Department of Agriculture what fraction of the corn planted in each county was planted from hybrid seed, and we know from numerous and widely distributed field experiments the comparative performance of different strains of corn when grown side by side under identical conditions. In these experiments adapted hybrids consistently outyield the varieties of corn formerly grown, with an average margin of over 25 per cent.

This is an increase in yield which costs nothing except the added cost of producing the special type of seed and the added cost of harvesting a larger crop. In practice the seed is commonly produced by specialized seed growers, and the production and sale of hybrid seed corn has now become an industry with an annual turnover of about $75,000,000.

A conservative estimate of the increase in national corn production during the four years 1942–45, due to the partial use of hybrid corn, is 1,800,000,000 bushels. The money value of this increase on the basis of farm price per bushel is more than $2,000,000,000.

It is, therefore, no exaggeration to say, speaking in terms of the over-all national economy, that the dividend on our research investment in hybrid corn, during the war years alone, was enough to pay the money cost of the development of the atomic bomb.

Fig. 1 shows three ears of corn from my experimental crop of 1912, one ear each from my purebred strains, designated in my papers as Strains A and B and the hybrid produced by crossing A with B. These illustrate the truth of the old saying: "Great oaks from little acorns grow." Also shown in the figure is an ear of commercial hybrid corn of recent production from hybridized seed corn. It is a hybrid grown in 1940 from seed produced by the De Kalb Agricultural Association, one of the largest of the commercial hybridizers.

Obituary

Thomas Hunt Morgan

1866-1945

Thomas Hunt Morgan was an outstanding member of what may be called the heroic generation of American biologists—those whose work raised American biology to a position second to none among the countries of the world. This was the generation which, under the stimulus provided, in the first place, by Darwin's theory of evolution, second, by the rise of cell study in Central Europe, and third, by the sensational results of the experimental method of approach to problems of generation and development, set afoot that great series of researches in these related fields which made all general biology a really exact science, and which gave hope that ultimately the gap between it and the sciences dealing with inanimate matter could be bridged. With respect to both its versatility and the far-reaching nature of the conclusions convincingly established by a continuation of the lines of attack opened by his pioneer experiments, Morgan's work stands pre-eminent among the accomplishments of his generation.

Morgan's nature was iconoclastic: he took no stock in the pseudophilosophical mumbo jumbo rampant among many biologists even in the era immediately following Darwin, and would not let himself be overawed by the air of mystery surrounding such subjects as regeneration, embryology, heredity, and evolution. His approach was essentially of the type sometimes referred to (especially by those out of sympathy with it) as "mechanistic," although he did not commonly attempt to reduce his formulations all the way down to the level of the already-known physics and chemistry. As an ardent believer in, and practicer of, experiment, and again experiment, in whatever field, he belonged to that group which at the same time so abhorred what they termed "speculation" that they even distrusted the validity of the most essential lines of reasoning of Darwin himself, and he was a leader in that wave of skepticism whose participants "doubted the doubt till they doubted it out." Perhaps biological progress might have been even more rapid if the wheat had not been thrown away with the chaff, yet the end result of the skepticism, since it was combined with experiment and exact observation, was to lead some of this generation, and most of the next, to a vindication of the Darwinian essentials after all, and to an effective implementation of the Darwinian theory which joined it up with a scientific view of living matter in general. It is unusual to find a man who, like Morgan, is willing so to go back on his early preconceptions when the empirical facts demand it.

Starting out along morphological lines, Morgan, following Roux and Driesch, early went over to the experimental attack on problems of development, and his work helped to establish rational interpretations of such phenomena as the polarization of the frog's egg. His prolific work on regeneration pointed up a number of important problems in this field also and aroused much other American work on the subject, while his attempts to explain the phenomena of regeneration and development on a common basis, and in a rational way, as by the hypothesis of the influence of mutual pressures, though not finally confirmed, showed that there were grounds for hope of solutions being obtained here by experimental means. Turning to problems of evolution, he joined with those who called for more empirical facts, welcomed the results of the experimental breeders, and, after a more purely critical period in these lines (as in his *Evolution and adaptation* and *Experimental zoology*), set about the spadework in earnest himself. The "Mutation Theory," initiated by de Vries, seemed to him to provide a way out for the origin of species because he was distrustful of the theory of natural selection. Thus, when it appeared that the fruit fly, *Drosophila*, was amenable to breeding studies, as shown in the work of Castle, Moenkhaus, Lutz, and (in the Columbia laboratory itself) Payne, Morgan eagerly seized on this material for finding out the facts at first hand. Everyone knows of his remarkable assiduity in finding mutations, from about 1909 to 1912, and of the facts that they did not, individually, establish new species after all and did not (as he, following Darwin on this point, had thought they might) show a qualitative relation to the conditions under which they occurred, or a tendency to be repeated more often in a given direction after having once occurred. Undaunted by these seeming negatives, however, Morgan deflected the direction of his search and studied the method of their inheritance once they had occurred.

It was this reorientation of his attack, together with a concentration of attention upon those variants that could be recognized more definitely, which enabled Morgan to follow the transmission of sex-linked genes in *Drosophila* and so to show that this conformed with the chromosomal pattern which Miss Stevens had found cytologically in that organism. From this preliminary (which, after all, was more or less paralleled in other forms) Morgan passed on to what undoubtedly stands as by far his greatest contribution: the setting up of the genetic case for crossing over. This involved the obtaining of data which showed, first, that different genes connected with the same pair of chromosomes (the X) undergo interchange, and second, that they do so with various frequencies, all of them below that of random relations—that is, they are "linked." It involved, further, the recognition that these facts are just what is to be expected on the "chiasmatype" theory which had already been proposed by Janssens, especially if, as Morgan himself pointed out should be the case, genes further apart have more crossing over between them. This served as the forerunner to a multitude of researches by numerous workers, at first mainly on the *Drosophila* material, which have served to vindicate the crossing-over theory and along with it the chromosome theory of heredity in general.

That the early findings of Morgan were so quickly followed up and generalized upon was due in no small measure to his having opened the doors of his laboratory and, indeed, of his mind to a group of co-workers, already trained in the chromosome theory by Wilson, who chose entirely their own leads and who would not have had the opportunity to carry on freely in most European or even American laboratories. Had Morgan been more of an authoritarian and less willing to be merely an equal member of the group in discussions, the younger workers would not have had the opportunity they needed for the further development of the subject, and Morgan's own mind would not have become so opened to the full implications of the facts found in the *Drosophila* work as to have led him finally to agree that, after all, they lead inevitably back to a theory of natural selection, now on a more rational basis and provided with an elaborate mechanism for its operation. Morgan was won to this point of view only against his own very active opposition, yet it is to his enduring credit that he was finally willing thus to alter his whole viewpoint in accordance with the empirical facts. Having done so, he was able through the series of hammer blows of his successive expositions of the subject to persuade the world of the truth of this point of view. However much the story of the formative period of the *Drosophila* work may be rewritten and reappraised in the future, there must remain agreement in regard to the fact that Morgan's evidence for crossing over and his suggestion that genes further apart cross over more frequently was a thunderclap, hardly second to the discovery of Mendelism, which ushered in that storm that has given nourishment to all of our modern genetics.

H. J. MULLER

Indiana University

Clarence Erwin McClung

1870-1946

C. E. McClung died suddenly on 17 January 1946 at the age of 75. Thus, one more of that group of brilliant zoologists who appeared on the horizon of American science at the turn of the century has passed on. He was born at Clayton, California, on 5 April 1870, but spent most of his earlier years in Kansas, where he grew up, received his education, and became established in his professional career. As a boy he