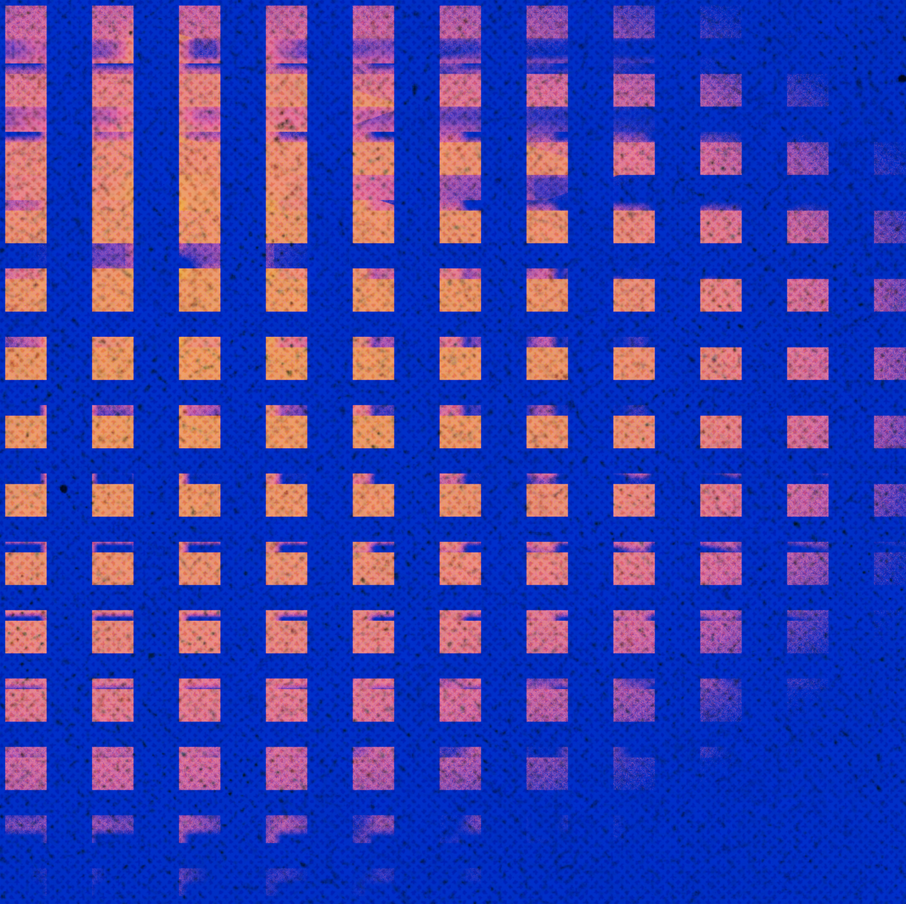
Philosophy of Science in the 21st Century

Contributions of Metatheoretical Structuralism

Edited by
Cláudio Abreu



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Philosophy of Science in the 21^{st} Century

Contributions of Metatheoretical Structuralism

NEL - Núcleo de Epistemologia e Lógica

Brasil
Dezembro - 2023

Universidade Federal de Santa Catarina
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COLEÇÃO RUMOS DA EPISTEMOLOGIA, V. 22

Cláudio Abreu
(ed.)

Philosophy of Science in the 21st Century

Contributions of Metatheoretical Structuralism

NEL – Núcleo de Epistemologia e Lógica
Universidade Federal de Santa Catarina
Florianópolis, 2023

ISBN: 978-85-8328-232-7 (e-book)

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Este volume

Editor:	Cláudio Abreu
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FICHA CATALOGRÁFICA
Catalogação na fonte pela Biblioteca Universitária
da Universidade Federal de Santa Catarina

P568 Philosophy of science in the 21st century [recurso eletrônico]:
contributions of metatheoretical structuralism / Abreu, Cláudio (ed.)
– Florianópolis : NEL/UFSC, 2023.
258 p.: – (Coleção Rumos da epistemologia; v. 22)

E-book (PDF).
ISBN 978-85-8328-232-7

1. Filosofia. 2. Lógica. 3. Ciência. I. Abreu, Cláudio II. Série.

CDU: 001.1

Elaborada pela bibliotecária Dênira Remedi – CRB-14/1396

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Prologue

C. Ulises Moulines[1]

The approach to the philosophy of science currently known as “Metatheoretical (or Metascientific) Structuralism” (henceforth “MS”, for short), originated in the pioneering work of the late Joseph D. Sneed, *The Logical Structure of Mathematical Physics*, a bit more than 50 years ago. After an initial phase of gradual consolidation and refinements, to which, besides Sneed himself, his closest collaborators Wolfgang Balzer, C. Ulises Moulines and Wolfgang Stegmüller essentially contributed in the 1970’s and 1980’s, followed soon by other authors mainly in Germany, this metascientific program reached its full maturity in 1987 with the treatise *An Architectonic for Science – The Structuralist Program*, co-authored by Balzer, Moulines, and Sneed. Since then, a great number of philosophers of science from many different countries around the world, from Australia and China through Canada and the USA to Western Europe, have discussed, refined, and/or further applied MS, both to the foundations of empirical science in general and to the detailed study of particular theories, ranging from classical and quantum physics to chemistry, biology, psychology, economics, and linguistics.

During the first two or three decades of MS’s development, its geographic center of gravity lay in Germany and some adjacent countries like The Netherlands and Sweden (a circumstance that led some commentators to use the label “German structuralism” for it), but in due time, and especially since the beginning of the 21st century, the MS program, while never disappearing from the countries above-mentioned, has been very intensively discussed, refined and further applied by philosophers of science and scientists from the

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Florianópolis: NEL/UFSC, 2023, pp. 13–15.

Spanish speaking countries, so that, somewhat ironically, instead of denominating this program "German structuralism", we could now speak of "Hispanic structuralism"... The present volume is an impressive confirmation of this.

Most of the approaches to the philosophy of science that have been developed in the course of the 20th century (logical positivism, operationalism, naïve or sophisticated falsificationism, "standard view", constructive empiricism, scientific realism) are characterized by the fact that they develop a general epistemological framework without worrying about the details of its application to real-life theories in empirical science. On the other hand, a great number of very detailed case studies, especially in the last decades, have been offered of particular theories from different scientific areas, but without engaging in the construction and defense of a general conception. MS is a most notable exception to this: Already in Sneed's pioneering work of 1971, the author made a consistent effort to combine the development of the general elements of his metatheory with a detailed analysis of particular theories belonging to classical mechanics to instantiate and illustrate the general framework. This trait is still more apparent in *Architectonic for Science*, where the general metatheoretical categories are applied to a number of empirical theories from classical physics through chemistry to economics. The present volume edited by Cláudio Abreu is a still more notorious instance of this feature of MS: Almost all contributions to the volume combine a general exposition of MS's epistemological and methodological conceptions with a detailed application to, and illustration by, case studies from real-life science.

Another highly notable feature of Abreu's compilation is the place that the reconstruction of the life sciences plays in it. In MS's pioneering phase, the program was exclusively applied to theories from the physical sciences (classical mechanics, thermodynamics, physical geometry). Some years later, several theories from economics (neo-classical, Marxian, or Keynesian) were added to the reconstruction program. It was only by the end of the 20th century that biology, especially in the form of genetics, came on the scene - first through the papers of W. Balzer and John Dawe, then, much more extensively, by Pablo Lorenzano's work, which is also exemplified in the present compilation. Besides genetics, this volume also lays out MS's application to evolutionary biology (illustrated by Ariel Roffé's, Federico Bernabé's and Santiago Ginnobili's joint work, as well as by the papers due to José A. Díez and Daniel Blanco), to nursery (Lucía Federico and Leandro Giri), and to medicine (César Lorenzano).

Though the structuralist reconstruction of the life sciences takes a most prominent position in the present volume, other very interesting discussions of MS in general or of its applications should fairly be noted—be it José L. Falguera's proposal for a new approach to the meaning of theoretical terms, Mercedes O'Lery's discussion of ontological reduction and its application to classical mechanics, Adriana Gonzalo's and Griselda Parera's reconstruction of Chomskyan linguistics, Daniel Blanco's application of structuralist categories to deal with some general epistemological issues like circularity in empirical theories, and last but not least José A. Díez' highly original approach to scientific explanation as a so-called "ampliative specialized embedding".

It goes without saying that this is not the place to lay out, even summarily, the essential contents of each of the ten contributions to MS contained in this book. The reader may have a look to the quite helpful summaries and comments to each article provided by the editor in the "Introduction". I'll conclude only by stressing the welcome fact that all contributors have made a real effort to develop their structuralist analyses and reconstructions in expositions that are as less technical as possible, so that even readers that are not well-acquainted with MS's conceptual framework (which is at places admittedly rather difficult to digest) will take much profit from reading their contributions.

Auxerre, February 2022

Introduction

Cláudio Abreu[1]

Since becoming an independent discipline at the beginning of the twentieth century, philosophy of science has gone through phases marked, among other aspects, by different conceptions regarding theories. For over ten years, those who participate in philosophical discussions about science have explicitly stated that "over the last few decades, the semantic view has increasingly replaced the syntactic view as the received view of theories" (Contessa 2006, p. 376). In fact, it is readily apparent that "over the last four decades the semantic view of theories has become the orthodox view on models and theories" (Frigg 2006, p. 51). The semantic conception, just like the syntactic one, in both its classical and historicist aspects, harbors different conceptions that overlap on some general ideas about theories but differ in the way in which each author (or group of authors) frames and discusses these ideas. Thus, the contributions of McKinsey, Sugar and Suppes (1953), Suppes (1957; 1967; 1970), Adams (1955; 1959), van Fraassen (1970; 1972), Suppe (1967; 1972; 1974; 1989), Giere (1979; 1988), Sneed (1971) and Balzer, Moulines and Sneed (1987) etc. compose not a single concept but rather a family of concepts.

