A Scientific Autobiography
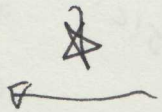
My original decision to devote myself to science was a direct result of the discovery which has never ceased to fill me with enthusiasm since my early youth—the comprehension of the far from obvious fact that the laws of human reasoning coincide with the laws governing the sequences of the impressions we receive from the world about us; that, therefore, pure reasoning can enable man to gain an insight into the mechanism of the latter. In this connection, it is of paramount importance that the outside world is something independent from man, something absolute, and the quest for the laws which apply to this absolute appeared to me as the most sublime scientific pursuit in life.

These views were bolstered and furthered by the excellent instruction which I received, through many years, in the *Maximilian-Gymnasium* in Munich from my mathematics teacher, Hermann Müller, a middle-aged man with a keen

mind and a great sense of humor, a past master at the art of making his pupils visualize and understand the meaning of the laws of physics.

My mind absorbed avidly, like a revelation, the first law I knew to possess absolute, universal validity, independently from all human agency: The principle of the conservation of energy. I shall never forget the graphic story Müller told us, at his *raconteur's* best, of the bricklayer lifting with great effort a heavy block of stone to the roof of a house. The work he thus performs does not get lost; it remains stored up, perhaps for many years, undiminished and latent in the block of stone, until one day the block is perhaps loosened and drops on the head of some passerby.

After my graduation from the *Maximilian-Gymnasium*, I attended the University, first in Munich for three years, then in Berlin for another year. I studied experimental physics and mathematics; there were no professorships or classes in theoretical physics as yet. In Munich, I attended the classes of the physicist Ph. von Jolly, and of the mathematicians Ludwig Seidel and Gustav Bauer. I learned a great deal from these three professors, and I still retain them in rever-

ent memory. But I did not realize until I came to Berlin that in matters concerned with science they had really just a local significance, and it was in Berlin that my scientific horizon widened considerably under the guidance of Hermann von Helmholtz and Gustav Kirchhoff, whose pupils had every opportunity to follow their pioneering activities, known and watched all over the world. I must confess that the lectures of these men netted me no perceptible gain. It was obvious that Helmholtz never prepared his lectures properly. He spoke haltingly, and would interrupt his discourse to look for the necessary data in his small note book; moreover, he repeatedly made mistakes in his calculations at the blackboard, and we had the unmistakable impression that the class bored him at least as much as it did us. Eventually, his classes became more and more deserted, and finally they were attended by only three students; I was one of the three, and my friend, the subsequent astronomer Rudolf Lehmann-Filhés, was another.

Kirchhoff was the very opposite. He would always deliver a carefully prepared lecture, with every phrase well balanced and in its proper

place. Not a word too few, not one too many. But it would sound like a memorized text, dry and monotonous. We would admire him, but not what he was saying.

Under such circumstances, my only way to quench my thirst for advanced scientific knowledge was to do my own reading on subjects which interested me; of course, these were the subjects relating to the energy principle. One day, I happened to come across the treatises of Rudolf Clausius, whose lucid style and enlightening clarity of reasoning made an enormous impression on me, and I became deeply absorbed in his articles, with an ever increasing enthusiasm. I appreciated especially his exact formulation of the two Laws of Thermodynamics, and the sharp distinction which he was the first to establish between them. Up to that time, as a consequence of the theory that heat is a substance, the universally accepted view had been that the passing of heat from a higher to a lower temperature was analogous to the sinking of a weight from a higher to a lower position, and it was not easy to overcome this mistaken opinion.

Clausius deduced his proof of the Second Law

of Thermodynamics from the hypothesis that *"heat will not pass spontaneously from a colder to a hotter body."* But this hypothesis must be supplemented by a clarifying explanation. For it is meant to express not only that heat will not pass directly from a colder into a warmer body, but also that it is impossible to transmit, by any means, heat from a colder into a hotter body without there remaining in nature some change to serve as compensation.

In my endeavor to clarify this point as fully as possible, I discovered a way to express this hypothesis in a form which I considered to be simpler and more convenient, namely: *"The process of heat conduction cannot be completely reversed by any means."* This expresses the same idea as the wording of Clausius, but without requiring an additional clarifying explanation. A process which in no manner can be completely reversed I called a *"natural"* one. The term for it in universal use today, is: *"Irreversible."*

Yet, it seems impossible to eradicate an error which arises out of an all too narrow interpretation of Clausius' law, an error against which I have fought untiringly all my life. To this very

day, instead of the definition I just mentioned, one often finds irreversibility defined as "An irreversible process is one which cannot take place in the opposite direction." This formulation is insufficient. For it is quite possible to conceive of a process which cannot take place in the opposite direction but which can in some fashion be completely reversed.

