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THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

THE SCIENTIFIC RETROSPECT[1]

IN selecting a title for this address at rather short notice, I took the precaution to provide the bow I was drawing (somewhat at a venture) with more than one string. I propose to avail myself of two such strings: first a few remarks on scientific history and secondly a glance at the remarkable way in which our available scientific retrospect has recently been expanding.

We scientific workers are perhaps too neglectful of our past history for the obvious reason that our present is so engrossing. It is a necessity of the game to concentrate attention on the present, and even deliberately to clear the past from our minds; just as in playing cards we must remember the particular hand we are playing and forget those which preceded it. But even in playing cards certain exceptional hands, illustrating a novel situation or calling attention to possible new developments, live in the memory, are described by writers on "bridge," and eagerly read by their readers; and similarly in scientific work there are incidents and epochs which we should do well to recall, and even to keep before our minds, as stimuli to our work or guides in conducting it. May I give a few instances, chiefly from my own department of astronomy?

The name of Kepler is widely and justly known in connection with his three great laws, but he may also be remembered as a man who faced heroically as great a disappointment as a scientific worker can well meet. He thought he had discovered the secret of the structure of the universe and his hopes were completely dashed. His universe was practically limited to the solar system, as Copernicus had explained it, of six planets circulating round the sun each on the surface of its own sphere. The six spheres were a legacy from older conceptions, which Kepler himself was presently to destroy, but in the five spaces intervening between these six spheres, as Copernicus had pictured them, Kepler found that he could fit the five regular solids. Some of you may hear for the first time that there are five regular solids and no more; the number of regular plane figures (triangles, squares, penta-

[1] An address to "scientific workers" delivered during the New York meeting of the American Association for the Advancement of Science, on the afternoon of Friday, December 28, 1928.

gons, and so on) is so obviously unlimited that the reason for limiting the solids does not readily occur to the uninitiated. To these I may perhaps explain how the limitation comes from the nature of a corner in a regular solid. Let us think for a moment of a cube, which is the most familiar of the five. To each corner there are three faces meeting in the point, and it is obvious that there must always be at least three such faces. We can not make a corner with two. Moreover, in the particular case when the faces are squares, we can not have more than three; if we try to make four squares meet together, they obstinately form a plane and the corner disappears. If we use pentagons instead of squares, we can again make three pentagons meet in a corner but four will behave like an umbrella blown inside out by the wind; we could not build up a closed solid with corners of that kind. And if we try to use hexagons for the faces, even three of them fit into a plane as did the four squares; they make the familiar honeycomb pattern. Hence we have so far got only two regular solids, made with squares and pentagons, and it is pretty clear that to go beyond hexagons is useless. We turn back to equilateral triangles, and here our luck improves. We can make a corner with three triangles or four or even five, but when we try to use six we get the same flat result as with four squares or three hexagons. Hence there can be no more than five regular solids, three formed with triangles, one with squares and one with pentagons. And Kepler's great discovery was that these five would fit in admirably between the six planetary spheres, so that if the corners of one solid were on the outer sphere its faces would touch the inner. Thus he placed a cube with all its corners on the sphere of Saturn; inside the cube he put a sphere touching the faces of the cube, and lo! that was the sphere of Jupiter. The other solids each occurred once and once only as in the following table, and if we allow our minds to unhook themselves for a moment from the prejudices of modern knowledge we must admit that here was marvelous evidence of a great design. Just six planets (nearly two centuries were to elapse before the number was increased) and just five solids and no more: one solid for each interval without repetition or omission. Surely the Plan of the World was detected? There was just one doubt: the figures given by Copernicus (without suspicion of the use Kepler was to make of them) did not precisely fit the geometry of the edifice. The following table shows (in the column headed "Kepler") what exact geometry required, and it will be seen that the figures differ somewhat from those in the column "Copernicus." Even then we must admire the closeness of the fit, which has no glaring exception. Small wonder that Kepler confidently believed the differences to be due to the imperfect estimates of Copernicus. He felt sure that if he could get more accurate measures of the planetary distances the errors would disappear and the perfection of the whole be made manifest, and he felt sure also that he could get better measures from the great Tycho Brahe. By that time Tycho had had to leave his native land and had been welcomed at Prague, so to Prague Kepler went in high hopes. And then his house of cards tumbled to bits. Tycho would have none of it, or of the kind of intuitive principles which it represented; he inculcated the severer methods of patience and toil in observing. Have we the imagination to sympathize fully with the terrible disappointment? Have we the heroism to follow Kepler, however distantly, from the abasement to which he had been cast down in his rise to the even greater discovery of the mechanism rather than the design of the solar system?

