secondly, because we almost always close part of the slit of the spectroscope with a shutter, so that the image is cut off sharply by the shadow of the shutter. Strong lines extend into the shadow more or less, and if there is a real reversal the extension of the reversed part into the shadow is trumpet shaped, whereas if it is only a pseudo-reversal it is closed."

I beg to be allowed to call attention to one or two points in the above quotation which I imagine may lead to a misconception of the phenomena observed, and of my remarks thereon.

First, as regards over-exposure, it is assumed that solarisation is an equivalent for this expression. This is the case only when speaking of the cause, but the word has been used by photographers for many years to describe the effect of over-exposure.

In all collodion processes, wet or dry, this effect is an undue intensity of the high lights and an overpowering of the intermediate tints and delicate shadows adjoining them. This appears to be due to the fact that from the intensity of the light not only the direct rays, but those reflected from the back of the glass plate, or even those which are scattered, have sufficient power to act upon the sensitive film. In photographs of spectra this is seen in the nimbus or halo surrounding the strongest metallic lines, which disguises their form. It is well illustrated by my photographs of the magnesium, cadmium, and other spectra, published in the *Journal of the Chemical Society*, vol. xli., *Transactions*, 1882, p. 90.

Although I have worked with dry plates of almost every description, and with some modifications prepared by myself which have never been described, I do not recollect having observed that over-exposure causes any other effect than a too dense deposit of silver, excepting when the vehicle for the sensitive salt is a film of gelatine. As far as my experience goes, it is a property peculiar to gelatine plates, that with such extreme facility they are incapable of development after too strong an action of light, and I carefully avoided the term solarisation, since it has been used to describe an effect so different from that to which I desired to call attention.

Secondly, with regard to difficulty in distinguishing reversals, the sentence above does not exactly represent my experience, and I think it may be seen by those who read my communication, that any want of distinction between real and pseudo-reversals had reference only to photographs which had been already taken with a fixed period of exposure, and that I advocated a method of comparative exposures as necessary in the study of spectra. It appears that this is one of the means whereby Professors Liveing and Dewar are able to draw distinctions between real and pseudo-reversals. The second method, namely, the use of a shutter, is extremely useful in observations on arc spectra, which have been so completely studied by them. I have been studying spark-spectra exclusively, and have not been giving special attention to reversals, in fact, endeavouring as far as possible to avoid them. The use of a shutter does not commend itself to me, since it would cut off a highly characteristic feature in spark-spectra which it is desirable to observe, namely, the extension of the lines, but I may here mention that a speck of dust on the slit, or a fine wire stretched across it, will answer the same purpose as a shutter, without obscuring any considerable portion of the spark, and may be conveniently employed. And now permit me to add one word: the same alteration in the intensity of the spark which results in real reversals also frequently causes pseudo-reversals. Sometimes simply a turn of the screw attached to the spring of the contact-breaker on the induction-coil is sufficient to effect this change.

W. N. HARTLEY

Royal College of Science, Dublin, May 18

The Northern Zoogeographical Regions

THE facts of zoogeography are so involved, and often apparently contradictory, that a skilful dialectician with the requisite knowledge can make a plausible argument for antithetical postulates. Prof. Heilprin, being a skilful dialectician and well informed, has submitted a pretty argument in favour of the union of the North American or "Nearctic" and Eurasiatic or "Palearctic" regions (*Proc. Acad. Nat. Sc. Phila.*, 1882, pp. 316-334, and *NATURE*, vol. xxvii. p. 606), but Mr. Wallace has, with perfect justice it seems to me, objected to his proposition (*NATURE*, vol. xxvii. pp. 482, 483). As Prof. Heilprin's arguments have not been entirely met, however, permit me to submit some further objections to his views.

Prof. Heilprin has contended "(1) that by family, generic,

and specific characters, as far as the Mammalia are concerned, the Nearctic and Palearctic faunas taken collectively are more clearly defined from any or all of the other regions than either the Nearctic or Palearctic taken individually; and (2) that by the community of family, generic, and specific characters the Nearctic region is indisputably united to the Palearctic, of which it forms a lateral extension."

Prof. Heilprin has formulated these conclusions after a summary of the families and genera common and peculiar to the regions in question.

