those who were interested in the matter would be able to form their own opinion as to the value of the arguments adduced on either side of the question. I very much regret to find, however, that Dr. Romanes—whose amount of spare time appears to be most enviably inexhaustible—still finds it necessary to prolong the correspondence. I am compelled, therefore, to enter the field once more, if only for the purpose of presenting my own case in its true light. What Dr. Romanes's position may now be I must confess is becoming distinctly less clear with each of his contributions to the subject, but I am not the first who has lost his way in attempting to thread the mazes of this writer's productions. As far as I am concerned it will suffice to say that the case is not "simply" as he presents it in the foregoing communication. In the review of Mr. Pascoe's book, from which this discussion originated, I did not merely reproduce "Mr. Wallace's argument against Mr. Spencer's defence of 'use-inheritance.'" I accepted that argument as valid, but I extended it by emphasizing the importance of the factor of superimposed useful characters accumulated during successive periods of the phylogeny. I pointed out that large numbers of cases of co-adaptation might be thus accounted for, and I used Mr. Spencer's own illustration by way of example. In summing up his own conclusion, Mr. Wallace says: "The difficulty as to co-adaptation of parts by variation and natural selection appears to me, therefore, to be a wholly imaginary difficulty which has no place whatever in the operations of Nature" ("Darwinism," p. 418). Not only, therefore, has Dr. Romanes misrepresented my view, but he has gone further. The other "argument on the same side" referred to in the above communication is this very denial of co-adaptation as a fact in Nature. This, with most amazing sangfroid, is now claimed by my correspondent, who speaks of it as "the one which I had stated"! I must leave it to others to decide what value can be attached to the statements of a writer who adopts the principle of appropriating an argument, and putting it forward in a manner which would lead most readers to consider that he had been the first to elaborate it simply because he has expressed the same idea in abstract symbols instead of in concrete terms.

The next phase in the discussion is the admission by Dr. Romanes that Mr. Wallace's conclusion is correct, *i.e.* that co-adaptation is non-existent: "As it appears to me, from his reply, that Prof. Meldola's views on the subject of 'co-adaptation' are really the same as my own, I write once more in order to point out the identity" (NATURE, vol. xliii. p. 582). Mr. Romanes did more, therefore, than simply point out that we were agreed that this was "the only argument which could be properly brought against Mr. Spencer's position." He said that our views were "really the same," and this after I had accepted Mr. Wallace's conclusion as to the non-existence of co-adaptation. To crown all, he now tells us that he has no fully-formed opinion to express, but that he is in a condition of "suspended judgment"! I must really leave the case as it stands. If "neo-Darwinians" have a language of their own, at any rate it appears to be intelligible among themselves, if only from the circumstance that they have been enabled to stereotype a phrase which conveys their views with respect to the difficulty of following my correspondent's reasoning. I have been no more fortunate than other "neo-Darwinians" in this attempt, but in the endeavour to carry on the discussion of a biological question with a writer who stops short as soon as the subject assumes a truly biological aspect (see NATURE, vol. xliii. p. 582), I have become keenly impressed with the utter sterility of Dr. Romanes's method, which not only fails to advance our knowledge of the origin of species by any substantial contribution of fact, but which degrades the theoretical side of the subject into mere verbiage. If this is "palæo-Darwinism," I am rejoiced to think that I am grouped with those who are outside the pale.

In conclusion, to prevent further misunderstanding, let me add that, in admitting that the chances are "infinity to one" against a number of independent *useful* variations occurring when required in the same individual, I merely quoted the expression as given by Mr. Herbert Spencer and repeated by Dr. Romanes. I do not for a moment suppose that Mr. Spencer used the words in any more than a colloquial sense as indicating that there were "heavy odds" against such a combination, and in this sense only is my admission made. That the phrase has no exact mathematical significance is, I imagine, sufficiently obvious, but I have thought it desirable to make this qualification.

R. MELDOLA.

Physiological Selection and the Different Meanings given to the Term "Infertility."

IN the discussion concerning the segregation of varieties occupying the same region, and the influence of physiological selection in securing this result, it is necessary that we consider the different meanings given to some of the terms by different writers. The general fact on which Dr. Romanes insisted, in his paper on "Physiological Selection," was compatibility in the reproductive system of some, and incompatibility in that of others belonging to the same species. On p. 360 of his paper we read that "racial incompatibility," "however produced," "is the primary condition required for the development of varieties into species." Infertility and sterility are also used by him as equivalents for incompatibility in the reproductive system. Thus, on p. 400 we find the statement that "All natural varieties which have not been otherwise prevented from intercrossing, and which have been allowed to survive long enough to develop any differences worth mentioning, are now found to be protected from intercrossing by the bar of sterility—that is, by a previous change in the reproductive system of the kind which my theory requires."

