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Argument and explanation in mathematics

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ABSTRACT: Are there arguments in mathematics? Are there explanations in mathematics? Are there any connections between argument, proof and explanation? Highly controversial answers and arguments are reviewed. The main point is that in the case of a mathematical proof, the pragmatic criterion used to make a distinction between argument and explanation is likely to be insufficient for you may grant the conclusion of a proof but keep on thinking that the proof is not explanatory.

KEYWORDS: Argument, explanation, proof, mathematics

1. INTRODUCTION

Mathematics is especially interesting in the argument versus explanation debate because it brings together the three notions of proof, argument and explanation which can easily overlap, at least in their day-to-day meaning when an explanation involves an inferential step.

In different ways, many authors subscribe to the idea that a pragmatic distinction solves this problem. For instance, granting Hamblin's statement (1970, p. 228) that "an argument is generally regarded as being whatever it is that is typically expressed by the form of words 'P, therefore Q', 'P, and so Q'; or perhaps, 'Q, since P', 'Q because P'", Johnson (2000, pp. 98-99) argues that the formal concept of argument must be supplemented by a pragmatic dimension, otherwise both notions can be confused. More recently, Walton wrote: "The purpose of an argument is to get the hearer to come to accept something that is doubtful or unsettled. The purpose of an explanation is to get him to understand something that he already accepts as a fact" (2004, p. 72). This distinction relies on a pragmatic alternative: either the interlocutors agree about the truth or acceptability of the conclusion and then the inferential structure is an explanation, or they disagree and it is an argument. I do not deny that such situations are familiar. But is this distinction sufficient to account for the variety of the uses of "P, therefore Q" structures? I doubt it for it leaves aside many common uses, especially when the agents have no fixed and clear-cut positions about the conclusion and when their positions are unknown to each other. This occurs in situations that can hardly be schematized on the face to face dialogue model, for instance when arguments are used in mass media.

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I do not deny that this pragmatic criterion that can be said “standard” is not helpful to acknowledge different epistemic situations. But it also sets a challenge for opposing approaches of the argument/explanation distinction which, in their most extreme versions, are exclusive. On the one hand, their difference is only pragmatic and depends on a difference of opinion between the speakers, on the other it is not pragmatic at all but structural.

Govier (1987) and Snoeck Henkemans (2001) both abide by the pragmatic approach but also hint at structural features. Snoeck Henkemans states that “argumentation is put forward when the speaker expects that the *acceptability* of the standpoint is at issue, whereas giving an explanation is pointless when the speaker does not believe that the explained statement *has already been accepted* by the listener as depicting a true state of affairs” (2001, p. 240). She follows the pragma-dialectical approach and, then, presumes that the positions of the speakers are explicit and clarified in the “confrontation stage”. However she leaves some room for structural features. She writes: “in explanations only causal relations may be employed at the propositional level, whereas for argumentation there are no such restrictions” (2001, p. 234). Unfortunately, not all explanations are causal. And if we grant that there is no room for causality in mathematics, this requirement will not work in this field.

Govier gives examples of what she calls “tough cases” where “passages are both argumentative and explanatory” (1987, p. 173). This conclusion is supported by her failure to make a decision on the basis of the pragmatic criterion. She explains that, in such cases, the difficulty comes from the coincidence and the equilibrium of pragmatic and structural features. She writes: “Statements are able to justify, because in a context where the conclusion is in doubt, they are more certain. They are able to explain in virtue of other complex conditions, which entails their being able, in a context, to specify a cause, underlying structure, or purpose that shows how and why the phenomena described in the conclusion-explanandum came to be as they are”. In the case of mathematics, cause and purpose could be missing but “underlying structures” could perhaps make the deal.

The standard pragmatic criterion is not an innovation of contemporary pragmatic or dialectical argumentation theories. It is already at the very heart of Hempel’s theory of explanation, at least in the DN (deductive nomological) model since the IS (inductive-statistical) one makes all this more difficult because of the uncertainty of the statements (Hempel, p. 1964). Hempel sees both explanation and argument as answers to why-questions and claims that it is only the status of the conclusion (is it dubious or not?) which makes the difference. But there is a major difference between the use of the standard pragmatic criterion in Hempel and in contemporary dialectical argumentation theories: although Hempel’s theory appeals to pragmatics, it is still monological. When he says that a conclusion “was to be expected” or is dubious, you never know who expects or doubts. It is usually an impersonal normative “we” or, at most, the ghost called “Science”. This is why I say that such impersonal views are only “half pragmatic”.