Among these conceptions, the most prominent is metatheoretical structuralism. It is recognized that:

> The structuralist model of theories is impressive in two respects: first, it presents a very detailed analysis of what may be called the *deep structure* of an empirical theory. Second, it has been shown that arrange of actual scientific theories can be reconstructed as theory nets. This is the reason why I have chosen structuralism

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Abreu, C. (ed.) *Philosophy of Science in the 21st Century. Contributions of Metatheoretical Structuralism*.
Florianópolis: NEL/UFSC, 2023, pp. 17–24.

> as the basis for my framework: the richness of the structuralist representation of theories will hopefully enable us to raise new and interesting questions about theory change, and also, it will allow us to ground the logic of theory change in an empirically adequate notion of "theory". (Enqvist 2011, p. 107)

That allows progress on many important issues, with good reason:

> The Sneedean school analyzed the theoretical and non-theoretical division, the division of typifications (or patterns) and laws, the problem of reduction and emergence of theories, and the unity or diversity of sciences with their approach of model theory and got successful results, while the Received View made no progress with any of those problems. It is very interesting that some philosophers want to develop this model-theoretic approach in philosophy of science and thus form the "new Vienna school". (Qi and Zhang 2012, p. 152)

In summary, metatheoretical structuralism is recognized both for its ability to elucidate the deep structure of theories and to address fundamental issues for the philosophy of science: "the German structuralists undoubtedly offer the most satisfactory detailed and well illustrated account of the structure of scientific theories on offer" (Cartwright 2008, p. 65).

The history of metatheoretical structuralism began 50 years ago with the publication of Joseph D. Sneed's *The Logical Structure of Mathematical Physics* (1971) in the United States. In this book, Sneed investigates how to make empirical statements based on theories developed in science itself, when those theories include theoretical terms. Initial reception of the book was tepid. The situation changed a few years later, when Wolfgang Stegmüller became interested in the publication. At that time, Stegmüller had established himself as one of the most important analytical philosophers in Germany, especially in the philosophy of science. Stegmüller saw the potential of Sneed's proposal, which was then only about physics:

> I would like to stress that I consider Sneed's contributions to be *such a fundamental advancement in modern philosophy of physics* that one can compare them to those made by Tarski on the formal level. Only Sneed's work has reminded us that the philosophy of physics can be clarified in the same way that Bourbaki has clarified modern mathematics. (Stegmüller 1979, p. 18, my translation)

Sneed further developed some of these initial ideas, and they were iterated upon by other philosophers of science, including Stegmüller himself and two of his former doctoral students at the University of Munich, C. Ulises Moulines and Wolfgang Balzer. By 1987, the developed and consolidated conception of theory that Sneed initially presented in 1971 was embodied in a joint effort by Balzer, Moulines, and Sneed, titled *An Architectonic for Science*.

This conception of theory presents a largely original apparatus of analysis. However, one can identify the influence of previous ideas. Significant references for metatheoretical structuralism include Carnap and Ramsey from the classical philosophy of science, Kuhn and Lakatos from the historicist, and Suppes (with whom Sneed studied). *An Architectonic for Science* presents a conception of theory that allows us to rigorously analyze both the formalizable aspects of theories and those that resist formalization. Moreover, that analysis can be carried out not only from a semantic perspective but also from a diachronic and pragmatic point of view.

There is no need to present in detail the conception of theory defended by metatheoretical structuralism. Those looking for an understanding of that conception of theory can refer to *An Architectonic for Science*. The general ideas of this perspective of philosophy of science are as follows:

> (a) The traditional *theoretical/observational* distinction is rejected and replaced by a *theoretical/non-theoretical* category relativized to each theory.
>
> (b) In terms of this new distinction, the "empirical basis" and the *domain of intended applications* are defined. Data permeated by theory, but not a theory about what data is.
>
> (c) With this novel characterization, a new formulation of the *empirical assertion* is given that clearly excludes the "self-justifying" interpretation of said assertion.
>
> (d) In addition to traditional laws for the determination of models, new elements that are less apparent but equally essential are identified; for example, *constraints*.
>
> (e) The *connections* between the models of various theories are identified.
>
> (f) The synchronous structure of a theory is characterized as a *network* with various components, some more essential and permanent, and others more specific and changing. The *evolution* of a theory consists of the development of such networks.
>
> (g) The traditional intertheoretical relations of *reduction* and *equivalence* are analyzed in terms of theoretical models. (Diez and Lorenzano 2002, p. 59, my translation)

Going well beyond physics with respect to the reconstruction of theories, structuralist philosophers of science work in areas such as linguistics, economics, sociology, neurophysiology, psychology, biology, biochemistry, medicine, nursing, ecology, chemistry, archaeology, geology, astronomy, computer science, political science, etc. In terms of notions and general problems that are characteristic of the philosophy of science, some subjects that have been analyzed include the relationship between theory and experience, scientific explanation, intertheoretical relationships, pragmatism in science, scientific holism, the comparison between coherentism and fundamentalisms, laws, contrastability, the hypothetic-deductive method, scientific explanation, approach, idealization, the teaching of science, etc. For references up to 2012, see the "Bibliography of structuralism" by Diederich, Ibarra and Mormann (1989; 1994) and Abreu, Lorenzano and Moulines (2013).

The fruits of work developed under this perspective continue to emerge. The compendium *Philosophy of Science in the 21st Century. Contributions of Metatheoretical Structuralism* is one example. In the first contribution, "Identity of Scientific Concepts and Theoretical Dependence," José L. Falguera analyzes the conditions of a T-theory upon which the conceptual identity, or meaning, of that T-theoretical term depends. The author adds the idea, taken from Kuhn, that certain laws have a non-absolute synthetic status *a priori* and argues that what metatheoretical structuralism presents as the fundamental law of a complex theory corresponds to a law with said status. He also argues that the conceptual identity of a T-theoretical term basically depends on the fundamental law of T and that for a T-theoretical term to have operational relevance for predictions and explanations, its meaning must be complemented in some sense or senses. In this context the "meaning" and "sense" of a T-theoretical term are introduced in a particular way.

In "Ontological Reduction: the Reduction of Classical Collision Mechanics to Classical Particle Mechanics," Mercedes O'Lery addresses the discussion of reduction, taking into consideration that the classical reduction model supposes two conditions: connectability and derivability. She also argues that in dealing with this topic, metatheoretical structuralism maintains the spirit of said conditions, but points out that to analyze the reduction relationship, it is necessary to start from a non-syntactic conception of scientific theories. O'Lery uses the structuralist characterization of the intertheoretical reduction relationship presented in *An Architectonic for Science* (1987) and an additional condition of ontological reduction presented by Moulines (1984). The author's objective is to consider the adequacy of applying structuralist analysis to reductions that are homogeneous. In this study, her analysis is inspired by a case that has been extensively analyzed by structuralism: the reduction of classical collision mechanics to classical particle mechanics.

In "The Problem of Explanation in Chomskyan Generative Linguistics. A Programmatic Proposal from the Metatheoretical Frame of the Structuralist View of Theories," Adriana Gonzalo and Griselda Parera deal with the problem of explanation within the framework of Generative Theory, particularly in the Chomskyan approach. From a reconstructive perspective, they offer an answer to the question of what the explanatory proposal is, according to Universal Grammar. To do so, they examine the central purposes of theory in a diachronic way, which may serve as a guide for what they call an explanatory level problem. The authors show that the emergence of biolinguistics and *explanada* of linguistic problems correlate with the explanatory field of psycholinguistic and computational theories. Considering Chomskyan Theory as a theoretical network with an element of Syntax Theory, they propose a reconstruction of Minimalist Theory, using the concept of "theory" from metatheoretical structuralism. Considering this conception, they draw a general picture of the central components, relations, and main laws that characterize Chomskyan theory. They perform this task considering the problem of explanation and, especially, the philosophical proposal of explanation that is presented in structuralism.

In "Theoricity and Testing," Ariel J. Roffé, Federico N. Bernabé, and Santiago Ginnobili point out that the evolution of the philosophy of science has shown that the distinction between observational and theoretical concepts, which has played an important role in classical philosophy of science, hides two non-identical distinctions: on the one hand, a concept can be either observational or non-observational, and, on the other, it can be either theoretical or non-theoretical. Similarly, metatheoretical structuralism also proposes a more sophisticated treatment of theoricity in terms of the dependence or operational independence of concepts with regards to the theories in which they appear. The authors note that despite this remarkable sophistication, it remains generally accepted that the distinction between theoretical and non-theoretical concepts coincides with the distinctions between explanatory and non-explanatory and contrastational and non-contrastational concepts. This coincidence is intially questioned in a paper by Ginnobili and Carman (2016), who defend the independence between theoricity and explanation. In "Theoricity and Testing," the authors focus on contrastability and defend their independence from both theoricity and explanation. In this sense, they propose an explanation of the contrast of theories based on the framework of metatheoretical structuralism and apply it to the theory of natural selection and cladistics.

José A. Díez's contribution is titled "Scientific Explanation as Ampliative Specialized Embedding: New Developments." In this chapter, he summarizes the main aspects of scientific explanation as ampliative specialized embedding (ASE), which retains the core of Hempel's idea of nomological predictability but substantially reforms its model by replacing logical inference with theoretical models that embed and add two crucial conditions: conceptual/ontological enlargement and nomological specialization. The author presents new applications of ASE to four biological theories, though the field was traditionally claimed to be unsuitable for nomological explanations. He also discusses some criticisms directed at the relationships between T-theorization, T-explicitation and T-contrastability that early versions of ASE presupposed. Díez accepts some of these criticisms and modifies ASE accordingly. However, these modifications, argues the author, do not affect the main aspects of ASE or its applicability. Díez states that these new developments confirm, in general, that ASE behaves well as a general model of scientific explanation, and in any case better than its rivals.

Daniel Blanco, in "Structuralism as a Resource to Detect and Solve Some Current Philosophical Problems," argues that even metatheoretical structuralism was originally conceived as a conceptual instrument to elucidate scientific theories; its usefulness is not exhausted in its formal reconstructions. On the contrary, numerous and significant philosophical problems can be addressed, argues the author, using even rudiments of this perspective as a platform. As the title of chapter suggests, Blanco presents three philosophical cases that can be successfully treated using conceptual tools provided by metatheoretical structuralism. These three cases are related to the recognition of rivalry between different theories and within a particular theory, to some aspects of what has been called 'revolutionary science' and, finally, to the search for and exclusion of circularity in empirical theories.

