

## LETTERS TO THE EDITOR

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## Liquid Air at One Operation.

It is to be hoped that personal matters will not divert attention from the very interesting scientific questions involved. The liquefaction of air at one operation by Linde and Hampson is indeed a great feat and a triumph for the principle of regeneration. But it must not be overlooked that to allow the air to expand without doing work, or rather to allow the work of expansion to appear as heat at the very place where the utmost cooling is desired, is very bad thermodynamics. The work of expansion should not be dissipated within, but be conducted to the exterior.

I understand that attempts to expand the air under a piston in a cylinder have led to practical difficulties connected with the low temperature. But surely a turbine of some sort might be made to work. This would occupy little space, and even if of low efficiency, would still allow a considerable fraction of the work of expansion to be conveyed away. The worst turbine would be better than none, and would probably allow the pressures to be reduced. It should be understood that the object is not so much to save the work, as to obviate the very prejudicial heating arising from its dissipation in the coldest part of the apparatus. It seems to me that the future may bring great developments in this direction, and that it may thus be possible to liquefy even hydrogen at one operation. RAYLEIGH.

Terling Place, Witham, June 26.

## Liquid Hydrogen.

I OBSERVE with some amusement that you still allow Mr. Hampson to embellish your columns with vain repetitions of accusations which he was compelled to withdraw when he met me face to face at the meeting of the Society of Chemical Industry.

It is idle to discuss any question with a man whose notion of argument is to restate in somewhat different language what has already been refuted, and then to assert that the accuracy of his propositions has not been questioned.

Mr. Hampson must be a singularly dull person if he fails to appreciate the magnitude of the draft he makes upon the credulity of the world. He asks men of the world to believe that he, being convinced of the general dishonesty of Royal Institution methods, and being in possession of a novel and valuable invention, fully completed but not protected by patent, came unbidden and unsought to reveal all the details to a man whom he knew to be my assistant.

He further expects the world to believe that having thus given himself away, he refrained from protecting his invention until the rival inventor had had ample time to profit by his childlike simplicity. But even this is not all; for the world is further asked to believe that after he had placed the Royal Institution in possession of full information concerning a finished invention, it took me more than a year to utilise his generosity, while in the interval Dr. Linde had published his method and apparatus. Does not all this amount to rather a large order?

But perhaps no one can answer Mr. Hampson so well as Mr. Hampson himself. At the meeting of the Society of Chemical Industry on May 2, Mr. Hampson expressed himself as follows (*The Journal of the Society of Chemical Industry*, No. 5, vol. xvii. p. 421):—"Prof. Dewar will do me the justice to say that I have nowhere published any statement that he had made use of anything I had communicated, or of what I had invented. I have, therefore, nothing to withdraw, since I have nowhere suggested that a communication had been passed on to him. . . . I am not to be understood as saying that my proposal was passed on to Prof. Dewar."

What is Mr. Hampson to be understood as saying in the letters you have published, if not the precise contrary of what he said when brought to book at the Society of Chemical Industry?

How otherwise is the "credit of science" involved?

It is worth noting that in March 1896, a year and a half after the famous interview with Mr. Lennox, Mr. Hampson threatened Messrs. Lennox, Reynolds, and Fyfe with legal proceedings on the ground that a lecture apparatus made for my Chemical

Society paper of 1895, and subsequently advertised by them in NATURE, was an infringement of his patent. They replied that he might take any action he pleased. He has never taken any.

Mr. Hampson's extract from my speech at the Society of Arts, reported in the *Journal* for March 11, 1898, is so completely isolated from the context as to convey a totally wrong impression. When Mr. Hampson made it, he had before him my statement that "although this regeneration system had been carried by Dr. Linde to the acme of perfection, no one who constructed low temperature apparatus rejected the cool gas without utilising it; the great advance was that Dr. Linde did so completely."

If all that Mr. Hampson wants is "recognition in historical or explanatory works" of his claim to be the inventor of a general claim to intensive refrigeration, he will find Solvay, Dr. Linde, and Prof. Onnes obstacles quite as serious as myself. Further, this attempt to justify going behind my back in his relations with a member of the staff of the Royal Institution, is a too transparent subterfuge to require further comment.

JAMES DEWAR.

## The Spectrum of Metargon?

IN the account given by Prof. Ramsay of his researches on the "Companions of Argon," he has omitted to draw attention to a very curious similarity between the spectrum of his new gas "metargon" and the ordinary spectrum of carbon, with which every student of spectrum analysis is familiar.

The following comparison of wave-lengths will make the similarity apparent.

