4TH WORKSHOP ON PHILOSOPHY OF SCIENCE: MODELS, REPRESENTATIONS, AND MODALITY

20-22 January 2025

ORGANIZED BY:

GRUPO DE ESTUDIOS DE FILOSOFÍA DE LAS CIENCIAS DE LA UNIVERSIDAD DE CHILE

REGISTRATION FOR ATTENDANCE:

La inscripción es gratuita y abierta a todxs, pero es obligatoria dada las restricciones de espacio. Para inscribirse como parte del público, contactar a Cristián Soto, cssotto@uchile.cl

Registration is free & open to everyone, but mandatory to best allocate available space. To register as part of the audience, contact: Cristián Soto, cssotto@uchile.cl

ADDRESS:

Room 501, fifth floor, Facultad de Filosofía y Humanidades, Universidad de Chile

GETTING THERE:

- Arriving by car or Uber, with car parking: Av. Capitán Ignacio Carrera Pinto 1035, Ñuñoa, RM, Chile.
- Using public transport, pedestrians or Uber: Av. Grecia 3401, Ñuñoa, RM, Chile
- Further instructions: https://filosofia.uchile.cl/pregrado/bienvenida-2024/Campus-jgm

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10.00-10.20	Cristián Soto (Universidad de Chile, Chile) Welcome and relevant information
10.20-11.00	Juan Larraín Correa (PUC de Chile, Chile) *Exploration and perspectival modelling with model organisms: developmental biology as a case study*
11.00-11.30	Coffee break
11.30-12.10	Ignacio Rojas (Universidad de Buenos Aires, Argentina) *Structural realism, mathematics and modality*
12.10-12.50	Maribel Barroso (Universidad Alberto Hurtado, Chile) *Inducción, Quo Vadis?*
12.40-14.30	Lunch
14.30-15.30	Alejandro Cassini, *keynote address* (Conicet-Universidad de Buenos Aires, Argentina) *What are cosmological models?*
15.30-16.00	Coffee break
16.00-17.00	Roman Frigg, *keynote address* (London School of Economics, UK) *Stabilising understanding*

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10.10-10.50	Felipe Morales-Carbonell (Universidad de Chile, Chile) *How-possible explanations of impossibilities*
10.50-11.20	Coffee break
11.20-12.00	Diego Maltrana (PUC de Valparaíso, Chile) *Laws and dispositions*
12.00-12.40	Bruno Borge (Universidad de Buenos Aires / CONICET, Argentina) *Grounding Modal Modeling: Modalism and the Limits of Empiricism*
12.40-14.30	Lunch
14.30-15.30	Joaquim Giannotti, *keynote address* (Universidad Mayor, Chile) *Metaphysical Models and Aptness: A Plea for Methodological Moderation*
15.30-16.00	Coffee break
16.00-17.00	Otávio Bueno, *keynote address* (University of Miami, U.S.A.) *How Can Models Represent Possibilities?*

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9.30-10.30	Juan Redmond, *keynote address* (Universidad de Valparaíso, Chile) *Hypothesis and Target System in the practice of modelling in science*
10.30-11.30	Guadalupe Mettini, *keynote address* (UNAM, México) *Thought Experiments, Modelling Attitude, and Styles of Reasoning*
11.30-12.00	Coffee break
12.00-13.00	Arezoo Islami, *keynote address* (San Francisco State University, U.S.A.) *Reversal of a Trend: Symmetry Principles and Laws of Nature*
13.10-15.00	Lunch
15.00-16.30	**Roundtable:** ***Assessing the modal modelling strategy*** Cristián Soto [15.00-15.15] Roman Frigg [15.15-15.30] Otávio Bueno [15.30-15.45] Q&A with Audience [15.45-16.30]

Book of Abstracts

Maribel Barroso
(Universidad Alberto Hurtado, Chile)

Inducción, Quo Vadis?

El análisis de la inducción en filosofía de la ciencia se ha llevado a cabo tradicionalmente dentro de un marco argumentativo o inferencial, el cual ha estado vinculado a una manera particular de entender las hipótesis o teorías científicas.

