a calcareous soil, viz., one that will 'effervesce with acids' (requiring the presence of at least three per cent. of carbonate), goes far beyond what insures the presence of calciphile plants in thousands of cases. I have elsewhere summed up what may be said on this point, to the effect that while in heavy clay lands as much as six tenths per cent. of lime in the soil may be necessary to secure the advantages of calcareous lands, in the case of light sandy soils one tenth per cent. may be sufficient to produce natural calciphile growth, and, therefore, also the cultures which, like the legumes, demand soils which are not only neutral, but which shall be able to supply to them freely the lime which forms so prominent an ash ingredient.

In this, the proper sense of the word, calcareous soils will be found to exist not only in limestone districts, but in all derived from hornblendic rocks, including black lavas and basalts, and also from the rocks containing either labradorite or some of the soda-lime feldspars. Such soils rarely effervesce, but when wetted they show with red litmus paper, at the end of twenty minutes, the blue reaction which is wholly independent of 'alkali.' Even dilute acetic acid will in that case readily dissolve from the soil enough lime to give a plain reaction with oxalates.

I trust that this point of view may be made the subject of verification by Mr. Wilder as well as others. E. W. HILGARD.

BERKELEY, CAL.,
December 8, 1905.

ISOLATION AS ONE OF THE FACTORS IN EVOLUTION.

IT was with much pleasure that I read the article of President D. S. Jordan on 'Isolation' in a recent number of SCIENCE,[1] and, aside from the fact that I am able to add a large number of cases, I have nothing to comment upon. But the subsequent article by Professor J. A. Allen[2] demonstrates again that the principle of isolation or separation is not generally understood in its full meaning.

Jordan expresses the opinion that isolation is a factor in the formation of every species on the face of the earth. I can not strongly enough endorse this view, for it is absolutely unthinkable that two species may be derived from one ancestral species without the action of isolation. All the instances introduced by Allen as opposed to this view are rather in support of it. He concludes that in variations of certain widely distributed species, which pass into each other from one extremity of the range to the other, no isolation by barriers exists, but that there is continuous distribution. Indeed, there is continuous *distribution,* but there is no continuity of *bionomic conditions.* These different bionomic conditions pass into each other, and, consequently, we have varieties, and not species. This is clearly the first step toward complete isolation, and for complete isolation 'barriers' in most cases are not absolutely necessary features.

It is not quite correct to conceive isolation only in its coarsest sense, as topographic or climatic separation. This mistake is often made, but I pointed out, about ten years ago, that the real and most important value of the principle of separation lies in its general *bionomic* sense. The same idea was maintained long ago by Gulick, and has been treated recently by him in an elaborate monograph.[3] I am fully in accord with most of Gulick's ideas as to the influence of separation upon the formation of species, chiefly as opposed to the senseless abuse of the term species introduced by the de Vries school. 'Bionomic separation,' as used by myself, and 'habitudinal segregation,' as used by Gulick, are practically identical terms.

With Jordan (and with Gulick) I believe that 'bionomic separation' is absolutely necessary for the formation of species, but that it is not the only factor taking part in the process called 'evolution.' With regard to this, I may be permitted to quote from a paper published by myself in 1896,[4] which seems to have been overlooked generally:

> * * * We have to distinguish *four factors* accomplishing the diversity, development and differentiation into species of organic beings: we

[1] SCIENCE, November 3, 1905, p. 545 ff.

[2] SCIENCE, November 24, 1905, p. 661 ff.

[3] Gulick, J. T., 'Evolution, Racial and Habitudinal,' Carnegie Institution, Washington, 1905.

[4] 'On Natural Selection and Separation,' *Pr. Amer. Philos. Soc.*, 35, 1896, pp. 175–197, especially pp. 188–190.

may call conveniently this whole process: *origin of species*.

I then proceed to characterize these four factors, which are the following: (1) variation; (2) inheritance of variations, 'consanguinity becomes morphologically visible'; (3) natural selection, acting upon the material produced by variation and inheritance, improving the average, and causing, under certain circumstances, 'mutation';[5] (4) 'bionomic separation' (p. 190, *l. c.*), forming what we call 'species.'

> The four factors named, *variation*, *inheritance*, *selection* and *separation*, must work together in order to obtain different species; * * * it is impossible to think that one of them should work by itself, or that one could be left aside.

I have further demonstrated in the paper referred to, that Darwin already held practically the identical opinion, although he did not properly recognize 'bionomic separation,' and introduced, in its place, the 'principle of divergence.' In the face of this fact, it is only to be regretted that bionomic separation or habitudinal segregation has not received due attention, and is generally not understood in its true meaning by those that have little experience in field work; indeed, it is impossible to get an appropriate idea of it in the museum or the laboratory, and also the botanical garden is entirely unfit to bring home its significance. I hope, however, that its real value and real meaning will become more generally known by and by. For those that have no chance to convince themselves in nature of the ever-presence of bionomic separation, the study of Gulick's book will be advantageous.

E. A. ORTMANN.

PITTSBURG, PA.

SPECIAL ARTICLES.

REACTIONS IN SOLUTIONS AS A SOURCE OF E.M.F.

PERMIT me to call to the attention of the readers of this journal certain observations which I have recently made relative to the chemical reactions in solution as a source of the electric current. So far as I am informed the phenomenon described below has not previously been recorded.

Some time ago, while carrying on a series of experiments upon photo-electric effects, certain features of the investigation led me to suspect that *any* and *all chemical reactions* give rise to a *measurable* quantity of electrical energy.

In order to test this I introduced into a very small glass vessel two platinum wires, No. 26, to serve as electrodes. These electrodes were as nearly identical in dimensions as it was possible to make them. They extended down into the cell about two centimeters, at a distance apart of, perhaps, two millimeters. The cell thus constructed held approximately 3 c.c. The electrodes were then connected by means of a short wire to a sensitive galvanometer.

About 2 c.c. of silver nitrate solution (5:25) were introduced into the cell. Two or three drops of concentrated HCl were then added to the silver solution in the cell. Immediately when the acid came in contact with the salt a decided deflection was manifest on the instrument. Stirring the reacting bodies increased the deflection and at times reversed the direction of the current. The maximum deflection was about twenty-five scale divisions.

At first the acid was introduced *between* the platinum electrodes. Later it was found that if the reagent was allowed to come in contact with the silver solution *about* either *one* of the electrodes the direction of the resulting current, as indicated by the galvanometer, could be predicted, *i. e.*, the current *in all cases* left the cell by that electrode about which the reaction was taking place *least* vigorously.

Different concentrations of the salt and acid were tried. It was found that the deflection of the needle was roughly proportional to the concentration of the reacting bodies. It was also observed that the current ceased when the reaction was complete, which, when the solution was not stirred, took at times a minute or more.

Other combinations were tried as follows: NaCl and H_2SO_4; BaCl and H_2SO_4; $CuSO_4$ and NH_4OH; KOH and HCl. Each of the above reactions gave rise to a decided deflection of the needle, the current continuing

[5] Not the 'mutation' of de Vries, which term is decidedly ill chosen, being preoccupied long ago by Waagen, Neumayr and W. B. Scott, and used in an entirely different sense.