In "Organizing Nursing Knowledge from Metatheoretical Structuralism's Viewpoint" Lucía Federico and Leandro Giri highlight metatheoretical structuralism's potential to improve the currently dominant proposal for organizing the scientific knowledge of nursing: Jacqueline Fawcett's structural holarchy. The authors analyze the philosophical foundations of structural holarchy and present the structuralist proposal to organize science through the notion of a theoretical network. Finally, they show that the structuralist framework captures Fawcett's intuitions more precisely since it is not attached to misconceptions inherited from the received view of the philosophy of science, as is the structural holarchy.

César Lorenzano, in "The Structure of the Medical Clinic," describes how a doctor examines a patient, diagnoses their disease, predicts its evolution, and even proposes a treatment. The author points out that the doctor does so from specific knowledge acquired during their years of training and in hospital and office practice. In his contribution, the author develops a largely unaddressed aspect of the structuralist conception that makes its aspects pragmatic, namely those that involve the availability of a theory, so that the epistemological subject, the scientist who uses that theory, becomes important. The doctor uses the clinical theory of diseases in their daily practice, and the description of their actions is also the elucidation of the theory at their disposal.

Finally, in his detailed study, "Laws, Models and Theories in Biology Within a Unifying Structuralist Interpretation: General Explication and an Account of the Case of Classical Genetics," Pablo Lorenzano presents an explanation of the concepts of law, model, and theory and their interrelations, carried out in the framework of metatheoretical structuralism. The author also applies this elucidation to a case in the field of biology: classical genetics. This analysis allows us to argue, contrary to the claims of some philosophers of science in general and of biology, that there are laws in the biological sciences, that many of the heterogeneous and varying models of biology can be accommodated under some theory, and that this is precisely what gives biological theories their great unifying power.

Diverse in many aspects, the contributions that make up *Philosophy of Science in the 21st Century. Contributions of Metatheoretical Structuralism* show that the structuralist program is alive and powerful as a metatheoretical proposition that still has much to offer to the philosophy of science in general and to specific philosophies of science.

Going beyond semantic aspects by also including diachronic and pragmatic aspects in their analysis, structuralist philosophers of science deliver more precise studies through much more nuanced and complex analyses of most of the fundamental themes within philosophy of science and concrete theories, that is, those with which scientists work regularly. It is these characteristics of metatheoretical structuralism that motivated the publication of this compendium. I hope that it can motivate those who are interested in topics of philosophy of science to learn more about metatheoretical structuralism, apply it usefully, and contribute to its development.

Philosophy of Science in the 21st Century. Contributions of Metatheoretical Structuralism is the result of a joint effort of structuralist colleagues from Spanish-speaking countries. I am grateful for the trust shown by all the colleagues who have contributed to making this publication possible. I must also thank Jaimir Conte and Jerzy Brzozowski, editors of *Rumos da Epistemologia* from the Center for Epistemology and Logic at the Federal University of Santa Catarina, for the opportunity to publish *Philosophy of Science in the 21st Century. Contributions of Metatheoretical Structuralism* in that collection.

Buenos Aires, July 2021

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Identity of Scientific Concepts and Theoretical Dependence

José L. Falguera[1]

1. Looking to Kuhn

I would like to begin with some considerations on the specific terms of a theory that we find in Kuhn's later works (mainly from 1983 onwards), considerations that I believe provide relevant insights.[2] Kuhn assumed that, given a theory, it is possible to differentiate between its specific scientific terms and those previously available to that theory (or antecedent vocabulary) (see Kuhn 1990, p. 302). The peculiarity of the specific terms of a theory T is that their meanings depend (in some way) on T, while the meanings of the other scientific terms (or antecedent vocabulary) of T do not depend on T (although, given a nonspecific term of T, it may depend on some other theory; not necessarily the same theory for each of these terms). In this sense, the distinction is relative to each theory (see Kuhn 1993, p. 333). The intuitive idea behind this distinction is denominated by some other account the T-theoretical and T-non-theoretical distinction of the descriptive terms of a given theory T.[3] In what follows, I will preferentially use the latter way of speaking of the distinction.

Kuhn (since his 1983a) conceived the semantic dependence that T-theoretical terms have on T, as a kind of semantic holism. Such holism is called local (see Kuhn 1983a, p. 682; 1983b, p. 566) because: (i) the semantic dependence of T is restricted to certain terms, the T-theoretical terms; (ii) T-theoretical terms are presented as semantically interdependent; (iii) such semantic

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[2] See Kuhn 1983a; 1983b; 1987; 1989; 1990; 1991; 1993.

[3] Specifically, by the structuralist metatheory (see Balzer, Moulines and Sneed 1987).

Abreu, C. (ed.) *Philosophy of Science in the 21st Century. Contributions of Metatheoretical Structuralism.*
Florianópolis: NEL/UFSC, 2023, pp. 25–39.

interdependence is due to how they are connected by some generalization(s) of T (not by all). I feel that this last requirement, (iii), of its local holism is especially relevant, since it involves delimiting the semantic dependence of T-theoretical terms to some generalizations of T (not to all). Attending to requirement (iii), we can speak of a delimited (local) semantic holism. However, it would seem that in his last period of philosophy of science work, Kuhn maintained two different standpoints:

- a highly delimited (local) semantic holism (see Kuhn 1983b, pp. 566-567);
- a weakly delimited (local) semantic holism (see Kuhn 1989, pp. 19-20; 1993, p. 317).

With regard to the highly delimited semantic holism, in certain texts Kuhn proposes that the semantic interdependence of the specific terms of a theory T is guaranteed thanks precisely to certain generalizations with a stipulative role.

Nonetheless, with regard to weakly delimited semantic holism, in other texts Kuhn also seems to propose that the meaning of each one of the specific terms of T is acquired/learnt (i) with certain generalizations with a stipulative role, and (ii) with certain others with a clear empirical role.

It should be noted that, for Kuhn, the generalizations of T by which the meaning of a T-theoretical term is acquired/learned are not definitions, not even those with a stipulative role. Moreover, Kuhn argues that the meaning of the theoretical T-terms is established (and is acquired/learned) in the use of appropriate generalizations in relation to certain applications—paradigmatic examples in his terminology (see Kuhn 1990, p. 302; 1993, p. 312).

Kuhn finally presented the generalizations with stipulative role as 'non-absolute synthetic *a priori*' statements.[4] This point of view is along a line which links up with similar notions handled by Reichenbach (1920) and by Friedman (1997; 2000; 2001), wherein they conceive such types of statements as 'constitutive of the object of knowledge'. In a similar sense, Kuhn comes to speak of being 'constitutive of possible experience of the world' (see Kuhn 1993, p. 331). Intuitively, it is possible to say that such generalizations with a non-absolute synthetic *a priori* status are:

(i) not absolute, because the way in which these generalizations constitute their respective objects does not involve unrevisability;
(ii) synthetic, because they are not definitions (in this sense, they contribute, but not alone, to establishing differences in the empirical consequences of the theory; i.e., consequences given as T-non-theoretical data); and
(iii) *a priori*, because they have a stipulative/constitutive role.

In any case, this intuitive way of presenting the status of 'synthetic *a priori* non-absolute' may seem not very satisfactory, leading to the belief that further clarification is needed. On the other hand, assuming the thesis of local semantic holism for the T-theoretical terms, it should be clarified which version is more plausible: a highly delimited one or, on the contrary, a weakly delimited one.

In light of these considerations, two problems would remain unsolved:

a) that of (better) elucidating the non-absolute synthetic *a priori* generalizations of a theory;
b) that of how delimited local semantic holism is: i.e., whether the semantic dependence of the specific terms should be limited to the non-absolute synthetic *a priori* generalizations or whether they should contemplate generalizations with an empirical role.

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[4] Another label used in the literature for these statements is 'relativized *a priori*' (see Reichenbach 1920; Friedman 1997; 2000; 2001).

2. Looking to structuralist metatheory

Kuhn does not provide a criterion regarding those generalizations which are non-absolute synthetic *a priori*, although he does take Newton's Second Law of classical particle mechanics ($F = m \cdot a$) as his favorite example (see Kuhn 1983b, p. 566-567). Taking this into account, one could think that the notion of 'fundamental law' from structuralist metatheory can help to shed some light on these types of generalizations. In fact, Newton's Second Law is a paradigmatic example of a fundamental law in the structuralist sense (see Balzer, Moulines and Sneed 1987, p. 103; Moulines 1991, pp. 233-235). Moreover, when Kuhn addresses these matters, he habitually refers to the initial developments of structuralist metatheory (see, for example, Kuhn 1989, p. 17 n. 15). Furthermore, the Kuhnian distinction between specific terms of T and previously available terms may be assimilated to the structuralist distinction between T-theoretical and T-non-theoretical terms. In fact, I have been using the structuralist expressions for the distinction with such assimilation possibility in mind. In any case, the structuralist distinction is established according to a more adequate methodological criterion than the historical one, where the latter bases the theoreticity of a term in relation to the theory with which that term is introduced. For our purposes in this article, it is sufficient to consider an intuitive formulation of the structuralist criterion of theoreticity (recognized as pragmatic criterion). According to this, a term is T-theoretical as long as all methods for determining its values ultimately presuppose the fundamental law of T.[5] It is important to note that, intuitively, a method of determination for a term f requires the existence of a condition (which can be expressed by a sentence) applicable through an operational procedure (e.g., in the case of quantitative terms an experimental instrument or a calculation procedure), such that given the values of other terms $r_1, \ldots, r_m$ from which the values of f are established, the values of f are uniquely determined—that is, given a certain value for each $r_i (1 \leq i \leq m)$, the value of f will be unique—in a systematic way—that is, the condition is applicable to many different values of $r_1, \ldots, r_m$ to obtain values of f.[6]

Having thus established the criterion of theoreticity, a term like 'classical mass' turns out to be theoretical with respect to classical particle mechanics, despite the fact that the term as such already had previous uses to the historical introduction of this theory; thus, 'classical mass' had already been incorporated with classical collision mechanics. In this respect, it should be noted that classical collision mechanics can be considered integrated into (reduced to) classical particle mechanics (see Balzer, Moulines and Sneed 1987, pp. 97-98, 255-267). We can assume that since a theory T_1 is historically integrated into (reduced to) a theory T_2, the latter is the relevant theory according to which the theoreticity must be established.[7] In any case, though Kuhn expressed the distinction of the descriptive terms of a theory according to the historical distinction, he in fact also presented 'classical mass' as theoretical relative to classical particle mechanics (see Kuhn 1983, p. 566). Therefore, we can assume that Kuhn, despite the expressions used for the distinction of descriptive terms of a theory, is implicitly assuming a distinction of a methodological character

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[5] For an elucidation of the structuralist (pragmatic) criterion of theoreticity, see Balzer, Moulines and Sneed (1987, pp. 47-73, 391-393).

