The Genesis of the Second Scientific Revolution

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Matching Diverse Mathematical Projections of Nature

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INTRODUCTION

WHY ARE WE SO INTERESTED IN THE GENESIS OF THE SECOND SCIENTIFIC REVOLUTION?

In physics as a practical science, it has frequently happened and still does happen that two theories, developed independently of one another, come into conflict when extended and must be mutually modified to remain compatible. In this mutual adjustment lies the germ of their further development into complete ***unity,*** *since the chief purpose of each science is, and always will be, the unifying of all its great theories into one that will embrace all the problems of that science and afford a solution to all of them. From this point of view, it can be said that the science, which is nearest its goal, is the one which combines the greatest number of its theories.*

Max Planck

Fundamental crisis set in, the seriousness of which was suddenly recognized due to Max Planck's investigations into heat radiation (1900). The history of this event is all the more remarkable because, at least in its first phase, it was not in ***any*** *way influenced by any surprising discoveries of an* ***experimental*** *nature.*

Albert Einstein

As is well-known, at the turn of the XIXth and XXth centuries there happened a second ("Planckian" and "Einsteinian") scientific revolution, which consisted in a transition from the so-called "classical physics" (Newtonian mechanics + Maxwellian electrodynamics + Boltzmann's statistical mechanics + Thomson's classical thermodynamics) to the "non-classical" one (relativity theory + quantum theory). What are its main *reasons*?

It would seem, what is there to talk about? Open, for example, a standard university textbook and you will immediately find the following authoritative standpoint, stated as a matter of course. In the second half of the XIXth century, it became possible to construct such experimental equipment, such precision measuring devices, which finally made it possible to experimentally test certain important consequences of the classical theories of mechanics, electrodynamics and thermodynamics. The manufactured equipment (the Michelson interferometer, for instance) made it possible to carry out such '*critical experiments'* that almost simultaneously refuted all the basic structural elements of classical physics - both classical mechanics and the classical theory of electromagnetic radiation.

As a result, the researchers were simply forced, under the pressure of an "avalanche of indisputable experimental data", to break with the wanted ideas on space, time, motion and causality and replace them with other, more "crazy" (in the sense of Niels Bohr) artifices that blatantly contradicted both the ordinary experience and common sense.

In more advanced courses, the steady standpoint is usually buttressed by a stout reference to the textbook speech of one of the leading representatives of classical physics of the second half of the XIXth century – Lord Kelvin (William Thomson). The illustrious speech was delivered in 1900 at the annual meeting of the British Association for the Advancement of Science, of which Kelvin was the chairman (Thomson 1901). In it, he 'prophetically pointed' to 'two clouds in the hitherto cloudless firmament of classical physics', with which the authors of textbooks easily identify the experiments of Michelson - Morley (refutation of the classical theory of light ether) and Lummer-Pringsheim and Rubens - Kurlbaum (refutation of the classical theory of black radiation body).

The described story, moving from one textbook to another and acquiring more and more novel details, have confidently acquired the inviolability of a scientific myth - one of the cornerstones of modern scientific ideology. This myth, as well as many like it, undoubtedly contributes to the formation of cognitive values in future researchers, engineers, and school teachers, strengthening the "spirit of objective scientific research" and setting bright examples of successful professional activity. Yet it seems very doubtful in the light of data of a different kind, abundantly provided by modern history and the methodology of science.

(I) Let's start with a textbook case, with the "radium revolutionary" (the catchphrase of the President of the French Academy of Sciences, Henri Poincaré). On Thursday, February 27, 1896, in Paris, physicist (and chemist) Dr. Henri Becquerel, carefully studying the ability of uranium salts to cause the phenomenon of phosphorescence, made an epoch-making discovery that changed the course of scientific development. Having mistakenly locked a bag of uranium-potassium salt together with a pack of photographic plates overnight in his laboratory in the Botanical Garden (in the house of the famous Georges Cuvier), the next morning he (again, completely by accident!) developed these photographic plates for some reason. And he was amazed to discover on one of them traces of unknown radiation emanating from the depths of the uranium atom.

Thus, the trigger was launched for a whole series of outstanding discoveries: the understanding of the complex structure of the atom, the splitting of mysterious rays emerging from the atomic depths into three types - alpha, beta and gamma radiation, Rutherford's startling experiments in the study of atomic nuclei, etc. etc. - right up to the experiments of Hahn and Strassmann on the fission of atomic nuclei (1938) and the launch of an atomic reactor at the Chicago stadium by Enrico Fermi's team in 1943.

However, wary and distrustful French historians of science, turning to the archives, discovered two circumstances that made it possible to cast doubt on the charming ‘Christmas tale for young amateurs of science’ outlined above.

Firstly, it turned out that Becquerel was by no means the first who came up with the idea of studying uranium for radioactivity. Thirty years before, at a meeting of the Paris Academy of Sciences, a message was heard from a certain Lieutenant Niège de Saint-Victor, who reported on the effect of uranium salt on photographic plates. But both the brave lieutenant himself and his enlightened listeners attributed the observed phenomenon only to the chemical action of uranium salt.

Secondly, the discovery of Becquerel was not a heap of accidents that allowed someone to caustically remark that "it was not Becquerel who discovered radioactivity, but radioactivity that discovered Becquerel." Just a few weeks before his discovery, Henri Becquerel met with an academician (and later president of the French Academy of Sciences), the great mathematician (and theoretical physicist) Henri Poincaré. It was he who set Becquerel the task of examining uranium salts for the possible detection of radioactivity in them (Tyapkin and Shibanov 1979).