Una manera de ilustrar cómo la tradición ha entendido las hipótesis o teorías es la descripción hempeliana de las teorías como "redes espaciales complejas" que se sostienen en la observación mediante reglas de interpretación empírica y en las que "los nudos" son los términos científicos y "los hilos" son, por un lado, las definiciones y por el otro, las hipótesis fundamentales y derivadas (Hempel 1952). De acuerdo con lo anterior, las hipótesis fundamentales y derivadas de la ciencia se representan como enunciados que se encuentran en una relación de apoyo inferencial con otros enunciados que representan la evidencia. Sin embargo, como es sabido, el apoyo inductivo en este tipo de inferencias es lógicamente inválido.

A principios del siglo XX se llevaron a cabo diversas propuestas para explicar las razones de nuestra creencia en estas inferencias y aunque se discrepó acerca de la naturaleza de la relación de apoyo en estas inferencias, la idea de que esta relación debía plantearse en términos enunciativos fue aceptada unánimemente (Hempel 1996, 351).

Contemporáneamente, Norton (2003; 2005; 2014; 2021) ha señalado que la búsqueda de reglas formales pretendidamente universales para las inferencias inductivas es un enfoque erróneo y ha instado a salir de las estrategias formales para abordar la inducción. En su lugar, Norton ha defendido un soporte local y material para estas inferencias.

Sin embargo, sigue habiendo buenas razones para pensar que tanto la propuesta contemporánea de inducción material como las estrategias justificacionistas de principios del siglo XX, fracasan en sus intentos de dar cuenta de la inducción, así como para considerar que estos fracasos se deben, en gran medida, a su adhesión a una perspectiva lingüística o enunciativa en el tratamiento de la inducción. El objetivo de esta presentación es mostrar que un análisis de la inducción en términos no enunciativos no solo es posible, sino que, además, es mucho más cercano a la práctica científica.

La presente discusión se dividirá en dos partes. En la primera, esbozaré brevemente los intentos de justificar las inferencias inductivas mediante el criterio de «validación» propuesto por Rudolf Carnap (1945; 1947; 1950) y la «vindicación» propuesta por Hans Reichenbach (1949; 1957), así como la reciente teoría material propuesta por John Norton (2003; 2005; 2014; 2021). En la segunda parte, presento los argumentos que he utilizado (Barroso 2023) para criticar el análisis tradicional de la inducción como inadecuado para representar la inducción en la ciencia y demandar un cambio en el tratamiento de la inducción. Finalmente, explico cómo la teoría inductiva del filósofo victoriano William Whewell puede ser el germen de un nuevo relato de la inducción como un razonamiento con base en modelos en lugar de enunciados.

Referencias

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Bruno Borge
(Universidad de Buenos Aires / CONICET, Argentina)

Grounding Modal Modeling: Modalism and the Limits of Empiricism

Several scientific modeling practices have an important modal character. Through the construction and study of models, science explores and justifies claims about what is possible, necessary, or impossible. These practices, usually labeled "modal modeling," raise significant questions for the epistemology and metaphysics of modality. The latter, in particular, is puzzled by modal modeling practices involving hypothetical, "false," idealized, targetless, fictional, and other types of models that appear to have at least partially non-actual targets. Some recent views suggest these models have as their targets possible worlds or possible systems. This perspective appears promising for those who believe that the truthmakers of modal claims are real *possibilia*—or at least entities outside the actual world.

In this paper, I address two issues concerning the scope of metaphysical commitments needed to account for modal modeling practices. Both involve the adoption of modalism, the thesis that reality is primitively modal and that modality is not reducible to non-modal facts.

First, I argue that we can account for modal modeling practices only by appealing to modal properties in the actual world. This is true even for models with apparently non-actual targets. Second, I explore the extent to which modalism can be integrated within an empiricist stance about science. Drawing from arguments in science and philosophy, I claim that the adoption of modalism involves a minimal amount of metaphysical commitments. However, these considerations motivate a reflection on the blurry limits between empiricist and realist stances. I argue that the modalist account of modeling practices stands as a clear example of how empiricists and realists can find a pragmatically fertile common ground.

Otávio Bueno
(University of Miami, U.S.A.)
How Can Models Represent Possibilities?

Scientific models are clearly designed to represent what is actual, about which predictions are typically made. On the inferential conception (Bueno and French [2018], and Bueno and Colyvan [2011]), a central component of scientific representation involves establishing suitable mappings between the empirical set up and parts of the relevant models. This seems to privilege representation of the actual. What goes on in the empirical set up clearly has a counterpart in the model—as long as the constraint of empirical adequacy is met. But how can merely possible, but nonactual, phenomena be similarly represented?