[6] For a precise definition of method of determination, see Balzer, Moulines and Sneed (1987, pp. 62-64). Of course, in the case of quantitative T-theoretical terms, these have to be uniquely determinable up to scale transformations.

[7] Before classical particle mechanics is historically introduced, "classical mass" should be considered as a theoretical term for 'classical collision'.

such as that established with the criterion of structuralist theory.

It should be taken in account that when Kuhn speaks of the semantic interdependence of the specific terms of T, an interdependence which is made explicit in generalizations of T with a non-absolute synthetic *a priori* status, structuralist metatheory establishes that the determination of each T-theoretical term ultimately presupposes the fundamental law(s) of T.[8] Hence, one could consider that the presupposition of the fundamental law for theoretical terms that structuralist metatheory proposes is related to the semantic interdependence of specific terms by means of the constitutive generalizations proposed by Kuhn.

According to structuralist metatheory (admitting some simplifications) a complex, mature theory T (if, in line with the simplification, we consider that the only relevant conditions of T are its laws) is a hierarchical tree structure with one root node representing the fundamental law, from which branches leave that terminate in nodes representing special laws (such that each special law is a restriction, with a smaller set of applications than the law immediately above, with which it is connected by a branch).[9] See Figure 1.

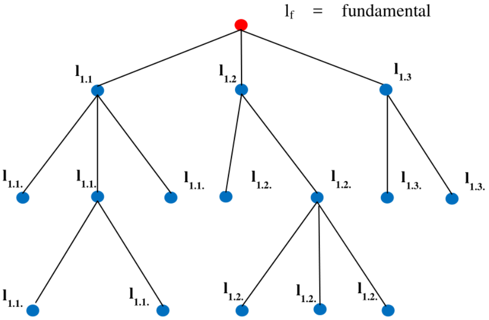

Figure 1

However, in line with what has been established in the literature on structuralist metatheory, it is assumed that a fundamental law satisfies necessary conditions such as following:

- (Quasi-)synoptic character: the formulation of a fundamental law of T includes (almost) all the characteristic terms of *T (i.e., T-theoretical and T-non-theoretical terms)*, connecting them in an essential manner (see Moulines 1991, pp. 233-235).[10]

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[8] There may be cases of theories with more than one fundamental law resulting in no genuine unification (except through mere conjunction); but, for the sake of simplicity, henceforth I shall speak of 'fundamental law' in the singular, as if it were unique for a complex, mature theory. To clarify the notions of 'complex theory' and 'mature theory' somewhat, let me say: (a) by a complex theory, I understand one with several laws (at least three, two of which are special laws directly specializing the fundamental law); (b) by a mature theory, I understand one that has reached a certain degree of development which has made it possible to clearly set up, for the community of users, some of its conditions, especially its characteristic laws (the fundamental law and certain special ones). A mature theory can be currently accepted or rejected as correct.

[9] For the notion of 'special law', see Moulines (1991, pp. 244-247).

[10] Moulines (1991, pp. 233-234) acknowledges that not all plausible candidates for fundamental laws appear

- Quasi-vacuous character: this, according to Moulines (1978; 1982, pp. 85-107), can be explained by the logical form (existential quantification over variables of higher order or functional variables) [although this may only be applicable to determined fundamental laws—those which Moulines presents as 'guiding principles']. At any rate, this entails that a fundamental law be highly unrestricted and irrefutable (owing to existential quantification—even if it was only first-order, and not spatiotemporally restricted); hence, the feature of being 'non-absolute *a priori*' which can be attributed to it would be captured. (See also Stegmüller 1976, pp. 162 and ff.; 1978, p. 168; 1979, pp. 74-75).[11]

Nevertheless, there has been a trend towards accepting that no criterion of a fundamental law with sufficient and necessary conditions is available. From my point of view, accepting the aforementioned conditions which are considered necessary, I consider that the character of a fundamental law of a theory T goes hand in hand with the character of theoreticity of some of the characteristic terms of T (to a certain extent, they are two sides of the same coin). In light of this, as a sufficient and necessary condition for a law to be fundamental (at a determined moment) for a complex, mature theory T, I propose that:

(*) *a fundamental law for T is presupposed in every determination of the values of at least one characteristic term of T* (in fact, this is correct for any T-theoretical term).

It should be clear that there is no strict circularity between the (pragmatic) criterion of theoreticity from structuralist metatheory and this fundamental law criterion. According to the latter criterion, in order to establish whether a law is fundamental, we do not need to know beforehand what the theoretical terms of the theory for which this law is fundamental are, as would be the case if such circularity were given. What we need to show is that this law is presupposed in every determination of at least one term. In the end, as this comes about, the term turns out to be theoretical for the theory which has this law as a fundamental law. (It is quite another matter that the criterion of the fundamental law supplied makes it possible to easily identify when we are dealing with a fundamental law.)

In light of the above, we can suppose that the conceptual identity, or the meaning, of a specific (or T-theoretical) term for T is given by (depends exclusively—or almost exclusively—on) the fundamental law of T (insofar as it is considered in relation to certain intended applications). We could say that this point of view is in line with the Kuhn's 'highly delimited (local) semantic holism'.

But it should not be forgotten that, according to Kuhn, there is another possibility: i.e., that the meaning of each one of the T-theoretical terms is acquired/learnt with generalisations of T with a stipulative role and with others with an empirical role (insofar as they are considered in relation to certain intended applications). This leads us to consider that the meaning of a T-theoretical term depends on more laws than the fundamental law of T. This point of view would be in line with the Kuhn's 'weakly delimited (local) semantic holism'.

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to possess this synoptic feature. In this regard he indicates the fundamental laws of relativistic continuum mechanics and electrodynamics, according to Bartelborth's reconstructions (see Bartelborth 1988, Chapter I), as examples that contravene this synoptic character.

[11] P. Lorenzano (2019, p. 115-119) considers other necessary conditions for the fundamental law(s) of a theory, such as: a) being valid for any intended application of the theory; b) having a systematizing role; c) having modal force (related to the possible applications of the theory) providing counterfactual support (see also Lorenzano 2006; 2008).

Some considerations formulated within structuralist metatheory (by Gähde, 1990; 1996; 2002) seem to endorse a weakly delimited semantic holism. Gähde has referred to two conditions which, given a theory T, affect its T-theoretical terms:

1. the fundamental law of a theory T does not by itself uniquely determine values for its T-theoretical terms, once the values for the T-non-theoretical terms of an intended application are given.
2. a T-theoretical term may only be uniquely determined, once all the T-non-theoretical terms of an intended application are given, thanks to the fundamental law and some appropriate special law(s), which are used or presupposed in some method(s) of determination—applicable through corresponding operational procedure(s)—for that T-theoretical term.[12]

3. Looking for what the meaning of a T-theoretical term depends on

Regardless of the use that Gähde aims to make of both conditions, they could be interpreted as a corroboration that the meaning of a T-theoretical term does not depend solely on the fundamental law of T, but also (at least) on special laws. In this sense, when Kuhn (1989, pp. 17 and ff.) appealed to what was here previously called a weakly delimited semantic holism, it seems that he was considering that (given certain applications) the mastery of a T-theoretical concept is achieved with methods of determination to determine its values.[13] If this were the case, such a consideration could be associated with different alternative ways of understanding the meaning of a T-theoretical term.

It might be thought that different methods of determination provide different meanings for a same T-theoretical term. We could say that, to a certain extent, it is a proposal with an operationalist orientation. But this is not a very acceptable possibility, since it entails dispensing with the conceptual unity that a T-theoretical term is supposed to have through the theory T, especially if it is assumed that the different methods of determination for a same T-theoretical term presuppose the same law: the fundamental law of T, as it is accepted according to structuralist metatheory. In any case, Kuhn did not endorse such a plurality of meanings for the same T-theoretical term. On the contrary, according to his thesis of incommensurability, a scientific term—and so, a theoretical term relative to a theory—retains the same meaning within the framework of a theory, and it changes the meaning only if that term is used homographically in a clearly alternative theory (on the occasion of a scientific revolution).[14] Furthermore, Kuhn's acceptance of a version of local

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[12] Certain methods of determination for a term are at times considered as conditions that have a lower status than that of a scientific law. In these cases, we would need to contemplate the fact that each method of determination presupposes certain special law(s), in addition to the corresponding fundamental law.

[13] Here we should remember that a method of determination for a term, as this notion was introduced before, ensures a uniquely determination of the values of the term.

[14] Not all cases of incommensurability between theories are cases with homographic terms. What is peculiar about the thesis of the incommensurability between theories is that there are changes of concepts between the theories, without it being possible to express the statements that are established with the terms of one of the theories with the terms of the other.

semantic holism for the T-theoretical terms conflicts with the consideration of several conceptual identities for each of such terms.