(II) Let's take the second, more famous example. In 1881, American physicists Albert Michelson and Henry Morley, in the basement of the Kaiser Wilhelm Institute in Berlin, staged what is now recognized as a classic (and for its time, the most expensive) experiment to measure the speed of the ethereal wind. (Fortunately, A. Michelson's father was a multi-billionaire - the owner of the Chicago slaughterhouses, famous for the history of class struggle; he presented his son a check for his birthday for 1 million dollars, sacrificed to the progress of science).

Let us recall that "aether" in the classical theory of electromagnetic radiation was a hypothetical medium whose disturbances were electromagnetic waves. (Just like changes in the cohesive forces of water molecules constitute the 'water waves').

The Earth, as part of the Universe, was submerged for XIXth century physicists. into the 'ocean of aether'. So that the observer on the surface of the Earth rapidly rotating around its axis and the Sun, should have felt the ethereal wind. The interferometer, placed by Michelson and Morley on the surface of a marble slab floating in a lake of mercury, was supposed to record the difference in the path of coherent rays emitted along the direction of the Earth's movement and perpendicular to it.

But no wind was detected. The stubborn Americans spent a whole year in the basement, breathing harmful mercury vapours and trying to figure out what was wrong. Eventually they published an article in one of the American scientific journals reporting an odd negative result (Michelson 1881). The article could not help but cause controversy about the reliability of the experimental data obtained, the tone of which was set by one of the most authoritative theorists of that time - Hendrik Antoon Lorenz himself. And the experiment, after harsh criticism, had to be repeated in 1887 - Nothing again (Michelson & Morley 1887).

This circumstance - much later, during the heyday of analytical philosophy - was interpreted by its luminaries such as Moritz Schlick, Hans Reichenbach and Karl Popper, as a paradigmatic example of Bacon's "decisive experiment" (experimentum crucis), which refuted classical physics. Only in 1905, the "crisis in physics" caused by this experiment was marvellously resolved by 26-year-old Albert Einstein. He put forward a special theory of relativity (STR), which directly generalized the results of the Michelson-Morley experiment in one of its basic postulates.

This time, other distrustful historians of science - Americans Martin Klein, Gerald Holton et al. – again strived to double-check the explanation. And again it turned out that this story is also a myth, although this time somewhat more plausible (Klein 1963, 1970, 1980; Holton 1969, 1980).

Firstly, neither Albert Michelson himself, nor his colleague Henry Morley, either in the article of 1881, in the 1887 paper, or anywhere else, ever interpreted their results as a refutation of the entire classical theory of radiation and the underlying ether concept. They humbly indicated only that the results of their experiment allowed, at best, to choose between different versions of the classical theory of radiation. The dispute was only on the choice between the competing hypotheses of Stokes and Fresnel about the complete and partial entrainment of the ether by moving bodies.

As a result, in 1909 Albert Michelson rightfully received the Nobel Prize not for ‘refuting the theory of the ether’, but for “the development and improvement of precision methods of optical measurements” (Rayleigh’s 1908 term) which included, but was not limited to, the construction of interferometers. The latter are still famously involved in modern science: it was they who finally made it possible to detect gravitational waves in 2017.

Secondly, just a few years after the experiments of A. Michelson and G. Morley, continental physicists - the Dutchman H. Lorenz and the Irishman L. Fitzgerald - independently put forward a reasonable hypothesis, according to which all weighty bodies (and, in particular, the arms of the interferometer) when moving through the ether contracted in the direction of movement - but in such a way that the Michelson-Morley experiment would lead to zero results (Lorentz 1892a).

Indeed, much later, in the classic works of Karl Popper, this hypothesis was aptly called the *ad hoc* hypothesis, a “trick of refutation”, introduced only to adjust the theory to the experiment that contradicted it (Popper 1935). But that comes later. And then neither the leader of post-Maxwellian theoretical physics, Professor of Leiden University Hendrik Antoon Lorentz, nor the President of the French Academy of Sciences Henri Poincaré thought so (Poincarè 1906).

Indeed, the Lorentz-Fitzgerald contraction hypothesis quite naturally followed from the previously introduced Molecular Forces hypothesis. According to this hypothesis, the electromagnetic forces of attraction between the molecules of any solid body (including between the molecules of the arms of the Michelson interferometer) depend on its speed in full accordance with the so-called “Lorentz transformations” (Zahar 1973; 1989). By the way, the entire kinematics of Einstein’s special theory of relativity is based on them.

Thirdly, not in the 1905 STR article itself, nor in the later reviews written by Einstein during the most productive period of his scientific activity, there is not a single direct reference to the Michelson–Morley experiment. Why?

It would seem that this is the most convincing argument in favour of abandoning the theory of the ether. Use it!

Moreover, in conversations with the American physicist and historian of physics Robert Shankland, which took place in the last years of his life, Einstein gave a clearcut answer to a directly posed question: while writing an article on the special theory of relativity, he knew *nothing* about the Michelson-Morley experiment. “I learned about it later, from the reviews of Hendrik Lorenz” (Shankland 1963; 1964).

Fourthly, for some reason, Lord Kelvin’s textbook report mentioned above also says nothing about the Michelson-Morley experiment. Accordingly, by ‘clouds’ in the immaculately bright firmament of classical physics, it is understood not the results of experiments, but the shortcomings and *paradoxical* nature of theories that interpret the interaction of moving bodies with the ether. It’s not for nothing that Einstein’s famous article of 1905 was unpretentiously called “*On the electrodynamics of moving bodies*”.

(III) Let us turn to the third textbook example - the experiments of Rubens and Kurlbaum designed to refute the classical theory of blackbody radiation. According to the received view, these experiments, carried out in the late XIXth century by leading German experimentalists, both grossly contradicted classical radiation theory and led directly to Planck's formula (Norton 1993).

