PROBLEMS OF THE SCIENCE OF SCIENCE [1946] *

LUDWIK FLECK

It is highly interesting to establish to what extent scientists who devote the whole of their life to the problem of distinguishing illusions from reality are unable to distinguish their own dreams about science from the true form of science.

In the first place there does not exist, beyond dreams, only one kind of science; there are at present only some specific sciences which, in many instances, lack any connection among themselves, and which are sometimes divergent in their basic features. We can discuss science only in the same way in which we use the word 'art' to document the common nature of trends in music, painting, poetry, etc. Similarly all sciences possess a common trend towards an ideal end-state which is known as true knowledge. But just as art is not the sum total of music, painting, poetry, etc, so also sciences do not add up to form a consistent homogeneous whole.

For instance, the connection between linguistics and chemistry is indeed very slender. Let us assume that this should be not so, let us even admit that this will be not so at some time in the future – but, before this happens, both chemistry and linguistics will undergo changes. Today's chemistry is far distant from today's linguistics.

Moreover, no science comprises an objective image of the world, even in the sense of its one-to-one semantical representation. It does not even contain any part of such a representation. Were it so, we would have in science a constant, unchanging part, and scientific knowledge would grow due to simple increase of information; now experience does teach us that this knowledge changes continuously as a whole. Even the most certain, basic elements undergo changes. Every specialist will distinguish an old textbook of his science from the new one: the former is completely anachronistic. If it is a textbook of physics, chemistry or bacteriology of 1910 or 1920, we [in

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1946] shall immediately find that it is obsolete, not only because of the lack of more recent discoveries, but from the trend of basic reasoning.

Sciences do not grow as crystals, by apposition, but rather as living organisms, by developing every, or almost every, detail in harmony with the whole.

I may ignore the constant, final results in my field, but I do know that every result becomes, sooner or later, a source of new problems, and if these problems are solved, then the old results have a sense which differs from that assumed by the author himself. I know that scientific workers often try to reason themselves or other people into believing that it is they themselves who had foreseen this new sense by way of some miraculous intuition; however, documents show that this is not the case. One sees sometimes bricks of one's own work built into the building constructed by other workers. And the author is surprised as a matter of fact that this particular surface has been used for the front face after polishing, while other surfaces were hidden. Sometimes a certain edge of this brick, enhanced by hewing, becomes a part of an ornament which had not been included in the plan. Sometimes one would like to retract, as wrong and unable to exist, one's own thought which had been published at an earlier date – only to find with stupefaction that this very idea had developed and grown up into the scientific surroundings. Scientific results have their own life and pass through their own vicissitudes – changing faster with the faster development of science. It is only the prejudices and superstitutions that last, changeless, over centuries, being similar in this respect to the tautological theorems of mathematics or logic.

One would assume that continuous changeability is a transitional state which proves the imperfection of today's science and its tendency towards improvement; that a final state which does not undergo any changes is possible and that we are nearing this state. True, no science at present contains an assured portion of the objective picture of the world, but all sciences are getting nearer and nearer to it.

Since every major discovery has its repercussions upon the whole of science, such a final state, were it only for one only major problem, would be attainable only after all of the problems had been solved. But what does 'all of the problems' mean, when new problems can arise again and again? One would have to stop the movement of the planets, the vibration of pollen grains in air, the evolution of living beings and – what is most important – the movement of human thought; otherwise there will arise new, unexpected problems whose solution will force us to revise the entire system.

Simplicius. You are wrong; the number of truly separate problems is

limited. As sciences make progress, entire groups of problems will be reduced to only one fundamental problem, and, in the first place, the apparent problems will be eliminated.