An apparently more satisfactory proposal would be to assume that the meaning of a T-theoretical term is given by the conjunction of the different methods of determination for that term. However, such a proposal could not be considered to conform to any delimited semantic holism. In fact, with such a proposal it would be accepted that all the laws of a theory T contribute to the meaning of any T-theoretical term, to the extent that the different special laws of T are used in the different methods of determination for that term. We could always conclude that the reasonable thing to do is to disregard any proposal of delimited semantic holism for T-theoretical terms and adopt a non-delimited semantic holism (according to which the meaning of a T-theoretical term—its conceptual identity—depends on all the laws of T). However, such a proposal has a serious drawback. We have to keep in mind that theories are entities that change over time; i.e., they can change some of their intended applications (dispensing with some or incorporating new ones) and they can also change some special law(s) of a theory (excluding some, refining others, including new ones); for example, when a special law of a theory T fails to ensure (by an intended method of determination) the univocal determination of the values of a T-theoretical term. Such changes of special laws do not entail a change of theory, as long as the fundamental law is preserved. (Of course, changes in a theory are intended to achieve as few special laws as possible.) But if changes of special laws are admissible for a theory, and if all laws of a theory are accepted to contribute to the meaning of its theoretical terms, then we would have that the meanings of the T-theoretical terms would change with changes of the special laws of T. But this is not acceptable. It is not clear what it means to say that the conceptual identity of a T-theoretical-term—its meaning—changes, while it is accepted that the theory T is preserved (through changes of its special laws): it seems that a necessary condition for identifying a theory as the same through its development is that its characteristic terms (theoretical and non-theoretical) have the same meanings.

The possibility of selecting just some special laws of a theory T as those that contribute to the meaning of its T-theoretical terms does not seem to be a better option. For example, assuming that a complex theory has a hierarchical structure as a tree-like net (see Figure 1), such as it is assumed by structuralist metatheory,[15] it could be proposed that the meaning of each of its theoretical terms would depend on the special laws that are direct specializations of the fundamental law (i.e., the special laws that are at the hierarchical level immediately following the fundamental law) together with that fundamental law. However, the situation now would not be better than in the previous case, since any of these special laws could be excluded with the development of the theory; or, given a certain level of development of a theory, a new special law could subsequently be incorporated, at that same level. If any of these cases occur, the theoretical terms of the theory would change their meanings, even though the theory remains the same. But, as already noted above, this does not seem acceptable.

Everything seems to lead us to the proposal of highly delimited semantic holism. That is, to suppose that the meaning of each T-theoretical term depends only on the fundamental law of T. Obviously, that would avoid the problem of changes in the conceptual identity, or meaning, of the theoretical terms of a given theory. As long as the fundamental law remains, we can speak of the same theory; i.e., changing the fundamental law entails changing the theory. Furthermore, if,

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[15] For the notion of 'tree-like theory-net' see Balzer, Moulines and Sneed (1987, pp. 168-177).

according to previous comments, the fundamental law of a theory has a constitutive role in that theory, it could be argued that the meaning of a T-theoretical term—its conceptual identity—is fixed with the fundamental law of the theory T.

Nevertheless, the proposal that the meaning of a T-theoretical term is fixed with the fundamental law of T seems initially unsatisfactory, given that such a law underdetermines the values of the T-theoretical terms (as Gähde indicates). Behind such dissatisfaction one can identify the idea that presents the meaning of a scientific term as a rule (or rules) to uniquely determine its values (for the intended applications). But conceiving the meaning of a scientific term as a rule (or rules) to uniquely determine its values leads us back to the possibilities previously explored, which were dismissed as inappropriate. On the other hand, speaking of the meaning of scientific terms as something disconnected from how to determine their values entails contemplating an obscure notion of meaning for such terms. Far from being faced with a dead end, it may be appropriate to think about whether there is an adequate way of understanding the connection between the meaning of a scientific term and methods of determination of its values, which preserves the idea that the meaning of a scientific term is not a mere rule to determine its values.

4. Clarifying what the meaning of a T-theoretical term depends on

In light of the foregoing, it seems that we have to accept that (a) the meaning of each T-theoretical term—its conceptual identity—depends only on the fundamental law of T. At the same time, according to Gähde, we have that (b) the fundamental law of T underdetermines the values of the theoretical T–terms and requires some special law(s) to uniquely determine them in each intended application of T. If we accept the above conditions (a) and (b), it is intuitive to think that the meanings of these terms must be complemented in some way in order to establish their values in each application. If the *meaning* of a T-theoretical term is not a mere rule to determine its values, then it needs something that complements it to uniquely determine such values.

Let us call 'sense' each complement of the meaning of a scientific term that contributes to the determination of its values. Let us assume that a method of determination for a scientific term will be given by the *meaning* of the term together with a certain *sense* for that term. If, in addition, we assume that a scientific term of a complex theory (as a tree-like net) usually has different methods of determining its values, we will have that, given a T-theoretical term, there will usually be several *senses* which complement its *meaning*; as many senses as there are methods of determination for the term in question. Each sense for a T-theoretical term is given by those special laws (or other conditions with less status) that are used or presupposed in each method of determination.

The previous proposal supposes considering two types of intensional entities for the theoretical terms of any T theory: *meaning* and *sense*. Both are important, but they play different roles. With this proposal, a T-theoretical term has a single meaning delimited by the fundamental law of T, while (T being a complex theory) it has different *senses*; as many *senses* as conditions (special laws or other conditions of lesser status) for operational procedures for that term. Furthermore, the *senses* of a T-theoretical term are characterized by being an open group when T is considered diachronically from a stage of its development (while T is in use), both because some sense(s) can be discarded and because new senses of the term can be incorporated. The senses of a T-theoretical term can change with the development of T, its meaning is fixed. In this proposal, if, as has been assumed, the fundamental law of a T-theory underdetermines the values of its T-theoretical terms, and therefore such a fundamental law needs to be complemented with special laws, for a T-theoretical term to have operational relevance (for predictions and explanations) its *meaning* must be complemented with some sense(s), in order to provide restrictions on the values of this term (given the T-non-theoretical values of certain intended applications), and thus be useful for predictions and explanations.[16]

Throughout this article, when asking ourselves the question of the conceptual identity of a T-theoretical term, we have been talking basically about the laws of T (the fundamental law and the special laws) as if they were the only aspects to take into consideration. According to structuralist metatheory, this is a simplification. I have deliberately adopted this simplification as I consider that it did not affect the central aspects that I wished to raise; nonetheless, a more detailed proposal should have taken into account that each scientific law of a complex theory (a tree-like net-theory) is part of an elementary theory (a theory-element, according to structuralist metatheory) in which there are usually other conditions besides scientific laws, such as the kind of conditions called *constraints* in structuralist metatheory.[17] While the role of a scientific law in a theory T is to regulate (to govern) each possible system (each possible application) of T, the role of a constraint is to establish compatibility conditions among different possible systems of T; for example, compatibility conditions for "Newtonian mass" among different possible systems of classical mechanics. For many complex theories we find the fundamental law together with some basic constraints forming a basic elementary theory of the corresponding complex theory.[18] A T-theoretical term can have associated some basic constraints. In these cases, the relevant basic constraints for a T-theoretical term establish compatibility conditions between the values that this term adopts in the different possible systems (or possible applications). Two paradigmatic examples of relevant basic constraints for 'classical mass', according to structuralist metatheory, are: (a) the conservative character of classical mass for any particle for any two possible systems this particle forms part of; (b) the additive character of classical mass, according to which the mass of a particle in a possible system is equal to the sum of the masses of the components of that particle regardless of the possible system in which the mass for the particle is determined and the possible

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[16] This proposal does not coincide with that which Díez formulates in order to identify scientific concepts in his (2002) (returning to it in his 2005). Here it is not possible to embark on a detailed consideration of Díez's proposal, but by way of a brief summary: Díez's proposal recognizes the need to consider (i) a law-like component, but he does not delimit the scope of this component, i.e., his proposal does not provide a criterion about which laws would fix the meaning of each T-theoretical term (rather he leaves it open), and it seems to me that this is a problem, for reasons given throughout the paper. In addition, he considers other components: (ii) the applicative component (which seems to require the relation of determined intended applications); (iii) the observational component (which seems to require the relation—direct or indirect—with a family of pre-scientific observational concepts; (iv) the operational component (which links certain scientific concepts with procedures for fundamental determination); (v) the ancestral-folk component (which connects a scientific concept with pre-scientific explicative or common-sense folk practices).

[17] For the structuralist notion of 'theory-element', see Balzer, Moulines and Sneed (1987, p. 89 and ff.); for 'constraint', see *idem* (p. 40 and ff.).

[18] In the hierarchical representation of a complex theory, T (see Figure 1), where before we had the fundamental law we would now have a basic elementary theory (a base element-theory), given by that fundamental law and the basic constraints (if any); and where before we had some special law(s) of T, now we would have a specialized theoretical element of T, given by that (those) special law(s) and the corresponding specialized constraints (if any).

system in which the mass for any component is determined. In this sense, it is especially important to bear in mind that the meaning of a T-theoretical term depends on its fundamental law together with, usually, some relevant basic constraints of T for that term.

Despite the arguments provided, there may be those who insist that the fundamental law of T, together with relevant basic constraint, are insufficient to account for the meaning of each T-theoretical term. In this sense, it could be argued that, in some cases, it is difficult to assume that a certain special law of the theory T in question does not play a relevant role for the meaning of certain T-theoretical term(s). For example, one could point out that the meaning of the term 'classical force', in addition to depending on Newton's Second Law (the fundamental law of classical particle mechanics), we must consider that it depends on Newton's Third Law (the *actio-reactio* principle). In this regard, it is worth taking into account that Balzer, Moulines and Sneed (1987, pp. 182-183) indicate that, in addition to the fact that the latter law is, in fact, not considered for some applications of classical particle mechanics: "there are forces that cannot even 'in principle' be considered to be counterbalanced by another equal and opposite force", as in the case of "the description of moving charges in an electromagnetic field, where we apply the notion of a Lorentz force".[19] Another example would be provided by considering that the meaning of 'classical mass' should contemplate that the masses attract each other, and with it Newton's law of gravitation.[20] But in this case we find that the latter law is not in fact essential for all applications of classical particle mechanics.[21]

In addition to the reasons already established (Section 3) for ruling out possibilities such as the ones I have just mentioned, I believe it is possible to provide an account of such points of view in light of the proposal defended in this paper, differentiating between the meaning and the senses of each T-theoretical term. In this regard, let us remember that, according to this proposal, the meaning of a T-theoretical term is an incomplete intensional element; it needs to be complemented by one or another sense (such as this expression is understood in this paper) for the theoretical term to have scientific relevance for predictions and explanations. Along with this, we should bear in mind that the process of familiarization with a T-theoretical term is achieved with determinations of its values (intending for them to be univocal), for which it is necessary that the meaning of the term be appropriately complemented by a sense; or if one prefers, it is necessary to manage a method of determination that makes use of or presupposes a certain special law(s) together with the fundamental law of T. Nonetheless, in this picture there is no reason to rule out the possibility that a certain method of determination *has a paradigmatic role in how to get operational familiarization* with a T-theoretical term. It may even be the case that for the same T-theoretical term there is more than one method of determination that has that paradigmatic role, and with it more than one path of special law(s). Having that paradigmatic role for a T-theoretical term is simply providing a path with which people usually learn to use that term to determine some of its values. In my opinion, that is what motivates possible objections such as those mentioned

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[19] On the possibility of including the Lorentz force law as part of classical particle mechanics, see Balzer, Moulines and Sneed (1987, pp. 189-190).