Kepler's Great Scheme

	Kepler	Copernicus
Saturn	1060	1000
Cube		
Jupiter	612	635
Tetrahedron		
Mars	204	212
Dodecahedron		
The Earth	162	160
Icosahedron		
Venus	128	127
Octahedron		
Mercury	91	92

The International Union of Geodesy and Geophysics was most hospitably entertained at Prague in 1927. Several astronomers attended the meeting and accompanied the Astronomer Royal of England when he laid a wreath on the tomb of Tycho Brahe. From an observatory tower we saw also the curiously roofed house in which Kepler lived and in which he must have worked out from his master's observations the three great laws known by his name. I throw on the screen one or two slides of these surroundings, which I owe to the kindness of Professor Nušl and Dr. Frič.

Let us now turn to an incident nearly contemporary with Kepler's disillusionment, the great demonstration experiment of Galileo when he dropped two unequal weights from the top of Pisa's leaning tower, and showed that they fell together—not, as Aristotle had said, with speeds in the ratio of their weights. The authority of Aristotle had sufficed to maintain this erroneous belief for a couple of thousand years, and in recoil from the error there is some temptation to underestimate Aristotle. But this temptation is to be resisted for many good reasons, and I take the opportunity to mention a comparatively new one, showing the important relationship of this great man to Greek astronomy (as we usually think of it). My friend, Dr. J. K. Fotheringham, recently delivered a lecture of thrilling interest on "The Indebtedness of Greek to Chaldaean Astronomy,"[2] in which he shows how the Chaldaean astronomers, especially Naburianos and Cidenas, had built up a wonderful series of observations of eclipses for about 360 years and discussed them so as to obtain the constants of the lunar theory with an accuracy quite marvelous for that era. To quote one or two sentences from the lecture:

(a) It is the rarest thing for a modern observatory to continue a program of observations through thirty years, let alone 360. The only modern observations that can compare in continuity with the Babylonian are the Greenwich meridian observations which have been maintained since 1750. Against these I have nothing to say, but their analysis is not so simple as it once seemed, and we could do with another Cidenas.

(b) So it seems that Cidenas's "Canon of Eclipses" actually contained a better value for [the motion of the Sun from the Moon's Node] than that which is used in our standard modern canon [Oppolzer's].

Now this wonderful reservoir of astronomical information became accessible to the Greeks through Aristotle's initiative: without it we might never have heard of Hipparchus and Ptolemy. To quote two more sentences from the lecture:

(c) The great event in the development of exact astronomy in Greece was the sending of a collection of Babylonian observations by Callisthenes at the request of his uncle, Aristotle.

(d) If the aim of astronomy is to give an accurate numerical representation of phenomena on which predictions may be based, then Naburianos, Cidenas, Hipparchus, Ptolemy stand by themselves and each builds on the work of his predecessors. There were no others of the same caliber till Tycho Brahe and Kepler.

If ever then we are tempted to picture Aristotle as neglecting observations we have an antidote in remembering this action of his which secured to the Greeks the advantages of observations which they did not themselves make. We may rest assured that Aristotle would not have approved the use made of his great name to uphold authority and prejudice against observation.

Nor does the conflict belong to those early days alone: we have seen it just as bitter in the nineteenth century over the doctrine of evolution. As I am here representing the British Association perhaps I may venture to recall the historic scene at the Oxford meeting in 1860, when Bishop Wilberforce undertook to destroy the new and pernicious hypothesis outlined by Charles Darwin, a scene which may not be so well known on this side of the Atlantic as on the other. It will at least serve the purpose of showing, when we are dealing with the historical aspects of science, how difficult it is to obtain an accurate record of facts, even when many witnesses are available. Up to a certain point the witnesses are all agreed. The battle between Wilberforce and Huxley, Hooker and others arose rather unexpectedly. The announcement that the eloquent bishop would speak on the thorny subject attracted a greater crowd than had been foreseen, so that the meeting was changed to a larger room. Wilberforce made an undoubtedly persuasive speech, following the lines of a review which had appeared shortly before and was obviously from his pen; but he made an important error of tactics (and perhaps of good manners) at one point of it. Turning to Huxley he inquired suavely whether it was on his grandfather's or his grandmother's side that he was descended from an ape. So far all accounts are in substantial accord: but they differ considerably as to the exact nature of Huxley's reply. I will quote two of them: the first written by the historian John Richard Green, then an undergraduate, in a letter to a friend;[3] the second by the biographer of Bishop Wilberforce. Green's letter gives the following version:

