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TOULMIN'S MODEL OF ARGUMENT AND THE "LOGIC" OF SCIENTIFIC DISCOVERY[1]

Abstract: The paper discusses Toulmin's substantial (jurisprudential) model of argument, as set out in *The Uses of Argument* (1958), in juxtaposition with his considerations concerning scientific discovery and scientific arguments, as presented in *The Philosophy of Science* (1953). The author finds Toulmin's search for understanding the nature of science to be a forerunner of his later conception of argument. In addition, he claims that the latter displays much more accurately the "logic" of both scientific discovery and the arguments in science than the patterns of formal (both inductive and deductive) logic. For actually, in Toulmin's view, no logic in the traditional, formal sense can be ascribed to discovery and scientific arguments – despite all the mathematical techniques they employ. Thus neither the neo-positivistic account nor even the Popperian one can do justice to their specific character. Although the Toulminian model of argument cannot be treated, in a strict sense, as a methodological instruction, it plays an explicatory role, throwing some light on our understanding of scientific enterprises and their rationality. In fact, the author finds Toulmin's concept of argument to be the core of his overall conception of rationality, and the considerations about science to be one aspect of this conception.

Keywords: Toulmin, argument, substantial argument, discovery, methods of representation, philosophy of science

In this paper I am going to explore the significance of Toulmin's model of argument for the philosophy of science, in particular with respect to our comprehension of discovery and scientific arguments. I will claim that, in Toulmin's view, the substantial, jurisprudential argument better fits our account of the scientific practice of arguing and looking for new methods of representation of physical phenomena than formal logic does. In my presentation I try to show that Toulmin's ideas concerning scientific arguments form one aspect of his whole conception of rationality, which is based on the theory of substantial arguments.

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[1] The presentation given in the paper has been developed wider and in greater detail in my book on Toulmin: *Od paradygmatu do kosmopolis. Filozofia Stephena E. Toulmina* (Zarębski 2005); similar discussion on the nature of scientific arguments can be also found in Polish in Zarębski 2003.

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1. Toulmin's model of substantial argument

Toulmin presented his model of argument in *The Uses of Argument* (Toulmin 1958) and later in *An Introduction to Reasoning* (Toulmin, Rieke, Janik 1984). In the former book, he calls it a *substantial* argument (Toulmin 1958, p. 125) as opposed to an analytic one (particularly the traditional syllogism), while in the latter he calls it a *practical* one as opposed to a theoretical one. The difference between the syllogism and the model proposed by Toulmin is that the syllogism has three elements: two premises and conclusion, whereas the Toulminian substantial argument consists of six components: *claim*, *data*, *warrant*, *backing for warrant*, *rebuttal* and *modal qualifier*.[2]

The *claim*, being a counterpart of the logical, syllogistic conclusion, is an asserted thesis that someone tries to justify. The second element is the *data* that are supposed to support the claim advanced; usually they are some sort of factual statements. The third element of the argument is the *warrant*, whose task is to show that the leap from data to "conclusion" is legitimate. The fourth component is the *backing for warrant*, which gives some additional support for a warrant and indicates the ultimate basis that makes the warrant legitimate. The task of the *modal qualifier* – the fifth element of the argument – is to express the strength of the step from the data to the conclusion and has an adverbial form such as "Probably", "Almost certainly," etc. Finally, the sixth and last component is the *rebuttal*, whose task is to point out the circumstances in which the leap from the grounds to the claim is not legitimate. The whole argument takes place between two people, paradigmatically, disagreeing about the assertion advanced; thus, one of them challenges the claim, and the other, who put it forward, tries to justify it. The pattern to follow has been taken from legal practice and this is why Toulmin also calls it *jurisprudential* (Toulmin 1958, pp. 41–43).

