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Toulmin's Evolutionary Theory of Scientific Development: The Study of a Failure

Abstract

This paper discusses main issues concerning Toulmin's theory of evolutionary development of scientific knowledge. The reconstruction of Toulmin's assumptions, which underlie the theory, and the presentation of the theory itself, is followed by the analysis of the difficulties of the conception in general. The author tries to show that these difficulties consist primarily in the discrepancy between what Toulmin aimed at and what he actually achieved. The problem is that although incorporating the means of Darwin's theory may itself be – in the context of describing the growth of scientific knowledge – justifiable, it is not consistent with Toulmin's demand for the rationality of science. The purpose of creating the evolutionary theory was, as Toulmin claimed, to give an account of the development of scientific knowledge which would be non-relativistic and rationalistic. However, this purpose is, as the author argues, not achievable on the grounds of Toulmin's accounts of rationality.

Ever since Charles Darwin published *On the Origin of Species,* more and more philosophers have found it tempting to employ his ideas and conceptual tools in their attempts to explain the problem of intellectual development of humankind. The tremendous explanatory power of Darwin's theory, when employed within its proper scope of application, has lured philosophers like Thomas H. Huxley, Ernst Mach or, more recently, Karl R. Popper to transplant some of its concepts and tools from the domain of the development of biological life to the domain of the development of scientific knowledge. However, such attempts had barely achieved the explanatory success of the original. This fact casts some doubts on the very possibility of founding a viable theory of the development of knowledge on Darwin's theory (or, for that matter, any other theory of biological evolution). However, answering the question of whether a theory of scientific development founded on the theory of evolution – if possible at all – would require the scope of analyses far extending the limitations of one article. Additionally, conclusions of such extended analyses would most likely offer us not an exhaustive answer

to this question, but rather many specific answers to the question of how such theory is impossible. Therefore, in this article I would like to confine myself only to the one case study exemplifying such attempts – the attempt undertaken by Stephen Toulmin in his *Human Understanding*. This particular proposition deserves attention and recalling for at least two reasons. The first is that the British philosopher explicitly adopted and employed the conceptual apparatus of Darwin's theory to describe the development of scientific knowledge. The second is that he spectacularly failed in doing so. By focusing on Toulmin's attempt I want to uncover some of the reasons of this failure. In what follows, I will first discuss the key notions of Toulmin's theory and then move on to the critical analysis of some of them (therefore, those well acquainted with Toulmin's conception may want to skip the first part of the article and go straight to the second).

I. Toulmin's theory

I.1 The road to evolution

Because Toulmin's theory owes much to his general criticism of contemporary epistemology it would be best to start with a brief recollection of the key assumptions and ideas which led him to the evolutionary approach to science. For Toulmin, the most fundamental problem of contemporary epistemology was its attachment to the view which equals rationality with reasoning based on a logical-mathematical model. This view traces back to Plato's dialogues and Euclid's *Elements* and it was reinforced by the seventeenth century rationalism, which shaped the tradition that has shaped our basic epistemological problems since then. The inability of contemporary theories of knowledge to account for the problem of change and continuity is a testimony to their roots in this tradition. This very problem divides contemporary theorists of knowledge into two camps – the absolutists; with the followers of Gottlob Frege on one hand, and the relativists; with philosophers such as Robin G. Collingwood or Thomas S. Kuhn on the other. The main difference between these camps is that while absolutists claim that the whole discrepancy in seeing the world – whether among cultures or historical periods – is nothing more than the effect of errors of cognition, the relativists go to the other extreme and claim that every concept has value only in relation to its original context. Absolutists, under the surface of discrepant accounts of physical reality in which the history of science is abundant, see only atemporal principles reflecting clear and ideal forms of concepts. Relativists see this as a reflection of the fact that there are no universal criteria or principles which would allow us to go from one conceptual system to the other, what leads them to the conclusion that changes in the systems of knowledge are either not entirely conscious or not entirely rational. In short, the absolutists completely ignore the problem of scientific change and the relativists are overestimating it, but neither of them managed to resolve it in a satisfying way. This inability is due to the fact that, while seemingly extremely different, both these approaches are founded on the same view, according to which scientific knowledge should be treated as an ordered structure or system which is describable in logical terms, which in turn, implies that if it is to be judged as rational, the development of knowledge should be given universal intellectual

foundations. As Toulmin puts it, by identifying rationality with logicalness, both camps "subscribe to the philosophical cult of systematicity." [1]

The postulate to transform the conception of rationality together with the deep conviction about the rationality of the process of scientific change are the very things which pushed the British philosopher towards making an attempt at designing a theory for the development of scientific knowledge which would be founded on the theory of evolution and which would offer a third way between absolutism and relativism. It is in the analogy to the development of living organisms that the continuity of the processes which have been shaping our scientific knowledge becomes evident. Additionally, such an approach should offer an account of this process which would allow for admitting to historical relativity without falling into socio-historical relativism.

What interests Toulmin in the theory of evolution is primarily the sole explanatory schema. He explicitly assumes that Darwin's theory is only one of the specific cases of employing a method which could be equally successfully applied in describing phenomena from other domains.[2] Therefore, he focuses primarily on general relations which can be distinguished in historical process between long-term schemata of conceptual changes, everyday activity of users of concepts, and stable conditions determining the preservation of short-term decisions. Assuming that the development of knowledge can be described in a way analogical to the Darwinian description of the development of living organisms, Toulmin claims that scientific concepts should not be treated as logical systems but as populations. They come into being and disappear in response to intellectual conditions of the intellectual environment and are subject to the process of variation and natural selection.

