Some Relations between Philosophy and Science in the First Half of the Nineteenth Century in Germany

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MSS. intended for publication and books, etc., intended for review should be sent to Professor J. McKeen Cattell, Garrison-on-Hudson, N. Y.

SOME RELATIONS BETWEEN PHILOSOPHY AND SCIENCE IN THE FIRST HALF OF THE NINETEENTH CENTURY IN GERMANY[1]

I PRESENT this paper in response to Dr. Councilman's request; and its choice of topics is determined wholly by the instructions that he has given me in asking me to prepare to meet you. It is not for me to judge in what way these hastily prepared notes can be of service to any of you; and as a fact, I confess myself unable to see that they can be of any service whatever to a company of pathologists. I am, of course, profoundly ignorant of pathology. And, as I learn from consulting the sources, the school of scientific men of whom Virchow was the leader felt, at the outset of their great undertaking, in the years before 1850, that philosophy, and, in particular, that what used to be called, in Germany, the *Naturphilosophie,* had formerly been, in the main, profoundly harmful in its influence upon medicine in general, and upon the beginnings of modern pathology in particular, so that one great initial purpose of Virchow and of his allies, during the years before 1848, was to free their young science from whatever was still left of these evil philosophical influences and to make it a true natural science. I not only learn that this was their opinion; but I see, as any student of the history of thought in the nineteenth century must see, that this opinion was in a large measure very well justified. Philosophy, in the first quarter of the nine-

[1] Read at a session of the Pathological Club, of the Harvard Medical School, at the request of Professor W. T. Councilman, President of the Club.

teenth century, in Germany, had done medicine a good deal of harm. The evil influence continued in some sense, although in much diminished degree, into the next decade or so. Yet I am now asked to tell you something about what this movement of thought called the *Naturphilosophie* was, and about what its relations to the natural sciences were up to, say, 1840. But what interest can you take to-day in the story of the evil influence of an enemy that is said indeed to have threatened the cradle of your infant science of modern pathology, but that very early lost all its power to harm. As a fact, the *Naturphilosophie,* viewed as an officially recognized tendency that could possess any strong direct influence in Germany, was very nearly dead before the great days of 1848 came. Since its death, the *Naturphilosophie* has seldom been mentioned by anybody except with contempt. Its later direct and overt influence upon the course of scientific discovery has been nothing. Nothing then that is of any critical importance to the later development of your science seems to be involved in the story that I have been asked to rehearse.

In fact, to speak in a figure, your science of modern pathology, as Virchow nourished it, proved to be a sort of Hercules. In his infancy this Hercules strangled various serpents. One of these is understood to have been so much of the *Naturphilosophie* as a hostile metaphysical power had sent forth to vex medicine, and as still survived to be strangled. Now the original Greek Hercules and his friends were no doubt always fond of telling over, in later years, the story about the strangling of the serpents by the infant. But I have not heard that Hercules and his friends ever put anybody into my present position by asking him to read them a paper on the natural history of snakes. I doubt whether either Hercules or his companions would have found such a paper interesting. Snakes, they would have said, are to be strangled, not studied. The difficulty of my own position in your presence to-night is of course further increased by the fact that I, who study philosophy, doubtless must seem to some of you to be myself a representative, in some sense, of the very generation of vipers in question. My task is therefore hard indeed.

One thing alone has given me the courage to attempt the enterprise. This is the fact that if the direct and easily visible influence of the *Naturphilosophie* upon the later growth of modern science was indeed small, its indirect and relatively invisible influence was probably large, while this latter influence was of a sort which not only may interest you, when I point it out, but which also probably determines some of your own scientific interests even at the present day. I can not show you then that the literal teachings of the *Naturphilosophie* accomplished much of direct moment or of critical importance for the science of that time. But I think that as a fact the spirit of the *Naturphilosophie* did enter, more or less unconsciously, and in ways which were not always evil, into the life of later scientific thinking. I do find that this spirit tends at the present time to be revived, and by some scientific men too,—to be revived, I say, in forms which, as I hope, will prove to be far nobler and more stable than were those which grew up in the first two decades of the nineteenth century. I see moreover that when we try to estimate what this more immortal part of the *Naturphilosophie* meant, we are led to certain considerations about the true spirit and methods of natural science,—to certain questions in which I, as a student of logic, am much interested, and in which, as I believe, you too may take some interest. And so, doubtful as

my task is, it is not wholly hopeless. Perhaps, after all, before I am done I may show you a few facts in which as students of the methods and of the general relations of your own science, you may find something that will be serviceable.

My plan will be this: First I shall sketch for you in the barest outline the external history of the movement called in Germany the *Naturphilosophie*—its rise, its brief success, its inglorious downfall and end. I shall lay stress, of course, on its relations to natural science, such as they were. Then, secondly, I shall try to indicate to you what the deeper ideas were which lay behind and beneath all the vanities and the excesses of the *Naturphilosophen*. Thirdly, I shall try to indicate how these deeper ideas, despite the vanishing of the *Naturphilosophie* from the scene, indirectly but seriously influenced the course of the later development of natural science in the nineteenth century, and how these ideas seem to be traceable even in some aspects of the history of your own science, so far as those aspects are visible to the layman. Fourthly, and lastly, I shall present to you the question whether some light is not thrown upon the logic of natural science, upon the ideals and methods of scientific work, by considering the relation between those deeper ideas that inspired the *Naturphilosophie* and the actual growth of scientific investigation in the years since 1840.

I

First then, for the purely external, and the least interesting aspect of our story.

At the opening of the nineteenth century, a very notable philosophical movement was under way in the thought of Germany. This movement had been initiated, in the years about and after 1780, by Kant—himself a man of considerable training in the physical sciences of his time, of considerable acquaintance also with the empirical study of human nature, and of a very sane, sober and critical judgment. Kant intended, amongst other things, to define and to formulate a philosophy of the principles and methods of the natural sciences. He succeeded so well that his ideas are still of great importance for any serious student of logic and of the theory of knowledge; and their value for such a student will not soon be exhausted.

