Progress has been made toward differentiating negroid types in our population through the work of government bureaus and by local health boards. Anthropologists of the United States have made valuable contributions in recent years to the subject of the black hybrids; the following among others have shown especial interest in this province of research: Bean, in defining negro types in America; Davenport, in his Jamaica studies; Estabrook and McDougle, in their analysis of mongrel Virginians; Herskovitz, working in urban negroid colonies; Hooton, by collecting negroid family lineages; Hrdlička, by studies of colored children and of African colonies; Schultz, investigating the negro fetus; Todd, in anatomical research upon the negroid skeleton. These lines of research, so fundamental and so necessary in connection with other problems of the negro, should be encouraged by the most generous support.

Progress in the solution of negro problems will follow the extension of the registration area for births and deaths into those states where at present the laws are not established. Continuation by the census bureau of efforts developing more fully plans for cooperation with the anthropologists would contribute materially to the at present incomplete knowledge of the racial constitution of our population. The Public Health Service is in position to furnish more accurate information on such questions as that of immunity whenever the basis of racial constitution has been laid. The great insurance companies are establishing valuable records available for research, and their cooperation is essential in learning the characteristics peculiar to the negro and to the brown hybrid relative to disease.

It seems to me that there is no problem before American anthropologists more urgent or more fundamental than that of the race mixtures represented in our American negro hybrids. Delay in attacking this problem will entail an increase in its complications. Vital questions are awaiting solution in the absence of a definite knowledge of race constitution.

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THE AGRICULTURAL EXPERIMENT STATION—AN INSTITUTE FOR FUNDAMENTAL RESEARCH IN RURAL AFFAIRS

I

NOT many years ago scientists discussed with sometimes more and seldom less heat the comparative merits of pure and applied science. To-day the reverberations of those thunderous discussions grow gradually fainter and fainter. But do not be deceived! The arena of the discussion has merely shifted, and the shift has been only on the pages of the dictionary. Research, impeccably pure in quality, emanating, nevertheless, from the realm of applied science, has effectively gassed the gunners who proclaimed applied science as necessarily impure and defiled. Of course not all of it is pure. Some of it has been, is, and always will be not only impure but impractical, unapplied and perhaps even useless. On the other hand, purity seems no longer to be the exclusive character of the sources of unapplied science, since it is obvious that much of the science springing from such sources is inspired by the hope of practical use, and caustic critics even add that much of our pure science can not claim a high degree of purity, if *quality* be the criterion.

It has therefore become convenient and popular to make a new distinction involving a word found on an earlier page of the dictionary. Fundamental science is now claimed as the peculiar field of those not sordidly engaged or, more accurately perhaps, not definitely paid for their labors in the field of applied science. I have never noticed any insuperable averseness on the part of these same fundamentalists to put their fundamental science to such occasional and profitable use as experts are wont to put it—for appropriate fees. Conversely, it is argued by these fundamentalists in science that institutions and individual scientists whose research is tarred by the stick of usefulness are somehow or other outside the pale of fundamental science, incapable of its pursuit or positively unethical in attempting to invade this sacred field.

Presumably the chief workshop for fundamental science has been located in the general science departments of our universities and colleges, where teaching is supposedly the primary function and research a "by-product of teaching." Where such departments are of sufficient size or enjoy especially generous support, the teaching burdens may be comparatively light or entirely lacking; and opportunity is thus afforded for personal research, together with that more or less vicarious type of graduate student research which has in its turn the by-product of a Ph.D. I have the greatest respect and profoundest admiration for the contributions such departments have made, not merely as the chief or well-nigh only training schools of investigators, but also for the numerous and valuable contributions they have made and are still making in the field of research itself. And I am also one who believes that the academic freedom of the college science teacher, *if* and *as* expressed in his unhampered freedom in the attack on any problem of science within the scope of his ability and opportunities, is a priceless possession to be treasured by the whole fraternity of science and to be used by those to whom it is entrusted with a reverent appreciation of its worth.

