Philosophical Problems in Science

Zagadnienia Filozoficzne w Nauce

ISSN 0867-8286 (print format)

e-ISSN 2451-0602 (electronic format)

Editorial Office
Philosophical Problems in Science (ZFN)
Copernicus Center Foundation
Pl. Szczepański 8, 31-011 Kraków
POLAND

e-mail: info@zfn.edu.pl
www.zfn.edu.pl

Publisher: Copernicus Center Press Sp. z o.o.
Cholerzyn 501, 32-060 Liszki POLAND
tel. (+48) 12 448 14 12
e-mail: zamowienia@ccpress.pl
www.ccpress.pl

Philosophical Problems in Science

Zagadnienia Filozoficzne w Nauce

LXXIV ■ 2023

Articles
Artykuły

Book reviews
Recenzje

Articles

Artykuły

Can fiction and veritism go hand in hand?

Antoine Brandelet
Université de Mons, Université de Namur, Belgium

Abstract

The epistemology of models has to face a conundrum: models are often described as highly idealised, and yet they are considered to be vehicles for scientific explanations. Truth-oriented—veritist—conceptions of explanation seem thereby undermined by this contradiction. In this article, I will show how this apparent paradox can be avoided by appealing to the notion of fiction. If fictionalism is often thought to lead to various flavours of instrumentalism, thereby weakening the veritist hopes, the fiction view of models offers a framework much richer than it seems at first sight. To do so, I will call upon the concepts of modality, counterfactual structure and credible worlds. In the end, veritism of explanation and fiction can indeed go hand in hand, but the scope of explanations we can hope to draw from models must be more precisely delineated.

Keywords

fiction, explanations, idealisations, models, veritism.

1. Setting the Problem

Models are ubiquitous in science. It is widely acknowledged that they are of central epistemological importance, and un-

Philosophical Problems in Science (Zagadnienia Filozoficzne w Nauce)
No 74 (2023), pp. 7–39 • CC-BY-NC-ND 4.0

derstanding their nature and function has become one of the most discussed topics in philosophy of science. There is no agreement on the general framework in which we need to address the modelling problem, and finding one is probably not even desirable. The reason for the variety of available approaches is obvious: diversity of uses and diversity of objects. Models are supposed to serve many functions: represent physical systems, provide explanations or help in theory construction, to cite only a few. Also, many kinds of objects, abstract and concrete, are considered as models: sets of mathematical equations, algorithms, graphs, drawings, or scale-models, for example. If we want to understand the explanatory power of models, which is our main topic here, we must tackle this problem of diversity.

With the acknowledgement of the wide use of models also comes a much more difficult conundrum. One of the main features of modelling practices is the idealisation[1]: models are always simpler than the systems or situations they depict or explain. "Models are generally caricatures of the natural world" (Chakravartty, 2001). The examples abound: the billiard-ball model of gases considers molecule collisions as perfectly elastic, predictions of planetary motion are obtained via the hypothesis that planets are perfectly spherical and with constant mass density, the Lotka-Volterra predator-prey model describes two populations with no outside influences, models in economics are concerned with perfectly rational agents making their decisions from all the available information.

This seems at first problematic, not to say paradoxical: models are vehicles for explanations, and yet they contain idealisations, distortions, purely fictional objects or even impossibilities. Following

[1] Here, I call "idealisation" the general process of simplification, the exact distinction between abstraction, idealisation and/or approximation is not relevant for my point.

Elgin (2017), facing the use of these "felicitous falsehoods", one could argue for the relaxation of truth as the main epistemic goal of science, hence her critique of *veritism*.

In the context of scientific explanation, veritism is the view that considers truth as a necessary condition for explanation (Pincock, 2021). Broadly conceived, I think a position needs to meet two conditions to be considered as veritist: firstly, there must be a form of correspondence or similarity between the model and the physical system it represents and, secondly, the framework must provide a way to distinguish between "good" and "bad" explanations. I will elaborate on that in section 5.

We are faced with a dilemma: either the idealised models refute the veritist position, or the idealised models are not epistemically legitimate and the explanations provided by such models must be rejected. The second horn is clearly not in line with scientific practice: highly idealised models are often used to explain the phenomena observed, and the model-based explanations are considered a fully legitimate part of scientific knowledge. Veritism then seems refuted.

A similar debate is taking place in the context of representation. Besides explanation, models are also used to represent target physical systems. Explanation and representation are two distinct but closely related problems. If explanation faces the conundrum of veritism, representation is concerned with the more general problem associated to the realism/antirealism debate. Representations cannot be strictly qualified as true or false, but there seems to be something like a "resemblance" or "correspondence" between the model and the target that is at work.

In both cases, the worries are concerned with conceiving a model-world relation as a basis of knowledge, even if the "veritist need not endorse any specific account of how models represent" (Pincock,

2021). I think this is true, but here, I want to show how one could use resources provided by responses to the representation problem to clarify the veritism conundrum.

Recently, Roman Frigg (see for example (Frigg, 2009), or, with James Nguyen (Frigg and Nguyen, 2016; 2020)) has developed the so-called "fiction-view of models", in which models are broadly conceived as *Epistemic Representations*. This solves the aforementioned problem of diversity, as representative models are any object that is used as a vehicle to support surrogative reasoning. According to Frigg, models are fictions in the sense that they are an invitation to engage in a prop-oriented game of make-believe, understood in its Waltonian sense (Walton, 1990). Leaving technical details aside (see e.g. (Toon, 2012) for a discussion of Walton's view), the main idea is that when using a model, we accept assumptions knowing that they are false but which acceptance function as principles of generation. We enter the fictional world they describe by pretending they are true. In other words, every proposition of a model could be preceded by an "as-if" clause referring to the rules of the game of make-believe.

The fiction view is intended to solve the problem of representation (i.e. how can models provide inferences from the model to the represented target system?). Here, I will focus on the explanatory power of models, but I think these fictional resources can be of great help to the explanation problem. This is what I will defend in section 2.

Expressed in this way, the fiction view seems to support purely instrumentalist and non-veritist conceptions of explanation. Models can incorporate any kind of false assumptions as soon as they empower explanatory or representational power, undermining all the truth-oriented hopes of the veritist or the realist. I think that even if make-believe puts fiction at the center, there is still a way to defend a slightly modified version of veritism. In subsequent sections, I

will examine in more detail the consequences of this fiction-oriented view to find a way for veritism in the make-believe framework. More specifically, engaging in a game of make-believe is better understood as the process of building a counterfactual story, which highlights the importance of counterfactual reasonings in model-based explanation (Section 3). Models depict possible worlds from which we hope to draw inferences, and justifying these inferences is at the core of our present problem. I claim it is the counterfactual structure exemplified by the model that supports these inferential steps, and as models contain something inherently modal, the kind of explanations models provide also have something to do with modality. This is what I will explore in Section 4.

Section 5 sets the counterfactual structure as the focal point of the model-world relation the veritist is looking for. I show how the fiction view can help demarcate between valid and invalid explanations once the modal aspect of the model is appropriately understood. I then examine how it influences the kind of explanations one could expect to derive from models.

In Section 6, I discuss how the fictional approach can help to clarify the opposition between the ontic and epistemic conceptions of explanation. The focus on idealisations and representations in model building if often taken as an argument in favour of epistemic approaches, that is conceptions that consider explanations to be products of some scientific activity involving various techniques, such as the fictional processes described in this paper. I argue on the contrary that critiques of the ontic conception focusing on idealisations and representations often miss the point: the ontic conception suitably understood does not deny the importance of fictional processes, but puts the focus on the referents of explanatory texts and representations. The

fiction view is able to provide a framework in which questions about explanation and questions about the ontic or epistemic expectations of explanations are clearly distinguished.

2. Fiction and Models

In this section, I will (very) briefly summarise the main aspects of the fiction view of models.

Epistemology was of course not the first to use the concept of fiction. This term refers primarily to "works of fiction", understood in the purely artistic sense of the term, and it has been the subject of much discussion in aesthetics, for example. Recent interest for fiction in epistemology and philosophy of science often refers to Walton's seminal work, *Mimesis as make-Believe*, which deals mainly with artistic representation, but offers a framework suitable to address epistemological issues.

The idea that science deals with theories or models that simplify reality is not new either, and one could find the premises of such an analysis for instance in Vaihinger's extensive use of the "as-if" statement (Vaihinger, 1911). That is an important shift in understanding the explanatory function of models. Models don't just simplify matters by isolating variables, idealising processes or abstracting properties. They are an invitation to think "as-if" things were so and so, just as a work of fiction is a prescription to imagine situations, people and places. The simple pendulum is a fiction in the sense that reading its theoretical description, we imagine a point mass oscillating at the end of a non-extendable, massless string, even if we know point masses and massless strings don't exist in reality.

Specifying a model by stipulating principles of generation is of course not the whole story. One could imagine any set of (at least coherent) assumptions and claim that it is a model by a mere *modelling fiat*. But it is not. For a set of hypotheses to constitute a model, it needs to be applicable to the target system: the terms involved in the assumptions need to be interpretable in terms of the target. Most of the time, scientific models come already partially interpreted by a theory. The simple pendulum is not a model of real world pendula because scientists just say so, but because we can assign to each term (e.g. m, l, g) a target-interpretation (respectively mass of the bob, length of the cord, gravitational field magnitude), and the relevance of these variables is inherited from Newtonian mechanics, the theory in which the model takes place.

The model is then studied, investigated and manipulated. New fictional truths (i.e. propositions true in the model) are derived from the principles of generation. In a famous quote, Hacking writes that "a model in physics is something you hold in your head rather than your hands" (Hacking, 1983). I think we can understand this quote literally. I take the derivation of new fictional truths as analogous to the manipulation, for instance, of a scale-model or a map: generally conceived as epistemic representations, models are manipulable because we can discover new propositions from postulated ones. It is clear for the example of the pendulum, but finding your way by making inferences from a map works in the exact same way: interpreting lines and color patches as roads and forests enable the map user to determine his position.

According to the fiction view of scientific representation, that is where the representative power of models comes from. It also explains why scientists often talk about abstract models as-if they were real objects, as we talk about Sherlock Holmes as-if he was a real person.

Starting from Walton's make-believe theory, Frigg and Nguyen (2020) have developed the DEKI account of scientific representation, but for our present purpose, only the basic concepts of the make-believe view are necessary.

As a matter of fact, fiction is often thought to clash with truth. In libraries, there is a strict distinction between fiction and non-fiction sections. In everyday language, fiction is associated with imagination, fantasy or lies. If models incorporate such imaginary or false assumptions, it seems impossible to reconcile this view with veritism.

In her book *True Enough* (2017), Catherine Elgin establishes this incompatibility on the very first page:

> Modern science is one of humanity's greatest cognitive achievements. To think that this achievement is a fluke would be mad. So epistemology has the task of accounting for science's success. A truth-centered, or veritistic, epistemology must treat models, idealizations, and thought experiments as mere heuristics, or forecast their disappearance with the advancement of scientific understanding. Neither approach is plausible. We should not cavalierly assume that the inaccuracy of models and idealizations constitutes an inadequacy; quite the opposite. I suggest that their divergence from truth or representational accuracy fosters their epistemic functioning. When effective, models and idealizations are, I contend, felicitous falsehoods. They are more than heuristics. They are ineliminable and epistemically valuable components of the understanding science supplies. (Elgin, 2017, p.1)

As the make-believe view bets on the central importance of fictional aspects of modelling, truth-based explanations cannot be derived from models at all, it seems, and the conclusion is the same as Elgin's: *Felicitous falsehoods* cannot be removed from the success of science, then *veritism* must be abandoned.

I think there is nonetheless a way to defend a modified form of veritism in the context of the fictional view. Such a defense must proceed in two steps. The first concerns epistemic virtues. Elgin acknowledges the virtue of idealisations, but dismisses the one of truth. On the other side, the veritist claims that the main epistemic virtue is truth, but still, that doesn't mean idealisations cannot also have some kind of value and that the two cannot be articulated in a common framework. The second step, which will be our main concern in the remainder of this article, is to establish what limitations, if any, the fiction view imposes on the scope of model-based explanations.

There is a large literature about epistemic virtues, and various authors have defended that veritism and idealisation may not be as incompatible as we may think. For instance, Nawar argues that "in grasping and idealizing claim *as an idealizing claim*, if seems that one in facts grasps a truth" (Nawar, 2019)(his emphasis). Sullivan and Khalifa (2019) admit that idealisation has virtues, but that they are non-epistemic. Idealisations are used for convenience, simplicity or tractability, in this sense they are felicitous falsehoods, but only their true components can provide understanding. In the same vein, Lawler argues that "falsehoods can play an epistemic enabling role in the process of obtaining understanding but are not elements of the explanations or analyses that constitute the content of understanding" (Lawler, 2019). Making the process/content of explanation distinction, called the *extraction view*, is interesting for our purpose. Concerning the former, the fiction view enables all the usual virtues granted to idealisations by taking these fictionalising procedures as the central feature of models. Concerning the latter, to be fully adequate in the fiction context, we must clarify in terms of make-believe what exactly the content of the explanations provided by the models is.

Admitting there is a place for the virtue of idealisations even if truth still constitutes the main goal of inquiry in providing explanations, we must now turn to the question of explanations themselves. How can fictional models generate virtuous explanations? How can the false explain the real? This is what will see in subsequent sections.

3. Counterfactuals at work

In this section, I will examine the lessons we can draw from the fiction view in the way models generate explanations. I will also see how my account may provide insights to understand the relation between models and laws via the use of counterfactual inferences.

Remember the fiction view proceeds in two stages: firstly, rules of the game that generate the fictional world are postulated; then, secondly, the model is explored and fictional truths are derived from these principles of generation.

Modulo the applicability of the model, any kind of assumption is *a priori* acceptable, whether it be idealisation, abstraction or distorsion. These are not the whole story. Some of the fictional processes the scientist may use to build the model are not reducible to these simplifying assumptions. As a matter of fact, models sometimes feature impossibilities, assumptions that are incompatible with the theory in which the model takes place.[2] Take the case of the simple pendulum as an example: point masses not only do not exist in reality, but are also impossible according to the Newtonian picture of the

[2] I here use the term theory in a very loose sense: a system of concepts and general principles. The question of the theory-model relationship is a debate in its own right and is beyond the scope of this article, but I will briefly sketch a possible answer that naturally arises in the fiction framework in the remainder of the article.

world. I take this observation to be a good reason to turn ourselves to a modal conception of models, where notions of possibility are directly implemented in the framework.

Models, like fiction, seem to describe possible worlds, that is worlds that resemble ours in many aspects, but where Sherlock Holmes is a detective, rabbits talk or point masses oscillate without friction. If we acknowledge models may contain impossibilities, the fiction view itself faces a conundrum, as coherence and consistency are often presented as necessary conditions for a work of fiction to be acceptable.[3] In the context of epistemology, this seems to suggest that some models contain clashing propositions, thus creating a contradictory story and undermining the potential veridic ground of explanations. What is an impossible world, according to the fiction view? The distinction is important, as any account oriented towards truth must be able to offer acceptability criteria. At this point, we also face the modal version of the initial conundrum: how can models be impossible descriptions of real (therefore possible) phenomena?

To resolve the apparent paradox, we must clarify the use of (im)possibilities in models to understand how they fit together. I think the resources of the fiction view are of great interest, here, as it draws our attention to the important distinction between the inside of the model (fictional—intradiegetic[4]—propositions) and the outside.

When qualifying assumptions as impossible, it is always with an implicit frame of reference in mind. Something is possible or impossible only according to a set of hypotheses or axioms. In this regard, all the fictional propositions are, by definition, diegetically possible. When we say that a model features impossibilities, it is

[3] This is also related to debates around the *willing suspension of disbelief*. Interestingly, Kendall Walton is one of the critics of this approach (see e.g., Walton, 1978, p. 7).

[4] Literally: inside (intra) the narrative (diegetic).

with regard to the exterior, to the laws, theories or principles we believe to be true in reality. Models are often thought to be interpreted structures that link the theories to the empirical world, the fiction view generalises this idea to any kind of inference vehicles (theoretical or material) and principles of generation (theories, laws or imaginary entities).

Newtonian mechanics is testable only if we build a model that generates, when applicable to a target system, empirical propositions. The theory itself acquires representational or explanatory power via the models that depict concrete situations. Remember the Ian Hacking quote. What does it mean to manipulate a theoretical model? Taking the simple pendulum example, that means plugging values for the free parameters end seeing what comes out. The model does not contain number values, but a network of relations between variables with potential inputs. Manipulating a model is then playing with counterfactual propositions: when applicable to the target, they generate propositions of the form "had the parameters had such and such values, the system would behave as such and such". That is how models are empirically testable. As the "as-if" clause is characteristic of the fictional process, these models generate "what-if" propositions about their targets. Models are sets of propositions arranged in a counterfactual structure and potentially applicable to a target physical system and some, but not all, their principles of generation are derived from a more general theory: the simple pendulum is a Newtonian model because it embodies what are considered to be the Newtonian laws.

So far so good, but does it still make sense to talk about being the model of a theory in this context? As we have seen, models often contain impossibilities as premisses, which makes them incompatible

with the theory. If models are believed to serve as intermediaries, they must respect the properties they pretend to exemplify. Models may be caricatures of the world, but not of the theories they are models of.

The analogy with fiction sheds light on the complex relation of models not only with physical systems, but also with theoretical principles. A model is neither a strict exemplification of theoretical principles, nor it is a faithful representation of target systems. Models are strange, sometimes abstract, objects, made of heterogeneous parts like an epistemic Frankenstein's creature.

Yet, the relation of fiction to truth is also a more complicated story, and we sometimes use them to learn about the real world. It is widely acknowledged that fiction is not reducible to falsity. One of the most used examples is Tolstoy's *War and Peace*, which contains lots of accurate details about the Napoleonian wars. This example shows there is a use of outside truths for diegetic purposes, and that a reader could learn about the real world by reading the novel. However, this could hardly be described as genuine knowledge as, in the absence of prior background knowledge, the reader may be as justified to believe in the love story of Pierre and Natasha. In the case of scientific models, we have seen that, because of postulated impossibilities, the same problem arises.

There is, I think, another example more suitable to our epistemic concerns that will illustrate the way the fiction view may solve the problem. Let's consider the fables of La Fontaine. At first glance, they describe anthropomorphic animals in imaginary situations. The characters themselves are less human-like in their attitudes than they are archetypes of certain behaviours. Yet, the fictional world depicted by the fable has the function of providing information on actual human behaviour. This is the role of the final moral. Here, I think, the analogy makes sense: idealised entities are postulated, some of their properties

are intended to be interpreted literally while others are not, and the final epistemic goal is to state something true of the exterior of the fiction.

The analogy also seems to suggest an important role assigned to the theories. What makes the moral of a fable a good indication of human behaviour? I claim it is a kind of legal compliance with respect to laws that are supposed to govern human behaviour. In this sense, the fable functions as a model: the fictional entities are embedded in a web of relations and these relations are described by certain laws. It is no threat to the compatibility with the background theory because it is made to describe humans and not animals[5]. The model is a model of that theory because the objects it depicts, even if purely fictional, enter the web of relations described by the theory. In this sense, models are intermediary objects between a theory and target systems.

This is, I think, the main contribution of the fiction view: it leads to a sort of structural *divide et impera* strategy. Models are considered as modal structures, mechanisms used to exemplify a web of counterfactual relations. The modal structure is the skeleton of the model, fictional hypotheses are the flesh that makes the model more tractable or easy to interpret. More importantly: the modal structure exemplified is partially independent from the fictional assumptions.

Manipulating the simple pendulum, we find a relation for the period that is independent of time, regardless of knowingly false hypotheses. That is this counterfactual structure that makes the work. When a model is empirically adequate, we have to ask ourselves: what makes the predictive job? When explaining with models, the question is: what is doing the explanatory job? Explaining in terms of the validity of the principles of generation (rules of the game) is a no-go (at least for the veritist): they are knowingly false. Then if something

[5] I leave aside the symbolic aspect of the use of certain animals in that particular case.

is doing the explanation, it is the modal structure itself, inherited from the laws[6] of the theory the model is a model of. Asking for an explanation of the independence on mass of the period of the pendulum, one may present the simple pendulum model, show how the equation $T = 2\pi\sqrt{l/g}$ is derived and explain why it is applicable to the target pendulum. In this context, idealisations, abstractions and all the fictional processes are epistemically valuable not because they constitute the explanation, but because they help make the model tractable by expliciting its counterfactual structure. As Bokulich (2016, p.1) puts it: "Fictional models can succeed in offering genuine explanations by correctly capturing relevant patterns of counterfactual dependence and licensing correct inferences."

Exploring the real via falsehoods still seems a dangerous strategy if we are not able to distinguish between valid and invalid explanations, as there is still some kind of *pessimistic meta-induction* (PMI) threat here. Bokulich takes the example of a non-explanatory fiction: the epicycles model of the Solar system. From a purely fictional-counterfactual point of view, this and the Newtonian-heliocentric models are on a par, but the former fails in capturing the right counterfactual pattern. This is of course circular reasoning if we can't provide a justification for what "right" here means. This is what Bokulich calls the "justificatory step", but she claims that a general account is impossible, thereby undermining the veritist hopes:

> However, what does it mean to say that a fictional representation is adequate? It has to be more than mere empirical adequacy. Unfortunately, here is where I think abstract philosophical generalizations purporting to hold across all model

[6] My account does not rely on any particular conception, or metaphysics, of laws, I use the term in a minimal sense: I take counterfactuals as the focal point of explanation and laws are known for supporting counterfactuals. Models exemplifying counterfactual dependencies are in this sense inherited from laws.

> explanations give out, and one needs to turn to the nitty-gritty details of the science in question. What is to count as an adequate fictional representation is something that has to be negotiated by the relevant scientific community and will depend on the details of the particular science, the nature of the target system, and the purposes for which the scientists are deploying the model (Bokulich, 2012, p.734).

I think that objection is no fundamental threat to my account for two reasons. Firstly, finding the common ground veridically interpretable for explanations and providing a general acceptability criterion are two different things. In this sense, my account is minimal, as it proposes a necessary condition. I agree that the sufficiency argument may be context dependent. Secondly, the resources provided by the fiction view seem compatible with the general structural arguments put forward by many scientific realists. The response to the PMI-like worries raised by Bokulich may follow the same path.

In the same vein as Bokulich, Potochnik claims that

> depicting causal patterns regularly motivates departures from accuracy of any given phenomenon; this is why idealizations are used to represent as-if. Put in these terms, the present idea is that idealizations positively contribute to generating understanding by revealing causal patterns and thereby enabling insights about these patterns that would otherwise be inaccessible to us (Potochnik, 2017, p.95).

Again, the highlighting role of idealisations is considered as one of the main aspects of models, but the exact role and nature of those *causal patterns* remain unclear. Potochnik writes her view does not rely on any particular metaphysics of causation, but she acknowledges causal patterns must be real: “How, then, can we tell if understanding is actual and not merely apparent? For this, the causal pattern

apparently grasped must be real. Briefly, for a causal pattern to be real, it must be embodied (to some degree or other) in some range of phenomena." (Potochnik, 2017, p.115) But still, she dismisses truth as the main epistemic goal of science:

> The clearest illustrations of this are idealizations themselves, which are quite far from the truth but, in the right circumstances, are epistemically acceptable nonetheless. So, in my view, science simply is not after the truth. There are some important ways in which truth still may be involved in the scientific enterprise, but in each case, it is only a means to other ends (Potochnik, 2017, p.117).

So, when truth is indeed put forward by scientists, it is always with other, more important, goals in mind: understanding of phenomena, which is not truth-oriented. But what would it mean to highlight real causal patterns in a non-truth-centered way? To me, it seems clear that qualifying something as real in a model, even highly idealised, involves some kind of correspondence and, in the end, (at least partial) truth. There is a tension at play, here: accuracy is a requirement of epistemic acceptability, but we must refuse to align it with truth, as idealisation help generate explanations and understanding. The initial puzzle is still unsolved.

Fiction view seems particularly adequate to treat this problem, as it makes clear the distinction between the fictional process by which the world of the model (the description of possibilities potentially not realised) and the counterfactual structure it brings out. The role of the fictional part is to bring counterfactual structure to the front, but the structure itself is immune to fiction, as we have seen. I propose to take this observation as the focal point of our veritist considerations.

One counterargument would be to argue that once fiction enters the picture, it propagates to the entire model. Setting aside approxi-

mations and idealisations, when a purely fictional entity like silogen atoms (Winsberg, 2006) are ineliminable parts of a model, even the counterfactual structure makes a truth-oriented interpretation and explanation impossible. Here again, I think fiction solves the conundrum.

From a make-believe point of view, postulating new knowingly-unreal particles like silogen atoms is generating a model in which these impossible particles exist, have properties and interact. The model itself is predictive and has good empirical adequacy, but to fully understand its use in generating explanation, we also must examine how it is applied to the target system. As fictional assumptions, silogen atoms have no interpretations in terms of the target system, they are not strictly applicable. Engaging in the silogen game of make-believe means we accept the assumptions inside the model-world, but not outside. The model is still capable of being veridically evaluated because what is confirmed is not a matter of entities or even physical processes. Silogen atoms are fictional entities that ground a set of properties, properties that feature in laws (quantum mechanics and solid-state physics, say), laws that are exemplified in the model, thus exhibiting a counterfactual structure. Interpreting silogen atoms as existing would be a misuse of the model, just like asserting that the moral of the fable is only valid for anthropomorphic animals.

The veritist base of the model explanation is then to be found in the way the laws are generating the skeleton of the model, and the model is evaluated by manipulating this structure to make it generate empirically testable propositions. Giving an interpretation of the counterfactual dependencies is the ground of any explanation. We are not forced to identify these patterns as causes and consequences. My proposition remains agnostic and offers room for different interpretations and levels of ontological commitments. This process is fiction-blind but empowered by the fictional freedom.

4. Modality and Explanations

Let us now turn to the kind of explanation such models can generate.

Verreault-Julien(2019) argued that models may provide "how-possibly explanations", which are propositions of the form $\diamond(p$ because $q)$. Facing the issue of how highly idealised models featuring impossibilities may provide such an explanation, Verreault-Julien insists on the importance of making a clear distinction between model-propositions (translated in my account, we could say fictional propositions, i.e. propositions true in the fiction) and world-propositions. "What model propositions (e.g., unrealistic assumptions) do is to give reasons to believe in the truth of the possibility claim", he writes. In his view, models may depict impossibilities and still support possibility claims, which are non-fictional world propositions.

With this I agree, only if we consider not propositions of the form $\diamond(p$ because $q)$ for given p and q, but a counterfactual function that assigns a value of q to a value of p. The model is used not to support a set of definite statements about the target, but a counterfactual structure supposed to be embedded in the phenomena we are interested in explaining. These functions also often have a higher arity, as they link several physical values.

More generally, and to avoid any metaphysical commitment to the term "because", I think suitably applicable models support claims of the form $\diamond(p \sim q)$, where $\sim$ is a relation between quantities exemplified by the model. The simple pendulum depicts an impossible object but nonetheless supports the modal claim that connects a certain number of quantities applicable to real-world pendula when interpreted as mass, length, etc.

Another support for the need for a clear fictional/world propositions distinction comes from the fictional process itself. As Verrault-

Julien makes clear, it is possible for a model to non-trivially support possibility claims only if we already have $\diamond p$ and $\diamond q$. But of course, these fictional assumptions may be impossible, understood as world propositions, hence the need to distinguish "diegetic" from "extradiegetic" claims, as I suggested in the previous section.

Sugden suggests that the posited similarities between the model and reality may license inductive inferences (see e.g. Sugden, 2000; 2013, p.240). He gives an argument that takes the following form: $p \rightarrow q$ in the model and p and q in the world gives good reason to infer that $p \rightarrow q$ in the world. As in any inductive argument, a similarity between specific observations is posited, and in the case of model-based inference, it is the model-world similarity that supports induction. The second step of Sugden's argument is analogous to what I call applicability, and the focus on the counterfactual structure may support the inferential step by providing ground for the justification of the similarity.

Sugden also takes modality to be an important feature of models: "So what might increase our confidence in such inferences? I want to suggest that we can have more confidence in them, the greater the extent to which we can understand the relevant model as a description of how the world could be." (Sugden, 2000, p.24). There are many ways the world could be, and Sugden proposes *credibility* as a way to sort them, but in his view, it is not clear how credible worlds (i.e. worlds that could be real) would deal with postulated impossibilities. Are impossible worlds credible?

Elsewhere (Brandelet, 2021), I have defended the view that the inductive framework proposed by Mill (Mill, 1843 / 2011) is transformed into a deductive system when causal laws have highlighted the relevant structure in experiments. Regularities are inductively explained and then serve as the ground for deductively making new

predictions. Avoiding the discussion about the nature of causality in Mill's work, I think we can export his view on the notion of laws to consolidate our fictional and counterfactual conception.

I disagree with Sugden when he writes: "To put this another way, the real world is equivalent to an immensely complicated model: it is the limiting case of the process of replacing the simplifying assumptions of the original model with increasingly realistic specifications." (Sugden, 2000, p. 23) because fiction allows for impossibilities that are not only simplifications or idealisations. Natural laws, understood as Mill's inductive generalisations, generate models. Models are then credible only if compatible with these laws, but credibility does not prevent impossibilities.

Laws express sets of relations obtained via inductive reasoning over observed regularities. These counterfactual relations are exemplified in models, and exemplification may involve all sorts of fictional processes such as, but not limited to, idealisations. That is where the felicitous falsehoods draw their epistemic virtues. Postulating impossibilities (i.e. incompatibilities with laws) is the main modelling freedom offered by the fiction view, but as the laws generating the models express a web of counterfactual dependences, only these relations need to be compatible for the model to be acceptable. In Sugden's terms, impossible worlds can be credible, as long as the model is robust through manipulation and the counterfactual structure remains applicable to the modelised phenomena.

There seems to be some kind of "truth-eligibility" in fictional statements. Manipulating a fictional model does not only mean deriving new fictional statements from old ones, it also means being able to give an interpretation of those propositions in terms of the target. Using the model adequately is also a matter of knowing what is not to be supposed to be true, or even possible at all. If a simple pendu-

lum user claims that the empirical adequacy of the model supports the existence of point masses, he is obviously not using the model properly, the non-existence of point masses is no argument against the model itself. We may be wrong about some aspects or properties of the depicted entities, even about their existence, but the conservation of the counterfactual structure through manipulation of the model is the focal point of our understanding and explanations. That is how we shed light on the fixed point of counterfactual dependencies the model exemplifies.

Model propositions must not be taken at face value. A model is more like a dynamical entity, a counterfactual engine that generates sets of propositions about a physical system and provides a justification for inferences to the world. They are "descriptions of how the world could be" (Sugden, 2013, p.241) equipped with inference rules (inherited from the laws) that guide the fictional reasonings.

5. Veritism reloaded?

As in many subfields of epistemology and philosophy of science, realist and truth-oriented positions have faced strong arguments from all sides. If, as I claim, veritism of explanation can be retained in the fiction view of models, what limitations does it impose on veritism?

I think a position needs to meet two conditions to be considered as veritist:

1. There needs to be some kind of correspondence or similarity between the world and the model at play, and this relation must be, at least partially, the ground of the explanation. In

particular, pure empirical adequacy, acceptance by the scientific community or compliance to scientific norms is insufficient if not based on the correspondence relation.

2. The correspondence must be equipped with a demarcation mechanism: we must be able to link the validity of the model-based explanation with the other epistemic virtues, such as empirical adequacy, and the difference between acceptable and non-acceptable explanation must be constructible in the framework.

Firstly, as I have explained in the previous sections, I take the counterfactual structure exemplified by the model to be the ground of the explanation. Clearly, the model must reproduce and predict empirical data in order to be veridically evaluated. Robustness for a range of input values, i.e. manipulability of the model, offers the ground for inductive inferences. This is where the correspondence relation comes into play: as in Sugden's example of inductive (and abductive) argument, a similarity gives the argument its skeleton. I claim this similarity to be a similarity of counterfactual structure inherited in the model from the laws, understood in the minimal sense of inductive generalisations supporting counterfactual reasonings.

Secondly, the lesson from the fiction view is that the demarcation between valid and invalid explanations is possible, but that we must refrain to interpret veridically anything that is not part of the counterfactual structure in our explanations. For example, using a model of silogen atoms to explain phenomena, we may talk about these atoms as-if they were real, but the veritist ground of the explanation are the quantum-mechanical processes at play in solid-state physics. Explaining with silogen would be a misuse of the silogen fiction.

6. Fiction and the ontic/epistemic accounts of explanation

In this section, I will show how the fiction-view of models defended in this paper can help clarify issues related to the conceptions of explanation and the OC/EC opposition.

Conceptions of explanation are typically classified into two categories: ontic and epistemic. Commonly conceived, ontic conceptions (OC) consider explanations to be concrete things that exist "out-there", independently of any theorising about them. On the other hand, epistemic conceptions (EC) take explanations to be the product of a scientific activity involving various techniques, such as representation of the phenomena to be explained. According to the EC, there is then no explanation if no scientist is doing the explaining.

In (Bokulich, 2016), the focus on the central role of idealisations and fictional processes in modelling is taken to be the sign of a need to move "beyond the ontic conception". Her line of argument is quite straightforward and easily phrased in fictional terms. Models feature deliberate falsities and idealisations which are part of their explanatory power. Models considered as fictions have representational power, and these representations are epistemic products build and used by scientists to explain phenomena. In this context, it seems clear that EC is a much more natural way of conceiving explanations.

Differently put, if, as it is claimed in OC, explanations are "full-bodied things" (Craver, 2013, p.40), "objective features of the world" (Craver, 2007, p.27) that exist independently of the epistemic goals, arguments and activities performed by scientists, then how could deliberate falsities be explanatory? The recognition of the explanatory power of fictions rules out this possibility, and the fiction-view provides an argument in favour of EC. That, of course, should be no

surprise: it is at the very heart of *make-believe* oriented approaches to take models as epistemic products designed and used by certain agents in order to achieve certain goals according to certain norms of evaluation.

Also, as (Wright, 2015) notes, the ontic conception, for example in Salmon's phrasing, involves something as an "exhibition". If it is deeply unclear to point out what could be an exhibition "in re", it is quite natural in the fictional perspective, where exhibition can be understood as a form of model-representation: it is exactly the role of idealisations and fictional hypotheses to exhibit features in models to support the explanation. This is the act of representing which is at the core of model-explanations and in which, as I have emphasised earlier, fiction plays a positive role.

Bokulich (2018) takes her analysis a step further where she proposes what she calls the "eikonic" conception. In this perspective, explanations also are "the product of an epistemic activity involving representations of the phenomena to be explained". Leaving the details aside, the eikonic conception is close to the fiction view discussed in this paper. What I think is more important is the distinction she introduces, as it may help clarify the roots of the OC/EC debate.

One of the key aspects of the eikonic proposition is that it constitutes a conception of explanation and not an account of explanation. An account of explanation is a claim about how explanations work, while a conception is a claim about what explanations are. The problem is that there is an ambiguity in the general treatment of the OC/EC debate: are these conceptions in the general sense, or in the restrained use of Bokulich?