As to families, Prof. Heilprin has presented the following figures:—

	All.	Peculiar.
Nearctic	26	1
Palearctic	36	0
Oriental	36	3
Australian	22	8
Ethiopian	44	9
Neotropical	31	8

The proportions of peculiar genera to the entire Mammalian faunas of the several regions are stated to be as follows:—

	All.	Peculiar.	Percentage.
Nearctic	74	26	35
Palearctic	100	35	35
Oriental	118	54	46
Australian	70	45	64
Ethiopian	142	90	63
Neotropical	131	103	78

The question may naturally recur why the line which separates "regions" from "subregions" should be drawn between 35 and 46 per cent, rather than between 46 and 63 or 64 per cent., or even between 64 and 78 per cent. Prof. Heilprin has not told us why, and I am unable to appreciate the reason therefor. Surely it is not sufficient to answer by simply asking the questions put in *NATURE* (p. 606).

But an analysis of more (but only approximately) correct figures and a more logical classification of mammals than that adopted by Prof. Heilprin reveal facts materially contravening the tabular statements of that gentleman.

First we must exclude the marine mammals, because their distribution and limitation are determined by other factors than those which regulate the terrestrial ones. A consideration then of the terrestrial forms leads to the following results:—

The Arctamerican or Nearctic region has 27 families, of which 11 are not shared with Eurasia and 4 are peculiar; it has 68 genera, of which 45 do not enter into Eurasia.

The Eurasiatic or Palearctic region has 32¹ families, of which 17 are excluded from North America, and it possesses 89¹ genera, of which 60 have failed to become developed in America.

Such contrasts will more than compare generally with those existing between Eurasia and India, and even between the "Triarctic" or "Holarctic" and Indian "regions," and the same destructive process by which the northern regions are abrogated would entail the absorption of the Indian as well into a heterogeneous whole. The three can in fact be well united (as Cænogaea), and contrasted with a group (Eogaea) consisting of the African, South American, and Australian regions, as I long ago urged (*Ann. and Mag. Nat. Hist.* (4), xv. 251-255, 1875), but the claims of each to be considered as "regions" or realms is not thereby affected.

THEO. GILL

Smithsonian Institution, Washington, May 12

Deductive Biology

THOUGH no writer has yet afforded any remarks in criticism of Prof. Thiselton Dyer's "word of warning" to biologists, given in *NATURE*, vol. xxvii. p. 554, it does not, I think, follow that the objection raised by him is to be accepted as unanswerable. As no one of authority in such matters appears to be forthcoming, perhaps one who can lay no claim to being heard may still be permitted to venture to doubt the validity of the objection as given forth in such emphatic terms by Prof. Dyer, and to point out that most, if not all, of the scientific conclusions of importance, especially those accomplished during the present generation, have been arrived at mainly by means of the deductive method.

¹ These are the groups admitted by Prof. Heilprin, exclusive of the Pinnipeds.

The objection started is a serious one, for, if the deductive method is wrong at all, it is so absolutely, and must on no occasion be allowed a place in scientific reasoning, but—without any half-measure allowances—must be excluded altogether as a false and dangerous element of philosophy. If, on the other hand, we take exception, as I think we may do, to the exponent's opening expression,—"having formulated a few fundamental assumptions, to spin out from these explanations of what we see in the world about us . . . is merely a literary performance,"—as misleading in its main idea, we may still hold the method to be a perfectly scientific one.

The evolutionist, who has once ascertained by various careful experiments and extensive researches that there is a direct natural sequence of events in connection with certain phenomena, may be allowed to adopt set principles as recognised laws of action, fully as much as Euclid in the demonstration of problems from his formulated axioms. But perhaps Prof. Dyer's argument rests in reality upon his use of the word "assumptions," and thus his objection is merely urged against the false method assumed in his premise, rather than against deductive biology as a method of procedure, as he would have us believe. And so far, of course, every one will readily enough accept Prof. Dyer's remark as it stands.

But the conclusion that "the deductive method is a bad way of solving morphological problems" is opposed to all the evidence of Darwin himself, who constantly applied those well-tested principles which he had discovered even by this very method, and upon the bases of such fundamental truths it was that he reared his wonderful system. Are not the studies of comparative embryology and osteology, of comparative histology and biology, each founded entirely upon the method of deductive analogy?

As another sufficient witness, Mr. Wallace may be quoted as having adopted the same course with such remarkable results, and throughout his writings bears testimony to the value of deductive inference as a method of procedure, and I will deduce a couple of sentences taken from his work on "Island Life," bearing directly upon our point.

