**Rinat M. Nugayev** **RECONSTRUCTION OF MATURE THEORY CHANGE :**

 **A THEORY CHANGE MODEL**  Peter Lang, Frankfurt/M-1999

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**INTRODUCTION. RECONSTRUCTING THE PROCESS OF MATURE THEORY CHANGE AS A PROBLEM OF MODERN PHILOSOPHY OF SCIENCE.**  According to Werner Heisenberg, “ *probably, as a kind of general supposition, it can be said, that those directions in the history of human thought appeared to be most fruitful, where different ways of thinking encountered. These ways of thinking have their roots in different spheres of human culture or in different times, in different cultural mileau or in different religious traditions. When they really meet with each other, when they correspond to each other so that an interaction between them takes place, one hopes that new and interesting discoveries will follow”* *(Heisenberg. Physik und Philosophie. Frankfurt am Main,1959).*

These words can be chosen as an inspiring motto to what shall be said on theory change. But let me start with a common brief review of what was already done in the domain under consideration.

Up to now a vast amount of history and sociology of science data have been accumulated. These has helped to create various models of scientific revolutions trying to explain many features in the development of scientific knowledge. Yet it seems obvious that the conception which integrates at least the most alluring features of the models proposed while remaining free of their well-known shortcomings is still in its infancy. The main shortcomings consist of the following. (1) All the models proposed lack the necessary criteria to distinguish periods of evolutionary scientific development from periods of revolutionary development. Moreover, the criteria applied to the models proposed, cannot distinguish ‘micro-revolutions’ from ‘macro-revolutions’. As a result, different models provide diverse (and even inconsistent) interpretations of the same periods in scientific development. (1a) One group of investigators consider them revolutionary processes whilst others describe them as examples of evolutionary development (the most vivid instance is the history of maxwellian electrodynamics). (1b) The same period of development of a particular discipline is characterised either as a “ global revolution” that leads to profound changes throughout the structure of previous understanding, or as a series of ‘micro-revolutions’ that take place in many different domains of the same old theory (see, for instance, the history of quantum theory). (2) The majority of scientific revolution models proposed are based either on *empiricist*, or on *pan-theoretic* approaches to the role of the so-called “ crucial” experiments. (2a) According to the empiricist approach, any scientific theory can be refuted by a single “crucial” experiment (or at least by a small number of them).The paradigmatic examples of such experiments are: Young’s experiment (the refutation of corpuscular light theory), the Michelson-Morley experiment (the refutation of the luminiferous ether theory), the experiments of Rubens & Curlbaum, Lummer & Pringsheim (the refutation of classical back-body radiation theory, and Mercury perihelion precession observations (the refutation of Newton’s gravitation theory). (2b) According to the pan-theoretical approach , developed, as a rule, by representatives of the so-called “historical” trend in Western philosophy of science (T. Kuhn, I. Lakatos, P. Feyerabend, etc.), there are no crucial experiments for such mature theories as quantum mechanics or maxwellian electrodynamics. ”Critical experiments work well with theories of a low degree of generality whose principles do not touch the principles on which the ontology of the chosen observation language is based (Feyerabend 1963, p.7). A local crucial experiment between subsequent versions of the same programme works quite well. But even famous key experiments do not carry sufficient weight to subvert scientific research programmes (Lakatos 1970, p.158). However, both approaches are apparent over-simplifications and are rather dubious. The first because it is based on - together with an empiricist theory of scientific knowledge - an incorrect supposition about the existence of observation language, independent of theoretical disseminations (see, for instance, Suppe 1974 and references cited therein).The second one is also unacceptable (see for details the works of Franklin 1986,1995; Galison 1987; Hacking 1983). It obviously contradicts the opinions of most working scientists. And the typical Lakatosian argument that a particular experiment can be described - although retrospectively - as crucial if it can very clearly be seen to uphold the triumphant programme, nothing changes for the following reasons. In Lakatos’s methodology the very content of a scientific research programme (including its “hard core” as well as its positive and negative heuristics) is the result of rational reconstruction, although by no means a fact of real history of science. Using the ambiguous aspects of Lakatosian methodology, a philosopher of science can always reconstruct the “hard core” of the concocted scientific programme in such a way as to make any given experiment look like an overwhelming reaffirmation of the programme. (3) None of the models proposed until now (1996) can theoretically reproduce (in the sense that will be described below) so-called “theory-choice situations”, i.e. situations where the same host of empirical data is described by semantically different theories. However, these situations are a hallmark of the process of transition from one mature theory to another. In particular, as is well-known, Lorentz’s ether theory and Einstein’s special theory of relativity co-existed for some time as empirically-equivalent ones. In particular, they had explained, though in different ways, the results of the experiments of Michelson & Morley, of Rayleigh, of Bucherer, of Trouton & Noble, etc. However, in the epistemological models proposed, the coexistence of different empirically-equivalent theories cannot be explained and must simply be accepted at its face value, as a fact of history of science. I believe that at least some of the shortcomings mentioned above are caused by insuperable difficulties in the (implicitly or explicitly assumed) mature theory change concepts on which the scientific revolution models are based. In order to bridge this gap and take the next step towards constructing a more satisfactory global model of scientific revolution it is necessary in turn to answer the following questions. *What are the real reasons for replacing the “old” mature theory with the “new”? What are the routs and the mechanisms of the theory-change process? When must the old mature theory give way to the new one? How did mature theory change actually take place in history of science? How should it take place?* It may seem that some concepts of scientific revolution have already dealt with at least some of these problems, and that, consequently, there is no real need to consider them separately. However, this is not the case. In the first place, the very notion of scientific revolution is so vague, that it is given many different and even most contradictory meanings throughout scientific philosophy. Scientific revolution is the change of Weltanschauungen, of Scientific Research Programmes, of scientific traditions, of simple theories, of instruments of knowledge (telescopes, computing devices, and even of quills and fountain pens), of scientific practices and finally of famous ‘paradigms’. Secondly, the notions of global scientific outlooks, research programmes, paradigms, scientific traditions and scientific practices are themselves no less vague, than the notion of Scientific Revolution. For instance, even the term “paradigm” has been used by one philosopher of science - Thomas Kuhn - in more than 20 different ways (Masterman 1970). Thirdly, the real , ’genuine’ scientific revolution (in any sense of the term) is a very complicated and intricate blend of the most varied (and often rather unexpected) factors, ranging from the socio-psychological features of scientific communities to the socio-cultural “atmosphere” in which the members of the communities are plunged. It will take generations of philosophers, sociologists and historians of science to unravel this problem; on my part I shall attempt merely to deal with the **logical -methodological, ‘internal’**  **facet.** Hence the notion of “scientific theory change” used in the title of my work is rather peculiar and is applied for the following reason: it is the logical-methodological facet which, relatively independently of all others, lends itself to investigation. I believe it should be philosophy of science which sets the trend of scientific development studies in the “first approximation”, followed by the “second approximation”, then the third and so on as established with the assistance of other specialists. (See, for instance, David Bloor’s analysis of the negotiation of a proof in mathematics – Bloor 1991, pp.146-56). What makes the problem of the logical-methodological model of mature theory change construction particularly important now? In actual fact the widely accepted “old” model or concept is based on the so-called ‘Received View’, ‘ hypothetical - deductive’ interpretation of the scientific theory. Those who uphold this interpretation maintain that all the scientific theories are structurally very similar to the interpreted calculi of axiomatic theories of mathematics. This means that any scientific theory can be reconstructed so that its content may be logically deduced from a certain number of initial hypotheses. In this case the refutation of a single result inevitably becomes the refutation of the initial hypotheses and, consequently, of the whole theory. One single critical experiment is sufficient to refute the whole theory. Nowadays, however, the majority of philosophers of science argue that standard hypothetical-deductive interpretation of scientific theory is insufficient. This interpretation cannot adequately grasp the whole group of scientific theories, as these theories obviously contradict accepted standard interpretation of canonical axiomatic reconstruction. Furthermore, Newtonian mechanics, for instance, can be represented by more than 10 systems of axioms. Which system should be chosen? The Standard Interpretation has been replaced by a so-called “nonstandard” one. The latter has yet to be fully defined, and its advocates differ significantly in their views. But they share the following ideas. A scientific theory contains at least three groups of hypotheses that differ from each other in their functions: (1) The *mathematical* hypotheses form the mathematical apparatus of the scientific theory. (2) The *semantical*  hypotheses are used to shape the scientific theory models. (3) The *fundamental* hypotheses are the basic suppositions of the scientific theory. Besides the third group of hypotheses, some additional, less fundamental, hypotheses are assumed during theory construction; these additional hypotheses cannot generally be obtained from the basic assumptions of the scientific theory. When added to the group of initial assumptions, some of the additional hypotheses may suggest a particular theory, which has a direct relationship only to a comparatively small group of phenomena. All the proponents of nonstandard interpretation share the point of view that **no basic theoretical assumptions can be directly compared with experimental and observational evidence.** This comparison should be made using the intermediate-level hypotheses. Hence their functions are two-fold. On the one hand, these hypotheses help to “create” (Ian Hacking) new phenomena and new experimental laws, as well as predict new experimental results. On the other hand, they can interpret existing experimental data. Thus, the Standard Hypothetical - Deductive Model was shown to be unsuitable, since it had imposed over-strict limitations on scientific theory structure. Yet it did have one indisputable advantage - a simple clarity. However, the acceptance of the nonstandard interpretation unavoidably poses the question:***if there are no strict rules governing the transition from basic theoretical assumptions to experimental results, how can a fundamental theory be refuted as a whole?*** And if it can not be refuted, how - in real history of science - did one fundamental theory actually replace another? Were the scientists responsible for the theory change process guided by rational considerations? Were they right or wrong? For example, according to an advanced nonstandard conception (Stepin 1976), a scientific theory is a set of propositions that describe the relationship between theoretical objects of two types - basic objects and derivative ones. The set of basic objects makes up the aggregate of initial idealizations (the Fundamental Theoretical Scheme or FTS) with no direct reference to experimental data. For instance, the FTS of classical mechanics consists of the Material Point, the Force and the Inertial System of Reference. The derivative theoretical objects are formed from the basic ones according to certain rules. The sets of derivative objects constitute partial theoretical schemes or PTS. Any mature scientific theory grows due to transitions, in order to describe each new experimental situation, from FTS to PTS. Hereafter, any scientific theory whose abstract objects are organized into the subsystems belonging to at least two levels - the Fundamental Theoretical Scheme level and the level of partial theoretical schemes - will be referred as a “**mature theory**”. Since each PTS construction from the FTS represents a problem that cannot be reduced to a strict algorithm, it seems, at the first sight, impossible to refute the FTS. On the contrary, if a certain PTS contradicts experimental results, it can always be replaced by another. But what then are the reasons for mature theory change? The discovery of a simple divergence between theoretical predictions and experimental results refutes the partial theories but not the fundamental ones. When such anomalies are discovered, PTS can be modified in order to coordinate the whole mature theory with experience (the so-called Duhem-Quine thesis).And if one single, arbitrary anomaly can be eliminated, we can just as easily eliminate two, three, four and so on - one after another. Just how many anomalies are needed to refute a mature theory? Clearly an infinite number of anomalies should be found: while a mature theory can be adapted to incorporate the results of any finite number of experiments, it can not accomodate an infinite number of them. But what is meant by the statement that theory “contradicts an infinite number of experiments”? - The fact is that theory contradicts **all** experiments of a certain type, i.e. not only that have been performed already, but also those of the future. However, it then follows that one mature theory may contradict another! Only theoretical law can predict the results of future experiments which have yet to be carried out. As is well-known, a theory is established not only to explain existing experimental data, but also to predict new data. Without claiming to be substantiated, even these preliminary considerations lead to the conclusion that ***a mature theory can be refuted only by using a critical experiment based on a different and contradictory mature theory.*** The mere interaction of one “old” theory with experience is obviously insufficient to theoretically reproduce the process of mature theory change. In addition, the interaction of several “old” mature theories should be taken into account. If an experimental anomaly arises as a result of encounter of several mature and contradictory theories, it can not be eliminated by usual PTS modification methods. Its ultimate elimination demands the resolution of the cross-contradiction. This can be done by establishing a more general theory, embracing the old mature theories. The aim of this study is to clarify and categorise the above heuristic arguments, constructing a methodological model of mature theory change (chapters II,III and IV), to compare this model with the results of other researchers (chapter I), and to demonstrate the model’s heuristical value in history of science (chapter V) and in modern physics (quantum theory in curved spacetimes, chapter IV). Chapter V serves to demonstrate that, like others, the model proposed can properly explain well-known facts of history of science. Moreover, it highlights some *new* facts (hitherto ignored by other models) , expanding the domain of rational reconstruction. I hope that the study of a particular period in the history of science can not merely be used as a tool for the critical analysis of alternative conceptions. I believe that my rational reconstruction of the Lorentz-Einstein transition is interesting in itself, since it both describes the origin of the special theory of relativity and explains why it was accepted by the scientific community, in some respects superseding existing literary accounts. Further to the belief that the context of justification cannot be understood outside the context of discovery, I maintain that the special theory of relativity was just a stage in the deployment of quantum theory and that the reasons why it was accepted can not be comprehended without taking into account Einstein’s works (and the works of his contemporaries) in the quantum domain. **The main idea** of this book is that well-known mature physical theories such as classical mechanics and classical electrodynamics were replaced not because of the type of interaction with experience described by the empiricists, and not due to the socio-cultural ‘fancies’ of scientific elites. I seek to show that mature theory change was caused by clashes with anomalies based on the other mature theories (which contradicted the initial one).These anomalies could be eliminated only by (complete or partial) resolving the cross-contradictions of the several mature theories, i.e. by constructing a general theory. For instance, special relativity and the early quantum theory were created within the same programme of statistical mechanics, thermodynamics and Maxwellian electrodynamics reconciliation. Quantum and relativistic revolutions were practically simultaneous since they had a common origin - the clash between this three mature theories of the second half of the XIX th century that constituted the “body” of classical physics. The very realization of reductionist and synthetic research programmes is brought about by the clash of mature theories which they are designed to eliminate. These programmes encompass the (partially) empirically-equivalent scientific theories. For example, Lorentz’s electron theory was elaborated within the reductionist programme, while Einstein’s special relativity - within a synthetic programme of mechanics, electrodynamics and thermodynamics unification. In the long run, the programme capable of effectively eliminating the cross-contradiction prevails. Having compared the heuristic potentials of the reductionist and the synthetic programmes, I favour the latter group since it has the following objective advantages. Firstly, synthetic programmes **should** provide a higher empirically-progressive shift of problems solved. Secondly, only these programmes can rationally explain the application of the so-called hybrid or crossbred objects which spring from the coincident theories. But if we consider the structure of two modern theories - quantum theory and general relativity - we can see that their global theoretical schemes were engendered by the unification of the crossbred theoretical schemes. This is in no way to maintain that each cross-contradiction should always be eliminated by choosing a synthetic programme or that all the cross-contradictions that took their places in the history of science were eliminated with the help of synthetic programmes. It means only that synthetic programmes have some *objective* alluring properties and that is all that can be said about the competition of synthetic and reductionist programmes in general. My concept and the critical experiment concept (as well as the standard hypothetical - deductive model) are not alternative. On the contrary, the former is merely a more sophisticated version of the latter. If a problem situation is created by the occurrence of cross-contradictions, any anomaly connected with this contradiction becomes a critical experiment, since it can be eliminated only by constructing a global theory. This is not to down the role of experiments in science. On the contrary, the model proposed seems to elaborate further the point of view stated in the current literature that both theorists and experimentalists have breaks in their respective traditions, but they are not typically simultaneous (Pickering 1985; Galison 1987). Theory development must have, to some extent, a life of its own. The development of two main cultures within science does not mean that the two do not speak to each other. However, the fact that the model proposed tries to depict breaks in theoretical traditions diminishes its domain of validity in history of science significantly. The split between theory and experiment has become an important part of the most highly cultivated part of science - of physics - only at the end of the XIX-th century. Up to that period physics was an experimental science sui generis. Only after the Second World War were theoretical physicists as numerous as experimentalists (Galison 1987). Hence the theory-change model proposed is not universal. It is not applicable to, say, XVIII-th century physics, but starts to function only with the establishment of modern physics. I think that the model proposed describes only logical-methodological **facet** of a very complicated process that should be described in general in sociological terms. That is why my aim is to open Pandora’s box only, and not to close it. Yet the aspect investigated proposes the following picture. The history of science can be looked through a fluctuating pattern of research traditions. Of all these traditions only those can survive that manage to support each other. ”A more powerful theory, we submit, is one that with fewer elements and fewer and simpler transformations makes it possible to get at every other theory (past and future).Every time a powerful theory is celebrated it is always possible to rephrase this admiration in terms of the most trivial struggle for power: holding this place allows me to hold all the others. This is the problem we have encountered right through this this paper: how to assemble many allies in one place” Latour 1990, p.50). For instance, the world of “old”, pre-Einsteinian physics was conceptually and socially fragmented. It was split on at least 4 research traditions belonging to Maxwellian electrodynamics, thermodynamics, Newtonian mechanics and Boltzmann’s statistical mechanics. Traditions organised around different groups of phenomena generated little support for one another. The practitioners of each theoretical tradition acknowledged the existence of the other but went their own separate ways. With the advent of relativity and quantum theory the conceptual unification of worldviews was accompanied by a social unification of practice. Hence the main idea of this book is rather simple. Accounts that had tried to describe mature theory change on the *monistic* grounds (i.e. considering the deployment of a single theory, a single scientific research programme, a single paradigm, etc.) failed. Thereby one should plunge into a “pluralistic” background. To understand the sound reasons of mature theory change one should consider the **interaction , interpenetration** of theories, research programmes, paradigms, traditions, practices, etc. It is crucial that paradigm consists not only of theories, but of values, metaphysical suppositions and research patterns as well. How the interaction of all these components should be described? - I don’t know. But I insist that the dialectic of the old mature theories is crucial for theory change.

**СHAPTER ONE. A CRITICAL ANALYSIS OF THE THEORY-CHANGE MODELS PROPOSED.**  ***Part one. A brief description of the models proposed***.

 The problem of theoretically reconstructing theory-change is not a new one for the philosophy of science. Attempts to formulate and solve the problem may be found in the works of Whewell (1867), Poincaré (1906) and Duhem (1908). But before proposing a brief account of the main concepts hitherto proposed, it is necessary to give the following explanation. The occurrence of several coexisting empirically-equivalent theories is one of the hallmarks of mature theory change. *Empirically-equivalent* theories are the ones that differ from each other in semantic respects, but that describe the same domain of reality and produce identical empirically-verifiable results (see, for instance, Hanson 1951, 1961, 1966). The necessity to eliminate uncertainty in the future development of science, caused by the existence of such problem situations, is usually identified with the necessity to make a definite choice in accordance with certain rules from a set of empirically-equivalent theories. Thereby the terms *theory-choice situation* and *theory-*choice *criteria* were introduced. The problem of constructing a theoretical model that can describe the laws of occurrence and resolution of theory-choice situations is usually called *the theory-choice problem* (see, for instance, Mamchur 1975; Pakhomov 1975). So far, various methodological models of occurrence and resolution of theory-choice situations have already been proposed. However, precisely because the resolution of theory-choice situations constitutes the final stage of the theory-change process, the authors of most models are in fact discussing different reconstructions of the change process, as it is not possible to give an adequate description of a theory-choice process without implicitly or explicitly assumed theory-change concept. Let us investigate the main theory-change concepts proposed.

 **I. The Monotheoretic Concept**.

 I.A. The Empiricist Version. The mature theory-change process takes place as a result of a *critical experiment* (Popper).The experiment reveals the old theory’s inexpedience which manifests itself as either a contradiction of the critical experiment, or as a trend to adapt to its results using auxiliary *ad hoc* hypotheses. I.B. The Nonempiricist Version. The criteria of choice between the “old” and the “new” theories are either the “instrumentalist” criteria of *Simplicity, Fruitfullness, Beauty,* etc. (Duhem 1906; Schlesinger 1963), or a combination of such criteria (see as one of the most vivid examples the articles collected under the title “Methodological Principles of Physics”, Moscow, Nauka, 1975). If neither of these alternatives are considered suitable, a new one must be found (Bransky1973).

 **II.The Lakatosian Concept**.

 Since the following concept is extremely well documented, I shall highlight only its most salient features. When the theory-change process is reconstructed, it is more suitable to speak of *scientific research programmes,* than of theories. Each Scientific Research Programme (SRP) ensures the construction of its own sequence of theories based on a particular set of conventionally accepted *(irrefutable*) hypotheses that constitutes the *hard core*. Each n-th theory of the sequence is the result of adding a definite auxiliary hypothesis to the preceding (n-1)-th theory. The continuity of the sequence is ensured by set of rules, part of which dictates the correct way to SRP realization *(the positive heuristic*), while the other part tells us which paths should be avoided *(the negative heuristic*).All the *heuristic* guides the construction of a *protective belt* around the *hard core*. The belt’s hypotheses should adapt or even be replaced when the n-th theory of the sequence meets with *refutations*. Any SRP has a *progressive* phase and a *regressive* one. The SRP progresses, if the auxiliary hypotheses that constitute the transition from the n-th version of the SRP to the (n+1)-th, satisfies the following conditions. (1) It possesses some empirical features that are absent in the preceding hypotheses and can explain their empirical success. (2) At least some of these additional empirical features can be confirmed by experiments. (3) The hypothesis is proposed in accordance with the SRP positive heuristic. If the auxiliary hypothesis does not suffice at least one of the conditions outlined, the SRP regresses. How does a theory-choice situation occur?

“When two research programmes compete, their first “ideal” models usually deal with different aspects of the domain (for example, the first model of Newton’s semicorpuscular optics described light-refraction; the first model of Huygens’s wave optics, light interference).As the rival research programmes expand, they gradually encroach on each other’s territory and the n-th version of the first will be blatantly, dramatically inconsistent with the m-th version of the second” (Lakatos 1970,p.158).

As a result, the first programme suffers defeat at the hands of the second. But the war is not over: any SRP can suffer several such defeats. Thus, SRP methodology cannot even so much as suggest a version capable of resolving the situation where several programmes compete.

”One may rationally stick to a degenerating programme until it is overtaken by a rival and even after. What one must not do is to deny its poor public record. Both Feyerabend and Kuhn conflate methodological appraisal of a programme with firm heuristic advice about what to do .It is perfectly rational to play a risky game: what is irrational is to deceive oneself about the risk” (Lakatos 1978, p.117).

But does Lakatos’s failure to propose a way of choosing between the competing programmes indicate, for instance, that in a choice situation, the theories of Newton and Einstein were both equally good? - According to Imre Lakatos (1970), this is not the case. Einstein’s theory is *better*, since it successfully explains all that Newton’s theory explained, as well as some well-known anomalies. Moreover, at least part of the new information of Einstein’s theory was experimentally confirmed. Hence the theory-choice situation should be resolved, according to Lakatos, by choosing rather than a programme, a theory from within the SRP progressing phase. Otherwise, how should Lakatos’s claim that Einstein’s theory was better than Newton’s be understood? Of course, this does not mean that the SRP methodology prevents scientists from cultivating P1 whilst it is regressing. It is quite possible that within the P1 framework a theory could be constructed that would be better than a modern one. But this means, that the successful resolution of theoretical and practical problems necessarily demands the use of the theory from the progressing programme P2 , as this one is better than the others. If in future P2 progresses over P1,the theory from P2 should be exploited.

 **III. Kuhn’s Concept**.

 In the process of Scientific Revolution, *paradigms* rather than theories or scientific programmes are changed. (Note: I have placed the review of Kuhn’s conception after the review of SRP methodology; in fact, the Lakatosian concept was created after the “Structure of Scientific Revolutions” and under the strong influence of its author - see Feyerabend 1970, for example). Each paradigm has at least two aspects. Firstly, it is a *disciplinary matrix* that characterizes the set of beliefs, values, technical means, etc., typical of the scientific community. On the other hand, a paradigm is a commonly accepted *pattern* of applying general laws of a given theory to actual situations. Each new paradigm always comes altogether with fixed pattern of application, though the latter cannot cover all the application spheres and in no sense can be interpreted as methodological rules. The correspondence rules, that, contrary to the naive applications of logical empiricists, can provide the empirical interpretation of the theoretical terms, do not exist at all. This interpretation depends upon the commonly accepted patterns. Two scientific communities with identical general symbolic conclusions, but different patterns, will differ in their interpretations of theoretical terms. They will interpret the general conclusions in rather different ways, being divided by a *gestalt switch*. Not even the observation language is neutral. The disciplinary matrix cannot be completely reduced simply to theories and methodological rules. Hence an individual scientist can master it only through education and direct participation in the research. It is in this way that a scientist masters the paradigm-disciplinary matrix with the help of the paradigm-pattern. According to Thomas Kuhn, the change of disciplinary matrixes is the main ingredient of any scientific revolution. Since the patterns are connected with matrixes, the incommensurability of the patterns leads to the incommensurability of the matrixes. The physical substance of the Einsteinian notions is in no respect equivalent to that of Newtonian notions, even if their names coincide. Newtonian mass is constant, whereas Einsteinian mass can transform into energy. Only at low velocities can both quantities be measured in the same way. The old paradigm is incommensurable with the new one. No objective basis for their comparison exists (see also Feyerabend 1976). No logical argument can prove the superiority of one paradigm over another. It is impossible to resolve a theory-choice situation by logical or mathematical proof. Simplicity, Beauty, etc. are usually referred to as criteria of choice, but they are actually mere *factors of value*. They are used by various scientists in rather different ways. But what is the mechanism of paradigm - change according to Kuhn? - In any scientific revolution a new paradigm emerges only *after* the obvious failure (*the crisis*) of the old one to solve its *puzzles.* Nevertheless, the inability to solve technical problems is not the only sign of crisis; it is, however the main one. Scientists never reject the old paradigm easily. Anomalies are not immediately considered as counterexamples. The transition to crisis begins only when an anomaly of major importance to the scientific community is transpired. For instance (as in the case of Copernican Revolution) , an anomaly can lead to crisis if the application which it hinders is of extraordinary practical significance (the creation of the Gregorian calendar). However, using the described concepts to analyse *concrete* theory-change situations occurring in real science meets with considerable difficulties. ***Part two. The inefficiency of the Monotheoretic Model***. I.A. As will be shown in the fifth chapter of the present study, the analysis of the theory-choice situation in the “electrodynamics of the moving bodies” reveals the following. At least in its present form, the empiricist alternative is incapable of resolving the theory-choice situation between the theories of Lorentz and Einstein. Moreover, it is unlikely that this alternative may be found within the empiricist approach, since the latter is based on an incorrect thesis about the existence of an *observation language* independent of theoretical presuppositions (see at full length Suppes 1974; Schwyrev 1978). I.B. Notorious debates about the precise nature of the instrumentalist criteria of choice appear to be fruitless, since no agreement has been reached about any of them. There exist actual situations when different versions of the same criterion point towards different theories. For instance, in the theory-choice situation of the modern theory of gravity, one version of the Simplicity criterion favours a metric theory, while the other chooses its nonmetric counterpart (Chudinov 1974). Historians of science are fed up with situations where the different criteria dictate different choices, different solutions.

”In such cases of value-conflict (e.g. one theory is simpler but the other is more accurate) the relative weight placed on different values by different individuals can play a decisive role in individual choice. More important, though scientists share these values and must continue to do so if science is to survive, they do not all apply them in the same way”(Kuhn 1970,p.262).

 For instance, in the theory-choice situation between the theories of Ptolemy and Copernicus the criterion of Scope favours Ptolemaic astronomy, whereas the criterion of Simplicity favours that of Copernicus. Ptolemy’s theory is based on the assumption that the Earth is situated at the centre of the Universe. This theory was in complete accord with Aristotle’s physics, which dominated science of the day. On the other hand, the theory of Copernicus was simpler than the theory of Ptolemy, since it was free of equants. It is of no wonder that the application of a system of criteria meets with even more difficulties. Obviously, such a system can be built only within a certain logical-methodological model of the theory-change processes. However, as far as I know, this has never been done. Moreover, on having successfully resolved the theory-choice situation using a non-empiricist criterion of choice, the problem of its justification on the basis of the theory of cognition must be tackled. For instance, justification of the Simplicity Criterion is based on the typically metaphysical argument that *Nature is Simple*. On the other hand, if we attempt to interpret this criterion *a la Kant* - as an absolute cognitive form (Meyerson 1908,p.8) valid for all the times, we will arrive at an obviously aprioristic philosophy. However, the non-instrumentalist criterion of choice proposed by Vladimir Bransky in his highly original book “The Problem of Synthesis of Relativistic and Quantum Principles” (1973) is also ineffective. This criterion was called the *Principle of* *General Observability* (PGO).It seems to me that the uncertainty of PGO’s basic notions *different and independent methods of observation* prevents the criterion’s successful application. Indeed, according to V.P. Bransky (1973, p.82), if the object is observed by at least two different and independent ways, the dependence of the observation results (of the natural phenomena f and g) on each other can be understood rationally only on the assumption that the object exists independently of these observations. Consequently, according to Bransky, only *Principally Observable* *Objects* (POO) can exist objectively, i.e. independently of the observer. These objects have the following properties. (1) The notion of POO is introduced within a given theory to explain the natural phenomena that have been observed. (2) This explanation is provided by endowing the object with property A1. (3) Besides A1 , the object has property A2, which explains one more natural phenomena that can be observed independently of A1. In theory-choice situations one must choose the theories that use POO’s. However, in the version described above the Principle of General Observability is unacceptable for the following reasons. 1. This version allows observations’ independence to be interpreted as follows: device measuring A1 must remain totally separate from A2.But such a set up allows the quantum-field vacuum to be observed and reveals the insufficiency of PGO even in quantum-field theory - in the domain that was specially chosen by Bransky himself to examine his criterion. In quantum electrodynamics (and in quantum field theory in general) the so-called Casimir Effect was discovered in 1947 in Philips Labs: vacuum fluctuations of the electromagnetic field produce attractive forces between neutral, flat, parallel conductors. There are no real particles between the conductors - only virtual ones. But experimental evidence forces us to take the energies of the virtual particles seriously (Boyer 1969; for a review of the Casimir Effect applications in theory of gravity( see Nugayev 1991). 2. Analysis of the theory-choice situation in the electrodynamics of moving bodies (Grunbaum 1973) reveals that the above interpretation of PGO, contrary to Bransky’s intentions, does not deny the existence of ether. Indeed, the latter may be observed in 2 different ways - by a 2-armed interferometer in the Kennedy-Thorndike device and a strong magnet in the Kaufmann experiment. Hence the PGO application is unable to resolve the theory-choice situation between the theories of Lorentz and Einstein.

 ***Part three.The inefficiency of the Lakatosian Model.*** 3.1. The only hint I could find in Lakatos’s works as to the source of his *hard* *cores* is in a footnote, where he makes a mythological comparison: ”The actual hard core of a programme does not actually emerge fully armed like Athene from the head of Zeus. It develops slowly , by a long, preliminary process of trial and error. In this paper this process is not discussed” (Lakatos 1970,p.133;there are also some obscure remarks in “Popper on demarcation and Induction” and in “Why Copernicus’s Programme superseded Ptolemy’s?” in vol.1 of Collected Papers). But the analysis of history-of-science data reveals that, as a rule, no scientist discloses the source of his hard core in any document (maybe because of his deliberate intentions to hide some aspects of his subconscious content - see John Worrall’s sarcastic comments in “ Rationality, Sociology and the Symmetry thesis”, Dubrovnik 1989, p.5,unpublished:

”Suppose, absurdly of course, but just to make the argument less abstract, that it turned out that Airy ’s parents liked to shower each other with small particles of dirt while having sexual intercourse and that little George was frequently the unseen witness of these “perverted acts””).

Moreover, during the course of their work scientists often contradict themselves and change their assessments of their own incentives and cognitive presuppositions from one paper to the next (see, for instance, the fifth chapter of this book).On examining the historical examples given by Lakatos (the programmes of Copernicus, Prout and Bohr), we can conclude that it is the philosopher of science himself who tries to reconstruct the content of the “hard core” (see especially convincing critique of Lakatos’s reconstruction of the Michelson &Morley experiment by Hacking 1983).

”For instance, Prout never articulated the ‘Proutian programme’ :the Proutian programme is not Prout’s programme. It is not only the (‘internal’) success or the (‘internal’) defeat of a programme which can be judged only with hindsight: it is frequently also its content”(Lakatos 1970,p.119).

