Research and Politics

Richard B. Goldschmidt
University of California, Berkeley

RESEARCH, AT LEAST IN THE SCIENCES, proceeded in the 19th century more or less apart from the nation's life. If the laboratories were not exactly ivory towers, they were at least sheltered oases, far from the noise of the market place. In the European nations scientists particularly were considered a pleasant and honored asset to the community, were respected, sometimes a little ridiculed, but generally left alone. Their discoveries were accepted, sometimes admired, and, if of practical importance, taken over and used by the applied sciences. Nobody asked a scientist why he attacked a special problem. Nobody asked him what practical good could come out of his results. It was accepted as a truism that the exploration of nature, the search for laws beneath the phenomena of nature, grew from a natural longing of the human mind and represented a goal in itself which did not need further justification. This appraisal of science had a corollary, namely, that science, being solely bent upon the discovery of truth, must be free from outside interference by society and the state, although of course governed by law. When Imperial Germany, a reactionary monarchy, led by the dyed-in-the-wool Junker, Count Bismarck, adopted a new constitution in 1871, a paragraph of this constitution read: "Science and its teachings are free." There were indeed very few instances in the European countries of the 19th century—that is, in the period in which the basis of modern science was laid—of interference with the freedom of science by governments, however reactionary, though there have always been organized groups of different colors who clamored for such interference. This was true even where the universities were completely controlled by government and the professors were civil servants. It happened, to take an example, that under the rule of the Catholic Centrist party in Bavaria, professors were denounced in the Diet for teaching evolution. But they were not prevented from continuing to do so, since even the worst reactionaries did not go so far as to abolish the freedom of teaching.

In our country, conditions were somewhat different. The Constitution does guarantee the freedom of speech, but science and its teaching are not mentioned. There are no federal universities. In endowed universities, only public opinion can prevent the trustees from setting up rules infringing upon the freedom of science. In state universities the regents, the governors, and the assemblies may, if public opinion permits, strangle the freedom of science and its teaching. Everybody knows that such things have happened. There are still states in which the teaching of evolution is forbidden. There are events happening from time to time in this or that university which force the watchdogs of liberty to boycott the offending school. But greatly to the credit of our democracy, these are rare exceptions and as a whole science and its teaching have remained free.

Does this mean that no future danger exists and that we may as well forget the problem of freedom of science? Twenty years ago such complacency would not have been too harmful. But since then the situation has been completely changed. First, we have seen the rise of dictatorships (and unfortunately thus far only two have fallen)—dictatorships which attempt to control not only the minds of their subjects but also to control what science is permitted to do and to think. Do not say this cannot happen here. It *will* not happen, I trust, but it *can* happen. Second, the applications of science to the industrial field and consequently to national defense have reached a level at which restrictions to science in some fields cannot in the general interest be avoided. This creates the problem of how to take unavoidable measures of caution without infringing upon the basic freedom of science. Third, even in the oldest and most solidly founded democracies an economy has developed which is dependent upon and interlaced with the economies of the entire world. Disappearance of distances has begun to work against traditional individualism and in favor of stronger centralization of power. Whether we like it or not, the trend everywhere is away from individualism and uncontrolled economy toward collectivism. Whether we like it or not, this tendency is bound to increase. Stronger central power means centralized control of the purse strings. This increases the danger of political control of how the money is spent. Where there are federal or state allotments to science, the control will certainly not be in the hands of the scientists. Fourth, events in science have taken place which lead more and more to the introduction of organized teamwork in research. The step from there to planned control and then control by the politicians of science is a very small one indeed. I propose to discuss mainly the first and the last of the four developments I have enumerated.

The experience of science in dictatorships is of greater significance to us than we might think. Even if there is no danger in the near future that similar tragic developments could happen here, it is instructive and important to see the pattern from which such things form and develop. You all know how the Nazis abused the science of genetics to further their criminal racial doctrines and how they made anthropology and eugenics, ethnology, and prehistory and even history in Germany a perversion and abomination. But these are things of the past—are they really?—and therefore I prefer to put in the foreground what is happening today in Soviet biology.

The whole story, part of which I had an opportunity to witness, is very remarkable.