But Kant's influence was not confined to the study of the foundations and methods of science. He still more immediately influenced his time with regard to questions of ethics, of theology, and of the more fundamental religious issues of life generally. As a fact, his age—which soon became the age of the French Revolution, and of the great classical literature of Germany, was in his country an age of the humanities, rather than of the natural sciences. His influence was therefore felt, at the moment, much more in the direction of the humanities, than in any other way. The philosophical movement to which he gave rise, accordingly, soon grew beyond what he had intended, and concerned itself with a constructive creation of idealistic systems of thought such as he himself considered unjustifiable. And in these systems, about and after the year 1800, the principal stress was laid upon what were essentially ethical and theological issues. The post-Kantian idealists conceived their philosophy as a sort of substitute for all that traditional religion had so far meant for the world, or at least as a discovery of the absolute rational warrant for new and higher stages of the religious consciousness. So a great part of their work had no direct relation to the business of natural science.

It came to pass, however, just before 1800, that one of the most enthusiastic of these young idealists, namely, Friedrich

Wilhelm Joseph Schelling, was led by motives, which I need not pause here to portray, to turn a large share of his attention to an effort to absorb into his absolute system an organized theory of the nature and meaning of the physical universe. Schelling called this portion of his doctrine the "Philosophy of Nature." That special use of the term *Naturphilosophie* with which we are here concerned was thus due to Schelling. It meant an interpretation of nature in the light of the principles of an idealistic philosophy.

Of Schelling's genuine significance as a philosopher this is not the place to speak. Of the man himself, a very general characterization is more possible. In 1800 he was twenty-five years of age. Yet he was already a professor at the University of Jena, to which he had been called in 1798 by Goethe's recommendation; and he was also, before the close of the eighteenth century, a celebrated man and a prolific author. He was, in this his decidedly wonderful youth, an intensely restless genius, all aglow with brilliant and often with very genuinely significant ideas—a man of a tropical intellectual fecundity, but also of dangerous self-confidence. In polemic he was merciless, in expression enormously complex, in literary form strangely unequal. The luminous and the hopelessly opaque stand side by side in his books in the strangest contrast. His industry was enormous, his sincerity unquestionable, his real power unmistakable, his waywardness exasperating, his frequent obscurity unpardonable, his contemporary influence vast, but most of his work, despite its frequent value, still far too unstable. He inspired a generation of young men, but did them little good that was at once direct and permanent. He wrote down some thoughts that deserve to be remembered for all time, yet so affected his contemporaries that the best of them later turned almost wholly away from him. He thus proved, in the long run, to be an irritant rather than an organizing power. His work was often like that of a whirlwind in the world of thought, disturbing, cloud-enshrouded, momentous, but dissatisfying. After 1803 he left Jena, lived long in South Germany, lost his place for many years as a leader of the national thought, passed through various periods of further philosophical development, lived to a stately and ineffective old age, came once more in 1841 into a brief prominence as a public lecturer in Berlin, but then, retiring yet again from public notice, died in 1854, nearly eighty years old. His published works number fourteen volumes octavo.

For our present purposes, in order to sketch the youthful Schelling's *Naturphilosophie* as he formulated it in the years about 1800, I shall content myself with the following: Certain reasons which I need not now try to portray, but which, in view of the history of human thought, are, to say the least, strictly intelligible reasons and which are in their true interpretation, as I myself think, quite defensible reasons, led Schelling to hold, as many philosophers had held before him, that the universe in which we live is in its inmost nature a single organized unity. In other words, Schelling was what you nowadays often hear called a monist. Moreover, Schelling was confident that philosophy, as it was in his time, was prepared to give a new and final interpretation of this unity of things. Now an account of the unity of the world would of course undertake to consider the problems of theology, of ethics, and of the philosophy of mind. But this same philosophical account, as Schelling held, would also include a discussion of the nature, the unity and the meaning, of the physical world. Such an account—such a philosophical theory of nature—as Schelling often and expressly

maintained, would be, in one aspect, an *a priori* theory, that is, it would be based upon the general character of our own knowledge of nature, and upon the demands which are made by our reason. For, as Schelling held, truth can not be accepted by us, unless we can recognize it as in some sense our own truth, the expression of our own rational demands. Great stress was thus laid, by the philosopher, upon the share which our own self-conscious insight has in defining for us the nature of things. It would be a mistake, however, to suppose that the youthful Schelling, even with all his enthusiasm, actually ventured to attempt to spin all the contents of his *Naturphilosophie* out of his bare and unaided inner consciousness. He was both ignorant and contemptuous of the well-disciplined procedure of the more abstruse experimental sciences; but he was not ignorant of the broader results which the natural sciences of his time reported; and he took considerable interest in these results. Moreover he was, in a way, an enthusiastic although very undisciplined observer of nature. His defect was thus not like the defect of a modern christian scientist who simply turns away from natural phenomena, and denying that they mean anything but mortal error, does indeed get a theory of nature only by means of deliberately ignoring natural truth. Schelling's defect was rather that of an esthetically minded enthusiast who revels in the study of a great variety of natural phenomena, but who undertakes to interpret these phenomena by means of personal intuitions. Meanwhile these intuitions themselves were, with Schelling, by no means those of a mere child, or of a savage, but of a wayward yet highly cultivated young man of the close of the eighteenth century. They were intuitions which presupposed, and undertook to interpret, the results of much miscellaneous reading, and of a good deal of undisciplined observation on Schelling's part relating to physical, chemical and biological facts and theories. You can not doubt Schelling's capricious but extensive industry in the study of nature. His fault lay in his self-assurance, in his impatience, and in his determination to tell nature at once upon meeting her precisely what she meant. Amongst his favorite classes of phenomena, about which he read and speculated, were those of electricity and magnetism, of chemical affinity, so far as these phenomena were then known, and of organic development. He was indeed far beyond the uncultivated fashions of interpretation which we know so well in ordinary cranks. Yet much of his work was as vain as circle-squaring in its actually resulting relation to any concrete business of natural science. Schelling had amongst other things a considerable and a somewhat mischievous interest in medicine. What now is called psychical research was a favorite occupation of the time; and that too won a good deal of Schelling's attention. In 1806, after Schelling had left Jena, he began to publish, in union with a friend and partial disciple of his, A. F. Marcus, a periodical called *Jahrbücher der Medicin als Wissenschaft*. Of this periodical three volumes appeared at Tübingen, the third and last in 1808. The articles to be found in it include an extensive series of aphorisms on the *Naturphilosophie* by Schelling, papers on animal magnetism by Schelling's brother (himself a physician), essays on the application of various metals (iron, mercury) in medicine by Marcus, papers on the relation of botany to medicine by Steffens, on inflammation by Marcus, and so on.