I asserted, and I repeat, that a man has no reason to be ashamed of having an ape for a grandfather. If there were an ancestor whom I should feel shame in recalling, it would be a *man*, a man of restless and versatile intellect, who, not content with an equivocal success in his own sphere of activity, plunges into scientific questions with which he has no real acquaintance, only to obscure them by an aimless rhetoric, and distract the attention of his hearers from the real point at issue by eloquent digressions, and skilled appeals to religious prejudice. ["Life and Letters of Charles Darwin," II, p. 322. The word "equivocal" was challenged as incorrect.]

The second version is from the "Life of Bishop Wilberforce," written by his son, and gives Huxley's reply as being "that he would sooner have been descended from an ape than a bishop." It is tolerably easy to understand the difference between the two versions: but it might have been harder to infer the truth if we had had only one of them.

[2] See *The Observatory* magazine for October last.

[3] The friend, Professor Boyd-Dawkins, died on January 15, 1929, in his ninety-second year, soon after these words were spoken.

I will add another illustration of the difficulties of getting exact history. It is impossible to think of Galileo and Tycho and Kepler without thinking of Newton, and though to give rein to our thoughts would take us too far, we may find in the very greatness of his name and fame reason for surprise that some of the stories about him should be so ill founded. Doubtless most of us have heard or read of the burning of some of his papers by the agency of his little dog, and of the gentleness of his reproof "Oh! Diamond, Diamond, thou little knowest the mischief done!" Again we know the story of the hole under the door for his cat to enter or leave the room, and the additional smaller hole made for the convenience of the kittens when they came into the world. But it seems quite certain that these stories are baseless, for we have the statement of Dr. Humphrey Newton, who lived with him for the important five years, 1684–1688, that he heartily disliked both dogs and cats, and would never have either near him!

I turn now to the other string of my bow: the recent great expansion of the period of time over which we consider ourselves justified in looking backward. At the epoch to which I have just been referring, when Darwin put forward his theory of natural selection for which a long period of slow development was required, Sir William Thomson (Lord Kelvin) was only prepared to allow one hundred million years for the past age of the earth; his colleague, Professor P. G. Tait, went further in restriction and would only allow ten million. In 1869 Charles Darwin wrote:

> Thomson's views of the recent age of the world have been for some time one of my sorest troubles.

and again in 1871:

> I can say nothing more about missing links than what I have said. I should rely much on pre-Silurian times: but then comes Sir W. Thomson like an odious spectre.

There was in fact a sharp difference of opinion between biologists and geologists, who demanded a long past age, and physicists, who insisted on a short one—a difference which persisted almost to the end of the nineteenth century. Special attention was recalled to it by Lord Salisbury's presidential address to the British Association at its Oxford meeting of 1894, a third of a century later than the dramatic scene of 1860. Accordingly, in 1896, Professor Poulton devoted his presidential address to Section D (Zoology) to a consideration of the arguments of the physicists, in order to see how far the claims of the paleontologists could be satisfied. (May I acknowledge in passing my great indebtedness to this address for our present purpose?) He reviewed three lines of argument which had been used, and in all three great changes have been made in the thirty years which have since elapsed, though in the previous thirty, as we have seen, the situation had been scarcely modified at all. He considered the Moon's movements, the Earth's cooling and the Sun's past life, as regards all of which we have learnt much in the present century.