But then it is only the philosopher of science who can decide whether or not to consider the shift from one theory to another as *progressive*. The philosopher can always reconstruct the (self concocted) *hard core* of the SRP in such a way that it will seem *progressive* at least for a certain period of time. This precisely what Thomas Kuhn had in mind when writing: ”What Lakatos conceives as history is not history at all but philosophy fabricating examples” (Kuhn 1971,p.143). **Different reconstructions of the same SRP’s hard core can make different choices in the same theory-choice situation**. 3.2. The rational reconstruction of the Lorentz-Einstein transition done by Lakatos’s disciple (Zahar 1973) deviates significantly from real history of science (see chapter 5 of this book for details).Of course, the true Lakatosians can retort that “when rationally reconstructed, the growth of knowledge has a place in the world of ideas, in Plato and Popper third world, in the world of knowledge that is independent of the cognitive subject” (see Lakatos 1970, for instance).Some history-of-science data are anomalous with Elie Zahar’s reconstruction. Yet this does not make them counterexamples, since the methodologist’s task is to provide a *rational reconstruction* of history, i.e. the *description of objective scientific growth*. But why then is the SRP Methodology better than Inductivism, “naive” Methodological Falsificationism, etc., rejected by Imre Lakatos? After all, his main argument against them is that in Inductivist and Falsificationist reconstructions, scientists are either irrationally slow, or irrationally hasty:

”... 85 years elapsed between the acceptance of the perihelion of Mercury as an anomaly and its acceptance as a falsification of Newton’s theory... On the one hand, scientists frequently seem to be irrationally rash: for instance, Galileo and his disciples accepted Copernican heliocentric celestial mechanics in spite of the abundant evidence against the rotation of the Earth; or Bohr and his disciples accepted a theory of light emission in spite of the fact it ran counter to Maxwell’s well-corroborated theory” (Lakatos 1970,p.115).

And, according to Zahar, too, scientists can be irrationally hasty: they accepted special relativity nine years too early. Historians of science state that the physics community rejected the ether theories as early as in 1910-1912.But, according to Elie Zahar, *rational reasons* for accepting special theory of relativity appeared only after 1916, when the general theory of relativity had emerged. 3.3. According to the SRP Methodology, “...progress in the theory of scientific rationality is marked by discoveries of novel historical facts, by the reconstruction of a growing bulk of value-impregnated history as rational” (Lakatos 1978, p.133). Each methodology (or *theory of rationality of scientific progress*) provides a theoretical framework for the rational reconstruction of the history of science. All methodologies function as *historiographical* (or meta-historical) *research* *programmes*. All of them organize the *basic value judgements of scientific elite* in a certain substructure. These value judgements are commonly accepted amongst contemporary scientists. In addition to anticipating new basic value judgements, a good theory of rationality should re-evaluate the old ones. Hence Lakatos proposes the following choice-criteria for an adequate theory of rationality.

”We then reject a rationality theory only for a better one, for one which, in this quasi-empirical sense, represents a progressive shift in the sequence of research programmes of rational reconstructions. Thus this new - more lenient - meta-criterion enables us to compare rival logics of discovery and discern growth in ‘meta-scientific’ - methodological knowledge” (Lakatos 1978, p.132).

For instance, Popper’s theory of rationality should be rejected not because it contradicts basic value judgements of the scientific elite, but because of the advent of Lakatos’s theory of rationality. And the latter is better than Popper’s theory for the following reasons. (1) It gives a unified understanding of well-known but previously isolated basic value judgements. (2) This theory leads to new basic value judgements, which are *unexpected* in the light of Popper’s theory (see the example with Mercury perihelion described). Hence, according to Lakatos’s own meta-criterion, if in future a new theory of rationality is proposed that is able to provide an *empirically progressive problemshift* towards the SRP Methodology , Lakatos’s theory should be rejected and the new one accepted. In chapter 5 of this book I seek to show that my concept (chapters 2, 3 and 4) gives a certain rational reconstruction of the Lorentz-Einstein transition. This reconstruction: (a) anticipates new basic value judgements unexpected by the SRP Methodology; (b) gives a unified understanding of the well-known basic value judgements that are isolated in the SRP Methodology; (c) makes it possible for a historian of science to consider the greater part of basic value judgements as rational. Hence in the objective, Lakatosian sense this concept provides an empirically-progressive problemshift towards Lakatos’s theory of rationality. Does this mean, that, according to Lakatos’s own meta-criteria, the SRP Methodology should be replaced by the one presented in this book? 3.4. In the SRP competition process (Lakatos 1970,p.158), the ideal models of the first programme deal with one aspect of the domain of reality while the ideal models of the second programme are dealing with another, and only  *then* can the programmes come into competition with each other, producing (sometimes partly) empirically-equivalent theories. But, if this is so, how can the theories in both programmes have the same empirical consequences? The existence of a theory-change situation cannot be explained merely by “internal” history. 3.5. Lakatos’s primary model accepts the cases where K (K$ >$ 2) rival programmes compete. Though actual appraisals are always comparative in the SRP Methodology, the single criterion in terms of which such appraisals are made can only be applied to individual research programmes. Choosing between several programmes, one first locates each individually on the fruitfulness scale; and only then does one compare them (Kuhn 1980).Since the competing programmes deal first with different aspects of the domain, one can imagine a situation with K (K$ >$ 2) rival programmes. Some of them degenerate, while others successfully continue to predict new facts, each with respect to its own domain. In these circumstances Lakatos’s rules of SRP-elimination seem to be insufficient. 3.6. All the Methodology of SRP case studies consider **two** competing programmes (Lakatos 1970; Howson 1976).But why only two programmes? In Methodology of SRP the facts about competition between two programmes belong to external history. 3.7. Real competition process can arise only when the rival programmes are **alternative.** This means that the acceptance of one of them is an automatic rejection of the other. Therefore, the hard cores of alternative programmes must be *incompatible*. And this is exactly the case when each new prediction of one programme appears to be a vital factor in the degeneration of its rival. If their domains of validity coincide, any *novel facts* produced by one programme become *puzzling anomalies*for another. Yet Lakatos’s model, with research programmes dealing with different aspects of the domain, permits the existence of **complementary** programmes (corpuscular optics, wave optics and the quantum theory of light). So, the effective explanation of (3.1) – (3.7) obviously lies outside the *internal files* of SRP Methodology. However, in that case any methodological model (or theory of rationality) that transfers those “facts” from external to internal history, supersedes Lakatos’s methodology. Having criticized the methodology of Lakatos, I seek hereafter (chapters 2-4) to suggest a modified version of it (capable of fruitful cooperation with sociological studies) and show how this modified version can deal successfully with the historical facts considered by Lakatos and Zahar as examples of their methodology.

 ***Part four.The Inefficiency of the Kuhnian Model***. 4.1. Kuhn states that various paradigms are incommensurable in the sense that there is no objective basis for their comparison. Scientists, sharing different paradigms, live in different worlds. Since metaphysical presuppositions constitute an important part of a paradigm, in general the Incommensurability Thesis looks as a respectable one. Moreover, the sociological approach elaborated later by S. Barnes and D. Bloor with its famous symmetry thesis considers sciences as cultures. It is rather natural to demand a significant cultural break when considering the transformation from one culture to another. But when one leaves the ground of pure philosophical and sociological discourse to analyse **concrete** situations, the difficulties occur. The theory-choice situation in the modern theory of gravity is one of Kuhn’s modern examples (see, for instance, The Structure of Scientific Revolutions).In theory of gravity all available experimental data have been satisfactorily described by more than 20 relativistic theories of gravitation. Of course, not all of them can be labelled paradigms. All the relativistic theories are proposed within two main paradigms - metric and nonmetric. A metric paradigm (A. Einstein, H. Cartan, R. Dicke, J. Wheeler et al.) is based on the assumption that describing the gravitational field necessitates the application of the non-Euclidean mathematical calculus. Space-time is “curved” for a proponent of this paradigm and a bundle of photons changes the direction of motion in the gravitational field of the Sun not because of the force but because the space-time is curved there. A nonmetric paradigm (H. Poincare, W. Pauli, R. Feynman et al.) assumes that the gravitational field is a usual physical field analogous to an electromagnetic one or to a field of strong interactions. The obvious advantage of such an approach consists in that the gravitational field can be easily quantized. Gravity can be described only with the help of Minkowski metric. Kuhn argues that there is no objective basis for comparing these paradigms. But more thorough analysis of the situation reveals the opposite. A language that is neutral relative to both paradigms has already been cultivated in the modern theory of gravity. This language is by no means the neutral observation language of logical empiricists. Its rich vocabulary consists of *theoretical terms* of notions of theories that are “classical” relative to relativistic theories of gravity. Relativistic theories of gravity “contact” reality through the classical ones. The neutral comparison language was developed in the beginning of the 70’s by a group of prominent specialists in relativistic astrophysics at the California Institute of Technology under the supervision of Kip S. Thorne. This group included D. L. Lee, A. P. Lightman, W. T. Ni, C.M. Will and some others. The title of their basic “Physical Review” paper - “*Foundations for a Theory of Gravitation Theories*” - speaks for itself. But what does the neutral language, constructed “by trial and error”, looks like? – “In order not to prejudice ourselves, the language and concepts used in this calculation will be those employed in standard classical field theory with gravity treated as just another ordinary field. In particular, we will not use such phrases as ‘ curved space-time’ and will not make any coordinate transformations to real or pseudo-’freely falling frames’. The concept of gravity as a metric phenomenon should be forced upon us by WEP” ( Lightman & Lee 1973,p.364).Hence, the neutral language for comparing the paradigms consists of the languages of two theories: the special theory of relativity and Newton’s theory of gravity, since, to describe the observation results, it is necessary to use both theories. The Caltech Glossary includes such terms as “gravitational”, ”a local test experiment” ,”a theory of gravity”, ”a relativistic theory of gravity”, “a metric theory”, a “nonmetric theory”, etc. Careful study of the Caltech Glossary discloses that the metric and the nonmetric paradigms are quite commensurable, and significant progress can be made in the successful resolution of the theory-choice problem. Firstly, Will & Nordtvedt (1972) have cultivated a ‘parametrized post-Newtonian formalism’ for comparing metric theories with each other and with experimental evidence. Secondly, in the sixties A. Schiff made a conjecture as a means of choosing between two paradigms: all theories of gravity which satisfy the Weak Equivalence Principle (WEP), i.e. predict a unique composition-independent trajectory for any test body at a given point of space-time and with a given initial velocity through that point, must satisfy Einstein’s Equivalence Principle. i.e. must show that the non-gravitational laws of physics are the same in every freely falling frame. When applied to relativistic theories of gravity, Schiff’s conjecture says that every theory satisfying WEP is necessarily a ‘metric theory’ and belongs to a metric paradigm. Plausibility arguments have frequently been voiced in favour of this conjecture, but there have been few detailed calculations that bear upon its validity or invalidity. In 1973 Alan P. Lightman and David L. Lee of Thorne’s group came up with a method of testing a relativistic theory of gravity on WEP (well founded by the experiments of Eotvos, Dicke and Braginsky).On applying this method to all documented nonmetric theories they could find, Lightman and Lee ruled out all of them (nonmetric theories of Belinfante & Swihart, as well as of Naida & Capella, previously believed to agree with the current experiments).However, it should be pointed out that the theory-choice problem between the metric and nonmetric paradigms was not closed (see Nugayev 1976, for details).As Lightman and Lee correctly pointed out, “Schiff’s conjecture is so sweeping that it will probably never be proved with complete generality.(Such a proof would require a moderately deep understanding of all gravitation theories that satisfy WEP including theories not yet invented. Such understanding is well beyond one’s grasp in 1973”(Lightman & Lee 1973, p.364). It is of no wonder that Kuhn’s second example - from the current *history* of physics now - was shown to be dubious by Alan Franklin (1986, pp.110-113; it is a pleasure to thank professor Franklin for sending me his book on experiments in physics).Indeed, according to Thomas Kuhn and Paul Feyerabend, in different paradigms, terms, describing experimental results, have different meanings, even when the words used are the same. One of Kuhn’s favourite examples cited many times is Einstein’s Revolution. In Newtonian mechanics the term *mass* is a constant, whereas in special relativity it depends on velocity. However, Franklin describes an experiment with colliding billiard balls, in which one of the measured quantities - the angle between the balls’ velocities - is equal to 900 in Newtonian mechanics and is less than 900 in relativistic mechanics. Thus, Franklin demonstrated that an experiment, described in procedural *theory neutral* terms (that may be equally appropriately applied to both competing paradigms), gives different theory-neutral results depending on the paradigm used. Measuring a quantity derived unambiguously distinguishes between the two. Measurement of the angle between the velocities of the two outgoing objects will clearly distinguish between the two paradigms, and demonstrate their commensurability. But again, as in the gravity case, Franklin’s experiment will not be a ‘crucial’ one.

 ”A clever classical physicist might very well accomodate the results by postulating new mechanical effects depending on the motion through ether. These two issues of commensurability and theory choice are often conflated, as exemplified in the statement by Barnes cited earlier. But it is useful to keep them separate and distinct” (Franklin 1986, p.113).

4.2. Like Lakatos, Kuhn does not explain why only *two* paradigms can take part in the competition. Kuhn’s concept permits the competition of *any* number of paradigms. This would not be the case, if the competing paradigms were strictly *alternative.* But they cannot be alternative since they are incommensurable. For example, classical mechanics is incompatible with the theory of Aristotle. Whereas Aristotle’s theory states that there is a force that compels a body to move uniformly and in a straight line (proposition T), classical mechanics denies existence of such a force (proposition T’). Hence these theories are incompatible. But, in order for the assertions (T) and (T’) to be incompatible, the term *force* must have the same meaning in both theories. Otherwise, T and T’ are expressions of different things. The terms’ invariance law must be fulfilled. However, if, as Kuhn and Feyerabend suggest, this law is open to doubt, how can the expressions (T) and (T’) be compared? Paradigms are either incommensurable and compatible, or commensurable and incompatible. 4.3. Even according to Kuhn’s own standards his *structure of scientific* *revolutions* is merely a first approximation. In spite of the fact that Thomas S. Kuhn gave an illuminating and productive analysis of a single paradigm development, his concept provides almost no analysis of the paradigms’ **interaction**, especially of the interaction of several “old” paradigms. However, the history of science is full of cases where three or more paradigms are present in the theory-choice situation (see Howson’s volume, for instance).That is why the model in which only two paradigms are present - one “old” and one “new” is a very idealized , oversimplified description of reality. It is of no wonder that , as a historian of science, Kuhn cannot avoid this issue. In particular, in his masterpiece - “*Black-Body Theory and Quantum Discontinuity,1894-1912*” (1978) he frequently points out that Max Planck was one of the first to understand the necessity of analysing the interrelations between statistical mechanics, thermodynamics and maxwellian electrodynamics. ”An unexpected product of their interaction was the quantum theory, which, during the next three decades, transformed the classical physical theories from which it had developed” (Kuhn,1978, p.3).Is it not incommensurability thesis that hampers the description of paradigm interaction? 4.4. On analysing Kuhn’s Model, it should never be forgotten that he was bothered first of all with demonstrating the unfitness of the “old” Empiricist Model. The author of “The Structure of Scientific Revolutions” is fully aware that he is only indicating the directions of future investigations at best.

”How do the scientists make the choice between competing theories? How are we to understand the way in which science does progress? Let me at once be clear that having opened that Pandora’s box, I shall close it quickly. There is too much about these questions that I do not understand and must not pretend to” (Kuhn 1977, p.288).

Hence Wolfgang Stegmuller’s attempt to develop Kuhn’s concept using a more subtle analysis of the paradigm structure is of special interest. According To J. Sneed and W. Stegmuller, scientific theories can be defined in an “objective” way, independently of linguistic expressions. Any theory can be represented by the pair {S,I}, where I is the set of applications. The structure S consists of a core which contains the mathematical framework M of the theory, a function R related to the distinction between theoretical and non-theoretical terms, constraints C, and various expansions of the core which introduce applications, etc. For this study the most significant consequence of the *structuralist* view is that theories defined in the way described above cannot be refuted. Of course, though the theory itself cannot conflict with facts, it can be a basis for statements that do. The relation between theory and fact is more complicated in the structuralist approach than is assumed in inductivism, falsificationism, etc. All these is fine, but what about theory-change? And this brings us to the crux of Stegmuller’s argument excellently pointed out by Feyerabend (1977). According to Stegmuller, the theory-change process has a practical side which has been wrongly confused with its irrational one.

”The practical side is that a theory, being not a statement but a complex instrument for the production of statements , serves a purpose even if it serves it badly. It is as reasonable to retain a leaky roof until a better roof has been found. Of course, we cannot prove that man should behave in this way, but this is not disadvantage as we are not dealing with a case for which a proof is required. Practical considerations suffice and that is all that can be said”(Stegmuller as cited in Feyerabend 1977,p.358).

We can conclude that structuralist attempts to define Kuhn’s Model more precisely did not eliminate at least one of its shortcomings, i.e. its lack of rational reconstruction of paradigm change. On the contrary - they highlighted this shortcoming. Moreover, while Kuhn appeals for the socio-psychological study of this process, Stegmuller refuses to consider it at all. He does not solve the problem (maybe it was not his aim at all), but merely reformulates it so as to be able to put it to one side with an easy conscience.

**CHAPTER TWO.WHAT IS A THEORY-CHANGE MODEL?**

***Part one. Descriptive versus normative approaches to historical reconstructions***. Not one of the mature theory-change models considered is able to produce a version capable either of effectively resolving the theory-choice situation in the modern theory of gravity, or of theoretically reproducing its predecessor (electrodynamics of moving bodies).What are the reasons for this inefficiency? Logically there can be only two answers: I. The version considered does not exist at all. II. This version has not yet been found. Let us consider both possibilities. The so-called *descriptive* methodology illustrates the hopelessness of searching for an effective theory-choice resolution version. Advocates of the descriptive methodology believe that the methodologist’s duty is to describe the criteria *actually employed* by scientists. They maintain that cultivating methods, norms and ideal cognitive models is a hopeless task. For example, the falsificationist account of scientific history as a process of proposal and subsequent refutation of different theories is unacceptable for a descriptivist not because it contradicts historical data, but because it is a *theoretical description.* The historical situation is described from the point of view of a historian or an epistemologist, but not from the point of view of a real participant in the events described.

 “So far I have argued that the concept of history-of-science explanation is anomalous from the point of view of predictiveness, law-coverability and the “why” principle. I have also argued that the practice of explanation is unsatisfactory in the sense that the most obvious characteristic of historical accounts which are explicitly explanatory is that they are highly unsatisfactory” (Finocchiaro 1973, p.131).

According to the descriptivists, all the shortcomings of history of science are due to the following peculiarity. To interpret their facts, the authors of historical narratives borrow concepts indiscriminately from the philosophy of science. However, the philosophical concepts are unacceptable for the following reasons (Finocchiaro 1973). Firstly, it is essential to distinguish between the *logical-theoretical* structures in the minds of the philosophers and the *logical-historical* ones in the minds of the direct participants in historical events. For instance, the Popperians cannot and do not claim, that Newton believed that his law of gravitation was in any way inconsistent with Kepler’s laws. They do, however, maintain that the law of gravitation is inconsistent with Kepler’s laws, and that consequently Newton was wrong in the respect that he did not believe this to be the case. In other words, the law of gravitation is a refutation of Kepler’s laws only logical-theoretically speaking, and not logical-historically-speaking (Finocchiaro 1973, p.182).The transition from Kepler to Newton is one example where the logical-theoretical structure fails to coincide with the logical-historical structure. If it were just an isolated case, then we might regard the logical-theoretical structure as a good approximation of the logical-historical one. But alas, since historical agents are rational beings, they are more often than not correct. However, the philosophy of science tells us just the opposite. Secondly, the growth of knowledge is inevitable. It makes the epistemologist and historian of science wiser than historical agents. Hence the logical-theoretical structure does not coincide with the logical-historical structure. In this respect a logical-theoretical description is incomplete, since it provides no information about the historical situation. Thirdly, human behaviour depends on human beliefs, and reality plays only a secondary role. This being the case, historical transitions from one scientific theory to another can only be described in terms of the beliefs of historical agents. All three of Finocchiaro’s arguments against the normative methodology are based on his setting of the logical-theoretical descriptions in philosophical minds against the logical-historical descriptions in the minds of historical agents. His arguments would be correct, if these descriptions were complementary, equivalent views held by different men on particular historical events. However, I think that these types of description refer to different *levels of understanding*. The descriptivist picture refers to an *empirical* level, and the normative one to a *theoretical* level. These two levels are inextricably linked. so that the theoretical description shows itself through the descriptive one. The “laws” of scientific methodology exist only as the aspects of individual scientists’ actions. But let me consider the interrelation of theoretical and empirical aspects of methodological studies in more detail. To give a sense of this interrelation I would prefer to consider an oversimplified example from physics - just to catch one important trait that is common for models in natural sciences and in humanities. A simple description of a period of scientific development can at best help to ascertain its empirical regularity. In order to elevate it to the ranks of theoretical proposition, i.e. to give it the status of *universality*, we must reformulate the empirical regularity as an expression connecting certain *theoretical terms*. To be more exact, let me consider an example from Stepin (1976, pp.53-55): Boyle’ law. Let us suppose that Boyle’s equation is derived on the basis of some observations (on the metahistorical level this corresponds to a certain regularity in the theory-choice processes).We consider N gases (investigate the transitions “Ampére-Maxwell”, “Lorentz-Einstein”).Yet no the increase in the number N of gases studied (investigation of the “Planck-Bohr” transition) can ever make the discovered empirical regularity universal. No finite number of observations can guarantee against the regularity being violated in some future observation or experiment. Moreover, such violation is inevitable in the case of gases under high pressures. This case is described by Van der Waals equation. To obtain the law describing the connection between pressure and volume of a gas, the *ideal model* must first be constructed. The set of abstract objects in this model are ideally rigid and infinitesimally small particles , that collide with each other. These particles move according to the laws of classical mechanics, striking the walls of the experimental vessel. Pressure of the gas is defined as the sum force of their blows per unit surface area. Mathematical investigation of relations between these abstract objects makes it possible to *theoretically reproduce* the law **PV= const** previously established experimentally. As a result the inductively obtained expression becomes a law, describing the behaviour of sufficiently rare gases. The equation PV = const is now a theoretical expression. Whilst the form of the equation remains the same, P and V express the relations between abstract objects in a theoretical language, rather than the relations between real empirical gases and their vessels. Thus, since the predictive force of inductive generalizations is probabilistic, the extrapolation of an empirical regularity is not enough to set it above the ranks of hypothetical supposition. The required transition is possible only when the connection between the quantities in the empirical expression is reproduced as a result of operations on the abstract objects that form the ideal model of the particular domain of reality. To obtain the genuine law describing the connection between volume and pressure of a gas, the ideal gas model must be constructed and the empirical regularity derived from this model. This is in no ways to defend the Naive Realist position and to state that the theoretical law obtained is an accurate” picture of reality”. On the contrary, as Nancy Cartwright (1983) correctly put it, in nature there may be no deep and completely uniform regularities. Most of the regularities we have are features of the ways in which we construct theories (and experimental devices - see the next chapter) in order to think about things. As for ideal models, Cartwright emphasizes that in several branches of quantum mechanics one uses a bundle of models of the same phenomena. Nobody thinks that one of this is the whole truth, and they may be mutually inconsistent. They are intellectual tools that help us understand phenomena and build pieces of experimental technology. They enable us to intervene in processes and to create new and hitherto unimagined phenomena (see Ian Hacking’s book for a further discussion). Moreover, our usual idea of approximation is that we start with something true, and, to avoid mess, write down an equation that is only approximately true. But although there are such approximations away from the truth, there are far more approximations towards the truth. Phenomenological laws, obtained as a result of approximating the fundamental laws to concrete situations, are *closer to the truth* than the fundamental ones. Most of the fundamental laws describe extremely oversimplified and idealized situations that can have nothing to do with experimental facts (see the next chapter for details).For instance, the four potential of the electromagnetic field in Maxwell’s equations Ai = (**A,** ϕ) has no direct empirical meaning serving only as a means for mathematical description of electromagnetic phenomena. But if the fundamental idealized models of a theory can be very far from truth, what are they for? - Firstly, they supply one with a theoretical language to describe and to understand new phenomena, new empirical data. Secondly, in the most advanced *natural sciences* they help to predict new phenomena and even to construct experimental device to check the predictions (see the next chapter for details). Similarly, a given resolution of a theory-choice situation obtained inductively at the expense of already resolved theory-choice situations, can only be seen as an empirical regularity. In order for it to be called universal, i.e. to attain the status of theoretical proposition, it must be derived from the *ideal model of the theory-change process.* This model should *theoretically reproduce* the theory-change process by analysing relations between the cognitive being (scientific community in general) and the results of its cognitive activities. The model’s specific nature must be determined by its system of basic abstract objects, which in turn are determined by those empirically-fixed features of the cognition process that necessarily lead to theory-change. The fundamental ideal theory-change model can be as far from the truth as Newton’s fundamental theoretical scheme( describing the relations between the materials points) is. But **partial theoretical model** constructed to analyse concrete theory-change situation is closer to the truth, to “what actually happened” in history of science. By taking into account the peculiarities of the theory-change process under consideration we can construct a sequence of ideal models that are more and more **closer to the truth**. The description of the completed theory-change process, based on the ideal model, will be the history of events that were chosen and interpreted in a *normative* way. Such a description will be a *reconstruction* of real history, since it will contain only those facts of scientific history that are important for the model used. On being applied to the past, the ideal model will determine the *rational reconstruction* of the real theory-change process. And being applied to the present state of scientific development, it will set some *standards* in problem-solving. Thus, analogy between the ideal models in natural sciences and in humanities leads to the interpretation of theory-change models that can better be described in terms of Max Weber’s notions of action, social action and **historical and sociological ideal types.**

 ***Part two.Theory-change models as Max Weber’s ideal types*.** The notion of *ideal type* is central in Max Weber’s methodology of historical and sociological cognition. Ideal type cannot be derived from empirical reality, but is constructed as a theoretical scheme. It is significant that Weber called the ideal types *utopias.*

“The more sharply and unambiguously the ideal types are constructed, the more they are aligned to this world, and the better they fulfil their task, - in terminological and classification, and in heuristic respects as well” (Weber 1964, band 2,p.15).

As was already pointed out, Weber’s ideal types are analogous to ideal models in natural sciences. Ideal types “may be as rarely met in reality as physical reactions that are calculated under the assumption of absolutely empty space”(Weber 1964,band 2,p.10).Such notions as “economical exchange”, “homo oekonomikus”, “ craft”, “capitalism”, “church”, “christianity” are merely ideal constructs, serving as a means of describing real individual historical formations. According to Weber, we must distinguish between the *sociological* and the *historical* ideal types. Both have a “character of some utopia, emerging under conceptual amplification, isolation of certain elements of reality” (Weber 1964, band 2,p.190).But there are important differences. ”Sociology, as has been presupposed many times, creates types notions and looks for general rules of events, unlike history, which seeks to give a causal analysis... of individual, culturally significant actions, formations, persons” (Weber 1964,p.545,band 2). In sociology ideal types are more universal than in history and can be called “pure ideal types”. For example, a sociologist constructs ideal models of dominion that can be found anywhere at any time. Sociological ideal types have two features. (i) The human actions which they encompass are constructed as if they have a place in ideal conditions. (ii) This ideal form is independent of local conditions of place and time. Hence the historical ideal type serves to isolate the causal relationships that occur at least *once*, while the sociological ideal types isolate the relationships that exist *constantly*. Sociology for Weber is a discipline that studies *social actions*. “Action is the name given to human behaviour regardless of whether it is an external or internal act, nonact or suffering, if when and in as much as those who act give it a subjective meaning. But ‘social action’ should be the name given to such an action which in meaning - as implied by those who act - is related to the behaviour of others which in turn maps out its course”(Weber 1964, band 1,p.1).Social action is an “atom” of Weberian sociology and cannot be divided into pieces. Moreover, neither society, nor other forms of collective organizations can be treated as subjects of action. Only individuals can. ”For other (juridical, for instance) cognitive purposes or for practical reasons it can be expedient and simply necessary to treat social formations (“state”, “association”, “company”, “institution”) as exactly that, as if they were single individuals (for example, as if they have rights, duties and - when accused - legal powers. But from a point of view of sociology which gives a comprehensive interpretation of action, these formations constitute the processes and links of individuals only, since the latter are comprehensible for us carriers of actions, having semantic orientation” (Weber 1964,p.10).Thus, Weber does not forbid the application in sociology of such notions as state, family, nation, etc., but he demands we do not forget that they are not subjects of social actions. We should employ the notions of collective thought or collective will only in *metaphorical* sense. Weber describes 4 types of action. Two can be characterized as nonrational, in the sense that they are not the results of deliberate choices. These are *traditional action* , guided by habit, and *affectual action*, produced by emotions. A further two are types of rational action. The first is *wertrational action*, which is undertaken as a result of believe in the ultimate value of acting in a certain way. The second is *zweckrational action*, which is a calculation of the appropriate action to be taken to achieve a desired end. It was Weber’s contention that in modern society zweckrational action, precisely because it is calculable and predictive, becomes the dominating form. This action is the most comprehensible for us since it is: (1) directed towards achieving objectives that are clearly recognised by the individual; (2) employs for this attainment means that are considered adequate by the individual himself. Weber does not consider zweckrational action as universal or even of particular significance in empirical reality. On the contrary, it is an *ideal type of action*, a *paradigm of social action*, a scale for comparison with other types of action. Zweckrational action is a cognitive rather than ontological notion: it is a **means of analysing reality,** but is not reality itself. The real behaviour of an individual is directed towards several types of action; it contains them as its own aspects. Let me now turn to the theory-change model. In complete accordance with Weber, this model constitutes the ideal sociological type of scientist’s action. The sociological type stands in opposition to the historical ideal types used by the descriptivists. The methodological model as a paragon of all the ideal types reconstructs the scientists’ social actions as if they have a place in ideal conditions. And the ideal norms it produces are independent of local conditions of place and time. The ideal methodological models serve as a scale for measuring the deviations from ideal norms and as a paradigm for analysing new theory-change situations occurring in modernity. More important that we must separate *two kinds* of ideal methodological models - the Basic Ideal Model that is very abstract and is extremely far from reality and Particular Ideal Models constructed from the Basic one to analyse the concrete situation just as Weber separated his ideal type of Capitalism from concrete analysis given in his masterpiece on “Protestant Ethik”. It is also important for us that the methodological model describes an aspect of an individual scientist’s activities and not the activities of the scientific community, with the latter constituting the “links of individual persons only”. However, on introducing an ideal methodological model that does not emerge directly from history-of-science data generalizations, we inevitably come up against the following problem. Let us assume that we have two (or more) basic ideal models, two ideal types of theory-change, that can be applied to any theory-change process. As I have already pointed out, to choose between two basic models, we have to compare them as the tools for analysing concrete theory-choice situations. We must compare them as *instruments of understanding*. So, a definite theory-change period should be chosen, and to particular reconstructions of the period produced by different basic models should be compared. But which reconstruction should we choose? Appealing to “facts” can occur pointless, since the facts are often fabricated by the models. Different models can produce different facts. For instance, the falsificationists will stress examples of theory refutation, such as Einstein’ s refutation of Newton’s theory, Maxwell’s refutation of the Weber-Neumann theory, etc. The inductivists will appeal to inductive generalizations such as Kepler’s laws, Faraday’s laws of induction, etc. Even when facts are shared, they are weighted differently from one model to the next. So, how can the disparity between the models and the facts be detected when the facts are chosen by the models? What is the meta-criterion of choice? Applying Max Weber’s evaluation of scientific action as the closest to zweckrational action and his notion of social ideal type, we can conclude that this particular ideal model must be chosen, according to which most history-of-science data can be understood from a unified stance, and most scientists’ opinions about the given period appear to be correct. Moreover, the ability of the model not only to explain established history-of-science facts, but to help to find new ones is also important. Thus, no version proposed can resolve either the theory-choice situation that took place in the past, nor the current theory-choice situation. I maintain that the reason for such deficiency lies not in the general lack of such a version, but in the fact that all the models of theory-change processes, on which the versions are based, are too ineffective instruments for understanding the real process of scientific cognition.