The field of research of the science teacher is of his own choosing. He it is who may direct the activities of research in his department. He may be no less an autocrat in his small field than is a gigantic bureau of the U. S. Department of Agriculture in its larger field of research activities. But academic freedom by no means automatically confers upon the professor the exclusive possession, or even a due appreciation, of what is fundamental in research, and much less the ability to pursue it successfully.

The increasingly large output of research of all kinds: applied, technical, pure or fundamental—call it what you will—outside of college walls, makes it perfectly obvious that the center of the population of working scientists is shifting. And so it happens, since research has become the every-day instrument of national, state and municipal agencies, industrial organizations, and the like, that a new expression, suspiciously suggestive of the former worship of science, pure and undefiled, has come to the fore. The pure-science idol has been rechristened "fundamental" science.

The new temple is called an institute for fundamental research in this or that special field. You will notice at once that this is in the nature of an admission that fundamentalism in science may now be worshiped outside of collegiate sanctuaries, but please also note carefully that these new temples must be specially dedicated to fundamentalism and not defiled by contact with the debasing influences of application. The traditional ghost must still haunt the house dedicated to research for a practical purpose. It is even conceded that application of results may be taken care of by proper affiliation with practical institutions which shall follow in the wake, and apply or polish, by day-labor methods, as it were, the gems of scientific truths brought up from staggering depths by the institute for fundamental research, which seeks only the priceless stones but declines to cut them or put them in their proper setting.

And, behold! One of the greatest of our universities has recently discovered that there is such a thing as a rural problem and forthwith proposes to establish, presumably in the largest urban center on our continent, an institute of rural affairs whose aim

> would be to make original researches in the field of rural affairs and to interpret and give publicity to the best available knowledge concerning the fundamental problems of agriculture and country life, the most promising methods of their solution, the relationships of the urban and rural groups, and the international aspects of the farm question.

A modest program indeed! And perhaps our existing forty-eight institutes of rural affairs may now rest on their shovels, or hoes, or what have you?—and complacently view from afar their problems solved by the diggers in the subways and the sweepers on the sidewalks of New York.

When it is pointed out that institutions already exist for research in such special fields, the reply is prompt that such institutions are engaged in practical research and not suitable for research in the fundamentals—which may or may not at all be true. It is said that such institutions are pressed for results of practical value and for immediate use, and that such demands preclude the possibilities of the deeper digging which may uncover underlying veins of richer ore. While this contention is justifiable in many instances, it is just as true that the time-honored sources of fundamental research in our colleges and universities have equally distracting activities in teaching and administration. Indeed, such institutes for fundamental research as already exist can hardly be said to have completely established the hypothesis that dissociation from application of their results has made them conspicuously successful in their primary function of fundamental research.

Does the history of science show that a segregation of laborers into fundamental researchers and the garden variety polluted with practical objectives has resulted satisfactorily? Can the two types of research, if there are two types, be separated? And, if possible, is it even advisable to do this? Can fundamental research and application go hand in hand?

Certainly, Pasteur was engaged in solving exceedingly practical problems. And perhaps Newton was supposed to be picking the apples instead of waiting for them to fall.

The truth of the matter is that such a distinction between fundamental and applied science is purely artificial. "Fundamental," as applied to problems in science, is merely a relative term. If pressed to a logical conclusion, only those problems which deal with the ultimate constitution and origin of the simplest units of matter may be considered as fundamental to all of the physical and biological sciences. The physicist or physical chemist—I am never quite sure which of these two is overlord of those ultra-basic problems—would thus, in the last analysis, be the only simon-pure investigator of fundamental problems. Of course, the grand old army of working scientists is not for a minute going to agree to this conclusion. Each and every one of them is perhaps secretly hoping that his researches will turn up one of these priceless gems of fundamental value. And many of them are impertinently frank enough to tell the world of their hopes—yea, even their confident expectations!

No, there is something just a little dubious, not to say utterly fallacious, about this apparently desirable and conveniently discriminating distinction between fundamental and applied science.

