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Mathematical Explanations and the Piecemeal Approach to Thinking About Explanation

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Abstract

A new trend in the philosophical literature on scientific explanation is that of starting from a case that has been somehow identified as an explanation and then proceed to bringing to light its characteristic features and to constructing an account for the type of explanation it exemplifies. This approach to thinking about explanation – the piecemeal approach, as I will call it – is used, among others, by Lange (2013) and Pincock (2015) in the context of their treatment of the problem of mathematical explanations of physical phenomena. This problem is of central importance in two different recent philosophical disputes: the dispute about the existence on non-causal scientific explanations and the dispute between realists and antirealists in the philosophy of mathematics. My aim in this paper is twofold. I will first argue that Lange (2013) and Pincock (2015) fail to make a significant contribution to these disputes. They fail to contribute to the dispute in the philosophy of mathematics because, in this context, their approach can be seen as question begging. They also fail to contribute to the dispute in the general philosophy of science because, as I will argue, there are important problems with the cases discussed by Lange and Pincock. I will then argue that the source of the problems with these two papers has to do with the fact that the piecemeal approach used to account for mathematical explanation is problematic.

Keywords: mathematical explanation, scientific explanation, mathematical realism, non-causal explanations.

1. Introduction

The philosophical discussion about scientific explanation has changed a lot since Hempel and Oppenheim's 1948 epoch-making paper "Studies in the Logic of Explanation". The recent trend is to focus on what scientists are actually doing when providing explanations instead of employing some type of (detached from the scientific practice) conceptual analysis. The *armchair a priori approach* associated with the traditional treatment of this topic has become disavowed lately.

We can distinguish between two traditional ways of dealing with the topic of scientific explanation. One of them is to try to *clarify the meaning of explanatory claims* by analysing how scientists are using the concept of explanation and by appeal to one's linguistic intuitions. Something like this can be found in Friedman (1974). In Friedman's opinion, among the desirable properties that a theory of explanation should have is that it must "square with most of the important, central cases" of theories that we all take pre-theoretically to be explanatory (Friedman 1974, p. 13). But what if the way scientists usually use the concept of explanation is too ambiguous or imprecise? What if there is no such a thing as a clear idea about what counts as a scientific explanation? Hempel thinks that this is the case,[1] so his approach to scientific explanation is different. Instead of trying to "describe how working scientists actually formulate their explanatory accounts," he aims "to indicate in reasonably

[1] He claims that "there is no sufficiently clear generally accepted understanding as to what counts as a scientific explanation" (Hempel 1965, p. 489).

precise terms the logical structure and the rationale of various ways in which empirical science answers explanation-seeking why-questions" (Hempel 1965, p. 412). He deals, then, with this topic by providing an *explication of scientific explanation* in Carnap's sense.[2] According to Carnap (1950, p. 7) the purpose of an explication is that of replacing a vague concept (the explicandum) with a precise one (the explicatum), i.e. one that is simple and fruitful and whose rules of use have an exact form, but which is also sufficiently similar to the explicandum to replace it in most of the cases in which it has been used. Hempel DN-model of explanation is meant as a (partial)[3] explicative definition of the "explicatum"-concept of scientific explanation.

As it turned out, both Friedman's and Hempel's models of explanation suffer from important problems. But what is more important from the perspective of our discussion is that, for reasons that need not concern us here, the traditional approach to thinking about scientific explanation itself has fallen from favour nowadays. Many philosophers are no longer content with just analysing the concept of scientific explanation. They take their projects to go beyond – but not exclude it altogether – the traditional conceptual analysis. James Woodward, for example, points out that his approach goes beyond conceptual analysis because – among other things – unlike the traditional approach, it takes into account the larger practices of explanation, including those that involve nonverbal components and it also focuses on the goals behind our explanatory practices (Woodward 2003, p. 7). Even though "a significant portion of what [Woodward attempts] does involve a description of ordinary and scientific usage and judgment" because "anything that qualifies as an account of causation (explanation, etc.), whether descriptive or prescriptive, must be significantly constrained by prior usage, practice, and paradigmatic examples," the central part of Woodward's project consists in describing our explanatory practices, and not in describing how scientists or ordinary people are using words such as "explanation" (pp. 7-8).

A similar practice-centred approach can be found, among others, in Strevens (2008). Like Woodward, Strevens considers that the description of the actual scientific explanatory practice has to be the focal point of a philosophical study of scientific explanation. In his opinion, the goal of such a descriptive project is…

> "…to say what kinds of explanations we give and why we give them. This is what I call our explanatory practice. The most important source of evidence concerning our explanatory practice is the sum total of the explanations regarded as scientifically adequate in their day, together with an understanding of the background against which they seemed adequate" (Strevens 2008, p. 37).

The main point in favour of this reorientation of the philosophical attention from concepts to practices is that there are strong reasons to think that by focusing on the way scientists actually provide explanations we can produce accounts of scientific explanation that are a lot more adequate than anything an armchair approach can deliver.

An important thing that needs to be emphasised at this point is that this concern with the scientific explanatory practice can take two forms. First of all, one can be concerned with this practice taken as a whole, with the aim of revealing those distinctive features that makes it explanatory. The main idea behind this *general approach* is to find out why we regard this practice as explanatory and try to construct a comprehensive account of scientific explanation. Secondly, instead of a diversity blurring point of view, one can take a *piecemeal approach* to this practice, i.e. look at it more closely and try to bring to light its complexity. The idea is

[2] For more on this see Salmon (1989, p. 5) and Weber, Van Bouwel and De Vreese (2013, pp. 25-27).

[3] Hempel (1965, p. 273, note 33).

that by concentrating on finding a comprehensive account we disregard the complexity of the scientific explanatory practices, so a more appropriate approach is a close scrutiny of the details of these practices. What usually prompts this second type of concern is the belief that *our view about scientific explanation can become more complex if we look more carefully at the details of the actual scientific explanatory practices*.[4] One of my aims in this paper is to argue that there are big problems with the way the piecemeal approach tries to flesh out this belief.

Among those that use a piecemeal approach in their discussions of scientific explanation, we can count Sober (1983), Nerlich (1994), Batterman (2002, 2010), Batterman and Rice (2014), Rice (2012, 2015), Huneman (2010), Irvine (2015), Lange (2013) and Pincock (2015). In this paper I will concentrate mainly on Lange's and Pincock's papers. They use this approach in the context of their discussion of the problem of mathematical explanations of physical phenomena. My aim is to argue that they fail to make significant contributions to the two philosophical disputes in which this problem occupies central stage: the dispute over the existence of non-causal scientific explanations and the dispute between realists and antirealists in the philosophy of mathematics. The main idea behind doing this is to show that the problem with Lange (2013) and Pincock (2015) has to do with their use of the piecemeal approach to thinking about explanation. My strategy is to use this as an illustration for what is wrong with the piecemeal approach.[5]

2. The piecemeal approach to explanation

As I said above, the general and the piecemeal approaches are motivated by different ideas. The (implicit) idea behind a general approach to thinking about explanation is that there is (are) some feature(s) that everything we call an explanation (in general, in science, or in some particular domain)[6] has in common, and it is the job of any theory of explanation to say what this feature is. For example, in Strevens's (2008) view, this feature – in science – has to do with causal relations, so to explain "a phenomenon is… a matter of understanding how the phenomenon was or is causally produced" (p. 3). According to Craver (2007), in neuroscience this feature has to do with mechanisms, so an account of good explanations in neuroscience has to be a causal-mechanical one. Also, according to Craver (2006), Kaplan and Craver (2011) and Kaplan (2011), in relation to scientific models, this feature has to do with the accurate description of the causal mechanism(s) responsible for the phenomenon of interest, so only mechanistic models are explanatory.