Sympatius. Well, the final state will look as follows: A finished *Codex Pansophiae* and, as an indispensable addition, the *Commentary* to that *Codex*, which would contain, in the first place, all principles of transformation, reduction and elimination of problems. Thus, e.g., the Codex would include no mention about the problem of the philosopher's stone, but the Commentary would contain a comprehensive article about the development of chemistry from alchemy, and references to the Codex, to the chapter of physics dealing with the transformation of elements, to the biology chapter dealing with hormones, old age and death (*elixir vitae*), to the pathology chapter dealing with illnesses which this stone is likely to cure. On the other hand, the entire mass of practical questions derived from chemistry, viz. as regards the solubility of a certain substance, its melting point, its optical properties etc. – the commentary would refer to the one only formula in the Codex, from which all this could be deduced. The Commentary would likewise contain prescriptions for use of this formula for practical purposes. Every specialist from a foreign field, looking for an answer to the problem would have in the first place to identify it and to transform in the Commentary. A stubborn philosopher would not find any answer in the Codex to the problems of the Absolute, the primary cause, the Idea of well-being, the Essence of all things, but all this would be in the Commentary along with the explanation of the elimination of these questions. A pupil who would like to know what does the wind do when it does not blow, or why did it appear to sophists that Achilles would not be able to overtake the tortoise, and many similar things, will find these problems not in the Codex, but in the Commentary, where it was clearly explained to the questioner why such questions must not be put.

Simplicius. Rightly so.

Sympatius. I doubt whether logic (along with the logistics beloved by logic) would be found in the Codex or in the Commentary. And what about mathematics? And theoretical physics? In other words, I fear that the Commentary will be much more interesting than the Codex. It will contain the entire history of the sciences, all of the directly practical problems, 99% of philosophy, probably the formal sciences, and popular science for the half-educated ones, as people will probably not be born with a complete education. The Commentaries will also include practical regulations for experimenters, for, even when the experiments of discovery will be unnecessary, they still will remain indispensable for practical purposes.

On the other hand, I believe that the *Codex Pansophiae* will be, once for all, composed by an international commission in the form of a collection of formulae and graphs, arranged in a theoretically grounded order. Instead of the table of contents we shall have on its first page the formula of this order. The graphs of the text will be 'n-dimensional coloured stereo-graphs', to be seen through spectacles with glasses which would change their colour $20(n - 2)$ times per second, in order that the details of the graph, when watched successively, would yield the impression of n dimensions, just as the illusion of movement arises in the cinema. What a magnificent experience it will be to watch such a graph! It would look for all the world like Faust above the sign of the Cosmos!

Since a daredevil among the commentators could desire to change the system of the codex (under the pretext of an improvement), and this could give rise to unpleasant shocks, it will be necessary to give the codex the protection of the law. On the contrary, the commentaries will eagerly change, improve, develop . . . .

Do not you think, Simplicius, that, in such a case, your *Pansophia* would remain unchanged only thanks to the police force and that it would become similar to those dead regulations, such as rituals of religious cults, and that it is these commentaries that would become science proper? That new, unofficial codices would arise again and again, which would acquire more and more adherents? That, indeed, there would be basically no difference between that final state and the present one? That, therefore, this 'final state' – to use your own language – has no meaning at all? If you want, you can assume that we have already reached that state: *panta rei* – such is the codex of all things or, if you prefer it; $A = A$. Everything else – are commentaries to that codex. On the contrary, if you admit that this knowledge is too vague and you want to have a completely detailed and precise knowledge, you have to consider that the universe itself is a system of total-knowledge, while our science is a commentary to it.

Simplicius. I think you are exaggerating again. You will not gainsay that today's science is closer to the objective picture of the world than the science of 100 years ago? And besides, this 'codex', as you call it, must not be separated from the commentaries. On the contrary, beside the positive, exact and certain side of our scientific cognition, we must mention historical data, surmounted errors, pedagogic notes, practical indications, everything in a freer, less exact, tone, in other words artistically.

Sympatius. Thank you for the hope of saving artistry. I am sorry not to be able to reciprocate your compliance. I do not think that today's science is

closer to the objective picture of the world than the science of 100 years ago. On the other hand, I am sure that today's science is closer to our world of today, while the science of 100 years ago was closer to what was then the world of the creators of science. You yourself do affirm that *consensus omnium* is the ultimate touchstone of science. Do the unborn have a say in this parliament? Do the grandsons vote instead of the grandfathers? In such a case I can assure you that, to our grandsons, the science of 1940 will not seem much better than that of 1840. I am convinced that the progress of science will, in the future, proceed very rapidly, and that ten years will mean more than 100 years before. We, the science workers, are more numerous today than 100 years ago, we have a longer history behind us, our world contains more details, it is more complex – that is why our science is vaster, richer in details and more profound, because of a larger number of intra-scientific relations, but that is all. If the 'final state of science' does not mean anything, is one entitled to discuss the process of getting nearer to it?

