of J. Spencer Smith, president of the New Jersey Board of Commerce and Navigation, met in Chicago on May 31 and June 1. This was the first national conference on coastal erosion problems and allied subjects held since the association suspended its regular sessions for the duration of the war. The Beach Erosion Board of the Corps of Engineers of the United States Army held a meeting concurrently with the conference of the association.

BY the will of Dr. Oscar M. Stewart, emeritus professor of physics at the University of Missouri, his estate, the estimated value of which is in the neighborhood of $100,000, is bequeathed to the department of physics, "the net income to be used for some educational or scientific purpose connected with the department."

THE *Experiment Station Record* reports that the Inter-American Institute of Agricultural Sciences has now acquired a permanent status through the ratification of the convention for its establishment by eight republics—the United States, Costa Rica, Cuba, Ecuador, Nicaragua, Panama, Honduras and the Dominican Republic. A permanent faculty is being selected, and construction is proceeding in the building program at Turrialba, Costa Rica. A substation for the development of high-yielding and disease-resisting rubber trees has also been opened in Panama.

THE British Mycological Society, according to *Nature,* is making a collection of surplus reprints and pamphlets on mycology and plant diseases for distribution after the war to libraries and centers of research at home and abroad which have suffered loss or damage. Authors are invited to send reprints of their own published work and any other reprints or pamphlets which they can spare to G. C. Ainsworth, secretary of the British Mycological Society, Imperial Mycological Institute, Ferry Lane, Kew, Surrey.

IT is reported in *Nature* that the Royal Society of Edinburgh has been made the residuary legatee of the estate of Robert Cormack, of Edinburgh, who died on August 13, 1942. The society is directed to administer this bequest for the purpose of promoting astronomical knowledge and research in Scotland, so far as practicable, on the lines of a memorandum prepared in 1931 by the late Professor R. A. Sampson, Astronomer Royal for Scotland. The council, on behalf of the Royal Society of Edinburgh, has accepted the bequest, which represents an amount somewhat over £30,000; and, after careful consideration, has approved a general scheme for the administration of the trust, which will include: (*a*) research fellowships to be awarded on suitable conditions to students engaged on research in Scottish observatories and to students or graduates of Scottish universities desirous of engaging in research in foreign or Dominion observatories; (*b*) the publication of the results of such research; (*c*) lectures to be delivered in suitable centers by eminent foreign or Dominion astronomers; (*d*) grants in aid of the purchase of special equipment for use in Scottish universities or observatories in research work; (*e*) lectures and demonstrations of a more popular character under the auspices of the Scottish educational institutions. A large part of this scheme will not be developed during the war.

DISCUSSION

NOTE ON STABILITY OF INCIDENCE OF THE "COMMON COLD"

IN one of the best statistical analyses upon data assembled under good control which has been offered for discussing stability of resistance to the common cold, Gafafer and Doull[1] found that the hypothesis that the colds of their samples were distributed by chance was not improbable. Their fundamental technique was that of showing that in each year with each sample the distribution of the number of colds was according to the point-binomial or law of small numbers. They found that the numbers of colds suffered by the same individuals in different years were correlated to a small extent in a variety of groups of persons. If we should calculate the correlation coefficients for the three pairs of years for the 111 persons under continuous observation for the three years (Table 5, p. 720) we should find $r = .44, .38, .35$, each of which is alone significant. This, as they must have known, was in itself some evidence that the distribution of the colds was not quite by chance, even though the distributions were well fitted by the law of small numbers.

There are indeed some limitations to the use of the law of small numbers as evidence of chance behavior; of these one is the difficulty of determining whether the law is satisfactorily fitted. Consider, for example, the distribution of colds of each of the 111 persons for the whole three years, the numbers varying from 0 to 21. The mean of the distribution is 8.315 and the variance is 13.00, which indicates a hypernormal distribution with too much dispersion about the mean as would be the case if some were resistant and others susceptible to colds. Now $\sigma = 3.61$ as computed from the distribution and $\sigma = 2.88$ as estimated from the

[1] W. M. Gafafer and J. A. Doull, *Amer. Jour. Hygiene,* 18: 712–726, 1933, and SCIENCE, 78: 314–315, 1933.

square root of the mean on the assumption of the law of small numbers are significantly different[2] (diff. = .73 ± .19) if we make a rough estimate throwing all the variability on to the observed value of σ. On the other hand, for the three distributions for each of the three years, we have for the means and variances m = 3.00, $\sigma^2 = 2.68$; m = 2.83, $\sigma^2 = 2.70$; m = 2.49, $\sigma^2 = 1.93$, which indicates that in each year the distribution is subnormal. Even if these three distributions be thrown together despite the inter-year differences of m, the resulting distribution is subnormal, with m = 2.77, $\sigma^2 = 2.48$. Thus there is evidence that the variation is less than that due to chance, as well as evidence that the variation is greater than that due to chance, entirely apart from the evidence of the correlations cited above.