This is an important point because examples traditionally associated with the ontic conception may fall under the epistemic umbrella if seen not as explanations, but as ways of explaining. For example,

causal or mechanistic accounts are on the ontic side: it is the electron that hit the screen that explains the presence of a white dot. But one could also argue that causes, mechanisms and unobservable entities are features of idealised models that are not explanations but descriptions of how explanations work, thus constituting an account of explanation. A mere reference to a cause or mechanism is not sufficient to decide whether it is part of an ontic or epistemic conception of explanation. A causal account is compatible with both the ontic and epistemic conceptions depending on what is considered to be the explanation itself.

If, as Bokulich asserts, early literature on explanation fails to draw the account/conception distinction, it becomes necessary to examine if such a distinction helps clarify the ontic/epistemic opposition. Unlike her, I don't think it does, and I claim the concept of fiction I am focusing on in this paper may explain why. That is what I will illustrate in the remainder of this section.

The lack of the aforementioned distinction and the only recent critique of the ontic conception may seem surprising at first glance. The central theses of the ontic conception, namely that explanations are not arguments but things and that explaining doesn't involve representation, have been left uncriticised for a long time, even if they are obviously false. Some proponents of EC, including Bokulich, share this surprise, but on the contrary, we might as well be surprised that such obviously false positions could be attributed to anyone. This type of astonishment could just as easily have been the result of a misunderstanding as of an ill-considered philosophical position.

To shed light on the tension at play in this defence of EC, let us extend the analysis proposed in the eikonic conception. As a conception of explanation, the eikonic position is a claim about what explanations are and it falls under the epistemic umbrella for reasons

discussed just before. Also, the eikonic conception is independent of the particular account of explanation one chooses to defend. Bokulich also claims that her view is compatible with scientific realism, just as I claim with the fiction view. Explanations are then considered as epistemic products involving a representational activity via idealised models. The compatibility with different accounts comes from the fact that the model may produce the explanation via different means, e.g. by using covering laws, causes, causal patterns, mechanisms, etc.

If a realist justification is to be found in this context, it must be via some kind of correspondence. I elaborated on the realist requirements and the correspondence with the causal patterns the model exemplifies (or exhibits!) in the previous sections. The question that arises is then the following: how could a purely epistemic explanation be realistically justified? Objective features of the world cannot be deliberately false as models are, but if we look for a justification in the realist sense, it must be grounded on a correspondence with objective features, may it be entities, mechanisms, causes or structures, depending on the flavour of realism one prefers.

In his response to (Bokulich, 2016), Craver develops an argument along these lines. Contrary to many critics of OC, Craver starts by asserting that proponents of the ontic conception simply do not dismiss the explanatory use of texts and arguments:

> When defenders of the ontic view write about explanations as if they are, "out there", as they are, independently of what anyone knows or thinks about them, they are expressing realism about the appropriate referents of explanatory texts, not abandoning the idea that scientists use texts to express explanations (Craver, 2019).

If proponents of EC reject OC because of the obvious use of epistemic products to express explanations, it is a misunderstanding

of what makes OC ontic. Now, if we come back to the fictional or the eikonic view, we may as well claim they fall under the ontic conception. Idealisations and fictional models are ways to extract and convey information, but they do not constitute the explanation. What explains is the correct correspondence with real causal patterns. On that topic, Craver is explicit:

> But conveying explanatory information about X and truly representing the explanation for X are not the same thing. Friends of the ontic conception should say that idealized models are useful for conveying true information about the explanation, but that they are not true representations of the explanation (Craver, 2013, p.50).

And, in the next paragraph, he adds:

> Once these are separated, the problem of idealization is clearly not a problem for theories of scientific explanation; rather it is a problem for philosophical theories of reference. The question at the heart of the problem of idealization is this: What is required for a given explanatory text to convey information about the ontic structure of the world? This is an important question, but it is a question about reference, not a question about explanation. We only invite confusion if we fail to keep these questions distinct (Craver, 2013, p.50).

This is exactly what the fictional view is able to provide: accounting for the representational and idealised component of explanation while justifying realistically these explanations. Fiction makes a clear distinction between the world of the model and what it may refer to. Both OC and EC can make room for various epistemic strategies in conveying the relevant information: idealisations, representations, modelling in terms of causes, mechanisms, laws, etc. In the fictional

view I defend, the model is not the explanation, the model is supporting the explanation by making a set of hypothesis manipulable. When asking, in the vein of Bokulich, about the fictional view as a conception of explanation, there are two equally valid answers: it is ontic if the model provides a justified representation of real causal patterns, it is epistemic if the fictional models are considered as tools for predicting and manipulating physical systems with no reference to real entities or causes or mechanisms. In this understanding of "conception", the OC/EC debate aligns with the realism/antirealism opposition: what makes the "electron hit the screen" explanation ontic is the reference to the real photon and its properties. Following Craver, as conceptions of explanation conflate with reference problems, if Bokulich claims her eikonic view is compatible with realism, it then falls under the ontic umbrella, contrary to her claim.

On the other hand, fiction naturally handles the division of labour between questions about what models refer to if they are to count as explanatory and questions about how explanations work, i.e. the question of *accounts* of explanation. Make-believe and exhibition of causal pattern is a description of how model-based explanations work, while the justification in terms of realist-oriented correspondence provides an ontological understanding. In the same vein, Bokulich's eikonic view is an *account* of explanation, neither ontic nor epistemic, but compatible with realist as well as antirealist views on reference, just as the fiction view is to be considered as an account of explanation, irrespective of questions of reference and OC/EC classification. As I showed earlier, this account is compatible with veritism and, more broadly, with a realist attitude towards science.

7. Conclusion

Is there a place for veritism of explanation in the fiction view of models? I think it does, but exploring the aspects of models as games of make-believe about credible worlds imposes limits on the kind of veritism one can hope to achieve.

First of all, the fictional view can accomodate the epistemic virtue of idealisations and approximations: they epistemically contribute to explanations and understanding by simplifying matters, and the fictional freedom in the construction of models offers all the strategies to scientists in doing so.

The central conundrum of the fiction/veritist approaches may be clarified by turning our attention to the modal aspects of models. Models are descriptions of credible worlds which we can manipulate to generate, when suitably interpreted, propositions about physical target systems. Manipulating a model means we explore the robustness across a range of input values of the embedded counterfactual structure. Fictional processes, like postulating non-existent entities can help in this exploration, but the counterfactual structure itself is immune to fiction and remains veridically interpretable.

The relation between laws and models also appears clearly in the fictional context. Laws generate models in the sense that they are the expression of the counterfactual dependences the model contains. The structure is the skeleton, fictional hypotheses are the flesh that facilitates reasoning and interpretation. When the model is found to be robust, we can say that the counterfactual structure reproduces the one of the physical system. This is the ground of explanations, and it remains veritist in the sense that it is the agreement between the two structures that strengthens the inductive inferences we draw from the model to build explanations. That also explains why laws are

so important for explanations. The observation of regularities needs to be explained, the law is the explanation, and the models make the link between laws and the world by making them manipulable. Models are not the explanation, but we need models in order to express explanations.

Regarding the OC/EC debate, the fiction view puts forward the positive role of representations and idealisations while making room for realist and truth-oriented justification of explanations. The ontic slogan according to which explanations are objective features of the world must be interpreted as a claim about the reference of explanations rather than a claim about the nature of explanations qua arguments or representations. This shows that the opposition at play is less about explanations that it is about the more general problem of reference and realism. The fiction view marks clearly the distinction by making a claim about how explanations work (i.e. it provides an account of explanation) and by providing the conceptual resources to tackle the problem of the reference of idealised claims and models.

Bibliography

Bokulich, A., 2012. Distinguishing explanatory from nonexplanatory fictions. *Philosophy of Science* [Online], 79(5), pp.725–737. https://doi.org/10.1086/667991.

Bokulich, A., 2016. Fiction as a vehicle for truth: moving beyond the ontic conception. *The Monist* [Online], 99(3), pp.260–279. https://doi.org/10.1093/monist/onw004.

Bokulich, A., 2018. Representing and explaining: the eikonic conception of scientific explanation. *Philosophy of Science* [Online], 85(5), pp.793–805. https://doi.org/10.1086/699693.

Brandelet, A., 2021. Induction et loi naturelle chez Mill. *Le Philosophoire* [Online], 55(1), pp.205–224. https://doi.org/10.3917/phoir.055.0205.

Chakravartty, A., 2001. The semantic or model-theoretic view of theories and scientific realism. *Synthese* [Online], 127(3), pp.325–345. https://doi.org/10.1023/a:1010359521312.

Craver, C.F., 2019. Idealization and the ontic conception: a reply to bokulich. *The Monist* [Online], 102(4), pp.525–530. https://doi.org/10.1093/monist/onz023.

Craver, C.F., 2007. *Explaining the brain*. Oxford University Press, USA, p.272.

Craver, C.F., 2013. The ontic account of scientific explanation. *Explanation in the special sciences* [Online]. Springer Netherlands, pp.27–52. https://doi.org/10.1007/978-94-007-7563-3_2.

Elgin, C.Z., 2017. *True enough*. MIT Press Ltd. 352pp.

Frigg, R., 2009. Models and fiction. *Synthese* [Online], 172(2), pp.251–268. https://doi.org/10.1007/s11229-009-9505-0.

Frigg, R. and Nguyen, J., 2016. The fiction view of models reloaded. *The Monist* [Online], 99(3), pp.225–242. https://doi.org/10.1093/monist/onw002.

Frigg, R. and Nguyen, J., 2020. *Modelling nature: an opinionated introduction to scientific representation*. Springer International Publishing. 260pp.

Hacking, I., 1983. *Representing and intervening* [Online]. Cambridge University Press. https://doi.org/10.1017/cbo9780511814563.

Lawler, I., 2019. Scientific understanding and felicitous legitimate falsehoods. *Synthese* [Online], 198(7), pp.6859–6887. https://doi.org/10.1007/s11229-019-02495-0.

Mill, J.S., 1843 / 2011. *A system of logic, ratiocinative and inductive* [Online]. Cambridge University Press. https://doi.org/10.1017/cbo9781139149839.

Nawar, T., 2019. Veritism refuted? understanding, idealization, and the facts. *Synthese* [Online], 198(5), pp.4295–4313. https://doi.org/10.1007/s11229-019-02342-2.

Pincock, C., 2021. A defense of truth as a necessary condition on scientific explanation. *Erkenntnis* [Online]. https://doi.org/10.1007/s10670-020-00371-9.

Potochnik, A., 2017. *Idealization and the aims of science*. University of Chicago Press. 288pp.

Sugden, R., 2000. Credible worlds: the status of theoretical models in economics. *Journal of Economic Methodology* [Online], 7(1), pp.1–31. https://doi.org/10.1080/135017800362220.

Sugden, R., 2013. How fictional accounts can explain. *Journal of Economic Methodology* [Online], 20(3), pp.237–243. https://doi.org/10.1080/1350178x.2013.828872.

Sullivan, E. and Khalifa, K., 2019. Idealizations and understanding: much ado about nothing? *Australasian Journal of Philosophy* [Online], 97(4), pp.673–689. https://doi.org/10.1080/00048402.2018.1564337.

Toon, A., 2012. *Models as make-believe*. Palgrave Macmillan. 148pp.

Vaihinger, H., 1911. *The philosophy of 'as if '*. 2009th ed. Martino Fine Books. 420pp.

Verreault-Julien, P., 2019. How could models possibly provide how-possibly explanations? *Studies in History and Philosophy of Science Part A* [Online], 73, pp.22–33. https://doi.org/10.1016/j.shpsa.2018.06.008.

Walton, K., 1978. Fearing fictions. *The Journal of Philosophy* [Online], 75(1), pp.5–27. https://doi.org/10.2307/2025831.

Walton, K., 1990. *Mimesis as make-believe : on the foundations of the representational arts*. Cambridge, Mass: Harvard University Press.

Winsberg, E., 2006. Models of success versus the success of models: reliability without truth. *Synthese* [Online], 152(1), pp.1–19. https://doi.org/10.1007/s11229-004-5404-6.

Wright, C., 2015. The ontic conception of scientific explanation. *Studies in History and Philosophy of Science Part A* [Online], 54, pp.20–30. https://doi.org/10.1016/j.shpsa.2015.06.001.

The ontic-epistemic debates of explanation revisited: The three-dimensional approach

Jinyeong Gim
Seoul National University, Republic of Korea

Abstract

After Wesley Salmon's causal-mechanical stance on explanation in the 1980s, the ontic-epistemic debate of scientific explanations appeared to be resolved in the philosophy of science. However, since the twenty-first century, this debate has been rekindled among philosophers who focus on mechanistic explanations. Nevertheless, its issues have evolved, necessitating scrutiny of the new trends in this debate and a comparison with the original controversy between Carl Hempel and Salmon. The primary objective of this paper is to elucidate three categorical dimensions in the ontic-epistemic debates, spanning from the original to the recent controversies. Subsequently, it will explore why the conception of explanation is linked to representations, what conditions are necessary for linguistic expressions to be explanatory, and what roles norms play in explanation. Consequently, contrary to the common stereotype, it will be argued that mechanistic explanations are more likely to be epistemic rather than ontic.

Keywords

mechanistic explanation, scientific representation, explanatory norms, Wesley Salmon.

Philosophical Problems in Science (Zagadnienia Filozoficzne w Nauce)
No 74 (2023), pp. 41–111

Introduction

Wesley Salmon's initial distinction between the ontic and epistemic accounts of scientific explanation stood out as one of the most prominent differences between Hempel's and Salmon's perspectives (1984; 1989). In broad terms, the epistemic account posits that scientific explanation involves an inferential argument aimed at understanding or predicting natural phenomena (see Hempel and Oppenheim, 1948; Hempel, 1965). On the other hand, the ontic account asserts that scientific explanation showcases the causal structures in the world that lead to the production of explanandum phenomena. Hempel's view faced numerous counterexamples, leading to the widespread acceptance of Salmon's perspective in the philosophy of science. Salmon's causal-mechanical explanation emerged as an alternative to Hempel's covering law model.[1]

Mechanistic explanation serves as a prototypical example of the ontic account of scientific explanation in line with Salmon's ideas. However, in the twenty-first century, some advocates of mechanistic explanation have posited that it is not ontic but rather epistemic (Bechtel and Abrahamsen, 2005; Wright and Bechtel, 2007; Wright, 2012; Wright, 2015). For instance, Wright and Bechtel (2007, p.51) reject the ontic perspective, stating, "Characterizing explanation as non-epistemic is problematic insofar as explanation is through-and-through an epistemic practice of making the world more intelligible." Bechtel (2008, p.18) underscores that "Explanation is fundamentally an

[1] The original debate between the ontic and epistemic accounts of scientific explanation was not historically limited solely to Hempel and Salmon. Ellis (1956) initiated this kind of controversy by focusing on the difference between explanation and description. Coffa (1973) also engaged in criticizing Hempel's view (see Wright and van Eck, 2018). However, this paper concentrates on Salmon's criticisms of Hempel for the purpose of philosophical clarification.

epistemic activity performed by scientists." In contrast, Craver (2007, p.21) continues to support the ontic view, aligning with Salmon's perspective: "Mechanistic explanation is associated with fitting a phenomenon into the causal structure of the world." Wright (2012, p.376), an epistemic theorist of mechanistic explanation, notes, "Perhaps because of their common interests in causality, most New Mechanists have hitched their wagon to Wesley Salmon's ontic conception of scientific explanation." Considering Salmon's original distinction, the ongoing ontic-epistemic controversy among mechanists appears somewhat peculiar.

Interestingly, the ontic-epistemic dispute remains ongoing. A decade ago, Craver (2014, p.28) argued that the ontic view plays a normative role in distinguishing good from bad explanations. He doesn't insist on defining what an explanation is but rather on identifying the essential constraints for recognizing, discovering, and using good explanations. This issue appears distinct from past ontic-epistemic debates, such as Salmon vs. Hempel and Craver vs. Bechtel & Wright, yet Craver still emphasizes the ontic stance (see Kaplan and Craver, 2011; Craver and Darden, 2013; Craver, 2014).

In contrast, van Eck (2015) contends that epistemic norms for identifying the causal roles of mechanisms must be satisfied before any ontic norms are applied to discover relevant causal factors of phenomena. He insists on the autonomy of epistemic norms from ontic norms, particularly in neuroscience. Sheredos (2016) supports van Eck's autonomy thesis by demonstrating that general and systematic explanations cannot be simultaneously fulfilled or simply conjoined with ontic norms, as the two epistemic norms take precedence over ontic norms. While mechanists continue to use the terms 'ontic' and 'epistemic,' their current controversies differ from the original debate between Hempel and Salmon.

As noted, the ontic-epistemic debate on mechanistic explanation encompasses three distinct yet interconnected issues. While these debates have transpired over different periods and concerning various topics, each issue overlaps. Notably, philosophers who align with an ontic position on one issue may adopt an epistemic stance on another, contributing to ongoing confusion in this debate. Despite decades since the original dispute between Hempel and Salmon, new questions continue to emerge within the philosophy of science. Consequently, reviewing these debates in light of contemporary trends is imperative. Key questions persist, such as: How do individual issues interrelate? Where do proponents of the New Mechanisms find agreement or disagreement? In what ways are mechanistic explanations connected to cognitive concepts like representation and models? Should we delineate between good and bad explanations, and if so, how do we make such distinctions? Lastly, how can we differentiate between how-possibly and how-actually explanations?

Section 2 aims to scrutinize various dimensions of the ontic-epistemic debates over the past four decades. These issues can be categorized into three dimensions: (i) A relational dimension concerning *explanatoriness* (or being explanatory) between explanans and explanandum; (ii) A conceptual dimension addressing the *nature* of explanation in science; and (iii) A normative dimension evaluating the *goodness* of explanations. The first dimension of explanatoriness can be further broken down into three sub-dimensions: (i-1) What *form* does explanation take? (i-2) What imparts explanatory *force* to the explanandum phenomenon? (i-3) How do we ascertain the *relevance* of explanans to the explanandum phenomenon? Additionally, what is the overarching purpose of our pursuit of explanation? These

sub-dimensions of the first relational dimension of explanatoriness commonly pertain to the conditions that ensure the qualifications of scientific explanations.

In each dimension of the ontic-epistemic debates, it's essential to note that the distinctions are categorical, and we don' allow degrees between the two extremes to enhance clarity in our discussions. Specifically, (i) In the dimension for explanatoriness, we consider the categorical items of causal relevance vs. logical necessity. (ii) Within the dimension addressing the nature of explanation in science, the categorical items are facts or things in the world vs. representations as to actual mechanisms. (iii) In the normative dimension evaluating the quality of explanations, the categorical items are completeness & accuracy vs. intelligibility (or generality or systematicity).

Next, I will critically evaluate the widely accepted claim among proponents of the ontic view that "Mechanistic explanations are ontic." Utilizing the dimensional framework, I will delineate various phases of the ontic perspective on mechanistic explanation. Subsequently, I will analyze individual ontic theses, cautiously suggesting that mechanistic explanations do not necessarily have to be ontic.

By reviewing the ontic-epistemic debates spanning several decades and classifying them under three dimensions, I will underscore that the epistemic nature of explanation should be the focal point in the ontic-epistemic debates. Each section will delve into questions related to the three dimensions of this debate. In Section 3, focusing on the conceptual dimension of the nature of explanation, I will explore two key questions: (1) Why is the concept of representation required when comprehending the conceptual meaning of mechanistic explanation? (2) Is the relationship between the explanation and the target mechanism a binary pair or something more? Moving to Section 4, centered on the relational dimension of explanatoriness,

I will address two additional questions: (3) What stance can we adopt toward Hempel's account from the epistemic viewpoint? (4) How can we develop an epistemic account of mechanistic explanation while avoiding many of the criticisms of Hempel? Finally, Section 5 will concentrate on the normative dimension of explanation, delving into the following questions: (5) Are ontic norms like completeness actually achievable? Should we appeal to ontic norms when evaluating explanations? (6) Must we distinguish good from bad explanations? In conclusion, contrary to stereotypical thinking, the answers to these questions will reveal that mechanistic explanations may be epistemic in each dimension.

1. Three Dimensions in Ontic-Epistemic Debate

The ontic-epistemic debate of explanation traces its roots to a disagreement on the qualifications of explanation between Hempel and Salmon. Hempel puts forth specific philosophical conditions for scientific explanation, outlining four key criteria: (i) the explanandum must be a logical consequence of the explanans; (ii) the explanans must contain general laws; (iii) the explanans must have empirical content; and (iv) the sentences constituting the explanans must be true (Hempel, 1965, pp.247–249; Hempel and Oppenheim, 1948, pp.137–138). As a logical empiricist, he distinguishes the first three conditions as a set of logical conditions of adequacy, while the last is an empirical condition of adequacy. These four conditions are integral to Hempel's conception of scientific explanation, a viewpoint that Salmon outrightly rejects, labeling it as the epistemic view.

1.1 The Relational Dimension: Explanatoriness

Explanatoriness means the quality or property of being explanatory. Hempel's conditions provide clues of explanatoriness, including explanatory form, relevance, force, and goal. Salmon (1984, p.84) describes Hempel's claims of explanatoriness as the *inferential* version of the epistemic conception. As we will see, Salmon (1989) enumerates counterexamples against Hempel's claims and guides us into the ontic view of explanatoriness

1.1.1. Explanatory Form: Arguments

Hempel's first condition of scientific explanation concerns the relation between the explanans and the explanandum. He introduces logical terms to stipulate their relation, deduction, and induction. (1965, pp.335–393) suggests a form of scientific explanation, *argument*. Many philosophers, including Michael Scriven, Richard Jeffrey, Bas van Fraassen, and Peter Railton, reject this inferential conception of explanation (see Salmon, 1989 for overall discussions). Salmon also denies Hempel's idea that scientific explanation is a form of argument. One of the most potent counterexamples is Bromberger's flagpole case (and Salmon's eclipse case) (Salmon, 1989, pp.46–47). According to the deductive-nomological (DN) model, we may explain the length of a shadow cast by a flagpole using specific laws of optics and geometry and the sun's position in the sky. However, using the same laws, the shadow's length, and the sun's position, we can also explain the height of the flagpole. Both cases satisfy Hempel's requirements of explanatory form, but the former seems explanatory, whereas the latter does not. The so-called asymmetry of explanation implies that arguments as explanatory forms are an insufficient condition of scientific explanation. In addition, because logical forms of

argument are closely related to subsequent explanatoriness, including explanatory relevance, force, and goals, many philosophers indirectly refuse Hempel's formal idea of scientific explanation.

1.1.2. Explanatory Force: Universal or Probabilistic Laws

For a deductive (or inductive) argument to qualify as a scientific explanation, universal (or statistical) laws should be included in the premises. If the lawful propositions are empirically true, all the deductively derived propositions are also true through valid arguments. The empirical content of the explanans is the third requirement of Hempel's account. If statements have *empirical* content supported by experimental evidence, they become true, and vice versa. The laws of nature in science are generally empirically confirmed either directly or indirectly. Kepler's laws are derived directly from vast observational data about the motions of celestial bodies in the solar system. Newton's laws are indirectly justified by deducing Kepler's laws from Newton's law of gravitation. Universal or highly probabilistic laws provide explanatory forces for the explanandum phenomena in Hempel's account.

However, Salmon points out that the requirement of universal or probabilistic laws is insufficient for scientific explanation. A typical counterinstance is the barometer case (Salmon, 1989, p.47). A sharp drop in the gauge of a barometer is highly correlated with the occurrence of a storm. By assuming a law stating that whenever the barometric pressure sharply decreases, a storm will happen, we can deductively (or highly probabilistically) infer the realization of the storm. However, the barometer's reading never explains the occurrence of the storm because atmospheric conditions in a particular region are common causes of both the drop in the barometric reading

and the occurrence of the storm. A lesson from this case is that causal patterns producing the explanandum phenomena must be exhibited whenever we explain the world. Salmon (1989, p.128) states, "According to the ontic conception, explanatory knowledge is knowledge of the causal mechanisms, and mechanisms of other types perhaps, that produce the phenomena with which we are concerned."

1.1.3. Explanatory Relevance: Deductive Necessity or High Probability

Hempel's first condition of scientific explanation is also associated with explanatory relevance between the explanans and the explanandum. As Hempel introduces two kinds of arguments, two epistemic criteria support relevant relationships between them. In deductive arguments, either nomological or statistical, logical necessity holds a specific relation between the explanans-statements and the explanandum-statement. Logical necessity relies upon the laws of deductive logic. The deductive implications of laws under initial conditions are the very explanandum. Similarly, in inductive arguments, high probabilities guarantee a highly probable possibility for the occurrence of the explanandum phenomenon. That is, if statistical laws are close to 1, then it is certain that the explanandum phenomenon happens. Explanatory relevance can be achieved within arguments through deductive necessity and high probability.

Salmon raises questions about Hempel's two epistemic conditions. The first question regarding deductive necessity is why irrelevancies are harmless to arguments but fatal to explanation. Salmon (1984, p.93) states, "In deductive logic, irrelevant premises are pointless, but they do not undermine the validity of the argument." However, explanation requires that "*Only* considerations relevant to the explanandum be contained in the explanans" (Salmon, 1984, p.94). A typical coun-

terexample is the contraceptive pill case. The fact that a man faithfully consumes his wife's contraceptive pills does not explain *his* failure to become pregnant (Salmon, 1989, p.50). Donald Trump could have avoided getting pregnant during the past year because he took his wife's birth control pills regularly, and it is well-known that every man who regularly takes birth control pills avoids pregnancy (Salmon, 1971, p.34). Even if men take birth control pills, the pills can never work because only women have babies, not men.

Salmon points out a similar difficulty between inductive argument and explanation. According to Carnap (1950, sec.45B), the requirement of total evidence is essential in inductive logic. Salmon (1984, p.93) says, "This requirement demands the inclusion of all relevant evidence." Inductive arguments face no harm when irrelevant evidence is included in the premises. However, if irrelevant evidence overwhelms relevant factors in the explanans, it fails to explain the explanandum phenomenon. For instance, the massive consumption of vitamin C is statistically irrelevant to alleviating the severity of common colds because vitamin C does not have a directly alleged efficacy for common colds (Salmon, 1984, p.94; 1989, p.58). That is, probabilities based on relevant factors to the explanandum phenomenon are essential in inductive arguments, too.[2]

One of the main consequences in Hempel's account is the symmetry of explanation and prediction. This symmetry thesis originates from the explanatory form of explanation in Hempel's idea.

[2] Further, *high* probability is not a necessary condition to achieve explanatory relevance in inductive arguments (Salmon, 1989, p.49). For example, it is noted that approximately 1/4 of all victims of latent untreated syphilis develop paresis. The attack rate from syphilis to paresis is below 50%, but the low probability contributes to explaining the occurrence of paresis. In short, deductive necessity and high probability are insufficient conditions for explanatory relevance. Besides, high probability is not a necessary condition for it.

Along with Newton's laws of movements of celestial objects under suitable initial conditions, we can deduce the occurrence of the appearance of a comet. According to Hempel, the logical form of explanation as an argument applies to scientific prediction and explanation (Hempel and Oppenheim, 1948, p.138; Hempel, 1965, p.249).[3] Salmon refers to this thesis as *nomic expectability* by saying that an explanation could be described as "*An argument to the effect that the event-to-be-explained was to be expected by virtue of the explanatory facts*" (Salmon, 1984, emphases original). Salmon (1989, pp.129–130) points out a gap between explanation and prediction with the barometer example; "The sharply falling barometric reading is a satisfactory basis for predicting a storm, but contributes in no way to the explanation of the storm. The reason is, of course, the lack of a direct causal connection. For the ontic conception, [...] There must be a suitable causal relation between the explanans and the explanandum—at least as long as we steer clear of quantum mechanical phenomena."

To summarize, the first dimension of the ontic-epistemic debate is what linguistic descriptions must achieve to be explanatory. Hempel contrasts with Salmon on the form of explanation, its power, and the relation of relevance between the explanandum and its explanans. Hempel's epistemic view holds that we can secure explanatory relevance only if we make a valid argument in which the explanans

[3] "The difference between the two is of a pragmatic character. If E is given, i.e. if we know that the phenomenon described by E has occurred, and a suitable set of statements C_1, C_2, ..., C_k, L_1, L_2, ..., L_k, is provided afterwards, we speak of an explanation of the phenomenon in question. If the latter statements are given, and E is derived prior to the occurrence of the phenomenon it describes, we speak of a prediction. It may be said, therefore, that an explanation is not fully adequate unless its explanans, if taken into account in time, could have served as a basis for predicting the phenomenon under consideration. Consequently, whatever will be said in this article concerning the logical characteristics of explanation or prediction will be applicable to either, even if only one of them should be mentioned" (Hempel and Oppenheim, 1948, p.138).

is empirically true and if the explanandum is a logical consequence of the explanans. Inferential argumentation is an essential performance for scientific explanation. On the contrary, Salmon's ontic view holds that we can retain explanatory relevance only if we discover causal structures in the world that give rise to an explanandum phenomenon. According to Salmon, without information about the causes of phenomena under empirical inquiries, a scientific explanation cannot avoid many counterexamples that Hempel's epistemic view confronted. The first dimension concerning explanatoriness between Hempel and Salmon is a categorical classification between logical arguments and causal mechanisms.

1.2 The Conceptual Dimension: Nature of Explanation

After the late twentieth century, Salmon's ontic view has been widely accepted or discussed in the philosophy of science. Recently, many philosophers of science who focus on mechanisms to understand biology and biochemistry are also based on Salmon's ontic view. As Salmon emphasizes exhibitions of causal structures, most mechanists believe that mechanisms result in explanandum phenomena, so discovering mechanisms is an essential scientific practice.

Understanding mechanisms is a prerequisite condition for mechanistic explanation. There is no disagreement about the main features of a mechanism (Craver and Darden, 2013, pp.15–26). In short, a description of a biological mechanism serves as an explanation for a phenomenon presumed to be the outcome brought about by the mechanism. Thus, a mechanistic explanation is also a binary relation between a mechanism and its behavior, which needs to be explained;

consequently, explanatory relevance in mechanistic explanation can be achieved when we discover the mechanism that brings about the behavior of the mechanism.

Recall that the first dimension of explanatoriness is a criterion to find sufficient conditions that all scientific explanations must satisfy. Notably, most mechanists are not interested in defending such a sufficiency thesis, but they assume that mechanistic explanation is an explanatorily relevant scientific explanation. Instead, they began to dispute the ontic-epistemic debate differently. The second dimension of the debate is about the nature of explanation relating to mechanistic inquiries. According to ontic theorists, including Craver, a biological mechanism explains its regular phenomenon (Machamer, Darden and Craver, 2000; Craver, 2007; Craver and Darden, 2013). For instance, the mechanism of protein synthesis explains how linear sequences of amino acids can be synthesized from DNA (see Darden, 2006). The mechanism of neuronal depolarization explains how neural signals are transmitted from one neuron to another. These biological mechanisms are realizers of the phenomenon. To explain the phenomenon is to discover the mechanism. This idea is widely accepted in the New Mechanism because the descriptions of the mechanism are descriptions of the causes for the phenomenal occurrence. As Salmon defends that explanations are objective features of the world, Craver (2007, p.200) also "Advocates an ontic view of explanation according to which one explains a phenomenon by showing how it is situated in the causal structure of the world." In other words, ontic theorists think that mechanistic explanations are causal structures of the world.[4]

[4] When Salmon (1984, p.127) states that "the exhibition of causal mechanisms is an essential part of scientific explanation," the term "exhibition" does not refer to an active agent's cognitive outcome, such as an argument, but rather a passive result produced

However, discoveries of biological mechanisms are inevitably dependent on artificial experiments or observations under ideally stipulated conditions and manipulations. Additionally, mechanisms in nature are never observed directly. Whenever scientists employ experiments to find causal factors of phenomena, idealizations are methodological presumptions in laboratories. For this reason, knowledge about a biological mechanism is not identical to the mechanism itself; thus, all linguistic or diagrammatic descriptions can be regarded as a representation of them (Bechtel and Abrahamsen, 2005; Bechtel, 2008). Bechtel holds that "Explaining is still an epistemic activity" and that "The mechanism in nature does not directly perform the explanatory work" (Bechtel and Abrahamsen, 2005, p.425). "Explanation is fundamentally an epistemic activity performed by scientists" (Bechtel, 2008, p.18). Similarly, Wright (2012) also emphasizes human cognitive performances to make an explanation in science by saying that "Explanations are not produced until an explainer contributes her or his epistemic and/or cognitive labor." Hence, epistemic theorists think that explanations are human cognitive processes.

Although Bechtel and his colleagues stress that explanations (and even mechanistic explanations) are products from cognitive processes, not things in the world, Craver still adheres to an ontic view:

> The task is to develop an account of scientific explanation that makes sense of the scientific project of connecting our models to structures that can be discovered through experience and objective tests. In domains of science that concern themselves with the search for causes and mechanisms, this amounts to the

---

by causal processes and interactions in the physical world (see Salmon, 1984, p.132). In line with Salmon's causal thinking, ontic theorists, including Craver, believe that mechanistic explanations are the mechanisms themselves.

> idea that the norms of explanation fall out of a commitment by scientists to describe as accurately and completely as possible the relevant ontic structures in the world (Craver, 2014, p.48).

Craver acknowledges that we cannot but depend on laboratory epistemic procedures by constructing various models. However, only some models as to mechanisms can show a genuine causal structure to bring about an explanandum phenomenon. For instance, Hodgkin and Huxley's model of action potential in neuroscience helps predict all phases of electrical pulses in a neuron. However, this model cannot provide how individual ion channels within a neuron membrane produce the shape of potential, how they temporally interact, what electromagnetic activities occur, etc. Ptolemaic models of the solar system allow us to predict the motion of planets, but they cannot provide what causes the motions of planets at all. Craver (2006, p.367) argues that models can be explanatory only when they describe mechanisms in the world. Mechanistic schemata, not mechanism sketches, are worthy of explaining phenomena.

To summarize, the second debate within the New Mechanism is controversial about the conception of mechanistic explanation. Ontic theorists hold that causal structures existing in the world explain phenomena. Epistemic theorists hold that all procedures to exhibit causal structures are cognitive, so explanations are epistemic.

1.3 1.3. The Normative Dimension: Explanatory Norms

The third dimension of the ontic-epistemic debate is about explanatory norms or constraints to evaluate explanations. Craver (2007, p.21) argues, "Good explanations in neuroscience show how phenomena are situated within the causal structure of the world." Shortly speaking,

"Good explanations explain effects with causes" (Craver, 2007, p.26).[5] While Craver insists on the ontic constraints, he never argues that we should not cast our glance upon epistemic constraints. Craver (2014, p.28) says, "I do not claim that one can satisfy all of the normative criteria on explanatory models, texts, or communicative acts by focusing on ontic explanations alone. There are questions about how one ought to draw diagrams, organize lectures, and build elegant and useable models that cannot be answered by appeal to the ontic structures themselves." That is, Craver never denies the existence of epistemic norms. However, he emphasizes the 'demarcation' between good and bad explanations by ontic norms rather than epistemic (Craver, 2014, p.27).

