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SOME CONCERNS REGARDING EXPLANATORY PLURALISM
THE EXPLANATORY ROLE OF OPTIMALITY MODELS

Abstract

Optimality models are widely used in different parts of biology. Two important questions that have been asked about such models are: are they explanatory and, if so, what type of explanations do they offer? My concern in this paper is with the approach of Rice (2012, 2015) and Irvine (2015), who claim that these models provide non-causal explanations. I argue that there are serious problems with this approach and with the accounts of explanation it is intended to justify. The idea behind this undertaking is to draw attention to an important issue associated with the recent pluralist stance on explanation: the rampant proliferation of theories of explanation. This proliferation supports a pluralist perspective on explanation, and pluralism encourages such a proliferation. But, if we are not careful about how we arrive at and how we justify new accounts of explanation — i.e., if we do not try to avoid the sort of problems discussed in this paper — we may end up trivializing the concept of explanation.

Keywords: optimality models, non-causal explanation, optimality explanations, explanatory pluralism, explanation in biology

INTRODUCTION

Optimality models are used in different parts of biology to deal with the evolution and fitness of traits (physical characteristics or behaviors) by using only the phenotypic strategies available given a set of constraints, with no reference to the system of genetic and epigenetic transmission. Despite their

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utility, not all biologists agree about the long term value of such models. Some authors take optimality models to be poor substitutes for more comprehensive models that incorporate the full details of genetic transmission. One way to try to convince such scientists of the value of optimality models is to argue that these models offer *the best explanation* for a certain class of biological phenomena, so replacing them with more comprehensive models will not improve our understanding of such phenomena. Before doing this, one needs to answer two questions: "Are optimality models explanatory?" and "If so, what type of explanations do they offer?" — many philosophers answer the first question in the affirmative (e.g., Orzack, Sober 1994, Elgin, Sober 2002, Potochnik 2007, 2010, Rice 2012, 2015, Batterman, Rice 2014, Irvine 2015) but opinions are divided as to the second question. Some writers, such as Angela Potochnik, take optimality models to offer causal explanations, whereas others argue that these models provide some type of non-causal explanations. My concern here is not with the type of explanation offered by optimality models but with the approach that Collin Rice and Elizabeth Irvine take in order to show that these models provide non-causal explanations. I argue that there are serious problems with this approach. I will start by presenting Potochnik's account of how optimality models explain (section 1) and will then analyze Rice's (2012) reaction to this account and his proposed alternative account. I will argue that there are important problems with Rice's alternative account (section 2). We can find a possible solution to these problems in (Rice 2015). I will discuss it in section 3 and show why it does not work. In section 4, I will discuss Irvine's (2015) argument for the existence of non-causal explanations that appeals to optimality models; I will argue that it runs into the same kind of problems as those discussed in connection with Rice's account.

The overall idea is not to criticize these two philosophers but to use their work to draw attention to an issue associated with the recent pluralist stance on explanation: the rampant proliferation of theories of explanation. This proliferation supports a pluralist perspective on explanation, and pluralism encourages such a proliferation. But, if we are not careful, the literature on scientific explanation will very soon look like the product of an out-of-control account generating machine.

Many philosophers, stimulated by the new pluralist trend, propose new accounts of what makes something explanatory. Only a few reflect on whether this is done the right way, and even fewer think about what would "the right way" be in this context. Does it make sense to worry about this? I believe it does and, in this paper, I take upon myself the task of showing that there are good reasons for being concerned. I aim to do this by arguing that the accounts found in three recent papers on the explanatory role of optimality models are deeply problematic. Now, just to be clear, my aim is not to argue that there is something wrong in general with explanatory pluralism but to show that we need to be extremely careful about how we arrive at and how we justify new accounts of explanation. If we fail to do this, we may end up trivializing the concept of explanation.

1. ARE OPTIMALITY MODELS EXPLANATORY? POTOCHNIK'S ANSWER

Potochnik (2007, 2010) takes optimality models to provide causal explanations because they represent the way the causal processes of natural selection influence the evolution and fitness of traits. Since such models disregard all other factors that contributed to an evolutionary outcome except natural selection, they offer a type of *censored causal explanations* (Rice 2012) that focus only on "a *modular part* of the causal process that led to the event to be explained" (Potochnik 2007: 685).

The explanations offered by optimality models are adequate, in Potochnik's view, because they (sometimes) satisfy the following criteria of adequacy:

1. $\Pr(E|C_{expl}) \approx \Pr(E|C)$

2. $\Pr(E|C_{expl}) \approx \Pr(E|\, C_{expl} \wedge C_k)$ *for all* C_k (Potochnik 2007: 684).

