Four Epistemological Considerations Concerning the History of the Wassermann Reaction

1. General Conclusions

If we compare the description of the history of syphilis with that of the Wassermann reaction, we note that the latter requires a much greater number of technical expressions. More basic preparation in the form of greater reliance on expert opinion is necessary, for we are moving away from the world of everyday experience and are entering more deeply into that of scientific specialization. At the same time we are coming into closer contact with the persons involved in such cognition, both collectively and individually. More names must be mentioned.

This is a general phenomenon. The more deeply one enters into a scientific field, the stronger will be the bond with the thought collective and the closer the contact with the scientist. In short, the active elements of knowledge increase.

A parallel shift occurs. The number of passive and inevitable connections produced increases as well, because for every active element of knowledge there corresponds a connection that is passive and inevitable. We have already mentioned a few such linkages, for instance, that the mere use of alcohol in preparing extracts is an active element of knowledge, whereas the actual usefulness of such extracts is a passive one and therefore a necessary consequence.

The same spectacle can be observed in other scientific disciplines. To describe the history of the chemical elements, for instance, we would have to distinguish between two great stages: that of the so-called prescientific theory of the elements and that of scientific chemistry. Active and passive elements of knowledge exist in both. The concepts of the element and of the atom can thus be constructed very effectively from historical factors as well as from those of the thought collective. Such concepts are derived, one might say, from the collective imagination. But the usefulness of these concepts in chemistry is a circumstance which is really independent of any individual knower. The origin of the number 16 for the atomic weight of oxygen is almost consciously conventional and arbitrary. But if 16 is assumed as the atomic weight for O, oxygen, of necessity the atomic weight of H, hydrogen, will inevitably be 1.008. This means that the ratio of the two weights is a passive element of knowledge.

The situation we want to demonstrate consists in the fact that, during the first stage of its history, both the active and the passive elements of knowledge are smaller in number than in the second. Every rule and every chemical law can be divided into an active and a passive part. The more deeply we penetrate into a field, the greater will be the number of *both parts* and not just of the passive ones as might be expected at first glance.

For the time being we can define a scientific fact as *a thought-stylized conceptual relation which can be investigated from the point of view of history and from that of psychology, both individual and collective, but which cannot be substantively reconstructed in toto simply from these points of view.* This expresses the inseparable relation between active and passive parts of knowledge as well as the phenomenon that the number of both these parts of knowledge increases with the number of facts.

Another phenomenon must be noted. The more developed and detailed a branch of knowledge becomes, the smaller are the differences of opinion. In the history of the concept of syphilis we encountered very divergent views. There were far fewer differences during the history of the Wassermann reaction, and as the reaction develops further, they will become even rarer. It is as if with the increase of the number of junction points, according to our image of a network (on page 79), free space were reduced. It is as if more

resistance were generated, and the free unfolding of ideas were restricted. This is very important, though it belongs no longer to the analysis of fact but to the analysis of error.

2. Observation, Experiment, Experience

Observation and experiment are subject to a very popular myth. The knower is seen as a kind of conquerer, like Julius Caesar winning his battles according to the formula "I came, I saw, I conquered." A person wants to know something, so he makes his observation or experiment and then he knows. Even research workers who have won many a scientific battle may believe this naive story when looking at their own work in retrospect.

At most they will admit that the first observation may have been a little imprecise, whereas the second and third were "adjusted to the facts." But the situation is not so simple, except in certain very limited fields, such as present-day mechanics, in which there are very ancient and widely known everyday facts to draw upon. In more modern, more remote, and still complicated fields, in which it is important first of all to learn to observe and ask questions properly, this situation does not obtain—and perhaps never does, originally, in any field—until tradition, education, and familiarity have produced *a readiness for stylized (that is, directed and restricted) perception and action;* until an answer becomes largely pre-formed in the question, and a decision is confined merely to "yes" or "no," or perhaps to a numerical determination; until methods and apparatus automatically carry out the greatest part of our mental work for us.

Wassermann and his co-workers experimented according to the method of Bordet-Gengou, trying to detect the presence of the syphilitic antigen in organ extracts and of syphilitic antibodies in the blood. From the early work we glean far more of hope than of concrete results. Successful experiments are discussed along with those that were unsuccessful, without the reason for failure being accurately known to the authors. It is certain that they were on the wrong track concerning the significance of the titration level with the immune serum from monkeys. In the second experiment the number of successful tests, which means those yielding the expected result, had already risen sufficiently for statistics to be published. Of 76 extracts from syphilitic organs, the syphilis antigen was detected in 64 cases. Of the 76, 7 were from progressive-paralytic brains, all of which were unsuccessful, and Weil had his own ideas about this. If these 7 cases using brain extracts are ignored, the success rate is almost 93 percent. All 14 control tests with confirmed nonsyphilitic extracts were negative; that is, they conformed 100 percent to expectations.

But today we know that such results are beyond all reasonable expectations. First, antigen detection in organ extracts is difficult, and even with the best technique yields only very irregular results. Second, extracts from organs which are definitely nonsyphilitic can also fix the complement with syphilis serum. The control tests with negative results are therefore unintelligible, and the high percentage of positive results is very fortuitous. At any rate, the first experiments by Wassermann are irreproducible.

His basic assumptions were untenable, and his initial experiments irreproducible, yet both were of enormous heuristic value. This is the case with all really valuable experiments. They are all of them uncertain, incomplete, and unique. And when experiments become certain, precise, and reproducible at any time, they no longer are necessary for research purposes proper but function only for demonstration or ad hoc determinations. To understand Wassermann's first experiments, we must imagine ourselves in his position. He had a complete plan and felt certain of the result. But the method was still very crude. It seriously disturbed him, for instance, that he had to use human syphilis material for the immunization of most of his monkeys, since pure cultures of *Spirochaeta pallida* could not yet be produced at the time. There were of course control animals which were inoculated with monkey material. But quite a large number of his monkeys yielded a serum which in addition to syphilis antibodies also contained antibodies against human albumin. The complement fixation with this serum was therefore not always specific to syphilis. Furthermore, titration of the extracts and all other preliminary experiments had not yet been perfected. Hence, the reagents were not yet precisely matched. Moreover, it was not yet known what degree of hemolysis inhibition was to be regarded as positive and what as still negative (see chap.

3 at notes 16 and 17). It is therefore clear that the indicators of the experiments were not well defined. The results of some were ambiguous, and it often had to be decided whether the result of an experiment should be considered positive or negative. It is also clear that from these confused notes Wassermann heard the tune that hummed in his mind but was not audible to those not involved.[1] He and his co-workers listened and "tuned" their "sets" until these became selective. The melody could then be heard even by unbiased persons who were not involved. Who could define the moment when this became possible for the first time? The community of those who made the tune audible and of those who listened increased steadily. It is not appropriate to speak of either correctness or incorrectness in these first experiments, because something very correct developed directly from them, although the experiments themselves could not be called correct.

If a research experiment were well defined, it would be altogether unnecessary to perform it. For the experimental arrangements to be well defined, the outcome must be known in advance; otherwise the procedure cannot be limited and purposeful. The more unknowns there are and the newer a field of research is, the less well defined are the experiments. Once a field has been sufficiently worked over so that the possible conclusions are more or less limited to existence or nonexistence, and perhaps to quantitative determination, the experiments will become increasingly better defined. But they will no longer be independent, because *they are carried along by a system of earlier experiments and decisions,* which is generally the situation in physics and chemistry today. Such a system could then become a self-evident law unto itself. We would no longer be aware of its application and effect. And if after years we were to look back upon a field we have worked in, we could no longer see or understand the difficulties present in that creative work. The actual course of development becomes rationalized and schematized. We project the results into our intentions; but how could it be any different? We can no longer express the previously incomplete thoughts with these now finished concepts.

Cognition modifies the knower so as to adapt him harmoniously to his acquired knowledge. This situation ensures harmony within

the dominant view about the origin of knowledge. Whence arises the "I came, I saw, I conquered" epistemology, possibly supplemented by a mystical epistemology of intuition.

This exemplifies the effect of the harmony of illusions (or, as we can now call it, the intrinsic harmony of thought style), which makes the scientific results applicable and generates a firm belief in a reality existing independently of us. Rational epistemology, however, is based upon the acceptance of the *threefold function of cognition* and the reciprocal relations between cognition and its three factors. It necessarily leads to the investigation of thought style as its proper object.

Our remarks about experiment apply to an even greater degree to observation, for experiment is observation directed in a certain way. Let us consider some observations which I recently published in the area of bacterial variability. These were new to me, at any rate.[2]

We grew a streptococcus from the urine of a female patient. Its unusually rapid and profuse growth attracted our attention, as did pigment formation, which is very rare with streptococci. I had never seen streptococci producing such intense pigment and remembered only vaguely having read about them. I therefore wanted to find out about the germ in greater detail. I had intended to grow regular nutrient cultures and perform animal inoculations, as well as a few serological experiments and especially a chemical analysis of the pigment. But the project turned largely into a study of variability. How could this have happened?

A few months previously, at the request of some colleagues, I had prepared a comprehensive survey on the concept of species in bacteriology, which brought me into close contact with the phenomena of variability in bacteria. The colityphus group, difficult to systematize because of its special variability, particularly attracted my attention. I collected details about such factors as mutation, habitat modifications, and so-called germ transmission and saw that without order in the field of variability no consistent concept of species would be possible. Such order, however, could not be established without a fundamental discussion of the concept of the individual, which brought me into contact with the relevant work of Van Loghem's school.

This was the psychological foundation for the observations on streptococcus. Now streptococcus habitually reminds laboratory scientists of staphylococcus. I remembered having read of the splitting off of staphylococcus colonies of different colors. I therefore suggested to my colleague that she find out whether our strain split into lighter and darker colonies. I received the answer the next day. Such a dissociation had just occurred. In addition to the hundreds of ordinary yellowish, transparent colonies, a few very small, white, and more opaque ones had grown. We next carried out an entire series of experiments with several generations of the streptococcus to determine: (1) whether the few small colonies belonged to our strain, (2) the extent to which these differed from the others.

The answer to the first question was positive because these colonies contained organisms that were morphologically, biochemically, and zoopathologically identical with those of the typical colonies. The second part of the investigation called both for many exploratory tests to select the method and for many reformulations of the problem. We could not even claim with any certainty and assurance that a real problem existed at all. Were the new colonies definitely different from the old ones? Differences noticed initially, such as the small size, the lighter color, and the opacity all became unstable in subsequent generations. Strangely, however, a difference remained which at first could not even be clearly understood—the difference between the offspring of the special colonies and that of the others. Not only did it persist, but it in fact increased with the transfers, by the partly subconscious selection of the most divergent colonies during inoculation. All attempts to formulate this difference had to be dropped right after the next reinoculations; until at last, after we had gained comprehensive experience, a formulation crystalized. We were dealing with splitting off not of variants more strongly or weakly pigmented but of colonies with a different structure, although of the same color. In other words, the structural variations of the colonies were much more marked than those of color intensity. Moreover, structural variants were produced which, unlike the color variants, could be perpetuated through transfers. Inoculation of these different colonies finally produced what we later called the smooth

type (type G) of streptococcus colony in contrast to the curly type (type L).

The smooth types arising later were always more transparent than the curly ones. The more opaque colonies, which were noted in the initial observations on dissociation and which formed the starting point of the investigation, were therefore not identical with them. Was it, then, a dissociation phenomenon at all? This question must remain undecided, for our first observations are irreproducible. We cannot even describe them clearly, because the descriptive terms and concepts which developed during the work are inadequate for unconditioned observation.

This description of our limited experiment with streptococci can serve as an epistemological example. It shows (1) the material offering itself by accident; (2) the psychological mood determining the direction of the investigation; (3) the associations motivated by collective psychology, that is, professional habits; (4) the irreproducible "initial" observation, which cannot be clearly seen in retrospect, constituting *a chaos;* (5) the slow and laborious revelation and awareness of "what one actually sees" or *the gaining of experience;* (6) that what has been revealed and concisely summarized in a scientific statement is an artificial structure, related but only genetically so, both to the original intention and to the substance of the "first" observation. The original observation need not even belong to the same class as that of the facts it led toward.

Consequently it is all but impossible to make any protocol statements [*Protokollsätze*] based on direct observation and from which the results should follow as logical conclusions. This can be done only during the subsequent confirmation of a finding [*eines Wissens*] but not while making the effort of acquiring it. The results can be no more expressed in the language of the initial observations than, vice versa, the first observations in the language of the results.

Every statement about "First Observations" is an assumption. If we do not want to make any assumption, and only jot down a question mark, even this is an assumption of questionability, which places the matter in the class of scientific problems. This is also a thought-stylized assumption.

One might think that the statement, "Today one hundred large, yellowish, transparent and two smaller, lighter, more opaque colonies have appeared on the agar plate," could in our case be regarded as a description purely of what is observed, devoid of any assumptions. But the statement contains much more than "pure observation" and much more than could in the first instance be claimed with certainty. It anticipates a difference between the colonies, which could actually be established only at a later stage of a long series of experiments. The difference of course—and this is very important—was ascertained as of quite another kind than that anticipated.

No two completely identical colonies were found. We therefore had 102 differently structured colonies. First of all it was necessary to determine whether this or that difference was *important enough* to enable us even to speak of different colonies, and whether such a distinction was scientifically worthwhile. We still had to determine *whether* and *how* common *types of colony* could be established from such different colonies. That these two colonies could constitute something different from the other hundred, and that they somehow belonged together, was not "pure observation" but already a hypothesis, which may or may not prove to be true or, alternatively, from which another hypothesis may evolve.

For all practical purposes, the knower is initially unaware of the hypothetical nature of his assertion. Although the statement mentioned here does not describe a "pure observation," it might well be taken to express a "direct observation" or what a *trained person* would see without difficulty when looking at our agar plate. An expert or specialist in variability phenomena of bacteria, for example, would not be in the least misled by the various forms of all the colonies. He would not stop at "unimportant differences" but would recognize the two types of colony at first glance, without any analysis or hypothesis.

