The Lysenko Affair

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HARVARD UNIVERSITY PRESS
Cambridge, Massachusetts
1970

them his ways of self-defeat. But it is unhistorical to attribute so much to a mere human being. All the Bolsheviks, Stalin included, were interacting with evolving situations when they moved close to a suicidal extreme of self-deceiving tyranny in agriculture and science, and when they began to flounder back toward self-correcting liberty. To note that they still have a long way to go is not as meaningful as to discover the route they traveled, for such a map may suggest their future path. The critical reader is probably dissatisfied with the map that has been drawn in the preceding pages. Almost no attention has been paid to the theoretical outpourings of the extended clash between science and pseudoscience, though they must surely indicate something about the evolving pattern of Bolshevik thought. On the practical side, which has been the main theme, the reader has probably come to feel a nagging sense of disbelief. Can it be true that Bolshevik officials, who prided themselves on their practicality, were simply deceived for thirty-five years about the value of agrobiological schemes? It may seem to the reader that a correction must be made. Either the Bolshevik leaders were swayed by theoretical considerations that have been improperly slighted so far, or agrobiological recipes did in fact generate some practical gains. The way to settle such doubts is to take a close look at the theoretical outpourings, and an even closer look at some representative agrobiological recipes.

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Academic Issues: Science

PLANT PHYSIOLOGY

Outside the Soviet Union many people have sought the origin of Lysenkoism in the genuine difficulties and serious disputes of twentieth century biology. This refusal to face the fact that a complete crank won mastery over the Soviet community of biologists is sustained by the belief that Lysenko began as a scientist. He is supposed to have gained entrance to the community by doing good work in plant physiology; indeed, he is sometimes credited with opening a major new field of inquiry.[1] Then, in genetics, he is supposed to have gone wrong, by taking the Lamarckist side of the celebrated debate over environmental influences on heredity.[2] These stories have certain elements of empirical truth, as pictures of dragons have elements of real reptiles, but the composite results are myths. They express the anxieties of the artist rather than the facts of the empirical world, which are sometimes too bleak to be accepted unadorned. The painful truth is that Lysenko was never seriously involved in any genuine scientific problem, and therefore—not nevertheless—he won mastery over Soviet biologists. From first to last he and his devotees simply brushed aside or double-talked their way around serious scientific inquiry, which was nothing more to them than a scholastic impediment to quick solutions of agricultural problems.

An understanding of science is not essential to the perception of this basic fact. It is obvious in "agronomist Lysenko's" use of political force to effect entry into the communities of plant physiologists and geneticists, and to establish his dominion there.[3] Indeed, his strangeness in those communities was apparent in the name that was constantly applied to him until his mastery was beyond dispute — agronomist, not scientist. This should be a trivial matter; in the republic of learning titles should be insignificant, and no one should believe that pure science is superior to applied. Unfortunately this is not a trivial matter. The attitudes of academic snobs are exaggerated versions of values inherent in the scientific community, indeed, in society at large. Everywhere self-esteem rests on disdain for others. President Kalinin used the language of old-fashioned radicalism, but he was reacting to a fact that no one can avoid, when he told the students at the Timiriazev Academy that there was "a chasm between the people and the priests of science." He blamed it on the superiority that scientists feel toward the rest of mankind, and concluded with

> a wish to all our new scientists who are being created here, that they should make this the principle of their life, that they are not exceptional people, that they realize that they are carrying out a portion of work which is not superior in its significance to the work of an ordinary tailor or shoemaker.[4]

Kalinin won extended applause with his version of the ancient equalitarian faith, but the applauding students who went on to *higher* degrees in *pure* science — note how invidious comparisons are built into the language — almost certainly felt themselves superior to mere *praktiki*.[5]

A *praktik*, says a Soviet dictionary, trying to hide the hierarchy of values inherent in the word, is "a person who knows his business in a practical way, who has great experience in his specialty."[6] The examples of usage given by the dictionary — "he is both a learned theorist and a good *praktik;* he knows engineering as a *praktik*" — cannot hide the implicit sneer: a *praktik* lacks theoretical understanding of his skill, and is therefore inferior to the person who has such understanding. Soviet dictionaries are not likely to give an example of usage which was fairly common in the 1920s — "Marx and Plekhanov were theorists of socialist revolution, Lenin was the great *praktik*" — for this remark was officially denounced as a disparagement of Lenin.[7] And Stalin, who had the deserved reputation of being the chief *praktik* of politics in the 1920s, who insisted that practice (*praktika*) has priority over theory, sanctioned an inference that jolted Soviet philosophers at the end of the 1920s: Therefore Stalin is the chief theorist. Whatever the logical value of this syl-

logism, it performed the vital social function of elevating Stalin to a higher point than any mere *praktik* could occupy.[8] Paradoxes of that kind were made keener by the fact that the official ideology exalted the lowliest of *praktiki*, the workers and peasants, while boasting of the rapidity with which some of them were rising above their comrades, to the status of specialists and administrators. Nowadays this incongruity is hardly more disturbing than the traditional sentiment in Christian communities about the ultimate superiority of the humble and the meek. In the 1920s and 1930s, however, it was a strong element of the operative ideology, justifying such things as *vydvizhenchestvo* ("pushing up" workers and peasants into professional and administrative positions), concomitant efforts to drag down the *kastovye* and *zamknutye* (caste-ridden and ivory-tower) specialists, and Stalin's assurance to Stakhanovite workmen that they knew the capacity of machines better than engineers. In all these outbursts the persistent effort was not to realize Kalinin's dream, that is, to put everyone on an equal level, but to make selected tailors and shoemakers the superiors of learned specialists. In short, the pledge that the last shall be first was designed to reshuffle people within a traditional hierarchy of status, not to destroy the hierarchy itself.[9]

These paradoxes in the relationship between scientists and *praktiki* must be kept constantly in mind, if one wishes to understand Lysenko's interaction with scientists. He received his secondary education in a school of gardening, his higher education as an extramural student at the Kiev Agricultural Institute, working all the time at a rural experiment station.[10] He had no postgraduate training or higher degree, no formal claim to the title of scientist, yet he aspired to the theoretical heights from which, as he told a *Pravda* correspondent in 1927, practical problems could be solved by a few calculations "on a little old piece of paper."[11] Thus he awed the correspondent into granting him the rank of "barefoot professor," an "outdoor scientist," who "holds a plow with one hand, a flask with the other." The figures of speech were clumsy imitations of a pioneer in journalistic agriculture,[12] but the subject's picture of himself came through clearly enough. He and his press agents never stopped this overcompensating boasting of the difference between Lysenko and the usual scientist. "Science in the Hands of a Muzhik's Son" was the headline in 1936 over a public letter to Stalin from Lysenko's mother and father, thanking the revolution for making such a big man of their son, who would otherwise have been a gardener (*sadovnik*) all his life.[13] Three years later, a leading ideologist tried to effect a compromise between Lysenkoites and geneticists by imploring the Lysenkoites to overcome their anti-intellectual (*makhaevskie*) moods, the geneticists their seignorial (*barskoe*) attitude.[14] There is no evidence that they did. In 1948 an agrobiologist was furious that the Academy of Sciences still abounded with talk of "Michurin the gardener, Williams the ignoramus, Lysenko the illiterate." [15] As usual in cases of class consciousness, epithets were an exaggerated description of reality and simultaneously an influence upon it. The feeling and the fact of social distance interacted, each intensifying the other.

Assigned to a remote agricultural research station in the North Caucasus, and given the humble job of finding a good winter-habited crop for the locality, Agronomist Lysenko impatiently shunned the usual systematic testing of all the likely candidates. (Throughout his career he kept this antipathy to extended trials on little experimental plots.) He worked out a formula that would tell him "the amount of heat" which a plant needs to get through its stages of development from seed to the production of new seed. After failure with the heat formula[16] he made a discovery about cold, which he presented as a revolution in plant science: when he moistened and chilled the seed of winter-habited plants and then planted them in the spring, they completed their life cycle in a single season, as spring-habited plants do. He was mortified to discover that learned specialists in plant physiology were unimpressed. They were put off by Lysenko's shocking ignorance of previous work, which included many such transformations of winter into spring habit, by his sloppy reasoning, and by his poor Russian.[17] Even his famous coinage, *iarovizatsiia* (vernalization), seemed to one professor "not especially successful and grammatically not altogether correct." [18] At a major conference in January 1929 he was coolly brushed aside by N. A. Maksimov, the country's leading plant physiologist: "The results obtained by Comrade Lysenko do not represent anything new in principle; they are not a scientific discovery in the precise sense of the word." [19] In October 1929, after *Pravda* had carried an article by the Ukrainian Commissar of Agriculture praising "young agronomist and plant breeder Lysenko" for a discovery of enormous practical benefit (the Commissar thought it would put an end to winter-killing of grain),[20] the learned specialists showed some respect for the practical achievement, but persisted in brushing off the scientific. Lisitsyn, who had been Lenin's

favorite plant scientist, made the same point as Maksimov: "From the point of view of plant physiology, Agronomist Lysenko has made no discovery." [21]

Many years later an admiring biographer reported Lysenko's reaction in appropriate Stalinist bombast:

> The superciliousness and sneers that rose in the way of the young researcher will not detain us. Those people [the sneering plant physiologists] learned that "the young man" would not repeat Gassner's experiment [which opened the field of inquiry that Lysenko claimed to be opening]; he would not add to the thousandth experiment his thousand and first. He would not in this way give them the satisfaction of proving him wrong. Of course it would have been reasonable to return to Gandzha [the agricultural research station in the Caucasus], and spend long years in stubborn labor, so that he might present them with a work resting on formidable columns of figures. They would graciously approve his success, in order to upset it by an experiment of verification. He knew what these experiments of verification sometimes amount to! An inexperienced fellow, without considering the conditions, the environment, the circumstances, and the time, pokes a grain in the soil. Without knowing the requirements of the plant, its needs, the fellow raises a little green monster, and announces that the theory has not been confirmed. No, he would not let them prove him wrong![22]

That might be called the essential law of Lysenko's methodology (and of agrobiologists in general): avoid verification, do not let them prove you wrong.