By contrast, what motivates the piecemeal approach is the (implicit) idea that the scientific explanatory practices are a complex heterogeneous bunch of different types of explanations, so the aim when discussing this topic has to be to bring to light this complexity. Of course, there is more than one way to try to do this, so what characterizes the piecemeal

[4] My paraphrase of Sklar (1993, p. 269).

[5] Before proceeding, I need to emphasize an important point: I am not aiming in this paper to show that the piecemeal approach is bad *compared* with other approaches to thinking about explanation. My aim is only to show that, despite what the recent interest in this approach may suggest, it is very problematic and cannot be used to make contributions to some important philosophical debates.

[6] After Hempel's attempt to give a truly general theory of explanation, philosophers became less ambitious and relativized their accounts to specific domains. We have, for example, theories that aspire to account only for *scientific* explanation, and there are even less ambitious theories that try to account only for the explanations within a particular scientific discipline, as e.g. biology or neuroscience. What is important to understand from the perspective of our discussion is that a general approach to thinking about explanation is not the same thing as a general theory of explanation. The general approach has to do with the way one sees the task of constructing an account, not with the domain that such an account is supposed to apply to. So it can be adopted without problems even if one tries to account for the explanatory practice encountered within some particular discipline.

approach is the way it pursues this aim. In broad strokes, the piecemeal strategy can be taken to consist of four steps. The first and most important one is to start from a case that is *somehow identified as an explanation* and that seems different from the other scientific explanations. The next step is to analyse this case in order to determine what its characteristic features are. The third step is to argue that some influential account(s) of scientific explanation doesn't accommodate explanations with such features as those revealed at the previous step. The last step is to tackle the problem of accounting for the kind of explanation exemplified by the case discussed. This resembles very much with what Pincock (2015) dubs *the case-driven approach to thinking about explanation*. Pincock presents his strategy this way:

> "In this article I work with a different case-driven approach to thinking about explanation. I begin by discussing a case that has been identified as an explanation by expert practitioners. Then I try to figure out what features of this case are responsible for its explanatory import. Finally, I will see to what extent these sorts of cases can be incorporated into some influential theories of explanation. The risk of this approach is that it may turn out that explanations are not all of the same kind" (Pincock 2015, p. 858).

The main difference between the piecemeal approach and Pincock's case-driven approach is that in a piecemeal approach what counts as evidence[7] that in a particular case we are dealing with an explanation doesn't need to be limited to the testimony of expert practitioners (e.g. one can rely for this on intuition).

To illustrate this strategy, let's take a glimpse at Irvine's (2015) discussion about the explanatory worth of optimality models. In Irvine's opinion, there is more to model explanation than what the popular causal-mechanical accounts let us think. She believes that "models are used in far more explanatory contexts than this [those in which we have knowledge of mechanisms], and in fact are particularly useful when very little is known about underlying concrete mechanisms" (p. 3947). We can find such a context in biology, where optimality models are used to explain, for example, phenomena such as the distribution of phenotypes within a population of organisms (step 1). What characterizes these models is the fact that they "come from an abstract mathematical template in which the 'currency' in a system is maximised by taking into account constraints and trade-offs within the system" (p. 3950) (step 2). So, these models "do not in any sense represent concrete mechanisms, and neither do they describe networks of causal connectivity" (p. 3952). If this is the case, then they cannot provide a sort of mechanistic explanation (step 3). Keeping in mind what is said at steps 1 and 2, it can be argued that optimality models provide structural explanations, i.e. a type of explanations in which what is doing the explanatory work is the abstract structure of the model and the target system (step 4).

An important point that needs to be emphasised here is that, unlike in other approaches to thinking about explanation, in a piecemeal approach the aim is to analyse what

[7] Molinini (2016) draws an interesting distinction between indicators and evidence and argues that, unlike what we find in some papers by Baker and Colyvan, the "claims from scientific practice should not be considered evidence of genuineness, but rather indicators of the fact that there is a genuine mathematical explanation" (p. 417). Unlike evidence, an indicator needs justification, i.e. it needs to be complemented with an account able to "inform us on how these explanations work" (p. 418). So, Molinini can be taken to suggest that something like the piecemeal approach should be adopted by the mathematical realists, otherwise "the debate on EIA [Enhanced Indispensability Argument] will continue to be opaque and suffer a profound indeterminacy" (p. 418). I personally am not persuaded that we should operate with such a distinction between indicator and evidence. If we take something as evidence that in a certain case we are dealing with an explanation it doesn't mean that we don't still need an account that can tell us how such an explanation works.

characterizes a particular case of scientific explanation – how it works – not to show that we are actually dealing in that case with a genuine scientific explanation. This is supposed to be determined somehow from the start.[8]

In broad strokes, the gist of this paper is to show that, depending on the context and on one's aim,[9] two things are important (but not necessarily together) in order for this approach to work properly. First of all, we need a good (pre-theoretic) explanation identification tool,[10] otherwise one can arbitrarily choose something as an explanation and use this approach to obtain an account of what makes it and similar cases explanatory – this way any part of the scientific practice can be taken as explanatory. Secondly, it needs an objective criterion for determining the characteristic features of an explanation – without this, one can take as explanatory whatever aspect of the case discussed one fancies.

3. Two debates, one strategy

In order to have a better idea about how important the piecemeal approach is in recent philosophy, I think it is helpful to take a look at the way it has been/can be used in two explanation related philosophical debates: the debate over the existence of mathematical entities and the debate about the existence of non-causal explanation.

3.1. The mathematical realism debate

An important discussion in the philosophy of mathematics concerns the existence of mathematical entities. The participants in this discussion can be divided into two camps: those that believe that there are such abstract entities – the realists – and those that deny their existence – the nominalists. The most successful argument that the mathematical realists used in favour of their position, i.e. the indispensability argument, is considered by many to have been advanced many years ago by Quine and Putnam. The gist of this argument is that, if we are scientific realists, besides the concrete unobservable posits, we ought to be ontologically committed to the existence of abstract mathematical entities because they play an indispensable role in our best scientific theories. One way to unpack the strategy behind this argument is the following:

<u>The Q-P indispensability argument</u>

(1) Some scientific realists are ontologically committed only to the entities that are indispensable for our recent well confirmed scientific theories.
(2) Mathematical entities play an indispensable role in our recent well confirmed scientific theories.
(3) Scientific theories are confirmed or disconfirmed as a whole.
(4) Hence, if a scientific realist of the type delineated by (1) is a holist (i.e. accepts premise (3)) and accepts (2) as a fact, then she ought to be ontologically committed also to the existence of mathematical entities.

[8] Lange (2013), for example, aims to specify how the cases of explanations that he considers work – that is, to identify the source of their explanatory power (p. 486). Also, Pincock (2015) is concerned with figuring out what features are responsible for the explanatory import of the case he discusses.

[9] See the discussion in section *6*, especially footnote 19.

[10] By 'explanation identification tool' I mean here something (like e.g. intuition or the testimony of expert practitioners) that can be taken as evidence (or something that is an indicator, if one adopts Molinini's distinction – see footnote 7) that in a certain case we are dealing with an explanation.

As it turned out, there are many scientific realists that are not very happy with confirmational holism and there are some, such as Hartry Field, who are not convinced that (2) is actually a fact. So, despite being so extensively discussed, the target of this argument proved to be very small.

A very peculiar thing about this argument that can be easily noticed at a closer look is the complete disregard for the actual role played by mathematics in science. All it requires is that mathematics is indispensable for science. But, as Baker points out, "the phrase 'indispensability for science' is vague. What, exactly, is the scientific purpose (or purposes) for which mathematics is supposed to be indispensable?" (Baker 2005, p. 223). This is a crucial question because, depending on its answer, one can either give a final blow to the indispensability argument or can open a new path for its recasting. The final blow was attempted in Melia (2000, 2002) where it is argued that mathematics is not indispensable for science in the right kind of way (Baker 2005, p. 224) to rationally constrain a scientific realist to be committed to the existence of mathematical entities. For Melia, *the right way* is "the way in which the postulation of theoretical physical entities increases the utility of our scientific theories" (Melia 2002, p. 75).