2. MATHEMATICAL ARGUMENT

Mathematics is especially interesting here because it brings together three crucial notions of human rational interaction, namely proof, argument and explanation. The existence of mathematical proofs is not really controversial, but are there mathematical arguments and are there mathematical explanations? These two questions are topics which have brought fresh air to the philosophy of mathematics in the last few decades. Both topics are controversial connected to much older philosophical debates.

Are there arguments in mathematics? In a handout for students, the mathematician Michael Hutchings states that: “A mathematical proof is an argument which convinces other people that something is true”.[1] Can you be more concise? Which student will then doubt that there are arguments in mathematics? When the word “argument” is taken structurally, proofs are arguments or rather chains of arguments as Descartes said in his *Discourse on Method* (1966, p. 47) when he talks of “Those long chains of reasons, all simple and easy, that geometers commonly use to succeed in their most difficult proofs”.[2] First, let us avoid a fallacy of composition: a proof is not simple and easy because its steps are simple and easy. The relation between proof and argument becomes more complex when they get longer and seem to involve more than the mere succession of elementary inferential steps.

Mathematical arguments can be simply ruled out of the focus of argumentation theories when these theories have an essential dialectical slant. Here are two examples.

The keystone of Perelman’s rhetorical theory is a clear-cut distinction between proof and argument: “The proper field of argument is what is likely, plausible or probable as far as it escapes from the certainty of calculation” (1992, p. 1). The rejection of uncertainty in favor of the “geometric style” is the reason why Perelman considers Descartes as one of the undertakers of ancient rhetoric. So, Perelman clearly disavows Hutchings’ view that proofs are arguments. After having bound together both logic and mathematics (and even experimental science), he set the border of his rhetorical empire at the door of the hard sciences. In some ways, he is right: the rigor of mathematical proofs is unusual when compared with most arguments of daily life and even of other specialized fields. But mathematicians are often embarrassed when asked for a strict delimitation of their discipline and sometimes smile when hearing about the alleged absolute certainty of mathematics. As shown by History, the border of their kingdom is fuzzy, it changes over time and its foundations can be shaken (Kline, 1980; Giaquinto, 2002). Then, even a perelmanian should grant the existence of a trading zone for argumentation in mathematics or, at least, in metamathematics.

Like Perelman, Johnson (2000) doubts the existence of mathematical arguments. He thinks that an argument has two faces: a logical (the illative core) and a pragmatic one (the dialectical tier). Hence, the mere utterance of an illative core is

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[1] http://math.berkeley.edu/~hutching/teach/proofs.pdf

[2] My translation.

not enough to make you an arguer: you must also adopt a convenient dialectical behavior (2000, p. 164). This demands a manifest open-mindedness spreading beyond the critical attitude of examining and anticipating objections or considerations running against the conclusion of the illative core. To make manifest her rationality, the arguer must also take into account misguided or irrelevant criticisms (2000, p. 270). This dialectical care is a necessary condition for a genuine argument. And this is why there is no argument in mathematics because, according to Johnson, "no mathematical proof has or needs to have a dialectical tier". In a nutshell, a proof talks for itself. For instance, "The proof that there is no greatest prime number is conclusive, meaning that *anyone who knows anything about such matters*[3] sees that the conclusion must be true for the reasons given" (2000, p. 232). Johnson is right that proofs often work that way. But this could also suggest that the notion of argument that is under-contextualized in the structural approach should not be over-contextualized, for instance by becoming specific to each addressee. I agree on the importance of the pragmatic dimension of the uses of argument but I do not think that mathematical proofs are exceptions. Moreover, the lack of a manifest dialectical tier, quite common in mathematics, also happens in other fields. And it may happen not because pragmatic considerations do not matter but because they do matter. The lack of a manifest dialectical tier in mathematics could be the manifestation of the right pragmatic attitude of experts taking into account the epistemic competence of their pairs.

To be fair, it must be said that in another place Johnson supports a spectrum theory granting that the word argument can be applied to scientific theories and proofs (2000, p. 168). But he insists that the prototype of the concept of argument is outside of the scientific field. Why? This is not obvious to me and I suspect that the equivocation of the word "argument" in English could partly explain Johnson's position.