I.2 Concepts as the subject of evolutionary theory

The very notion of science is as general as it is vague and it would be difficult to find a more unambiguous answer than that science is what people who call themselves scientists are employed in doing. Though trivial, this constatation is in fact quite close to the way in which Toulmin conceived science. He claimed that if we want to consider science as a process, we cannot separate the subject of science from the work of scientists. Therefore, we should search for the answer to the question of what, in the development of scientific knowledge, undergoes evolution somewhere on the borders of (the products of) scientific practices and the actual activities of scientists. And what we find is that there are concepts. They are the constituents of the subject and object of science; they are the products of scientific practices, but as such they belong to and are dependent upon those practices.

The very notion of concept is for Toulmin a complex one. He distinguishes three elements in it: language, representation techniques and application procedures.[3] The former two are related to the symbolic aspect of scientific explanation

[1] S.E. Toulmin, *Human Understanding*, Princeton 1972, p. 83.
[2] *Cf*. *ibidem*, p.135.
[3] *Cf*. *ibidem*, p. 161.

which is "the scientific activity that we call 'explaining"'.[4] They include the technical language in which the concepts, laws and generalizations are expressed,the laws described, as well as the products of the explanations. The third element is related to "the recognition of situations to which those symbolic activities are appropriate"[5] and as such it constitutes a reason for the existence of the first two because it determines the possibility and the scope of their application.

Concepts are therefore not merely abstract products of science but they have a concrete existence as the subjects of scientists' research. Therefore, *in abstracto* concepts are never exhaustive of any scientific discipline, but only present it a specific moment in time. As with institutions, we can understand a concept only by analysing how, over time, it allows scientists to achieve the stated goals. This brings us to the question of how the very process of conceptual evolution proceeds and what the criteria of selection are governing it.

I.3 The process of conceptual evolution

The notion of concept is closely related to the notion of a scientific discipline. When we focus on a scientific discipline in a certain historical moment, we are dealing only with temporary products or cross-sections of complex, historically developing enterprises. The basic question which Toulmin encourages us to ask, is: "What makes the later phases of science the 'legitimate heirs' of the earlier?"[6] As an evolving organism, scientific disciplines are developing over time and the continuity of this development becomes visible in that any later phase owes its legitimacy to the fact that it managed to solve at least some of the problems left unsolved by its predecessors. At the same time, the problems are not in any way a stable and invariable element of science but quite the contrary. According to Toulmin they are changeable, but the very process of their change is continuous. Scientific problems form genealogies of difficulties by explaining nature in which later generations follow the earlier. If, however, it is problems that constitute the basis for determining the continuity of scientific disciplines, the question is now about the very process in which they are growing one from another and, first of all, how do they come into being in the first place. As Toulmin points out, "the problems of science have never been determined by the nature of the world alone, but have arisen always from the fact that, in the field concerned, our ideas about the world are at variance either with Nature or with one another".[7] This being-at-variance is the source of scientific problems and the large-scale and long-term changes in science are the consequence of the gradual accumulation of smaller modifications. Those modifications appear always as responses to some specific problems and are preserved as a result of giving a successful answer to them. It is not the case, therefore, that the problems in science result simply from comparing our claims about nature with observations. In reality, they result from discrepancies between explanatory ideals, i.e. ambitions of a given discipline

[4] *Ibidem*.
[5] *Ibidem*.
[6] *Ibidem*, p. 146.
[7] *Ibidem*, p. 150.

concerning the explanation of phenomena, and its actual capacities to fulfill those ambitions. This claim Toulmin summarises in one formula: "Scientific Problems = Explanatory Ideas – Current Capacities".[8]

Within a given discipline, researchers aim to solve specific problems they have with explaining nature. However, their aims are never solely consequences of empirical observations nor the introduction of new mathematical models, but rather of adopting certain views about the problem's situation. For example, what, according to Toulmin, was the real achievement of Rutherford and Thomson – regarded as the inventors of atomic physics – was the creation of a new intellectual ideal of science.[9] In an effort of imagination, they managed to break away from settled conceptions and sketch a new problem situation. By doing so they determined the future character of the discipline, because only the adoption of a given intellectual ideal introduces a link between the disciple and the empirical world and, as such, is primary to the empirical investigations. On this account, "The chief explanatory patterns, forms of theory or 'themata' of science were all worked out in advance of any clear recognition of their empirical scope."[10]

The coming into being of a new concept is always preceded by the recognition of a new problem situation and is a consequence of introducing innovative procedures of dealing with difficulties. Concepts develop and evolve when they are applied to solving specific problems. Theories are thereby separated from considerations about truth – propositions of science do not apply to the real object directly. However it is not to their reference to the empirical domain that they owe their validity but rather to the fact that they may be applied to it – scientists are not asking whether a given proposition is true, but how and under what circumstances it may be applied.[11]

And so, as members of a population must constantly prove their value in order to maintain their position, so must concepts. The moment a given concept ceases to deliver what is expected of it in terms of explaining certain phenomena, the evolutionary mechanisms of change start to work. This eventually leads to the substitution of an older conception by a new, better-adapted one. However, particular problem situations differ highly from each other and that is why the very process of change may take different forms. Depending on the context, Toulmin introduces different classifications for the types of conceptual change – which correspond to the types of problems which they are supposed to solve. In order to discuss the actual process of conceptual change, I will focus on the distinction between, what we may call here, routine and extraordinary problem situations.