Where C_{expl} is the set of causes used in an explanation of event E; C is the set of all causal factors that influence E; and C_k represents each event that is a causal influence on E.

These criteria ensure that an explanation is considered to be adequate only if it does not omit any causal information that, if taken into consideration, would *drastically* affect the expected probability of the explanandum-event. But optimality models do ignore all other causal factors that affect some phenotypic evolutionary outcome except natural selection, so it is safe to admit the possibility that "even an optimality model that correctly represents the selection dynamics and issues an accurate prediction may fail to satisfy this criterion" (Potochnik 2007: 687). This does not mean, of course, that optimality models fail in general to offer adequate explanations. According to Potochnik's, "there are plenty of instances in which the . . . [adequacy criteria are] met. Indeed, the customary assumptions of optimality models succeed for the most part for the same reason that this condition is met. For instance, the assumption that a particular set of phenotypes is available typically succeeds when any unrepresented constraints do not significantly affect the expected outcome" (2007: 687).

Potochnik believes that there are two other conditions besides providing adequate explanations that, if satisfied by optimality models, ensure that they offer the best explanations for a certain class of biological phenomena: (i) they represent those causes that figure into the causal relationship of interest in a certain context of inquiry, and (ii) are maximally general given the two previous conditions (2007: 683). Arguably, there are circumstances in which optimality models meet all these conditions. They satisfy condition (i) because there are contexts of inquiry in which the ecological influences on the course of selection are of particular interest, and in such contexts the focus is exactly on the type of causal relationship represented by optimality models — i.e., the causal relationship obtaining between natural selection acting on a certain phenotypic trait, and the value(s) of this trait present in the population (Potochnik 2007: 686). They also satisfy condition (ii) because they disregard any genetic information and so are maximally general — i.e., they avoid all information unrelated to the causal relationship of interest and so are applicable to all the systems that instantiate this relationship.

In sum, if Potochnik is right, optimality models provide a sort of censored causal explanation that is the best explanation for the phenomena related to the phenotypic outcomes of long-term evolution by natural selection in the context of a biological inquiry whose central interest is the ecological influences on the course of selection.

2. RICE'S ALTERNATIVE ACCOUNT

Potochnik's (2007) discussion of optimality models is aimed at establishing two points: (i) that optimality models provide a type of causal explanation and (ii) that they provide, in some contexts of inquiry, the best explanations for a certain class of phenomena. Rice (2012) undertakes the tasks of showing that there are serious problems with both these points, and that there is a better approach to understanding how optimality models provide explanations. He argues that Potochnik (together with all the other philosophers that adopt a censored causal model approach to understanding the explanatory virtues of optimality models, such as Orzack and Sober (1994), or Elgin and Sober (2002)) fails to "establish a permanent explanatory role for optimality models in biology and mischaracterizes the explanatory virtues provided by biological optimality models" (Rice 2012: 687). This part of Rice's paper is of little importance for our discussion. My interest here is with Rice's proposal of an alternative account of the way optimality models provide explanations. I will argue that Rice fails to make a good case for the need to take into consideration such an alternative.

Before discussing the details of Rice's argument, let's consider what conditions have to be satisfied by a good proposal of an alternative account in some domain. There are two such conditions that apply generally: (a) to provide a good justification for the need to abandon the old account (i.e. to show, among other things, what is wrong with this account that prompts the need for its abandonment), and (b) to specify the features that the new account has to have to be better than the one it replaces (it is insufficient to show that there are problems with the old account, it is important to be shown also what needs to be done right in order for some new account to be considered better). Does Rice's approach satisfy these two conditions? Let's take them in turn.

Does Rice justify the need to abandon the censored causal model approach? What Rice (2012) does in this regard is to argue that optimality models do not provide a sort of censored causal explanation, as Potochnik and others would have it, because they do not work by representing a modular part of the causal evolutionary process and they make essential use of idealizations. According to Rice, what distinguishes optimality models from other evolutionary models is not the fact that they work by disregarding all the information concerning the causal processes of genetic biological evolution — interpreting them this way ignores the fact that such models are also used in other disciplines than biology, such as economics. What distinguishes them is the use of a mathematical technique called Optimization Theory, which is usually applied to determine the values of some control variable(s) that will optimize the value of some design variable(s) given a set of trade-offs and constraints (Rice 2012: 695-696). According to Rice, this realization prompts a reconsideration of the way optimality models work. They work not by representing a modular part of the causal process underlying the phenomena of interest, but by describing "a function, which relates each possible set of control variables (i.e., the strategies) to values of the design variable(s) to be optimized. This function and the set of available strategies are determined by the constraints and trade-offs of the particular design problem. Once the strategy set and objective function of the optimality model are specified, one can deduce which of the available strategies will yield the optimal values of the design variables" (Rice 2012: 696).