One could, however, argue that, although a "pure observation, that is, one without assumptions" does not occur psychologically, it is logically possible and even necessary as a subsequent construction for the legitimation of a finding. Specifically in our case, such an expert would immediately identify the two different colonies among the 102 but neglect the accidental and unimportant differences among the other 100. This ability, acquired through experience, of immediately drawing a conclusion, during observation, from a long series of comparisons and combinations could, and in fact must, also be carried though very strictly and in detail. The corresponding procedure would be to investigate *all* 102 colonies as to *all* their properties and their theoretically possible combinations and in this way to find the various types of colonies according to their *complete* nature. This is what one might find:

I. Colonies of 5–6 mm diameter	30
4–5 mm "	60
3–4 mm "	10
½–1 mm "	2
	102

II. Colonies of color 100 (arbitrary scale)	70
" 80 (lighter)	25
" 70 "	5
" 5 "	2
	102

Then the procedure would be repeated for transparency and for *all* other properties. If one were to compare the data in the two tables with each other and to place the relevant colonies beside one another, tabulated according to their ranking, one would find that very light color, together with other conspicuous properties, occurs only in the two very small colonies. Furthermore, the differences between these two colonies and all the others far exceed the fluctuations among the properties of the others when they are compared with one another. They would therefore constitute a distinct type of colony, which was the point to be demonstrated and which would thus have been demonstrated without any assumptions having been made.

This description contains some gross errors, which are committed by many theoreticians. *First,* assumptions are already incorporated within the choice and limitation of the object of investigation. With 102 undoubted colonies, there are certain to be a few doubtful features such as grains or dots that might be regarded as colonies or even as accidental structures, depending upon the assumptions.

Second, it is altogether pointless to speak of *all* the charac-

teristics of a structure. The number of characteristics can be as large as desired, and the number of *possible determinations of characteristics* depends upon the habits of thought of the given scientific discipline; that is, it already contains directional assumptions. Accordingly such mechanical combinatorial analyses are either arbitrary or actually conditioned by thought style.

Third, new discoveries cannot be carried out by such tabulations and mechnically exhaustive combinations any more than, for instance, a poem can be composed by means of combining letters mechanically.

Observation without assumption,[3] which psychologically is nonsense and logically a game, can therefore be dismissed. But two types of observation, with variations along a transitional scale, appear definitely worth investigating: (1) *the vague initial visual perception,* and (2) *the developed direct visual perception of a form.*

Direct perception of form [*Gestaltsehen*] requires being experienced in the relevant field of thought. The ability directly to perceive meaning, form, and self-contained unity is acquired only after much experience, perhaps with preliminary training. At the same time, of course, we lose the ability to see something that contradicts the form. But it is just this readiness for directed perception that is the main constituent of thought style. Visual perception of form therefore becomes a definite function of thought style. The concept of being experienced, with its hidden irrationality, acquires fundamental epistemological importance, which will presently be discussed in detail.

By contrast, the vague, initial visual perception is unstyled. Confused partial themes in various styles are chaotically thrown together. Contradictory moods have a random influence upon undirected vision. There is a rivalry among visual fields of thought. Nothing is factual or fixed. Things can be seen almost arbitrarily in this light or that. There is neither support, nor constraint, nor resistance and there is no "firm ground of facts."

All empirical discovery can therefore be construed as a supplement, development, or transformation of the thought style.

Why did bacteriologists for a time almost fail to see the phenomena of variability? At first there was a period of controversy,

involving unconnected details, when variability was too much taken for granted. Billroth, for instance, firmly believed in a universal coccobacterium septicum, which could transform itself into all possible forms. This was followed by the classical Pasteur-Koch period. The all-persuasive power of practical success and personalities created a rigid thought style in bacteriology. Only a strictly orthodox method was recognized, and the findings were accordingly very restricted and uniform. For example, cultures were reinoculated generally for only twenty-four hours. Very fresh cultures (two to three hours) or very old (about six months) ones were not even considered worth examining. As a result, all secondary changes in the cultures, which were the starting point for the restyled theory of variability, escaped attention. Whatever failed to conform completely to the standard scheme was regarded as a "form of involution," a kind of pathological phenomenon, or an "artificial" modification caused by external conditions. The harmony of illusions was thus preserved. Species were fixed, because a fixed and restricted method was applied to the investigation. The thought style, developed in this particular way, made possible the perception of many forms as well as the establishment of many applicable facts. But it also rendered the recognition of other forms and other facts impossible. Now things are turning around. The notion of variability was never quite extinct, but the successors of the classical school regarded any such observations as technical mistakes to be simply passed over in silence or rejected. The first detailed observation of variation to be taken somewhat seriously was made in 1906 by Neisser and Massini. This concerned the so-called bacterium *Coli mutabile.* It could not very well be suppressed, because it was couched throughout in terms of the current thought style and was expressly revolutionary in only one point. The authors used the classical method with only a *single* modification. They examined* the cultures not only after twenty-four hours but again after several days. Had they introduced several modifications all at once, they would have had to wait much longer for a consideration of their findings. They found that after a few

*"Examined" here renders *untersucht,* but further reinoculation (*Umimpfen*) was presumably involved; otherwise the desired contrast with traditional method is lacking.—Eds.

days buds containing modified germs were growing within the colony. Reinoculation of these buds and with them also other secondary growth phenomena within the bacterial colonies soon became popular topics for investigation. The spell cast by the harmony of illusions was thus broken, and the conditions were created without which many discoveries would have been impossible. It is typical that the new theory of variability found roots in a country other than that of classical bacteriology. It thrived in America with its paucity of tradition and was attacked most strongly in Koch's native country. It is also typical that this did not constitute a simple regression to the age of transformation of species. The very concept of species as well as many other concepts now became construed in a manner different from that in the past. What is involved here is neither mere accretion of knowledge nor a simple link-up with the period before Koch, but a change in thought style. It is also characteristic that during this change in thought style, or learning by experience, the observation of Neisser and Massini, which was its first stimulus, remained outside the new field. Today it is not considered "classical" variability (the word "classical" can already be used in such a context) but as a bacteriophage effect.

This example also exhibits three stages: (1) vague visual perception and inadequate initial observation; (2) an irrational, concept-forming, and style-converting state of experience; (3) developed, reproducible, and stylized visual perception of form.

This description demonstrates how a finding originates. Many a research scientist will certainly recognize an analogy here with his own method of research. The first, chaotically styled observation resembles a chaos of feeling: amazement, a searching for similarities, trial by experiment, retraction as well as hope and disappointment. Feeling, will, and intellect all function together as an indivisible unit. The research worker gropes but everything recedes, and nowhere is there a firm support. Everything seems to be an artificial effect inspired by his own personal will. Every formulation melts away at the next test. He looks for that resistance and thought constraint in the face of which he could feel passive. Aids appear in the form of memory and education. At the moment of scientific genesis, the research worker personifies the

totality of his physical and intellectual ancestors and of all his friends and enemies. They both promote and inhibit his search. The work of the research scientist means that in the complex confusion and chaos which he faces, he must distinguish that which obeys his will from that which arises spontaneously and opposes it. This is the firm ground that he, as representative of the thought collective, continuously seeks. These are the passive connections, as we have called them. The general aim of intellectual work is therefore maximum *thought constraint with minimum thought caprice.*

This is how *a fact* arises. *At first there is a signal of resistance in the chaotic initial thinking, then a definite thought constraint, and finally a form to be directly perceived.* A fact always occurs in the context of the history of thought and is always the result of a definite thought style.[4]

It is the aim of all empirical sciences to establish this "firm basis of facts." Two points are important in epistemology. *First,* this work is continuous. It has no demonstrable beginning and is open-ended. Knowledge exists in the collective and is continually being revised. The store of facts also changes. What has previously been classed with the passive elements of knowledge may later join the active ones. The ratio between the atomic weight of oxygen and that of hydrogen, 16:1.008, for instance, we explained as a proportion resulting passively under given conditions. If, for instance, it were possible to split O into two elements, this proportion would be accounted for by the inadequacy of the earlier method and would have to be replaced by another ratio.

Second, however, it is impossible to exhibit the passive elements of knowledge on their own, as has already been pointed out.

The passive and the active elements cannot be separated from each other completely either logically or historically. Indeed, it is not even possible to invent a fairy tale which does not contain some inevitable connections. Myth differs from science in this respect only in style. Science seeks to include in its system a maximum of those passive elements *irrespective* of inherent lucidity. Myth contains only a few such passive elements, but they are artistically composed.

The necessity of being experienced introduces into knowledge an

irrational element, which cannot be logically justified. Introduction to a field of knowledge is a kind of initiation that is performed by others. It opens the door. But it is individual experience, which can only be acquired personally, that yields the capacity for active and independent cognition. The inexperienced individual merely learns but does not discern.

Every experimental scientist knows just how little a single experiment can prove or convince. To establish proof, an entire system of experiments and controls is needed, set up according to an assumption or style and performed by an expert. The state of being experienced [*Erfahrenheit*], as it will here be designated, consists in just such factors as (1) the ability to make assumptions and (2) both manual and mental practice together with a research scientist's entire experimental and nonexperimental fund of knowledge, including features clearly conceived, those that are uncertain, and those that are "instinctive." The summarized report about a field of research always contains only a very small part of the worker's relevant experience, and not even the most important. Missing is that which makes the stylized visual perception of form possible. It is as if the words of a song were published without the tune.

Wassermann's reports about his reaction contain only the description of the relation between syphilis and a property of the blood. But this is not the most important element. What is crucial is the *experience* acquired by him, by his pupils and in turn by theirs, in the practical application and effectiveness of serology. Without this *experience* both the Wassermann reaction and many other serological methods *would not have become reproducible and practical.* Such a *state of experience* became general only slowly and had to be practically acquired by each initiated individual. A state of this kind is what the first critics of the Wassermann reaction lacked. The roots of this state in Wassermann and his co-workers have already been described. But, even today, anybody performing the Wassermann reaction on his own must first have acquired comprehensive experience before he can obtain reliable results. Only through this experience will he participate in the thought style, and it is experience alone that enables him to perceive the relation between syphilis and blood as a definite form.

We might also mention some cases where such experience involving the irrational "serological touch" is specifically needed.

1. The preparation and titration of the organ extracts perhaps calls most for experience. Here the need is not confined to theory but includes the skill of preparing uniform dilutions of the extract. An inexperienced individual obtains irregular results through having diluted the extract either too rapidly or too slowly. In this respect the Wassermann reaction is particularly sensitive. It can be confirmed now and again that the kind of extract dilution determined by a given individual does not always automatically work with another person. Psychological and physical differences among the performers of this serological test lead to appreciable differences in the degree to which the colloidal solution from the alcoholic extract disperses. The solution must thus be freshly prepared for each test.

2. The matching of all the five required reagents, so as to maximize the effect of the reactions and ensure that the results are as clear as possible, requires experience. Even quasi-*orchestral practice* is needed if, as is usual, the test is performed by a team. Change in personnel often produces a disturbance in the progress of the reaction, even if the new member of the team had worked well with other associates. This explains the poor results obtained even by excellent research workers at the previously mentioned Wassermann conferences held under the auspices of the League of Nations.

3. Obviously, general competence is also necessary in the elementary operations such as measuring, pipetting, storing of the sera, washing of the vessels, etc.

We can summarize as follows our theory of the recognition of the relation between the Wassermann reaction and syphilis. The discovery—or the invention—of the Wassermann reaction occurred during a unique historical process, which can be neither reproduced by experiment nor confirmed by logic. The reaction was worked out, in spite of many errors, through socio-psychological motives and a kind of collective experience. ***From this point of view the relation between the Wassermann reaction and syphilis—an undoubted fact—becomes an event in the history of thought.*** This

fact cannot be proved *with an isolated experiment* but only with broadly based experience; that is, by *a special thought style* built up from earlier knowledge, from many successful and unsuccessful experiments, from much practice and training, and—epistemologically most important—from *several adaptations and transformations of concepts.* Without this experience the concept of syphilis and that of serum reaction could not have been established and research workers could not have been *trained* to practice accordingly. Error and the failure of many experiments are also part of the building materials for a scientific fact. The perfection of the Wassermann reaction can be seen from this point of view as the solution to the following problem: How does one define *syphilis* and set up a *blood test,* so that after *some experience* almost any research worker will be able to demonstrate a *relation between them* to a degree that is adequate in practice? The collective character of this finding readily manifests itself in such a formulation of the problem; it is based on the need to obtain indispensable experience by comparing working methods with those of other workers, as well as on the need for some kind of connection with the traditional and incomplete concept of syphilis and that of the blood test.

The factuality of the relation between syphilis and the Wassermann reaction consists in just this kind of solution to the problem of minimizing thought caprice, under given conditions, *while maximizing thought constraint.* The *fact* thus represents a *stylized signal of resistance in thinking.* Because the thought style is carried by the thought collective, this "fact" can be designated in brief as the *signal of resistance by the thought collective* [*denkkollektives Widerstandsaviso*].

3. Further Observations Concerning Thought Collectives

The preceding chapter tried to show how even the simplest observation is conditioned by thought style and is thus tied to a community of thought. I therefore called thinking a supremely social activity which cannot by any means be completely localized within the confines of the individual.

Teamwork can take two forms. It can be simply additive, as when a number of people join together to lift something heavy. Alternatively it can be collective work proper—not merely the summation of individual work but the coming into existence of a special form, comparable to a soccer match, a conversation, or the playing of an orchestra. Both forms occur in thinking and especially in the act of cognition. How could the performance of an orchestra be regarded as the work only of individual instruments, without allowance for the meaning and rules of cooperation? It is just such rules that the thought style holds for thinking. *All paths toward a positive, fruitful epistemology lead toward the concept of thought style,* the varieties of which are mutually comparable and can each be investigated as a result of historical development.