Fundamental, I repeat, is merely a relative term. One fact or process or phenomenon is merely closer to the bottom of the question; it is nearer the basement or the sub-basement. Research starts from a known field or stratum of facts and proceeds to the field of the unknown. Facts for which we have causal explanation are of greatest value in science and permit of natural classification. When groups of facts, even though not apparently related, are shown to be connected by a common causal agency, you have a more basic fact or cause. Just so may isolated outcrops of ore be connected with a common ore body far below the surface. But "fundamental" to this, again, lies the contribution which shows the exact geologic formation which will produce such an ore body and the certainty of predicting such an ore body from similar formations.

Research logically and methodically proceeds from known facts to the immediately preceding or underlying facts—all of them are more fundamental. The new facts may prove to be of importance only in explaining the particular phenomena under investigation. They may be of no practical value or they may be of enormous importance practically. But sometimes one of these innocent-looking facts, turned up perhaps by the lone digger, may be connected with another or with a great series of other phenomena, and then the scientific world announces a great piece of fundamental research. Perhaps the original discoverer, so completely absorbed in his own particular shaft, never senses the relation of his newly discovered fact to the other groups of phenomena to which it may be fundamental, as a child who has found a valuable diamond might merely add it to his collection of pretty stones. It is true that we have too many of such investigators working in practical science, but they are not unknown in the field of pure science.

Now the fundamentalist in research would reverse this process in a certain sense. His institute would not bother about the smaller veins of ore—slight contributions of knowledge. It would seek only those diamonds of Kohinoor size. It would strike directly for the great ore bodies that lie far below the surface. How big must these ore bodies be? The fundamentalist can not tell you that. The biggest ore body, basic to all of the rest, is the constitution of the ultimate unit of matter. But between that body and our present knowledge lie untold depths of unexplored ground through which we must dig our biological, chemical, geological and countless other shafts. The institute dedicated only to fundamental research would be like the miner who would sink a shaft far below the bottom of all existing shafts and then start his horizontal exploration with the hope of finding facts of great importance to all the diggers above. Of course he might do that very thing, but how would he know that the new vein of ore actually connected with one or all above? "Well, that," he says, "is a practical problem and we are not interested in it."

More than sixty years ago an Austrian monk became interested in growing peas, the edible garden variety as well as fundamentally educated peas, round peas that roll easily off the knife and square ones that do not. Perhaps he was not so practically minded in his researches as to care particularly about the balancing power of peas. He made investigations and discovered a real research gem of very great biological value. It seems hardly possible that Mendel could have realized the fundamental importance of his discovery. Certainly, the scientific world of his day either overlooked it or failed to appreciate it. For forty years Mendel's shaft lay unnoticed, sides caved in and the opening obscured by weeds. Then simultaneously in three different places in Europe Mendel's law was "rediscovered," and the fact of its fundamental importance in a better understanding of heredity was heralded to a breathless scientific world.

Perhaps Mendel's monastery was the prototype of an institute for research in genetics. I am more inclined to think he ran a sort of farm and was moved by the same spirit as a good experiment station worker of to-day. I think he probably liked peas, both round and square, and had a genuine scientific curiosity and an inquiring mind. He may even have had in mind the simple matter of developing a superior round pea in order to discourage as much as possible those of his brethren who still clung tenaciously to the vulgar belief that the knife was a more effective conveyor of peas than the fork. Or maybe he merely said to himself, "I wonder, when I cross these round and square peas, if they will all stay on or fall off of a knife." Now whatever Mendel had in mind, his research, though lost for forty years, is quite universally accepted as fundamental. Any research institute for plant genetics would gladly enrol "Mendel's laws" in its own historical record. It is equally true that Mendel's results were of immense practical value, not merely in the certainty of growing ideally poised and superbly balanced square ones, but everywhere and otherwise in the field of plant and animal breeding.