Knowing that the defining characteristic of optimality models is that they use Optimality Theory to determine the optimal solution to some design problem after identifying the key constraints and trade-offs that hold within a system makes it easy to figure out what feature the new account of how optimality models explain must have in order to be superior to the censored causal account. It has to take these models to provide a sort of non-causal explanation. This is mostly because the trade-offs among the variables within the model are not the kind of thing that can enter into causal relationships but also because these models usually make essential use of idealizations.

So, it looks like Rice's (2012) approach satisfies both conditions mentioned above and, apparently, can be taken as a good proposal of an alternative account to the censored causal model. It shows that there are problems with the censored causal account (condition (a)) and it points in the direction of a better account (condition (b)). Despite this, there is an important problem with Rice's approach. Let's look at the structure of Rice's (2012) approach.

(1) According to some account ***3***, ***X*** is explanatory in virtue of having property ***E*** because, according to model of explanation ***Σ***, being explanatory amounts to having ***E***. [In our case, according to the censored causal account, optimality models are explanatory because they describe a modular part of the causal process responsible for some phenomena and, according to the causal model of explanation, this (or something like it) is the property that something needs to have in order to count as an explanation].

(2) But a closer look at ***X*** reveals that, instead of ***E***, its characteristic feature is ***F***. [This corresponds to Rice's argument that (most) optimality models do not work by representing a modular part of the causal evolutionary process, but what characterizes them is the use of the mathematical technique called the Optimization Theory].

(3) Hence, in light of (2), ***X*** is not explanatory in virtue of having property ***E*** as account ***3*** would have it, *so it must be explanatory in some other way*. [If (2) is correct, then optimality models cannot be explanatory in virtue of tracking causal processes, so they must be explanatory in some other way].

(4) Since ***F*** replaces ***E*** in this new approach, it is only reasonable to suspect that ***X***'s explanatory capabilities have to do with ***X***'s having ***F***. [As Rice argues, the characteristic feature of these models has to do with the fact that they represent various relationships that hold between the constraints, trade-offs, and the optimal strategy of a system, so a new account of their explanatory power has to focus on this aspect (2012: 698)].

(5) In the event that there is no model of explanation available that can replace ***Σ*** and take being explanatory as amounting to having ***F***, the discussion above prompts the need to develop such an alternative account. [In our case, such an account should take as explanatory the set of relationships found in optimality models: the constraints and trade-offs that hold between the represented variables and those variables' relationship to the equilibrium point of the system (Rice 2012: 698)].

It is easy to see that there is an important problem at step (3). All that follows from (2) is that account ***Ʒ*** has it all wrong, so ***X*** *is not explanatory* — i.e., that Potochnik's account fails because it misidentifies the characteristic feature of optimality models and so these models are not explanatory. In order to make the further point that optimality models *must be explanatory in some other way*, one needs to come up with a different reason for thinking that, despite not having the property taken by the causal model of explanation as essential for something to be an explanation, optimality models do nonetheless provide explanations. The problem consists in first criticizing the very reason for thinking that ***X*** is explanatory and then asserting, without further argument, that ***X*** is explanatory after all. This is exactly what Rice (2012) does. He argues only that something like Potochnik's account of how optimality models explain faces serious problems. But instead of concluding that these models are not explanatory as he should, he takes the failure of this account to justify the need to look for a different account of how these models explain.

This discussion suggests that there is a further condition that a good proposal of an alternative account of something has to satisfy: (c) the new account needs some independent motivation/justification (relying for its justification on elements of the old account would amount to sawing off the branch on which one is sitting). As I tried to show here, this condition is not satisfied by Rice (2012), so his proposal of an alternative to Potochnik's account cannot be considered good.

The type of argument found in (2) affects only the account that uses ***Σ*** to make a case for the view that ***X*** is explanatory, but it doesn't affect in any way ***Σ*** — i.e., the theory about what something needs to have in order to count as an explanation. So, in order to make (3) acceptable, the argument in (2) needs to be supplemented with an independent justification for considering that optimality models are explanatory. To do this, one needs to provide an independent reason for thinking that there is more to explanation than what ***Σ*** tells us. There are at least two ways to do that:

(i) appeal to a different theory of explanation that can (presumably) be taken to accommodate better than ***Σ*** cases such as the one under discussion, or

(ii) employ a reliable pre-theoretic explanation identification tool.