Like any style, the thought style also consists of a certain mood and of the performance by which it is realized. A mood has two closely connected aspects: readiness both for selective feeling and for correspondingly directed action. It creates the expressions appropriate to it, such as religion, science, art, customs, or war, depending in each case on the prevalence of certain collective motives and the collective means applied. We can therefore *define thought style as* [the readiness for] *directed perception, with corresponding mental and objective assimilation of what has been so perceived.* It is characterized by common features in the problems of interest to a thought collective, by the judgment which the thought collective considers evident, and by the methods which it applies as a means of cognition. The thought style may also be accompanied by a technical and literary style characteristic of the given system of knowledge.

Because it belongs to a community, the thought style of the collective undergoes social reinforcement, as will shortly be discussed. Such reinforcement is a feature of all social structures. The thought style is subject to independent development for generations. It constrains the individual by determining "what can be thought in no other way." Whole eras will then be ruled by this thought constraint. Heretics who do not share this collective mood and are rated as criminals by the collective will be burned at the stake until a different mood creates a different thought style and different valuation.

But every thought style leaves remnants. First, there are the small, isolated communes which adhere unchanged to the old style. This explains the existence even today of astrologers and magicians: eccentrics who associate with the uneducated of the lower social classes or become charlatans because they do not share the community mood. Second, every thought style contains vestiges of the historical, evolutionary development of various elements from another style. Probably only very few completely new concepts are formed without any relation whatsoever to earlier thought styles. It is usually only their coloring that changes. Just as the scientific concept of force originated from the everyday concept of force, so also the new concept of syphilis descended from the mystical.

A historical connection thus arises between thought styles. In the development of ideas, primitive pre-ideas often lead continuously to modern scientific concepts. Because such ideational developments form multiple ties with one another and are always related to the entire fund of knowledge of the thought collective, their actual expression in each particular case receives the imprint of uniqueness characteristic of a historic event. It is, for instance, possible to trace the development of the idea of an infectious disease from a primitive belief in demons, through the idea of a disease miasma, to the theory of the pathogenic agent. As we have already hinted, even this latter theory is already close to extinction. But while it lasted, only one solution to any given problem conformed to that style. (See chap. 2, sec. 4, on Schaudinn's "causative agent" versus that of Siegel.) *Such a stylized solution, and there is always only one, is called truth.* Truth is not "relative" and certainly not "subjective" in the popular sense of the word. It is always, or almost always, completely determined within a thought style. One can never say that the same thought is true for A and false for B. If A and B belong to the same thought collective, the thought will be either true or false for both. But if they belong to different thought collectives, it will just *not* be *the same* thought! It must either be unclear to, or be understood differently by, one of them. Truth is not a convention, *but rather (1) in historical perspective, an event in the history of thought, (2) in its contemporary context, stylized thought constraint.*

Even unscientific statements contain compulsory connections.

Consider a myth, such as the Greek myth of Aphrodite, Hephaistos, and Ares. Aphrodite cannot but be the wife of Hephaistos and the lover of Ares. As any poet knows, a web of fantasy spun for long enough always produces inevitable, "spontaneous" substantive and formal connections. In a romance about chivalry, for instance, one cannot simply write "horse" instead of "steed," although these words are logically synonyms differing only in style. There are consequential links in musical imagination too, which correspond to the example: "Assuming O = 16 then H = 1.008." An artistic painting also exhibits its own constraining style. This we can easily demonstrate by placing part of a second painting over a good painting executed in a definite style. The two parts would clash with each other, even if the two paintings were matched in content. Thus every product of intellectual creation contains relations "which cannot exist in any other way." They correspond to the compulsory, passive links in scientific principles. These relations can be, as it were, objectivized and regarded as expressions of "beauty" or "truth." There actually are special individual and collective conditions which favor just such objectivization.

In the field of cognition, *the signal of resistance* opposing free, arbitrary thinking is called a *fact.** This notice of resistance merits the adjective "thought collective," because every fact bears three different relations to a thought collective: (1) *Every fact must be in line with the intellectual interests of its thought collective,* since resistance is possible only where there is striving toward a goal. Facts in aesthetics or in jurisprudence are thus rarely facts for science. (2) *The resistance must be effective within the thought collective. It must be brought home to each member as both a thought constraint and a form to be directly experienced.* In cognition this appears as the connection between phenomena which can never be severed within the collective (see chap. 3 at note 26). This linkage seems to be truth and conditioned only by logic and content. Only an investigation in comparative epistemology, or a simple comparison after a change has occurred in the thought style, can make these inevitable connections accessible to scientific treatment. The principle of immutability of species characteristics was

*Cf. chap. 4, sec. 2, at note 4 and end of section.—Eds.

valid for classical bacteriology, according to the interpretation of the time. If a scientist of that time had been asked why the principle was accepted or why the characteristics of species were conceived in this way, he could only have answered, "Because it is true." Only after a change in thought style did we learn that the opinion was constrained mainly by the methods applied. The passive linkage between these principles was transformed into an active one (cf. the definition in chap. 1, p. 8).[5] (3) *The fact must be expressed in the style of the thought collective.*

The fact thus defined as a "signal of resistance by the thought collective" contains the entire scale of possible kinds of ascertainment, from a child's cry of pain after he has bumped into something hard, to a sick person's hallucinations, to the complex system of science.

Facts are never completely independent of each other. They occur either as more or less connected mixtures of separate signals, or as a system of knowledge obeying its own laws. As a result, every fact reacts upon many others. Every change and every discovery has an effect on a terrain that is virtually limitless. It is characteristic of advanced knowledge, matured into a coherent system, that each new fact harmoniously—though ever so slightly—changes all earlier facts. Here every discovery is actually a recreation of the whole world as construed by a thought collective.

A universally interconnected system of facts is thus formed, maintaining its balance through continuous interaction. This interwoven texture bestows solidity and tenacity upon the "world of facts" and creates a feeling both of fixed reality and of the independent existence of the universe. The less interconnected the system of knowledge, the more magical it appears and the less stable and more miracle-prone is its reality, always in accordance with the thought style of the collective.

The communal "carrier" of the thought style is designated the thought collective. The concept of the thought collective, as we use it to investigate the social conditioning of thinking, is not to be understood as a fixed group or social class. It is functional, as it were, rather than substantial, and may be compared to the concept of field of force in physics. A thought collective exists whenever two or more persons are actually exchanging thoughts. This type of

thought collective is transient and accidental, forming and dissolving at any moment. But even this type induces a particular mood, which would otherwise affect none of the members and often recurs whenever these members meet again.

Besides such fortuitous and *transient* thought collectives there are *stable* or comparatively stable ones. These form particularly around organized social groups. If a large group exists long enough, the thought style becomes fixed and formal in structure. Practical performance then dominates over creative mood, which is reduced to a certain fixed level that is disciplined, uniform, and discreet. This is the situation in which contemporary science finds itself as a specific, thought-collective structure [*denkkollektives Gebilde*].

A thought community [*Denkgemeinschaft*] does not fully coincide with the official community. The thought collective of a religion comprises all true believers, whereas the official religious community includes all the formally accepted members, irrespective of their way of thinking. It is thus possible to belong to the thought collective of a religion without being formally accepted as a member of that congregation, and vice versa. The internal structure and organization of a thought collective also differs from the organization of a community in the official sense. The intellectual leadership and the circles that form around it do not coincide with the official hierarchy and organization.

A closer investigation of thought style and of the general social characteristics of thought collectives in their mutual relations can be made by concentrating upon stable thought collectives. Such stable (or comparatively stable) thought communities, like other organized communes [*Gemeinden*],* cultivate a certain exclusiveness both formally and in content. A thought commune becomes isolated formally, but also absolutely bonded together, through statutory and customary arrangements, sometimes a separate language, or at least special terminology. The ancient guilds, for instance, are examples of such special thought communes. But even more important is the restricted content of every thought

**Gemeinde:* often used for the smallest administrative district of local government in some European countries.—Eds.

collective as a special realm of thinking. There is an apprenticeship period for every trade, every religious community, every field of knowledge, during which a purely authoritarian suggestion of ideas takes place, irreplacable by a "generally rational" organization of ideas. The optimum system of a science, the ultimate organization of its principles, is completely incomprehensible to the novice. Yet this is the only valid yardstick for the expert. We have already described this situation in the case of the closure of thought within serology, which has only a traditional and not a "rational" initiation.

Every didactic introduction is therefore literally a "leading into" or a gentle constraint. The history of science is pedagogically helpful, because long-established concepts have the advantage of less thought specialization and are therefore more easily understood by the novice. Furthermore, the public at large, and therefore many an apprentice, are already familiar with them. The initiation into any thought style, which also includes the introduction to science, is epistemologically analogous to the initiations we know from ethnology and the history of civilization. Their effect is not merely formal. The Holy Ghost as it were descends upon the novice, who will now be able to see what has hitherto been invisible to him. Such is the result of the assimilation of a thought style.

The organic exclusiveness of every thought commune goes hand in hand with a stylized limitation upon the problems admitted. It is always necessary to ignore or reject many problems as trifling or meaningless. Modern science also distinguishes "real problems" from useless "bogus problems." This creates specialized valuation and characteristic intolerance, which are features shared by all exclusive communities.

Corresponding to any thought style is its practical effect or application. Any thought can be applied. Even the confirmation or refutation of conjectures calls for mental activity. Verification is therefore just as much bound by thought style as is assumption. Thought constraint, habits of thought, or, at least, a definite aversion to alien thinking that does not conform to a given thought style all help to guard the harmony between application and thought style. Guild associations are communities that are clearly directed to practical aims. It is instructive to see how differently,

depending on the nature of the trade, similar practical problems are solved. A crack in the wall plaster, for instance, presents a painter with a problem different from that which a bricklayer has to face. The painter sees only the surface damage and treats it accordingly. But the bricklayer worries about the wall structure and is likely to "work in depth." The way in which their thinking is stylized is revealed by the way it is applied.[6]

Independently of the possible organization in form and content of a stable collective, such as has been noted for the organization of a church community or a trade union, there are also structural characteristics shared by all such communities of thought. The general structure of a thought collective consists of both a small esoteric circle and a larger exoteric circle, each consisting of members belonging to the thought collective and forming around any work of the mind [*Denkgebilde*], such as a dogma of faith, a scientific idea, or an artistic musing. A thought collective consists of many such intersecting circles. Any individual may belong to several exoteric circles but probably only to a few, if any, esoteric circles. There is a graduated hierarchy of initiates, and many threads connecting the various grades as well as the various circles. No direct relation exists between the exoteric circle and that creation of thought [*Denkgebilde*] but only one mediated esoterically. Thus most of the members of the thought collective are related to the works produced by the thought style [*Gebilde des Denkstiles*] only through trusting the initiated. But the initiated are by no means independent. They are more or less dependent, whether consciously or subconsciously, upon "public opinion," that is, upon the opinion of the exoteric circle. This is generally how the intrinsic self-containment of the thought style with its inherent tenacity arises.

The esoteric circles thus each enter into a relation with their exoteric circles known in sociology as the relation of the elite to the masses. If the masses occupy a stronger position, a democratic tendency will be impressed upon this relation. The elite panders, as it were, to public opinion and strives to preserve the confidence of the masses. This is the situation in which the thought collective of science usually finds itself today. If the elite enjoys the stronger position, it will endeavor to maintain distance and to isolate itself

from the crowd. Then secretiveness and dogmatism dominate the life of the thought collective. This is the situation of religious thought collectives. The first, or democratic, form must lead to the development of ideas and to progress, the second possibly to conservatism and rigidity.

Individuals too take up special mutual positions in the communication of thoughts within a collective. If there exists a relation of definite mental superordination and subordination between two individuals, as between teacher and pupil, it is really not a relation between individuals but between elite and masses. On the one hand there is basically trust, and on the other, dependence on public opinion and "commonsense." Between two members of the same thought collective on the same mental level, there is always a certain solidarity of thought in the service of a superindividual idea which causes both intellectual interdependence and a shared mood between the two individuals. No question, once raised, can remain totally without effect. Each is pondered and has a place within the thought style. This comradeship of mood can be sensed after only a few sentences have been uttered and makes true communication possible. Without it, the speakers are at cross purposes. A special feeling of dependence therefore dominates all communication of thought within a collective. The general structure of a thought collective entails that *the communication of thoughts within a collective, irrespective of content or logical justification, should lead for sociological reasons to the corroboration of the thought structure* [*Denkgebilde*].* Trust in the initiated, their dependence upon public opinion, intellectual solidarity between equals in the service of the same idea, are parallel social forces which create a special shared mood and, to an ever-increasing extent, impart solidity and conformity of style to these thought structures [*Denkgebilde*].** The greater the distance in time or space from the esoteric circle, the longer a thought has been conveyed *within the same thought collective,* the more certain it appears. If

*In this context the ambiguous "thought structure" is selected, since an indirect reference to the patterns of thought *may* be implicit in this direct reference to the products of thought.—Eds.

**Thought products and the thought style under which these arise are both of them socially constrained. Cf. Preface.—Eds.

the bonds consist in mental training during childhood years or, better still, in a tradition several generations old, they will be indissoluble.

At a certain stage of development the habits and standards of thought will be felt to be the natural and the only possible ones. No further thinking about them is even possible. But once they have entered personal consciousness, they can also be regarded as supernatural, a dogma, a system of axioms, or even a useful convention. In this context it would be of interest to compare the history of science or the history of sports from semireligious practices in antiquity to the health-oriented sports of our own day.