Toulmin understands the interrelation between all parts of his argument as follows (cf. Toulmin 1958, pp. 94–107). One person offers a *claim* (C), for example a statement, "Petersen is not a Catholic". His opponent may question this original statement and demand a justification of it; thus he may ask "Why? What have you got to go on?" In that situation, i.e. when the claim has been challenged, sufficient *data* (D) should be given for supporting our claim; some facts or information should be delivered to be appealed to

[2] For a critique of Toulmin's model from logical perspective, see Castañeda 1960.

as a ground for the statement concerning Petersen's religion. As a result, the answer may be: "Petersen is a Swede". Then, again, the adversary may continue to question the claim by asking "How do you get there?" In it, what the adversary wants is not any further particular data, but rather demands the claim's defender to show that the mere step from the data to the claim is appropriate and legitimate. To answer such a demand, the defender should put forward a *warrant* (W): a kind of rule, principle or inference-license, which entitles one to draw the conclusion from the data. The warrant usually has a hypothetical form possible to be interpreted as "If D, then C" or "Given data D, one may take it that C" and the like. Correspondingly, in the exemplary argument concerning Petersen, such a warrant would be: "Scarcely any Swedes are Roman Catholics".

But even being provided with the data and the warrant, the opponent may still keep on challenging and ask why, in general, just this warrant ought to be accepted; he may put forth the question: "Why do you think that?" And then, in the defender's turn, what should be given is the explanation why, on what account, the warrant should be regarded as binding. In other words, the proponent has to display a *backing* (B) for the warrant, has to indicate a relevant basis for this warrant. Depending on the kind of claim, it could be, for example: some appropriate statutes and legal provisions; taxonomical classifications of an animal (for a warrant, say, "A whale will be a mammal"); or statistics which record relevant information. What is essential here is that different kinds of warrants often call for different sorts of backings, and various rules of inference require various sorts of backing support. Returning to the example of Petersen, its warrant could be supplemented by a backing such as: "The proportion of Roman Catholic Swedes is less than 2%".

There are two more elements in Toulmin's account. The first is a *modal qualifier* which expresses "some explicit reference to the degree of force that our data confer on our claim in virtue of our warrant. In a word, we may have to put in a *qualifier*" (Toulmin 1958, p. 101). The qualifier (Q), is represented by such modal words as "presumably", "probably", "almost certainly" or "necessarily", and is supposed to reveal the strength of the step from the data to the claim – on account of the particular warrant. In practical discourse, agreed conclusions, even if recognized as valid and reliable, are, by their nature, probable rather than unequivocally certain. Lastly, the sixth element of Toulmin's layout is the *rebuttal* (R), which indicates circumstances under which the general validity of the warrant ought to be set aside. Toulmin explains its role in the following way: "Again, it is often necessary in the law-courts, not just to appeal to a given statue or common-law doctrine, but to discuss explicitly the extent to which this particular law fits the case under consideration, whether it must inevitably be applied in this particular case, or whether special facts may make the case an exception to the rule or one in which the law can be applied only subject to certain qualification" (Toulmin 1958, p. 101). In the argument with Petersen, the rebuttal would look like: "Unless Petersen is a catholic priest". Both the qualifier and the rebuttal are expected to tell us whether we should accept our claim with a very high degree of certainty or rather tentatively, with some conditions, exceptions or qualifications. The rebuttal tells explicitly what those possible exceptions (some of them) are.

The whole scheme has the structure below (Toulmin 1958, p. 104):

$$
\begin{tikzcd}
\mathbf{D} \arrow[rr] \arrow[d, dash] & & \textit{So}, \mathbf{Q}, \mathbf{C} \arrow[d, dash] \\
\textit{Since} \ \mathbf{W} \arrow[d, dash] & & \textit{Unless} \ \mathbf{R} \\
\textit{On account of} \ \mathbf{B} & &
\end{tikzcd}
$$

And the exemplary argument would look like the following:

$$
\begin{tikzcd}
\text{Petersen is a Swede} \arrow[r] \arrow[d, dash] & \textit{so, almost certainly,} \ \text{Petersen is not a Catholic} \arrow[d, dash] \\
\textit{Since} \arrow[d, dash] & \textit{Unless} \arrow[d, dash] \\
\text{A Swede can be taken almost certainly not to be a Roman Catholic} \arrow[d, dash] & \text{Petersen is a catholic priest} \\
\textit{On account of} \arrow[d, dash] & \\
\text{The proportion of Roman Catholic Swedes is less than 2\%} &
\end{tikzcd}
$$

