ported by those of three other observers, two of them being the Government geologists, render more singular the statements of Messrs. Officer and Spencer (NATURE, June 29, p. 198) as to their not finding any traces of glaciation in the country around Lake St. Clair, which they explored for a month. Lake Dixon, which Mr. Johnston describes as presenting all the evidences of glaciation in their fullest development, appears to be less than ten miles from the lower end of Lake St. Clair, according to the best map I can refer to; while Lake Petrarch, which Mr. Officer describes as seeing from the top of Mount Olympus, lies between the two in the Cuvier valley, and is also mentioned by Mr. Johnston as being within the highly-glaciated region. It is quite possible that the lakes on the great plateau may be due to damming up, owing to movements of the superficial gravels and clays by the ice or *névé* sheet; but there are evidently an abundance of small valley-lakes and tarns in the western valleys so surrounded by all the marks of extensive glaciation as to render it almost certain that they are true ice-eroded rock basins. It is much to be wished that a more detailed account of this interesting district, with a good map showing all the mountains, lakes, and valleys referred to, would be given us by one of the local geologists. ALFRED R. WALLACE.

The Supposed Glaciation of Brazil.

MR. WALLACE observes in his letter on this subject, published in NATURE (vol. xlviii. p. 589), that "no authoritative disproof has yet been given of the exceedingly strong and positive statement of Agassiz and Hartt."

I confess to my mind the matter had seemed disposed of by the interesting discussion of the subject to be found in the "Notes of a Naturalist in South America" (1887), by the late Mr. John Ball, F.R.S. This experienced and accurate observer arrived at the conclusion from a study of the phenomena on the spot, that they could be sufficiently accounted for by subaërial denudation (see, in particular, pp. 313–8).

In the following passage he rejects the agency of glacial action as definitely as his habitual caution and modesty would allow:—

"I was unfortunately not acquainted at that time with the observations made near Tijuca by Prof. Alexander Agassiz, which appear to him to give evidence of glacial action in this part of Brazil. It would be rash, especially for one who has not been able to examine the deposits referred to, to controvert conclusions resting on such high authority; but I may remark that the evidence is confessedly very imperfect, and that the characteristic striations, either on the live rock or on the transported blocks, which are commonly seen in the theatre of glacial action, have not been observed. I lean to the opinion that the deposits seen near Tijuca are of the same character as those described by M. Liais[1] as frequent in Brazil. The crystalline rocks are of very unequal hardness, and while some portions are rapidly disintegrated, the harder part resist. The disintegrated matter is washed away, and the result is to leave a pile of blocks of unequal dimensions lying in a confused mass." (P. 342.) W. T. THISELTON-DYER.

Royal Gardens, Kew, October 23.

The Nativity of Rama.

I HAVE been much interested in the letter of "Kanhaiyalal," which appears in your issue of August 31. I fully agree with him in the view taken in regard to the verification of dates by astronomical methods, and it really does seem somewhat singular that the example of Sir William Jones, the pioneer of Orientalism in Europe, should have been entirely neglected by his learned colleagues and successors in this department of research. From many considerations it must be obvious that wherever mention of planetary "yogams" or conjunctions, siderial and lunar positions, &c., are given in the text of any classical work, they are to be preferred to any arguments drawn merely from literary style and other empirical data—so much relied upon by Orientalists and scholars generally—when the question is one of a calendaric date.

I have endeavoured to work out the calculation of Rama's birth figure. In *Ramayana* is the following slokam, or stanza, referring to Rama's birth:—"*Chaitre navamike tithau Nakshatre aditi daivatye sewochha samstheshu panchasu Griheshu karkate lagne.*" From this we learn that Rama was born in the ninth day of the Moon's age, and that five planets were in their exaltation signs, the rising sign (*lagnam*) being Cancer (of the Hindu Zodiac). The planets' places are given in Section 18 of the English translation of *Ramayana*, by Manmatha Nath Dutt, M.A., in the following words:—

"And then, when six seasons had rolled away after the comp'etion of the Sacrifice, in the twelfth month, on the ninth lunar day, under the influence of the Punarvasu asterism, when the Sun, Moon, Saturn, Jupiter, and Venus were at Aries, Capricorn,[1] Libra, Cancer, and Pisces, and when Jupiter had arisen with the Moon at Cancer, Kaushalya gave birth to that lord of the universe, bowed unto by all the worlds, Rama, &c."