Craver's emphasis on the ontic nature of mechanistic explanation in the conceptual dimension is closely associated with the normative dimension of explanation. Craver believes that one of the essential requirements for a successful mechanistic explanation is a successful representation of mechanistic models to target mechanisms. Craver (2006, p.360) introduces the notion of "Ideally complete descriptions of the mechanism," which are models including all the entities, properties, activities, and organizational features relevant to every dimension of the phenomenon to be explained. In addition, the target mechanisms should be responsible for the occurrences of the phenomenon to be explained in the world. Similarly, Kaplan and Craver (2011, p.611) suggest a model-to-mechanism-mapping (3M) requirement that satisfies the causal commitment with an ontic point of view:

[5] Craver (2007, p.26) enumerates five norms that good explanatory texts reveal the causal structure of the world: (E1) mere temporal sequences are not explanatory (temporal sequences); (E2) causes explain effects and not vice versa (asymmetry); (E3) causally independently effects of everyday causes do not explain one another (common cause); (E4) causally irrelevant phenomena are not explanatory (relevance); and (E5) causes need not make effects probable to explain them (improbable effects).

> In successful explanatory models in cognitive and systems neuroscience, (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.

Whereas Craver explicitly advocates ontic norms of mechanistic explanation, Bechtel and Wright do not. It is unclear whether Bechtel and Wright conversely defend epistemic constraints, but they are just concerned about such norms: "Explaining refers to a ratiocinative practice governed by certain norms" (Wright and Bechtel, 2007, p.51). Because of such implicit commitment, Wright and Bechtel do not complain about Craver's ontic normativity. That is, among Craver, Bechtel, and Wright, there seems to be no disagreement on the normative dimension of mechanistic explanation, such as Craver's idea, "The ontic structure of the world thus makes an ineliminable contribution to our thinking about the goodness and badness of explanations" (Craver, 2014, p.41).

Instead of Bechtel and Wright, other philosophers criticize Craver's ontic view in the normative dimension (van Eck, 2015; Sheredos, 2016). First, van Eck (2015) focuses on experimentation in neurosciences: "Achieving the ontic aim, as well as the epistemic aim of understanding, associated with mechanistic explanation, therefore, is deeply *constrained* by epistemic considerations" (van Eck, 2015, emphasis added). Van Eck mentions two reasons to support epistemic constraints: (i) explaining extinct mechanisms cannot be possible under ontic constraints; (ii) identifying performance to discover causal factors of an explanandum phenomenon is governed by epistemic

norms before ontic norms are applied. Thus, van Eck argues epistemic norms have more explanatory power than ontic norms. Sheredos follows van Eck's basic idea but further focuses on the normative priority of explanation between ontic and epistemic norms. He defends the priority of epistemic norms by mentioning two epistemic norms: generality and systematicity. The generality of a mechanistic explanation is determined by its scope of application. The more an explanation applies to other cases, the more general it is. If the mechanistic explanation is general, its systematicity is explicit because it demands a principle of unity or extrapolation that unifies similar cases under a limited scope (Sheredos, 2016, p.932). He says that the two epistemic norms are prior to Kaplan and Craver's 3M norms by arguing that (i) fulfilling the norm of generality and systematicity is logically independent of fulfilling 3M norms, and (ii) ontic norms ontologically depend on generality and systematicity.

Contrary to radical defenders of the epistemic norms, Illari, Kastner & Haueis regard ontic and epistemic constraints as equally important. As noted, Bechtel and Wright (2007), who emphasize the epistemic nature of mechanistic explanation in the conceptual dimension, acknowledge that exhibiting real mechanisms is essential to examine mechanistic explanations. Bechtel and Wright are involved in this debate only at the conceptual dimension and do not seem to have yet taken a clear position on the normative dimension. Illari (2013) was the first philosopher who tried to integrate ontic and epistemic views in the normative dimension. She argues that "good mechanistic explanations must satisfy both ontic and epistemic normative constraints on what is a good explanation" (Illari, 2013, p.237). That is because "we will often not be in a position to know whether a particular mechanistic explanation is there to satisfy epistemic constraints alone, or ontic, or both" (Illari, 2013, p.254). The ontic conception

concerns mechanisms themselves in the world, whereas the epistemic conception concerns our knowledge-building practices regarding the mechanisms. In more detail, ontic aims of mechanistic explanation are to "describe the entities and activities and the organization by which they produce the phenomenon or phenomena" (Illari, 2013, p.250), and epistemic aims of mechanistic explanation are to "build a model of the activities, entities and their organization that scientists can understand, model, manipulate, and communicate, so that it is suitable for the ongoing process of knowledge-gathering in the sciences" (Illari, 2013, p.250). Thus, Illari says, "the real achievement of mechanistic (and possibly other forms of) explanation is satisfying both ontic and epistemic constraints simultaneously, to get a story constrained by all the empirical contact with the world that ingenuity can design" (Illari, 2013, p.253).

Kastner and Haueis (2021, p.1636) more clearly argue that "ontic and epistemic norms are equally important in mechanism discovery." (Kästner and Haueis, 2021, p.1637) subsume mechanism discovery and mechanistic explanation under mechanistic inquiry and show that "an adequate account of mechanistic inquiry should not only distinguish ontic and epistemic norms but also make explicit how they work together in practice." That is because both types of norms are interdependent. Kastner and Haueis (2021, p.1658) say, "practitioners must share the commitment that (1) they search for the entities and activities responsible for the phenomenon in question and (2) that their epistemic activities make intelligible how the mechanism is responsible for the phenomenon to be explained" to represent patterns in the world successfully. Illari, Kastner & Haueis agree that ontic and epistemic norms are equivalently essential for building mechanistic explanations.

To summarize, the third dimension of the ontic-epistemic debate also occurs within the New Mechanism. Craver, an ontic theorist, holds that mapping the relationship between representations and mechanisms is essential to making good explanations. Epistemic theorists hold that ontic norms are subordinate to two kinds of epistemic norms, so epistemic constraints have priority over ontic constraints. Other philosophers pursue unifying ontic and epistemic norms because both kinds of norms interact in pattern recognition of mechanisms.

2. On the Nature of Mechanistic Explanation: Things vs. Representations

In the previous section, I chronologically enumerated three dimensions of the ontic-epistemic debates of explanation. As an ontic theorist, Salmon is engaged with all dimensions by suggesting the distinction of ontic-epistemic conceptions of explanation. Fundamentally, he asserts that scientific explanations are things in the world to acquire objective explanations. Salmon categorizes Hempel's covering law account of explanation as the inferential version of the epistemic conception, contrasting it with his causal-mechanical explanation as the ontic conception. He identifies two philosophical demarcation problems when emphasizing the ontic conception (see Salmon, 1984). One is distinguishing scientific explanations from other scientific achievements related to the first dimension of explanatoriness. The other is demarcating good from bad explanations, related to the third dimension of explanatory normativity, which will be discussed in Section 5. Salmon contends that the ontic conception of explanation provides

a critical standard for addressing these two problems and advocates for it by implicitly revealing that Hempel's inferential version of the epistemic conception fails to fulfill these philosophical requirements.

From now on, I will delve into each dimension of explanation, focusing on mechanistic explanations. Salmon's ontic stance leads many philosophers of science to assume that mechanistic explanations are inherently ontic (see Craver, 2007; Kaplan and Craver, 2011; Povich, 2018). However, Bechtel and Wright argue that mechanistic explanations stem from cognitive efforts to uncover mechanistic patterns, even though they acknowledge mechanistic explanations as scientific. Given the three-dimensional distinctions of the ontic-epistemic debates, my initial exploration will assess whether mechanistic explanations qualify as "things." This inquiry poses a significant challenge to ontic theorists, who persist in their belief that Bechtel and his colleagues lead us astray in the realm of mechanistic explanation (see Craver, 2014). In the first conceptual dimension, I will advocate for the epistemic stance on mechanistic explanation by demonstrating that the central ontic thesis yields counterintuitive results.

2.1 Explaining As Representing Mechanisms

The conceptual dimension of explanation delves into the very nature of explanation itself, addressing the fundamental question: "What is explanation?" The response typically takes the form of "Explanation is something like *X*." This line of inquiry is distinct from asking, "How is the explanandum phenomenon related to the explanans?" The latter question can be further subdivided to explore the various forms of explanation, identify what imparts explanatory power to the explanandum phenomenon, and ascertain how to determine the relevance of the explanans to the phenomenon. Salmon's causal-mechanical account

of explanation provides a set of answers to these questions, shaping the initial debate between Salmon and Hempel around the nature of explanation. Despite Salmon presenting numerous counterexamples to Hempel's model, the distinction he draws between ontic-epistemic *conceptions* of explanation can be perplexing, as he seems to primarily address the question related to the ontic *account* of being explanatory rather than the ontic *nature* of explanation itself. The conceptual dimension of explanation is about the semantic meaning of explanation. When Salmon emphasizes the ontic conception of explanation, his ontic view must be an idea of the nature of explanation in the conceptual dimension. The dimensional approach to ontic-epistemic debates aids in distinguishing among various issues within the controversies. As noted, Salmon's response to the question in the conceptual dimension of the debate is, "Explanation is a thing in the world producing the explanandum phenomenon."

Wright and Bechtel are pioneers in hightlighting the distinction between the account and the conception of explanation.[6] Wright and Bechtel (2007, p.73) argue that "Explanation is inherently an epistemic or cognitive activity" by focusing on "How investigators reason with the models and representations of mechanisms." Their view contrasts with the ontic perspective, where the "Explanans just is a mechanism or parts is taken literally" (Wright and Bechtel, 2007,

[6] Bokulich (2016, p.263) also emphasizes distinguishing the account from the conception of explanation. An account of explanation is a view of *how explanations work*. On the other hand, a conception of explanation is a view of *what explanations are*. Interestingly, Bokulich is in line with Wright and Bechtel's position when we look at the following Bokulich's mention: "Salmon endorses both the *ontic conception* of explanation and the *causal account* of explanation. One can, however, reject the ontic conception of explanation (i.e., deny that explanations are things in the world, independent of human theorizing), but endorse the view that many explanations are indeed causal (i.e., involve citing and representing the relevant subsets of causes of the phenomenon)" (ibid, *emphases original*).

p.49). There are compelling reasons to abandon the ontic view of the nature of mechanistic explanation in the conceptual dimension independent of the first relational dimension of explanation. First, mechanistic explanation relies on mechanistic *inquiries* to search for relevant components of the target mechanism and their organizational features. These inquiries encompass all scientific investigations aiming at discovering mechanisms and developing mechanistic explanations. All agential acts revealing the target mechanism's relevant parts, operations, and organizations "minimally need to be captured and codified in a structural or functional *representation* of some sort" (Wright and Bechtel, 2007, p.51).[7] Bechtel and Richardson (2010) suggest methodological strategies, decomposition, and localization as epistemic constraints of mechanistic inquiries.

Wright and Bechtel's epistemic view of the nature of mechanistic explanation is grounded in the idea that explaining is an agential activity of human beings. Wright and Bechtel (2007, p.51) emphasize that the mere existence of mechanisms does not suffice to construct an explanation because the presence of a phenomenon is not explained "until a cognizer contributes his or her explanatory labor." There is a limited possibility that all non-living creatures can explain natural phenomena. To explain a phenomenon, one must determine the initial and terminal conditions. Subsequently, relevant parts and their activities are identified by choosing adequate instruments or methods. Determining and choosing are decision-making activities for explaining the phenomenon. Mechanistic explanations arise from from *goal-oriented* inquiries of humans who seek mechanisms. If one

[7] The dependence of mechanistic explanation upon epistemic activities is widely recognized (see Kästner and Haueis, 2021, p.1637). This point will be discussed in Section 5 again.

embraces the epistemic nature of mechanistic explanation, it becomes apparent that the ontic conception leads to an absurd consequence: only humans, among all external materials, can explain phenomena.

Mechanisms are non-cognitive systems in the world. If one believes that a mechanism itself explains a phenomenon, one cannot help but confront another counterintuition. For instance, all explanations are potentially misleading and/or incomplete because explanation is a cognitive activity. Under the ontic view, by contrast, an erroneous explanation implies the non-existence of the mechanism. Probably, explanation errors originate from the objective reality of the target mechanism and practical activities through mechanistic inquiries. Of course, if there is no mechanism to produce the phenomenon, all mechanistic explanations are undoubtedly erroneous. However, the linkage between the failure of explanation and the non-existence of the mechanism also leads to an absurd consequence: no mechanisms exist in the world whenever we identify irrelevant parts or non-organized features. The synthesis of proteins exists independently of whether molecular biologists explain how the primary sequence of amino acids is produced. The action potential in a nerve cell occurs regardless of whether physiologists explain how voltage-gated ion channels control the flow of ions across a membrane. If we adopt the possibility of providing erroneous and/or incomplete explanations as to mechanisms, then the epistemic view seems more promising than the ontic view. As we will see, failures of explanation originate from misrepresentations. Wright and Bechtel (2007, p.51, fn.10) also say, "Erroneous explanation would not be possible since there would have been no mechanism for the erroneous explanation to have identified."

Nevertheless, ontic theorists still argue that mechanistic explanation must be a thing or fact in the world, not a representation. Bechtel and Wright point out a metaphysical confusion of the ontic

theorists, which is called the *de re* & *de dicto* confusion. Wright and Bechtel (2007, p.51) say that "mechanisms themselves are not the sorts of things can be constituents of any explanans." Constructing explanations requires the identification of relevant parts and comprehension of organizational structures. Identifying and comprehending are cognitive activities. Mechanisms *de re* are passive or inefficient in conducting both activities. Epistemic outcomes, either linguistic or diagrammatic, are *de dicto* mechanisms employed to explain the explanandum phenomena. For this reason, Wright and Bechtel (2007, p.51) say, "Explaining refers to *ratiocinative practice* that cognizers engage in to make the world more intelligible." Thus, the ontic assertion of the nature of mechanistic explanation must be understood as *metonymic*, not realistic (*ibid*).

To summarize, it is crucial to distinguish the account of explanatory relationships between the explanandum phenomenon and explanans from the conception of explanation itself. Salmon primarily focuses on the former when critically analyzing Hempel's account of scientific explanation. The first relational dimension of the ontic-epistemic debate pertains to this issue, whereas the second conceptual dimension is linked to the latter. Regrettably, Salmon occasionally uses the term *conception* when differentiating his ontic theory of explanation from Hempel's inferential version of the epistemic theory of explanation. Using the term *account* rather than *conception* was more suitable when Salmon criticizes Hempel's covering law model. As pointed out by Wright, Bechtel, and Bokulich, the assertion that non-agential things outside of our mind explain something appears absurd, as explaining itself is a form of cognitive activity. Mechanistic explanations are also outcomes dependent upon human research inquiries regarding resources such as components and their organizations within a mechanism.

2.2 Mechanistic Explanation and Scientific Representation

If a mechanistic explanation is an outcome of epistemic activities, representation arises as a central concept for understanding the nature of mechanistic explanation. The contents of mechanistic explanations emerge from scientific endeavors to represent mechanisms. Let me further develop an epistemic view of mechanistic explanation by considering the concept of representation. Generally, representation is equated with presenting a likeness or resemblance to what is represented. Representation as a resemblance involves a binary relation between the representer and what is represented. I think the ontic theorists implicitly adopt this binary relationship when discussing a relation between the explanandum phenomenon and explanans. Craver and Kaplan's 3M criteria is a typical case of this binary idea. The mapping relations between a mechanistic model and a mechanism are based on the resemblance relation independent of epistemic agents or human activities.

Note that we need further reasons to adopt the epistemic position of explanation in the conceptual dimension, even though explanations inherently involve a cognitive activity. That is because the binary relation of representations allows for the non-subjective interpretation of a mechanistic explanation. To adopt an epistemic position in the conceptual dimension, wherein explaining is representing, representations should transcend a binary relation based solely on resemblance. Instead, they should be characterized by a tertiary relation, enriched by agential performances. The question then becomes: Is a representation inherently binary, or can it involve more complex relationships?

To advance the epistemic understanding of explanation in the conceptual dimension, I will draw upon the philosophical discourse on *scientific representation* in the philosophy of science. Specifically,

I will introduce Bas van Fraassen's ideas to contemplate the nature of mechanistic explanation.[8] Van Fraassen distinguishes representations-as from representation-of. **Representation-as** is a binary relation between the representer and what is represented and is formulated by "*X* represents *Y* as *F*," where *X* is linguistic or pictorial things to represent, *Y* is the target object or referent represented, *F* is a predicate indicating the object's properties. For example, in the case of linguistic expressions, "Snow is white" represents snow as colored (van Fraassen, 2008, p.16). Similarly, the phrase "DNA is double-helical" linguistically represents DNA as having a double-helical shape. In the realm of non-linguistic expressions, a famous photograph of the Eiffel Tower, *Au Pont de l'Alma* by Doisneau, represents the Eiffel Tower in Paris. If we consider Rosald Franklin's B-form picture of DNA, we can assert that Franklin's B-form picture non-linguistically represents DNA as being double-helical. Despite the different modes of representation for DNA, whether linguistic or non-linguistic, they share a commonality in having a binary relation with DNA.

[8] Before doing this, I simply talk about whether linguistic representations, such as statements or propositions, are suitable forms of mechanistic explanation. A statement basically consists of a subject (or the referent) and a predicate (or its property). Interestingly, Bechtel and Abrahamsen argue that there are reasons to prefer diagrams as representations of mechanisms rather than linguistic descriptions. Diagrams help in depicting spatiotemporal information. Arrows in a diagram represent sequential orders of stages within a mechanism. Dots represent variables or individual objects. Pictorial illustrations help in visualizing spatial structures among objects. Bechtel and Abrahamsen (2005, p.429) do not deny the possible usage of linguistic descriptions, but Bechtel and his colleagues explicitly emphasize that diagrammatic representations are preferable forms (see Bechtel and Abrahamsen, 2005, p.432; Wright and Bechtel, 2007, p.52; Bechtel and Richardson, 2010, p.xix). However, I do not discuss the privileged form of mechanisms between linguistic and diagrammatic representations. Rather, I presume that both forms of representation have their own advantages to depict the mechanisms independent of whether we take Hempel's model of explanation. Both linguistic and diagrammatic descriptions simultaneously fill in most textbooks in biological sciences.

Contrary to the binary relationship of representations as a resemblance between a source and a target, van Fraassen defines representations by adding the indexical feature to scientific representation.

> What is represented, and how it is represented, is not determined by the colors, lines, or shapes, in the representing object alone. Whether or not *A* represents *B*, and whether or not it represents the represented item as *C*, depends largely, and sometimes only, *on the way in which A is being used* (van Fraassen, 2008, emphases original).

According to van Fraassen, 'use' assimilates the terms 'make' and 'take.' For instance, when an English person linguistically describes the shape of DNA as double-helical with the statement "DNA is double-helical," Watson and Crick take Rosalind Franklin's B-form X-ray figure as evidence to denote physical properties of DNA, including a 20-angstrom diameter, 34-angstrom height, and a 3.4 interval length between two bases. According to van Fraassen (2008, p.23), "*Use*, in the appropriate sense, must determine the selection of likenesses and unlikenesses which may, in their different ways, play a role in determining what the thing is a representation of, and how it represents that" (*emphasis original*). Based on the intentional situation, **representation-of** refers to the tertiary relation as "Someone, *Z*, uses *X* to represent *Y of F* in a context, *C*." The use of representation is relative to the intentional activities of agents.[9]" In pragmatics, van Fraassen also stresses the perspectival indexicality. For instance, "An apple is red" depicts the apple as red, while a *normal-eyesighted* person represents the snow *under the sun*. A red-blind person cannot represent the apple in normal light circumstances like that sentence.

[9] Similarly, Ronald Giere (2006, p.60) defines scientific representations as such "*S* uses *X* to represent *W* for purposes *P*.

A normal-eyesight person cannot represent the apple in the darkroom. From the pragmatic view of scientific representation, a representation-of results from an intentional activity. In short, "*There is no representation except in the sense that some things are used, made, or taken, to represent some things as thus or so*" (van Fraassen, 2008, emphases original).

How does van Fraassen's view of scientific representation align with the epistemic view of the nature of mechanistic explanation? In the epistemic view of the conceptual dimension, a mechanistic explanation arises from cognitive activities that involve representing relevant parts and their organized features of mechanisms. Mechanistic inquiries, aiming at representing mechanisms, are permeated with cognitive decision-making and choices.

Firstly, the objective is to explain a phenomenon by characterizing it, and considering which instruments are suitable for understanding its properties. This characterization determines the aspects that an agent explains. For instance, the mechanistic model of protein synthesis varies depending on the topics of the explanandum. James Watson, a molecular biologist, focused on the genetic flow from DNA to protein. At the same time, Paul Zamenick, a biochemist, was interested in enzymatic reactions leading to ATP synthesis, a process of energy production preceding the amino acid sequence synthesis (see Judson, 2013, part 2). Other biochemists, such as Francois Jacob and Jacques Monod, explored the regulation of the β-galactosidase gene expression when lactose is present (see Pardee, Jacob and Monod, 1959). Additionally, numerous molecular biologists conducted various experiments to study specific aspects of the protein synthesis process according to their interests.

Watson and Francis Crick proposed a diagram illustrating the flow of genetic information from DNA to protein (see Fig. 1). Zamecnik

and Mahlon Hoagland discovered a crucial intermediate molecule, transfer RNA (tRNA), which carries free amino acids into the process of synthesizing polypeptides. Furthermore, Robert Holley et al. (1965) successfully isolated tRNA and analyzed its structure (see Fig. 2). Jacob and Monod identified key components, including the regulator gene, promoter, operator, and lac operon (structural genes), interacting with each other in synthesizing messenger RNA (see Fig. 3). Hence, the characteristics of the explanandum phenomenon determine the scope, extent, and relevant entities of the mechanistic explanation.

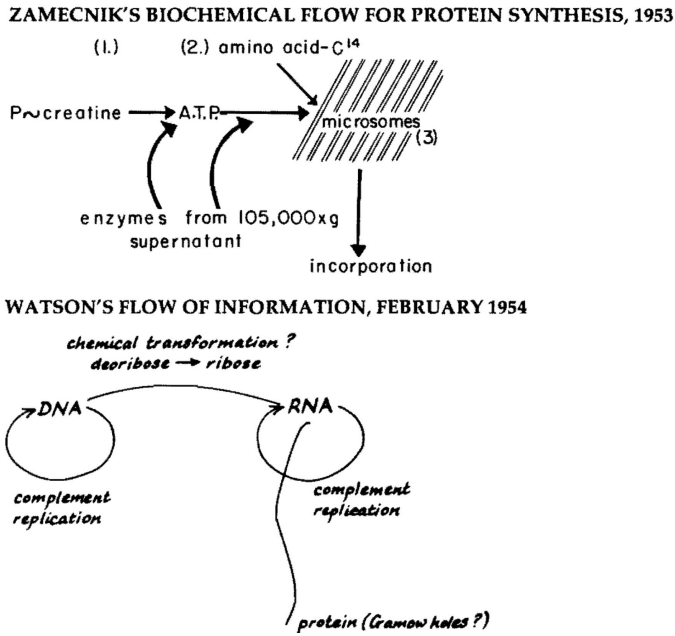

Figure 1: Zamecnik and Watson's diagrams (Judson, 2013, p.273)

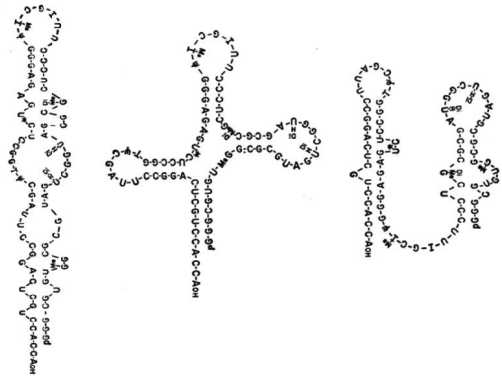

Figure 2: The two-dimensional representations of tRNA (Holley et al., 1965, p.1464)

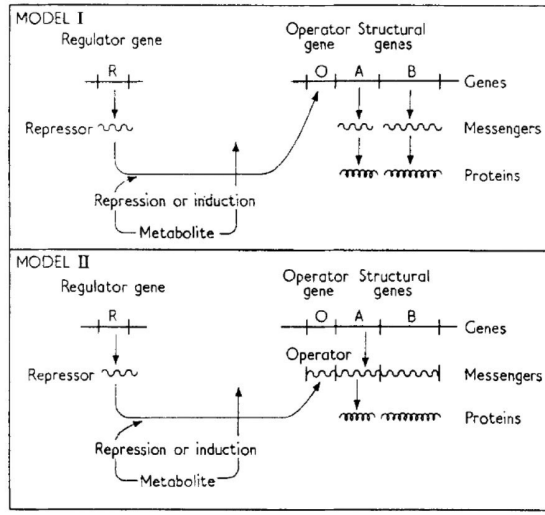

Figure 3: Models of the regulation of protein synthesis (Jacob and Monod, 1961, p.344)

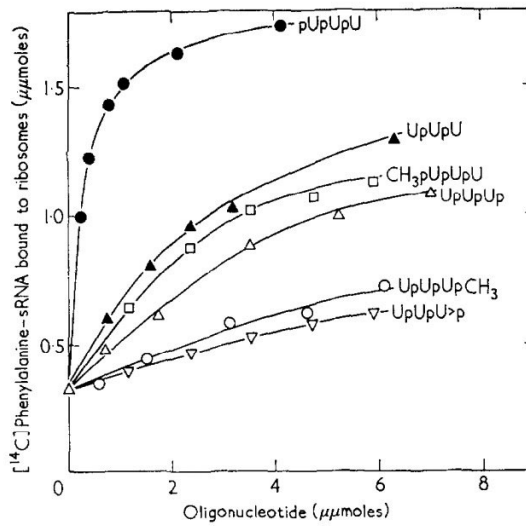

Figure 4: Effect of poly-U nucleotides with terminal phosphate on activities of synthesizing Phenylalanine (Rottman and Nirenberg, 1966, p.562)

After determining the phenomenon to be explained, researchers hypothesize what objects are relevant to the phenomenon. They either formulate hypotheses about the relations between candidate objects or conduct experiments to identify whether the phenomenon occurs due to chemical interactions between objects. Watson, in 1954, and Crick, in 1958, proposed the RNA template hypothesis between DNA and protein based on existing results at that time. Zamecnik conjectured that a ribonucleoprotein particle, later named a 'ribosome,' within the microsome is engaged with protein synthesis (Hoagland et al., 1958). The most famous experiment in molecular biology, the so-called PaJaMo experiment, performed by Arthur Pardee, Jacob, and Monod (1959), implies DNA itself is not a direct template of

protein synthesis. Other biologists, including Severo Ochoa, Marshall Nirenberg, and Har Gobind Khorana, contributed to explaining how genetic information is transferred from one molecule to another.[10]

Those theoretical or experimental results play a crucial role in explaining the phenomenon of protein formation in distinct ways. Watson and Crick's RNA template hypothesis represents the genetic flow from DNA to protein via messenger RNA. This hypothesis is instrumental in explaining how DNA's genetic information is transferred to proteins. Zamecnik's diagram delineates the biochemical flow for protein synthesis utilizing the formation of ATP. This diagram proves useful in explaining the energetic components required in protein synthesis (see Fig. 1). Holley's two-dimensional scheme from 1965 represents the structural shape of alanine RNA. This schematic representation is valuable in explaining how free amino acids are transported into the formation of polypeptides (see Fig. 2). Jacob and Monod's models from 1961 illustrating the regulation of protein synthesis represent the interaction between regulator, operator, and structural genes. These models are effective tools in explaining how different genes contribute to synthesizing transcribed RNA (see Fig. 3). Nirenberg's model of data shows that 5'-terminal phosphate poly-uracil sequences are more active in synthesizing phenylalanine linkages than in other sequence cases. This model is utilized to explain the structural features of mRNA required to form polypeptide linkages (see Fig. 4).

It's important to note that researchers characterize the phenomenon of protein synthesis based on their individual interests.

[10] Ochoa was a Nobel Prize winner in 1959 for their discovery of the mechanisms in the biological synthesis of ribonucleic acid. Nirenberg and Khorana were also Nobel Prize winners in 1968 for their interpretation of the genetic code and its function in protein synthesis.

Molecular biologists, for instance, concentrate on genetic mappings from DNA to protein, while biochemists explore different facets of the phenomenon, such as energetic aspects (Zamecnik), structural analysis (Holley), enzymatic processes (Ochoa, Jacob & Monod), and reaction rates (Nirenberg). Their research interests guide the representation of relevant parts of the protein synthesis mechanism. Watson and Crick, for example, speculated on the existence of messenger RNA between DNA and protein, while other biochemists employed various experimental instruments and interpreted data models. Some outcomes represent temporal steps of the mechanism, while others focus on the structural features of specific components. These diverse results prove valuable in explaining how proteins are formed in a cell. In essence, most representations serve as worthy explanans for mechanistic explanations when used to depict the physical features of candidate parts, revealing their structural shape and considering them as relevant components. In a nutshell, "A researcher *uses* linguistic and diagrammatic *representations* of relevant parts and their organizational features of a *mechanism* to *explain* a phenomenon depending on the researcher's interests."

Certainly, it is crucial to closely identify the relevant parts and their organizational features that are intricately linked to the occurrence of the explanandum phenomenon. However, the judgments regarding whether certain parts or organizational features are pertinent to the occurrence of the explanandum phenomenon are not issues within the conceptual dimension of explanation but rather fall under the relational dimension of explanatoriness. Here, I emphasize that explaining a phenomenon is conceptually identical to representing something that produces the phenomenon, and that representations are not binary but tertiary relations involving linguistic or non-linguistic representations, target entities or their organizations in the world, and

human agents. These two emphases form the foundation for subsequent discussions about the relational and normative dimensions of the ontic-epistemic debates.

3. On the Explanatoriness of Mechanistic Explanation: Sufficiency vs. Necessity

As noted, Salmon has led us to consider mechanistic explanation as an alternative to Hempel's covering law model, presenting it as the ontic conception of explanation, in contrast to the epistemically inferential nature of the latter. In the original ontic-epistemic debate, it is well-known that Hempel's model faces numerous counterexamples, as detailed in Section 2. Consequently, many proponents of the New Mechanism, including Bechtel, argue that mechanistic explanation naturally falls into the ontic realm within the relational dimension of explanation.

However, caution is warranted because the prioritization of the ontic conception of explanation against Hempel's viewpoint is situated within the dimension of explanatoriness. I previously underscored the importance of representation in understanding the nature of explanation within the conceptual dimension. Herein, several questions arise. If we adopt an epistemic position in the conceptual dimension of the nature of explanation, are we compelled to align with Hempel's viewpoint in the dimension of explanatoriness once again? If not, does that imply a rejection of Hempel's ideas? Suppose we choose not to adhere to Hempel's inferential version of the epistemic account of explanatoriness. How can we maintain a non-ontic position of expla-

nation within the dimension of explanatoriness? Furthermore, how compatible are the two epistemic positions in the two dimensions - explanatoriness and the nature of explanation - with each other?

One of the most fatal shortcomings in Hempel's model is that his conditions for scientific explanation are insufficient in the relational dimension of explanatoriness. For instance, the flagpole or eclipse case serves as a counterexample to the condition of the explanatory form, which is an argument. The barometer case functions as a counterexample to the condition of the explanatory force, whether it be universal or probabilistic laws. The contraceptive pill case serves as a counterexample to the explanatory relevance condition, which is a logical necessity. Similarly, the vitamin C case is a counterexample to the condition of explanatory relevance based on high probability. These counterexamples demonstrate that arguments, laws of nature, logical necessity, and high probability alone are insufficient for scientific explanations. I pursue a way to adopt the epistemic position of *mechanistic* explanation in the relational dimension without falling into the swamp that Hempel's model confronted.

My primary strategy to advocate the epistemic view of mechanistic explanation does not involve searching for other ideal conditions of scientific *explanation*, as Hempel did. Instead, I will regard Hempel's covering law model as a pursuit of *necessary* conditions for scientific *representation*, assuming that mechanistic explanations are representations relative to pragmatic interests. In other words, Hempel's four conditions are necessary for the format of representations of mechanisms in the context of "An *agent* uses *linguistic representations* to depict *mechanisms* to *explain* the explanandum-phenomenon." If some descriptions of a mechanism fail to satisfy Hempel's conditions, then the linguistic representations of the mechanism are also deemed inadequate.

Let me begin with a question: Are all linguistic representations, such as statements, propositions, and descriptions, inadequate for depicting mechanisms? Bechtel and Abrahamsen (2005) seem to prioritize non-linguistic representations of mechanistic explanations. However, I do not intend to discuss the priority of representational forms here. Instead, I presume that linguistic and diagrammatic representations are essential modes of mechanistic explanation. Although many mechanistic explanations are diagrammatic, they never completely replace all linguistic expressions. Instead, linguistic descriptions support us in illustrating spatiotemporal processes of mechanisms, understanding detailed information, and communicating with diagrams. Most philosophers of science who advocate the New Mechanism believe that diagrammatic representations are primary modes of mechanistic models. Still, I will concentrate on linguistic representations of mechanisms.

3.1 Validity as a Necessary Condition of Linguistic Representations

As to the first condition about explanatory form, I suggest adopting Hempel's first condition for linguistic representations of mechanisms, not for qualifying or eligible provisos of scientific explanation. I never intend to advocate Hempel's model as an account of scientific explanation. I am in alliance with the mainstream to criticize Hempel's covering law model. However, I will deal with Hempel's conditions of *linguistic representations* to satisfy them. My starting point was the dimension of the nature of mechanistic explanation. If we seriously take the epistemic conception of the nature of mechanistic explanation being representations, I think Hempel's conditions help satisfy the necessary conditions for linguistic statements. In other words, all statements must be true, and an argument must be deductively valid.

In the case of protein synthesis, the ontic theorists may believe that facts, including transcription from DNA to mRNA and subsequent translation from mRNA to protein, explain how a protein is synthesized from genetic materials. However, there seems to be a wide gap between the belief that an actual mechanism exists and the observational evidence needed to justify that belief. No biologists directly observe the entire process from DNA to protein via mRNA. Strictly speaking, the mechanism of protein synthesis is unobservable. All processes of protein synthesis are inevitably decomposed and discovered separately in different laboratories. The results of experiments conducted on protein synthesis in each laboratory can be briefly summarized *verbally*. For instance, Roger Kornberg observed a specific case of transcription: an RNA polymerase synthesizes an mRNA sequence from DNA in a eukaryotic cell. Marshall Nirenberg and Heinrich Matthaei observed a particular case of translation such that a typical complex of ribosomes and additional components synthesizes a polypeptide containing only phenylalanine from a poly-uracil sequence that is artificially composed by sole uracil. We can abstractly imagine that Kornberg's mRNA and Nirenberg's poly-uracil sequence are theoretically equivalent because both are a single strand of nucleotide synthesized by an RNA polymerase in principle within a typical environment. Under this theoretical condition, the two observations can be integrated into a productively continuous process from DNA to protein via mRNA. Of course, diagrammatic representations are more intuitive to figure out detailed occurrences within both processes, but linguistic expressions are also possible as follows:

C_1. Macromolecules, including DNA, mRNA, and protein, exist in a eukaryotic cell.

C_2. Enzymes, including RNA polymerase and a complex of ribosomes, also exist in eukaryotic cells.

P_1. An RNA polymerase synthesizes a single nucleotide (mRNA) strand from DNA.

P_2. A complex of ribosomes synthesizes a primary sequence of proteins from mRNA.

C_3. mRNA's physical properties are always invariant, independent of concrete syntheses.

C_4. A transcribed mRNA is abstractly identical to a template of translation in principle.

