positions of authority to a regrettable carelessness and inaccuracy that beginners in physiological and experimental biological work tend to show, which should be corrected by those who are responsible for their training.

A simple case occurs in a recent paper issued by one of our leading research foundations which refers at the start to "the ovum of the spatangoid sea-urchin, *Clypeaster rosaceus*." Now this is much better than to have said merely "the ovum of Clypeaster," for the writer wished to be definite and let the reader know what kind of an animal he was talking about. But ought not the writer to have known that a clypeaster simply can not be a spatangoid? Is that too much to expect from a worker in the field of physiology?

A much worse case is that of a paper in one of our leading zoological journals which bears the amazing title: "The Effects of Ultraviolet Light on Pond Snails (Linnaeus)." Obviously this is an inexcusable blunder—Linnaeus never named a group "pond snails." Probably the word printed Linnaeus was written "Limnaeus" (= Lymnaea, a genus of freshwater mollusks), but the writers, in reading the proof, failed to note the error. This blunder in the title might be regarded as a joke if it were not characteristic of the paper as a whole. The authors give no clue as to when or where their work was done. There is no hint as to whether more than one species of "pond snail" was used, but the statement is made that "snail embryos seem to be well adapted for this type of experimentation." Very likely—but "snails" and "pond snails" are two very different groups. When one considers that there are hundreds of species of pond snails in the United States (assuming that this piece of work was done in this country—for which assumption there is no evidence), belonging to many genera and several families, is it not absurd to make statements about the effects of ultraviolet light on pond snails and not specify the forms used? Granting that the writers intended to limit their "pond snails" to the genus Lymnaea, as is quite possible, the situation is not much better, for the genus Lymnaea, in its old, wide sense, includes hundreds of species showing a great range of characters. Now, *some one ought to have required these writers first of all to know what animal or animals they were working with.* Without such knowledge, clearly stated at the start, work of this kind has little if any value.

A third case has just come to hand. Here is a paper entitled "A Study of the Genetic Relationships of the 'Amebocytes with Spherules' in Arbacia." The lay reader will naturally want to know what Arbacia is. He gets a hint that it is an echinoid, from references to other publications, but there is no definite statement on the point nor is there any clue as to where or when the observations recorded were made. We are told that the perivisceral fluid of *Arbacia punctatum* was used; this seems definite until we discover that there is no such species of Arbacia known as *punctatum*. There are half a dozen species of Arbacia recognized and twenty or more names have been given to them—but there is no *punctatum*. However, Lamarck did give the name *punctulatum* to a species which is found at Woods Hole, Mass., so that most readers will guess that this piece of work was done at Woods Hole and that *Arbacia punctulata* was used. But is there really any excuse for publishing the name as *punctatum*? Is it unfair to think that so doing indicates carelessness, or indifference to accuracy or both? In my opinion the paper should have had the title: "A Study of the 'Amebocytes with Spherules' in a Sea-Urchin." In the first paragraph should have been a statement to the effect that the work was based on material of the sea-urchin, *Arbacia punctulata* (Lam'k.), obtained at Woods Hole, Mass., in the summer of 1925 (or whenever it may have been). Such a statement forms a definite foundation on which the work may rest; without it every statement made is open to doubt.

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SOME NEW LECTURE DEMONSTRATIONS IN GYROSCOPIC MOTION

IF an ordinary gyroscopic top is equipped with hooks at each end of the axis in the form of wire loops, the top when rotating will walk down an incline made of two rods (Fig. 1).

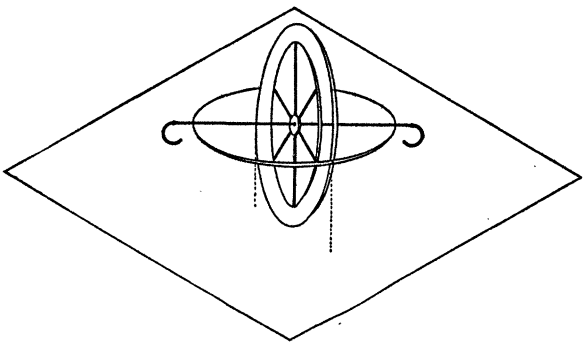

FIG. 1. This top will walk down two parallel inclined rods which pass through the hooks.

The top starts to slide down the wires, but friction retards one end more than the other—this causes the top to precess, the free end rises and moves forward. After this end has moved forward it strikes the rod on

which it hangs, stopping its precession so that this end falls. Then the other end precesses and so on alternately with a motion that simulates walking or going hand over hand down the two rods.