Since the question whether a process is reversible or irreversible depends solely on the nature of the initial state and of the terminal state of the process, but not on the manner in which the process develops, in the case of an irreversible process the terminal state is in a certain sense more important than the initial state—as if, so to speak, Nature "preferred" it to the latter. I saw a measure of this "preference" in Clausius' entropy; and I found the meaning of the Second Law of Thermodynamics in the principle that in every natural process the sum of the entropies of all bodies involved in the process increases. I worked out these ideas in my doctoral dissertation at the University of Munich, which I completed in 1879.

The effect of my dissertation on the physicists

like Hume

of those days was nil. None of my professors at the University had any understanding for its contents, as I learned for a fact in my conversations with them. They doubtless permitted it to pass as a doctoral dissertation only because they knew me by my other activities in the physical laboratory and in the mathematical seminar. But I found no interest, let alone approval, even among the very physicists who were closely concerned with the topic. Helmholtz probably did not even read my paper at all. Kirchhoff expressly disapproved of its contents, with the comment that the concept of entropy, whose magnitude could be measured by a reversible process only, and therefore was definable, must not be applied to irreversible processes. I did not succeed in reaching Clausius. He did not answer my letters, and I did not find him at home when I tried to see him in person in Bonn. I carried on a correspondence with Carl Neumann, of Leipzig, but it remained totally fruitless.

However, deeply impressed as I was with the importance of my self-imposed task, such experiences could not deter me from continuing my studies of entropy, which I regarded as next to

energy the most important property of physical systems. Since its maximum value indicates a state of equilibrium, all the laws of physical and chemical equilibrium follow from a knowledge of entropy. I worked this out in detail during the following years, in a number of different researches. First, in investigations on the changes in physical state, presented in my probationary paper at Munich in 1880, and later in studies on gas mixtures. All my investigations yielded fruitful results. Unfortunately, however, as I was to learn only subsequently, the very same theorems had been obtained before me, in fact partly in an even more universal form, by the great American theoretical physicist Josiah Willard Gibbs, so that in this particular field no recognition was to be mine.

While an instructor in Munich, I waited for years in vain for an appointment to a professorship. Of course, my prospects for getting one were slight, for theoretical physics had not as yet come to be recognized as a special discipline. All the more compellingly grew in me the desire to win, somehow, a reputation in the field of science.

Guided by this desire, I decided to submit a paper for the prize to be awarded in 1887 by the Philosophical Faculty of Göttingen. The subject to be discussed was, "The Nature of Energy." After I had completed my paper, in the spring of 1885, I was offered the associate professorship in theoretical physics at the University of Kiel. This offer came as a message of deliverance. The moment when I paid my respects to Ministerial Director Althoff in his suite in the Hotel Marienbad, and he informed me of the particulars and conditions of my appointment, was, and will always be, one of the happiest of my life. For even though my life in my parents' house was as beautiful and contented as any man could wish for, my longing for independence kept growing within me, and I was yearning for a home of my own.

To be sure, I suspected, and by no means without reason, that this smile of good fortune was actually not so much a reward for my scientific accomplishments as a practical result of the circumstance that Gustav Karsten, Professor of Physics in Kiel, happened to be a close friend of my father. Nevertheless, this realization could not

mar my supreme happiness, and I was firmly re-
solved to justify the confidence in me in every way
in my power.

I soon moved to Kiel, where I put the finishing
touches on my paper, and submitted it in Göttin-
gen. It won second prize. Besides my entry, two
other papers had been submitted on the subject,
but these two were awarded no prize at all. Ob-
viously, I was wondering why my paper had failed
to win first prize, and I found the answer in the
text of the detailed decision of the Faculty of
Göttingen. The judges set forth a few points of
criticism of minor import, and then stated:
"Finally, the Faculty must withhold its approval
from the remarks in which the author tries to
appraise Weber's Law." Now, the story behind
these remarks was: W. Weber was the Professor
of Physics in Göttingen, between whom and
Helmholtz there existed at the time a vigorous
scientific controversy, in which I had expressly
sided with the latter. I think that I make no mis-
take in considering this circumstance to have
been the main reason for the decision of the
Faculty of Göttingen to withhold the first prize
from me. But while with my attitude I had in-

curred the displeasure of the scholars of Göttingen, it gained me the benevolent attention of those of Berlin, the results of which I was soon to feel.