New tables of the Moon's detailed movements have been prepared in this country by E. W. Brown, building on the foundations laid by G. W. Hill. They represent a very fine piece of successful work, but they do not directly concern the problem reviewed by Professor Poulton, which considers the relations of the Earth and Moon introduced by the tides. The Moon holds up the tides against the Earth's rotation, causing a certain amount of friction which slowly retards that rotation. This again reacts on the Moon to drive it further away from the Earth: hence its present distance of 240,000 miles was less in the past, and looking backwards we can trace it to be less and less until the Moon was very near the Earth, and looking back further still we come to a time when the Moon was probably just detached from the Earth, of which it previously formed part. All this was realized long ago, and Sir G. H. Darwin had mathematically traced the Earth-Moon's history backwards in this way to a time when the two bodies were both rotating in about six hours as a twin body. His result for the length of the history from that time to the present was something over fifty-seven million years. But there were two uncertainties affecting this calculation: the first as to the amount of the frictional effect at the present time, which rendered the starting point for the backward glance rather vague, the second as to the precise character of the friction. It was apparently difficult to trace an adequate friction in the tides of the deep oceans, which rolled too smoothly. An alternative suggestion of finding a sufficient effect in the elastic deformation of the solid earth succeeded no better. It was only in the present century (and in fact during the great war) that G. I. Taylor was led to study the behavior of shallow seas, and to find an adequate response. The shallow seas differ from the deep oceans in somewhat the same way as a brawling stream from a placid river: they have a character which he called "turbulence," of which he found sufficient in the Irish Sea (do we recognize here a certain appropriateness?) to cause a sensible part of the estimated frictional delay of the Earth's rotation. H. Jeffreys carried on the good work to other seas, and J. K. Fotheringham showed that the correspondence of the total to his estimate from eclipses was satisfactorily exact.

Eclipses of the Sun and Moon provide the means of estimating the amount of this slow change in the Earth's rotation and the Moon's orbit. It requires no telescope to recognize that on one occasion the Moon is totally eclipsed, or that on another half the Sun is covered up a few hours after midday: so that we get exact observations of the relative positions of the Sun and the Moon in ages past. At least it was hoped that they were exact up to nearly the end of the last century: but it had gradually become clear that there was something wrong in either the historical records or the extension of our modern astronomical tables back to past times. Few people were competent to criticize both elements of the problem, so that the astronomers were inclined to blame the historians as inaccurate and the historians doubted the capability of the astronomers to project themselves back into the past: in either case the problem had almost been put aside as insoluble. Interest in it was revived by P. H. Cowell, and promptly followed up by J. K. Fotheringham, who was almost the first scholar to combine remarkable historical knowledge with astronomical. (The value of his work has been recognized by his appointment as reader in ancient astronomy and chronology in the University of Oxford.) He deduced values for the elements of the slow changes in the movements of the Moon and Earth which satisfactorily fitted the ancient records when certain changes were allowed in the historical interpretation of them. Let me give one illustration from the most famous eclipse of antiquity—that predicted by Thales of Miletus and recorded by Herodotus:

> There was war between the Lydians and the Medes for five years: each won many victories over the other, and once they fought a battle by night. They were still warring with equal success, when it chanced, at an encounter which happened in the sixth year, that during the battle the day was turned to night. Thales of Miletus had foretold this loss of daylight to the Ionians, fixing it within the year in which the change did indeed happen. So when the Lydians and Medes saw the day turned to night they ceased from fighting, and both were the more zealous to make peace. Those who reconciled them were Syennesis the Cilician and Labynetus (= Nebuchadnezzar) the Babylonian. [Translation of Herodotus by A. D. Godley.]

The eclipse was that of May 28, 585 B. C., but "astronomers, misled by historical students, have generally assumed that the fighting would be near the river Halys (in the north of Asia Minor) . . . and have attempted to amend the lunar theory accordingly." Dr. Fotheringham's new information shifts the track to the south, where not only is the battlefield near Cilicia (over which Syennesis was ruler) but it includes the Pisidian road, the importance of which for military purposes was quite independently pointed out by Sir William Mitchell Ramsay.[4] It will be seen that in this instance both history and astronomy required adjustment. Dr. Fotheringham has had the further satisfaction of finding confirmation of his suggested figures in the tablets for the rising and setting of Venus,[5] and in others relating to commercial contracts. The general outcome is that we now have a good accordance between astronomy and history, with a satisfactory chronology back to the time of Abraham. So far as this it is not *very* different from that of our old friend, Archbishop Ussher, though we do not thus learn anything of times before Abraham wherein lay the differences, not merely between the biblical chronology and the geological, but between the geological and the physical.