Luckily for the mathematical realist, there is an answer to the above question that can be exploited to save the indispensability argument. Baker (2005, 2009) provides the most extensive and influential discussion of this answer. Baker argues that *mathematics plays an indispensable explanatory role in science*. If this is the case, then we can give the following modified indispensability argument:

<u>The Enhanced Indispensability Argument (EIA)</u>

(1) Some scientific realists believe in the existence of any entity that plays an indispensable explanatory role in our best scientific theories.
(2) Mathematical objects play an indispensable explanatory role in science.
(3) Hence, the scientific realists referred to in (1) ought to rationally believe in the existence of mathematical objects.[11]

This argument is more powerful than the Q-P argument. First of all, its target is considerably bigger – there are many scientific realists that use inference to the best explanation to argue for their position. Secondly, it doesn't need to use something as controversial as confirmational holism to prevent the scientific realist from dissociating between entities worthy and unworthy of entering one's ontology. All is needed for it to work is that inference to the best explanation is taken as central for defending scientific realism and that there is a convincing case for premise (2). The first part is trivially obtained so all that the mathematical realist is left with is the problem of showing that (2) is indeed the case. But how can she do that? An important thing that needs to be acknowledged is that (2) can be split into two parts: (a) mathematical objects play an indispensable role in science; (b) mathematical objects play an explanatory role in science.[12] Some mathematical realists (most notably Baker) seem to argue that a good way to construct a case for (2.b.) is by showing that (2.a.) holds for scientific explanations (i.e. that mathematics is in some cases indispensable for scientific explanations). But indispensable in the context of an explanation doesn't necessarily mean explanatory, so the realist is still left with the problem of showing that (2.b.) is the case. This is where the piecemeal approach to thinking about explanation can enter the scene.

[11] This is only a presentation of what I take to be the idea behind this indispensability argument for mathematical realism and it should not be confused with the actual argument as it can be found in Baker (2005).

[12] For EIA to work, these two aspects need to overlap in some cases, i.e. it has to be the case that there are mathematical objects whose role is both indispensable and explanatory.

The classical literature on scientific explanation (i.e. the work of Hempel, Friedman, Salmon, Kitcher, etc.) doesn't address the problem of mathematical explanations of physical phenomena. So trying to find something that can help with showing that (2.b.) is the case by looking in that direction is not a good strategy. Also, taking a general approach to explanation is unlikely to be of much help because the idea behind such an approach is to search for a comprehensive account of scientific explanation by analysing the scientific explanatory practices. But constructing a comprehensive account usually requires an artificially created uniformity, so it implies a certain disregard for potential differences in the scientific explanatory practice, or a focus on the most common aspects of such a practice. So, it seems that a good strategy when trying to show that mathematics plays an explanatory part in science is to adopt a piecemeal approach, i.e. to start from cases of scientific explanation in which mathematics is apparently used in a different way than it is used in regular scientific explanations and offer an account for the way such explanations succeed in explaining.

To sum up, the idea in this section was to point out that the problem of the existence of mathematical explanations of physical phenomena is of central importance for the debate in the philosophy of mathematics between realists and nominalists and to show that a good strategy for dealing with this problem, from the perspective of the mathematical realist, can be to adopt a piecemeal approach to thinking about explanation.

3.2. Non-causal scientific explanations

A different discussion for which the piecemeal approach is of central importance is that in the general philosophy of science about the existence of non-causal scientific explanations.

After overcoming the initial qualms about accepting that something which makes heavy use of idealisations and fictionalisations – and so seems far from satisfying the usual requirement that for something to be an explanation, the explanans has to be true – can have explanatory worth, philosophers started recently to be interested in developing accounts about what makes a scientific model explanatory. This interest grew so much that we can almost speak nowadays about a reorientation of the philosophical focus towards model-based approaches to scientific explanation (e.g. Batterman 2002; Craver 2006; Kaplan and Craver 2011; Bokulich 2008; Rice 2012, 2015; Batterman and Rice 2014; Irvine 2015). An important contribution to this change of heart about model explanations was made by Cartwright's (1983) and Teller's (2001) objections to the idea that there are true laws of nature (which gave a powerful blow to the Hempelian DN-model of explanation and to all the other scientific laws involving accounts).

So, one of the main problems in recent philosophical discussion about scientific explanation is no longer whether scientific models can explain, but what kind of explanations can they offer. The mechanistic accounts seem to offer the most well received answer to this problem. According to Kaplan's and Craver's version of such an account, in order to "explain the phenomenon, the model must … reveal the causal structure of the mechanism" (Kaplan and Craver 2011, p. 605), i.e. it must satisfy the following model-to-mechanism-mapping requirement:

> "**(3M)** In successful explanatory models … (a) the variables in the model correspond to components, activities, properties, and organizational features of the target mechanism that produces, maintains, or underlies the phenomenon, and (b) the (perhaps mathematical) dependencies posited among these variables in the model correspond to the (perhaps quantifiable) causal relations among the components of the target mechanism" (Kaplan and Craver 2011, p. 611).

Not everybody agrees though that the only way models gain their explanatory power is by representing mechanistic organization and causal relations. There are philosophers (most notably Batterman (2002, 2009, and 2010)) who consider that idealisations and abstractions can also play a role in scientific explanation because, in some cases, they are "explanatorily ineliminable. That is to say ... the full understanding of certain phenomena cannot be obtained through a completely detailed, nonidealized representation" (Batterman 2009, p. 428). This and similar considerations made the debate gravitate around the following question: *are there non-causal scientific explanations?* There are several positive answers to this question in the literature. Between the contenders for the title of non-causal explanation, the most discussed are equilibrium explanations (Sober 1983), optimality explanations (Rice 2012, 2015), structural explanations (Bokulich 2011) and mathematical explanations (Nerlich 1994, Batterman 2010, Huneman 2010, Lange 2013, Pincock 2015).

Why should this be of any interest to us in the context of the present discussion? Because in all the papers that argue for non-causal explanations listed above (except Bokulich 2011), the piecemeal approach to thinking about explanation plays an essential role in the argumentative strategy. Take, for example, Rice (2015). What Rice wants to do in this paper is to argue against the causal-mechanical account of model explanation. His strategy for doing that is to show that scientists often use in order to provide explanations a type of optimality models that instead of representing the causal structure of their target system(s), work by eliminating or distorting many of its causally relevant details. Rice explicitly disavows the traditional a priori approach to explanation and he is far from interested in a comprehensive account, so he adopts a piecemeal approach which, in this case, amounts to starting first by analysing "scientists' use of optimality models independent of any particular theory of explanation and then investigate how these cases fit with our current philosophical theories of explanation" (p. 591).

4. Mathematical explanation: some examples

The discussion in the previous section was meant to emphasise two things: the importance of the problem of mathematical explanations of physical phenomena for two recent philosophical debates and the relevance of the piecemeal approach for dealing with this problem. What I want to do next (sections *5* and *6*) is to analyse two different papers that use the piecemeal approach to develop accounts for mathematical explanations and see if they manage to make significant contributions to two important disputes in recent philosophy.

Before I do that, since the main idea behind the piecemeal approach to explanation is to start from the way scientists provide explanatory informations in different contexts, it is important to see if there are any cases in which mathematics was used to explain physical phenomena, i.e. if we can find in science examples of mathematical explanations. As even a quick survey of the literature on this topic revels, finding such examples doesn't appear to represent a problem – it seems that scientists make use of mathematics to explain physical phenomena even in unlikely disciplines such as biology.