The first of the distinguished types of situations is defined by the problems which, in a way, impose the adoption of specific solutions,[12] i.e. their recognition determines the choice of the concepts which will serve as their solutions. This choice depends only on scientists' estimations about which, out of the available

[8] *Ibidem*, p.152.
[9] *Cf*. *ibidem*, p. 153.
[10] *Ibidem*, s. 152.
[11] *Cf*. *ibidem*, p. 170.
[12] *Cf*. *ibidem*, pp. 224–225.

conceptual variations, would best fit the problem. The scientists are thus comparing concepts from the perspective of their potential capabilities in explaining a given phenomenon. This is not to say that we can talk here about some universal formal criteria. It is rarely the case that scientists have a ready innovation which would solve the problem completely on the one hand, without it generating any new ones on the other. Moreover, even explicitly accepted at a given point, criteria my point in different ways – for example, when a potential innovation offers a higher degree of simplicity and coherence, but is, at the same time, less accurate. Instead of formal criteria, scientists follow their intuitions which reflect their disciplinary ideals and they always ask themselves whether a loss in light of one criteria is compensated by a gain in light of the others.

Despite lacking universal criteria, the ways of solving the problems of the discussed type are characterized in every case by scientists' collective agreement about the existence of *any* criteria; that is, that they can explain what they would count as the solution of the problem. Yet, there are also cases in which such collective agreement is lacking and then we are dealing with extraordinary problem situations which Toulmin describes as "intrinsically 'cloudy' ".[13] This cloudiness is a result of a lack of consensus among scientists within a given discipline as to what sort of strategy this discipline should employ and "is a direct consequence of the fact that our disciplines are in the course of historical change, *even in their deepest rational strategies*."[14]

To illustrate the nature of such problem situations, let us consider Toulmin's example concerning the dispute which took place in 1910–1911 between Ernst Mach and Max Planck.[15] The dispute concerned the situation in which the then contemporary physics found themselves when the program for this discipline outlined in Newton's *Optics* was at the point of becoming exhausted – and scientists were questioning and arguing about the very foundations of their discipline. In *Physikalische Zeitung,* Planck criticised Mach's sensationalism arguing that the historical development of physics makes it evident that any subjectivist elements, which Mach's theory wants to bring back, are and should be successively eliminated from it. In response, Mach argued that what should be eliminated from physics is metaphysics. Regardless of whose arguments prevailed, the character of this dispute clearly shows that it was exactly about the very disciplinary strategies. In particular, Planck's position deserves notice because he explicitly drew conclusion about the future of his disciplines by analysing its past development. As Toulmin emphasises, he acknowledged that

the new strategies appropriate to the problems of theoretical physics in his own day must make it the 'legitimate heir' of all previous physical investigations; they had, therefore, to be formulated and judged not in formal or abstract terms, but with an eye to the entire historical evolution of physics, and its ideals of 'physical explanation'.[16]

[13] *Ibidem*, p. 232.
[14] *Ibidem.*
[15] See *ibidem*, pp. 232–233.
[16] *Ibidem*, s. 233.

When there is no collective agreement in a discipline about a strategy of explanation, the central disciplinary question is not about what would be the best solution to the problem, but what is the problem which the discipline should seek to solve. In such situations we cannot talk about an established or universal criteria of selection, because these are cases in which all the previously latent disagreements among scientists are coming to the fore and there is no higher instance to which all or most scientists would agree. It is exactly the higher instance that becomes the subject of the dispute.[17] However, what Toulmin emphasises, even in such situations we cannot say that scientists simply give up rational argumentation for persuasion. The core of such disputes – as well as the thing that secures their rationality – is that they "call for appeal, not to the codified rubrics of an established theory, but to broader arguments involving the comparison of alternative intellectual strategies, in the light of historical experience and precedents".[18] Although initially these disputes may not be exactly substantial or to the point, they are becoming to be such, as scientists are starting to realize the position they found themselves in. From that point they eventually move from formal to historical argumentation and this argumentation is governed by rules resembling those of argumentation in a courtroom. Toulmin even compares their decision making process to the one of a Supreme Court of the United States in which it is reinterpreting constitution while taking always into account the function that a given law should serve in contemporary socio-historical circumstances. When the very foundations of science are being reconsidered, it is obvious that criteria and decision-making procedures will not be stated in an unequivocal manner. Even though, if only the scientists are able to consider their situation and aims in the context of the current situation of their discipline and appeal to its history, they will secure the rational foundations for their decision.[19]

Both described above types of processes of change can be – according to Toulmin – analysed in terms of reasons, that is rational, intra-scientific factors influencing scientists' decisions. Apart from them he distinguished also a type of change characterised by the crisis of rationally. Cases of this kind cannot be judged in terms of reasons, but rather in terms of causes, which are extra-scientific factors determining the decision-making processes within science. In reality, no decision within a discipline is free from such factors, but in both discussed situations, they play a marginal role. These very factors can, however, sometimes decide not only about the character of science, but about the very possibility of its coming into being. If the development of a concept is a matter of a collective work on a problem situation, the necessary condition for the appearance of variations is always the existence of a forum within which potential innovations can be discussed and modified. The development and survival of a concept is dependent on the environment, and specifically on such social conditions as the existence of scientific institutions – which are exactly what creates the "ecological niches". Barriers of those niches

[17] *Cf*. *ibidem*, p. 237.
[18] *Ibidem*.
[19] *Cf*. Toulmin's account of the dispute about the status of quantum mechanics, *ibidem*, pp. 236–242.

are places where professional factors meet the environmental ones. For the optimal course of the process of conceptual evolution, the relations between those factors must be optimal. The mentioned barriers cannot be too low, since in that case the new concepts would not be able to establish stable relations within some established edifice of science. As a result they would dissolve in endless debates and lose their unique characteristics or they would be eliminated precisely because of their innovative nature. This was the case, as Toulmin noticed, in ancient China, where despite the advancement of applied sciences, no pure sciences have emerged; i.e. there was not a programme of theoretical investigations, which would determine the scope of problem situations. On the other hand, when the barriers of an ecological niche are too high, new concepts, although enjoying recognition among the professionals, cannot enter into the broader market of ideas – as it was in Babylon, where highly professionalised astronomers isolated themselves from other social groups and protected their secrets, which led to the disappearance of their findings and methods with the downfall of the state.[20]