In this paper, I argue that the surplus structure that is typically offered by mathematics provides a significant avenue to address this issue. Models often have built-in structure that locates the phenomena in a space of possibilities. How can such a structure be known? Isn't this precisely the information that a model is supposed to provide in the first place? I address this challenge by identifying what is a condition for a model to represent—a blind spot, or a hinge, depending on the metaphor one prefers to invoke—and the representational possibilities that the model encodes. Both features are contingent and revisable, but they need to be held properly fixed otherwise representation *via* models becomes impossible. I conclude by indicating that the result is a form of modalist empiricism about scientific representation.

References

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Felipe Morales Carbonell
(Universidad de Chile, Chile)
How-possible explanations of impossibilities

Dray (1957) introduced the notion of a *how-possible explanation* (HPE) to handle cases where an explanation seems to answer, not a question of the form '*why* did event E happen?', but a question of the form '*how could* have event E happen?'. Drays' observed that in many cases when an unexpected event occurs that we try to explain, we are satisfied to know what made the event possible, independently of whether we also know what made it necessary for things to happen in that way. Since Dray's book a lively debate has examined the status of this kind of explanation.

Verrault-Julien (2018) has proposed that what distinguishes HPEs from other types of explanations is the type of modal information that they provide; according to them, HPEs provide information to the effect that possibly, p because q (where the relevant sense of possibility is contextually fixed, which would allow us to distinguish between genuine HPEs and so-called 'just-so' stories).

As part of his discussion, Verrault-Julien addresses a potential objection from van Riel (2015), namely that models provide HPEs even though models can be literally impossible (for example, models can embody idealizations that would prevent them to be realized). Verrault-Julien's response is that while models can be impossible, they can nonetheless illustrate how things could have happened (that is, that impossible models can have possible targets). Weisberg (2013) argues, however, that some models are useful even if the *targets* themselves are impossible. Verrault-Julien's answer to this challenge is to say that even in these cases the information the models provide is relevant to possibilities. This is somewhat unsatisfactory.

Here, I want to argue that the class of HPEs Verrault-Julien characterizes is a special case of a more general type of explanation that also covers HPE-*like* explanations that target impossibilities, independently of what they might say about what is possible. The basic idea is roughly as follows. Suppose there is, besides the stock of possible worlds, a stock of impossible worlds (cf. Berto & Jago 2019, Tanaka & Sandgren 2024). In principle (with some caveats), it is not ruled out that explanatory questions arise in or about impossible worlds, similarly to how explanatory questions arise in and about possible worlds. So, if p is true in any world, the question *how-p* can arise. If these questions have answers, they will be HPE-like explanations.

Some well-known issues with impossible world frameworks leave them in an awkward position with regard to why- and how-questions. If the assignment of truth values to atomic propositions in impossible worlds is arbitrary (cf. Priest 2016 and Berto et al 2018), there will not be a good sense of neither why those propositions are true at those worlds nor how it is that they are true. I will sketch some possible solutions to the issue and how they can be applied to the case of HPEs, as well as discuss how the approach accommodates some ideas from structural conceptions of scientific understanding.

Alejandro Cassini

(Conicet-Universidad de Buenos Aires, Argentina)

What are cosmological models?

It is widely acknowledged that modern cosmology reached its scientific status after 1917 when Einstein applied the theory of general relativity to the universe as a whole and produced his first cosmological model. Significantly, although special and general relativity were always called theories, the term model was generally used for the different cosmological models that proliferated during the 1920s and 1930s. Not all those models, however, were models of the theory of general relativity. The standard FLRW models are just a broad family of relativistic cosmological models. Some of those models have a *Big Bang* at the origin of time ($t = 0$), whereas others do not. Besides, there are different cosmological models -with or without a *Big Bang*- that are not FLRW models. Consequently, the popular expression "*Big Bang Theory*" cannot be understood as referring to a theory based on a set of postulates or fundamental hypotheses but rather to an open family of models, composed in turn by several families of models. There is no unique way of classifying all cosmological models and locating them in a single coherent framework.