However, appeal to the real history of science calls this story into question. The classical theory of blackbody radiation was proffered by R. Rayleigh and J. Jeans only in 1904, a few years *after* Planck's report, in which he proposed his formula for the spectrum of blackbody radiation. During the production of these experiments, there was nothing to refute! They were not installed for this sublime purpose, but to obtain at least some reliable information about the radiation spectrum of a completely black body (Kuhn 1978).

When Planck derived his formula, he did not intend to undermine the edifice of classical physics, actively using in the derivation of his law the results and methods of all the main *classical* theories of his time - Newtonian mechanics, Maxwellian electrodynamics, Boltzmann statistical mechanics and classical thermodynamics (Dugas 1959). Correspondingly, no terrible “crisis” in the theory of radiation was recorded in the documents of that time - articles and reports.

It appeared later - for example, in textbooks on quantum mechanics, the authors of which strived to quickly convert students to the new, “Copenhagen” faith.

Another thing is, as the founder of statistical mechanics Ludwig Boltzmann himself warned Max Planck in personal correspondence, that the necessary condition for the applicability of statistical methods to radiation was the judicious assumption that the energy of blackbody radiation consists of small portions (quanta) of energy (Dugas 1959). But Planck hoped that this “strong” assumption would somehow be able to be justified classically in future. In any case, he tried not to draw attention to it, taking it only as an intermediate stage, as a tool for calculations, analogous to complex numbers in mathematics.

However, the first person who generally referred to this article and “made hype” was Albert Einstein, in his own great 1905 article – on the photoelectric effect – who put forward the revolutionary idea of the reality of the light quantum (Einstein 1905a).

In his 1900 report Lord Kelvin did not talk about the experiments of Rubens and Kurlbaum, but expressed quite fair doubts about the applicability of the theorem on the uniform distribution of energy over the degrees of freedom to the Maxwell-Lorentz ether. The electromagnetic field (ether) has an infinite number of degrees of freedom, and doesn’t this mean the infinity of field energy in the so-called “ultraviolet catastrophe”?

Already three considered textbook examples (and there are many more) impugn the concept of a ‘critical experiment’, which has a long tradition in educational literature, once and for all refutes the classical mature theory and thereby significantly contributes to the advancement of science, - this is too much of a simplification.

The history of science convincingly exhibits that even the same observations and experiments (not to mention theoretical interpretations and explanations) are evaluated differently in different historical eras, depending on the context, ‘mentality’, ‘spirit of the times’ (the Germans call it “Zeitgeist”).

Responding to the quite reasonable resentments of the physicist Alan Sokal, the philosopher Bruno Latour emphasized that the latter refuses to admit that

> "[hard] facts remain hard only when they are supported by the culture of everyday life, by social institutions that can be trusted, by more or less public life, more or less reliable media "(Latour 1990, 18).

Therefore, with the advent of alternative “hard facts”, whether or not a statement can be trusted depends much less on its reliability than on the conditions of its construction. Namely, from who makes it, to whom it is addressed, and to what social institutions its emergence is due (Latour 1990, 19).

Not a single researcher seeking to reason sensibly can, I think, claim to describe the entire spectrum of this context of scientific activity, to reveal *all* the cogs and gears of the most complex mechanism of its influence on scientific knowledge. As Friedrich Schelling admitted at the end of his life, “human life in general revolves mainly around two poles: state and religion.” But how much controversy is caused by taking into account the influence of religious factors alone on the development of scientific knowledge, starting from the “*Protestant Ethic and the Spirit of Capitalism*” (and the so-called ‘Merton’s thesis’) and ending with Galileo’s ‘and yet it revolves’.

Nevertheless, the works of authoritative historians and sociologists of science convincingly indicate that in their daily activities, natural scientists try to distance themselves as much as possible from the influence of ‘external’ - socio-political, socio-economic and even socio-cultural factors. As Thomas Kuhn astutely noted, the more advanced a science is, the less influence external factors have on its internal dynamics and structure (Kuhn 1977).

Even if the influence of external factors really takes place, the scholars stubbornly try not to notice it, and if they notice it, they do not recognize it, and if they recognize it, it is only through clenched teeth, when they are ‘pinned to the wall’. And even if scientists consciously follow in their professional activities certain religious, class, national, racial and, finally, ethnic ‘prejudices' (‘Galileo’s case’, ‘Lysenko’s case’, etc.), they try to posit their laboratory reports, articles and monographs in such a discreet way as to distance themselves as much as possible from these preferences. It is no coincidence that such an ethical maxim as ‘disinterestedness’ is squeezed, along with “communism”, “organized scepticism”, into the “ethos of science”, scrutinized by Robert Merton's classical sociology of science.

The myth of the "ivory tower" in which every true scientist, a true professional in his field, lives internally, I would like to believe, is useful for the successful functioning of science as a social institution, and for the formation of the ethical professional standards of future researchers.

But this is still a myth. And to one degree or another, the epistemological models of Karl Popper, Imre Lakatos and even (in some respects) Thomas Kuhn still ignore, albeit to varying degrees, the hard fact that modern science is a substantially '*community effect*" (Russ Payne). Unfortunately, "epistemologists" continue to adhere to the point of view according to which the progress of science consists primarily in the activities of "great people".

According to this "romantic" standpoint, it is the amazing insights of a narrow group of very, very peculiar individuals, a kind of "chosen ones of the Gods", that mainly pushes science forward. But this, of course, is a very distorted and one-sided picture. Great luminaries such as Newton, Maxwell and Einstein can only begin a revolution in scientific thinking - at best - but only when the 'silent majority' of researchers, the entire scientific community, has diligently prepared the ground for this. It loosened the soil so that the seeds of a new, revolutionary paradigm could grow on it. Accordingly, the history of science must be understood in new terms - in terms of how the entire scientific community 'progresses' from one era to another, from a situation of 'stagnation' to a situation where revolutionary thinking is in demand. The celebrated great 'insights' and 'breakthroughs' never occur in a social vacuum.