All this, so far, has to do with the emotional interaction of Lysenko and the scientists. It does not touch the substantive issues. The most distinguished scientists could have been wrong, for all their learned degrees and rigorous methods; the most willful agronomist writing the crudest Russian could have been right, if only by accident. The only way to judge is to examine the substantive issues, which are fortunately easy to understand. Winter-habited plants start their growth in the fall, stop during the winter, and start again the following spring, completing their life cycle with flowering and the production of seed (earing) in the summer. If they are deprived of the chilling period, their stage of vegetative growth will extend indefinitely; they will not ear, that is, they will not enter the stage of reproduction. This winter habit can be changed by moistening and chilling the seed or the seedling before planting; then, if planted in the spring, it will behave like a spring-habited plant, going through all the stages of its life cycle in a single season. This transformation of winter to spring habit was known as early as the midnineteenth century. The Ohio State Board of Agriculture reported experiments with it in 1857, and a Russian agricultural newspaper told of similar work in 1875.[23] These are historical curiosities with little scientific significance, because the experimenters' purpose was to discover whether the transformation of winter into spring habit could be of economic value; when that purpose was disappointed, interest evaporated. It was revived by plant physiologists in the early twentieth century, who wanted to explain the phenomenon rather than find an immediate profit in it. George Klebs saw it as an important test case for his mechanistic approach to the physiology of development, and Gassner in 1918 published the pioneering paper, that is, the paper that described the transformation of winter into spring habit with sufficient precision and theoretical relevance to start a number of people working on it.[24]

They were trying to discover what mechanism is activated by chilling winter-habited plants as they start to grow, and how this mechanism determines the onset of reproduction many weeks later. This was part of the general search for mechanisms, presumably biochemical, which regulate the progression of all plants through their diverse stages of development. The favorite technique in the search was not temperature manipulation but photoperiodic effects, that is, hastening or delaying the reproductive stage by manipulation of the plant's alternating periods in light and dark. It was established, for example, that winter wheat that has not been chilled can still be made to ear by subjection to continuous light.[25] Plant physiologists have preferred to work with photoperiodic rather than temperature aftereffects, because they are more diverse and involve many more plants. As a recent Soviet textbook points out, the scientists' preference is grounded in nature's: the changing length of day acts as a precise astronomical calendar, signaling forthcoming changes in the seasons much more reliably than randomly fluctuating changes in temperature.[26] In either case, whether working with light or temperature, students of plant development have seen "importance" or "significance" or "relevance" in research that has groped for the mechanisms of change from one stage to another. They have tended to assume that they are looking for biochemical substances, generated under various conditions of light and temperature, which stimulate or inhibit the various stages of development.[27]

That was the outlook and the state of knowledge that prevailed already in the late 1920s, when agronomist Lysenko entered the field with vernalization, *iarovizatsiia*, his coinage to describe what he believed to be a great discovery: the transformation of winter into spring habit by moistening and chilling the seed. He was deeply incensed at those who told him that he had made no discovery, not only because the transformation was long since known, but even more because his experiments did not help answer the question of how it worked. The most that learned critics could say for Lysenko was that he focused attention on two minor aspects of the problem that had not been given sufficient attention. First, the temperature aftereffect can be achieved by chilling germinating seeds as well as seedlings, which had previously been the favorite objects of experiment. And second, varieties of wheat differ in their degree of responsiveness to chilling. But even these modest tributes were spoiled by qualifications. Lysenko insisted that all varieties of wheat, spring as well as winter, will respond to chilling of the seed by hastening the onset of earing and therefore, he reasoned, their yield will be increased. His learned critics had data to show that the gradient of responsiveness ranges from the virtual requirement of extended chilling in varieties that are strongly winter-habited to no response or a negative response in varieties that are strongly spring-habited, with an intermediate range that includes "ambidextrous" plants (*dvuruchniki*), as Maksimov called the varieties that follow the spring or the winter habit depending on circumstances. The learned experts also had reason to doubt that hastening the onset of earing will always increase yields. In general they showed much more respect for the work of other students of temperature aftereffects, in particular, for Tolmachev, a professor at the institute that had trained Lysenko to be a provincial agronomist. Tolmachev published, about the same time as Lysenko, a report of very similar work with chilled seed instead of the usual seedlings. He won greater respect because he was more rigorous and far more knowledgeable in his discussion of the significance or relevance of his data.[28]

Lysenko responded to his supercilious critics by flouncing angrily away from any effort to play the professional scientists' game according to their rules. After 1929 virtually all his articles were published in mass newspapers, or in journals created for him by government decree, or in pamphlets and anthologies of which he was author and ultimate judge.[29] He would not submit to the usual procedure of scientific publishing, which is to send out manuscripts for confidential reviews by established specialists as a condition for publication. Neither would he submit to the usual rules of rational discourse within the institutions where he worked. First at the Odessa Institute, where he moved in 1929, then at the Lenin Academy and the Institute of Genetics in Moscow, where he moved in the late 1930s, he would tolerate no criticism of his views.[30] A dictatorial position in biological science was not thrust upon Lysenko in 1948; he sought it from an early moment in his career, and fought hard to keep it at the end.[31]

It is accordingly difficult, indeed it is impossible to stick to the substantive issues in analyzing his clash with the plant physiologists, for it did not follow the rules that enable a scientific community to stick to substantive issues. The usual efforts to maintain impersonal, rational discourse broke down, and the historian, like the participants, is obliged to try psychological analysis of the arguers along with substantive analysis of their arguments. That is what we are doing when we say — and we can hardly avoid saying it — that Lysenko called political power into play, forcing his scientific opponents to various types of evasion, hypocrisy, and dishonesty. From the start he would not comply with the first demand on a person offering knowledge to a scientific community: show the relevance to previous knowledge. He either ignored the demand or declared that he did not need to concern himself with trivial scholasticism, which had no relevance to the needs of Soviet agriculture. "It is better to know less," he told a group of critics in 1934, "but to know precisely what is necessary for practice, both for the present day and for the immediate future." [32]

It was Lysenko's self-appointed mission to create from scratch a new theory of plant development that would be of immediate benefit to farmers. He began by pasting the name of vernalization — that at least was indubitably his — on almost any kind of stimulation *and* almost any kind of resulting growth; in almost any kind of "seed" (tubers and cuttings as well as true seed); with raised temperatures as well as lowered; in light or dark; with moist or dry conditions. He drew the line, however, at the photoperiodic effect, which was the favorite experimental technique for studying plant development in the 1920s, and at the use of chemical growth substances or plant hormones, which came to be the favorite technique in the 1930s. It is impossible to understand these antipathies of Lysenko's without recourse to psychological analysis, for he never discussed photoperiodic

and chemical effects at any length. He simply ignored them or brushed them aside, leading his disciples to declare that he had "disproved the theory, which used to exist among plant physiologists, of the existence of the so-called photoperiodic reaction." [33] Lysenkoite hostility to chemical stimulants and inhibitors was great enough to reveal the psychological cause:

> In essence the "hormonal theory" is a poorly masked attempt to distract our scientific research thought from the advanced and progressive theory of the stage development of plants, as worked out by Academician T. D. Lysenko.[34]

A purely logical Lysenko might have taken a more tolerant view of photoperiod and growth substances; his theory of stages was vague enough to encompass virtually anything. Indeed, in the time of declining power his learned followers began to stretch the stage theory to allow for phenomena that they had denied or brushed aside in the time of rising power. In the heady period from the 1930s to the mid-1950s Lysenko's vagueness was not an instrument of a bureaucratic chameleon or a political broker, who rise by offending nobody and changing nothing. In the agrobiology of that time, as in Stalinist thought generally, vagueness had a willful, activist function; it allowed the chief to tell his inferiors what new things must be believed and done at every turn in the hazardous struggle. Lysenko was annoyed by scientists who studied plant hormones as Stalin was annoyed by academic economists, because they undercut his claim that practical utility and theoretical truth were fused in every intuitive judgment that he made.

The reader may have the uncomfortable feeling that this is *ad hominem* nastiness rather than objective analysis of a man's beliefs. Unfortunately the analysis of ideological beliefs cannot avoid statements that are uncomfortably similar to accusations. Consider the following example. Lysenko pinned his concept of vernalization to the plain old practice of sprouting potatoes before planting them. Assume, as I do, that he was not consciously faking. His statements about the vernalization of potatoes must still be characterized as a verbal shuffle, whose function was to enhance his fame as a practical biologist. Can the reader discover any other meaning in the following passage?

> The speeded up development of such plants [that is, sprouted] we explain basically not by the fact that the eyes of the tubers are sprouted before planting, but by the fact that the sprouts (though

> they are very small) are subjected to the influence of certain external conditions, namely: the influence of light (of a long spring day) and of a temperature of 15–20° C. Under the influence of these external conditions (and that precisely is vernalization [*a eto i est' iarovizatsiia*]), in the potato tubers' eyes as they start to grow there occur those qualitative changes which, after the tubers are planted, will lead the plant to more rapid flowering, and hence to more rapid formation of young tubers.[35]

Aside from the factual error at the end — the implication of a causal connection between flowering and tuber formation[36] — this characteristic passage is inane. It conveys the feeling of an explanation without its substance. It has a striking similarity with the explanations offered by "peasant experimenters" for the supposed benefits of the seed stimulants that were tried in their "hut labs" during the 1920s. Their views were put in print by an organizer of the "hut lab" movement, who subsequently became one of Lysenko's chief disciples:

> Some peasants explain that, during the time of moistening, the seeds go through, as it were, "a part of their life's journey." Sown in the ground such seeds sprout more quickly than unmoistened ones, and in their further development outstrip them.[37]

Lysenko did not always write clear inanity about plant development. Sometimes he was obscure. But clear or cloudy, meaning must usually be sought in the social function of his statements, rather than their relevance to the life process of plants. The grand theory of plant development, which he promised in the early 1930s, never got past the first two stages, vernalization and "light." After a brief flurry of articles and pamphlets, none of them reporting anything like genuine experimentation or theorizing, he simply dumped the whole project on his disciples, while he went off to make a revolution in genetics. It is possible to read scientific meaning into these articles and pamphlets, which were quite careless of its requirements, but then inconsistency becomes the problem. One can, for example, compile a list of seven meanings that Lysenko gave, often inadvertently, to his key concept of vernalization:

1. the transformation of winter into spring habit by chilling moistened seed
2. the hastening of the reproductive stage, in spring- as well as winter-habited plants, by chilling seed
3. the hastening of the reproductive stage by warming seed

It was suggested to Lysenko that he make a meaningful generalization of these three, and of the behavior of perennials: Vernalization is simply another name for temperature aftereffect (or thermal induction), that is, the hastening or retarding of flowering by suitable alterations of temperature. But he angrily rejected this limitation of his concept, insisting that vernalization was something much broader:

4. the initial stage in the development of any plant or part of a plant, when certain conditions of air, moisture, and temperature are essential for the onset of the next stage and for ultimate flowering, but light is irrelevant

This amounted to a verifiable assertion, which could be proved wrong on many counts. There are seeds, for example, that need light to germinate, and — to take the example that especially enraged Lysenko — winter wheat will flower without chilling, if it is subjected to continuous light.[38] As if that were not trouble enough for the theory, Lysenko declared that potatoes are vernalized by warmth *and light,* thereby implying — if one wishes to be logical, as he did not — still more meanings for vernalization:

5. the stimulation of buds by warmth and light
6. the initial stage as in no. 4 but with light essential

And finally the logical reader must find a place on the list for such passages as the one quoted above, "Under the influence of these external circumstances (and that precisely is vernalization). . . ." That comes under number

7. null[39]

An equivalent list of meanings for the light stage would be less impressive, for Lysenko had far less to say about it. One example will suffice to show the difficulty he had in trying to discuss it without getting entangled in the distasteful concept of photoperiod:

> To get through the *light* stage some plants require *light,* temperature, moisture; other plants, of which millet is one, get through the light stage if *darkness,* moisture, air are available. . . . For millet to get through the *light* stage light is not a necessary factor. However, if when getting through the light stage, a millet plant is kept in the dark for 5 to 10 days, this will lead either to retardation of growth or to the death of the plant. Thus, light is necessary not for getting through the light stage, but for the process of nutrition.[40]

As Kuzma Prutkov might have said, one cannot avoid the unavoidable or embrace the unembraceable.

Within five years of starting to build his own stage theory Lysenko simply abandoned the effort. Some disciples and flatterers kept trying to complete it for him, but there is little point in reviewing their efforts. They were trying to breathe scientific life into a clot of inanity and inconsistencies, and they tended to quit as political bosses quit nodding their approval. From the mid-1950s to the end of 1964 the chief point of Lysenkoite talk about the stage theory was to prove that the data of hostile scientists could be accommodated within the master's doctrine.[41] Since his final fall in 1965 the few plant physiologists who have discussed it at all have been mainly concerned to keep a shred of self-respect as they abandon the doctrine in favor of standard science.[42] In short, Lysenko's stage theory has failed to survive his loss of political power, though some effort was made to achieve that miracle.[43] All that survives is the term "vernalization," now confined to sense one in the list above. Even that, in my opinion, is too much. Other terms, such as "thermal induction" or "temperature aftereffect," describe the phenomenon more sensibly and without unpleasant associations.

How then are we to account for the fact that many plant physiologists once credited Lysenko with important contributions to their discipline? At first glance the answer seems simple and uninteresting, depressingly void of significance for the history of science: Soviet plant physiologists were forced to praise Lysenko, and some of their foreign colleagues thoughtlessly took them at their word. Actually the matter is not so simple. Soviet geneticists were subject to equal or greater pressure to acclaim Lysenko's contributions to their discipline, yet very few did so, and hardly any of their foreign colleagues followed their example. Are we to conclude that plant physiologists are generally less perceptive or less dedicated to their calling than geneticists? Hardly. If we have the stomach to dig in the grubby record, to examine the actual reactions of these two communities as pseudoscience was forced upon them, we discover that the different patterns were shaped by essential differences in the two disciplines.

Consider the case of Maksimov, the most eminent plant physiologist at the end of the 1920s, when Lysenko first intruded in that community. Maksimov's initial response was to tell the simple truth: "The results obtained by Comrade Lysenko do not represent any-

thing new in principle; they are not a scientific discovery in the precise sense of the word."[44] By 1933, responding to a British request for a report on vernalization, he was no longer straightforward, but he made a strenuous effort to be honest with his scientific colleagues abroad, almost none of whom could read Russian. He began with a display of great respect for Lysenko's work, stressing especially its practical value to Soviet agriculture, and then he spent the bulk of his article on the work of predecessors and contemporaries, bringing out in this indirect way the inanity and confusion of the contributions that were distinctively Lysenko's.[45] To this and other signs of Maksimov's continuing skepticism the Lysenkoites responded with a virulent attack,[46] culminating in a special meeting at his institute, in April 1936, at which he was subjected to a string of denunciatory speeches. At the end Maksimov rose to apologize for criticizing Lysenkoism, saying that he had changed his "point of view" since he wrote the British article.[47] Indeed he had. He never again criticized Lysenkoism in print. Worse yet, the few lines of tribute to vernalization that he had inserted in a 1931 edition of his famous textbook, grew to several pages of uncritical praise for the stage theory in subsequent editions.[48] Following the August Session of 1948 he made another abject public apology, no longer for criticizing Lysenkoism but simply for allowing non-Lysenkoite work within the Institute of Plant Physiology that he directed.[49] In 1949 he put his name to an utterly Lysenkoite effusion on the stage theory,[50] and died in 1952, too early to take advantage of the reviving possibility of criticizing this pseudoscience.

I do not set this down in order to perpetuate the humiliation that a fine scientist endured in his lifetime. My purpose is to analyze the pathological condition of a scientific community. There were far worse symptoms than Maksimov's insincere praise of Lysenkoism. In a sense his dishonesty was a service to his discipline. By humiliating himself he held on to administrative power, and so was able to protect men like Chailakhian, who did the research on plant hormones for which Maksimov made public apology in 1948. By inserting some pages of Lysenkoism in his textbook he managed to keep it in print, thus spreading many more pages of instruction in genuine plant physiology among students throughout the Soviet Union. In short, Maksimov was engaged in pliable defense of his science, letting the enemy in at points of greatest pressure to prevent the complete destruction of the whole enterprise. Of course this was a painful strategy and a dangerous one, shading off by insensible degrees into complete opportunism and total surrender to pseudoscience. A fine specimen of the complete opportunist is V. I. Razumov, who began his scientific career as a junior colleague of Maksimov's, earning Lysenkoite hostility for studies of photoperiodism that cast doubt on Lysenko's hasty generalizations.[51] In the mid-1930s, as soon as Lysenko's dominance was clearly established, Razumov suffered a complete conversion. He did not, to be sure, become a militant ignoramus like Lysenko; very few scientists did. He retained enough knowledge to provide the stage theory with sophistic arguments that the ignoramuses were incapable of. Thus he was especially valuable in the periods before 1948 and after 1952, when it was not politic simply to deny the existence of plant hormones or to banish the term "photoperiod." But crude or sophisticated, Razumov became so completely identified with Lysenkoism — by the end he was Lysenko's chief plant physiologist — that in 1965 he went into eclipse along with his master.[52]

Few plant physiologists committed themselves so wantonly to Lysenkoism. Vlasiuk may serve as our specimen of the canny opportunist, a far more common type. He turned against research in plant hormones not in the 1930s, as soon as Lysenko's hostility to them became apparent, but in 1948, as soon as it became official. Unlike Maksimov, who made a public show of turning against such research in 1948, Vlasiuk really did, which was a serious matter because he was a very important administrator of science.[53] He was, among other things, the president of the Ukrainian Academy of Agricultural Sciences, who shocked Khrushchev in 1961 by confessing that his endorsement of *travopol'e* had been insincere, the product of his loyalty to the Party.[54] It was extremely gauche to recognize that loyalty to political authority might be at odds with dedication to one's scientific calling, yet Vlasiuk managed to keep his career going, in part because he was one of the most energetic proponents of Lysenko's fertilizer scheme. In fact, he published an article on behalf of the famous composts *after* Khrushchev's dismissal had deprived them of political support.[55] But once Lysenko's fertilizers were formally investigated and officially condemned, Vlasiuk adapted himself once again to the new situation, and remained a major figure in the Soviet community of plant physiologists.[56]

Thus the forceful intrusion of a militant ignoramus into plant physiology reduced some of its adepts — such as Razumov and Vlasiuk — to opportunistic surrender, while provoking others — such as

Maksimov — to pliable defense of their scientific principles. To complete the typology we must add the intransigent specialist, the stiff-necked man or woman who would concede nothing to pseudoscience except silence, when political authority made honest speech or writing impossible. D. A. Sabinin, for example, professor of plant physiology at Moscow University, virtually refused to give printed tribute to Lysenko and made fun of him in the lecture hall, even after the university newspaper insinuated that Sabinin's loyalty was placed in question by this ridicule.[57] For a year in the late 1930s he was suspended from his post, and then, after the August Session of 1948, he was exiled from Moscow altogether, to wander without employment until 1951, when he shot himself.[58] He left behind a manuscript on the physiology of development, in which he firmly disposed of Lysenko's stage theory.[59] Another specialist of similar character was Chailakhian, who survived to see Sabinin's manuscript published in 1963. One of the country's leading authorities on plant hormones, Chailakhian made extremely few concessions to the Lysenkoites while defending his subject against them.[60] Indeed, he resisted the temptation to which Kholodnyi, his elder and rival in the study of plant hormones, succumbed. When they disagreed in their theoretical speculations about these obscure substances, Kholodnyi was not above an attempt to sick the Lysenkoites onto Chailakhian.[61]

Intransigent specialists did not control most centers of research and higher education in plant physiology, but neither did militant ignoramuses or complete opportunists. Pliable men of scientific principle seem to have held most of the important posts during the thirty-five years when Lysenkoism enjoyed the support of political authority.[62] They were obliged to skimp some of the biochemical research that was the cutting edge of their science, and they had to tolerate Lysenkoite nonsense in their journals and textbooks. Thus they dropped behind the world's leading centers of their discipline, and blushed to see foreign colleagues use "Russian" or "Soviet" as a synonym for Lysenkoite. But they managed to keep functioning as a scientific community. In this respect their fate was strikingly different from that of the geneticists, who were overwhelmingly intransigent in their reaction to Lysenkoism and were overwhelmed by complete disaster. Equally committed to two different disciplines, the two communities behaved in different ways, provoking different reactions from their common foe.