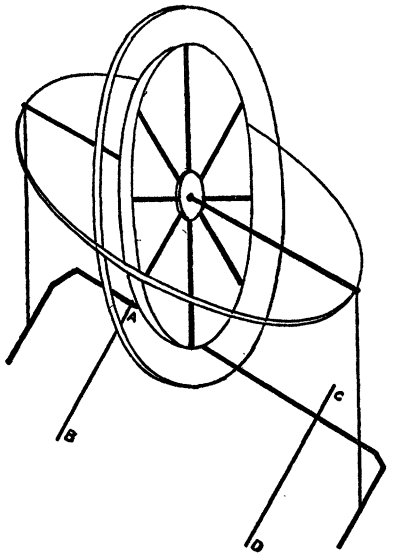

FIG. 2. This top when rotating will walk down an inclined plane. There must be two wires parallel to the board shown by AB and CD.

If the top is provided with feet it will waddle down an inclined plane. It is necessary, however, to have two wires (AB and CD of Fig. 2) at a height of an inch or so above the inclined plane and parallel to it. The wires stop and start the precession.

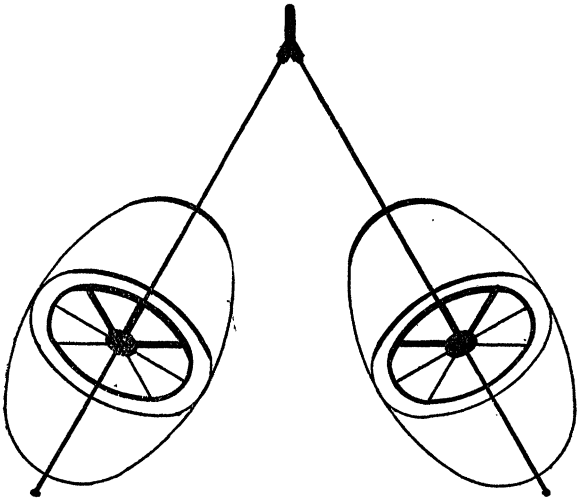

FIG. 3. A double top to show precession.

Two gyroscopic tops hinged to the end of a metal rod form a useful device for illustrating the action of precession. If both tops are spinning in the same sense, they will rise when the rod is spun clockwise (say) and fall when the rod is spun in an anti-clockwise rotation. If, however, the tops are spinning in opposite directions and the rod rotated, one top rises while the other falls.

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THE PRECIPITIN REACTION AS A MEANS OF DETERMINING THE CONGENIALITY OF GRAFTS

RIVES[1] has suggested the possibility of using serodiagnostic methods for predicting the "affinity" between stock and scion in the grape. He reports success with this method for a number of common grape stocks. Green[2] found that by using extracts of seeds differences between families could be demonstrated, but not differences between species of the same genus. With this in mind, a test involving two plums, Beauty and Santa Rosa (*Prunus salicina*), and the almond (*Prunus communis*) was carried out. The former plum makes an unsuccessful union with the almond, while the latter makes a good union. Since the vegetative portion of the plant is the part involved in grafting, we followed Rives in using one-year-old shoots as a source of material, but followed Green's procedure for the most part.

Sap was extracted by pressure of about 6,000 pounds per square inch, a special press made from a concrete tester being used. After filtration the extract was injected, at first in the dilution of 2 to 1, later without dilution. Three pairs of rabbits were used, one for each extract. The schedule called for injection at four-day intervals for a period of six weeks. A total of 80 cc extract (on the basis of undiluted sap) was injected into each rabbit. None of the six showed any infection or other disorder save loss in weight toward the close of the experiment. Each extract was tested with each immune serum at the end of the period and with normal serum at the beginning and the end of the experiment.

It was found that although a distinct reaction was obtained, showing a precipitate in dilution of 1 to 1,600, there was no differentiation between them. This confirms Green's data on the lack of differentiation in closely related species, but does not confirm the conclusion that the method can be used to predict affinity. Green has apparently overlooked the fact that many varieties of the genus Prunus can not be intergrafted, and that the apple and pear (which give an indication of affinity by the precipitin test) rarely make successful unions. It seems to the authors that the uniformity of the reaction in reciprocal tests and in the comparative tests indicates that more

[1] Rives, L., "Sur l'emploi du sero-diagnostic pour la determination de l' 'affinite' au greffage des hybrides de vigne." Prog. Agr. et Vit., 79: 118–119. 1923.

[2] Green, F., "The Precipitin Reaction in Relation to Grafting." *Genetics*, 11 (1): 73–82. 1926.