**CHAPTER THREE. A SIMPLE THEORY-CHANGE MODEL.** Careful studies of the development of scientific theories, that have been carried out in the West (N. Cartwright, N. Hanson, G. Holton, I. Lakatos, T. Kuhn, J. Sneed, W. Stegmuller et al.) and in the East (A.V. Akhutin, E.A. Mamchur, N. Kuznetzova, A.A. Pechenkin, V.S. Stepin, V.P. Vizgin et al.) have revealed that the Standard Hypothetical-Deductive scientific deployment Scheme is merely a first approximation. In the real expansion of scientific knowledge , together with the development in mathematical formalism and formally-logical operations with terms and theoretical statements, a very important role is played by the *gedankenexperiments* with theoretical abstract objects. For instance, the small oscillations equation in Newtonian mechanics cannot be derived from the basic equations of motion merely by using mathematical formalism. There we must make a number of important assumptions: it is necessary to define the type of force, to establish a system of reference, etc. (Stepin, 1976. It should be pointed out that gedankenexperiments’ important role appeared to be a revelation for philosophers only and not by scientists themselves - one can only remember Galileo’s billiard balls, Maxwell’s ether models and Einstein’s magnets, conductors and freely-falling elevators). Already the classical mechanics example shows that in the expansion of this theory new properties of the basic theoretical objects are obtained on examining the links between them. These properties can transform into independent theoretical objects and into systems of such objects. Hence the abstract objects of the same mature theory belong to different subsets. The more thorough analysis of mature theory development requires a preliminary look at its structure.

 ***Part one.A mature theory structure***. An abstract theoretical object from a set of abstract theoretical objects of any mature theory belongs either to a subset of *basic* theoretical objects or to a subset of *derivative* theoretical ones. (The content of this part constitutes a brief description of pp. 21-56 of the first chapter of Stepin’s 1976 book, published in Russian).By definition, the relationship between basic objects is described by the fundamental laws of the mature theory. The relationship between the derivative objects is described by the consequences of the fundamental laws. For instance, the *electric field at a point* **E**, the *magnetic field at a point* **H**, and the *current density* **J** are the basic theoretical objects of Maxwellian electrodynamics. The relationship between them is described by Maxwell’s equations. The *material point*, the *force* and the *inertial system of reference* are the basic objects of Newtonian mechanics. The relations between them are described by Newton’s laws. The derivative objects of Newtonian mechanics are : *an absolutely rigid body*, *central field*, *harmonic oscillator*, etc. The relationship between them is described by certain laws of Newtonian mechanics: that is, by the laws of rigid rotation, movement in central field, etc. The basic objects form the **basis** of a mature theory. This means that in order for a derivative object to join the system of theoretical objects it must first be constructed from basic objects according to certain rules. Basic theoretical objects are of independent construction: that is, they cannot be constructed from each other. For instance, the *oscillator*, a derivative object of Newtonian mechanics, is constructed from the basis of Newton’s theory in the following way. It is assumed that the force which changes the state of motion of a material point is a quasi-elastic one. It tends to return the point to a state of equilibrium.T he system of reference chosen is the one in which movement of the material point resembles a periodic movement. Thus the derivative object - the oscillator - is constructed as a basis for deriving the small-amplitude oscillations equation. According to the model considered, one can substitute the quasi-elastic-force expression Fx= - kx into the equation Fx = m (d2x/dt2) obtaining instead m (d2x/dt2) + kx = 0.Here x is the equilibrium state delay, and k is the coefficient of proportionality of the quasi-elastic force. The set of the mature physical theory’s basic objects forms the basis, i.e. a definite *subsystem* of theoretical objects. All the basic theoretical objects are *idealizations*  and cannot exist as real bodies like tables and chairs. For example, the material point is defined as a body free of dimensions. As for the other basic objects of Newtonian mechanics, it is assumed that an *inertial system of reference* can be totally isolated from external influence, though in reality bodies, that can be completely isolated, do not exist at all, even the famous black holes. However, all the basic objects can be compared with certain *fragments of Nature.* *Material points* may be compared with bodies whose dimensions can be ignored in the solving of certain problems (see, for instance, Landau & Lifshitz,1973).The *force* can be compared with the interactions that change the bodies’ states of motion. Hence all theoretical statements about these objects describe natural processes. Therefore the basic objects’ subsystem is an *idealized model* of the domain of reality described. For instance, in classical mechanics, correlations between basic objects form an ideal model of mechanical movement. This model represents mechanical processes as displacements of material points throughout the point continuum of the inertial system of reference. According to the model, the change in the material point state of motion is determined by the action of the force. The equations describing the relations between basic theoretical objects are considered now as the *“Laws of Nature*”. Accordingly, the *partial theoretical laws*, describing relations between derivative objects, obtain the status of *particular models* of the domain of reality considered. Hence all the derivative objects of a mature theory should also be organized into subsystems. Theoretical statements must not contradict each other. Therefore, with the introduction of a new object, existing ones should not acquire new properties, that are incompatible with those they had in the past. The derivative subsystems are subordinated to the basic one, but are independent from each other, referring to different fragments of the same domain of reality. Each subsystem is characterized by its own set of notions and mathematical equations that form a special part (section) of the mature theory. For instance, classical mechanics consists of several independent sections: small-oscillations mechanics, mechanics of rigid body rotations, mechanics of movement in a central field, etc. Each of these sections is characterized by its own subsystem of derivative objects. Each subsystem is a model of a particular type of mechanical motion (the small oscillations model, the rigid rotations model, etc.).Relations between the subsystem’s elements are described by particular laws of classical mechanics. In general, the relations between a subsystem of basic objects and a subsystem of derivative ones can be described in the following way. Each derivative system is obtained from the basis by a process of **reduction**. It means that any mature theory is developed not by formal - logical, mathematical - means only, but also through gedankenexperiments with abstract theoretical objects. The reduction is put into effect by analysing the *specific character of the empirically fixed domain of reality*. This domain can be “seen through” the prism of an ideal model, formed by correlations of basic objects. According to the peculiarities of each concrete experimental situation, various restrictions may be imposed on the system of basic theoretical objects. This enables one to define the system, transforming it into a subsystem of derivative objects. The fundamental equations are then applied to the subsystems of derivative objects. In accordance with the system specific features they are transformed into the partial laws. The informal nature of such manipulations converts such an inference into a special problem solving operation. The solutions of such problems are included in a theory at its origin. To a theoretician bothered by applying a theory, they serve as a *pattern* for subsequent activity. Each problem is solved in accordance with primary **paradigms** (in Kuhn’s sense). In classical mechanics, the paradigm examples consist of “derivations” from Newton’s laws: the small-oscillations law, the movement in a central field law, the rigid body rotations law, etc. In classical electrodynamics the paradigm examples are the laws of Biot & Savart, Coulomb, Ampére, Faraday et al., derived from Maxwell’s equations. The construction of derivative objects from basic ones enables one to compare theoretical knowledge with experience, to explain the results of real experiments. To this end, an empirical equation - an intermediate relation - is derived from the partial law. In this equation the special constructs are introduced. In contrast to abstract objects, the newly born constructs are no longer idealizations and can be compared with real bodies now. These constructs are called *empirical objects* , and their systems - special representations of empirical situations - are called *empirical* s*chem*es. Empirical objects are not equivalent to real bodies. An empirical object cannot be compared with a single body with which an experimentalist operates, but only with a *class* of such objects. Consequently, an empirical scheme corresponds not to a concrete experimental situation, but to a *type* of such situations. For example, the empirical scheme of the Biot & Savare experiment with a magnetic needle and a conductor refers to any experiment with any current in the conductor and any small magnetic needle. NOTE. I have introduced the notions *basis, basic object, derivative object, constructive independence* to generalize the notions **element of Fundamental Theoretical Scheme & element of Partial Theoretical Scheme,** introduced by Vyacheslav S. Stepin (1976).Consequently, Stepin’s notions can be extrapolated on theories with ideal models that are not generalizations of experimental situations. Any element of a Fundamental Theoretical Scheme is at the same time an element from mature theory basis, but not vice versa. This is also the case with the relationship between partial theoretical schemes and derivative subsystems. ***Part two.The Cross-Contradiction Genesis*** . Completion of any mature theory T1 (i.e. the derivation of its fundamental laws) inevitably gives rise to questions about the relation of T1’s basis, **B1** , to the system of basic objects **B2**of another mature physical theory T2.Are basic theoretical objects from both theories constructively independent? Or is it likely that **B1** belongs to a subsystem of derivative objects of T2 (or vice versa)? It is impossible to answer these questions without taking into account the following constructional peculiarities of the derivative objects. (1) The rules for building derivative objects from the basis are not clearly formulated algorithms. They are vaguely determined by the problem-solving examples or paradigms included in the theory at its origin (Kuhn, 1969). (2) Application of these rules for reducing the basis to a subsystem of derivative objects takes the peculiarities of empirical reality for granted. These peculiarities vary from one field to another. (3) When the physical theories are different, the construction rules differ from each other, being determined by different paradigms. Points (1) to (3) demonstrate how difficult it is to show that T1 is subordinated to T2.Therefore, in everyday scientific practice, the simple conjunction of B1 and B2  is assumed to form a new basis. The true necessity of analysing the interrelations between B1 and B2 emerges only when both theories are needed to explain certain experimental data. It is assumed that experimental data can be described by a system of derivative objects constructed from the basic objects of both theories. Such derivative objects will be called (using Bransky’s notion) *crossbred objects* or simply *crossbreeds*. This, of course does not stop scientists from trying to justify a mature theory from the very moment of its creation, by subordinating it to another mature theory. The most vivid example is Maxwell’s and Boltzmann’s attempts to justify the electromagnetic field theory building mechanical ether models. However, these attempts were necessary because of their metaphysical standpoints. They ‘ontologized’ the dominant theory’s basic objects to give them the status of Weltanschauung’s Objects. It was believed that only Weltanschauung Abstract Objects represent “reality”. All scientific theories were believed to be true only in as much as they accorded with the “True Weltanschauung”. However, such attempts do not make an essential impact on the real process of explaining and predicting empirical data. The crossbred objects are constructed from the T1 basic objects. Consequently, the crossbred system will be a subsystem of T1 derivative objects. On the other hand, the crossbred objects also are formed from the T2 basis. Hence the crossbred system will simultaneously be the crossbred system of T2. Consequently, relations between crossbred objects will be described by the partial laws of both T1 and T2.Several “domains of reality” can exist, the description of which may necessitate the joint application of two mature theories.(For the sake of simplicity I shall only speak of two theories; in reality three or more theories are applied, but all these cases can be reduced to 2-theories ones).Hence several crossbred systems may exist simultaneously. The joint application of T1 and T2 for solving a problem will be called, using Podgoretzky & Smorodinsky notion (1980), *theories’ cross,* while T1 and T2 will be referred as *cross theories*. The set of statements describing the relations between the crossbreeds will be referred as a *crossbred theory*. For instance, the completion of Maxwellian electrodynamics gave rise to questions about the relationship between its basis and the system of basic objects of Newtonian mechanics. The task of theoretically describing a black-body radiation spectrum, the electromagnetic radiation process, and the construction of a theoretical model of an atom , demanded the joint application of both theories (see, for instance, Poincare’s course of lectures “Electricite et Optique”, Paris,!890).The best illustration of this statement is the following quotation from Max Planck’s paper:

“Nowadays, (the following ) two significant fields are set against each other: mechanics and electrodynamics, or, as they are sometimes called, the physics of matter and the physics of ether. The first includes acoustics, heat and chemical processes; the second includes magnetism, optics and radiant heat. Is this subdivision final? I don’t think so, mostly because neither of these fields of investigation are divided by strict and firm lines. For instance, does radiant heat belong to mechanics or to electrodynamics? Or to which field must the law of electron movement be attributed?” (Planck 1906, p.616).

 Let us consider the examples mentioned above more thoroughly. (a) In order to theoretically describe a black-body radiation spectrum, J. Jeans (1905) investigated the system of standing electromagnetic waves in a closed cavity. By treating such waves as a system of harmonic oscillators (the construction of crossbred theoretical objects) he was able to use a well-known law of statistical mechanics (the equipartition theorem).In this way the dependence of black-body radiation energy on temperature and frequency was discovered. The system of crossbred theoretical objects, the correlations of which form a model of black-body radiation, is a subsystem of classical electrodynamics (i.e. the system of standing electromagnetic waves).On the other hand, this same model forms a subsystem of derivative objects of classical mechanics (a mechanical system with infinite number of degrees of freedom). (b) Lorentz’s Theory of Electrons, which explained and predicted a large number of phenomena referring to the electrodynamics of moving bodies, provides a classical example of a crossbred theory. Initially, following the traditions of Maxwell and his disciples (Lenard, Hertz), it was assumed that the charges could be considered as a kind of perturbations of the ether. This assumption was based on the key idea of Maxwell’s electromagnetic theory, that displacement current is identical to conduction current. From here he was able to represent the density of electric current in the form of electromagnetic field flow through a cover. But, under the influence of atomistic ideas, Lorentzian electrodynamics was based on the notion of charges’ currents as a system of electrons interacting with the electromagnetic field. Being a system of moving particles, the system of electrons is a classical mechanics subsystem. But, as a system of electromagnetic field sources, it is also a subsystem of Maxwellian electrodynamics. © The hypothesis of atomic structure, which assumes the existence of atomic nucleus, was proposed by Nagaoka in 1905 - *before* Lord Rutherford’s experiments with alpha-particles. When creating planetary model of an atom, the positive charge was defined as an “atomic nucleus”, and electrons were described as being “on a stable orbit round the nucleus”. The system of crossbred theoretical objects, the correlations of which form the planetary model of an atom, is a subsystem of classical mechanics and a component of “motion in the central field” model. On the other hand, this system is a subsystem of classical electrodynamics (Hertz’s oscillator).

 “The atom’s rigidity and the puzzling constancy of all their properties in general, - on the one hand, - and the lack of any radiation, - on the other hand, - indicate that in spite of acceleration, the normal motion of the electrons in atoms, contrary to the requirements of classical mechanics, is not accompanied by the emission of inductive pulses of radiation or by ‘internal friction’ that is connected with it. Thus, this normal motion has a conservative nature and consequently, at least in the first approximation, obeys the laws of classical mechanics” (Frenkel 1923,p.8).

On the other hand, “normal conservative motion of electrons in atoms can give way to regressive motion that is accompanied by an emission of electromagnetic pulses and by internal friction, i.e. that obeys - again in first approximation - the laws of classical electrodynamics” (Frenkel,1923,p.8). Relations between the crossbred objects are described by T1 and T2 statements. The crossbred objects belong to the subsystems of both theories. Hence***the crossbred construction operation is identical to that of endowing derivative objects, belonging to both theories, with new properties.*** These additional properties of the first theory’s objects correspond to a new set of relationships transported from another mature theory.

Systems of derivative objects of each cross-theory were constructed before they had encountered. Each of them is a summary of corresponding experimental studies carried out independently of investigations connected with another mature theory. Therefore, it is not surprising that, as a result of the formation of crossbred objects, *theoretical objects* *with mutually incompatible properties* occur in the same subsystem of derivative objects of one of the crossbred theories. In the above case studies the appearance of objects with mutually incompatible properties was characterized by physicists as “ultraviolet *catastrophe”* (Paul Ehrenfest’s notion), “the *paradox* of unstable atom” (Wilhelm Wien’s notion), etc. Let us consider those paradoxes in more detail. (a) As a result of constructing the crossbred system. forming the blackbody radiation model, the free electromagnetic field appeared to possess two additional properties: “to be a system of standing electromagnetic waves” and “to be a mechanical system with infinite degrees of freedom”. Indeed, from the mechanical point of view, the electromagnetic field is a field of electric and magnetic forces that are continuously diffused throughout space. Such a formation has infinite degrees of freedom, in contrast to the finite number of degrees of freedom of a usual body. The material point has 3 degrees of freedom, and its position is determined by 3 coordinates, while the electromagnetic field at a point is determined by all the electromagnetic forces throughout space, i.e. by an indefinite number of magnitudes. Independently of Rayleigh and Jeans, by making use of classical statistics, Einstein demonstrated that at an arbitrary but finite temperature, the density of the electromagnetic field energy should be infinite. This is quite natural, since at an arbitrary finite temperature, the same amount of energy (proportional to temperature) falls on each degree of freedom. However, an infinite electromagnetic field density is incompatible with the second principle of electrodynamics which is based on a statistical-mechanical point of view. One can always extract energy from the cavity containing such radiation and set “perpetuum mobile” of the second kind to motion. Therefore, being a mechanical system with an infinite number of degrees of freedom, the thermal radiation property is incompatible with its property as a “system of standing electromagnetic waves”. (b) As was later emphasized by Albert Einstein, “the weakness of the theory lay in the fact that it tried to determine the phenomena by combining partial and exact differential equations. This method is unnatural. The insufficient part of the theory manifests itself in the admitting finite dimensions of elementary particles and besides, in the necessity of evading the fact that the electromagnetic field on their surfaces should be infinitely great. The theory was unable to explain the tremendous forces that hold charges together on the surfaces of elementary particles” (Einstein 1936). © The planetary model of an atom was constructed as a system of crossbred objects. As a result, the electron was found to have a new property imported from classical mechanics:” to move in a circular orbit round the nucleus”. But, like any accelerated charged particle, the electron must continuously radiate electromagnetic energy. The energy loss should lead to the electron collapsing on the nucleus. Therefore, the electron property “to move in a stable orbit round the nucleus” is incompatible with the property “ to be a negative charge inside the atom”. This paradox, which first appeared in the Nagaoka model, was retained in the Rutherford model. Because of this paradox the scientific community was rather sceptical about the Nagaoka model, but was forced to accept the model of Rutherford because of the experimental evidence. The system of theoretical statements contains expressions regarding the relations between abstract theoretical objects. Therefore, the objects with mutually exclusive properties in the system of derivative objects, should give rise to mutually contradictory statements in the cross-theories. Bearing in mind Podgoretzky and Smorodinsky’s notion(1980), I would like to denote the appearance of incompatible statements when the theories cross by *cross-contradiction*. There are many examples of such cross-contradictions in the black-body radiation theory, in electrodynamics of moving bodies and in planetary atom theory. (a) “Heat equilibrium of radiation with matter exists” (theorem following from the second law of thermodynamics, see Planck 1906) and “heat equilibrium of radiation with matter does not exist” (consequence of Rayleigh-Jeans law, see Lorentz 1909).One participant at the first Solvay conference (1911) remembered later: “This argumentation, analogous to Jeans’s thermal equilibrium analysis, led, however, to the well-known paradoxical result according to which no thermal equilibrium is possible” (Niels Bohr). (b) “What causes all these difficulties? Lorentz’s theory contradicts the purely mechanical notions to which the physicists hoped to reduce all the phenomena of the Universe. Indeed, while there is no absolute motion of bodies in mechanics, and there exists a relative one only, in Lorentz’s theory there is a peculiar state corresponding physically to an absolute rest state: the state when a body is immobile relative to the ether” (Einstein 1910). © “The atom is stable” and “the atom is unstable”. ***Part three.Eliminating the Cross-Contradictions***. The cross-contradiction results from the crossbred-object construction. To eliminate the cross-contradiction, theory T3 must be created, comprising both cross-theories so as to exclude the possibility of constructing crossbreeds from the basises of both theories. Theory T3 will be called a *global* theory. According to the methodological model I am developing, there are two ways of creating a global theory: *reductionist* and *synthetic*. ® Application of a reductionist method of creating a global theory is based on the assumption that the basises of both cross-theories refer to different levels of theoretical object organization. Hence D1, the domain of validity of T1, is a part of D2, the domain of validity of T2.The basis of T2 is called a “true” basis. And T2 itself is declared a “**fundamental**” theory, while T1 a “phenomenological” one (Tisza,1963). The authenticity of a phenomenological theory must then be established by constructing its basic objects from the basis of a fundamental theory and providing that its main laws follow from those of a fundamental theory. Lastly, the basis of a phenomenological theory takes its place as a derivative system of a fundamental theory. The possibility of constructing a phenomenological basis from the basis of a fundamental theory must also be substantiated. The relation between a fundamental theory and a phenomenological one is identical to that between the classical mechanics and one of its parts - the rigid body rotation theory, for instance. The problems of constructing a phenomenological basis from the basis of a fundamental theory are of special significance for a reductionist programme. They are called *fundamental problems* (Laszlo Tisza, 1963; it is a pleasure to thank prof. Tisza for criticism and helpful advice). Classical mechanics, classical electrodynamics, quantum mechanics, quantum electrodynamics, quantum field theory, general relativity were all in turn given the status of “fundamental theory”. Accordingly, reductionist development in the history of physics is linked with the names of Ampére, Weber, Neumann, Riemann, Ritz, Fokker et al. (reduction of electromagnetism to mechanics), of Lorentz, Langevin, Wiehert, Drude, Abraham, Kaufmann, Mie, Wien et al. (reduction of mechanics to electrodynamics), of Einstein, Weil, Cartan, Wheeler et al. (reduction of physics to geometry in the unified field theory - see Wheeler,1988; it is a pleasure to thank prof. John A. Wheeler for helpful advice and for sending me his papers). © The synthetic method of creating a global theory is based on the following assumption. The basic objects of both cross-theories are constructively-independent of each other. Their basises belong to the same object-organization level. Hence, cross-contradiction must be eliminated by creating a system of global objects from which the basises of both cross-theories can be constructed. The fundamental laws of both cross-theories should be deduced from those of the global theory. Finally, the basises of T1 and T2 take up their positions as derivative subsystems of the global theory. What are the differences between reductionist and synthetic methods of creating a global theory? First of all, realisation of the reductionist programme rules out the use of crossbred objects. They are simply rejected by such a programme. In contrast, the synthetic programme merely rules out using the crossbreeds as derivative objects of the cross-theories. *This programme considers the crossbreeds as belonging to the highest theoretical systems’ organisation level* (see chapter four of this study for details).Moreover, application of the synthetic programme results in the creation of a new system of abstract theoretical objects. The rules of reducing the basis to derivative subsystems are determined in the global reductionist theory by puzzle-solving examples contained in the fundamental theory. In contrast, there are no rules for constructing the basic objects of a synthetic global theory. Hence a synthetic global theory can be built by unifying consecutive cross-basises according to a new World Picture (the most vivid example is creation of Maxwellian electrodynamics). Secondly, synthetic global theory can be formed using mathematical hypothesis method (the creation of general relativity).In the latter case, construction of a theory begins with a search for fundamental equations, and only when they have been found can philosophical interpretation and empirical justification begin. For this reason, 20-th century ways of building a theory differ radically from those of the past. Use of the described ways of eliminating cross-contradiction is based on two *equally reasonable*, but **mutually incompatible** assumptions. Therefore, these methods must be used in *alternative* global theory construction programmes - two reductionist and one synthetic. Since several crossbred systems can be created simultaneously, several synthetic s*ub-programmes* can exist at the same time.(For instance, Bohr’s and Einstein’s sub-programmes were carried out relatively independently, but subsequently merged into a single one). Each programme creates its own sequence of scientific theories on the basis of one of the above assumptions. Each theory from the sequence is produced by adding an auxiliary hypothesis to the preceding one. Each of the propositions considered above leads to specific assumptions exclusive to the given programme. Therefore, following the example of Imre Lakatos, I call these fundamental assertions the *hard cores of reductionist and synthetic research programmes*. On examining his “Falsification & the Methodology of Scientific Research Programmes”, the good sense of Lakatos’s concept becomes obvious.

”For instance, Prout never articulated the ‘Proutian programme’: the Proutian programme is not Prout’s programme.It is only the (internal) success or the (internal) defeat of a programme which can be judged only with hindsight: it is frequently also its content” (Lakatos 1970, p.119).

 The following statements constitute the core of the synthetic scientific research programme (SSRP):” the basic objects of all the cross-theories are constructively-independent”. The opposite is true of the reductionist SRP (RSRP) .In addition, the reductionist “hard core” indicates which theory’s basic objects constitute the true basis. RSRP content is specified by its own Weltanschauung (Special World Picture).These Special World Pictures are sometimes called *Pictures of Physical Reality*. They are the canals through which metaphysical principles influence the theory construction process. Neither a single *crucial* experiment, nor a sequence of such experiments can definitely show which programme - reductionist or synthetic - is able to successfully eliminate the cross-contradiction. For example, it can be concluded that a reductionist programme is unable to eliminate the contradiction, only if solving its fundamental problems is shown to be impossible. Hence each “hard core” is *irrefutable*. In SRP methodology the role of the hard core can be played by any metaphysical proposition that is “irrefutable by the methodological decision of its protagonists” (Lakatos 1970, p.134).Thus Lakatos’s hard cores are irrefutable *by convention*. Each (n+1)- th version of a reductionist or synthetic sequence of theories represents a more perfect realization of a programme than the n-th version. Each of these sequences tends to a certain limit or *ideal* of the global theory. It is this ideal which “determines” the direction of development of each SRP type. The third feature of a programme that enables it to develop successfully is the so-called *protecting belt of auxiliary hypotheses* around the core, against which the *modus tollens* are redirected. The term “protective belt of auxiliary hypotheses” was introduced by Imre Lakatos to highlight its main function: “to bear the brunt of tests and get adjusted and re-adjusted or even completely replaced to defend thus a hardened core” (Lakatos 1970,p.133).In its final form this notion is not altogether appropriate for my model since reductionist and synthetic hard cores are irrefutable as means for eliminating cross-contradiction. Nevertheless, I maintain Lakatos’s term guided by *Ockam’s ranzor*. In the model proposed, *protecting belt* is a set of hypotheses that guarantees concordance between the SRP theories and new experimental data. Indeed, the protective belt of the reductionist SRP consists of a number of assertions describing the relations between the abstract objects of a fundamental theory. The protective belt of a synthetic SRP “does not actually emerge fully armed like Athene from the head of Zeus. It develops slowly by a long preliminary process of trial and error”. In this book the process is not discussed. Some auxiliary hypotheses setting out the order of research are important if the SRP is to develop successfully. Following the example of Lakatos, I shall call this set of hypotheses a *positive heuristic* of SRP. The SRP positive heuristic consists of a set of rules for building derivative objects from the basic ones. The SRSP positive heuristic consists of the laws governing the crossbreeds’ construction and of the laws for transforming those systems into the global theoretical scheme. Thus, my model can distinguish a *hard core* from a *positive heuristic*. In Lakatos’s model, the boundary between these important SRP features is too vague. The positive heuristic of Lakatos’s SRP consists of metaphysical principles that are more flexible than those of the hard core. Hence it is the methodologist himself who decides which metaphysical principles to attribute to the positive heuristic and which to the hard core. This further increases the arbitrariness of the hard core, since Lakatos permits scientists to alter the content of a positive heuristic. Moreover, “it occasionally happens that when a research programme gets into a degenerative phase, a little revolution or a creative shift in its positive heuristic may push it forward again” (Lakatos 1970, p.137).A methodologist working with Lakatos’s historiographical research programme can always characterize an arbitrary sequence of theories as a progressive programme, having reconstructed its hard core and positive heuristic in an appropriate way. The series of works by Bohr and Sommerfeld on the so-called *Old Quantum Theory* (1913-1916) is a classical example of real progressing SRP. These works belong to the Bohr sub-programme that merged with Einstein’s sub-programme of eliminating the cross-contradiction between mechanics and electrodynamics (1905).This cross-contradiction was revealed by Poincare, Jeans, Rayleigh and Einstein in 1900-1905.The first of Einstein’s papers on quantum theory is based on the hard core of synthetic programme (1905a).The protective belt of the programme was self-crystallized during the course of its development. The positive heuristic (or the research strategy) of the Bohr sub-programme was shaped by the idea that atoms are analogous to planetary systems. According to Bohr’s plan, it was necessary first to develop a theory for the hydrogen atom. His primary model B1 presupposed a stable proton nucleus with an electron on a circular orbit. In his second model Bohr intended to calculate the elliptical orbits in fixed planes. He then aimed to eliminate the artificial limitations of a fixed nucleus and elliptical orbits. The next step would be to consider a situation whereby electrons rotate not only around the nucleus, but around their own axis as well. Afterwards Bohr intended to describe complex atomic and molecular structures and their behaviour in an electromagnetic field. According to Lakatos, “Bohr’s problem was not to explain Balmer’s and Paschen’s series but to explain the paradoxical instability of the Rutherford atom. Moreover, Bohr had not even heard of these formulae before he wrote the first version of his paper” (Lakatos 1970, p.147). However, the following quotations from the works of one of Bohr’s contemporaries - the well-known Russian physicist V.Ya. Frenkel - leads me to make a stronger claim: Bohr’s main problem was the elimination of cross-contradiction between electrodynamics and mechanics disclosed in Nagaoka-Rutherford planetary-atom model. Indeed, “the modern quantum theory created by Danish physicist Niels Bohr, is a new system of dynamics that is a kind of compromise between the classical system of the Newtonian mechanics and Lorentz-Maxwell-Einstein electrodynamics that replaced it at the beginning of this century. Therefore, to grasp the essence of quantum theory, it is necessary to consider the contradictions between classical mechanics and classical electrodynamics which it tries to reconcile” (Frenkel 1923, p.3).But how do these contradictions manifest themselves? Firstly, “the notion of energy is central to the notions both of mechanics and electrodynamics. However, the substance of this notions in both cases differs” (Frenkel 1923, p.3). Secondly, in classical mechanics attractive and repulsive forces are transferred from one particle to another instantaneously. On the other hand,

“as is well-known, the forces between the charged particles (the electrons) do not satisfy this requirement. Regardless of their nature, they ae transported through space with finite velocity c = 300000 kilometres per second, i.e. with the velocity of light through vacuum. Thus, the force acting on a particle at a given moment in time, depends upon the particles’ positions at the preceding moments determined by their conditions of movement. The charged particles are influenced not only by electrostatic repulsive and attractive forces but also by other force which depends on their velocities and accelerations “ (Frenkel 1923,p.4).

Thirdly, “from the formal side”, the main law of motion in classical electrodynamics coincides with that of classical mechanics. However, “when the force definition becomes more strict this coincidence disappears” (Frenkel, 1923, p.5). Finally, the “energy conservation law, in its mechanical sense, has no place in classical electrodynamics” (Frenkel 1923, p.7). As a result, Frenkel concludes that “classical mechanics and classical electrodynamics represent only two approximations to reality, from which they decline to diametrically-opposed sides, - since the first rejects radiation and regressive motions completely, while the second rejects all conservative motions. The true dynamics of atoms and of material bodies in general must be a sort of synthesis or - more accurately - a compromise between them. The necessity of such a compromise was first understood by Bohr. In his new theory of dynamics he retained the notion of mechanical energy as a function of electron coordinates and velocities; mechanical energy is added rather than replaced by electromagnetic energy” (Frenkel 1923, p.8). But did the Bohr theory fulfil its purpose? - “Without explaining details that will be cleared up by further exposition, we have merely to note that, in spite of its enormous achievements, Bohr’s quantum dynamics has a purely qualitative character (especially relative to regressive motions), representing, as has already been pointed out, no more than a sort of compromise between classical mechanics and classical electrodynamics, rather than their natural synthesis which continues to be one of the most essential problems of modern physics” (Frenkel 1923,p.18). The ideal of the Einstein-Bohr programme was determined by the correspondence principle, which “endeavoured to utilize to the utmost extent the concepts of the classical theories of mechanics and electrodynamics, in spite of the contrast between these theories and the quantum of action”(Bohr 1922).The detailed description of the sub-programme development has been given already (Lakatos 1970).

