small quantities of cottonseed meal the nitrate nitrogen may begin to accumulate again after ten days' incubation. With larger quantities the time is longer. This fact has probably caused many to overlook the first disappearance.

Just what becomes of the nitrate nitrogen, under such conditions, has not been determined. There are two possibilities. It may be liberated in the elementary form through the process of denitrification. This seems improbable since the soil in the writer's experiments has never been much over an inch in depth and never more than two thirds saturated, hence aeration was good. Another possibility is that it may be assimilated. There is always a very copious growth of soil fungi of various forms when cottonseed meal is applied. In fact, so abundant are the mycelial threads that they bind the soil together in a mass which is rather difficult to disintegrate by the ordinary shaking method. It is highly probable that at least a portion of the nitrate nitrogen is assimilated by this growth. It would seem that it would be very easy to determine the above question, but the volatilization of ammonia where large amounts are being formed, as is always the case in the presence of cottonseed meal, makes it difficult.

It is evident, from what has just been said, that deducting the nitrate nitrogen originally present or that in an incubated check, will not give us correct results under the conditions mentioned above. The more nitrate nitrogen initially present, the less reliable will be our results. These two methods must then be abandoned. Simply taking as the correct factor the amount found at the final analysis will probably approach nearer the truth than any other method now in practise. However, we have no assurance that this gives us an accurate idea of the relative amount formed in different soils. There is absolutely no way of determining the actual amount formed that immediately disappears. We only know that in this method the actual amount present at one time (unless very large amounts were initially present) was zero, and that all formed at any future date must have been formed during the course of the experiment.

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ON THE APPARENT ABSENCE OF APOGAMY IN ŒNOTHERA

IN a previous note in SCIENCE[1] I described certain experiments which suggested that *Œnothera* was occasionally apogamous. Three imperfect seeds were obtained from a castrated flower of *O.* mut. *lata,* suggesting the possibility that a small percentage of the seeds might develop apogamously. Last year (1912) the experiments were carried out more extensively, however, and the results were wholly negative, showing that if apogamy occurs in *O.* mut. *lata* it must be very rare indeed.

Six *lata* plants were experimented upon, whose history was as follows: one was a mutant appearing in a culture of a race of *O. Lamarckiana* from the Kolosvar Botanical Garden; two were derived from *lata* self-pollinated in the cultures of de Vries; two were mutants occurring in a *rubinervis*-like race obtained from Heribert-Nilsson in Sweden; and one was *O. biennis* mut. *lata* appearing in a race of *O. biennis* from the Madrid Botanical Garden. On these plants over 20 flowers were castrated and covered with bags during the height of the blooming season, from July 22 to August 13, 1912. In addition, a whole branch of one pollen-sterile *lata* plant, containing 20 flowers, was covered with a large bag. No growth of the capsules took place in any case, and not a single seed could be found in any of the capsules. It would appear, therefore, that under these circumstances at least, apogamous development practically never occurs in *lata,* for the number of ovules in the capsules observed must have numbered several thousands. The plants were, moreover, all well nourished.

Similar experiments made with eight flowers belonging to four plants of *O.* mut. *gigas* also

[1] Gates, R. R., "Apogamy in *Œnothera*," SCIENCE, N. S. 30: 691–694, 1909.

gave wholly negative results, not a single seed developing. This race of *gigas* came from an independent source, having originated, apparently by a mutation, in the Palermo Botanical Garden.[2] Theoretically, it might have been anticipated that the eggs of these plants, having already the diploid number of chromosomes (14) would be most likely to show a tendency to apogamy, but so far as the evidence goes this does not appear to be the case. The unbalanced chromosome number (15) in *lata* might on the other hand perhaps be expected to predispose megaspores or egg cells of *lata* having this number of chromosomes (assuming that such occur) instead of the reduced number, to degeneration.

Nevertheless, from the observation by Geerts of a megaspore mother cell of *Lamarckiana* containing 28 chromosomes, and from the discovery, by Miss Lutz[3] and Stomps,[4] of triploid mutants containing 21 chromosomes, it seems quite certain that eggs containing 14 chromosomes must occasionally be formed in *Lamarckiana*. Why such cells might not develop embryos occasionally, even if unfertilized, is not clear, and perhaps an extensive series of experiments with *Lamarckiana* and some of its 14-chromosome derivatives may yet show that this type of apogamy sometimes occurs.

In the light of these facts it appears desirable that the experiments of Mrs. Rose Haig-Thomas[5] on apogamy in *O. biennis* be repeated before the results are accepted as facts.

I may add that I have very recently observed a case of parthenocarpy in a race of *O. muricata* L. from eastern Canada grown in my cultures this season. This culture contained only four plants, all alike. Earlier in the season normal capsules were produced filled with seeds, but the later capsules (observed November 19) though full size and with normally developed walls, were hollow, containing undeveloped ovules instead of seeds. It seems possible that the drop in temperature at the end of the season may have been sufficient to kill the young embryos shortly after the eggs were fertilized, while the capsule walls, already stimulated by the process of fertilization, continued their development. However, nothing of the kind was observed in any other race.

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[2] Gates, R. R., "Tetraploid Mutants and Chromosome Mechanisms," *Biol. Centlbl.*, 33: 92–99, 113–150, Figs. 7, 1913.

[3] Lutz, Anne M., "Triploid Mutants in *Œnothera*," *Biol. Centlbl.*, 32: 385–435, Figs. 7, 1912.

[4] Stomps, Theo. J., "Mutation bei *Œnothera biennis* L.," *Biol. Centlbl.*, 32: 521–535, Pl. 1, Fig. 1, 1912.

[5] Haig-Thomas, Mrs. Rose, "Note sur la parthénogénèse chez les plantes," *Comptes Rendus*, IV^e^ Conf. Intern. de Génétique, Paris, 1913, p. 209.

SOCIETIES AND ACADEMIES

THE AMERICAN PHILOSOPHICAL SOCIETY

On November 7, before the American Philosophical Society, a paper was read by Professor J. M. Macfarlane on "The Phylogeny of Plants in Relation to their Environment." Recalling the conclusions already published in the centennial volume of the Academy of Natural Sciences, the speaker showed that, of the simplest non-nucleate plants the great majority seem to have had a thermal-water or fresh-water origin. Of the simpler nucleate plants, such as the Desmids, the Protococcoid, Pleurococcoid, Chætophoroid, Cladophoroid and related groups, all or the preponderating number were fresh-water. Even the simplest brown algæ like *Lithoderma*, *Pleurocladia* and *Heribaudiella* were now in part or wholly fresh-water, as were the simplest groups of the red algæ. An estimation of the genera and species of algæ and fungi now living revealed that 3,008 genera and about 28,660 species were fresh-water or land forms, 658 genera and 5,930 species were marine. Interesting data were advanced as to transition or brackish species. The phylogenetic origin of bryophytic, pteridophytic and higher classes from certain fresh-water algæ was advocated, while the extreme rarity of any genus of these classes in marine surroundings was emphasized. The speaker, therefore, concluded that marine life was probably derived from a thermal-water and later from a fresh-water source. Though contrary to the whole trend of zoological consideration, he indicated that in an abstract already published he had advocated a like origin in fresh water for animal as for plant life, and a derived distribution of many groups of animals into the sea.