Simplicius: I am afraid, Sympatius, that your ultra-criticism and your exaggerated relativism will lead to a barren scepticism. There must be a certain solid and stable foundation of science, otherwise the entire building would be top-heavy. The contemporary splendid technology and its farther possibilities sufficiently justify our science. Our cognitive, technological and mental apparatus becomes better and better – and science is ever progressing!

Sympatius: Science is not a terrestrial building which stands on a foundation, with an attic at the top. Science is rather like a round fruit, with a juicy pulp, and a thick, indigestible skin. It may be turned at will, the base can be the top, or the top the base, depending on our desire, but they are both equally tough and indigestible. Only the center of science is useful, while the foundations of mathematics, physics, chemistry, biology are equally tough, doubtful, probably useless, and the top is also the same. In order for this miraculous fruit to grow, it must be taken between two fires: the hot, though dark, fire of romanticism and the cold, but bright, fire of scepticism. For, the romantic daydreaming of the creator is equally as necessary as the envious scepticism of competitors. I would even say that it is precisely this envy that creates the social value of cognition, by deprivatizing the results. The aim of my inferences is not to belittle the value of science, but, on the contrary, to raise it.

There are people who think that one can construct a science of cognition without fundamental observations, experiments and studies in this field. They even believe such observations and studies to be superfluous as they know everything beforehand, by embracing idealism or materialism, intuitionism or

conventionalism, positivism or realism. It is on the basis of a few anecdotes from the history of the sciences, a few of their own life experiences and a large number of suggestions from other people, that they assume a 'world outlook' which explains everything to them.

One cannot look upon the sciences as being only a set of sentences or a system of thoughts. They are complex cultural phenomena, at one time perhaps individual, at present collective ones, made up of separate institutions, separate actions, separate events. Written sentences, unwritten customs, one's own aims, methods, traditions, development. Preparation of the mind, cleverness of hands. A special organizational structure, with its hierarchy, ways of communication and co-operation, an organizational court, public opinion, press and congresses. A distinct relation to other aspects of cultural life, to society, to the state, etc.

I had a very rare opportunity of watching, for nearly two years, the scientific work of a collective composed of laymen only. The results of this observation explain some problems of the science of science much better than speculative discussions. The collective worked on complex problems from the field of typhus; they had at their disposal fully equipped laboratories, plenty of experimental animals and an extensive specialist literature. This was in the Buchenwald concentration camp (Thuringia), so there was a tragic responsibility as far as the results were concerned; and the workers were thrown upon their own resources for, though the German *Leiter* did hold, true, a wartime doctor's diploma, yet his specialist education was non-existent. His role consisted in supplying materials and urging his subordinates to work.

The collective consisted of: (1) a young Polish physician, without any specialist preparation; he was the head of the collective; (2) a doctor of laws and philosophy – an eminent Austrian political figure; (3) a worker from a factory making rubber articles – a German Communist activist; (4) a young Czech physician, with rudiments of bacteriological preparation; (5) a practising Czech veterinarian, without bacteriological preparation; (6) a Dutch student of biology, with his assistant, a student of the 3rd or 4th year of medical studies; and (7) a Vienna confectioner. I did not belong to this collective, nor did I take part in its work, but I was able to observe it from a direct vicinity. One of the tasks given to the collective was: to examine whether the germ of typhus (*Rickettsia prowazecki*) is found in the lungs of mice and rabbits which had been infected by a certain method. True, the workers had never seen Rickettsia, nor did they know the standard bacterial flora of lungs and bronchi. Likewise they did not know the cellular elements

of these organs. Consequently, they had to learn to see elementary things on the basis of descriptions and illustrations, i.e. to pass, so to say, backwards along the path normally chosen for knowledge.

There were two descriptions of *Rickettsias*: the older by the German lady-author Sikora, and the more modern one by M. Giroud, a Frenchman. Both contain descriptions and illustrations of the complex developmental cycle of these microbes, though not an unquestionable one. Now the members of the collective found in their microscopic preparations, which had been conducted with an extreme meticulousness according to the instructions contained in books, all stages of the developmental cycle of *Rickettsias* and the required sequence – and yet, at that time, the germ was completely absent from their material: from the dyestuff precipitates, fat globules, various bacteria and cellular remnants they managed to arrange the entire developmental cycle. This did not happen at once. This construction grew slowly, in the atmosphere of a mutual stimulation and strengthening of opinions. The collective mood, which became the motor of this fantastic synthesis was composed of a tense expectation of the effect, of the desire to be the first to establish something, not to be too late with the confirmation that something had been established, and to satisfy the boss who had been urging them along all the time. Thus the components of the atmosphere were, as a rule, identical with those usually met. I have observed such a situation – the birth of discovery.

