I LATELY discussed Murray's theory of coral formation with a class of boys and girls (fourteen to sixteen years of age), and they raised two questions which I am unable to answer. (1) If sea water dissolves the coral near the surface at such a rate as to form a lagoon, why does it not dissolve the limestone foundation even more rapidly? (2) After a reef has progressed a considerable distance from the shore, and a channel of open water is formed between, why should not the reef extend back again shorewards? How could such a channel as exists between Australia and its Great Barrier Reef ever have been kept open? These seem to be valid and serious objections: will some expert be kind enough to answer them? CHARLES R. DRYER.

Fort Wayne, Indiana, U.S.A., April 16.

Density and Specific Gravity.

THE point raised by Mr. Cumming in last week's NATURE (vol. xxxvii. p. 584), as to the use of the words density and specific gravity is, it seems to me, of some importance. For many years past I have, in my lectures, taken the law into my own hands in this matter, and, defining density as the mass of unit volume, I have defined specific gravity, in the way Mr. Cumming suggests in the last paragraph of his letter, as the weight of unit volume (or rather, lest I should cause any to offend against the examiner, I have thus defined *absolute* specific gravity, or specific gravity proper, and have pointed out that the definition commonly given was the definition of *relative* specific gravity). We thus get the parallel relations—

$$M = \rho V \text{ and } W = sV,$$

also

$$W = gM \text{ and } s = g\rho.$$

Thus regarded, specific gravity is to density just what weight is to mass. When force is expressed in absolute units of any kind, specific gravity and density must of course have different numerical values, just as weight and mass have. But in the very large number of cases in which weights are the only forces that have to be considered, and in which it is not needful to take account of the small changes of weight dependent on changes of geographical position, the local weight of the unit of mass may be conveniently taken as the practical unit of force—that is, we may take $g = 1$. In all such cases we have, *numerically*, weight = mass, and specific gravity = density, though the idea of weight is essentially different from that of mass, and the idea of specific gravity from that of density.

Of course, as Mr. Cumming points out, when specific gravity is defined as weight of unit volume, its numerical value for a given substance depends on what is taken as unit of weight and what as unit of volume. With the weight of 1 pound avoirdupois and the cubic foot as units, the specific gravity of water becomes 62.5, and that of platinum 1312.5, instead of 1 and 21 as given in the ordinary tables of (relative) specific gravities. If, on the other hand, we take as unit of weight the weight of unit volume of the standard substance, as is done when weights are expressed in grammes and volumes in cubic centimetres, or weights in kilogrammes and volumes in litres, absolute specific gravities and relative specific gravities become equal, and the ordinary specific gravity tables can be used for practical purposes, which is *one* of the great advantages to be gained by using the metrical system of weights and measures. With any other system, the numbers given in the tables require to be multiplied by the specific gravity of water—that is, they must be translated into absolute specific gravities—before they are of use for almost any real calculation, such as occurs either in experimental physics or in engineering practice. For instance, we weigh a measured length of copper wire and want to know its diameter, or we weigh the quantity of mercury that fills a glass bulb of which we require the capacity, or that fills a measured length of a tube of which we require the bore; or an engineer compares his pressure-gauge against a mercury-manometer in order to convert its indications into pounds-weight per square inch; or he has to calculate the pressure exerted by a brick wall so many feet high, or the weight of a mass of rock of so many cubic feet. In all these cases it is the absolute specific gravity that comes into account; it is no use to tell us that copper is 8.9 times as heavy as water, and mercury 13.6 times as heavy, unless we are told how heavy the unit volume of water itself is.

I maintain, in short, that the weight of unit volume of a substance is a quantity of very great practical importance, for which specific gravity is a very suitable name, whereas the ratio usually defined as specific gravity is of little or no use outside

examination questions, and that if it needs a name it should be called relative density.

Further, my experience is that the definition here advocated presents considerable advantages from the point of view of systematic teaching. G. CAREY FOSTER.

University College, London, April 21.

JE crois que la notion de *specific gravity* donnée par M. Cumming dans NATURE du 19 avril (vol. xxxvii. p. 584) est de nature à puzzler les étudiants plus encore que la *vraie* définition physique de la densité.

La densité d'un corps est le rapport de sa masse à son volume—

$$\rho = \frac{M}{V}.$$

Dans le système C.G.S. la densité doit donc être exprimée en grammes masse par centimètre cube (voy. Everett, "Units and Physical Constants"). Le poids spécifique est le rapport du poids d'un corps à son volume et devrait être exprimé, dans le système C.G.S. en dynes par centimètre cube. Mais il y aurait alors le grave inconvénient pratique à cette définition *rigoureuse* que le poids spécifique varierait avec g , accélération due à la pesanteur, tandis que la densité resterait constante.

La confusion provient de ce que le mot *weight*, comme le mot poids en français, s'applique indistinctement à la masse d'un corps en grammes-masse et à la force qu'exerce la pesanteur sur le corps exprimée en grammes.