		Ramsay's metargon.		Carbon (Angström and Thalén).
Citron band	1	5632.5	...	5633.0
	2	5583.0	...	5583.0
	3	5537.0	...	5538.0
Green band	1	5163.0	...	5164.0
	2	5126.5	...	5128.0
Blue band	1	4733.5	...	4736.0
	2	4711.5	...	4714.5
Indigo band	...	4314.5	...	4311.0

There are three of Ramsay's bands not included in this list, but these are nearly coincident with known bands in the cyanogen spectrum.

It seems hardly credible that Prof. Ramsay has not guarded against the possibility that all these bands may be due to carbon, and not to a new gas; but some explanation seems required, for though the coincidences in the two sets of bands is not complete, there is no case known in which two different elements have spectra so nearly alike as those of carbon and metargon seem to be.

ARTHUR SCHUSTER.

## Anatomy of the Swallows.

MY friend Dr. R. Bowdler Sharpe, of the British Museum, has favoured me with a copy of his recent and very useful memoir upon the swallows (*Hirundinidae*), and we find the group treated under the several heads of (1) an introduction; (2) geographical distribution; and (3) the literature of the Subject. In the last, the author of this contribution has evidently intended to present a very complete list of the titles of works that have been written about swallows, extending between the years 1731 to 1894 inclusive; while in the introduction he makes the statement that "The Swallows appear to us to be such a well-marked and isolated Family of Passeres, that, in the absence of any detailed account of their anatomy and general structure, which, so far as we know, has not been attempted, there remains little for us to say." As one, perhaps, who has had occasion to keep a little better track of the literature of hirundine morphology, permit me to invite the attention of this distinguished systematist to a memoir published by me in the *Journal of the Linnean Society of London* for 1889 (vol. xx. pp. 299-394, with 39 lithographic figures); he will find in it, under the title of "Anatomy of the North-American *Hirundinidae*," not only a complete account of the pterylography of every species of swallow in the United States, but myological descriptions of the same; with references to their visceral anatomy; and an entire chapter devoted to the osteology of all the United States genera. Not only this, but on the plates, illustrating the same memoir, Dr. Sharpe will find very accurate figures of the skulls (nat. size) of *Progne subis*, *Chelidon erythrogaster* and *Tachycineta thalassina*—all important forms

of swallows, of which the "anatomy and general structure" are very well known. In that paper he will also see that I have attempted to compare the anatomy of all our swallows, with the structure of the American swifts, and with *Ampelis*, and a great many other birds. This paper of over one hundred pages, and numerous plates, is not found in Dr. Sharpe's works upon the life-history and structure of swallows find no place "Literature" of the *Hirundinide*. Numerous other important works upon the life-history and structure of swallows find no place in Dr. Sharpe's bibliography of this group. In this connection, then, it may be said that our author distinguishes but *twelve* genera of swallows in the world's avifauna, and of these I have carefully compared, illustrated and published full accounts of the anatomy of no less than *six* genera, or in other words fifty per cent. of those known at present to science. And, as *Stelgidopteryx* was included among these, I very much question that any very marked anatomical differences will be found to exist among the unexamined types.

Further, as has been the case with not a few other anatomists, I have treated the subject of the systematic position of the swallows in numerous places, but more particularly in my "Contributions to the Comparative Osteology of the Families of North American Passeres," in which the skeletons of all the passerine birds in the United States were, in a comparative way, passed in review, the swallows with the rest. This is another formal work dealing with the *Hirundinide*, overlooked by our bibliographer of this family of birds. R. W. SHUFELDT.

2508 University Place, Washington, U.S.A., June 11.

#### Rotifers in Lake Bassenthwaite.

It may be of some interest to readers of NATURE to call attention to the fact that during the warm days of June 16-18, the beautiful Rotifer *Asplanchna priodonta* was to be found in the surface waters of Lake Bassenthwaite, Cumberland, in very great abundance. After dragging a small tow-net through the water from a row-boat for twenty minutes, the water collected in the bottle attached to the end of the net was perfectly turbid with the multitude of these animals, interfering very materially with the observation of the other constituents of the plankton. Observations taken by Mr. Ashworth in different parts of the lake in the early morning, mid-day and the evening, proved that they were not present merely in a localised cloud, but distributed in immense numbers all over the lake, from the surface to a depth of ten feet or more.

The observation is of interest, as the "Lakes" are not given in the great work on Rotifers, by Hudson and Gosse, as a locality for this genus, nor is there mention made of its occurrence in such great numbers. Perhaps some of your readers may be able to inform me if this phenomenon has previously been recorded in England. SYDNEY J. HICKSON.

The Owens College, Manchester.