A revolution in science will never happen until the entire scientific community is convinced that the old paradigm is 'no good.' Only when this happens will alternatives begin to be developed or become in demand.

Therefore, the meaning and correlation of "*internal*" and "*external*" factors for the "internal observer" - a scientist, on the one hand, and for the "external observer" - a philosopher, sociologist and historian of science, - on the other, are not the same.

Let's look at an example. If you want to understand the behavior of an athlete during a football match, then, of course, taking into account his mood, temperament, the degree of wear and tear of equipment - T-shirts and sneakers, the amount of remuneration ("socio-economic factors"), the attitude of the public (roar in the stands – "socio-cultural background"), etc. But in this analysis, *the rules of the game* - those principles that a player consciously follows during a match - occupy a special place. We are forced to admit that we will understand nothing in the behavior of a football player if we do not take these rules into account. Further, any analysis of a player's behavior must be based on an analysis of his success in following these rules.

The rules of the game constitute the “internal context” (the so-called “field of rationality”), forming the internal conditions within which the entire spectrum of conscious research activity unfolds.

Similarly, the behavior of a scientist constitutes a social action, oriented in its course towards other “players”. A professional researcher differs from an amateur in that he “goes out” to study Nature not on his own, not as an “epistemological Robinson”, but as armed with ideas, values, methods and techniques. They are collectively developed and accepted by a team of professionals, of which he is a part, which, with the light hand of Thomas Kuhn, is usually called a “*paradigm*”.

Therefore, Max Weber’s comprehension of social action as behavior to which the “actor” *himself* (the one who acts) attaches a special meaning is also applicable to the behavior of a scientist. The important thing is that, as Max Weber himself never tired of emphasizing, the meaning of an act should be understood as the meaning that is given to it by the actor *himself,* and not, say, the metaphysical meaning that exists in the head of God or, at worst, an epistemologist and philosopher of science.

According to Karl Marx, to comprehend a social phenomenon means to reduce it (“ultimately”) to certain socio-economic conditions, and according to Emile Durkheim, - to derive it from the laws of evolution and the functioning of social consciousness.

However, according to Max Weber, to understand human behavior towards other people, to understand a social action means to understand the *meaning* that is given to it by the actor *himself,* by the person who performs (or conceived) this or that act. Therefore, by Weber's startling *methodology of social cognition*, the scientist is neither a squalid toy in the hands of powerful socio-economic forces, nor a pathetic blind man led by the hand of the hidden laws of social consciousness. All forms of scientific sociality - elements of the social structure of the scientific community, values, norms, and patterns of behavior - have meaning for an individual researcher only as *regulators* of his own behavior. They exist only in activity as its separate aspects.

Respectively, following Weber, all social actions in science can be divided into four groups according to the degree of rationality: (1) goal-rational, (2) value-rational, (3) traditional and (4) affective. The most rational actions are goal-oriented ones, and the least are affective actions, performed in a state of 'passion'.

Purposeful action is when the actor is perfectly aware of the goal he is striving for and consciously chooses the most effective means of achieving it. Between them are value-rational actions, performed by certain values, and traditional actions, performed unconsciously, by certain traditions.

The fact that goal-oriented action is the highest, most rational type of social action does not mean that it is the limit to which all other types strive in their course. On the contrary, this type forms a kind of scale, a conceptual framework, providing a (neutral) language through which other social actions are described. Any specific social action, even though it, as moments, embraces all the abovementioned types at the same time, can, nevertheless, be described in terms of deviation from goal-rational action.

Accordingly, the analysis of the action of internal factors is of particular importance both in history and even in the sociology of science. It constitutes the framework, the "skeleton" on which the "meat" of other disciplines is "built up". Moreover, internal history forms the template with which all the facts of the real history of science are compared. But this does not mean the automatic validity of the famous thesis of Imre Lakatos that "internal history is primary, and external history is secondary". And that when comparing two different rational reconstructions of the same period in the development of science, one should prefer the one that explains more facts 'internally' (Lakatos 1970). History thus reconstructed does indeed tend to become "philosophy conjuring examples", as Thomas Kuhn (1971) aptly remarked.

As is well known (see Kuhn 1977, for instance), all internal factors in the development of science are divided into four groups: (1) agreement with experiment; (2) simplicity; (3) self-consistency; (4) consistency with other theories that have been developed or are being developed at the time. Moreover, all these, as noted above, factors are *regulatory principles* that in a certain way guide the activities of professional researchers.

This means that in every frequently encountered problematic situation characterized by the presence of several rival theories, the professional should give preference to the theory that is better consistent with the experiment, simpler, self-consistent, and well-consistent with other theories. Moreover, even considered within a single system, the factors under consideration do not form the so-called "*selection algorithm*". After all, in science situations arise too often when one theory from the totality of those being compared is more accurate, but another one is in better agreement with the mature theories accepted at a given time. Let us recall the first scientific revolution with its Ptolemy's and Copernicus's rival cosmologies.

To understand the relationship between "internal" factors, let us turn to the views on the structure of scientific theory, developed within the framework of Russian philosophy of science. They are most fully presented in the works of Vyacheslav S. Stepin (see his 2005 thought-provoking monograph and references cited therein). According to Stepin, any mature scientific theory (for example, classical mechanics classical electrodynamics, or quantum mechanics, etc.) is a system of statements that describes connections and relationships between certain abstract objects, such as "material point", "electric field vector", "wave function", "metric tensor", etc. Abstract objects of a mature scientific theory are organized into a complex hierarchical system, which contains three basic levels:

(1) the level of the *fundamental theoretical scheme* (FTS);

(2) the level of partial theoretical schemes (PTS);

(3) the level of empirical schemes (ES).