La solution logique est de supprimer le mot *poids* du langage, à cause de son double sens, et de ne parler que de la *masse* ou de la *force* exercée par la pesanteur, suivant que l'un ou l'autre facteur intervient dans les calculs.

En tout cas, exprimer le poids spécifique en livres ou en grammes est aussi absurde que d'exprimer les vitesses en mètres, et la puissance (*power*) d'une machine en ergs ou en foot-pounds. Le respect de l'homogénéité des formules est la condition essentielle des définitions des quantités physiques, et cette homogénéité n'est pas respectée dans la définition donnée par M. Cumming. E. HOSPITALIER.

Paris, le 23 avril.

The Ignition of Platinum in Different Gases.

AN abstract appeared a few weeks ago in NATURE relating to the "Occlusion of Gases by Platinum and their Expulsion by Ignition," which induces me to mention some curious results obtained by Mr. Lowndes and myself by the ignition of platinum in different gases. We were led to the experiments by another investigation on the behaviour of carbon at high temperatures in various gases. We find that when a platinum wire is heated to nearly melting by a current in an atmosphere of chlorine, the walls of the glass vessel become covered with a yellow deposit, which is insoluble in water, but dissolves in hydrochloric acid, and then, after addition of a little nitric acid, gives all the reactions of platinic chloride. The yellow deposit is in fact platinous chloride. At the same time the thick part of the platinum wire conveying the current, and which was not heated very highly, became incrustated with very fine long crystals of platinum. Some of these were more than the sixteenth of an inch in length, and apparently considerably more were located on that end of the thick wire leading to the negative pole than on the other.

There was also a very decided but lambent flame playing around the ignited and part of the cooler wire during the passage of the current. The arrangement used was a wide-necked flask, stopped with a glass bulb, through which a delivery-tube for the chlorine, and the two No. 12 platinum wires leading the current, passed. The ignited parts of the wire are little coils of No. 24 wire separated by a 1-inch piece of No. 12. On heating the flask externally up to the softening of the glass, the appearance of a flame around the wire increased slightly.

On repeating the experiment with bromine, very nearly the same effects were observed. The amount of platinous bromide was much less than in the case of the chloride, but the flame appearance was very much more pronounced. On passing chlorine into the bromine, so as to form chloride of bromine, both the flame appearance and the action on the platinum were largely increased. With iodine in the flask, vaporized by heating externally, little chemical action on the platinum was observed, only the slightest deposit being formed of a platinum-iodine compound on the glass; but, on passing chlorine into this also, a still more vigorous action on the metal took place, the deposit containing only chlorine and platinum. The flame

appearance filled the entire flask. The spectrum of these flames shows no lines in any case. They are all continuous. The largest crystals of platinum were obtained with the ICl_3 . Bromide of iodine behaved like iodine.

We have tried a number of other substances in a similar manner. Oxygen, sulphur, sulphur dioxide, nitric oxide, mercury vapour gave negative results as far as we could see. With hydrochloric acid some PtCl_2 was formed, but no flame appearance. Phosphoric chloride gave a slight flame, and some PtCl_2 ; but phosphorus is liberated, and then unites with the platinum, melting it. A current of very dry hydrogen fluoride was passed through the flask; before the wire was ignited no action on the glass of the flask was apparent, but almost immediately on passing the current the glass became much corroded by, probably, liberated fluorine. Owing to the flask breaking, we cannot say if platinous fluoride was formed.

With silicon fluoride a singular action took place, the wire, especially the negative half, becoming covered with long semi-transparent crystals of, we think, silicon. The silicon fluoride was very dry, and passed for a long time through the flask without any action until the wire was ignited, when simultaneously with the production of these crystals the glass vessel became much corroded. A small quantity of a soluble platinum salt was formed at the same time. We are continuing these experiments.

We do not think the platinum salts formed in this way are simply shot out by "volcanic" action, as they are quite uniformly spread over the sides of the glass vessel, and seem to be really volatile at the temperature and under the conditions. We have failed to find any record of platinum salts being volatile when heated under ordinary conditions, but it is probable that in the presence of free halogen they would be volatile.

Whether there be any true electrolytic action in these cases we are not at the moment prepared to say.
Royal Military Academy. W. R. HODGKINSON.

"The Nervous System and the Mind."

WILL you allow me to account for one or two of the discrepancies in my book which your very able reviewer points out in the current issue of NATURE?

He cannot reconcile the statement that "everyone nowadays admits that the evolution of mind and the evolution of the nervous system have proceeded *pari passu*, and are indeed but two aspects of the same process," with the further statement that "this way of studying them is so greatly neglected, is indeed derided and scouted." It is pointed out, however, in the passage from which he quotes, that the latter charge is laid at the door of my brother alienists only; while the former statement applies to psychologists at large.