As the mention of this journal shows you, the *Naturphilosophie* of Schelling had from the first the tendency not to remain the expression of the individual philosopher, but

to form a school, to apply itself to various arts and sciences, to publish in journals special researches—in brief, to assume the outward seeming of a progressive and humane science. Ere long it had representatives, exemplifying various grades of discipleship, in academic chairs in Germany. To the young men who fell under its influence it sometimes meant, no doubt, a chance simply to spare themselves serious effort in their study of natural science. A young medical man might learn phrases instead of making laborious observations. On the other hand, one can not accuse most of the prominent *Naturphilosophen* of laziness. They were for the most part very industrious writers and thinkers and some of them did a great deal of empirical investigation. Their enthusiasm was due to their belief that they had found a general way of interpreting the results of natural science so far as these were known to them. As the age was one when, in Germany, the teaching of the natural sciences had been for some time at a low ebb in the German universities, there is something to say for the view that the whole movement of the *Naturphilosophie* was the first crude and eager beginning of a new era of scientific activity in that land, rather than a hindrance to an already developed scientific movement. For the rest, the fact that results of natural science, obtained for the most part outside of Germany, had suggested to that period new and attractive ideas, which seemed to promise surprising generalizations—this fact, I say, serves in some measure to excuse the enthusiasm of the *Naturphilosophen.* The discovery of galvanism, the general progress of the knowledge of electricity, the beginnings of chemistry, the various beginnings of discovery in the biological sciences —all these things constituted fascinating temptations to overhasty generalization. To these temptations the *Naturphilosophen* fell a prey. As to the precise extent to which the *Naturphilosophie* directly affected the scientific thought of Germany, mere statistics may show something. Three only of the philosophers who were especially identified with the movement are now remembered as of note in the history of philosophy. These are Schelling himself; the Norwegian Steffens, who mostly lived and wrote in Germany, and was professor in Halle and Berlin; and Oken, the one amongst the *Naturphilosophen* who had the most serious and varied training in natural science, and the most direct influence upon important scientific activities outside of philosophy. Oken instituted, for instance, the yearly gatherings of the German *Naturforscher* and *Aertzte.* In addition to these men, Ueberweg, in his "History of Philosophy," finds it worth while to mention, amongst the followers and allies of Schelling, ten different men who may be said to have been in the main *Naturphilosophen.* None of these are of great historical importance from the point of view of later thought, although they are men of decidedly various degrees of power and service in their time. Some philosophers of the first rank, such as Hegel, who also belong to that age, and contributed to some form of the *Naturphilosophie,* are nevertheless not to be reckoned among the *Naturphilosophen* proper, because their main work and influence lay elsewhere. Hegel's *Naturphilosophie* was only a small part of that thinker's encyclopedic system, and that part of his system contributed little to his historical influence.

If one turns to the directer influence of the *Naturphilosophie* upon the more special sciences, I find that Siegmund Günther in his "Geschichte der anorganischen Naturwissenschaften im 19ten Jahrhundert," mentions only five or six names as those of men sufficiently important on the side of

their relations to natural science to need consideration from his point of view as representatives of the *Naturphilosophie.* On the other hand, F. C. Müller, in his "Geschichte d. organischen Wissenschaften im 19ten Jahrhundert," beginning his mention of the *Naturphilosophen* who influenced the organic sciences with Schelling and Oken, adds thereupon the names of fifteen others whom he classes as "*Bedeutendste medicinische Naturphilosophen.*" Of these Steffens and Marcus have already been mentioned. The rest are described as men of various caliber—some of them medical authors, most of them professors—some of them contributors of important special researches in medicine—others less fruitful. To the most important belong Kielmeyer, who greatly influenced some portions of the work of his contemporary Cuvier, and Ignatius Döllinger, who was a center of great importance in medical teaching at Würzburg. Hirsch, in his "History of Medicine in Germany," enumerates a still somewhat larger list of more or less pronounced *Naturphilosophen* who deserve mention from the medical point of view—altogether more than a score. Hirsch, J. C. Müller and Haeser, in his "Geschichte der Medicin," agree in giving much the same impression of the activities of these men—several of them special investigators of much industry and productivity, several of them persons who gradually worked themselves free from the formulas of their philosophy—all of them injured, in the eyes of later science, by a tendency to constructive formulas of an unjustifiable type. Where they did good work, in the general biological sciences, their work was usually, as I gather, in relation to some aspect of the study of the evolution and the comparative morphology of living forms.

It is customary to say that these *Naturphilosophen* stood altogether in the way of the new awakening of the natural sciences in Germany. But as I have already said, while philosophy no doubt did medicine mischief in those days, it is still at least partly true that these *Naturphilosophen* constituted a transition from a time of scientific stagnation to one of great activity. They must be judged, accordingly, as beginners rather than as mere mischief makers. Their most characteristic work falls before 1820. Before 1830 the school had been led, in their relations to pure philosophy, by the official success of Hegel's doctrine at Berlin, to occupy a less notable place as a subordinate part of a philosophical movement in which, for Hegel himself, religious, political, and ethical issues were more important than were those of the interpretation of nature. After Hegel's death, in 1831, the movement of the *Naturphilosophie* ere long began to lose the sort of moral support that his type of constructive idealism could give to it. For the Hegelian school became absorbed in religious and in political conflicts, split up into parties, and soon lost whatever touch it had possessed with the progress of natural science. The consequence was that after 1830, the *Naturphilosophie*, neglected by the philosophers themselves, generally denounced by the academic leaders of natural science, and little defended by its own now aging followers, rapidly lost its hold upon the public. Virchow still regarded it as a danger until 1848. After 1848 he too speaks of it as altogether dead.

II

So much for the external history of the movement. But now for some words as to its leading ideas and as to its indirect influence.