#### Lion-Tiger Hybrid.

SOME of the readers of NATURE who have the opportunity of visiting the exhibition at Earl's Court may be interested to know that one of the members of the "Happy Family" now on show there is evidently a hybrid between a lion and a tiger. The animal appears to be about two years old. By artificial light the ground colour closely resembles that of a lion, being tawny rather than reddish yellow; but the tiger-stripes, though faint, are quite visible, especially on the tail. Such stripes might perhaps be mistaken for unusually strong cub-markings of the lion retained for an unusual length of time. But apart from the stripes, the tiger-strain comes out strongly in the blackness of the corners of the mouth, the hairs of the lips in this place being jet black in the tiger, white in the lion. R. I. POCOCK.

Natural History Museum, June 22.

#### Transference of Heat in Cooled Metal.

J'AI l'honneur de vous envoyer pour votre si intéressant journal, une remarque qui pourra intéresser peut-être quelques lecteurs de NATURE.

Il s'agit d'un phénomène certainement bien connu et qui n'a peut-être pas attiré l'attention des physiciens, comme il semble le mériter. Prenons dans la main l'extrémité d'une barre de métal et chauffons l'autre extrémité aussi fortement que possible, mais pourtant de manière à pouvoir tenir la barre sans se brûler par la première extrémité. Cela étant, refroidissons brusquement l'extrémité chauffée, soit en la plongeant dans l'eau, soit au moyen d'un jet d'eau. Nous constatons alors que

la température de la partie non chauffée monte et que nous sommes obligés de lâcher la barre, si nous ne voulons pas nous brûler. C'est ce que savent très bien, tous ceux qui ont travaillé à la forge ou qui ont fait des soudures de petites pièces métalliques tenues à la main. Les ouvriers disent que la chaleur est repoussée par le froid vers la partie non chauffée. Le phénomène a-t-il été étudié scientifiquement et connaît-on sa cause? HENRY BOURGET.

Astronome adjoint à l'observatoire de Toulouse, juin 14.

#### Parker and Haswell's "Text-book of Zoology."

IN reply to Prof. Ray Lankester's references to me in his review of Parker and Haswell's "Text-book of Zoology" in this journal for May 12th, I should like to state as follows:— (1) That I had nothing to do with correcting the "final revise" of this book. (2) That the new English edition of Prof. Wiedersheim's "Comparative Anatomy of Vertebrates" is not a translation, but an "adaptation." (3) That the assertion with regard to the ossification of parts of the skeleton in Elasmobranchs in the latter work is not the same as that to which Prof. Lankester objects in the "Zoology," whether the latter be right or wrong. (4) That Götte in 1878 distinctly stated that true bone is undeniably present in the vertebral centra of several Elasmobranchs the histology of which he describes, and that all kinds of intermediate stages between calcified cartilage and true bone occur in these centra. (5) That in the fourth edition of Marshall and Hurst's "Practical Zoology" true bone is said to occur in the centra of *Scyllium*, and that this statement does not appear in previous editions of the book. (6) That in the fourth German edition of Wiedersheim's "Grundriss der vergleichenden Anatomie," which was published a week or two ago, the centra of Elasmobranchs are described as being "kalkknorpelige resp. knöchernen." W. N. PARKER.

#### SOME RESULTS OF MY RESEARCHES ON OCEANOGRAPHY.

BY ALBERT, PRINCE OF MONACO.

THE devotion that has been quite lately given to the new science called "oceanography," has decided me to dedicate some of the strongest efforts of my life to its advancement. I set about my work in 1885 with a small sailing schooner of 200 tons, the *Hirondelle*, and I

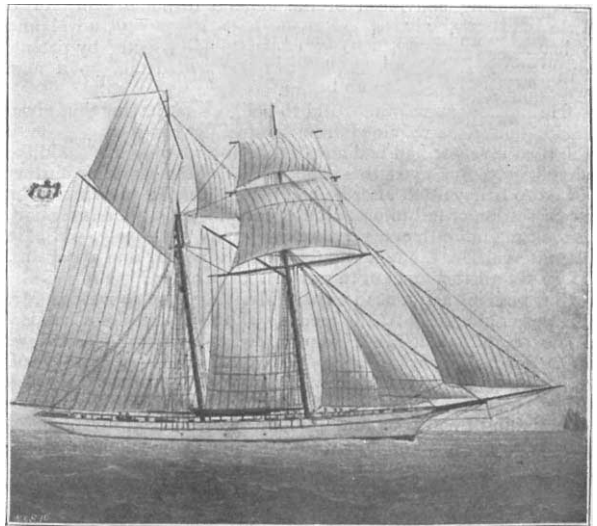


FIG. 1.—The *Hirondelle*.

explored the Atlantic as far as the coast of Newfoundland, and as deep as 1600 fathoms, without any power greater than the arms of my fourteen sailors. Later on I built a steam vessel of 560 tons, better fitted for such rough work; this was the first *Princess Alice*. Now I