Without something along the lines of (i) or (ii), it makes no sense to even consider the possibility that ***X*** might be explanatory in some other way. If we have no reason to suspect that ***Σ*** does not account for all the scientific explanatory practices, all that we can get out of (2) is that ***X*** is not explanatory.[1] Nothing resembling (i) or (ii) can be found in (Rice 2012).

Before continuing my discussion, I will address several potential objections.

OBJECTION 1: My interpretation is not charitable because Rice does have independent justification for treating optimality models as explanatory even though this is not explicit in the paper under discussion. If this is the case, going from (2) to (3) in the above argument is not at all problematic. When criticizing someone else's work it is hard to avoid being accused of misinterpretation or lack of charity, but I believe charity has its limits. The present discussion is a good illustration of this point. One of the main things I'm trying to do in this paper is show that such a justification is crucial for understanding and evaluating a new account — if there is no justification or if the justification is inappropriate, then the proposed account can run into all sorts of problems (I will say more about this below). If I am right, letting the reader fill in the blanks is unacceptable in this kind of context. So, my answer to this objection is that it is not uncharitable to assume that Rice does not have independent justification for treating optimality models as explanatory and it would clearly be a stretch of charity to try to guess what that justification may be.

OBJECTION 2: I have failed to see that the papers discussed here are part of a debate in which there is agreement that optimality models are explanatory because the scientists take them as such. So although Rice (2012) does not explicitly provide an independent reason for thinking that there is more to explanation than what a certain theory tells us, there is something uncontroversial that he uses to distinguish the explanatory from the rest of scientific practices: what the scientists are saying. This, of course, makes the transition from (2) to (3) unproblematic. There is a problem, though. I will say more about this later, but, to anticipate a little, saying that scientists have a specific attitude towards something implies that there is agreement in the scientific community regarding that thing. Given that the scientific community does

[1] To be clear, this is not my position, but what I take to be the correct conclusion of Rice's paper.

not always stand in agreement, relying on what scientists are saying should always be accompanied by some evidence that all scientists are in accord. Otherwise one opens oneself to the charge of bias. Furthermore, showing that *most* scientists hold a specific opinion does not make things better: there have been many cases in science when the majority turned out to have wrong opinions about a particular topic (see, for example, the case of Wegener's continental drift hypothesis).

OBJECTION 3: It is mistaken and unfair to present Rice's account as a mere corollary to his critique of Potochnik. I agree. This is far from what I am doing, though. I argue that Rice (2012) has failed to satisfy condition (c) — i.e., his proposal of an alternative account of the explanatoriness of biological optimality models relies in an important respect on elements from Potochnik's account. So, my problem is not with his account but with his justification for looking for a new account. I think I understand why someone can misinterpret my argument and be inclined to formulate the objection addressed here. Consider the following situation: in the scientific literature about *x*, there is a hypothesis, H, linking *x* with *y*. A scientist, dissatisfied with H but convinced that there is a correlation between *x* and *y*, publishes a paper in which she presents some reasons for why H is mistaken and proposes an alternative hypothesis, H'. It would be strange to attack her account on the grounds that part of her paper is concerned with rejecting H. Is this in any way similar to what can be found in Rice's paper? No, but if it were, this objection would have a different force. Actually, this is a very simplistic and therefore misleading way of depicting this kind of situations. A better representation takes into account the fact that every hypothesis, in order to be plausible and therefore acceptable, needs good supporting reasons. If we fill this gap in the scenario considered above and add that our scientist doesn't come up with an independent justification for thinking that there is a relationship between *x* and *y*, we are getting close to what can be found in Rice's paper. Rice does not tell us why optimality models should be taken as explanatory, so he either unjustifiably assumes that they are explanatory[2] or he imports the reason for considering them to be so from the account he criticizes.

[2] In my presentation of the structure of Rice's approach I didn't discuss this possibility because I believe it goes over what can be reasonably inferred from Rice's paper.