The sole existence of scientific institutions marks the fact that a given society has fulfilled the fundamental condition for conceptual evolution, i.e. that its people recognised that their system of knowledge is insufficient for them. Additionally, the coming into being of a scientific discipline depends often on the emergence of institutions which expect to benefit from providing scientists with conditions to conduct their research. But of course the sole existence of scientific institutions and professions does not necessary have to mean that there exist conditions for the unconstrained development of science,

For the life of science is embodied in the lives of these men: exchanging information, arguing, and presenting their results through a variety of publications and meetings, competing for professorships and presidencies of academies, seeking to excel while still vying for each other's esteem.[21]

Scientific disciplines evolve along the evolution of scientific institutions and professions and the scientists holding achievements are gaining authority and defining the institutional framework of their professions. The new concepts and people behind them have practically no chance of entering the forum if they are lacking support of the authorities. However, science in its social aspect does not function as homogenised whole with strict intra-relations. On the contrary, the populational analysis shows that within the domain of science various institutions are in a state of permanent competition for prestige and authority. The existence of authority groups is a result of the function they fulfill within the institutional structures of science which is the supervision of institutions in their achieving the disciplinary goals. When such groups fail to fulfill their function they may be "dethroned" – although they have an influence on the shape the domain of science, authority groups are always subject to criticism from the world of science which may boycott or even overthrow them.[22] Parallel to the existence of leading institutions, smaller associations of scientists are being formed which try to develop

[20] *Cf*. *ibidem*, pp. 215–220.
[21] *Ibidem*, p. 262.
[22] *Cf*. *ibidem*, pp. 274–275.

concepts alternative to the mainstream. The function they serve is twofold. First of all, the innovation proposed and discussed within them feed the mainstream. Second of all, the moment they manage to achieve successes they themselves enter the mainstream and start to shape it.[23] It is these types of processes that, to a significant extent, influence the development of scientific professions and institutions and, at the same time, of the intellectual content of disciplines. Analogically to the evolution of concepts, in the evolution of scientific institutions, scientists are passing authority to these groups which within their own enterprises managed to adapt to the changing ecological situation by solving previously unsolved problems. The professional development of science should be seen as a process in which new generations replace the older ones. The intellectual content of science cannot change independently of the changes within institutions which determine this content and the lack of institutional development goes in hand with intellectual stagnation. And vice versa, the institutional changes are always related to the intellectual ones. If conceptual evolution is to proceed optimally, these aspects of science must be interrelated. However, Toulmin emphasises that "The Social factors are *necessary*, but the intellectual ones are *crucial*."[24]

II. The failure of Toulmin's theory

The evolutionary theory of conceptual development outlined in *Human Understanding* has met with some serious criticism on the part of philosophers of different traditions. Although not all of this criticism has been equally apt nor justified, much of it has been. In the next paragraphs I will discuss some of the crucial arguments levied against Toulmin's conception. As we shall see, although some of them were missing the point, others were on the right track and, as I will be trying to show, what made them valid was the evident discrepancy between what Toulmin claims he is arguing for, and to where his arguments are actually leading. Among the most serious challenges to Toulmin's conception, Tomasz Zarębski lists Toulmin's unjustified use of the concepts of evolution and the charge of relativism.[25] Let me start my discussion of the reasons of the failure of Toulmin's project with a few words about the first one.

II.1 Scientific vs. biological evolution

It is usually the case that when a theoretical device designed for describing a specific class of phenomena is being used outside its intended domain, many question the justification of such a measure. It is therefore not surprising that such doubts were also cast on Toulmin's use of the concepts of the theory of biological evolution in order to describe the development of science.Most notably, in his review article *Is the progress of science evolutionary?* Jonathan L. Cohen argued that the fundamental problem of Toulmin's conception is the fact that it is founded on a misguided analogy between both types of development – of scientific knowledge and living organisms. Cohen pointed out many discrepancies between

[23] As Toulmin notices this was the path of the Royal Society. See *ibidem*, pp. 273–274.
[24] *Ibidem*, p. 221.
[25] *Cf*. T. Zarębski, *Od paradygmatu do kosmopolis*, Wrocław 2005, p. 183.

the theory of biological evolution and the discussed conception and stated that "Toulmin's claim to be using the term 'evolutionary' in the precise and strict neo-Darwinian sense seems hardly more accurate than the claim of some cultural relativists to be generalising from relativity physics".[26]

Instead of discussing or contesting here the details of this line of criticism, I would rather like to suggest a way of defending Toulmin's conception against this and any similar arguments. As Zarębski aptly noticed, the conformity of Toulmin's conception with the theory of biological evolution is not a *sine qua non* condition for it and proving any disconformities does not compromise it.[27] A careful reading of *Human Understanding* confirms that this is also Toulmin's own view, since he often mentions that there are no necessary connections between both theories. And so, although at one point he writes that his "analysis should be an 'evolutionary' one, not just in the broad sense of being non-revolutionary, but in a quite precise and strict sense of the term",[28] he quickly adds, that "it will not be necessary to assume – as Ernst Mach unfortunately supposed – that intellectual evolution has something 'biological' about it, or even that the process of conceptual change in the sciences displays any substantial resemblance to the process of organic change."[29] The ambiguity of the first quotation is most likely due to the fact that Toulmin sought a rationale for his theory as something more than just a critique of Kuhn's conception while the latter clearly shows that Toulmin did not want to find his conception on any strict analogy to the actual theory of evolution. Hence, just proving that Toulmin's conception is not entirely consistent with the theory of biological evolution does not undermine it.

