species. It is true that the somatic divisions of *D. melanogaster* are quite normal, but that in general is true of all somatic divisions of all hybrids, as has been recently emphasized by the present author. This confusion of thought is probably the cause of a highly critical paper published recently in this journal by Huskins.[2] He expresses the view that a contrast in regularity between the somatic and reduction division is of no importance from the standpoint of diagnosis of hybrids. This opinion seems to have remarkably little justification. We are now acquainted with the remarkable meiotic or reduction phenomena in many hybrids, both plants and animals, and an outstanding feature of the maturation metaphase is, in the great majority of cases, the lagging of chromosomes, polypolidy, sterility and extreme variability. It is true there are certain hybrids which are fixed invariables from the first, and it is also true that there are forms of heterozygotes which present little or no sterility. This does not alter the fact, however, that sterility, variability and abnormal cytological phenomena are outstanding features of the reduction divisions in known hybrids.

We are now well acquainted with many large orders occurring in all parts of the world and including practically all groups of the higher plants in which all the criteria of hybridism are presented. Excellent examples of these are the genera Rosa, Rubus, Crataegus (in the northern hemisphere), and Eucalyptus, Acacia and Veronica (in Australasia), which manifest extreme variability, intergradation of species, polypolidy and the cytological phenomena of known hybrids.

It will probably become more and more apparent as a result of the increasing correlation of cytological and experimental work in biology that specific change more than anything else is the result of the crossing of species, in other words, of hybridization. Purely experimental work divorced from morphology seems to have as little future as the proverbial faith without works. The dangers of this way are well illustrated by the deplorable case of Paul Kammerer, whose experiments purporting to prove the production by experimental means of heritable characteristics were demonstrated by unprejudiced investigators to be based on fraud or extreme credulity or both.

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THE DECOMPOSITION OF OZONE IN THE PRESENCE OF NITROGEN PENTOXIDE

THE rate of the catalytic decomposition of ozone in the presence of nitrogen pentoxide has been shown by Schumacher and Sprenger[1] to follow the expression

$$-\frac{d[O_3]}{dt} = k_1[N_2O_5] + k[N_2O_5]^{2/3}\,[O_3]^{2/3}$$

where the first term on the right-hand side of the equation gives the rate at which ozone is used in reoxidizing the nitrogen dioxide formed from the decomposition of nitrogen pentoxide, k_1 being the specific reaction rate constant for the first order decomposition of nitrogen pentoxide. When an appreciable amount of ozone is present the first term is small compared with the second and the expression can be written in the simplified form

$$-\frac{d[O_3]}{dt} = k[N_2O_5]^{2/3}\,[O_3]^{2/3}.$$

Similar results on the rate of this decomposition will be found in a thesis which I presented to the California Institute of Technology in May, 1928. Experiments were carried out at 25° C. and 35° C. The partial pressure of the nitrogen pentoxide was varied in the ratio 1:30. The maximum partial pressure of ozone was forty millimeters mercury. Two pyrex reaction vessels were used, one of 20 cc volume and the other 300 cc volume. The pressure was followed by means of a click gauge, so the gases were in contact with only glass during the decomposition. Most of the experiments were carried out with a large excess of oxygen, the total pressure approximating one atmosphere, but in three experiments the pressure was reduced to one half atmosphere and in one experiment to one quarter atmosphere. All partial pressures were determined from direct pressure measurements, the start of the nitrogen pentoxide decomposition being marked by a "kink" in the pressure increase time curve, and also by the appearance of a brown color. Tank oxygen was used in all experiments.

In my work the order of the reaction was found to be two thirds with respect to the nitrogen pentoxide pressure, and slightly lower than first with respect to the ozone pressure. The ozone pressure was not varied over a sufficiently wide range to determine exactly the order with respect to the ozone.

The data have been fitted to the simplified expression given by Schumacher and Sprenger and constants calculated. These constants were compared with those calculated to 25° C. and 35° C. from the work of the above experimenters. An agreement within 10 per cent. was obtained.

The work was carried out at the suggestion of Professor R. C. Tolman, and the present note is

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[2] SCIENCE, n. s., Vol. 69, No. 1789.