Recently, the notion of mathematical argument has become a lively topic among authors who are more explicitly involved in the history and philosophy of mathematics than Perelman and Johnson whose discussions of mathematical proof matter for their theory but remain a side topic. A partial but telling overview of this blooming field focusing on mathematics can be found, for instance in Aberdein and Dove (2009).

3. MATHEMATICAL EXPLANATION

Mathematical explanation is an old topic that has known a renewed interest in the last few decades (Mancosu, 2001). From Pythagoras' idea that numbers are the key to the cosmos to Wigner's famous "unreasonable effectiveness of mathematics"(1960) through God seen as a geometer by the XVIIth and XVIIIth philosophers, the explanatory power of mathematics in 'natural philosophy' is still a philosophical challenge. It is linked to arch-traditional problems like the ontological status of mathematical objects, the status of causes in explanations and in

[3] My emphasis.

mathematics, and so on.

The questions “Are there explanations in mathematics?” and “Are there arguments in mathematics?” can be seen as twin sisters. Next question is: “Are there any connections between the answers to these two questions?” If yes, can it be clarified only on pragmatic grounds?

A theory of mathematical explanation faces a specificity of the field: the necessity of mathematics even if mathematical rigor is not an historical invariant (Kitcher, 1981b). Now, let us have a look at Walton’s example to illustrate the distinction he makes between argument and explanation: “Cows can digest grass because they have a long system with enzymes that can gradually break down the grass into nutriments” (2004, p. 72). Walton thinks that this is an explanation because “Cows can digest grass” is not in doubt. But how does he know it and that nobody doubts it? It is likely that he relies on common lore since his own observations would not be sufficient to ascertain that nobody doubts it. But this is not possible in mathematics as far as theorems are concerned. You cannot say that a theorem is not in doubt independently of a proof. This has an important consequence. If we rely on the standard pragmatic criterion to distinguish explanation and argument, a theorem cannot be explained before it is proved (since its truth is still dubious) and there can be no argument about a theorem after its proof as long as it is not challenged. Of course, this is true from a normative point of view presuming that a proof proves and that the opinions, feelings or intuitions of mathematicians about the truth of the theorem before it has been proved do not matter. The practice can be quite different.

The very idea of mathematical explanation is controversial among philosophers and mathematicians. First, there is no agreement about the meaning of the word ‘explanation’ that can be applied to quite different things and situations. Moreover, not everybody agrees about what is explanatory and what is not. For instance, Lange (2009) complains that some authors decide on the basis of their intuitions alone to settle the matter of the existence of explanatory inductive proofs. But his own defense of the view that no inductive proof is explanatory is based on a principle of non-circularity “that presupposes that when one mathematical truth helps to explain another, the former is partly *responsible* for the latter in such a way that the latter cannot then be partly responsible for the former” (2009, p. 206). But this principle of non-circularity is open to discussion.

According to Von Wright (1971, p. 2), there are two main traditions in the history of ideas differing in their conceptions of a correct scientific explanation: the *Aristotelian* and the *Galilean.* The Aristotelian would be causal or mechanistic, when the Galilean would be teleological or finalistic. This distinction is a bit surprising since Aristotle is famous for his finalistic conception of the world (including politics) and the name of Galileo has often been associated with a mathematical conception of the universe. But I only partly agree with Von Wright when he writes that “Philosophers have long been accustomed to making a distinction between the relation of cause and effect on the one hand and the relation of grounds and consequence on the other” but that “this distinction was often ignored or blurred by the rationalist thinkers of the seventeenth century” (1971, p. 34). I grant that general theories on scientific explanation are still roughly divided into rather causal-Aristotelian and rather mathematical-Galilean, mathematical being understood in the broad sense of inferential. Hempel's famous DN model is an example of Galilean explanation. But I think that theories of explanation are "rather" causal or mathematical because the distinction that is supposed to have been blurred by the rationalists was already blurred before them and is still far from clear. You can find mixed causal-mathematical explanations, even in pure mathematics (Mancosu, 1999). And granting the existence of explanations in mathematics and even of explanatory proofs, I agree with Hafner and Mancosu (2005) that there is no a priori reason to think that there is one single model of explanatory mathematical proofs. This prudent view can be supported by the vagueness of the notion of mathematics, by the fluctuating borders of the discipline over time, by the diversity of mathematical practices such as the occasional use of diagrammatic explanations in elementary geometry or arithmetic.