Were it worth while, I could substantiate my charge by chapter and verse, but as the general movement is at last beginning in the direction I advocate, to do so would be to cause the cry from the wilderness to approximate too much to the character of the voice of chanticleer.

Your reviewer states, as if in contravention of my doctrine, that "experienced alienists tell us they find it necessary to admit a moral insanity with an average amount of intelligence." This I have never denied. My position is not that in "moral insanity" intelligence is deficient in amount. What I say is, that in "moral insanity" intelligence is always *disordered*. Disorder of intelligence is very different from deficiency of intelligence.
CHAS. MERCIER.

Canford, S.E., April 23.

I AM glad that Dr. Mercier has found so little to complain of in the review of his recent work. I am bound to accept his explanation of the discrepancy I ventured to point out, although, on re-reading the two apparently antagonistic passages again, I do not find the distinction between psychologists and alienists, to which he now refers, clearly stated. The expression "everyone" (p. 4) appears to include both. Dr. Mercier's "brother alienists" are, it seems, excluded from the class that can grasp the truth that the evolution of mind and the nervous system are but two aspects of the same process, and belong to that uninformed class that "deride and scout" it. I certainly should have hesitated to understand this to be the author's meaning, but, being so, I must leave his benighted *confrères* to settle their account with him. They may perchance think that in this reading of the passage, "the voice of chanticleer" has already become associated with the *vox clamantis* in the wilderness!

In regard to the association of moral insanity with an average amount of intellect, I would only observe that the brother alienists of Dr. Mercier, including Dr. Maudsley, contend that, not only may this be met with, but that moral insanity may co-exist with an undisturbed intelligence. Dr. Mercier's contention that "in moral insanity intelligence is always disordered" would therefore be still in conflict with the experience of some experienced alienists, which was the position I took.

Both these points, however, are only small matters compared with the general subject-matter of the work under review, and I repeat that it is gratifying to find there does not appear to have been any important mis-statement of Dr. Mercier's views in the friendly criticism of
THE REVIEWER.

April 24.

Nose-Blackening as Preventive of Snow-Blindness.

MY friend Mr. Edmund J. Power sends me the following account of what appears to me to be an interesting fact. I should like to obtain suggestions from physiologists as to the possible explanation of the phenomenon, on the assumption that the blackening of the nose and eyelids really does prevent the injurious action of sunlight on the eyes; and further, I should like to know whether (quite apart from the fact of its utility or futility) the custom has possibly a remote origin in some ceremony or ritual.
E. RAY LANKESTER.

"Can you or some of your friends explain the following?"

"When in Colorado shooting the end of last year, my friend had a very bad attack of snow-blindness, caused by a long march on snow with bright sun. My eyes also were very bad the next day and caused much pain.

"Some days after I was under similar circumstances, when my guide stopped, and taking some burnt wood from a stump blackened his nose and under the eyes well down on the cheek-bone.

"On asking him the reason, he told me it stopped snow-blindness, and as the glare was very strong I did the same, and found immediate relief.

"I did this all the time I was out, and never found the snow affect my eyes in any way.

"Everyone I spoke to about it could give no reason for it, but all used it on the march. Some use glasses, but, as my man remarked, 'glasses cost dollars, dirt nothing.'

"Perhaps some of your friends can enlarge on the subject, as it is of great interest to me, and may be so to Alpine people, as glasses are hot to climb in, and from my own experience it is not easy to stalk in glasses and then take them off and shoot."

"Antagonism."

THE author of "The Correlation of the Physical Forces" has, I am sure, our sympathy when he relates how he has been forestalled by Prof. Huxley.

As Sir William Grove subsequently says that "it is always useful to know the truth," he will, perhaps, excuse my suggesting that his views upon antagonism as pervading the universe have been anticipated in a work published more than a quarter of a century ago. I allude to "First Principles," and more especially to the chapter in it upon "The Rhythm of Motion," in which the effects of antagonist forces are shown to be everywhere present, and are copiously illustrated and expounded from the stand-points of astronomy, geology, biology, psychology, and sociology. After reading this chapter, and especially its concluding sentence—"Given the co-existence everywhere of antagonist forces, a postulate which, as we have seen, is necessitated by the form of our experience"—we cannot, I think, but add another eminent name to that of Prof. Huxley as anticipating Sir W. Grove: it is that of Mr. Herbert Spencer.
F. HOWARD COLLINS.

Churchfield, Edgbaston, April 29.

Sense of Taste.

THE curious difference between male and female observers in detecting feeble traces of quinine, sugar, acid, &c., in water as mentioned in NATURE on p. 557 (vol. xxxvii.), is possibly owing to the sense of taste being injured in the males by the use of tobacco.

I have had occasion to apply delicate tests of smell and taste, and I find that even moderate smokers are unable to detect odours and tastes that are quite distinct to non-smokers.

Dunstable.

W. G. S.