The current philosophy of scientific models has, by and large, neglected or overlooked cosmological models. For that reason, my approach here will be strongly tentative and provisional. First, there is the problem of the target of cosmological models. This one has been generally described as "the universe as a whole", to use Einstein´s expression. However, the target can be limited to the observable portion of the universe. In any event, each cosmological model must be assessed based on data collected within the observable universe. Second, there is the problem of the requirements for building a cosmological model. They can be reduced to two: a physical theory or set of laws that can be applied to the entire target, and a set of data concerning the initial and boundary conditions necessary to apply that theory or laws and to obtain testable predictions. In present standard cosmology, the fundamental theory is general relativity, but it must be supplemented with many theories, laws, and hypotheses, for instance, from atomic, nuclear, and elementary particle physics. The observational data are those required to assign values to the many empirical parameters of cosmological models, such as the density and the Hubble parameters. Third, there is the question of the idealizations embedded into cosmological models, such as the homogeneity and isotropy of the universe. Many of those idealizations are introduced when some parameters are fixed, for example, when the cosmological constant and the overall pressure of the universe are set to zero (as happens in all FLRW models). Such idealizations can be removed, producing in this way different deidealized models, such as inhomogeneous and/or anisotropic models, or models with positive or negative values for the cosmological constant. Some of those idealizations are introduced for reasons of simplicity and mathematical tractability, not because of empirical constraints. Fourth, there is the question of the known facts about the observable universe that any empirically adequate cosmological model must explain or, at least, accommodate. These facts go from the darkness of the night sky to the absence of antimatter in the observed universe and include the existence of complex structures, such as clusters of galaxies. Fifth and last, there is the question of the aim or purpose of cosmological models. Given the uniqueness and special status of their target, cosmological models cannot intend to be applied to intervene in nature. They are purely theoretical, with no foreseeable practical uses, unlike other scientific models.

I conclude that the study of the idealizations embedded into cosmological models and the reasons why they are introduced will be a fruitful line of research in the philosophy of scientific modeling.

Roman Frigg
(London School of Economics, England)

Stabilising Understanding

Successful science doesn't just represent certain parts or aspects of the world, those representations are the means through which we understand them. Important parts of science achieve this goal through the construction of models. This raises the question: how do models provide understanding of their target systems? Factivists insist that understanding is factive; non-factivists demur and insist that radical departures from the truth need not be an impediment to understanding and should be embraced rather than excised. In this paper we take issues with both sides, but for different reasons. Against non-factivism we argue that a model cannot provide understanding unless it gets those aspects of the target that it aims to understand right: to provide understanding a model must be veridical. We endeavour to establish this conclusion with a thought experiment inspired by the history of physics. This places us on the factivist side of the divide. However, we take issue with existing articulations of factivism, particularly with respect to how they attempt to accommodate the idealised aspects of scientific models within a factivist framework. We then provide our positive account of understanding, which emphasizes the importance of stability across model variations for the noetic value of the involved models and idealisations.

Joaquim Giannotti
(Universidad Mayor, Chile)

Metaphysical Models and Aptness: A Plea for Methodological Moderation

There is a naturalising tendency in contemporary analytic metaphysics. Despite their many forms and shapes, naturalistic approaches share the idea of vindicating a kind of continuity, be it in methods or subject matter, between metaphysics and science. L.A. Paul (2012) has defended the methodological continuity of metaphysics by arguing that metaphysical theorising is analogous to scientific modelling in that metaphysical models are evaluated and constructed in ways analogous to scientific models.

I argue that there is no obvious way of extending evaluative criteria for scientific models to the metaphysical case, even if one sets aside empirical adequacy. I make my case by discussing three evaluative approaches to a model's quality: (i) representational accuracy, (ii) relevant similarity, and (iii) fitness-for-purpose. I argue that the latter criterion is a more promising evaluative standard. However, its application reveals that the methodological continuity between metaphysics and science is coarse-grained or minimal. This result, rather than being an obstacle, reveals a constructive division of labour between the two disciplines.

Arezoo Islami
(San Francisco State University, U.S.A.)