It may be well to state for the benefit of those not acquainted with the Hindu zodiac, that an asterism includes 13° 20′ of the ecliptic circle, and consequently there are twenty-seven asterisms in all. Of these, Punarvasu is the seventh. The zodiac commences with the asterism *Aswini*, and the fixed star *Revati* is the point from which enumeration of longitude begins. This star is said to have been coincident with the equinoctial point ♈ in the year 3600 of the Kali Yuga, *i.e.* 498 A.D.

The last conjunction of Saturn and Jupiter in the sign Libra was in K.Y. 4224, and the one previous in K.Y. 1344; and from this we must subtract three Signs to bring Jupiter into Cancer (its exaltation). This equation referred to the "period" of Jupiter, *i.e.* twelve years, gives three years to be subtracted. The year K.Y. 1341, therefore, would see Saturn in Libra, and Jupiter in Cancer as required.

The Moon being nine days old at the birth of Rama, and its motion in respect to the Sun being 12° per day, its distance from the place of conjunction must be taken as over 96°. But it is stated in the *Slokam* that the Moon is in Punarvasu, and as this asterism ends at 93° 20′ from the star Revati, it is evident that the conjunction of the luminaries took place in the twenty-sixth degree of *Minam* or Pisces; and that on the ninth day the Moon was in the first degrees of Cancer (Hindu *Kartaka*) and the Sun in the fifth degree of Aries (Hindu *Mesham*).

To determine the date of this planetary epoch we must have recourse to the Ayauámsha, the distance between the fixed star Revati and the Vernal Equinox. The Hindus compute this to be 54″ per year, and in accordance therewith their month of Mesham (Aries) begins on April 11. At the present time Revati is behind the Equinox, but in K.Y. 1341 it was in front of it, regarded by the order of the Signs. The calculation for K.Y. 1341, according to Suryasiddhauta, is:—

$$(3600 - 1341) \times 54'' = 33^\circ\ 53'\ 6''.$$

Referring this to the Equinox, it gives a point corresponding to the twenty-seventh degree of Aquarius in *our* zodiac, which was the point at which the Hindu zodiac began in the year K.Y. 1341; and from this we must take 4° to bring us to the 26th of *Minam*, wherein the Sun and Moon were conjoined at the birth of Rama. The result is the twenty-third degree of Aquarius in our zodiac.

We have already obtained the year K.Y. 1341 from the positions of the planets Jupiter and Saturn, and we may now apply this luni-solar position as a test.

On February 11, 1888, the Sun and Moon were conjoined in the twenty-third degree of Aquarius. This date corresponds to the beginning of the tenth month of the K.Y. year 4989. Applying the Metonic cycle, we find that a conjunction of the luminaries also took place in the twenty-third degree of Aquarius (Hindu twenty-sixth *Minam*) in K.Y. 1341, thus:—

$(4989-1341) \div 19 = 192$ *exactly*. I have not yet made reference to the position of Venus as given in the above *Slokam*, but I think there is strong evidence of this being the correct epoch, and I think it not unlikely that Venus had less than 30° west longitude of the Sun, in which case it would be in the Hindu sign corresponding to our Pisces, *i.e. Minam*, as required by the *Slokam*.

This epoch corresponds to noon (local time) February 10, 1761 B.C., disregarding the change of Style; and, if correct, may be the time of the birth of Rama; but on this point I should not care to judge too hastily, for in view of the recurrence of these positions at some earlier or later date, we have no evidence which should lead us to select one rather than another epoch.

One thing strikes me as sufficiently curious to record in

[1] See his valuable work, "Climats, Géologie, Faune et Géographie Botanique de Brésil."