What, however, could undermine Toulmin's conception is an examination of how the development of scientific knowledge – accounted for in terms derived from the theory of evolution–fits the realties which it is supposed to explain and to what extent the adoption of Darwin's apparatus allows Toulmin to fulfill his main declared goal which was to defend the development of science as a rational process. This is the line of thought I shall follow in the oncoming paragraphs, and the mentioned charge of relativism offers here a good starting point.

II.2 Toulmin's relativism?

Let me recall from the previous sections, that for Toulmin the adoption of tools and concepts from Darwin's theory was connected with some precise goals which it was supposed to achieve. First of all, it was supposed to be helpful in designing a theory which would allow us to offer one consistent explanation of both, the changes in science and lack of them. To achieve this aim the theory would have to exclude relativism and at the same time admit for some sort of historical relativity. Secondly, such a theory should be compatible with the claim that the historical change within our systems of knowledge is a rational process. Both these issues

[26] L.J. Cohen, 'Is the Progress of Science Evolutionary?', *The British Journal for the Philosophy of Science* 24 (1973), p. 49.
[27] *Cf*. T. Zarębski, *Od paradygmatu...*, p. 139.
[28] S.E. Toulmin, *Human understanding*, p. 134.
[29] *Ibidem*, p. 135.

are for Toulmin closely connected. It is therefore not unreasonable to conclude that if it is not the conformity with Darwin's theory that secures the success of Toulmin's proposition, it is the extent to which the adoption of evolutionary tools allowed him to achieve his own stated goals.

With this conclusion in mind let us first examine how Toulmin's theory deals with the problem of relativism. There are at least two ways of doing this. We can either look at Toulmin's conception 'from the outside' and ask, whether it possess features typical of conceptions we usually deem relativistic, or we can look at it 'from the inside' and confront its aspects with Toulmin's own claims about relativism.

In respect of the first way, some reasons for giving a positive answer to the stated question can be found in the work of Alina Motycka.[30] The Polish author claims that the moment Toulmin's states that there are no universal criteria of choice and emphasises the influence of psychological and sociological factors on the shape of science, he faces the problem of relativism, "which cannot be resolved by changes in terminology."[31] As she notices in Toulmin's situation of competition, the conceptual change is influenced by the role of individual scientists, the socio-cultural context and the social background. As a result, in the case of the clash of values, we cannot say anything about the decision-making process if we will not see it as relative to the given situation. For Motycka it is futile to argue here that the adaptive capacity of concepts allows them to adapt to changing scientific environments and that change is a necessary condition for continuity, because "such argumentation is viciously circular–what is assumed by the theory of evolution is becoming an argument for evolutionary continuity of change."[32]

Zarębski tries to refute Motycka's arguments on the ground of Toulmin's conception by claiming that although Toulmin admits the role of extra-rational factors, he denies that scientific change can be caused only and solely by them and emphasises that it is the rational factors that always prevail. In a situation of competition, scientists appeal to their own experience and substantial arguments. Zarębski emphasises also that on the ground of Toulmin's conception, wrong decisions may always be verified in the light of empirical data. Although I am quite sympathetic with his claim that Motycka's arguments result from her "undue devotion to the logical model of rationality and are put forward from the absolutist position",[33] I also think that they are nonetheless quite accurate, while Zarębski's refutation amounts to the restating of the claims from *Human Understanding* (like the above claim about the prevalence of rational factors) and is passing over their insufficiency. The claim–that scientists in the situation of competition are appealing to their own experience–may well serve for as against Toulmin's relativism because they appeal to their disciplinary experience, which is what their decisions are relative to. As to the empirical verifiability of innovations, it is worth noting

[30] A. Motycka, *Relatywistyczna wizja nauki: Analiza krytyczna koncepcji T.S. Kuhna i S.E. Toulmina*, Wrocław 1980.
[31] *Ibidem*, p. 89.
[32] *Ibidem*.
[33] T. Zarębski, *Od paradygmatu...*, p. 142.

that by introducing various concepts or levels of objectivity,[34] Toulmin is perhaps not entirely separating experimental practices from science as a methodological tool, but without a doubt he is diminishing their role in science. In fact, by introducing the notion of objectivity as a feature ascribed to science not only in light of the confrontation of theoretical claims with empirical data, but which also can be ascribed to strategies which can generate concepts, which can in turn, generate claims liable of empirical verification.[35] Toulmin trivialises the role of the empirical verification of scientific theories and experimental practices of science. It is, perhaps, even more striking if we realise that he simultaneously claims that his theory is supposed to do justice to the actual practices of scientists and to the history of scientific practices that shows the importance of empirical evidence at the point of adopting new concepts. As to Motycka's vicious circle argument, it is refuted by Zarębski by a repetition of this circle, because he claims that the lack of continuity of science is in Toulmin's theory only alleged, because "when the change of scientific strategy is well justified, and therefore rational, then the continuity of science is preserved".[36]

On the other hand, it is doubtful that Motycka's arguments simply prove that Toulmin's conception is unviable as non-relativistic. This is partly due to her "absolutist approach" which makes any sign of relativity tantamount to relativism, and partly because of a more general problem of stating a precise commonly accepted definition of relativism. The latter is an issue for any "external" criticisms of relativist conceptions and it is probably not something that cannot be overcome, but if we would manage to show that a given idea is not a form of relativism in light of some external criteria, but judged by its own terms, it would make all further arguments unnecessary.

Toulmin formulated his theory to a significant extent in opposition to T.S. Kuhn conception of the revolutionary development of science – which he explicitly deemed relativistic. If, therefore, by confronting his criticism of Kuhn with his own claims, we could show that certain aspects of his theory are consistent with certain aspects of Kuhn's conception, we might prove that Toulmin's conception is a form of relativism by Toulmin's own standards. Let us then proceed in this fashion.