Facts of fundamental importance may lie in the path of any digger after the truth. He may be digging for gold, platinum or precious stones, for Indian arrow-heads, for prehistoric bones, or he may merely be digging potatoes. It can perhaps be claimed that the importance of research contributions is roughly proportional to the depth at which they are found, that conditions and circumstances greatly influence contemporary judgment. Mendel died entirely unconscious of the value which history places on his discovery. Researches heralded as epic sometimes turn out to be investigational "duds." We have recently witnessed such a one with a tragic ending. The significance of facts is frequently not appreciated until their relations with other facts are worked out by succeeding investigators. Science grows largely by the small accretions of apparently insignificant contributions, each one fundamental to its predecessor. Milestones may be marked by exceptional finds or, again, merely by the addition of an apparently insignificant fact which fortunately completes a puzzling design.

The conclusions which I am forced to draw then are: first, that any attempt to exclude fundamental problems from applied science not merely ignores dictionary definitions, but, what is far worse, gives a distorted picture of the nature of problems in science, of methods of research and the history of its growth; second, that fundamental results of the most fundamental importance are not at all incompatible with research in applied fields, provided the investigator keeps on digging at the bottom of his shaft and not merely enlarging its entrance.

II

Is there anything really grotesque in the idea of a state agricultural experiment station functioning as an institute for fundamental research? I can not see that there is. More than that, I can not conceive of an agricultural experiment station which is living up to its duties and responsibilities which is not engaged in at least some problems of fundamental research. Experiment stations have justly earned a reputation for practical research; they have established beyond doubt a devotion to service in the improvement of agriculture. Are they also actually engaged in fundamental research? My observation is that some of them are to a high degree, many to perhaps a limited degree, and to some perhaps such research is entirely foreign.

I have already taken considerable pains to establish to the best of my ability the fact that fundamentalism in research is merely relative. A similar relativity is also observable in the work of our forty-eight or more agricultural experiment stations and needs little or no amplification. Some experiment stations are more fundamental than others. Our experiment stations as a whole are not regarded in the world of science as institutions largely devoted to fundamental research, and rightly so. Perhaps this is as it should be, and then, again, perhaps it is not.

How do we get this reputation for shallow rather than fundamental research? Too frequently our investigators, after sinking their shaft and bringing out ore of practical value, stop digging at the bottom and merely enlarge the opening. This frequently results in a maximum amount of practical results at minimum effort and expense—like open pit mining.

As an illustration, take new and better varieties of wheat. Our first shafts sank to the level of merely empirical testing of known varieties and more or less careful selection. We expanded the opening by the importation of varieties from all parts of the world. Everybody was testing varieties and writing voluminous bulletins filled with a few facts surrounded by oceans of useless information or misinformation. Then came a few deep diggers who made empirical crosses, with little or no knowledge of what was happening in these matings of strange varieties. From crosses without number and plots in endless array, here and there emerged new and valuable wheats, and the opening was again enlarged by scores of imitators in as many experiment stations. Desirable varieties shot forth like meteors, only to disappear under the cloud blanket of our ignorance of pure lines and genetics. Then came a third group of fundamentalists equipped with knowledge of Mendelism, mutation and other newer instruments for digging. Intelligence in crossing and a greater knowledge of how to produce specifically desired varieties were the results of this new series of shafts. Again we find the army of open pit investigators enlarging the entrance on this new level, with their endless correlations in inheritance of this, that and the other character to this, that and the other habit. And in the meantime the true fundamentalist is again sinking a new shaft. It seeks the chromosome in the wheat cell; it demands a knowledge of the intimate structure of the chromosome and the relation of its parts to the character factors in heredity. Fundamental, indeed! and getting fundamentaler! The wheat breeder is now talking a language no more intelligible to the farmer—and perhaps to his station director—than Greek or Sanskrit.