 ***Part four.Occurence of theory-choice situations***. If a programme is finished successfully, and the global theory is created, say, in a reductionist way, it cannot be created by synthetic means. Otherwise it would be possible to construct phenomenological theory basis from the same basic synthetic global theory system for totally different “domains of reality”. Nevertheless, suppose that all three programmes are successfully realised and three global theories (two reductionist and one synthetic) are created simultaneously. Let us compare them. All the ideals contain both basic objects of T1 and basic objects of T2.The domain of validity of each ideal contains the domain of validity of T1 together with the domain of validity of T2.The subsystems of theoretical objects of each ideal contain the same derivative objects of both cross-theories. The relations between basic and derivative theoretical objects are described in each ideal by the same equations, i.e. by the partial and fundamental laws of both cross-theories.E ach ideal describes, explains and predicts the experimental results, using the languages of the same partial theories belonging to both cross-theories. Any verification (or refutation) of the reductionist global theory is also a verification (or refutation) of the synthetic global theory. Any consequence from the reductionist global theory may also be obtained from the synthetic one. Likewise, any consequence from the synthetic theory, referring to T1 and T2 domains of validity, can also be derived from the reductionist global theory. Therefore, with respect to both cross-theories’ domains of validity, the limits of all alternative programmes are *empirically-equivalent*. In fact, different global theories are distinguished by their different ways of organising the same objects of both cross-theories into a single entity. For instance, the consideration of the point of origin of theories of gravity reveals that the metric theories are empirically-equivalent to nonmetric ones, because metric and nonmetric rival programmes (or paradigms) are in fact two different programmes of unification for the same cross-theories. The metric theories, as well as nonmetric ones, describe, explain and predict new experimental data using the languages of the Special Theory of Relativity and Newton’s Theory of Gravity. This is why the terms of both cross-theories form the Caltech Vocabulary of *neutral observation language* for comparing the rival programmes (see the first chapter for details). But it is impossible to attain more than one ideal. Only *one* of these strictly alternative sequences will tend finally to its limit. That is why, in real “alive” history of science, one can register the simultaneous existence of theories from sequences that belong to unfinished alternative programmes. Theories from *unfinished* programmes cannot be strictly empirically-equivalent with respect to each other. They are only approximately empirically-equivalent. For example, the theories from metric and nonmetric programmes are empirically-equivalent in respect to 3 critical General Relativity tests: the bending of light in the gravitational field of the Sun, the red shift of spectral lines and the anomalous behaviour of the Mercury perihelion. But almost all metric theories contradict WEP (the results of the Dicke-Braginsky experiment).On the other hand, in the domain of gravity field quantisation nonmetric theories supersede metric ones. Quantization of the gravitational field within a nonmetric programme causes no substantial difficulties at all. Moreover. Many nonmetric theories were specially designed for this. The majority of nonmetric authors are specialists in quantum field theory (to mention only Pauli, Thirring, Feynman, Deser, Weinberg, Belinfante, Logunov ).The abstract of a typical paper on nonmetric theory begins: “the Lorentz-invariant linear field theory constructed that is easily quantized”(Belinfante & Swihart 1957,p.168).On the other hand, within the General Theory of Relativity it is unclear whether or not even to mention the existence of gravitational waves. Moreover, in metric theories the gravitational field is satisfactorily quantized only in the so-called *linear approximation* ,i.e. in the approximation of the weak field. However, it is only in strong fields that the effects of quantum gravity are significant. *It is the fact that theories from unfinished alternative programmes can co-exist simultaneously that was registered in the philosophy of science as the theory-choice situation.* The methodological analysis given enables me to propose a solution to the well-known *equivalence paradox* (Moore, 1922; Malcolm, 1940; Hanson, 1951, 1961, 1964) explained below. For a theory H1 to be equivalent to theory H2, H1 must be deducible from H2, andH2 must be deducible from H1(Hanson,1961).This is the case when H1 and H2 are empirically-equivalent , i.e. when any confirmation of H1 is equally a confirmation of H2 (and vice versa), correspondingly, each refutation of H1 is a refutation of H2 (and vice versa).However, if H2 follows from H1 and H1 and H2 follows from H1, then they are one and the same theory (Wittgenstein).But if Wittgenstein is correct in characterising H1 and H2 as being identical, then there is nothing left between which to establish equivalence. It seems to me that there must be an alternative way of viewing the matter. Moor provides such an alternative. Two theories can remain separate, while yet being such that the one entails the other, and vice versa. If the very concept of theoretical equivalence is not to disappear into that of theoretical identity, then there must be a sense in which H1 and H2 remain logically distinguishable. Malcolm argued (1940) that H1 and H2 may be equivalent, but still have different meanings. He maintained that if H1 contains expressions that are not synonymous with any expressions or combination of expressions to be found in H2, then they cannot be reduced to a single theory, but remain equivalent. The examples usually cited to demonstrate such equivalence are matrix mechanics & wave mechanics (as these theories were understood in 1926),Ptolemy’s & Copernicus’s astronomies, Newton’s corpuscular theory of light & Huygens’s undulatory optics (18-th century).But in the case of nonsynonymous expressions the following question inevitably arises.

“As suggested in my earlier paper, one seems to have two different theories, dealing (presumably) with non-identical subject matters, which are yet mysteriously connected via the entailment relation. How can disparate factual subject matters be necessarily connected by logical equivalence? How, by inference alone, can one pass from one characterisation of a subject matter to a different characterisation of a (presumably) distinguishable subject matter, and then back again?” (Hanson 1966, p.419).

 According to the study I have developed, reductionist and synthetic limits of alternative sequences may, indeed, be empirically-equivalent, but only *homomorphically.* Any confirmation (or refutation) of the reductionist global theory is simultaneously a confirmation (refutation) of the synthetic global theory; but the opposite, however, is not true. The reductionist and synthetic ideals are homomorphically identical; all the notions of reductionist global theory are contained in the synthetic one. In this sense these two theories are identical: they are the same global theory. Yet they can never be realised simultaneously: the realisation of one ideal prevents the realisation of another. Consequently, in the real history of science, only *partially* -equivalent theories can be found. These real theories have the same empirical consequences only in certain, limited empirical domains. Outside these domains, theories from reductionist and synthetic programmes explain and predict *quite different* results. Therefore full empirical equivalence is impossible in real scientific practice. The Paradox of Equivalence occurs in oversimplified model of scientific development. Nevertheless, the occurrence of partially-equivalent theories at least sometimes can be explained on the basis that nonsynonymous theories contain the same cross-theories as their partial ones. Consequently, they use the same theoretical notions to describe experimental data. As a conclusion, the author can state that he is far from giving the one-for-all cases resolution of the old paradox. My aim is more modest - to propose a solution that works in some cases. The question of applying their solution to other cases described by Norwood Hanson is left open.

**CHAPTER FOUR. COMPARING THE MODEL CONSTRUCTED WITH REAL PROBLEM SITUATIONS.**  ***Part one.The problem situation genesis***. Can *any* anomaly create a problem situation? - Obviously not. Otherwise science could not proceed further explaining A couple of well-known phenomena. What anomalies help to create the problem situations? What anomalies indicate the disagreement of several mature theories with each other? - To answer the question it is necessary to investigate the *link* connecting the occurrence of the anomalies with the appearance of the cross-contradictions. It is necessary to reveal the ways by which the cross-contradiction influences the disagreement of theoretical statements with experimental data. To explain the empirical data, a Theoretical Scheme (i.e. a system of theoretical abstract objects) is transformed into an Empirical Scheme. The constructs of Empirical Scheme differ radically from the constructs of the Theoretical Scheme since they are not idealizations now. They can be compared with real objects taking part in real experiments. (In what follows I shall use the ideas of Stepin’s 1976 book, pp.79-97). For instance, consider the situation with a magnetic needle in the vicinity of the conductor carrying a current. To examine the Biot-Savare law validity, one has to compute the needle’s angle of deflection. However, the Biot-Savare formula has nothing to do with real objects. It describes the correlations of theoretical objects only. The theoretical objects involved are : “infinitely small current” and “magnetic field generated by the current”. Hence the Biot-Savare equation cannot be applied directly to explain empirical data. It is necessary to interpret the corresponding quantities as relating to the concrete experimental situation first. That is why a definite consequence of the Biot-Savare law - the empirical formulae - is derived. New quantities are introduced into it. They characterize the needle’s deflection angle and integral conductor configuration. The empirical data should be compared with this formulae only, and not with the Biot-Savare law. Though the elements of empirical schemes can be compared to real objects, they are not identical to them. (Moreover, an empirical scheme contains theoretical information absent in experiments - see Lynch,1990).For example, real magnetic needle and real conductor carrying a current have many properties. But in the Empirical Scheme they are represented by the following properties only: “to be guided by the magnetic field” and “to conduct the electric current” and “to have a definite configuration” respectively. Any element of Empirical Scheme can be compared not with a single real object or real experimental device, but with a *class* of such objects. For instance, the “needle & conductor” empirical scheme refers to any experiment with any conductor and magnetic needle. An empirical scheme can be obtained not only from theoretical law, but from statistical treatment of empirical data as well. All the objects interacting in real experiments - the object of investigation and the experimental device - are described as having statistically-invariant properties only. It means that they are described as having properties that manifest themselves only under many observations. An Empirical Scheme describes the features typical for many experimental situations. Correspondingly, any subsystem of derivative theoretical objects can be compared with a *class*  of empirical schemes. Hence *any system of derivative objects is an invariant content of the empirical schemes.* The notion *invariant content* should not be understood as referring to inductive method of theoretical laws derivation. To obtain a theoretical invariant, one should know beforehand that the set of empirical schemes forms a class. This class is easily revealed only with a help of reduction of derivative subsystems to empirical schemes. Yet comparing the empirical schemes with each other, one cannot easily come to a conclusion that they have common content. The latter is determined by the basis and by the rules of derivative objects construction from the basis. The correlations of the derivative objects describe a special idealized experiment reflecting the most general and important features of experimental practice. The system of crossbred objects belongs to the derivative subsystems of both cross-theories. Consequently, the occurrence of inconsistent statements in the crossbred theory is identical to occurrence of inconsistent statements in both cross-theories. And the latter is identical to endowing the derivative objects with *new properties*, incompatible with the old ones. But it was already pointed out that any system of derivative objects is an invariant content of a certain class of empirical schemes. That is why the occurrence of the cross-contradiction should lead not only to the contradiction between the statements of the crossbred theory and the results of a single experiment, but to the contradiction with the *whole class of experiments*.

 For instance, the occurrence of the cross-contradiction between mechanics and electrodynamics in the crossbred theory - the planetary atom theory - is identical to endowing to theoretical objects of electrodynamics with new properties incompatible with the old ones. And it leads to the following result. The crossbred theory contradicts to the experiments not only with a certain types of atoms - with alkaline metals, for instance, - but to the experiments with all the elements of Mendeleev’s table as well. According to the planetary theory predictions, *all the atoms should be unstable*. The occurrence of the cross-contradiction in the theory of blackbody radiation leads to the contradiction of the crossbred theory not only to the experiments of Lummer & Pringsheim or Rubens & Kurlbaum. The infinity of blackbody radiation energy density led to the situation, when “all the bodies should continuously radiate the electromagnetic waves, and not only outside, but inside also, until all the energy of the bodies is transformed to electromagnetic field energy and the temperature is fallen down to absolute zero” (Landau,1958,p.81). The cross-contradiction manifests itself not in ordinary deviations of theoretical predictions from experimental results. No theory ever managed to eliminate all its anomalies. When the problem situation is created by the cross0contradiction occurrence, the anomalies occur that are *out of the domain of usual deviations common for the theory under consideration.* Namely these ‘paranormal’ anomalies attract researchers’ attention and the period of” crisis” begins. Such stages of development of science were carefully described by Thomas S. Kuhn . Kuhn’s accounts had embraced a vast amount of history-of-science data. His data include the transition from heliocentric astronomy to geocentric one (“the state of Ptolemaic astronomy was a scandal before Copernicus’s announcement” – Kuhn 1957,pp.138-40), from the Aristotelian physics to the classical mechanics (“both Galileo’s and Newton’s contributions to the study of motion were initially focused upon difficulties discovered in ancient and medieval theory” – Kuhn 1957,pp.237-260).They include also the transitions from corpuscular theory of light to the wave theory (“the wave theory that replaced Newton’s was announced in the midst of growing concern about anomalies in the relation of diffraction and polarization to Newton’s theory” – Kuhn 1958,1977, pp.206-207), from phlogiston chemistry to the oxygen one (“Lavoisier’s new chemistry was born after the observation of anomalous weight relations in combustion” – Kuhn 1977, pp. 206-207).Kuhn’s generalizations embrace transitions from classical mechanics to special relativity (Kuhn 1980), from classical physics to quantum one( the latter was created due to “a variety of difficulties surrounding black-body radiation, specific heat, and the photoelectric effect” – Kuhn 1977, p.208).The crisis starts due to revelation of such anomalies

 “which, either because they are particularly striking or because they are educed repeatedly in many different laboratories, cannot be indefinitely ignored. Though they remain unassimilated, they impinge with gradually increasing force upon the consciousness of the scientific community. As this process continues, the pattern of the community’s research gradually changes. At first, reports of unassimilated observations appear more and more frequently in the pages of laboratory notebooks or as asides in published reports. Then more and more research is turned to the anomaly itself. Those who are attempting to make it lawlike will increasingly quarrel over the meaning of the concepts and theories which they have long held in common without awareness or ambiguity. A few of them will begin critically to analyse the fabric of belief that has brought the community to its present impasse” (Kuhn 1977, p.262).

 I think that in his works Kuhn correctly points out the necessity of crises as preliminary stages of paradigm change. The simple model proposed here can theoretically reproduce the necessity of crises, and it can *explain* some important historical examples cited in Kuhn’s 1977 paper “A Function for Thought Experiments” .For instance, Kuhn describes the “Aristotle-Galileo” transition and finds Galileo’s gedankenexperiments very important.

”The concepts that Aristotle applied to the study of motion were, in some part, self-contradictory, and the contradiction was not entirely eliminated during the Middle Ages. Galileo’s thought experiment brought the difficulty to the fore by confronting the readers with the paradox implicit in their mode of thought...Similarly, Aristotle’s concept of speed, with its two simultaneous criteria, can be applied without difficulty to most of the motions we see about us. Problems arise only for that class of motions, again rather rare, in which the criterion of instantaneous velocity and the criterion of average velocity lead to contradictory responses in qualitative applications. In both cases the concepts are contradictory only in the sense that the individual who employs them runs the risk of self-contradiction. He may, that is, find himself in a situation where he can be forced to give incompatible answers to one and the same question” (Kuhn 1977, p.251).

 ***Part two. Cross-theories as Feyerabendian alternatives***. I must point out the next important factor that helps to reveal the paranormal anomalies: all the cross-theories play (relative to each other) the roles of Feyerabendian alternatives.

”The function of such concrete alternatives is, however, this: they provide means of criticizing the accepted theory in a manner which goes beyond the criticism provided by a comparison of that theory ‘with the facts’: however closely a theory seems to reflect the facts, however universal its use, and however necessary its existence seems to be to those speaking the correspondence idiom, its factual adequacy can be asserted only after it has been confronted with alternatives whose invention and detailed development must therefore precede any final assertion of practical success and factual adequacy” (Feyerabend 1963,p.7).

 A notion’ *alternative theory’* denotes, according to Paul Feyerabend, a theory created in conformity with instructions of the so-called *Proliferation Principle*. For successful development of science, the latter demands to create and to elaborate theories incompatible with the accepted ones, even when the latter are very well confirmed by facts. This principle is justified by Feyerabend with the help of the following arguments. (1) Not a theory that ever existed did agree with *all* the observation data. Hence one ought to have the means to reveal the deviations of theoretical predictions from facts. (2) All the theories agree with facts only to some extent. Some deviations from facts are obvious. Yet sometimes the certain laws hide the deviations. Only alternatives can help in these cases. However, not any alternative can be applied to criticise the established theory. If a theory T’ exchanges the prediction P of the theory T by the prediction P’, incompatible with P, but experimentally indistinguishable from it, one can say that the theory T’ is not a real alternative. To exclude such cases, Feyerabend had elaborated the following criteria. (a) Except the prediction P’, that contradicts the theory T, the alternative T’ should contain some **set** of propositions. (b) This set should provide a closer connection with the prediction than a simple conjunction. An organic unity of the refuting prediction with the other part of the theory is preferable. It seems to me that Lakatos’s non ad hoc3 claim expresses this intention better ( see Zahar 1973; Leplin 1975). (c) There should be at least one empirical potential evidence supporting the alternative . (d) The alternative’s ability to explain the former successes of theory T under criticism is supposed. (It seems to me that this claim is rather obscure: if the alternatives are incommensurable, how can one of them explain the successes of the other). (e) A real alternative should differ semantically from the theory that is criticized. If one applies Feyerabend’s Proliferation Principle to mature theory change, he will arrive at the following conclusion. *Each of the cross-theories T1 and T2 satisfies all the five Feyerabend’s conditions.* Hence T1 I s strict alternative of T2, and vice versa.T1 is a means of T2 criticism, and vice versa. From the point of view of T1, the deviations of its predictions from the experimental data are caused by another theory T2.From the point of view of T2, deviations of its predictions from the experimental data are due to the drawbacks of T1.Hence any experiment that contradicts the crossbred theory reveals the drawbacks of both T1 and T2. This peculiarity of theory change can be illustrated by the following quotation from Einstein concerning Planck’s formula.

”This was a great success which Planck clearly understood. But the matter has a serious drawback, which Planck fortunately overlooked at first. The same considerations demand that the relation E= kT would also have to be valid for low temperatures. In that case, however, it would be all over with Planck’s formula and with the constant h. From the existing theory, therefore, the correct conclusion would be as follows: the average kinetic energy of the oscillator is either given incorrectly by the theory of gases, which would imply the refutation of (statistical ) mechanics; or the average energy of the oscillator follows incorrectly from Maxwell’s theory, which would imply the refutation of the latter. Under such circumstances it is most probable that both theories are correct only within some limits, but are false out of them”(Einstein 1949, p.43).

Feyerabend’s works , as well as his critical remarks helped me in elaborating my methodological model. However, I must emphasize an important point where our approaches diverge. Just to quote Feyerabend (1969, p.6):

“You can be a good empiricist only if you are prepared to work with many alternative theories rather with a single point of view and ‘experience’. This plurality of theories must not be regarded as a preliminary state of knowledge which will at some time in the future be replaced by the One True Theory”.

Putting aside the perplexed question of the existence of One True Theory, I should like to note that, according to my model, the cross-theories *should be* exchanged by a global theory that is *better* than the cross-theories. The global theory is not an alternative to the cross-theories. To quote Feyerabend (1969,p.6) once more: ”Alternatives must rather be developed in such detail that problems already ‘solved’ by the accepted theory can again be treated in a new and perhaps also more detailed manner”. In my model the global theory belongs to a higher level of theory construction in comparison to the cross-theories. They are real alternatives only relative to each other but not to the global theory. The global theory is better than the cross-theories not because it can solve some problems of the cross-theories in a “new and perhaps also more detailed manner”, but because *it can solve the problems that cannot be treated by the cross-theories in principle.*  The paranormal anomalies differ from the usual ones not only in their magnitude. It was not rare in the history of physics when the anomalies were eliminated by non ad hoc modifications of the partial theories. Yet in the cases of paranormal anomalies it is the cross-theories’ inability to eliminate such anomalies that represents a vital factor in the occurrence of the problem situation. Why ordinary anomaly cannot be considered as a refutation of a theory? - It is because such a theoretical object can be introduced into a derivative system of a theory that is able to change the relations between other theoretical objects. But if the anomaly is not an ordinary one but is created by the cross-contradiction, the situation changes gradually. At least two theories deal with the anomaly. In this case any concordance of one cross-theory with the observation data will inevitably lead to deviations of the other cross-theory with the same data (the swing case).As a result, the cross-theories can be brought to agreement with the experimental results only after the cross-contradiction elimination, i.e. only after the creation of the global theory.

 ***Part three. The role of the crucial experiments***. The model proposed brings some new light on the role of crucial experiments in science. According to the empiricist conception, to refute any scientific theory a single crucial experiment is sufficient. According to the pan-theoretical model, critical experiments for mature theories do not exist at all. It was already mentioned in the first chapter that the empiricist conception is based on the fallacious thesis: there exists the observation language independent of theoretical propositions. The pan-theoretic conception exaggerates the theory-laidenness of observations. Hence it is quite natural that, being the supporters of extreme standpoints, the empiricists and the pan-theoreticians are right only partly. According to the model developed, *there can be any number of critical experiments but only in the problem situation generated by the cross-contradiction*. Any anomaly, caused by the existence of the cross-contradiction, is a” crucial experiment”. However it is crucial not in Popperian sense. It cannot refute completely - it can limit the domain of validity only. If the experimental data are out of the crossbred-theory predictions, it means that to eliminate the deviations one has to create the global theory. The usual method of proposing some auxiliary modifications in any of the cross-theories would be useless. It is clear now why certain experiments were considered as ‘crucial’ only with hindsight, only some years *after* they were performed. Even up to now some of the historians of science debate what experiments - of Rubens & Kurlbaum or of Lummer & Pringsheim - did really refute the classical theory of radiation. However, in reality both experiments were crucial, as well as many other ones forgotten up to now. Besides the already mentioned, the list of real crucial experiments includes Young’s experiment, abnormal Mercury perihelion precession observations, etc. But the Michelson-Morley experiment is not a crucial one. It is a typical “small crucial experiment” that helped to make a choice between the theories of Fresnel and Stokes debating about partial or complete ether drag. Both theories were proposed within the same Lorentz- Langevine-Wien programme. Let me give an example. The classical argument of the empiricists is Lord Kelvin’s prophetic speech, devoted to Two Clouds “that obscure the beauty and clearness of the dynamical theory”. Yet a more thorough analysis reveals that that the outstanding protagonist of the Classical Physics spoke not about “facts” and “refutations” but about two *paradoxes*. And the experimental facts were taken into account by Kelvin only when illustrating the paradoxes.

 For instance, the First Paradox (“Cloud”) - “Relative Motion of Ether and Ponderable Bodies” - was dealt with even by Fresnel and Young. It “ involved the question, how could the earth move through an elastic solid, such as essentially is the luminiferous ether?” (Kelvin 1901,p.1).The First Paradox consists in the question: if “ether does occupy the same space as ponderable matter” and if “that ether is displaced by ponderable bodies moving through space occupied by ether”, “how then could matter act on ether?” (Kelvin 1901, p.3).The Michelson & Morley experiment is considered only as making one of the paradox’s solution (that of Fresnel) dubious, and the contraction hypothesis of Lorentz & Fitzgerald is called “brilliant” (Kelvin 1901,p.6). The Second Cloud is the Maxwell-Boltzmann doctrine regarding the partition of energy. It was called “the greatest difficulty yet encountered by the molecular theory” even by Maxwell in 1873.It is not surprising the experiments of Lummer & Pringsheim and Rubens & Kurlbaum are not even mentioned.

 ***Part four.How to construct a synthetic global theory***.

 The occurrence of the problem situation (of “crisis” in Kuhnian terms) leads to gradual changes in the activities of scientists involved. They try to exchange the “old” mature theory by new ones. The scientists are inspired by the ideas that will later be reconstructed by the methodologists as synthetic and reductionist “hard cores”. As a rule, they cannot realize that the sequences of theories they propose belong to strict and definite Procrustean forms of reductionist or synthetic programmes. Moreover, as a matter of fact they are unaware that they are constructing the global theory. Imre Lakatos used to say that scientists are aware in the laws of development of their disciplines as fishes in the laws of hydrodynamics. (Alas, the same is true for philosophers of science too). But is it possible to construct the global theory? Is the probability that scientists can stumble against the global theory in their chaotic wanderings more than zero? Can the scientific community be compared to a flock of monkeys printing on typewriters? Is the probability to find a global theory equal to the probability that the monkeys will print “*Hamlet*”? Or “*Two Dogmas of Empiricism*”? In reality (see chapter 5 for details) a synthetic theory is constructed by trial and error. Contrary to a reductionist global theory (with definite rules of construction), a synthetic theory is constructed gradually, little by little. In the course of the construction the intermediate hypotheses are proposed that can provide constant empirically-progressive problemshift in comparison with reductionist programmes. Let me demonstrate it. Basic system of theoretical objects, and systems of derivative objects as well, have two indissoluble properties (Stepin 1976). Firstly, any system of theoretical objects, any theoretical model is a ***representation*** (see Wartofsky 1979) of some class of *objects* under consideration. For example, the basis of classical mechanics explains the mechanical motions by an ideal model. The model represents the motions of real bodies by the motions of material points in the inertial systems of reference under the influence of forces. On the other hand, any system of theoretical objects is a model, that represents the features of real *experimental device*. This peculiarity permitted Vyacheslav Stepin to call basic system “The Fundamental Theoretical Scheme” (FTS) and derivative system “The Partial Theoretic Scheme” (PTS).For instance, the FTS of classical mechanics, that represents mechanical motion by the motion of a material point in an inertial frame of reference under the influence of the forces, describes a *gedankenexperiment.* The latter describes the most important features of the various mechanical experiments with real bodies. The basis of Newtonian mechanics is a generalization of practical operations with real bodies moving along the inclined planes, operations with oscillating pendulums, with billiard balls collisions, etc. In particular, the PTS of small-oscillations constitutes a model describing the features of the experiments with oscillations of real pendulums, real stretched strings, etc. Let me imagine now that the reductionist programme had been realized successfully. It means that the global theory was constructed in such a way that the place of the Global Basis was taken by the basis of one of the cross-theories. The basis of the second cross-theory became the system of its derivative objects. But this means that one and the same system of theoretical objects describes now the features of radically *different* measuring operations. Their difference is caused by the fact that the basic systems of both cross-theories had been created *before* they met. They were created as generalizations of ***absolutely independent and different*** operations. For instance, successful realization of Lorentz-Langevine-Wien reductionist programme (reduction of electrodynamics to mechanics) would mean the following. One of derivative subsystem of maxwellian electrodynamics, consisting of “differentially small current” and “magnetic field of a current” - reflects the important features of the experiments with the bodies moving along the inclined planes, of the experiments with the rotations of rigid bodies, etc. It is obvious that the paradox described is caused by the fact that the basis of one of cross-theories is unable to generalize the features of radically different measuring operations. The paradox can be eliminated (and it was - in Lorentz’s theory, for instance) by introducing the “operationally indefinite” object. It helps to “wash away” the operational foundations of the ”fundamental” theory to such an extent that it can accumulate the measuring operations of the “phenomenological” theory also.(For the definitions of fundamental and phenomenological theories see chapter three of this study).The ideal object “ether” was introduced into the reductionist programme of Lorentz, Langevine and Wien as a “carrier of electromagnetic oscillations”. It is a vivid example of operationally indefinite object. No physical experiment could determine the motion through ether. This construct was introduced into the system of abstract objects of Lorentz’s theory not due to certain generalizations of measuring operations. It was introduced for the construction of mechanical basis from that of electrodynamics. The material points (the ‘particles’) were to be considered as “ether perturbations”, whereas the forces acting on them were to be determined via the tensions of the ether. Yet the price of such an elimination often appears to be too high. It can lead to a *radical decrease of predictive power* of a theory. Indeed, what is the cause of this power? If a mature theory is not a direct generalization of empirical data, how can it predict the results of new experiments that are not even performed? How can a mature theory forecast future? According to Stepin (1976), this opportunity is based on the *rigid connection* between the theoretical basis and real experiments. The link of the basis with experiments is intermediated by the systems of derivative objects. Any basic objects represents the characteristic features of the relations of the derivative objects that belong to the lower level of theoretical objects’ organization. Hence to give an operational definition of a basic object is to describe idealized measurement operation, a *gedankenexperimen*t, but not the real one. For instance, the operational definition of electric field density **J** is given not by the description of real measurements. It is given by the description of relations of Maxwell’s theory abstract objects of “electric field at a point” and “test charge”. But these abstract objects are the entities of the partial theoretical schemes of Maxwell’s theory. And their operational status is determined now by real, and not by ideal, measuring operations. For example, the “test charge” is determined through such an action of a massive charged body on the other, when the reciprocal influence of the second body on the first can be neglected. The link between the PTS level and the level of empirical schemes is installed because all the PTS are the idealized schemes of real interactions observed in real experiments. So, all the bonds between all the levels of a mature theory are rigid ones. This rigidity allows one to connect a prediction referring to the upper level with all the levels of a mature theory. Hence it allows one to construct an experimental device to check the prediction. A new result, obtained to the development of mathematical apparatus, immediately influences all the levels of a mature theory. Hence a theory can predict, and the predictions can be verified. Consequently, introduction of operationally-indefinite abstract objects, usual for reductionist theories, should lead to decrease of the predictive power. It does not mean, of course, that a reductionist programme cannot predict at all (example: mechanical interpretation of heat). Successful elimination of the cross-contradictions is possible only in a theory that contains all the cross-basises in ***different*** systems of derivative objects. Only in synthetic theory can the global theoretical scheme be a model that combines all the important features of idealized experiments with the abstract objects of both cross-theories. I had previously pointed out that the creation of the global synthetic theory is possible due to constant empirically-progressive problemshift .Let me demonstrate it. To describe genesis and structure of a synthetic theory, one has to look through the history of physics for such a theory that contains the regularities we are looking for in the most “purified” forms. To my mind Maxwellian electrodynamics is such a theory. Let me consider the history of its creation first. As is well-known, the main goal of James Maxwell was a unification of various descriptions of electricity and magnetism. Michael Faraday pioneered in considering electromagnetism as a kind of perturbations transmitted from one point to another in accordance with the Principle of Direct Action. Except Faraday’s intuitive standpoint, there existed an action-at-a-distance picture. It depicted electromagnetic interactions as a kind of instant forces between point charges and infinitely small currents. The picture was exploited by Ampére in his programme of electricity & magnetism unification that competed with Maxwell’s programme for a long time. So, the question raised can be reformulated in the following way: *what were the properties of the maxwellian synthetic programme that provided its victory over the reductionist programme of Ampére and Weber?* Reconstructions of the history of maxwellian electrodynamics (Tisza 1963; Stepin & Tomilchik 1976) revealed the following properties. (i) Interrelations between the laws of electrostatics (Coulomb, Faraday), magnetostatics and electromagnetic induction (Faraday) are described by fig.1.Symbol $\rightarrow $ denotes the relations of complementarity. MAXWELLIAN ELECTRODYNAMICS $\downright $ $\downright $ *Theory of Electric Circuits* *Wave Optics* $\downright $ $\downright $ $\downright $ *Electrostatics* *Magnetostatics* *Geometrical Optics* Figure 1. The main peculiarity of Fig.1 consists in its triangle structure. Laszlo Tisza was the first to recognize it. Basis of each triangle is formed by two incompatible theoretical systems. The incompatibility is overcome by the third system, dominating over the incompatible ones.

 (ii) When the Fundamental Theoretical Scheme of the maxwellian electrodynamics was constructed, the “stuff” part was played not by “protokolsatze” of observations, but by *theoretical knowledge of the lower level*. The experimental data were taken into account by Maxwell in indirect way by operating with the already elaborated theoretical schemes of Faraday, Coulomb, Ampére et al. The global theory is constructed by synthesis of theoretical schemes of the lower level, and not by generalization of the experimental data. (iii) Considering the transition from electrostatics and magnetostatics to the theory of electric circuits, V.S. Stepin has noticed the peculiarity of basic importance for unification of any incompatible theoretical schemes. At first Maxwell derived the generalized law of electrostatics and introduced the equation that generalized the laws of Ampére, Savare and Biot (as well as Coulomb’s law for magnetic poles).But later he realized that these unconnected pictures should be unified. However, the unification on the basis of stationary lines of forces appeared to be fruitless, and Maxwell introduced the model of vortex in incompressible liquid , admitting irregular rotations of the vortices. Now Maxwell can perform the crucial gedankenexperiment - the derivation of the Biot - Savare law from the pictures obtained.

”At first he mentally marshals magnetic field from closed lines of force, and then finds out that the field obtained corresponds to a conductivity current of certain magnitude. Such an experiment cannot be performed in reality. But it stood Maxwell in good stead. It proved that the introduction of new abstract object - current with “substantial” properties (to flow along the conductor, to produce heat) - into the model did retain the former property of the current - “to be an origin of magnetic field” (Stepin 1976, p.163).