Up to the end of the first World War genetics was practically nonexistent in Russia, though Russian biology was on a very high level. It was H. J. Muller who brought the first *Drosophila* stocks to USSR. Calvin F. Bridges soon afterwards served as a professor of genetics in Leningrad. Russian biologists took to the new science with enthusiasm and soon centers of research sprang up in which excellent work was done. In fundamental genetics it was first the zoologist, my late friend Philipchenko, who organized a fine group center in animal and plant genetics and did important work himself. The second great center developed in Moscow where Russia's leading zoologist of that time, the great scholar, observer, experimentalist, and thinker, N. K. Koltzoff (one of my oldest friends), realized the overwhelming importance of the new science and organized genetical and cytological departments in his laboratory of experimental biology. These leaders were followed by a host of excellent men and one has only to look at the volumes of the *Biological Journal* and other Soviet periodicals of the time between 1920 and 1930 to witness the brilliant upsurge of genetical and cytogenetical work in USSR. Parallel with this development went an enthusiastic realization of the importance of genetics for plant and animal breeding. Here the recognized leader was Vavilov. Though originally trained as a geneticist, he never did any outstanding work in fundamental genetics. His interest was in the application of genetical work to plant breeding. Thus, he devoted all his explosive energy to the building of an organization of modern experimental stations all over the country in which genetical and cytogenetical methods were used for the improvement of crop plants. When I visited his laboratory in 1929, he had a staff of about 2,000 employees, and the government furnished ample means for his work. His own line of thought, which earned for him leadership in his field, led to the recognition of the genic reserve available in old cultural varieties and their surviving ancestors. In a continual series of collecting expeditions he searched for such forms and brought them home to test their usefulness. This was work of a long view, and it might have led to important practical results had it been continued long enough. How much in the way of practical results it did achieve is probably known only to the agronomic specialist.

I had the pleasure of seeing all this development of genetics near its height in Russia. At Leningrad in 1929 an all-Russian genetics congress was held at which three guests of honor from abroad were present: Erwin Baur, the plant geneticist, Harry Federley, the cytogeneticist, and myself. All of us, none sympathetic to the political regime in USSR, were unanimous in praising what we saw at this congress. There were innumerable papers and lively discussions. There were huge plenary sessions, with Vavilov and Philipchenko presiding, at which Soviet politicians of the highest rank praised the work of the Russian geneticists. There was a scientific enthusiasm among the old leaders as well as among the brilliant young scientists such as none of the three guests had ever witnessed before. There was an exhibition of genetical specimens and charts which was admirable, quantitatively and qualitatively. Fundamental geneticists at that time were discussing most heatedly Serebrovsky's ideas, and applied geneticists, discussing their wheat-breeding experiments, were equally represented in the very large gathering. Inside and outside the lecture halls the meeting proceeded smoothly and in a most friendly atmosphere.

This does not mean that the political leaders of the USSR did not watch our science. Actually, I was told that studies in human inheritance were fraught with danger. One leading biologist, who in his search for the hereditary basis of genius studied the pedigree of Russia's most beloved poet, Pushkin, had mentioned correctly that the poet hailed from a family of petty nobility. This biologist got into trouble with the authorities. By chance, I saw a little more of the official attitude. One day, walking along the street with my friend Philipchenko, I saw in front of a movie house a large poster of "Salamandra" decorated with pictures of this harmless animal. My surprised question was answered by my friend with an invitation to see the film. This we did, and my friend interpreted the text. To understand the implications of this propaganda film, which was featured all over Russia, we must turn back to European biology in the first decade after the rediscovery of Mendel's laws.

The Lamarckian doctrine of the inheritance of acquired characters had been completely dropped by most biologists since the time of Weismann's piercing analysis. It had hardly any adherents, except in France, where the doctrine lingered on for a long time, and in the science of paleontology. The rise of genetics, the factual basis of which is irreconcilable with the tenets of Lamarckism, had given the coup de grâce to this doctrine.

In the first decade of the century, a young Viennese zoologist, a brilliant speaker, and clever, popular writer, Kammerer, stirred European biology with a long and voluminous series of papers in which he claimed to have found proof for the inheritance of acquired characters. Two sets of his experiments were especially exciting. In one he compared the black viviparous alpine salamander (newt) with the black- and yellow-spotted oviparous salamander of the lowlands. He claimed that by breeding the black one on yellow background and the spotted one on dark background he could transform appearance as well as reproduction from one type to the other. The new induced type was claimed to be hereditary, even to Mendelize if crossed to the original form. The second experiment was done with the midwife toad, the male of which seizes the egg strings laid by the female, winds them around his hindlegs, and carries them about until hatching time. An exception to the rule, this male is devoid of the thick pigmented thumb pads which other frog and toad males need for the standard type of copulation. Kammerer claimed that he could change the mating habits of the midwife toad to those of ordinary toads by breeding them under conditions used for other toads. Such males not only stopped "midwifery" but also developed the thumb pad and, again, this new induced character was said to be inherited and to Mendelize if crossed to the original form.

In the discussion of these claims, statements were found in Kammerer's papers which did not tally. There had not been sufficient time, according to his own records, for the generations he claimed to have bred. The explosion came when an American visitor, Noble, looked at the specimens exhibited in Kammerer's institute and found that the "induced" dark thumb pad was injected with India ink. Soon after this Kammerer, who was said to have become completely despondent, accepted an invitation to live in USSR. Nothing was heard of what he did there except that, soon after, he committed suicide.