3. RICE'S ALTERNATIVE (IMPROVED) ACCOUNT

Rice (2012) aims, among other things, to show that optimality models do not provide censored causal explanations and to point in the direction of an alternative approach to understanding how these models explain biological phenomena. Our discussion in the preceding section was meant to reveal a crucial flaw in Rice's approach: tells us (i.e., without supplementing his argument with something along the lines of (i) or (ii) above), this approach cannot be taken to establish more than the fact that optimality models don't provide explanations — i.e., it cannot be taken to offer proper support for the alternative account of how optimality models explain biological phenomena. This situation seems to be remedied in (Rice 2015), where we find what can be taken as Rice's solution to the problem of identifying explanations as such: explanations are somehow linked with *expectability*, so, if a scientific model tells us why the phenomenon of interest is to be expected, we are justified in considering it explanatory. This is what Rice writes in this connection:

> Now that we understand what an optimality model is, we can turn to the question of how an optimality model might be used to provide an explanation. The main thing to note is that merely showing that a strategy is the best available is insufficient to provide a satisfactory explanation. Therefore, if an optimality model is going to provide a satisfactory explanation of biological explananda — e.g., the current trait distribution of a biological population — the model must also include assumptions about why the optimal strategy *is to be expected*. I refer to these additional assumptions as *optimization assumptions*. . . . Several accounts connect explanation with some notion of *expectability* (Hempel 1965, Salmon 1984, Strevens 2009, Batterman 2002). That is, an explanans explains the explanandum in part because it allows us to see why the explanandum was expected to occur. This minimal — though by no means sufficient — requirement for being an explanation provides some insight into why optimality models are able to provide satisfactory explanations. An optimality model uses mathematical representations to demonstrate that a particular strategy (or set of strategies) is the best available given certain constraints and tradeoffs — i.e., the mathematical model demonstrates why a particular strategy is *locally optimal*. The optimization assumptions then show *why the target system is expected to arrive at (and perhaps maintain) the optimal strategy* (Rice 2015: 593-594, his emphasis).

What Rice suggests in these passages is that explanatoriness seems to have something to do with expectability since many views of explanation — such as Hempel's, Salmon's, Streven's, and Batterman's — despite their differences, make a connection between these two concepts. If this is the case, then, details aside, we have found a good pre-theoretic way of identifying explanations: something can or should be considered to be explanatory insofar as it tells us why the phenomenon of interest is to be expected. So, we can use this to provide an answer to the problem of (pre-theoretically determining) the explanatory worth of optimality models: these models can or should be considered explanatory if they tell us why the optimal strategy is to be expected. Once we have this, we can safely proceed to the problem of providing an account of how these models explain. In what follows, I will argue that expectability cannot play the role attributed to it by Rice, but, before getting to it, it is important to clarify a few things. I will try to do this by addressing a potential misunderstanding lurking in these lines.

Rice writes that expectability is only an *insufficient* requirement for being an explanation, but someone can interpret the argument I will give below as concerned with expectability understood as a *sufficient* condition for explanations and, therefore, conclude that what I am saying does not actually apply to Rice's view. In order to have a better grasp of what we are dealing with here, it is crucial to acknowledge the distinction between *the theoretically determined conditions for something to be an x* and *the pre-theoretical judgements about what makes something an x*. Our concern here is with the latter. The claim made above about the explanatory worth of optimality models is not meant as a presentation of (one of) the theoretically determined conditions for optimality models to be considered explanatory.

An important feature of pre-theoretical judgements is that they cannot be the whole story about *x* (they come before an adequate investigation of *x*) and so are intrinsically insufficient. Despite this, they can provide us with insights into *x*'s essential feature(s) — these insights can be used as a starting point for an adequate account of *x*. Something can be insufficient in this sense and be a good identification tool. I believe that something along these lines is what Rice has in mind when he discusses the link between explanation and expectability: a pre-theoretical "insight into why optimality models are able to provide satisfactory explanations" (Rice 2015: 593). The crucial problem then is to see if expectability can indeed be taken as a good pre-theoretic explanation-identification tool. My aim is to show that this is not the case and that, even if it were, it would not be very useful in the context of our discussion because what Rice calls the optimization assumptions do not actually tell us why the optimal strategy is to be expected. Let's start with this last point.

What are the optimization assumptions and what role do they play in optimality models? To answer this, we first need to look at the structure of model building in this case. The first step in building an optimality model is to ask an explicit biological question that is assumed from the start to have an adaptive answer[3] (e.g., why dung flies copulate on average for 36 minutes?).

[3] Cf. Geoff A. Parker and John Maynard Smith (1990: 27-29).