"On the theory of evolution," he says, "nothing can be more certain than that groups now broken up and detached were once continuous, and the fragmentary groups and isolated forms are but the relics of once widespread types which have been preserved in a few localities where physical conditions were especially favourable, or where organic competition was less severe. The true explanation of all such remote geographical affinities is that they date back to a time when the ancestral group of which they are the common descendants had a wider or a different distribution," &c., p. 296. And, in summary of the chapters on Madagascar, Mr. Wallace remarks: "The method we have followed in these investigations is to accept the results of geological and palæontological science, and the ascertained fact as to the powers of dispersal of the various animal groups; to take full account of the laws of evolution as affecting distribution, and of the various ocean depths as implying recent or remote union of islands with their adjacent continents; and the result is that wherever we possess a sufficient knowledge of various classes of evidence we find it possible to give a connected and intelligible explanation of all the most striking peculiarities of the organic world" ("Island Life," p. 419). We may then assuredly decide that the deductive system of logic,—the use without abuse of certain known factors,—instead of being in any way "bad," is (granting always that the general laws of Nature applied are sufficiently trustworthy) found to be even superior to the older and tardier processes of induction, to which the mere collectors of the facts dealt with have limited themselves, and has proved itself to be the *only* means of elucidating many of those abstruse problems, the solution of which has been conducive of such immense gain to the scientific philosophy of our day.

WILLIAM WHITE

Science and Art

As a rule it would be the extreme of absurdity for me to venture an adverse remark on the criticism of an art critic on paintings, yet there is one single exception regarding which I may perhaps be permitted to say a word or two.

In the very interesting critique on some pictures in the Royal Academy, written evidently by a master-hand, there is one picture—No. 764, "a snowstorm" (see NATURE, vol. xxviii. p. 76), not only somewhat severely, but I think unjustly or

incorrectly, commented upon, because "there is not a single snowflake to be seen in the first twenty yards."

I have witnessed and been in the midst of many snowstorms in America, and some in Scotland. In a large proportion of these not a snowflake was to be seen, the snow being in very minute particles, so fine as to penetrate all openings in the clothing, however small. Snowstorms of this kind are the most dismal, bitter, and chilling of any.

On looking at Mr. Farquharson's picture, I was struck with its resemblance to a most unpleasant evening and night spent in the hills between the Coppermine River and Great Bear Lake, about 50 miles north of the Arctic circle. It being early winter, the weather was not very cold, but there was a combination of fog, fine snow (no snowflakes), and snowdrifts, which produced one of the most dismal and dreary scenes imaginable.

4, Addison Gardens, May 29

JOHN RAE

Transit Instrument

IN your issue of the 17th ult. (vol. xxviii. p. 51) you notice a cheap form of transit instrument introduced by me, and you point out the defect that no means are provided for placing the cross wires truly vertical. In all the most recent instruments which have been made this difficulty has been met by an arrangement which answers so effectively that I think it may interest others beyond those who are likely to use the instrument in question.

I employ the ordinary diaphragm with the usual four stretching screws, and the collimation is corrected in the usual manner by these screws. Into this diaphragm I insert a tube with a very fine screw, on the outside of which is fixed a plate carrying the cross hairs; by screwing this tube in or out the focus may be perfectly adjusted for objects at an infinite distance, while a slight additional movement to the right or left enables one at the same time to adjust the cross hairs truly vertical.

LATIMER CLARK

Sea-Shore Alluvion, Dungeness

REVERTING to an article in your journal of July 28, 1881, and a letter of mine in that for April 20, 1882, the following at the present time may be of interest:—

As regards the local wasting away in the bays east and west of Dungeness and the redistribution of the materials at Lighthouse Point;—on the west side the whole margin from Rye Bay to "Denge Marsh Gut," a distance of eight miles, has receded of late years; this is shown by the fact that the Denge Marsh authorities have recently erected a clay counter sea-wall at the back of the modern shingle "full," in front of the "Midrips" and "Wicks" (small land-locked pools of water) to check the overflow of the sea in south-west gales. This action is felt to the eastward in front of the "Holmstone," the Lydd coast-guard station, and up to "Denge Marsh Gut," eastward of which we have modern "fulls,"—the resultant in part of this waste, overlapping and adding to the south-east outline of the "Ness" or extreme projection of this natural mole of shingle, and thence travelling northward until reaching "Great Stone End," which forms the southern boundary of Romney "Hoy" or Bay, northward of which Dymchurch Wall, an artificial stone-faced sea-wall three to four miles in length, is sufficient evidence of the modern local waste and necessity for sea-defences to the rich grazing district of Romney Marsh. Still further northward the sea-wall recently constructed by the municipal authorities of Hythe is equally suggestive of this recession. Going no further back than Cole's survey of 1617, we have a *status quo ante* very nearly, as regards outline from Rye Bay to within two miles of the lighthouse, and this accompanied by a local south-east increase and movement around the lighthouse,