Reversal of a Trend: Symmetry Principles and Laws of Nature

Symmetry principles play a uniquely important role in contemporary physics and comprise a fundamental part of our understanding of the physical nature. Most of these symmetries, unlike those of ancients, are not concerned with objects but with *laws of nature*. Yet prior to the 20th century they were thought to be the *con- sequences* of (dynamical, experimental) laws of nature and not primary to them. Newton, for instance, thought of the Galilean relativity (the equivalence of the inertial frames) as the corollary to the laws of motion in *Principia*. This trend continued till the end of the 19th century, even after Maxwell had formulated his equations of electrodynamics on the basis of Faraday's work. It was Einstein who reversed the trend: he recognized the symmetries hidden in Maxwell equations and regarded them as symmetries of space-time itself. For Einstein, symmetry principles were more fundamental than dynamical laws. They were not only structures governing the laws, as *laws of laws of nature*, but also constraints on the forms the laws could take. The success of this method led to the use of symmetries in astrophysics (e.g. cosmological principles), to the invention (discovery?) of a wide range of symmetries such as gauge symmetries (not symmetries of space-time) and a deeper understanding of symmetry breaking.

The physicist David Gross in his 1995 and 1996 papers argued for the fundamental role of symmetry principles in physics and praised Einstein for the reversal of a trend, as his predecessor Eugene Wigner did (1960, 1963, 1964). Wigner went further and explained that there is a three-layered hierarchy in modern physics: events, laws of nature and symmetry principles. Laws of nature allow us to find and formulate regularities among the phenomena, and symmetries do the same with laws. This scheme is used to explain why laws of nature are mathematical (Islami 2016).

Gross argued that symmetries did not play an explicit role in physics prior to the twentieth century. Conservation laws were considered the consequences of the dynamical laws and not symmetry principles. It was Einstein who, as Gross noted, first realized that symmetry principles must be raised to the status of postulates. A notable case that demonstrates this shift in the attitude toward symmetry principles is the principle of relativity. Galilean relativity which was considered to be a corollary to the laws of motion in Newton's *Principia*, in Einstein's special relativity it was considered to be a postulate from which one derives the laws of motion. Hence was the reversal.

Until the 20th century principles of symmetry played little conscious role in theoretical physics. This situation changed dramatically in the 20th century beginning with Einstein. Einstein's great advance in 1905 was to put symmetry first, to regard the symmetry principle as the primary feature of nature that constrains the allowable dynamical laws. In the latter half of the 20th century symmetry has been the most dominant concept in the exploration and formulation of the fundamental laws of physics. Today it serves as a guiding principle in the search for further unification and progress (Gross 1996).

Gross is right to emphasize a change in physics. In fact, not only symme- tries but also symmetry breaking has been fundamental to the work of theoretical physicists of the 20th and 21st century especially in particle physics. Gross' own work on string theory (necessarily supersymmetric) sits at the current height of such movement. Physics of the twentieth century is physics of symmetry (and symmetry breaking), and this conception is in no small way due to the works of Wigner himself. While Gross is insightful about a change in physics, in my view, he misconstrues the change. It was not Galilean relativity as Galileo and Newton conceived it that was later raised to a postulate by Einstein (1905) but a *fundamentally* different principle. And thus the reversal must be understood in a different way.

David Gross talks as if at issue here is only the status of such principles and their role in the "deductive" structure of physical theories, and as if had Galileo and Newton been smarter, the principle of relativity would have been understood as a postulate. On this we (Gross and I) diverge. Nature as the subject of modern mathematical sciences in my view is not given but is re-conceptualized (or to use Husserl's term, constituted) through use of conceptual, mathematical tools. Every new constitution takes for granted the previous ones, layers are composed on layers. But this very constitution is hidden from the eye of the scientists who take "mathematics and mathematical sciences" which are based on idealization to "represent... as objectively true and actual nature".

For the philosopher, however, this very constitution is of utter importance. Without attention to its historical origins, "science as given in its present form... is mute as a development of meaning" (Husserl 1954, p.58). Peeling away these layers (i.e. de-sedimenting if such task is possible) we hope to understand how physics as physics of symmetry is constituted. Thus we ask, how is such physics possible? In this way, we attempt to revise and correct the Wignerian (and Grossian) historiography of physics which doesn't emphasize its gradual constitution.