E. The product of proteins is produced from DNA via mRNA.

The two conditions C_1 and C_2 are true on the ground that Watson & Crick's model of DNA, Wilkins & Franklin's X-ray diffraction evidence, and Kornberg & Nirenberg's experimental models. Crick's central dogma, DNA $\rightarrow$ mRNA $\rightarrow$ Protein, is filled with objects. The two premises, P_1 and P_2, are also true in Kornberg and Nirenberg's research. That is, arrows in Crick's central dogma are filled with enzymes. The additional conditions, C_3 and C_4, are essential to integrate separate transcription and translation. These additional conditions are not ontic but epistemic claims because they are based on abstractly regular patterns from empirical research findings. Without direct observations of the mechanism of protein synthesis, the explanandum-phenomenon, E, is a deductive consequence of initial conditions and premises. In other words, a deductive structure between statements is essential for linguistic representations to be useful in explaining phenomena.

Note that the mechanistic explanation above is linguistic. Premises are expressed lawfully and include empirical contents. All statements must be true either empirically or in principle. Due to two

conditions C_3 and C_4, the conclusion, E, can be derived from C_1, C_2, P_1, and P_2. Be cautious that I never argue that the mechanistic explanation satisfies Hempel's covering law model. I intend to show that mechanistic explanation can be expressed linguistically as well as diagrammatically, and that if expressed linguistically, it must suffice Hempel's condition of explanatory form. Validity is a fundamental requirement of linguistic representations to be an argument. I never deal with valid arguments as a sufficient condition of scientific explanation. My focus is not on finding sufficient conditions of scientific explanation but on finding the necessary conditions of linguistic representations. In other words, Hempel's logical condition is a *necessary instruction* of linguistic representations, not a scientific explanation. If linguistic representations are not valid, then they are absurd and non-informative in the end.

3.2 Laws as a Necessary Condition of Understanding for Explanatory Force

The ontic theorists claim that universal laws (or highly probabilistic laws) are insufficient conditions for scientific explanation. I agree. However, I emphasize that laws of nature are *necessary* to understand how activities (or component operations) are engaged with entities (or component parts) and how they are spatiotemporally organized.

According to the ontic view, only things and facts are explanatory. In the case of protein synthesis, DNA, mRNA, and proteins are relevant entities. Moreover, transcription from DNA to mRNA means that the genetic information of DNA is transmitted into that of mRNA. Translation from mRNA to protein is also a factual process that determines the types of amino acids and their order based on the genetic information of mRNA. In Crick's central dogma, all pertinent objects (DNA, mRNA, protein) are discovered, and two sequential transitions

Type	Activities
Geometrical	Shaping
Mechanical	Fitting, Colliding, Pushing, Pulling, Opening, Closing
Electrical	Attracting, Repelling
Chemical	Bonding, Breaking
Energetic	Thermodynamic
Electro-magnetic	Electrically Conducting

Table 1: Types of activities.

are also identified empirically. A question arises: Can we understand, with only those objective facts, how entities act in reactions, how activities occur, and how components are organized?

Most advocates of the New Mechanism, whether they are ontic or epistemic, believe that activities (or component operations) give explanatory forces. According to Machamer, Darden, and Craver (2000, p.14), there are four types of activities: (i) geometrical-mechanical, (ii) electro-chemical, (iii) energetic, and (iv) electromagnetic. I will classify such activities in Table 1.

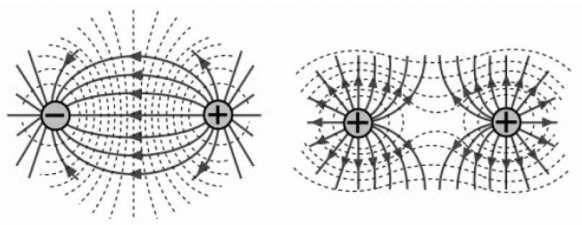

Figure 5: Attracting and repelling

As noted in Table 1, activities are referred to as metaphorical terms. However, I think that metaphorical terms are not fundamental foundations for providing explanatory forces because they must be elucidated under lower-level scientific theories. I suggest metaphorical terms gain explanatory grounds when anchored in scientific facts, laws, or theories.

For example, electrical terms are based on Coulomb's law.[11] Assume that atoms are the fundamental entity in our discussion.[12] Fig. 5 illustrates the explanatory ground of the electrical activities. The electrical field vector $\overrightarrow{E}(\overrightarrow{r})$ is represented by lines pointing in the direction of the electric field at any point; that is, the electric field vector is tangent to the electric field lines at any point. The number of lines per unit area passing through a surface perpendicular to the lines is proportional to the strength of the electric field in a given region. For a positive point charge, the lines radiate outward from a point, whereas for a negative point charge, the lines converge inward. The number of field lines leaving the positive charge equals the number of lines terminating at the negative charge. Fig. 5 illustrates the electric field lines for either two positive point charges or two negative point

[11] The potential energy, $\overrightarrow{E}(\overrightarrow{r})$, of a test charge Q is the Coulombic energy of interaction between Q and an arbitrary charge q separated by a distance $\overrightarrow{r}$ such that $\overrightarrow{F}(\overrightarrow{r}) = Q\overrightarrow{E}(\overrightarrow{r}) = Q\frac{q}{4\pi\varepsilon_0\overrightarrow{r}}$, where $\overrightarrow{E}(\overrightarrow{r})$ is the electric force and ε_0 is the permittivity constant of vacuum.

[12] This assumption is based on the atomic structure. All atoms consist of a nucleus and one or more electrons. A nucleus is electrically positive, whereas electrons are negative, so electric activities occur between them. There is attraction between the nucleus and the electrons, while there is repulsion between electrons. Repulsion also occurs between the nuclei of one atom and those of another atom adjacent to it. Electric activities are starting points to formalize entities and other activities because all materials contain a nucleus and electrons.

charges. The attraction and repulsion activities, describing the electrical interaction between charges, are mathematically formulated by Gauss' law.[13]

What about chemical activities? Those activities are also based on laws and theoretical backgrounds in physics. The two activities based on Coulomb's and Gauss's laws play a fundamental role in constructing molecules with interactions between the nucleus and the electrons of atoms. The second step in constructing molecules is to examine the *chemical* activity relating to how atoms combine in a molecule. A molecule comprises more than two atoms that share electrons. Covalent bonding is the most potent chemical bond when atoms share electrons. Molecular orbital theory (MO theory) is a widely accepted theory to explain how covalent bonding occurs. Further, the Schrödinger equation can be solved analytically only for $H^+{}_2$, but the solution cannot be applied to hetero-polyatomic molecules. In MO theory, the linear combination of atomic orbitals (LCAO) is an algebraic method to calculate the overall wavefunctions of electrons within a molecule.[14]

---

[13] The direction of electric flux is from a positive point charge to a negative point charge. In other words, electric flux, Φ_E, is a measure of the electric field vectors penetrating a given surface. It is proportional to the number of field lines that pass through a given region, $\overrightarrow{A}$, oriented perpendicular to the field, which is represented by the following equation: $\oint_S^{\square} \overrightarrow{E} \cdot d\overrightarrow{A} = \Phi_E = \overrightarrow{E}\overrightarrow{A}\cos\theta$, where a vector perpendicular to the region, $\overrightarrow{A}$, is at an angle θ concerning the field. The equation represents Gauss's law, $\nabla \cdot \overrightarrow{E} = \Phi_E = \frac{Q}{\varepsilon_0}$. Gauss's law implies that the electric flux through any closed surface is equal to the net charge inside the surface Q divided by ε_0. Gauss' law is a fundamental ground for the electrical activities.

[14] An atomic orbital is a wave function that describes the behavior of an electron within an atom. Molecular orbitals can be calculated using the available atomic orbitals within various chemical contexts. (i) Electrons supplied by the atoms are accommodated in the orbitals to achieve the lowest overall energy, adhering to the constraints of the Pauli exclusion principle, which states that no more than two electrons may occupy a single orbital, and they must be paired. (ii) If several degenerate molecular orbitals are available, electrons are added singly to each orbital before doubly occupying

Hence, laws and theoretical backgrounds provide a scientific understanding of why metaphorical terms are explanatory. I do not argue that universal laws are a sufficient condition of scientific explanation. Instead, I emphasize the necessity of laws for an intellectually ultimate understanding of the explanandum phenomenon.

3.3 Counterfactual Inference as a Necessary Condition of Explanatory Relevance

Logical necessity is not a sufficient condition for explanation. Even if an argument concludes the occurrence of a phenomenon from sentences about relevant components and their organizational features is valid, the presence of a sentence about an entity being causally inefficacious to the occurrence of the phenomenon in the premises renders the argument not a mechanistic explanation. Moreover, no matter how much an inductive argument is based on high probability in the case of most prokaryotic cells, there is no guarantee that the argument will immediately apply to the case of eukaryotic cells. For this reason, Hempel's two conditions of explanatory relevance are deemed insufficient.

Assuming that explanations are representations, I emphasize that Hempel's conditions should be considered necessary but insufficient. As noted earlier, logical necessity is crucial when explaining a phenomenon through a set of linguistic descriptions. It serves as a prereq-

---

any one orbital because that minimizes electron-electron repulsions. (iii) Hund's rule implies that if two electrons occupy different degenerate orbitals, a lower energy is obtained if they do so with parallel spins. In short, the structure of a molecule is determined by the configuration of electrons within the molecules under the minimal energetic state. All these descriptions of physicochemical backgrounds are prepared with reference to Atkins and de Paula's Physical Chemistry textbooks (see Atkins, De Paula and Friedman, 2014).

uisite for the explanatory form rather than a condition guaranteeing explanatory relevance. Within Hempel's account of explanation, there are no adequate conditions for mechanistic explanation.

Alternatively, I introduce an inference to affirm the relationship of constitutive relevance between the explanandum phenomenon and descriptions of parts and organizational features: *counterfactual conditional*.[15] All phenomena to be ontologically explained depend on their lower-level mechanisms. This ontological dependence can be linguistically reformulated by a 'because' complex sentence. To ensure explanatory relevance in the relational dimension, it must be demonstrated that the statement 'The description of the phenomenon clause (P) is true because a set of descriptions of mechanism clauses (M) is true.' The truth conditions of this sentence are provided by the counterfactual condition, stating that 'P-clause because M-clauses' is true if and only if: (i) 'P-clause' is true, (ii) all 'M-clauses' are true, and (iii) 'If it were not the case that M, it would not be the case that P' is true.[16]

A sentence 'P because M' indicates the explanatory relevance of mechanistic explanation between the explanandum phenomenon and

[15] In most cases of biological mechanisms, descriptions of the explanandum phenomenon are at a higher level than descriptions of component parts and their activities. Advocates of the New Mechanism, particularly Craver, regard explanatory relevance in mechanistic explanation as *constitutive* relevance. Although the philosophical debate remains, we will tentatively assume here that the two concepts of relevance are identical.

[16] This kind of formal investigation of the explanatory relationship based on the 'because' complex sentence has been slightly shown in Wright and Bechtel's paper in 2006. However, they did not formulate their idea at all. I will adopt the counterfactual truth conditions of the 'because' sentence as an epistemic condition of counterfactual inference for explanatory relevance. They said, "Descriptions of mechanisms are not just coincident with, or derivative from, explanations – they *are* explanations. But explanations are not merely lists of descriptions of mechanisms or sets thereof: they include inferential and simulatory operations on them. (Considerations of the semantics of the explanatory connective 'because', as well as what it is that arrow in

explanans, assuming that every phenomenon (P-clause) depends on its mechanisms (M-clauses). Suppose a description within the P-clause is false. In that case, all claims of explanatory relevance are false, as the first condition is not satisfied, regardless of the truth of the descriptions of mechanisms. It highlights that characterizing the explanandum phenomenon by determining its initial and terminal conditions is the primary cognitive activity of explanation. Similarly, if a description within the mechanism clauses is false, the 'because' sentence also becomes false, as the second condition is not satisfied, irrespective of the truth of the phenomenon description. These considerations presuppose that mechanistic explanation is a cognitive activity in the first conceptual dimension of explanation. It is important to note that some explanations can be false due to misrepresentations, not the non-existence of relevant components and their organizational features.

The third truth condition is crucial in judging the relevance between P and M descriptions. Let's assume that M is a description of a candidate for the relevant part of a mechanism. If the occurrence of the P-description is observed even when it is not the case that M ($\sim$M), then it is certain that the referent in M is irrelevant. The counterfactual conditional "If it were not M, it would have been P" is false since the P-description is true even if the M-description is false. For instance, from the molecular biologists' perspective, let me first discuss a linguistic description of M-clause: "X is a genetic intermediate between DNA and protein" (M_1). Suppose a biologist makes M_1 by filling the description with ribosomal RNA within a microsome. In that case, ribosomal RNA is not relevant because it has been proven

box-and-arrow diagrams represent, help in grasping this point.)" (Wright and Bechtel, 2007, emphasis original). I will develop their basic idea by pursuing counterfactual conditions of the 'because' sentence.

that protein synthesis occurs even if M_1 is false. On the other hand, suppose another biologist makes M_1 by filling the description with messenger RNA. In this case, the counterfactual conditional "If it were not M_1, it would have been not P" is true since protein synthesis cannot be realized without mRNA.

It should be noted that we are not suggesting here that ribosomal RNA is an entirely incorrect component in protein synthesis. Rater, We found that ribosomal RNA was not an appropriate referent in the mechanistic description, "*X* is a genetic intermediate between DNA and protein." Ribosomal RNA is the correct referent for other mechanistic descriptions, such as "*X* synthesizes polypeptides based on messenger RNA sequence" (M_2). In other words, it is not realistic to determine whether a single referent is correct or incorrect for an entire mechanism. A referent may be correct for a description in one context and may be incorrect for other descriptions in another context.[17]

Additionally, from the biochemists' point of view, let me focus on enzymatic activities, particularly RNA polymerase. The counterfactual inference is well applicable to cases of step-by-step processes. An RNA polymerase is a key entity synthesizing mRNA with DNA. When explaining the production of mRNA, ontic theorists may provide a mechanistic explanation as follows. Suppose that an explanandum-phenomenon, *P*, is the production of mRNA sequence through transcription. The emergence of mRNA is the terminal out-

[17] Reversely, the occurrence of the P-description may not be observed even if a mechanism description is true. In this case, the counterfactual condition "If it were M, it would have been ∼P" is false so that the mechanism description includes irrelevant information. Here, all true mechanism descriptions can be proven as totally irrelevant to the phenomenon because all referents and their properties in M-clauses never contribute to generate the occurrence of the phenomenon. As a result, we easily ignore all such mechanism descriptions.

come of the mechanism of RNA synthesis. The initial condition of the production is nothing of any ribonucleic acids in a cell's nucleus. However, we must assume that there are a lot of building units of the mRNA, including adenine, uracil, guanine, and cytosine. A M-clause is "mRNA sequence emerges in a cell's nucleus." The truth condition of the M-clause could be determined by empirical manipulation like centrifugal identification. In the 1960s, the existence of mRNA was proven by Brenner et al. What are the M-clauses as explanans of the P-clause? Briefly speaking, there are five statements of the M-clauses as follows:

M_3. An RNA polymerase attaches a DNA region called the promoter sequence.
M_4. The RNA polymerase unwinds the two strands of DNA.
M_5. The RNA polymerase moves along the DNA.
M_6. At that time, the RNA polymerase synthesizes individual RNA nucleotides to the growing mRNA strand based on a single strand of DNA template.
M_7. The RNA polymerase detaches from the DNA template when stopping the synthesis.

If M_6 is not true, the P-description, "A transcription is generated," may also not be true since M_6 implies the P-description. M_6 temporally depends on M_5, which also depends on M_4, which depends on M_3. M_7 indicates the end of the series from M_3 to M_6. As a result, if a sequence of successive M-descriptions is false, then the P-description as a consequence of the terminal M-description is also false. That is, if $M_3 \sim M_7$ is not true, then P is not true, too. In conclusion, M-descriptions, $M_3 \sim M_7$, are explanatory relevance to the

P-description. An inference with the counterfactual conditional is worthy of being an adequate epistemic condition to discern whether any mechanistic descriptions are relevant to the phenomenon description.

Finally, I do not argue the structural identity between mechanistic explanation and prediction. We do never adopt the demarcation problem because prediction is an explanatory virtue. Hempel's four conditions are insufficient for scientific explanations to satisfy but necessary to evaluate whether linguistic representations are adequate. No matter how logical necessity (or high probabilistic necessity) is satisfied from the premises and their conclusion, it does not guarantee that linguistic representations are scientific explanations. However, if all statements are employed to explain something, they must be logically consistent. If not so, independently of whether logical necessity is a sufficient condition to determine the eligibility of scientific explanation, all linguistic representations are not informative. Moreover, laws of nature are not sufficient conditions for scientific explanations. However, in the absence of laws of nature, it is difficult to identify the explanandum phenomenon and further understand how organizational features of mechanisms contribute to producing the occurrence of the phenomenon.

Hempel's account is epistemic because epistemic inferences, such as logical calculus or derivations, establish explanatory relevance. In contrast, Salmon's causal-mechanical account is ontic because explanatory relevance is established not by mental inferences based on logic but by actual target systems in nature, expected to bring about a phenomenon to be explained. Attempts to evade Hempel's shadow were unsuccessful in establishing the epistemic view in the relational dimension of explanatoriness in the original ontic-epistemic debate.

To advocate the epistemic conception of mechanistic explanation, I propose two epistemic ways to illuminate the explanatory

relevance between the explanandum phenomenon and its explanans: the necessity thesis of linguistic representations and the counterfactual inference. Hempel's epistemic conditions are necessary for mechanistic explanations, which are linguistic outcomes representing actual mechanisms under the assumption that linguistic descriptions express the contents of mechanistic explanation. Additionally, a counterfactual inference of the 'because' sentence helps check whether the relation between the explanandum and explanans is relevant. I illustrate this with molecular and biochemical cases. These alternative interpretations of Hempel's account of explanation in the relational dimension of explanatoriness are based on the previous consequence that the nature of explanation is a cognitive activity in the conceptual dimension.

4. On the Norms of Mechanistic Explanation: Completeness vs. Purpose-Dependence

Most philosophers have discussed the normative dimension by emphasizing the superiority or interaction between ontic and epistemic norms (Kaplan and Craver, 2011; Illari, 2013; van Eck, 2015; Sheredos, 2016). In contrast, I will focus in this Section on these two types of norms as criteria for evaluating mechanistic explanations. Many ontic theorists defend ontic norms by mentioning that ontic norms are the most powerful standard for evaluating explanations (see Craver, 2014; Povich, 2018). Participating in the ontic-epistemic debate as a standard for evaluating mechanism explanations is noteworthy since it is an issue of the normative dimension of explanation that has yet to receive much attention among epistemic advocates. However, I would

like to point out that, contrary to the wishes of ontic theorists, when they use ontic norms when evaluating mechanistic explanations, they encounter dilemmas that run counter to our common sense.

4.1 Pessimistic Induction and Demarcation of Explanation

If we adopt the idea that mechanistic explanation is a cognitive achievement to represent a mechanism linguistically or diagrammatically in the conceptual dimension, what implications does it have for normative issues? Generally, mechanists refer to representations of mechanisms as mechanism schemes. Assuming this equivalence of abstract two terms, we can say that Craver emphasizes two ontic norms of mechanistic explanation: (i) completeness and (ii) correctness. He, with Darden, defines the two terms as critical criteria to evaluate mechanism schemes (Craver and Darden, 2013, p.9): "Completeness presumes that there is a complete target mechanism in the world, and one can assess the extent to which the schema includes all and only the entities, activities, and organizational features in the target. Correctness presumes that there is a fully instantiated target mechanism in the world, and one can assess the degree of fit or mapping between items in the scheme and items in the target mechanism." As noted previously, completeness is a chief ontic goal of mechanistic explanation. Correctness is closely related to Craver & Kaplan's 3M requirement between a scheme and its target mechanism.

Note that most ontic theorists, including Craver, Kaplan, and Povich, further employ those ontic norms to demarcate good from bad explanations. Ontic theorists seem to believe that a philosophical search for any criteria to evaluate the quality of explanation is a critical task of the philosophers of science. They think of mechanism sketches (or how-possibly models) as mere conjectures of the target.

On the other hand, they appear confident that we can attain correct and complete representations of the entire target mechanism as how-actually models. They tend to regard mathematical descriptions of natural patterns as trifling or partial explanations. Hodgkin and Huxley's equations of action potential are the typical case. No matter how the equations allude to regular patterns across a membrane of a neuron quantitively, ontic theorists underestimate the explanatory values of mathematical descriptions (see Craver, 2007). They stress the significance of only qualitative descriptions (or diagrams) with causal terms within Table 1. They seem to believe that referring to relevant components and their organizational patterns is the superior prerequisite of mechanistic explanation than depicting them mathematically.

Let's delve into ontic norms by addressing the following questions: Is the ontic norm of completeness attainable? How can we determine whether completeness is achieved without prior knowledge of the target mechanism? These inquiries pose metaphysical doubts about ontic norms. Moreover, most mechanism schemes are inevitably labeled as bad explanations when completeness is employed as a criterion for evaluation. The question then arises: Should we rely on completeness to distinguish between good and bad explanations?

Firstly, is the ontic norm of completeness applicable to the evaluation of hypothetical mechanism schemes? According to Craver & Darden's definition of completeness, an agent must possess complete information about a target mechanism before employing this ontic norm. This comprehensive information includes the types of entities, activities, and their organizational features. However, completeness is an unattainable criterion when applied to practical contexts. Information about the target must be known in advance to use the completeness norm in evaluation. Yet, as demonstrated earlier, direct access to a target mechanism is not feasible. We cannot grasp informa-

tion about mechanisms all at once, and scientific instruments do not aid in perceiving the entire process of the explanandum phenomenon. Instead, we must partially grasp mosaic information about the mechanism and then piece the information together.[18] Completeness is not a tangible concept that can be achieved in one fell swoop. Instead, it is a variable concept that gradually changes in magnitude and degree as the obtained evidence increases.

Second, completeness is not an appropriate criterion for evaluating whether an explanatory representation is good or bad. This ontic constraint is a variable concept depending on how far the research has progressed. Is Crick's central dogma, DNA $\rightarrow$ RNA $\rightarrow$ Protein, a mechanism scheme? Most ontic theorists seem to answer yes because the missing link was filled with RNA. Further, this scheme opens the way to explain how proteins are synthesized from DNA. However, this mechanism scheme seems to be a bad explanation under the completeness criterion because the scheme does not include which enzymes act when RNA is transcribed from DNA and when protein is translated from RNA. Perhaps ontic theorists believe a complete explanation can be achieved by enumerating the complex processes by which RNA and protein are synthesized separately, using causal terms. Or they may believe that a complete explanation can be more easily achieved if the two syntheses are presented concisely, using diagrams.

Perhaps ontic theorists would say this is on the way to complete a mechanism description. Of course, pursuing completeness by adding unknown sub-processes in more detail than the previous mechanism descriptions is not problematic. The genuine problem arises from the

[18] Craver emphasizes the mosaic unity in neuroscience for a different reason (see Craver, 2007). He argues that mechanistic explanations in neuroscience are not reduced to the fundamental level but are unified by multilevel results of different fields.

fact that proposing a mechanistic explanation depends on additional research findings. More novel components will be continuously discovered in pursuit of a complete mechanism description. And new interactions between the components will also emerge. The additional achievements of these discoveries are typical of biological research, which is entirely natural. But do biologists always classify their explanations as good or bad based on the yardstick of completeness? That rarely happens. All of their explanations are on a *continuum*, and it is highly unrealistic to divide the sequence of achievements into good and bad explanations solely based on completeness.

Let's reconsider the case of protein synthesis, particularly examining whether the mechanism schema in Fig. 6 serves as a complete and correct model to represent the full processes of protein synthesis. Regrettably, the schema is inherently incomplete. Firstly, the discovery of reverse transcriptase in 1970 unveiled a reverse flow of genetic information from an RNA virus to a DNA provirus. Secondly, eukaryotic mRNA is not immediately ready for translation. RNA at this stage is termed precursor-mRNA and must undergo additional processing before transitioning from the nucleus to the cytoplasm as mature mRNA. These processes involve RNA capping, polyadenylation, and splicing, modifying mRNA in various ways. These modifications enable a single gene to produce more than one protein.[19] Third, the full

[19] RNA capping modifies the 5' end of the RNA transcript, which is the end that is synthesized first. RNA is capped by the addition of an atypical nucleotide, which is a guanine nucleotide containing a methyl group attached to the 5' end of RNA in an unusual way. This capping occurs after RNA polymerase II has produced about 25 RNA nucleotides long before completing transcription of the entire gene. Polyadenylation provides newly transcribed mRNA with a special structure at the 3' end. Unlike bacteria, where the 3' end of an mRNA is simply the end of a chain synthesized by RNA polymerase, the 3' end of a eukaryotic mRNA is first trimmed by an enzyme that cleaves the RNA chain at a specific sequence of nucleotides. The transcript is then finished by a second enzyme that adds a series of repeating adenine nucleotides

mRNA sequence after transcription is not the genetic code for synthesizing proteins. Precursor RNAs synthesized by an RNA polymerase in the nucleus must be edited. Particularly, the splicing mechanism in eukaryotes is essential; non-genetic portions within the precursor RNAs, the so-called introns, must be deleted, and genetic portions, the so-called exons, must be ligated. Splicing implies that both synthesizing processes, transcription, and translation, are not the only processes within protein synthesis. In other words, Fig. 6 is never a complete model of protein synthesis.

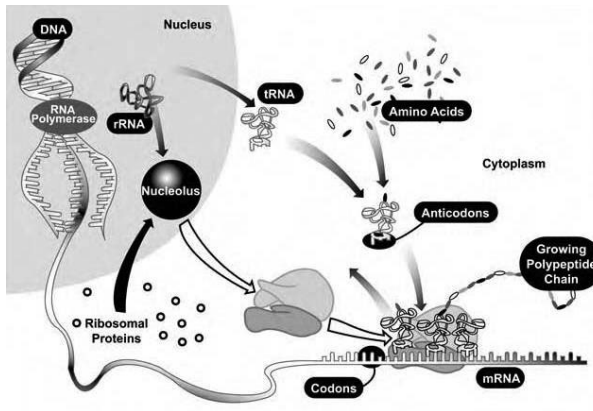

Figure 6: Mechanistic schema for protein synthesis (Darden, 2006, p.117)

---

to the cleavage ends. These poly-A tails are typically hundreds of nucleotides long. Splicing removes introns from precursor-mRNAs. Introns are unnecessary regions for encoding proteins. The rest of the mRNA consists only of regions called exons that are needed to synthesize the protein. Editing is the process of changing some nucleotides in mRNA. For example, a human protein called APOB, which helps transport lipids in the blood, has two different forms due to editing. One form is smaller than the other because editing adds an earlier stop signal to the mRNA. RNA editing processes such as these occur all the time in real cells. In order for the mechanism by which RNA is synthesized from DNA to meet the criteria for completeness, an explanation including these processes is required.

Maybe ontic theorists complain that those two cases, retrovirus and splicing reactions, do not refute their ontic norms to evaluate mechanism schemes because they are not counterexamples but evidence of their ontic criteria. The more processes are filled with the previous scheme, the more complete and correct explanations are achieved. Suppose that it is okay to accept the ontic theorists' claim such that the mechanism scheme, including (i) Watson's central dogma, (ii) biochemists' discoveries of enzymatic reactions from DNA to protein, (iii) a special case of genetic information flow in a retrovirus, and (iv) splicing mechanism in eukaryotes, is the complete and correct model of protein synthesis. Regrettably, again, Ochoa's discovery misrepresents RNA synthesis because he never found RNA polymerase but polynucleotide phosphorylase. Roger Kornberg successfully analyzes the structural features of RNA polymerase and its functions in eukaryotes in the early twentieth century. In other words, all mechanistic schemes in the absence of Roger Kornberg's achievements are certainly incomplete and incorrect.

Again, ontic theorists would complain that the system of mechanisms, up to and including Roger Kornberg's discovery, is a complete and accurate model of protein synthesis. The problem with answering this way is that ontic theorists can never seem to qualify themselves as tools for evaluating mechanism schemes other than holding their ground. Whenever a discovery emerges, the existing mechanism scheme becomes a false description, and a more complete and accurate explanation is achieved. As biologists' research accumulates, situations like this will occur more frequently in the future. It is clear, then, that any mechanism scheme currently believed to be complete and correct will one day become a bad explanation. The ontic norms applied to evaluating explanations inevitably allow for such *pessimistic* prospects in situations where theory propagation is

the norm. In this scenario, we must reluctantly conclude that achieving a complete and precise mechanism is an elusive goal, both at present and in the foreseeable future.

This conclusion is analogous to the conclusion of the pessimistic induction argument against scientific realism. The pessimistic induction argument shows that if past successful and truly accepted theories were false, we fail to believe the realist's claim that our currently successful theories are true because current theories will also be false as past successful theories were confronted. Similarly, if one adopts the realistic point of view of the existence of a mechanism and admits that mechanism schemes are continuously developed toward more complete and more correct models, then it is skeptical to attain the complete and correct model of the mechanism.

Advocates of the ontic conception of explanation believe (i) that we can one day achieve complete and accurate mechanism descriptions and (ii) we must assess mechanism schemes under the ontic norms. However, suppose the two beliefs juxtapose with each other. In that case, we are inevitably unable to obtain complete and accurate mechanism descriptions, or we are forced to brand almost all known mechanism descriptions as bad. Ontic theorists accept that mechanistic models must be developed from how-possibly to how-actually models. Mechanism sketches become just how-possibly models when acquiring more complete and correct models. By filling in intermediate components between the initial and terminal conditions, mechanism sketches must be developed into more complete and correct schemes as how-actually models. Suppose one realizes the acquisition of a complete and correct model of the explanandum phenomenon. In that case, the how-actually model is a good explanation, and all the early sketchy how-possibly models become bad explanations. Note

that both ontic norms of completeness and correctness are criteria for evaluating mechanistic schemes from a *realistic* point of view. Craver and Darden reveal their realistic stance by saying that:

> At the most abstract level, mechanism schemas are evaluated in terms of their depth, their completeness, and their correctness. We discuss these dimensions of evaluation and some tests for evaluating a schema along these dimensions. Our orientation toward these questions might be described as a *garden-variety realism* moderated by a sensible pragmatism (Craver and Darden, 2013, emphasis added).

Craver and Darden (2013, p.94) emphasize the close relationship between an ontic norm such as correctness and realism: "The difference between how-possibly, how-plausibly, and how-actually a mechanism works is a difference in correctness. Mechanists are garden-variety realists about such things: the goal is to describe correctly enough (to model or mirror more or less accurately) the relevant aspects of the mechanism under investigation." It is taken for granted that ontic norms are realistic criteria because they are based on the external existence of relevant entities and activities of the target mechanisms.

A problem of ontic norms as criteria for evaluating mechanism schemes is that all past achievements of mechanism schemes are just bad and provisional explanations. In other words, this decision is so hasty that it raises the problem of treating all existing scientific achievements as useless and bad things. But such a judgment is too extreme. Is the discovery that genes are passed from DNA to mRNA to proteins a bad achievement? Are all the diagrams missing the splicing process a bad diagram of how proteins are synthesized? Are all of Roger Kornberg's schemes where RNA polymerase is not named or omitted bad descriptions? No matter how imperfect or inaccurate these achievements may ultimately be, they must be our historically

significant assets. It is very questionable whether these achievements are appropriately treated as useless or bad simply because they are not complete and accurate mechanism schematics.

To summarize, ontic proponents may accept that the mechanism of protein synthesis, like the diagram of Fig. 6, explains the production of the primary sequences of proteins in molecular biology. Is this mechanism complete? If they think this mechanism is complete, this judgment is false because the splicing mechanism is ignored. If they think this mechanism is incomplete, then ontic norms seem sterile to distinguish how-actual from how-possible models. Ontic theorists believe that ontic norms such as correctness and completeness play a role in demarcating good from bad explanations. Unfortunately, as long as they continue to hold on to this belief, ontic theorists end up labeling a mechanistic scheme, considered a very important biological achievement, as a 'bad' explanation. This tragic situation has resulted from adhering to a particular philosophical position, the ontic criteria for evaluating mechanism schemes. Regardless of whether the splicing mechanism was discovered, Fig. 6 is still a good representation that explains a fairly important phenomenon in protein synthesis.

4.2 Ontic Norms as Methodological Instructions

Suppose ontic norms such as completeness are difficult to achieve, and it is problematic to classify incomplete mechanism schemes into bad explanations according to these norms. What role do these norms play? Suppose we need not seriously consider mechanism schemes to be tagged as good or bad. Ontic norms can motivate researchers to provide more advanced explanations than previous explanations of mechanisms. As noted, mechanistic explanation relies on mech-

anistic inquiries. Completeness plays an instruction in developing existing schemes by filling unidentified processes or illuminating known entities' uncovered properties.

Again, for instance, the self-splicing mechanism in eukaryotes was discovered after most processes within the mechanism of protein synthesis were known in the 1980s. When discovering the structure of DNA in 1953, Watson and Crick never acknowledged the two distinctions, exons and introns, within nucleic acids. Exons are portions that include genetic information, whereas introns do not. So, introns must be removed before a ribosome synthesizes proteins based on the genetic sequences of mRNA. Thomas Cech discovered the splicing mechanisms in *Tetrahymena* (see Fig. 7) (Zaug, Grabowski and Cech, 1983). Due to the discovery of splicing processes, understanding of protein synthesis has increased. Moreover, Cech discovered a new property of RNA. Until the 1980s, most biochemists believed that all enzymes are proteins. However, while investigating the splicing mechanism, Cech was also aware of the enzymatic property of RNA molecules. That is, Cech shed light on a novel fact that biological reactions from pre-rRNA to spliced rRNA occur in the absence of enzymes (see Fig. 7). Cech (1986) proved it experimentally so that he won the Nobel Prize in 1989. Cech's discovery of ribozyme, a compound of RNA & enzyme, contributes to making more complete lists of referents of enzymes in the world. Cech's achievements are totally compatible with the previous outcomes. Biologists investigate their research to explain biological phenomena at the molecular level more completely. Ontic norms are methodological instructions in mechanism inquiries.[20]

[20] Although I only focus on completeness here, correctness is also an important ontic norm in mechanism inquiries. Without referring to referents as correct entities in a mechanism, we cannot explain the explanandum phenomenon successfully.

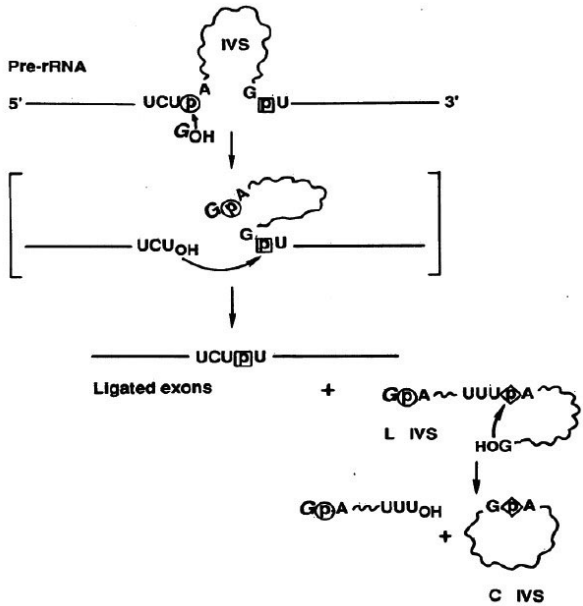

Figure 7: Cech's self-splicing mechanism (Zaug, Grabowski and Cech, 1983, p.582)

Finally, I would like to emphasize that whether an explanation is good or bad is not determined by any criteria but rather by its appropriateness or relevance to the topic determined in the given context. Ontic norms are instructions of mechanistic inquiries to discover patterns, not criteria to judge whether a description is good. Ontic norms are self-contradictory because scientific achievements are thrown away as bad explanations. Completeness is an infeasible Utopian standard to be realized in science. It rather is realizable following the explainer's interests. A philosophical distinction on explanatory normativity based on ontic norms seems a pseudo-project.