[20] Apparently, such an approach is defended by Díez (2002, p. 6). Díez and also Ginnobili have debated with me about this issue. It seems to me that they identify something important related to a T-theoretical term.

[21] The reconstruction of classical particle mechanics by Balzer, Moulines and Sneed (1987, pp. 180-191) allows this to be inferred.

in the previous paragraph.[22] Furthermore, I understand that this is what motivates the Kuhnian 'weakly delimited (local) semantic holism' proposal. Moreover, this explains why Kuhn raises this proposal of 'weakly delimited semantic holism' for 'classical mass' considering different possible paradigmatic paths (see Kuhn 1989, pp. 18 and ff.).

In light of the foregoing, I understand that:

- if it is a matter of establishing the conceptual identity of a T-theoretical term, or its meaning, then this justifies adopting 'a highly delimited (local) semantic holism';
- if it is a matter of establishing how familiarity with a T-theoretical term is reached, then that justifies adopting 'a weakly delimited (local) semantic holism'.

5. The constitutive role of a fundamental law

I have borrowed from Kuhn the idea that there are laws that are non-absolute synthetic *a priori* (an idea also considered by Reichenbach and Friedman). I have assumed that the fundamental law of a complex theory, given the conditions that characterize it, has the status of non-absolute synthetic *a priori*. I should point out now that each fundamental law for a complex theory has a constitutive role.[23] The latter means that:

a) The fundamental law for a complex theory T establishes, by means of the characteristic terms (T-theoretical and T-non-theoretical terms), a certain way of conceptual representation of possible parts of the world (the possible applications of T, and among them the intended ones): the way of representing with T; such conceptual representation requires expanding with T-theoretical notions the possible representation through T-non-theoretical notions.

b) The fundamental law for a complex theory T provides the meaning of each T-theoretical term, together with the relevant basic constraints for each of those terms (if any).

c) The fundamental law for a complex theory T sets the ontological possibilities according to T: kinds of entities (domains, properties, relations and functions) for the different characteristic terms (T-theoretical and T-non-theoretical). These ontological possibilities do not have to be understood in the sense of kinds of entities actually existing in the external world, but rather as kinds of entities conceived according to T: nothing ensures that a theory gets to carve up the external world at its joints.[24]

d) The fundamental law for a complex theory T provides the basic requirement according to which possible parts of the world (their possible applications, and among them the

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[22] It seems to me that this important aspect corresponds to what Díez and Ginnobili identify as part of the meaning of a T-theoretical term as 'classical mass'.

[23] For the constitutive role of fundamental laws, see Falguera (2019). See also two papers initially presented as lectures in 2004 in Xalapa (Veracruz): Jaramillo (2012) and Falguera (2012); Lorenzano (2006; 2007a; 2007b; 2008; 2011; 2019). Díez (2002) talks about the constitutive role of guiding principles in relation to the individuation of theoretical concepts. Ginnobili (2007) defends that Darwin's Theory of Natural Selection contains a guiding principle.

[24] At least this is Kuhn's point of view and the one predominant among structuralists. Among the latter, see Moulines (1991, Chapter II.2) This is also my point of view.

intended ones) are regulated (governed) as systems; however, such a way of regulating is almost devoid of empirical content; rather it provides the admissible limits under which to establish different special laws for specific groups of intended applications.

e) The fundamental law for a complex theory T is the condition that must be preserved, even if other laws of T change, in order to change other special laws of T (negative heuristic, in terms of Lakatos (1970)). Changing the fundamental law of T for intuitively similar problems involves changing T to another alternative theory.

f) The fundamental law for a complex theory T provides a poorly determined guiding criterion for the development of T, that is, a guiding criterion to establish special laws for T (positive heuristics, in terms of Lakatos (1970), or a guiding principle in terms of Moulines (1982, pp. 88 and ff.)).

Taking into account the issue analyzed throughout this paper, it remains to give some specificity to the previous condition "b)"; namely, that the fundamental law provides the meaning of each T-theoretical term. In this regard, we must say that with the fundamental law of a theory T, the following aspects (some implicitly) are provided:

- Information about the set-theoretical conditions that correspond to the possible entities of the set-extensions of the characteristic (or basic) terms of T (T-theoretical and T-non-theoretical terms). What, in structuralist metatheory, is called the *characterization of a T-term* makes explicit such conditions for that term. There is a characterization for each characteristic term of a theory T. A characterization for a term establishes: (i) if the possible entities of its extension belong either to a base domain for T or to a certain set construct obtained from some base domain(s) (for example, the set of particles, or that of spatial positions) and some auxiliary domain(s) (for example, the set of real numbers); and (ii) certain restrictions on such base domains or such set constructs.[25]
- The basic scheme of interdependence of the characteristic terms of T (excluding those corresponding to base domains); the fundamental law of T shows such a basic scheme of interdependence.

There is another aspect to take into consideration regarding the meaning of T-theoretical terms:

- As indicated previously, the meaning of a T-theoretical term together with its fundamental law sometimes depends on some relevant basic constraints of T for that term.
- The meaning of each T-non-theoretical term depends either on some other theory *T'* for which that term is *T'*–theoretical (e.g., spatial position, temporal moment), or on pre-theoretical considerations and paradigmatic exemplars in the case of some terms for basic domains (e.g., particle in classical particle mechanics). That is, the theory T imports the meaning of each term T-non-theoretical either from some other theory *T'* or from pre-theoretical considerations.

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[25] For 'characterization', understood according to the structuralist metatheory framework, see Balzer, Moulines and Sneed (1987, p. 14) and Moulines (1991, pp. 231-232).

6. Conclusion

Throughout this paper:

- I have accepted that what in structuralist metatheory is known as a fundamental law for a complex theory captures what Kuhn presented as a law with a stipulative character, and later as a law with the status of a non-absolute synthetic *a priori*.
- I have formulated a proposal to elucidate the notion of fundamental law, which goes beyond the symptoms that have been considered as necessary conditions in structuralist metatheory. This elucidation rests on a kind of inversion of the (pragmatic) criterion of theoreticity of structuralist metatheory.
- I have attempted to show that the conceptual identity, or meaning, of a T-theoretical term, depends solely on its fundamental law (along with the relevant basic constraints).
- I have defended that the meaning of a T-theoretical term must be complemented with senses, where each sense is given by those special laws (or other conditions with less status) that are used or presupposed in each method of determination.
- I have differentiated between the conceptual identity, or meaning, of a T-theoretical term and the paradigmatic ways of familiarization with that term, where each paradigmatic way of familiarization requires the meaning of that term complemented by a certain sense.
- I have argued that the conceptual identity, or meaning, of a T-theoretical term only requires the so-called highly delimited semantic (local) holism; whereas each paradigmatic way of familiarization with a T-theoretical term requires what I have called loosely delimited (local) semantic holism.

Acknowledgments

This paper has received financial support from ERDF/Spanish Ministry of Science and Innovation under the project PID2020-115482GB-I00. This article also contributes to the projects PICT 2018-3454 with financial support from National Agency for the Promotion of Research, Technological Development and Innovation-Argentina. I would like to thank C. Ulises Moulines, José A. Díez, Pablo Lorenzano and Santiago Ginnobili for their comments on the preliminary versions of this text. It goes without saying that any remaining shortcomings are all mine.

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Ontological Reduction: the Reduction of Classical Collision Mechanics to Classical Particle Mechanics

María de las Mercedes O'Lery[1]

1. Introduction

A little over seventy years ago, in an attempt to justify the need to clearly define scientific reduction, Ernest Nagel affirmed that there is a recurring pattern in the history of modern science which entails a relatively autonomous branch of science being absorbed by or "reduced" to another (Nagel 1949, p. 100). Several years before, he claimed that not one serious study existed on the nature of reduction, not even from those whose exclusive preoccupation was the logic of science (Nagel 1935, p. 46). Nagel's analysis of reduction was mostly linked to the study of explication in science. Similarly, others were also worried about clearly defining scientific reduction, such as John Kemeny and Paul Oppenheim. However, for them, the importance of examining reduction was rooted in its connection to the notion of scientific progress. Nonetheless, regardless of each of these authors' motivation, Nagel's analysis together with that of Kemeny and Oppenheim about reduction in science enabled the establishment of two basic intuitions about reduction-relations: a) the theories involved in a reduction phenomenon are somehow semantically related; b) the reducing theory offers stricter assertions about the world than the reduced theory. These are the intuitions that are the foundation of the two conditions established by the classical model of reduction: *connectability* and *derivability* (Nagel 1935; 1949; and especially 1961, Chapter XI; Kemeny and Oppenheim 1956).