The second step is to determine the strategy set — i.e., the alternative answers that we consider plausible in light of what we take as possible for evolution to achieve. In the dung flies case, Parker (1978) observed that the total behavior cycle time is 156 + *c* minutes, where 156 represents the time spent by the male searching the female and guarding it after copulation to prevent it to mate again, and *c* is the actual copulation time, and so represents the model's strategy set. The third step consists in choosing the optimization criterion (in this case, it is obvious that this has to be the overall rate of fertilization produced by an individual male fly). The next step is to find out what are the pay-offs of the different strategies by analyzing the trade-offs between the costs and benefits of adopting a certain strategy — this is usually done by constructing a mathematical model. The final step is to deduce (using the appropriate mathematical techniques) the optimal strategy (Parker determined that this occurs in the dung flies case when *c* is 41 minutes).

Where do the optimization assumptions fit in this picture? According to Rice (2015: 595), these assumptions appear at the third step, when we are concerned with the accuracy of the model's optimization criterion, and at the beginning, when we make sure that natural selection is the only factor affecting the strategy set by a series of idealizations meant to eliminate the influence of other possible evolutionary factors. Now, if Rice is right, we should be able to find out from these assumptions why the optimal strategy was to be expected. But this is not the case. Actually, it is the other way around: we find out something about our assumptions from the optimal strategy, or more precisely from the fact that — if this is indeed the case — the system under study exhibits such a strategy. Taking into consideration these assumptions, an optimality model shows what the optimal strategy is in a certain case, given a certain specific set of ecological and developmental constraints, if the selected optimization criterion is accurate and if natural selection is the only selective force at work. If the predictions of the model fit the observations, "then the model may really reflect the forces that have moulded the adaptation" (Parker, Maynard Smith 1990: 29). So, instead of finding out with the help of these assumptions why the optimal strategy was to be expected, the fact that it is confirmed that a certain system exhibits what an optimality model determines to be the optimal strategy assures us that our assumptions were correct.

If I am right, then using expectability the way Rice (2015) does in the context of determining the explanatory import of optimality models is a bad strategy. But this is not all. I believe that using expectability as an identification tool for explanations is a bad strategy in general, because, with no other qualifications, expectability is too broad a concept to play such a part (for example, one's expectations may be based on astrology, superstitions, religion, etc.). Aside from this, the problem with trying to make such use of expectability is that it will make all predictive models come out as explanatory because they can be used to say why something was to be expected: a certain phenomenon, strategy, etc. was to be expected because a model predicted its occurrence and such a model was reliably used in the past.

Before moving on to the next section, let me stress once again that our concern here was with expectability understood as a potential pre-theoretical explanation-identification tool. This should not be confused with Rice's suggestions about what a full-fledged account of non-causal explanations should look like. In the last sections of his *Noûs* paper, Rice writes that optimality models explain because they provide counterfactual information about a system's behavior, but the counterfactual relations between the explanans and the explanandum should not be understood along the lines of Woodward's account because that would not allow us to move beyond causal explanations. According to Rice, what would put us on the right path to an account of explanation that goes beyond the causal approach is a combination of some of the features of Batterman's account of asymptotic explanations with features of Woodward's account of explanation. This is, no doubt, an interesting proposal, but it is based on considerations made after determining that optimality models can have explanatory virtues — i.e., after solving the problem we are preoccupied with in this paper.

4. IRVINE'S ACCOUNT

So far, I have tried to show what is wrong with a particular type of attempt to account for the explanatory value of optimality models. Although I have discussed two of Rice's papers on this topic, the problem is not restricted to Rice's approach. Actually this approach (and the problems associated with it) is quite pervasive in the recent literature on the explanatory value of scientific models. Take, for example, Irvine's (2015) proposal of an alternative to the causal-mechanical accounts of model explanation. Like Rice, Irvine (2015) starts with the unjustified assumption that there is more to the way scientific models can provide explanations than we are led to think by the causal-mechanical accounts. She claims that "models are used in far more explanatory contexts than this, and in fact are particularly useful when very little is known about underlying concrete mechanisms" (Irvine 2015: 3947). It is, of course, obvious that models are used in contexts where not much is known about the underlying mechanisms of a system. What it is far from obvious is that models can be explanatory even in such contexts. What reasons do we have for thinking that this is the case? Like Rice (2015), Irvine comes up with a very controversial solution to this problem. What she tells us is that:

> Models in many areas of cognitive science and biology are constructed by translating a computational template into a model of a target phenomenon. Choice of appropriate targets, templates, and translations is often joined together, and relies on a wealth of different kinds of background knowledge about the target system and about the template. Models are tested and updated. *This kind of modelling practice does not therefore generate merely predictively accurate (phenomenological) models, and there is reason to treat models that meet the kind of normative constraints described above as capable of providing explanations*. (Irvine 2015: 3951-3952, emphasis added)

But this is nothing more than taking as criteria of explanatoriness the adequacy conditions that have to be satisfied by any model. So, if we accept this, any adequate model is automatically explanatory. I believe it is obvious what is wrong with this: it leads to an unacceptable trivialization.

