formula for albumin quoted by Herbert Spencer on the authority of Mulder, it must be approached by this substance. This formula— $10(C_{40}H_{121}N_5O_{19}) + S_2P$ —gives us an atomic weight of 3912.5, and if we assume the specific gravity of albumin to be 2, this will give us—

$$\frac{2}{3912} \times 11,200 = 5.8 \text{ about.}$$

So that 1 c.c. of albumin, on this assumption, would be only about five times as heavy in the colloid as in the gaseous state. This fact may help to throw some light on the peculiar properties of colloids, and taken in conjunction with Herbert Spencer's reflections, on that most curious of all colloids:—protoplasm.

W. J. SOLLAS

A Claim for Precedence

IN reference to the notice of Favre in NATURE, vol. xxi. p. 417, I shall be obliged if you will kindly allow me space for a few remarks.

Credit for much valuable work is given justly to Favre, but I must be allowed to protest against having the few grains of corn belonging to others added to his well-filled granary. One of the discoveries ascribed to Favre in your notice is "the relative diminution in the heat evolved by the combination of a compound body compared with that due to the combustion of its varied constituents," or rather, the cause of it. Now he was not the discoverer of this, but I was. In October, 1851, I published in the *Philosophical Magazine* a paper proving that decomposition absorbs heat, and exactly to the same extent that the previous combination of the constituents had evolved it. I proved it by passing a galvanic current through water and finding the increase of temperature. This gave the heat produced by the resistance lessened by the decomposition of the water. Then, to find the heat of resistance undiminished by decomposition, I passed a similar current, as shown by a galvanometer, through a platinum wire offering the same resistance as the water, and surrounded by an equal quantity as in the first experiment. The difference of temperature in the two experiments was, of course, the heat absorbed by the decomposition.

In some twelve months or so after this publication, Favre and Silbermann published in vol. xxxvii. of the *Annales de Chimie et de Physique*, p. 507, the very same experiment to prove the same principle, giving it as their own.

In 1852 a paper from me was read at the Belfast meeting of the British Association, and published the same year, in November, in the *Philosophical Magazine*, giving the first experiments made in thermo-chemistry in which decomposition is taken into account, and showing the principle by which the heat of com-

position of bodies can be known from that absorbed in their decomposition, and which has since been used in all thermo-chemical researches.

This principle and my experiments were published six months afterwards by Favre and Silbermann (*Annales de Chimie et de Physique*, vol. xxxvii. p. 484) without allusion to me.

These publications and dates are easily to be referred to. It is no matter of opinion whether I should be looked on as the originator of thermo-chemistry as it at present stands, but it is a matter of fact that I am, as can be proved by the references I have given; and I think I should not be acting wisely to allow the credit of much labour and thought which is due to myself to be given to another, at least without protest. I do not, of course, want to compare myself with Favre, but I certainly claim, and prior publication establishes for me, the discovery of the principle that originated all the thermo-chemistry of the day which is generally given to him; nor do I expect that I will ever be given as an authority as long as scientific men can quote the names of Favre and Silbermann; but I ask that, in fairness, my claim should be put on record, for although it makes very little difference to the world in general who first works out a discovery, yet to the individual whose only gain is the consciousness of having done it, the matter is quite a different thing.

Parsonstown, Ireland, March 9

THOMAS WOODS

The Origin of Man

SEEING that the doctrine of evolution has gained ground now almost universally among naturalists, it is surprising that the problem of the origin of man does not raise up an army of investigators, resolved to establish it by "demonstrative evidence" on an unassailable foundation.

It is true that this question has been engaging the attention of naturalists, and that individual explorers have devoted themselves to its solution, but little, if any, united effort has been organised hitherto. Were the matter taken up with as much earnestness as has been brought to bear on explorations in Assyria or Palestine, or in the Rocky Mountains of America, it is hard to believe that this question would long remain unsolved.

If the organisation of a society for the purpose of prosecuting research of the kind above indicated were widely agitated, there would not be wanting, I am convinced, either members or funds to further its success. By a strong united effort—international, if necessary—there is no doubt success would be achieved.