The *Boss* (reproaches the biologist with having failed, so far, to learn how to dye *Rickettsias*). Had they been properly dyed, one would have been able to see them in the preparations from the lungs of infected animals, for, according to literature, they must be there.

Biologist (to his assistant, in order to turn away the attention of the boss). Today s preparations look rather different from the usual ones.

Assistant. I kept them just a bit longer in xylene.

Biologist. What can be these shining, uniformly pink bodies? We have not seen them thus far. Is it possible . . . .

Assistant. I have noticed them, too; their presence struck me at once. Perhaps they are those *corps homogènes rouges* according to Giroud?

Biologist. This is what I was thinking.

Boss (looking into the microscope). Yes, they might be that.

Assistant. Of course, what else could they be?

Biologist. At last, we've got them.

Boss. And it is high time, too. At last something positive.

In truth, they were the eosinophilic grains from the rabbit's leukocytes,

as I found later. However, among the collective which was thirsting for a positive result, the news was spread: at long last, *Rickettsia* has been found in the preparations obtained from rabbits' lungs. When the joyful tidings spread among the collective, the certainty of the result became doubtless: the collective placed its trust in the boss, the boss relied on the opinion of his 'specialists' which he had corroborated in order to bear out his own authority, and these 'specialists' might have, at the outset, felt that this might have been something rather involuntary, but the *consensus omnium* dispelled all doubts. The confectioner and the rubber-factory worker, who represented 'common sense', popularized the discovery seriously and with appreciation. In other words, the social forces acting in the collective were identical with those usually encountered.

Next, from one link to another, the entire cycle grew. What did not fully tally with the findings was explained away by the permissible divergence of statements in this field. Why, even Giroud and Sikora fail to agree between themselves! And then, we know that biology is not mathematics. *Die unvermeidliche biologische Unexaktheit* – such was the slogan proclaimed by the Ll.D. and Ph.D. mentioned before in the list of members; he was the last critical resort of the collective.

The development of this 'science' did not proceed at all rapidly – on the contrary, one indulged long discussions and repetitions of tests. Sometimes even some findings were cancelled; in other words, admission of errors was in order.

Just as the developmental cycle of *Rickettsias*, so also the complex building of other observations and experiments grew: the guinea-pigs had temperature when inoculated with the *n*-th lung passage (in which no germs were present, and the fever came from the boils near the anus of the guinea-pig, due to unskilful insertion of the thermometer). The virulence tests, in the skin of the rabbits, yielded the expected results according to Giroud, as skin tests, in incompetent hands, always do confirm whatever we wish to expect. The immunity tests of guinea-pigs which had allegedly passed through typhus infection, were positive, for, even if the second infection produced a temperature rise, one found the cover for it in the non-existent pneumonia which was constructed by the collective daydreaming just as were those *Rickettsiae*.

This collective illusion functioned for one-and-a-half years; it was formulated in a system which did not have more logical lacunae than an average scientific output. The epoch of 'discoveries' was followed by the epoch of 'routine', with established methods, with a specific acquired fund of experience and skill. And everything was in agreement among the members

of the collective, no less and no more than in true science. The records of experiments, the summaries of results, the suggested modifications of methods were sent to the world outside the camp to genuine German specialists, men well-known in the world of science, and returned with words of praise. The German boss did get a high decoration. So great is the persuasive power of a harmonious system, and so limited is the verifying value of testing the inner harmony of the system.

An interesting shock occurred only when rabbits' lungs with typhus germs arrived from a genuine scientific institute. The preparations from these lungs showed what no description and no drawing can fully replace: the real material. But he would err who believed that a single direct contact with scientific reality would bring about the downfall of the entire edifice. Only some most extreme deviations from what was seen in the material that was received were abandoned. The collective failed even to admit in private that its entire construction was faulty; quite on the contrary, it created a synthesis of the old theory with the new facts. The members of the collective only became more careful and less naive. One can admit that a number of such shocks originating from a collective of real science would have ultimately put them on the right track to official science.