In 1978, Steiner published a paper that was an important landmark in the revival of interest in mathematical explanation. He is only interested in the explanatory value of proofs. After criticizing rival theories, he develops his own view which "exploits the idea that to explain the behavior of an entity, one deduces the behavior from the essence or nature of the entity" (1978, p. 143). Steiner finally dropped the vague notion of essence to build his theory on the alternative concept of "characterizing property", that is "a property unique to a given entity or structure within a family or domain of such entities or structures". But his program keeps a rather 'causal Aristotelian' flavor because of this idea of a nature producing a certain effect. In any case, Steiner's theory has no pragmatic dimension besides the implicit presumption that the conclusion of an explanatory proof is more easily understood than one supported by a non-explanatory proof. This also suggests that the main point is not the truth of the conclusion but something like the easiness or the 'naturalness' of the proof.

Resnik and Kushner (1987) are suspicious about Steiner's 'essentials' or 'independent' properties and doubt that any proofs explain, unless explaining something is to give a systematic account of it. In such a case, almost any proof explains. Moreover, Resnik and Kushner claim that mathematicians "rarely describe themselves as explaining" (1987, p. 151). But if you define explanation as an answer to a why-question, a lot of such questions can be addressed to various mathematical topics beyond proofs and theorems and can receive an explanation. Moreover not all requests for explanation are answered by proofs, and explanations often point to a number of results or informal glosses about them rather than to a single theorem. So, there are many explanations in mathematics but Resnik and Kushner maintain that proofs are not explanations. However, they do not deny that some proofs "seem to reveal the heart of the matter" or show "what is really going on". But they wonder how someone could understand a proof and still ask why its conclusion is true. Finally, they doubt that any clear criterion can be used to identify explanatory proofs.

To account for the notion of mathematical explanation they turn to Van Fraassen's pragmatic theory of explanation (1980) which states that explanation is not essential to science but is only "an application of science". An explanation is an answer to a why-question that is context dependent since its topic is selective and often depends on the field of interest of the person who asks it. According to Van Fraassen, "Why *p*?" amounts to "Why p rather than the other members of (what Van Fraassen calls) the *contrast class*", this class being context dependent. Therefore, since different proofs offer different information, Resnik and Kushner claim that an explanatory proof is a proof providing the right kind of information expected. It is noteworthy that this is compatible with the standard pragmatic criterion about the explanation/argument distinction since the truth of the conclusion is imbedded in the why-question, even if the question focuses on part of it as in the famous distinction between, say, "Why did *Adam* eat the apple?" and "Why did Adam *eat* the apple?"

According to Mancosu (1999, 2008), Resnik and Kushner's claim that mathematicians are rarely interested in explanatory proofs is dubious since many of the leading XVIIth and XVIIIth century mathematicians and mathematician-philosophers of the XIXth century as important as Bolzano and Cournot made a clear difference between explanatory and non-explanatory proofs and preferred the former. Tappenden (2005, p. 151) reports a similar view expressed by Hermann Weyl in the XXth century: "We are not very pleased when we are forced to accept a mathematical truth by virtue of a complicated chain of formal conclusions and computations, which we traverse blindly, link by link, feeling our way by touch. We want first an overview of the aim and of the road; we want to understand the *idea* of the proof, the deeper context". In the same way, Poincaré (1908, p. 25) held that a global understanding is essential to the mathematician's activity, not only to understand but also to discover a proof. If you want to find or follow a proof that is longer than a mere syllogism, he said, you would better be an architect than a mason stacking syllogisms up, without any plan. In the preface to his *Mathematics and Plausible Reasoning*, Polya (1968) also stresses that you must guess the general principle of a proof before getting into details.

Is this a revenge of Descartes' requirement of clearness against Leibniz' blind thought? In any case, this suggests that when a mathematician examines a mathematical argument aiming at becoming a proof (remember it is just an argument according to the pragmatic criterion since its conclusion is not yet beyond any reasonable doubt) she should pay a close attention to its architecture besides the logical constraints. Moreover, if a kind of architectonic understanding is necessary to be 'deeply' convinced, as Weyl said, the truth of the conclusion is not sufficient to make an explanation since what matters is not only this truth but the whole inferential scheme where it fits. If this is what is at stake in the distinction between explanatory and non-explanatory proof, it is also reminiscent of the difference between two of three kinds of explanation that Walton (2004) borrows from AI, namely 'trace explanation' which is the sequence of inferences leading to the conclusion but may not be enlightening for the addressee who is not an expert, and 'deep explanation' – the very term used by Weyl! – that uses her knowledge base. In such a case, Johnson's claim that a mathematical proof does not need a dialectical tier should be seriously reconsidered.