The complex structure of modern society results in multiple intersections and interrelations among thought collectives both in space and time. We see professional and semiprofessional thought communities in commerce, the military, sports, art, politics, fashion, science, and religion. The more specialized a thought community is and the more restricted in its content, the stronger will be the particular thought nexus among the members. It breaks down boundaries of nation and state, of class and age. Compare the social role of sports or of spiritualism. Special terms such as *match, foul,* and *walkover* in sports; *demarche* and *exposé* in politics; *Saldo* [balance], *Konto* [account], *hausse* [bulls], and *baisse* [bears] on the Stock Exchange; *staffage* [props] and *expression* in the arts, each within its own thought collective, are used even across the barriers of national languages. The printed word, film, and radio all allow the exchange of ideas within a thought community. They also make possible the connection between the esoteric and the exoteric circles even across long distances and in spite of little personal contact.

A good example of the general structure of a thought collective is provided by the thought community of the world of fashion, as long as we examine only the common mental outlook of the followers of fashion and disregard either the general economic and social factors or the special professional and commercial factors of that field. What is of interest is fashion consciousness as such, independent of the content of fashion. The special mood of the thought collective of fashion is constituted by a readiness immediately to notice that which is fashionable and to consider it of absolute importance, by a

feeling of solidarity with other members of the collective, and by an unbounded confidence in the members of the esoteric circle. The most dedicated followers of fashion are found far out in the exoteric circle. They have no immediate contact with the powerful dictators forming the esoteric circle. Specialized "creations" reach them only through what might be called the official channels of intracollective communication, depersonalized and thus all the more compulsive. Nothing is motivated in petty style; they are simply told "ce qu'il vous faut pour cet hiver" [what you need for this winter], or "à Paris la femme porte" [in Paris, women are wearing], or "Lancé au printemps par quelques jeunes femmes de la société parisienne" [presented to the public in the spring by several young ladies of Parisian society]. It is coercion of the strongest kind, because it appears in the guise of a self-evident necessity and is thus not even recognized as a coercive force. And woe to the true believer who does not or cannot conform. She feels cast out and branded, because she knows full well that every fellow member of the collective immediately notices her act of treason. For the esoteric members the coercion is much reduced. They can permit themselves many a new-fangled idea, which does not become a "must" until subsequent communication has taken place throughout the thought collective. But they too are held by the style of their own creations to particular "obligatory matchings": baroque sleeves may not be worn with an Empire waistline, to name only one example.

If we compare various thought styles, we can easily see that the differences between two such thought styles can be greater or smaller. The thought style of the physicists, for instance, does not differ all that much from that of the biologists, unless the latter happen to adhere to the thought style of the vitalists. There is a much greater difference in style between the physicists and the philologists, and a much greater one still between a modern European physicist and a Chinese physician or a cabalistic mystic. Here the divergence between thought styles is so wide that in comparison, the divergence between the thought styles of the physicist and of the biologist dwindles into nothing. One could actually speak of nuances of style, of varieties in style, and of different styles. But it is not the aim of this book to construct a complete

theory of thought styles. All I want to do is point out a few distinctive properties of the communication of thoughts between collectives.

The greater the difference between two thought styles, the more inhibited will be the communication of ideas. Collectives, if real communication exists between them, will exhibit shared traits independent of the uniqueness of any particular collective. The principles of an alien collective are, if noticed at all, felt to be arbitrary and their possible legitimacy as begging the question. The alien way of thought seems like mysticism. The questions it rejects will often be regarded as the most important ones, its explanations as proving nothing or as missing the point, its problems as often unimportant or meaningless trivialities. Depending upon the relation between the collectives, single facts and concepts are considered either free inventions, which scientists simply ignore like, for instance, "psychic facts" [*spiritistische Tatsachen*]. Less divergent collectives, alternatively, may produce only different interpretations, translations into another dialect of thought, as, for instance, theologians would translate these same psychic facts. Scientists have similarly adopted many individual alchemic facts. So-called commonsense, as the personification of the thought collective of everyday life, has become in this same way a universal benefactor for many specific thought collectives.

Words as such constitute a special medium of intercollective communication. Since all words bear a more or less distinctive coloring conforming to a given thought style, a character which changes during their passage from one collective to the next, they always undergo a certain change in their meaning as they circulate intercollectively. One could compare the meaning of the words "force," "energy," or "experiment" for a physicist, a philologist, or a sportsman; the word "explain" for a philosopher and a chemist, "ray" for an artist and a physicist, or "law" for a jurist and a scientist.

In summary, the intercollective communication of ideas always results in a shift or a change in the currency of thought. Just as the shared mood within a thought collective leads to an enhancement of thought currency, so does the change in mood during the intercollective passage of ideas produce an adjustment in this cash value

across the entire range of possibilities, from a minor change in coloration, through an almost complete change of meaning, to the destruction of all sense. Compare the fate of the philosophical term "absolute" in the thought collective of scientists.

In chapter 1 we described the passage of the syphilis concept from one thought community to another. Each passage involved a metamorphosis and a harmonious change of the entire thought style of the new collective arising from the connection with its concepts. This change in thought style, that is, change in readiness for directed perception, offers new possibilities for discovery and creates new facts. This is the most important epistemological significance of the intercollective communication of thoughts.

Something remains to be said about the individual's belonging to several thought communities and acting as a vehicle for the intercollective communication of thought. The stylized uniformity of his thinking as a social phenomenon is far more powerful than the logical construction of his thinking. Logically contradictory elements of individual thought do not even reach the stage of psychological contradiction, because they are separated from each other. Certain connections, for instance, are considered matters of faith and others of knowledge. Neither field influences the other, although logically not even such a separation can be justified. A person participates more often in several very divergent thought collectives than in several closely related ones. There were and still are physicists, for instance, who profess the religious or spiritualist thought style, but few of them have been interested in biology once it became an independent discipline. Many physicians are engaged in historical or aesthetic studies but only a few in natural science. If thought styles are very different, their isolation can be preserved even in one and the same person. But if they are related, such isolation is difficult. The conflict between closely allied thought styles makes their coexistence within the individual impossible and sentences the person involved either to lack of productivity or to the creation of a special style on the borderline of the field. This incompatibility between allied thought styles within an individual has nothing to do with the delineation of the problems toward which such thinking is directed. Very different thought styles are used for one and the same problem more often than are very closely

related ones. It happens more frequently that a physician simultaneously pursues studies of a disease from a clinical-medical or bacteriological viewpoint together with that of the history of civilization, than from a clinical-medical or bacteriological one together with a purely chemical one.

As I select out of an abundance of data these few phenomena concerning the communication of ideas, I am fully aware of the fragmentary nature of my presentation. But they may suffice to demonstrate to science-oriented theoreticians, in particular, that even the simple communication of an item of knowledge can by no means be compared with the translocation of a rigid body in Euclidean space. Communication never occurs without a transformation, and indeed always involves a stylized remodeling, which intracollectively achieves corroboration and which intercollectively yields fundamental alteration. Those who fail to grasp this point will never reach a positive epistemology.[7]

4. Some Characteristics of the Thought Collective of Modern Science

In the previous section we described the general structure of thought collectives—their esoteric and exoteric circles, and the general rules of intra- and intercollective communication of thought. We shall now discuss the special structure of the thought collective of modern science, particularly the effect of both the esoteric circle and the exoteric circle within the framework of science. We shall disregard characteristic features of any specialized thought collective such as that of the physicists or that of the sociologists, because the structure of modern Western science has many common features.

Take the case of a researcher who creatively approaches a problem and is a "specialized expert" informed in the greatest depth—for example, a radium specialist in the science of radioactivity. He constitutes the center of the esoteric circle of this problem. The circle includes, as "general experts," scientists working on related problems—all physicists, for instance. The exoteric circle comprises the more or less "educated amateurs." A contrast between *expert* and *popular* knowledge is hence the first effect of the

general structure of the thought collective in science. The richness of this field requires that even within the specialized esoteric circle, a sphere of special experts must be distinguished from that of general ones. Let us then consider both *journal science* and *vademecum science,* which together constitute expert science. Because initiation into science is based on special methods of teaching, we must list *textbook science* as yet a fourth socio-intellectual form, which, however, is less important in our context.

Let us begin the discussion of these circles by considering *popular science.* This furnishes the major portion of every person's knowledge. Even the most specialized expert owes to it many concepts, many comparisons, and even his general viewpoint. It thus constitutes the general operative factor in cognition and must accordingly rank as an epistemological problem. When an economist speaks of the *organism* of the economy, or a philosopher of *substance,* or a biologist of the *syncytium* [*Zellstaat,* lit. "cell state"], they use, each within his own discipline, concepts derived from their fund of popular knowledge. They build up their specialized sciences around these concepts. We shall presently have the opportunity of repeatedly finding items of popular knowledge from other fields within the depths of these sciences. Such items have often set the standard for the content of expert knowledge and have determined its development for decades.

Popular science is a special, complex structure. Since speculative epistemologists have never investigated actual knowledge but only a fanciful picture of it, an epistemological investigation of popular science has yet to occur, at least so far as I am aware. But this is not the place to close this gap; a few hints must suffice.

Popular science in the strict sense is science for nonexperts, that is, for the large circle of adult, generally educated amateurs. It cannot therefore be classed as introductory science. Normally a textbook, not a popular book, is used for purposes of introduction. Characteristic of the popular presentation is the omission both of detail and especially of controversial opinions; this produces an artificial simplification. Here is an artistically attractive, lively, and readable exposition with last, but not least, the apodictic valuation simply to accept or reject a certain point of view. Simplified, lucid, and apodictic science—these are the most important char-

acteristics of exoteric knowledge. *In place of the specific constraint of thought by any proof, which can be found only with great effort, a vivid picture is created through simplification and valuation.* The ultimate aim of popular knowledge is a Weltanschauung: a special structure [*Gebilde*] emerging from an emotive selection of popular knowledge from various fields.

Little as any Weltanschauung can meet the demands of specialized knowledge, it does provide the background that determines the general traits of the thought style of an expert. This may sometimes be no more than an exalted feeling about the solidarity of all human knowledge. Or it may be a belief either in the possibility of a universal science or in the albeit limited potential for further development in science. This closes the circle of intracollective dependence in knowledge. Popular exoteric knowledge stems from specialized esoteric knowledge. Owing to simplification, vividness, and absolute certainty it appears secure, more rounded, and more firmly joined together. It shapes specific public opinion as well as the Weltanschauung and in this form reacts in turn upon the expert.

A good example of this situation is provided by a bacteriological examination, where the findings are recorded in the diagnostic laboratory by the esoteric expert team for the exoteric general practitioner. The diagnosis of a specimen from a throat swab, for instance, reads as follows: "The microscopic specimen shows numerous small rods whose shapes and positions correspond to those of diphtheria bacilli. Cultures grown from them produced typical Löffler bacilli." This finding is specially written to suit the general practitioner, but it does not represent the knowledge of the expert. It is vivid, simplified, and apodictic. The general practitioner can rely upon it. But the expert reporting *the same finding* to another expert would write in the following terms. "*Microscopic aspect:* numerous bacilli, many of which are club-shaped and slightly curved, others rather slim and straight or uncharacteristically plump. Arrangement in several places finger- and pallisade-shaped, elsewhere singular and irregular. Gram-positive. Several bacilli Neisser-positive. Löffler methyl blue: many lacerated bacilli. *Culture:* Costa medium: purplish-red, slightly smeary, sharply defined colonies, in which bacilli were found mostly typical

in staining characteristics, morphology, and arrangement. *Toxin production* and neutralization tests were not performed. In view of the origin of the examined material, and the morphological and culture characteristics of the bacilli, the diagnosis of Löffler bacilli seems sufficiently well established."

This version, although theoretically far more precise, would not appeal to a general practitioner, least of all the passage according to which the origin of the examined material is considered one of the supports (and, indeed, one of the important supports) for the conclusion. "What is going on? I just asked you what this throat swab really contains and you reply: because it is a throat swab the conclusion is justified that it is diphtheria. That is being mischievous. I wanted your support, but you went and used me to support yourself." Yet this expert finding is already purposively simplified and apodictic in many respects. Everything that is unimportant from a scientific viewpoint is omitted, such as accompanying bacteria—or what are currently thought to be unimportant accompanying bacteria. The vagueness of the limits of speciation for corynebacteria also remains unconsidered. The conclusion that the rods found in the microscopic specimen of the swab are identical with those in the culture is actually a complicated, specialist thought construction, although it is presented here as just a simple fact. Furthermore, the case is extremely elementary. It is not very often that everything works in such perfect agreement. Frequently the arrangement of the bacilli is not quite so typical. Staining is not always so unambiguous, for it can be positive, negative, or indeterminate. Finally, the culture may contradict the microscopic specimen.

No matter how a given case may be described, the description is always a simplification permeated with apodictic and graphic elements. *Every communication and, indeed, all nomenclature tends to make any item of knowledge more exoteric and popular.* Otherwise each word would require a footnote to assign limitations and provide explanations. Each word of the footnote would need in turn a second word pyramid. If continued, this would produce a structure that could be presented only in multidimensional space. Such exhaustive expert knowledge completely lacks clarity and is unsuitable in any practical case. It must be remembered that such

a pyramidal structure does not yield more general and recurrent elements, which would basically simplify construction if they could be described separately. We always remain within the same stratum of concepts, equidistant from "fundamental concepts," the possible construction of which constitutes a cognitive effort in its own right and presents the same difficulties. *Certainty, simplicity, vividness originate in popular knowledge.* That is where the expert obtains his faith in this triad as the ideal of knowledge. Therein lies the general epistemological significance of popular science.

Our example presents a part of exoteric science which is still very close to the esoteric center. The general practitioner is not all that far removed from the bacteriological specialist. If we proceed to the large circle of the "generally educated," knowledge becomes pellucid and facile; at the same time, thought-constrained proofs disappear: it becomes even more apodictic. The mother of the child whose throat swab had been examined is simply informed: "Your child has diphtheria."