For example, the FTS of classical mechanics is an ideal model in which mechanical motion is represented by the movement of a material point under the influence of a force in an inertial reference frame.

All these levels are closely interconnected, forming a kind of epistemological pyramid. The fundamental theoretical scheme (FTS) generalizes particular theoretical schemes (the PTSs), while the particular theoretical schemes are peculiar generalizations of empirical schemes. The other side of the relationship between all these levels is that, in the process of deployment, particular theoretical schemes are constructed from the basic one (FTS), and empirical schemes are properly constructed from particular ones. This allows theoretical predictions to be compared with experimental data.

However, the following hallmark of mature theory structure is important: there is *no* strict connection between the objects of three levels (see Suppe 1974 for details). Firstly, there is no strict and unambiguous algorithm for constructing PTS objects from the FTS ones. Every time, the construction of PTS from FTS is a peculiar creative task that does not have a clear-cut solution. For example, to construct theories of small vibrations from the FTS of Newtonian mechanics, one needs to specify the type of force, then introduce the elasticity coefficient phenomenologically, etc.

Therefore, this construction is carried out by analogy, by the patterns of problem-solving that are included in the paradigm (for more details, see Kuhn [1962], 1970). The same is true for the transition from PTS to ES.

But precisely because the strict connections between all the layers of a mature theory are lacking, the discrepancy between the conclusions from the empirical scheme and experience refutes only the ES, but not the entire mature theory. One can always modify the fundamental laws of a mature theory in such a way as to achieve its agreement with experience. As rightly noted at the beginning of the XXth century the eminent French physicist and philosopher of science Pierre Duhem,

> "the postulates that serve as the starting point for the theory, the intermediate links leading from postulates to conclusions, cannot be subject to this verification from the facts" (Duhem [1906], 1954).

One refutation, second, third... How many refutations are needed to discard the developed ES once and for all, and, if necessary, the PTS, to preserve the FTS and the entire scientific theory? – Obviously, an *infinite* number. If the theory can always adapt to a finite number of anomalies - one by one, first to the 1st, then to the 2nd, to the n-th, it will, nevertheless, not be able to adapt to an infinite number of anomalies.

But what does it mean that a theory contradicts an *infinite* number of experimental data? This means that the theory contradicts another theory (Nugayev 1999). Only a theoretical scheme that generalizes several empirical schemes contains information not only about the experiments performed but also about future experiments that have not yet been performed. Theory, as is known, is needed not only to organize already-known experimental data but also to predict new ones.

The above means that discrepancies with data from individual experiments can only cause changes in empirical patterns. But changes in particular theoretical schemes, and even more so in the FTS, can be caused primarily by clashes between theories (Nugayev 1999).

It is clear that the proposed point of view decisively rejects not empiricism and falsificationism as such, but only primitive versions of the latter, associated with the notorious Baconian concept of 'critical experiment'. This concept undoubtedly played a significant role in the mortal combat against primitive qualitative Aristotelian physics (Bacon) and in the establishment of non-classical relativistic physics (Popper). But it has turned into an archaism in the XXIst century when we are dealing with such cumbersome, multi-level, sophisticated interdisciplinary complexes as molecular biology, genetics, synergetics, solid state physics, relativistic astrophysics or quantum cosmology.

Physics, of course, was, is and will remain an experimental science. But the thorny path from experience to the foundations of the theory, as had been pointed out at the end of the XIXth century, both by Pierre Duhem and Max Planck, as physics evolves, becomes longer and longer. The connection between the levels of physical knowledge and its sections becomes more and more flexible, and the theoretical ideas themselves become further and further removed from the clarity of everyday experience. Contradicting the well-developed, well-established, well-tested scientific theory T_2, the testable theory T_1 contradicts not just one experiment, but many experimental data at once, accumulated and generalized in the theory T_2. Therefore, such an Encounter with Experience is extremely difficult to avoid. Yet it is precisely this hallmark that leads to particularly catastrophic consequences for the theory being tested (Nugayev 1999).

Accordingly, being considered from the "internal", intertheoretical side, the second (Planckian and Einsteinian) scientific revolution appears primarily as the emergence and partial resolution of the contradictions of the encounter between the most developed theories of classical physics - Newtonian mechanics, Maxwellian electrodynamics, Boltzmannian (and Gibbs's) statistical mechanics and Thomson's classical thermodynamics (Nugayev 1985).

Consideration of the interactions of these theories with each other and with experimental data constitutes the so-called "*intertheoretical context*" of Einstein's revolution.

It is the consistent study of this context that should make it possible to more accurately and consistently explain several important circumstances that were previously considered independently, in isolation from each other. First of all, it allows one to explain the well-known historical and scientific fact that two such radical breakthroughs as the Quantum and Relativistic revolutions had started almost *simultaneously* (in 1900 and 1905), with an interval of 5 years, and proceeded in parallel for a certain time.

Secondly, in both cases, the *same* people took the most active part in these changes - Albert Einstein, Max Planck, Arnold Sommerfeld, Wolfgang Pauli et al., which allows one to describe these events as two interconnected sides of the same coin - the second scientific revolution.

Thirdly, an "internal" explanation is also required for the following important circumstance: an unknown young man - a modest clerk at the Bern patent office, Albert Einstein - in the same year of 1905 laid the foundations of three significantly different from each other, and in several aspects opposed research traditions of modern physics - quantum physics, the theory of relativity and statistical thermodynamics. It is unlikely that young Einstein would simultaneously and with great enthusiasm, on his initiative, sedulously plunge into three complex and quite distant fundamental theories, guided simply by a passion for sports or boundless curiosity. A person with experience in creative work will confirm that this work, say, solving a complex puzzle, captures a person *completely* and does not let go until the person finally solves this problem.