Juan Larraín Correa
(PUC de Chile, Chile)

Exploration and perspectival modelling with model organisms: developmental biology as a case study

Model organisms are at the centre of progress in biology but attributing them an excessive representational power and concentrating on a limited group of them, although efficient for research, can have negative consequences, mainly of epistemic nature. Here, I argue that model organisms are exploratory models with a perspectival modelling function, and that a deflated representational power is needed for their proper use. In support of this argument, I will analyse developmental biology as a case study. Firstly, I show that model organisms in developmental biology are not selected because of their representational capabilities, but mainly based on practical criteria. Secondly, I defend that the epistemic organization of developmental biology around questions fosters exploration and perspectival modelling and I propose that developmental biology is a 'model organism situated knowledge'. Lastly, I use the study of the mechanisms of cell fate acquisition during early embryonic development in *C. elegans* and mice as a case study to illustrate how a plurality of model organisms allows exploration and perspectival modelling. The use of model organisms for exploration and perspectival modelling, with a limited representational power, should allow more adequate inferences about human embryonic development and encourage the introduction of more model organisms for a comprehensive navigation of the space of possibilities.

Diego Maltrana
(PUC de Valparaíso, Chile)

Laws and Dispositions

Here, I explore a metaphysics of natural laws that balances dispositionalism and structural elements of scientific theories. The proposal suggests that laws of nature can be understood through a dual approach: interaction-based theories grounded in causal dispositions and structural theories described by regularities or symmetries.

This proposal is connected to a broader topic in the philosophy of science, specifically the distinction between mechanistic and structural theories. While laws of nature associated with interactions are underpinned by causal dispositions, structural elements—such as symmetries in quantum mechanics or classical mechanics—are best understood through a Humean "Best Systems" account.

This approach avoids the metaphysical burden of treating laws as primitive abstractions with causal power. Instead, it proposes that structural regularities emerge from the mechanistic interactions described by dispositional properties. Symmetries and structural laws, such as invariance principles in physics, function as overarching constraints on the behaviors of systems, arising either from contingent arrangements and logical relationships or indirectly through dispositions.

Mechanistic descriptions, on the one hand, pertain to laws that are inherently dispositional, such as those governing the behaviors of charged particles in physics. Properties like mass or electric charge are defined by their causal profiles and interactional capacities. Dispositionalism is a necessary part of metaphysics, particularly for explaining causal interactions in the natural world. However, these dispositions alone are insufficient to account for the full scope of physical laws, especially those that do not involve specific interactions or objects, such as the Principle of Least Action (PLA) or symmetries in quantum mechanics.

Nevertheless, dispositionalism can be reconciled with classical and quantum mechanics, particularly with the PLA and the non-local nature of quantum mechanics, by recognizing that both Lagrangian mechanics and realist interpretations of quantum mechanics correspond to structural theories. These theories constrain interactions but do not directly embody causal dispositions. Instead, the interaction-specific Lagrangian, embedded within the PLA framework and the path integral formulation of quantum physics, carries the dispositional properties that govern physical interactions. Thus, these theories are compatible with dispositionalism when understood as a structural framework for interactional laws.

In summary, the proposal advocates for a refined dispositionalism that acknowledges the limitations of causal explanations while incorporating structural regularities into a naturalistic metaphysics of laws. This dual approach offers a robust account of how laws of nature operate both at the level of interactions (via dispositions) and at the level of general structural constraints (via a Best Systems Account).

Guadalupe Mettini
(UNAM, México)

Thought Experiments, Modelling Attitude, and Styles of Reasoning

Thought experiments are widely used in various scientific disciplines, especially theoretical physics. They are often used to illustrate theoretical principles, to highlight contradictions within a conceptual framework, or to expose inconsistencies in a theory. However, they raise an epistemological problem: how can seemingly speculative strategies such as thought experiments function as valid tools for expanding knowledge? In particular, some thought experiments seem to provide evidence in support of theoretical hypotheses, raising a philosophical puzzle: what accounts for their epistemic efficacy if they do not introduce new empirical data?

Although thought experiments have gradually gained attention within the philosophy of science since the 1980s, it is intriguing that they were not a significant focus within the field earlier. If we accept that the use of thought experiments for various cognitive purposes was a common practice in modern science, it is puzzling that they received little attention in the early development of the philosophy of science. This neglect cannot, at first glance, be explained by the scarcity of thought experiments in the scientific literature. Therefore, the relative absence of this topic in philosophical reflection also requires an explanation.