The following popular description of the classical period of bacteriology is found in Gottstein's excellent book on epidemiology.[8] "An examination was carried out on a patient, or on a susceptible animal made ill through inoculation with products of the disease in question. Certain micromycetes were found here which were proved absent in other diseases. Methods were devised for obtaining a pure culture of them on suitable artificial media. Many generations of the germ were grown on this culture medium with the strictest prevention of any contamination by other schizomycetes. Their properties were studied and the disease was reproduced by inoculating other animals. The chain of proof was thereby completed. Production of the characteristic disease has always been successful in isolated experiment and remains so today." How simple, certain, and lucid does this bacteriological discovery appear! The description can certainly not be replaced by a better *popular* version. As a "general scheme" it is basically correct. It just does not correspond to detailed expert knowledge. Apart from ignoring the many restrictions and complications as well as the contradictory views and errors of the research workers, this presentation completely conceals the interaction between the genesis of a

discovery and the genesis of concepts. The description reads as if definitive concepts and ideas existed a priori. The concept of a disease entity of "*certain* micromycetes" is an example, as are the pure culture and the connection between disease and microbes. It is as if their "consistent" application alone led to the discovery and no other concepts were possible. Truth is thus made into an objectively existing quality. Scientists are accordingly divided into two classes; the "bad guys," who miss the truth, and the "good guys," who find it. This valuation, which is a general trait of exoteric thinking, was also created by the demands of the intracollective communication of thought and subsequently reacts upon expert knowledge.

Let us introduce another example. On page 5 of Gottstein's book, the history of syphilis is described as follows.

In 1495 a disease erupted suddenly and with unprecedented violence, spreading among the French mercenaries fighting in Italy, who quickly carried this "syphilis" across the whole of Europe. The rapid spread of this epidemic soon suggested that a new disease was involved. The suspicion naturally arose that it had been introduced from newly discovered America, where it was known at the time to exist, although in a milder form. Controversy still rages today over the American provenance of syphilis. Alternatively it is claimed that syphilis was already found in the Old World in antiquity. Be that as it may, at the end of the fifteenth century it spread unusually widely and with great severity. From that time to our own day, syphilis has never lost its significance as a common disease, although its manifestations have changed greatly.

How simplified and crystal-clear is this history! Where is the assiduous elaboration of the specific disease concept of "syphilis"? The whole metamorphosis of the thought style from the fifteenth to the twentieth century and the historico-cogitative as well as socio-cogitative dependence of its stages have become invisible in the description. From descriptions such as this, the general conviction follows that there is no development of thought. This is a conviction that in turn also influences the expert, and it is decisive for the epistemologists who regard their task exclusively as the treatment of the question of "right" or "wrong" knowledge.

The achievement of vividness in any knowledge [*eines Wissens*] has a special inherent effect. A pictorial quality is introduced by an expert who wants to render an idea intelligible to others or for mnemonic reasons. But what was initially a means to an end acquires the significance of a cognitive end. The image prevails over the specific proofs and often returns to the expert in this new role. We can study this phenomenon well by looking at the effect of Ehrlich's clear symbolism mentioned in chapter 3. The lock-and-key symbols became the theory of specificity and for a long time dominated the very depths of the specialized science of serology.

In addition to such general influences fed back from popular science, every field contains many specific influences. As an example, the whole lipoid theory of the Wassermann reaction is founded on a popular chemical concept of the lipoid bodies, which is in no way identical with the specialized chemical one. We thus have the strange phenomenon that the lipoid of the serologist differs from that of the chemist, just as the concept of "state" in biology, which construes the organism as a syncytium, a "cell state," is very different from the state as construed by political science.

If we move still further away from the esoteric center toward the exoteric periphery, thinking appears to be even more strongly dominated by an emotive vividness that imparts to knowledge the subjective certainty of something holy or self-evident. No more thought-constraining proofs are demanded, for the "word" has already become "flesh." As an example of such grossly popular science, consider an illustration representing the hygienic fact of droplet infection. A man emaciated to a skeleton and with greyish-purple face is sitting on a chair and coughing. With one hand he is supporting himself wearily on the arm of the chair, with the other he presses his aching chest. The evil bacilli in the shape of little devils are flying from his open mouth.... An unsuspecting rosy-cheeked child is standing next to him. One devil bacillus is very, very close to the child's mouth.... The devil has been represented bodily in this illustration half symbolically and half as a matter of belief. But he also haunts the scientific speciality to its very depths, in the conceptions of immunological theory with its images of bacterial attack and defense.

In contrast with popular science, whose aim is *vividness,* pro-

fessional science in its *vademecum* (or handbook) form requires a *critical synopsis in an organized system.*

In the history of the discovery of the Wassermann reaction and in the chapter on observation and experiment, we have attempted to describe the creative expert as the personified intersection of various thought collectives as well as of various lines of development of ideas and as a personal center of new ideas. The report that he writes has, in the first instance, a form we may call *journal science.*

Any attempts to organize journal science into a unified whole will soon encounter difficulties. The various points of view and working methods are so personal that no organic whole can be formed from the contradictory and incongruent fragments. It is not possible to produce a vademecum simply from a collection of articles that have appeared in journals. Only through the sociocognitive migration of fragments of personal knowledge within the esoteric circle, combined with feedback [*Rückwirkung*] from the exoteric circle, are these fragments altered so that additive, impersonal parts can arise from the nonadditive personal ones.

Journal science thus bears the imprint of the provisional and the personal. Its first feature is that despite a pronounced limitation of the problems under consideration there is always an urge to link up with the entire complex of problems associated with the field in question. Any paper published in a scientific journal contains in the introduction or conclusion just such a connection with vademecum science as proof that the author aims at incorporating his paper in the vademecum and regards its present state as provisional. This provisional aspect also comes through in the details about both plans and hopes as well as in the polemics. There is also a specific caution characteristic of journal contributions. This can be recognized in the typical turns of phrase such as, "I have *tried* to prove . . .," "*It appears* possible that . . .," or negatively, "It was not possible to prove that . . ." Such jargon serves to shift the "holy of holies" of science, that is, any judgment about the existence or nonexistence of a phenomenon, from the individual worker to the solely authorized collective. Only in impersonal vademecum science will we find expressions such as, "This exists or that does not," or "This or that exists," or "It has been firmly established that . . ."

It is as if every competent scientist required, in addition to the control inherent in the style conformity of his work, a further control and processing by the collective. It is as if he were aware that only intracollective communication of thought can lead from cautious uncertainty to certainty.

The personal aspect is a second feature of journal science somehow related to the first. The fragmentary nature of the problems, the contingency of the material (as, for instance, casuistics in medicine), the technical details, in short, the uniqueness and novelty of the working material tend to associate it inseparably with the author. Every researcher is aware of this but at the same time feels that any such personal element in the work is a fault. He almost always wants to hide his identity. This is recognizable, for instance, in the characteristic "we" instead of "I," the specific "plural of modesty," which is a hidden invocation of the collective. The specific discretion and duty of the individual research worker to remain in the background is formed from such modesty as well as from the characteristic caution just discussed.

Describable in terms of laboriously established, disjointed signals of resistance in thinking, this provisional, uncertain, and personally colored nonadditive journal science, then, is converted next into vademecum science by the migration of ideas throughout the collective. As we have already pointed out, this striving toward community, which expresses the dominance of the rank-and-file members of the thought collective of science over its elite, will be found in every work of the scientist. A "general verifiability" is officially demanded as a demagogic postulate, as it were. Yet, first it is not a general[9] examination but one by the thought collective, and, second, it consists solely in scrutinizing the stylization of any item of knowledge.

The vademecum is therefore not simply the result of either a compilation or a collection of various journal contributions. The former is impossible because such papers often contradict each other. The latter does not yield a closed system, which is the goal of vademecum science. A vademecum is built up from individual contributions through selection and orderly arrangement like a mosaic from many colored stones. The plan according to which selection and arrangement are made will then provide the

guidelines for future research. It governs the decision on what counts as a basic concept, what methods should be accepted, which research directions appear most promising, which scientists should be selected for prominent positions and which should simply be consigned to oblivion. Such a plan originates through esoteric communication of thought—during discussion among the experts, through mutual agreement and mutual misunderstanding, through mutual concessions and mutual incitement to obstinacy. When two ideas conflict with each other, all the forces of demagogy are activated. And it is almost always a third idea that emerges triumphant: one woven from exoteric, alien-collective, and controversial strands.

In the history of the Wassermann reaction we described the process by which personal and provisional journal science becomes transformed into collective, generally valid vademecum science. This appears initially both as change of conceptual meaning and as reformulation of the problem, and subsequently as an accumulation of collective experience, the formation of a special readiness for directed perception and specialized assimilation of what has been perceived. Some of this esoteric communication of thought occurs already within the scientist himself. He conducts a dialogue with himself as he ponders, compares, and makes decisions. The less his decision rests on adaptation to vademecum science and the more original and bolder his personal thought style, the longer it will take to complete the process of collectivizing his results.

The following event may serve as an example of esoteric communication of thought within a transient collective. At a meeting of medical historians, the members discussed a case history contained in an ancient text and considered the possibility of making a modern diagnosis according to this old description. One of the members claimed that it would be impossible in the present case, because the methods of examination given by the author diverged too far from current ones. A second member argued that basically diagnosis was always possible, since after all the diseases themselves remained unchanged. All one had to do was to construct a picture of it from textual analysis. The first member rebutted by granting that the diseases certainly remained unchanged; but our training is different, and we simply cannot form a picture from so

many emotive words that describe the gravity and horror of the disease but provide no objective clues for a diagnosis. It is true that the many terms in the text describe with extraordinary accuracy the patient's odor, the stratification of his excretions, changes in his perspiration, and even his cries of anguish. But we cannot even find out whether or not there was a fever. A lively discussion ensued lasting more than an hour, shifting from the casuistic to the fundamental. Strangely enough, however, the basic claim was maintained throughout that diseases as such, that is, disease entities, had not changed. This claim was a kind of lapse by the second speaker, and he admitted the point to me afterward. It became reinforced by the rather offhand confirmation by the first speaker and thereby acquired, oddly, the value of an axiom. But when the thought collective dissolved, not a single member of the discussion was prepared to take responsibility for it. The claim is doubtless untenable and was therefore only short-lived. But the rather impersonal mechanism of its origin, without anybody's deliberate intention or responsibility, can serve as a paradigm for typical principles of vademecum science. Very often it is impossible to find any originator for an idea generated during discussion and critique. Its meaning changes repeatedly; it is adapted and becomes common property. Accordingly it achieves a superindividual value, and becomes an axiom, a guideline for thinking.

A statement appears ipso facto more certain and more soundly established in the organized system of a discipline as presented in a vademecum than it does in any fragmentary description found in a journal. It becomes a definite thought constraint.

Let me give an example. The etiological concept of disease entity is not derived directly from individual contributions to the journals. Emerging originally from exoteric or popular ideas and from ideas formed outside the collective in question, it gradually acquired its present significance in the esoteric communication of thought and now forms one of the basic concepts of vademecum bacteriology. It could be attained only through a directed selection of individual investigations and a directed compilation. But once part of the vademecum, it is taught and generally used. It forms the keystone of the system and thus exerts a constraint on thinking. A statement such as the following becomes meaningless: "The

French pox, or syphilis, or the carnal scourge, which is the consequence of contagious and leprous affections of the genitals, is a daughter of leprosy and can in certain circumstances in turn become the mother of leprosy."[10] Yet it is meaningless only for our thought style. According to the etiological concept of disease, syphilis is a spirochaetosis and leprosy a disease caused by the specific bacillus, so that no relation exists between the two diseases. If, however, the diseases are defined symptomatically, their relation becomes undeniable and the statement deeply meaningful. It has been explained that the etiological concept of disease is not the only logically possible one. Nor does it just arise spontaneously in the presence of a certain quantity of knowledge. Nevertheless contemporary scientists, or most of them, are constrained by this concept and cannot think in any different way. This also affects the whole of pathology and bacteriology. The latter has become a medical science and has almost severed its connection with botany. The thought style of pathology in general and of bacteriology is therefore nonbiological, a point that manifests itself both in methodology[11] and in the narrowness of the problem complex with its strict limitation to medical applications.

The situation is very similar as regards the formulation of the modern concept of the chemical element, which is based on proportions by weight. This concept is also the result of truly collective work, which began with esoteric communication of thought about individual contributions. It thus became systematic and impersonal vademecum science. "After Boyle's day, however, it gradually came to be seen that certain substances resisted all such attempts to change them into others without increasing their weight. For example, all changes which iron can be made to undergo are accompanied by an increase in the weight of the iron.... It was slowly discovered that at least seventy such different substances must be classed as elements."[12] Lavoisier contributed a great deal to this concept of element. It was actually during his lifetime that proportions by weight came to be accepted as stable relations. In describing these events, Ostwald mentioned a "strange psychological phenomenon, which occurs very often at moments of important progress in science."[13] It was indeed Lavoisier who, with his theory of combustion and his law of the

conservation of weight, provided the necesssary support for the idea that proportions by weight were decisive in formulating the concept of element. Yet it was this same Lavoisier who introduced such imponderable elements as heat and light in addition to the ponderable elements and who thus "contradicted his own idea." Ostwald, maintaining a completely individualistic psychological point of view, could explain this strange phenomenon only in terms of psychology. He stated that often "the ultimate step, which confirms a new idea and rejects old ones, is precisely the one which remains unnoticed and neglected by the creator of the new idea." He tries to account for this in terms of the exhaustion of the researcher, who has no strength left for this last refinement of his idea. I believe that our observations so far have shown clearly that this incongruence between an idea as examined retrospectively and the description given by the "originator" himself, that is, by the research worker concerned, can be explained simply by the fact that the true creator of a new idea is not an individual but the thought collective. As has been repeatedly stressed, the collective remodeling of an idea has the effect that, after the change in thought style, the earlier problem is no longer completely comprehensible. That the modern concept of the chemical element also has a prehistory is well known. Like that of the etiological concept of disease it can be traced back to the mythical age. In this case again, the modern vademecum version is derived from alien collectives, exoteric sources, and esoteric communication of thought. These examples, to which similar ones could be added at will, make the role of vademecum science obvious. This is the means by which exoteric knowledge, knowledge originating in other collectives, and strictly specialist knowledge are all selected, blended, adapted, and then molded into a system. Concepts originating in this manner become dominant and binding on every expert. The preliminary signal of resistance has become thought constraint, which determines what cannot be thought in any other way, what is to be neglected or ignored, and where, inversely, redoubled effort of investigation is required. The readiness for directed perception becomes consolidated and assumes a definite form.