The best way to distract yourself is to switch to another kind of activity, say, playing chess or violin (as the creator of the theory of relativity did). But the transition from the solution of one problem to the (inconclusive) solution of another can be rationally comprehended precisely when the solutions to these problems work to implement the same "internally planned" programme when there is a certain *consistency* between research in such different areas. In other words, there must be an internal *connection* between Einstein's three different "research projects."

Fourth, many of the proposed explanations for the reasons for the adoption of the special theory of relativity (STR) are insufficient due to the limitations of the falsificationist approach. It links the reasons for both the rejection of classical mechanics and the adoption of STR with the notorious Michelson-Morley "critical experiment". The creator of the SRP (scientific research programmes) methodology Imre Lakatos and his student Eli Zahar went the furthest in explaining the real reasons for the adoption of STR. They convincingly exhibited that any scientific revolution consists of a change not in theories, but in research programmes, i.e. two completely different "projects" for the further development of science, expressed in significantly different sequences and chains of theories. Each of these chains is constructed according to its own rules (i.e., by the "SRP heuristics") based on its ideas about the rational structure of nature ('hard cores' of the SRP).

These programmes begin to compete with each other striving to demonstrate which of them will not only explain already known experimental data (Lakatos 1978; Zahar 1989; Lakatos & Zahar 1974) sooner, more accurately, but also predict new ones.

However, unfortunately, the Lakatos-Zakhar explanation is also one-sided, although to a much lesser extent than the previous one. And it allows, of course, further development - clarification and generalization. In particular, it correctly describes the behavior of an influential part of the scientific community - specialists in mathematical physics - who commenced to believe in SRT only *after* the general theory of relativity (GTR) was invented and significant facts were recorded in favor of its adequacy. The data on the deflection of light rays in the gravitational field of the Sun, reported in 1919 by British astronomers under the leadership of Sir Arthur Eddington, had a particularly great impact on the world and scientific community. By the way, they largely contributed to Einstein receiving the Nobel Prize in Physics in 1921.

But for many years, until the 70-80s of the XX century, for many physicists, general relativity continued to remain a section of Riemannian geometry, i.e. a very exotic part of theoretical physics.

The extremely narrow GTR empirical basis for more than half a century consisted of three notorious (due to their small number) the so-called. "critical" effects - the shift of the perihelion of Mercury (known 50 years before the creation of GTR), the deflection of light rays in the gravitational field of the Sun (following from any relativistic theory of gravity that recognizes the validity of the identity of mass and energy) and the red shift in the gravitational field (measured only in 60s of the twentieth century using the so-called "Mossbauer effect"). It should be added that for decades, GTR remained on the sidelines of the mainstream development of physics. It was replete with paradoxes and almost philosophical disputes about the physical, operational interpretation of the obtained solutions and basic concepts (for example, the energy of a gravitational wave).

On the contrary, it can be taken generally known that within the first ten years after its creation, STR became generally accepted. It entered, in the words of a contemporary of Planck and Einstein, the German physicist Arnold Sommerfeld, “into the arsenal of every physicist”. Why?

In the historical and scientific literature, numerous explanations have been proposed for the various reasons for the adoption of STR by the scientific community. They range from the concept of a critical experiment to very exotic explanations (associated, for example, with a radical (feminist) revaluation of the contribution of Einstein's classmate and first wife, Mileva Maric, to the creation of SRT (Einstein/Maric 1992)). Many of them, proposed by serious professional researchers, do reflect the real circumstances that played important roles in the revolution in question. The factors involved did motivate certain physicists or even certain research groups, “invisible colleges”.

However, even in the most favorable case, with the complete independence of the theories encountered, we will never be able to assert with complete confidence that this or that philosophical concept, logical-methodological scheme, or epistemological “model” reveals certain hidden ‘laws’ of the movement of the history of science, or at least bares some “essences” of these processes.

The trends in the development of science set by this scheme are in no way binding. These, at best, are only certain guidelines for the cognitive activity of individuals, which they may or may not follow. In this case, turning to the etymology of the word “law” is especially appropriate, since initially the word “law” did not mean repeating features of natural phenomena, but certain *norms* of human activity. It is in this sense that the word is used now, when speaking, for example, about “the constitution as the fundamental law”. It is precisely this meaning that is closer to the understanding of the laws of scientific activity that is used in this monograph. Even though every citizen of a state can obey the law or ignore it, the laws nevertheless exist “objectively”, regardless of whether anyone likes them or not, whether anyone is going to fulfil them, or whether they continue to serve as a cover for endless corrupt deals.

Let us finally summarize. What are the true reasons for the second, Einsteinian scientific revolution? – There are plenty of them, but it is possible, by tradition, to distinguish “external” reasons and “internal” ones, the complex interaction of which led to the replacement of classical scientific theories with non-classical theories.

External – are briefly apprehended by Friedrich Nietzsche's aphorism: "God is dead" ("Gott ist tot"). This is an accelerated modernization of social life, which consisted of the formation of an industrial society, a radical change in the economic structure, political and cultural institutions, values and value orientations of modern society. This is the "secularization" of public life, the separation of church and state, and the growth of egoism, cynicism and materialism, mercantilism, pragmatism and positivism. External reasons must have an unequal impact on the development of scientific research since their duration and intensity of manifestation in society itself are uneven and heterogeneous. Let us note that the main mediating factor through which the influence of socio-political and socio-economic factors occurs is the so-called "socio-cultural background" containing values, value orientations, patterns of behavior, and, most importantly, cognitive values, ideas about truth, etc.

In this regard, the importance of the influence of both the religious ideas themselves and their eminent split (Catholicism and Protestantism), as well as the process of their decomposition ('secularization') can hardly be overestimated.

