contraction occurred, and the spectrum of the gas was unaltered, after removing oxygen.

(3) An artificially made mixture of carbon monoxide and argon—about equal volumes of each—was mixed with oxygen. It was sparked and exploded. It was then further sparked over soda for a quarter of an hour. On introducing the gas into a vacuum tube, after removal of oxygen, no carbon lines or bands were seen, but only the spectrum of pure argon.

The bands in the green of metargon are exceedingly brilliant, and the spectrum is by no means of the character of a subsidiary one. It does not appear to be possible to enfeeble them relatively to the rest of the spectrum.

We have found it possible, in hundreds of cases where it was necessary, to remove traces of carbon compounds from gases evolved in heating minerals—chiefly helium—to remove the carbon bands by "running" the tube, *i.e.* by increasing the intensity of the current until the aluminium pole melted. The green and red bands, under these circumstances, slowly disappear, and the spectrum of helium or of argon, as the case may be, shines out "clean-cut," and shows as bright lines on a black background. This process is impossible with metargon; no change is produced even after long "running."

We must again call attention to the facts that this gas shows the ratio of specific heats 1·66; that it possesses sensibly the same density as argon; and that it is a solid at the temperature of liquid air, boiling under atmospheric pressure.

Although, therefore, we are the first to admit that the spectrum of this gas requires further investigation, yet, from what we have observed, we provisionally adhere to our original view that it possesses the characteristics of a definite chemical individual.

We would take this opportunity of correcting a misprint in the *Comptes rendus*, cxxvi. p. 1762, where the wave-length 5849·6 is attributed to metargon, instead of to neon.

W. RAMSAY.
M. W. TRAVERS.
EDWARD C. CYRIL BALY.
University College, London, Gower Street, W.C.

Liquid Hydrogen.

PROF. DEWAR'S letter in your last issue is such a pronounced personal attack on me, that I feel I ought to deal with the remarks to my prejudice which it contains, though I will try to avoid imitating its tone.

(1) He refers to the statements on which I base my claim to the invention of the self-intensive method as matter which "has already been refuted." I should be glad to know when and by whom. They are clearly numbered 1, 2, 3, 4, in my last letter, and form the substance of my first. At the Society of Chemical Industry Mr. Lennox, though he was present and heard the statements repeated, with every opportunity of contradicting them, did not do so. Prof. Dewar, far from refuting statements 1 and 3, did not even deny them; and his attack on the second (respecting the novelty of the invention) resulted in strengthening it, since it showed that he was reduced to building up an anticipation by taking material from several different sources, having been unable to find any account of the combination before my proposal in November 1894. The fourth statement had not then been made, as hydrogen had not been liquefied. Where then has the refutation taken place? In both his letters to you Prof. Dewar keeps all four statements at a very respectful distance.

(2) Prof. Dewar uses the words "accusations which he was compelled to withdraw when he met me face to face," and "when brought to book at the Society of Chemical Industry." It is quite untrue that I withdrew anything at all. On the contrary, I said that "I had nothing to withdraw," and that my assertions were "a simple and direct statement of historical facts," repeating more frequently than is shown in the printed report that the facts were exactly as I had stated them. As to what took place between Prof. Dewar and his assistant it is obvious that, not having been present, I could have no knowledge; and I can only publish what I know of my own knowledge, or can prove by conclusive evidence. Deductions from the facts must be made by every one for himself, and I reminded Prof. Dewar that as I had published no such deductions I could not withdraw them.

(3) I was not, at the time of my communications to Mr. Lennox, "convinced of the general dishonesty of Royal Institution methods," as Prof. Dewar suggests. I regarded the Royal Institution as one of the temples of science, and Mr. Lennox as its chief acolyte, who might, perhaps, when my offering had been examined and found worthy of acceptance, introduce me to the favourable notice of higher authorities.

(4) What I am "to be understood as saying in the letters you have published," is so clearly set forth in my four numbered statements in your issue of June 23, that Prof. Dewar's doubts on the point cannot be so puzzling as his question implies.

(5) Prof. Dewar's acquaintance with patent-law cases involving a host of partisan expert witnesses and costly counsel is too extensive and familiar to leave him in any doubt as to the reason why a man without means does not begin a prosecution for infringement. I could, however, warn the infringers; and this I did. The protest having been made, I am still free to prosecute when circumstances render it possible and advisable to do so, and the present prospects of low-temperature work make it by no means unlikely that action may yet be taken.