This relation between journal science and vademecum science shows up in modern progressive science as a characteristic struc-

ture of the esoteric circle. It resembles a column of troops on the march. Every discipline, in fact almost every problem, has its own *vanguard,* the group of research scientists working practically on a given problem. This is followed by the *main body,* the official community. Then come the somewhat disorganized stragglers. This structure becomes the more conspicuous the greater the progress in the field of investigation. Journal science, which comprises the latest work, becomes more or less removed from vademecum science, which always lags behind. The vanguard does not occupy a fixed position. It changes its quarters from day to day and even from hour to hour. The main body advances more slowly, changing its stand—often spasmodically—only after years or even decades. Its path does not closely follow that of any one of the vanguards. The main body adjusts its advance according to reports received from the vanguard, but maintains a certain independence. The direction that the main body actually chooses from the many suggested by the vanguards is always unpredictable. Paths must first be widened into roads, and the ground leveled, so that the terrain undergoes considerable change before it can become the garrison of the main body.

This indubitable phenomenon is obviously social in character, and has important theoretical consequences. If a scientist is asked about the status of a given problem, he must first specify the vademecum view as something impersonal and comparatively fixed, although he knows full well that it is inevitably already out of date. The various views of his fellow researchers currently working on the problem must be added to this, but only as their *personal* views, even though he knows that some of these may one day form the future vademecum views. It is characteristic of the social nature of science that it takes a typically binding position (sometimes this is less typical and more provisional) on almost any problem. It is particularly important, epistemologically, that the binding position is considered more exoterically conditioned than the provisional one. This indicates the dominance of the mass over the elite in a democratic thought collective.

If a fact is taken to mean something fixed and proven, it exists only in vademecum science. The preliminary stage of disjointed signals of resistance within journal science really constitutes only

the predisposition for a fact. Later, at the stage of everyday popular knowledge, the fact becomes incarnated as an immediately perceptible object of reality.

5. Thought Styles

The special *thought style* of the thought collective of modern science becomes intelligible against this specific structural background. To make the concept of thought style even more tangible and more familiar it is appropriate to compare the modern scientific version with a few older ones.

In those days, to quote the words of Dr. Samuel Brown, the metals were suns and moons, kings and queens, red bridegrooms and lily brides. Gold was Apollo, sun of the lofty dome; silver, Diana, the fair moon of his unresting career, and chased him meekly through the celestial grove; quicksilver was the wing-footed Mercury, Herald of the Gods, new-lighted on a heaven-kissing hill; iron was the ruddy-eyed Mars, in panoply complete; lead was heavy-lidded Saturn, quiet as a stone, within the tangled forest of material forms; tin was the Diabolus Metallorum, a very devil among the metals, and so forth in not unmeaning mystery.—There were flying birds, green dragons, and red lions. There were virginal fountains, royal baths, and waters of life. There were salts of wisdom, and essential spirits . . . , etc.[14]

This is how chemistry was described before it entered the modern age. Such mystical allegories and comparisons and the strongly emotional images exhale an atmosphere that is completely alien to our scientific thinking. The comparison of gold with the sun and of silver with the moon survives only in popular imagination. Associating lead with Saturn and tin with the devil has lost all meaning even in popular thinking. It is a special, self-contained style, consistent from its own point of view. Those people thought and saw differently than we do. They accepted certain symbols that to us appear fanciful and contrived. What if we could present our symbols—the potential, or physical constants, or the gene of heredity, etc.—to thinkers of the Middle Ages? Could we expect them to be delighted with the "correctness" of these symbols and instantly listen to reason? Or, conversely, would they find our symbol-

ism just as fanciful, contrived, and arbitrarily devised as we find theirs?

If we want to investigate an earlier thought style, we must examine the original sources, not modern summaries of old viewpoints. Consider a passage from Paracelsus:[15] "If you have faith small as a mustard-seed and you are yet earthly spirits, how much higher would you be if your faith were large as melons. Again, how far should we surpass the spirits, if our faith were like huge pumpkins." To illustrate the strength or weakness of faith by comparing it with mustard-seed can be accepted, if only because of biblical tradition,* so long as we remain conscious of its metaphorical character. But that it should be possible to establish a scale or a system by which to measure the strength of a person's faith against objects of various sizes is an idea we find startling. Anyone could use this sentence, for instance: "It is bad if you refuse to deviate from your demands by a finger's breadth." But the following sentence appears impossible to use in a sober frame of mind: "It is bad if you refuse to deviate from your demands by even a finger's breadth, when it is actually necessary that you should deviate from them by a foot or even a yard." For to us this sentence is either eccentric poetry or a foolish fancy of using geometrical yardsticks for psychological events. And what did Paracelsus do? Did he consider his faith-measuring system only a metaphor, or an adequate measuring system as well? This becomes clear in his treatise on the begetting of sensitive things in reason.[16] "As long as the womb has a seed within it, it no longer draws another into it. It must only remain quiet and consummate, and it will be fertile. But when it becomes cold in old age, nothing more will happen, once the drawing power dies in the cold." He explains the infertility of old women in terms of the coldness of old age, which makes the (apparently temperature-sensitive) seed-drawing power of the womb die. Coldness of old age is to him not a metaphorical circumlocution for frigidity, but absolutely identical with physical cold. We often also read in ancient writings that ravenous hunger [*Heisshunger,* literally "hot-hunger"] cooks raw food as fire does and thus makes it digestible.

In a book published two hundred years later we read:[17] "Why is

*Matthew 17:19–20.—Eds.

a person with an empty stomach heavier than after a meal? Because eating increases the quantity of the spirits, which owing to their airy and fiery nature lighten the human body; for fire and air generally do this. For the same reason a cheerful person is much lighter than a sad one, because a cheerful one harbors more of these little spirits than a sad one. A dead person is also much heavier than a living person, because the latter is full of little spirits whereas the former is deprived of them." The feeling of heaviness (sluggishness) as well as the modern concept of physical weight, heavy-heartedness, and even the difficulty (unwieldiness) of lifting a corpse are here regarded as identical phenomena,* and all explained in terms of a common cause, namely the absence of airy and fiery little spirits which, like air and fire, always lighten everything. We have here a self-contained, logical system built up on a kind of analysis of feelings—at least on an identity of feelings. Yet it is a system completely unlike our own. Just as we do, these people observed, pondered, found similarities, and associated them. They set up general principles, and yet they constructed a system of knowledge completely different from our own. The "heaviness" in this last example is a concept totally different from that of our physical "weight." Many such examples could be given all of which go to prove that comprehending objects and phenomena in a way similar to our own was completely alien to their way of thinking. Our physical reality did not exist for them. On the other hand, they were prepared to regard many another feature as real which no longer has meaning for us. Hence we have these symbols, parallels, profound comparisons, and astonishing statements.

To compare what for us is an alien and earlier thought style with that of modern science, medical papers, especially anatomical or physiological ones, are very suitable. These are easier for us to comprehend than early physical or chemical papers, which for us have become highly unintelligible.

I have before me on my desk a book about urine by Joseph Low, M.D., dated 1815. The author was not one of the champions of today's thought style. The book is steeped in the spirit of eighteenth century *Naturphilosophie.* We read, for instance: "The

*German *Schwere,* "heaviness," "weight"; *Schwermut,* "heavy-heartedness"; *Schwierigkeit,* "difficulty."—Eds.

manifestation of life is perfected only through its own creation. Life itself is only procreation and creation. The visible and perfected image of this continuous inspiration is the organic body, as the basis of that image.... For it is through the most intimate intercourse with life that organic matter receives life in its fullness. It thus perfects itself to become that first, primeval, and universal substance which is both procreating and birthgiving, which the ancients called prime matter and which is known by the moderns as nitrogen, azote, or more widely still as phosphorus" (p. 10). "Production of urine as a liquid in the human body corresponds to the formation of bone as a solid. Straddling the line dividing the inorganic from the organic forms, phosphorus becomes metallic as it returns to the liquid state in the urine together with all products accompanying it in bone formation. It thereby causes the substance even in the osseous system to change. The process of urine and bone formation therefore develops in only two directions and both meet at some stage of development throughout all the animal classes" (p. 41). "The quantity of phosphoric acid increases with age. And urea becomes uric acid, a spirit which is found only in human urine and indicates the perfect 'animalness' of man" (p. 56).

Far from being a pioneer, Löw is clearly a straggler. Phlogiston (p. 128) still haunts his book, and his concept of weight [*Schwere*] is quite out of keeping with the spirit of his time. ".... just as the stillness of the dead represents a sinking back into the metallic world, and when a person dies his body becomes heavier [*schwerer*][18] or metallic" (p. 43). Nevertheless, his thought style can be compared with the modern one, because many details in his book are directly comparable with details found in modern science. Löw considers himself a rational scientist and condemns the fanciful uromancy of the Middle Ages (p. 246): "Not until the sixteenth century, when the fanciful and extravagant uromancy of the Arabs aroused suspicion, did scientists return to the simple natural observation of urine." He thus regards his own theory as simple observation of nature, in just the same way as many contemporary scientists regard theirs.

Phosphorus is a rather fundamental idea for his chemical observations. But it is far from identical with the modern element of

the same name, although there are some undeniably common features. "In all these natural properties of urine, phosphorus as the consummate product of the animal life process is the dominant, truly inspirited principle found in those many salts with an alkaline, nitrous base in the animal gluten, where it appears in a mucous, gelatinous state, in its primeval life-bearing predisposition either as decomposed basis of food or as element in the production of the first vegetable-animal beings, and in benzoic acid and hydrochloric acid as constituents thereof" (p. 12). "It is precisely the phosphorus in the urine which, during urine retention and as a ferment of death, rapidly induces a transition from a state of inflammation to gangrene. Its presence in urine may also stimulate the formation of phosphorus throughout the entire organism as, for instance, in urinary fever, which is a putrefactive fever of the worst kind caused by prolonged urine retention. That the meteoric-electric phenomena of the atmosphere...wield their enormous influence on the urinary system is also due to phosphorus" (p. 12). "Phosphoric acid is always augmented in carnivorous animals and predators.... The production of characteristic olfactory substances in the scent of such animals as well as the greater animality of the perspiration of mainly meat-consuming persons is very closely connected with more animalistic urine and especially with the augmented production of phosphoric acid in it" (p. 27). "Because of the increased quantity of urea, phosphoric acid, and the urinary salts, a copious crystalline precipitate occurs in the urine of the male as well as of the female, who always remains more true to the primeval life-conceiving state, and accordingly her phosphorus formation is even more gelatinous, soapy, oily, and greasy" (p. 44). "Phosphoric acid, called by Gärtner phosphorous acid, because it often causes urine and some types of sweat really to phosphoresce, is the only free acid in the urine" (p. 63). "The fibrin of the blood...represents the metallically formed phosphorus" (p. 100). "The increased production of phosphorus in inflamed urine is unmistakable in the color, temperature, consistency, quantity, and quality of the urine" (p. 115). "The two acids, uric acid and phosphoric acid, do not occur at all in urine from a nervous condition, because these express the inspiriting principle directly. But this must necessarily be absent here and cannot be generated,

because of the inhibited influence of the nerve spirit" (p. 157). "The phosphorus in urine as uric acid, and as constitutive of the urate, in addition to all the other salty and earthy qualities... follows this urge" (p. 206). "Most of these urinary salt-concretions have phosphorus as uric acid for a basic constituent" (p. 206).

Modern science has no term that properly describes this *phosphorus.* It is a principle, an axiom, and a symbol of the inspiratory, animalistic as well as of the lethal forces "as the symbol of death." It is related to meteoric electricity, the production of characteristic olfactory substances, phosphorescence, inflammation, and putrefaction. Like a chameleon it appears in such various guises as metallic, gelatinous, soapy, or oily. It manifests itself as uric acid and as a pigment, it forms concretions, and it gives urine its color, temperature, and consistency. It is a principle, and yet it becomes materialized in precipitates together with salts. It is ponderable, can increase or decrease and even disappear altogether. It therefore does not share the properties of a modern principle, because principles and symbols are now considered imponderable. Yet it shares some features in common with the modern concept of phosphorus. In particular there are some phenomena and connections which can be organized around either concept. Phosphorescence is predominant followed by high inflammability, the ozone smell in the presence of phosphorous, and the same residual smell following some "meteoric, electric phenomena," and then it is present in large quantities in urine, in bone, and in the nervous system. There is no doubt that some relation exists between the modern scientific concept of phosphorus and that of Löw. Just what it is, however, is difficult to describe in strict scientific terms. It would be well to borrow the word "motif" from the field of art and speak of an *identity of some motifs of both configurations.* Both the source and the special relation to fire and smell would thus be common motifs, which occur both in Löw and in the modern scientific concept of phosphorus.[19]

These same attributes exhibited by phosophorus à la Löw—half principle and half substance in a contemporary scientific sense—are also exhibited in his other substances such as metal, water, and urea. This gives his science a special stamp. Principles unite to form moving [*pathetischen*] ideas, grandiose correlations and comparisons. In his kind of reality everything has the value of a symbol, which has an outer and less important form and an inner and profound meaning. The aim of his exploration is not to unearth and simply to explain this meaning but to divine it as a profound secret. We read, for instance, that "the kidneys, which developed from the mucosa of the genitals, have a special hidden relation and sympathy with the sexual system" (p. 43). "But it is procreation, the preparation of that all-procreative and birth-giving substance in the life process of the sexual organs, of phosphorus in its highest form of exaltation and inspiration, that connects the sexual and the urinary system in a way that is as profound as it is mysterious" (p. 44).