[1] This should be *Cancer*, not Capricorn, as is seen from the fact of the Moon's rising with Jupiter.

this connection, viz. that in Sáukaravijaya of Vidyáranya, the same positions are given for the planets at the birth of Sáukarácharya, with the exception of the Moon, which is in Árthra, *i.e.* Gemini, 6° 40′ to 20° 0′ of the Hindu zodiac. These positions of the Sun, Moon, Jupiter, and Saturn took place on the 1st of *Mesham*, Kali Yuga 4221, corresponding to March 30, A.D. 1119, without change of the present style.

I am afraid, however, that these dates will hardly suit my Hindu friends, whose devotion to these great personages gives them a sense of "distance" which is best satisfied when expressed in *years!* I give these notes, however, for what they may be worth.

Adyar, Madras. WALTER R. OLD.

NOTE.—According to the Suryasiddhanta rules for computing the longitudes of the planets, I find that Mars was in Capricorn, its "exaltation" Sign, in the month of *Mesham*, K.Y. 1341, as required by the data given for Rama's epoch, its longitude in the Hindu zodiac being Capricornus 13°.—W. R. O.

On the Latent Heat of Steam.

SINCE the invention of M. Berthelot's extremely elegant and simple apparatus, described in his "Mécanique Chimique," vol. i. p. 288, the approximate determination of the latent heat of vaporisation of liquids has become comparatively easy. The exact evaluation of the correction due to the heating of the calorimeter from extraneous sources is, however, a matter of considerable difficulty with the original form of apparatus. The correction is necessarily calculated from data supplied by the thermometric observations made previously to, and after, the actual condensation of the liquid has taken place. For this calculation to be as simple and satisfactory as possible, it is essential that during the whole experiment the temperature of the bodies in the immediate neighbourhood of the calorimeter shall remain approximately constant. In M. Berthelot's method of determination this condition is however not strictly fulfilled. For during the "preliminary period," although the flame is lighted over the calorimeter, the liquid in the flask has not yet begun to boil, so that the radiation to the calorimeter varies, and during the "final period" the flame is extinguished and no further heat reaches the calorimeter from this source. Also during the beginning of the "middle period," a considerable amount of liquid which has been volatilised from the flask at a temperature below its boiling-point, reaches the worm and is there condensed. We therefore modified the apparatus in such a way that the flame was at a constant height and the liquid was boiling *during the whole time of the experiment*, including both the preliminary and final periods. We found that under these circumstances, with a rise of 3° or 4° in ten minutes, the Regnault-Pfaundler correction is perfectly accurate. We propose shortly to publish a complete description of our apparatus, and shall not therefore go into details at present. It differs mainly from that of M. Berthelot, by the insertion in the interior of the boiling flask of a glass valve, which is opened when the rise of the thermometer in the calorimeter has become steady, and closed when sufficient liquid has been condensed in the worm. The vapour during both the preliminary and final periods passes into a reversed condenser.

Our main reason for this communication is to record the somewhat remarkable results obtained with water, and to ask if any of your readers can give information as to any accurate work upon the latent heat of steam published since that of Regnault (*Mémoires de l'Académie des Sciences*, t. 21) in 1847.

We give the results of five experiments (done at pressures differing but little from 760 mm.), which are still subject to certain corrections not exceeding ± 1 unit.

	Wt. of water condensed in grams.	Time of condensation in minutes.	Rise of temp. in calorimeter in deg. C.	Latent heat of steam (L).
(1)	10·122	7½	3·491	525·6
(2)	12·546	15	4·416	524·7
(3)	9·278	8	3·235	526·6
(4)	9·854	7	3·439	525·0
(5)	2·742	6	·991	523·9

It will be noticed that in experiment 5, where the amount of water condensed was purposely reduced, so as to increase as far as possible the experimental error, the result obtained differs but slightly from the mean. This mean, 525·2 (omitting experiment 5, 525·5) is over 2 per cent. lower than that of Regnault. The thermometer used was one divided into fiftieths of a degree, by Baudin, and was compared with a thermometer calibrated at the International Bureau of Weights and Measures. Every precaution was taken to ensure accuracy of reading.