In this paper I will argue that thought experiments and scientific models belong to the same category of scientific practices because they both employ similar representational strategies. This convergence allows me to offer a plausible account of the epistemic efficacy of thought experiments, using some philosophical conceptualisations developed to understand the epistemic functioning of scientific models. I will argue that, from a historical perspective, both practices can be interpreted within the framework of the modelling attitude, understood as a methodological stance focused on the construction, development and application of models for various cognitive purposes (Suárez, 2024). This form of surrogate reasoning is characteristic of modern science and finds paradigmatic examples in the models developed by Maxwell and Thomson in the 19th century. Thought experiments, like scientific models, are framed in forms of knowledge production based on the construction of representations through the exercise of imagination (McAllister, 2013), which are endowed with specific rules of inference. I will show that one way to make this idea more precise is to link it to the notion of reasoning styles, which are understood as mechanisms for representing possibilities in a domain of inquiry and drawing inferences from them (Bueno, 2012).

I will also argue that the philosophical interest in thought experiments can be reconstructed in an analogous way to the development of the philosophical interest in modelling. Although modelling has been a significant practice in modern science, philosophical interest in models only emerged in the late twentieth century. Something similar happened to thought experiments. In both cases, the suspicion of speculative thinking and the role of imagination in the production of scientific knowledge that prevailed during the period of logical empiricism may explain the lack of substantive philosophical reflection on these devices.

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Juan Redmond
(Universidad de Valparaíso, Chile)

Hypothesis and Target System in the practice of modelling in science

The aim of my talk is to give space to the logical understanding of the notion of hypothesis in the practice of modeling in science. In particular, from this logical approach to the notion of hypothesis we will propose a pragmatic and interactive notion of target system.

Ignacio Rojas
(Universidad de Buenos Aires, Argentina)

Structural Realism, mathematics and modality

Among the many debates that have arisen regarding the ontological proposal of Ontic Structural Realism (OSR), discussed in the literature on the semantic approach to scientific theories and the preponderant role of mathematical models in it, one refers to the possible confusion between mathematical and physical structures. Although, for OSR, the fundamental ontological commitment must be centered on the structures, which formulate our best scientific theories through mathematical formalisms, there is an obvious risk, critics argue, of confusing a formal mathematical entity, the structures, with an ontological category.

Faced with this problem, which Soto (2019) has called the 'collapse thesis', the author has suggested that the structural realist would have to adopt the 'inferential conception' of mathematical representation, which, on the one hand, would allow a clear distinction between the mathematical and the physical, but, on the other hand, would imply weakening his original ontological commitment to structures.

While Ladyman & Ross (2007) had introduced the distinction between the 'formal mode' and the 'material mode' in order to avoid, precisely, the problem of the collapse of the ontological into the mathematical, Soto's (2019) observations open the door for a deeper discussion regarding the role of abstract, formal, mathematical structures in the representation of an structural ontology. Even though Ladyman and Ross (2007), with French (2014), rejects that the OSR adheres to vision according to which the world would be a mathematical entity, the debate on the fundamental ontological role of relations in the structuralist project has brought more doubts than solutions to the criticisms.

According to French (2014), if we understand a structure $\langle D, R \rangle$ as a domain of elements D and their relations R, OSR proposes a reading of 'inverted' ontological dependency (from left to right), where relations are fundamental and elements are derived. However, this 'formal mode' of set-theorical representation could introduce some problems that we would like to address in this work:

(a) it seems to deny, implicitly, one of the main theses of the OSR: that the best contemporary science, such as quantum mechanics, invites us to abandon the ontological concept of individual object as fundamental,
(b) if we deny the fundamentality of objects, the only alternative would be for relations to assume the ontological role, giving space to the criticism of the incoherence of postulating 'relations without relata',
(c) if, for OSR, the modal character of structures is fundamental (Ladyman & Ross 2007 and French 2014), the possibilities of expressing such a commitment through this kind of structures are strongly restricted to only certain set-theoretical formulations of 'possible worlds.

The aim of this work is to defend the idea that the most appropriate 'formal mode' for OSR could be to adopt an approach based on category theory, where the notions of objects, relations and modality can be expressed in a more flexible way, and without set-theoretical commitments, leaving open the task to articulate the most appropriate ontological concepts for a structuralist vision of the physical world.

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