GENETICS

Plant physiology was — and still is — a sprawling, highly empirical discipline, relatively lacking in unifying theories. With his fuzzy concept of universal vernalization and his inchoate theory of stages Lysenko pitted willful vagueness against simple methodological injunctions (try to be consistent, look for biochemical stimulators and inhibitors, and so on), and against a great mass of factual information, not against a full-fledged theory of plant development. Such a theory is still music of the future.[63] Even so, the conflict between dogmatic vagueness and biochemical empiricism occurred in one field of the discipline, which could be separated from the others precisely because unifying theories were — and still are — lacking. A plant physiologist who wanted to keep out of trouble did not have to convert to pseudoscience. He could work in some other field than plant development. Even if he was bold enough to stay in that special preserve of the Lysenkoites, he could do empirical research and report the results as such, without waving theoretical red flags at the Lysenkoite bulls. He could, for example, write about "phases" rather than "stages," or about auxins and gibberellins rather than plant hormones or growth substances, and thus avoid provoking the Lysenkoites, except in the wildest years, 1948 to 1952.[64]

In sharp contrast genetics was — and is — a highly theoretical discipline, elegantly centered on a few basic concepts and theories. Some admirers have acclaimed its similarity with a formal or deductive science. Here for example, is the great mathematician Hilbert, reacting to the construction of genetic maps on the evidence of crossing over:

> The numbers [percentages of crossing over] are in accord with the linear Euclidean axioms of congruence and agree with the axioms concerning the geometrical concept "between." Thus the laws of heredity emerge as an application of the linear axiom of congruence, that is, of the elementary geometrical propositions concerning the displacement of line segments — so simply and precisely, and at the same time so wonderfully that no one could have imagined it in his boldest fantasy.[65]

Hilbert was probably exaggerating the axiomatic nature of genetics, as the Lysenkoites were, when they made "formal genetics" a favorite epithet. But exaggerated or exact, admired or despised, there *is* a

pronounced formal quality in the science. When Lysenko attacked its axioms, the whole discipline was in mortal danger; there were no sidelines where geneticists could take refuge, as the plant physiologists did. Of all biological disciplines the geneticists came closest to the formal ideal that has attracted the scientific mentality since its origin, and they paid for this success. They had no empirical maze to hide in, when militant ignoramuses denounced rigorous thought as an obstacle to practical activity.

The simple principles of genetics seem so easy to grasp, to people who find them in textbooks, that the original difficulties of finding them in nature are usually overlooked. They are worth reviewing here, for they tend to recur whenever an untrained practical person has to deal with problems of heredity, as Lysenko did when he decided to breed an improved variety of wheat in three years. With the advantage of hindsight we can see that the basic difficulty was finding a way to distinguish between that part of an organism which is hereditarily determined and that part which is not, between the genotype and the phenotype, as Johannsen in 1909 named these inseparable aspects of an indivisible whole. The distinction must be made, if one is to think about heredity, yet there are no obvious morphological structures that stimulate the mind to visualize it, as there are, say, leaf and flower to prompt the distinction between vegetative and reproductive functions. Like the atoms of Democritus or Newton's particles of light, the genes are apprehended by abstract thought. Indeed this is a kind of abstract thought that suggests metaphysics, for the evidence of our senses seems flatly opposed to the argument that matter and light and living organisms are mosaics of particulate entities. Yet this method of thinking about heredity became increasingly necessary for practical breeders as well as theoretical biologists during the eighteenth and nineteenth centuries. The breeders were stumbling toward it by their intensified efforts to separate or combine, to emphasize or diminish, particular characters of domestic plants and animals; the biologists, by trying to arrange species in a natural system, defining their "essences" in such a way as to explain similarities, differences, and patterns of transition. The breeders, as we would say nowadays, were grappling with the problems of individual heredity and how it may be changed, the biologists with the problem of evolution, that is, changes in the pooled heredity of breeding populations. It seemed obvious that the second or larger problem could not be solved without making certain assumptions about the first or smaller problem. Indeed theoretical biologists rarely thought of them as separate problems.

Darwin and Mendel made the separation; that is how their great breakthroughs were effected. They were not quite conscious of what they were doing. Darwin worried a lot about the origin of individual variations, and repeatedly tried to formulate the theory of natural selection in which a way as to connect it with a theory of individual heredity. But logically the theory of natural selection was independent of his speculations about individual heredity. Mendel's thought process is much harder to analyze, for he left only a small record of it, but it seems fairly clear that he too worried about the connection between these two problems. In the introduction to his famous article he expressed the hope that some day a connection might be established between his laws of individual heredity and "the evolution of organic forms."[66] But that was only a fleeting glance at the big problem of his time; in the rest of the article he modestly confined himself to a problem so small and simple that it probably seemed trivial to his contemporaries, if they paid any attention to it at all: What is the most elementary statistical pattern of characters that persist through successive generations of hybrids?

Mendel was not only setting aside the grand problem of species transformations, which enthralled laymen as well as professional biologists in his time; he was not even facing the subsidiary problem of individual variations. He was simply trying to turn into precise ratios the banal observation that hybrids sometimes resemble one parent, sometimes the other, and sometimes an intermediate mixture. That seems to be the main reason why his work was ignored from its publication in 1866 until its simultaneous rediscovery by three different biologists in 1900. Biologists had to realize which extremely simple problems required solution before the big complex ones could be effectively dealt with. After a generation of inconclusive speculative debate over the sources of individual variations — and at the same time, a generation of intensive work at cytology — a number of biologists came all at once to appreciate the significance of Mendel's simple ratios and the method by which he had established them.[67] By counting individual characters, as they combine and segregate in successive generations of hybrids, Mendel had found the only way to make a precise distinction between what is hereditarily determined and what is not. Ultimately, said the enthusiasts, this method would explain not just the disappearance and reappearance of green peas in

successive generations of hybridization with yellow; this was the way to solve all the big puzzles about individual variation and even about the transformation of species.

Pooh-pooh was the instant response of many biologists, including the celebrated Haeckel in Germany, and his analogue in Russia, K. A. Timiriazev, a very learned biologist and superb popularizer, whose influence among the Russian intelligentsia was enhanced by his political radicalism. Timiriazev respected Mendelism, especially for its answer to an antievolutionary argument that had disturbed Darwin: An individual variation from the species type, however advantageous, could not be the source of a new species because it would be swamped in breeding with the multitudinous individuals of the older type. Darwin had two answers to this objection, and Timiriazev refused to choose between them, as the extreme Mendelians insisted must be done, in order to achieve consistency. On the one hand, Darwin knew that some characters cannot be swamped because they do not blend in hybridization. As in the case of yellow and green peas, the progeny may have either one parent's distinctive trait or the other, and the supposedly lost trait will reappear in later generations, repeatedly available for natural selection. On the other hand, Darwin knew that many of the most important characters do not have this simple discontinuous nature. They blend in hybridization, which seems to substantiate the argument that a single variation, however advantageous, will get swamped. In that case the only defense of evolution seemed to be Lamarckian: A species is transformed as many individuals simultaneously change their heredity in the same direction, adaptively responding to a common environmental stimulus.*

Darwin, and Timiriazev after him, did not believe that this Lamarckian assumption was necessarily a teleological departure from the mechanistic theory of natural selection. More about the philosophical aspects of the problem later. Here the point is simply that the radical Mendelians of the early twentieth century shocked learned biologists with the extravagance of their mechanistic assertions. They denied the reality of blending heredity, which we can all see in our daily observation of parents and progeny. It is only an appearance, they insisted, the product of complex combinations of particulate hereditary characters. Indeed the most ex-

* It is unfair to Lamarck to perpetuate his name only in this meaning, but usage decrees the injustice whether we approve or not. For a fair appraisal of Lamarck's contribution to science, see the references in chap. 7, n. 69.

treme position was that hereditary characters are not only particulate but eternal; their combinations may be shuffled by selection, but the elements being shuffled are themselves changeless. To monistic radicalism of this sort Timiriazev replied with eclectic common sense (his radicalism was confined to human problems): Mendelian and Lamarckian assumptions are not mutually exclusive; they are complementary contributions to the great science of evolution, Darwinism.[68]

With the advantge of hindsight, we can see the mistake of Timiriazev and those who contined to think as he did through the 1920s. They were resisting the choice between studies of evolution and studies of individual heredity, which would have to proceed separately before they could be joined in a scientific rather than a speculative union. To keep them separate, to work out Mendelian ratios of simple discontinuous characters, while ignoring complex characters *and* the sources of individual variation *and* the transformation of species — this seemed extremely one-sided and simple-minded. It seemed to old-fashioned biologists that the Mendelians were making far too hasty and far too sweeping generalizations from their limited studies of one pattern of heredity. The expectation that other patterns would be discovered therefore continued into the 1920s. Relatively few biologists actually designed experiments to prove the Lamarckian case. To design experiments on heredity was almost inevitably to be trapped in the distinction between genotype and phenotype, which is death to Lamarckism.* But many biologists working in other fields than genetics, especially those in evolutionary systematics, continued to think that a synthesis of Lamarckism and Mendelism would be the ultimate result.[69]

And then, in the 1920s, geneticists discovered a way to explain complex characters *and* individual variations *and* the transformation of species, all on the basis of Mendelian concepts. They were very far from final solutions of these enormously complicated problems. They simply demonstrated, by some crucial case studies, that they *could* solve them; it began to appear that they knew how to go about it. That was enough. Lamarckism rapidly withered away, for

* The fatality is virtually imposed by definition alone. The genotype is defined as the hereditary aspects of an organism. Hereditary means self-replicating, which rules out innovation except as accidental aberrations of the self-replicating mechanism. If we think of it as capable of adaptive changes, we have ceased to think of it as a purely self-replicating mechanism; we are spoiling our effort to distinguish between that which is hereditary and that which is not.

it had lost its useful service. Speculation about the sources of variation and the mechanisms of evolution seemed no longer necessary, now that rigorous generalization was possible, generalization of experimental data that splendidly complemented and enriched the initial theoretical assumptions. The community of biologists unreservedly accepted genetics as a major discipline — many said it was *the* major discipline, which would revolutionize all the others. In any case the community began to hear less and less talk of Mendelism, Lamarckism, Darwinism, or any other eponymic doctrines, which are on everybody's tongue when a highly theoretical science is in the formative stage, as genetics was in the first three decades of the twentieth century.[70]