But social factors also include such factors as the transformation of science into a mass profession, a kind of "industrialization" of scientific research, the transformation of the research profession into a mass profession, and scientific knowledge itself into a product of mass spiritual production. Knowledge fatally turns into a kind of "commodity" that allows almost conveyor processing by different specialists, a product that can be "exchanged" or "sold". This blurs the strict criteria of truth inherent in the romantic stages of the development of science, leading to the proliferation of not only pragmatist and positivist but also (neo) Platonist concepts.

Internal reasons are, of course, the emergence of new experimental data, which were especially difficult to fit into the Procrustean bed of old research traditions because they were closely connected with their internal faults. Significant discord in the structure of scientific knowledge should be added, the intense struggle between 'mechanism' and 'electrodynamic', mechanistic and field concepts, further unbridled mathematization of scientific knowledge, exacerbating the "contradictions of the meeting" between the leading scientific traditions, etc.

The *interaction* of these two groups of factors, at least since the New Time, is determined by the fact that external factors do not cancel internal ones, but change, first of all, the "weights", the meanings of these factors in situations of changing basic paradigms. Increasing from the XV-XVII centuries (Industrial Revolution) mathematization of scientific knowledge, due to the dominance of the quantitative approach to the description of natural phenomena, leads to the fact that, in both the first and second scientific revolutions, the intertheoretical context turns out to be leading, determining the importance of such a significant factor as the consistency of those encountered with each other “old” paradigms.

> “Modern science’s way of representing pursues and entraps nature as a calculable coherence of forces” (Heidegger [1954], 1977, p.21).

In the most general case, a scientific revolution is a complex interweaving of various internal and external factors, such as an interweaving whose ideological and methodological contexts cannot be defined uniformly, once and for all. But it is already clear that no simple, one-sided, ‘extreme’ explanations - from Popper’s “critical experiment” to vulgar Marxist “economic determinism” - will no longer work today.

The relationship between internal, cognitive, and external - socio-economic, socio-political and sociocultural - factors does not mean that they all line up within a certain era and begin to compete with each other in which one they will have a greater impact on the scientific community and its representatives. Representatives of the modern sociology of knowledge rightly emphasize that, in their influence, social factors are inevitably mediated, transformed, and turned into cognitive ones.

As the eminent contemporary sociologist of science Bruno Latour notes, existing epistemological approaches are undoubtedly interesting, but they still try to answer the question that should be avoided: “How our cognitive abilities relate to our societies”.

The key point is that this complex and confusing question (and the various answers to it) is grounded on the idea that the composition of our lives is somehow different from the composition of our sciences, our views and our information. Wherein

> “We are dealing with the same ethnographic puzzle: some societies - extremely rare ones in fact - are created through capitalization over a larger area. The fixation on rapid movement and stable invariance, for powerful and secure connections, is not simply part of our culture, or something ‘conditioned’ by social interests: it *is* our culture. Too often sociologists look for indirect, mediated relationships between 'interests' and 'technical' details. The reason for their myopia is simple: they limit the meaning of 'social' to

society, not realizing that mobilizing allies and, in general, transforming weak associations into strong ones is what the term 'social' also means. Why look for far-fetched relationships when the technical details of science directly speak of invariance, association, divergence, impenetrability, and the like?" (Latour 1990, 64).

CHAPTER 1

SCIENTIFIC REVOLUTIONS AS SUBSTANTIALLY INEXTRICABLE PROCESSES: PIECEMEAL MATCHING COGNITIVE AND SOCIAL ASPECTS OF SCIENCE

Abstract. What are the thorough reasons of a scientific revolution? The ponderable answers (grounded on the illustrious epistemological models) are reduced to the following ones.

(I) *Inductivism* (Whewell); (II) *falsificationism* (Popper); (III) *conventionalism* (Duhem); (IV) *sophisticated falsificationism* (Lakatos);(V) standing aside *social-psychological* version of Kuhn).

Some substantial drawbacks of the (I- IV) options are determined by one and the same reason – lack of scrutiny of subtle relations between cognitive and social aspects of science. The epistemological models ignore (to varying degrees) that science is substantially "*community effect*". They stubbornly hold a "great genius" view of the progress of science: the startling insights of elite individuals, the 'chosen ones of the gods', are what drive science forward. However, this is defiantly *unilateral* picture. The luminaries (Newton, Maxwell, Darwin, Einstein, Bohr) can s*tart* a scientific revolution – but only when a broader community of researchers properly stodgily prepared the field. Furthermore, the scientific community can soak the 'crazy ideas' up only if it is pre-prepared (by the Zeitgeist, etc.).

Hence sociology of scientific knowledge (SSK) in (V) wake decidedly broke away from positivist view of advancement of science as an asocial process revealing perennially valid truths. Overcoming the limitations of Kuhn's reduction of sociology to social psychology, SSK took scientific knowledge as an *artificial* product of social, political, and economic interactions.

However, the majestic painting inherited from Kuhn fatally contains notorious '*incommensurability* tenet': the partisans of "old" and "young" paradigms live in inscrutably alien socio-cultural worlds divided by relentless scientific revolution. Since the 'old' and 'new' paradigms are radically separated from one another by impenetrable 'incommensurability' barrier, it is baffling to see how one gets from one to the other. Hence Kuhn applies quasi-religious metaphor of 'conversion to belief' instead of cogently explaining *how exactly* new ideas emerge against the background of their predecessors, from which they are so totally distinct.

A spellbinding way out is to comprehend the rival paradigms not as merely *beliefs* that ensure the unity and coherence of members of a scientific community, but as a powerful and comparable means of *intervention* in specific problem situations. Accepting a paradigm is more like acquiring many skills and abilities than gaining profound Verstehen and stout belief in the truth of any provision. Crisis arises when anomaly cannot be properly localized, when scientists sense that they no longer quite know how to go on. Crisis is debilitating because it cannot be localized as the falsification of some beliefs; it is instead the inevitable *incoherence* of research practice. Genuine alternative to common scientific practice is not error, but *confusion* (Joseph Rouse).