Interestingly, Craver and Darden also agree on the pragmatic aspect of mechanistic explanation by acknowledging that ontic norms are compatible with agents' interests or given contexts by saying:

> In speaking this way, we also intend to allow that a schema can be complete and correct enough for the *purposes* at hand without being fully complete or correct. One can acknowledge the ideals of completeness and correctness for descriptions of mechanisms while, at the same time, recognizing that science often traffics in *idealized* and *incomplete* schemas (Craver and Darden, 2013, emphasis added).

The pragmatic point of view was shown in the discussion of the conceptual dimension of the nature of explanation. Remind that the nature of mechanistic explanation is the tertiary relation among the representer, targets, and agent's goal in a given context. When judging whether a mechanistic explanation is successful, the explanation must be an adequate answer to a question about why the explanandum phenomenon happens. According to van Fraassen (1980), an explanation is an answer to a why-question. Here, I do not apply van Fraassen's theory of explanation to mechanistic explanation. Still, I emphasize that mechanistic explanations are evaluated by whether adequate representations explain the explanandum phenomenon under the agent's interest.

The same phenomenon can be explained differently depending on what aspects, to some degree, explain the phenomenon. For example, protein synthesis can be explained diversely concerning researchers' interests. Molecular biologists Watson and Crick were interested in the genetic flow from DNA to protein. The number of bases within nucleic acids is four, but the number of amino acids is twenty. So, the order of the bases in DNA determines what kinds of nucleic acids are in protein. By guessing the existence of a single linear sequence of

nucleic acids, or messenger RNA, they imagined how sequences of two or three bases correspond to 20 different types of amino acids. As a result, the simple mechanism scheme, DNA→RNA→Protein, explains a temporal order among essential entities within the mechanism. By contrast, biochemists Zamecnik and Hoagland focused on the energy flow in the form of ATP when amino acids are activated in synthesizing the peptide bond. Their chemical interest in protein synthesis led to the discovery of transfer RNA, which links the genetic code to amino acids. As a result, Zamecnik's diagram in Fig. 1 explains how biomolecules interact with each other to synthesize the peptide bond.

Further, another biochemist, Cech, was interested in what enzymes mediate the splicing reactions of precursor ribosomal RNA before mRNA moves out of a nucleus to synthesize protein. As a result, Cech's diagram in Fig. 7 explains how splicing reactions, including cleavage of pre-rRNA, ligation, and cyclization, occur without enzymatic proteins. Besides them, numerous molecular biologists and biochemists have proposed models representing various aspects of the protein synthesis process, depending on their respective fields of interest. For the same protein synthesis process, molecular biologists focus on genetic information, and biochemists focus on energy equivalence. Even among the same biochemists, someone discovers the energy components needed for chemical reactions, and someone else discovers new functions of RNA. Likewise, the topic, scope, and purpose of explaining the protein synthesis process vary depending on the researcher's interests. The important thing is that all of them are significant achievements in biological sciences, even if each incompletely explains the full processes of protein synthesis. Whether a mechanistic explanation is good or not depends upon the issue of *how much an answer to the question is attained*.

As noted, Craver, Bechtel, and Wright adopt Salmon's distinction between ontic and epistemic. However, they disagree about the nature of mechanistic explanations. Craver (2006; 2007; 2014) argues that the mechanistic explanation is also ontic, whereas Bechtel and Wright (Wright and Bechtel, 2007; Wright, 2012; Wright, 2015) claim it is epistemic. Both agree that the mechanistic explanation should be based on something other than Hempel's account of scientific explanation. More precisely, they claim that a mechanistic explanation does not include the laws of nature, and a linguistic form of argument does not constrain the explanation units. Based on a consensus of explanatory relevance in the first dimension, the three philosophers disagree on what mechanistic explanation is in the second dimension. Craver over-concentrates on the relational dimensions of mechanistic explanation in line with the first dimension of explanatoriness. On the contrary, Bechtel and Wright over-concentrate on the conceptual dimension of mechanistic explanation, such cognitive procedures independently from the first dimension. Remember that Craver rejects cognitive processes to identify causal factors of explanandum phenomena. As Illari mentions, Craver wants to proclaim an ontic constraint to examine mechanistic explanation in terms of mechanisms.

I restate the first and second dimensions to show that the normative dimension can depend on the two dimensions. Ontic norms of mechanistic explanation originate from the first relational dimension on which all mechanists agree. Epistemic norms of mechanistic explanation stem from the second conceptual dimension of the nature of explanation. I am emphasizing the non-existence of disagreement among ontic and epistemic theorists about a claim that mechanistic explanation is an achievement from cognitive performances in practice. What is important is that epistemic norms are also instructions in

mechanistic inquiries. This point has been similarly pointed out by many philosophers of science (Illari, 2013; van Eck, 2015; Sheredos, 2016; Kästner and Haueis, 2021).

Nonetheless, I cannot entirely agree with the defenders of the epistemic priority of the mechanistic explanation as they maintain that there are separate epistemic constraints, such as generality or systematicity, which are either independent of or additional to the ontic constraint (van Eck, 2015; Sheredos, 2016). The goal of understanding entities, activities, and their organizations is not independent of the ontic constraint, according to which we should discover the causal factors to explain a phenomenon. Other epistemic norms for the mechanistic explanation may be helpful only after discovering all causal factors. Without the ontic constraint, any epistemic norms independently do not support the explanatory relevance of the mechanistic explanation. Discoveries, encompassing not only the components but also their organizational features within a mechanism, serve as primary methodological guidance in biological sciences. Thus, ontic norms are not less worthy than any epistemic norms.

Are ontic and epistemic norms equally valuable? Illari (2013) and Kastner & Haueis (2021) appear to answer this question affirmatively, a stance with which I concur. However, my focus extends beyond the relative significance of these norms. I explored whether ontic norms warrant consideration in evaluating mechanistic explanations. I demonstrated that ontic norms alone are insufficient for distinguishing between good and bad explanations. Consequently, both ontic and epistemic norms play essential roles, as neither norm conflicts independently of the demarcation problem concerning the quality of mechanistic explanations.

5. Conclusion

The paper analyzes ontic and epistemic debates in scientific explanation through the three-dimensional interpretation. Each debate dispute concerns a particular issue: (1) explanatory formal framework, explanatory force, explanatory relevance between explanans and an explanandum; (2) nature of mechanistic explanation; and (3) normative constraints. This dimensional approach does not simply enumerate or give three names to different issues but is helpful to compare with participants' viewpoints in this debate. In addition, it provides a practical framework to comprehend several philosophical perspectives in scientific explanation. Furthermore, those three dimensions have their philosophical issues but are also interconnected, so it can be helpful to clarify how to reconcile different normative constraints.

Recent ontic-epistemic debates are displayed among proponents of the New Mechanism. The mechanistic explanation is a widespread explanatory practice in biological sciences. A consensus exists that the explanatory relevance between the explanans and the explanandum is a fundamental ontic constraint for a mechanistic explanation. However, some philosophers have either focused on the epistemic nature of the mechanistic explanation or insisted on the epistemic norms or constraints over the ontic constraints. I have shown that mechanistic explanations are better associated with the epistemic conception than with the ontic conception of explanation. I emphasized the epistemic nature of mechanistic explanations by highlighting their strong dependence on the activities that represent the mechanism based on van Fraassen's pragmatic viewpoint. Contrary to stereotypical views held by New Mechanism advocates, I proposed the linkage of mechanistic explanations to epistemic aspects such as logical validity, lawful grounds, and counterfactual inference. This is achieved by exploring

the necessary conditions in linguistic representations of these mechanistic explanations. Further, I addressed the philosophical issue of how mechanistic explanations should be evaluated.

Unlike many philosophers in the New Mechanism who stubbornly reject Hempel's model, I suggested to view Hempel's conditions as necessary conditions for linguistic representation rather than sufficient conditions for scientific explanation. By noting that the usage of the ontic norms as criteria for distinguishing between good and bad explanations makes the mistake of dismissing many existing scientific achievements as 'bad' explanations, I assert the compatibility of mechanistic explanations with pragmatic aspects of explanation. Through this, I carefully tried to reveal that the mechanistic explanation is significantly more related to the epistemic position than the ontic one.

Bibliography

Atkins, P.W., De Paula, J. and Friedman, R., 2014. *Physical chemistry: quanta, matter, and change*. Second edition. Oxford, United Kingdom: Oxford University Press.

Bechtel, W., 2008. *Mental Mechanisms: Philosophical Perspectives on Cognitive Neuroscience*. New York: Routledge.

Bechtel, W. and Abrahamsen, A., 2005. Explanation: a mechanist alternative. *Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences* [Online], 36(2), pp.421–441. https://doi.org/10.1016/j.shpsc.2005.03.010.

Bechtel, W. and Richardson, R.C., 2010. *Discovering complexity: decomposition and localization as strategies in scientific research*. Cambridge, MA: MIT Press.

Bokulich, A., 2016. Fiction As a Vehicle for Truth: Moving Beyond the Ontic Conception. *The Monist* [Online], 99(3), pp.260–279. https://doi.org/10.1093/monist/onw004.

Carnap, R., 1950. *Logical Foundations of Probability*. Chicago: University of Chicago Press; Routlege & Kegan Paul.

Cech, S., 1986. A model for the RNA-catalyzed replication of RNA. *Proceedings of the National Academy of Sciences* [Online], 83(12), pp.4360–4363. https://doi.org/10.1073/pnas.83.12.4360.

Coffa, J.A., 1973. *Foundations of Inductive Explanation*. PhD Thesis. University of Pittsburgh.

Craver, C.F., 2006. When mechanistic models explain. *Synthese* [Online], 153(3), pp.355–376. https://doi.org/10.1007/s11229-006-9097-x.

Craver, C.F., 2007. *Explaining the Brain: Mechanisms and the Mosaic Unity of Neuroscience*. Oxford: Oxford University Press.

Craver, C.F., 2014. The Ontic Account of Scientific Explanation. In: M.I. Kaiser, O.R. Scholz, D. Plenge and A. Hüttemann, eds. *Explanation in the Special Sciences: The Case of Biology and History* [Online]. Vol. 367, *Synthese Library*. Dordrecht: Springer Netherlands, pp.27–52. https://doi.org/10.1007/978-94-007-7563-3_2.

Craver, C.F. and Darden, L., 2013. *In Search of Mechanisms: Discoveries across the Life Sciences* [Online]. Chicago, IL: University of Chicago Press. Available at: <https://press.uchicago.edu/ucp/books/book/chicago/I/bo16123713.html> [visited on 29 September 2023].

Darden, L., 2006. *Reasoning in biological discoveries: essays on mechanisms, interfield relations, and anomaly resolution*, *Cambridge studies in philosophy and biology*. Cambridge: Cambridge University Press.

van Eck, D., 2015. Reconciling Ontic and Epistemic Constraints on Mechanistic Explanation, Epistemically. *Axiomathes* [Online], 25, pp.5–22. https://doi.org/10.1007/s10516-014-9243-x.

Ellis, B., 1956. On the relation of explanation to description. *Mind* [Online], 65(260), pp.498–506. https://doi.org/10.1093/mind/65.1.498.

van Fraassen, B.C., 1980. *The Scientific Image*, *Clarendon library of logic and philosophy*. Oxford; New York: Clarendon Press; Oxford University Press.

van Fraassen, B.C., 2008. *Scientific Representation: Paradoxes of Perspective* [Online]. 1st ed. Oxford University PressOxford. https://doi.org/10.1093/acprof:oso/9780199278220.001.0001.

Giere, R.N., 2006. *Scientific Perspectivism*. Chicago, IL: University of Chicago Press.

Hempel, C.G. and Oppenheim, P., 1948. Studies in the logic of explanation. *Philosophy of Science* [Online], 15(2), pp.135–175. Available at: <https://www.jstor.org/stable/185169> [visited on 23 August 2019].

Hempel, C.G., 1965. *Aspects of Scientific Explanation and Other Essays in the Philosophy of Science*. New York: The Free Press.

Hoagland, M.B. et al., 1958. A soluble ribonucleic acid intermediate in protein synthesis. *The Journal of Biological Chemistry*, 231(1), pp.241–257.

Holley, R.W. et al., 1965. STRUCTURE OF A RIBONUCLEIC ACID. *Science* [Online], 147(3664), pp.1462–1465. https://doi.org/10.1126/science.147.3664.1462.

Illari, P., 2013. Mechanistic Explanation: Integrating the Ontic and Epistemic. *Erkenntnis* [Online], 78(S2), pp.237–255. https://doi.org/10.1007/s10670-013-9511-y.

Jacob, F. and Monod, J., 1961. Genetic regulatory mechanisms in the synthesis of proteins. *Journal of Molecular Biology* [Online], 3(3), pp.318–356. https://doi.org/10.1016/S0022-2836(61)80072-7.

Judson, H., 2013. *The Eighth Day of Creation Makers of the Revolution in Biology* [Online]. New York: Cold Spring Harbor Laboratory Press. Available at: <https://www.academia.edu/32355053/Horace_Judson_THE_EIGHTH_DAY_OF_CREATION_Makers_of_the_Revolution_in_Biology> [visited on 8 December 2023].

Kaplan, D.M. and Craver, C.F., 2011. The Explanatory Force of Dynamical and Mathematical Models in Neuroscience: A Mechanistic Perspective. *Philosophy of Science* [Online], 78(4), pp.601–627. https://doi.org/10.1086/661755.

Kästner, L. and Haueis, P., 2021. Discovering Patterns: On the Norms of Mechanistic Inquiry. *Erkenntnis* [Online], 86(6), pp.1635–1660. https://doi.org/10.1007/s10670-019-00174-7.

Machamer, P., Darden, L. and Craver, C.F., 2000. Thinking about Mechanisms. *Philosophy of Science* [Online], 67(1), pp.1–25. https://doi.org/10.1086/392759.

Pardee, A.B., Jacob, F. and Monod, J., 1959. The genetic control and cytoplasmic expression of "Inducibility" in the synthesis of β-galactosidase by E. coli. *Journal of Molecular Biology* [Online], 1(2), pp.165–178. https://doi.org/10.1016/S0022-2836(59)80045-0.

Povich, M., 2018. Minimal Models and the Generalized Ontic Conception of Scientific Explanation. *The British Journal for the Philosophy of Science* [Online], 69(1), pp.117–137. https://doi.org/10.1093/bjps/axw019.

Rottman, F. and Nirenberg, M., 1966. RNA codons and protein synthesis: XI. Template activity of modified RNA codons. *Journal of Molecular Biology* [Online], 21(3), pp.555–570. https://doi.org/10.1016/0022-2836(66)90027-1.

Salmon, W.C., 1971. *Statistical explanation & statistical relevance*, *Pitt paperback 69*. Pittsburgh: University of Pittsburgh Press.

Salmon, W.C., 1984. *Scientific Explanation and the Causal Structure of the World* [Online]. Princeton: Princeton University Press. Available at: <https://press.princeton.edu/books/paperback/9780691101705/scientific-explanation-and-the-causal-structure-of-the-world> [visited on 27 October 2023].

Salmon, W.C., 1989. Four decades of scientific explanation. In: P. Kitcher and W.C. Salmon, eds. *Scientific Explanation* [Online], *Minnesota studies in the philosophy of science*, 13. Minneapolis: University of Minnesota Press, pp.3–219. Available at: <http://conservancy.umn.edu/handle/11299/185700> [visited on 4 October 2023].

Sheredos, B., 2016. Re-reconciling the Epistemic and Ontic Views of Explanation (Or, Why the Ontic View Cannot Support Norms of Generality). *Erkenntnis* [Online], 81(5), pp.919–949. https://doi.org/10.1007/s10670-015-9775-5.

Wright, C., 2015. The ontic conception of scientific explanation. *Studies in History and Philosophy of Science Part A* [Online], 54, pp.20–30. https://doi.org/10.1016/j.shpsa.2015.06.001.

Wright, C. and Bechtel, W., 2007. Mechanisms and psychological explanation. In: P. Thagard, ed. *Philosophy of Psychology and Cognitive Science* [Online]. Elsevier, pp.31–79. https://doi.org/10.1016/B978-044451540-7/50019-0.

Wright, C. and van Eck, D., 2018. Ontic Explanation Is either Ontic or Explanatory, but Not Both. *Ergo, an Open Access Journal of Philosophy* [Online], 5(38). https://doi.org/10.3998/ergo.12405314.0005.038.

Wright, C.D., 2012. Mechanistic explanation without the ontic conception. *European Journal for Philosophy of Science* [Online], 2(3), pp.375–394. https://doi.org/10.1007/s13194-012-0048-8.

Zaug, A.J., Grabowski, P.J. and Cech, T.R., 1983. Autocatalytic cyclization of an excised intervening sequence RNA is a cleavage–ligation reaction. *Nature* [Online], 301(5901), pp.578–583. https://doi.org/10.1038/301578a0.

Dimensions of Explanation

Eric Hochstein
University of Victoria, Canada

Abstract

Some argue that the term "explanation" in science is ambiguous, referring to at least three distinct concepts: a communicative concept, a representational concept, and an ontic concept. Each is defined in a different way with its own sets of norms and goals, and each of which can apply in contexts where the others do not. In this paper, I argue that such a view is false. Instead, I propose that a scientific explanation is a complex entity that can always be analyzed along a communicative dimension, a representational dimension, and an ontic dimension. But all three are always present within scientific explanations. I highlight what such an account looks like, and the potential problems it faces (namely that a single explanation can appear to have incompatible sets of norms and goals that govern it). I propose a solution to this problem and demonstrate how this account can help to dissolve current disputes in philosophy of science regarding debates between epistemic and ontic accounts of mechanistic explanations in the life sciences.

Keywords

evaluative dimension, communicative concept of explanation, representational concept of explanation, ontic concept of explanation; mechanistic explanation.

At its metaphysical core, what exactly is a scientific explanation? Is it a communicative act whereby we make some phenomenon intelligible or understandable to an audience (e.g. Nancy explains the

Philosophical Problems in Science (Zagadnienia Filozoficzne w Nauce)
No 74 (2023), pp. 113–154

orbits of the planets to John)? Is it a type of theory, model, or representation (e.g. *The theory of evolution* explains why elephants have long trunks. Einstein's *General Theory of Relativity* explains why light bends when it passes a large gravitational body)? Or is it a collection of entities and relations out in the world (e.g. The car's velocity and the cut break line explains why the car crashed into the storefront)? Historically, philosophers have disagreed about which of these provides the best metaphysical account of what a scientific explanation is. For example, Peter Achinstein insists that "the illocutionary concept of explaining is fundamental and that other explaining concepts are explicable by reference to this" (1984, p.22). Conversely, Ruben insists that "explanations work only in virtue of the determinative relations that exist in the world" (Ruben, 1990, p.231).

In recent years, some have suggested that perhaps there are simply distinct concepts of "explanation" at work. Each is employed in different contexts within scientific practice, but we must be cautious not to equivocate between them. This idea has strong support. The goals and norms that seem to govern the various concepts of explanation appear to be different, and indeed can even be at odds with one another. Moreover, it seems we can often have one concept of explanation apply in a context where the others are not applicable at all. All this suggests that we may simply mean different things by the word "explanation" in different scientific contexts.

Despite the intuitive strength of such a position, in this paper I argue that this is not the case. Instead, we should think of a scientific explanation as an extremely complex and multifaceted entity. Every explanation can be analyzed along different dimensions. When each dimension of explanation is considered in isolation of the others, it provides a distorted account of what explanations are, and how they

function. But understanding how these different dimensions constitute one and the same explanation can help to clarify current disputes in the philosophy of science.

In order to make this argument, I start in section 1 with an account of why it is plausible to think that there are at least three distinct concepts or definitions of explanation: explanation defined as a *communicative* act (hereafter the "communicative" concept of explanation), explanation defined as an appropriate kind of model or theory (hereafter the "representational" concept of explanation), and explanation defined in terms of ontic structures, causes, and relations in the world that produce and sustain the phenomenon in question (hereafter the "ontic" concept of explanation). In section 2, I demonstrate why we should not consider these as distinct concepts of explanation after all, and why explanation always requires the interaction of all three elements. In section 3, I highlight why this shift from "concepts" of explanations to "dimensions" of explanation is not a trivial one, and explore how this new account can shed light on debates in the philosophy of science. Lastly, in section 4, I highlight the complications that this new account faces, and how they might be addressed.

1. Different Concepts of Scientific Explanation

An important insight that some have emphasized in recent years is that the term "explanation" may itself be ambiguous, denoting entirely distinct concepts. Waskan et al. (2014), for instance, argue that "'Explanation' appears to be ambiguous between a representational-artifact, an objective, and a doxastic sense" (Waskan et al., 2014, p.3090). Meanwhile, Craver (2014, p.29) argues that we should "disambiguate four ways of talking about explanation: as a communicative

act, as a representation or text, as a cognitive act, and as an objective structure". Likewise, Gilpin et al. (2022, p.3) argue that "the term 'explanation' is a classic example of what Marvin Minsky referred to as suitcase words: words that contain multiple meanings which are interpreted in different ways for different people in different contexts".

For the purposes of this paper, I want to focus on 3 different concepts of explanation, what I will call the "communicative" concept, the "ontic" concept, and the "representational" concept.[1] To begin, consider the communicative concept of explanation. This concept defines explanation as a social activity we engage in whereby we try to make some phenomenon understandable or intelligible to a given audience. As Achinstein (1983, pp.16–17) puts it, "S explains q by uttering u only if S utters u with the intention that his utterance of u render q understandable". This definition of explanation can also be seen in Craver's example of Jon, a professor trying to *explain* the action potential of the neuron to a classroom full of students (Craver, 2014, p.30). Similarly, Andrea Woody describes the Communicative concept of explanation in the following way:

> There is always, at least implicitly, a request (question) and a response, and these are typically negotiated or shared among individuals or groups. Explanations frequently pass between individuals or groups with different levels of expertise. A person requesting an explanation often is not in a good position

[1] This is not intended to be an exhaustive list, and other concepts of explanation may well be worth including. For instance, both Waskan et al. and Craver acknowledge a *psychological* or *cognitive* concept of explanation. In this paper, I collapse the "communicative" and "cognitive/doxastic" sense of explanation into one (since the goal of the communicative concept of explanation is taken to be providing *understanding* to an audience, which is itself a psychological/cognitive phenomenon). That being said, if one wishes to differentiate these then the argument in this paper will still go through. For the sake of brevity, however, I will focus on these three concepts of explanation.

> to judge the adequacy of the response, and must rely on the authority of the responder to accept the explanation as legitimate (Woody, 2015, p.81).

When thought of in this way, we can view explanation as having particular goals and norms that dictate its proper application. In Craver's example of Jon giving a lecture to a classroom of students, he tells us that if all goes right, then "the audience comes to understand how action potentials are produced." (Craver, 2014, p.30). Or consider Achinstein's definition above in which an explanation involves an utterance from a speaker *with the intention that their utterance renders some phenomenon understandable*. In this respect, we can see *understanding* or *intelligibility* as an essential goal of explanation, and the norms of good explanation can be thought of in terms of the norms of good pedagogy.

The communicative concept of explanation can be contrasted with another: the ontic concept of explanation. This concept of explanation is most commonly attributed to José Alberto Coffa (1974), but has been advocated for by many others since (1989; 2000; 2006; 2014; 2011; 2011; 2011; 2018; 2020). Salmon describes the ontic concept of explanation as follows:

> Proponents of this conception can speak in either of two ways about the relationship between explanations and the world. First, one can say that explanations exist in the world. The explanation of some fact is whatever produced it or brought it about. [. . .] [I]t seems entirely appropriate to say such things as the gravitational attraction of the moon explains the tides, or the drop in temperature explains the bursting of the pipes. The gravitational attraction and the drop in temperature are out there in the physical world; they are neither linguistic entities, nor abstract entities. Second the advocate of the ontic con-

> ception can say that an explanation is something—consisting of sentences or propositions- that reports such facts (Salmon, 1989, p.86).

The idea is that a good explanation must identify the appropriate *causes, structures, or features out in the world* responsible for the occurrence of the explanandum phenomenon in order to be genuinely explanatory. To borrow an intuitive example from Craver & Kaplan (2020, pp.299–300), the explanation for why sea levels are rising is because of *global warming*. Note, the explanation of sea levels rising is not *my saying the words* "global warming" to an audience, nor is it a model or a theory of global warming by itself. What explains the sea levels rising out in the world is the actual warming of the planet. A scientific model of global warming is thought to be explanatory in the ontic sense in virtue of identifying the relevant dependencies in nature (i.e. the changing climate) which genuinely produces and thus explains the rising sea levels.

While many think that the ontic concept of explanation tends to presuppose a largely mechanistic view of explanation (i.e. that good explanations must identify *ontic mechanisms*), this need not be so and may include other kinds of entities/relations as well. Craver and Povich, for example, claim that "we adopt a more inclusive understanding of the ontic that embraces any natural regularity, e.g., statistical relevance, natural laws, or contingent compositional relations might also figure fundamentally in explanation" (Craver and Povich, 2017, p.32; see also: Povich and Craver, 2018). If one adopts this ontic definition of explanation, then there are different goals and norms that present themselves. The goals associated with this definition of explanation are tied to things like identifying the relevant or appropriate natural regularities, laws, mechanisms, or dependencies out in the world that produce or sustain the phenomenon.

Traditional explanatory virtues such as *precision* and *depth* are often associated with this definition of explanation, as the more precise our descriptions of the ontic structures, causes, and regularities becomes, the deeper the explanation becomes. This idea is explicitly defended by Mackonis, who claims that "[explanation] H1 is *deeper* than H2 if H1 explicates a causal-nomological mechanism that produces the abductive trigger and H2 does not" (my emphasis), and that H1 is deeper than H2 if the mechanism posited by H1 is more "specific, precise, [and] fundamental" than one posited by H2 (Mackonis, 2013, p.985). Others have likewise associated the explanatory virtues of depth and precision with the ontic definition of explanation

According to this definition of explanation, an essential norm of good explanation is that the more we can identify the ontic dependencies relevant to the production of the phenomenon in the world, the better the explanation becomes. As Craver (2014, p.41) argues, "the norms of scientific explanation fall out of a prior commitment on the part of scientific investigators to describe the relevant ontic structures in the world".

Lastly, let us consider the representational concept of explanation. We frequently talk of explanations in terms of scientific *models* or *theories*. For example, we might talk about how G.A. Parker's (1978) *model of dung fly copulation* explains why dung flies copulate for approximately 36 minutes or how Heeger's (1992) *Normalization Model* explains cross orientation suppression As Waskan et al. (2014, p.3090) argue, "On one common manner of speaking (e.g., 'There is an explanation for the odd trait on page 25') the noun 'explanation' does seem to refer to a set of representational-artifacts" .

When dealing with this concept of explanation, we again have different norms and goals that govern our explanatory practices. The goal of explanation under this definition involves our ability to represent

phenomena in the appropriate sort of way to gain relevant insights into it. These insights are traditionally cashed out in terms of our ability to predict when the phenomenon occurs, describe its behaviour, identify constraints on the phenomenon, or the patterns/principles it obeys (e.g., Batterman, 2000; 2002; Batterman and Rice, 2014; Chirimuuta, 2014). This definition of explanation is often associated with the traditional explanatory virtues of *breadth* and *unification*. Breadth and unification are often characterized as the ability of an explanation to account for a wide range of disparate phenomena. The more phenomena that can be explained by the model or theory, the greater its explanatory breadth and ability to unify (see, Mackonis, 2013; Lombrozo, 2016; Mantzavinos, 2016). For instance, Mantzavinos characterizes unification in terms of explanations that seek "laws and principles of high generality with the aim of constructing a coherent world picture and fitting particular facts within this framework" (Mantzavinos, 2016, p.6, footnote). Thus, under this definition of explanation, a model that describes a set of patterns or principles that we can subsume a greater range of phenomena under, or which identifies specific constraints that apply to a greater range of phenomena, will be better explanations than models that do not. For instance, a continuum model is taken to be a good explanation of the flow behaviour of liquids given that it can successfully predict and describe the behavioural patterns of a wide range of different fluids using the same formalism (see, Batterman and Rice, 2014; Izadi, Anandakrishnan and Onufriev, 2014; Bokulich, 2018) Different kinds of predictions, constraints on phenomena, patterns that phenomena adhere to, and principles that govern phenomena, require different kinds of models or theories to be properly identified.

All this suggests that the distinct concepts of explanation ("communicative", "ontic", and "representational") involve different goals

and norms that govern good explanation. Moreover, it also seems to be the case that one concept of explanation can apply in situations where the others do not. For instance, let us return to our previous example of rising sea levels. Global warming explains the rising sea level even if no one understands that the globe is warming, or whether we have a scientific model or theory which can adequately represent it. In this case, it seems like the ontic definition of explanation applies even though the communicative definition, and the representational definition, do not. Conversely, we might claim that Darwin's Theory of Evolution is a *good* explanation of why elephants have long trunks even if the audience I try to explain this to doesn't understand it (if, for instance, I am explaining the theory to a group of toddlers), and even if we do not yet know what the underlying ontic dependencies are that influence selection and genetic transmission. Lastly, we might do pedagogical research about the best ways to explain scientific principles to non-experts, and discover that certain kinds of techniques (e.g. frequent quizzes, having class discussions, one-on-one tutoring sessions, etc) will allow us to better explain. In which case, the communicative concept of explanation would apply, but the representational and ontic concepts do not (as I can discuss good explanatory techniques without invoking any particular scientific model or theory, or identifying any particular ontic structures and causes in nature).

To sum up, there are good reasons to think that the word "explanation" is ambiguous, referring to at least three distinct concepts that are invoked in science. Each brings with them distinct goals for explanation, and distinct norms that govern good vs bad explanation. Moreover, it seems that each definition of explanation can apply in situations where the others do not. Working scientists thus need to be careful not to equivocate between the different concepts. While this sounds intuitive and plausible, I propose that this is, in fact, the

wrong story to tell. Explanation is far more complex that it may initially seem, and these different concepts of explanation are in fact far more interconnected and interdependent then they may appear. In the section that follows, I want to propose an alternative way of thinking about scientific explanation and motivate this new way of approaching it.

2. One Explanation with Multiple Dimensions

Instead of assuming that we are working with three distinct *concepts* of explanation (each defined in a different way), I propose that a single explanation always has a communicative *component*, a representational *component*, and an ontic *component*. In other words, these are not distinct concepts of explanation as much as they are different evaluative dimensions along which we can analyze a single explanation. This point is not a trivial one and has the potential to help reframe current disputes within the philosophy of science. In order to demonstrate this, let us begin with the hypothesis that these *are* intended to be entirely distinct concepts of explanation, and then explore how each concept of explanation in fact requires or incorporates the others.

Let's begin with the ontic concept. To say that we can have an ontic concept of explanation apply independent of a communicative and representational one would be misleading. To illustrate, consider the phenomenon of phase transitions in statistical mechanics. When we boil a pot of water, it transitions from a liquid state to a gaseous one. Yet our ability to explain this phenomenon is notoriously difficult since we can only model this sort of transition if we mathematically treat the pot of water as being infinitely large, allowing the molecules an infinite degree of freedom (Batterman, 2002). Now, we know that

ontically the transition from liquid to gas is somehow accomplished by the interaction of the finite molecules that make up the water, and the pot containing it. Yet, it would seem unhelpful to suggest that we've *always* had a good scientific explanation of phase transitions by merely *pointing to* a pot of boiling water. Here, we are denoting the ontic structures and causes out in nature that produce the phenomenon. Yet, we are no better at *scientifically explaining* the phenomenon than we were before. This is precisely why William Bechtel (2008, p.18) notes that "the problem with this ontic view is that mechanisms do not explain themselves".

This has led several theorists to point out that those who claim to be working strictly within the ontic concept of explanation do not, and cannot, eliminate the communicative and representational dimensions from how they talk about explanations (Bechtel, 2008, p.18; Wright, 2015; Bokulich, 2018). For instance, Bechtel argues that:

> Even the advocates of the ontic perspective are unable to avoid invoking epistemic notions, although they try to minimize them. Machamer et al. sometimes refer to "giving a description of the mechanism" (p. 3) and "revealing … productive relations" (p. 21), and Salmon uses such words as exhibiting. But these terms understate the cognitive labor involved (Bechtel, 2008, p.18).

Or consider Cory Wright (2015, p.27), who notes that "Salmon offered a putative example of ontic explanation that centers on an epistemic agent who makes discoveries, and so is thereby positioned to elaborately communicate a causal story". Many who endorse the ontic definition of explanation have granted that there is an essential representational and communicative component to explanation that is intertwined with the ontic one. Mark Povich, for example, points out that:

> Craver's ([2014]) most recent formulation of the ontic conception backs away from the metaphysical claim that explanations are ontic structures in the world and focuses on demarcatory and normative constraints on explanation. Craver ([2014]) writes that according to the ontic conception, 'in order to satisfy these two objectives [of explanatory demarcation and explanatory normativity], one must look beyond representational structures to the ontic structures in the world' (Povich and Craver, 2018, p.129).

Similarly, Waskan (2011, p.4) points out that "ontic theories might take many forms, so long as what they propose is that explanations (primarily) reveal something about objective states of affairs. [...] Explanations are, on this view, representations—objective facts are not in the business of revealing. Specifically, they are descriptions". This point has likewise been emphasized by Phyllis Illari, who argues that advocates of ontic explanation acknowledge the essential role that representation and modeling plays in scientific explanation, and that the issue instead is one of normative constraints. In other words, that "ontic explanation is essential for marking several crucial normative dimensions by which scientific explanations are and ought to be evaluated" (Illari, 2013, p.243).[2] This provides evidence that there is

[2] This shift from ontic structures being explanations in and of themselves, to the idea that identifying the appropriate ontic structures provides essential normative constraints on explanation may lead some to conclude that we've weakened the ontic account of explanation to the point of triviality. If the claim merely reduces to some form of epistemological realism (i.e. that there are objective features of the world that we can learn about), then the claim is not particularly informative. Even those who explicitly reject the ontic definition of explanation would be willing to grant such a claim. Such a criticism would be uncharitable, however. The ontic concept of explanation does not merely insist that our models must describe some objective world, but that a good explanation must identify the *appropriate* ontic dependencies in the world necessary to account for the explanandum phenomenon. Specifically, those responsible for the production and sustaining of the phenomenon. This claim is directly

not a distinct *ontic concept* of explanation, there is instead an ontic *dimension* to a scientific explanation that comes along with both representational and communicative dimensions.