Among the most discussed examples in the literature we can find the following ones:[13]

The honeycomb – biologists explain the fact that bees build their honeycombs as hexagonal grids with the help of the following mathematical theorem: a hexagonal grid is the optimal way to divide the Euclidean plane into regions of equal area with least total perimeter. The

[13] Besides these, there are other cases that are either non-scientific and therefore irrelevant if one wants to use EIA (e.g. the sticks example (Lipton 2011, p. 51), the division of toys example (Lange 2013, p. 488), the trefoil knot example (Lange 2013, pp. 489-90)), or are highly controversial (e.g. the antipodal weather patterns example (Colyvan 2001, p. 49)).

explanation goes as follows: in order to win the natural selection fight, bees had to choose the most economic (in terms of labour and amount of wax used) way to build their honeycombs. As it is clear from the mathematical theorem presented above, from all the possible shapes, the hexagonal grid is the most economical in the relevant respects. This is why the honeycombs have that boggling shape (Lyon and Colyvan 2008, p. 228).

The cicadas – biologists explain the fact that the North American periodical cicadas (fly like insects that spend many years underground in larval form) have life-cycle period lengths that are prime, with the help of the following mathematical theorem: the lowest common multiple of two numbers is maximal when the numbers are co-prime. The structure of the explanation is this:

> (1) Having a life-cycle period which minimizes intersection with other (nearby/ lower) periods is evolutionarily advantageous.
> [biological 'law']
> (2) Prime periods minimize intersection (compared to non-prime periods).
> [number theoretic theorem]
> (3) Hence organisms with periodic life-cycles are likely to evolve periods that are prime.
> ['mixed' biological/mathematical law] (Baker 2005, p. 233).

The bridges of Königsberg – why no one can ever succeed in crossing each bridge in Königsberg only once and then return to the starting point. The explanandum here is the impossibility of finding (no matter how many times one tries) a way to cross only once each of the seven bridges and then return to the starting place. The explanation is that such a crossing is impossible because the geometrical graph of the bridges cannot have an Euler trail, i.e. a closed path that includes each edge of the graph only once. For such a trail to be possible, the graph must not include any vertex of odd degree or more than two such vertices,[14] because every time we get to a vertex we must be able to leave it using a different edge. But in the graph of the bridges, all the vertices are touched by an odd number of edges.[15]

5. Lange's and Pincock's accounts of mathematical explanation and mathematical realism

Of course, presenting such examples, as the ones discussed in the previous section, does little to convince one that there is indeed such a thing as mathematical explanations of physical phenomena. So, in order to tip the balance in their favour, the mathematical realists, for example, have to do more than point to one, some or all the examples listed above (or to similar ones). What more do the advocates of mathematical explanations need to do in order to construct a convincing case? In Alan Baker's opinion, what is needed is a philosophical account for the type of explanatory relation involved in such examples:

> "A striking feature of this recent literature is the almost total absence of any analysis of just what kind of explanatory relation is involved in a typical MES [mathematical explanation in science]. This is a puzzling lacuna since the availability of an analysis of this sort would enable the discussion to move

[14]The degree of a vertex or its valence is the number of edges touching it.
[15] One of the vertices is touched by five edges and the other by three.

beyond the trading of intuitions about individual candidate cases of MES" (Baker 2012, p. 3-4).[16]

Lange (2013) and Pincock (2015) use a piecemeal approach to explanation to obtain such accounts. My aim in the rest of this section is to argue that, despite what may seem as evidence to the contrary,[17] Lange's and Pincock's accounts of what makes some mathematics using scientific explanations distinctively mathematical are useless in the context of the recent debate in the philosophy of mathematics between the realists and nominalists.

In Lange's view, a "distinctively mathematical explanation" is one that works by exploiting "what the world is like as a matter of mathematical necessity" (Lange 2013, p. 496). In order to get to his account, Lange uses a piecemeal approach to explanation. He starts his discussion (step 1) by presenting some examples of what he considers to be "scientific explanations that are mathematical in a way that *intuitively differs profoundly* from ordinary scientific explanations employing mathematics" (p. 486, my emphasis). After that he discusses what sets apart these explanations (steps 2 and 3). What characterizes such cases is not the fact that they fail to cite the explanandum's cause, because some of them do specify such causes (pp. 493, 495). It is also not the fact that they incorporate a mathematical explanation in mathematics, as Steiner (1978) suggested, because none of Lange's examples fit this account. And it is not even the fact that they are "carried out by essential appeal to mathematical facts" (Mancosu as quoted in Lange (2013, p. 491)) because this criterion is not restrictive enough in Lange's opinion. In the end (step 4) Lange offers an account of how these explanations work: a distinctively mathematical explanation works by "showing how the explanandum arises from the framework that any possible causal structure must inhabit, where the 'possible' causal structures extend well beyond those that are logically consistent with all of the actual natural laws there happen to be" (p. 505).

Take, for example, the case of crossing the Königsberg bridges. In Lange's opinion, unlike the usual scientific explanations, this explanation doesn't get its explanatory power from revealing something about the world's causal structure. What it does is to show that crossing the bridges in that particular way is mathematically impossible, and the fact that it is so explains why no trial to find such a path will ever succeed. So, the explanation is mathematical because it "exploits what the world is like as a matter of mathematical necessity" (Lange 2013, p. 496).

Pincock (2015) uses a similar piecemeal approach as Lange (he dubs it the case-driven approach) but he comes up with a very different account. In Pincock's view, a mathematical explanation is an abstract explanation. Since he favours an ontic conception of explanation, he takes abstract explanations as those explanations that describe an abstract dependence relation which obtains in the world and is completely objective. He starts by discussing Almgren and Taylor's alleged explanation of Plateau's laws for soap-film surfaces and bubbles – "a case that has been *identified as an explanation by expert practitioners*" (Pincock 2015, p. 858, my emphasis). In this case, we start by showing how soap films can be taken as instances of almost minimal sets and then prove that this requires the soap films to obey Plateau's laws. "This explained why Plateau's laws held for actual soap films and soap bubbles due to the

[16] For a similar remark, see Molinini (2016, p. 418).

[17] Lange (2013) and Pincock (2015) both make it clear that the ontological dispute in the philosophy of mathematics is irrelevant for their discussion. So, why am I assessing their contributions to it? For two reasons. First of all, the mathematical realists can try nonetheless to use these results to support their position by reasoning along the lines sketched here. Second of all, our interest in not with the actual contribution, but is with bringing to light some of the problems faced by the piecemeal approach and I believe that a good way to do this is by looking at these papers from the perspective of this dispute in the philosophy of mathematics. One can still wonder whether it isn't more appropriate to show what is wrong with a case of a piecemeal approach actually aimed at contributing to this dispute. From what I know, there is no such thing to be found in the literature.

intimate connection between these physical systems and the mathematical model" (p. 865). The next step is to account for this "intimate connection." Pincock takes it to be an abstract dependence relation that is explanatory pretty much in the same way as a causal dependence relation.

Does this help in any way the mathematical realists in their debate with the nominalists? Apparently it does. As we said above (section *3.1.*), the main idea behind the new indispensability argument is that it is possible to argue for the existence of mathematical entities by using the same type of argument that the scientific realists are using in favour of their position. All is needed for that is to show that there are scientific explanations in which mathematics plays an indispensably explanatory role. One way to do this is to argue that, in some cases of scientific explanations using mathematics, the mathematical part is indispensably explanatory because removing it (or trying to replace it) destroys the explanations. There is a problem with this argumentative strategy, though: it leaves open for the nominalist the possibility to reply that, even though mathematics is indeed indispensable in the cases discussed, it doesn't play an explanatory role, but it has other function. An apparently better strategy – one that seems to get rid of such problems – is to give an account for the explanatory relation that (allegedly) holds between mathematics and the physical phenomenon in cases as the ones presented above.[18] But this is exactly what Lange and Pincock did in their papers. So, yes, we can consider that Lange (2013) and Pincock (2015) make a significant contribution to the recent ontological dispute in the philosophy of mathematics by providing an important support to the mathematical realists.