Very likely there are few men left who knew Kammerer and who had seen his work in Vienna under his own guidance, so I should like to give you my interpretation of this much-discussed tragedy. I do not believe that Kammerer was an intentional forger. He was a very highstrung, decadent but brilliant man who spent his nights, after a day in the laboratory, composing symphonies. He was originally not a scientist, but what the Germans call an "Aquarianer," an amateur breeder of lower vertebrates. In this field he had an immense skill, and I believe that the data he presented upon the direct action of the environment are largely correct. (Some of them were actually anticipated a long time before in Weismann's laboratory by M. von Chauvin, who made the experiments for just the opposite reason.) He then conceived the idea that he could prove the inheritance of acquired characters and became so obsessed with this idea that he "improved" upon his records. I have reason to believe, from what I have seen in his laboratory, that he continued his experiments, which ended by the death of the specimens, by starting again with similar looking animals. His "Aquarianer" mind did not consider this wrong. He simply did not know what an experiment amounted to. In later years he probably became so absorbed with the necessity for proving his claims that he started inventing results or "doctoring" them. Though the actual results of all this amounted to falsification, I am not certain that he realized it and intended it. He probably was a nervous wreck in the end.

Why did the USSR invite Kammerer to take refuge there from "bourgeois persecution"? One of the strange features of the Soviet religion of dialectic materialism is that it postulates the inheritance of acquired characters as a dogma. The biologist is unable to understand this. One of the underlying ideas is probably that the masses are kept down by a bad environment and that therefore a good environment would make them equal to anyone. If this change is inherited, the future lies in inducing this hereditary improvement. But if this were true, it would follow that the old aristocracy, which had lived so long in the good environment, should be genetically supreme. Of course, one notion is as wrong as the other. Why, then, the dogma of the inheritance of acquired characters? Why can the believer in communism not assume that just as much genetic talent is found in the "suppressed masses" as in the "bourgeois" part of the population and that therefore the communist regime should pick out this talent and give it a chance to blossom? I shall never be able to understand why the latter idea should not be just as good communism as the former and why genuine communism needs the dogma of the inheritance of acquired characters.

This leads us back to the film "Salamandra," which turned out to be nothing but a propaganda film for the doctrine of the inheritance of acquired characters.

It uses the tragic figure of Kammerer, his salamanders, and mixed up with them, for the story, his midwife toads. The importance attached to the subject is revealed by the facts that none other than the then all-powerful Commissar for Education, the highly cultured and intelligent Lunacharsky, is the author of the film, that his wife plays the leading lady and that Lunacharsky, playing himself, appears in one scene. Leaving out the interwoven love story written to fit the beautiful Mme. Lunacharsky, the plot is this: In a central European University a young biologist (model Kammerer) is working. He is a great friend of the people and endowed with all the qualities of a Communist movie hero. Working with salamanders, he has succeeded in changing their color by action of the environment. One day the supreme glory is achieved; the effect is inherited. The bad man of the play, a priest, learns of this, comes to the conclusion that the discovery will spell an end to the power of the church and the privileged classes, and decides to act. He meets at night in the church (I recognized with surprise that these pictures were taken in the glorious double cathedral of Erfurt in Thuringia) with a young prince of the blood whom he had succeeded in having appointed as assistant to Kammerer. (This is obviously a typical job for a German prince!) Here in the dark sacristy the plot is hatched. The prince (or the priest?) proposes to Kammerer that he announce his glorious discovery at a formal University meeting, and the scientist gladly accepts. During the following night the priest and the prince enter Kammerer's laboratory, to which the prince has the key, since he poses as the scientist's devoted collaborator. They open the jar in which the proof specimen of salamander is kept in alcohol, and inject the specimen with ink. Then follows the scene at the University meeting. All the professors and the president appear in academic robes, the young scientist is introduced and makes a brilliant speech announcing the final proof for the inheritance of acquired characters. When the applause has ended the priest (or was it the assistant? I am quoting entirely from memory) steps up, opens the jar, takes out the salamander, and dips it into a jar of water. All the color runs out of the specimen. An immense uproar starts and Kammerer is ingloriously kicked out of the University as an imposter. Some time later, we see the poor young scholar walking the streets and begging with an experimental monkey which had followed him into misery. He is completely forgotten until one of his former Russian students arrives and tries to call on him. She succeeds in finding him, finally, completely down and out, in a miserable attic. She takes the train at once to Moscow and obtains an interview with Lunacharsky (this is the scene where he appears in person), who gives orders to save the victim of bourgeois persecution. Meanwhile, the character Kammerer has sunk so low that he decides to make an end of it. The very moment he tries to commit suicide, the Russian student returns with Lunacharsky's message and prevents him from taking his life. The last scene shows a train in which Kammerer and the Russian savior are riding east and a large streamer reads "To the land of liberty."