We have sought for confirmation of our results in the indirect determinations of other observers. If we insert the latest values for the specific volume of steam at 99·6° given by Perot (*Ann. Chim. et Phys.* [6] 13, p. 159) and for the mechanical equivalent of heat by Griffiths[1] (NATURE, vol. xlvii. p. 476) in the thermodynamic formula,

$$L = \frac{T}{J}(S - S')\frac{dp}{dt},$$

we find the number 527·43 for the value of L at 99·60° C.[2] The number given by Regnault for 100° C. is 536·7. We have also selected from the numerous results obtained by Joly (*Proc. Roy. Soc.* vol. xli. p. 358) with his steam calorimeter those relating to silver, which is a substance easy to obtain in a state of purity. If we take the number given by Regnault for the specific heat of silver, we find his own determination of the latent heat of steam confirmed. On the other hand the concordant numbers for the specific heat of silver, given independently by Kopp and Bunsen, lead to a result about 1½ per cent. lower than that of Regnault.

The complete discussion of such results, however, is a matter of great difficulty owing to the uncertainty which prevails with regard to the specific heat of water. We have not as yet succeeded in discovering any constant error capable of explaining the discrepancy between our result and that of Regnault, but further experiments are now in progress.

The question, as need hardly be pointed out, is of considerable practical importance in connection with problems relating to the steam engine.

P. J. HARTOG.
J. A. HARKER.

Physical Laboratory, Owens College, October 19.

Artificial Amœbæ and Protoplasm.

IN No. 1251 of NATURE, Dr. John Berry Haycraft has written a review on Prof. O. Bütschli's investigations of microscopic foams and protoplasm.

The biological parts of the contribution I may leave my colleague, Prof. Butschli, to answer, but as my investigations are also mentioned, and my name several times quoted, though always mis-spelled as "Nuincke," instead of Quincke, I may perhaps be allowed to call attention to the fact that I, not Prof. Bütschli, as the reviewer asserts, was the first who tried to explain the movements of amœbæ and protoplasm by physical laws, by the periodical spreading of a soap solution. In 1879 I explained the voluntary formation of an emulsion observed by Prof. Gad, and the amœboid movements of oil-drops by the periodical spreading of a soap solution upon the common surface of oil and water, and I said "that foam is an emulsion of air instead of oil, and that the durability of foam depended on the same conditions as the durability of an oil emulsion."[3] In a continuation of these investigations I explained in the year 1888 the movements of protoplasm by the same physical principles, making the supposition that it was intermixed with thin oil-films, and in the cells of plants, surrounded by an oil-coat.[4] I therefore believe I was the first to point to the foamy structure of protoplasm, which was later on further investigated by Prof. Bütschli.

Is Dr. John Berry Haycraft acquainted with my investigations, and from whence does he deduce the right of calling them "toys for the physicist"? They form the conclusion of a series of researches on capillarity which I began 37 years ago, and by which I, for the first time, showed that surface-tension is considerably altered by layers of a foreign substance of far less thickness than 1/10 of a light-wave; for the first time, also, the

[1] We understand that Mr. Griffiths' number is still subject to a slight correction, but that this does not amount to 1 part in 1000.

[2] $\frac{dp}{dt}$ was calculated from Roche's formula quoted by Hirn, Théorie Mécanique de la Chaleur, *t.* I. p. 325.

[3] G. Quincke, "Ueber Emulsions bildung und den Einfluss der Galle auf die Verdanung" (*Pflüger's Archiv.* 1879, p. 144).

[4] G. Quincke, "Ueber periodische Ausbreitung an Flüssigkeits oberflächen und dadurch hervorgerufene Bewegungserscheinungen" (*Sitzungsber. der Berliner Akad.* 12, 7, 1888. *Wiedem. Ann.* 35, p. 580-642, 1888). "Ueber Protoplasma bewegungen und verwandte Erscheinungen" (*Tageblatt der 62 Versammlung Deutscher Naturforscher und Aerzte*, Heidelberg, 1889, p. 204-7).