There is a big problem with this way of assessing the importance of Lange (2013) and Pincock (2015) for the recent ontological dispute in the philosophy of mathematics, namely that it overlooks a key aspect of this debate: the complete lack of agreement about the cases that are discussed. If we take this into consideration, we can show that Lange's and Pincock's approaches are either question begging or useless, depending on the type of disagreement one is concerned with.

We can distinguish between two types of disagreement common in the recent philosophical discussion of the problem of mathematical explanations of physical phenomena. First of all, there is the disagreement between realists and nominalists about whether we are dealing or not, in the cases under discussion, with examples of scientific explanations and, if this is the case, whether mathematics plays indeed a genuinely explanatory part in them – the nominalists typically argue that the examples provided by the mathematical realists are not cases of mathematical scientific explanations. Second of all, there is disagreement among the advocates of mathematical explanation about which examples are special and which are just regular scientific explanations. In this case there is agreement about the existence of a class of special mathematics using scientific explanations, but not about the examples that belong to that class. In the first case there is no agreement about the existence of mathematical explanations; in the second case we have such an agreement, but we lack consensus about what characteristic features these explanations have.

Using a piecemeal strategy to account for mathematical explanations in a context in which there is disagreement of the first type amounts to begging the question against the nominalist. There are many philosophers who argue that there is no such thing as mathematical explanations of physical phenomena (e.g. Melia (2002), Daly and Langford (2009), Saatsi (2011)). So, in order to help the mathematical realist in this context, what is needed is a way to *show that* the examples presented are indeed cases of scientific explanation and that mathematics plays a genuinely explanatory part in them. But the piecemeal approach completely bypasses these problems. In this approach to thinking about explanation, the

[18] See also footnote 7 above.

examples discussed are somehow taken (Lange appeals to intuition for this and Pincock to the testimony of expert practitioners) from the start as special mathematics employing cases of scientific explanations, i.e. the starting point is exactly what the mathematical realists need to show to be the case in order to answer to the nominalists. So, if one intends to use Lange's and Pincock's accounts in this dispute all she will manage to do is to beg the question against the nominalist.

Someone can point out that the next steps in a piecemeal approach (especially step two) are meant to bring to light the characteristic features of the cases under discussion. So, it should, in principle, count as *showing that* these cases are not only apparently different, i.e. that we are indeed dealing with special cases of mathematics using scientific explanations (Lange (2013), for example, takes his analysis to show how the examples presented "differ from ordinary scientific explanations that use mathematics and how do they succeed in explaining" (p. 492)). Also, in Pincock (2015) the main task is that of determining what features can be taken to characterize explanations such as Almgren's and Tylor's). The problem is that this is helpful only to the unlikely extent that the nominalists share Lange's intuitions about the cases or agree with Pincock's reliance on and interpretation of what the expert practitioners are saying. Otherwise, all that their analysis can be taken to do is to show what characteristic features some cases of mathematics using scientific (non-explanatory) practice have, because there is no independent reason for taking such features as explanatory other than the fact that we decided/agreed from the start that we are dealing with genuine cases of scientific explanations.

The piecemeal strategy is not of much help for dealing with the problem of mathematical explanations even if used in a context in which there is agreement about the existence of a class of special mathematics using scientific explanations, because here too we can find disagreement about the examples – which should and which shouldn't be included in this class. Baker (2005), for example, is not happy with Colyvan's (2001) meteorological example, and Lange (2013) dismisses both Baker's cicada example and Lyon and Colyvan's (2008) honeycomb example. If there is no agreement about the starting cases, there will be no agreement about the account that is obtained with the help of the piecemeal approach and so such an account will be useless – it will not convince the nominalists and it will not be taken seriously by the other advocates of mathematical explanation.

6. Are there non-causal scientific explanations?

Lange (2013) and Pincock (2015) both provide accounts for what makes mathematical explanations different and how they succeed in explaining, but, for the reasons discussed in the previous section, these accounts are useless in the context of the recent explanation related ontological debate in the philosophy of mathematics. What about the debate over the existence of non-causal scientific explanations? The problem of mathematical explanations is of central importance for this debate also. Is Lange's and Pincock's way of dealing with it more suited for this context? I believe it is, because in this context agreement over the cases or the lack of it is no longer a concern. All that those who are discontent with the causal account need to do in order to challenge it is to construct a plausible case for considering that there are non-causal explanations and then let the burden of proof fall on whoever doesn't agree with it.[19] The most straightforward way to do this is by using a piecemeal approach and reason along the following lines:

[19] Someone can wonder at this point why such a strategy is good here, but something similar cannot work in the context of the ontological debate discussed above, i.e. why the mathematical realists cannot use the piecemeal approach and then let the burden of proof fall on the nominalists. As I see it, it all depends on one's aim. If the

1. The most popular account of scientific explanation take it as being essentially linked with causal relations.
2. But there are examples in which physical phenomena is explained some other way than by pointing to the chain of events that led up to it.
3. Therefore, in such cases other relations than the causal ones have to be taken as explanatorily relevant.[20]

But this is, in part, exactly what Lange and Pincock are doing in their papers. So, unlike the ontological dispute in the philosophy of mathematics, this is a far more appropriate context against which to try to understand and assess their contributions.

The question that will preoccupy us in the rest of this section is whether Lange and Pincock manage to construct convincing cases for thinking that there are non-causal mathematical explanations. My aim is to argue that they fail to do this. In order to dismiss a piecemeal case for non-causal explanations, one has to argue that the examples presented in support of this case are not genuine scientific explanations, but, obviously, without resorting for this to the fact that they don't work by describing causal relations.

Let's start with Lange (2013). In his case it can be argued that the examples he discusses are not of explanations, but of justifications. This is problematic for several reasons. First of all, it casts great doubt on the adequacy of the explanation identification tool used by Lange. Also, his account becomes dubious: he claims that the examples he analyses are cases of scientific explanation that are mathematical in a different way than ordinary mathematics using scientific explanations; but since they are in fact something else, there is no wonder that he finds that there is a fundamental difference between them and "other" scientific explanations. The worst part is that it doesn't seem to be a way out of this situation that doesn't make the approach circular.

Lange gives several examples of what he considers to be distinctively mathematical scientific explanations. Between these, the division of toys, the trefoil knot and the bridges in Königsberg examples. The problem is that, in each of these examples, the justification for believing that things stand in that particular way comes after and as a consequence of the mathematical reasoning. How do we know that the bridges in Königsberg *cannot* be crossed in that particular way, or that the trefoil knot *cannot* be untied or that one *cannot* distribute evenly five toys among three children? The mathematical facts in these examples are not used to answer a "why is *x* the case?" type of question, but a "why should we believe that *x* is the case?" question. What Euler did, for example, in the bridges of Königsberg case, was not to give an explanation but to provide a powerful justification for believing that it is impossible to find a route that would allow one to cross all the bridges only once. That this is so was not known independently before Euler's treatment of the problem – it was at most suspected. The same goes for other similar cases: we don't explain why a trefoil knot cannot be untied by showing that it is distinct from the unknot, but we justify the belief in this impossibility; and by pointing to the mathematical fact that twenty-three cannot be divided evenly by three we don't explain why a particular attempt will fail but we give reasons for believing that it will fail. So, what Lange takes as being distinctively mathematical explanations are actually mathematical justifications. But then he chooses a bad starting point in his piecemeal approach to the problem of mathematical explanations and this compromises his entire

aim is to tip the scales in favor of one of the sides in a debate, then trying to do this by assuming from the start the very thing around which the debate gravitates can only end in question begging. If, on the other hand, the aim is only to challenge a preexistent account by coming up with cases that (apparently) are not accommodated by it, as I take it to be the case with the second debate, then it makes sense to appeal to a shift of burden of proof kind of strategy.