Most convincing was not the 'truthfulness' of the material obtained, but, in the first place, the respect enjoyed by the 'true institute'. I am positive that if submitted anonymously the same material would have made no impression at all. It might have been considered completely pointless, nay, it might have been utterly disregarded. I saw symptoms which entitled me to such a conclusion.

Thus we faced the following situation. A closed collective of intelligent people, left completely to their own devices, have found, when working using the standard scientific apparatus, between the scientific view of a certain field of knowledge and the observed phenomena which undoubtedly did not belong to this field, a relation which (according to the members of the collective) entitled them to state that that view was the image of those phenomena.

Simplicius. You do not describe anything out of the ordinary. We all know that it is always possible to err, to lose one's way, and to stray Lord knows how far. We know quite many such examples.

Sympatius. This is not a simple error, but a complex system of errors. What was at stake was not to establish one isolated fact (if such facts do exist at all) but the relations of numerous facts, i.e. that which we call the structure of a certain field, and which some people consider to be something

that is doubtless ascertainable intersubjectively, i.e. something with respect to which one can always find a complete mutual understanding.

Simplicius. It is so, indeed. Two different structures can be similar in one narrow field, but in every case the divergence will be discovered sooner or later. In the case just examined, the researchers would undoubtedly find sooner or later that the practical consequences of their erroneous views differ from those expected on the basis of true views.

Sympatius. Your 'sooner or later' comprises the *regressus ad infinitum*. How many structural details are to be established in order to ascertain the consistency of an image with reality, or rather: how many details in two structures ought to be compared with one another, in order to ascertain the identity of both structures? Five, or five thousand? Each later detail which had not been taken into account could be always the crucial one. We cannot compare 'all' details, because the expression 'all details' has no sense with regard to real problems.

Simplicius. In principle the more the better. But, in practice, a small number usually suffices.

Sympatius. I am glad you give practical conditions, i.e. conditions which are adhered to in scientific life. We pass thus from speculation to observation. Now, in practice – as we learn from observation – there exists, for every scientific worker or, better still, for every collective body of workers – as these are collective matters – a characteristic moment at which the worker or the collective body assume that no further verification is required. The opinion becomes rounded, systematized, limited, in short it becomes mature and obtains its form which is consistent with the thought-style of the given collective. The collective body considers that any further questions are superfluous, simply indecent. Some things must not be asked of the members of religious, political or scientific collectives. Did not you yourself speak of the elimination of certain problems from science, because they are meaningless? Yet they are meaningless only when we apply to them the style of scientific thinking. The question of the Absolute, which is meaningless for both of us, did, and will, have a deep meaning for many people who live and die for it – just as we live for the sake of Progress. Thus the problem of admitting that the relevant problems do not require any further study is a problem of thought-style. At the given moment, it is routine that enters the ring of the collective body, instead of turbulent creative thought. "One does not need to look for anything new, verification has been completed; any attempts at falsification would be against good manners. Let us quietly enjoy the fruits of our work." All of this

can be observed in the progress of events in the camp collective mentioned above.

An error of this nature, or rather such a closed, harmonious system of errors, cannot occur at all in the case of an individual work, just as a developed discovery which yields a closed system of ideas is always the result of collective work. There was no individual author of the error; the error grew out of the collective atmosphere, out of the integration of individual actions and omissions, thoughts and reticences, of misunderstandings arising because the individual A had formulated the misunderstood thought of the individual B – a thought that was not entertained by anybody, but which was often of a crucial nature, as it followed the line of the collective mood. A mood that had produced a specific observational readiness which determined what is to be the object of the attention, and what can be disregarded. Finally, the view had filled the bowl of social interest, had fallen into a systematic framework, had created for itself an axiomatic foundation, was petrified in routine, and would have lasted for centuries – had the collective lasted for centuries and had no external influences made themselves felt. No automatic process whatever would be able to give rise to corrections, to an increased experience, to reflection.