The deep mystery that the author finds here does not amount to a puzzle to be solved or a relation to be revealed by research. Conversely, the knowledge acquired about the relation consists only in *its being declared to be profound and mysterious.* It is the experience of mystery as mysterious. The awe he experiences when he looks at the veiled Isis is the intellectual gratification the author seeks, and what satisfies him.

When Löw finds what we would call a purely mechanical connection in some case, he is not satisfied and he looks for a deeper one. "During general paralysis of all the organs governed by the will, those of movement, the sarcoderms and the sphincters, all excretions occur spontaneously, simply because of the weak dilution of all humors." And urinary incontinence is only the expression of general "colliquative profluxia," which manifest themselves in the blood, making it "diluted, discolored, black, and foaming," as well as in the form of bloody sweat and diarrhea. He only has to look at an object to notice at once and to describe the profound, mysterious connections. "Just as the skin of the seriously ill produces a highly nitrous, cadaverously-smelling, contagious atmosphere, the urinary system produces turbid, black, foaming urine from which a black, sooty deposit resembling coffee-grounds soon precipitates giving off a putrid, stinking odor" (p. 111). Note the congruence in the dark coloring given to the details of this aspect. A "black" pessimistic prognosis corresponds to black urine; and with danger of death and contagion goes a cadaverous and putrid stench. This is not simply emotional fantasy. What we are faced with here is an explicit parallelism between the properties of the

symptoms described and the meaning of the entire aspect. It is as if every part in concord betokened the meaning of the whole entity. The black color of the urine betokens the pessimistic prognosis, just as "the color, temperature, consistency, quantity, and quality of the urine" immediately and "unmistakably" betoken "the profuse production of phosphorus." Löw is ready to see such *signatures*[20] of profound meaning everywhere. He mentions a list of "indications of the urine" in the context of chronic diseases [pp. 140–41] and also of a "biliary signature of the urine" [p. 146]. These very signatures imprint the character of symbols upon the objects of his reality.

When reading his descriptions we are immediately struck by the descriptive terms he uses that are foreign to us. On page 120 we read about "ichorous fluid with mummy-like corrugation" into which the humors of a gangrenous organ degenerate. On pages 142 and 146 he writes about "jumentous urine" to describe a visual resemblance to the urine of grass-eating animals. His reports specify far too many qualities, which seems pleonastic to us. "The presence of synochal fever also becomes conspicuous by means of the urine through its discoloration and turbidity as well as through a lack of coction [*Mangel an Kochung*] and of homogeneous mixture. Its appearance is crimson or dark red, turbid, gluey, and it is rich in a diffuse, floccular, multicolored, dirty-white, often gray, mucous deposit, which consists of decomposed mucus, gluten, urea, and phosphorus. After sedimentation it always remains turbid and opaque, being already opaque when excreted; the urine retains this aspect right to the last stage common to all fevers" (p. 107). It is interesting to compare this profuse description with a modern account of the same condition. "Turbid, crimson or dark red urine with floccular deposit" is sufficient. Everything else is either useless to us (discoloration, of turbid complexion, gluey appearance) or has been replaced by the microscopic investigation of the deposit (the complex specification of the deposit). Statements such as "lack of coction and of homogeneous mixture" are wholly unfamiliar. The significance of these expressions, however, can be explained. They correspond to a pathological theory according to which all diseases progress in definite stages. The first is called the "stage of crudity" (*cruditas*). Corresponding to this stage

is a "crude" urine which is "thick, turbid, discolored and shows no homogeneous mixture." It is characteristic that Löw should have listed "lack of coction" among the visible properties of the urine he describes. Although he construes such a lack as a directly visible property, we no longer regard it as such. It is a theoretically constructed gestalt*—*which Löw saw immediately, but which we do not.* Many other descriptions that are alien to us, such as the "jumentous urine" of grass-eating animals mentioned earlier, correspond to theoretically appropriate ready-made *gestalten, which we do not see* but which Löw, possessing the relevant stylized readiness to perceive them, *perceived directly*, analogously to the gestalten and qualities in present-day knowledge that are immediately perceivable without further ado, as we discussed in section 2 of this chapter.

All in all, Löw was ready to see features different from those we see and to convert what he saw into a different kind of cognition. To avoid any misunderstanding it must be added that Löw was certainly no great light among his contemporaries. He cannot even be regarded as their typical representative. My sole purpose was to cite an example of scientific thinking differing from that of today. In his particular mood, which to us seems fanciful and mystical, Löw is ready to see mysterious, deep connections. Furthermore, the objects of his world have a specific, symbolic coloring. This is his own particular *thought constraint, which becomes intensified until he directly perceives the appropriate gestalts.* At the same time, he considers himself to be a rational researcher, because after all he is only describing what he sees.

To obtain an even clearer picture of how scientific observation differs when two different thought styles are involved, it is perhaps appropriate to compare anatomical descriptions and illustrations in early and recent textbooks. I browsed through several seventeenth- and eighteenth-century anatomical textbooks, all of which provide almost equally suitable examples. Let me cite the description of the collarbone (clavicle) from Thomas Bartholin, *Anatomy, Old and New Observations, Especially the Teaching of*

*Where the alleged agency for properly homogenizing the mixture was missing.—Eds.

my Father, Caspar Bartholin, about Harvey's Blood Circulation and Lymph Vessels, fourth edition, Leyden, 1673.

Clavicles are called keys because they lock the chest, and like a key also lock the shoulder blade to the breastbone, or because they recall the keys to houses of ancient times, noted by Spigel at Padua in ancient houses. Celsus calls them jugula because they connect. Others call them tongues, the forked bone, or the upper support. They are located transversely below the lower neck, in the highest part of the chest, one on each side. They are shaped like an elongated Latin letter *S,* that is two semicircles sigmoidally joined, convex towards the outside joint and slightly hollow so that no vessels, which are large there, are compressed. In the male they are more curved so that the movement of the arms is less obstructed. In the female they are less curved to enhance her beauty, so that the depressions in this area are less conspicuous in the female than in the male, wherefore she is less proficient in throwing stones. The material is thick, but perforated and spongy. It is therefore often fractured, but easily knits together again. The surface is rough and uneven. They are connected to the upper process of the shoulderblades by cartilage, which joins them so that movement of the shoulderblades and arms is not restricted. But they are immobilized by ligaments surrounding the joint, with a broad and longish end, and joined to the breastbone at the other end as previously described. The collarbone is utilized for various movements of the arm, and because it is fixed by a bone like a stake, it can be more easily moved backward and forward. Hence animals except monkeys, squirrels, mice, and hedgehogs have no collarbone. [P. 745]

This description consists of: (1) a linguistic analysis of terms, taking up one-fifth of the chapter; (2) a brief description of the arrangement and a fairly detailed description of the connection with other bones; (3) a very graphic description, although poor in detail, of the shape; (4) a very brief description of the surface ("rough and uneven") and of the internal structure ("thick, but perforated and spongy"); (5) some comparatively comprehensive and very detailed teleological remarks, taking up about a quarter of the entire exposition; (6) and some brief remarks on comparative anatomy, such as, "Hence animals have no collarbones."

Let us compare this with a modern description, for instance the one under the heading "clavicula, clavicle" by Möller and Müller in a very concise compendium of anatomy.[21]

Concerning the *S*-shaped bone inserted between shoulder blade and breastbone, we speak of the middle section and the sternal and acromial ends. The middle section has an upper and a lower face with a shallow groove (musculus subclavius), an anterior (mm. pectoralis major and deltoideus) and a posterior edge. The sternal extremity is prism-shaped with anterior (m. pectoralis major), posterior (m. sterno-hyoideus), inferior and median faces (=facies articularis sternalis), superior (m. sternocleidomastoideus), inferior and posterior edges. The costal tuberosity is situated on the inferior face (ligamentum costoclaviculare). The acromion has a superior, inferior, and lateral face (=facies articularis acromialis), an anterior (m. deltoideus) and a posterior (m. trapezius) edge. The coracoid tuberosity (lig. coracoclaviculare) is on the inferior face. Development: main core is in the middle section with epiphysis at the sternal extremity.

Compared with the seventeenth-century description, the following changes will be noted. *No trace remains* of (1) the pseudo-linguistic analysis of nomenclature, (2) most of the vividness in the description of shape and arrangement, and (3) the teleological observations. *On the other hand,* (4) detailed information about muscle and ligament connections with the bones is provided, and (5) the description of the surfaces, the edges, the various parts of the bones is far *more detailed.* The shift in intellectual interest is very clear. What Bartholin described in just a few words has become ten times as detailed, but what he described in great detail has almost disappeared. In the place of the nomenclature analysis and teleological observations, constituting almost half his text, detailed connections of bodily organization are now described. Personal names as well as any popular aspect of form and purpose have been relegated to the background by a detailed description of connections in terms of a mechanico-technical theory.

The characteristics listed here can be found in all early anatomical descriptions, often in a style even more pronounced and gross. There are nomenclature analyses occupying half a page, with citations, discussions, deductions, and opinions. In an epitome of Vesalius' *Anatomy,* edited by Fontanus,[22] the chapter on the thighbone (femur) devotes only 31 words to its anatomical structure, in the modern sense of the term, compared with 135 to a description of the name "femur" and its meaning in Pliny, Plautus, Virgil, Horace, etc. In Bartholin we read, for instance,

"The belly-as-stomach is called as it were a little belly-as-abdomen" (p. 66), or "testes or testicles attest to a man's virility" (p. 208), or "The ticker-as-heart is so called in virtue of its ticking motion" (p. 353). A name here has a completely different significance from what it has today. It is not an arbitrary, conventional designation or one that arose by historical accident. The meaning is inherent in the name, and its investigation constitutes an integral part of acquiring knowledge about what it names. The name ranks as a property of its object of reference.

Early anatomical descriptions and illustrations are characterized by their graphic quality. We noted this in the description of the collarbone. Bartholin wrote about the kidney as follows: "Its shape is that of a kidney bean, or of a liverleaf when looked at in profile. Outside, the shape is gibbous and round in the back as well as towards the abdomen. The inside upper and lower parts are gibbous in shape, but the middle section concave and snubnosed" (p. 177). Books on anatomy from the seventeenth and eighteenth centuries contain absolutely superb graphic pictures of nerve men and vein men which can never be found in modern textbooks. But this clarity has a distinct coloring. Figures of skeletons, for instance, do not just illustrate bones, or even a systematic arrangement of the bones, but express an emotive symbolism. They symbolize Death by carrying spades, scythes, or other insignia of death.[23] Figures of muscular men are represented as martyrs. Other figures also assume pathetic postures. Faces do not exhibit the empty expression of corpses or the diagrammatic features typical of modern anatomical illustrations, but are expressive and distinguished. In the representation of an unborn child, both the proportions of the fetus and the position of the limbs are arranged in a conventional, amoretto-like way. The head is much too small and the limbs assume a comely position not corresponding to the compact position of the embryo.[24] If we look at the earliest anatomical illustrations, such as the accompanying illustration from the twelfth century, the first feature that strikes us is their schematic and primitive symbolic character. We see figures set in conventional uniform postures, the organs are indicated symbolically, such as the circular duct in the thorax, meant to represent the circulation path of the pneuma in the chest, and below on the right

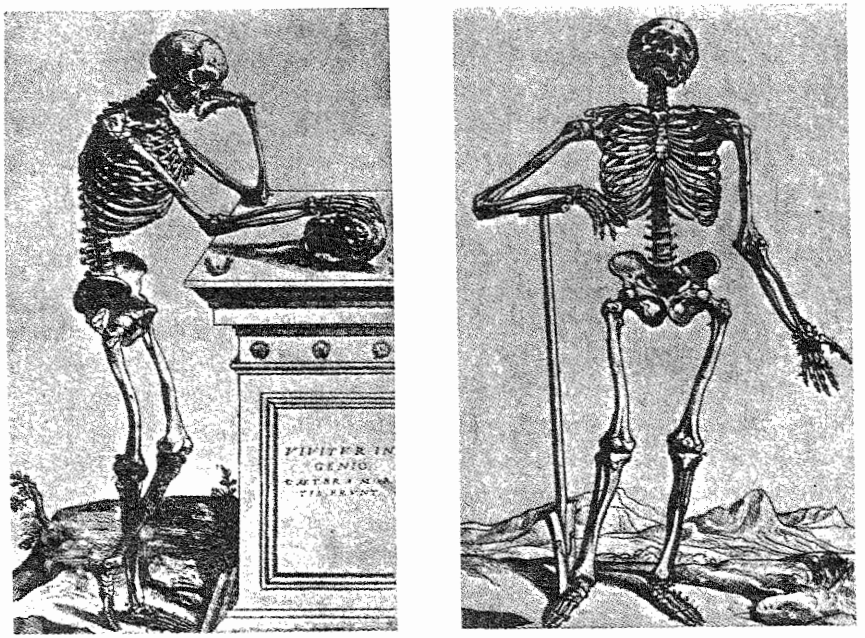
the schematic five-lobed liver. What we have here are ideograms [*Sinnbilder*] corresponding to then-current ideas, not the form which is true to nature as we construe it. Intestinal loops, for instance, are not portrayed as a certain number of sections positioned in a certain way but as spiral lines symbolizing the loops (see illustration). Nor do we see definite convolutions of the brain but the "curliness of the brain surface in general"; not a certain number of ribs but the "ribbing of the chest wall in general." The cross section of an eye does not reveal a definite number of wall layers but its multilayered structure schematically represented, which makes the illustration resemble a cross section through an onion.

We are thus confronted with ideograms [*Ideogramme*], or graphic representations of certain ideas and certain meanings. It involves a kind of comprehending where the meaning is represented as a property of the object illustrated.