A young plant physiologist and agronomist also attended the congress at Leningrad. He was presumably completely ignorant of genetics but he had made a great and well-deserved reputation as an agronomist by the introduction of the method of vernalization which is said to have increased production of crops considerably. His name was Trofim Lysenko—for Russian biology the man of destiny. For some years after 1929 genetics still bloomed in Soviet Russia, especially when H. J. Muller lived there and trained a group of excellent young scientists. But soon rumors began to arise that something strange and untoward was happening in Soviet genetics. The rumors centered around the name of Lysenko. One heard of public debates taking place in which Lysenko attacked genetics as not only scientifically wrong but also not conducive to improvements in agricultural breeding. Simultaneously, he advanced claims of being able to improve plants by environmental action which is at once inherited. Heredity of acquired characters was once again the slogan. Rumors increased that Lysenko's power was in ascendency; they were borne out when Vavilov was prevented from accepting the presidency of the International Genetics Congress in Edinburgh in 1939 and when the expected Russian delegation did not turn up. (I personally think that it was a mistake to elect Vavilov president of that congress. As the honor was clearly intended to back him up in his fight against Lysenko, it appeared to be a political demonstration and as such had just the opposite effect from that intended.)

The first concrete information arrived about 10 years ago when an American monthly, *Science and Society*, published part of the speeches made in the first big debate between Lysenko and the geneticists. These speeches, pro and con, did not impress one favorably. Lysenko's statements of the theories with which he wanted to replace modern genetics sounded like confused nonsense. His antagonists did not come out with clear, simple statements of what is fact and what is interpretation in genetics. One had the uncomfortable feeling, when reading these reports, that Lysenko's opponents were already frightened and spoke cautiously, making unnecessary concessions. It

soon turned out that they had reason to be frightened when Vavilov was deposed and died mysteriously in a concentration camp during the war years, and when all work in plant breeding was taken over by Lysenko. Again one heard from time to time of Lysenko's ascendance, of the honors and titles given to him, among them that of *"Hero of the Soviet State"* and of president of the Lenin Academy of Agriculture. But there were still laboratories of genetics, though one heard that Koltzoff, not long before his death, was forbidden to do genetics work in his institute. Good publications were still appearing in genetics and cytogenetics, and some even reached us by the ordinary mails. During the last three or four years rumors came from Russia that Lysenko's star had passed its zenith and was setting. Then, suddenly last year papers all over the world reported that Lysenko had finally taken over *all* biology in the USSR, that teaching and research in genetics were forbidden, and that all those who opposed Lysenko were deprived of honors, or even jobs, or worse.

Were the rumors of Lysenko's sinking star wrong? A translation of Lysenko's speeches at the decisive meeting has just been published in New York. The complete frankness of Lysenko's statements leaves no doubt about what happened. He quotes a paragraph written by Academician Zhukovsky, stating that the number of theses in genetics is decreasing and that this might be due to fear of what the Lysenkoists may plan. Lysenko counters that the real explanation is that the faculties refuse to accept theses by "geneticists" of the Lysenko school. Simultaneously, he complains that such great Russian scholars as Schmalhausen do not quote him or his worshipped god, the fruit breeder Michurin, in their books. From these and similar statements, it becomes clear that all prominent Russian biologists, geneticists or not, have the same opinion about Lysenko's "discoveries" as we have. He cannot quote in his favor a single living Soviet scientist whose performance is established in the scientific world. The rumor clearly was true that Lysenko's star was sinking in Soviet "science." But the rumor overlooked one thing: that his star was rising among the professional politicians. For both reasons, it seems that he considered the time ripe for action. If he could not win by persuasion, he would win by a coup d'état; this he carried out in the best tradition. In his speech he said suavely that nobody is prevented from working in genetics, though he does not think much of this science. Then he allowed his opponents to say what they wanted. (Thus far, I have not seen translations of their futile speeches.) Then he rose for his final remarks. He began mildly by saying that among the question slips handed to him there was one asking whether his ideas had the official stamp of the party. He answered in the affirmative. I should have liked to see the faces of the scientists who heard this completely friendly and mellow statement. Translated into plain English, it would read correctly: "To all whom it may concern: Lysenko, or else!" Thus, he took over Russian biology exactly after the political pattern established in the satellite countries, by bloodless coup d'état, and we know that the "or else" has already begun.