At the time Gafafer and Doull were writing (1933) the analysis of variance had hardly got into the literature.[3] It would appear that this method of analysis might be one very appropriate to the discussion of their exceptionally good data. If we consider that the number of colds n_{ij} of the ith person in the jth year ($i = 1, 2, \ldots, 111$; $j = 1, 2, 3$) consists of one part appropriate to the individual, one appropriate to the year and a residual independent of both, Table 1 may be constructed:

TABLE 1

	Sum of squares	Degrees of freedom	Mean square
(1) Between year means ..	15.2	2	7.60
(2) Between person means .	480.7	110	4.37
(3) Residual fluctuations ..	330.8	220	1.50
Total $S(n_{ij}-n)^2$	826.7	332	...

The values of the mean squares indicate that the variation from year to year ($\sigma_y = 2.8$), poorly established because of the small number of degrees of freedom, is considerably larger than the variation ($\sigma_p = 2.1$) between persons; but as the ratio 7.60 : 4.37 of the variances is not significant, one may not claim the difference as meaningful.

Compared with the residual variance 1.50, both that between years and that between persons is significant, for the former at around the 1 per cent. level and the latter at well below that level. The evidence is therefore corroborative of that offered by the inter-year correlation coefficients to the effect that there is significant evidence that the distribution of colds was not by chance but was affected by an inter-individual variation of resistance to the common cold or by some systematic influence which simulated it, such as a higher exposure rate among some than among others of the persons under observation.

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[2] Although the distribution seems to be very significantly hypernormal, the chi-square test applied to it gives a variety of results from P = .05 to P = .50, according to the manner of grouping, and fails to indicate in any clear manner that the fit is significantly bad.

[3] R. A. Fisher, "Statistical Methods for Research Workers," 1925, and subsequent editions, Chapter VII; G. W. Snedecor, "Calculation and Interpretation of Analysis of Variance and Covariance," 1934; G. U. Yule and M. G. Kendall, "An Introduction to the Theory of Statistics," 1937, pp. 444–449. Reference should also be made to two articles by J. O. Irwin, *Jour. Roy. Stat. Soc.*, London, pt. II, 1931, 284–300, *ibid.*, Supplement, 1: 1934, 236–251.

RESUSCITATION APPARATUS

THE recent controversy between the late Professor Yandell Henderson and Council on Physical Therapy of the American Medical Association contains a most unhappy and serious implication. It would appear, from the matter presented, that the recovery of the asphyxiated patient turns exclusively upon the operation of mechanical gadgets designed to meet and overcome all conditions accompanying the unconscious state. If the patient recovers, he has been saved; if he dies, he was beyond all hope of rescue.

This implication would seem to confirm the layman's everyday observation, the patient with a persistent headache, an abdominal pain or a fracture is a serious medical problem. He demands, and if it is available he receives, the best medical care obtainable. But should unconscious or respiratory failure supervene, his condition is suddenly simplified to a point where the police, the fire or the consolidated gas squad is all the assistance required. If the squad can bring or borrow an inhalator or a resuscitator, everything is perfect. Or—

If a child inhales a peanut or a pin, he is hustled off to the hospital and receives every advantage of the bronchoscopic clinic. Should he be so unfortunate as to inhale something larger, a marble or a chunk of meat, he is no longer in need of medical help. Everything will be solved if the suck and blow apparatus used last week in the stevedore down the street can be secured.

Asphyxia-conscious physicians are well aware of the phenomena which occur between the onset of unconsciousness and the death of the patient. They are prepared to control and to direct these phenomena. Treatment must necessarily vary with the stage of asphyxia treated. The depressed, the spastic and the flaccid patient each requires a different approach. It is frequently fatal to attempt to fit the desperately ill patient into treatment suited to the patient who is merely depressed. It would seem as reasonable and in many cases safer to attempt to employ a mechanical robot for a brain operation, on the basis of electrical potentials, than it is to attempt to subordinate respiratory rate and rhythm in a patient about to die.

Controversy over mechanical gadgets plays directly