2. Toulmin on scientific discovery

In his earlier book, *The Philosophy of Science* (Toulmin 1967; first published in 1953), Toulmin explored – in the spirit of the later Wittgenstein – the nature of arguments in the sciences. The crucial part of his investigations constitutes the question of the rationality of scientific discovery. Toulmin presents one of the first anti-positivistic accounts of the rationality of science, implying that scientific arguments – along with arguments of many other fields except the pure sciences – are both indescribable and unexplainable in terms of formal logic. Neither deductive methods of inferring nor the variety of inductive ones capture the specific character of scientific enterprises, especially those practices that lead to new discoveries. Science, according to Toulmin, does not – in the proper sense – discover *new* facts or regularities in nature, but rather offers some new ways of seeing and understanding the physical world; its basic, fundamental purpose is not the pursuit of the objective true knowledge in a traditional manner, but rather the foresight and understanding – through a relevant theory – phenomena, many of which we are already familiar with. "Physics – Toulmin says – presents a new way of regarding old phenomena" (Toulmin 1967, p. 16) and goes on to say that discoveries do not simply reveal some new, unknown facts, but rather interpret in a new, different way what we somehow already knew.

The essential and decisive role in such a discovery is played by the "method of representation" (Toulmin 1967, p. 31), as Toulmin calls it. By this phrase he means – on the most basic level – a sort of graphical, pictorial image of a given physical phenomenon as sketched on a board (on a more sophisticated level, this role is performed by mathematical models). He emphasizes the fact that, during the early history of science, the possibility of drawing a diagram of explored phenomena exerted a great influence, because it enabled scientists "to see" the power of proposed solutions: a clear and convincing graphical presentation could significantly contribute to the acceptance of those solutions by the community of scientists. This was also the reason why some of the 17^{th} century's mathematicians lent more weight to geometry than to algebra, finding the latter to offer only a shortcut way of the matters geometry *displays* in full account (*ibidem*, p. 30).

The branch of science to be regarded as a good exemplification is optics, where a range of basic phenomena is interpreted geometrically: the rays of light are represented as straight lines, and, on that basis, the angles of elevation and reflection are examined, the length of rays worked out, etc. Another example can be found in the atomistic theory of matter, where many physical and chemical phenomena are depicted in terms of hits, pushes, connections and disconnections of particles. In accordance with Toulmin's intuitions, Adam Grobler glossed the merits of ancient atomism as follows: "The significance of that vision [i.e. atomism, T. Z.] is difficult to be overrated. Apparently, between the naïve images of hooks or fastenings and the notions of contemporary science opens a gulf. Yet are not, say, the notions of chemical bonds or virtual molecules equally funny? Scientists do not even pretend to camouflage the humor of their language when they introduce with deliberation such terms as 'colors' or 'smells of quarks'" (Grobler 2001, p. 11). Although in this passage Grobler is talking about the extension of the sense of the ancient concept of being, and does not refer to Toulmin, his remark also points out that an inventive method of picturing reality (as it is in the case of atomism) may make a great contribution to the development of a scientific concept and to more fertile theories.

The method of representation is crucial in physics and chemistry since it also provides the whole range of methods and techniques for drawing conclusions; e.g. having accepted a geometrical way of interpreting a given phenomenon, we can employ most laws established by geometry (say, Thales' Law, that of Pythagoras, the laws of trigonometry, etc.) as well as the relevant computational techniques. Obviously, it would be a cliché to say that not every scientific field may rely on relatively simple geometrical ways of representing, especially in dealing with advanced and complex problems. In fact, the majority of questions in the sciences require much more sophisticated formulae and abstract mathematical operations. However, according to Toulmin, even very abstract mathematical models play a role similar to the pictorial methods of representation: like geometrical diagrams, they are also used to interpret old phenomena in a new way, and then to predict future ones; as such, they are a counterpart of geometrical images (Toulmin 1967, p. 32). Newton's Laws of Motion in classical dynamics might be a representative example of an algebraic counterpart of geometrical representations.