[20] This is very similar with the way Batterman and Rice (2014) present their argumentative strategy against the "common features account of what makes a model explanatory" (Batterman and Rice 2014, p. 350).

account. This may not make a lot of sense to someone who adopts something resembling Hempel's symmetry thesis: every explanation is a potential prediction[21] and every prediction is a potential explanation (Hempel 1965, p. 234 and p. 367). Of course, we can find in the literature plenty reasons for rejecting such a thesis[22] and even Hempel expresses doubts about the second part (p. 367). As Kim (1964) argues, "there are important conceptual differences between explanation on the one hand and predictive and retrodictive arguments on the other, and these differences have some interesting consequences" (p. 361). Between them, we can count the fact that – against what is stated in the first part of the symmetry thesis – there are explanations that lack predictive power (i.e. explanations of phenomena that could not have been predicted before they actually occurred) like e.g. the explanation for earthquakes. There are also problems with the second part of the thesis. This is usually exemplified in the literature with cases such as the flagpole, the barometer and the tides (Salmon 1989, p. 47). So, pace Hempel, justifications and explanations are not always different sides of the same coin. This doesn't mean, though, that there are no justifications that can be potential explanations. So, it may seem that there is nothing wrong with Lange's account – if it can be shown that the cases he discusses belong to this category. In order to do this, he needs to show that the information used in these cases can also be explanatorily relevant. But this doesn't work at all well with a piecemeal approach (remember that in such an approach the task is to show how the cases discussed succeed in explaining, not that they are successful in this regard (see section *2*)). This, first of all, because, for a piecemeal approach to make sense, the starting cases have to unequivocally be cases of explanations, otherwise the account developed would be unacceptable (it makes no sense to develop an account about what characterizes e.g. a certain type of plant when we have no idea whether the thing under consideration is indeed a plant). Now, the worst thing one can do in this situation is to try to use the account arrived at to remove the doubts about the starting cases. This would, of course, make the approach circular. So, I see no way for Lange to do what is needed to save his account – i.e. show that the starting cases are cases of justification that can be potential explanations.

There is something else problematic with Lange's account.[23] As we said above, in Lange's view a distinctively mathematical explanation works by showing that the fact to be explained is more than physically necessary, it is mathematically so. But is showing that a phenomenon is more than physically necessary equivalent to providing an explanation for it? Before attempting an answer, I think it is instructive to look for a second at another problem: when does something stand in need for an explanation? This topic has drawn very little attention, so there are not many accounts in the literature; but many philosophers[24] seem to adopt a sort of surprise account of the need for explanation: something is in need of an explanation if it is surprising,[25] i.e. if we have reasons to believe that it would not be the case.[26] Now, since in the case of necessary truths there are no reasons for believing that things could have been otherwise, they are not surprising and so – if we accept this view about the

[21] A prediction can be taken as an *ex-ante* justification (see Schurz (1995, p. 438).

[22] See for example the discussion in Rescher (1958), Kim (1964), Salmon (1989) and Schurz (1995).

[23] What follows should not be taken as a further argument against Lange's account, but only as an attempt to show that there are strong reasons to consider it strange.

[24] For example Peirce (1908/1968), Hempel (1965), Laudan (1977), Berkovski (2002), Baker (2011), and Schupbach and Sprenger (2011).

[25] See for example Schupbach and Sprenger (2011, p. 108): "a hypothesis offers a powerful explanation of a proposition, in this sense, to the extent that it makes that proposition less surprising."

[26] Hempel, for example, says that "explanation-seeking questions of the standard type 'Why is it the case that p?' are often, though by no means invariably, prompted by the belief that p would not be the case – a belief which, again, may seem to the questioner to be more or less strongly supported by certain other empirical assumptions which he accepts as being true." (Hempel 1965, p. 429)

need for explanation – they are not in need of explanation. Starting from here, we can give a negative answer to our question, because showing that something happens or cannot happen as a matter of mathematical necessity is better taken as a dismissal of the search for an explanation rather than as a way of providing an explanation. This means that Lange's account – granted we adopt a surprise account of the need for explanation (or something related to it)[27] – instead of being an account about how distinctively mathematical explanations work, can be better taken as a strategy for dismissing the need for explanation in some cases.

In conclusion, what Lange does is this: he misidentifies some examples of mathematical justifications as cases of mathematical explanation and then he proceeds to give an account of what makes such cases distinctively mathematical scientific explanations which for many may look more as a way of showing that in such cases there is no need for explanation.

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We said above that one way to argue against a piecemeal case for non-causal explanations is to try to show that the examples used to support such a case are not genuine scientific explanations. But what can we do if someone adopts a sort of rigid attitude towards these examples, i.e. takes any attempt at showing that these examples are not scientific explanations as a violation of the piecemeal methodology? This is exactly what Pincock does. The case study discussed in Pincock (2015) is that of the alleged explanation for why Plateau's laws hold for actual soap films/bubbles provided by Almgren theory of (M, ϵ, δ)-minimal sets (including Taylor's proof that these sets satisfy Plateau's laws). As Pincock draws the attention repeatedly, there is not much that one can say against this case, because trying to reject that we are dealing with a genuine explanation amounts to a violation of the "case-driven methodology. It is expert practitioners who should guide our judgements on cases and influence our philosophical theory of explanation" (p. 870).

There is a way, though, to question the example while still playing by the rules of Pincock's case-driven game, namely by arguing that the 'expert practitioners' themselves are not really taking such a case to be an example of explanation. I believe we can show that this is the case with the example discussed by Pincock. The explanandum in this example is supposed to be the fact that the soap film/bubble systems obey Plateau's laws. What would explain such a fact? In Pincock's opinion this fact is explained by the theory of (M, ϵ, δ)-minimal sets as developed by Almgren and Taylor. The alleged explanation can be decomposed into three parts:[28] a definition of a mathematical analogue for soap films and soap bubbles; a derivation of Plateau's laws about the way surfaces meet, starting from the assumption that we deal with smooth surfaces with a finite total area; a proof that every soap-film-like or soap-bubble-like geometric configuration is composed of smooth surfaces whose total area is finite. This does seem to accurately represent the structure of Almgren and Taylor 1976 paper, except for one crucial detail. In their paper, Almgren and Taylor's aim to "demonstrate how a few observations concerning the way in which soap films and soap bubbles are free to change to decrease their energy form the basis of a mathematical model of soap-film-like and soap-bubble-like configurations of surfaces" and so, to show that "the area-minimizing principle alone is sufficient to account for the overall geometry of soap films and soap bubbles" (Almgren and Taylor 1976, p. 93). They do not undertake the task of explaining why soap film/bubble systems obey Plateau's laws.

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[27] This account has been criticised recently in Grimm (2008) and Wong and Yudell (2015), but the alternative accounts proposed in these papers are far from making things better for Lange. For lack of space I will refrain from entering into details here.

[28] Pincock (2015, p. 861) attributes this way of presenting the 'explanation' to Almgren and Taylor (1976).