There is an alternative but, as I will try to show next, equally problematic way of interpreting what Irvine is doing. Namely, one can take her strategy to be similar to what Christopher Pincock (2015) calls *the case-driven approach to thinking about explanations*. The main idea behind this strategy is that we should rely on "expert practitioners . . . [to] guide our judgements and influence our philosophical theory of explanation" (Pincock 2015: 870). Pincock writes:

> 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: 858)

So, a case-driven strategy consists of four steps. The first and most important one is to start with a case that is identified (based on what the expert practitioners are saying) as an explanation that seems different from other scientific explanations. The next step is to analyze this case in order to determine its characteristic features. The third step is to argue that some influential account of scientific explanation does not accommodate explanations with such features as those revealed at the previous step.[4] The final step is to tackle the problem of accounting for the kind of explanation exemplified by the case discussed.

[4] This step is not explicit in Pincock's presentation of this strategy, but it is implicit in his analysis of the case of Plateau's laws for soap-film surfaces.

Does Irvine employ such a strategy in her paper? As we have seen above, the first step is crucial for a case-driven strategy, but, if what I said about Irvine's starting point is correct (namely, that she offers no justification for her claim that models are used in explanatory contexts that cannot be covered by a mechanistic model of explanation), then her approach is different from a case-driven strategy for accounting for the explanatory role of optimality models. Nevertheless, she does seem to come close enough to such an approach. With some small adjustments, Irvine's approach can look like this: if we take a closer look at the way models are actually used in science, we inevitably come to the conclusion that there is more to explanation than what the popular causal-mechanical accounts would have us believe. This is clearly the case in biology, where optimality models are *used (by expert practitioners) 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" (Irvine 2015: 3950) (step 2). So these models "do not in any sense represent concrete mechanisms, and neither do they describe networks of causal connectivity" (2015: 3952). If this is the case, then they cannot provide a 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).

Rice (2015) can also be interpreted as adopting, at least partly, a case-driven approach because he claims to start with something resembling its first step. He writes: "my approach will be to first analyze 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" (Rice 2015: 591).

A crucial question that one may ask at this point concerns the extent to which a case-driven approach is better in this context. Before tackling this question, it is a good idea to reiterate some of the main points made above. The most important point I have tried to make in this paper concerns the characteristic features of (what I believe to be) an approach to arguing for a new account of the explanatory role of optimality models proposed by Rice (2012, 2015) and Irvine (2015). In my opinion, this approach consists of the following steps. First, we start with a case that, according to some account found in the philosophical literature, is an explanation. Secondly, we analyze this case in order to determine its characteristic features. We then argue, based on what is said at the previous step, that the account we started with (usually one based on a causal theory of explanation) is flawed. Finally, we propose an alternative account centered on what has been determined to characterize that case.

As I argued above, the most important problem with such a strategy is that it is self-defeating because it relies on the account it ends up rejecting for the reason for thinking that in the case under discussion we are dealing with an explanation. The solution to this problem is obvious: we need to provide an independent reason for considering such a case to have any explanatory worth. The best way to do this, from the perspective of such an approach, is by finding a pre-theoretic explanation-identification tool. This solution comes with its own problems, though. For example, a pre-theoretic explanation-identification tool is not an easy thing to find. We have seen above why Rice's (2015) appeal to expectability doesn't work in this connection and why Irvine's (2015) use of the adequacy conditions that have to be satisfied by a model is equally bad. This is where the case-driven approach can come to the rescue, because it does employ what may be seen[5] as a good pre-theoretic explanation-identification tool: the expert practitioners. So, long story short, we have good reasons to consider the approach that I attribute to Rice and Irvine to be capable of improvement if it can be transformed into something along the lines of a case-driven approach.[6]

Let us return now to our question: how good is a case-driven approach in the context of the discussion about the explanatory worth of optimality models? Not very good, I'm afraid. The main idea behind this approach is that expert practitioners can be used as a sort of pre-theoretic explanation-identification tool. But what happens when the expert practitioners disagree about a particular case? I trust everybody would agree that, in such a situation, appealing to expert practitioners is out of the question. Therefore, at least in this kind of context, a case-driven approach is inappropriate. But this is exactly what we are dealing with in the case of optimality models. "Expert practitioners," such as Gould and Lewontin (1979), Pierce and Ollason (1987), Rose, Service, and Hutchinson (1987), and others, provide compelling reasons against the explanatory import of optimality models.