[1] Schumacher and Sprenger, *Zeit. für physikalische Chem.*, 140: 267, 1929.

published as a confirmation of the results presented by Schumacher and Sprenger.

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MECHANICS OF FLUIDS IN RECENT TEXT-BOOKS OF PHYSICS

EVERY few years it becomes necessary for a teacher of physics to choose a new text-book from the large variety offered by the publishers, or at least to look over the list to see if there is a new one which is more suitable than the one he has been using. If he has plenty of time, he may look carefully through many books; if not, he may, as I have done, choose one subject in the books and compare the authors' treatment of that. It has been my habit to study the chapters having to do with the mechanics of fluids, particularly liquids. Why is it that less care seems to be exercised in writing this part of a physics text than any other?

A quite recent and excellent text has this statement in italics: *"The force which a liquid exerts against any surface is equal to the area of the surface times its average depth times the density of the liquid,"* and later makes use of the formulas: $F = Ahd$ and $p = hd$. As the authors of this text define density in the usual way as the mass per unit volume of a substance, pressure can not be equal to height times density. The dimensions will not agree. The pressure at a given point in a liquid depends not only on the depth and the density but also on the pull which the earth exerts on each unit mass of the liquid, that is, on the value of the acceleration of gravity. $P = hdg$. We may write $P = hd$ if we choose to define density as the weight per unit volume as is sometimes done, but in that case we may not write the formula for the velocity of a compressional wave in an elastic medium in the form $V = \sqrt{\frac{E}{d}}$.

Very few text-books make a complete statement of the principle of buoyancy or Archimedes' principle. The usual statement is that a body wholly or partly immersed in a fluid is buoyed up by a vertical force equal to the weight of the fluid displaced. Is it not important to state that we may consider the center of gravity of the fluid which the body displaces as the point of application of the buoyant force? I find that the average student assumes, often incorrectly, that the buoyant force acts at the center of gravity of the immersed body.

The subject of "The Siphon in Text-books" has been very well discussed by Professor Harold C. Barker in SCIENCE.[1] I can add nothing to that except to call attention to the fact that some of the most recent texts discuss the siphon under the subject "Fluids at Rest" and that a very careful reading of some of them will fail to show even why a siphon starts to operate. No effort is made to show why it continues to operate, or to show that stable operation can take place only when the highest part of the tube is less than barometric height above the *intake and outlet.* If we discuss this with the author, perhaps he will say he considers the subject of fluids in motion and Bernoulli's theorem too difficult for beginning students. Perhaps it is, but it would be better to tell what a siphon does and state its limits of operation without explanation than to give an explanation which pretends to tell the story and does not do so.

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PETRIFIED WOOD IN THE NEW ALBANY SHALE

THERE have been frequent references to petrified wood in the black shales of Indiana, Kentucky and Ohio. Silicified tree trunks were reported from the vicinity of Lebanon, Kentucky, at the New Haven meeting of the American Association for the Advancement of Science in 1850. Reports of the state geological survey occasionally mention such wood in Indiana, and in 1914 Elkins and Wieland described an Indiana specimen as *Callixylom Oweni.* Petrified wood has also been found in the concretions of the black shale of Ohio, and several years ago Dawson described one Ohio specimen as *Dadoxylon Newberryi.* Aside from this, very little systematic study of this wood has been made.

Field investigations during the past summer showed that petrified wood is quite common, though widely scattered, throughout parts of these states. It is especially abundant in the New Albany shale of Scott County, Indiana, and probably occurs at numerous other places throughout the state where this shale is exposed.

The thickness of the New Albany shale and the readiness with which it weathers produce a rolling topography which is devoid of striking geologic formations or extensive rock exposures. Consequently the contained silicified wood is rarely found *in situ* but as loose material in stream beds and on gently sloping weathered hillsides. The shale is quarried extensively in southern Indiana for road construction, but in most instances the petrified logs are removed from the quarry as impediments as soon as they are discovered.

One exception, however, is worthy of note. In a quarry in the vicinity of Scottsburg a stump with the lower portion of the trunk attached was found still partly buried in the shale. The specimen was three feet broad near the base, tapered to two feet at the

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[1] 51: 489–491, May 14, 1920.