We are not utterly without a clue as to the time and place of man's origin. At least such hints as we possess it is our duty to follow up instead of standing by in idle perplexity as to where research should begin.

We now know that it is useless to look for the remains of Simian man in deposits later than pliocene, since the remains of man—not Simian man, but man truly so-called—have been found in pre-glacial and pliocene deposits in the New World as well as in the Old.

The simple fact of the distribution of man over both eastern and western hemispheres having been accomplished as far back as pliocene times, compels us to suppose that man had probably originated not later than the latter part of the miocene age. The discovery of anthropomorpha (*Phopithecus* and *Dryopithecus*) in miocene deposits, while as yet eocene strata have only yielded lower members of the primates seems to point to miocene times as somewhat near the date of man's origination. So much for our present clue as to Time.

Next as to the Place. Inasmuch as the tertiary formations in the New World have produced hitherto no higher members of the primates than inferior types of monkeys and lemurs, except the recently discovered remains of man himself in pliocene deposits, the inquiry may be confined to the Old World.

In the Old World the most northerly position at which the remains of Anthropomorpha have as yet been found is about the forty-fifth parallel of latitude, namely, in Switzerland and in the south of France.

Now as the higher existing apes are only found in Western Tropical Africa and the Malay Archipelago, while the lower apes (the gibbons) extend into Southern China and Northern India. The migration, since miocene times, of the Anthropomorpha, has probably passed along a grand *Main Line* extending from China through Northern India, Balochistan, Persia, Asia Minor into the south and south-west of Europe; and from this main line by *Two Branches*: one from Southern China into Malaysia, the other from the region of the Caucasus through Syria, and perhaps Arabia into Africa.

The nearer the form and habits of man in his most primitive stage resembled those of the apes, the more in all probability would his habitat or range have been identical with theirs. Therefore an examination of miocene or early pliocene deposits along this line and its diverging branches would scarcely be unattended with success in producing many fossil remains of very primitive or Simian man.

I trust I have said enough to indicate the direction which inquiry ought to take as far as present evidence goes, and I hope that a gigantic combined effort may ere long be made by all naturalists and all lovers of truth to attempt in a downright earnest manner the solution of this great question of the origin of man.

W. S. DUNCAN

The Stone in the Nest of the Swallow

WOULD any of your readers be kind enough to give me some information about the origin of the fable to which Longfellow refers in the following passage of his "Evangeline," Part I., at the end :—

" Oft in the barns they climbed to the populous nests on the rafters,
Seeking with eager eyes that wondrous stone, which the swallow
Brings from the shore of the sea to restore the sight of its fledglings;
Lucky was he who found that stone in the nest of the swallow!"

Leiden, March 19

P. P. C. HOEK

Carnivorous Wasps

SIR DAVID WEDDERBURN'S inquiry (NATURE, vol. xxi. p. 417) reminds me of my experience on this subject. Many years ago I was examining an apple-tree, when a wasp alighted on a leaf which formed a caterpillar's nest neatly rolled up. The wasp examined both ends, and finding them closed, it soon clipped a hole in the leaf at one end of the nest about one-eighth of an inch in diameter. It then went to the other end and made a noise which frightened the caterpillar, which came rushing out at the hole. It was immediately seized by the wasp, who, finding it too large to carry off at once, cut it in two and went off with his game. I waited a little, and saw the wasp come back for the other half, with which it also flew away.

After witnessing such evidence of intelligence I have had a great respect for wasps, and gave orders to my gardeners never to destroy one.

I gained some further evidence of their carnivorous taste when I once took my children to Switzerland for a holiday, and on a butterfly hunting expedition. We had spread out the day's find in the evening, and next morning I placed the boards in the sun to dry. On looking at them some hours later I found nearly all the bodies gone, only the thorax and wings left; and while examining them a wasp alighted on the board, and I soon proved that he was the culprit.

I have no doubt that wasps are most serviceable to gardeners by destroying caterpillars.