No sooner had I finished my paper for Göttingen than I returned to my favorite subject, and wrote a number of monographs, which I published under the collective title, *On The Principle of the Increase of Entropy*. In these articles I discussed the laws of chemical reactions, of the dissociation of gases, and finally the properties of dilute solutions. With respect to the latter, my theory led to the conclusion that the values of the lowering of the freezing point, observed in many salt solutions, could be explained only by a dissociation of the substances dissolved, and that this finding constituted a thermodynamic foundation for the electrolytic dissociation theory advanced by Svante Arrhenius approximately at the same time. This conclusion, unfortunately, got me into an unpleasant conflict. For Arrhenius challenged, in a rather unfriendly manner, the admissibility of my arguments, pointing out that his theory related to ions, i.e. electrically charged particles. I could reply only that the laws of thermodynamics

date ?

were valid regardless of whether or not the particles carried a charge.

In the spring of 1889, after the death of Kirchhoff, I accepted the invitation, extended to me upon the recommendation of the Faculty of Philosophy of Berlin, to take his place at the University, to teach theoretical physics. First, I was an associate professor, and from 1892, a full professor. These were the years of the widest expansion of my scientific outlook and way of thinking. For this was the first time that I came in closer contact with the world leaders in scientific research in those days—Helmholtz, above all the others. But I learned to know Helmholtz also as a human being, and to respect him as a man no less than I had always respected him as a scientist. For with his entire personality, integrity of convictions and modesty of character, he was the very incarnation of the dignity and probity of science. These traits of character were supplemented by a true human kindness, which touched my heart deeply. When during a conversation he would look at me with those calm, searching, penetrating, and yet so benign eyes, I would be overwhelmed by a feeling of boundless filial trust and devotion, and I would

feel that I could confide in him, without reservation, everything that I had on my mind, knowing that I would find him a fair and tolerant judge; and a single word of approval, let alone praise, from his lips would make me as happy as any worldly triumph.

I had this experience on several occasions. One of them was when he thanked me emphatically after my memorial address on Heinrich Hertz, delivered before the Physical Society; another, when he expressed his agreement with my theory of chemical solutions, shortly before my election to the Prussian Academy of Sciences. I shall treasure the memory of every one of these thrilling moments to the end of my days.

Besides Helmholtz, I was soon on amicable terms with Wilhelm von Bezold, whom I had known from Munich. Likewise, with August Kundt, the temperamental Director of the Physical Institute, universally liked for his genuine kind human feelings.

The other physicists were not so easy to approach. There was, for instance, Adolph Paalzow, the physicist of the School of Engineering of Charlottenburg, a gifted experimenter, and a

typical Berliner. He would always treat me cordially, yet always make me feel that he had really not much use for me. In those days, I was the only theorist, a physicist *sui generis,* as it were, and this circumstance did not make my *debut* so easy. Also, I had a distinct feeling that the instructors at the Physical Institute were politely but clearly trying to keep me at arm's length. But in the course of time, as we got better acquainted, our relationship assumed a friendlier aspect; one of them, Heinrich Rubens, eventually became my close personal friend, and our friendship was ended only by his death, at an all too early age.

By a sheer whim of fate, no sooner had I reported to my post in Berlin than I was temporarily assigned a task in a field quite remote from my self-chosen special branch of physics. Just at that time, the Institute for Theoretical Physics happened to receive a large harmonium, of pure untempered tuning, a product of the genius of Carl Eitz, a public school teacher in Eisleben, built by the Schiedmayer piano factory of Stuttgart for the Ministry. I was given the task of using this musical instrument for a study of the untempered, "natural" scale. I delved into the prob-

lem with keen interest, in particular with regard to the question concerning the part played by the "natural" scale in our modern vocal music without instrumental accompaniment. These studies brought me the discovery, unsuspected to a certain degree, that the tempered scale was positively more pleasing to the human ear, under all circumstances, than the "natural," untempered scale. Even in a harmonic major triad, the natural third sounds feeble and inexpressive in comparison with the tempered third. Indubitably, this fact can be ascribed ultimately to a habituation through years and generations. For before Johann Sebastian Bach, the tempered scale had not been at all universally known.