In the wake of Stephen Shapin, Philip Kitcher and Helen Longino socially colored accounts, a further step is taken by the author on the path of explanation for the *mundane* (Bruno Latour) reasons for the triumph of the "novel" paradigm over the "old" one. And a corresponding expansion of the epistemological base of research requires a historical analysis of the forms of rationality in a "phenomenological perspective" (Husserl, Heidegger). The history of cognition constitutes a series of epochs of' "unconcealment" with the integrity of each "*mathematical projection of nature*" provided by reconciliation, plexus and interpenetration of sundry practices. Scientific truth is a way of coherence between ideal and material practices, their "attunement", a way of assemblage of these practices into a single whole.

Profound breakthroughs in science were *not* due to ingenious contrivances of brave novel paradigms, startling invention of 'send by God' immaculate ideas "ex nihilo". The breakthroughs were caused, among other things, by the harrowing piecemeal processes of accommodation, interpenetration, and intertwinement of the 'old' research practices.

The paradigmatic example is the second scientific revolution took place at the turn of the XIXth and XXth centuries. Its origin consists in the stiff clash of different 'mathematical projections of nature' each represented by a structured bundle of mathematical and experimental practices consolidated by metaphysical "hard core". Contrivance of new mature theory, be it the special relativity or quantum theory, is by *no* means the result of a sudden individual insight, an uncanny "Gestaltswitch", but mundane payoff of *piecemeal* reconciliation of sundry research practices.

It is via passionate "language games" of conceptual resources that a startling new math is born, destined to form the core of prolific research tradition. This suggests a conservative though flexible mode of a scientific upheaval: it turns out not *substantially* different from other types of socio-cultural changes.

Keywords: Kuhn, paradigm, Rouse, practice, Husserl, Heidegger, Latour, Sheffer, Schaffner, Kitcher, Longino, cognitive factors, social facets.

I.1. Epistemological Accounts of Scientific Revolutions as "Great Genius" Visions.

What are the good reasons of a scientific revolution? Why, for instance, did bold Copernican research program squeeze out entrenched Ptolemaic one commencing the first scientific revolution? What were the sober reasons of Maxwell's victory over Ampère and Weber? Or the reasons of Einstein's victory over Lorentz's? Or Planck's victory over Rayleigh and Jeans?

The pivotal *epistemological* approaches to broaching the subject and solving the stubbornly problem are commonly laid out by the following options.

(**I**) Guileless *inductivist* version (William Whewell); (**II**) *classical falsificationist* version of Karl Popper; (**III**) intricate *conventionalist* version of Pierre Duhem; (I**V)** *sophisticated falsificationist* version of Imre Lakatos and Elie Zahar; (**V**) promising yet insufficiently advanced *social-psychological* version of Thomas S. Kuhn.

Nevertheless, the deft explanations for the ultimate epistemological reasons for the' new' paradigm triumph over the 'old' one seem insufficient in the light of the following plain counter-arguments.

(**I**) Guileless *inductivist* account famously turns out to be especially squalid. For instance, already in the Ptolemy-Copernicus paradigmatic example the theories from both competing research projects practically *equally* deviate from the available observational data (see, for instance, Kuhn 1977). Even the notorious Buridanists, in their controversy with Aristotle, contended that, on the unsteady ground of astronomical observations, “it is impossible to assert definitely whether the Earth or the stellar sphere moves” (Oresme; quoted from Kokowski 1996). Or, in the case of Lorentz-Einstein transition, the competing theories were so similar to each other that both were related to ‘Lorentz-Einstein’ theory (Nugayev 2018).

(**II**) Classical *falsificationist* explanations of the substantial causes of the “old” paradigm defeat are commonly reduced to the following two perspicuous options.

(**II. a**) According to the austere one, the ‘old’ paradigm becomes unscientific for conjuring up notorious ad hoc hypotheses. For example, Ptolemy’s deft theory was illicitly *irrefutable* and therefore unscientific while Copernicus's superlative theory was just the opposite. Ptolemy’s ingenious heuristic was blatantly *ad hoc*. Any odd celestial fact could be aptly accounted for in retrospect by immense multiplying the intricate paraphernalia of heterogeneous epicycles, epicyclets, deferents, equants, and so forth. Likewise, Lorentz’s deft mature 1904 theory was blatantly ad hoc for it strived to explain the negative results of the Michelson-Morley experiment by the ad hoc Lorentz-Fitzgerald contraction hypothesis.

Regrettably, the “unrestricted proliferation” of manifold epicycles in Ptolemaic whimsical astronomy constitutes a wonted “historical myth” (Gingerich 1997). In actual research practice, to compensate for the flagrant equant triumphal stave off, Copernicus had to insert a *new* species of no less stale epicycles. Eventually, Ptolemaic “antediluvian” programme transpired to embrace fewer epicycles than the “bold revolutionary” Copernican one. Likewise, the Lorentz-Fitzgerald hypothesis was not an ad hoc one since it was grounded on the profound “Molecular Forces Hypothesis” (Zahar 1973).

(**II. b**) According to the second, more sophisticated version (Popper [1935], 2002), eventually, the smashing blow of the “critical experiment” relentlessly refutes the “old” theory in favor of the “new” one. For instance, Ptolemy’s and Copernicus’s competing contrivances were practically equally lame for a sufficiently long time. But eventually, the smashing blow of the ‘critical experiment’ relentlessly refuted Ptolemy, buttressing Copernicus at the same time. Though when did this textbook miracle happen? Alas, historians of science have not come to a consensus yet.