Among other things, Toulmin criticises Kuhn's conceptionof revolutionary development – presented in *Human Understanding* in a very tendentious way – as not doing justice to the historical realities and for the fact that his conception of paradigms is independent of his conception of scientific revolutions.[37] The letter is supposedly due to the fact that for Kuhn, the notion of a paradigm is identical to the notion of a logical system. However, Toulmin himself *de facto* introduces two types of change in science – the change in response to the routine and to the extraordinary problem situations – and those types are also to a significant extent independent. Moreover, both these types seem to correspond with those of Kuhn.

[34] S.E. Toulmin, *Human Understanding*, p 242–244.
[35] Cf. *ibidem*, p. 243.
[36] T. Zarębski, *Od paradygmatu...*, p. 144.
[37] For Toulmin's discussion of Kuhn's conception see *Human Understanding*, pp. 96–130.

The development of science within a paradigm is supposed to be characterised by the fact that the paradigm organises the work of scientists to the extent that it influences the way they perceive phenomena and makes their work tantamount to "solving puzzles" that are, in a way, assigned by the paradigm.[38] But, as we have seen, the first distinguished types of change in Toulmin's conception involves the problems and solutions to them which are determined by the accepted intellectual or explanational ideals. Furthermore, according to Kuhn, the exhausting of the pool of puzzles to solve within a paradigm and the appearance of more and more cases which scientists are unable to account for, leads to a crisis in a discipline which eventually results in a revolution – i.e. substitution of old paradigm by a new one.[39] Toulmin, in turn, claimed that extraordinary problem situations arise when disciplinary ideals and concepts accepted within them are losing their capability to explain newly discovered phenomena. In other words, they are being exhausted and, as a result, the very goals of a discipline must be rethought and new ideals established. What is more, both authors emphasise the change in the nature of scientific reasoning and the role of extra-scientific factors in times of this type of change and the differences between their conceptions are to a large extent the differences of terminology – what one calls 'persuasion', the other calls 'argumentation' from the history of science.

Toulmin eagerly grasps at the letter of *The Structure of Scientific Revolutions* and accuses the conception outlined there of reducing the paradigmatic change to persuasion and of postulating complete incommensurability of paradigms which make it impossible for the adherence of two different paradigms to communicate with each other. He, in turn, claims that we are never dealing with communication breakdowns.[40] It does not occur to him, that even at this point his claims are quite compatible with Kuhn's and that the too literal reading and related criticism of Kuhn leads him to contradict himself. Consider, for example, the contrast between his claim about the substantial discussion throughout the time of Copernican revolution and what he said about the coming to being of atomic physics (i.e. the Thomson-Rutherford case mentioned in I.3). Both are cases of the second type of scientific change – of revolution for Kuhn and of dealing with extraordinary problem situations for Toulmin. However, in the first case Toulmin stresses – while opposing Kuhn – the existence of mutually communicable reasons throughout the whole extended process of change from Ptolemy's to Copernicus' astronomy[41] and in the second case, which is quite analogical but brought about in a different context, he emphasises the problems of communication between scientists resulting from their adherence to different intellectual ideals (he mentions how many scientists contemporary to Thomson and Rutherford were unable to accept their postulated ideal and thus understand them[42]).

[38] *Cf*. T.S. Kuhn, *The Structure of Scientific Revolutions*, Chicago–London 1996, pp. 35–42.
[39] *Cf*. *ibidem*, pp. 123-163.
[40] *Cf*. for example his account of the debate during Copernican revolution in *Does the Distinction between Normal and Revolutionary Science Hold Water?*, [in:] I. Lakatos, A. Musgrave (eds.), *Criticism and Growth of Knowledge*, Cambridge 1970, pp. 43–44.
[41] *Cf*. *ibidem*; as well as *Human Understanding*, p. 105.
[42] *Cf*. S.E. Toulmin, *Human Understanding*, p. 153.

The existence of a rational debate is for Toulmin a warrant of not falling into relativism, but considering the insufficient justification of this rationality (which will be yet discussed) it seems quite evident that many differences between his and Kuhn's conception have a rather verbal character. The fact that Kuhn is not clear on the issue whether the process of scientific change is rational or what sort of rationally would it be while Toulmin does not mean that if judged by some same criteria, both conceptions wouldn't be equally postulating rationality (or lack of it).

II.3 Rationality and logicality

Before moving on to discuss the difficulties with Toulmin's justification of his rationality claim, it is worth to consider a broader issue that to a significant extent is influencing the whole concept. The rejection of traditional notions of rationality – i.e. those which supposedly equal rationality with reasoning founded on the principles of formal logic – serves as peculiar *leitmotif* of much of Toulmin's philosophy and, while not questioning here the reasons of his criticism, I would like to focus on some problems of the positive or constructive side of this criticism. The most problematic issue here concerns the very postulate of separating rational reasoning and logic.

Although central to Toulmin's though, this postulate is far from being clear and it can be understood in two ways. We can conclude that what Toulmin is claiming is just that what is rational does not correspond completely to what is logical. Should this be the case however, his postulate would be barely novel. It is familiar to philosophers at least since times of David Hume and only a few would question the rationality of many of our behavioural patterns despite of their lack of logical rationale. The other way of making sense of Toulmin's claim is to conclude that according to it, the categories of rationality and those of logicality are mutually exclusive. Indeed, many of his comments suggest that while ignoring any options for a middle ground, Toulmin consents to the claim that being logical has nothing to do with being rational. Larry Briskman notices[43] that the reason why Toulmin leans to this view is his very limited view of logic as "concerned simply with the inner articulation of intellectual systems whose basic concepts are not currently in doubt".[44] On this account, logic cannot be anything more than a set of directives, which allow us to transfer the truth or acceptance of premises to the inferred conclusions. As such, it is not capable of accounting for the processes of accepting new concepts in science. This process – as described in I.3 – is not based on the pattern of deduction and confirmation, but rather it requires, from time to time, a break with the old standards and the introduction of new ones – which is supposed to confirm its rationality. Thus, Toulmin has to claim that that scientific rationality is not connected in any way with logicality, because only then can he postulate the rationality of the second distinguished types of change. The talk about ties between logic and rationality is possible only in relation to the method-

[43] *Cf*. L. Briskman, 'Toulmin's Evolutionary Epistemology', *The Philosophical Quarterly* 95 (1974), pp. 160–169.