My removal to Berlin not only enabled me to associate with interesting personages, but also brought about a sizable expansion of my scientific correspondence. First of all, I became interested in the extremely fruitful theory formulated by W. Nernst, of Göttingen. According to this theory, the electric stresses occurring in electrolytic solutions with non-homogenous concentrations arise from the joint effect of the electric force, due to the moving charges and the osmotic

pressure. Using this theory as a basis, I succeeded in calculating the potential difference at the point of contact of two electrolytic solutions, and Nernst wrote to me later that my formula had been confirmed by his measurements.

In connection with the problems of the electric dissociation theory, I was soon also engaged in a voluminous correspondence with Wilhelm Ostwald, of Leipzig. Our correspondence led to many a critical debate, yet these were always carried on in the friendliest tone. Ostwald, by his very nature a firm believer in systematization, distinguished three different types of energy, corresponding to the three spatial dimensions, namely: Distance Energy, Surface Energy, and Space Energy. Distance Energy, according to him, was the force of gravitation; Surface Energy, the surface tension of liquids; and Space Energy, the volume energy. I replied, among other comments, that there was no such thing as a volume energy in the sense specified by Ostwald. For instance, the energy of an ideal gas does not in fact even depend on the volume, but on the temperature of the gas. If an ideal gas is made to expand without doing any work, its volume increases,

but the energy remains unchanged, whereas according to Ostwald, its energy ought to decrease with the decrease of the pressure.

Another controversy arose with relation to the question of the analogy between the passage of heat from a higher to a lower temperature and the sinking of a weight from a greater to a smaller height. I had emphasized the need for a sharp distinction between these two processes, for they differed from each other as basically as did the First and Second Laws of Thermodynamics. However, this theory of mine was contradicted by the view universally accepted in those days, and I just could not make my fellow physicists see it my way. In fact, certain physicists actually regarded Clausius' reasoning as unnecessarily complicated and even confused; and they refused, in particular, to admit the concept of irreversibility, and thereby to assign to heat a special position among the forms of energy. They created in opposition to Clausius' theory of thermodynamics, the so-called science of "Energetics." The first fundamental proposition of Energetics, exactly like that of Clausius' theory, expresses the principle of the conservation of energy; but its second proposi-

tion, which is supposed to formulate the direction of all occurrences, postulates a perfect analogy between the passing of heat from a higher to a lower temperature and the sinking of a weight from a greater to a smaller height. A consequence of this point of view was that the assumption of irreversibility for proving the Second Law of Thermodynamics was declared to be unessential; furthermore, the existence of an absolute zero of temperature was disputed, on the ground that for temperature, just as for height, only differences can be measured.

It is one of the most painful experiences of my entire scientific life that I have but seldom—in fact, I might say, never—succeeded in gaining universal recognition for a new result, the truth of which I could demonstrate by a conclusive, albeit only theoretical proof. This is what happened this time, too. All my sound arguments fell on deaf ears. It was simply impossible to be heard against the authority of men like Ostwald, Helm and Mach. I was firmly convinced that my claim of the basic difference between the transmission of heat and the sinking of a weight would eventually be proved to be right. But the annoying

thing was that I was not to have at all the satisfaction of seeing myself vindicated. The universal acceptance of my thesis was ultimately brought about by considerations of an altogether different sort, unrelated to the arguments which I had adduced in support of it—namely, by the atomic theory, as represented by Ludwig Boltzmann.

Boltzmann succeeded in establishing, for a given gas in a given state, a function, H, which has the property that its value constantly decreases with time. It suffices, therefore, to identify the negative value of this H with entropy, to arrive at the principle of the increase of entropy. This discovery demonstrates, at the same time, irreversibility to be a characteristic of the processes occurring in a gas.

As events transpired, therefore, my claim concerning the fundamental difference between heat conduction and a purely mechanical process was victorious over the view previously entertained by outstanding authorities. Nevertheless, my contribution to the struggle was entirely superfluous, for even without it the outcome would have been the same.

Obviously, this battle, in which Boltzmann and

recall these remarks of those authors who in on the debates.