Dr. Romanes did not attempt to catalogue the different forms of discriminative incompatibility that are included in the incompatibilities of the reproductive systems of different races, but reference was made to three forms: (1) to compatibility in the time of flowering in those of the same race, as contrasted with incompatibility in those of different races, as on pp. 352 and 356; (2) to greater numerical fertility when the male and female elements of the same race unite, than when those of different races unite, as in the note on p. 354; and (3) to numerical infertility through deficient production by hybrids, as on p. 369, and p. 357 in the note, and in the suggested experiments on p. 405, in which the pure and hybrid seed are both to be sown, and the comparative "degrees of fertility" to be noted. To these forms which were mentioned, we may add, as coming under the category of physiological incompatibilities, (4) lack of vigour in hybrids; (5) lack of adaptation in hybrids; (6) lack of escape from competition with kindred in hybrids; and (7) the superior energy and promptness with which the male and female elements unite in pure unions, as contrasted with cross unions. Dr. Romanes probably refers to this principle when he speaks of sterility as "failure to blend" (p. 365).

This last, when associated with the free distribution of the fertilizing elements, ensures the segregation (that is, the discriminative isolation) of two or more varieties occupying the same area and propagating during the same season, and therefore seems to me the most important of the forms of physiological segregation. This segregative principle, which I call potential or prepotential segregation, must, in almost every case, be operative between species and varieties that continue distinct while indiscriminately mingled on the same area and while fertilized by elements freely and indiscriminately distributed during the same season, for no other principle is able to secure free propagation and at the same time to prevent crossing under such conditions. Seasonal segregation is here excluded, and the other forms of physiological segregation when acting under such conditions are of little avail in preventing swamping unless carried to the extreme, and they then involve a waste of from one-half to the whole of the germs of the less numerous variety; for the most favourable case possible is when two varieties occupy the area in equal numbers, and such cases rarely exist, especially in the initial history of species.

Though numerical infertility and tardy potency are readily distinguished, complete impotence and complete numerical sterility are more likely to be confounded; for the complete incapacity of the male and female elements of different varieties for uniting involves failure to produce hybrids, as complete as when the elements unite without producing living offspring or germinating seed. The great difference is that in the case of cross impotence the germ remains unaffected by the alien fertilizing element, and therefore ready to be fertilized by any fertilizing element of its own kind that may reach it; while in the case of simple numerical cross sterility (if there be any such case) the alien elements promptly unite, and therefore leave no opportunity for subsequent fertilization by the coming of the kindred fertilizing elements. Cross impotence, with prepotence of pure unions when associated with the free distribution of the fertilizing elements, produces positive segregation; for, when characterizing varieties occupying the same area, it ensures the

propagation of each with its own kind while preventing crossing ; but numerical infertility of first crosses produces what I call negative segregation, for, though it is unable to secure segregate breeding, it lessens or obliterates the swamping effects of any crossing that takes place, and is therefore of great importance in the preservation of distinct varieties and species when the positive forms of segregation only partially prevent crossing. The four forms of hybrid inferiority mentioned above are also forms of negative segregation, and, though of the highest importance when co-operating with prepotential segregation or any other principle that partially prevents cross unions, are, it seems to me, incapable of preserving distinct varieties or species, when unassisted by any degree of positive segregation.

We are now prepared to see how the different meanings of infertility have occasioned more or less misunderstanding in the discussion of physiological selection and its effects. With Dr. Romanes, the seven forms of segregation above-mentioned are all forms of infertility between races, and therefore are all causes of physiological selection ; while in my nomenclature, all but the first are included under impregnational segregation, and only the second and third are considered forms of cross infertility (or, in other words, of segregate fecundity). Using the term in this restricted sense, I have elsewhere maintained that it is very improbable that cross infertility is, in any case, the only isolative principle securing the continuance of distinct varieties and species indiscriminately commingled on the same area, even when the elements are freely distributed ; and as this statement is liable to be taken as equally applicable to physiological selection, I wish to have it clearly understood that, in my usage, the two terms are not equivalent, and what I have said of cross infertility is not in the same sense true of physiological selection.

In Dr. Wallace's criticism of physiological selection, he seems to limit the meaning of infertility between races to numerical infertility of first crosses, and then assumes that this is the only incompatibility that is included under physiological selection. This limitation, if correct, would of course limit the effects that could properly be attributed to this principle.

Before closing I wish to raise the question whether a high degree of selective numerical fertility between races is not always associated with some degree of selective potential fertility. Or, using infertility in the more restricted meaning given in my nomenclature, is not a high degree of segregate fecundity and cross infertility always associated with some degree of segregate prepotence and cross impotence ? As we know that these two forms of incompatibility are usually, if not always, associated in the segregation of species, is it not probable that they are similarly associated in the segregation of varieties ? Again, as we know that segregate prepotence, when associated with the free distribution of the fertilizing elements, will produce prepotential segregation, effectually preventing crossing, without impairing powers of survival, and as there are many cases in which the continued segregation of varieties occupying the same area is due entirely to this principle, and many other cases in which it is due to weakened forms of this principle associated with other forms of incompatibility in the reproductive system, and still other numerous cases in which partial isolation (produced by a slight diversity of habits, or by the occupation of adjoining districts) would be speedily broken down except for these physiological incompatibilities, are we not fully warranted in the assertion that physiological selection is an essential factor in the evolution of many species ?