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Abreu, C. (ed.) *Philosophy of Science in the 21st Century. Contributions of Metatheoretical Structuralism*.
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This model of reduction remained present in later debates on the notion of reduction attracting both innumerable adherents as well as an assortment of objections. As for the latter, it bears mentioning that the classical model includes the following caveat:

> It is hopeless to attempt a rigorous logical analysis of science in the form in which we normally find it. It is not the role of the working scientist to make his fundamental assumptions clear, and it is not reasonable to expect that he will proceed according to rigorous logical rules. Hence, it is customary for the philosopher of science to consider science in an idealized form. (Kemeny and Oppenheim 1956, pp. 7-8)

Yet, in spite of this caveat, the classical model, especially the Nagelian theory, has been criticized both for being too "broad" in one sense as well as for being too "narrow" in another. In the first place, if the reduction is sometimes a derivation with bridge laws, then any theory would reduce to itself (because any theory is derivable from itself). Moreover, any theory would reduce to any inconsistent theory, and contrary to what one would expect, the reduction would not result in an asymmetric relationship. Secondly, he introduced criteria that did not seem to be compatible with his model. His criteria described how the reducing theory should be fertile with useful suggestions for development of secondary science and should produce theorems that refer to the same subject matter as the last one and that increase or correct the body of laws currently accepted (Nagel 1961, p. 330). The criteria seemed incompatible with his model because if the deduction is the foundation for the reduction, it does not seem to be clear then how it is possible for a reducing theory to correct it (van Riel and van Gulick 2019). Thus, one could expect that the supposed historical cases of reduction without replacement could not be captured under this notion of reduction.

On the other hand, from a diachronic perspective, a common criticism has been to point out the difficulty to reconcile the thesis of incommensurability with a drastic change of theory such as one produced by a scientific revolution, with the possibility of proposing a reduction of the displaced theories by the superseding ones.

In response to these objections as well as others that are not mentioned here, in recent analysis, some authors have tried to elaborate models that serve as alternatives to the classical model, grounding them in refinements of the logical framework (van Riel and van Gulick 2019).

However, these formal conditions for establishing the reduction from one theory to another have not been sufficiently problematized in the case of homogenous reductions. Nagel (1961) appeals to two cases to exemplify this type of reduction. The first one is that of the reduction of the mechanics of rigid bodies to Newtonian mechanics. This is a clear case exemplifying a theory that is initially formulated for a restricted group of phenomena and later expands, covering a wider group. The second case is that of the reduction of the laws of Galileo about the movement of earthly bodies to Newtonian mechanics. In this case it would be the absorption of two types of phenomena (the movement of earthly bodies and heavenly bodies) under the umbrella of just one theory (Newtonian mechanics). For Nagel, in a homogenous reduction, the laws of the reduced theory do not offer descriptive terms that have not been used approximately with the same meaning in the reducing theory (Nagel 1961, p. 339). Given that the vocabulary used in both theories is homogenous, it follows not only that a deductive relationship will be satisfied between the declarations of both theories but also, it is to be expected that the condition of connectability is met, with no need for additional suppositions to connect the meaning of the terms of the reduced theory with those of the reducing one. From there, Nagel, and the general debate about intertheoretic reduction, downplay the treatment of this type of reductions and prefer paying special attention to the cases of heterogenous reduction. In fact, Nagel himself, though he recognizes that some cases of homogeneous reduction may involve a degree of complexity, accepts that this does not manage to problematize the formal conditions of connectability and derivability.

Therefore the following observations are considered: a) Structuralism has been one of the first approaches to respond to the problems that plagued the Nagelian model of reduction. Structuralism maintains the spirit of the conditions connectability and derivability but has pointed out that to analyze the reduction-relation one must begin from a non-syntactical conception of the scientific theories; b) Structuralism has put forward reconstructions of classical collision mechanics that illuminate its intertheoretic reduction-relation with classical particle mechanics (Balzer and Mühlhölzer 1982; Moulines 1984; Moulines 1985; Balzer, Moulines and Sneed 1987). In light of these observations, the aim of this work is to consider the adequacy of structuralist analysis for treating the reductions that are homogeneous in light of a case sufficiently analyzed by structuralism, the reduction of classical collision mechanics to classical Particle Mechanics. The theoretical framework that will be used here mainly incorporates the structuralist characterization for the intertheoretic reduction-relation (Balzer, Moulines and Sneed 1987), the additional condition of ontological reduction (Moulines 1984) and a recent reformulation of this condition in terms of echeloned partial substructures (O'Lery 2018).

2. Reduction and structuralism

The structuralist program was one of the first to offer a response to the problems of the classical model of reduction. Initially, the structuralist proposal consisted in signaling the possibility of more adequately characterizing the notion of reduction, modifying the way of conceiving scientific theories. Structuralism warned that the problems of the classical model of reduction have their origin in an inadequate vision of science since the analysis of classical reduction rests on a syntactic conception of the theories which are plagued by problems that can be overcome by starting from a "more global" scientific approach (Díez and Moulines 2008, p. 376).

Keeping in mind this change of perspective about theories, Niebergall (2002) has distinguished at least three attempts to characterize the reduction-relation since 1955. The first of such attempts can be attributed to the works of Ernest W. Adams in the mid to late 1950s.[2] A second attempt was the analysis offered by Wolfgang Balzer in the early 1980s. Finally, the third and most recent characterization of the notion of reduction from the structuralist framework is the version offered by Wolfgang Balzer, C. Ulises Moulines and Joseph D. Sneed in *An Architectonic for Science* (hereafter *An Architectonic*). The notion of reduction described in *An Architectonic* can be characterized in the following way:

If T and T^* are two theories such that $T = \langle M_p, M, M_{\mathrm{pp}}, \mathrm{GC}, \mathrm{GL}, I\rangle$ and $T^* = \langle M^*_p, M^*, M^*_{\mathrm{pp}}, \mathrm{GC}^*, \mathrm{GL}^*, I^*\rangle$, one could affirm through a *reduction-relation* ρ, that T^* directly reduces to T (T is reduced by T^*) whenever the following conditions are met:

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[2] Even though Niebergall mentions the works of Adams as the first attempts to define the notion of reduction in the structuralist framework, it would be more appropriate to consider those to be precursory or anticipatory works of structural analysis of reduction given that J. D. Sneed's 1971 book, *The Logical Structure of Mathematical Physics*, is considered to be the first publication of the structuralist conception.

1_{BMS}. There is a global reductive relationship ρ which is a subset of the Cartesian product of M_p^* and M_p.

2_{BMS}. The range of the function ρ is the class M_p.

3_{BMS}. For every x^*, x, if $x^*\rho x$ and x^* is present in M^*, then x is present in M.

4_{BMS}. For every X^* that is a subset of the domain of ρ, if X^* belongs to the class GC^*, then $\overline{\rho}(X^*)$ belongs to the class GC.

5_{BMS}. For every x^*, x, if $x^*\rho x$ and x^* is present in GL^*, then x is present in GL.

6_{BMS}. For every y present in I, there is a y^* present in I^* and a pair x, x^* such that $x^*\rho x$, $\mathbf{r}^*(x^*) = y^*$ and $\mathbf{r}(x) = y$ and y^* is present in I^*.

Where M_{p}, M, M_{pp}, GC, GL and I are the components of an "idealized theory-element", T, through which a scientific theory can be identified. M_{p} is named the class of *potential models* and is made up of every system that can be subsumed in the conceptual framework proposed by T. M is the class of *models* for all systems that in addition to being able to be subsumed in the conceptual framework of the theory, also satisfy the laws of T. The set, M_{pp} is the class of *partial potential models* and is made up of every system that can be described in the non-theoretical vocabulary about T. The set GC *(global constraint)* groups together every condition that expresses real and/or conceptual connections among different applications of the theory. The set GL *(global link)* includes every connection that represents transference of information between different theories, and finally, I is the set made up of all the *intentional applications* of T. The same will be said of the components $M_{\text{p}}{}^*$, M^*, $M_{\text{pp}}{}^*$, GC^*, GL^* and I^* with respect to the theory-element T^*.

The condition 1_{BMS} simply establishes that both theories are "globally correlated" in terms of their conceptual frameworks. The second condition indicates the sense in which that relationship occurs.

Condition 3_{BMS} expresses the derivability of the laws. Conditions 4_{BMS} and 5_{BMS} express respectively the compatibility of the reduction-relation between the classes of constraints and intertheoretical links. Condition 6_{BMS} corresponds to one of the conditions already established for the reduction-relation offered previously by Balzer (1982; 1985).[3]

With respect to this approach to reduction, Niebergall (2002) has stated that formal conditions for ρ could be used to infer mathematical theorems that could cast doubt on some of the structuralist reduction definitions. He explains this exercise would not in and of itself be enough

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[3] The fourth condition imposed by Balzer (1982; 1985) for the definition of a reduction-relation affirmed the following: For every y present in I, there is a y^* present in I^* with $y\hat{\rho}y^*$. This condition also corresponded to a second condition demanded by the reduction in the characterization of Adams (1955; 1959) though his implied intermediate steps of inferences. In the first place, Balzer considered a function of restriction r from which each model y belonging to the class of partial models of a theory, there is an x belonging to the class of the potential models such that $r(x) = y$. In the second place, Balzer defined a relationship of translation $\hat{\rho}$ between the classes of the partial potential models of T and T^* from affirming that two partial models y and y^* belonging to the theories in question are related through the relationship $\hat{\rho}$ only if there are two models x and x^*, belonging to the class of the potential models of T and T^*, respectively, and $x\rho x^*$, $r(x) = y$ and $r^*(x^*) = y^*$; meaning, only if the partial models in question are the result of applying the function of restriction to the potential models of the involved theories, and, moreover, there is a relationship of translation ρ between these potential models. Finally, the mention of the class I in this condition was justified from conceiving I to be a subset of the partial models.

of an argument to affirm that every structuralist version of the reduction-relation is inadequate. Similarly, he mentions this exercise would not be enough to deny the merits of trying to describe this notion from a theoretical framework like the structuralist one. Even so, his results lead him to concede that there is still a certain general weakness in these attempts to be precise in the notion of reduction. Assuredly, this insufficiency had already been described by Moulines who proposed an additional condition—ontological reduction—to solve it:

> However, the present structuralist concept of reduction still does not tell all there is to tell about reduction. There is at least one further aspect of reduction that is overlooked by scheme (R) [the structuralist reduction scheme]. This is what I would like to call "the ontological aspect". I wish to argue that, for a complete picture of a reductive relationship between two theories, one has to take into account some sort of relation between the respective domains. Otherwise, when confronted with a particular example of a reductive pair, we would feel that all we have is an ad hoc mathematical relationship between two sets of structures, perhaps by chance having the mathematical properties we require of reduction but not really telling something about "the world". We could have a reductive relationship between two theories that are completely alien to each other. (Moulines 1984, p. 55)

This attempt to establish additional conditions that could revert this debility, like the additional condition proposed by Moulines, is recognized by Niebergall as a well directed proposal (Niebergall 2002, pp. 155-156).