Thus, when a scientist begins his investigation of nature, he does not approach it as a neutral, unmediated observer, but he is equipped with some intuitions concerning the ways of representing of the examined phenomena; and tries to employ these representational methods – depending on their explanatory power – as a sort of interpretive key. When a physicist – Toulmin says – starts to claim that "heat is a form of motion" or that "light travels in straight lines" or that "X-rays and light-waves are varieties of electro-magnetic radiation" (Toulmin 1967, p. 16), he does not state the pure facts and does not discover anything to be in fact ascribed to reality. His discovery is mediated through a given method of representation and thereby takes for granted the adequacy of such methods of drawing inferences in his approach to a particular problem. Toulmin declares explicitly that: "The heart of all major discoveries in the physical sciences is the discovery of novel methods of representation, and so of fresh techniques by which inferences can be drawn – and drawn in ways which fit phenomena under investigation" (Toulmin 1967, p. 31).

If, then, a scientist sets out the Principle of the Rectilinear Propagation of Light, he, strictly speaking, does not say anything about the nature of light, but simply submits a report about his earlier assumptions: that a light ray can by interpreted as travelling in straight lines, and that what happens with it can be described in a geometrical way. Given these assumptions, he introduces a new way of perceiving the light and confers a new sense to this notion, putting it in a new context. It might be said that his considerations constitute tentatively a fragment of a new language game in science: he introduces some novel expressions whose meaning should be clarified from a new perspective, or, as Wittgenstein would say, whose "Grammar has yet to be explained to us"[3] (Wittgenstein 1998, p. 10). On this basis, many other problems for further investigation arise as resulting from those suppositions. For changing the old, ordinary, talking about, say, "light" (like in the expressions "Turn off the light", "There is much light in the room" etc.), and shifting to geometrical understanding of it, we have to – following the claim that "light travels in straight lines" – answer such questions as "Where from?", "Where to?", "How fast?" etc., which are not intelligible within the ordinary, non-geometrical account (Toulmin 1967, pp. 19–20). In the above view, what gives rise both to the problems of science and to the suggested solutions to them is again the *quasi*-diagrammatical code of reading off the phenomena in question.[4]

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[3] Although in *The Philosophy of Science* Toulmin does not speak explicitly about "language games" and "grammar", but only about "*language-shift*" (p. 13, 152) in moving from ordinary usage to science, the reference to these Wittgenstein's terms seems to be quite obvious; all the more so because Toulmin repeatedly points out Wittgenstein, his teacher in 1940s, as a figure having the most important impact on his philosophical development.

[4] Other historical examples, concerning, for example, the changing of concepts of movements in Newton are included in another Toulmin's book on science, *Foresight and Understanding* (Toulmin 1961).

3. The "logic" of scientific arguments

When we ask about the nature of scientific arguments, about how – on what basis – the physicist draws inferences, it will turn out that the key role in it is played not by any form of traditional logic, but, again, by the accepted methods of representation. The most basic, but yet representative, examples can be provided, according to Toulmin, by the field of optics. Accordingly, he takes the Principle of the Rectilinear Propagation of Light as an example that "(...) for all its appearance of obviousness, displays many of the features characteristic of discoveries in the exact sciences. Its very commonsensicality is indeed a merit, reminding us how the sciences grow out of our everyday experience of the world" (Toulmin 1967, p. 17). It is worth emphasizing here that this principle is rather obsolete and does not agree with the later, more sophisticated and exact theories of light: corpuscular and wave theories. Yet, for the sake of its simplicity and fair computational exactness, it is still applied in simple, common optical problems. It is also taught in the introductory courses of physics.

Then, let us assume – as Toulmin does – that we deal with the following, simple situation:[5] The sun is shining on a wall that is 3 meters high, so that the angle of elevation is 30°. Now if, given the above data, we want to ask how deep the shadow cast by the wall is, the answer will be: $3\sqrt{3}$ meters. How should we explain why this is so? When we asked the physicist about how we do know that the depth of the shadow is just $3\sqrt{3}$ meters, his response would probably be of such a sort: "Light travels in straight lines, so the depth of the shadow cast by a wall on which the sun is directly shinning depends solely on the height of the wall and the angle of elevation of the sun. If the wall is 3 meters high and the angle of elevation of the sun is 30°, the shadow must be $3\sqrt{3}$ meters. In the case described, it just follows from the Principle of the Rectilinear Propagation of Light that the depth of the shadow must be what it is" (Toulmin 1967, p. 22). Thus, the whole inference here draws on the principle that light propagates in straight lines, which provides the relevant formula that enables us to work out the result.