All this to give the patient reader some understanding of what Lysenko assaulted, *not* what he started with. The simple fact is that Lysenko's "genetics" originated entirely apart from intellectual processes in the community of biologists. His ideas about heredity were not derived from Lamarckism, or from any other trend in science, whether speculative or experimental, moribund or alive. Between 1933 and 1935 he created his own genetical concepts in a series of intuitive strokes, to suit the simple practical purpose of breeding an improved variety of wheat in two or three years, and to beat down the objections of learned critics. Any resemblance to genuinely scientific thought was purely accidental. Justice to the Lamarckists requires insistence on that fact, for many of them were serious biologists. They had nothing to do with the birth of Lysenkoism, and most of them kept their distance as it became a dominant force, if only because most of them were abandoning Lamarckism as the geneticists showed that they could solve the problems to which it was addressed. Komarov, to take the most notable example, was a botanist whose sympathetic interest in Lamarckism became increasingly historical during the 1930s, as it ceased to be a living trend in science. At the same time he became vice president (1930–1936) and then president (1936–1945) of the Academy of Sciences, where he protected genetics against the onslaught of Lysenkoism. And not only as an administrator. In successive editions of his popular books on plant evolution he stuck firmly to genuine science, showing how genetics was replacing Lamarckist speculation, and inserting only tiny, ritualistic nods to Lysenko.[71]

Furthermore, justice to *Lysenko* requires insistence on the fact that he did not derive his ideas from academic Lamarckism. He was very proud of the practical, agricultural origin of his genetical concepts. (Of course he would not tolerate the suggestion that he was merely juggling ancient superstitions and folklore of farmers. He insisted that he was transmuting the most advanced agricultural experience into revolutionary theoretical concepts.) For a time he denied any kinship with the Lamarckists. At the crucial meeting of December 1936, he asserted that "the geneticists, who have not really got to the bottom of Darwin's evolutionary doctrine, which we have mastered by action [*deistvenno*], . . . have no basis for calling our views Lamarckist."[72] He was wrong. There *was* an important similarity between his homemade "genetics" and Lamarckism; in the late 1940s he and his disciples finally admitted and even came to boast of it.[73] But the similarity was the unforeseen product of an essentially different approach to the problems of heredity.

Lysenko got involved in these problems without any intention of proving the inheritance of acquired characters, as the basic doctrine of Lamarckism is misleadingly named.* When he first reported on his work in plant breeding (January 1934), he praised Mendel, for showing that hereditary characters can be torn apart and put together again in many different patterns.[74] He criticized Mendel's latter-day disciples, the geneticists, for trying to inhibit practical activism. They had told him that he could not breed an improved variety of wheat in two or three years, because extended progeny tests are required to discover what will come of crossing various types. Lysenko was sure that he knew in advance which particular crosses would give him exactly what he wanted, and in any case he could select what he wanted from the first generation of hybrids, without worrying about segregation in further generations or about useful though recessive characters in the rejects. His theory of stages gave him this foreknowledge:

> By this means we have already succeeded in solving a problem that is quite unclear for formal genetics to this very day, i.e., the problem of finding two parents such that early or late forms can be obtained by crossing them. The explanation of this, it seems to me, consists in the fact that we operate with the plant's development, i.e., with the interaction of the "internal" and a definite "external." But genetics, as it seems to me, operates all the time exclusively with characters, i.e., only with the results of development. It seems to me that genetics (formal genetics, of course) is essentially interested not in genes but in phenes.[75]

Scientific sense cannot be made of such effusions. This one joins factual inaccuracy — he did *not* have foreknowledge when hybridizing — with an emotional intuition of the Stalinist type: The geneticists must be wrong to tell him that he was wrong, for their academic prescriptions and prohibitions would slow down a practical breeder. Here one can see the beginning of his unintended approach to Lamarckism. His haste was a practical version, or perhaps we should say a gross anti-intellectual caricature, of the impatience that Lamarckist scientists had shown to solve big problems before the little subsidiary ones were settled. Theirs had been an academic impatience to know the connection between individual variation and species transformation before the simple rules of heredity were established. His was the impatience of a breeder — or a publicity hound — to get an improved variety without a lot of progeny testing. In both cases the result was a hasty leap over the distinction between what is hereditarily determined and what is not. When pressed on this point by geneticists, who showed precisely how the laborious distinction must be made, most Lamarckists quietly abandoned their speculations. Even if they were not completely convinced, they were professional scientists; they belonged to a problem-solving community and had to abide by its collective verdicts, if they could not dispute them rationally.[76] When Lysenko was pressed to distinguish between the hereditary and nonhereditary characters of his plants, he leaped to the conclusion that the distinction was an academic absurdity, and fought back irrationally, with power.

Thus in its practical consequences no less than its origin Lysenko's confusion of genotype and phenotype was drastically different from that of the Lamarckists. Far from submitting to the verdict of the scientific community, he used political power to create an opposing camp, an army of people who would unquestioningly recognize the truth of anything he said. (That is not an exaggeration; the moment a Lysenkoite began even limited criticism of the master's doctrine, he was denounced as a turncoat.)[77] The name of

* The name is misleading because it calls attention to irrelevancies, such as mutilations and other nonadaptive alterations. More serious is the false implication that environmentally induced change in heredity is affirmed by Lamarckism and utterly denied by genetics. Actually the geneticists have shown precisely how such change is effected. They differ from the Lamarckists in denying the "adequacy" or "specificity" of environmentally induced changes in the heredity of an individual organism. That is, they deny that an individual hereditary mechanism can make an adaptive response to an environmental influence, except by accident. On this basis they have shown how multitudes of breeding individuals, populations, can and do make finely adaptive responses to environmental influences.

Michurinism was fixed to this militant school, and genetics was totally rejected as Mendelism, or Weismanism (after August Weismann), or Morganism (after T. H. Morgan), or some hyphenated combination of those eponymic pejoratives. By 1937 Lysenko ceased to speak of genes or genotypes except to deny their existence, and chromosomes were mentioned only to deny that they had a special role in heredity. The basic concepts of genetics were replaced with notions so vague as to allow practical men complete freedom in altering living things to suit their needs (or publicity hounds complete freedom to create sensational impressions of such mastery). "In our conception," Lysenko wrote,

> the entire organism consists only of the ordinary body that everyone knows. There is in an organism no special substance [*veshchestvo*] apart from the ordinary body. But any little particle [*chastichka*], figuratively speaking, any granule [*krupinka*], any droplet [*kapel'ka*] of a living body, once it is alive, necessarily possesses the property of heredity, that is, the requirement of appropriate conditions for its life, growth, and development.[78]

If scientific content has to be read into such pronouncements, Lysenko can be interpreted as dissolving genetics into physiology, identifying the function of self-replication with all the other life functions. But such a paraphrase is misleading in its precision, for his understanding of the other life functions was almost as vague and evasive as his understanding of self-replication. The simple truth must be faced. Any part of biological science that Lysenko touched was turned into a vague, personal dogmatism.

To attempt a coherent outline of Lysenkoite "genetics" is thus a self-contradiction: it began and ended with opposition to clearcut thought and rational experimentation. What is more, Lysenko and especially his disciples shifted their stance as their political influence waxed and waned. By 1936 he overcame his initial reluctance to condemn genetics altogether, but he hesitated to come right out and say so, for the highest authorities decided at that time to subordinate genetics to agrobiology, not to abolish it. Lysenko and his zealots chafed at this limited victory even as they were winning it. "Why do we need this science?" one of them exclaimed at the crucial conference of December 1936; "it only hinders our work." [79] Within a few months Lysenko began an open campaign for complete abolition. He denounced "the whole logic of thinking with mechanical models, the very basis of the corpuscular theory in biology." [80] He

went far beyond claims of foreknowledge in hybridization, as the term is ordinarily understood. He insisted that vegetative hybridization — alteration of heredity by grafting — is a fact that utterly subverts genetics (as indeed it would, if it were a fact, for stock and scion do not exchange germ cells).[81] His people reported that reruns of Mendel's famous experiments with peas did *not* yield the ratios Mendel claimed to find.[82] When the country's most eminent student of statistics showed that these people misinterpreted their own data,[83] Lysenko replied that mathematics has no relevance to biology. "That is why we biologists do not take the slightest interest in mathematical calculations that confirm the useless statistical formulas of the Mendelists." [84] He called genetics "real barefaced metaphysics" and rejected out of hand any attempt at compromise. "In my view it is time to eliminate Mendelism in all its varieties from all courses and textbooks." [85]

After another major conference, in 1939, reasserted the official effort at compromise, the Lysenkoites showed a little moderation, but dropped it as the great war with Germany ended. Now Lysenko endorsed the views of Olga Lepeshinskaia, an elderly physician who had been arguing since the early 1930s that she could grow cells from bits of egg yolk and other noncellular globs of matter.[86] She had also been demonstrating that there was an affinity between her views and Lysenko's,[87] but he had held back from agreement, for in the 1930s he conceded that his school ought to show how its notions of heredity accorded with the data of cytology.[88] In 1945 he finally summoned the courage to deny that unfulfilled obligation. The geneticists could have their scholastic delight at the correlation between Mendelian ratios and the intricate pattern of meiosis. By asserting that cells can grow from noncellular material, Lysenko and Lepeshinskaia made meiosis irrelevant, thereby opening the door to "cytological" justification of any kind of transmutation. In short, cytology was rejected along with genetics.[89] Even the theory of natural selection, which had seemed to be an unassailable part of the official theoretical ideology, was virtually rejected in the late 1940s, to make room for Lysenko's discovery that competition does not occur among organisms of the same species.[90] Though the August Session of 1948 was not supposed to give official sanction to these extreme views,[91] the Lysenkoites behaved afterward as if it had, as if they had won unlimited political support for their wildest speculations. A new law of species formation by direct transformation was announced. Wheat, for example, was transformed directly into rye when raised in "appropriate" conditions.[92] That was equivalent to saying that dogs give birth to foxes when raised in the woods, and very soon Lysenko's followers were making even more extravagant claims. One man turned viruses into bacteria, while another changed plant tissue into animal tissue, and a third drew chicken from a rabbit.[93] In effect living matter was becoming structureless goo, ready to be shaped at will into anything the Soviet farmer (or the publicity hound) might wish.