An idea may be advanced by a man who has no sufficient logical right to hold it. That idea may later become fruitful in the

minds of wiser men. The originator is then often either forgotten or condemned. But the idea is none the less potent and valuable. Now amongst the leading ideas of the *Naturphilosophie* were a number which have since proved to be of no small importance in the sciences. The first of these ideas is a vague and an ancient, but a powerful idea, which the *Naturphilosophie* simply translated into more modern terms, and so prepared, as it were, for use in the new century. This is the idea that all science must strive to be one, that special research must be governed, in the long run, by the aim to bring truth into unity, and that unity is always beneath all sorts of plurality, as the basis and the meaning thereof.

I have said that this idea is vague. It always remains vague until you discover, in some field of knowledge, in what sense it is true. Then it always appears very luminous, and you rejoice in it. I have said that this idea of the essential unity of truth is ancient. The Greeks began with it. The sages and the saints lived and died for the sake of it. The church tried to secure its recognition by means of a catholic creed. The medieval mystics revelled in it. Yet many heretics also gloried in it as their own peculiar possession, and Giordano Bruno was burned for the sake of it. The modern philosophers renewed the idea. Spinoza reared a beautiful monument of thought in its honor. The *Naturphilosophen* spent their strength in proclaiming it. And since their time modern science, in the later theory of energy, in the doctrine of evolution, in various other ways which I need not enumerate, has illustrated it with unexpected brilliancy, and with marvelous precision.

Now this idea, that the unity of the truth is deeper than is even the most baffling variety of phenomena—what does this idea mean? In what sense is it a leading idea of science as well as of religion and philosophy? To this question it is easy to answer that by the unity of truth one means nothing that one would have a right to assert of any world that is foreign to human thought. One means only that man always strives and must strive for his own rational purposes, to get his ideas into some sort of rational connection, and to view them as a system. The demand that truth shall hang together and be one whole is man's demand. His reason restlessly searches for such unity, and is discontented until the quest succeeds. This is indeed the fact. Man's reason demands that man's experience shall be viewed as a connected whole. Well—this, apart from their obscurities, is precisely what the *Naturphilosophen* taught. Since they were idealists, they did not view the world as anything foreign to the human reason. Hence they founded their interpretation of the unity of things expressly upon the needs and the interpretation of man's own rational nature. Vague as their thinking was, it did therefore express a decidedly sound consciousness of the motives that lead us to seek for unity in the world of scientific truth. Now you may rightly say that the *Naturphilosophen* had no right to prescribe to nature, as they did, just how her laws should be interpreted even before they had been adequately observed. But, on the other hand, men generally do not find until they eagerly seek. The *Naturphilosophen* set their countrymen eagerly seeking for unity in nature. They specialized the vaguer ancient idea of unity by giving it conscious relations to the newer fields of natural science. I am tolerably certain that the eager search thus begun had a very real, even if a mainly indirect, influence upon the successful prosecution of the search which so soon followed the decay of the *Naturphilosophie* itself. I

shall show you in a moment a little evidence bearing upon the subject.

The second of the leading ideas of the *Naturphilosophie* related to the special form which they conceived the unity of natural truth to take. They were very fond of speculating upon the unity of what we now call the various forms of natural energy. Light, electricity, magnetism, the vital processes, these, they were disposed to insist, were forms or stages of a single, all pervasive natural process. Now, nobody with the least sense for logical connections can for a moment confuse the modern doctrine of energy, with its exactness of quantitative definitions and relations, with the vaguely conceived teleological unity that the *Naturphilosophen* ascribed to the natural world. On the other hand, nobody who considers fairly the history of the topic can fail to see that the modern doctrine of energy had two very distinct, but marvelously related sources. One of these sources was the state of modern technological knowledge in the early part of the nineteenth century. The other source is the state of general philosophy in the same period. The modern doctrine of energy is due, I insist, to a curious and unintended alliance between the interests of the engineers and the ideas of the philosophers. I shall recur to this topic again very soon. For the rest, one may say that a conception like that of the modern doctrine of energy is not found until one learns to look for it in the right spirit. The *Naturphilosophie* had its indirect part in creating this right spirit with which later men, far better equipped than were the *Naturphilosophen* themselves, looked for the truth which took form in the doctrine of energy.

Thirdly, the *Naturphilosophie* had another leading idea which more directly concerns your own science. This was the idea of comprehending organic products by conceiving them as results or at any rate as stages, of a process which has the form of an evolution. The more modern evolutionary ideas are prefigured in all sorts of vaguer and of more concrete forms by the various *Naturphilosophen,* from Schelling onwards. Oken comes nearest of all of the group to using categories like those of a modern evolutionist. When, in the generation that was in its early prime in the thirties and the forties, various naturalists made a systematic method of appealing to a study of the embryology, of the early stages, of any natural form, as a principal means of understanding its mature structure, they were following a leading idea which was again in one sense a very ancient idea, since the Greeks already possessed cruder forms of this idea. But, on the other hand, this leading idea had assumed, by the time in question, shapes which it could not have assumed had not the *Naturphilosophen* preceded. Herein lay, in all probability, one of the most substantial of their indirect influences upon the course of later science. In the minds of the *Naturphilosophen,* this idea of conceiving organic nature as a process to be understood in evolutionary, or at least in quasi-evolutionary ways, was a direct result of their philosophical principles. They not only possesed the idea; but they applied it in ways which brought it into relations with modern science. The predominance of *Entwickelungsgeschichte* in all the later studies of German science in the nineteenth century is in all probability largely influenced by the indirect effects of the *Naturphilosophie.*

As you see, no one of the three leading ideas just mentioned can be regarded as originated by the *Naturphilosophie.* Each is, in some sense and in some degree, a very old idea. But the interest of the *Naturphilosophie* lies in the fact that just be-

cause of its enthusiastic efforts to reform and to conquer the natural science of its time, it gave to these old ideas a new turn, a new setting, a new application, a new translation. The *Naturphilosophie* supposed itself to be interpreting the world of natural science in the light of its own philosophical ideas. As a fact, it was rather interpreting certain ancient philosophical ideas in the light of the facts which it learned in the course of its rather undisciplined study of science. But by thus reshaping the old ideas into modern forms, it prepared them to become leading ideas for a later generation of serious scientific workers. For, when it thus translated them into more modern terms, it rendered them comprehensible and attractive to men of the new time. It made them seem portentous to its own generation. The *Naturphilosophie* itself was soon dead, and mouldering in the grave. These leading ideas, its soul, went marching on.