On account of that, Toulmin raises the question: under what known logical scheme does this sort of argument fall? What type of logic may be ascribed to it? Obviously, what happens here cannot be *induction*. The fact

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[5] In the following example, I use the metrical system, not an English one, which Toulmin used to employ in his original version.

that the conclusion seems to flow from the premises with logical necessity would rather hint of *deduction* from some general claim to a particular instance. The candidate for a general claim would be the principle that "All light travels in straight lines" and the particular instance would be the sentence that "The shadow's depth is $3\sqrt{3}$ meters". However – Toulmin goes on – it is not the case here, because of at least two reasons. First, in deduction, for a conclusion to flow with logical necessity (what allegedly happens in the above case), the general principle in the major premise must cover unconditionally all cases of light's propagation; in other words, it must be true. But, in this respect the Principle of the Rectilinear Propagation of Light is not absolutely true: it does not apply, for example, to the cases of diffraction, refraction or scattering. So, since the major premise is not true, then the conclusion drawn from it is not necessary, but only probable. As such, our argument cannot be counted as deductive (Toulmin 1967, p. 23).

Second, if the inference in question were to be a deductive one, it would have to be written (i.e. have the potential to be written) in the form of a relevant syllogism. But, actually, it is not possible; for from the principle that "Light travels in straight lines" it does not follow that "The shadow's depth is $3\sqrt{3}$ meters". Strictly speaking, the only logically correct syllogism to be formed on the account of this principle would look as follows:

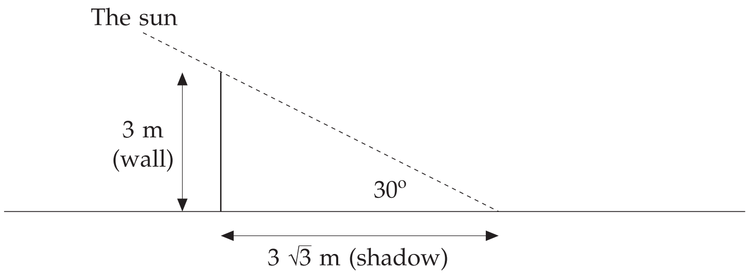
All light travels in straight lines (MaP);
What we have here is light (SiM);
Then, what we have here travels in straight lines (SiP).[6]

If, from the principle in the major premise ("All the light travels in straight lines"), it were to follow logically that "The shadow's depth is $3\sqrt{3}$ meters", its major term in this principle (P) would have to concern the shadows (*all* shadows) of the $3\sqrt{3}$ meters' depth; for only this element occurs in our argument under investigation as an major term (P) in the conclusion. But this is not the case. Hence, the conclusion in our argument does not follow deductively from the Principle of the Rectilinear Propagation of Light and the whole argument does not rely on logical deduction (cf. Toulmin 1967, p. 23).

Toulmin's own claim is that the case described above is "*a novel method of drawing physical inferences* – one which the writers of books on logic have not recognized for what it is" (Toulmin 1967, p. 23). And the

[6] It is an example of the syllogistic mode called *Darii*.

core of it is, of course, the method of representation. The author of *The Philosophy of Science* is convinced that in order to give a more precise exposition of the argument, the physicist would probably draw a diagram like the one below, where the horizontal line represents the ground and the vertical one – the wall, and the slanted, dotted line – symbolizes the sun-ray (*ibidem*).



Thus the physicist does not deduce his conclusion in a logical sense *stricto sensu*, but, as a result, infers on the basis of his drawn diagram. The drawing, or the method of representation, plays a central role in his explanation, since, due to this account of the light, he possesses the inferring-techniques that enable him to work out the shadow's depth. The final conclusion does not follow with absolute necessity, but only – so to say – with relative necessity, i.e. one limited to a given method of representation.

The scope of application of a given way of representing, to repeat, is not universal. On that account, the acceptance of the Principle of the Rectilinear Propagation of Light should be supplemented with at least several provisions, such as: "unless the phenomenon under investigation occurs in an optically homogenous environment (otherwise the light would be succumbed to, say refraction or diffraction)", "unless there is no some opaque obstacles in the way of the light-ray, such as prisms", "unless the light does not pass through very narrow slit (in which case the light would be scattered)" etc. Any complete list of such restrictions in fact cannot be given. The limits of application do not follow directly from the mere method of representation, but they are fixed through experiments and further examination. Yet, we may conclude, in the scientific arguments – at least of the sort described above – a separate place should be designated for such cases. Taking for granted the universality of any such principle would be premature and thus irrational.

