while A, E and I are found to have improved strikingly. It is even possible to show that the improvement is statistically significant, for their average before treatment was 2.00 ± 0.38, while after treatment it was 6.33 ± 0.80.

The shuffling of the cards and the subsequent dealing are, of course, analogous to the taking of measurements by means of instruments so poor, or under conditions influenced by so many uncontrolled variables, that the readings are completely determined by chance. But it is evident that the phenomenon must be at work whenever one works with tests whose results have a perceptible degree of chance error. This is the case with many of the tests used in biochemistry, physiology and psychology. This phenomenon must be suspected of being at work whenever one finds that the "supernormals" selected by the same test (subjects F, G and H in the above illustration) are adversely affected by the therapy. This fact, that *both* extremes on reexamination are found to gravitate in the direction of the mean for the whole group, suggests the name "centripetal drift" for the phenomenon.

A remarkable thing about this fallacy is that it can not be avoided by taking more readings on the subjects of the experiment. Thus one may deal the cards *twice*, and average the results:

	A	B	C	D	E	F	G	H	I	J
1st	3	10	2	8	7	4	9	6	5	1
2nd	6	2	10	7	9	1	3	8	4	5
Ave.	4.5	6	6	7.5	8	2.5	6	7	4.5	3

The four lowest are A, F, I and J. After treatment one again deals the cards twice:

	A	B	C	D	E	F	G	H	I	J
1st	8	2	5	6	7	9	3	4	10	1
2nd	4	1	2	6	9	8	5	7	3	10
Ave.	6	1.5	3.5	6	8	8.5	4	5.5	6.5	5.5

The experiment has now been done more carefully than before, and the improvement shown by A, F, I and J is very convincing; their average before treatment was 3.62 ± 0.44, and after treatment it was 6.62 ± 0.82.

One way to avoid the fallacy of the centripetal drift is to compute the index of reliability of the instrument by the method of self-correlations.[2] When this method is applied to the last set of readings above (8 and 4, 2 and 1, 5 and 2, etc.) one obtains an index very near zero; a very reliable instrument gives readings whose index is very near one. Some instruments used clinically give disturbingly low indexes.

Another way to avoid the fallacy is the time-honored device of dividing the subnormal group itself into a treated and an untreated (control) group.

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2 H. Sorenson, "Statistics for Students of Psychology and Education," New York, McGraw-Hill, 1936, p. 339*ff*.

THE CONCEPT OF ATAVISM

DURING the days when biologists were sedulously engaged in supplying the finishing touches to the house that Darwin built, it was the usual practise to regard every possible arrest in development or unusual character of an organism as the persistence or recurrence of an erstwhile normal feature of the particular organism's ancestor or ancestors. Such a reversion to the presumed ancestral condition was termed an *atavism*, from the Latin *atavus*, an ancestor. This concept, and the term expressing it, abounds in the writings of nineteenth century biologists. Haeckel's Biogenetic Law really represents a generalized synoptic version of this concept applied to a particular case, and calculated to resume a certain supposed routine of phenomena under a particular law. To-day few biologists believe that in its ontogenetic development any animal actually repeats the developmental stages of its phylogenetic history. In development the organism apparently passes only through those stages of development which are akin to similar stages passed through in the *ontogenetic* development of its ancestors; and this is essentially what von Baer said in his Hypothesis of Recapitulation, which is not to be confused with the so-called Biogenetic Law. In development the organism does not repeat the *adult* stages of its ancestors, but only those stages of development through which its ancestors as a whole have, more or less, in common passed. This, as von Baer originally pointed out, is why the early stages of related animals resemble one another more closely than do the differentiated adults. Modern recognition of these facts has brought the Biogenetic Law into disrepute.

The conception of atavism, however, persists. Reference to many modern texts on embryology, general biology and the writings of a fair number of morphologists, will supply examples of the uncritical usage of this term. One recent work by a notable worker has a section entitled "Reappearance of Lost Ancestral Structures in Man," and as *the* example of such structures we are given "the gill-pouches . . . [of] the early embryo . . . [which] may . . . persist and form an open fistula on the side of the neck." Surely, it is clear that such a fistula is due to the mal-development or arrest in development of an embryonic character of the individual and not to the reappearance of a character which the species and class has lost but which may have been present in some remote phyletic ancestor. In this connection it is worth drawing attention to the fact that the conventionally accepted homology between the gill-pouches or arches of fishes and the branchial arches of mammals is open to serious question.[1]

1 E. Gaupp, *Ergeb. Anat. u. EntwickGesch.*, Bd. 14, p. 808, 1905; A. C. Bruni, *Arch. Ital. Biol.*, vol. 51, p. 11, 1909; G. R. De Beer, "The Development of the Vertebrate Skull," Oxford, p. 406, 1937.