As concerning our earth itself, the main line of physical argument was from its rate of cooling, on the assumption that it had no way of renewing its heat, but was just a body that had once been hot. The cooling of our earth or of a plate of porridge is most obvious near the boundary: as we dig down into the earth we find it warmer: and from the rate at which it grows warmer Lord Kelvin estimated how long it had been cooling. Even then he had to make certain assumptions as to its conductivity: and after Lord Salisbury's address in 1894, John Perry pointed out that a change in these assumptions might make a good deal of difference. If, for instance, a conductivity ten times as great were assumed, the age of the earth would be multiplied, not by ten but by fifty-six. Of course there was no way of deciding between such assumptions because we then knew very little about the inside of the earth. And the discovery of radioactivity vitiated the whole argument, which, as above stated, assumed that the earth had no way of renewing its heat. We now know that it has such ways, and though we do not know exactly what its stores of radioactivity are, they would probably suffice to extend its past life to five thousand million years, an estimate well supported by other considerations. Let us, however, remark that we have meantime learnt some facts about the previously unknown interior of the earth from the new science of seismology. Up to near the end of the nineteenth century our knowledge of earthquakes was practically limited to the destruction near the spot where

[4] See the Halley Lecture for 1921: Oxford University Press.

[5] See "The Venus Tablets of *Ammizaduga*: a solution of Babylonian chronology by means of the Venus observations of the First Dynasty," by S. Langdon, J. K. Fotheringham and C. Schoch. Oxford University Press, 1928.

they occurred: we now have sensitive instruments which receive and record messages sent to all parts of the earth through its body, which incidentally tell us about the nature of the interior through which they travel. The transmitted waves are of various kinds, but we may here consider two of them usually denoted P and S. The letters stand for Primus and Secundus, indicating that P arrives before S; we may also remember them as exPress train and Slow train; or again we may use the letters to mean Push and Shake, for the P waves are longitudinal while the S are transverse. Consequently the P waves will traverse a fluid, as sound waves do: but the S waves will not. When therefore we find P waves arriving without accompanying S waves, we may fairly infer that fluid has intervened somewhere in the path: and this in itself suggested that some part of the earth's interior was liquid. But this suggestion was crystallized into much more definite shape by Gutenberg, who identified certain waves as S waves received before their expected time, explaining that they had traveled as P waves through the earth's liquid core. He postulated a molten iron core to the earth, of radius rather more than half that of the surface. When S waves emitted by an earthquake reach the boundary of this molten core, they can not travel further as S waves, but (by the well-known properties of a surface of discontinuity) they can generate P waves. The Slow-train passengers must go on by exPress. On arrival at the other boundary they can either continue as P waves or change back into their Slow train for the rest of the journey to the surface, where a seismograph receives them; but since they have traveled part of the way by exPress, they arrive before their expected time. Thus from the study of earthquakes which occur near the earth's surface we have learnt this important fact about its interior.

But how much more have we learnt about the Sun, by means of the radiation from its surface! The new theory of relativity has revolutionized completely our views of the origin of this radiation. The old view was that the mass of the Sun remained unaltered, but that by its slow contraction under gravity heat was generated and radiated out to us. The possibilities of radiation were thus limited by the possibilities of contraction from indefinite diffusion to possible compactness. Einstein has changed all that. Without specifying how the transformation was effected he showed from his beautiful mathematical theory that radiation came from the very body of the Sun himself; that by radiating he is shedding four million tons of himself every second, or one hundred and twenty million million tons every year, since there are thirty million seconds in a year. And yet the recognition of this spendthrift rate, far from reducing the Sun's expectation of life, has enormously increased it; for his available capital, in the shape of his own body, is immensely greater than that formerly represented by the mere shrinking of his body into compact order. Think merely how often we must mention a million in estimating the Sun's body or mass. He is a million miles high, a million miles wide, and a million miles through: to assess the cubic miles in his content we must therefore multiply together *three* millions. And then a cubic mile represents millions of tons. Think of a cartload of coal—measuring a few feet each way: in a cubic mile we could put some thousand million such carts: that makes at least *four* factors of a million for the Sun's mass in tons. If to get his life in years, we divide by his annual expenditure we drop two of the factor millions; but there are still two left: millions of millions of years instead of the hundreds of millions which were all that the physicists would allow on the old theory. The estimate works both ways, back and forward: we need have no anxiety about our immediate future, but also it becomes clearer and clearer that in the past "the geologists can have all the time they want," to use a phrase in which the closing of the controversy, at one time rather bitter, was marked by an eminent physicist.