4. Toulmin's model as applied to scientific arguments

Since neither inductive nor deductive logic[7] can be ascribed to the arguments in science, it is reasonable to ask whether Toulmin's model of jurisprudential, substantial argument can fit the example analyzed above. In fact, the scheme set out by Toulmin in *The Uses of Argument* (1958) seems to match his earlier considerations from *The Philosophy of Science* (1953). The structural elements of a substantial argument look as following:

$$\begin{array}{ccc}
\mathbf{D} & \longrightarrow & \textit{so}, \mathbf{Q}, \mathbf{C} \\
| & & | \\
\textit{since} & & \textit{unless} \\
\mathbf{W} & & \mathbf{R} \\
| & & \\
\textit{on account of} & & \\
\mathbf{B} & &
\end{array}$$

The argument from optics, in which the task was to work out the depth of the shadow cast by the wall, when arranged to fit the above scheme, would take the following ("jurisprudential") form:

$$\begin{array}{ccc}
(\mathbf{D})\ \text{“The wall's height} = 3\text{ m,} & \longrightarrow & \textit{so}\ (\mathbf{Q})\ \textit{almost certainly} \\
\text{the angle of elevation} = 30^\circ\text{”} & & (\mathbf{C})\ \text{“The shadow's depth} = 3\sqrt{3}\text{”} \\
| & & | \\
\textit{since} & & \textit{unless} \\
(\mathbf{W})\ \text{Computational techniques} & & (\mathbf{R})\ \text{Circumstances in which such} \\
\text{saying that “If D, then C”} & & \text{phenomena as diffraction,} \\
 & & \text{refraction, scattering etc. occur} \\
| & & \\
\textit{on account of} & & \\
(\mathbf{B})\ \text{The Principle of Rectilinear} & & \\
\text{Propagation of Light} & &
\end{array}$$

[7] Although Toulmin does not mention other sorts of formal logic, he implies that none of the formal logical constructions could do justice to the practice of scientific inferring, because there would always be a sort of substantial gap between any formal procedure and its practical application in a given case.

Thus, we may conclude – in agreement with Toulmin's considerations – that the model of substantial argument better corresponds to the real process and structure of scientific arguments than the patterns offered by formal logic. *Backing* **B** points out explicitly the ultimate basis (i.e. the method of representation) on which a given inferring is built; *warrant* **W** expresses a practical rule (inference-technique) allowing to skip from the *data* **D** to the particular *conclusion* **C**; *rebuttal* **R** leaves a place for possible exceptions from the practical, general rule embedded in the warrant, and the *modal qualifier* **Q** shows the force of the conclusion to be drawn, suggesting that it is legitimate on account of the method of representation (explicitly expressed in the backing for the warrant). Indeed, Toulmin does not analyze other examples of physical arguments in detail, but his considerations concerning optics and the nature of scientific discovery permit him to acknowledge that he finds his model more relevant and instructive.

Here, it is worth drawing attention to the problem of induction in science. In Toulmin's view, induction from observational, empirical data to general claims concerning observed phenomena is not of much use in explanatory sciences. It is successfully employed by researchers in "natural history", when they examine and describe each kind of, say, butterfly, mouse, raven etc., in order to make their generalizations as certain as possible. In physics, the situation is quite the opposite: we do not simply make generalizations; correspondingly, any particular (i.e. not general) conclusion is not drawn as a result of deductive inference from a general sentence to a particular claim. This would be like inferring about things we already know: having accepted inductively that "All As are Bs", after examining all As and all Bs, it goes automatically that also "This particular A is B". In physics, Toulmin says, it is not the case; here – when it comes to particular conclusions, as in our earlier optical example – we want to get know something new: if we measured the height of the wall and we have the angle of elevation of the sun, we still need to work out the shadows' depth. What enable us to know it are (allowing for some restrictions) only the computational techniques connected with geometrical optics (cf. Toulmin 1967, pp. 45–48). "Natural historians, then – Toulmin says – look for regularities of given forms; but physicists seek the form of given regularities" (Toulmin 1967, p. 48). And this form of regularities is captured in a given method of representation.