[44] S.E. Toulmin, *Human Understanding*, p. 84.

ological principles of science, whose counterpart in Toulmin's conception are the explanatory ideals stating not only their scope but also the ways of conducting research (another thing is the fact that Toulmin does not distinguish clearly the methodological principles of science from its subject and content). From the types of change distinguished by Toulmin, the first one occurs within an established ideal and the second involves a break with it and the introduction of a new one– which implies also the substitution of the methodological principles of a discipline. The acceptance of such a view of scientific change seems to be precisely what drives Toulmin to the negation of any ties between rationality and logicality.

The central problem of this approach derives from the fact that while it is not hard to see that with the development of science the methodological principles are changing, it is hard to see how it would be true about all of them – including the most fundamental principles of logic which are no more connected to scientific reasoning than to reasoning in general. Imagine a scientist who is not accepting the principle of non-contradiction – what form would his research take if he would be unwilling to see why from two mutually exclusive propositions only one may possibly be true? This would be of course reducing Toulmin's postulate to the absurd and it is doubtful that what he had in mind while insisting on making a distinction between logicality and rationality was the complete separation of these two domains. But this also shows the fundamental ambiguity of Toulmin's own view of rationality and this ambiguity is something that influences his whole conception as a proposition for the account of scientific development as rational.

II.4 Rationality and the development of science

Although the world "rational" appears in *Human Understanding* in all possible cases, it seems, as I have already suggested, that Toulmin finds it much easier to talk about what rationality is not, than about what it actually is. All in all, his argumentation is lacking any clear explication, definition or criteria that would allow one to judge certain behaviours or views as rational (or not). At best, he offers us an intuitive notion of rationality as the capability for critical analysis of one's beliefs. The motto opening *Human Understanding* declares that "A man demonstrates his rationality, not by a commitment to fixed ideas, stereotyped procedures, or immutable concepts, but by the manner in which, and the occasions on which, he changes those ideas, procedures and concepts."[45] Unfortunately this is the most complete insight to the positive side of Toulmin's conception of rationality we can possibly find in *Human Understanding*. The insufficiency of the positive description of the notion of rationality becomes even more problematic when Toulmin explicitly postulates the break with *a priori* in deciding whether a given enterprise is scientific or not[46] (which is, in this context, tantamount to it being rational or not). He claims that only apparently we are forced to choose between accepting fixed definitions of what it is to be scientific and accepting that each historical period has its own standards of assessing intellectual enterprises. The first choice would mean a return to absolutism and the second to relativism

[45] *Ibidem*, p. X.
[46] *Cf*. *ibidem*, pp. 495–503.

and for Toulmin absolutism and relativisms are extreme positions between which a middle ground is possible. To achieve this middle ground we must liberate ourselves from any demarcation criteria whatsoever. Only then would we be able to compare– from the perspective of being scientific–intellectual enterprises of different epochs and cultures.[47] And to compare them we would simply have to take into account the extent to which alternative strategies of different epochs managed to achieve the developing goals of given enterprises.

If, however, only those actions within intellectual enterprises are rational which result in the realisation of stated goals, we cannot, on the grounds of this conception, talk about the rationality of any enterprise until we find out in what way it has managed to achieve its goals. So on this view, rationality can be, in practice, attributed to a given action only *ex post* and, as a result, we are lacking any tools for analysing the rationality of the choice of goals and, to some respect, also the means employed to achieve them. When rationality is made completely relative to the aims of an enterprise, we are forced to judge many enterprises as rational only because they managed to achieve their stated goals – even if we otherwise would consider either their goals or their methods irrational.

Consider, for example, the case of beliefs of the proponents of Copernican astronomy. In his classic *The Copernican Revolution,* Kuhn distinguishes two factors driving the work of Johannes Kepler.[48] First of all, he came into possession of the very accurate data collected by Tycho Brache; and second of all – and what will interest us here – he was an ardent supporter of Neo-Platonism – convinced that the whole Universe is governed by simple mathematical laws and that the Sun is the only possible cause of the motions of the heavenly bodies. There can be little doubt that the Neo-Platonic worldview of the German astronomer played a crucial role in his discovery of the famous three laws of planetary motion, which we commonly judge as scientific today. However, the same system of beliefs and methods induced him to derive conclusions, whose scientific character is widely contested today – to say the least.[49] How should we therefore account, on the grounds of Toulmin's conception, for Kepler's investigations in respect of their rationality, if his research was to a large extent determined by unscientific (and, as such, irrational) factors? Since Toulmin's conception offers us no practical tools for assessing the rationality of methods and goals, we can only conclude that it was rational of Kepler to accept a certain metaphysical worldview, as it was rational of him to pursue an account of the structure of solar system in terms of harmony between "cosmic" solids. Of course, this would go far beyond of what Toulmin would like to include under the label of rationality. It seems, though, that while Toulmin emphasised the difference between scientific and extrascientific factors influencing the development of science, this distinction breaks down when we try

[47] Provided that in this epochs and cultures there exist collectively established problems which are the subject of collective investigation and that these problems are sufficiently similar. *Cf*. *ibidem*, p. 498.

[48] *Cf*. T.S. Kuhn, *The Copernican Revolution*, Cambridge–London 1995, pp. 209–219 (212–214).