The importance of this form of segregation having been recognized, the question naturally arises as to what have been the causes through which the incompatibility has ceased to be sporadic, and has become racial. As Dr. Romanes has not entered on the discussion of this point, I have given the more attention to it. I think I have succeeded in showing : (1) that any portion of a species subject to temporary isolation, through occupying a new station or district, is more or less liable to become incompatible with the rest of the species, owing to the cessation of reflex selection, by which the mutual fertility and other compatibilities of an inter-generating stock are kept in force (see NATURE, vol. xlii. pp. 28 and 369) ; (2) that partially segregate endowments are, through the very laws of propagation, cumulative (see "Divergent Evolution," Linn. Soc. Journ.—Zool., vol. xx. pp. 246-250) ; (3) that all the transformations that arise in forms thus segregated are inevitably divergent, and not parallel (see "Intensive Segregation," Linn. Soc. Journ.—Zool., vol. xxiii. pp. 312-322).

JOHN T. GULICK.

26 Concession, Osaka, Japan.

Propulsion of Silk by Spiders.

THE author ("O. P. C.") of the article on "Arachnida" in the "Encyclopædia Britannica," says :—"The emission of silk matter appears to be a voluntary act on the part of the spider ; but it is a disputed question among arachnologists whether spiders have the power forcibly to expel it, or whether it is merely drawn from the spinnerets by some external force or other. Mr. Blackwall, author of the 'History of Spiders in Great Britain and Ireland,' is of the latter opinion. Mr. R. H. Meade (Yorkshire) in Report of the British Association, 1858, thinks, that (from microscopic, anatomical investigations which he has himself made) there is good evidence of spiders having the power to expel it ; for he finds a certain muscular arrangement which would apparently suffice to give this power, and observers have actually seen the lines propelled."

Owing to the doubt herein expressed, may I ask your insertion of a chance observation lately made by me upon a spider, which has convinced me of the truth of the theory that spiders do expel their lines at will, and this, too, as secondary to one still remaining attached to the spinnerets ?

She was hanging from the ceiling about 3 feet from a mullioned window, against which I was able to observe her movements most accurately. I was first led to observe her closely, by finding myself attached to her within one minute of my approach. On my breaking this line, she attempted to regain the ceiling ; a breath of air from me stopped and brought her down again, when I saw her draw her legs together, pull her head up higher than the spinnerets of her abdomen by means of her ceiling-line, and, following upon no visible effort of hers whatever, I was the next moment conscious of the presence of another line stretching out from her spinners to a distance short of 3 feet, and at an angle of about 75° with the first. This line failing to find an attachment floated upwards and lay alongside of the other, and the spider again made for the ceiling. Nine times during the space of one hour, I got her to repeat this attempt to make a horizontal connection. Between two of the intervals of her attempts, I called in two naturalist friends who both witnessed with me, and at the same instant of time, the sudden appearance of the new line.

With each successive trial, I was able to substantiate and improve my observation ; at first the appearance of the line seemed instantaneous, as to its whole length ; next I was able to detect its elongation of itself after about 2 feet of its length was visible ; then I could see it leaving the spinnerets ; and finally, during the last moment of its travel, I could perceive very distinctly that it drew the spider slightly forward.

From these premises I can but infer that the viscid matter contained by the silk-glands, which, at the ordinary slow rate of emission, turns to gossamer immediately upon its exposure to the air, when expelled as now, violently, remains viscid sufficiently long to reach a certain distance.

These secondary threads, carried towards the ceiling by the spider, were never brought down again when she fell to the length of the main line, but were each time left, disconnected from her, at the spot from whence she fell when I blew her. Their loose end invariably floating upwards until alongside of the spider's main line, was, I think, noteworthy.

In conclusion, I would say that sight seemed to play no part in her choice of a direction for the connecting line ; though I was close to her all the time, and indeed the only object apparently which was close enough, she only hit me the first time, when perhaps she had heard my approach ; this may strengthen the remarks made by Mr. C. V. Boys in your number for November 13, 1890, where he says : "... sight, as we understand the term, in spite of their numerous eyes, seems to be absent." S. J.

St. Beuno's College, St. Asaph, N.W., April 27.

The Crowing of the Jungle Cock.

I THINK there can be little doubt that Mr. H. O. Forbes has fallen into the same mistake as I had, in regard to Mr. Bartlett's statement that "none of the known wild species are ever heard to utter the fine loud crow of our domestic cock."

At first I took this to mean that the jungle cock did not crow at all, and was collecting notes from sporting men here, to supplement my own 26 years' experience, when yours of February 5 arrived, and by it I see that Mr. Bartlett implies that the crow is not so full, loud, and long, as that of our barn-door cock.