Lysenko never explicitly repudiated or amended the extremes that were reached at the apogee of his power in the late 1940s and early 1950s; he merely refrained from active propaganda for some of them as his political influence ebbed. He also tacitly approved the efforts of his more learned disciples — the ones who did not desert him — to revive something like the compromise of the late 1930s. They dropped Lepeshinskaia and conceded the legitimacy of cytology, of mathematical calculation, and even of biochemical research in nucleic acids. They tried to maintain the dominance of Lysenkoism by arguing that none of those supposedly new trends proved the existence of genes or justified the resurrection of genetics.[94] When political support evaporated, Lysenkoism collapsed altogether. The leader and a few of his disciples lapsed into enforced silence, while most of the school quietly abandoned their former notions in order to hold places in or around the scientific community. It is a distasteful and depressing sight; even the man who once turned rabbit into chicken continues as an editor of an important biological journal.[95] The admirer of persistent cranks can take heart at the occasional appearance in an agricultural journal of a Michurinist article defending vegetative hybrids.[96]

Through all the politically motivated shifts a characteristic style of thought and stubbornly unchanging doctrines persisted in Lysenkoite "genetics." They resist coherent presentation — to the end an authoritative textbook was never achieved[97] — but they do exhibit regularities, as, say, American advertising does. Of course, the Lysenkoite style of thought was never as cynically realistic and cunning as that of American advertising. Sometimes it resembled the ordinary stream of inarticulate consciousness, sometimes the savage thought process that Lévi-Strauss has named *bricolage*,[98] sometimes the kind of emotional thinking that is called feminine (as if women alone have strong opinions which they refuse to defend by reason-

able argument). But none of these types of plainly "prerational" thought is as close to the characteristic style of Lysenkoism as the one that might be called "surrational" (or masculine, if we wish to keep an even score in the battle of the sexes): a show of rational discourse camouflaging a basic refusal to meet the tests of genuine reason.

The Lysenkoites endlessly expatiated on a few basic ideas, piling up reasons and facts (and "reasons" and "facts") to confirm them, but steadily evading or resisting the necessary first step to confirmation: an effort to define the ideas precisely. The beginning and the end of their thinking about heredity was the simple insistence that it is a property of the whole organism, with the supposed corollary that the organism contains no special substance or matter of heredity. Biologists had passed so far beyond this kind of simple-mindedness that they were inclined to respond with impatient gibes: Does the fuzz on a leaf play the same role in heredity as the pollen or the ovule in the flower? Does the cell wall have the same function in self-replication as the cell nucleus? Leaving aside sarcasm, as Soviet biologists were usually obliged to do, and considering the basic Lysenkoite idea dispassionately, as they were usually unable to do, one can find in it vestiges of scientific thought. Darwin, for example, pondering heredity when views on it were "utterly formless, diffuse, indefinite,"[99] assumed that the whole organism must contribute elements to the germ cell. Otherwise, how can we explain the replication by the germ cell of the entire organism with all its different parts? Darwin answered with the hypothesis of pangenesis: microscopic particles or gemmules flow from all the parts of an organism to the germ cell, which becomes in effect a package of seedlets, each one ready to recreate the part that formed it. That kind of speculation was rampant in the late nineteenth century, but it could not be tested, and the further development of cytology made it utterly unrealistic. However, when Darwin made it, it was a scientific speculation. It did not flout known facts. It was offered tentatively, to be accepted as true only if it could be proved true. And it was essentially mechanistic: to understand heredity as a property of the whole organism Darwin tried to reduce it to the interaction of constituent elements.

On all counts Lysenko's basic idea was not scientific speculation but an atavistic simulacrum of it, recalling Albertus Magnus[100] rather than Charles Darwin. Even when he seemed on the verge of reducing the hereditary property of the whole organism to the action of special particles within it, in his famous explanation of species transformation, Lysenko kept from falling into the mechanistic mode of thought:

> We conceive the matter as follows: In the body of a wheat plant organism, under the influence of appropriate conditions of life, granules [*krupinki*] of a rye body are born. But this birth does not arise by means of a transformation of the old into the new, in this case of wheat cells into rye cells, but by means of the rise in the depths [*nedrakh*] of the body of the organism of a given species, out of substance [*veshchestvo*] that does not have cellular structure, of granules of the body of another species.[101]

Lysenko's granules, unlike Darwin's gemmules, dissolved into an intangible substance even as he talked about them. It was pointless to ask how one might test for them, not only because the facts of cytology ruled out their existence, but even more basically because the mind had nothing to grasp in thinking about them. Anyhow Lysenko was far from requesting or tolerating tests. He ignored even the modest request for clear photographs of rye seeds growing on wheat plants; a careful search by a reliable man had turned up rye seeds in harvested wheat, and that was proof enough for Lysenko.[102] We will ignore his defense of simple frauds, who doctored photographs to show hornbeam trees producing hazelnut limbs and pine spruce,[103] as we will ignore his tacit approval of complex frauds, who made belated, grotesquely illogical efforts to prove that the master's doctrine of heredity was compatible with data concerning nucleic acids.[104] We are trying to understand Lysenkoite thought at its best, in its purest formlessness.

Just as the Lysenkoites resisted the attribution of the hereditary function to particular material forms, so too they resisted the reduction of species formation to the mechanistic processes of natural and artificial selection. At first their resistance seemed little more than a verbal shuffle: they insisted that selection does more than favor certain variations from the type, it "creates" them in some unspecified way. Then they specified: variation is directed by grafting and by alteration of the environment. Then they came to the sudden creation, "under suitable conditions," of new species in the depths of the body of the old. At the same time Lysenko's cluster method of tree planting also led on to his grand summation, "the law of life of a biological species." He saw that individual saplings do not

die because they are deprived of light and moisture by surrounding trees; the scrawny ones die so that the healthy may live and the species flourish.[105] Even the roots of the self-sacrificing saplings know their duty; they graft themselves onto the roots of healthy trees to help the species live though the individual dies.

> If we proceed from the law of life of a species, then we begin to understand not only the causes of roots growing together, in other words, not only the transfer of roots from a tree that is internally ready to die to those that are not as yet ready to die, but also [we understand] why the growing together goes on not at the time when the tree is drying up and dying, but at the proper time, in some cases many years ahead of that. The roots grow together not because they touch one another. On the contrary, not infrequently they touch in order to grow together.[106]

The farsighted roots act in accordance with the species' law of life:

> The entire life of an organism and of any part of its body is directed in one way or another to the multiplication, to the increase of the mass of that biological species, one of whose forms of existence is the given organism, the given living body. . . . In any being (organism), in any particle of a living body, everything is directed to one and the same thing, to one "purpose" — to the increase of the living mass, the numbers of the biological species; but not any species, only that one which the given organism or the given particle of the living body is, the one to which it belongs.[107]

One is tempted to say that the Lysenkoites were simply vitalists, though prevented from admitting it by the Marxist philosophical tradition.[108] Yet their mode of thought was essentially anti-intellectual; they were incapable of creating a coherent, fully developed doctrine, whether mechanistic or vitalistic. They simply would not be pinned down, which is the function of any articulated body of thought. Consider, for example, their talk of "marriage for love" among plants and animals. The phrase was intentionally playful, designed to offend the mechanistic sensibilities of most biologists, but the underlying thought was quite serious. Fertilization, in the Lysenkoite view, was not a random process; sperms seek and eggs select their most suitable mates. This anthropomorphic language was not merely a handy way of pointing to the subtle mechanisms by which, say, pistil and pollen of the same species recognize their compatibility and mate. Biologists frequently use such anthropomorphic shorthand without ceasing to be scientists, for most of them are constantly searching for the mechanisms that accomplish the goal-directed feats of mindless protoplasm. Even those few, the vitalists, who insist that mechanistic explanations are not enough, begin their arguments for something more (life force, entelechy, or whatever) at the point where the mechanists allegedly fall short. They try, in short, to speak to the scientific community in its own language. Not so the Lysenkoites. Very belatedly they made some clumsy efforts in that direction, during the last decade of their politically sustained existence as a school. As their power to repress or ignore biochemical studies of life processes dwindled, they conceded that these studies were useful. Some Lysenkoites even looked through them, hunting proof of their inadequacy, insisting on the need for purely biological laws, such as "selective fertilization," the dignified name for "marriage for love." Lysenko himself never bothered with such donkeywork, and the assistants to whom it fell were so tendentious and unconvincing as to make one wonder whose side they were really on.[109]

The plain fact is that "marriage for love," like most of the pet notions of Lysenkoism, originated and ended as an intuitive rationalization of an agricultural practice, in contemptuous indifference both to the scientific study of fertilization and to the vitalists' intellectual dissatisfaction with that study. In the 1930s Lysenko decided that pure varieties of rye and other cross-pollinating plants do not have to be planted as far from each other as the seed laws required — until 1938, when he got them changed.[110] There was no reason, he said, to fear that proximity would pollute varietal purity, for the pistils of each variety could be counted on to choose the pollen most suitable to them:

> It has been proved by Darwinism, and also confirmed by special experiments, that in all those cases when the economic demands presented to a given plant coincide with its biological demands, in all those cases it will never be useful, and in some cases it will even be harmful, to limit the freedom of cross-pollination.[111]

Passages like this suggest not the scientific but the bucolic type of vitalism, such as D. H. Lawrence's hymns to the natural union of men with their living environment. And in fact the Lysenkoites did get a reputation for opposing artificial insemination of livestock on the ground, as a critic sneered, that it does not satisfy "the 'demands' of the cow, the ewe, or the mare." [112]

But once again we are dealing with a vestigial or abortive suggestion of coherent thought, in this case poetic, in all cases the

product of an incoherent eagerness for sensationally quick agricultural progress (or for quick sensations of progress). Lysenkoite talk of "marriage for love" was an escape, by adman fantasy, from the painfully slow struggle to achieve varietal purity in peasant agriculture. In this case as in much else Lysenkoite vitalism was neither scientific nor bucolic. It was an agitprop (Soviet adman) pseudo-activist version of the famous *nichego!* (no matter!), with which Russian peasants responded to criticism of their slovenly fields. The traditional peasants may not have romanced about "marriage for love" in the plant world, but they did tell stories of cultivated plants changing into weeds long before Lysenko came along with his semi-literate version of the ancient fable. They saw it happen all the time.

THE AUTONOMY OF SCIENTISTS

This then was Lysenko's "genetics." Small wonder that Serebrovskii shouted "*Mrakobes!*" ("Obscurantist!" or, literally, "Demon of darkness!")[113] in his first debate with Lysenko, or that M. M. Zavadovskii issued a sober declaration of war in 1936:

> Proceeding from the need to be honest with myself and in the interests of the proper search for truth, I am obliged to say that Lysenko proposes to us the replacement of Mendel's conception by a miserable, wretched, primitive conception, unworthy of his remarkable powers.
>
> Compare, Comrade Lysenko, your crude homemade fabrication, which allows the most extreme teleological and vitalist explanations . . . with the coherent, finished, splendidly unambiguous conception of contemporary science. . . .[114]

The solemnity of this declaration, with its inconsistent tribute to Lysenko's unspecified powers,[115] suggests what must be obvious to anyone who has read the previous chapters of this book. Great courage was required to speak these plain truths in 1936,[116] and even more in the years following, until 1948, when criticism of Lysenko's doctrine was totally prohibited and the courageous specialists were those who fell into refractory silence.