And how about it, Mr. Director? Is this practical? Can you stand for it? Are you going to get results? Well, who knows when you will get results in fundamental research? Research is an exploration of the unknown. Your investigator may come home with a valuable side of the bacon of achievement, or on the other hand he may lose his airship and most of his men and equipment. But of such stuff is fundamental research made! Have you faith in it? Are you convinced that digging deep solves, in the end, more problems in a sounder fashion? How great is your faith? How strong are your men? How well are they equipped? Do you give them hearty support? Or are they a little apprehensive that you will choke off the air supply and leave them to suffocate far below the surface? Are you seeing that they cooperate with each other for mutual protection and help? Has your plant breeder ample support in pathology, physiology, soils and biochemistry, or is he way down there all alone with only one life-line?

If you lack that faith, you would better stay near the top and keep your whole staff there. Mining was always a dangerous business, and fundamental research is equally hazardous. And, after all, hole amplifiers and open pit miners are useful citizens of the scientific brotherhood. Sometimes a shallow digger who covers much ground finds the top of a new and valuable ore vein.

Now, of course, no experiment station or, for that matter, any other institution for fundamental research, could possibly afford to sink deep shafts into every problem laid at its doors by the agricultural public. Every director knows that these problems are myriad in number, each one backed by insistent demands and earnest clienteles. Our resources, though generous indeed, must be carefully husbanded. You must have competent investigators for any deep digging, and their number is even as the proverbial dental equipment of the domestic fowl. Such researches demand modern equipment and ample support. It is quite obvious that you will have to discriminate. Here and there you will concentrate, perhaps, on a few deep shafts, but not in every department. But if we are to make real progress, some one will have to attack the bottom of the shaft.

Your state and your station may be eminently suited for an attack upon a specific problem of vital economic importance to other states as well as to your own. A deep shaft in that field will yield truly fundamental results, if not actually in a more basic solution of the problem, at least in minor or even negative results of value. And do not overlook the morale which ideals of courageous and serious research build up in your whole staff of investigators.

No experiment station that encourages shallow digging or a gleaning process in its research to the exclusion of fearless fundamental investigations, no experiment station that demands of its staff that all of its results be expressed or understandable in the simplest terms of extension circulars, is worthy of the name of an institute of fundamental research or, for that matter, of the name of a real experiment station. Your courage needs only the faith that below the level of our present knowledge lie facts of ultimate value in the application as well as the theory of scientific agriculture.

Quality of research, not quantity, determines the status of an experiment station in the brotherhood of institutes for fundamental research. Not the number of shallow and wide open pits but the depth of its deepest shaft. One deep shaft may bring in a gushing oil well where scores of shallow pits remain dry.

And make no mistake about it—such shafts are steadily being sunk in experiment stations all over our land. They are ample justification for the inclusion of agricultural experiment stations among the institutes of fundamental research.

Our greatest experiment station, the U. S. Department of Agriculture—and probably one of the greatest institutes for fundamental research in the world—is honeycombed with shafts of basic research. The field of bacteriology owes a tremendous debt to the outstanding researches of the late Erwin F. Smith and his coworkers. Can there be any question of the fundamental nature of those investigations? The work of the Wisconsin Experiment Station in its fundamental attack on the relation of temperature to plant diseases would reflect credit on any institute for fundamental research. The Minnesota station, cooperating with the Cereal Office of the U. S. Department of Agriculture, has sunk deep the shaft of physiologic forms in the study of rusts of wheat. The minute and difficultly measured differences in infection behavior of rusts from various sources have required years of patient research, apparently far removed from the field of application. Yet these results have not only proved of primary importance in the breeding of wheats for rust resistance, but they have also brought about a profound change in the fundamental methods of attack in the investigations of plant diseases all over the world. The investigation of any plant disease to-day is incomplete without a knowledge of the possible physiologic forms of its pathogene. Varietal resistance to disease becomes more intelligible and hence more easily obtainable because of this shaft which has opened up the rich ore body of physiologic forms. Small wonder, therefore, that in May, 1928, at Copenhagen, Denmark, the Emil Christian Hansen prize and medal was awarded to a Minnesota Experiment Station investigator "in appreciation of the pioneer work accomplished by him in developing new ideas and methods for investigating the rust problem, methods applicable not only to the study of the wheat rusts but to the investigation of diseases due to the fungus parasites in general."