Does the distinction between explanatory and non-explanatory proofs pave the way to an explanatory individualistic relativism? Although there is some disagreement among authors about the value of some examples of proofs which are explanatory or not, it seems to me that most of the explanatory proofs should be telling to most people. Moreover, the fact that a proof is explanatory could be a matter of degree, some proofs being more (or less) explanatory than others, a point that is not discussed in the literature as far as I know. Of course, all this should be more systematically and empirically documented.

Sandborg (1998) contests Van Fraassen's pragmatic theory of explanation, especially in mathematics. He claims that it cannot account for the distinction between explanatory and non-explanatory mathematical proofs because it grades rival explanations on the basis of their probability, a practice that cannot work in mathematics since one proof is not more probable than another.[4] Then, to show some limits of the why-question approach to explanation, Sandborg borrows from Polya (1968, vol 2, p. 147) an example of proof that is non-explanatory. It is the proof of an inequality.[5] Polya first gives the version found in a professional work that is correct but puzzling because of the introduction of an auxiliary mathematical entity. This entity is quite useful to reach the conclusion but it gives, as Polya says, the impression of a *deus ex machina* so strange that someone could distrust the author of such a proof. And this is why Polya insists on the fact that it is not sufficient for a proof to be adequate; it must also appear so to the reader. Perelman and Johnson should appreciate this dialectical care of the audience.

Notice that, in the case at hand, the concern is not for an explanation of the conclusion that may, or may not, be already granted, but for an "explanation of the proof" or, more precisely, of a moment of the proof. The problem is not that the proof goes too fast, skipping some logical steps as commonly happens in mathematics, but comes from the choice of the auxiliary entity. Polya finally provides a proof that is an explanatory strategic enhancement of the professional proof to dissolve the initial puzzlement.

One of Sandborg's points it that the second proof goes conceptually further than the presupposition contained in the why-question. It goes further than the possibilities presumed by erotetic logicians or Van Fraassen' contrast class, even if this second proof does not answer all questions and may not be the most explanatory. So, Sandborg seems to come back to the common idea that an explanation provides some information to someone who is in a state of ignorance and then cannot provide all the relevant resources to explain. And he rightly stresses that Polya uses conceptual resources that may not be accessible to the questioner and then to the contrast class he can provide, even if he acknowledges the truth of the conclusion and the correctness of each step of the proof. This suggests the possibility of a trade and a cognitive enrichment between proof and explanation or, to use the terminology of AI, between 'trace explanation' and 'deep explanation'. Perhaps, the central question is not if a proof is explanatory or not, but how explanatory is a proof or how do you make it more explanatory.

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[4] Sandborg's detailed criticism of Van Fraassen's theory requires getting deeper into the theory, but I think that this short account gets to the core of the objection.

[5] The theorem to be proved is that if the terms of the sequence a_1, a_2, a_3, ... are non-negative real numbers, not all equal to 0, then $\sum_1^\infty (a_1 a_2 a_3 \ldots a_n)^{\frac{1}{n}} < e \sum_1^\infty a_n$.

4. UNDERSTANDING

Explanation and understanding are commonly seen as closely linked. A most familiar picture is someone explaining something to someone else who tries to understand. Of course, you sometimes get some understanding from someone who does not intend to explain and you sometimes call on yourself to explain to yourself something that you do not understand.

This preferential link between explanation and understanding should not obscure the fact that argument and understanding also commonly work hand in hand. We often hear sentences like "They have an argument because they don't understand each other". Understanding is no more sufficient than the existence of a dialectical tier to distinguish argument and explanation.

Information, education and enlightenment are also notions commonly associated with explanation. But their links remain vague even if you postulate that providing information is a necessary condition for an explanation. Moreover, information does not always provide understanding just like deduction would not always provide a feeling of understanding since it is not certain that the 'deeper' understanding is provided by a regular proof. As Hermann Weyl puts it, the end of the road sometimes matters more than the rigor to reach it. Worse, understanding is sometimes an illusion (Trout, 2002). This is why this notion is often dismissed as too subjective or whimsical. But almost no theory of explanation has succeeded in getting rid of subjective or psychologically sounding considerations: even Hempel's logical DN model appeals to notions like expectation, certainty and understanding (1965, p. 337). Unless you appeal to distinctions like that between trace explanation and deep explanation or, as suggested by Salmon (1998, p. 90) between scientific and psychological understanding, it seems that you cannot drop understanding without dropping explanation too.