[49] With the so-called Kepler's Fourth Law as a prime example.

to apply his conception to the actual historical cases. The apparent reason for this is Toulmin's tacit reversal of reasoning, in which he starts with the assumption that science is rational and only afterwards tries to make his conception of rationality to agree with this assumption. That's probably also the reason why he never discusses cases of, in the long run, ineffective scientific enterprises – if genuine scientific enterprises are thoroughly rational in character and rational enterprises are successful we cannot talk about rational and, at the same time, unsuccessful scientific enterprises. Such reversed reasoning also accounts for his insistence on the rational character of scientific change – if science is rational and science is changing than this change is rational.[50]

As I have already suggested in the previous paragraph, Toulmin needed to break with what for him was the paradigm of rationality as logicality in order to maintain the claim that rationality can be attributed to any type of conceptual change, including one involving the substitution of whole disciplinary ideals. I have already tried to show that the introduction of this particular kind of scientific change caused Toulmin's falling into relativism. The same may now serve as an argument for the irrationality of science in Toulmin's conception. After accepting the claim that in the face of problems which call for the transformation of the established scientific approach in a given discipline, scientist are changing more formal argumentation for arguments appealing to analogical situations in the history of their discipline Toulmin cannot avoid the assertion that their arguments are to a significant extent persuasive in nature. Motycka points out to this very moment when she notices that "the irrationality of science in Toulmin's conception is that in the case of change of disciplinary and theoretic principles, science does not provide sufficient reasons and arguments for the selected choice; it is motivated by causes (i.e. external factors)".[51] That this is truly the case we can easily convince ourselves by reflecting on Toulmin's claim concerning the nature of argumentation in the face of this type of change. His postulate of rationality is founded here on the analogy between scientific and legal argumentation, in which assumed rationality of the latter warrants rationality of the former. But it what way is legal argumentation rational? By claiming that the rationality of courts of law and legal argumentation is reflected in their objectivity because the sentences of justice oriented on interpreting law in the context of a given socio-historical situation, Toulmin contradicts himself. To reinterpret law and adjust it to the current context is nothing else but to relativise it to the socio-cultural circumstances. The key problem here is that it does not spring to Toulmin's mind that court verdicts are, like nothing else, reflecting not the collective agreement on what is best for society, but of the interests and beliefs of ruling or dominating classes or simply of (as it is usually the case) socio-cultural beliefs of a given historical epoch. To

[50] Motycka reaches similar conclusions when she writes: "Toulmin's reasoning appears to be, in short, of this form: since the changeability of science (a historical fact) is at odds with the accepted notion of rationality, then if we want to maintain the rationality of science and can't deny the fact that it is changeable, we have to take the changeability of science for its rationality", A. Motycka, *Relatywistyczna...*, p. 90.

[51] *Ibidem*, p. 101.

insist on something else is to claim that it is equally rational to sentence convicts to death in the past as it is to sentence them to life imprisonment today and if we are willing to agree that this is the case, we are falling into relativism, what, on the ground of Toulmin's conception, is tantamount to falling into irrationalism.[52]

Conclusions

In the second part of the article I have tried to show that the adoption of conceptual apparatus of the theory of biological evolution did not allow Toulmin to fulfill the hopes he had for such a measure. Although I dismissed the charge of Toulmin's unjustified use of the theory of evolution, I think that what is actually problematic is the very choice of Darwinian concepts for designing a conception of the development of science as a rational process in the first place. It is worth noting that although he accounts for the development of science in categories of evolutionary change, he cannot avoid the claim that this development is progressive. It is evident in many moments of *Human Understanding* because Toulmin does not limit himself to the claims that the conceptual change is tantamount to substituting old concepts for new ones but he says that this process "involves replacing one set of concepts by another *improved* set."[53] By claiming this, Toulmin tacitly departs from Darwin's theory in order to defend the rationality of his image of science. To create a foundation for such a defense he must, at least implicitly, accept that the development of science is a goal-oriented process, whereas the theory of evolution accounts for biological change as not directed towards any particular end. One of the main achievements of Darwin's theory was exactly that it accounted for the process of the development of biological organisms as "blind" and "accidental." It means that, firstly, accidental is the very way in which variations are coming into being and, secondly, their preservation is an effect of their better, from the point of survival, adjustment of their contingent features to the conditions in certain ecological niches in certain periods (the very process of preservation is of course not accidental).[54] And it is precisely survival by adaptation that is the only goal about which we can talk (and still with some oversimplification) in the case of Darwin's conception of the development of biological organisms. The process of achieving this "goal" cannot be in any case called progress, because there is no fixed direction or end point of the process of evolution. However, Toulmin, while talking about the historical process of the development of science which he sees as rational, does not avoid deeming it progressive. And it cannot be the case that what he had in mind was progress in a purely adaptive sense – which is connected with another problem of Toulmin's adoption of Darwin's explanatory schemata. Namely, Darwin's theory of evolution talks about the adaptation of organisms to ecological niches which are given. In Toulmin's conception, evolving concepts do not only adapt to the ecological niches, but are also – to a large extent – creating

[52] One could, perhaps, point out here that I am ignoring the seemingly extended discussions of rationality and models of rational reasoning presented by Toulmin primarily in his *The Uses of Argument* (Cambridge 1958); however, as

[53] *Ibidem*, p. 486; emphasis mine.

[54] *Cf*. for example Ch. Darwin, *On the Origin of Species*, London 1859, pp. 80–87.

them. Toulmin's theory cannot therefore limit itself to, as it is the case, accounting for changes in our systems of knowledge by appealing to the process of adaptation to the ecological conditions of the environment, because science itself is shaping these conditions. If so, then Toulmin's argument in defense of the rationality of scientific change as adaptation seems to be completely losing its ground.