Quoted by M. J. Klein
p. 467 54p

Ostwald represented the opposing views, was fought rather heatedly, and produced also many a drastic effect, for the two antagonists were each other's equals in quick repartee and natural wit. After all that I have related, in this duel of minds I could play only the part of a second to Boltzmann—a second whose services were evidently not appreciated, not even noticed, by him. For Boltzmann knew very well that my viewpoint was basically different from his. He was especially annoyed by the fact that I was not only indifferent but to a certain extent even hostile to the atomic theory which was the foundation of his entire research. The reason was that at that time, I regarded the principle of the increase of entropy as no less immutably valid than the principle of the conservation of energy itself, whereas Boltzmann treated the former merely as a law of probabilities —in other words, as a principle that could admit of exceptions. The value of function H might also increase at times. Boltzmann did not go into this point in the deduction of his "H-Theorem," and a talented pupil of mine, E. Zermelo, noted emphatically this gap in a strict proof of the theorem. In fact, Boltzmann omitted in his de-

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does not mention it.

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duction every mention of the indispensable presupposition of the validity of his theorem—namely, the assumption of molecular disorder. He must have simply taken it for granted. At any rate, he answered young Zermelo in a tone of biting sarcasm, which was meant partly for me, too, for Zermelo's paper had been published with my approval. This was how Boltzmann assumed that ill-tempered tone which he continued to exhibit toward me, on later occasions as well, both in his publications and in our personal correspondence; and it was only in the last years of his life, when I informed him of the atomistic foundation for my radiation law, that he assumed a friendlier attitude.

Boltzmann eventually triumphed in the fight against Ostwald and the adherents of Energetics, as it had been self-evident to me that he would, in view of all that I have just mentioned. The basic difference between the conduction of heat and a purely mechanical process became universally recognized. This experience gave me also an opportunity to learn a fact—a remarkable one, in my opinion: A new scientific truth does not triumph by convincing its opponents and making

many others have said the same

them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it.

Otherwise, the controversies just mentioned held comparatively little interest for me, as they could not be expected to produce anything new. My attention, therefore, was soon claimed by quite another problem, which was to dominate me and urge me on to a great many different investigations for a long time to come. The measurements made by O. Lummer and E. Pringsheim in the German Physico-Technical Institute, in connection with the study of the thermal spectrum, directed my attention to Kirchhoff's Law, which says that in an evacuated cavity, bounded by totally reflecting walls, and containing any arbitrary number of emitting and absorbing bodies, in time a state will be reached where all bodies have the same temperature, and the radiation, in all its properties including its spectral energy distribution, depends not on the nature of the bodies, but solely and exclusively on the temperature. Thus, this so-called Normal Spectral Energy Distribution represents something absolute, and since I had always regarded the search

for the absolute as the loftiest goal of all scientific activity, I eagerly set to work. I found a direct method for solving the problem in the application of Maxwell's Electromagnetic Theory of Light. Namely, I assumed the cavity to be filled with simple linear oscillators or resonators, subject to small damping forces and having different periods; and I expected the exchange of energy caused by the reciprocal radiation of the oscillators to result, in time, in a stationary state of the normal energy distribution corresponding to Kirchhoff's Law.

This extended series of investigations, certain ones of which could be verified by comparisons with known observational data, such as the measurements of damping by V. Bjerknes, resulted in establishing the general relationship between the energy of an oscillator having a definite period, and the energy radiation of the corresponding spectral region in the surrounding field when the exchange of energy is stationary. From this there followed the remarkable result that this relationship is absolutely independent of the damping constant of the oscillator—a circumstance which was very pleasing and welcome to me, because it

permitted the entire problem to be simplified, by substituting the energy of the oscillator for the energy of the radiation, thus replacing a complicated structure possessing many degrees of freedom, by a simple system with just one degree of freedom.

To be sure, this result represented a mere preliminary to the tackling of the real problem, which now loomed all the more formidably before me. My first attempt to overcome it was unsuccessful, for my original silent hope that the radiation emitted by the oscillator would differ, in some characteristic way, from the absorbed radiation, turned out to have been mere wishful thinking. The oscillator reacts only to those rays which it is capable of emitting, and is completely insensitive to adjacent spectral regions.

Moreover, my suggestion that the oscillator was capable of exerting a unilateral, in other words irreversible, effect on the energy of the surrounding field, drew a vigorous protest from Boltzmann, who, with his wider experience in this domain, demonstrated that according to the laws of classical dynamics, each of the processes I considered could also take place in the opposite

direction; and indeed in such a manner, that a spherical wave emitted by an oscillator could reverse its direction of motion, contract progressively until it reached the oscillator and be reabsorbed by the latter, so that the oscillator could then again emit the previously absorbed energy in the same direction from which the energy had been received. To be sure, I could exclude such odd phenomena as inwardly directed spherical waves, by the introduction of a specific stipulation—the hypothesis of a natural radiation, which plays the same part in the theory of radiation as the hypothesis of molecular disorder in the kinetic theory of gases, in that it guarantees the irreversibility of the radiation processes. But the calculations showed ever more clearly that an essential link was still missing, without which the attack on the core of the entire problem could not be undertaken successfully.