(6) Prof. Dewar's admission, referring to Dr. Linde's method, which he had just heard described, "that the practicability of such a mode of working had never struck him," was made in the opening sentences of his remarks, without any limiting qualifications, but with express inclusion of both "the mechanical ingenuity and knowledge of thermodynamics" involved; so that its only fair interpretation is with reference to the description that had just been given of Dr. Linde's combination, which is, except in details, the same as mine. The force of the admission is not lessened by quoting a subsequent passage which refers to one part of the combination. Dr. Linde and I had invented a combination which made it possible to liquefy air without using any other refrigerant than water. Prof. Dewar admitted that he had never thought out the whole combination. Whatever therefore he and others had done with some parts of it, when the combination came out he ought to have recognised its novelty, instead of endeavouring to piece it together out of old patents and experiments.

(7) Neither M. Solvay nor Prof. Onnes claims to have invented a combination by which continuous free expansion from a nozzle is able, without using other refrigerants, to liquefy air: so that Prof. Dewar misleads his less instructed readers by putting those gentlemen forward as my rivals on the ground that they claim to have used parts of the combination.

My communications to Prof. Dewar's assistant were, however, of earlier date than any publication of Dr. Linde's process. This is the fact of which, with its corollaries, I had hoped to obtain a frank admission from Prof. Dewar, and I would have much preferred that the discussion in your columns had been confined to the points raised in my first letter. Prof. Dewar, however, instead of frankly admitting my claims, as other prominent scientific men have done, or discussing the statements on which they are based, has seen fit to give his attention almost entirely to the more personal elements in the controversy. In two letters he has called my action "dubious" and "not straightforward," and has said that either I am "a singularly dull person" or am consciously imposing "upon the credulity of the world," that I contradicted myself "when brought to book," and that I "was compelled to withdraw accusations" which in fact I explained that I had never made, while refusing to withdraw anything at all. Under these circumstances I think that few of your readers will blame me for asserting the justice of my claims, though I regret that so much of your valuable space should have been occupied by matters of this nature. W. HAMPSON.

July 1.

The Distribution of Prepotency.

NO numerical estimate appears to have been made of the frequency with which different grades of prepotency are distributed. Breeders are familiar with the fact that certain animals are peculiarly apt to impress their personal characters upon offspring, but how frequently and to what extent this tendency occurs has never, I believe, been investigated. The following attempt is therefore of interest, though not free from objection in minor details. In *Wallace's Year Books* of the American Trotting Horses, lists are given (1) of the sires of offspring, any one of which has succeeded in trotting one mile in 2 minutes and 30 seconds or less, or who has "paced" (= ambled) the same distance in 2 minutes and 25 seconds or less; (2) of the dams of at least two such offspring, or else of one such offspring and one such grandchild. A selection was made from lists (1) and (2) of sires and dams who were themselves foaled before 1870 and who therefore were, or would have been, at least 25 years old at the date of the last *Year Book* in my possession, which is for 1896. This is practically a sufficient allowance, giving say 5 years to the foals in which to make their record, and 20 years as the limit of the breeding age of either parent. My selection from list (1) contained 716 sires, and that from list (2) contained 494 dams. Reducing to percentages, the distinguished offspring (standard performers) to 100 sires and to 100 dams from these lists respectively, are tabulated below, disregarding decimals. Thus out of each

Distribution of the Parents of Standard Performers.

	Number of standard performers produced by a single parent, sire or dam.											Total parents.
	1	2	3	4	5	6 to 10	11 to 20	21 to 30	31 to 40	41 to 50	51 and above	
Sires ...	46	17	10	7	3	9	4	1	1	1	1	100
Dams ...	50	35	10	3	1	1	—	—	—	—	—	100

100 selected sires, we see that 46 produce only one standard performer, 17 produce two, 10 produce three, 7 produce four, and 5 produce three. Thus far the distribution of prepotency is not particularly abnormal, and we might have guessed that there would be about 3 cases more, none of which would contain more than from seven to eight standard performers, but the facts are surprisingly otherwise. Although the frequency of the successively larger families decreases with fair regularity, the rate of their diminution is far too slow to be compatible with the normal law of frequency. Instead of the expected 3 cases, each containing six, seven or eight standard performers, we find 17 cases of far higher contents. Thus in the list of 716 sires, the number of distinguished offspring are,—60 to *Blue Bull*, 71 to *Strathmore*, 83 to *George Wilkes*, 92 to *Happy Medium* and 154 to *Electioneer*. Making full allowance for the tendency of breeders to send the best mares to the best horses, the prepotency of the sires just named is enormous, that of *Electioneer* superlatively so. The same results are indicated by the produce of the dams, though the figures are less striking owing to the relative fewness of their offspring. A sire produces some 30 foals annually, a dam only one, while the period of production is presumably longer for the sire than for the dam. Consequently out of the list of 494 dams, the three mares *Emeline* (*sic*), *Minnehaha* and *Green Mountain Maid*, who produced respectively 7, 8 and 9 standard performers, seem as phenomenal as the five horses mentioned above. Again, prepotency is as we should have expected, heritable in a marked degree; thus all of the above five sires except *Blue Bull* are sons of "*Hambletonian* 10," and one of the three mares, *Green Mountain Maid*, was dam of *Electioneer*.