It is important that earlier the theoretical object representing the “substantial” current properties and the theoretical object that characterized the magnetic effects of the current differed from each other since they belonged to different theoretical schemes. The unification appeared to be possible because such a FTS was constructed that contained an object uniting all the complementary properties of the objects from incompatible partial theoretical schemes. Two different objects were “exchanged” by a single one belonging to the FTS level now, and not to the lower level of partial theoretical schemes. Many years later Maxwell’s method was applied by Niels Bohr in the process of creating the quantum theory. According to Bohr, “the impossibility of fusing the effects, observed under different experimental conditions, into a single classical picture leads to consideration of such apparently contradictive effects as complementary in the sense that all of them - taken altogether - exhaust all the definite conclusions about the atomic objects”. Thus, ***the Global Theoretic Scheme represents a model containing the important features of the gedankenexperiments with basic objects of both cross-theories.*** This property can be explained from a sociological standpoint also: “A more powerful theory, we submit, is one that with fewer elements and fewer and simpler transformations makes it possible to get at every other theory (past and future”(Latour 1990, p.50).The Global Scheme has this property because it contains the objects possessing the properties of objects from different theories T1 and T2.In this sense those objects are *complementary-observable*: their properties earlier belonged to different objects from complementary theoretical systems. Now I can methodologically explain the Principle of Complementary Observability that was earlier obtained as a modification of Bransky’ s Principle. Factors (i)-(iii) make it possible to explain why the synthetic programmes *should* provide larger empirically-progressive problemshifts in comparison with the reductionist ones. The reason consists in the crossbred -objects utilization by the synthetic programmes and their prohibition in the reductionist ones. In spite of the fact that the crossbred objects should be thrown out of the cross-theories ,they are given the rank of global theoretical objects in synthetic programmes. Indeed, according to their definition, the global objects are the objects from which the basises of T1 and T2 are constructed. The global objects contain information about B1 and B2, but the crossbred objects have this property also! They are constructed from B1 and B2.Moreover, since the construction of the crossbreeds from the basises is possible, the reverse process of reconstructing basises from the crossbreeds is possible also. I have pointed out that in general it is possible to construct several systems of crossbred objects. Each crossbred system contains only part of the information about their basises. Only the whole set of various cross-systems possesses all the information about their “parents”. But this set is always open for new elements, since it is impossible to declare beforehand that all the cross-domains are known to us. Any mature theory can suffer unrestrained development by the construction of partial theories from the FTS for any new domain of application. That is why the global system must occur only as a result of unification of all the cross-systems and cannot live a secluded life. It is always open for new crossbred systems, and the introduction of new elements can endow the old ones with new properties. The methodological scheme described can be illustrated by the example of transition from 19-th century physics to the physics of the XX-th century. The modern quantum theory was created by the unification of Bohr’s crossbred atom models, of Einstein’s crossbred semicorpuscular radiation theory, of special relativity, etc. This process is still not finished. Indeed, the creation of modern relativistic quantum field theory became possible as a result of special relativity and quantum mechanics unification. In connection with it Paul Dirac had pointed out that the spinors - the mathematical objects corresponding to half-integer-spin particles - change their signs under 2π rotations. And if the spin number is equal to 1? - The theory meets with difficulties.

”I feel that it is rather against the spirit of relativity to have quantities which transform in a non-local way. It means that we have a certain quantity in space-time referred to one direction of the time axis, and when we change the direction of time axis the new quantity does not refer to the conditions in the neighbourhood of the point where the original quantity was, but refers to physical conditions some way away. That is rather like an action at a distance theory...This difficulty occurs when we have several particles interacting with each other...I think one ought to say that the problem of reconciling quantum theory and relativity is not solved. The concepts which physicists are using at the present time are not adequate. They become rather artificial when one just applies them in a formal way. The difficulty becomes most apparent when one takes into account the interaction between, let us say, electrons and the electromagnetic field” (Dirac 1973, p.10-11).

It can be added to the quotation that it is namely the cross-contradiction between quantum mechanics and special theory of relativity that is the origin of many other difficulties in the Quantum Field Theory (QFT) foundations. These difficulties are not the old conceptual problems in Quantum Mechanics (like the wave-particle duality) that are simply carried over into the QFT, where they are obscured by the more sophisticated mathematics. “It can also be argued that serious new difficulties arise in the foundations of QFT, e.g. in connection with renormalization procedures, the status of virtual particles, and the question of particle localizability, which have no real counterparts in the older theory” (Brown & Harre 1990, p.1). Coordination of Special Relativity with Newton’s theory of gravity led, as was described already, to the General Relativity (GTR) creation. Einstein’s efforts to incorporate Newtonian gravity into the STR framework began in 1907 when he had to prepare a review for “*Jahrbuch der Radioaktivitat*” (see Vizgin,1981 for details).His first effort was unsuccessful since simple-minded (though lorentz-invariant) generalizations failed to explain the anomalous perihelion motion of Mercury. Yet the creation of GTR led to the problem of the unification of GTR and OFT. This problem is not yet solved, and Stephen Hawking called elucidation of the relation between gravity and quantum theory the most important problem facing theoretical physics today. In spite of the fact that the first efforts to reconcile GTR and the quantum theory started from the very moment of GTR creation (Theories of Weyl, Cartan, Kaluza & Klein), the substantive progress was attained quite recently, and, at least at the first stages, it seems to have nothing to do with superstrings, supergravity and so on. In full accordance with my model I think that the real progress in QFT and GTR unification began with the crossbred objects construction in new crossbred domain called the “Quantum Field Theory in Curved Spaces”(Birrell & Davies wrote in 1982 the first review under the same title).Hawking’s astonishing 1975 discovery of particle creation by black holes is an obvious example of a cross-theory. But the real story started earlier, with Jacob Beckenstein’s pioneer analysis of the black hole entropy. His paper is a masterpiece combining deep physical intuition with outrageous flies of imagination so rare in the dark waters of mathematical speculations. Beckenstein started with a mere analogy between entropy and the surface of black-hole horizon. From the point of view of theory of information, entropy and horizon of a black hole are the quantities of the same type. If a particle gets under the horizon of a black hole, its surface increases due to the mass addition. But at the same time entropy increases also, since we cannot look under the horizon to know the state of the particle there. The area of the horizon should be equated to entropy with some proportionality coefficient. Beckenstein’s intuitive results were generalized and elucidated by J.M. Bardeen, B. Carter and S.W. Hawking in “*The Four Laws of Black Hole Mechanics*” (1973) where the analogy between black hole physics and thermodynamics was treated in more strict and mathematically sophisticated terms. Expressions were derived for the mass of a stationary axisymmetric solution of the Einstein equations containing a black hole surrounded by matter and for the difference in mass between two neighbouring solutions.

”Two of the quantities which appear in these expressions, namely the area A of the event horizon and the ‘surface gravity’ k of the black hole, have a close analogy with entropy and temperature respectively. This analogy suggests the formulation of four laws of black hole mechanics which correspond to and in some ways transcend the four laws of thermodynamics” (Bardeen, Carter, Hawking 1973, p.161).

Now GRT and thermodynamics were found to be merged into a general theoretical scheme, and the results showed up rather quickly. According to the Second Generalized Law of Thermodynamics, formulated by Beckenstein and Hawking, the flow of entropy across the event horizon of a black hole should increase the area of the horizon A. Entropy + some multiple of A never decreases. But if we equate the surface gravity k to temperature, a *paradox* occurs. If a black hole is immersed in a sea of blackbody radiation at a lower temperature (than that of the black hole), the Second Law will be violated! The only way to avoid the paradox is to admit that a black hole *should* emit particles at a steady rate with temperature equal to k/2πM. Namely that was done by Stephen Hawking in 1975 by taking quantum-field effects into consideration. Just to quote the abstract of his masterpiece: “In the classical theory black holes can only absorb and not emit particles. However it is shown that quantum mechanical effects cause black holes to create and emit particles as if they were hot bodies with temperature hk/2π 10-6 (M0/M) where k is the surface gravity of the black hole. This thermal emission leads to a slow decrease in the mass of the black hole and to its eventual disappearance: any primordial black hole of mass less than about 1015g should have evaporated by now. Although these quantum effects violate the classical law that the area of the event horizon of a black hole cannot decrease, there remains a Generalized Second Law: S + (1/4) A never decreases where S is the entropy of matter outside black holes and A is the sum of the surface areas of the event horizons” (Hawking 1975, p.199). At least several lessons can be learned from the story. Firstly, three theories were involved in the occurrence of the cross-contradiction: thermodynamics, GTR and QFT. And QFT is shown to be a necessary link connecting GTR with thermodynamics. Without QFT general relativity cannot be reconciled with thermodynamics and statistical mechanics. All the story looks like that: classical thermodynamics “knows” something about the quanta, as if the quantum theory is “contained” in the thermodynamics. In the light of the fifth chapter of this book this conclusion does not seem strange since the quantum theory is a sort of fusion of thermodynamics, mechanics and electrodynamics. We can conclude that Hawking’s result could be obtained at the end of the XIX -th century if we assume that the axioms of thermodynamics are universally valid, which is to say they are assumed to apply to black holes . ”This has perhaps contributed to the idea that gravity has a somehow deeper relationship to thermodynamics than to other branches of physics” (Gould 1987, p.449). From the beginning the Quantum Field Theory in Curved Spaces met with a lot of paradoxes caused by the lack of proper agreement between GTR and QFT (Fulling 1973; De Witt 1975).The notion of a particle appeared to be observer-dependent; conformal anomalies occurred; various renormalization methods gave different results for vacuum energies; the negative energy fluxes began to fill the space of GTR, and many other nice things began to bother unhappy theorists and continue to puzzle them now. It is important for the model proposed that even one of the leading black-hole theorists had pointed out that the cause of all these paradoxes lies in semiclassical black-hole evaporation theory.

”The gravitational field is treated ‘classically’ (as a space-time metric) there, while its sources are described in terms of quantum field theory. The basic equations of GTR, Gμν= Tμν contain incompatible quantities on the two sides of the equals sign. Furthermore, this incompatibility has consequences even at the present experimental scale” (Unruh 1980, p.154).

One of the most interesting crossbreeds here is the QFT vacuum. When it was embedded to GTR derivative system (the black-hole physics), it did acquire a new property - “to produce infinitely great gravitational field”. But the methods of its regularization (the cut-off procedures) were elaborated for flat space-times only. They are useless in strong gravitational fields. The occurrence of the QFT vacuum restructured the relations between the GRT objects, and the process of the global theory construction began. Modern super-unification theories (see Chyba,1985), various inflation scenarios are the stages of global theory construction. Vacuum plays the central role here, being the global object of the future theory: “making everything out of nothing”(as Robert Weingard used to say – Weingard 1991) during the process of spontaneous symmetry-breaking. To my mind I.J.R. Aitchison’s paper “*The Vacuum and Unification*” is the best illustration of the vacuum’s role in the unification of the modern physical theories and in the global theory construction. The Quantum Vacuum played the leading roles at each stage of the unification of the 4 fundamental forces of nature - in Higgs mechanism establishment (Higgs vacuum), in fifth coordinate compactification in Kaluza-Klein models (the Casimir effect), in Green’s and Schwartz’s superstring model of Quantum Gravity free of gravitational anomalies. ”All the same, it is pretty mind-blowing to reflect that our own four-dimensional world, its geometry, its matter content, and its force may all be just the dynamically determined ground state of vacuum - of a superstring!” (Aitchison 1991, p.193).So, the creation of the synthetic global theory seems to follow here the lines of Einstein and Bohr. However, let me return to the general scheme of global theory creation. Why should the synthetic theories empirically supersede the reductionist ones? The introduction of the crossbreeds creates contradictions in all the cross-theories and forces them to accommodate each other, to “interpenetrate” and to “enrich” each other. For instance, in the case considered, “the general theory of relativity and the quantum theory of fields, taken together in a new synthesis, have thus far enriched each other” (De Witt 1980, p.683).The enrichment manifested itself in the penetration of QFT into GTR (the gravitational field quantization).On the other hand, much less known GTR penetration into QFT manifested in the discovery of the so-called *Unruh effect* in flat space-time.

”A recent example of this, closely connected with the black hole evaporation process mentioned by Hawking, is the behaviour of accelerated particle detectors. Since in GR(i.e. in General Relativity) one often has to worry about the behaviour of accelerated observers, the behaviour of such detectors in flat, Minkowski space-time is of interest. By building a simple model of a particle detector, and accelerating it in the vacuum state in flat space-time, it is possible to show that such a detector behaves exactly as if it were immersed in a thermal sea of particles of temperature T = 8πa/kc “(Unruh 1980,p.154).

The encounter of T1 and T2 increases their domains of validity on D2 and D1 respectively. The domain of validity of T1 expands owing to D2, and vice versa. The domain of validity of both cross-theories becomes equal to 2(D1+D2).For example, the creation of the photon theory (and of special relativity) was due to interpenetration of statistical mechanics and thermodynamics (Willard Gibbs’s and Albert Einstein’s investigations in the domain of *statistical thermodynamics*),of statistical mechanics and electrodynamics (Einstein’s 1904 papers on fluctuations of electromagnetic field), of thermodynamics and electrodynamics (Max Planck’s pioneered introducing the notions of *temperature of radiation* and *entropy of radiation*), and of Newtonian mechanics and Maxwellian electrodynamics (principle of relativity, corpuscular theory of light).The development of Einstein’s ideas by Louis De Broglie (see Pais 1980, for details) consisted in endowing the “ponderous matter” with wave properties, etc. Thus the crossbred objects are the “channels” through which the ideas and methods of the cross-theories interpenetrate. Let us compare synthetic theories with reductionist ones, that forbid the existence of the crossbreeds. Let us consider the case that is most favourable for reduction - when the fundamental problems are successfully solved and the domain of validity of the global theory became larger because D2 was added to D1.The “fundamental” theory T1 have penetrated into the “phenomenological” theory T2, but the reverse process is forbidden. That is why the domain of validity of the synthetic ideal is two times larger than the domain of validity of the reductionist ideal. Namely that peculiarity *should* provide the victory of synthetic programmes over their reductionist rivals. As was already mentioned, the occurrence of the crossbred objects leads to contradictions in each cross-theory. For instance, the introduction of photons contradicted classical theory of radiation, that explained interference and diffraction of light waves.

”This outlook showed in a drastic and direct way that a type of immediate reality has to be ascribed to Planck’s quanta, that energy of radiation must, therefore, possess a kind of molecular structure, which of course contradicts the Maxwell theory” (Einstein 1949,p.51).

 In spite of the fact that each global theory *should* eliminate the cross-contradictions according to the model proposed, the real history of physics is more complicated. The process of global theory construction may last for centuries embracing new cross-domains. Hence the cross-contradictions play the role of “driving forces” in creation of new theories. In the case of a clash between GTR and QFT the methodological model described can be illustrated be the following quotation.

”It is ironic that just at the time when the experimental tests are becoming possible, are being performed, and are verifying the predictions of the theory, the most determined assault on structure of the theory itself is taking place. In the attempt to make general relativity compatible with quantum theory of fields, many feel that the theory must be altered in some fashion. The most notable example of this is, of course, supergravity, with its addition of the gravitino” (Unruh 1980, p.153).

The most remarkable feature of the modern supergravity theories consists in that these theories were constructed without experimental results. ”The second reply to such criticisms is that we have already a powerful ‘experimental’ fact that needs to be explained, and that is the logical inconsistence of the two theories” (Unruh 1980, p.154).When constructing the global theory, the experimental data are taken into account *indirectly*, being embedded into pieces of theory. The role of contradictions between Theory and Experiment is played by the cross-contradictions between *several theories*. John Archibald Wheeler had expressed the view that we must find the essential physical fact and philosophical outlook that relate quantum mechanics and gravity, and the proper mathematical formulation will then suggest itself. But the fact is that the construction of the Global Theory had already begun and very important results including supergravity were obtained *without any new philosophical outlook and without experimental results.* However, for too many people the investigation of the role of quantum theory in gravity seems premature. Order-of-magnitude estimates suggest that quantum effects should become important only over distances of order 10-33cm, energies over 1022 Mev, and so on. As no experiments exist, or have any possibility of existing at this level, the theoretical endeavour under consideration seems to them doomed to failure for lack of any guidance in our speculations by the real world. The methodological model proposed helps to justify the necessity of all the steps made on the road of the supergravity creation. It demonstrates that the objections - the lack of radically new philosophical outlook and the lack of radically new mathematical formalism - miss the target. They are unimportant. This is in no ways to establish the complacent standpoint, since too many steps are to be made until we declare that the unification machine is at our disposal. What is really important in establishing the supergravity is its *empirical justification*. But the latter should consist in establishing the *operational status* of the concepts involved. And this status can be obtained by ideal measurement procedures first of all that are analogous to Bohr-Rosenfeld procedures in quantum electrodynamics. To give an operational definition of a physical quantity , connected with a basic object, is to give a description of idealized measurement procedures. ”It was only by asking questions about measurements, about how the various phenomena would affect measuring instruments such as real-model gravity wave detectors or observers falling through the ‘singularity’ at R=2M of the Schwarzchild metric, that these issues were resolved”(Unruh,1986,p.128).It is no wonder that the most interesting results were obtained in this direction of research. I am talking about the famous *Unruh effect*:

“the lesson of general relativity was that the proper questions to ask of a theory are those that concern the behaviour of detectors that have been fashioned to detect the effects that concern one. For example, in trying to decide whether or not particles are created, one needs to define a particle detector *within the theory*; i.e. some model of a physical system that will respond to the presence of a particle. In my attempt to apply this philosophy in the mid 1970’s, I looked at the behaviour of a simple model of a particle detector (1) for some quantum field, which, for simplicity, I took as a massless scalar field F” (Unruh 1986, p.128).

The operational analysis of the concepts involved is necessary for making correct predictions. Only after the experimental confirmation the appropriate philosophical picture will quickly suggest itself. However, the establishment of modern unification theories needs the analysis of the history-of-science data too. Too many scientists (John Wheeler, Stephen Hawking, for instance) note a very close similarity between modern situation in theory of gravity and the pre-revolutionary situation that took place in physics at the beginning of the XX-th century. In what follows I’ll provide a rational reconstruction of Einstein’s Revolution.

**CHAPTER FIVE. A RATIONAL RECONSTRUCTION OF THE LORENTZ-EINSTEIN TRANSITION.**  **A brief** **introduction.** As is well-known, Einstein’s Special Theory of Relativity (STR) and Lorentz’s theory co-existed at the beginning of the 20-th century as *empirically-equivalent* ones (see, for instance, Bucherer, 1904; Poincare, 1905, 1906; Langevin,1905; Kaufmann,1906; Ehrenfest,1906; Laue,1911; Lorentz, 1915). In particular, both of them, though in different ways, explained the results of the Michelson-Morley experiment, as well as the results of the experiments of Fizeau (1853), Respiga & Hook (1858), Klinkerfus (1870), Ketteler (1872), Mascare (1873), Rowland (1876), Rontgene (1885), Descoudre (1889),Lodge(1893), Rayleigh(1902),Smith (1902), Trowton (1902), Eihenwald (1903), Nordmeyer(1903), Trowton & Noble (1903), Wilson (1904), Brace (1905), Kenigsberger (1905), Strasser (1907), and of some others (see Bursian,1904, for details). In his review of the STR foundations Norman Campbell had specially pointed out that

“it is important to notice that there is another theory, that of Lorentz, which explains completely all the electrical laws of relatively moving systems, that the deductions from the Principle of Relativity are identical with those from the Lorentzian theory, and that both sets of deductions agree completely with all experiments that have been performed” (Сampbell 1911,p.503).

 Up to now, the various reconstructions of the Lorentz-Einstein transition have been proposed. Their number considerably exceeds my abilities to take into account all of them, hence I tried to scrutinize the most important ones only. I chose inductivist and falsificationist trends, and the Lakatos-Zahar reconstruction. All of them consider the results of the Michelson-Morley experiment , but it seems unnecessary to describe its history here. I suppose that the reader is acquainted with classic papers of Lakatos[[1]](#footnote-1) (1970) and Zahar (1973). Correspondingly, the first part of this chapter deals with inductivism and falsificationism, the second one - with Lakatos-Zahar account, and the last ones offer a new rational reconstruction. It owes much to the ideas of Lakatos and Zahar, but even more I apply their works in the first part.

***Part one. The Inefficiency of the Inductivist and the Falsificationist accounts.*** (1.1) Inductivist explanation. It is based on the proposition that the second STR postulate (that states the invariance of the speed of light) is a direct generalization of the Michelson-Morley experiment. According to Max Born, “... the second statement, that of the constancy of the velocity of light, must be regarded as being experimentally established with certainty” (Born 1962,p. 225).

According to Hans Reichenbach, “... it would be mistaken to argue that Einstein’s theory gives an explanation of Michelson’s experiment, since it does not do so. Michelson’s experiment is simply taken over as an axiom” (Reichenbach 1958, p.20). Inductivist explanation is incorrect simultaneously from two standpoints. Firstly, it is incorrect from the history-of-science point of view (Lakatos, 1970).Secondly,

“from the logical point of view, it has by now become a platitude that observation reports neither establish nor even probabilify high-level theories. None of Michelson’s observational statements is equivalent to the proposition that in all inertial frames the speed of light is a universal constant independent of the velocity of the source” (Zahar 1973, p.214).

Many contemporaries of Michelson & Morley were fully aware of it. Russian physicist of Kazan University Dmitry A. Goldgammer wrote:“...in Einstein’s theory the speed of light is invariant just according to the convention” (Goldgammer 1911, p.157).In the well-known all over the world textbook of the prominent Russian physicist of the University of St. Petersburg Orest D. Chwolson it was specially punctuated out that the second postulate“ is an a priori stated axiom, a postulate on which new worldview is based. In no respects we can say that it had been justified experimentally” (Chwolson 1912, p.402). It is rather curious that, contrary to the inductivists just quoted, *the inductivists of the XX-th century beginning thought that it was Lorentz’s theory, and not Einstein’s, that was established experimentally!* Just to quote a popular review of electrodynamics of the moving bodies:

“We have described a broad range of experimental data referring to the questions of electromagnetic and optical phenomena in moving bodies, and have linked it with the development of Lorentz’s theory. The latter states the existence of the ether and considers the motion of the bodies relative to it. We did observe the way according to which, following the progress of Art of the Experiment, by purely inductive means, the theory under consideration was led to an important result, analogous to a well-known principle of classical mechanics, that in all the electromagnetic cases, as well as in purely mechanical ones, all the phenomena depend on only relative motions” (Bursian 1904, p.35).

In 1907 Lord Rayleigh had awarded professor Albert Michelson the Copley Medal for his “experimental investigations in optics” and had assessed the results of his experiment in the following way:

“In conjunction with E.W. Morley, he devised and carried out a very remarkable method by which, on the assumption of ether at rest, an effect depending on quantities of the order of (v/V)2 would appear to be appreciable. No displacement of the rings was found. On this result the simplest explanation would be that the ether near the earth partakes fully in its orbital motion; but modern electric and optical science appears to demand a quiescent ether, and the existence of this and similar null results is fundamental for its theory”(Rayleigh 1908, p.247).

“Thus, we can consider it as established once and for all that the ether takes no part in the motion of the terrestrial bodies” (Kotovich 1909; see also Goldgammer 1911,p.155). Moreover, in a paper published in a prominent English scientific journal a proposition is contained that:

“.... the principle of relativity is not strictly a logical law but the expression in mathematical symbols, of the general philosophical law of the finite nature of the human mind which has been accepted for centuries”(More 1911, p.196).

And the last and, it seems to me, the strongest argument against the inductivist explanation of the Lorentz-Einstein transition consists in the following. Let us turn to the so-called “emission theories of light” that threw the light-constancy postulate out and exchanged it with the Galilean law (that added the velocities of light and of its source).These theories (see Tolman 1912, for details) had **no problems** in explaining the Michelson-Morley result! They were specially invented to explain it. And they did. But they should not do it , if the inductivists were right. (1.2) The Falsificationist Explanation.

 It attributes Lorentz’s failure and Einstein’s success to their respectively ad hoc and non ad hoc responses to the results of the Michelson-Morley experiment. “An example of an unsatisfactory auxiliary hypothesis would be the Contraction Hypothesis of Fitzgerald and Lorentz which had no falsifiable consequences but merely served to restore the agreement between theory and experiment mainly the findings of Michelson and Morley” (Popper 1935,section 20). I agree completely with Imre Lakatos and Elie Zahar that ad-hocness should be defined , on the basis of SRP-methodology, not as a property of an isolated hypothesis but as a relation between two consecutive theories.

“A theory is said to be ad hoc1 if it has no novel consequences as compared with its predecessor. It is ad hoc2 if none of its novel predictions have been actually ‘verified’; for one reason or another the experiment in question may not have been carried out , or - much worse - an experiment devised to test a novel prediction may have yielded a negative result. Finally the theory is said to be ad hoc3 if it is obtained from its predecessor through a modification of the auxiliary hypothesis which does not accord with the spirit of the heuristic of the programme” (Zahar 1973, p.217).

To investigate the ad-hocness of the Lorentz’s programme one has to describe its structure - the hard core, the heuristic and the protective belt. According to Zahar, the hard core of Lorentz’s programme consisted of Maxwell’s equations for the electromagnetic field, of Newton’s laws of motion and of the Galilean transformations, to which Lorentz had added the expression for the “Lorentz force”. The heuristic of his programme was determined by the following metaphysical principle: all physical phenomena are governed by actions transmitted by the ether. Any theory from SRP is a conjunction of the hard core and an auxiliary hypothesis proposed according to the rules of heuristic. Lorentz’s ether programme consisted of the sequence of theories L,L’,L’’. Theory L was formed by the conjunction of the hard core with the following two assumptions: (a) moving clocks are not retarded; (b) material rods are not shortened by their motion through the ether. L’ was obtained from L by substituting the Lorentz-Fitzgerald contraction (LFC) hypothesis for assumption (b).The LFC hypothesis states that a body moving through ether with velocity v is shortened by the factor (1-v2/c2)1/2.L’’ is the conjunction of the hard core, of the LFC and of the assumption that, contrary to (a), clocks moving with velocity v are retarded by the factor (1 - v2/с2)1/2. Zahar convincingly demonstrated that nor L’, neither L’’ were ad hoc1 hypotheses. For instance, STR, L and L’ predicted different results of the Kennedy-Thorndike experiment. L’ is not an ad hoc3 hypothesis also. Lorentz derived the LFC hypothesis from a deeper theory - from Molecular Forces Hypothesis (MFH): “ molecular forces transform and behave like electromagnetic ones”. It was quite natural for Lorentz to admit that there is no special “molecular” ether to transmit the interactions between the bodies. All the interactions should be transmitted in the usual “luminiferous” ether. Zahar considers L’ as non ad hoc2, but his arguments are based on his well-known definition of the novel fact. ”A fact will be considered novel with respect to a given hypothesis if it did not belong to the problem situation which governed the construction of the hypothesis” (Zahar 1973, p.218).I agree with Musgrave (1974, pp.13-14) that Zahar’s definition is dubious since it gives the procedures of empirical justification from the hands of experimentalists to the hands of historians of science. Such an understanding of the novel fact deviates from Lakatos’s “temporal novelty”. It seems to me that Zahar’s redefinition of the novel fact is unnecessary for the defence of L’.L’ is not ad hoc2 but due to the other reasons. The following quotation is of importance here:

“This assumption of a shrinkage, although bold and thus far entirely hypothetical, is not impossible and is the only suggestion yet made which is capable of reconciling the negative results of second and third order experiments with a quiescent ether. Poincaré (Rapports du Congres de Physique de 1900, Paris, i, pp.22,23) has raised objection to the electromagnetic theory for moving bodies, that each time new facts are brought to light a new hypothesis has to be introduced. This criticism seems to have been fairly met by Lorentz in his latest treatment of this subject” (Brace 1905, p.72).

Describing and developing Lorentz’s arguments, Brace uses the results of Hasenorl (Annalen der Physik, 1903, band 13,p.367).Reasoning from a cyclic process in a moving radiating system, Hasenorl had proved that the second law of thermodynamics is contradicted unless a second order contraction takes place. Hence not only the Michelson-Morley experiment, but *all the variety of the experiments establishing the second law of thermodynamics support L’.* This is an outstanding empirical confirmation! It is of no wonder that many Lorentz’s contemporaries supported Brace’s conclusions.

 “...indeed, the possibility of contraction of the moving body in airless space

seems extremely strange. But let one look on the essence of matter, and he will change his opinion. Indeed, according to the information, all the forces inside the material bodies and the elastic ones in particular are the forces of the common (electromagnetic) origin” (Goldgammer 1911, p.156).

 Norman Campbell of Trinity College (1912, pp.432-434) too found the LFC hypothesis artificial at the first sight. But if one considers it carefully, he can deduce, with Larmor, that the hypothesis has a sufficient theoretical basis. From this point of view the negative Michelson & Morley result shows that the optical properties of matter have electrical origin. Nobody doubts it now. Hence the LFC hypothesis met unanimous support and approval, and all the physicists thought that the difficulties that arose due to the Michelson-Morley experiment were solved sufficiently well. Paul Ehrenfest, one of the closest Einstein’s friends, had compared STR and L’. He deduced that “the critical experiment deciding in favour of one of those theories, is impossible in principle” (Ehrenfest 1913, p.160). Even Henri Poincaré, who was the first to accuse the LFC in ad hocness, had admitted at St.-Louis Congress (1904, USA) that in the light of L’’ the LFC was not an ad hoc hypothesis at all. In spite of the fact that LFC was initially invented to explain the results of single experiment, **later** it become able to explain a series of experiments. Lorentz himself, of course, did not consider the Michelson-Morley results as refutation of L’. For comparison it is interesting to consider his reaction on the results of Kaufmann’s experiments. The results seemed to contradict his theory (as well as STR).So in a letter to H. Poincaré (8 March 1906) Lorentz wrote:

“Unfortunately my hypothesis of the flattening of electrons is in contradiction with Kaufmann’s new results, and I must abandon it. I am, therefore, at the end of my Latin. It seems to me impossible to establish a theory that demands the complete absence of an influence of translation on the phenomena of electricity and optics. I would be very happy if you would succeed in clarifying the difficulties that arise again” (quoted from Miller 1980,p.83).

But soon Kaufmann’s results were shown to be erroneous. L’’ or the Theory of Corresponding States was proposed by Lorentz in 1904.Its genesis and acceptance was described by one of Lorentz’s contemporaries in the following way:

“However, if we introduce this contraction into Lorentz’s theory, another difficulty appears. If a piece of glass is flattened in the direction of motion, it should become double refracting for light coming perpendicular to its motion.T his double refraction can be measured by light interference. But the experiments, however, did not reveal any double refraction. Again the difficulty occurred. But looking on his formula, Lorentz in 1904 noticed that the necessity of double refraction is eliminated, if a new assumption is made. According to it, motion changes velocities of the physical processes, and, consequently, all the time intervals measured by local clocks .Any place in the moving frame of reference gets its own time. It delays from our usual time, and the delay is proportional to the velocity of the system” (Goldgammer 1912, p.173).

Thus, L’’ was not ad hoc3, ad hoc2 and ad hoc1.Some of its consequences were confirmed by the experiments of Rayleigh, Brace and Bucherer on the deflections of “radium rays” in the electric and magnetic fields in 1902-1908.