So I had no other alternative than to tackle the problem once again—this time from the opposite side, namely, from the side of thermodynamics, my own home territory where I felt myself to be on safer ground. In fact, my previous studies of the Second Law of Thermodynamics came to

spherical wave —

does not guarantee !

stand me in good stead now, for at the very outset
I hit upon the idea of correlating not the temper-
ature but the entropy of the oscillator with its
energy. It was an odd jest of fate that a circum-
stance which on former occasions I had found
unpleasant, namely, the lack of interest of my
colleagues in the direction taken by my investiga-
tions, now turned out to be an outright boon.
While a host of outstanding physicists worked on
the problem of spectral energy distribution, both
from the experimental and theoretical aspect,
every one of them directed his efforts solely
toward exhibiting the dependence of the intensity
of radiation on the temperature. On the other
hand, I suspected that the fundamental connec-
tion lies in the dependence of entropy upon
energy. As the significance of the concept of en-
tropy had not yet come to be fully appreciated,
nobody paid any attention to the method adopted
by me, and I could work out my calculations com-
pletely at my leisure, with absolute thoroughness,
without fear of interference or competition.

Since for the irreversibility of the exchange of
energy between an oscillator and the radiation
activating it, the second differential quotient of

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its entropy with respect to its energy is of charac-
teristic significance, I calculated the value of this
function on the assumption that Wien's Law of
the Spectral Energy Distribution is valid—a law
which was then in the focus of general interest; I
got the remarkable result that on this assumption
the reciprocal of that value, which I shall call
here R, is proportional to the energy. This rela- *actually* $\frac{1}{U}$
tionship is so surprisingly simple that for a while
I considered it to possess universal validity, and
I endeavored to prove it theoretically. However,
this view soon proved to be untenable in the face
of later measurements. For although in the case of
small energies and correspondingly short waves
Wien's Law continued to be confirmed in a satis-
factory manner, in the case of large values of the
energy and correspondingly long waves, appre-
ciable divergences were found, first by Lummer
and Pringsheim; and finally the measurements of
H. Rubens and F. Kurlbaum on infrared rays of
fluorspar and rock-salt revealed a behavior which,
though totally different, is again a simple one, in
so far as the function R is proportional not to the
energy but to the square of the energy for large   *now* $\frac{1}{U^2}$
values of the energy and wave-lengths.

Thus, direct experiments established two simple limits for the function R: For small energies, R is proportional to the energy; for larger energy values R is proportional to the square of the energy. Obviously, just as every principle of spectral energy distribution yields a certain value for R, so also every formula for R leads to a definite law of the distribution of energy. The problem was to find such a formula for R which would result in the law of the distribution of energy established by measurements. Therefore, the most obvious step for the general case was to make the value of R equal to the sum of a term proportional to the first power of the energy and another term proportional to the second power of the energy, so that the first term becomes decisive for small values of the energy and the second term for large values. In this way a new radiation formula was obtained, and I submitted it for examination to the Berlin Physical Society, at the meeting on October 19, 1900.

The very next morning, I received a visit from my colleague Rubens. He came to tell me that after the conclusion of the meeting he had that very night checked my formula against the results

of his measurements, and found a satisfactory con-
cordance at every point. Also Lummer and
Pringsheim, who first thought to have discovered
divergences, soon withdrew their objections; for,
as Pringsheim related it to me, the observed di-
vergences turned out to have been due to an
error in calculation. Later measurements, too,
confirmed my radiation formula again and again,
—the finer the methods of measurement used, the
more accurate the formula was found to be.

But even if the absolutely precise validity of
the radiation formula is taken for granted, so long
as it had merely the standing of a law disclosed
by a lucky intuition, it could not be expected to
possess more than a formal significance. For this
reason, on the very day when I formulated this
law, I began to devote myself to the task of in-
vesting it with a true physical meaning. This
quest automatically led me to study the interrela-
tion of entropy and probability—in other words,
to pursue the line of thought inaugurated by
Boltzmann. Since the entropy S is an additive
magnitude but the probability W is a multiplica-
tive one, I simply postulated that $S = k \cdot \log W$,
where k is a universal constant; and I investigated

whether the formula for W, which is obtained when S is replaced by its value corresponding to the above radiation law, could be interpreted as a measure of probability.