Most important in our story is the fact that – as this became evident – the social mechanism of the origination of an error is the same as that of the origination of true knowledge, as examined in the source material and the history of science.[1] The history of the fundamental chemical discoveries, the history of the transformation of the phlogiston theory into the oxygen theory and therefore the history of the composition of water, serve as a good illustration. Similarly more recent discoveries in the field of pathology or biology demonstrate the collective character of the work of discovery and the style-character of a closed view which appears as an organic whole. Both in faulty and in true science it is the same collective forces that play the role of a motor, while the individual is the representative of certain social functions rather than a conscious source of action. In both false and true knowledge a view does not arise by a logical calculation of some elements, but by way of a complex process of stylization. There exists no observation that would not be forestalled by a directing and limiting readiness of thought.[2]

Simplicius. Do you want, following the example of the sophists, to convince me that there exists no difference between truth and illusion?

Sympatius. No, my dear friend, I am not as naive as that. What I want to do is to say that scientific results and views are basically determined exclusively as single historical events at successive development stages of the

scientific thought-style which is the outcome of the specific structure of the scientific thinking collective. Neither a Robinson Crusoe, nor a group of Robinsons, even if equipped with technical means, will glide automatically onto the tracks of science, if they are isolated from the scientific community. Even a partial isolation, brought about e.g. by political limitations, causes a partial difference between the results – and here lies the secret of the effect of the environment on science.

Let us, however, revert to our camp collective. its thought-style was characterized, in the first place, by replacing the fundamental specialist knowledge (which was not available) and the experiments (in which one had no confidence) with speculative considerations, and by replacing the practical specialist experience (which was also not available) with so-called common sense.

When paraphrasing the well-known sentence of Gauss',[3] one can say that the lack of specialist education in the empirical field can best be recognized by the limitless accuracy of logical inference. I have listened to consultations and discussions lasting for weeks on end, in which people tried to solve specialist problems by a speculative method, starting from a few textbook theorems which play the role of axioms, and from some data obtained from one's own experience – which were not linked together so as to form knowledge, but which were taught and commented upon – just as one expounds dreams or comments on the declarations of a diplomat.

Let us take the following example: in the experimental zoo an epizootic broke out among rabbits, caused, as I was able to establish, by the group D paratyphoid bacteria (according to Kauffmann). During the first day some rabbits died which had been inoculated the previous day with a vaccine from a broth suspension of heat-killed paratyphoid B bacteria (*B. paratyphi Schottmüller*). Vaccinations were conducted in order to weaken the rabbits which were to be used for the passaging of the typhus virus. During the following days the non-vaccinated rabbits also started dying.

This is the theory as developed by the collective: since, during the same period, cases of meat poisoning took place, caused by *Bac. Gärtner* (another group D paratyphoid germ), one has to admit that in the meat from which the vaccine broth had been prepared there was also the endotoxin Gärtner which, being resistant to heating (according to the book data) survived in spite of heating. The paratyphoid B group bacteria grown in this broth underwent transmutation.

endotoxin D + *Bac. paratyphi* B = *Bac. paratyphi* D.

It is clearly written in the textbook that the proper difference between *paratyphus* B and *paratyphus* D lies in the difference of the endotoxins (antigen O). Moreover, it is written elsewhere that the group I pneumococci can be converted to group II pneumococci by cultivating the former in a solution of an endotoxin characteristic of the group II. Hence such transmutations are feasible.

The group D bacteria obtained by transmutation are pathogenic for rabbits; apparently, being more immune to temperature, they had not been killed during the heating of the broth culture from which the vaccine was prepared. Hence the vaccinated rabbits were infected and, from them, the epizootic was transferred to other rabbits which began to die during the following days.

These are the elements of this theory:

(1) Axiom I: the difference between *paratyphus* B and *paratyphus* D lies exclusively in the endotoxin. (However, a specialist knows that, much as one uses the difference of endotoxins for diagnostic purposes, this difference is many-sided.) Axiom II: that transmutation modelled on pneumococci is a rule. (A specialist knows that it is an exception and that it cannot be compared with the transmutation of paratyphoid bacteria, since, in the case of pneumococci, it is the difference in the structure of the cell membrane that is important, while in the case of the paratyphoid bacteria the question concerns the difference of the cell-interior structure).