The very detailed teleology, endeavoring to find a meaning in every detail, is perhaps connected with such ideovision [*Sinn-Sehen*]. The book of Fontanus (p. 7) contains such a description. "The lower ribs are shorter, so that the full stomach is not compressed, and they are more pliable for the same reason." The bone sutures of the crown of the skull have the purpose of releasing "vapors" from the skull (p. 3). That the fingers each have three phalanges, that the cartilaginous rings of the trachea are not completely closed, etc. are further details each allotted a simple, as it were, primitive purpose.

The interpretation of the anatomical illustrations as ideograms* forces itself all the more upon us the more alien the author's thought style and the further removed from us the era concerned. All we see in medieval, in Persian, and in Arabic illustrations is schematic sign language but almost no realism.[25] The difference between one of these alien thought styles and the modern one does not rest simply on our greater knowledge. They have actually more to say about that which in their particular reality has a greater value than it does in ours. Bartholin has also written a chapter on the sesamoid bones

*Here Fleck uses *Sinnbilder*, followed by *Ideogramme* in parentheses, thus identifying in this context the two terms with one another.—Eds.



Vesalius' drawings of skeletons. After Roth 1892.

The rib cage. After Heitzmann 1888.

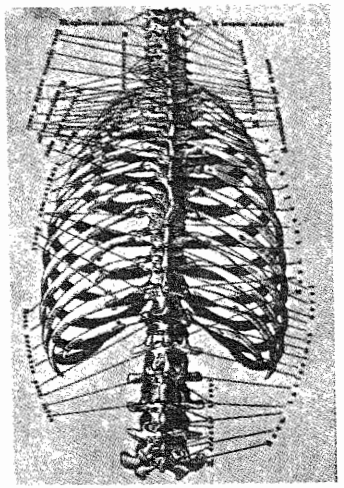
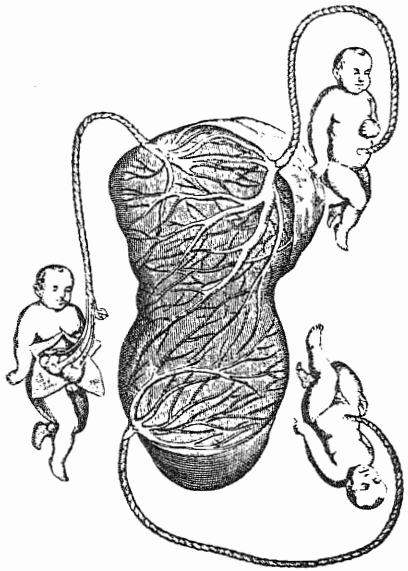


Figure 3

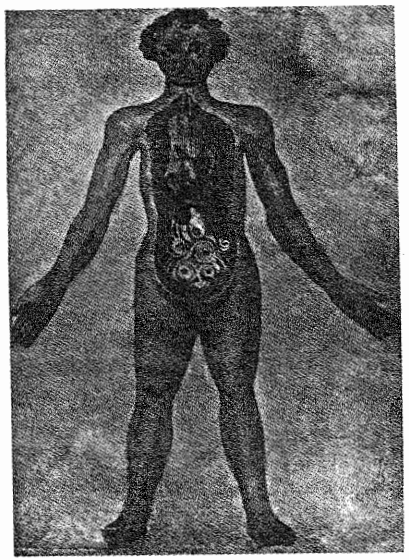
(p. 756). This is even longer than the chapter on the cervical or collar muscles and consists of about thirty times as many words as the few that are included in modern anatomies about these bones [or superfluous cartilaginous nodules].[26]

These [sesamoid] bones are important in Bartholin's osteology but without importance in ours, standing as it were beyond the pale of the osseous system altogether. Bartholin still subscribed to ancient, fanciful legend according to which these little bones are seeds from which bodies will one day again grow "like a plant from its seed." He did not believe very firmly in it, but he nevertheless felt obliged to cite the other authors, discuss the purpose of the bones, deal with their form and position, show surprise at the variability of their number, etc. Accordingly he had more to say about them than we do, and even more than about the cervical muscles, which today constitute an important part of myology.*

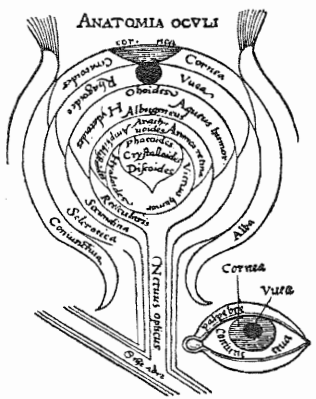
He wrote almost five pages about the hymen, which today is described in one or two sentences. A great deal of space in these old descriptions is devoted to counting the number of anatomical parts. Fontanus notes: "There are twenty bones in the skull, of which eight are in the head and twelve in the upper maxillary" (p. 36). He tells us that there are twenty-eight bones in the toes, and the total number of human bones is 364; that there are seven pairs of muscles which move the eye and four pairs the cheeks and lips; that the portal vein forms five branches, etc. Today such counting is impossible, since we often regard it as arbitrary whether three bones or four, for instance, can be separately identified in a given articulation. But thought styles exist in which the number, just like the name of the object described, is important not as a means of description but in its own right. Only a vestige of such number mysticism remains with Fontanus. But in many thought styles such as the Indian thought style and that of the Chinese, such a system was elaborated until it formed a rich number cabala, in which numbers were accorded special signification and meaningful connections were established among them. If a thought style is so far removed from ours as this, no common understanding is any longer possible. Words cannot then be translated and concepts have nothing in common with ours. Even shared motifs such as the



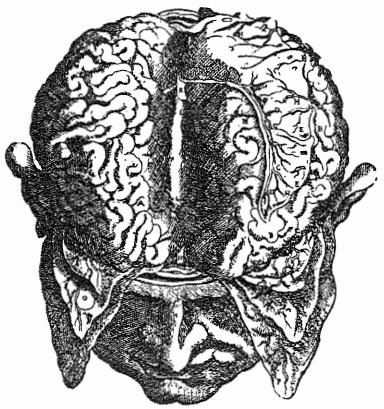
Triplets. From Bartholin 1673.



Bloodletting figure, fifteenth century. After Sudhoff.



Cross section of the eye, 1539. After Sudhoff.



Surface of the brain. From Vesalius 1543.

Figure 4

affinity exhibited between Löw's concept of phosphorus and the modern one are missing.

To the unsophisticated research worker limited by his own thought style, any alien thought style appears like a free flight of fancy, because he can see only that which is active and almost arbitrary about it. His own thought style, in contrast, appears imperative to him,* because although he is conscious of his own passivity, he takes his own activity for granted. It becomes natural and, like breathing, almost unconscious, as a result of education and training as well as through his participation in the communication of thoughts within his collective. Modern anatomists would regard as a useless emotional frill any representation of the skeleton as a symbol of death, such as was typical of Vesalius himself as well as of his predecessors and contemporaries. But we can learn to see their particular intellectual mood even in our present-day anatomical illustrations. Consider, for example, figures 120 and 121 of Heitzmann's anatomical atlas, which represent the rib cage.[27] A mechanico-technical cage motif is in keeping with this representation, just as much as a salient death theme with the skeleton figures of Vesalius. It cannot be claimed that the resemblance to a cage arises "automatically." It appears only after (1) a purposeful stripping of the ribs, (2) a purposeful assembly of the plexus, as well as (3) a purposeful arrangement of the whole to bring about this resemblance in perspective, in a manner analogous to the purposeful ideography [*Aufstellungen der Sinnbilder*] of early anatomy. Furthermore, (4) the lines added to indicate muscular insertions underscore the symbolism of a mechanical apparatus every bit as much as the scythe underscores the symbolism of Death for Vesalius. *These modern figures are ideograms just like those of Vesalius.* There is no visual perception except by ideovision and there is no other kind of illustration than ideograms.

A technico-mechanical motif is in keeping with all osteological figures of modern anatomy. Accordingly the skeletal system is regarded as a supporting frame. Everyone is so familiar with this idea both from school and from our thought style that we are

*"Der eigene Denkstil erscheint ihm dagegen als das Zwingende," in the original, implies that the individual *can be aware* of the coercive function of his own thought style. Cf. p. 41—Eds.

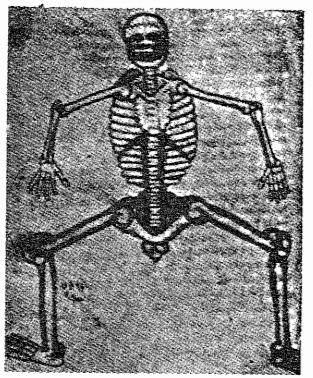
forced to exclaim that of course "it really *is* the supporting frame." It certainly is, provided we are thinking according to the thought style of modern science. But it is not difficult to imagine a system of knowledge in which the skeleton is not construed as a frame supporting the body. If one adheres to the concept of heaviness [*Schwerebegriffe*] found in Schreger and even in Löw, for instance, it is by no means impossible to look to the airy and fiery spirits as supporting the body, because these keep the body upright by their urge to rise. Here the bones would really be the opposing element, which is lifeless "metallic" and non-"inspirited." "As all persons, when they die, become heavier or metallic..." As the non-inspirited principle of the body, and mere ballast, the skeleton would attract much less attention and be depicted as a pile of bones rather than the frame shown in modern anatomical illustrations. In about the same way, fatty tissue appears in modern anatomical illustrations not as a continuous system but as a kind of photographic negative. It "appears" as that which has been cut away [much like the "lack of coction" which Löw "observed"].

We have defined thought style as the readiness for directed perception and appropriate assimilation of what has been perceived. We have already mentioned the particular mood which produces this readiness for any particular thought style. An exhaustive investigation of thought styles cannot be assigned to this book, for it would take up the working capacity of a lifetime. There is but one element of the thought style of modern science that ought to be discussed, namely the specific intellectual mood of modern scientific thinking, especially in the natural sciences. This mood stands in direct relation to the specific structure of the thought collective of science as has already been described.

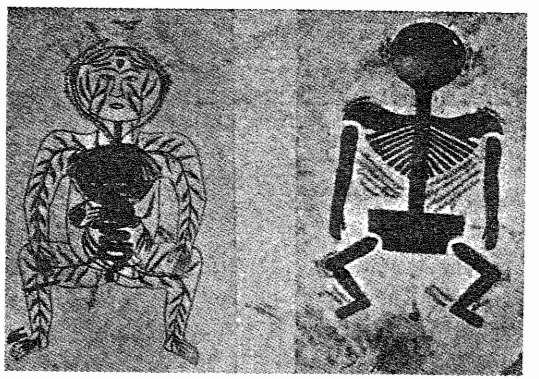
It is expressed as a common *reverence* for an ideal—the ideal of objective truth, clarity, and accuracy. It consists in the *belief* that what is being revered can be achieved only in the distant, perhaps infinitely distant future; in the *glorification* of dedicating oneself to its service; in a definite *hero worship* and a distinct *tradition.* This would be the keynote of the common mood in which the thought collective of natural science lives its life. No one already initiated would claim that scientific thinking is devoid of feeling. Nor can there be any doubt, according to our argument, that the particu-

Figure 5

Anatomical illustrations, 1158. After Sudhoff.



Skeleton figure, 1323. After Meyer-Steineg and Sudhoff 1928.



Persian anatomical illustrations. After Meyer-Steineg and Sudhoff 1928.

Fetus in utero, about 1460. After Weindler 1908.

lar attitude influences not only the work method but also the results. It manifests itself concretely as a readiness for directed perception.

But how is this mood put into effect? First, every scientist has the obligation to remain in the background. This obligation is also expressed in the democratically equal regard for anybody that acquires knowledge. All research workers, as a matter of principle, are regarded as possessing equal rights. And all, in the service of the common ideal, must equally withdraw their own individuality into the shadows, as it were. Personal supposition in science is regarded as provisional; this is a specific structural aspect of the thought collective of science. We previously discussed in detail the centrifugal tendency of the products of scientific thought [*der naturwissenschaftlichen Denkgebilde*] and the centripetal feedback of this tendency in the form of a migration of ideas throughout the collective between the esoteric and the exoteric circles. We emphasized the distinctive "modesty of the plural" as well as a characteristic personal modesty and caution.

The mood of the thought collective of natural science is further realized in a particular inclination to objectivize the thought structures [*Denkgebilde*] that it has created. This is the counterpart to the obligation of the scientist to withdraw as a person. This tendency to reify and objectivize the conceptual creations of scientific thought [*Denkgebilde*] arises, as has already been described, during the migration of ideas throughout the collective and is inseparably bound up with it. Graduated in several steps, it begins with statements by different scientists as well as the historical development of a problem, so that it becomes depersonalized. Special expressions or "technical terms" are introduced. To these are added special symbols and possibly a whole sign language such as is used in chemistry, mathematics, or symbolic logic. Such a lifeless [*lebensfremde*] language guarantees fixed meanings for concepts, rendering them static and absolute. A further factor is the particular reverence for number and form as well as the striving for vividness and a closed system. A maximum of information is demanded, the greatest possible number of mutual relations between individual elements, in the belief that the ideal of objective truth is

all the more closely approached as more and more relations are found.[28]

Thus, a structure [*Gebilde*] is created step by step. Starting as a unique event or *discovery,* as seen from the history of thought, this is developed by the extraordinary forces of the thought collective into what seems to it to be a necessarily recurrent and thus objective and real *finding.*

The disciplined, shared mood of scientific thought, consisting of the elements enumerated, connected with the practical means and effects, yields the specialized thought style of science. Good work done according to style, instantly awakens a corresponding mood of solidarity in the reader. It is this mood which, after a few sentences, compels him to regard the book highly and makes the book effective. Only later does one examine the details to see whether they can be incorporated into a system, that is, whether the realization of the thought style has been consistently achieved and in particular whether procedure has conformed to tradition (= to preparatory training). These determinations legitimize the work so that it can be added to the stock of scientific knowledge and convert what has been presented into scientific fact.