These last speeches give a much better appraisal of Lysenko and what he stands for and what he is now permitted to enforce than do the earlier publications which have been published in English. He appears clearly in his political roles, which have been more important for his success than his biological claims. (As a corollary one should read the almost simultaneous pronouncements of his brain-truster and *advocatus diaboli,* Prezent.) There does not seem to be any doubt that Lysenko, though a clever politician, is honest and that he believes in his queer ideas, believes in them so religiously that he is willing to impose them by the sword. First of all, he is a fundamentalist. Just as the Christian fundamentalist believes in the divine origin and truth of every word of the Bible, so Lysenko believes in every word of his gods, Marx, Lenin, Stalin, and the Russian Burbank, Michurin. He can refute every argument finally by quoting from these sources. He has set up his rules of the scientific game according to what he considers to be the tenets of dialectic materialism. His views are materialistic and therefore good. All others are decreed idealistic and therefore bad. He does not realize that atomistic classic genetics is extremely materialistic, while his own views border on mysticism. Being a fundamentalist, he simply decides what is right and what is wrong, and there is no appeal. There is also no need for him to study what he condemns, because he knows a priori that it is wrong.

Next to being a fundamentalist, Lysenko is an extremely clever lawyer, almost a shyster. It is revealing to read how he sets up first a completely wrong description of Weismann's ideas and then ridicules his version of them; or how he fights pre-Mendelian ideas as the tenets of present day Mendelism; or how he recounts some technicalities which the nongeneticist cannot understand by quoting literally from Dubinin in order to ridicule genetical work in the eyes of the agronomists and politicians; or how he says on one page that chromosomes, Mendelian segregation, etc., are all figments of the imagination and on another page that he has never doubted the facts relating to all these things; or how he seizes (Prezent does this still better) upon mystical utterances of nongeneticists,

even of popular writers (Prezent quotes from the popularizer Francé, from Smuts, Bergson, Bertalanffi, for all of whose statements genetics is made responsible), to show up genetics as idealistic, which means to him, criminal.

A third part of Lysenko is his chauvinism. Nothing printed appears without mention of *Soviet* science, *Soviet* Darwinism, progressive *Soviet* scientists. The ignorant reader must believe that what Lysenko does and says is 100-percent Soviet Russian, while all his opponents are traitors, imitators of foreign ideas, even promoters of capitalism and similar crimes. He does not mind attacking a Russian scholar of the pre-Soviet group for his heresies, then setting him up on another page as a Soviet scientist, when he defends that man's priority of a good idea (which Lysenko thinks, however, to be bad). I will not insist here that the incense he burns to Stalin is of a type which one formerly knew only from Nazi writers towards Hitler. This is probably the outflow of his religious fanaticism, though it sounds like repulsive adulation. Lysenko is also an able rabble-rouser. The way he presents Dubinin's work on inversions in populations of *Drosophila* is a masterpiece of this type of oratory.

Fourthly, Lysenko is a very skillful politician who knows very well the rules of the game and how and when to act.

But now let us turn to his so-called work. Not, of course, his early physiological work on vernalization but his recent work which convinced him that he is the Allah and the Mohammed of the new Soviet biology which must supplant bourgeois biology, especially genetics, even by fire and sword where mere persuasion fails. One can probably reconstruct Lysenko's development. Being successful as a plant physiologist in improving crops by changing environmental factors and not being trained in genetics, the official governmental support of heredity of acquired characters easily impressed him and suggested a way of helping the breeders with much simpler methods than those of genetics. He found support in the works of his hero, Michurin, who had successfully produced new varieties of fruit and given his own Lamarckian interpretation. Thus, Lysenko set out to develop a Lamarckian theory of heredity and to prove it experimentally. One can consider his biological activities from three aspects: his criticism of the facts and conclusions of genetics, for which he uses the rather silly term Mendelism-Morganism; his own theoretical explanations of heredity; and his alleged proofs of his views.

Lysenko does not think much of genetics and refuses to accept such simple facts as the uniformity of F_1, the numerical rules of segregation, or the chance assortment of chromosomes in meiosis, not to speak of the more advanced facts of genetics. He approvingly quotes Michurin's words that Mendelism ". . . contradicts natural truth in nature, before which no artful structure reared out of wrongly understood phenomena can stand up" and thinks that such mumbo-jumbo settles Mendelism. (He also quotes approvingly Michurin's modest statement that Michurin bequeathes his conclusions "to naturalists of coming centuries and millenniums." (One hears Hitler's voice!) Such elementary facts as the chance assortment of chromosomes Lysenko considers to be mystical nonsense. It is not an overstatement that almost every thing he says about genetics and cytogenetics in his three major translated books and speeches exhibits a complete ignorance of the subject. How is it possible that he has never taken the trouble to see with his own eyes what thousands of students all over the world are unfailingly shown in the laboratory courses in genetics and cytology, year after year? Is this bad will or obscurantism? When I read, for example, that the most important thing for Soviet biologists to do is to read "over again and again" Michurin's works, the only answer I can find is that Lysenko does not want to know the facts which he is attacking because he is a fundamentalist believer, for whom everything is decided in advance. Someone should take the trouble to extract all his statements on modern genetics, so as to have them side by side. The collection would make a stone weep.