As a result,* I found that this was actually possible, and that in this connection k represents the so-called absolute gas constant, referred not to gram-molecules or mols, but to the real molecules. It is, understandably, often called *Boltzmann's constant*. However, this calls for the comment that Boltzmann never introduced this constant, nor, to the best of my knowledge, did he ever think of investigating its numerical value. For had he done so, he would have had to examine the matter of the number of the real atoms —a task, however, which he left to his colleague J. Loschmidt, while he, in his own calculations, always kept in sight the possibility that the kinetic theory of gases represents only a mechanical picture. He was therefore satisfied with stopping at the gram-atoms. The letter k has won acceptance

* This finding, containing the introduction of the ultimate energy quanta for the oscillator, was reported by Max Planck again before the Physical Society of Berlin on December 14, 1900. That was the birthday of the Quantum Theory. (*Max von Laue*)

only gradually. Even several years after its intro-
duction, it was still customary to calculate with
the Loschmidt number L.

Now as for the magnitude W, I found that in
order to interpret it as a probability, it was neces-
sary to introduce a universal constant, which I
called h. Since it had the dimension of action
(energy $\times$ time), I gave it the name, *elementary
quantum of action*. Thus the nature of entropy as
a measure of probability, in the sense indicated
by Boltzmann, was established in the domain of
radiation, too. This was made especially clear in
a proposition, the validity of which my closest
pupil, Max von Laue, convinced me in a number
of conversations—namely, that the entropy of two
coherent pencils of light is smaller than the sum
of the entropies of the individual pencils of rays,
quite consistently with the proposition that the
probability of the happening of two mutually in-
terdependent reactions is different from the
product of the individual reactions.

While the significance of the quantum of action
for the interrelation between entropy and proba-
bility was thus conclusively established, the part
played by this new constant in the uniformly reg-

ular occurrence of physical processes still re-
mained an open question. I therefore, tried
immediately to weld the elementary quantum of
action h somehow into the framework of the
classical theory. But in the face of all such at-
tempts, this constant showed itself to be obdurate.
So long as it could be regarded as infinitesimally
small, i.e. when dealing with higher energies and
longer periods of time, everything was in perfect
order. But in the general case difficulties would
arise at one point or another, difficulties which
became more noticeable as higher frequencies
were taken into consideration. The failure of
every attempt to bridge this obstacle soon made it
evident that the elementary quantum of action
plays a fundamental part in atomic physics, and
that its introduction opened up a new era in
natural science. For it heralded the advent of
something entirely unprecedented, and was des-
tined to remodel basically the physical outlook
and thinking of man which, ever since Leibniz
and Newton laid the groundwork for infinitesi-
mal calculus, were founded on the assumption
that all causal interactions are continuous.

My futile attempts to fit the elementary quan-

tum of action somehow into the classical theory continued for a number of years, and they cost me a great deal of effort. Many of my colleagues saw in this something bordering on a tragedy. But I feel differently about it. For the thorough enlightenment I thus received was all the more valuable. I now knew for a fact that the elementary quantum of action played a far more significant part in physics than I had originally been inclined to suspect, and this recognition made me see clearly the need for the introduction of totally new methods of analysis and reasoning in the treatment of atomic problems. The development of such methods—in which, however, I could no longer take an active part—was advanced mainly by the efforts of Niels Bohr and Erwin Schrödinger. Bohr, with his atom model and Correspondence Principle, laid the foundation for a reasonable unification of quantum theory with classical theory. Schrödinger, through his differential equation, created wave mechanics, and thereby the dualism between wave and particle.

I have just described how the quantum theory came gradually to occupy the focus of my interest in the field of physics. Eventually, it had to share

ignores
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this prominent position with another principle, which introduced me to a new sphere of ideas. In 1905, Albert Einstein published a paper in the *Annalen der Physik* which contained the basic ideas of the Theory of Relativity, and my acute interest in their development was immediately roused.