 ***Part two. The Inefficiency of the Lakatos-Zahar reconstruction.*** Elie Zahar (1973) had demonstrated that Lorentz’s theory can be developed to such an extent when all its empirical consequences will coincide with those of STR. Thus, in order to make a choice between two rival programmes - Einstein’s and Lorentz’s – non-empirical criteria should be invoked. Zahar suggests criteria that take into account the *heuristic power* of competing programmes. Lorentz’s programme progressed up to 1905, but was superseded by Einstein’s programme in 1915 due to GTR (General Theory of Relativity) creation. Einstein’s theory of gravitation provided explanation of three “critical” effects of GTR - Mercury perihelion precession, bending of light in the gravitational field and the red shift of spectral lines - that could not be explained by the Lorentz programme. Zahar’s rational reconstruction deals with the following three questions. (1) What were Einstein’s reasons for objecting to the Classical Programme and hence for starting his own? (2) Once Einstein’s programme was launched, why did other scientists like Planck, Lewis and Tolman work on Einstein’s programme rather than on Lorentz’s? (3) At what stage did the Relativity Programme empirically supersede Lorentz’s? However, none of Zahar’s answers on (1) - (3) is acceptable, since his reconstruction has the following drawbacks (revealed by the joint efforts of historians and philosophers of science). (A) Majority of the leading physicists rejected ether already in 1910-1912 (Schaffner 1974).They did not wait for GTR creation. (B) Elie Zahar argued that it was the GTR creation that led to elimination of Lorentz’s programme. But: (a) by or very shortly after 1915 special theory of relativity had been professionally accepted and put into use to such an extent that the general theory of relativity had not achieved even thirty or forty years (Kuhn 1980); (b) Lorentz’s programme yields the constancy of c (speed of light) as a contingent fact and in this respect closer to a general theory of relativity than Einstein’s STR where constancy of c is a basic law (Feyerabend 1974). (C ) Zahar never mentions the hard core of full Einstein’s programme (STR+ GTR) though he mentions a programme that contains the relativity principle together with the principle of the constancy of c. Scrutinising Zahar’s reconstruction, one can conclude that the programmes of Lorentz and Einstein were partly empirically-equivalent since their hard cores contain common elements (Maxwell’s equations, in particular). But then the well-known features of Lakatosian methodology - ambiguity in determining the content of the hard core, ambiguity in SPR decomposing on the hard core and the heuristic, permission for scientist to change the positive heuristic gradually - help to arrive at rather tough conclusion. One can describe the Lorentz-Einstein transition not as programmes’s competition, but as a development *within the* *same* Lorentz-Einstein scientific research programme. Einstein’s STR and GTR can be considered as steps in developing Lorentz’s programme. Einstein’s heuristic can be easily represented as a radical reformulation of Lorentz’s heuristic, since Zahar places ether - the central element of Lorentz’s programme - in the positive heuristic, but not in the hard core. Zahar’s arguments consist in that “ the hard core is by definition unalterable and there were as many different ethers as there were ether-theorists; probably even more since ether-theorists often changed their minds about the properties of their own ether” (Zahar 1978, p.51).The Lorentz-Einstein transition can be described in the following way. Heuristic of Lorentz’s programme, after the failures of mechanical models of ether, had exhausted. The function of the ether did reduce to be a carrier of electromagnetic oscillations. Step by step, instead of knowing about field from ether, the physicists began to learn about the ether from the field studies. The true competition between the programmes is possible only when their hard cores are *alternative.* But to give such a description one should modify the frames of Lakatosian methodology. (D) (a) The scientific community shared no “Relativity Programme” from 1905 to 1910: Einstein’s work had not been disentangled from Lorentz’s. At the time there was only one theory in existence for the majority of scientists namely the Lorentz-Einstein theory (Miller, 1973).Contemporaries of Lorentz and Einstein spoke about “Einstein-Lorentz electron model”, “relativity principle of Lorentz and Einstein”, etc. (Kaufmann 1906;Planck 1906; Born 1911; Juttner 1911; Page 1912). (D) (b) As it was already mentioned, Zahar never describes the hard core of full Einstein’s programme though he mentions a programme that contains the relativity principle together with the principle of the constancy of c. In 1905 this programme started to degenerate while Lorentz’s programme was advancing. Derivation of E= mc2 was not a privilege of the Relativistic Programme. This equation was first obtained by Oliver Heaviside (see Bolotovsky 1987, for details) and then in 1904 by Lorentz himself. (D) ( c ) Lorentz highly assessed Einstein’s reinterpretation of the electron theory equations, but never agreed with it. Until the end of his life he believed in ether, as well as in the notions of absolute time and absolute space (see Nersessian, 1986, for example).But at the same time he admired GTR and made an important impact to it in 1914-1917.He did consider the GTR physical space as playing the role of ether of his electron theory. And Einstein agreed (Mc-Cormmach 1970). (d) Moreover, since Zahar had determined the novel fact as not belonging to the problem situation in which the new theory is proposed, he has to convince the reader that Mercury perihelion precession did **not** belong to the problem situation of GTR creation (Illy 1981,p.209).Yet history of science tells the opposite. Abnormal Mercury perihelion precession **did belong** to the problem situations of all non-stationary theories of gravity (Subbotin 1956; Vizgin 1981). And Einstein by himself did specially mention the perihelion precession as a special problem to be solved even in 1907, eight years before the GTR creation.

Consideration of (A) - (D) drawbacks enables one to deduce that Zahar’s reconstruction is inefficient even according to the SRP methodology standards, in spite of the fact that Lakatos’s methodology was a foundation of Zahar’s study. Indeed, the SRP methodology develops standards that apply to research programmes, not to individual theories. These standards judge the evolution of a programme over a period of time, and not its shape at a particular time. And they judge this evolution, as Paul Feyerabend had noticed, only in comparison with the evolution of rivals, not by itself. According to Imre Lakatos, all the methodologies of science - inductivism, falsificationism and the SRP methodology - function as historiographical (or meta-historic) research programmes and can be criticized by comparison of rational reconstructions that follow from them. But this criticism should not be understood in naive-falsificationist sense. All the methodologies should be falsified in pairs. And those one should be chosen that embraces more history-of-science data. Lakatos’s methodology supersedes the others because it constitutes progressive problemshift in the sequence of rational reconstructions. In Lakatos’s methodology we can reconstruct as rational more aspects of scientific activity than in inductivist and falsificationist “theories of rationality”. In their rational reconstructions scientists are either slow, or irrationally rush. However, in Zahar’s reconstruction behaviour of scientists also deviates from rationality standards (though in less extent than in inductivism and falsificationism).According to Zahar, the scientific community was too hurry to join Einstein before 1915 and too slow to disentangle Einstein’s programme from Lorentz’s before 1910. (E) “Lorentz wanted to show that electromagnetic phenomena in moving bodies can be explained on the basis of a variety of processes, all of them taking place in accordance with Maxwell’s equations in vacuo, referred to an all-pervading aether at rest. Only part of this research programme was superseded by R (i.e. Einstein’s programme) in 1915.The remainder was eventually superseded by the quantum theory which cannot be reconciled with R. We must admit that the most comprehensive research programme in physics that existed around the turn of the century, viz. the programme of Lorentz, was never superseded in its entirety by another single research programme, but broke up into a variety of mutually irreconcilable programmes unless we assume that the lifework of Einstein was guided by a programme as comprehensive as that of Lorentz and unless we succeed in reconstructing its hard core. Neither Zahar, nor anyone else has done this” (Feyerabend 1974, p.26). I think that some of the drawbacks of Zahar’s reconstruction are caused by the following drawbacks of Lakatos’s methodology. I. Lakatos’s theory of rationality lacks the description of the genesis of the hard cores. The SRP methodology can work only with ready-maid cores. But the lack of such a description causes a methodological drawback: who can guarantee that a certain rational reconstruction had revealed the hard core correctly? The most vivid example is “Method and Appraisal in Physical Science” edited by Colin Howson. As was correctly pointed out by Thomas Kuhn, the authors of this book “seldom present or even seek evidence of what actually attracted scientists to or repelled them from the various research programmes under study” (Kuhn 1980, p.188). Hence Lakatos never formulated *criteria of choice* between different reconstructions of one and the same period of history of science. These criteria can be introduced by analogy with rival methodologies. The choice between different interpretations of the same hard core should be made by comparing internal historical descriptions. But discussing such comparisons, Imre Lakatos presupposed the existence of what he called “actual history”, presumably the pool of all historical facts.

“But ‘actual history’ of the sort Lakatos requires is a myth. The pool of all data potentially relevant to history cannot be either identified or scanned. More to the point, the data in most parts of the pool are not, until after much interpretation, the facts which appear in historical narratives. When Lakatos provides an historical case to illustrate the comparative merits of the methodology of research programmes, he is not selecting the elements of his internalist narrative from ‘actual history’ but creating them from often distant data or else choosing from the similar creations of earlier historians”(Kuhn 1980,p.184).

It is no less important that in historiography Lakatos admits positivistic standpoint on theory and facts that was severely criticized by him in methodology of inductive sciences. This standpoint in history was convincingly ridiculed by Robin Collingwood and Benedetto Croce, and it is unnecessary to repeat it here. II. SRP methodology attributes an exclusive role in the evaluation of competing programmes to “fruitfulness” , but it is not the only criteria of choice. Its importance was overstated by Lakatos, and it distorted his view on theory-change process. Moreover,

“though actual appraisals are for Lakatos always comparative, the single criterion in terms of which such appraisals are made is applicable to individual research programmes taken alone. Choosing between two research programmes, that is, one first locates each individually on the fruitfulness scale, the progressive/degenerative continuum, and only then does one compare the two” (Kuhn 1980,p.189).

So, SRP methodology underestimates the *dialectic* of competing programmes . However,

“historically, when a choice must be made between two research programmes, the more recent one is usually an attempt to resolve concrete difficulties encountered in the practice of its older rival. In the sciences, unlike the arts and probably also unlike mathematics, new approaches do not ordinarily emerge simply when old ones stagnate, cease to produce novelty. Instead, they await a time when the traditional approach is seen to have failed in resolving problems acknowledged to lie within its competence” (Kuhn 1980, p.190).

When scientists choose between the rival programmes, they take into account not only their fruitfulness. The history of the “old” programme is also important: could it solve the well-known anomalies before the occurrence of its rival? For instance, in the case of Lorentz-Einstein transition, many Einstein’s adherents were attracted by the easiness with which the STR threw away (yet not solved) the difficulties of ether theory.

 III. The STR methodology standards are ineffective. They provide the description of the problem situation, in which a researcher can find himself, but not the advices how he can get out of it. If the research programme is regressing, scientist can reject it and turn to the progressing one. But he has a right also, according to Imre Lakatos, to work in the frame of the old one. Hence the SRP methodology standards “...are arbitrary, subjective, and ‘irrational’. They do not provide objective reasons for eliminating Marxism, or Aristotelianism, or Hermeticism, or for attacking new developments in the sciences. They merely indicate what critical rationalists would ‘like to have’ at this stage of the development of their ideology” (Feyerabend 1976, p.321). Alan Musgrave, one of Lakatos’s dissenters, tried to get rid of this drawback by pointing out that the SRP methodology advices are to be addressed to science, to the community of scientists as a whole. They would forbid wholesale persistence with degenerating programmes, or premature mass conversion to a budding one, but permit the individual scientist to go his way: “we cannot condemn Priestley for his die-hard adherence to phlogistonism; but we could condemn the community of late nineteenth-century chemists had they all done the same “(quoted from Feyerabend 1976,p.324). Musgrave thinks that the SRP methodology directs science to devote energy to investigating unsolved scientific problems. According to Musgrave, a progressive research programme throws up more unsolved problems than a degenerating one, hence science ought to devote more energy to progressive programmes than to degenerating ones. But this claim obviously contradicts the basic conclusion of SRP methodology: every success of a progressing programme is a problem for its degenerating rival. Thus, it is degenerating programme that should create the majority of the problems, and not its progressive rival (Feyerabend 1976). Musgrave’s effort to get rid of Lakatos’s drawback fails.

IV. Lakatosian division of the history of science on the “normative-internal” and “empirical-external” parts causes problems. According to Lakatos, each methodology provides its own internal (rational) reconstruction of the history of science. Any internal explanation should be supplemented by an external one, explaining the velocity, the place, etc. of influence of rational factors. Moreover, in SRP methodology rational reconstruction or internal history is primary, and external history is only secondary, since the most important problems of the external history are determined by the internal one. But the subsequent application of the “internal “ criterion should lead to such a narrative in which **all**  the behaviour of scientists is rational. Such a conclusion obviously contradicts the history of science data and common sense as well. On the contrary, the more thoroughly the historical research is carried out, the more mistakes (including the arithmetic ones - see part 4) are revealed. The clue to eliminate this paradox consists in Lakatos’s own dictum: “internal history is not just a selection of methodologically interpreted facts, it may be on occasion their radically improved version” . One can “correct the error”, admitting that the subsequent methodology is better than the preceding one since it provides one by **deeper understanding**.

“I fully agree with Lakatos that all four methodologies are normative, and that there is gradual improvement; the MSRP is by far the most fruiful. But the reason for calling it the best is rather that in this methodology reconstruction can be and is nearest to the historically true story (true in the primitive sense in which it is clear that Bohr did not discover the spin, Newton had no energy concept etc.).Indeed, I shall try to show that the demand to present rational reconstruction as against historical narrative is now unnecessary and that the internal-external dichotomy constitutes a degenerating problem-shift” (Elkana 1978, p.245).

Moreover, the SRP methodology admits a programme to progress even on inconsistent foundations. However, the formal logic states that from the system of inconsistent axioms **any** consequence could be derived. Hence any scientific fact could be predicted (or postdicted) by such a programme, and theoretical growth will always outstrip the empirical one (Kulka 1977, p.342).This programme will always be progressive. Consequently, such parasites are not only admitted by SRP methodology, but are welcomed by it (Bohr’s programme). The methodological model that takes into account the critical remarks I-III was proposed in chapters 2-3.The aim of the following parts of chapter 5 is to introduce such a reconstruction of Lorentz-Einstein transition that takes into account (A) - (E) and provides historiographically-progressive problemshift in respect to Zahar’s reconstruction. I’ll try to show that all three Einstein’s 1905 papers, as well as his papers on early quantum theory and on statistical mechanics, were all parts of a single programme of unification of classical mechanics and classical electrodynamics. Lorentz tried to solve the unification problem by reducing mechanics to electrodynamics. Inefficiency of his ether programme was revealed by repeated failures to construct purely electromagnetic electron model. Holistic programme initiated by Albert Einstein to reconcile mechanics and electrodynamics was separated later into two sub-programmes - quantum and relativistic - that began to develop relatively independently of each other. Zahar’s results describe the development of Relativistic Sub-programme. His description of an **aspect** of Lorentz-Einstein is true. But to obtain deeper understanding of the reasons of STR creation, as well as of the reasons of Einstein’s victory over Lorentz, one should expand the domain of history of science data to include the early quantum history. One cannot understand the reasons of STR creation and acceptance without taking into account Einstein’s works on statistics and early quantum theory. It should be specially pointed out that it is namely the consideration of the development of Einstein’s programme as a process of reconciliation of mechanics and electrodynamics that enables one to explain internally the fact of simultaneous publication of Einstein’s 1905 three masterpieces . Besides that, my reconstruction can internally explain the following history of science data. (i) Einstein’s paper on light quanta was published three months **earlier** the STR paper. (ii) The scientific community of the beginning of the 20-th century rejected the Lorentz programme and classical radiation theory practically **simultaneously**. (iii) Attempts to construct electromagnetic electron model and the competition of Einstein’s and Lorentz’s programmes are tightly connected. (iv) 1905 Einstein’s papers are connected with the subsequent and especially with his preceding papers.

***Part three. The genesis, development and failure of the Lorentz programme.***

All the pre-Maxwell descriptions of interactions (the theories of Newton, Ampére, Weber, Riemann et al.) were the theories of interactions between several material points. Owing to Faraday and Maxwell, the Electromagnetic Field entered into physics as an element of physical reality having equal rights with the Material Point. Presumed incompatibility between the laws of mechanics and the laws of electrodynamics ( see the examples of the second chapter, as well as Miller 1974) gave rise to the first programme in which attempts were made to reduce electrodynamics to mechanics. Newtonian mechanics was declared as a “fundamental theory”, while electrodynamics was acknowledged as a “particular theory”. The purpose of reductionist programme can be achieved in two steps. (i) Construction of basic theoretical objects of the particular theory from those of the fundamental theory . ( The basic theoretical objects of mechanics are : “force”, “material point” and “system of reference” and the basic objects of electrodynamics are “electromagnetic field density” and “current density”). (ii) Deduction of the laws of the particular theory from those of the fundamental one. The fundamental problem of Maxwell’s programme consisted in construction of an ether-mechanical model capable to transmit the electromagnetic interactions by its elastic properties. But this programme had failed because of two insurmountable difficulties. The first consisted in inseparable existence of both transversal and longitudinal waves in ether. The second one consisted in the paradoxical conclusion that the ether was a solid body, as, for instance, was demonstrated by Lorentz in his doctoral dissertation. All these enabled Maxwell to begin to consider the mechanical ether models as “illustrative, but not explanatory” ones. In a paper written for “Britannica” and published in 1879, just before his death, Maxwell considers all the proposed (beginning from Descartes’s) ether models. He comes to a conclusion that all of them are unsatisfactory and that the single ether that remains is the one invented by Huygens for light transmission. Initially Hendrik Lorentz began his career by incorporating Maxwell’s displacement current and the concept of electric waves into the Continental tradition of action-at-a-distance theories (see Saunders & Brown,1991, pp.29-43, for details).His investigations forced him to assume provisionally a dual character of physical reality. He had introduced the notion of “electric charge” and completed Maxwell’s equations by Newton’s equations with the expression for “Lorentz’s force” that described the interaction between charge and field . Just as James Maxwell in his late years, Hendrik Lorentz was against mechanical interpretation of ether.

“ Though we all are interested in that ‘state’, there is no real necessity to imagine it in some way; and indeed we cannot tell much about it. However, we can imagine the existence of internal tensions in the medium surrounding charged body or magnet... All these was done by many physicists, and Maxwell by himself was a beginner. But there is no real necessity in it; we can develop the theory broadly and conceive a number of phenomena without such speculations. And indeed, due to the difficulties to whom they lead, in the last years a tendency occurred to avoid them all and to build the theory on a small number of general assumptions” (Lorentz, 1909; translated from Lorentz 1953, p.20).

These views were common among many of his contemporaries.

“We must take the properties of ether so as we find them, investigate them with a help of the experience we have, and try to unify into a picture that lacks contradictions. We should not be confused, as it is unfairly done, if it occurs that these properties differ radically from those of solid, liquid or gaseous matter”(Lenard 1910,p.11).

As it was pointed out in one of the popular textbooks,

 “all the processes in ether can be studied only by their actions on material bodies. These peculiarities of the physics of ether forced many physicists to abandon the very notion of ether, since it became too habitual to link with this word the ideas of somewhat material” (Gustav Mie 1912, p.10).

One of the most prominent Russian physicists of the 20-th century beginning wrote that “ the apparent simplicity of the physical properties of the void cannot be expressed better than by Greek notion ‘ether’; and if because of mechanical abuses of the last times this notion is depreciated, it should not be rejected completely, but we should try to return its former sense”(Chwolson 1912,p.10). It is important that from the very beginning Lorentz tried to unify Fresnel’s ideas on the interaction of ether and matter with atomistic representations of electricity of Weber and Clausius (Goldberg, 1969, p.983).In his doctoral dissertation, completed in 1875 and devoted to light refraction, Lorentz had demonstrated that longitudinal oscillations of ether (if the latter is considered as elastic liquid) should not damp. It was a reason why Lorentz preferred ether with indefinite properties. Thus, bringing together the basic objects of both theories,

“... Lorentz achieved the synthesis of Newton’s mechanics and Maxwell’s theory. The weakness of this theory consists in that it tries to conceive the phenomena by combining partial differential equations with ordinary differential equations (that describe the motion of a point).This method is unnatural. The insufficient part of the theory manifests itself in avoiding from the fact that the field existing on surface of the charge should be infinitely great. The theory was unable to explain the tremendous forces that hold the electric charges on separate particles... However, Lorentz had an idea that went beyond the boundaries of his theory. A charged body is always surrounded by a magnetic field which makes important contributions to body’s inertia. Is it possible to explain the whole inertia in the electromagnetic way? It is clear that the problem can be solved successfully if the particles can be interpreted as regular solutions of the electromagnetic field equations...” (Einstein 1936, p.316).

Thus, Lorentz had an opportunity to realize the programme opposite to Maxwell’s. Both he and Henri Poincare saw such a programme as one offering the possibility to provide a coherent basis for all physics. *Classical electrodynamics became the fundamental theory of this programme. Newton’s mechanics was acknowledged as a particular theory. The fundamental problem of Lorentz’s programme consisted in construction of electromagnetic -field model of electron in which one had to “consider the particles of matter as some local perturbations of ether” (Lorentz 1909).*

“Indeed, one of the most important our statements will consist in the proposition that ether not only occupies all the space between molecules, atoms and electrons, but it penetrates through all these particles. We add the hypothesis that, although those particles are in motion, ether is always at rest. We can submit to this at first sight striking idea, if we consider the particles of matter as some local changes in the state of ether. This perturbations, of course, can easily move, while the elements of the medium, in which they are observed, remain at rest”(Lorentz,1909; quoted from Lorentz 1953,p.32).

This assumption led to the statement that in Lorentz’s theory density of charge is “continuous function of coordinates, so a charged particle lacks the sharp boundary; on the contrast, it is surrounded by a thin layer in which the density of charge continuously diminishes from the quantity it has inside the electron, up to zero” (Lorentz 1953,p.32). In Germany the same programme was realized by E. Wiechert beginning from 1894.He had declared that the most important property of a body - its mass - should be explained from the action of charged particle on itself (see Mc-Cormmach 1970, for details). In Great Britain sir Joseph Larmor was one of the active proponents of the programme described: he considered matter as consisting of rotational tensions in ether. Lines of force, electric particles, molecules, etc. were thought by Oliver Lodge, J.J. Thomson and other British physicists of the Victorian era to be reducible to vortices and strains in the ether. In Russia in speech made at 5 November 1890 in “solemn meeting of Kazan University” prof. Dr. Dmitry Goldgammer asserted that

“aspiration after revealing unity in diversity does not allow us to be satisfied with what we already have; it invites us to advance further and further, it prompts us further generalizations. Indeed, if there is an agent to whom more than a half of all the physical phenomena is obliged for their existence, will the second half be reduced to this same cause in future? And this generalization is not bolder and firmer than others. The shift from ether origin of light and electromagnetism to ether origin of usual matter with its inertia and gravitation is not bolder than the shift from two electrical and magnetic liquids to electromagnetic origin of the light ray. Not too much boldness is needed to state now that future physics will be mechanics of ether” (Goldgammer 1890,p.7).

But the most passionate adherent of the reductionist programme was Wilhelm Wien. It was he who restlessly advocated the main purpose of physics - to bring together isolated domains of mechanics and electrodynamics. While Tomson, Maxwell, Boltzmann and Hertz tried to derive the laws of electrodynamics from the laws of mechanics, Wien, beginning from 1900, tried to demonstrate that Newton’s laws of motion were a special case of the laws of electrodynamics. He showed that the first law of Newtonian mechanics is satisfied - as a consequence of the law of electromagnetic energy conservation. And the second law retained as long as it was taken to mean that the work done by a force was precisely balanced by a change in electromagnetic energy. But the third law was applicable only to forces acting between charges at rest in ether, failing for moving charges (see Mc Cormmach 1970, for details). To arrive at a more precise picture of the reductionist programme, it is necessary to quote one of the contemporaries of Lorentz and Wien:

“Whether the inertia of matter has or has not a complete electromagnetic explanation is a question that will perhaps take many years to answer with any degree of generality. The experiments of Kaufmann seem to prove that in the case of a single electron the mass is entirely of this origin; and it is impossible therefore to avoid conclusion that at least a fraction of ordinary material inertia is also electromagnetic. Doubtless there is a psychological cause for our reluctance to accept the electromagnetic explanation as complete, constant familiarity with ponderable bodies having blinded us to the possibility of anything being more fundamental; but certain it is, that if we free ourselves from prejudice as much as possible and adopt the well-tried policy of choosing the simplest theory which adequately represents the phenomena, - the theory that is, which involves the least number of variables, - we must decide in favour of the complete electromagnetic explanation, which involves only the aether and its properties” (Comstock 1908,p.1).

Beginning from its genesis the Lorentz programme did advance. It had predicted a plenty of phenomena and most of its predictions were confirmed experimentally. The achievements include: calculation of the Fresnel coefficients(1892-1895), experiments of Rontgen(1892), Eichenwald (1903), Blondlot (1901), Wilson (1905) that went against Hertz and for Lorentz; dispersion formula (1892), Zeeman effect, law of Wiedemann-Frantz and other consequences of the electron theory of metals (1904-1905).Some of this facts were absorbed into Einstein’s programme, while others had to wait for about 20 years until they were finally absorbed by quantum theory (Feyerabend 1974). However, besides the considerations of unity, beauty, etc., **the real necessity of carrying out the reductionist programme consisted in reconciling the Lorentz ‘s theory with the results of the Michelson-Morley experiment.** The latter was a puzzle for Lorentz for a long time. He clearly understood that the only way to reconcile it with stationary ether was to suppose that interferometer arms contracted (the LFC hypothesis).Moreover, “in this connection , reference should be made to the proof which Hasenorl (Annalen der Physik, Band 13,p.367) , reasoning from a cycling process in a moving radiating system, has given, that the second law of thermodynamics is contradicted unless either a second order contraction takes place in the direction of drift” (Brace 1905,p.72). But the contraction is possible only if “the molecular forces, determining the sizes of all the bodies, propagate through ether analogously to electric forces” (the so-called MFH - Molecular Forces Hypothesis).It was quite natural for Lorentz to admit that there was no special “molecular ether” to transmit interactions between the molecules of a solid body. The intermolecular interactions propagate in the usual “electrodynamical” ether. In his LFC deduction Lorentz made use of the MFH and of his famous transformations merely. So, the MFH and consequently the LFC were transitions to the Electromagnetic World View. Hence, **the validity of the MFH and of Lorentz’s explanation of the Michelson-Morley experiment should be evaluated in the context of the reductionist programme.** The fate of the explanation began to depend on the fate of the reductionist programme. *Lorentz’s explanation of the Michelson-Morley experiment could be considered not artificial, non ad hoc, etc. only if the Electromagnetic View of Nature is correct, i.e. only if electrodynamics is considered and treated as the fundamental theory of physics.* And the same heuristic, that made possible to explain the Michelson-Morley results, forced Lorentz and his adherents to advance. Up to 1904 the MFH was applied only macroscopically, but it was necessary to disseminate it on a single electron too. And experience seemed to support the possibility of such an extrapolation. The experiments on deflection of “radium rays” in electric and magnetic fields were carried out already (see Mc Cormmach 1970, for details). Bucherer’s experiments appeared to be important also: they confirmed Lorentz’s theory in the long run. But they were theoretical investigations that appeared to play a decisive role. In 1906 Poincare’s paper was published. It was convincingly demonstrated that  *a contractable electron can be considered as stable only under the assumption that essentially non-electromagnetic forces are invoked.* However, bold extrapolation of the electromagnetic field transformation properties on essentially non-electromagnetic forces is a rough violation of “spirit” of the Lorentz programme. It is an ad hoc3 hypothesis. Alas,  *Lorentz had only one way to avoid it : structureless electron*. Hence the history of attempts to construct point electromagnetic electron is of extreme importance here. The opportunity of treating mass of an electron in purely electromagnetic way was discovered by Thomson (1880,1881). He considered an interaction of the charge on the surface of the spherical particle with its own electromagnetic field. Thomson disclosed that this interaction causes the force that tries to diminish the acceleration of the particle. The electromagnetic field of a charged particle moving with velocity v has a kinetic energy T = fe2v2/2Rc2, where f is a formfactor, R - the radius of the particle and e - its charge. Consequently, we can consider Mem = fe2/Rc2 as electromagnetic mass. The whole kinetic energy of the particle is T = (Mo + Mem) v2/2. One has an opportunity to put Mo equal to zero, and Thomson used it. And already in 1894 German physicist Emil Wiehert proclaimed the principles of Electromagnetic View of Nature. The ultimate reality is ether. All the electric particles are the perturbations of ether. But soon the difficulties of the “brave new world” were revealed. In 1903 Abraham had calculated the momentum of the electromagnetic field of moving charge **P** = ∫ **S** d3r, where **S**  is the Poynting vector. After messy calculations it appeared that **P** em = (4/3) Mem **v,** while for usual momentum **p** = m **v.** Furthermore, linear and angular electromagnetic momenta entering Abraham’s calculations were not proportional to their corresponding velocities, as they were in mechanics. Indeed, since they were defined by integrals all over the space, they did depend on the whole history of electron motion. It should be stressed that Abraham’s calculations were valid for rather special case when the charge was uniformly distributed on the surface of the sphere. *But if we suppose that the charge is distributed in all the volume of the sphere, the result will occur no less disappointing.* As it was demonstrated by M. Abraham (1904) and H. Poincare (1905,1906) , deformable electron could be considered as a stable system only if non-electromagnetic counter-pressures[[2]](#footnote-2) were invoked. Those involved in the rapidly advancing field of “electrodynamics of moving bodies” clearly understood it. When Abraham first demonstrated that Lorentz’s deformable electron required inner potential energy to preserve equilibrium, Lorentz assessed his calculations as having ***extraordinary*** value for his theory (Lorentz 1904).He had admitted that ***if inner energy of electron would not be completely electromagnetic, the whole explanation of the Michelson-Morley experiment would become ad hoc***. Abraham himself considered the necessity of introducing of nonelectric forces as a critical *internal flow* of Lorentz’s theory. Others who were inclined to an electromagnetic viewpoint shared this estimate of Lorentz’s failure. For instance, A. Bucherer agreed with Abraham that the contradictions between Lorentz’s theory and pure electrodynamics were more damaging to Lorentz’s theory than even the deflecting data of Kaufmann. *The only opportunity of further realization of the reductionist programme consisted in construction of a structureless electron model.* But Lorentz’s 1909 calculations said good-bye to all these hopes. The author of “Theory of Electrons” had calculated the force by which an electron acts on itself. If we try in the expression for the force to eliminate the structure-dependent terms by letting the radius of electron to zero, the energy of self-interaction will diverge (see Rohrlich 1973, for details).This is meaningless from a physical point of view. The failure to construct elementary particles from field appeared to be crucial for Lorentz’s programme . In 1909 stagnation of the reductionist programme began. Subsequent artful attempts to eliminate the divergences included Paul Dirac’s half-retarded and half-advanced potentials, electrodynamics of Max Born and Leopold Infeld, Casimir’s zero-point model. None of them succeeded. Number of unsuccessful models increased, making the ether explanation of the Michelson & Morley experiment more and more dubious and ad hoc. Contrary to Lorentz’s theory, relativity had nothing to say about the structure of electron. In 1907, in the discussion with Paul Ehrenfest, Albert Einstein explained that his relativity principle yielded the laws of electron motion without assuming anything about the distribution of charge in electron. Yet it does not make the STR dubious simply because the problem of constructing electromagnetic electron is not fundamental for Einstein’s programme. Nevertheless, stubborn attempts to eliminate Lorentz’s electron divergences reveal the most attractive feature of ether:

“...so long as there is an ether, there is a substructure to electromagnetism; there is a definite direction in which theory must be deepened... It seems that Lorentz lamented the absence not only of the ether concept, but of any basis by which to deepen the theory, that is to conceive of the field at an instant of time, and then to consider: what kind of things is this field; what is it made of? To respond - it is what it is, as described by the solutions to the Maxwell-Lorentz-Einstein equations of electrodynamics - simply terminates the discussion. Electromagnetism is made a closed book” (Saunders & Brown 1991, p.48).

***Part four. The Special Theory genesis: Relativity as a stage in the development of the Quantum Programme.*** In order to comprehend the STR context of justification, one should try to understand its context of discovery first. And in order to explain why the majority of scientists accepted STR only in 1910-1912 (and why until 1910 Einstein’s work had not been disentangled from Lorentz’s), one should refrain traditional comparison of Lorentz’s electron theory solely with Einstein’s “On the Electrodynamics of Moving Bodies”. One should consider a *sequence* of Einstein’s papers - **Einstein’s Programme**. This is so much so, since we have **direct** Einstein’s instructions made in connection with Paul Ehrenfest’s small paper.

“In the above note there is the following remark: ‘Relativistic electrodynamics of Lorentz in the form of Einstein is considered as a closed system’... On this occasion it is necessary to remark the following. The relativity principle, or, strictly speaking, the principle of relativity together with the principle of constancy of light should be understood not as a ‘closed system’ and not as a system at all but as some heuristic principle which by itself contains assertions about rigid bodies, clocks and light signals. Everything else is provided by a special theory of relativity only because it demands the existence of connections between the phenomena which earlier seemed to be independent” (Einstein 1907).