R. S. NEWALL

March 21

Intellect in Brutes

SOME time since I observed the following conduct of two spiders, which will show how they sometimes overcome difficulties in the way of capturing their prey. A rather large house-spider had its web in the corner of a room, and during the summer it feasted upon the insects that were unlucky enough to be caught. One evening I noticed a large dipterous insect strike the web; the spider darted out and succeeded in fastening one foot of the fly. The spider then leapt running back and forth, attaching a thread to a wing, then to a leg, which was soon broken by the violent efforts of the fly to release itself. The spider worked without ceasing for over half an hour to secure its victim; it then quitted operations, and retired to a distant corner of its web. After seeming to reflect for a while what was best to do, it left the web, went up the wall eight or ten inches distant, and entered a crack in the ceiling. I supposed at the time that the spider had been injured in the scuffle, but what was my surprise after a few moments to see the spider coming back, and close behind another followed; the two went on the web near the centre, and stopped side by side, apparently consulting as to the best mode of attack. Then at the same instant both spiders darted upon the insect, one towards the head, the other towards the tail. So rapid were their movements I could hardly follow them. In a short time the insect was securely fastened. Both spiders then returned to the centre of the web. Soon after the

friendly spider went to the crack in the ceiling, while the other enjoyed the feast alone.

A. M.

North Manchester, Indiana, U.S., February 25

Diatoms in the London Clay

YOUR correspondent, Mr. W. H. Shrubsole, inquires where sections may be seen in the lower part of the London clay. He will find a good exposure in a brick-yard, half a mile south-west of Ruydon Station on the Great Eastern Railway; in another at Hadham Ford, on the Buntingford branch line, and several in the brick-fields near Bishop Stortford. In all these sections the lowest part of the London clay may be seen, resting upon sands, or loams, of the Reading series. Upon direct application, or otherwise, I shall be happy to supply Mr. Shrubsole with further information.

W. H. PENNING

Granville House, Finsbury Park, N.

VISUALISED NUMERALS

SINCE I addressed a preliminary memoir to you on this subject,¹ so much curious matter has reached me that I trust you will permit me to state my views afresh, and to deduce some inferences. Many of my readers do not and cannot visualise, and few have the habit in a pronounced degree. I must, however, beg them not to consider their own minds as identical with those of every other sane and healthy person. Psychologists ought to inquire into the mental habits of other men with as little prejudice as if they were inquiring into those of animals of different species to their own, and should be prepared to find much in many cases that is quite unlike their own personal experience.

Persons who have the tendency to use mental pictures as the symbols with which they carry on their processes of thought, do so especially in the case of numerals. Thus, when they think of "six," the figure "6" arises before the mind's eye more readily and vividly than the sound "six" echoes in their mind's ear, or than any other perception of that numeral manifests itself. Now the peculiarity that I accidentally found out is this, that about one out of every thirty males, or fifteen females, not only visualise their numerals in this way, but also invariably assign to each of them a definite place in their mental field of view, where it seems to have a home. Thus 6 may always lie low down to the left, 7 may be found a little higher and more to the front, and so on. It follows that whenever these persons think of a series of numbers, as 1, 2, 3, 4, 5, &c., they always appear to the mind's eye as ranged in a definite pattern or "form." This form is stated in all cases to have been in existence at the farthest period to which recollection goes back, though in many cases it has insensibly grown until it included the higher numbers and even negative values. It is usually of a rambling irregular shape, and though constant for the same person, it differs very greatly in different persons. It may consist of a row or rows of faintly marked figures, suspended in the air or lying on a hazy ground, and when the mental eye travels along the row, each as it is looked at in succession becomes for the moment vivid. Or it may consist of a faint line with nothing on it, along which the eye is wont to travel until it reaches the place where the figure it wants is known to reside, and then the figure starts into sight. Or it may be a haze penetrated by faint lines. Or there may be no figures at all in the line, but only dots denoting position. The planes on which the forms lie slope in some cases up to the heavens, in others down to an immeasurable abyss. They often start a little below the level of the eye and rise gently upwards, reminding one of what the appearance of objects on a table would be to a child whose head hardly overtopped it. All these forms can be drawn in a way more or less satisfactory to those who see them, and I have now received nearly eighty drawings, in about

NATURE, vol. xx., p. 252.