In that case, we may say, the crucial problem that arises concerns how a physicist comes to a given method of representation. And the answer constitutes the crux of Toulmin's discussion. He finds that no formal rules could be given for it, neither in a neo-positivistic manner nor – we may also add – in a Popperian[8] sense of the logic of scientific discovery. Toulmin says – quoting Einstein – that they are products of human imagination, but immediately puts stress on the fact that this imagination cannot be untutored or accidental. In the relevant passage he writes:

> Perhaps, too, the recognition of fresh and profitable ways of regarding phenomena is, in part at least, a task for the imagination, so that Einstein can say of them, as he says of the axiomatic basis of theoretical physics, that they 'cannot be abstracted from experience but must be freely invented... Experience may suggest the appropriate [models and] mathematical concepts, but they most certainly cannot be deduced from it'. But we must not be tempted to go too far. (...) there are certain kinds of imagination which only a man with a particular training can exercise (Toulmin 1967, pp. 39–40).

Yet, as we see, no formal rules can be demanded for discovering new methods of representation and new profitable theories.[9]

5. The significance of Toulmin's model for understanding scientific arguments

Toulmin's considerations concerning scientific discoveries and arguments plainly cannot be treated – in a strict sense – as methodological hints. Being treated so, they would contain serious shortcomings and ambiguities. To mention some possible objections: first, his critique of (deductive) logic focuses mainly on traditional syllogistics. And thus, despite its being sound, it falls short; it does not refer to other nontraditional logics and does not discuss their, at least partial, usefulness in scientific reasoning. Perhaps, it would be better if he accepted even limited the applicability of some formal methods and then considered how far they reach, in what contexts they are binding, etc. Second, with respect to jurisprudential arguments, it seems that the mere concept of legal discourse is not very clear in Toulmin; for

[8] The main difference between Popper's and Toulmin's view is that, according to Popper, we can notice a sort of progress in the development of science: one leading from problem situations to better and better theories; whereas in Toulmin this development proceeds in a *quasi*-Darwinian manner, which means that science develops both institutional and theoretical mechanisms both of producing new innovations and of selecting them. The direction of evolution depends on both of them (Cf. Toulmin 1972).

[9] Toulmin's elaborate presentation of evolutions of concepts and factors that are decisive of in the acceptance of theories can be found in *Human Understanding* (Toulmin 1972).

example, he does not clarify unambiguously whether he has in mind the Anglo-Saxon system or the continental one.[10] Furthermore, it is difficult – if at all possible – to find close analogies between methodological procedures of science and particular legal procedures. Third, he does not refer to the achievements of the general methodology of science that try to clarify and organize the practice of science. However, one cannot deny they are, in many ways, important and useful.

Instead, on the contrary, Toulmin's ideas about scientific discoveries have some *explicatory* value: they contribute to the way we understand science, its discoveries and arguments. Consequently, one can defend Toulmin's considerations from the sorts of objections brought up above. Starting from the last (third) objection, it has to be emphasized that Toulmin is, first of all, a philosopher, not a methodologist. And as a philosopher, he points out some limitations of methodology, particularly of its formal methods; eventually, he comes to the conviction that scientific practice cannot be properly understood solely in terms of formal methods. Such methodology along with inferring-techniques follows only from the accepted model of the phenomena under investigation, and as long as we remain within this model, our techniques are legitimate. But there are at least two crucial moments in which we, as scientists, go beyond any methodology and any kind of formal logic. One is the discovery of a given, profitable method of representation (given model); coming to such a discovery is in no way a question of methodological instructions or formal algorithms, but rather – as Toulmin says – a question of free, though professionally trained, "imagination" that is able to reach beyond the present practice. Another moment concerns the scope of application of a given model. Determination where a given method of representation is no longer adequate also goes beyond the formal procedures. The mere computational techniques may be used impeccably and the final results worked out correctly, but, nevertheless, a given model does not have to find application in a given case and thus the whole reasoning may be erroneous. Sometimes the scientists do not have to be fully aware of the moments in which their practice do not comply with formal methods – being like one Molière hero who does not realize that he actually speaks in prose. However, Toulmin, as a philosopher, places himself in a sense "outside" the practice of science and thus, from a distance, wants to see more than practitioners themselves sometimes could see. In doing this, he obeys

[10] Here, it should be said that although none particular system plays a distinctive role in Toulmin's model of argument, yet in other contexts, also when it comes to science, he appreciates as a pattern to follow the English common-law tradition.