In order to preclude a likely misunderstanding, I have to insert here a few explanatory remarks of general character. In the opening paragraph of this autobiographical sketch, I emphasized that I had always looked upon the search for the absolute as the noblest and most worth while task of science. The reader might consider this contradictory to my avowed interest in the Theory of Relativity. But it would be fundamentally erroneous to look at it that way. For everything that is relative presupposes the existence of something that is absolute, and is meaningful only when juxtaposed to something absolute. The often-heard phrase, "Everything is relative," is both misleading and thoughtless. The Theory of Relativity, too, is based on something absolute, namely, the determination of the matrix of the space-time continuum; and it is an especially

stimulating undertaking to discover the absolute which alone makes meaningful something given as relative.

Our every starting-point must necessarily be something relative. All our measurements are relative. The material that goes into our instruments varies according to its geographic source; their construction depends on the skill of the designer and toolmaker; their manipulation is contingent on the special purposes pursued by the experimenter. Our task is to find in all these factors and data, the absolute, the universally valid, the invariant, that is hidden in them,

This applies to the Theory of Relativity, too. I was attracted by the problem of deducing from its propositions that which served as their absolute immutable foundation. The way in which this was accomplished, was comparatively simple. In the first place, the Theory of Relativity confers an absolute meaning on a magnitude which in classical theory has only a relative significance: the velocity of light. The velocity of light is to the Theory of Relativity as the elementary quantum of action is to the Quantum Theory: it is its absolute core. In this connection, it turns out that a

general principle of classical theory, the least-action principle, is also invariant with respect to the Theory of Relativity; accordingly, the quantum of action retains its significance in the Theory of Relativity as well.

This was what I tried to establish in all details, first for point masses, and then for black-body radiation. These researches yielded, among other results, the inertia of radiation and the invariance of entropy in systems possessing relative velocities.

But this is not all. The absolute showed itself to be even more deeply rooted in the order of natural laws than had been assumed for a long time. In 1906, W. Nernst came out with his heat theorem, often referred to as the "Third Law of Thermodynamics." As I immediately established, it amounted to the hypothesis that entropy, until then defined only up to an additive constant, possessed an absolute positive value. This value, from which all equations of equilibrium follow, can be calculated beforehand. In the case of a chemically homogeneous solid or liquid (in other words, a solid or liquid composed of homogeneous molecules) of which the absolute temperature

is zero, this value is likewise zero. This principle in itself expresses an important fact, namely that the specific heat of a solid or liquid vanishes at the absolute zero of temperature. For other temperatures, fruitful inferences follow, with respect to the melting points of a body and the transition temperature of allotropic changes. If now one passes from chemically homogeneous solids and liquids to bodies with heterogeneous molecules, or to solutions and gases, the absolute entropy is calculated by means of combinatory considerations, in which the elementary quantum of action, too, must be included. In this way one can obtain the chemical properties of any given body, and thus a complete answer is found to all problems dealing with physico-chemical equilibrium. However, in questions concerned with the temporal developments of processes other forces must be taken into account, and problems about such forces are not resolved by considering the value of the entropy.

Even though as a consequence of my advancing age I have been able to take an increasingly smaller direct part in scientific research, there was compensation for this in the considerable expan-

sion of my scientific correspondence, which I found enormously stimulating and invigorating. In this respect, I would like to mention, in particular, my correspondence with Cl. Schaefer, whose *Introduction to Theoretical Physics* I consider as pedagogically unexcelled. Our correspondence concerned his presentation of the Second Law of Thermodynamics. I also carried on an interesting correspondence with A. Sommerfeld, on the problem of the quantization of systems with several degrees of freedom. This particular correspondence even culminated in a final exchange of poetic tributes, which I shall take the liberty to quote here, although I must demur in all fairness that Sommerfeld seriously underestimated his own achievements in this field. This is how he referred to my studies on the structures of phase space:

> *You cultivate the virgin soil,*
> *Where picking flowers was my only toil.*

My only possible reply was:

> *You picked flowers—well, so have I.*
> *Let them be, then, combined;*

Let us exchange our flowers fair,
And in the brightest wreath them bind.

I have satisfied my inner need for bearing witness, as fully as possible, both to the results of my scientific labors and to my gradually crystallized attitude to general questions—such as the meaning of exact science, its relationship to religion, the connection between causality and free will—by always complying willingly with the ever increasing number of invitations to deliver lectures before Academies, Universities, learned societies, and before the general public, and these lectures have been the source of a many a valuable personal stimulation which I shall gratefully cherish in loving memory for the rest of my life.