It is hardly surprising that only a handful of geneticists and allied specialists (M. M. Zavadovskii was an embryologist) consistently displayed such courage, not only in the years of accumulating disaster, but also in the years of painfully slow recovery, when terror no longer threatened the scientific critic of Lysenkoism, but jobs and other prerogatives could still be forfeited by radical truthtelling.

Some people will automatically assume that the handful of bold radicals spoke for all their scientific comrades in genetics and the allied disciplines. This romantic expectation will be doubted by those who regard scientists as a species of white collar worker — *sluzhashchie*, serving people, is the Soviet term for the class — dedicated above all to climbing the ladder of reputation and position. Cynics will even expect to find mass conversion from science to Lysenkoism between 1935 and 1952, and the other way round during the next thirteen years, as political support moved to and from Lysenkoism. Fortunately we need not limit ourselves to idle guessing, for scientists make a public record of their behavior when they are faced with a conflict between professional and political considerations. When we examine this record, we find elements of truth in all three expectations. There were intransigent professionals, there were complete opportunists, and there were a lot of people who tried to avoid the choice between those extremes.

When Lysenko first invaded breeding and genetics, a number of important people in those fields tried to reach a compromise with him, as they had with Michurin. The most important would-be comprimiser was Vavilov, director of the Institute of Genetics at the Academy of Sciences and simultaneously of VIR (All-Union Institute of Plant Industry), which were the country's two most important centers of pure and applied genetics respectively. In striking contrast to bold alarmists like Serebrovskii, Vavilov tried to defend science by appeasement of its enemies. He praised Lysenko's theory of stages as

> a major achievement in plant science. It discloses broad horizons. We have not yet fully utilized this radical new approach to a plant, which, in addition to its significance as an agricultural technique, has supplied a new theory of breeding, and allows us to utilize more fully the world's resources of varieties.[117]

Vavilov chose his words carefully, so as to allow a little gentle criticism along with the strong praise, trying to win Lysenko away from his most flagrant attacks on science. But abuse was all he got for his pains. At the decisive conference of December 1936, a protégé who was defecting to Lysenkoism ridiculed Vavilov for his "heterozygous condition, or rather, the condition of a vegetative chimera, whose individual parts are incompatible. He is both . . . a Michurinist and an anti-Michurinist; he is both a Lysenkoite and an anti-Lysenkoite."[118] That may have been the unbearable limit of

degrading diplomacy for Vavilov; or maybe, personal feelings aside, the conference as a whole proved to him the impossibility of compromise. Whatever the cause, he changed. His concluding remarks were quite different from the bland soothing syrup with which he had opened the conference. Firmly and neatly he took the side of the embattled geneticists, and stayed there until his arrest in August 1940.[119]

Vavilov was a very unusual individual, but the pattern of his response to Lysenkoism was symptomatic. Many other scientists, especially those in administrative positions, tried to protect their institutes by flattering the Lysenkoites. In plant physiology such pliable men of principle held most of the most important posts through the whole period from the rise of Lysenkoism in the 1930s to its collapse in the 1960s. In genetics a different pattern became apparent very early. Not only did this discipline produce utterly intransigent critics of Lysenkoism — the severest plant physiologists were not nearly as hard-hitting — but would-be compromisers like Vavilov were quickly driven to choose between utter intransigence and utter capitulation. Zhebrak, to take another example, was a peasant's son and Party member from the age of seventeen, had experienced the condescension of aristocratic scientists,[120] and was capable of seeing reactionary connotations in Vavilov's law of homologous series.[121] Yet he felt compelled to follow Vavilov into the camp of intransigence. Even as Zhebrak wrote a major appeal for reconciliation, in the spring of 1937, he was attacked for "denigrating" the achievements of Lysenkoites.[122] That was part of their campaign to drive genetics from higher education. Zhebrak, who had been "pushed up" at age thirty-four to be chairman of the Department of Cytogenetics at the Timiriazev Academy, the country's leading institution of higher education in agriculture, became one of the most unyielding, durable opponents of Lysenkoism.[123] His was one of the last eight centers of research and teaching in genetics that still remained in 1948, when the August Session precipitated their closing, and Zhebrak felt obliged to publish a wishy-washy self-criticism in *Pravda*.[124] The editors demanded total surrender, and he responded, when he got a professorship of botany at the Moscow Pharmaceutical Institute, by creating a little center of genetics there *sub rosa*.

If the terror had not snatched Meister in 1937, he too might have joined the militant defenders of genetics, though he seemed at the time of his disappearance to be the conciliator most favored by the ideological establishment. A plant breeder with a strong interest in theoretical genetics, Meister was codirector of the All-Union Institute of Grain Culture in Saratov, the celebrated creator of several famous varieties of wheat. His effort to recast genetics on the basis of dialectical materialism[125] — he had joined the Party in 1930 at age fifty-seven — was treated respectfully by the ideological establishment, and his summation of the Conference of December 1936 was officially billed as "the platform that can unite all the participants in the discussion." [126] But a careful examination of Meister's arguments makes it clear that he could not have maintained much longer his appearance of evenhanded criticism and praise for geneticists and Lysenkoites alike. On all essential issues he stood with the geneticists. Yet the chances were small that he would have said so openly and strongly. He was after all a plant breeder, with an option that was unavailable to most of the pure geneticists: he could occupy himself with wheat improvement and keep his mouth shut about the principles of genetics. That was the dominant trend among eminent Soviet plant breeders. At the Conference of December 1936, fourteen of the twenty-four people who criticized Lysenkoism were plant breeders; by the August Session of 1948 the ratio dropped to one of nine. And already in December 1936, almost half of the seventy-seven plant breeders who were prevailed upon to speak evaded the disputed issues, limiting themselves to a description of their creations.[127]

Of course, silence can be a form of protest. At the August Session, for example, though only one plant breeder criticized Lysenkoism, only one with any serious claim to distinction spoke for it, which went far to sustain the geneticists' argument that the best breeders were not Lysenkoites.[128] Two of the anti-Lysenkoite speakers went further; they explicitly claimed for genetics the celebrated varieties produced by Konstantinov and Shekhurdin, famous plant breeders who had publicly criticized Lysenko in the 1930s and now, at the August Session, sat quietly in the crowd of seven hundred. When Lysenko and his lieutenants called on them to repudiate any connection with reactionary Mendelism, and still they sat quietly, silence became a distinctly eloquent form of protest.[129] It tells us, to be sure, even less than polemical speeches about the roles that were actually played by genetics and by Lysenkoism in the day-to-day work of breeding improved varieties. That practical question will be considered later on. Here we are examining the academic issues that Lysenko raised, as he gathered political power to crush the science of genetics. Plant breeders could rise to the defense of the science, as a dwindling minority did; or they could avoid the conflict, as most of them did, by falling silent on contested issues or by making occasional Lysenkoite noises. They could even enlist in the Lysenkoite army, as a minority did, without necessarily and completely betraying their professional commitment to breed improved varieties.[130] That range of choice was denied to pure geneticists. Whether they engaged in militant defense of their discipline, or made compromising efforts to appease its enemies, or quietly concentrated on their special research and teaching, sooner or later the stark choice was forced upon them: to honor their professional commitment, or to betray it. If they honored it, they would almost certainly be fired from their jobs, and might follow Vavilov to prison or Agol to execution. If they betrayed it, there was far less chance that the terror would snatch them, and there was virtual certainty that they would get or keep high position.

Only the barest handful of people who were trained geneticists in the mid-1930s, when the trial began, chose to betray their professional commitment. Of the thirty-five people who were staff members at the Academy of Science's Institute of Genetics in 1937, only four turned Lysenkoite and kept their jobs past 1940, when Vavilov was arrested and Lysenko took his place as director.[131] Those four* are almost the complete list of Soviet geneticists who became Lysenkoites. Some others mouthed the phrases of conversion, especially after the August Session of 1948 made this a clear test of political loyalty.[132] Usually honest hypocrisy was evident in the confessions of these minor Galileos, as in Zhebrak's. And even in the rare cases when a Communist geneticist made a strenuous effort to be simultaneously loyal both to his scientific and to his political disciplines, the scientific won out over the political.

Alikhanian is a fine specimen of this rare but significant type, whose behavior illuminates the clash of the two disciplines. During the turmoil of the late 1930s he was a shrewd young geneticist and Party leader at Moscow University, taking a major part in "the liquidation of wrecking," and enthusiastically endorsing a chimerical textbook that had Lysenkoite passages interspersed in the usual ex-

* They deserve to be known: R. L. Dozortseva, K. V. Kosikov, Kh. F. Kushner, and N. I. Nuzhdin.

position of genetics.[133] To answer the charge that genetics was practically useless, he began to breed an improved variety of chicken, and won official praise at the conference of October 1939, where he described his chicken and criticized the Lysenkoites for trying to abolish genetics.[134] Subsequently he dropped chickenbreeding and proved his loyalty by sacrificing a leg in the war against German aggression. At the time of the August Session in 1948, Serebrovskii had just died and Alikhanian had taken his place as chairman of Moscow University's department of genetics, though he was only a *dotsent* in academic rank. Thus he was a prime, vulnerable target for Lysenkoite hostility toward the university that had been a major center of anti-Lysenkoite agitation. (As late as February 1948, a large conference had gathered there to defend natural selection.) Great pressure obliged Alikhanian to mount the tribune and tell the August Session that genetics was a true and useful science, fully compatible with Michurinism.[135] Lysenko shook him up with heckling, but he rallied and stuck to his views until the very end of the conference, when Lysenko announced that the Central Committee had read and approved his report. The audience responded with a stormy standing ovation, and Alikhanian joined two other anti-Lysenkoite speakers (out of a total of nine) in instant recantation.[136] His seemed the most complete, utterly lacking the quibbles and cavils of the other two, entirely rejecting his science in favor of "the morality of the state, the morality of the people [*narod*]," as another repentant biologist at another meeting described the force that brought him to his knees.[137]

It seems likely that Alikhanian was entirely sincere as he delivered his recantation:

> It is important to understand that we must be on this side of the scientific barricades, with our Party, with our Soviet science. . . . I, as a Communist, cannot and must not, in the heat of polemics, directly oppose my own personal views and concepts to the whole forward movement of the development of biological science. . . . As of tomorrow I will not only begin to emancipate all my own scientific activity from the old reactionary Weismanist-Morganist views, but I will also begin to remake, to transform all my students and colleagues.
>
> It is impossible to conceal the fact that this will be an extremely difficult and agonizing process. . . . I categorically declare to my colleagues that in the future I will fight with those who formerly thought as I did, unless they understand and go along with the Michurinist trend. . . . We will transform Moscow State University into a center of propaganda of the Michurinist doctrine, into a center of work on Michurinist biology.[138]

He was a dutiful Communist. But he was also a professional geneticist, and as soon as he got the chance he resumed research in his science, within the shelter of the Institute of Atomic Energy. He was almost certainly insincere when he included in his description of the results patent nonsense about vegetative hybridization — of *Penicillium*.[139] As the Lysenkoites lost the support of Party leaders Alikhanian dropped such talk, and eighteen years after the August Session, when he was restored to his position at Moscow University, he published the study of Michurin he had outlined in 1948, a tendentious argument that Michurin's views were fully compatible with genetics.[140] That has been the official view since Lysenko's fall. Once again Alikhanian has every reason to believe that there is no conflict between his political and scientific disciplines. After all, he is a geneticist, not an historian of ideas.