To further highlight this idea, let us turn to the representational concept of explanation. It can often be the case that various theories or representations are treated as good explanations even when they are not understandable to a given audience (which runs counter to the communicative definition of a good explanation), or when no ontic dependencies are being identified by the representation in question (which runs counter to the ontic definition of a good explanation). This seems to provide compelling evidence for the idea that there are distinct concepts of explanation at play. Let us examine the intuitive strength of these claims before responding to them.

Suppose a theoretical neuroscientist uses a computational model to provide a scientific explanation of the behaviour of a particular neural circuit. If the audience of the explanation happens to be a group of kindergarten children, then the computational model in question will fail to provide them with any sort of understanding of neural circuits.

---

at odds with those who insist that highly idealized models which do not identify any such ontic dependencies can still count as genuine explanations (e.g. Batterman, 2002; Batterman and Rice, 2014; Chirimuuta, 2014; Rice, 2015; Rice and Rohwer, 2020). Put another way, the ontic definition need not be committed to the idea that the ontic dependencies in the world are somehow self-identifying or self-explaining. It instead need only be committed to the idea that there are relations that exist between events/dependencies in the world such that some set produces and sustains the other, and that appropriate scientific explanations are those that correctly highlight such relations and dependencies. This idea has always been implicit in the ontic definition of explanation, with Salmon (1989, p.86) telling us that "the advocate of the ontic conception can say that an explanation is something—consisting of sentences or propositions- that reports such [ontic] facts", and that "one can properly say either that the explanandum-fact is explained by the explanans-facts or that the explanans-statements explain the explanandum-statement". Although it is worth noting that debates about whether this weaker interpretation of the ontic view is still problematic are ongoing. For details, see: (Illari, 2013; Wright and van Eck, 2018).

Thus, it would fail to be a good explanation under the communicative definition of explanation. However, such a failure on the part of the children to understand the model does not undermine the idea that the model is still considered a good explanation by the scientific community, suggesting that a different concept of "explanation" is at work.

We must be cautious with such a conclusion, however. This is because when dealing with *scientific* explanations, the audience who must understand the model or theory is not *any* audience. Put another way, the computational model is a good scientific explanation because it provides *working scientists* with an understanding of the neural circuit. Indeed, studies have shown that "high-science participants were less likely to regard a passage of text as an explanation when told that the representation, because of qualitative barriers, lacks intelligability" (Braverman et al., 2012, p.1372). In other words, if a model or theory cannot provide an explanation *in accordance with the communicative definition* to working scientists, then scientists themselves do not consider the model or theory to be genuinely explanatory. The importance that understanding-to-a-scientific-audience has for the representational concept of explanation can be seen in the way that scientists explain to non-scientists. As Woody (2015, p.81) notes, "explanatory discourse often involves the communication of exemplary explanations among members of a given scientific community, and one aim of explanatory practice seems to be training novice practitioners to recognize typical explanatory patterns." And so to suggest that a scientific model or theory is explanatory independent of whether it provides understanding to an audience is deeply misleading, and one cannot tease apart a representational concept of explanation from a communicative one in this regard.

But what about the relationship between the representational concept and the ontic concept? There seem to be many examples in which a model is considered explanatory despite not identifying (or indeed deliberately misrepresenting) the ontic variables in nature responsible for the production of the explanandum phenomenon. Optimality models in evolutionary biology, for example, deliberately ignore or distort causal features in the evolutionary process in order to mathematically determine what sorts of traits would be locally optimal for an organism to have, thus explaining why such traits were selected for (Potochnik, 2010; Woods and Rosales, 2010; Rice, 2015). Likewise, continuum models in physics are used to explain the flow behaviour of liquids despite deliberately saying nothing as to the molecular components and causal processes that produce such behaviours (Batterman and Rice, 2014; Izadi, Anandakrishnan and Onufriev, 2014; Bokulich, 2018, p.803). In these instances, it appears like there is no ontic component to the explanation, suggesting a distinct concept of explanation.

Yet these cases are not as straightforward as they initially seem either. For instance, take optimality models. While it may be true that an optimality model does not identify the ontic structures and causes in nature responsible for the production of phenotypic traits, it is not true that a commitment to what those structures and causes are is not an essential part of the explanation. This is because the application of optimality models *only* works when scientists already know essential ontic structures and processes at work in the production of the phenomenon. Angela Potochnik, for example, notes the following regarding the use of optimality models in evolutionary biology:

> Because optimality models use highly simplified assumptions as placeholders for complex dynamics, their successful use depends upon evolutionary dynamics that the models themselves

> do not explicitly represent. In other words, optimality models are epistemically dependent on unrepresented dynamics. Information about these unrepresented dynamics helps establish whether an optimality model's simplifying assumptions are problematic, and thus how successful the model is (Potochnik, 2010, p.226).

And so the ontic dimension of the explanation is essential here. The model in question cannot function appropriately without the relevant ontic structures and processes being identified and understood by scientists, even if they are not explicitly stated in the model or theory itself. While a focus on the *representational dimension* of explanation does not highlight the ontic features of the explanation, they are a necessary part of the explanation itself, since they are required for the representations to function appropriately.

For another example, consider continuum models. As Bokulich (2018, p.803) rightly points out, "at large scales, continuum representations of water and the Navier-Stokes equations are typically more relevant" than other kinds of scientific models or representations for characterizing flow behaviour. The Navier-Stokes equations predict how fluids will behave even though they do not identify any of the underlying molecular components of the fluid. Instead, they treat the fluid as a single continuous entity. Such models would appear to explain why fluids behave as they do despite saying nothing as to the ontic variables responsible for it.

Just like the case with models in evolutionary biology however, such models in physics are likewise *still dependent on scientists being commitment to ontic variables that are not included in the model itself*. In this case, the ontic properties of the molecules that compose the fluid must be correctly understood by working scientists for proper application of the Navier-Stokes equations. Mark Povich, for example,

points out that the Navier-Stokes equations cannot be used to account for the behaviour of fluids like liquid crystals. This is because "their often rod-shaped particles result in directional preference and lack of symmetry. Liquid crystals thus cannot be accurately modelled using the unmodified Navier-Stokes equations. The addition of a stress tensor or coupling with a Q-tensor system is required to take into account the anisotropy of liquid crystals" (Povich and Craver, 2018, p.124). Note that while the Navier-stokes equations themselves do not identify the molecular properties of the fluid, scientists must be committed to various ontic properties of the molecules that make up the fluid in order to tell if and when the equations will work, or if and when they will need to be modified.

Some, like Potochnik (2010), acknowledge the importance that these ontic commitments play in constructing and applying these idealized representations, but insists that it is the idealized model that is explanatory independent of these commitments. This is because while these ontic commitments are necessary to construct and apply something like an optimality model, the model itself can only identify the relevant evolutionary patterns or principles when these ontic structures and causes are excluded from the representation itself. By trying to add such details to the representation, it becomes worse at identifying the patterns and principles we seek to represent. Thus, the model is explanatory independent of those ontic details. Similar arguments are also made in the context of highly idealized models in physics (see Batterman, 2002; Batterman and Rice, 2014).

The problem with this style of response is that it equates an explanation with a scientific model or theory. In this case, since the model or theory does not identify the relevant ontic variables, then neither does the *explanation*. However, a scientific explanation should not be conflated with a model or theory. A scientific representation

is an integral *part* of an explanation, but the explanation itself includes elements that go beyond what is included within any single representation.

We have already seen evidence of this regarding the role that understanding plays in explanation. A model or theory may, for instance, correctly identify the pattern we care about, but if working scientists cannot understand the model or theory (i.e. if it is abstracted away from the communicative dimension of explanation), then scientists will not consider it explanatory. This already suggests that what constitutes an explanation goes *beyond* what is explicitly stated by any particular model or theory. Just as a communicative dimension is essential for any particular representation to count as a *part* of the explanation, so too is a commitment to essential ontic features of the world (even if they are not stated by the model or theory itself). A commitment to what the actual ontic variables in nature are plays an essential role in the creation of both optimality and continuous flow models, their boundary conditions, their applications, and what inferences we are licensed to draw from them (see Hochstein, 2019). In this regard, these ontic commitments are intertwined with the representational content of the models to instantiate the explanation. To pull them a part is to strip the representation of its ability to carry explanatory content. In other words, we cannot cleanly demarcate a *representational* concept of explanation from an *ontic* concept of explanation.

Lastly, let us turn to the communicative concept of explanation. While we can talk about an act of explaining abstracted away from any particular representation or ontic commitments, the representational and ontic components of explanation are always present and ineliminable in any particular instance of scientific explanation as a communicative act. As Illari notes, when talking about scientific

explanation (even as a communicative act), one cannot ignore the ontic dimension of explanation given that "explanations cannot ignore worldly things" (Illari, 2013, p.251).

To illustrate, recall that the primary goal of the communicative definition of explanation is to provide *understanding* in an audience. However, our understanding of a phenomena cannot be divorced from our ontic commitments about essential structural and causal features in the world since part of what it is to understand is to correctly account for such features. Waskan, for example, points out that "understanding" is frequently treated as a "success verb much like (a sense of) 'see'—that is to say, in order to do it you must be successful at it. We might say, for instance, that whereas alchemists *felt* that they understood combustion, chemists do *genuinely* understand combustion" (Waskan, 2011, emphasis in the text). Bechtel (2008, p.14), Strevens (2011, p.3), and Illari (2013, p.245) make similar claims. Likewise, both Elgin (2004) and Potochnik (2015) argue that truth is an essential *threshold* concept when it comes to understanding. In other words, understanding requires that one correctly accounts for at least *some* of the ontic dependencies out in the world that are directly responsible for the production of the phenomenon. As Potochnik (2015, p.73) puts it, "a claim must be 'true enough' in order to be epistemically acceptable; that is, any divergence from the truth must be negligible, or safely neglected". While both Elgin and Potochnik argue that the amount of truth required for an account to provide genuine understanding will vary based on our pragmatic needs, it highlights how the ontic dimension of explanation cannot be conceptually *divorced* from any account of understanding, making it an eliminable part of the communicative concept of explanation.

A very different kind of argument for the essential connection between the communicative, ontic, and representational definitions

of explanation can also be found in the way that the various goals associated with scientific explanation are unavoidably intertwined and dependent on one another for their explanatory power (Hochstein, 2017). Consider that the primary goal associated with the ontic concept of explanation is the identification of relevant ontic dependencies existing in the world. Meanwhile, one of the primary goals associated with the representational concept of explanation is the ability to represent certain behavioural patterns that a given phenomenon adheres to. But what justifies the explanatory status of such goals? Put another way, *why* is the identification of ontic dependencies explanatory? What *makes it* explanatory? The same with the identification of behavioural patterns, or with prediction. Why consider such goals relevant to explanation? Typically, theorists will justify the explanatory status of one explanatory goal *by appealing to the other goals.*

For instance, we often justify the explanatory status of identifying ontic dependencies by appealing to the fact that knowing such dependencies allows us to better control and manipulate the phenomenon, which in turn allows us to better *predict* and *understand* it. Thus, we justify the explanatory goal of the ontic concept of explanation by appealing to the fact that it allows us to better satisfy the goals of the communicative or representational concepts of explanation. Similarly, we might ask why predicting the occurrence of the phenomenon in a range of cases counts as explanatory. In such cases, it is common to justify the explanatory status of prediction on the grounds that "predictions help us check whether our accounts of the world have any veracity" (Douglas, 2009, p.453). Here we justify the explanatory status of a goal identified by the representational concept of explanation by appealing to how it helps us to better attain a goal identified by the ontic concept. This shows that the various goals of explanation are in fact inter-defined and inter-dependent Building on this idea, I pro-

pose that instead of having distinct concepts of explanation, we have different evaluative dimensions of an explanation that are similarly inter-defined and inter-dependent.

All of this provides substantial evidence for the idea that there is always an unavoidable ontic, communicative, and representational dimension to one and the same explanation. While we can talk about these different evaluative dimensions abstracted away from one another, we must be cautious not to assume that these dimensions denote entirely distinct concepts of explanation that are autonomous and that apply in different contexts.

3. A Distinction Without a Difference?

Before proceeding further, it is important to highlight why this alternative view of scientific explanation is worth articulating. After all, are "dimensions" of explanation really that different from "concepts" of explanation? Am I merely drawing a distinction without a difference? Why is it important to demarcate these two positions? There are important implications to the view being defended here.

To assume that there are simply distinct concepts of explanation leads to the deeply problematic assumption that the goals and norms of a particular concept of explanation, in the appropriate context, is always sufficient by itself to account for a good scientific explanation. However, this would be to distort how explanations work in science, and why they are successful. To illustrate, consider Mantzavinos's example of neoclassical economics. While Mantzavinos does not distinguish explanations in terms of communicative, representational, or ontic concepts, he does imply such a distinction when insisting that models from neoclassical economics count as genuine

scientific explanation in virtue of meeting the criteria associated with the representational concept, but not those associated with the ontic one. Specifically, he notes that neoclassical models are considered good scientific explanations within the field of economics because they unify a range of economic phenomena under a particular set of principles (Mantzavinos, 2016, p.12). However, he notes that such models *do not need* to identify relevant ontic structures or causes in nature to count as explanatory, since in the context of economics, this explanatory goal is simply not what is adopted by working scientists.

For example, he tells us that "microeconomics textbooks never pay any attention to 'causal processes, causal interactions, and causal laws', in the analysis of any type of market, be it the competitive market, a monopoly or an oligopoly", and that such models do not identify the sort of ontic dependencies in the world that we can intervene on or manipulate because "it operates at a level of abstraction that makes it extremely difficult to test the theory empirically" (Mantzavinos, 2016, p.13). He ultimately concludes that the "decision calculi of neoclassical economic theory are clearly argument patterns that can be only accommodated by the unification model of explanation" (Mantzavinos, 2016, p.13). For our purposes, we can reasonably interpret this as saying that for economists, it is the representational concept of explanation that matters (since it is the unification of various market phenomena under a single model or theory that makes it explanatory), not the ontic concept (since identifying ontic structures, causes, and variables that can be manipulated, are not taken to be explanatorily relevant in this context).

But we have good reasons to challenge Mantzavinos on this point. Whether such neoclassical models identify genuine principles and regularities in market forces, or merely notes contingent correlations that do not genuinely unify market phenomena at all, cannot be determined

unless we appeal to ontic dependencies and causal variables in the world. Neoclassical economics has built into it certain implicit ontic commitments about the structures and dependencies in the world that must be true in order for the model to genuinely provide a unifying account. If these sorts of ontic dependencies are false, then the model's ability to unify successfully, and thus meet its own standards of good explanation, are undermined. Similarly, if the model does not identify ontic variables we can intervene upon, we have no way of testing or determining whether the neoclassical model is actually identifying unifying market forces since there is no way to manipulate relevant features of the market to see whether its underlying principles apply or not. This is precisely why *economists themselves* have questioned whether such models should be taken to provide genuine scientific explanations.

For a rather stark example, consider Alfred Eichner's now famous argument for why neoclassical economics fails to count as a science, and to provide scientific explanations. He notes that neoclassical economics is build on numerous implicit ontic assumptions about the underlying structures and dependencies in the world which it uses as the foundation for its unifying models, and that many such ontic assumptions have proven false (for discussion of such incorrect ontic comments Eichner, 1983, p.511). In virtue of getting these ontic dependencies wrong, Eichner argues that the explanatory value of such models is undercut given that they fail to account for the market forces that genuinely exist.[3] In other words, the model fails to unify the actual range of phenomena in the world it is attempting to explain. As he puts it:

[3] For others who likewise emphasize this point, see: (Hall et al., 2001; Hausman, 2008; Keen, 2022).

> Economists as a group have adopted the view that formal, or mathematical, proofs are sufficient to establish the validity of a theory rather than just being necessary. [...] Thus it is common for the "theorists" to set up their models in such a way that the postulated behavior runs counter to all that is known about actual economic systems without this fact impugning either the argument or the economists' reputation. So sharp a distinction between theory and empirical research is unknown in the natural and biological sciences, and for good reason. It leads to an outpouring of useless theories that waste the time and energy of empirical researchers (1983, p.517).

Similarly, the lack of intervention that such models provide is likewise taken to undermine the scientific and explanatory status of such models. This is because "the theory must be shown to make a difference to society, when translated into one or more public policies, that will lead to certain clearly distinguishable results. The policies must then be adopted and the predicted effect confirmed. This is the praxis test of a social science theory" (Eichner, 1983, p.510). Based on these very reasons, Eichner concludes that neoclassical economics fails to be scientific, and its models fail to provide genuine scientific explanations. Note that the issue here is not whether economists think that unifying market forces within a model or theory is explanatory. The issue is that the ability of the model to successfully meet this criterion *requires* that the norms and goals of the ontic dimension of explanation be adopted. Mantzavinos suggests that economists themselves don't consider the goals of the ontic dimension relevant to explanation, but this is untrue. Even Milton Friedman himself grants that keeping track of these implicit ontic commitments are "extremely valuable in suggesting leads to follow in accounting for divergences between predicted and observed results; that is, in constructing new hypotheses or revising old ones" (Friedman, 1953, p.31n). Put sim-

ply, *economists themselves* have emphasized the importance of the ontic dimension of explanation when evaluating whether neoclassical models are successful at unifying phenomena under an appropriate scientific representation.

To assume that there is a representational concept of explanation that is distinct from the ontic concept and the communicative concept, and that the application of this one concept is sufficient for explanation in the right contexts, ignores that each "concept" in isolation does not, and cannot, do the explanatory work that it is expected to do unless we smuggle in the other concepts of explanation with it. It is important to distinguish "concepts" of explanation from "dimensions" of explanation since one implies that we can satisfy one concept of explanation without having to say anything about the other concepts, and that *good* scientific explanation is determined entirely by the particular concept of explanation we employ. If we understand these as different dimensions of the same explanation, on the other hand, then we can better understand the interplay between the different dimensions, and why we do not have sufficient resources for determining a good explanation when focusing only on one dimension in isolation of the others.

By changing our perspective from concepts of explanations to different evaluative dimensions of an explanation, we can also better clarify some confusions that have been at the heart of certain debates in the philosophy of science. Take the ongoing debate amongst mechanists in the context of biology and neuroscience. These theorists argue that explanation in the life sciences, especially biology and neuroscience, primarily proceeds by identifying the physical mechanisms responsible for the target phenomenon (often characterized in terms of organized parts and operations that produce regular change). However, some argue that mechanisms are out in the world, and that explana-

tions merely describe or reveal such mechanisms (Machamer, Darden and Craver, 2000; Craver, 2006; Kaplan, 2011; Strevens, 2011; Craver and Darden, 2013). Meanwhile others argue that mechanisms are interpretations of the world (by way of mechanistic *models*), and that we should think of mechanistic explanation as cognitive and representational products we create to understand the world (Chirimuuta, 2014; Bechtel, 2015; Wright, 2015; Austin, 2017). The first group argues that mechanistic explanations are *ontic*. Meanwhile, the second group argues that mechanistic explanations are *epistemic*. So how ought we to understand mechanistic explanation?

Here the dispute rests on which dimension of explanation that theorists focus on at the expense of the others. For instance, Bechtel (2015) notes that causal variables in the world responsible for a cognitive phenomenon go well beyond the structures and processes occurring within the brain, and include events in the distant past, as well as all kinds of environmental structures and processes that are not typically treated as part of the biological system. In this respect, he argues that the boundary of a cognitive mechanism is often not determined by the world itself, but by the interests of working scientists who focus on only a subset of the causal variables responsible for a given cognitive process. Since every mechanistic explanation idealizes numerous causal variables in the world and are dependent on the interests of working scientists, this suggests that we are not merely describing the ontic joints of mechanisms in the world but interpreting the world in mechanistic terms. In his words,

> It is the scientists who impose boundaries around entities and activities in nature and impose a time scale on which their functioning is characterized. For different explanatory purposes researchers may draw these boundaries in different locations or at different time points. These choices, though,

> while not simply responsive to pre-existing boundaries, are not entirely arbitrary. [. . .] The networks of entities found in nature commonly exhibit small-world organization as well as being scale-free. This entails that while real-world networks are highly inter-connected, there are clusters within them that are semi-independent of the rest and productively posited to be the mechanisms responsible for specific phenomena (Bechtel, 2015, p.85).

Conversely, those who defend the ontic account note that even in such cases, there *are* "real-world networks" that cluster in ways that are semi-independent and productively posited to be mechanisms. Thus even if such models are highly idealized, they still carry explanatory content in virtue of revealing genuine ontic structures, organizations, and processes in nature that play an essential part in the production of the phenomenon.

How then should we settle such disputes? I propose that the framework presented in this paper can help untangle the confusion here. Those who advocate for *ontic* accounts of mechanistic explanation focus on the ontic dimension of explanation at the cost of the representational or communicative dimension. Meanwhile, those who argue for epistemic explanation focus on the representational and communicative dimensions at the cost of the ontic dimension. Yet if we consider that explanation always has a communicative, representational, and ontic dimension to it, then the two accounts don't so much represent conflicting accounts of mechanistic explanation, as much as highlight why we should care about, and pay attention to, different dimensions of the same explanation.[4]

[4] This account of explanation may also help to disentangle some confusions regarding the dispute between explanatory monists, and explanatory pluralists, in the philosophy of science. Explanatory monists argue that there is a single set of criteria for determin-

Illari (2013) similarly argues that the contrast between ontic and epistemic views of mechanistic explanation ignores the inseparable and intertwined nature of the various goals and norms that make up the two positions. For instance, she argues that many of the traditional virtues of explanation, such as unification, simplicity, elegance, and intelligibility, in fact depend on the *interaction* of the various goals and norms that make up both ontic and epistemic accounts of explanation. As she puts it:

> This means that what looks to us intelligible, simple and unified is not a static feature of human psychology, but is affected by our empirical engagement. Newtonian action at a distance used to seem quite impossible to us; so did quantum me-

---

ing what counts as a good scientific explanation that applies across fields and contexts. Meanwhile, explanatory pluralists argue that there is a plurality of types of explanations in science, and that there is no single set of criteria that unities them all (e.g. the sorts of criteria that determine a good explanation in the context of physics may be distinct and unrelated from the sorts of criteria that determine a good explanation in the context of biology). We may be able to help clarify some of these issues by noting that if we focus on a single dimension of explanation, like the representational dimension, then we seem to find a plurality of types of models that can and do count as explanatory (e.g. statistical, mechanistic, topological, dynamical, etc). However, if we focus more generally on the essential set of norms that all three dimensions impose on each other, we may be able to find a general monistic set of criteria that all explanations require. For instance, a good explanation must be understandable to the scientific community. It must represent the phenomenon in a way that either unifies phenomena under a set of principles, identifies constraints on its behaviour, or makes predictions. Since a model that fails to allow us to predict what the phenomenon will do, provides no constraints on what the phenomenon cannot do, and provides no information as to what the various instances of the phenomenon have in common, may strip the explanation of its ability to say anything of use to scientists. Lastly, an explanation must identify essential ontic dependencies and regularities in nature, since otherwise we cannot tell if our representation is genuinely identifying principles the phenomenon obeys, or constraints on its behaviour, as opposed to mere contingent correlations. And so explanations may turn out to be pluralistic in one regard (say, in terms of model types), but monistic in others (say, the set of general constraints that guide communicative, representational, and ontic practices).

> chanical indeterminism, and non-locality. Physicists sincerely describe quantum mechanical equations as elegant, a claim that can generate hilarity in those unused to working with such theories. If empirical engagement continually forms what we find intelligible, simple and unified, then epistemic constraints on explanation, even the more 'psychologistic' constraints, are deeply entangled with ontic ones (2013, p.254).

In this regard, the ontic and epistemic camps are not providing distinct or incompatible views of mechanistic explanation, so much as emphasizing different essential features that a given explanation must have.

3. Implications and Concerns

On the surface, such a view may appear either obvious ("of course explanation involves all three elements!") or naïve ("It's too simplistic to assume that we can easily merge these accounts"). And so it is worth exploring the complexities and difficulties that such an account now presents us with. As noted previously, different dimensions of explanation emphasize different norms for evaluating good and bad explanations. Problems begin to arise, however, when we discover that these norms can often conflict with one another. For instance, if we focus on the ontic dimension of explanation in isolation, it seems like the more we can identify and describe the ontic dependencies in the world, the better our explanation becomes. It is this intuition that underlies Kaplan's claim that it is a "highly plausible assumption that the more accurate and detailed the model is for a target system or phenomenon the better it explains that phenomenon, all other things being equal" (Kaplan, 2011, p.347). Yet this can run counter

to some of the other norms of explanation associated with the other dimensions. For instance, adding too many accurate details to a model or theory can make it more difficult to understand, and thus makes it a worse explanation according to the norms associated with the communicative dimension of explanation.

Indeed, conflicts between the various explanatory goals of the different dimensions can be seen explicitly in scientific practice itself. Consider explanations of the action potential of the neuron in neuroscience. The Hodgkin & Huxley model of the action potential, developed in the early 1950s, mathematically characterized electrochemical features of the action potential (specifically: the ion flow of sodium and potassium channels). However, it did not provide an account of the ontic variables responsible for the production of these features. In this regard, it satisfied the explanatory goals of the *representational* dimension (by identifying constraints or patterns that the phenomenon adheres to), but not of the *ontic* dimension (by failing to denote the relevant ontic variables that actually produce these constraints and patterns). This led Hodgkin & Huxley to make inconsistent claims regarding whether their model is explanatory or not. In their original paper, they claim that their model provides "a sufficient explanation of the wide range of phenomena that have been fitted by solutions of the equations" (Hodgkin and Huxley, 1952, p.541). Yet they also claim in the same paper that their model is in fact *not* a good explanation of the action potential, since it merely provides an "empirical description" of the phenomenon and not an account of what produces it (Hodgkin and Huxley, 1952, p.541; for the discussion, see also: Bogen, 2005; Craver, 2006).

So how then do we determine whether a model (like the Hodgkin & Huxley model) counts as a good scientific explanation or not given that the norms that govern explanation are inconsistent or contradic-

tory across evaluative dimensions? This is a serious concern. One potential solution, recently proposed by Rice & Rohwer (2020), is to claim that a scientific model or theory need only satisfy *a sufficient number* of the norms that govern all the various dimensions of explanation to count as genuinely explanatory in a given context, as opposed to satisfying *all* of them. They claim that "many single models are sufficient to explain because the information they provide will satisfy a sufficient subset of the conditions included in the cluster concept" (Rice and Rohwer, 2020, p.1043).

The problem with this solution is that it makes the common mistake of conflating *explanation* with *representation* (i.e. the idea that explanation is always constituted by a particular model). However, as I have argued, features beyond the representational texts are also constitutive parts of an explanation. For instance, scientists have certain implicit commitments to ontic dependencies that are essential to how they construct, interpret, and draw inferences from their models despite such dependencies not being explicitly stated in the model itself. Without these commitments, the model cannot function. In this respect, the background beliefs and commitments of scientists are a component part of an explanation in addition to the particular models and theories being used (see: Bokulich, 2018; Hochstein, 2019). Similarly, howwe convey models and representations to others is also an essential component to explanation:

> The history of science, meanwhile, provides rich empirical support for the claim that production of explanations serves to constitute, rather than merely communicate, "intelligibility" for a scientific discipline. Precisely because explanatory discourse inculcates particular patterns of reasoning, it functions to sculpt and subsequently perpetuate communal norms of intelligibility. In effect, explanations encode the aims and val-

> ues of particular scientific communities, telling practitioners what they should want to know about the world and how they should reason to get there (Woody, 2015, p.81).

This means that a single explanation may be constituted by collections of *representations*, *social practices*, *psychological processes*, and *accurate commitments to the relevant ontic features of the world*. Likewise, determining whether an explanation is good or bad will involve an evaluation of *all* these components.

At first glance, this would appear to make the evaluation of scientific explanations a herculean affair! This idea is not without precedent however. Mantzavinos (2016) proposes an account of explanation that makes a similar claim. Mantzavinos proposes that instead of talking about explanations as entities, we should understand them as collections of inferential practices governed by the norms of the scientific community, which he calls "explanatory games". Most notably, he tells us that determining "which rules of representation guide the explanatory activities is fundamentally important for the quality of explanations generated during the game" (Mantzavinos, 2016, p.42). Here, the *representational* dimension of explanation is emphasized. However, he also notes that:

> No explanatory game can take place in a metaphysical vacuum. The metaphysical assumptions act as constraints on the generation of the other rules, and belong therefore to the constitutive rules. The structure of the game is predicated on prior assumptions concerning the way the world is and by what means it is explainable in principle. These rules can be implicit or explicit and they can vary from stone-age metaphysics to highly refined metaphysical assumptions (2016, p.41).

Here, the *ontic* dimension is emphasized. Lastly, he tells us that explanatory practices are "a process of social interaction unfolding

within the given institutional rules." (Mantzavinos, 2016, p.68). In this way, the *communicative* dimension of explanation is highlighted. For Mantzavinos, we evaluate whether an explanation is good or bad by evaluating whether all of these inferences are appropriate and in accord with the inferential rules of the explanatory game.

The problem, of course, is that we must now evaluate a huge number of variables (communicative, representational, and ontic) in order to determine whether any explanation is good or bad, which is no small feat. Yet I propose that a great deal of how we evaluate explanations in science already implicitly does this. For instance, knowing which parts of a model count as idealizations and which don't require that working scientists already have ontic commitments regarding the variables in the world that the model is deviating from, and the wrong sorts of ontic commitments are often taken to be signs of a problematic explanation. As Anya Plutynski (2013, p.472) points out:

> Good modelers are careful to be clear about when an assumption used to construct a model is deliberate simplification or simply false, and when it is a hypothesis supported by evidence. Unfortunately, what starts as deliberate simplification may often be confused with actual hypothesis and latter reified into theory.

Thus we often evaluate good explanations in terms of whether one's ontic commitments are appropriate and being used correctly in the construction of our models. Similarly, the way that knowledge is communicated throughout the scientific community is also evaluated when determining good explanation. As Woody (2015, p.85) points out,

> Explanations must be minimally intelligible to be accepted by individuals. But explanations, especially in educational

> contexts, condition reasoning, and thus sculpt social norms of intelligibility. Explanations intelligible to members of one community are frequently opaque to outsiders, even other scientists.

Therefore the appropriate means of communication can be essential to whether something is adopted by the scientific community as a good explanation. Likewise, various constraints on the communicative practices we have (such as passing peer review in order to be published in scientific journals, or accepted at conferences and workshops) are also considered essential to whether something should be accepted or rejected as a good *scientific* explanation.

Like evaluating any complex system, we may have to trust that some parts are in better working order than others or look for faults only when something has gone astray. Instead of attempting to evaluate all parts of the explanation simultaneously, we focus our attention on certain features of it depending on our interests and needs, or evaluate the particular features of the explanation that we think may be problematic.

By analogy, imagine evaluating whether a car is in good working order. We might have particular concerns about a given type of car ("I hear it gets bad gas millage!", "The engine is known to frequently stall!"), and use this to gauge if the particular car we want to purchase has these problems. If not, we may conclude it is in good working order. Yet if someone comes to us and points out that the break line is cut, or that the spark plugs need replacing, or that the trunk is welded shut, then those concerns will point our attention to parts of the car we assumed were in working order, and now causes us to doubt whether the car is worth purchasing. But of course, of the thousands of things that could potentially be wrong with a car, we do not and cannot evaluate all of them simultaneously. Instead, we focus our attention

on some features we consider to be most important or worrisome, and assume the rest of the car is in good working order *until given reason to doubt it*. This is because there are certain standards we assume the car must have met to be sold on the lot.

The same will apply to scientific explanations. We might question some aspects of an explanation, while assuming that others are in working order in virtue of scientific communal standards. For instance, if a paper has been published in a respected peer reviewed journal, then we might assume that it is intelligible to the relevant scientific audience. Of course, if we discover that a paper was accepted due to a case of fraud, then it would immediately cast into doubt that the paper has in fact met the appropriate standards of the communicative dimension. Similarly, if scientists working in various labs report being able to control and manipulate episodes of depression by changing the concentrations of various gut bacteria, then we may be justified in assuming that the gut microbiome is part of what explains those episodes of depression. However, if we learn that their ability to control and manipulate depressive episodes in this way was greatly overstated, or far more limited than they let on, then it will call our attention to the ontic features of the explanation that we assumed were in good working order.

Scientists often have little reason or incentive to identify all component features of an explanation during their everyday activities. To ask whether a particular model is a good explanation for example, is not to suggest that the communicative or ontic dimensions to that explanation *do not apply*, or that *we mean something different by explanation*. It is merely the case that the other dimensions are either obvious to those involved, not points of contention, or not their primary focus. Sometimes it is the ontic component of the explanation that we may disagree about, at other times the best way to convey

models to an audience. We must not confuse the fact that we can analyze explanations along different dimensions with the idea that these different dimensions correspond to distinct concepts of explanation, or denote entirely metaphysically distinct explanatory entities.

Conclusion

Traditionally, philosophers have adopted either a representational definition, communicative definition, or ontic definition of scientific explanation. This has given way more recently to a pluralistic view which argues that each concept of explanation may be scientifically appropriate in particular contexts and for particular purposes. In this paper, I have argued for an alternative view which better highlights the complex ways in which representational, communicative, and ontic features of a scientific explanation interact and depend on one another. Instead of distinct explanatory concepts, we have different evaluative dimensions along which we can analyze any instance of a scientific explanation. The upshot of this view is that it not only better accounts for why scientific explanations work when they do, but can also help to clarify disputes within philosophy of biology and neuroscience between ontic and epistemic views of mechanistic explanation.

This new account is not without its own set of challenges, however. Evaluating whether an explanation is good or bad is substantially more difficult under this new framework. Evaluations must be done in a piecemeal way, and we may have to trust that some features of an explanation are better justified than others until given reason to think otherwise. While this makes the evaluation of explanations more difficult, it is in-line with how scientific practice in fact works.

Bibliography

Achinstein, P., 1984. The Pragmatic Character of Explanation. *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association* [Online], 1984(2), pp.274–292. https://doi.org/10.1086/psaprocbienmeetp.1984.2.192509.

Achinstein, P., 1983. *The Nature of Explanation* [Online]. New York ; Oxford: Oxford University Press. Available at: <http://archive.org/details/natureofexplanat0000achi> [visited on 29 September 2023].

Austin, C.J., 2017. The Philosophy of Biology. *Analysis* [Online], 77(2), pp.412–432. https://doi.org/10.1093/analys/anx032.

Batterman, R.W., 2000. Multiple Realizability and Universality. *The British Journal for the Philosophy of Science* [Online], 51(1), pp.115–145. https://doi.org/10.1093/bjps/51.1.115.

Batterman, R.W., 2002. *The Devil in the Details: Asymptotic Reasoning in Explanation, Reduction, and Emergence*, *Oxford studies in philosophy of science*. Oxford: Oxford University Press.

Batterman, R.W. and Rice, C.C., 2014. Minimal Model Explanations. *Philosophy of Science* [Online], 81(3), pp.349–376. https://doi.org/10.1086/676677.

Bechtel, W., 2008. *Mental Mechanisms: Philosophical Perspectives on Cognitive Neuroscience*. New York: Routledge.

Bechtel, W., 2015. Can mechanistic explanation be reconciled with scale-free constitution and dynamics? *Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences* [Online], 53, pp.84–93. https://doi.org/10.1016/j.shpsc.2015.03.006.

Bogen, J., 2005. Regularities and causality; generalizations and causal explanations. *Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences* [Online], 36(2), pp.397–420. https://doi.org/10.1016/j.shpsc.2005.03.009.