My conclusion is that high prepotency does not arise through normal variation, but must rank as a highly heritable sport, or aberrant variation; in other words its causes must partly be of a different order, or else of a highly different intensity, to those concerned in producing the normal variations of the race. In a sport, the position of maximum stability seems to be slightly changed. I have frequently insisted that these sports or "aberrances" (if I may coin the word) are probably notable factors in the evolution of races. Certainly the successive improvements of breeds of domestic animals generally, as in those of horses in particular, usually make fresh starts from decided sports or aberrances, and are by no means always developed slowly through the accumulation of minute and favourable variations during a long succession of generations.

FRANCIS GALTON.

Zoology as a Higher Study.

THE following, necessarily condensed, comments on Prof. Ray Lankester's criticisms may be permitted.

(1) Prof. Lankester's views on the citation of authorities in text-books have been published before. To the best of my belief "authoritative public opinion," if it had expression, would favour the side of common sense in this matter. A text-book, adapted to the needs of the elementary student, in which the "historical method of exposition" should be followed, and each discoverer awarded his due meed of recognition, is an impossibility, within reasonable limits of size and cost. Our reasons for omitting all references to authorities really were those given in the preface, which I invite Prof. Lankester to re-peruse, not those which he ungenerously ascribes to us.

(2) Where the names of the original authors of figures have not been quoted, and the proximate source from which the block was borrowed or the figure copied has alone been given, the name of the original author is, in most instances, a matter of no consequence whatever. In a very few cases the omission is regrettable.

(3) The main responsibility for the "most astonishing" of the errors which Prof. Ray Lankester has noticed in the text-book, viz. the statement that ossification occurs in the skeleton of Elasmobranchs, rests with me, and not with the two sons of W. Kitchen Parker. The most astonishing thing to the initiated onlooker will doubtless be Prof. Lankester's evident confidence that this is an error.

(4) The "error" with regard to the nephrostome of Lumbricus is Prof. Lankester's. If he will read over that part of the "Text-book" as it would be read by a student, taking the description of *Nereis* as the foundation, he will understand what I mean. "Corresponding segment" is not "same segment."

(5) The criticism of the statement regarding cœlome and hæmocœle in Peripatus would have lost all its apparent cogency had Prof. Lankester quoted only three lines more (see "Text-book," vol. i. p. 561).

WILLIAM A. HASWELL.

The Nature and Habits of Pliny's Solpuga.

I READ with much interest Mr. Pocock's article on "Solpuga" (NATURE, vol. lvii. p. 618). It may be worthy of note that a species of Galeodes is met not infrequently in Southern California, and is one of the few Arthropodous animals that is bold enough to attack and devour the honey-bee. It enters the hive and seizes the bee, worker as well as drone, and soon makes away with it. Were these Arachnoids as abundant as the Robber-flies (Asilidæ), they would be nearly as serious enemies of the bee-keepers of Southern California as are those insects. They are not, however, sufficiently numerous to do any serious mischief, and so are not feared or dreaded.

A. J. COOK.

Claremont, Cal., May 12.

The Weather of this Summer.

IN your notice of Symons's *Met. Mag.* this week, I seem to be credited with (discredited by?) the announcement that this summer will probably be wet. May I point out that it is one thing to announce this, and another to say that in the five years ending with the next sunspot minimum year (say 1901, or thereabouts), there will probably be more wet summers than dry? Further, the two rules cited in the notice are based on data extending from 1816, not merely from 1841.

July 8. ALEX. B. MACDOWALL.

THE NATURAL HISTORY MUSEUM.

THE following memorial has been addressed to the Trustees of the British Museum:—

Sir, My Lords, and Gentlemen,—We, the undersigned, being persons interested in the science of Natural History, venture to address to you the following observations suggested by the retirement of Sir W. H. Flower from the post of Director of the Natural History Museum (British Museum).

It is, in our opinion, of great importance to the welfare of Natural History that the principal official in charge of the national collections relating to this subject should not be subordinate in authority to any other officer of the Museum. The Natural History Collections are in a part of London remote from the National Library and the other departments of the British Museum; the supervision of these collections and the direction of the large staff entrusted with the care of them are sufficient to tax the whole energies of any one entrusted with those duties. For the purpose of facilitating this task and avoiding possible friction, it seems to us necessary that the Directors should meet the Trustees and represent them before Her Majesty's Treasury as the responsible head of a department, and not as a subordinate.

A position such as we have described was held, to the great satisfaction of the scientific world, by Sir William Flower, who succeeded Sir Richard Owen; to abolish it now would involve a great change of policy. We believe that the existing system has given satisfaction to the staff of the Museum and to