Mr. Morgan's physiological definitions of reflex action, instinct, and intelligence. If we want such a definition it must be made independently of any zoological classification, *and with exclusive reference to the point whether the adaptive action requires for its performance the operation of the higher nerve-centres*—a point which can only be determined by vivisectional experiment. In other words, on the side of objective psychology the only distinction that can be drawn between a reflex and an instinctive action, is as to whether the action can be performed by the lower nerve-centres alone, or requires likewise the cooperation of the higher nerve-centres. And this is just what we should expect to find to be the case on the objective side if, as I have endeavoured to show, the one peculiarity which distinguishes actions classed as reflex from actions classed as instinctive, consists in the latter exhibiting in their performance a mental or conscious element which is not exhibited in the former.

Now, if the *raison d'être* of the term "instinct" is thus to denominate a class of adaptive actions in which there is a subjective, or rather let us say an ejective element, I cannot see that anything but confusion is to be gained by forcing this term into objective implications. Were any term needed to designate the neurosis of instinctive action, it would be far better to coin a new one than thus to abuse an old one. I am fully sensible of the difficulty which often arises in deciding whether a particular action should be assigned to the instinctive or to the reflex class; but, as I observe in "Mental Evolution in Animals," "this difficulty does not affect the validity of the classification any more, for instance, than the difficulty of deciding whether Limulus should be classified with the crabs or with the scorpions affects the validity of the classification which marks off the group Crustacea from the group Arachnida."

For the rest, Mr. Morgan's criticism on my psychological definition of in tinct hangs entirely upon his previous criticism as to the possibility of a science of comparative psychology, and as I have already endeavoured to answer the latter, I need not go over the same ground again by answering the former. There are only two points raised by his paper to which this general answer does not apply, and with these, therefore, I shall conclude.

The first of these two points is a charge of inconsistency. My critic observes that, after having said "it is enough to point to the variable or incalculable character of mental adjustments as distinguished from the constant and foreseeable character of reflex adjustments," I go on to define instinctive actions as mental adjustments which are nevertheless of a constant and foreseeable character. Now I think, if any one will read my chapter on "The Criterion of Mind," he will see that this apparent inconsistency is not a real one. It would be a real one if the passage above quoted referred only to this and that particular action of an animal, apart from all the other actions of the same animal, which, according to my criterion of mind, are competent to inform us whether or not the animal in question is a *choosing* and *perceiving* animal. But the passage quoted refers to the whole constitution of an animal so far as we can know it by observation of activities, and therefore the question whether this or that particular activity is to be regarded as mental or non-mental (instinctive or reflex) requires to be answered by all that we learn concerning the other activities of that animal. If none of its activities are other than those of a constant and foreseeable character, we have no reason to suppose that it is a choosing or perceiving animal; but if some of its other activities are indicative of choice and perception, our knowledge of this fact must be allowed due weight in any attempt that we may make at classifying this or that particular action as reflex or instinctive. The case, in short, is just the converse of that which I thus state in the chapter referred to:—"Many adjustive actions which we recognise as mental are, nevertheless, seen beforehand to be, under the given circumstances, inevitable; but analysis would show that this is only the case when we have in view agents whom we already, or from independent evidence, regard as mental."

The second point to which I have referred as the only one that now remains for me to consider, is to the effect that I have mistaken "Mr. Spencer's position with regard to the 'very subordinate importance of natural selection as an evolving source of instinct,' and with regard to the question of 'lapsed intelligence.'" Here I can afford to be brief, inasmuch as any one who cares to do so can compare my interpretation of Mr. Spencer's writings with the passages in those writings to which I refer. It seems to me perfectly clear that, although both the principles in question are alluded to by Mr. Spencer, neither of them holds the same prominence in his theory of the development of instincts from reflex action as they hold in the theory of Mr. Darwin.

In conclusion, I trust Mr. Morgan may feel that, in writing this somewhat elaborate reply to his criticism, I am marking as emphatically as I can my sense of its ability. And if the general effect of this discussion is to show that the phenomena of instinct present peculiar difficulties to any attempt at a fundamental analysis, I should like no less emphatically to express my conviction that such an analysis is not to be facilitated by closing our eyes upon the entire class of phenomena to which alone the word is applicable. We may, of course, abstain from any attempt at such analysis, and devote our attention exclusively to the physical as distinguished from the mental side of the subject. Only in this case we may not speak of *instinct*.

George J. Romanes

"Mental Evolution in Animals"

Mr. Romanes' comment on my communication in Nature of February 7 (p. 335) is not quite satisfactory. I do not suppose that he has any spite against my skate; but as he does not know me, and did not see the incident in the Manchester Aquarium, I think it is very possible that he may have been naturally predisposed to underrate the significance of the story. I do not admit that I can be reasonably blamed for saying that a repetition of the conditions would have been useful, if possible, while at the same time pointing out that the result would not necessarily have settled the question. Test experiments are always useful, even if they do not settle the main question. Mr. Romanes' terrier story was not necessary to make clear what he means by "accident," and there is no analogy between it and my skate story. In one case a trained, or at least tamed, dog did as he was told, and the conditions of success were prearranged; in the other, a fish spontaneously did something for his own advantage. As for the fish smelling the food, this does not harmonise with the circumstances as I described them, and had Mr. Romanes seen the incident I do not think this explanation would have occurred to him; the whole series of actions was too rapid, and had too much the appearance of co-ordination. The propulsion of the food into the ready mouth was the work of an instant. Had the mouth not been ready, as the cricketer's bat is the instant the ball leaves the bowler's hand, the morsel would have been missed. Finally, Mr. Romanes tells us ("Animal Intelligence," p. 351) that the bear observed by Mr. Hutchinson was a Polar bear. Now this species is "almost marine in its habits." It lives upon seal-flesh and also upon dead meat which it finds floating in the water. It is not infrequently cast adrift on an ice-floe or an iceberg. It is therefore not at at all improbable that the method of fishing described may be an instinct developed hereditarily. The fact that two bears behaved in precisely the same manner strengthens this supposition. Mr. Darwin does not say whether the bear observed by Mr. Westropp in Vienna was a Polar bear or not, but he observes that the action in question "can hardly be attributed to instinct or inherited habit," as it would be "of little use to such an animal in a state of nature." It seems to me that such action would be very useful to Polar bears in a state of nature.

Manchester, February 11 F. J. Faraday

The Remarkable Sunsets

At the present stage of the discussion upon the "green sun" and rosy sunsets it seems to me it would be well to recall attention to a few facts, for there seems to be a tendency on the part of some correspondents to allow imagination to carry them beyond the region of fact into that of fancy. First, then, I would point out that my observations show conclusively that at the time of the green sun there was an altogether abnormal amount of moisture in the upper regions of the atmosphere, while the ordinary hygrometric observations showed the air near the ground to be comparatively dry. I have studied the rain-band spectrum almost daily for the last six or seven years, and I have never before known such a long continuance of the heavy rain-band in a comparatively clear sky—a sky in which there was only a light haze. At sunset and sunrise the intensity of the bands was such as I have before seen only from an altitude of some six or seven thousand feet, and even then rarely. In this connection it may be well to point out that the spectrum as observed by Mr. Donnelly (Nature, vol. xxix. p. 132), though, as remarked by Mr. Lockyer, resembling that observed here in