III

I have now enumerated three of the leading ideas of the *Naturphilosophie*. You will properly ask what evidence there is that leading ideas derived from such sources actually influenced any serious scientific workers of a later period.

And so I come, hereupon, to a very inadequate report of an interesting class of phenomena, whose significance the historians of the nineteenth century science seem to me to have somewhat neglected. Let me call your attention to the following biographical facts regarding a number of notable scientific men.

Johannes Müller, the physiologist, born in 1801, studied from 1819 to 1822 in Bonn. His most notable teachers in medicine were *Naturphilosophen* in tendency. Bonn was then a center of medical *Naturphilosophie*. Müller later rejected the philosophy in question—how vigorously I need not tell you. But he always remained in spirit, as I have understood from the authorities, in the better sense a distinctly philosophical physiologist. He abandoned speculation, but he did not abandon synthesis. His *Habilitationsschrift* in 1830, at Bonn, related to embryology, which also received other contributions from him. His great work on physiology is a synthetic one. He always viewed his special work in its relations to the whole medical science. His influence was in the direction of unity as well as of thoroughness. Amongst his pupils were Helmholtz, Du Bois Reymond, Schwann and Virchow—all of them men of a distinctly philosophical universality of grasp.

J. L. Schönlein, born in 1793, studied in Würzburg from 1813 to 1816. Here he was under the influence of the *Naturphilosophie*. Later he, too, as I learn from the historians of medicine, achieved his scientific independence. He is called by Haeser the founder of exact modern clinical methods in Germany; and was the center of a great school of medical workers, to which Virchow also later belonged. He was a clinical organizer rather than a productive writer; but the influence of philosophical interests upon his work appears to have been decided.

To pass to another field of scientific work, Von Baer, the embryologist, was a pupil of Döllinger in Würzburg. Döllinger was a prominent medical *Naturphilosoph*. It was he who seems to have first set both Von Baer and Von Baer's contemporary and coworker Pander to work upon embryological researches. Döllinger himself, as *Naturphilosoph*, had been led to work upon comparative anatomy. His merit as the inspirer and teacher of Von Baer is expressly recognized by Franz

Müller in the latter's just quoted "Gesch. d. org. Naturw. im 19ten Jahrh."

Nägeli, the botanist, whose philosophical predispositions were very manifest in all his work, was born in 1817, was for a time under the influence of Oken, heard Hegel in Berlin, soon turned away from the *Naturphilosophie* with a decided sense of disillusionment, contributed largely to science, but remained in spirit a philosopher to the end of his days.

More indirect, but extremely obvious, is the relation of Virchow himself to the *Naturphilosophie.* Born in 1821, and growing up as he did in the generation when the *Naturphilosophie* was generally regarded with disfavor by all the strongest scientific men, Virchow, like Helmholtz, had not first to live through and overcome an adherence to the doctrines of the *Naturphilosophen.* But he too was as full of a philosophical spirit as if he had been a speculative thinker. His essay, "Die Einheitsbestrebungen in der wissenschaftlichen Medecin," belonging to the late forties, is a defense of certain leading ideas which he never could have formulated if he had not come to consciousness under the influence of the philosophical problems of his time. His interesting conception of the relation of medicine to social science, and even to politics, his definition of his own philosophy as "Humanism," his insistence upon the search for unity of knowledge as the justification of all specialism—these are all philosophical notions which one can only understand in their relations to German thought at large. Virchow's frequent return, in his various addresses, to the portrayal of the history and the merits of the controversies of the period of the *Naturphilosophie,* show how much he was dependent for his original inspiration and his spirit upon the issues that the *Naturphilosophie* defined. In what sense does science seek for unity? How is science related to religion, to the humanities, to the social interests of mankind, to the problems of the theory of knowledge? These are problems which Virchow repeatedly faces. His vindication of the right and the duty of special research is a philosophical one. Moreover, he too, as you well know, founds his work as a pathologist upon the leading idea that the study of the *Entwickelungsgeschichte* of tissues, and, in particular, of morbid growths, must be a central task for the pathologist. Experience vindicated the value of this idea. But the history of philosophy had a good deal to do with the importance which the idea had obtained during the time of Virchow's own youthful process of development.

So far for a few examples of tendencies which were in those days quite prevalent. But now for a somewhat more general view. Nobody who takes a broader survey of the history of German scholarship in the second and third and fourth decades of the nineteenth century can fail to see how wide-spread was the influence of what may in general be called the evolutionary idea upon the whole conduct of special research. It makes no difference whether you turn to pathology or to Indo-European philology, to the work of the students of jurisprudence or to that of the comparative embryologists, whether the cell-theory or Bopp's Comparative Grammar is used as your illustration—all sorts of branches of special natural research, outside of physics and chemistry themselves, and especially in Germany, were in those days guided by the idea that the most important aspect of natural objects and processes that could be studied was their historical aspect, their growth, the history of their evolution, unless indeed, as in physics and chemistry, the phenomena presented few or no points

of attack for such a type of research. In my "Spirit of Modern Philosophy," twenty years or so since, I pointed out the meaning and the historical source of this general tendency of German science and scholarship in the period in question. While preparing that book I at one time made for myself a list of those great treatises belonging to the years between 1815 and 1835—treatises issued in Germany, each one of which may be called epoch-marking in its own branch of historical or of more or less definitely evolutionary research. It is a list of notable works, which shows a constant widening and deepening interest in the growth of institutions, civilizations, art, religion, organisms, languages—in short, of whatever lives and can grow.