The stubbornness of the people who were already trained geneticists when Lysenkoism invaded their field is clear evidence that their professional commitment was stronger than their political, even in the rare case of Alikhanian, who made a great effort to be loyal to both. Nevertheless the strength of the professional commitment should not be exaggerated. It provoked only a small number of geneticists to challenge the authorities directly, by publishing criticism of Lysenko when that was frowned upon, and by falling into eloquent silence when demands for recantation were addressed expressly to them. The majority of geneticists did their work quietly until they were pushed away from it, and returned to it just as quietly — those that survived — when the political bosses allowed them to return. Even the minority who publicly criticized Lysenkoism were usually very circumspect, carefully avoiding an open and explicit attack on the political authorities who stood behind it. With very few exceptions they shied away from the basic issues of agricultural recipes and of academic autonomy, confining themselves almost entirely to criticism of Lysenkoite views in pure science. When the authorities reimposed a ban even on scientific polemics, between 1959 and 1964, almost no defiant articles were published by geneticists.[141] That kind of daring was left to a small group of Varangians, as an admiring journalist called the chemists, physicists, and mathematicians who ventured out of their special fields in defense of genetics.[142] The Varangian phenomenon has, naturally enough, occurred most among the scientists who have been least pressed to defend academic autonomy within their own disciplines. Among the hard-pressed biologists, as among social scientists, the rule has been don't-trouble-trouble, while an uncomfortably long list of such specialists gave endorsements to Lysenkoism when it was dominant, and stopped giving endorsements as political support ebbed away from it. If the political authorities allowed biologists a bare minimum of autonomy necessary for performance of their special services, most of them were obedient *sluzhashchie*, white-collar workers.

Of course the commitment to scientific inquiry did keep most biologists from turning into complete opportunists, it did turn nearly all geneticists into intransigent though largely silent resisters, and it did turn a few scientists into knights errant of the free spirit. But anyone who is inclined to draw romantic generalizations from these facts should read the stenographic record of a gathering at the Academy of Sciences in 1948, the aftermath of the more famous August Session.[143] The real issues had been made crystal clear and utterly unarguable by Lysenko's announcement, on the last day of the August Session, that the Central Committee had read and approved his report. It was impossible in Stalin's Russia to engage in debate with the highest political authority. Now the heroic free spirits were those who stayed away from the meeting, in particular four men whom the Lysenkoites repeatedly denounced as virtual enemies of the people for their refusal to make the required self-criticism.* Those who came and spoke were of three types. Some were Lysenkoites, celebrating their total victory with uninhibited frenzy.[144] Some were honest hypocrites like the plant physiologist Maksimov or the ecologist Sukachev, who humiliated themselves in order to save their institutes. And some were complete careerists, intent on securing their positions regardless of other individuals or principles. A prime specimen of the last type was the histologist Khrushchov, director of the Institute of Cytology, Histology, and Embryology, which had continued to be one of the main centers of resistance to Lysenkoism even after it had been taken from its founder and director, Kol'tsov. Director Khrushchov labored to

* They deserve to be known: the evolutionary theorist Shmal'gauzen (or Schmalhausen), and the geneticists Dubinin, Rapoport, and Zhebrak. I. V. Pan'shin and Timofeev-Ressovskii, who could not attend because they were in prison, were denounced as actual enemies of the people. The principal denouncers were Glushchenko and Kushner.

prove that not he but this, that, and the other subordinate, working behind his back and over his head, had fostered Mendelism-Morganism in the Institute.[145] Similar speeches were delivered by a neurophysiologist and two leading philosophers.[146] Each philosopher tried to convict the other of responsibility for the evasions and compromises that had been the characteristic reaction of the philosophical establishment to Lysenkoism. Only an obscene metaphor can do justice to this meeting. The Lysenkoites had forced political salts into the bowels of Soviet scientists, and some began to void themselves in public, the honorable ones fouling themselves alone, the dishonorable trying to rub it off on others.

In judging the strength of the scientist's professional commitment when it conflicted with political loyalty, one is tempted to ignore the army of Lysenkoites. The bulk of them were probably ignorant opportunists, that is, people who understood only political discipline, who could not betray a scientific commitment for the simple reason that they did not understand it. Lysenkoite publications are the main evidence for this judgment; they tended to be as primitive caricatures of scientific reasoning as the model set by Lysenko himself. That is hardly surprising; as late as 1948 the Minister of Higher Education characterized the Lysenkoites as mostly young, mostly in agricultural lines of work, and mostly in the provinces.[147] Increasingly they were the products of an educational system that was reorganized in the late 1930s and 1940s to produce Lysenkoite pseudoscientists. But some of these ignorant opportunists, if they worked hard at beating down the criticisms of scientists, became learned opportunists — in plain English, conscious frauds. They learned the scientific standards for distinguishing right and wrong merely to use them sophistically, while actually adhering to the political standard (truth is the opinion of influential people). This type, which understands the scientific commitment but places no value on it, is worth considering. What prevented them from destroying the scientific community, from turning it into a mere instrument of the political bosses?

Consider the eminent example of Glushchenko. Trained as an agronomist, he was one of the first young men to do postgraduate work under Lysenko at Odessa in the 1930s. There he was given the job of proving that the master's intuitions were right. Simply ignoring the elementary rules of genetical experimentation, he "proved" that self-pollinating plants such as wheat will degenerate unless forced to crossbreed. In the same crude way he "proved" that cross-pollinating plants such as rye may freely breed according to their nature, without fear of varietal degeneration, for they practice "marriage for love." [148] In 1939, after the Council of People's Commissars had ordered Vavilov to start some Lysenkoite work in the Institute of Genetics, Glushchenko moved into that main center of research in pure genetics.[149] When Vavilov was arrested and Lysenko became director, Glushchenko became head of the laboratory of plant genetics, the country's leading authority on vegetative hybridization. That was the equivalent of being the country's chief authority on squaring the circle or building perpetual-motion machines, and it is a measure of Glushchenko's native intelligence and industry that he got to the point where he did not sound like a semiliterate crank. He got to be almost as skillful as Kushner and Nuzhdin, who had been properly trained in genetics before they became Lysenkoites.

All three were masters of sophistry designed to prove that genetics was breaking down because of a growing contradiction between its empirical data and its metaphysical concepts.[150] Glushchenko became the Soviet Union's leading "geneticist" in foreign lands, as the absolute isolation of late Stalinism gave way to restricted intercourse.[151] (Real geneticists were almost always judged unsafe for travel abroad.) Thus he reinforced the belief of a few foreign biologists that the Lysenkoites constituted "the Soviet school of genetics," somewhat akin in their views to Hinshelwood or Waddington, who doubt that natural selection acting on random mutations and genic recombination can fully explain such things as the great persistent patterns of evolution or the adaptations of microorganisms.[152] Of course, belief in that kinship does not survive a single hour of comparative reading of the alleged cousins. But the fact that Lysenkoite opportunists tried to foster such a belief during their final decade — at the height of their power they spat on the very thought of world science — betrayed their fatal weakness. As they lost the power to say "we are the boss," Lysenkoite opportunists had nothing to say except "we agree with the boss." When Lysenko and the hard core of militant ignoramuses lost all power, Glushchenko and the few other learned opportunists simply dropped their Lysenkoite sophistry and began, or resumed, the speech of ordinary geneticists.[153] In an important sense it is meaningless to say that they were frauds. Like the ideal type of politician they were simply indifferent to any rule except the necessity of pleasing influential people.

Thus we can see why learned opportunists were unable to destroy scientific communities, whether of geneticists or plant physiologists, or any other where they provided militant ignoramuses with apologetics. The growing need for apologetics was itself a sign that their days were numbered. Political bosses, increasingly aware that pseudoscience was not passing the tests of "practice," were sullenly preparing to restore autonomy to the scientific communities, as the only way to get real help in the improvement of agriculture. It was very hard for them to admit that they had made a losing bet on the militant ignoramuses, and they had some worries about the political consequences of restoring full autonomy to more and more communities of scientists. But Stalin's successors had no more interest than he in the theoretical issues of natural science. They backed ignoramuses in a number of fields for a long time — in some of the social sciences they are still doing so — as protection against the discouraging truth that came from autonomous communities of science. The sophistic chatter of learned opportunists merely served to fill the empty days of worry, while the bosses tried to resist the painful admission that they were dependent on yet another autonomous community of truth-tellers. The moment the fateful admission was muttered,[154] the Lysenkoite army of opportunists dissolved, the learned ones showing the way for the ignorant to follow. Scientific communities were there to receive them. The stubborn adherence of white-collar workers to the rules of their trade, the heroic agitation of a minority, and the fraternal help of an even smaller minority of Varangians had kept those communities alive through a thirty-year period when Stalinist bosses believed them useless. That is one of the most remarkable achievements of Soviet scientists. It is also evidence of some residual rationality in the outlook of Stalinist bosses.