The stress put by mathematicians on the virtue of explanatory proofs supports the idea that to explain amounts to integration within a favored frame of intelligibility. But what counts as such a frame? Moreover, we can wonder whether mathematicians have something specific or original to tell us about this. For sure, expert mathematicians have an authoritative opinion about mathematical topics, especially about long reasonings. This is an important point because the standard pragmatic criterion to distinguish proof and argument is often illustrated by short reasonings, often involving a single step, and inspired by day-to-day situations where people can have opinions about the truth of a statement coming from quite different sources. However, in spite of their expertise, you may still doubt that the opinions of mathematicians are decisive on broad topics like understanding and explanation and when Mancosu replies to Resnik's denial of mathematical explanations that first-class mathematicians believe in explanatory proofs, you could even suspect him of a fallacious call to authority. But if the feelings or intuitions of mathematicians are only the learned expression of a common psychological artifact, Mancosu's view can also be supported by an argument *ad populum* weakening the charge of whimsical subjectivism. This would confirm not only a folk agreement about the existence of proofs more explanatory than others but also a folk preference for them even if there is not such a folk agreement about which proof is more explanatory. Even beginners in mathematics prefer explanatory proofs for the same reasons as Hermann Weyl: like him, they do not like "complicated chains of formal conclusions" that they cross "blindly" but prefer to begin with – or to end with – "an overview of the aim and of the road". This seems a modest and common demand. And it seems plausible that a non-explanatory proof may turn to an argument (according to the pragmatic criterion) because the audience remains doubtful about the proof and its conclusion and asks for an explanation of the whole argument.

The idea of understanding has long been connected with ideas like architecture, organization, simplification, systematization or unification which would provide a kind of satisfaction when you get it. Remember Peirce saying that inquiry begins with the irritation of doubt "from which we struggle to free ourselves and pass into the state of belief" (1992, p. 114) and that "it has always been agreed that the tendency of understanding was merely toward synthesis, or unification" (1998, p. 92). This idea has quite a long pedigree. The Latin root of 'comprendre', the French verb for to understand is 'cum prendere', that is to take together, to bind or to link. And English dictionaries confirms that I am not wrong when I hear 'to stand under' in 'understanding' and explain that, in this case, 'under' should rather be understood as 'among'. Etymology is not an infallible authority but even Hempel gave us a 'covering' model although he wanted his DN theory not to depend on the opinions and explanatory practices of scientists.

Kitcher's theory of explanation (1981a, 1981b, 1989) is rather Galilean even if it also tries to account for the 'causal structure of the world'. It links explanation and understanding so explicitly that it could be called a theory of understanding. For him, "A theory of explanation should show us how scientific explanation advances our understanding" (1981a, p. 329) and this is also true for mathematics since "we should approach the topic of mathematical explanation via the concept of understanding or, more exactly, of a failure of understanding" (1981b, p. 473).

Its main idea is unification, which is achieved by means of a 'pattern of argument', 'argument' having here only its structural meaning. This pattern is not formal, in the logical sense, but can be instantiated in different logical structures. It also has a substantial dimension since it uses specific non-logical terms. In the case of Newtonian explanation, for instance, some of these terms would be "force", "mass" or "acceleration". For short, let us say that an explanatory argument is achieved by the introduction, according to "filling instructions", of non-logical terms into an inferential structure which belongs to the pattern.

Kitcher writes: "To explain is to fit the phenomena into a unified picture insofar as we can" (1989, p. 500) and "To grasp the concept of explanation is to see that if one accepts an argument as explanatory, one is committed to accepting as explanatory other arguments which instantiate the same pattern" (1981a, p. 334) having specific features allowing us to make it "the basis of an act of explanation" (p. 330). Kitcher also introduces the notion of an *explanatory store over K,* where K is a set of statements, or beliefs, and the explanatory store "the set of arguments acceptable as the basis for acts of explanation by those whose beliefs are exactly the members of K" (p. 332). A pragmatic dimension is implicit here as far as beliefs are held by believers. So, the explanatory virtue of an argument is relative to a set of arguments and to a set of believers. Notice that Kitcher proposes a unique model for any kind of scientific explanation, but explanatory stores are relative to various fields of knowledge and may change over time since "the set of arguments which science supplies for adaptation to acts of explanation will change with our changing beliefs" (p. 332).