 It is clear that the Lorentz Programme should be compared with the Einstein Programme. But what sequences of Einstein’s papers belong to it? The first rational reconstruction of Lorentz-Einstein transition that conforms the SRP methodology liberal standards, was presented by Elie Zahar. His account of the transition is accurate but insufficient. It cannot provide a proper explanation why the STR was accepted by the scientific community. Using Zahar’s account, it is difficult to explain the fact of indistinguishability of STR and of Lorentz’s theory, as well as the fact of almost simultaneous publication of Einstein’s 1905 masterpieces. The STR paper “Electrodynamics of Moving Bodies” was received by “Annalen der Physik” editorial board at 30 June 1905, and the photon paper - at 18 March 1905.Moreover, in the 1906 paper “On the Method of Determining the Relation Between Longitudinal and Transversal Masses of the Electron” the STR creator compares the consequences of three theories: of theory of Bucherer, of theory of Abraham and the theory of *Lorentz-Einstein.* It is worth noting that in 1912 the STR became commonly-accepted. Moreover, the explanation of STR recognition by the experimental confirmation of the consequences of General Relativity is unacceptable. Lorentz’s last ether theory L’ is closer to General Theory of Relativity than STR for “it yields the constancy of c as a contingent fact”, while in Einstein’s theory the constancy of c is a “basic law” (P. Feyerabend).To produce a more advanced reconstruction, one has to assume that three papers of 1905 and, at least, the part of previous and subsequent Einstein’s works are all the parts of a single research programme. It is obvious that for determining its hard core, heuristic, etc. one has to turn to Einstein’s papers, letters, etc. However, one should be aware of the following difficulties. Einstein’s views on one and the same subject varied sometimes to such an extent that even became opposite to early ones (Seelig,1960).This is true first of all of his philosophical standpoint. At the end of his career Einstein confessed that he had maintained an eclectic position during his life. I think that he had applied various metaphysical systems mainly as a means of propaganda. On the other hand, one cannot find the system the adherents of which did not try to demonstrate that Einstein’s successes were due to application of their philosophy. Positivists were the first to exploit Einstein’s name, and even now the efforts continue. But I think that Hirosige’s (1967) study can serve an example of adequate treatment of philosophical influence on Einstein’s creativity. Hirosige correctly attributed Einstein’s sensitivity to the inconsistencies between mechanics and electrodynamics to influence of Mach, whose writings supposedly **freed** the STR creator from the mechanistic worldview. Einstein could therefore juxtapose mechanics, thermodynamics and electrodynamics without reducing one to the others. Of course the STR arose not from philosophical reflections, but from Einstein’s own daily considerations about current physical problems and from his concrete physical investigations. Moreover, leaving philosophy aside and turning to professional interests, one can reveal the same picture. For instance, Lewis Pyenson in his “Young Einstein” points out that in youth Einstein considered the mathematical formalism of a scientific theory only as a means in respect to disclosing its “physical sense”. Such an attitude was probably induced by school teachers. This period is characterized by a following quotation: “I am afraid that I am wrong again. I cannot express my theory in words. I only can formulate it mathematically, and this is suspicious”. But already in 1918 his attitude to mathematics changes drastically.

 “I am convinced that we can discover by means of purely mathematical constructions the concepts and the laws connecting them with each other, which furnish the key to the understanding of natural phenomena. Experience may suggest the appropriate mathematical concepts, but they most certainly cannot be deduced from it. Experience remains, of course, the sole criterion of the physical validity of a mathematical construction. But the creative principle resides in mathematics. In a certain sense, therefore, I hold it true that pure thought can grasp reality, as the ancients dreamed” (quoted from Pyenson 1985,p.153).

Thus, Einstein’s views on various subjects varied sometimes to such an extent that became opposite to the early ones. The fact is not surprising at all since this happens with most thinking people. *But quoting Einstein one can prove anything* *he wants* (just as with a help of any theory based on inconsistent foundations). So, *the solution of the problem is not to be found in Einstein’s writings about his beliefs, but in his substantive papers themselves.* And one should also take into account the fact that hindsight statements by Einstein do not really throw much light on his early-years reasoning, because Einstein’s later views on the earlier work were bound to be coloured by his more mature attitudes. Thus, to demarcate (Einstein)o - direct participant of the historical events considered - and (Einstein)1 - the author of unified field theories - and (Einstein)2 - “philosopher-scientist”, - one must restrict the scope of the papers analysed by the upper limit of 1912.Restriction imposed undoubtedly leads to Einstein’s report “On the development of our views on the essence and structure of radiation” as to his practically **first** serious effort to analyse his works as a whole. Even such an exigent critic as Wolfgang Pauli called his report “ a landmark in development of theoretical physics” (Klein,1966). The report was made at the 81-st meeting of German Natural Scientists and Physicians. It is one of the first public reports of the STR author dedicated to explanation of its foundations. The report begins with a brief account of the ether theory, which ends with a phrase: “*But today we must consider the hypothesis of ether* as *obsolete*”. Why? It is important that for the answer Einstein turns not to the Michelson-Morley experiment and not to criticism of LFC hypothesis. He resorts instead to

“numerous facts in the domain of radiation which show that light possesses a number of fundamental properties that can be understood with the help of Newton’s emission theory considerably better than with the help of the wave theory. That is why I consider that the further phase of the development of physics will give us a theory of light which would be in some sense the unification of the wave theory with the theory of Newton” (Einstein 1909, p.482).

So, Einstein’s goal was to unify wave and emission theories of light. But the following fact suggests that Einstein’s victory over Lorentz and his STR activities can only be understood in the broader context of mechanics and electrodynamics unification. At the German University in Prague Ferdinand Lippich, professor of mathematical physics, was to retire and a successor was sought to commence from the summer semester (April - June, 1911).At its session the professorial board of the faculty had instructed three of its members - the professor of experimental physics, the professor of mathematics and the professor of physical chemistry - to submit a list of possible candidates. ”Emphasizing that the basic question of contemporary physics was how to link mechanics with electromagnetism, the committee had taken pains to select individuals who had already achieved results in this field”(Illy 1979,p.76). At April the committee proposed three people. And *it was Albert Einstein who was proposed in the first place!* Many years later in his Nobel Lecture Einstein pointed out that “the special relativity theory resulted in appreciable advances” - first of all “it reconciled mechanics to electrodynamics” (Holton, 1980,p.59).In general, the Prague committee efforts, as well as Einstein’s activities, were all the manifestations of the XIX-th century rationality tenet - to unify the different laws of nature into an integral system (see Pyenson,1985, for more detailed description of this ambitious programme). But what was the hard core of Einstein’s programme? - To answer the question one should turn to Einstein’s **first** 1905 famous paper “On an heuristical point of view concerning the processes of emission and transformation of light” being led be the following quotation from August 1899 letter to Maric:

“The introduction of the name ‘ether’ into electric theories has led to the idea of a medium, about whose motion one can speak without , in my opinion, ascribing any physical sense to the expression. I believe, as Hertz also emphasized, that electric forces are definable directly only for empty space. Furthermore, electric currents should be understood not as ‘temporal extinctions of electric polarization’, but as the motion of true electric masses, whose physical existence seems proven ...Then, electrodynamics would be the theory of the motions in empty space of moving electricities and magnetisms. *Which of these two pictures must be chosen, an investigation of radiation* will *surely make clear*” (Einstein, 1899; quoted from Abiko 1990,p.8).

*The facts* consist in the following.1905 paper on Special Relativity was published three months **later** than the photon paper and is only a **part** of the unification programme. Einstein’s quantum interests obviously dominated over the other themes. M. Besso, who kept up friendly relations with Einstein beginning from 1897, remembered him later as “a young man with passionate interest in science, preoccupied by the problems of reality of atoms and ether” (Besso to Einstein, letter 151).Moreover, Einstein’s 1905 STR paper was the first and at the same time the last fundamental paper on special relativity. His next publications on this topic are only small additions and reviews. The same is true for statistics: he almost completely lost interest to it after 1905.But the quantum theory was quite the opposite! Igor Kobzarev correctly had pointed out (1979, p. 8) that Einstein is known now as the creator of STR and GTR first of all. But for his contemporaries who worked in physics his role appeared to be rather different. For them he was mainly a specialist in atomic theory and in quanta. Einstein saw himself in the same way too. That is why he had chosen the theme “On atomic theory role in physics” for his first professorial speech in 1909.And it was his 1905a photon paper for which he got the Nobel Prize in 1921. Of course, now we have reminiscences of the STR author about his imaginary travels on the light rays at the age of 16 which would lead to Relativity Principle. But the *documents* of history of science do not confirm them. On the contrary, Einstein’s first scientific paper written when he was 16 as a letter to his uncle (found and published in “*Physikalische Blatter*”, August 9, 1971 by Jagdish Mehra) tells us something different. In this paper Einstein takes ether as an ordinary element of physical reality just the same as electric and magnetic fields. The paper was entitled “*On the Investigation of the State of the* ***Ether*** *in the Magnetic Field*” and was written in 1895, before Einstein entered the Aargau Cantonal School. The main problem of Einstein’s first scientific survey consisted in how “three components of elasticity influence ether wave velocity”. Even at the second course of Eidgenossiche Technische Hochschule he did believe in the existence of ether and intended to investigate the motion of the Earth through ether experimentally. He thought of constructing several measuring devices in connection with it (Feuer, 1974).In the Kyoto Lecture he had described one of the devices consisting of a system of thermocouples (Ono,1983).Another proposal, based on the interference phenomena, was mentioned in Einstein’s 1901 letter to Marcel Grossman (Kostro,1988). Although one often reads the statement that in 1905 Einstein was concerned with an explanation of the photoelectric effect, the study of 1905a paper reveals that this was not the case. The measurements of the effect at that time were not sufficiently accurate to point without any doubt to a violation of classical behaviour (Ter Haar,1967).Einstein was worried not so much by the evidence dealing with photoeffect and appealed to fluorescence, photoelectricity and photoionization data only as to **indirect** evidence in favour of his thesis. Rather, Einstein was concerned mostly with contradiction between mechanics and electrodynamics. Look at the beginning of his 1905a paper:

*“There exist an essential formal difference between the theoretical pictures physicists have drawn of gases and other ponderable bodies and Maxwell’s theory of electromagnetic processes in so-called empty space”.*

What does this difference consist in? -

“Whereas we assume that the state of a body to be completely determined by the positions and velocities of an albeit very large, still finite number of atoms and electrons, we use for determination of the electromagnetic state in space continuous spatial functions, so that a finite number of variables cannot be considered to be sufficient to fix completely the electromagnetic state in space”. (Translated by Ter Haar 1967).

But this difference can give rise to a situation where

“a theory of light involving the use of continuous functions in space will lead to contradictions with experience, if it applied to the phenomena of creation and conversion of light”.

Hence

“it seems to me that the observations of black-body radiation, photoluminiscence, the production of cathode rays and other phenomena involving the emission and conversion of light can be better understood on the assumption that the energy of light is distributed discontinuously in space”.

And in the first part of his 1905a Einstein discloses that the joint application of mechanical and electrodynamical “theoretical pictures” for description of black-body radiation leads not only to contradiction with experiment (his paper did not cite the results of Lummer & Pringsheim and Rubens & Curlbaum), but to *paradox*  that cannot be eliminated by usual methods. To demonstrate it Einstein uses *gedankenexperiment* with both theories. He considers a cavity containing free electromagnetic field, gas molecules and Hertz’s resonators. As a result one can conclude that joint application of mechanics and electrodynamics leads unavoidably to Rayleigh-Jeans law for energy density of black-body radiation. But

 “this relation which we found as the condition for dynamic equilibrium does not only lack agreement with experiment, but it also shows that in our picture there can be no question of a definite distribution of energy between aether and matter”, since

“the greater we choose the range of frequencies of resonators, the greater becomes the radiation energy in space and in the limit we get ∞0∫ ρν dν = (R/N) (3π/L3)0∞∫ ν2dν = ∞.”

Thus, Einstein pioneered in demonstrating how the cross-contradiction of mechanics and electrodynamics led to “ultra-violet catastrophe”. How did Einstein intend to eliminate the contradiction in his 1905a? - To answer the question one should turn to his first papers published in the “*Annalen*”. All the Einstein’s papers from 1901 to 1905 have one trait in common: statistical-thermodynamics approach. Thomas Kuhn correctly pointed out that *what brought Einstein to idea of photon was a coherent development of a research programme started in 1902, a programme “so nearly independent of Planck that it would almost certainly have led to the black-body law even if Planck had never lived”*(Kuhn 1978, p.171).Einstein’s first two papers, published in 1901 and1902, studied intermolecular forces by applying phenomenological thermodynamics. From the start of his career Einstein was deeply impressed, as Martin Klein has emphasized, by the simplicity and scope of classical thermodynamics. But for him thermodynamics included the statistical approach he had learned from Boltzmann’s works, and he began to develop statistical thermodynamics. The result was a series of three papers published in 1902,1903 and 1904.They provide the clue for understanding his 1905a on quanta,1905b work on Brownian motion and 1905c paper on STR. In describing 1902-1904 papers I’ll follow Kuhn’s 1978 masterpiece. The first important result consisted in the fact that for physical systems of extraordinary general sort Einstein has produced, by the summer of 1903, both a generalized measure for temperature and entropy, containing some universal constant χ.By the time he finished his 1903 paper, Einstein had recognized that χ could be evaluated in terms of the values of the gas constant and of Avogadro’s number. But the theory that had led him to the constant was, however, applicable to systems far more general than gases. It should therefore have a correspondingly general physical basis. The basis should reflect statistical-mechanical nature of the approach that led him to the constant, explaining not only its role as a scale factor for temperature, but also its position as a multiplier in the probabilistic definition of entropy. Physical significance of χ was the central problem attacked in Einstein’s third statistical paper, submitted to “Annalen” in the spring of 1904.Solution of the problem consisted in the phenomena of energy fluctuations. Einstein demonstrated that ‾ε2 = 2χ Td‾E/dT, where ‾ε2 is a measure of thermal stability of the system. And it was recognition of the constant χ physical role that directed his attention to the black-body problem.

 ”The equation just found would permit an exact determination of the universal constant χ if it were possible to determine the energy fluctuation of the system. In the present state of our knowledge, however, that is not the case. Indeed, for only one sort of physical system can we presume from experience that an energy fluctuation occurs. That system is empty space filled with thermal radiation” (Einstein 1904, p. 360; translated by Kuhn 1978).

At least one more step in development of the programme of statistical thermodynamics was needed, and Einstein took it in a famous paper published in the following year, in 1905. Its content strongly suggests that Einstein had begun to seek a black-body law of his own, that he had quickly encountered the paradox - contradiction between statistical mechanics and maxwellian electrodynamics - and that he had dropped the search for the law in favour of an exploration of the paradox itself. This is clear from the very beginning of his paper which was already quoted (translated by Ter Haar, 1967).The first part of 1905a ended by discovery of “ultraviolet catastrophe” . How did Einstein resolve the paradox? In the second part of his 1905a Einstein applies thermodynamics (dS=1/T), statistical mechanics (S=k log W) and maxwellian electrodynamics (E=V ∫ ρν dν ) to describe the domain of empirical reality covered by Wien’s radiation law. Einstein takes β = h/k = Nh /R as undefined constant in 1905a paper and hence he writes Rβ/N everywhere instead of h. Joint application of the three fundamental theories enables Einstein to arrive at apparently deductive argument: if monochromatic radiation of frequency ν and energy E is enclosed in the volume V0, then the probability W that at any moment all the radiation energy will be found in the partial volume V of the volume V0 is given by W = (V/V0)E/hν (i) Yet in the same paper Einstein demonstrates that in the case of n independently moving particles enclosed in a volume V0 the probability of finding them all momentarily in the sub-volume V is W = (V/V0)n (ii) Comparing (i) and (ii), Einstein comes to a conclusion that *“monochromatic radiation of small density behaves in thermodynamic respects as though it consists of distinct independent energy quanta of magnitude hν“.*He did not appreciate that the existence of photons was rendered more than a plausible hypothesis by the derivation of eq.(i).Only 66 years later Jon Dorling (1971) convincingly demonstrated that the argument by analogy which Einstein used to introduce photons is as a matter of fact redundant and that his conclusion already follows deductively from what he had already established. As had been pointed out by Igor Yu. Kobzarev,

“applying Boltzmann’s rule, Einstein сould think that the main principles of classical mechanics are valid for field of heat radiation, but Maxwell’s equations - are not...In general, all the situation appeared to Einstein as development of classical atomistic. Maxwell and Boltzmann had introduced atoms instead of continuous media; now the same was to be done for electromagnetic field which is something like the gas consisting of interacting photons”(Kobzarev 1979,p.18).

Many of Einstein’s contemporaries described genesis of early quantum theory in the same way. For instance, Arthur Haas in a sequence of lectures delivered in Wien in winter 1919-1920 and in summer 1920 in Leipzig had pointed out that

“quantum theory was created because of intention to extrapolate the atomistic principle, that appeared to be very fruitful in the domains of matter and electricity, on more abstract questions. Classical theory of heat and classical electron theory applied atomistic principle to the carriers of physical phenomena; the quantum theory applied the atomistic principle to the domain of physical processes themselves”(Haas 1924,p.68).

Dmitry Goldgammer (1923, pp.118-120) underscored that

“ it is very curious that the idea of quantum should be born fifty years ago, when kinetic theory of matter was created, since this idea is inseparably connected with molecular structure of matter and is nothing more than manifestation of this molecular structure...By analogy explanation of physical properties of bodies from the point of view of atomic and molecular motions for a long time was independent of explanation of light and sound radiation . In both domains, from a well-known point of view, the explanations were simple. *But science brought them together, and quanta of energy occurred as a result.* In both cases the situations became more complicated, at least at first sight; but, due to idea of quanta, these phenomena, that stood independently , were unified, so to say, under the common flag of molecularity of energy and matter”.

The most important feature of Einstein’s programme consisted in persistent application of Boltzmann’s ideas to radiation. The following story is of special importance here (De Broglie 1962,p.139).Planck, who intended to broaden the domain of validity of statistical thermodynamics, developed (with a help of classical continuous notions) thermodynamics of electromagnetic radiation and tried to introduce entropy of radiation by analogy with its energy. Being an admirer of famous Ludwig Boltzmann, Max Planck informed the founder of statistical mechanics about the investigations done, and presented one of his papers to Boltzmann’s judgement. However, Boltzmann answered that Planck ***would never be able to create a correct theory of statistical thermodynamics of radiation without introducing previously unknown element of discontinuity into processes of radiation.*** Thus, the conclusion that radiation in the cavity consists of independent energy quanta follows directly from application of general principles of thermodynamics and statistical mechanics to processes of radiation. It can be independently confirmed by studying J. Larmor’s “Baikerian” lecture “*On the Statistical and Thermodynamical Relations of Radiant Energy*” that was read in 1909.The lecture contained an analysis of the papers of Planck and Lorentz on black-body radiation .But the crux of the lecture was the study of the well-known paradox according to which application of consecutive principles of classical statistical mechanics to energies of Planck’s oscillators is possible only under the condition h → 0.And what were Larmor’s methods of resolving the paradox?

“The motive of this present discussion is the conviction expressed at the beginning, that the statistical method, in Boltzmann’s form, must in some way hold the key of the position, no other mode of treatment sufficiently general being available. The writer has held to this belief, with only partial means of justification, ever since the appearance in 1902 of Planck’s early paper extending that method to radiation. In the “British Association Report”, 1902,p.546, there is a brief abstract of a communication “On the Application of the Method of Entropy to Radiant Energy”, in which it was essayed to replace Planck’s statistics of bipolar vibrators by statistics of elements of radiant disturbance. ’It was explained that various difficulties attending this procedure are evaded and the same result obtained, *by discarding the vibrators, and considering the random distribution of the permanent elements of the radiation itself, among the differential elements of volume of the enclosure, somewhat on the analogy of the* *Newtonian corpuscular theory of optics*” (cf. “British Association Report, 1900)” (Larmor 1909,p.95; italics added).

Thus, “if the monochromatic radiation (of sufficiently small density) in the sense of entropy dependence upon volume behaves itself as a discontinuous medium, consisting of energy quanta Rβν/N, a question occurs: *if they are not the laws of* *creation and conversion of light such as if it consists of similar energy quanta*?” (Einstein, 1905a, p.236; italics added). That is the question put up by Einstein at the end of one of the sections in his 1905a. *But the ether conception prevents the positive answer.* Indeed,

 “mechanical and purely electromagnetic interpretations of optical and electromagnetic phenomena have in common that in both cases electromagnetic field is considered as a special state of hypothetical medium filling all the space. Namely in that point two interpretations mentioned differ radically from Newton’s emission theory, in which light consists of moving particles. According to Newton, space should be considered as possessing neither ponderable matter, nor light rays, i.e. absolutely empty” (Einstein 1909, p.198).

*To create quantum theory of radiation, one needs electromagnetic fields as independent entities that can be emitted by the source “just as in Newton’s emitting theory” (i.e. energy transmitted in process of emission should not be dissipated in space, but should be completely preserved until an elementary act of absorption).*But within the Lorentz programme electromagnetic field is considered as a specific state of ether - a state of medium that is continuously distributed in space. An elementary process of radiation is connected in such a medium only with a spherical wave. Moreover,

“while in the molecular-kinetic theory there exists the reverse process for each one, in which only few elementary particles take part (for instance, for each collision of molecules), the picture is quite different for elementary processes within the wave theory. According to it, an oscillating ion radiated an outgoing spherical wave. The reverse process, as elementary one, does not exist. Nevertheless, the ingoing spherical wave is mathematically possible; yet its approximate realization needs a great number of elementary radiating centres. Hence, an elementary process of radiation is irreversible. It is in this case where, to my mind, our wave theory does not fit reality. It seems to me that in this point Newton’s emitting theory is more valid than the wave theory of light” (Einstein 1909).

Joseph Illy, historian of science, had pointed out (in 1981) that many Einstein’s contemporaries also thought that the rejection of ether leads to corpuscular theories (Lodge 1914;Snyder 1907,p.111; the latter wrote that “Newtonian corpuscles again appear at the sake of ether’).In general the idea of light particles was in the air of the beginning of the 20-th century. In April 1905 Poynting punctuated:

“A 100 years ago, when the corpuscular theory held almost universal sway, it would have been much more easy to account for and explain the pressure of light than it is today, when we are all certain that light is a form of wave-motion. Indeed, on the corpuscular theory it was so natural to expect a pressure that numerous attempts were made in the 18-th century to detect it... But had these 18-th century philosophers been able to command the more refined methods of today, and been able to carry out the great experiments of Lebedev and of Nichols and Hieb; and had they further known of the emission of corpuscles revealed to us by the cathode stream and by radioactive bodies, there can be little doubt that Young and Fresnel would have much greater difficulty in dethroning the corpuscular theory and setting up the wave theory in its place” (Poynting 1905, p.393).

It should be stressed that the rejection of ether and acceptance of “emission theory” is not equivalent to acceptance of two basic postulates of STR. Rejection of ether and acceptance of emission theory are compatible with Walter Ritz’s 1908 theory. According to his “ballistic hypothesis”, velocity of quantum should depend on the velocity of its source. In Ritz’s theory velocity of light is not constant, but is equal to v+c, where v is a relative velocity of the observer and the source. Ritz’s theory was in certain respects a return to old conceptions of action-at-a-distance. It was analogous to theories of Weber and Riemann. Ritz rejected basic notions of the Maxwell-Lorentz electrodynamics. He threw out the notions of electric and magnetic field and began operating with notion of direct interactions between the charges only. In his theory the force of interactions depended upon the distance between the charges and upon their states of motion. After successful explanation of the sequence of optical and electric phenomena (in particular, the experiments of Michelson & Morley, Trouton & Noble, Kaufmann, etc.), the theory gave an original interpretation of “ultraviolet catastrophe” (see Ritz’s discussions with Einstein) and even obtained the equation of anomalous precession of Mercury perihelion that coincided with observation data (but, alas, contained the constant that should be determined from experiment).However, the theory met with significant difficulties explaining double stars observations (Ehrenfest,1913).In fact, one should speak about Ritz’s reductionist programme (reduction of mechanics to electrodynamics) .M. Abraham, J. Kunz (1910),R. Stewart (1911),G. Trowbridge (1911), R. Tolman (1912), I. Laub (1912) et al. made important contributions to development of this programme. The papers of Richard Feynman and John Wheeler owed much, as the authors had admitted, to Ritz’s ideas. Ritz’s theory met insurmountable difficulties in explaining experimental data (for instance, two of three emission theories were refuted just after their proposal - see Tolman 1912, p.143).Moreover, it was too phenomenological (containing more than 10 constants to be determined from experiments).But maybe in future the theory could advance. However, Ritz died in 1909, leaving his theory in unfinished form.

“Abovementioned notes on the emission theory or the theory of ether drag clearly indicate that the interpretation of the Michelson negative result by the contraction hypothesis is not a single one in logical respect. But history made a different choice” (Ehrenfest 1910, p.81).

In any case, in the form developed by Ritz, his theory directly contradicts modern experiments with μ and ν mesons, as well as the measurements of electron velocities dependence upon their masses made on accelerators (Fox 1965, pp.12,16). Thus, in Ritz’s ballistic theory velocities of light and of its source should add in accordance with addition law of classical mechanics (the Galileo formula). Yet it contradicts the field conception, on which Maxwell’s theory is based. Indeed, in Maxwell’s theory the finite velocity of electromagnetic perturbations in vacuum is independent of their forms and of the velocities of their sources. But, opposite to Ritz, Einstein never thought of rejecting Maxwell’s theory, just as Newton, author of emission theory, did not reject the wave theory 300 years earlier. In his 1905a photon paper Einstein had specially pointed out that “wave theory operating with point continuous functions is excellently justified when describing purely optical phenomena and perhaps would not be replaced by another theory” (p.237). In Lorentz’s theory this difficulty did not exist at all. Indeed, in the reference frame, that is at rest relative to ether, light propagates with constant velocity independent of the velocity of the source. An analogy with water waves is especially appropriate here, since in the first approximation their velocities do not depend on the velocity of the ship that creates them. Hence, *if one wants to give up the idea of ether, but to retain Maxwell’s theory at the same time, he should disown ballistic hypothesis and postulate a special “principle of constancy of velocity of light”.* In a conversation with Robert Shankland, Einstein told him that he had consider the ballistic hypothesis as a possible version but left it before 1905 since he could not find differential equation, the solution of which represented waves with source-dependent velocities (Shankland 1963). *The second fundamental principle of STR - “the principle of relativity” - follows immediately from the “fact” that there is no ether and, consequently, no absolute system of reference.* The two postulates - the relativity principle and the principle of light constancy - are quite sufficient, according to Einstein, to create the electrodynamics of moving bodies. Namely these statements were chosen as the foundation of the hard core of Relativity Sub-programme described thoroughly by Zahar. Yet, for “the theory based on these two principles not to lead to contradictory consequences, it is necessary to reject the common rule of velocities’ addition” (Einstein 1910). And namely that was done in 1905c “On the Electrodynamics of Moving Bodies”, published several months after the photon paper. Einstein had revealed the hidden assumption - the basis of the Galileo addition law - that the statements of time, as well as of the shapes of moving bodies have the sense independent of the state of motion of the reference frame. He demonstrated that the acceptance of “principle of relativity” together with the “principle of constancy of light” is equivalent to modification of the simultaneity concept and to clock delay in moving reference frame. For comparison of 1905a and 1905c the following observation of Gerald Holton is important. When 1905c paper was published, Einstein was persistent refusing to call it a new theory. And only after it was christened so by Planck in 1907, he began to call it publicly as “the so-called theory of relativity”. For example, in the first (brief) review of 1907 he presented his work as “unification of Lorentz’s theory with relativity principle “(quoted from Holton 1980, p.57).

“Thus, the theory of relativity changes our views on the nature of light in the sense that light steps forward in it without any connection with a hypothetical medium, but as some thing that exists independently, similar to matter. Then this theory, in analogy to the corpuscular theory of light, differs in that it acknowledges mass transition from a radiator to absorber” (Einstein 1909).

So far so good. Nevertheless, if all these is true, a question arises: why did Einstein in his STR paper not cite his paper on light quanta? - Writing to his friend Conrad Habicht in **1905** and sending him the fruits of his labours at that time, Einstein called his light quanta paper “very revolutionary”, while he noted that the relativity paper might be interesting in its kinematical part. The contemporaries saw it in the same way. ”The spirit of revolution is seen at its boldest in the theory of radiation” (Mac Laren 1913,p.43). Moreover. Reference in the paper, introducing significant changes mainly of metaphysical character, on the hypothesis that had already introduced revolutionary changes and had contradicted Maxwell’s theory, could hardly make the arguments stronger. Even in 1916 R. Millican declaired that “despite ...the apparently complete success of the Einstein equation (for the photoeffect) the physical theory of which it was designed to be symbolic expression is found to be so untenable that Einstein, himself, I believe, no longer holds it”. Einstein himself at the first Solvay Congress had to admit “provisional character of this concept (light quanta) which does not seem reconcilable with the experimentally verified consequences of the wave theory” (quoted from Pais1979, p.884).The situation was even complicated also since **direct** experimental evidence in favour of light quanta hypothesis was absent. It appeared only in 1923 (the Compton effect).Hence German scientists, for instance, did their best to elect Einstein to Berlin Academy of Sciences in 1914, but had to make a reservation that his defence of light quanta was an unavoidable price which should be payed for his creative genius. However, negative attitude of pre-Compton scientific community to photon hypothesis should not be exaggerated ( see Wheaton 1983, and T.S. Kuhn’s Foreword). On the contrary, Abraham Pais insists that “from 1905 to 1923 he (Einstein) was a man apart in being the only one, or almost the only one, to take the light quanta seriously”. Was it really so? But how can one explain then a large amount of American publications proposing various emission theories - the papers of Comstock (1910), Tolman (1910),Stewart(1911),Trowbridge(1911), Laub(1912) and others? Just to quote Jacob Kunz (1910,p.314) “whereas the principle of relativity and the theory of radiation of Planck assume discontinuities in the emission and the absorption of light, there are many optical phenomena pointing towards a corpuscular constitution of light and Rontgen rays”. And how G.N. Lewis’s theory (see the next part) should be understood? Furthermore, the following remark of Einstein’s centennary conference participant is especially convincing.

“H.D. Smith (Princeton University).The point I wish to put before Professor Klein is based on my understanding of his comment that the community of physics did not really accept the idea of the particle nature of light until the Сompton-effect experiment. I was taught atomic physics here at Princeton in 1918 and 1919, and there was not any question about this. We knew the Millikan experiment; we knew the various other experiments that had tried to get the maximum energy of photoelectrons; we knew of the experiments of Franck and Hertz. The whole experimental program of Karl Compton really was based on the idea of the particle nature of light at least as a working hypothesis.

Klein. Well, I cannot quarrel with your memory...”(Klein 1980,p.193).

Smith’s evidence is confirmed by another direct participant at the events - by Walter Gerlach (1979, p.191).

In the 1911 review “*Modern Hypotheses on the Nature of Light*” French physicist Leon Block indicated that:

 “In any case, as we have already mentioned at the beginning, the new hypotheses are interesting not because they threaten the old doctrines. On the contrary, one can hope that the part of the truth, contained in the atomistic theory of light, will be fused easily with fruitful ideas of Einstein and Lorentz” (Block 1911,p.253).

Russian physicist B.V. Ilyin (from Peter Lebedev’s laboratory) had pointed out (1913,p.143) that

“...at present time much new and interesting was brought into the theory of photoelectric effect by the so-called theory of atomistic structure of radiant energy...The theory of atomistic structure of light cannot be considered as completed; too many aspects of it are obscure ; but unquestionable value of the theory consists in that it generalizes, embraces by a single formulae the whole sequence of various physical phenomena”.

In a traditional address to British Association at Birmingham (August 1913) sir Oliver Lodge declared:

 “Furthermore, radiation reveals also the properties, according to which one is eager to admit it as atomic or discontinuous; corpuscular radiation theory is not as dead as it seemed in my youth. Some radiations are undoubtedly corpuscular and even show the ether properties, maybe erroneous, or spotted nature” (Lodge 1914, p.15).

So, it’s no wonder that Einstein’s 1905a photon paper differs from 1905c STR work both in a **more careful heading** - “*On Heuristical Point of View*...” and **by less definite tone of the main conclusion: “**In the following**,** I shall communicate the train of thought and the facts which led me to this conclusion, in the hope that the point of view to be given ***may* turn out to be useful for some research workers in their investigations”(**paper on light quanta). Compare with: “Insufficient understanding of these peculiarities ***is*** the root of the difficulties that have to be overcome by electrodynamics of the moving bodies” (paper on STR). Thus, Einstein had sober reasons not to cite his photon paper in the STR exposition. But of course his hidden sympathies with light quanta manifest themselves through neutral description of electrodynamics of moving bodies. In his STR paper Einstein uses the evasive term ‘*light complex*’. The paragraph number 8 of his 1905c uses the ratio of amplitudes obtained in the preceding paragraph to obtain the ratio of energies of the fields within some closed surface in the two coordinate systems. The ratio just coincides with that of frequencies; of course, there could be no contradiction between STR and the photon theory. Well, if Einstein had grave reasons not to reveal the link between 1905a and 1905c at the beginning of the 20-th century, why he did not confess about the link much later, after the Nobel Prize, for instance, when his photoeffect formula were confirmed and Compton’s experiment had been performed already? - Einstein’s activity in General Relativity can provide the clue for the answer. Writing to Lorentz at June, 1916, he admits that “general relativity is nearer to ether hypothesis than special theory of relativity”(quoted from Illy,1987).Why? - The answer can be found in his 1918 paper:

“The general theory of relativity does not know of a preferred state of motion in a point which could be interpreted as a so-to-say velocity of an ether. While, however, according to special relativity a region of space without matter and without electromagnetic field proves to be absolutely empty, i.e. characterized by no physical quantities, according to the general theory of relativity even the space, empty in this sense, has physical properties, mathematically characterized by the components of gravitational potential, which determine the metric behaviour as well as the gravitational field of this region of space. We can comprehend this situation by speaking of an ether whose state always varies from point to point” (quoted from Illy 1987, p.41).

