

institution, and this year when an increase was asked, representatives of the state in both its legislative and executive branches charged with the task of preparing the budget for maintaining the state institutions during the ensuing biennium visited the institution, went over with the scientific staff and business manager in considerable particularity the work being prosecuted, and were unequivocally assured that the problems under investigation are all first and foremost scientific, and that only some of them might be expected to have a money value to the state.

Great emphasis was, however, laid by the men of the institution on the two facts that all increase of knowledge of nature is capable of being made useful to the people of the commonwealth in one way and another, either for their enlightenment or pleasure or material gain; and that the institution holds itself under as much obligation to make its discoveries utilizable in some form, as it does to prosecute the investigations themselves.

The attempt has always been made to impress upon officials and public that this institution is one in which private benefaction wishes to join with state benefaction for serving the community through *research in pure science*. And it is pleasant to record that the officers of the state government have been found to be at least not less responsive to the appeals for financial aid than have been the president and regents of the university.

Mr. John F. Neylan, chairman of the state board of control has taken the pains to expressly state that the placing of the institution's item specifically in the allotments to the university, which allotment is in turn a permanent element in the state budget of running expenses, should be understood to mean that the state accepts the institution with its avowed commitment to research as a definite and perpetual charge upon the state. And from Mr. H. W. Wright, chairman of the ways and means committee of the last assembly, comes the declaration: "We recognize that the state must support institutions of this kind."

From what California has done toward

maintaining the Lick Observatory through a considerable term of years, and is now doing for the Scripps Institution, the conclusion seems justified that the state is definitely committed to the principle of state aid to scientific research, even though such research has no direct and primary industrial aims. In discussing these matters with officials, I stoutly contend that in the long run about the most telling criterion of success of popular government will be the extent to which it contributes to the highest development, spiritual and physical, of the naturally best endowed persons who live under and who participate in such government. The facts and reasonings that can be presented in support of this proposition, particularly those touching the question of leadership in scientific discovery, seem to appeal with special force to men grappling earnestly with the practical problems of government for a modern community.

Experience strongly inclines me to the view that the serious dereliction of our national and several state governments in the support of scientific investigation is chargeable quite as much to scientific men themselves as to government officers and the people at large.

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A REPLY TO DR. LITTLE

IF we are ever able to discover what part hybridization plays in evolution, it is immeasurably more valuable to find out the behavior of natural species rather than of forms created in the laboratory under more or less artificial conditions, and which are never found outside the laboratory. This effort to place hybridization among evolutionary causes has been one of the chief aims of students of heredity.

My repetition of the standard cross between grays and albinos to discover the behavior of coat color in mice was carried on with wild housemice and not with artificial laboratory grays.

It is still open to question whether the wild housemouse (*Mus musculus*) inevitably fur-

nishes actual "homozygotes" which will stand every test of the theoretical "homozygote." Tower's work with chrysomelid beetles gives him pure-breeding species which behave as homozygotes in one hybrid cross with other pure breeding species, and as heterozygotes in other such crosses; and this order controlled environmental conditions never equalled in experiments with mice.

Moreover, the assumption of the exact similarity of every first generation hybrid in a given cross with respect to a given "unit character" leaves no place in nature for variation in any "unit character." Variation therefore would become wholly a matter of environment.

The divergence from the accepted canon of my results of color transmission in crossing wild housemice with laboratory albinos, involves a difference of data which in no way affects the question of the transmission of cancer.

Moreover, it is increasingly difficult to know the established canon in the behavior of characters in heredity. Exceptions to what was the canon have become so numerous as to be a part of the rule; and Riddle's work on melanin formation makes it particularly dangerous to be dogmatic on the transmission of such pigmentation in heredity, particularly in mammalian species where pigment is melanin.

My attack upon the problem of the inheritability of cancer was made almost with the sole end of solving the practical questions as to its inheritability and its nature, in order that we might get light upon the methods for its prevention and its cure, since these facts are so desperately needed.

In the face of the tremendous difficulties which the study of these things involve, it is essential that all minor considerations should be laid aside. It is essential also that the presentation of results should be simplified as much as possible and be kept as free as they can be from the disputes involved in the study of general problems in heredity. These details are not desired by the two classes most concerned, viz., humanity, who suffers from cancer, and the medical profession who must deal with it.

My practical results in the matter of the inheritability of cancer are these:

1. I have established strains of mice which neither in inbreeding nor in crosses with other noncancer-bearing strains ever in any generation have produced cancer.