Bokulich, A., 2018. Representing and Explaining: The Eikonic Conception of Scientific Explanation. *Philosophy of Science* [Online], 85(5), pp.793–805. https://doi.org/10.1086/699693.

Braverman, M. et al., 2012. Intelligibility is Necessary for Scientific Explanation, but Accuracy May Not Be. *Proceedings of the Annual Meeting of the Cognitive Science Society* [Online], 34(34), pp.1368–1373. Available at: <https://escholarship.org/uc/item/32m3q2sp> [visited on 29 September 2023].

Chirimuuta, M., 2014. Minimal models and canonical neural computations: the distinctness of computational explanation in neuroscience. *Synthese* [Online], 191(2), pp.127–153. https://doi.org/10.1007/s11229-013-0369-y.

Coffa, J.A., 1974. Hempel's ambiguity. *Synthese* [Online], 28(2), pp.141–163. https://doi.org/10.1007/BF00485232.

Craver, C.F., 2006. When mechanistic models explain. *Synthese* [Online], 153(3), pp.355–376. https://doi.org/10.1007/s11229-006-9097-x.

Craver, C.F., 2014. The Ontic Account of Scientific Explanation. In: M.I. Kaiser, O.R. Scholz, D. Plenge and A. Hüttemann, eds. *Explanation in the Special Sciences: The Case of Biology and History* [Online]. Vol. 367, *Synthese Library*. Dordrecht: Springer Netherlands, pp.27–52. https://doi.org/10.1007/978-94-007-7563-3_2.

Craver, C.F. and Darden, L., 2013. *In Search of Mechanisms: Discoveries across the Life Sciences* [Online]. Chicago, IL: University of Chicago Press. Available at: <https://press.uchicago.edu/ucp/books/book/chicago/I/bo16123713.html> [visited on 29 September 2023].

Craver, C.F. and Kaplan, D.M., 2020. Are More Details Better? On the Norms of Completeness for Mechanistic Explanations. *The British Journal for the Philosophy of Science* [Online], 71(1), pp.287–319. https://doi.org/10.1093/bjps/axy015.

Craver, C.F. and Povich, M., 2017. The directionality of distinctively mathematical explanations. *Studies in History and Philosophy of Science Part A* [Online], 63, pp.31–38. https://doi.org/10.1016/j.shpsa.2017.04.005.

Douglas, H.E., 2009. Reintroducing Prediction to Explanation. *Philosophy of Science* [Online], 76(4), pp.444–463. https://doi.org/10.1086/648111.

Eichner, A.S., ed., 1983. *Why Economics is Not Yet a Science*. Armonk, N.Y: M.E. Sharpe.

Elgin, C.Z., 2004. True Enough*. *Philosophical Issues* [Online], 14(1), pp.113–131. https://doi.org/10.1111/j.1533-6077.2004.00023.x.

Friedman, M., 1953. *Essays in Positive Economics*. Chicago, IL: University of Chicago Press.

Gilpin, L.H. et al., 2022. *"Explanation" is Not a Technical Term: The Problem of Ambiguity in XAI* [Online]. arXiv. https://doi.org/10.48550/arXiv.2207.00007.

Hall, C. et al., 2001. The Need to Reintegrate the Natural Sciences with Economics: Neoclassical economics, the dominant form of economics today, has at least three fundamental flaws from the perspective of the natural sciences, but it is possible to develop a different, biophysical basis for economics that can serve as a supplement to, or a replacement for, neoclassical economics. *BioScience* [Online], 51(8), pp.663–673. https://doi.org/10.1641/0006-3568(2001)051[0663:TNTRTN]2.0.CO;2.

Hausman, D.M., 2008. Why look under the hood? *The Philosophy of Economics: An Anthology* [Online]. Cambridge: Cambridge University Press, pp.183–187. https://doi.org/10.1017/CBO9780511819025.012.

Heeger, D.J., 1992. Normalization of cell responses in cat striate cortex. *Visual Neuroscience* [Online], 9(2), pp.181–197. https://doi.org/10.1017/S0952523800009640.

Hochstein, E., 2017. Why one model is never enough: a defense of explanatory holism. *Biology & Philosophy* [Online], 32(6), pp.1105–1125. https://doi.org/10.1007/s10539-017-9595-x.

Hochstein, E., 2019. How metaphysical commitments shape the study of psychological mechanisms. *Theory & Psychology* [Online], 29(5), pp.579–600. https://doi.org/10.1177/0959354319860591.

Hodgkin, A.L. and Huxley, A.F., 1952. A quantitative description of membrane current and its application to conduction and excitation in nerve. *The Journal of Physiology* [Online], 117(4), pp.500–544. https://doi.org/10.1113/jphysiol.1952.sp004764.

Illari, P., 2013. Mechanistic Explanation: Integrating the Ontic and Epistemic. *Erkenntnis* [Online], 78(S2), pp.237–255. https://doi.org/10.1007/s10670-013-9511-y.

Izadi, S., Anandakrishnan, R. and Onufriev, A.V., 2014. Building Water Models: A Different Approach. *The Journal of Physical Chemistry Letters* [Online], 5(21), pp.3863–3871. https://doi.org/10.1021/jz501780a.

Kaplan, D.M., 2011. Explanation and description in computational neuroscience. *Synthese* [Online], 183(3), pp.339–373. https://doi.org/10.1007/s11229-011-9970-0.

Keen, S., 2022. The appallingly bad neoclassical economics of climate change. In: B.K. Gills and J. Morgan, eds. *Economics and Climate Emergency*, *Rethinking globalizations*. London; New York: Routledge, pp.79–107.

Lombrozo, T., 2016. Explanatory Preferences Shape Learning and Inference. *Trends in Cognitive Sciences* [Online], 20(10), pp.748–759. https://doi.org/10.1016/j.tics.2016.08.001.

Machamer, P., Darden, L. and Craver, C.F., 2000. Thinking about Mechanisms. *Philosophy of Science* [Online], 67(1), pp.1–25. https://doi.org/10.1086/392759.

Mackonis, A., 2013. Inference to the best explanation, coherence and other explanatory virtues. *Synthese* [Online], 190(6), pp.975–995. https://doi.org/10.1007/s11229-011-0054-y.

Mantzavinos, C., 2016. *Explanatory Pluralism* [Online]. Cambridge: Cambridge University Press. https://doi.org/10.1017/CBO9781316440599.

Parker, G., 1978. Searching for Mates. In: J.R. Krebs and N.B. Davies, eds. *Behavioural Ecology: An Evolutionary Approach*. Oxford: Blackwell Scientific, pp.214–244.

Plutynski, A., 2013. Cancer and the goals of integration. *Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences* [Online], 44(4), pp.466–476. https://doi.org/10.1016/j.shpsc.2013.03.019.

Potochnik, A., 2010. Explanatory Independence and Epistemic Interdependence: A Case Study of the Optimality Approach. *The British Journal for the Philosophy of Science* [Online], 61(1), pp.213–233. https://doi.org/10.1093/bjps/axp022.

Potochnik, A., 2015. The diverse aims of science. *Studies in History and Philosophy of Science Part A* [Online]. Special Section: Formal Epistemology and the Legacy of Logical Empiricism, 53, pp.71–80. https://doi.org/10.1016/j.shpsa.2015.05.008.

Povich, M. and Craver, C.F., 2018. Because Without Cause: Non-Causal Explanations in Science and Mathematics (review). *Philosophical Review* [Online], 127(3), pp.422–426. https://doi.org/10.1215/00318108-6718870.

Rice, C., 2015. Moving Beyond Causes: Optimality Models and Scientific Explanation. *Noûs* [Online], 49(3), pp.589–615. https://doi.org/10.1111/nous.12042.

Rice, C. and Rohwer, Y., 2020. How to Reconcile a Unified Account of Explanation with Explanatory Diversity. *Foundations of Science* [Online], 26(4), pp.1025–1047. https://doi.org/10.1007/s10699-019-09647-y.

Ruben, D.-H., 1990. *Explaining Explanation*. 1st ed., *The problems of philosophy: their past and present*. New York: Routledge.

Salmon, W.C., 1989. Four decades of scientific explanation. In: P. Kitcher and W.C. Salmon, eds. *Scientific Explanation* [Online], *Minnesota studies in the philosophy of science*, 13. Minneapolis: University of Minnesota Press, pp.3–219. Available at: <http://conservancy.umn.edu/handle/11299/185700> [visited on 4 October 2023].

Strevens, M., 2011. *Depth: An Account of Scientific Explanation*. Cambridge, MA: Harvard University Press.

Waskan, J., 2011. Intelligibility and the CAPE: Combatting Anti-Psychologism about Explanation [Online]. Available at: <http://philsci-archive.pitt.edu/8530/> [visited on 4 October 2023].

Waskan, J., Harmon, I., Higgins, A. and Spino, J., 2014. Three senses of 'Explanation'. In: P. Bello, M. Guarini, M. McShane and B. Scassellati, eds. *Proceedings of the 36th Annual Conference of the Cognitive Science Society*. Austin, TX: Cognitive Science Society, pp.3090–3095.

Woods, J. and Rosales, A., 2010. Virtuous Distortion. In: L. Magnani, W. Carnielli and C. Pizzi, eds. *Model-Based Reasoning in Science and*

Technology: Abduction, Logic, and Computational Discovery [Online], *Studies in Computational Intelligence*. Berlin, Heidelberg: Springer, pp.3–30. https://doi.org/10.1007/978-3-642-15223-8_1.

Woody, A.I., 2015. Re-orienting discussions of scientific explanation: A functional perspective. *Studies in History and Philosophy of Science Part A* [Online], 52, pp.79–87. https://doi.org/10.1016/j.shpsa.2015.03.005.

Wright, C., 2015. The ontic conception of scientific explanation. *Studies in History and Philosophy of Science Part A* [Online], 54, pp.20–30. https://doi.org/10.1016/j.shpsa.2015.06.001.

Wright, C. and van Eck, D., 2018. Ontic Explanation Is either Ontic or Explanatory, but Not Both. *Ergo, an Open Access Journal of Philosophy* [Online], 5(38). https://doi.org/10.3998/ergo.12405314.0005.038.

Ontic or epistemic conception of explanation: A misleading distinction?

Michał Oleksowicz
Nicolaus Copernicus University in Toruń, Poland

Abstract

In this paper, I discuss the differences between ontic and epistemic conceptions of scientific explanation, mainly in relation to the so-called new mechanical philosophy. I emphasize that the debate on conceptions of scientific explanation owes much to the Salmon's ontic-epistemic distinction, although much has changed since his formulations. I focus on the interplay between ontic and epistemic norms and constraints in providing mechanistic explanations. My conceptual analysis serves two aims. Firstly, I formulate some suggestions for recognising that both sets of norms and constraints, ontic and epistemic, are necessary for scientific theorising. Secondly, I emphasize that there are multiple dimensions involved in scientific explanation, rather than clear-cut alternatives between ontic and epistemic aspects. I conclude with a general observation that although contextual aspects of explanations are unavoidable, the epistemic-relativity of our categories, explanations and models can in fact be compatible with their objectivity. Instead of making hastily drawn ontological implications from our theories or models, we should carefully scrutinize them from the ontic-epistemic perspective.

Philosophical Problems in Science (Zagadnienia Filozoficzne w Nauce)
No 74 (2023), pp. 155–187

Keywords
new mechanical philosophy, mechanistic explanation, ontic, epistemic, explanatory norms, explanatory constraints.

1. Introduction

The word "explain" is used in very different contexts. Explaining some phenomenon involves performing operations on its representations to understand the "how" or "why" of this phenomenon. The explanation is then a matter of representing what depends upon what. In this paper, I want to explicate the difference between the ontic and epistemic conceptions of scientific explanation (OC and EC, respectively), mainly linked to the so-called new mechanical philosophy (NMP). For those who are not familiar with the latter approach, I will briefly mention that the NMP is a novel revision of Old Mechanism that takes on theoretical problems from the last fifty years of post-logical empiricist philosophy of science. It is particularly focused on the issue of causal explanations of natural phenomena and offers an overview of various methodologies employed in different sciences (2014a,b). According to the new mechanists, the idea of mechanisms as complex causal systems in the world, and mechanistic explanations (MExs) as tools for discovering such complex systems, are both crucial for understanding natural phenomena (Machamer, Darden and Craver, 2000). In the beginning, NMP aimed at examining the causal "talk" within different sciences and discussed the normative properties that a good explanation ought to have (Craver, 2014). New Mechanists undertook both these aspects, focusing on the mechanisms

in their ontic and epistemic aspects, i.e., as real causal systems in the world and representations of worldly things (2005; 2006; 2008; 2013; 2013; 2014).

In what follows, I will briefly introduce the origins of the NMP's distinction between the ontic and epistemic conception of explanation. Those who are proponents of NMP literature can skip this section and go directly to the second section, in which I analyze the debate over the ontic and epistemic norms and constraints of explanation. In the third section, I discuss the solutions offered by Illari (2013), Kästner (2018), and Kästner and Haueis (2021) to the long-lasting opposition between OC and EC. In the fourth section I point out further problems linked with this debate, i.e., the ambiguity of the term "mechanism", and I articulate a dual ontic-epistemic approach, showing in what sense it may benefit for the philosophical approach to discuss the conceptions and accounts of scientific explanation. I conclude with general observation that although contextual aspects of explanations are unavoidable, the epistemic-relativity of our categories, explanations and models can be compatible with their objectivity. Instead of hastily drawing out ontological implications from our theories or models, we should carefully scrutinize them from an ontic-epistemic perspective.

2. New wave of ontic and epistemic conceptions

W. Salmon, in his analysis regarding scientific explanation, points out that OC originated with José Alberto Coffa, who was:

> a staunch defender of the ontic conception of scientific explanation, and his theory of explanation reflects this attitude. For Coffa, what explains an event is whatever produced it or

> brought it about. [...] The linguistic entities that are often called 'explanations' are statements reporting on the actual explanation. Explanations, in his view, are fully objective and, where explanations of nonhuman facts are concerned, they exist whether or not anyone ever discovers or describes them. Explanations are not epistemically relativized, nor (outside of the realm of human psychology) do they have psychological components, nor do they have pragmatic dimensions (Salmon, 1989, p.133).

This conception was further developed by W. Salmon, who at the same time wavered between two ways of thinking about it (Bokulich, 2016, p.262): whether explanations exist in the world or whether they are something that reports such facts (Salmon, 1989, p.86). Without entering into the historical complexities of the development of the OC and EC to the present, it suffices to say that Salmon further contrasted the OC with EC. In fact, he mainly situated his philosophical focus on explanation against C. Hempel's account. For Salmon "two grand traditions of scientific explanation" (Salmon, 1989, pp.68–69) are: the EC, characterized by its focus on logic and laws, according to which the act of explanation is to show that a phenomenon fits into nomic nexus (generally identified with Hempel's covering model of explanation); and the OC, characterizing causality and explanation as a causal-mechanical explanation, fitting phenomena into natural patterns and regularities (Salmon, 1984, pp.84–134; 1989, pp.320–330).

The OC originated with work of J. Coffa, who had no interest in the discussion on mechanisms, but it was further elaborated by W. Salmon, who was directly engaged in formulating causal-mechanical account of the OC. Although Salmon himself had a conception of mechanisms which at first glance does not comport with conceptions of Glennan, Craver, Bechtel, Darden, Illari, Kästner, etc., it nevertheless seems that the mechanistic revival is deeply indebted to his

philosophical approach (Campaner, 2013). For instance, Salmon's theory already pointed out the crucial role of such notions as production and interaction, the distinction between constitutive and etiological aspects of causal explanation and the usefulness of counterfactuals if interpreted experimentally. Although further nuances of Salmon's view on scientific of explanation are not the aim of my examination here, it is essential to emphasize that Salmon's discussion of OC and EC have profoundly influenced the content of the new mechanistic debate on the metaphysics of explanation.

Among proponents of OC can be included W. Salmon, C. Craver, L. Darden, S. Glennan, P. Illari, M. Povich, T. Knuuttila. Let us now focus on core aspects of OC. While L. Darden (2008, p.959) argues that "mechanism is sought to explain how a phenomenon is produced, how some task is carried out, or how the mechanism as a whole behaves", S. Glennan (2002, p.S348) argues that "the explanation lies not in the logical relationship between these descriptions [of the parts of mechanisms] but in the causal relationships between the parts of the mechanism that produce the behaviour described".C. Craver (2007, p.22) asserts that "the explanandum is the release of one or more quanta of neurotransmitters in the synaptic cleft. The explanans is the mechanism linking the influx of Ca^{2+} into the axon terminal". In another place, C. Craver suggests that "all higher-level causes are fully explained by constitutive mechanisms" (Craver, 2007, p.548).What seems to be common to the above claims is that scientific explanations, conceived in an OC manner, are mechanisms existing in the world. Thus, these explanations are not constituted by sentences, diagrams, models, but by fully objective worldly facts. The most explicit advocate of OC is C. Craver. He defends it in the following words:

> Conceived ontically, however, *the term explanation refers to an objective portion of the causal structure of the world, to the set of factors that produce, underlie, or are otherwise responsible for a phenomenon. Ontic explanations are not texts; they are full-bodied things.* They are not true or false. They are not more or less abstract. They are not more or less complete. They consist in all and only the relevant features of the mechanism in question. There is no question of ontic explanations being "right" or "wrong," or "good" or "bad." They just are (Craver, 2014, p.40 italics added).

The crux of the problem, clearly expressed in the quote above, consists in the fact that some advocates of OC begin with the distinction between representations and worldly mechanisms that are represented, but then they claim that the term "explanation" refers to both the depiction of the things in the world and to things in the world. However, talking about ions in the world and talking about representations of ions in the world is not the same thing. In fact, "what our understanding proceeds 'through' are the representations and models of those entities and activities and the ratiocinative procedures thereon—not the activities and entities themselves" (Wright, 2015, p.26). In other words, identifying explanations with the causes themselves is not only not self-evident (Wright and van Eck, 2018), but confusing. The source of such confusion seems to stem from the attempt to sanction the dependence of OC on how the world is (Craver, 2014). But such a dependence is merely postulated. In reality, there is no conception of explanation that denies this sort of dependency. Any view of scientific explanation that takes explanations to be directed at or about anything at all will be compatible with such commitment. Explanations are about the world, and thus dependent on how it is. This is hardly the special feature of the OC and does not in any way distinguish ontic from non-ontic conceptions. H. de Regt (2017, p.24)

rightly argues that "Salmon's distinction is misleading: explanations, including Salmon's causal-mechanical ones, are always epistemic and not ontic, in the sense that they are items of knowledge". Any explanation seems thus to be an epistemic item or an argument in the broad sense.

Among the main defenders of EC we can find W. Bechtel, B. Sheredos, C. Wright, A. Bokulich, A. Levy, M. Nathan, D. van Eck, R. Frigg, H. de Regt. For proponents of EC, MExs are not things existing in the world but something that reports facts about things in the world. For instance, the tools of EC are descriptions, texts, diagrams or models that provide understanding on how mechanisms are responsible for certain phenomena. Wright and Bechtel (2007, p.51) rightly argue that "explaining refers to ratiocinative practice governed by certain norms that cognizers engage in to make the world more intelligible; the non-cognizant world does not itself so engage". Bechtel and Abrahamsen (2005, p.425) echo the previous claim, emphasizing that:

> it is crucial to note that *offering an explanation is still an epistemic activity and that the mechanism in nature does not directly perform the explanatory work*. *Providing explanations, including mechanistic explanations, is essentially a cognitive activity*. This is particularly obvious when one considers incorrect mechanistic explanationsin such a case one has still appealed to a mechanism, but not one operative in nature [. . .] Thus, *since explanation is itself an epistemic activity, what figures in it are not the mechanisms in the world, but representations* of them." [italics added]

The EC stresses the fact that explanations are cognitive activities. For this reason, they highlight the ways in which science tries to grasp the *explanandum*. The object of explanation is never a mechanism

simpliciter, but the *explanandum* is always embedded within a broader explanatory context. The latter can be understood as the arrangement of instruments, scientific concepts and models, skills and activities of scientists engaged in the research programme aimed at explaining certain phenomena.

Considering the impressive development of the literature and studies dedicated to the modeling view of science, mainly are trying to answer the question of how to understand, provide, and evaluate scientific theories, laws, statements, and models (Meheus and Nickles, 2009; Frigg, 2022). One might think that the debate between OC vs EC has been settled. In the last decade, however, the debate has shifted from the question "what is an explanation" to the *querrelle* on ontic and epistemic norms and constraints on good MEx (Illari, 2013). Such a shift means that philosophers are focused on the question about the kinds of norms and constraints that guide MEx. There is the consensus that scientific explanation is the epistemic phenomenon under which agents develop hypotheses or models and reason under assumptions in very specific contexts. Thus, the concern is not about what scientific explanations are, but what the criteria of good scientific explanations are. We now enter into the intra-epistemic debate on norms and constraints of MEx.

3. Norms and constraints

From the beginning, it is important to distinguish a "conception of explanation", understood as a view about what explanations are, from an "account of explanation", conceived of as a view about how explanations work (Bokulich, 2016, p.263). For instance, one can reject the OC, but at the same time endorse that many explanations are causal. It

is very useful to distinguish between norms and normative constraints when looking at philosophical accounts of explanation. I will apply the same notions that L. Kästner and P. Haueis used in their recent paper (Kästner and Haueis, 2021). According to them norms can be "understood as general instructions of how to search for mechanism and how to construct good mechanistic explanatory texts" (Kästner and Haueis, 2021, p.1638). Furthermore, "ontic and epistemic norms can be achieved by using specific normative constraints. Different such constraints are the determinates of the determinable epistemic norm of intelligibility or the ontic norms of accuracy and completeness, respectively" (Kästner and Haueis, 2021, p.1638). Briefly put, while norms work as the general instructions for successful mechanistic inquiry, the normative constraints determinate the latter by limiting the search space for mechanisms in different ways.

Kästner and Haueis give the example of an ontic norm in the instruction to describe the causal structure of a mechanism. In the case of an epistemic norm, they point out the need to increase the intelligibility of the *explanandum*. How can one justify the importance of such ontic or epistemic norms? For instance, Craver stresses "the fact that an explanation that contains more relevant detail about the responsible ontic structures are more likely, all things equal, to be able to answer more questions about how the system will behave in a variety of circumstances than is a model that does not aim at getting the ontic structures that underlie the phenomenon right" (Craver, 2014, p.41). In other words, according to Craver, when we follow the ontic norm of parsing the causal structure of the phenomena, we are far more likely to provide a *bona fide* understanding of the *explanandum*. In the case of epistemic norms, Sheredos argues that generality and systematicity are two prototypical epistemic norms, since they make "intelligible any explanation's scope [i.e., explanations in a commu-

nicative, textual and cognitive sense], unifying explanatory practices, and facilitating research and testing by delineating a category of cases to which any explanation is presumed applicable" (Sheredos, 2016, p.933).

Let us now focus more specifically on how ontic and epistemic norms can be determined through the use of ontic or epistemic normative constraints. In the case of ontic constraints, the fundamental ones are: 1) spatial or temporal constraints, 2) the mechanism-to-model-mapping (3M) constraint. Since "the entities and activities in a mechanism are organized spatially, temporally and actively such that they produce the phenomenon" (Craver and Darden, 2013, p.20) it is crucial to regard the spatial and temporal organization of mechanisms. The first one consists in locations, sizes, shapes, and orientations of components; while the second one pertains to the orders, rates, and durations of stages. Both spatial and temporal constraints are then crucial for the identification of parts and activities of mechanisms. Apart from the spatiotemporal constraints, one may opt for determination of ontic norms by the use of 3M constraint. D. Kaplan and C. Craver specify the 3M requirement in the following words:

> (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).

This quote suggests that explanation should rely on ontic constraints to help in providing a good causal explanation. That it is not to be fulfilled *simpliciter* is obvious, if one considers that explanatory accounts are strongly idealized and contain falsehoods (Potochnik,

2017). Being aware of the puzzling character of idealization or the explanatory incompleteness of models, Craver and Kaplan, offer a more nuanced view of the completeness and accuracy constraints. They call it "the ontic notion of Salmon-completeness" and define this norm as follows: "The Salmon-complete constitutive mechanism for *P* versus *P`* is the set of all and only the factors constitutively relevant to *P* versus *P`*" (Craver and Kaplan, 2020, p.300). This ontic norm does not imply that a model of phenomena has to be complete. Not "all details are necessary", but only those explanatory relevant. The ontic norm of Salmon-completeness itself points out that scientific explanation should be precise about a given *explanandum* via expressing it in contrastive terms (*P* versus *P`*). However, as formulated so, the ontic sense of including in explanation everything that makes a difference to the precise phenomenon in question, does not fulfil "ontic" within the OC. In the latter case "ontic" was referring to the fact that the things in the world do account for phenomena. The ontic norm of Salmon-completeness implies that only some details are necessary for explaining the phenomenon in the broader class of explanatory relevance. This means that ontic constraints play their explanatory role if they are referred to the proper class of epistemic relevance. Thus, employing the ontic norm of Salmon-completeness pushes us to adopt the non-ontic conception of explanation.

In the case of epistemic constraints, the following are crucial: 1) heuristic strategies of decomposition and localization, 2) abstraction and idealization. In the first case, Bechtel and Richardson (2010) rightly note that such heuristic strategies can be seen as basically compatible with the above-mentioned ontic spatiotemporal constraints. The main point of these strategies is to approximate the behaviour of the system based on the interaction of working parts of mechanisms. These strategies need, at the same time, the integration of functional

and structural descriptions of mechanisms and the mapping of activities to working parts (as suggests, e.g., 3M constraint). This shows that epistemic and ontic constraints can and should be combined in mechanistic inquiry. The strength of decomposition and localization is that they facilitate "an increasingly realistic representation of the explanatory domain, even when the initial representation is seriously distorted" (Bechtel and Richardson, 2010, p.8).

The case of abstraction and idealization strategies suggests that there is no straightforward mechanism-model-mapping in the case of scientific explanation (Parker, 2020). If we use the metaphor of the map, one rather should say that scientists offer the atlas of "explanatory maps" when dealing with *explananda*. In fact, the relation between different mechanisms and the MEx representing stuff in the world may be further illuminated by applying such a metaphor. R. Giere, when discussing the issue of representation, points out that maps represent spatial regions from particular perspectives determined by various human interests (Giere, 1999, pp.81–82). According to him, the operative notion to describe the relationship between models and the world, is not the truth, but rather the similarity or fit, between the model and the world. The map analogy shows that maps are always partial and that they are always maps of something. Map makers and map readers, using interpretative rules, must be able to understand what certain maps represents and how to understand conventions used to prepare the map. The model-based understanding of scientific theorizing proposed by Giere means that scientists generate models using principles, specific conditions, and focus on relevant aspects and relevant degrees of similarity between the model and the target (Giere, 2004).

Craver, well aware of the model-based character of MEx, while defending OC, wants to avoid the problem of idealization by stressing

that "terms like 'true', 'idealized', and 'abstract' apply to representations or models. They do not apply to the ontic structures they represent" (Craver, 2014, p.50). He further argues that the problem of idealization is not a philosophical problem of explanation but of reference. According to him, "the very idea of an idealized model of an explanation commits one, at least implicitly, to the existence of an ontic explanation against which the model can be evaluated" (Craver, 2014, p.50). Contrary to Craver, I think that the very idea of an idealized model faces us with the problem of how such models convey explanatory information? Moving the issue of the idealization from the philosophical domain of explanation to that of reference, would not be a solution to the problem of explanation. When looking at the history of science, we find cases where one can achieve understanding without having a true representation of facts. For instance, one can evoke Maxwell's fictional vortex model treating light as electromagnetic radiation (de Regt, 2015), showing that not only the veridical representations can be explanatory (Bokulich, 2016). It is true that "the goal of building an explanatory text is not to provide the illusion of understanding but rather to provide bona fide understanding" (Craver, 2014, p.49). However, the commitment to a truthful account conveying explanatory information works more as an explanatory ideal in scientific practice than the matter of facts. An *explanandum* is the sum of observational data and concepts, models, hypotheses, etc. Both empirical data and scientific conceptualization have their own limits and only describe a part of reality.

Whether science obtains an explanation of reality or the representations of reality is the question that should not receive yes/no answer if we want to further explore how scientific explanations and objects of scientific investigation are formulated. Following J. Bickle I argue that there is no straightforward mapping of mechanisms onto the world in

the case of scientific explanation. J. Bickle (2008) enumerates four key principles in explaining neural mechanisms. These are principles of observation (occurrences of the hypothesized mechanism are strongly correlated with the occurrences of the behaviors used as experimental measures), negative alteration (intervening to decrease activity of the hypothesized mechanisms must reliably decrease the behaviors used as experimental measures), positive alteration (intervening to increase activity of the hypothesized mechanisms must reliably increase the behaviors used as experimental measures), and integration (the hypothesis about the causal nexus that produces the behaviors used as experimental measures must be connected with as much experimental data as is available about the hypothesized mechanism). These convergent four principles show that the relationships between phenomena and entities' activities are to be discovered and described in a piecemeal way rather than merely "given to us" (whatever the latter claim would mean). These relationships are rather "inferred based on correlations between changes in monitored behaviours or effects that are taken to be indicative of changes in these phenomena and activities. [. . .] This makes explanation fundamentally epistemic" (van Eck, 2015, p.15).

The MEx basically involves, on the one hand, conveying an explanatory and empirical understanding of how entities and activities are organized in the production of the phenomenon to be explained. The explanatory understanding can be expressed as a kind of understanding-why, while the empirical understanding means that we are dealing with domains of empirical inquiry (Khalifa, 2017, pp.1–3). On the other hand, mechanistic understanding results are intimately connected to expertise in the specific scientific field or research programme. In fact, understanding results from the cooperative scientific enterprise performed by many scientists, using a plurality of methods

and experimental practices. I concur with Khalifa that representing mechanisms or causal structures„may be treated as one of the local constraints that should be satisfied in addition to the global constraints (such as, the *explanandum* is approximately true, the *explanans* makes difference to the *explanandum*, the *explanans* satisfies our reasonable ontological requirements) placed on the explanation. This focus on representational processes conveying understanding does not imply a purely psychologistic view on scientific explanation, but rather explores the space of complex methodological and experimental aspects present within the scientific endeavour. It is beyond the aim of this paper to treat this issue in more detail, but it is certainly worth of being further explored.

It is now time to take stock. First of all, in this section I focused on the ontic and epistemic norms and constraints of good MEx, which apply to non-ontic conceptions of explanation. This entering into the intra-epistemic debate has shownthe model-based character of MEx and the importance of mechanistic understanding if one is dealing with the specific accounts of explanation. In my analysis of EC and critics of OC, I followed the convention that "sentences are strings of visual or audible symbols that express propositions; propositions are the abstract entities that carry the meaning of the sentences; facts are concrete things in the world and, unlike sentences or propositions, are not capable of bearing truth or falsity" (Khalifa, 2017, p.148). Secondly, the current debate on conceptions of explanation has shifted from what explanations are towards a discussion on the norms and constraints of MEx. Arguing for normative constraints of explanation was called by some authors "the normative turn" (Sheredos, 2016, p.921; Wright and van Eck, 2018, p.1023). According to these authors, with whom I concur, this debate shows that the OC after the normative

turn is abandoned (Bokulich, Sheredos, van Eck, Wright) or at least "in retreat" (Potochnik, 2018). In what follows I want to shed further light on the consequences of "the normative turn".

	Ontic conception (OC)	**Epistemic conception (EC)**
Main representants	W. Salmon, C.F. Craver, L. Darden, S. Glennan, P. Illari, M. Povich, T. Knuuttila	W. Bechtel, C. Wright, B. Sheredos, A. Bokulich, A. Levy, M. Nathan, D. van Eck, R. Frigg, H. de Regt
Core idea	the causal structure of the world, that is, the entities and activities and the organization by which they produce the phenomenon of interest	an intelligible model of the activities, entities and their organization that scientists can understand, manipulate, and communicate, in order to move ahead in the research program
Vehicles of explanation	full-bodied things; neither representations nor texts these things can be: causes, mechanisms in the world, facts, events, set of factors	representations of mechanisms in the world; such as internal mental representations or external to the cognitive agent (diagrams, linguistic descriptions, mathematical equations, physical models, etc.)
Norms	truth	explanation aims to attain the understanding ontic norms: the instruction to describe the causal structure of the mechanism epistemic norms: to increase the intelligibility, generality, systematicity, integration
Constraints	all and only the relevant features of the mechanism in question	ontic constraints: accuracy and completeness, spatial and temporal, the mechanism-to-model-mapping (3M) epistemic constraints: heuristic strategies of decomposition and localization, the abstraction and idealization

4. What is left after "normative turn"?

P. Illari (2013, pp.248–252) has tried to argue that both ontic and epistemic constraints should be recognized and reconciled since both of them are essential. "Without the first constraint, we are not explaining the production of a phenomenon by a mechanism; without the second, we do not achieve the understanding essential to explanation" (Illari, 2013, p.250). Her reading of ontic and epistemic constraints clarifies that the goal of science to reveal the causal structure of the world and the goal of science to achieve a communicable understanding are both necessary. This sort of clarification is unproblematic. However, when Illari tries to spell out differences between Craver's (OC) and Bechtel's (EC) accounts, she discusses the case of prioritising one norm over the other. I agree that we may grant priority to the ontic or epistemic norms, but such a move should not be guided by *a priori* principles which we employ in scientific reasoning. Moreover, according to her account, both norms work together in order to generate a successful MEx. Without the first one we cannot describe the (causal) structure of the world, while without the second one we cannot build a model of the activities, entities and their organization (Illari, 2013, p.250). Again, I wholeheartedly agree with the relevance of ontic and epistemic norms and normative constraints for building MEx in an integrative way. Let us now focus on how to provide such an integration. but how to do it?

L. Kästner and P. Haueis (2021) fill the gap left by Illari, since they convincingly show how ontic and epistemic norms work together through mechanistic inquiry as a whole. First of all, they emphasize that mechanistic discovery typically starts with characterizing phenomenon via different epistemic activities (e.g., modeling, experimenting) in which scientists perform various epistemic operations

(e.g., injecting a current in a neuron) to track different activities and parts of the mechanism which is investigated. Secondly, they argue that epistemic activities containing different models, skills and instruments in the process of discovery do not stand in opposition to the realism of the entities and activities constituting mechanisms actually investigated. In other words, there are interacting multiple dimensions in the elaboration of scientific explanation, rather than a clear-cut alternative between ontic and epistemic norms and constraints. It stems from their analysis that it remains crucial to trace out different stages of the discovery process, with some phases related to the ontic point of view, and others more oriented to epistemic aspects. What is the most interesting aspect of their paper (Kästner and Haueis, 2021)(Kästner and Haueis, 2021) is that they aim to show how ontic constraints may directly or indirectly constrain some epistemic activities; and vice versa, i.e., they show how epistemic norms may help in choosing the accuracy or completeness ontic norms when dealing with empirical findings that conflict with the currently most plausible models of a mechanism.