According to a physical principle governing the behaviour of soap films and soap bubbles, a physical system tends towards the state of lowest energy, so it preserves a certain configuration only if it cannot easily alter it to one with less energy. In the case of soap films/bubbles, this principle is related to a physical system's tendency to minimize its surface area. The surface energy (tension) of a soap bubble is the result of the unbalance of the attractive forces between molecules at the boundary of the surface of the liquid. In the absence of gravity and differences in air pressure, the existence of these unbalanced forces produces an interesting effect: a liquid's surface turns into (sort of) an elastic membrane that tends to minimize its area and so its surface energy. Almgren and Taylor capture this area-minimizing principle in mathematical terms this way: a geometric arrangement of two-dimensional surfaces attached to a frame or enclosing one or more regions of space is soap-film-like, respectively soap-bubble-like if it cannot be forced to have a smaller area by any small deformation that leaves the frame fixed and/or that doesn't alter a region's volume (p. 85). In order to show that the mathematical model created with the help of this principle accurately represents the manner in which actual soap films/bubbles are formed, two things have to be shown: that given a frame or the volume of some region of space being specified, there are soap-film-like and soap-bubble-like configurations of mathematical surfaces spanning that frame or enclosing that region, and, secondly, that any such configuration of surfaces conforms exactly to the Plateau's laws. Almgren and Taylor managed to prove that this is the case, and this is what they discuss in their 1976 paper.

As it is obvious – I hope – from this short presentation, this has nothing to do with explaining why the actual physical systems obey Plateau's laws. Nowhere in their paper Almgren and Taylor claim otherwise. Taylor's proof that for two-dimensional (M, ϵ, δ)-minimal surfaces in $\boldsymbol{R}^3$ there are only two possible kinds of singularities (i.e. those discovered experimentally by Plateau) is taken as showing only that these surfaces provide a good mathematical model for soap films and soap bubbles and not that it explains something about these physical systems.[29] Why would Pincock claim otherwise?

Actually, Pincock doesn't credit Almgren and Taylor with taking their mathematical theory as explaining something about the actual soap films and soap bubbles, but gives the following quote from Frank Morgan:

> "Physical surfaces such as soap films often consist of pieces of surface meeting along whole singular curves. These curves, although not part of the given boundary, unfortunately count as boundary for rectifiable currents. *Explaining the structure of soap films required a new theory of (M, ϵ, δ)-minimal sets developed by F. Almgren and J. Taylor*" (Morgan 1996, p. 376, my emphasis).

I believe that taking Morgan's claim at face value is a mistake. Morgan asserts this in the context of his discussion of the problem of finding an inclusive definition for a general surface in $\boldsymbol{R}^3$. The best contender for providing such a definition is the theory of rectifiable currents.[30] There is a problem, though: rectifiable currents do not allow surfaces that are non-orientable, but many surfaces, such as the Möbius strip, are not oriented. Also, as Morgan

[29] Pincock has a different opinion. According to him, "Taylor's purely mathematical proof established that the almost minimal sets satisfy Plateau's three laws. This explained why Plateau's laws held for actual soap films and soap bubbles due to the intimate connection between these physical systems and the mathematical model." (2015, p. 865) But Taylor doesn't say anything like this. A philosopher can, of course, take the relation of instantiation as an explanatory relation and argue that a model, due to its "intimate connection" with the modeled physical system, explains something about it. But that is arguably far from what a mathematician would say and is definitely not something that Almgren and Taylor are saying.

[30] A rectifiable current is a current (i.e. a linear functional on differential forms) associated to a rectifiable set.

remarks in the above quote, the curves along which surfaces such as soap films meet count as boundary for rectifiable currents. In order to tackle these issues, new theories have been developed. Among them, the one that seems to best *model* the kind of surfaces which arise in soap films and soap bubbles is Almgren and Taylor theory of (M, ϵ, δ)-minimal sets. I take this to be the meaning of Morgan's claim. By "explaining" he means constructing a model. If we look carefully at the broader context, we realize that it wouldn't make sense for him to say that the theory of (M, ϵ, δ)-minimal sets is required for explaining something about the actual physical systems.

Remember that the problem here is that of determining if the expert practitioners explicitly take this case as an explanation for why soap films/bubbles obey Plateau's laws. The point in doing this is that of finding out if it is possible to apply a case-driven accounting strategy to it. As I argued above, neither Almgren and Taylor, nor Morgan take the theory of (M, ϵ, δ)-minimal sets as explaining something about soap films or soap bubbles, so Pincock uses a bad starting point for his case-driven accounting methodology and this compromises his entire account.

7. Problems with the piecemeal approach

The piecemeal approach is a relatively new but fairly widespread strategy for dealing with explanation related philosophical topics. But how good is it? Until now I tried to make a little bit clearer what this approach amounts to (section *2*) and how important it is (and can be) in recent philosophy (section *3*). I also drew attention to how it can go wrong if it is misused. We have seen (section *5*) that if one tries to use such an approach in the context of a dispute in which there is strong disagreement about the explanatory worth of the cases discussed – as it happens in the recent ontological dispute in the philosophy of mathematics – it will degenerate into question begging. But, even when used in a more appropriate context, this approach is not problems free (section *6*). The most important problem plaguing such an approach has to do with the correct pre-theoretic identification of something as an explanation (we have seen that both Lange and Pincock used bad starting cases in their piecemeal approaches to the problem of mathematical explanations). As we said above, in a piecemeal approach we start from a case that has been somehow identified as an explanation and then we proceed to bringing to light its characteristic features and to constructing an account for the type of explanation it exemplifies. But the characteristic features of such a case have to do with its explanatory power only to the extent that the case is a genuine example of scientific explanation, and the account constructed at the end is an account of how such explanations succeed in explaining to the same extent. So everything depends in such an approach on managing to correctly identify as explanatory those aspects of the scientific practice that are indeed so. The crucial question that anyone who wants to use a piecemeal approach to thinking about explanation has to answer then is this: *what can be reliably used to identify something as an explanation?* This is not a trivial task! My aim here is to analyse three answers to it and show why they are not good. We already encountered two of these in our discussion of Lange's and Pincock's papers: intuition and the testimony of expert practitioners. Beside them, I will discuss one other contender that, even though not used (as far as I know) in the literature, is important because of its relation with the testimony of expert practitioners: the feeling of understanding.

7.1. Intuition

Can we rely on intuition for distinguishing between the genuinely explanatory and the other parts of the scientific practice? Lange (2013) reserves a special place for intuition in his endeavour to characterize the distinctively mathematical scientific explanations. Also, a closer look at the literature reveals that intuition is indeed taken by many to play an important (even if only backstage) role in guiding the (front stage) discussion on explanation. It is usually considered, for example, that the DN-model of explanation failed because it did not "capture the *intuitive relation of explanatory relevance*" (Hitchcock 1995, p. 304, my emphasis) and so it allows for "derivations which are *intuitively non-explanatory* to meet the conditions of the model" (Kitcher 1981, p. 508, my emphasis). And so we can say that an important (if not the main) task on the agenda of many philosophers preoccupied with accounting for scientific explanation seems to be that of capturing the intuitively explanatory judgments encountered in a scientific context.[31] This suggests that many philosophers may agree with the following thesis:

> ***I****: something is an explanation or has explanatory value if it intuitively seems this way.*

There are obvious problems with this thesis, the most important one for our discussion having to do with the fact that intuition is notoriously unreliable. In our discussion this surfaced in section *6*, where we have seen that Lange's reliance on intuition in delimiting from ordinary scientific explanations that use mathematics those examples of scientific explanation that are distinctively mathematical made him take some cases of mathematical justification as examples of mathematical explanation. Another problem has to do with the fact that people usually have conflicting intuitions. We have seen above that there is little agreement even among the advocates of mathematical explanation about which examples should be considered genuine cases of mathematical explanation and which not. I believe this is enough to suggest that relying on intuition when selecting the starting case in a piecemeal approach to thinking about explanation is a very poor choice.