Kitcher's discussion of rival explanatory systems and the dynamic of these systems, especially in mathematics (1983, chs. 7-9; 1988), could be useful in dealing with the topic of explanatory proofs. But it brings no decisive contribution to the resolution of the explanation/argument problem. First, Kitcher makes explicit that his unification approach is about *acceptable* explanations. But arguments often make *acceptable* explanations and Kitcher grants that when you have a proof of *p,* sometimes it would be weird to ask for an explanation of *p.* Hence, a proof, which is also an argument for Kitcher, can also count as an acceptable explanation.

In spite of the stress he puts on explanation acts, Kitcher's theory mostly focuses on structural rather than pragmatic considerations. Like Hempel, Kitcher remains half-pragmatic and says nothing about dialectical or interactive features in the use of explanation and argument. His very formulation is symptomatic: "scientific explanation advances *our* understanding". Whose understanding? Yours, mine? In this normative theory it seems that there is nothing and no one to argue. Expressions like "our understanding" or "our theories" leave no room for controversial arguments and seem to presume that all of us share the same understanding, even if it shifts with time. Although several of Kitcher's examples of explanatory unification are scientific theories which have been controversial in their time, there are no parties in his account beyond "we" and, sometimes, a ghostly 'scientific community'. We could expect room for controversial arguments in his discussion of scientific change or of traditional problems in explanation theory, like the vexed questions of irrelevant explanations or explanatory asymmetries, but he just proposes a simplified account of the right explanatory side to take when a competition occurs.

To illustrate the trade between argument and explanation, let us finally turn to one of Kitcher's examples of successful explanatory unification, the *Principia Mathematica Philosophiae Naturalis,* Newton's mathematical masterpiece which was also full of causal considerations. Its reception was notoriously controversial in France (Brunet, 1931; Cohen, 1980; Guerlac, 1981). One year after its publication, the *Journal des Sçavans* (1688), published by the French Academy of Science, criticized Newton's proofs. But it was neither their conclusion nor the validity of their inferential steps that was at stake. The journal was quite explicit about it: "it is not possible to give proofs more precise and exact than the one given in the two first books". The problem lay rather in the kind of proof used by Newton for "the author acknowledges […] that he did not consider their principles as a Physicist, but as a mere Geometer. He confesses the same thing at the beginning of book 3 where he nevertheless tries to *explain*[6] the system of the world. But it is only by means of hypotheses, most of which are arbitrary and, therefore, can only be the basis of a

[6] My emphasis.

treatise of pure mechanics. […] So, to write the most perfect book, Mr. Newton will just have to give us Physics as exact as his Mechanics. It will be done when he will have substituted true motions to his suppositions." In short, Newton's explanations were acceptable as a mechanical device, i.e. a mathematical device since mechanics was still often considered as a mathematical science. But it was clearly unacceptable as a physical explanation.

This is clearly not a case of explanation in pure mathematics and brings us back to the unreasonable efficiency of mathematical explanations and to the fluctuating border between mathematics, mechanics and physics. But the important point is that the unifying argument pattern offered by Newton was certainly not what the French readers were ready to admit. They were definitely not ready to listen to the Newtonian "filling instructions" about gravity. Here, as in Polya's example, the explanatory status of a proof does not only depend on the status of the conclusion but mostly on the kind of pattern that the audience is ready to accept as explanatory.

5. CONCLUSION

The standard pragmatic criterion to distinguish an argument from an explanation and from a proof seems convenient in some familiar situations and so you can decide to use it as a normative criterion. But the case of mathematics shows that it is likely to be insufficient and sometimes leave you in trouble for you may agree about the conclusion of a proof but keep on thinking that it is not explanatory. It seems that this phenomenon is especially salient when alternative proofs are already available and when proofs are long enough to make visible a structure or a pattern. The fact that a proof is explanatory may be a matter of degree and seems to be bound to preferences about the structure and the ontology used in the proof. But to speak of preferences does not entail that this is a subjective matter, for these preferences may be relative to a community or to a particular field. You can also wonder whether the question of the truth or the certainty of the conclusion is not sometimes weakened by the importance given to the structure of the inferential pattern. Can't a dubious conclusion supported by an explanatory reasoning be preferred to a true conclusion supported by a perfect deduction that is not telling? This is probably rare in mathematics but what about other fields?

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