Lysenko's second action is the development of his own ideas. Environment is everything and the operation of the environment upon the body changes it permanently. How this comes about is repeatedly stated, e.g. "Heredity is the effect of the concentration of the action of external conditions assimilated by the organism in a series of preceding generations" or, on another page, "Heredity is the property of a living body to require definite conditions for its life and development and to respond in a definite way to various conditions"; then, concerning changes in heredity: "Changes in heredity are as a rule a result of the organism's development under external conditions which, to some extent or other, do not correspond to the natural requirements of the given organic form" or "The extent of hereditary transmission of alterations depends on the extent to which the substances of the altered section of the body join in the process which leads to the formation of reproductive sexual or vegetative cells. Once we know how the heredity of an organism is built up, we can change it in a definite direction by creating definite conditions at a definite moment in the development of an organism" and so forth *ad nauseam*. The same man who produces this

mystical and empty phraseology (though it can be called by a more appropriate name) derides modern genetics, with its clear-cut verifiable facts and wonderful parallelism of experimental results with cytological facts, both easily verified by any beginner; he calls modern genetics "idealistic, mystical nonsense," and his brain-truster, Prezent, even denounces genetics as made to order for the benefit of the capitalist employers of the geneticists; and, *horribile dictu,* he believes fanatically in this evident nonsense. I repeat that not a single USSR biologist of renown, all of them probably good Communists, is willing to take seriously Lysenko's so-called biology. Lysenko had to pack the Lenin Academy with his cohorts, as he naively states in his last paper, to create an audience for his theories. As for those who do not follow him there is a sinister meaning in a list of errata appended to a German translation of his speeches. It reads: "p. 24, first §, instead of ". . . straight lined action of *our Soviet* Cytogeneticist-Morganists" read: "straight lined action of *our home-grown* Cytogeneticists-Morganists" (Italics mine).

Biologists, however, are interested mostly in the facts which Lysenko claims to have found in proof of the inheritance of acquired characters. One should expect Lysenko to put these facts in the foreground of his discussion when he wants to convince his fellow geneticists. Actually, reports on facts play a rather modest role in his accessible writings, and they are always presented in a vague, elusive way. In his decisive discussion of his views, just before he staged his *coup d'état,* he tells us rather vaguely, as before, that he can change hereditary winter grain rapidly and at will into hereditary summer grain simply by changing the time of planting. He must certainly be aware that no one will believe this story unless it can be shown that his experiments have been made with all the precautions, controls, and checks which a knowledge of genetics requires. But he quietly skims over this subject. Being a believer and not a critical student, he obviously expects others to believe him, without question. But no one will believe his statements before seeing exact data on experimental procedure, controls, and statistical checks. At present, it seems that Lysenko considers another line of his work to be most decisive. Following in the footsteps of Michurin, he and his school have grafted on a large scale and claim to have produced many "graft hybrids," which are supposed to show that the "juices" which flow from stock to scion and vice versa can change the scion (or stock) hereditarily into the type of the other. We can judge what he believes to be his greatest success, his star witness, because he staged a dramatic demonstration toward the end of his final remarks at the 1948 meeting mentioned above. He directly addressed the Academician Zhukovsky, who had doubted (like everybody else) the reality of the existence of vegetative hybrids, and declared in a triumphant tone that he now had the pleasure of demonstrating them. A tomato strain with full leaves and red oblong fruits was used as a stock to which a scion was grafted from a strain with pinnate leaves and yellowish white fruit. Seed was taken from fruits growing on the stock and on the scion and most behaved according to their origins. But a few plants taken from the seeds from the stock had pinnate leaves and yellow fruit. This, then, was his great result, produced dramatically at the height of his discussion. I am sure that the agronomists present were deeply impressed. But what did the geneticists think? Before Lysenko's unfounded interpretation could be discussed, one might ask, first, whether no genetical facts are known in explanation of the result, assuming that the experiment was made with genetically pure strains. It has been known ever since the classic work of Winkler and Baur that chimeras are produced in definite ways after grafting. This means that the tissues of stock and scion can grow together into a whole. It was shown further that the subepidermal layer, from which the sex cells originate, can enter such a chimera from one or the other form used. The seed then will belong to the form which furnished the subepidermal layer, as Winkler proved by the chromosome counts in his nightshade-tomato grafts. (Winkler, by the way, produced also a single real graft hybrid or burdo, not by "juices" but by vegetative union of cells, as again proved by the chromosomes.) Obviously, the pinnate, yellow-producing seeds of Lysenko were pure descendants of the scion, via subepidermal ingrowth into the stock. If this fact-founded interpretation cannot be disproved by real and decisive facts, Lysenko's top demonstration collapses. Thus far, I have not seen any hint that Lysenko was even aware of a simple genetical explanation for his results. [In his book, translated and published in 1946, Lysenko discusses chimeras (p. 57). In these paragraphs he surpasses himself in ignorance, if not actual ill will.]