7.2. The feeling of understanding

Let's try a different approach and start by attempting to answer the following question: how do we *usually* recognize an explanation as such?[32] Intuitively, this question should be easy to answer. After all, explanation occupies a central place in our cognitive lives: ever since childhood all of us are involved in some sort of explanatory practice – always asking/searching for, judging and producing explanations. So, recognizing such a thing should not be a difficult matter.[33] How do we do it then? It seems that the main identification resource that is commonly exploited in this context is the close link between explanations and our sense of understanding.[34] So, we normally consider (in a pre-theoretical and

[31] The literature is replete with considerations involving intuition so I will refrain from giving any more references here.

[32] What I have in mind here is not the different and much more difficult problem of recognizing a correct or good explanation.

[33] I am not saying here that because we are all involved in some sort of explanatory practice it should be easy to describe what we are doing when explaining something or how we manage to understand with the help of an explanation. I am making the much weaker claim that involvement in a certain practice presupposes being able to recognize and to play by the rules constituting that practice – in our case, recognizing an explanation when presented with one and being able to produce one.

[34] I am concerned in this section only with the subjective feeling of understanding and its relation with our pre-theoretic concept of explanation, not some philosophical objectivist conception about understanding. Someone can be puzzled by this choice, given the recent enthusiasm for understanding in epistemology and the philosophy of science. Instead of discussing about the feeling of understanding, wouldn't it be better to try to see how good can one of the accounts of understanding found in the literature be for helping us to determine if something is or not an explanation? I do not believe so, no. Due to space limitations, I cannot enter into much details here for

commonsensical way) that something is an explanation if it helps us understand some unexpected, surprising or puzzling fact, event, action etc. Of course, this is enough only when we want to know if something is an explanation or not; if, on the other hand, we are concerned with how good a certain explanation is, we focus on the measure of fulfilment of the goal. We say "I don't get it, so this is a bad explanation" if we don't understand the thing that is meant to be understood with its help, or "I find this explanation better than the other one" if it helped us understand something better than another explanation did.

Someone can use such considerations to argue that it is the relation with the sense of understanding that helps us determine (pre-theoretically) if something is or not an explanation and we rely on the way our feeling of understanding is affected to distinguish between bad, good, or better explanations. When realizing, for example, that we cannot have an explanation of the length of a certain tower in terms of its shadow, we are guided by the fact that we cannot understand the former with the help of the latter. What makes us reject as non-explanatory a derivation of the length of a pendulum from information about its period and the value of the acceleration produced by gravity is the fact that such a derivation doesn't produce understanding. Such considerations can be taken to lend support to the following thesis:

> ***SU:*** *something is an explanation or has explanatory value if it helps with making intelligible some unexpected, surprising or puzzling fact, event, action etc…*

Unfortunately, if we attempt to use it this way, the feeling of understanding suffers from the same kind of problems as intuition does: it is unreliable and subjective.[35] Let's take them one at a time. First of all, the feeling of understanding is nothing more than the subjective experience one may have when presented with an explanation, and, as any respectable subjective experience, it varies from person to person – the information one takes as contributing to understanding in the context of a particular explanation may lack such a quality or even be complete gibberish for someone else; so, using importance for understanding to distinguish between explanatory and non-explanatory scientific practices would render it hopelessly relative. Secondly, the feeling of understanding cannot be taken as reliable evidence for *good* explanations so whatever judgments we make with its help are worthless.

7.3. The testimony of expert practitioners

What about the testimony of expert practitioners (in our case, the scientists)? Can we rely on them to sort the explanatory from the rest of scientific practices? Does something like the following thesis makes sense?

> ***EP***: *something is an explanation or has explanatory value if the expert practitioners say so.*

There are philosophers who seem to think so. Baker, for example, says:

> "I do not know how to demonstrate that the mathematical component is explanatory. On the other hand, I think it is reasonable to place the

why I believe this to be the case. I will say, though, that it has to do with circularity: between understanding and explanation there is an intimate connection, so, if one wants to use an account of understanding for determining if something is an explanation or not, one has to make sure that that account is not derived from an analysis of some representative cases of explanation (and I am not sure that this is not the case with most of the accounts of understanding that I know about).

[35] See also the discussion in Trout (2002, 2007).

> burden of proof here on the nominalist. *The way biologists talk and write about the cicada case suggests that they do take the mathematics to be explanatory , and this provides good grounds, at least prima facie, for adopting this same point of view*" (Baker 2009, p. 625, my emphasis).[36]

But how reliable are scientists in this respect? Do they have some sort of objective criteria for distinguishing the explanatory from the rest of scientific practices? If not, relying on what scientists say in this context amounts to nothing more than relying on their intuitions or their feeling of understanding. But are their intuitions or their subjective experiences more reliable? If we cannot find good answers to these questions, and I see no way we can do this, than Baker's 'good grounds' vanish into thin air.

Let's elaborate. Prima facie, it seems to be something right in trusting/relying on what scientists have to say when it comes to discussing about scientific explanations. After all, being involved in the business of producing scientific explanations gives them more authority on matters related to such a topic. But how far should we take their authority to stretch? This is an instance of a more general question: how much knowledge the involvement in a certain practice gives one about that practice? Not much, I'm afraid, because, to use Peter Lipton's words, "it is one thing to be good at doing something, quite another to understand how it is done or why it is done so well" (Lipton 2004, p. 1). Being involved in a certain practice doesn't give one a privileged knowledge about that practice. So, the fact that scientists are experts at producing scientific explanations doesn't make them also experts in knowing what a scientific explanation is, what kind of explanatory relation is involved in a certain scientific explanation or what part is doing the explanatory job. Their expertise stops at producing scientific explanations. So, from the perspective of a philosophical concern with explanation, their opinions on it should not be of much value. To see why, let's look at the following quote from Steven Weinberg:

> … "it is a tricky business to say exactly what one is doing when one answers such a question [a why question]. Fortunately, it is not really necessary. *Scientific explanation is a mode of behavior that gives us pleasure, like love or art*. The best way to understand the nature of scientific explanation is to experience the *peculiar zing that you get when someone* (preferably yourself) *has succeeded in actually explaining something"* (Weinberg 1994, p. 26, my emphasis).

What Weinberg means here by the "peculiar zing" that one experiences when presented with or discovering an explanation is nothing more than the feeling of understanding we discussed about above. So, if we take his words as representative for how scientists in general think about explanation, and I see no reason why not do it, the explanation identification tool used in science is not that different from what lay people commonly use in everyday life – the only difference has to do with what the feeling of understanding is susceptible to be affected by. But then, looking at what "expert practitioners" have to say and permitting them to influence our philosophical theories of explanation amounts to nothing more than relying on the subjective, vague and misleading feeling of understanding as it manifest itself in a scientific context. Everything we have said in the previous sections applies here as well.

8. Conclusion

[36] See also Pincock (2015, p. 870).

Many philosophers share nowadays the conviction that our view about scientific explanation can become more complex if we look more carefully at the details of the actual scientific explanatory practices. This makes them repudiate the detached from the scientific explanatory practice armchair approach to this topic, and also the general approach that does take into consideration this practice but disregards its complexity in order to search for a comprehensive account. Instead of these, many of them prefer a piecemeal approach to thinking about explanation. In this approach, the idea is to look more closely at the way scientists are producing explanations in different contexts – at the type of information they are using for this and at the way they are using it – in order to provide a more adequate view of such a practice. A closer look is not without its problems though. If when looking from afar at something one runs into the danger of missing its complexity, when taking a very close look one risks to miss entirely what she is looking at. In order to avoid this, what the closer look needs is something to keep it on the right track. In our case, what the piecemeal approach needs is a reliable pre-theoretic way to distinguish between the genuinely explanatory and the other parts of the scientific practice. Without this, it risks to replace the general approach's diversity blurring point of view with a diversity booming one (i.e. a point of view that presents as explanatory parts of the scientific practice that are not so). I tried to show this here by analysing Lange's (2013) and Pincock's (2015) piecemeal approaches to the problem of mathematical explanation.

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