Now this interest in the evolutionary aspect of things had not been characteristic of the eighteenth-century science. It did not until much later become as prominent in English or in French science as, during the decades in question, it already was in Germany. Its relation during the years after 1815 in Germany to the leading ideas —to the dreams, if you will, of the previous romantic period of the *Naturphilosophie,* is historically obvious. Its relation to the later organization of the general doctrine of evolution is just as obvious. One has, therefore, to give credit to the *Naturphilosophie* for an indirect influence upon the course of the progress of the most various sciences—an influence as salutary as the direct influence of the *Naturphilosophen* had frequently been enervating or confusing. The special worker might well say, like Virchow, "You, the *Naturphilosophie,* were my enemy, from whom I happily escaped. For you counseled dreamy speculation; while I learned to look faithfully through my microscope at the facts as they were." But the *Naturphilosophie,* had it still lived to follow its own indirect influence, might have replied: "Yes, but I dreamed of evolution, and you special workers found it. I viewed the promised land from Pisgah and died. You crossed the Jordan of hard work and entered in."

To drop metaphor, the sober facts are these—facts of some importance in the history of science, although I have no wish to give them any false importance. Some of the most notable scientific discoverers of Germany in the years between 1820 or 1830 and 1860 were men who had been in their youth, sometimes directly, sometimes indirectly, under the influence of the *Naturphilosophie.* With this influence such men had in general learned to quarrel. They consciously turned away from it to special research. But the influence after all left in them a love for the universal, for the connections of things, for reflection upon the meaning of their special researches, for synthesis. And above all, this influence left in them an intense eagerness to study the connected story of the growth of organisms—a sense for the meaning of evolution—a disposition to interpret facts in the light of the growth of organized processes. Herein lay then an instructive although indirect relation between philosophy and science.

In the inorganic sciences, where the evolutionary idea was, at least at that time, and except in geology, out of place, the indirect influence of the *Naturphilosophie* showed itself mainly in a disposition to seek for the unity that binds into one system the various forms of natural energy.

As I before pointed out, the modern theory of the conservation of energy, of the equivalence of various forms of energy, and of the conditions which determine the transformations of energy, is not the product of any one set of motives. It is in fact

a remarkable example of the union of two sets of motives. The whole experience of modern industrial art gave rise to the induction that perpetual motion is in all forms impossible, that all sorts of energy must be paid for if you mean to use them, and that the expenditure of any form of energy takes place in one direction only, or, in other words, that energy will not, so to speak, run up hill without special costs due to the process whereby it is set running up hill. These were practical inductions, forced upon the users of machines by considerations of need, economy and expense. The steam engine especially taught lessons of this sort, and led Carnot to his famous "Reflections on the Motor Power of Heat." Here lay concealed one side of the coming energy theory. In England a similar union of technological and physical research also led to the threshold of the final generalization. But an important part of the theory was due to quite another sort of man, viz., to a medical man, and one who was in spirit a good deal disposed to large syntheses of a type similar to those of the former *Naturphilosophie*. In the early forties, Mayer had his attention called, while he was physician in charge of a ship's company in the tropics, to the fact that the venous and the arterial blood of his patients were not so different from one another in color as they were in a colder climate. This single fact aroused a long series of reflections upon the process of oxidation in its relation to the production of heat in the organism, and then upon the relation of chemical and organic processes in general, and then upon the relations of both to physical processes. Before Mayer returned to Europe, he had his mind full of an universal theory of the relations of the natural energies, organic as well as inorganic. The theory had the advantage over the Schellingian type of theory that it could be brought into exact relations to experience, and so tested. But in its origin it was a theory of a philosophical type such as the older *Naturphilosophie* might have used had it been acquainted with what the science of 1840 knew.

It was the union of philosophical interests and industrial needs that thus gave birth to the modern doctrine of energy. The moral seems to be that one very good foundation for important scientific generalizations lies in bringing into close relations widely philosophical and intensely and imperiously practical human interests. I think that, as the foregoing historical examples show, medicine itself has more than once greatly profited by just such an union. The industrial and the medical arts, if too much oppressed by the mere desire to accommodate themselves to the momentary needs of individual men, tend, when left to themselves, towards a shallow and unprogressive empiricism. Philosophy, by itself, tends, when applied to the subject matter of such arts, to fruitlessly vague dreams. But the union of the industrial or the strictly practical and the philosophical spirit tends to produce men like Virchow, or doctrines like the modern doctrine of energy. Hence I myself heartily welcome the introduction of technological enterprises into modern universities; and I also believe that the useful arts are all the better off for being troubled occasionally, by the neighborhood of philosophy. Philosophy, on the one hand, and the useful arts, on the other, are too often somewhat like the pine and the palm tree of Heine's well-known lyric. They are far apart; but they sometimes long for each other. It is a pity to keep them in such isolation.

IV

But now, finally, what follows from the foregoing historical sketch for our under-

standing of the logic of scientific method? I venture still to add these few summary comments as I close.

Inductive scientific generalizations, in the logically simplest cases, depend upon what Mr. Charles Peirce has defined as the method of taking a "fair sample" of a chosen type of facts. Thus one who samples, to use Mr. Peirce's typical example, a cargo of wheat, by taking samples from various parts of the cargo, carefully selecting the samples so that they shall not tend to represent one part of the cargo only, but *any* part chosen at random, employs essentially the same inductive method which, as I gather from inquiry, Virchow used in reaching the main fundamental generalizations of his cellular pathology. Samples chosen for investigation from a great variety of growths show, both in the case of normal and in the case of morbid tissues, that in the observed samples there is sufficient evidence of the origin of each cell from a previous cell, and evidence too that the tissue is formed of generations of cells whose beginnings, both in the normal and in the morbid growths, lead back to parent cells of certain definable types. This outcome of observation, repeatedly confirmed by samples fairly chosen, that is, by samples chosen from various organisms, from various tissues, and chosen not merely to illustrate the theory, but to represent as well as may be all sorts of growths—this, I say, leads to the *probable* assertion that this kind of origin of tissues is universal, and that one is dealing with a genuine law of nature. The probability of such a generalization can be tested in a more or less exact way, as Peirce has shown, by the principles of the mathematical theory of probabilities. Inductions of this type we may call statistical inductions. They presuppose nothing at the outset as to what laws are present in the world of the facts which are to be sampled. The technique of induction here consists wholly in learning, (1) how to take fair samples of the facts in question, and (2) how to observe these facts accurately and adequately. This kind of induction seems to be especially prominent in the organic sciences. Its logical theory is reducible to the general theory of probability, since fair samples, chosen at random from a collection of objects, tend to agree in their constitution with the average constitution of the whole collection.