Einstein's suggestion: "If you want to find out anything from the theoretical physicists about the methods they use, I advise you to stick closely to one principle: do not listen to their words, fix your attention on their deeds" (Toulmin 1967, p. 15).

Secondly, although Toulmin's jurisprudential analogy ascribed here to arguments in science is not very strict, we may still ask how far it reaches and what its crucial implications are. The most important aspects of this analogy are the following: first, coming to particular findings in science proceeds through the exchange of reasons and the mere findings are largely the results of consensus[11] acquired within a community of scientists,[12] but they do not come down to the mere principles of logic. Second, like in law there exist a given principle, regulations and rules of inferring; in science, correspondingly, we deal with laws, principles and inferring-techniques. And like in law judicature is not a question of *quasi*-mechanic, automatic application of legal rules, but requires a thorough examination of whether a particular regulation may be applied in a specific case; in science, similarly, the argumentative practice does not rely only on the employing of inferring-techniques, but requires examination of adequacy of these techniques with reference to the specific character of a given phenomenon. Third, scientific laws – like legal regulations – are not *ex definitione* universal, but in some circumstances or contexts they may be suspended or changed (which is not to say that we know in advance that every principle in science is not universal, but only that we do not know this in advance; and thus we should not take its universality for granted). Finally, as the fourth and last remark, we can develop a more general jurisprudential analogy. For in some cases the situation of scientists resembles the situation of the judge in the Supreme Court and, correspondingly, the decisions of both are in some respects similar (cf. Toulmin 1992, pp. 131–133). Namely, there are moments when they both deal with cases that are disputable, more complex or quite new, and that cannot be successfully explained by previous rules and methods. Rulings made by such a judge do not consist in simply using current or former procedures, but rather in resorting to *informal* modes of reasoning, weighting reasons and the whole significance of a case considered. This is why they may be called "substantial" in Toulmin's sense. Sometimes these

[11] However, it is not to say that this consensus has to be univocal and accepted by all parts of the scientific discourse. The legal analogy implies also the presence of a "judge" (or an "authority", or "board of specialists"), who settles a dispute, irrespective of whether the "defeated" part agrees with his verdict or not. But it does not, in turn, mean that the decision made by a "judge" is arbitrary and irrational.

[12] For further discussion, see 'Rationality and Scientific Discovery' (Toulmin 1974).

sorts of rulings may ultimately lead to the change of particular regulations, and also, in some special circumstances, to the change of fundamental purposes and tasks of the legal system. Similar things happen in science, when a scientist faces a phenomenon, or a problem, which he cannot explain in terms of available rules and laws. Then, his arguments simply have to go beyond the current established system. In some cases, it leads to the change of particular procedures or claims. Furthermore, in some specific situations – which Toulmin calls the "the moments of strategic uncertainty" (Toulmin 1992, p. 132) – it may contribute to revision and redefinition of the whole old paradigm, and then to the choice of a new scientific strategy (and a new paradigm).

Thirdly, when it comes to Toulmin's approach to formal logic, it seems that he does not criticize logic as such, but rather as a proper and universal pattern of rationality. On this basis, he also challenges its usefulness in understanding the physicist's work, particularly in making discoveries. For, as a matter of fact, each field of science develops its own methods and – as Toulmin calls it – "working logic", independent of allegedly universal "idealized logic" (Toulmin 1958, p. 146 et passim). This, in turn, is closely connected with the methods of representation accepted in that field. Because the methods of representation are not *ex definitione* universal, it is reasonable to admit that in the future a dimension can be found in which these methods have no application (in other words, it is reasonable to *qualify* our conclusions in our arguments and to leave some place for a *rebuttal*). And in this sense – and in others – we may say that Toulmin's jurisprudential model of argument better corresponds to scientific practice than do formal logical inference schemes.

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