It is not necessary to go on with a discussion of Lysenko's so-called factual material, when his own star-witness experiments are found wanting in every respect. To base a revolution in biology upon such material is certainly a feat of great optimism, though not of much self-criticism. *Parturiunt montes nascetur ridiculus mus.*

Pseudobiological literature of all times is full of books by philosophers, statesmen, theologians, and cranks who want to replace facts and laws found by the hard work of the active biologists with their own

pet ideas or creeds. Lysenko would certainly join this group, which always finds followers among laymen, if he lived in the Western World, and his doings would remain a curiosity on a library shelf. However, his case, otherwise completely uninteresting, has become entirely significant by the fact that he has succeeded in persuading the high command of the party, including, as it seems, the dictator himself, that his, Lysenko's, line is first, truly Marxian, second, of greatest importance for the economy of his country and full of practical promise, and, third, 100 percent Soviet Russian; further, that scientific genetics is anti-Marxian, capitalist-employed, a foreign importation, wrong in fact and theory and a hindrance to agricultural progress. Thus far, this is a local Russian affair. But when, in consequence, the politicians have given him the power to destroy a whole science of greatest achievement and to persecute its exponents, a situation has developed which must awaken the entire world. We know that Communism is a creed and that its believers everywhere will follow the party line religiously. We know that in the Eastern Zone of Germany, Lysenkoism is already being introduced in the high school curriculum. We know that the red-tainted Japanese youth is already flirting with Lysenkoism and that a large scale discussion of it is going on in the Japanese press. Thus, the freedom of science is in danger everywhere, and the local affair becomes one of universal concern.

I do not want to imply that we are in danger of having a Lysenko appear in our midst. But there are different degrees of such things. This brings me back to the fourth recent development in science which I recounted in the introduction to this address, when I said that only small steps lead from organized teamwork to professional planners and finally to political control. I cannot help saying that I have followed with serious misgivings the increasing trend towards planned and organized science, though realizing its need in certain fields. Formerly, a scientist thought out or encountered a problem and started working on it. As often as not he found something in the course of the work which led him in a new direction. An occasional observation, which most others would have overlooked, led the born researcher into new fields, an unexpected flash of an idea opened new vistas and led to solutions or to more problems. All great ideas and all great discoveries in the realm of pure science have come into existence this way.

But now a man does not work on some subject or problem. He has a "project." A plan has been laid out, even worked out in all detail, a staff has been brought together and each one has been assigned his duty. An organization has approved the plan and furnished the funds; in return it expects progress reports, visible and quick results, and no deviation from the plan agreed upon. Everybody is happy to have a "project," and only Minerva covers her face and sends the owl away to catch mice.

I realize certainly that there are types of work which should be handled as organized "projects." If you want to prepare 200 stereoisomers of some organic compound and test their action as insecticides, a project is in order. If you want to eradicate a certain mosquito in a certain place, go and organize it. But how a major discovery or idea can come from a project I am unable to understand. This, however, is not what I want to discuss. I want, rather, to point to the danger to the freedom of science which lurks behind this way of making science. The danger will come from the men who are attracted to such a type of scientific big business. The thinker, the blaster of new paths, the keen observer, the man of intuition whose thinking is ahead of his time, will not flock to the big Government-financed and -sponsored projects. Sooner or later leadership will fall to the university politician, the promoter, the men who make the headlines—headlines not in the history of discovery but in the press. Second-raters will attain the power that goes with the big funds, and then the moment of danger arrives. They will favor what they like and understand, suppress what is beyond their vision. Being not too intelligent, they will fall prey to the flatterer, and will always go along with the latest scientific fashion or even the doubtful schemes of fanatics or reactionaries, and certainly always with well-entrenched schools. They will easily find the ear of the politicians who run the funds, for both talk the same language. At this point the setting is ready for a Lysenko type. Though our political system will not give him a chance to act as savagely as is possible in Russia, he could do enormous damage to the progress of science and the freedom of research if not checked in time. This sounds very pessimistic, but human nature is the same everywhere, fanatical activists are available everywhere if not kept in check, and men who believe in "politics as usual" are not only more numerous than men of original ideas but are also more selfish and ruthless. Thus, I believe that the increasing financial support of research, especially by government and political agencies, tending to flow into the channels of organized research, is fraught with the danger of bossism in science, with the danger of subsidizing mediocrity, and in the end with a threat to the freedom of science and its teaching. This is not to say that I am opposed to government funds' being set aside for fundamental research. This is a need of our time, a necessity. But precautions should be taken