Looking backward our vista is, however, limited by the consideration to which Professor Eddington has recently called attention in his last new book, "The Nature of the Physical World," which I will heartily commend to you as well worthy of attention. He uses the symbol of "Time's arrow" to remind us that past time can not be like future time. The universe is running down: and he compares the method of its running to the changes made in a pack of cards by shuffling. The cards come from the maker beautifully organized, reds and blacks alternately: so that to play any reasonable game with them we must shuffle them, until the organization disappears. Would it ever reappear if we go on shuffling? The answer which tempts us is, "Yes, if you go on long enough," to which Professor Eddington replies by substituting another question. By playing at random with a typewriter a monkey *might* print an intelligible sentence: how long should we expect it to be before an army of monkeys playing on all the typewriters required would print off all the books in the British Museum? By such analogies does he try to bring home to us the certainty that the universe is departing more and more from organization as time goes forward. But this also necessitates organization increasing as we go backward: and though there is no difficulty in looking forward, we recognize at once the bewilderment of looking back. Shall we not come to a time when the organization is so complete that we can not imagine it improved? The question is a suitable conclusion to our Scientific Retrospect.

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RESEARCH STANDARDS IN ANIMAL HUSBANDRY[1]

THE applications of science to the problems of agriculture are of relatively recent date, both in America and in Europe. Although sporadic attempts at agricultural research had been made in earlier times, it was not until the middle of the last century that formally organized agencies were created in which the methods of science could be brought to bear upon the problems of this, the oldest field of human endeavor.

In 1843 an English gentleman organized at Harpenden, England, the first agricultural experiment station to be created in any nation. Sir John Lawes at that time, largely through his own private means, gathered together the essential features of a modest chemical laboratory and set to work upon a study of the soil as a medium of plant growth. Under the leadership of Lawes, followed subsequently by Gilbert, Hall and Russell, this station has attained an outstanding position of leadership in the field of soil science and has made noteworthy contributions to our understanding of the complex reactions of the soil.

A few years later, in 1853, a group of Saxony farmers organized an experiment station in the little German village of Möckern near Leipzig and called a young German chemist, Emil Wolff, as director of its work. Wolff's efforts were largely devoted to a study of the problems of animal husbandry with particular reference to the food requirements of farm animals. This institution, still in existence and still doing important work, may be regarded as the first animal husbandry experiment station of the world.

From these modest beginnings, one in the field of animal industry, the other in that of plant industry, the movement for the establishment of institutions for the scientific study of agricultural problems has spread into all civilized nations of the world. In the more advanced countries of western Europe and America such institutions have become both numerous and extensive, and great progress has been made.

Under the stimulus of the Federal Land Grant act and subsequent acts of Congress, the United States suddenly called into being, soon after the movement was started in Europe, a nation-wide system of agricultural colleges and experiment stations, when there were few precedents for guidance, no qualified teachers and no organized body of knowledge with which to make a fair beginning. The inevitable result was much mediocre introductory work from which escape has only recently been marked. In Europe the development has been somewhat longer in process, more gradual in growth and, particularly in the older scientific centers, more firmly grounded in the basic sciences. Such development has been the result of various factors. In the older countries of Europe many problems of agricultural practice have been solved through the cumulative experiences of farmers themselves, extending over a period of many centuries. Trial and error methods have indicated the wisdom of certain procedures and the folly of others. Furthermore, the vocational aspects of agricultural education and much of the more empirical type of investigation, such as variety tests, animal-feeding trials, fertilizer trials, plant and animal breeding, control work and other investigations of a less exacting character have been left to agricultural schools of the lower grades and independent experiment stations designed and established for this particular type of work. The agricultural colleges and research institutes of the higher types have thus been free to direct their efforts to more definitely scientific studies. In America, agricultural experiment stations have generally been established and conducted as integral parts of agricultural colleges. They have been regarded as agencies both for studies of an immediately utilitarian character and for fundamental researches requiring scientific ability of the first order. These two purposes have not always harmonized. Under the pressure for immediate results the less scientific work has too often prevailed.

While private initiative and private means established and fostered the first stages of agricultural education and research on both continents, the new movement early came to be recognized as a legitimate function of government and ever since has depended largely upon public grants for its support. This procedure finds its justification as well as its necessity in the fact that it is a primary responsibility of government to do what it can to insure an adequate supply of food and clothing for the growing needs of expanding populations. What is commonly called "aid to farmers," indeed, what is even called "farm relief" (a term to which I must confess some aversion) is primarily protection for urban populations, inasmuch as the farmer unaided can supply from the land most of the minimum necessities for human existence. Such aid to agriculture is at the same time

[1] An address delivered at the dedication of the Animal Science Building, of the University of California, College of Agriculture, at University Farm, Davis, California, November 12, 1928.