But now, as you well know, a great deal of scientific work consists of the forming and testing of hypotheses. In such cases the inductive process is more complex. Peirce defines it first as the process of taking a fair sample from amongst the totality of those consequences which will be true if the hypothesis to be tested is true, and secondly as the process of observing how far these chosen consequences agree with experience. If a given hypothesis, in case it is true, demands, as often happens, countless consequences, you of course can not test all of these consequences, to see if every one of them is true. But you select a fair sample from amongst these consequences, and test each of these selected consequences of the hypothesis. If they agree with experience, the hypothesis is thereby rendered in some degree probable. The technique of induction now involves at least four distinct processes: (1) The choice of a good hypothesis; (2) the computation of certain consequences, all of which must be true if the hypothesis is true; (3) the choice of a fair sample of these consequences for a test; and (4) the actual test of each of these chosen consequences. So far as you make use of this method of induction, you need what is called training in the theory of your topic, that is, training in the art of

deducing the consequences of a given hypothesis. This may involve computations of all degrees of complexity. You also need training in the art of taking a fair sample of consequences for your test; for a given hypothesis may involve numerous consequences that are already known, from previous experience, to be true. And such consequences furnish you with no crucial tests. In case of success, your hypothesis may become very highly probable. But induction never renders it altogether certain.

Classic instances of this method of induction exist in the physical sciences. In the organic sciences the process of testing hypotheses is frequent, but is less highly organized, and generally less exact than in the great cases that occur in the inorganic sciences. No theory of the consequences of any hypothesis in the organic sciences has ever yet reached the degree of precision attained by the kinetic theory of gases, or by the theory of gravitation.

So much for the two great inductive methods, as Peirce defines them. But now does successful scientific method wholly reduce to these two processes, viz., (1) sampling the constitution of classes of phenomena; and (2) sampling the theoretical consequences of hypotheses? Many students of the subject seem to think so. I think that the history of science shows us otherwise.

As a fact, I think that the progress of science largely depends upon still another factor, viz., upon the more or less provisional choice and use of what I have already called, in this paper, leading *ideas*.

A leading idea is, of course, in any given natural science, an hypothesis. But it is an hypothesis which decidedly differs from those hypotheses that you directly test by the observations and experiments of the particular research wherein you are engaged. Unlike them, it is a hypothesis that you use as a guide, or in Kant's phrase, as a regulative principle of your research, even although you do not in general intend directly to test it by your present scientific work. It is usually of too general a nature to be tested by the means at the disposal of your special investigation. Yet it does determine the direction of your labors, and may be highly momentous for you.

Such a leading idea, for instance, is the ordinary hypothesis that even in the most confused or puzzling regions of the natural world law actually reigns, and awaits the coming of the discoverer. We can not say that our science has already so fairly sampled natural phenomena as to have empirically verified this assumption, so as to give it a definite inductive probability. For as a fact, science usually pays small attention to phenomena unless there appears to be a definable prospect of reducing them to some sort of law within a reasonable time; and chaotic natural facts, if there were such, would probably be pretty stubbornly neglected by science, so far as such neglect was possible. On the other hand, the leading idea that law is to be found if you look for it long enough and carefully enough is one of the great motive powers not only of science but of civilization.

It may interest you to know that the modern study of the so-called axioms of geometry, as pursued by the mathematicians themselves, has shown that such principles as the ordinary postulate about the properties of parallel lines (as Euclid defines that postulate) are simply leading ideas. What the text-books of geometry usually assert to be true about the fundamental properties of parallel lines is a principle that is neither self-evident, nor necessarily true, nor even an inductively assured truth of experience. It turns out,

in the light of modern logical mathematical analysis, to be, I say, simply a leading idea, —that is, a principle which we can neither confirm nor refute by any experience now within our range, but which we use and need in geometry precisely because it is so serviceable in simplifying the geometry of the plane.

If I may venture to cite an example from your own science, I should suggest the following: That fundamental principle of Virchow's "Cellular Pathology" which asserted the origin of every cell from a cell was, as I already said, a perfectly straightforward induction, of Peirce's first type, that is, it was a probable assertion of a certain constitution as holding for a whole type of cases—an assertion made simply because this constitution had been observed to hold for a sufficient number of fairly selected samples of the type. But, on the other hand, consider another principle which Virchow asserted already in 1847 or earlier, and which, as I have long been told, has been of the first importance for the whole later development of your science: "We have learned to recognize," says Virchow, "that diseases are not autonomous organisms, that they are no entities that have entered into the body, that they are no parasites which take root in the body, but that they merely show us the course of the vital processes under altered conditions" (*"das sie nur den Ablauf der Lebenserscheinungen unter veränderten Bedingungen darstellen"*).

Now of course I have nothing to suggest regarding the objective truth of this assertion. But I venture to point out that, logically regarded, it is not an hypothesis to be definitely tested by any observation, but is rather an hypothesis of the type of Euclid's postulate about the parallel lines, that is, it is a leading idea. For, on the one hand, how could Virchow regard this principle as one that had been definitely tested, and already confirmed by direct observation and experience at a time when, as in 1847, he was not yet possessed even of his own general principle of a cellular pathology, and when he regarded the whole science of pathology as in its infancy, and the causation of disease as very largely unknown. On the other hand, what experience could one look for that would definitely refute the principle if it were false? Would the experience of such facts as those of your modern bacteriology refute that principle? No, at least so far as I understand the sense of the principle as Virchow stated it in 1847. For when bacteria, or when any of their products or accompaniments came to be recognized either as causing disease, or as affecting the course of disease in any way, it was still open to Virchow to say that the causes thus defined simply constitute these very *veränderte Bedingungen* under which the *Ablauf der Lebenserscheinungen* takes place. In other words, the principle, if understood with sufficient generality, simply asserts that a disease can not occur in an organism without the processes of the disease being themselves alterations of the processes of the organism, and such alterations as the altered conditions, whatever they are, determine. Such a principle, so understood, seems tolerably safe from empirical refutation. It would remain unrefuted, and empirically irrefutable, so far as I can see, even if the devil caused disease. For the devil would then simply be one of the *veränderte Bedingungen*. Thus when the devils on a famous occasion entered, in the tale, into the Gaderene swine, the *Ablauf* of the *Lebenserscheinungen* of the swine was such, under the *veränderte Bedingungen*, that, as we are told, they ran down a steep place into the sea. But I do not see that this just stated pathological postulate of Virchow's need have suffered ship