and a watchful eye should be kept to prevent such funds from working to the detriment of real science. It is the young generation, who will profit from the incoming funds, who should also be alerted against the danger that politicians, both those within and those outside the universities, will take over science. The young researcher must insist upon the right to think for himself, to plan for himself, to make his own mistakes, and to be happy over an unplanned, unforeseen discovery. Real progress in science has always been made and will always be made by the free mind, left to its own working under a system where science is free.

Based upon a presidential address given at a meeting of the Phi Sigma Society held December 30, 1948 at the University of New Mexico.

Age Determination by Radiocarbon Content: World-Wide Assay of Natural Radiocarbon[1]

W. F. Libby, E. C. Anderson,[2] and J. R. Arnold

Institute for Nuclear Studies, University of Chicago

SOME TIME AGO THE OCCURRENCE of radiocarbon in living matter and dissolved ocean carbonate was reported (*1, 2, 4, 5*) as a result of researches on sewage methane gas from the City of Baltimore. The postulated origin (*5*)—cosmic ray neutrons reacting with atmospheric nitrogen to give radiocarbon at high altitudes—clearly predicted that all material in the life cycle and all material exchangeable with atmospheric carbon dioxide, such as carbonate dissolved in sea water, would be radioactive. The long half-life of radiocarbon, 5,720 ± 47 years (*3*), further seemed to ensure that the mixing processes would have ample time to distribute the radiocarbon uniformly throughout the world.

Since completing the first tests using isotopic enrichment with Dr. Grosse and his associates, an improvement in counting technique has enabled us to investigate materials without enrichment to about 5–10% error. The samples are counted in the form of elementary carbon in a screen wall counter (*6*). Six grams of carbon are spread uniformly over an area of 300 cm², to give an "infinitely thick" layer; about 5.9% of the disintegrations register in this arrangement. The background of the counter has been reduced from 150 cpm (when shielded by 2″ of lead) to 10 cpm by means of anti-coincidence shielding and the addition of a 4″ iron liner inside the lead shield. The technique will be described in detail elsewhere. A world-wide assay has been completed, and the uniformity apparently established. The data are presented in Table 1.

TABLE 1

WORLD-WIDE ASSAY OF RADIOCARBON

Sample	Assay (cpm/gm of carbon)
Baltimore sewage methane (*1, 2*)	10.5 ± 1
Ironwood from Marshall Islands	11.5 ± 0.6
" " " "	12.6 ± 1.0
Elmwood, Chicago Campus	12.7 ± 0.8
" " "	11.9 ± 0.7
Pine, Mt. Wilson, New Mexico, (10.000′ altitude)	12.5 ± 0.6
Bolivian wood	13.5 ± 0.6
" "	11.3 ± 0.8
Ceylon wood	12.5 ± 0.7
Tierra del Fuego wood	12.8 ± 0.5
Panamanian wood	13.0 ± 0.5
Palestinian wood	12.4 ± 0.4
Swedish wood	12.6 ± 0.5
New South Wales wood	13.3 ± 0.4
North African wood	11.9 ± 0.4
Weighted average	12.5 ± 0.2
Sea shell, Florida west coast	13.3 ± 0.5
" " " " "	14.9 ± 0.7
" " " " "	14.6 ± 0.5
Weighted average	14.1 ± 0.3
Seal oil, Antarctic	10.4 ± 0.7

The numbers quoted are intended to be absolute disintegration rates per gram of carbon. It must be

[1] This research was supported in part by a grant from the Viking Fund, Inc., and the authors want to express their gratitude to the Viking Fund and its director of research, Paul Fejos. We are deeply indebted also to Robert Merrill, of the Department of Anthropology, University of Chicago, for assistance in the measurements reported, to the members of the Committee of the American Anthropological Association mentioned below, to Ambrose Lansing of the Metropolitan Museum, to John Wilson, of the Oriental Institute of the University of Chicago, and to Fred Eggan and R. J. Braidwood of the Department of Anthropology, University of Chicago. We are also indebted to Col. C. C. Gregg, of the Chicago Natural History Museum, for many of the samples used in the world-wide assay, to Junius Bird, of the American Museum of Natural History, and to the members of the Byrd and Ronne Antarctic expeditions, as well as to H. J. Deason, of the Fish and Wildlife Service, for the Antarctic samples and to the many others who encouraged and assisted us in this research.

[2] DuPont Fellow in Chemistry.