This is not the place to attempt anything like a true picture of the amount of fundamental research going on in our experiment stations. That it exists in large amounts is quite obvious to any careful observer. I can think of no finer tribute to these great institutions of service and science than such a picture carefully and faithfully portrayed by a discriminating survey. I, for one, shall be disappointed if such a picture does not emerge from the forthcoming survey of Land-Grant Colleges.

But directors of experiment stations who have only the faith in fundamental research are men of little faith. They have only the half of it. The other half is an unquestioned faith in your investigator. His is the lonely and hazardous field of exploration. He it is who is in the unknown forest seeking the way out. Here a deer trail lures him from his path, there a fallen tree makes necessary a detour. Heavy underbrush obscures the trail, and he must quickly follow such judgment as he may possess to determine his direction. Clues and "hunches" necessitate numerous unforeseen exploratory excursions. Trails may end in dense thickets, impassable bogs or steep precipices, and he must retrace his steps. Can you, Mr. Director, swing in the swivel chair of your office and tell him just where to go and what to do? Shall he ask you whether he must turn to the left or right around that fallen tree? Can you show him the way up the steep cliff? If so, you have no need for an investigator. You merely need a timber cruiser or a hired man.

Are you going to make this busy seeker for the trail report every move, every beaver run, every swamp he searches in his quest for the trail? Can he possibly make a detailed project of his every future move? And how much it will cost? Can he possibly tell you beforehand where he is going and what he is going to do? If he is a real and honest investigator, he will say that he doesn't know. He can not possibly foresee all of the obstacles. He is a searcher in the unknown.

True, he needs to have in mind a general project of his search. He must have a definite trail in mind. He is not merely camping or fishing for pleasure. He seeks a northwest passage and he would be hopelessly lost without a tentative chart of his proposed travels. But once started in the woods, he is on his own. His must be the decisions, his the responsibility. All that is left for the director is faith in his emissary. Of course, if he's gone too long and probably lost, a relief expedition may be necessary, or he may come back for larger supplies and more men. Then must the director decide the advisability of continuing or abandoning the search—of renewing or withdrawing his faith in his investigator. I take it that a station director, like the director of any other institute for fundamental research, needs an inexhaustible store of faith. And please don't forget that such faith can be quite tangibly expressed on the payroll in figures that are concrete demonstrations of your appreciation of the importance of this man and his work. For "faith without works is dead!"

It was a poet and not a scientist who so charmingly advocated safe and hopeless mediocrity: "Be not the first by whom the new is tried nor yet the last to lay the old aside." Our "jazzy" but withal discriminating youth of to-day repudiate that advice when they slangily reply, "We'll try anything once!"

I believe an agricultural experiment station should engage in solving the practical problems of agriculture; I believe that it should render real service to agriculture and thus to the whole people; I believe that it should extend its knowledge to all the people by every legitimate method; and last, but not by any means least, I believe that it should contribute generously to the investigation of those deeper problems which lie at the bottom of our present knowledge in every field of agriculture. Then, indeed, will it be able to render the greatest possible service to agriculture through a more profound knowledge and a more fundamental solution of practical problems. Then will it be secure in its conviction that its numerous and ever-branching streams of extension activity flow from a deeper and clearer source of knowledge. Then, and only then, may it lay claim to its proper title as an Institute for Fundamental Research in Rural Affairs.

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SCIENTIFIC EVENTS

THE STUDY OF AGRICULTURE IN NEW YORK STATE

GOVERNOR FRANKLIN ROOSEVELT sent to the New York State Legislature on March 17 three bills, the last of his farm relief measures, based upon the recommendations of his Advisory Agricultural Commission.

The bills submitted call for appropriations totaling $168,530 for investigation into problems of interest to the farmer, varying from crop adaptation and soil conditions to cooperative marketing and rural government. The work would be done by three state agricultural institutions.