wreck, or need even have received any damage, even on this occasion. The devils are indeed represented in the tale as entities that from without entered into the swine, as bullets might have done. But the running down into the sea is *nur der Ablauf der Lebenserscheinungen* of the swine themselves. Let bullets or bacteria, poisons or compressed air, be the *Bedingungen,* the postulate that Virchow states will remain irrefutable, if only it be interpreted to meet the case. For the principle merely says that whatever entity it may be, fire or air or bullet or poison or devil, that affects the organism, the disease is not that entity, but is the changed process of the organism. What then is this hypothesis, this rejection of every external-entity-theory of disease, as the hypothesis appears when Virchow writes these words in 1847? I reply, this is no hypothesis in the stricter sense; that is, it is no trial proposition to be submitted to precise empirical tests. It is, on the contrary, a very precious leading idea. It is equivalent to a resolution to search for the concrete connection between the processes of any disease and the normal process of the organism, so as to find the true unity of the pathological and the normal process through such a search. Without some such leading idea, the cellular pathology itself could never have resulted; because the facts in question would never have been observed. And I suppose that some equivalent leading idea, if not precisely that which Virchow stated in 1847, is just as precious to you to-day in your own pathological work.

The value of such leading ideas for a science lies in the sorts of research that they lead men to undertake, and also in the sorts of work that they discourage. They are, I repeat, regulative principles. Observation does not, at least for the time, either confirm or refute them. But, on the other hand, they awaken interest in vast ranges of observation and experiment, and sustain the patience and enthusiasm of workers through long and baffling investigations. They organize science, keep it in touch with the spirit of the age, keep alive in it the sense of the universal, and assure its service to humanity. Specialism, without leading ideas, remains but a sounding brass and a tinkling cymbal.

The sources of useful leading ideas seem to me to be various. Social, and in particular industrial interests, suggest some of them, as the perennial need of paying the coal-bills for the steam engines suggested, as we have seen, one of the leading ideas which pointed the way towards the modern theory of energy. The comparison of the results of various sciences awakens such leading ideas in various minds. Schleiden set Schwamm searching for the basis of the cell theory in animal tissues. That was the suggestion of an hypothesis in the narrower sense, to be tested. But when the physical sciences set the students of organic science to the work of conceiving organic processes as mechanical in their inmost nature, that was the suggestion of a leading idea.

But another source of such leading ideas has been, upon occasion, philosophy. Philosophy itself might be defined as a systematic scrutiny of leading ideas. It has also proved to be often an inventor and interpreter of such ideas. Its faults in its work have been frequent and obvious. In answer to Dr. Councilman's request I have tried, dispassionately, to point out such faults in the *Naturphilosophie.* It has also been my duty to point out some of the excellencies that went with these defects. The moral of my story is, I suppose, that it is the interaction of various types of human thought and investigation, and not mutual isolation or contempt, which helps us all, while he

does best who works as you do in medicine with the profoundest theoretical problems and the most intensely practical interests at once pressing upon him, with the widest and most philosophical breadth of view, and the most faithful special labor, at once demanding attention.

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SOME TABLES OF STUDENT HOURS OF INSTRUCTION

IN the days of President Dunster, the publications of Harvard University gave the curriculum leading to the first degree in arts in a single sentence thus: "The first year shall teach Rhetoric, second and third years Dialectics, and the fourth year shall add Philosophy." In no such simple form are the requirements for graduation set forth in a modern college catalogue. To determine exactly what studies must and what studies may be included in the college course calls in most cases for much study. To learn even approximately how many undergraduates, or what proportion of the undergraduates, are taking courses in any particular subject is in general impossible from the college catalogue. In some departments, many courses are offered, while few students elect; in other departments, few courses are offered and many students take them. At a few institutions the enrollment figures for all classes are now available in the published reports of the president or other officer, but in most cases one must call on the recording office to obtain such figures.

For the sake of the interest which the comparison of such statistics from many institutions may afford, the following tables have been prepared. They give the registration in the various subjects at eighteen more or less representative American colleges and universities. In the first table the numbers of "student hours of instruction" are given by subjects, while the second table gives the same facts in a form more suitable for comparison of the work of different institutions, since in it all the figures have been reduced to, and are expressed in, percentages. These statistics rest on a semester basis and include in general only undergraduates—candidates for the first degree; accordingly, special students and partial course students and all graduate students, so far as possible, have been omitted. Furthermore, in the cases of the universities, only the college of arts, or the college of letters and science, according as that school of the university is named, has ordinarily been included. Thus, the Columbia statistics refer only to Columbia College, the Yale statistics to Yale College, the Harvard statistics to Harvard College, the Wisconsin statistics to the college of letters and science, etc. It is only fair to state at once, however, that the great diversity in the grouping of the work of the universities in different schools makes the results here given unsatisfactory for comparison in the cases of the universities. One university appears to include all of its undergraduate work in engineering in the college of letters, while a second university includes only a little in that school, and a third none. Other differences of similar sort have been found in comparing the figures from the universities. No such difficulties arise with regard to the statistics of the colleges and it is believed that the tables are entitled to full credence for purposes of comparison so far as all the fourteen or fifteen smaller institutions included are concerned.

The figures have been submitted in most cases by the registrar for the purpose of this paper, but in a few instances they have been compiled from the printed report of the president, dean or registrar.

A "student hour of instruction," as that term is used here, means the taking of a course of one hour per week by one student through one semester. Thus, a class of twenty students taking a three-hours-per-week course in English for two semesters gives 120 student hours of instruction in English. The number of student hours of instruction in any course for any semester is obtained by multiplying the number of students in the course by the number of hours per week which that course counts towards graduation; ordinarily, in a