2. I have made hybrid crosses between cancerous and non-cancerous individuals and have extracted from such crosses lines of mice which neither in inbreeding nor in hybridization with other non-cancer-bearing strains of mice have ever afterward shown cancer.

3. I have produced a cancer strain in which every member (of a reasonable cancer age) still living after my cancer work began bred true to cancer, and carried it into every strain with which it was ever hybridized.

4. I have extracted from crosses between cancerous and non-cancerous mice, lines which produce as high a per cent. of cancer-bearing individuals as it is reasonable to expect in dealing with a characteristic like cancer which may not appear until a mouse is three years old or even more. Many of these mice in cancer strains inevitably die of other causes before the right provocation has induced cancer in them. But the hybridization test has shown them cancerous potentially, for they transmitted it to their offspring.

This test, of course, has not been made with every individual. To subject every individual in every strain to every test is obviously impossible. The best one can do is to make a reasonable number of such tests, the object being to give to the medical world as quickly as possible the evident facts.

The production of about a thousand spontaneous cancers in specified strains, and the non-occurrence among this entire number of any cancer in certain other specified strains, no matter what test is applied to them, demonstrates to every reasonable probability the inheritability of cancer, and when these results are characteristically and systematically obtained in such an immense stock as to furnish over ten thousand autopsies and a living stock of about eleven thousand mice, with a steady production of between seventy-five and a hun-

dred cancer patients all the time and almost without exception within proved cancer strains, this reasonable probability is raised to an almost indisputable fact; and whether or not my strains of housemice have behaved in hybrid crosses in accordance with the established canon has no bearing whatever upon the behavior of cancer. It is an academic dispute which lies in quite another field.

In regard to my use of the terms "dominant" and "recessive" with respect to cancer behavior: it is almost the established conviction to-day that these terms are descriptive and not dynamic, and they furnish in the description of the behavior of cancer in heredity a graphic and convenient tool. That is probably all they furnish in the exposition of any problem in heredity. They may be discarded for even that service within the next few years.

The chief value in the study of cancer of the use of a partial Mendelian background of comparison (although the details may be under dispute) is to show to those most interested how far back in a strain cancer may lie and still be transmitted, and by what sorts of crosses this can be done, and to make it plain that in deciding upon the inheritability of human cancer and of the method of elimination of cancer from a family, one can not take as a criterion of judgment whether or not the immediate parents exhibited cancer.

I do not desire or make a strict Mendelian interpretation of my results, indeed I should deplore such an interpretation. I have used Mendelian comparisons (1) to make clear the influence of a more or less remote ancestry upon later generations of progeny; (2) to show how cancer, like albinism, has been transmitted in my strains through generation after generation by individuals who did not exhibit it; and (3) to demonstrate how cancer thus transmitted finally leaps into expression in the offspring of a pair neither of whom expresses cancer, but both of whom bear it potentially.

The approximation to even the most conservative Mendelian expectation is strikingly close for such a characteristic as cancer.

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SCIENTIFIC BOOKS

The Mathematical Theory of Investment. By E. B. SKINNER. Boston, Ginn and Company, 1913. Pp. ix+245.

Skinner's *Theory of Investment* is divided into four parts: 1. Algebraic Introduction. 2. Interest and Annuities. 3. Probability and Its Applications to Financial Problems. 4. Tables.

The Algebraic Introduction gives a sketch of arithmetic and geometric progressions, limits, series in general (with particular emphasis upon the binomial, exponential and logarithmic series), logarithms and graphical representation.

In the discussion of Interest and Annuities, the old standard problems are taken up, including such applications as amortization, the valuation of bonds, sinking funds, depreciation, building and loan associations.

In the third part a short introduction to the theory of probability precedes the discussion of life annuities and some other problems in life insurance.

There are 12 tables, mostly 7 place, for dealing with interest (simple or compound), discounts, and present values, annuities, and life insurance data.

This work is elementary, clearly written and satisfactory throughout for a general introduction to the problems with which it deals. In regard to insurance, one finds just about enough for an introduction without too much to take the cream off a real course in insurance. The subject of interest and annuities, however, is handled in such detail that further work upon the subject might not be needed.

The work is entirely formal, that is to say, no reference is made to economic conditions which affect the real rate of interest. The question, for instance, of the price level is not mentioned. No theory of interest can be other than mere form, without substance, apart from a discussion of the effect of a change of price level upon the investment yield, both as to principal and interest.

In discussing the valuation of mining properties the author says: "When a sum of money is loaned the person making the loan not only receives interest at a stipulated rate at the