Kästner and Haueis, in a similar way to Illari, have bracketed the metaphysical issue of explanation from their discussion, by claiming that mechanistic inquiry is both ontically and epistemically constrained. They have gone further than Illari, since apart from integrating both sets of norms and constraints they have shown how ontic constraints guide mechanism discovery from the bottom up and how epistemic constraints help with anomaly resolution. It seems that the main solution to the controversy about the priority of certain norms or constraints comes from considering both of them from a reconciliatory and diachronic perspective. That is, in scientific practice fulfilling both epistemic and ontic norms "requires a kind of dynamic and temporally extended zig-zag between distinct explanatory prac-

tices" (Sheredos, 2016, p.943). In fact, Illari concludes her paper by noting that any successful disentangling of our theories, ontically and epistemically constrained, "will need to look at what is happening over time, rather than at a single time" (Illari, 2013, p.254). "Our good mechanistic explanations are always the result of a struggle to satisfy both ontic and epistemic constraints" (Illari, 2013, p.254). It is the apt correlation between the methods of observation/analysis (epistemic aspect) and the data itself (ontic aspect), that brings about the success of MEx. The ontic and the epistemic norms and constraints are neither alternatives nor directly reconcilable in a simple way. They express different moments of the explanatory strategies and complementary aspects of explanations aimed at the *explananda*. Only the integration of both aspects serves explanatory purposes.

The simplicity of the last claim is a bit striking, considering how long lasting the metaphysical debate on what scientific explanations are and the establishment of criteria for good scientific explanations. Although these debates were evolving in a parallel way, it is important to not conflate them. What gave rise to the debate between OC vs EC, as I suggested at the end of the previous section, was adopting the language suggesting that stuff in the world performs explanatory acts. But causal mechanisms are concrete things in the world and, unlike sentences, propositions, models, etc., are not capable of bearing truth or falsity. Finally, in order to further argue for the more nuanced view on the reconciliation and integration of various norms and constraints, I want to bring up the problem of the boundaries and levels of mechanisms.

The criteria for individuating the boundaries between entities, activities, and mechanisms themselves are the most difficult problems to solve (Kaiser, 2017). There are different principles that may be of help in carving the mechanisms and their components, such as the

individuation of natural boundaries of biological objects (e.g., the cell membrane, the skin, the chain of mountains that borders a specific ecosystem) (Darden, 2008) or deciding upon the strength of interactions (e.g., interactions among parts are generally conceived of as stronger than interactions between parts and environment) (Wimsatt, 1974). Even if one applied these principles, it is possible to get different results, since decompositions of a mechanistic system into parts depends on the explanatory context. For instance, the human body has many systems which are responsible for the various activities of the body, such as the cardiovascular, the respiratory, the nervous, the endocrine, the muscular-skeletal system, etc. The difficulty is that the part decompositions generated by the phenomena will play out in the body in different overlapping ways, e.g., the arteries and veins of the cardiovascular system are also involved in the respiratory system (Kaiser, 2017, pp.37–38). Hence, how we "cut the nature at its joints" depends upon the circumstances in different contexts of explanation.

In this context, L. Kästner (2018), rightly notes that the mechanistic talk of levels does not seem to be a satisfactory way of defining what is at the same level in terms of local composition relations. She proposes instead an approach of epistemic perspectives, largely indebted to Giere's (2006) approach, which I have already mentioned. Kästner characterizes these perspectives along five dimensions (i.e., resolution—different temporal or spatial scale; specificity—what kinds of things can be detected from certain a perspective; point of view—depending on the background theories and taxonomies assumed within certain perspective; sensitivity—with respect to specific factors; scope—allowing for investigating different portions or aspects of phenomena depending on various methodological constraints) and

stresses that the characterization and individuation of them is primary an empirical affair. She explicates the advantage of such an approach in the following words:

> Once the relations between them are worked out, epistemic perspectives characterized along dimensions [. . .] give us the means to locate observed entities and activities at 'the same level' or 'different levels', a way of distinguishing different kinds of dependency relations (such as causal and constitutive relations in mechanisms), and a solid foundation for integrating multiple scientific observations into complex multiperspective mechanistic explanations. We can thus piece together the mechanism mosaic while avoiding the problems associated with local, compositionally related, levels of mechanisms in this context" (Kästner, 2018, pp.77–78).

I am very sympathetic to Kästner's view that both the decomposition of mechanisms and carving their levels deeply depend upon contextual elements (Woodward, 2008, pp.217–220). In fact, "there is an inherent perspectival aspect" (Darden, 2008, p.960) in the case of levels or boundaries of mechanisms. Such perspectivalism, however, does not necessitate "arbitrary choices in individuating phenomena and mechanisms" (Darden, 2008, p.960), but evidences the search for context-sensitive considerations which are the best from the point of view of specific explanatory purposes. "That different models and observations depend on our purposes does not imply, however, that they do not show anything real. They just emphasize different features, carve out different aspects, or provide different filters" (Kästner, 2018, p.76). I think that MEx presents "a more sophisticated picture of the relation between realistic representation and explanatory understanding" (de Regt, 2015, p.3795), as mentioned in the previous section. On the one hand, epistemic perspectives on explanations show that

scientific knowledge is inextricably bounded to the modeler's knowledge, employed tools and pragmatic concepts. On the other hand, the perspectival approach does not merely trigger unlimited explanatory pluralism. Rather admitting many epistemic perspectives is useful in the function of finding the best explanation in certain context.

Analysing the MEx through epistemic perspectives sheds further light on the relevance of ontic and epistemic norms and constraints for building MEx in an integrative way. It shows that mechanisms in the world are not doing the explanatory job, but on the contrary, investigating a phenomenon from multiple different perspectives is what builds the MEx. Such an epistemic conception of MEx does not entail neglecting the objectively real character of causal mechanisms, but forces us to admit partial and perspectival character of the explanatory work.

5. A dual ontic-epistemic approach

The aforementioned characterization of boundaries, levels or causal mechanisms fits very well the EC, i.e., boundaries, levels and causal mechanisms constitute idealized representations of processes. In the case of causal mechanisms, D. Nicholson (2012, p.160) rightly argues that "explanations always presuppose a context that specifies what is to be explained and how much detail will suffice for a satisfying answer, [...] it is this very epistemic context that determines how causal mechanisms are individuated and what details are featured in them". If this is so, it seems that the term "mechanism" does not necessarily refer to worldly causal mechanisms. What does it refer to then?

When speaking about mechanisms, it is quite intuitive to think about mechanisms "out there in the world" and MExs or models that depict them. However, as Nathan (Nathan, 2021, pp.171–172) notes, the term "mechanism" is ambiguous and may refer to both aspects, that is, to things in the world and to their representations. Since MExs represent entities and activities in the world, it may seem to be quite obvious that claims about representations are claims about the mechanisms in the world. In fact, such a blending together of two distinct claims may stem from the OC erroneous assumption that "ontic explanations are not texts; they are full-bodied things" (Craver, 2014, p.40). The ambiguity of the term "mechanism" would be then an additional evidence of this initial error. Although the distinction between mechanisms and mechanistic models is at the core of mechanistic literature (Glennan, 2005), the free employment of the term "mechanism" to both entities and their representations probably has further added to the confusion that stuff in the world performs explanatory acts. One of the consequences of the aforementioned epistemic perspectival approach would be the claim that real mechanisms are not just the represented mechanisms.

If mechanisms "out in the world" should be kept distinct from the represented mechanisms, what about the use of the term "mechanism"? Should it be abandoned? According to Nathan (2021, pp.162–190), NMP has the merit of having shown how the concept of mechanism always has a two-sided nature: on the one hand, mechanisms are "out in the world", on the other hand, they are a model-theoretic construction and a mode of scientific representation. Mechanisms are central for constructing a scientific theory, although they are "black boxes". This means that mechanisms work as placeholders—frames or difference-makers—in a causal explanation represented in a model (Nathan, 2021, p.133). For Nathan, the practice of black-

boxing consists of three phases: 1) the framing stage of specifying the *explanandum*, 2) the difference-making stage of providing a causal explanation of the *explanandum*, 3) the representation stage which determines how the difference-makers should be portrayed via the abstraction and idealization strategies. Mechanistic models are distinguished by their underlying structure (e.g., real physical entities representationally related to some abstract systems) and their interpretational capability. Both actual reference and imagined elements are part of the model, and the model's interpretation must explain their relationship and how the model represents phenomena.

It is important to emphasize that representations in scientific practice are not only involved in the *explanans*, but are also the *explanandum*, which corresponds to Nathan's first stage of black-boxing. This is so because scientists explain the phenomenon-as-represented. In other words, the explanation is always conceptualized within the particular explanatory context (Bokulich, 2011). The phenomenon-as-represented means a representation shared by a community of researchers, not just the subjective state of cognition or particular linguistic or conceptual description. That these explanatory choices are not arbitrary, results from the fact that "the world constrains which representations are, or are not, going to be adequate for a given explanatory context" (Bokulich, 2018, p.802). Such a conception of explanation recognizes the presence and joint working of both ontic and epistemic norms and constraints.

My colleagues and I have discussed the problem of the definition of species, suggesting that the concept of species is better understood within a dual ontic-epistemic approach (Marcacci, Oleksowicz and Conti, 2023). We have chosen this concept as the case study, since it plays various roles in biological investigations. On the one hand, it is based on certain ontic assumptions about the properties of species

and the cause of an individual's belonging to a species; on the other hand, it describes this membership as a consequence of domain specific methodological operations and conceptual assumptions. First of all, we argued that the concept of causal mechanism is not a decisive argument for realism about natural kinds or species. In fact, both the consideration of underlying causal processes and explanatory interests play an indispensable role in recent approaches to natural kinds. Secondly, in agreement with Kästner's view on the possible variety of ontological commitments of epistemic perspective approach, we contend that the perspectival character of the concept of mechanism is not a decisive argument for nominalism about natural kinds or species. Thirdly, the application of MEx entails theory-dependent pluralism about natural kinds or species.

Let us briefly further comment these three points. The last one means that the explanatory pluralist stance is the viable option from the mechanistic point of view. The interest-relativity of scientific classificatory categories, such as species, entails that various epistemic strategies and constraints remain essential and irreducible features of any biological explanation in case of species. Referring to the first and the second point, we argued that neither realism nor nominalism about natural kinds or species is the solution to the problem. The viable solution, if species are to be grasped within ontic-epistemic approach, can be formulated in the following form:

> Close attention to the various accounts of species shows that they play important and distinct roles within the sciences. On the one hand, they work as metaphysical posits; on the other, as explanatory postulates. In the former case, the objectivity of species is what grounds the objectivity of explanations, and sound explanations of species require us to identify the relevant factors at work in evolutionary processes. As explanatory postulates, they play a specific role restricted to the context of

> a particular theoretical framework or model (Nathan, 2023). In this case, species are essentially preliminary hypotheses or theoretical units awaiting to be replaced by more perspicuous explanatory elements" (Marcacci, Oleksowicz and Conti, 2023, p.12).

This distinction between species *de re* and explanatory species provides the conceptual resources to rethink the presence of ontic and epistemic norms and constraints in the long-standing debate on natural kinds and the notion of species. This distinction parallels that one between mechanisms out in the world and explanatory mechanisms. Both these distinctions help us to note that while proponents of OC would argue that species *de re* or mechanisms *de re* do the explanatory job, proponents of EC argue that this is not the case. The inconclusiveness, as it may seem, of the debate on boundaries or levels of mechanisms, or on the definition of species can be expressed through the following philosophical maxim: "distilling metaphysical implications from scientific explanations requires close attention to explanatory practice" (Love and Nathan, 2015, p.773). If contextual aspects in the explanations are unavoidable, then we should moderate the ontological implications drawn from our models. Not because there is "nothing out there", but because there is in fact more than we expect to be there. The epistemic-relativity of our categories, explanations and models can be compatible with their objectivity. The mechanistic strategy is the illuminating one, since it pays attention to how epistemic and ontic norms and constraints are intertwined within scientific explanation, and how such an objectivity may be achieved.

Conclusions

NMP has the merit of having shown how the concept of mechanism as a tool for representation has always a two-sided nature: on the one hand, mechanisms are things in the world, on the other hand, they are model-theoretic constructions. Mechanisms are central for constructing a scientific theory, although they are theoretical placeholders, that is, indicators of what a theory is built around. If one neglects this feature of the term "mechanism", one will not understand the importance of integrating ontic and epistemic norms and constraints within MEx.

Today the crux of the debate is the interplay between different norms and constraints in providing MEx. It seems that the scale is tipped towards the proponents of EC. The OC remains under attack for the set of reasons previously discussed: the stuff in the world does not perform explanatory acts; the crucial role of general principles in scientific reasoning; scientific reasoning relies heavily on abstraction/idealization/generalization; in scientific practice, we deal rather with highly idealized representations of causal processes rather than with real mechanisms; we do not have direct access to the phenomenon, but we deal with phenomena-as-represented which are previously conceptualized; currently dominant modelling strategy of science evidences the crucial role of abstraction and idealization, etc. Briefly put, there is no scientific explanation without intentional agents who try to decipher things out there in the world. However, this does not imply that there are no things "out in the world".

What are then the main conclusions? First of all, the ontic features of the world do not alone settle all questions about the adequacy of an explanation, but the latter is rather settled by an evaluation of our explanatory practices and the features of our models. An increased

awareness of the presence of multiple dimensions within scientific explanations (e.g., ontic and epistemic norms and constraints involved in the elaboration of the concept of mechanistic boundaries or levels, or the definition of species), can aid in comprehending distinct strategies employed in the various sciences, and help to understand how they cooperate to adequately account for complex phenomena. This brings us to the last but not least conclusion.

The ontic and the epistemic norms and constraints express different moments of the explanatory procedures and two complementary aspects of specific accounts of explanations aimed at the *explananda*. The latter fact implies that only the integration of both aspects serves explanatory purposes. Our analysis of ontic-epistemic debate shows that one cannot simply read off truths, or the truth about what the world is like. At the same time, it does not imply that every claim is equivalent. On the contrary, via the use of our models, concepts, and theories, we have only a piecemeal formulation of partial knowledge about reality. Dependence on how the world is will be a commitment entailed by any good conception and account of explanation.

Bibliography

Andersen, H., 2014a. A Field Guide to Mechanisms: Part I. *Philosophy Compass* [Online], 9(4), pp.274–283. https://doi.org/10.1111/phc3.12119.

Andersen, H., 2014b. A Field Guide to Mechanisms: Part II. *Philosophy Compass* [Online], 9(4), pp.284–293. https://doi.org/10.1111/phc3.12118.

Bechtel, W. and Abrahamsen, A., 2005. Explanation: a mechanist alternative. *Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences* [Online], 36(2), pp.421–441. https://doi.org/10.1016/j.shpsc.2005.03.010.

Bechtel, W. and Richardson, R.C., 2010. *Discovering complexity: decomposition and localization as strategies in scientific research*. Cambridge, MA: MIT Press.

Bickle, J., 2008. Real reduction in real neuroscience: metascience, not philosophy of science (and certainly not metaphysics!). In: J. Hohwy and J. Kallestrup, eds. *Being Reduced: New Essays on Reduction, Explanation, and Causation* [Online]. Oxford: Oxford University Press, pp.34–51. https://doi.org/10.1093/acprof:oso.

Bokulich, A., 2011. How scientific models can explain. *Synthese* [Online], 180(1), pp.33–45. https://doi.org/10.1007/s11229-009-9565-1.

Bokulich, A., 2016. Fiction As a Vehicle for Truth: Moving Beyond the Ontic Conception. *The Monist* [Online], 99(3), pp.260–279. https://doi.org/10.1093/monist/onw004.

Bokulich, A., 2018. Representing and Explaining: The Eikonic Conception of Scientific Explanation. *Philosophy of Science* [Online], 85(5), pp.793–805. https://doi.org/10.1086/699693.

Campaner, R., 2013. Mechanistic and Neo-mechanistic Accounts of Causation: How Salmon Already Got (Much of) It Right. *Metatheoria*, 3(2), pp.81–98.

Craver, C.F., 2006. When mechanistic models explain. *Synthese* [Online], 153(3), pp.355–376. https://doi.org/10.1007/s11229-006-9097-x.

Craver, C.F., 2007. *Explaining the Brain: Mechanisms and the Mosaic Unity of Neuroscience*. Oxford: Oxford University Press.

Craver, C.F., 2014. The Ontic Account of Scientific Explanation. In: M.I. Kaiser, O.R. Scholz, D. Plenge and A. Hüttemann, eds. *Explanation in the Special Sciences: The Case of Biology and History* [Online]. Vol. 367, *Synthese Library*. Dordrecht: Springer Netherlands, pp.27–52. https://doi.org/10.1007/978-94-007-7563-3_2.

Craver, C.F. and Darden, L., 2013. *In Search of Mechanisms: Discoveries across the Life Sciences* [Online]. Chicago, IL: University of Chicago Press. Available at: <https://press.uchicago.edu/ucp/books/book/chicago/I/bo16123713.html> [visited on 29 September 2023].

Craver, C.F. and Kaplan, D.M., 2020. Are More Details Better? On the Norms of Completeness for Mechanistic Explanations. *The British Journal for the Philosophy of Science* [Online], 71(1), pp.287–319. https://doi.org/10.1093/bjps/axy015.

Darden, L., 2008. Thinking Again about Biological Mechanisms. *Philosophy of Science* [Online], 75(5), pp.958–969. https://doi.org/10.1086/594538.

van Eck, D., 2015. Reconciling Ontic and Epistemic Constraints on Mechanistic Explanation, Epistemically. *Axiomathes* [Online], 25, pp.5–22. https://doi.org/10.1007/s10516-014-9243-x.

Frigg, R., 2022. *Models and Theories: A Philosophical Inquiry* [Online]. London: Routledge. https://doi.org/10.4324/9781003285106.

Giere, R.N., 1999. *Science Without Laws*, *Science and its conceptual foundations*. Chicago: University of Chicago Press.

Giere, R.N., 2004. How Models Are Used to Represent Reality. *Philosophy of Science* [Online], 71(5), pp.742–752. https://doi.org/10.1086/425063.

Giere, R.N., 2006. *Scientific Perspectivism*. Chicago, IL: University of Chicago Press.

Glennan, S., 2002. Rethinking Mechanistic Explanation. *Philosophy of Science* [Online], 69(S3), pp.342–353. https://doi.org/10.1086/341857.

Glennan, S., 2005. Modeling mechanisms. *Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences* [Online]. Mechanisms in biology, 36(2), pp.443–464. https://doi.org/10.1016/j.shpsc.2005.03.011.

Illari, P., 2013. Mechanistic Explanation: Integrating the Ontic and Epistemic. *Erkenntnis* [Online], 78(S2), pp.237–255. https://doi.org/10.1007/s10670-013-9511-y.

Kaiser, M.I., 2017. The components and boundaries of mechanisms. In: S. Glennan and P.M. Illari, eds. *The Routledge Handbook of Mechanisms and Mechanical Philosophy*, *Routledge handbooks in philosophy*. London; New York: Routledge, pp.116–130.

Kaplan, D.M. and Craver, C.F., 2011. The Explanatory Force of Dynamical and Mathematical Models in Neuroscience: A Mechanistic Perspective. *Philosophy of Science* [Online], 78(4), pp.601–627. https://doi.org/10.1086/661755.

Kästner, L., 2018. Integrating mechanistic explanations through epistemic perspectives. *Studies in History and Philosophy of Science Part A* [Online], 68, pp.68–79. https://doi.org/10.1016/j.shpsa.2018.01.011.

Kästner, L. and Haueis, P., 2021. Discovering Patterns: On the Norms of Mechanistic Inquiry. *Erkenntnis* [Online], 86(6), pp.1635–1660. https://doi.org/10.1007/s10670-019-00174-7.

Khalifa, K., 2017. *Understanding, Explanation, and Scientific Knowledge* [Online]. Cambridge: Cambridge University Press. https://doi.org/10.1017/9781108164276.

Levy, A., 2013. Three kinds of new mechanism. *Biology & Philosophy* [Online], 28(1), pp.99–114. https://doi.org/10.1007/s10539-012-9337-z.

Levy, A., 2014. Machine-Likeness and Explanation by Decomposition. *Philosophers' Imprint*, 14(6), pp.1–15.

Love, A.C. and Nathan, M.J., 2015. The Idealization of Causation in Mechanistic Explanation. *Philosophy of Science* [Online], 82(5), pp.761–774. https://doi.org/10.1086/683263.

Machamer, P., Darden, L. and Craver, C.F., 2000. Thinking about Mechanisms. *Philosophy of Science* [Online], 67(1), pp.1–25. https://doi.org/10.1086/392759.

Marcacci, F., Oleksowicz, M. and Conti, A., 2023. Ontic and Epistemic Differentiation: Mechanistic Problems for Microbiology and Biology. *Foundations of Science* [Online]. https://doi.org/10.1007/s10699-023-09918-9.

Meheus, J. and Nickles, T., eds., 2009. *Models of Discovery and Creativity* [Online]. Dordrecht: Springer Netherlands. https://doi.org/10.1007/978-90-481-3421-2.

Nathan, M.J., 2021. *Black Boxes: How Science Turns Ignorance Into Knowledge*. Oxford, New York: Oxford University Press.

Nathan, M.J., 2023. Causation vs. Causal Explanation: Which Is More Fundamental? *Foundations of Science* [Online], 28(1), pp.441–454. https://doi.org/10.1007/s10699-020-09672-2.

Nicholson, D.J., 2012. The concept of mechanism in biology. *Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences* [Online], 43(1), pp.152–163. https://doi.org/10.1016/j.shpsc.2011.05.014.

Parker, W.S., 2020. Model Evaluation: An Adequacy-for-Purpose View. *Philosophy of Science* [Online], 87(3), pp.457–477. https://doi.org/10.1086/708691.

Potochnik, A., 2017. *Idealization and the Aims of Science* [Online]. Chicago, IL: University of Chicago Press. Available at: <https://press.uchicago.edu/ucp/books/book/chicago/I/bo27128726.html> [visited on 27 October 2023].

Potochnik, A., 2018. Eight Other Questions about Explanation. In: A. Reutlinger and J. Saatsi, eds. *Explanation Beyond Causation: Philosophical Perspectives on Non-Causal Explanations* [Online]. Oxford: Oxford University Press, pp.53–73. https://doi.org/10.1093/oso/9780198777946.003.0004.

de Regt, H.W., 2015. Scientific understanding: truth or dare? *Synthese* [Online], 192(12), pp.3781–3797. https://doi.org/10.1007/s11229-014-0538-7.

de Regt, H.W., 2017. *Understanding Scientific Understanding*, *Oxford Studies in Philosophy of Science*. Oxford, New York: Oxford University Press.

Salmon, W.C., 1984. *Scientific Explanation and the Causal Structure of the World* [Online]. Princeton: Princeton University Press. Available at: <https://press.princeton.edu/books/paperback/9780691101705/scientific-explanation-and-the-causal-structure-of-the-world> [visited on 27 October 2023].

Salmon, W.C., 1989. Four decades of scientific explanation. In: P. Kitcher and W.C. Salmon, eds. *Scientific Explanation* [Online], *Minnesota studies in the philosophy of science*, 13. Minneapolis: University of Minnesota Press, pp.3–219. Available at: <http://conservancy.umn.edu/handle/11299/185700> [visited on 4 October 2023].

Sheredos, B., 2016. Re-reconciling the Epistemic and Ontic Views of Explanation (Or, Why the Ontic View Cannot Support Norms of Generality). *Erkenntnis* [Online], 81(5), pp.919–949. https://doi.org/10.1007/s10670-015-9775-5.

Wimsatt, W.C., 1974. Complexity and Organization. In: K.F. Schaffner and R.S. Cohen, eds. *PSA 1972: Proceedings of the 1972 Biennial Meeting of the Philosophy of Science Association* [Online], *Boston Studies in the Philosophy of Science*, 20. Dordrecht: Springer Netherlands, pp.67–86. https://doi.org/10.1007/978-94-010-2140-1_5.

Woodward, J.F., 2008. Comment: Levels of Explanation and Variable Choice. In: K.S. Kendler and J. Parnas, eds. *Philosophical Issues in Psychiatry: Explanation, Phenomenology, and Nosology*. Baltimore: Johns Hopkins University Press, pp.216–235.

Wright, C., 2015. The ontic conception of scientific explanation. *Studies in History and Philosophy of Science Part A* [Online], 54, pp.20–30. https://doi.org/10.1016/j.shpsa.2015.06.001.

Wright, C. and Bechtel, W., 2007. Mechanisms and psychological explanation. In: P. Thagard, ed. *Philosophy of Psychology and Cognitive Science* [Online]. Elsevier, pp.31–79. https://doi.org/10.1016/B978-044451540-7/50019-0.

Wright, C. and van Eck, D., 2018. Ontic Explanation Is either Ontic or Explanatory, but Not Both. *Ergo, an Open Access Journal of Philosophy* [Online], 5(38). https://doi.org/10.3998/ergo.12405314.0005.038.

Book reviews

Recenzje

Exploring the epistemic and ontic conceptions of *Models and Idealizations in Science*

Alejandro Cassini & Juan Redmond (eds.), Models and Idealizations in Science: Artifactual and Fictional Approaches, Springer International Publishing, Cham 2021, pp.xv+270.

This book presents an insightful collection of papers that explore scientific modeling, idealizations, and representation from diverse philosophical perspectives. The book's first chapter offers an extensive, well-written introduction to the topic of scientific models, while the last chapter provides a relevant annotated bibliography on the philosophy of models and idealizations in science. The ten remaining chapters consist of previously unpublished articles that explore certain intricacies of both modeling practices and the ontology and epistemology of models. Although each chapter is self-contained and independent, one can find a few recurrent themes, such as analyses of artifactual and fictional approaches to modeling, or examinations of the role of surrogative reasoning and (de)idealization in modeling practices.

In what follows I provide a brief overview of these papers, pinpointing how some of their contributions might relate to the debate between ontic and epistemic conceptions of explanation.

Natalia Carrillo & Tarja Knuuttila's chapter examines idealization in scientific modeling. A big focal point of the debate on idealization concerns whether idealizations are beneficial or an epistemic deficiency to overcome. If one believes, as proponents of the ontic conception often do, that explanatory texts representing the ontic explanations need to be complete and accurate, then idealizations are something to be eventually replaced—whether by de-idealizing or by textually representing the 'actual explanation'. Here, de-idealization would not only be achievable, but would constitute a worthy goal. Those who adopt the epistemic conception tend to find intrinsic value in idealizations; for they allow scientists to identify the proper level of abstraction, enable selecting relevant factors, or are fundamental towards generalizing or towards unifying.

Nevertheless, both positions view idealizations as deliberate misrepresentations or distortions. Car-

Philosophical Problems in Science (Zagadnienia Filozoficzne w Nauce)
No 74 (2023), pp. 191–197

rillo & Knuuttila reject this view by adopting an artifactual approach. In this approach models are epistemic artefacts, and idealization is a set of assumptions that align different representational tools to construct a model aiming to answer research questions. This approach highlights the difficulty in disentangling epistemic benefits and deficiencies in the model and challenges both the idea of easy de-idealization and of idealizations simply being distortions.

Sympathisers of the epistemic conception might find a strong argument here in that idealization makes the model possible in the first place (Cassini and Redmond, 2021, p.57), for example, by enabling the application of mathematical and computational tools. However, their analysis runs deeper by noting that labeling idealizations 'distortions' assumes one has enough knowledge about the target phenomenon, a flaw which may be present in both sides of the debate. Furthermore, talk about distortions obscures an important dimension of scientific modeling: exploring the possible (how phenomena could be produced) rather than actual. Here, the ontic conception presents a clear disadvantage, as it does not have the resources to explicate such explanatory practices.

Mauricio Suárez & Agnes Bolinska's chapter apply communication theory to analyze the informational content of scientific models. They argue that models can be seen as communication channels, transmitting information about their targets, whereas idealizations and abstractions can be likened to sources of informational noise and equivocation. The authors argue that this analogy can clarify certain modeling practices—for instance, shedding light on the trade-offs involved in minimizing idealization and minimizing abstraction. In this analogy, the explanandum phenomenon is the informational source, whereas the model is the courier that codifies the information.

Surprisingly, the chapter does not discuss machine learning, despite the numerous parallels between their account and machine learning techniques. The authors appear to have missed an opportunity to establish a more fruitful analogy, especially since machine learning models are often explicitly used as scientific models. For instance, the encoder-decoder architecture shares many similarities with the examples discussed in the chapter and is often

employed in physics' simulations. In these contexts, concepts like loss functions, noise, or the dimensionality of encoder/decoder can provide more meaningful positive analogies for the topic at hand.

Nonetheless, the idea of quantifying idealization in terms of noise may be of interest to defenders of the ontic conception, as it measures how 'far away' a complete and accurate explanatory text might be. Detractors might however note that this overlooks the fact that idealizations often enhance the representational relationship between the model and target by sharpening the focus on what is of interest and 'carving the world' at the right seams.

Staying within the analogy, ontic proponents might see a pure signal devoid of noise of equivocation as a worthy goal (notwithstanding that a completely faithful representation is not equivalent to an ontic explanation). However, it is doubtful whether scientific modeling is actually concerned with trying to capture a pure signal (i.e., to represent faithfully, whatever that may mean). Much like Borge's perfect map, such a model would likely be of little use.

Alejandro Cassini's chapter discusses de-idealization in scientific models, emphasizing that its benefits and drawbacks depend on the model's aim. He argues that de-idealization should enhance explanations, predictions, or model effectiveness, rather than seek faithfulness. While he frames the debate in terms of (non-)representationalists and (anti-)realists, he raises points that are relevant to the epistemic/ontic debate. For instance, some models cannot be de-idealized due to their holistic nature (e.g., certain meteorological models), idealization might be irreversible in minimal models, and it is undesirable to de-idealize in models whose purpose is mathematical tractability.

José Diez's chapter outlines a monist account of modeling. His account posits that scientific models are ensembles of entities and their relations, with some entities intended to stand for those in the target system. This includes a contextual constraint determining the required degree of accuracy for a given purpose. The account consists of explaining the conditions for performing a representation and analyzing the success or adequacy of an existing representation. Supposedly this provides a unified account of scientific modeling by providing necessary and 'substantive' conditions without relying on strong fictionalist elements. However, it is

unclear whether this account actually solves, as the author claims, the problem of representation ('in virtue of what does the target [successfully] represent the model?'). The proposed account seems descriptive as to certain success conditions for when one can claim there has been (successful) representation, but it does not answer why the representation was successful.

Roman Frigg & James Nguyen's chapter defends the fictionalist view of scientific models, which takes them to be analogous to characters and places in literary fiction. Their main argument lies in showing how several 'myths' often used to discredit this view are incorrect, highlighting that it is possible to combine the fiction view with an account of scientific representation. From the perspective of the ontic/epistemic debate, the first and third 'myths' are particularly interesting. The first myth suggests that the fiction view regards scientific products as falsehoods, while the third implies that the fiction view opposes representation. The first myth is tackled by separating two notions of fiction: infidelity and imagination. The fiction view supports the latter: models prescribe certain things to be imagined without committing to the truth status model components. Thus, the fiction view conceives of fictions as tools for learning truths about the world. The third myth is tackled by underscoring that the fiction view primarily concerns the ontology of scientific models, not their representational content. Several approaches are then suggested to combine the fiction view with different accounts of scientific representation.

Many scientific developments can be traced back to fictional uses of the imagination. For the fictionalist, viewing models as fictions affords creative freedoms when investigating certain scenarios. While some might argue that this process should simply be seen as a simple heuristic which allows one to grasp the ontic explanation out there, there is also an argument to be made that fictions are part and parcel of the modeling endeavor.

Fiora Salis' chapter proposes an integrated fiction view for the ontology of theoretical models that combines insights from the fictional and artifactual perspectives. In the integration view, theoretical models are human-made artifacts, capable of serving different functions in various contexts while being analogous to fictional stories. These models are complex objects, consisting of model descriptions and proposi-

tional content. Model descriptions, which include linguistic and mathematical symbols prescribing specific imaginings, act as concrete representational tools and serve as props in a game of make-believe. Scientists build models by selecting (and interpreting) the model descriptions that best serve certain purposes and contexts. However, these imaginings do not imply the existence of any fictional entities. Model content is determined by model descriptions in collaboration with principles of generation.

By placing imagination at the centre of the modeling process, the integration view resolves issues that challenge the artifactual view (2021, p.173), such as explaining model building and development, attributing concrete properties to model systems, clarifying the notion of representation of so-called representational tools, and addressing how scientists engage in model-world comparisons. Similarly, and partly by how model descriptions and content are separated, this approach also solves several problems which afflict the fictional view, such as the non-existence of models, the unclear relationship between model descriptions and imaginary systems, difficulties in scientists sharing the same imaginings, and issues with resemblance between imaginary systems and their targets. Model descriptions and content exist and can, therefore, stand in relation. Moreover, model descriptions serve as props that, through their prescriptions to imagine, enable and constrain an agent's imaginings and allow them to share said imaginings.

I found the integrated fiction account to be a noteworthy account that addresses several important issues in the literature. The idea of combining the artefact and the fiction view, while simple, is well executed and makes for a useful tool for the philosopher of science interested in modeling.

Manuel García-Carpintero's chapter posits that utterances about fictional entities and scientific models involve figurative language with clear truth-conditions. He applies this perspective to debates in semantics, specifically addressing supervaluationist models of indeterminacy.

Otávio Bueno's chapter presents a structural account of scientific representation, arguing that reification of structures as abstract entities is unnecessary. Instead, four different strategies are proposed: adopting a modal-structural interpretation of set theory, reconstructing relevant mathematics using second-

order logic, resisting the need for a metaphysical interpretation of set theory, and employing ontologically neutral quantifiers when quantifying over sets.

Juan Redmond's chapter presents an inferential conception of scientific representation addressing the question of how are models used to represent the world. He rejects the idea of correspondence between a model and its target, emphasizing the importance of how users use models through interactive and dynamic processes. This dialogic approach calls into question whether there can be an 'accurate and complete' (textual) explanation irrespective of the uses and users of a model.

Andrés Rivadulla's chapter advocates for an instrumentalist approach to theoretical models in the physical sciences, emphasizing their utility as tools for explaining and predicting phenomena rather than as representations. In his view, theoretical models are idealized constructions that facilitate calculations, explanations, and predictions in scientific inquiry. A key observation is that incompatible theoretical models exist for the same phenomena, emphasizing their use as tools rather than faithful representations. While this observation is perfectly compatible with an epistemic conception, ontic proponents require additional effort to accommodate this observation. Here, they either have to resolve the puzzle of how there can be two or more valid ontic explanations for the same phenomenon, to show that in fact these models are targeting different phenomena, or to show that one (or more) of the incompatible explanations is incorrect.

Overall, this book is a valuable resource for philosophers of science, proficiently investigating topics such as modeling, the fiction view, and the artifactual view, as well as the role of de-idealization in scientific modeling. Although it was not originally focused on ontic and epistemic conceptions, it proves valuable for the debate by including several hidden gems that can be used to highlight problems with the ontic conception. This includes difficulties in accommodating various scientific practices (such as idealization and how-possibly modeling), the fact that there can be several possible explanations for the same phenomenon, or the fact that many explanations often target general, sometimes idealized, phenomena.

Kristian Campbell
González Barman

Cassini, A. and Redmond, J., eds., 2021. *Models and Idealizations in Science: Artifactual and Fictional Approaches* [Online]. Vol. 50, *Logic, Epistemology, and the Unity of Science*. Cham: Springer International Publishing. https://doi.org/10.1007/978-3-030-65802-1.