So, although the case-driven approach is an improvement over the approach taken — in my opinion — by Rice (2012, 2015) and Irvine (2015), it is

[5] I have argued elsewhere that, despite its initial appeal, this strategy is actually very problematic. For more on this strategy (and its shortcomings), see Târziu 2018.

[6] Since the two approaches are very similar (if we ignore the first step) and since some of the claims made by Rice and Irvine seem to show that they are at least sympathetic to a case-driven approach, one can easily misinterpret what their views as instantiations of a case-driven approach.

not very helpful when it comes to accounting for the explanatory import of optimality models, because expert practitioners cannot be used as reliable pre-theoretical explanation-identification tools in this context.

CONCLUSION

I want to end this paper with some thoughts about the new pluralist stance toward scientific explanation. The recent literature on scientific explanation is different from the traditional discussion in (at least) two important respects: the number of theories (theory-wise, the literature is much richer today than it was fifty years ago), and the search for a single unifying account of explanation (most philosophers seem more preoccupied nowadays with providing new accounts for different cases from science than with searching for a unifying account). It is not hard to realize that this environment favors the recently trending pluralist view on scientific explanation (i.e., the view according to which scientific explanatory practice is actually much more diverse than it was thought before, and that there is no single theory that can adequately capture this diversity). But, I believe it is safe to say that the main impetus behind recent pluralism has to do with the proliferation of examples of (alleged) non-causal explanations. Many philosophers nowadays believe that "there are plenty of compelling, real-life examples of *non-causal* explanations that causal accounts of explanation seemingly fail to capture. To be more precise, the new development in the philosophy of *scientific* explanation is the increasing recognition of interesting and varied examples of non-causal explanations of *empirical phenomena* to be found across the natural and social sciences" (Reutlinger, Saatsi 2018: 3). This proliferation supports the pluralistic stance, which in turn encourages the proliferation.

My aim in this paper was to try to raise a red flag about this proliferation by arguing that the way the accounts of non-causal explanation are constructed and justified in three recent papers on optimality models is deeply flawed. In section 2, I argued that the account proposed in Rice (2012) is faulty because it is not accompanied by an independent justification for treating optimality models as explanatory. Without such a thing, Rice's analysis can at best be taken to show that these models are not explanatory. One way to provide such a justification is to appeal to some sort of pre-theoretic explanation-identification tool. In Rice (2015), *expectability* is taken to play the role of such a tool: if a scientific model tells us why some phenomenon of interest is to be expected, we are justified in considering the model to be explanatory. By appealing to such a tool, the account proposed by Rice (2015) is definitely an improvement over the one in (Rice 2012). Unfortunately, as I argue in section 3, expectability is not a good explanation-identification tool and, moreover, even if it were, it would be a bad idea to appeal to it in the context of determining the explanatory import of optimality models. In section 4, I argued that similar problems affect Irvine's (2015) account of model explanation. An interesting attempt to overcome the problems faced by these accounts is to appeal to what Pincock calls the case-driven approach to thinking about scientific explanation. This approach uses what may be considered to be a very good pre-theoretic explanation-identification tool: namely, expert practitioners. The problem is that, relying on what expert practitioners are saying should always be accompanied by some evidence that they are in accord. Otherwise, one opens oneself to the charge of bias when it comes to picking a side on which to base one's account. But, in the case of optimality models, there is evidence of disagreement.

This discussion is intended to emphasize some of the problems associated with trying to provide a new account for what makes something explanatory. If what I say is correct, then we need to be extremely careful about how we arrive at and how we justify new accounts of explanation. This is meant as a warning for all those inspired by the new pluralist trend to look for new accounts. I take this trend to be motivated and stimulated by the belief that "the notion of explanation is one that becomes more complex the more one looks at the details of actual scientific explanatory practice" (Sklar 1993: 269). I agree that, if it is done right, a detailed analysis of actual scientific practice can lead to a better understanding of scientific explanation. But if, in the process of providing new accounts of explanation, we do not take care to avoid the kind of problems discussed in this paper, we may end up taking anything as an explanation and, instead of arriving at a better understanding, trivializing the concept of explanation.

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