(2) 'Common sense' had dictated to the members of the collective body that, if, during the first day, only those rabbits died which had been vaccinated on the preceding day, then the relation between vaccination and infection is obvious. (The specialist knows from experience that, if only a few rabbits from the many dozens of the vaccinated ones had died, then it can be due to chance that only the vaccinated rabbits had died, but it can also be the result of the impairing effect of the vaccination itself, that the incubation period for the vaccinated animals was shorter than for the non-vaccinated ones which fell ill 24 hours later. At any rate a direct relationship between vaccination and infection is improbable under these conditions.) 'Common sense' dictated to the members of the collective body that simultaneous meat poisoning of humans and a death of rabbits injected with a broth made from this meat ought to be related to one another, the more so as the humans did suffer from group D *enteritis Gärtner* while rabbits died as a result of an infection with bacteria of the same group. (A specialist knows from experience that meat from carcasses of animals which had succumbed to paratyphoid is often used for making broth for bacterial cultures and that

this does not entail any consequences of this kind. On the other hand, there exists a probability of bacteria D from the remnants of the infected meat being spread by rats to the rabbits' zoo, if we admit that the illness of humans and the death of rabbits were caused by the same germ – which had not been proved.)

(3) Speculative elements: the collective body applied a speculation which established a causal relationship of a few hypothetical possibilities, each of which was highly problematical, and obtained a complex theory (of course even more problematical) to explain the commonplace phenomenon: an epizootic in the rabbit zoo.

A specialist would not use events to explain such a frequent problem, which – he knows from experience – can easily be inessential i.e. of a chance nature, or others which take place only under entirely specific conditions. In any case he would try to back up such a bold hypothesis with experiments: to establish whether the broth from the meat of sick animals did contain a sufficient amount of endotoxin, whether the aforementioned transmutation can indeed take place (quite an improbable occurrence), etc. The technical terms such as endotoxin, transmutation, resistance to heating, pathogenicity of the germ, are to the specialist words which denote the results of certain definite experiments and observations, or else which follow from certain theories. To the layman they are concepts whose entire content is comprised by the verbal definition of a textbook, since only the specialist knows that no such definition tallies fully with the substance of these words. The layman knows the rule, the specialist knows besides also the exceptions and the possibility of further exceptions. The layman considers that the rule had been dictated by God or some demigods, while the specialist knows that it was drawn up by his colleagues. The word is therefore for a layman a full-valued equivalent of the real subject, and its manipulation – provided it is done according to the rules of logic – is for him the equivalent of an experiment. Hence the use of verbal speculation and the characteristic precision.

I did often hear endless discussions on the subject of definitions; finally one began to differentiate between filtering and filtration, cooling and chilling etc., while such terms as 'brain symptoms', 'infection', 'incubation period' did play the role of absolute beings in the discussion which, in the given case do or do not exist, and not the role of the names of phenomena which can appear more or less distinctly.

Simplicius. What are the final conclusions you wish to draw from your deliberations?

Sympatius. The logical nature of the structure is not the touchstone of

science, since systematic error frequently yields views which are more logical. An inference from the fundamental elements or elementary sentences is not the touchstone of science, since there exist no such elements. What we will consider to be the basic element depends solely on our standpoint; similarly it depends on our standpoint which of two structures we have to consider to be identical. The *consensus omnium* is not the touchstone of science, for there is never a *consensus omnium*, but only the agreement of 'our collective', and this also depends on false cognition. Every thought-collective considers that people who do not belong to it are incompetent. Practical applicability is not a touchstone, for due to the harmony of illusions even a false view is applicable. The alchemists' gold allegedly did enrich many people, and even the cost of wars was paid for by alchemists' gold.

The only touchstone of science is in the specific features of scientific cognition: the historic singularity of their development, the structure of the relevant thought-collectives, the characteristic of the scientific thought-style. It is only by way of a comparative method, in the framework of general sociology of thinking that we can get acquainted with the features of scientific thinking.

The science of sciences is a separate science based on observation and experiment, on historical and sociological investigations. It forms a part of the science of thought-styles.

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NOTES AND REFERENCES

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[2] L. Fleck: 'O obserwacji naukowej i postrzeganiu w ogóle.' (Scientific Observation and Perception in General), *Przegl. Filoz.* (1935).

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[3] "Der Mangel an mathematischer Bildung gibt sich durch nichts so auffallend zu erkennen wie durch masslose Schärfe im Zahlenrechnen" (Gauss).