The occasional occurrence of a "tail" in man or of an azygos lobe of the right lung, microcephaly, large canine teeth, the fourth molar, the divided malar bone, the "third trochanter" of the femur, the entepicondylar foramen of the humerus, supernumerary mammae and many other characters have been, and still are, cited as examples of atavism. Yet in every case it can be conclusively shown that such characters are not upon any view to be regarded as reversions to an ancestral condition. Changes in development and in developmental rates resulting in persistence, suppression, reduction, hypertrophy, duplication or multiplication of structures and normal variability, are processes quite adequate to account for the so-called "atavisms" which are commonly cited.

In short, it is more than doubtful whether the concept of atavism has any counterpart in reality; and, I think it will be agreed, that unless the concept can be applied to some demonstrable type of phenomenon, it were better that the term were altogether dropped from the vocabulary of the biologist.

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BIOLOGICAL ABSTRACTS HAS GONE TO PRESS

Biological Abstracts has been saved! Funds for Volume 12 are pledged in sufficient amount to guarantee publication.

The first number of Volume 12 went to press on April 19, and will consist mostly of abstracts appearing in the last half of 1937, together with a few from 1938. The second number will follow close on the heels of the first, appearing before June 10, and will consist of 1938 material. It is planned that Number 3 will be a supplement, and will bridge the gap between Volumes 11 and 12. Thereafter publication will be prompt. The indices to Volumes 10 and 11 are likewise provided for. Over one half the index of Volume 10 is already through galley proof.

The budget adopted has been set at the lowest figure consistent with this worthy project. Under it some 15,000 abstracts are planned. A list of journals will be published in an early issue. It is planned to extend the scope of *Biological Abstracts* as subscriptions are added. To insure satisfactory coverage, steps are being taken to secure the appointment of advisers from the various biological organizations having a stake in the enterprise.

The Board of Trustees, taking office as an emergency measure in mid-February with ten months of the fiscal year gone, are deeply grateful for the prompt and generous response given their appeals a month later in SCIENCE and the *Library Journal*. They regret the unfortunate features of the plan under which the present funds have been solicited, and pledge their energy to the liquidation of that plan at the earliest possible moment. The new editor-in-chief, Dr. John E. Flynn, is keenly aware of the problems facing *Biological Abstracts*, and is very eager to do his bit in keeping this journal up to the minute and in making it a more useful one.

Now that publication has been resumed, it is hoped that institutions and individuals will forward their orders at once. This is particularly necessary if they wish to avoid interruption of their files, since the edition must be kept reasonably close to the subscription list. That their institutions are saddled with various subsidies is lamentable, but the Board of Trustees promises relief in another year in the case of *Biological Abstracts*.

The present Board of Trustees has been maintained intact during the current emergency. While the membership has been criticized because of too great concentration in certain states, this has been a distinct advantage when personal consultations were needed. Committees have been appointed, however, to study the problem of a more satisfactory geographical distribution, together with other pertinent problems.

Biological Abstracts is *yours*. Its success depends upon an active interest taken by you and the societies of which you are a member during the coming years. If you do this, a creditable journal can be confidently forecast.

GEORGE W. HUNTER, III, *Chairman*
PAUL R. BURKHOLDER
M. L. RANEY
Executive Committee

QUOTATIONS

SCIENCE AND SOCIETY

THE correspondence shows a remarkable consensus of opinion. Practically all are agreed that some organized body is necessary which shall study the problems, many of them highly controversial, evoked by the impact of science on society, in an objective and rational manner. Such a body must have the closest linkings with the physical and biological sciences, with economics, engineering, psychology, anthropology and sociology. It must provide a platform for free and frank debate; it should conduct its investigations as much by means of research committees and discussions