The idea that space has physical properties (i.e. that independent existence is to be attributed to the ether) was for Einstein connected with a possibility that a metric can exist without matter. He was undoubtedly influenced by Lorentz. Einstein wrote him in a letter of November 15, 1919:

“I shall expound my standpoint in the problem of the ether exhaustively as soon as opportunity presents itself. *It would have been better could I have restricted myself in my earlier publications to insisting that it is the velocity of the aether which is not real instead of having defended at all the non-existence of the aether.*Because , since, I have realized that with word aether nothing is said but that the space must be considered as the carrier of physical qualities” (quoted from Illy 1987,p.54; italics added).

Now it is clear why , after creating the STR on the quantum basis, Einstein did not like to remember the real facts later. This is because of his General Relativity and his passionate beliefs in fundamental nature of metric. Until his last days he was an adherent of the programme that reduced everything to geometry. Photon was not an exception. He hoped through all his life that a suitable unified geometrical theory would explain quanta also. For instance, in 1927 letter to Sommerfeld he confessed:

“On quantum mechanics I think that, with respect to ponderable matter, it contains roughly as much truth as the theory of light without quanta. It may be a correct theory of statistical laws, but an inadequate conception of individual elementary processes” (quoted from Weingard 1987).

 So, in 1905c paper on STR, Einstein refers neither to light-quanta paper, nor to the black-body paradox. Instead he starts it describing the asymmetry between the motions of the conductor and the magnet. The asymmetry is obvious to anyone knowing that ether and absolute frame reference do not exist at all. However, *without necessary connections with quantum 1905a paper the STR postulates can be evaluated as ad hoc* *hypotheses*. And they were (Goldberg 1967)! Thus, Einstein did his best to convince the readers that

 “the similar examples, as well as unsuccessful attempts to find Earth’s motion relative luminiferous medium, lead to supposition that not only in mechanics but in electrodynamics as well no property of events corresponds to the notion of absolute rest”.

The connection assumed between Einstein’s special relativity and early quantum papers is confirmed indirectly by the following peculiarity, concerning the “*Annalen der Physik*” editorial policy. After Paul Drude’s suicide, Max Planck and Wilhelm Wien became the editors of the journal. While in Drude’s times only 5 - 10% of manuscripts were returned to their authors, owing to Planck’s and Wien’s efforts this number became 15-20%.”Completely without value”, “nothing new’, “contradictions” are evaluations that issue from Planck’s pen (Pyenson 1985,p.200).According to A.I. Miller (1982,p.22) , while an author’s initial contributions to the “Annalen der Physik” were scrutinized by either the editor or a member of ‘curatorium’, *subsequent papers were published with no refereeing.* However, Cullwick (1972) had pointed out that Einstein’s 1905c paper (on STR) has many tiny mistakes. He concluded that it was published without refereeing. The fact can be easily understood, if one takes into account that STR and photon papers were considered by the editors as belonging to one and the same programme! It should be mentioned here that the link between 1905a and 1905c was noticed by Lessen (1974) also. But his approach is reciprocal to the approach presented here. Lessen had demonstrated how Einstein could obtain the hypothesis of light quanta from “more elementary postulate and special theory of relativity”. Since 1905a and 1905c papers deal with the same cross-contradiction between mechanics and electrodynamics, their structures are analogous. This fact was noticed by outstanding American historian of science prof. Gerald Holton (1969, p.181):

“Contrary to the sequence one finds in many of the best papers of the time, for example H.A. Lorentz’s 1904 publication on electromagnetic phenomena, Einstein does not start with a review of a puzzle posed by some new and difficult-to-understand experimental facts, but rather with a statement of his dissatisfaction with what he perceived to be formal asymmetries or other incongruities that others might dismissed as being of predominantly aesthetic nature. He then proposes a principle of great generality. He shows that it helps to remove, as one of the deduced consequences, his initial dissatisfaction. And at the end of each paper he proposes a small number of predictions that should be experimentally verifiable, though the test may be difficult” (Holton 1980, p.55).

***Part five .Early Quantum Theory as a Decisive Argument Favouring Einstein over Lorentz.***  Being taken independently, the STR did not explain any **new** experimental fact. Predictions of the Lorentz theory were identical to that of the STR, so that it would not be possible in any case to distinguish between these theories on experimental grounds. Moreover, most of Einstein’s contemporaries wrote about “Lorentz-Einstein electron model”, about the “principle of relativity of Lorentz and Einstein”, etc. At the time of publication of Lorentz’s second order theory (1904) the only data available to test these theories were Kaufmann’s measurements of the masses of slowly moving electrons. But they were initially interpreted as contradicting **both** STR and Lorentz’s theory. It took a year for Einstein to answer on Kaufmann’s paper. One can imagine how the STR was evaluated by the scientific community in 1905 - 1906! Furthermore, Einstein did not reveal the connections between 1905a and 1905c until 1909.However, *without this connection his STR postulates can be evaluated as ad hoc hypotheses and they were!* - The reaction of Poincare and of the French school is the most obvious example.- Hence to explain the reasons for Einstein’s victory over Lorentz , comparison of Ether Programme with Relativity Programme is insufficient. One should consider the successes of Einstein’s statistical papers and the development of the quantum sub-programme. Firstly, an important stage of realization of Einstein’s programme consisted in application of the apparatus of his statistical thermodynamics to Brownian motion (Einstein 1905b;Gerald Holton was the first to notice the connection of this paper with 1905a and 1905c).Einstein had obtained the first results that could be verified by experiments. As in 1905a and 1905c, he reveals and tries to resolve the cross-contradiction between thermodynamics and statistical mechanics. In his next (1906) paper on Brownian motion he points out directly that

“according to classical thermodynamics which differentiates principally between heat and other forms of energy, spontaneous fluctuations do not take place; however, they do in the molecular theory of heat. In the following we want to investigate according to which laws those fluctuations must take place” (quoted from Holton 1980, p.55).

 Einstein’s predictions were put into trial soon and were confirmed. The coincidence of theoretical predictions with experimental results was so excellent that even W. Ostwald, one of the most active antagonists of the atomic theory, already in 1908 was compelled to admit that

“the Brownian motion coincidence with kinetic hypothesis demands ... gives to most careful scientists a right to declare the experimental confirmation of atomistic theory of matter”(Ostwald 1909,p.4).

 The history of quanta starts from Planck’s attempts to bridge the well-known gap between thermodynamics, statistical mechanics and Maxwell’s theory. And it was his quantum theory that appeared a product of interaction of these three famous “themes” of 19-th century scientific research. Before 1900 Planck has made important contributions to all of them. Thermodynamics was his “first love” (T. S. Kuhn).His work in it was well-known before he turned, at the age of 36, to electrodynamics. It is important that statistical technique entered Planck’s research later and against much resistance (Kuhn,1978).He began to study Boltzmann’s works with care only in 1897-1898.Unfortunately, he did not explicitly acknowledge his change of mind for almost 2 years, a delay that has reinforced the almost universal impression that his conversion to a statistical viewpoint was intimately associated with his introduction of the quantum hypothesis at the end of 1900.But only opposite statement is true: *Planck’s introduction of the quantum hypothesis is a firm and unavoidable consequence of his conversion to a statistical viewpoint, of application of Boltzmann’s technique and ideas in the study of radiation.*

It was the fact of origin of early quantum theory from the clash between classical electrodynamics and statistical mechanics that was indicated by one of the leading Russian theorists of the XX-th century beginning:

“But the most curious thing is that the quantum idea should be born half a century ago, when the kinetic theory of matter was created, since this idea is intimately connected with molecular structure of matter and is a specific reflection of this structure”(Goldgammer 1911).

 Max Planck was a cautious man who tried to avoid extreme conclusions. As was clearly demonstrated by Kuhn, Planck’s first quantum papers were not an attempt to supply an entirely new theory. On the contrary, they aimed to fill a previously recognized gap in the derivation of Planck’s older theory. His derivation of the famous radiation law that could pass the test of new, more refined experiments of Lummer & Pringsheim, remained firmly within classical tradition. Both in his original papers and far more clearly, in his 1906 book “*Lectures on the Theory of Radiation*”, Planck ‘s radiation theory is incompatible with the quantization of resonator energy. This theory does not require fixing the sizes of the small intervals into which the energy continuum is subdivided for purposes of combinatorial computation. In Planck’s theory resonators’ emission and absorption are governed by Maxwell’s equations. The motion of Planck’s resonators remains continuous. Nothing in his papers, manuscripts and notebooks suggests that the idea of restricting the resonator energies to a discrete set of values had even occurred to him until others forced it upon him during 1906 and the years following. Planck indeed does repeatedly write expressions like UN =Phν with P an integer number. But in such formulae UN is the total energy of N resonators. *Restricting it to integral multiples of hν does not impose any similar restriction on the energy of an individual resonator, which may vary continuously.* Planck’s subdivision of total energy into an integral number of equal finite elements is entirely modelled on Boltzmann’s.

“Planck derived his expression for black-body radiation energy with E = hν contained in the formula, but did not notice the sense that this behaviour has because the energy elements are finite, and only later Einstein pointed out that one cannot obtain the true law of black-body radiation energy without hypothesis of quanta”(Goldgammer 1912,p.118).

Conclusion on Planck’s conservative standpoint is supported by the following fact. It was Planck himself who proposed the hypothesis “saving” the ether conception from the Michelson-Morley destructive results. He did suppose a significant condensation of ether near the Earth. The importance of this hypothesis was appreciated even by Lorentz and Poincaré. Poincaré contrasted it to “ad hoc Lorentz - Fitzgerald contraction” ,while Lorentz remarked that “if the treatment of the influence of the second order terms met with insurmountable difficulties there would be no way out of this situation but that which had been proposed by Planck” (quoted from Hirosige,1976,p.3). Einstein’s arguments for light quanta presented in his 1905a are completely different from those of Planck given 5 years earlier. Contrary to Planck, in 1905a Einstein proceeds from the Wien law, using only Boltzmann’s law. He cites Planck twice. But one of this citations points to the paper written before 1900.In the second citation Einstein quotes Planck’s distribution law but only as an expression, adequately describing experimental radiation spectra. *What brought Einstein to idea of photon was a coherent development of a research programme started in 1902, a programme “so nearly independent of Planck that it would almost certainly have led to the black-body* law even if *Planck had never lived*” (Kuhn 1978,p.171). From the start of his career, Einstein was deeply impressed, as Martin Klein has frequently emphasized, by the simplicity and scope of classical thermodynamics. In that respect he was like Planck. But they differed radically in the attitude to statistical approach, and it is this difference that explains their conservative and revolutionary standpoints, respectively. *For Einstein thermodynamics included statistical approach he had learned from Boltzmann’s famous “Gas Theory”.* While Einstein took statistical approach **seriously**, Planck treated it in merely instrumental way. Einstein’s two first papers, published in 1901 and 1902, attempted to study intermolecular forces by applying phenomenological thermodynamics to such phenomena as capillarity, etc. Finding the results obtained inconclusive, Einstein abandoned the phenomenological approach and began instead to develop *statistical thermodynamics* applicable not only to gases, but to other states of aggregation as well. In 1906 Ehrenfest and Einstein were the first to recognize that Planck’s blackbody law could not be derived without restricting the resonator energy to integral multiples of hν. Ehrenfest’s conversion to quanta was not accidental. According to Martin Klein, in 1899-1900 Ehrenfest attended Boltzmann’s lectures on the mechanical theory of heat. His first publication was a paper dealing with a small point in the theory of gases, which Boltzmann presented to the Royal Academy of Sciences on July,1 903. Ehrenfest’s thesis “The Motion of Rigid Bodies in Fluids” Boltzmann characterized as “very fundamental”, “diligently and cleverly worked out”. When Ehrenfest cited Boltzmann’s work in a particularly complete way, Boltzmann remarked: ”If only I knew my own work that well”(quoted from Klein,1970,p.48). *Ehrenfest’s interests were in statistical mechanics;* so, at least initially, quantum theory seemed to him to be a branch of statistics. Even in his 1911 paper “Which Features of the Hypothesis of Light Quanta Play an Essential Role in the Theory of Thermal Radiation?” Ehrenfest explained why energy quanta were proportional to frequency .The origin consisted in the requirements of the second law of thermodynamics in its statistical form. Until 1908, Einstein’s and Ehrenfest’s demonstrations had little apparent impact (Einstein’s photon paper was the first sympathetic response to Planck’s blackbody investigation).But the paper, presented by Lorentz in 1908, caused a profound change in the attitude of the community towards the quantum: “... one cannot escape Jeans’s conclusion, at least not without profoundly modifying the fundamental hypothesis of the theory”(Lorentz 1908,p.160). The Rayleigh-Jeans law and the “ultraviolet catastrophe” did not initially pose problems for more than two or three physicists. But finally they became central in physics due to their repeated rederivation by a variety of different technique. Lorentz’s paper appeared in the early spring of 1908.By the end of the following year, Lorentz, Wien and Planck himself had been persuaded that radiation theory demanded discontinuity. Arnold Sommerfeld and James Jeans were moving towards that position in 1910, the year Lorentz provided particular clear and widely appreciated arguments for it. After 1910 leadership in quantum investigations passed to specific heats at low temperatures. Up to 1911, the Roentgen radiation, photoeffect (Stark’s and Barkla’s experiments), luminescence, atomic theories became important domains of application of the early quantum programme. They all had provided constant empirically progressive problemshift. A serious success became Nernst’s 1911 confirmation of Einstein’s 1907 specific heats formulae. ”If Planck’s theory strikes to the heart of the matter”, then one should, according to Einstein, make a fundamental change in the foundations of statistical mechanics. Quantum discontinuity appeared to be connected not only with interaction of matter and radiation. But what about the oscillators that appear in the molecular theories? - They too must obey the quantum restrictions in direct contradiction to classical statistical mechanics. Einstein found confirmation in the departures of some specific heats from the Dulong-Petit rule, that went against the equipartition theorem. Thus, Einstein’s specific heat theory was a statistical-mechanical one, independent of electrodynamics. He has quantized the energies of neutral atoms also. In 1907 Wien’s theory was confirmed. He considered the radiation of moving charged particles with the help of Planck’s theory. In the same year, Wien used his theory to analyse the Roentgen spectra. His predictions were confirmed in 1912, when X-ray diffraction was found. At the same time Paul Ehrenfest demonstrated that if one is to describe radiation by particle representation, then the “particles” one uses must have properties substantially different from those of any particles previously used in physical theories. The new particles are not independent, but must show a kind of correlation. Ehrenfest’s results agreed with that of Einstein obtained in 1909. The first Solvay Congress (1911) definitely enough revealed the inability of classical mechanics and classical electrodynamics to solve the problems concentrated in the radiation theory. ”The result of the discussion of these questions seems to be a general acknowledgement of the inadequacy of the classical electrodynamics in describing the behaviour of systems of atomic size” (Bohr 1913,p.2). Thus, *in spite of the fact that the light quanta hypothesis had to wait for general recognition for more than 10 years, the quantum theory successes had cut the ground from the feet of the wave theory and ether conception that had constituted the foundation of it.* Elizabeth Garber (1976, p.123) had already pointed out that of all the men who worked upon the blackbody radiation problem both before and after 1900 all but one (Rayleigh) in some way concerned themselves also in questions of relativity. Even Jeans’s first paper, in which he accepted the theory of quanta, had a relativistic ring to its title: ”Planck and non-classical mechanics”. Physicists of various countries and different cultural traditions had indicated that it was quantum theory that led to ether rejection. At first, Norman Campbell , Fellow of Trinity College, Cambridge, began his 1910 paper by stating that

“the position of the conception of ‘aether’ in modern physics is anomalous and unsatisfactory...*No doubt much of the dissatisfaction with the ‘aether’ is based on the recent theories of the atomic nature of radiation* and on the proof that the principle of relativity is an adequate foundation of electromagnetic theory...” (Campbell 1910, p.181; italics added).

Conclusion of the paper is no less exciting:

“My object is not to marshal all the arguments that might be brought against the use of that concept, but only those which appear to me especially destructive at the present time. *The recent work of Bucherer, and the atomic theories of J.J. Thomson and Planck (the latter recently developed by Stark so as to resemble the former very closely) will be found very difficult to the believers in the aether to assimilate or to explain away;* if they attempt to do so it will doubtless be in the belief that the concept of the aether is worth retaining” (Campbell 1910,p.189; italics added).

 In the USA one of prominent emission theorists Jacob Kunz have indicated that

 “while the electromagnetic wave theory of light accounted for the groups of phenomena of reflection, refraction, interference, polarization, etc., difficulties were found in the explanation of the aberration, and of the experiments of Airy, Fizeau and Michelson-Morley... The principle of relativity, giving up ether, points towards elements of electromagnetic energy, that have a certain analogy with material particles. Independently of the principle of relativity, the theory of radiation of the black body has been developed by Lorentz, Planck, Larmor , J.J. Thomson and others...*Whereas the principle of relativity and the theory of radiation of Planck assume discontinuities in the emission and absorption of light,* there are many optical phenomena pointing towards a corpuscular constitution of light and Roentgen rays. This is so much so, that a special corpuscular theory of Roentgen rays has been developed by Bragg, who considers them as made up of doublets of positive and negative particles” (Kunz1910,pp.313-314).

In Russia, Paul Ehrenfest had finished his paper called “*The Crisis in Light Ether Hypothesis*” by pointing out the group of “sophisticated questions that maybe takes the most important role in future of the ether hypothesis ; I have in mind the group of tangled questions connected at present with the war-cry ’atoms of light’”(Ehrenfest 1913,p.161). In the well-known, widely read and highly praised (by Einstein, for instance) textbook “*The Course of Physics*” Orest Chwolson analysed

“the series of propositions that are obtained either as consequences of theory of relativity, or emerged in tight connection with it. 1. The aether does not exist.....

5. The energy possesses the inertial mass; it is analogous to matter and the transformations of ponderable mass to energy (and vice versa) are possible. 6. Energy can exist independently of some material, in the most general sense of the word, substratum. It can be emitted and absorbed by the bodies and can propagate in absolutely empty space. 7. *Energy can have atomic structure.* It is true, first of all, for radiant energy (see one of the following chapters. Points 5 and 6, taken together, are a return to Newton’s theory of emission, though in modified form” (Chwolson 1912,p.400).

In his professorial speech at Kazan University Annual Meeting (5 November 1910) professor of theoretical physics Dmitry Goldgammer declared:

“Will the aether theory be rejected like many others? This is not an idle question since the trend to get rid of aether exists in modern physics. In Einstein’s theory one cannot find a single word about aether, just as in Minkowski’s theory of absolute world. Maybe it means that the aether is absent, that it is unnecessary? - No... However, Einstein and the others intend to reject the existence of the aether ; they think that light consists of material particles flying in absolutely empty space and not of aether waves, as was proposed by Huygens...The question occurs what are the reasons of rejecting the aether? It is curious, indeed, but the idea that aether does not exist at all has to some extent *empirical foundations*. Let us imagine a shell with a little slit...”

And on describing the notorious properties of black-body radiation Goldgammer turns to Lummer & Curlbaum experiment, explained in 1900 by Max Planck. But the most direct and astonishing evidence in favour of STR and the early Quantum Theory connection is the paper “*A Revision of the Fundamental Laws of Matter and Energy*” published in November 1908 issue of “Philosophical Magazine”. Its author was Gilbert N. Lewis, **who later (1926) invented the notion “photon”,** at that time an Associate Professor of Physical Chemistry at M.I.T. The paper begins as follows:

“Recent publications of Einstein (Annalen der Physik,18,p.639,1905) and Comstock (Philosophical Magazine,vol.15,p.1,1908) have emboldened me to publish certain views which I have entertained on this subject and which a few years ago appeared purely speculative, but which have been so far corroborated by recent advances in experimental and theoretical physics that it seems desirable to subject this views to a strict logical development, although in so doing it will be necessary to modify those fundamental principles of the mechanics of ponderable matter which have remained unaltered since the time of Newton”(Lewis 1908, p.705).

But what were the views that appeared “purely speculative” and why they were not published in the USA first? - In his letter to Robert Millikan Lewis remembered:

“In 1897 I wrote a paper on the thermodynamics of hohlraum which was read by several members of the chemistry and physics departments. They agreed unanimously that the work was not worth doing especially as I postulated a pressure of light, of which they all denied the existence. They advised me strongly not to spend time on such fruitless investigations, all being entirely unaware of the similar and more successful work that Wien was then doing. *A few years later I had very much the same ideas of atomic and molecular structure as I now hold,* and I had a much greater desire to expound them, but I could not find a soul sufficiently interested to hear the theory” (quoted from Kohler 1971,p.351; italics added).

Lewis’s 1908 paper was the first American paper dealing with relativity. It is important Lewis was not a professional theoretical physicist just as Einstein was a clerk in the patent office. Lewis was a physical chemist at Massachusetts Institute of Technology, but a chemist with wide-ranging interests. Besides papers in physics, he published in mathematics and even economic theory (Goldberg 1983).Later, in a letter to Arnold Sommerfeld (12 December,1910) ,Lewis confessed that **he had written the 1908 paper without the knowledge of Einstein’s work.** Someone had pointed it out to him after the fact (Goldberg 1983). In view of STR genesis conception proposed, this is quite reliable. Lewis arrived at relativity from light quanta realizing his own programme initiated before 1897.This hypothesis, of course, needs a thorough historical study. Lewis begins his 1908 paper by postulating that the energy and the momentum of a beam of radiation are due to a mass moving with the velocity of light. From this postulate alone, he demonstrates that the mass of a body depends on its energy content and that, therefore, it is necessary to replace that axiom of Newtonian mechanics according to which the mass of a body is independent of its velocity. On the contrary, the mass of a body is a function of the velocity and becomes infinite at the velocity of light. The equation obtained by Lewis agreed with the results of Kaufmann’s experiments on the relation between the electron mass and its velocity. Lewis obtained the equation E=mc2 which

 “has also been obtained by Einstein (loc.cit.) who derived it from the general equations of the electromagnetic theory, with the aid of the so-called principle of relativity. That a different method of investigation thus leads to the same simple equation we have here deduced, speaks decidedly for the truth of our fundamental postulate”(Lewis 1908,p.708).

As I have already pointed out, the idea of corpuscular theory renaissance was in the air beginning of the 20-th century (see at greater length Rowlands, 1990). Hence

“to anyone unfamiliar with the prevailing theories of light, knowing only that light moves with a certain velocity and that in a beam of light momentum and energy are being carried with the same velocity, the natural assumption would be that in such a beam something possessing mass moves with the velocity of light and therefore has momentum and energy’ (Lewis 1908, p.707).

What is this something? - The flow of light particles, of course. However, “the view here proposed, which appears at first sight a reversion to the old corpuscular theory of light, must seem to many incompatible with the electromagnetic theory. If it really were so, I should not have ventured to advance it, for the ideas announced by Maxwell constitute what may no longer regarded as a theory but rather a body of experimental fact. The new theory is offered, not in any sense to replace, but to supplement the accepted theories of light” (Lewis 1908, p.707).

The author, to my mind, undoubtedly understood all the dangers of pursuing the corpuscular theory of light straightforwardly, and at the end of his paper (p.716) Lewis has to make a reservation:” in the first place it should be noted that, while the theory is consistent with a modified corpuscular theory of light, it does not necessarily imply that light is corpuscular”. However the reservation could not save him from the sharp criticism of Louis T. More, professor of physics at the University of Cincinnatti. He had accused Lewis of attempting “two distinct things: first, to establish quasi-corpuscular theory of light, and second, to explain inertia, wholly or in part, as a function of velocity”(More,1908,p.519).Lewis’s opponent had described all the “difficulties all corpuscular theories of light plunge into when such phenomena as interference, polarization, diffraction, etc. are discussed, as they are not touched upon” (More 1908,p.519). Nevertheless, the job was done and the connection between the theory of light quanta and electrodynamics of moving bodies was revealed and publicly discussed[[3]](#footnote-3).Furthermore, in the next paper “*The Principle of Relativity and Non-Newtonian Mechanics*” (1909) Gilbert N. Lewis and Richard C. Tolman, at that time a student of physical chemistry at MIT, had specially underscored that

”the two laws taken together constitute the principle of relativity...Moreover *the system of mechanics which he (i.e. Einstein) obtains is identical with the non-Newtonian mechanics developed from entirely different premises by one of the present* *authors*” (p.517; italics added).

For instance,

“*the consequences which one of us obtained from a simple assumption as to the mass of a beam of light, and the fundamental conservation laws of mass, energy and momentum, Einstein has derived from the principle of* *relativity and the* *electromagnetic theory*”(Lewis & Tolman 1909, p.512).

In evaluating Lewis’s programme, his 1910 and 1912 papers on the mathematical apparatus of STR are also important. Interpenetration of STR and Newton’s theory of gravity brought to creation of General Relativity. Its explanatory success (Mercury perihelion precession) and enthusiastic support by non-scientific circles also had helped to force out the Classical Physics (Zahar, 1989).Of course, the successes of the General Relativity can appear too modest if we compare them with those of the Early Quantum Theory. This is so much so, if one takes into account that famous bending of light can be explained by any 4-vector gravitational theory in flat space-time (see Rowlands 1990,p.260 for details). The limits of this book do not allow me to give a more detailed account of the successes of the quantum programme. I shall highlight only the most important steps of its development. Firstly, the results of Charles Barkla (1908), later confirmed by E. Wagner, should be mentioned. For X-ray fluorescence, the frequency of the secondary radiation was found to be smaller than the frequency of exciting it primary radiation in agreement with Stokes’s rule. *Einstein’s photoeffect formula was confirmed in 1916* by R. Millikan, who at the same time was sceptical in relation to light quanta (see Mehra & Rechenberg 1982, for details). A leading advocate of light quanta became Johannes Stark. I n 1909 he had treated the X-ray production by the influence of electrons on the anticathode. His treatment was based on the conservation of energy and momentum for the system consisting of an electron and a single quanta. So, in 1910 the STR postulates began to be considered as **independent** principles, and not as consequences from Lorentz’s theory (Illy 1981).It can be said that at that time the “divergence” of Lorentz’s and Einstein’s programmes had begun. The Quantum Sub-programme starts to produce the effects that are difficult to explain by Lorentz’s programme, and begins to “force out” Classical Physics from the domain of research gradually. But from 1905 up to 1910 Einstein’s passionate appeals to throw the ether out were simply ignored. In 1908 and 1909 A. Bucherer obtained experimental results favouring both Einstein’s and Lorentz’s theories and declared at the same time about his belief in immaterial ether (Mc Cormmach 1970). Until 1910 very few physicists did pay attention to quantum theory. This fact is sometimes interpreted in favour of “mathematical” explanation of Einstein’s victory over Lorentz. According to it, STR was universally accepted due to authority of leading mathematicians (Illy, 1981).The latter have established the links between the STR and some domains of geometry. Indeed, many contemporaries had pointed out the mathematical merits of the STR. D.A. Goldgammer, for instance, indicated that

“the ideas of Einstein and Minkowski surprise us not only by their boldness: they are elegant in mathematical respect. Because of that they win all the hearts , and everybody desires them to be true. Most probably these ideas will be true in mathematical respects. I stress the word ‘mathematical’ since mathematical truth is not equivalent to physical one”(Goldgammer 1911,p.159).

In 1920 R. Mises had pointed out that in 1908, due to Minkowski’s efforts, Einstein’s theory began to become a leading one. The influence of the mathematical merits of the STR on its reception by the scientific community is self-evident. However, it should not be exaggerated due to the following reasons. Firstly, as Louis Pyenson had demonstrated, the Gottingen mathematicians accepted Lorentz’s theory, but not the STR. They worked out Minkowski’s four-dimensional absolute space-time theory that differed from the STR in many respects. For instance, it was based on the conception of electromagnetic ether (as is clearly manifested in Minkowski’s 1907 lecture “The Relativity Principle”- see Pyenson 1985). Secondly, all the participants of the Gottingen Seminar, including David Hilbert, Herman Weyl, Felix Klein, wrote for a very *narrow* circle of readers. And their ideas were popular in that circle only. Indeed, even Max Planck wrote about the STR in 1911 that

“ ....no wonder this abstract questions interested the mathematicians only...But the true unprejudiced physicists - the experimentalists - are not hostile to the theory of relativity, but leave the question to develop placidly, and their attitude to it depends on the experimental results”(Planck 1911,p.145).

The other editor of “Annalen der Physik” - Wilhelm Wien - in 1907 began his first exposition of the STR by stating that, opposite to speculations of mathematicians, it was the experimental physics that forced the physicists to change their views on space and time (Pyenson 1985).Wien clearly distinguished Lorentz’s impact from that of Einstein, stressing that the discovery of STR was of “inductive nature”. To my mind the “Gottingen Docenten” - the members of Gottingen electron seminar - did not create the STR because they had ignored the Early Quantum Theory (that was tightly connected with experiments) . Thus, already in September 1911 Arnold Sommerfeld could promulgate that the STR was “the safe possession of the physicist” and the frontier problem of physics became the understanding of Planck’s energy quanta and Einstein’s light quanta. Black-body theory and specific heats were the two quantum topics well established by the end of the period 1911-1912.However, Rontgen radiation, luminescence, Bohr spectra became new important areas of the development of the Quantum Sub-programme. The Bohr theory was one of the last blows for ether-based wave theory. His paper “On the Constitution of Atoms and Molecules” begins by stating the “inadequacy of the classical electrodynamics in describing the behaviour of systems of atomic size”. It contains the lots of expressions like “the failure of classical mechanics”, “obvious contrast to the ordinary ideas of electrodynamics”,etc. One of Bohr’s main conclusions consisted in that

“the intention, however, has been to show that the schematic generalization of the theory of the stationary states possibly may afford a simple basis of representing a number of experimental facts which cannot be explained by help of ordinary electrodynamics, and that the assumptions used do not seem to be inconsistent with experiments on phenomena for which a satisfactory explanation has been given by the classical dynamics and the wave theory of light”(Bohr 1913,p.13).

**Only the Bohr theory explained the facts that could not be explained by the Lorentz programme.** “Einstein was extremely surprised and told me: it follows that the frequency of radiated light does not depend on the frequency of electron rotation in an atom at all. This is a great achievement. Consequently, Bohr’s theory should be correct”(Hevesy’s letter to E.Rutherford,14 October 1913, quoted from Klein 1970,p.278). In 1922 in the paper “*Doppler’s Principle and Bohr’s Frequency Condition*” Erwin Schrodinger obtained an important result. In the theory of light quanta the same Doppler effect followed for the frequency of spectral lines as in the wave theory for moving atoms. And finally, Compton’s experiments convincingly demonstrated that “the scattering of X-rays is a quantum phenomena”(Compton,1923,quoted from Mehra & Rechenberg 1982,p.527). In 1924 S. Bose came to first derivation of Planck’s blackbody radiation formula by endowing light quanta with new statistical properties. On the basis of new statistics Einstein in 1924 and 1925 had quickly developed quantum theory of monatomic ideal gases. In 1924 Louis de Broglie in the doctoral thesis came to conclusion that matter should possess certain wave properties. In spite of the successes, the quantum theory of radiation raised even more difficult problems, connected to its relations to classical radiation theory, classical statistical mechanics and classical thermodynamics. Mutual interpenetration of these theories led to the creation of quantum mechanics, quantum electrodynamics and quantum field theory. To sum up: **Einstein’s and Lorentz’s programmes were empirically-equivalent and, hence, inseparable in the domain of “electrodynamics of moving bodies”. But their consequences diverged in quantum domain. And it was namely the domain of explanation of quantum phenomena where the Einstein Programme little by little forced out the Lorentz Programme.**

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1. see also valuable corrections to Lakatos’s account on interplay between Michelson’s and Lorentz’s research activities provided by Ian Hacking (1993,pp.253-261). [↑](#footnote-ref-1)
2. Richard Feynman called them ‘Poincare’s elastics’ in his “Lectures” [↑](#footnote-ref-2)
3. Einstein had revealed the link between 1905a and 1905c only in 1909, at Saltzburg congress [↑](#footnote-ref-3)