2.1. Homogeneous and heterogenous ontological reduction

In correspondence with what Spector has called "*domain preserving reduction*" and "*domain eliminating reduction*", Moulines makes a distinction between two classes of ontological reduction, which he chooses to call "homogenous" and "heterogenous".

Moulines characterizes the homogenous ontological reduction as a type of relation of identity on an ontological level between the domains of the theories that are involved in a reduction-relation. This identity between domains can be total or partial. The homogenous ontological reduction is a relationship of total identity when a base set of the reduced theory is identified with a base set of the reducing theory. It is partial when a base set of the reduced theory is identified with a subset belonging to a base set of the reducing theory.

On the other hand, an ontological reduction is considered heterogenous when one of the base sets of the reduced theory is related to one or more base sets of the reducing theory in a way that does not imply identification of elements.

One such case would be when we can intuit that the ontological units of each theory (especially the reduced one T) are made up of only one class of ontological units of the other theory (the reducing one T^*). This, at the same time, could imply that one of the elements of a base set of the reduced theory T is related to: a) a *set* of elements from a base set of the reducing theory or b) a *sequence* of elements from the base sets of the reducing theory. Nonetheless, in neither of these cases could it be affirmed that the elements of the domains that participate in the relation are identical, as it would be more adequate to affirm that there is a correspondence between them. This is the connection of supposed correspondence that reveals the need for a specific function that, together with the other elements, makes it possible to characterize the reduction-relation. Moulines puts forward, then, a function ω to fill this role.

Thus, be T (reduced) and T^* (reducing) both theories such that

$$T = \left\langle M_p, M, M_{\text{pp}}, GC,\ GL, I \right\rangle$$

and

$$T^* = \left\langle M_p^*, M^*, M_{\text{pp}}^*, \mathrm{GC}^*,\ \mathrm{GL}^*, I^* \right\rangle,$$

and additionally each potential model $x \in M_p$ y $x^* \in M_p^*$ has the form

$$x = \left\langle D_1, \cdots, D_n,\ A_1, \cdots, A_m,\ r_1, \cdots, r_p \right\rangle$$

and

$$x^* = \left\langle D_1^*, \cdots, D_n^*,\ A_1^*, \cdots, A_m^*,\ r_1^*, \cdots, r_p^* \right\rangle,$$

a function ω in such cases of correspondence would have the form:

a) $\omega : D_i \longmapsto Pot\left(D_j^*\right)$, or rather

b) $\omega : D_i \longmapsto \left(D_j^* \times \cdots^n \times D_j^*\right)$, for $n \in \mathbb{N}$.

In addition to these cases of correspondence, it could be hoped that in cases of more complex reduction the base domains $(D_1, \cdots, D_n)$ of the reduced theories correspond to various basic domains $(D_1^*, \cdots, D_n^*)$ of the reducing theory, or with some subset(s), or even with an auxiliary domain $(A_1^*, \cdots, A_m^*)$ in such a way that relationships can be established that require the function ω to take on some of the following forms:

c) $\omega : D_i \longmapsto \left(D_{j_1}^* \times \cdots^n \times D_{j_n}^*\right)$, for $n \in \mathbb{N}$ with $\left\{D_{j_1}^*, \cdots, D_{j_n}^*\right\} \subseteq \left\{D_1^*, \cdots, D_n^*\right\}$

d) $\omega : D_i \longmapsto \left(Pot(D_{j_1}^*) \times \cdots^n \times D_{j_n}^*\right)$

e) $\omega : D_i \longmapsto \left(Pot(D_{j_1}^*) \times \cdots^n \times Pot(D_{j_n}^*)\right)$

f) $\omega : D_i \longmapsto \left(D_{j_1}^* \times \cdots^n \times D_{j_n}^* \times A_{k_1}^* \times \cdots^n \times A_{k_m}^*\right)$, with $\left\{A_{k_1}^*, \cdots, A_{k_m}^*\right\} \subseteq \left\{A_1^*, \cdots, A_m^*\right\}$

g) $\omega : D_i \longmapsto \left(Pot(D_{j_1}^*) \times \cdots^n \times D_{j_n}^* \times Pot(A_{k_1}^*) \times \cdots^n \times A_{k_m}^*\right)$

h) $\omega : D_i \longmapsto \left(Pot(D_{j_1}^*) \times \cdots^n \times Pot(D_{j_n}^*) \times Pot(A_{k_1}^*) \times \cdots^n \times Pot(A_{k_m}^*)\right)$

All these forms from a) to h) that the function ω could take on, depending on the level of complexity that reduction-relation implies, would be expressed under the following typification for said function:

$$\overline{\omega}(D_i) \in \tau_i \left(D_{j_1}^*, \cdots D_{j_n}^*, A_{k_1}^*, \cdots, A_{k_m}^*\right).$$

Finally, considering a projection function Π and the function ω typified as $\overline{\omega}(D_i) \in \tau_i\left(D^*_{j_1}, \cdots D^*_{j_n}, A^*_{k_1}, \cdots, A^*_{k_m}\right)$, what was said informally about two types of ontological reduction can be formalized in the definitions that are presented below.

The homogeneous ontological reduction can be characterized by the following definition:

Definition I. Given x and x^*, two models such that

$$x = \left\langle D_1, \cdots, D_n, \; A_1, \cdots, A_m, \; r_1, \cdots, r_p \right\rangle$$

and $x \in M_p$ and

$$x^* = \left\langle D^*_1, \cdots, D^*_n, \; A^*_1, \cdots, A^*_m, \; r^*_1, \cdots, r^*_p \right\rangle$$

and $x^* \in M^*_p$; and given Π_i and Π_j, two projection functions such that $\Pi_i x$ is the i^{th} base set of x and $\Pi_j x^*$ is the j^{th} base set of x^*, it can be affirmed that ρ is a *homogeneous reductive relationship* of T to T^* iff:

1) For every $x \in M_p$ and every $x^* \in M^*_p$ it occurs that $x^* \rho x$.
2) There is a $i, \; j \in \mathbb{N}$ with $1 \leq i, j \leq n$ such that $\Pi_i x$ is the i^{th} base set of x and $\Pi_j x^* j^{\text{th}}$ base set of x^* and $\Pi_i x \subseteq \Pi_j x^*$.

Condition 1) demands that there be an effective reduction-relation between the potential models of the respective theories T and T^*. Condition 2) basically demands that there be at least one basic domain in the potential models of T that are subsets of a basic domain present in the potential models of T^*.

What was said informally about the nature of the heterogenous ontological reduction will be formally explicated in the following definition:

Definition II. Given x and x^*, two models such that

$$x = \left\langle D_1, \cdots, D_n, \; A_1, \cdots, A_m, \; r_1, \cdots, r_p \right\rangle$$

and $x \in M_p$, and

$$x^* = \left\langle D^*_1, \cdots, D^*_n, \; A^*_1, \cdots, A^*_m, \; r^*_1, \cdots, r^*_p \right\rangle$$

and $x^* \in M^*_p$; given Π_i and Π_j being two projection functions such that $\Pi_i x$ is the i^{th} base set of x and $\Pi_{j_n} x^*$ is the j^{th} base set of x^*, given ω a bijective function and τ_i a typification, it can be affirmed that ρ is a *heterogenous reductive relationship* of T to T^* iff:

1) For every $x \in M_p$ and every $x^* \in M^*_p$ it occurs that $x^* \rho x$;
2) There is an $i, j_1, \cdots, j_n, k_1, \cdots, k_m \in \mathbb{N}$, a function ω and a typification τ_i such that $\Pi_i x$ is the i^{th} base set of x, $\Pi_{j_1,k_1} x^*, \; \cdots, \Pi_{j_n,k_m} x^*$ are the $j_1, k_1, \cdots, j_n, \; k_m{}^{\text{th}}$ base sets of x^* and

   $$\overline{\omega}\,(D_i) \in \tau_i\left(D^*_{j_1}, \; \cdots D^*_{j_n}, A^*_{k_1}, \; \cdots, A^*_{k_m}\right)$$

   and

   $$\overline{\omega}(\Pi_i x) \in \tau_i\left(\Pi_{j_1,k_1} x^*, \; \cdots, \Pi_{j_n,k_m} x^*\right).$$

Condition 1) demands the same as the first condition in the definition of homogeneous ontological reduction. On the other hand, condition 2) demands that there be at least one basic domain in the potential models of T that may be correlated to one or more of the domains of T^* and in one of the ways established by the function ω.

2.2. Ontological reduction and echeloned partial substructures

In recent years, Moulines has proposed a way of improving the formal characterization of four types of theoretical developments in the empirical sciences that he himself breaks down into: crystallization, theoretical evolution, incorporation and replacement with partial incommensurability. Aiming to carry out a more adequate treatment of these diachronic structures, Moulines introduces the notion of echeloned partial substructures (Moulines 2011) for the first time in the structuralist conceptual framework. From his treatment of these theoretical developments using the notions of theory-net and echeloned partial substructures, especially the theoretical incorporation, Moulines concludes that the intertheoretical reduction-relation (as well as the equivalence and the approximation) can be considered a special sub-type of incorporation:

> I have not formally demonstrated this theorem, but it seems plausible to me in light of the notions in question. Whether or not a formal proof of the theorem is possible depends on, essentially, the adopted definition of reduction (since equivalency and approximation are "variations" of reduction). The problem lies in the fact that there is still no consensus (neither among structuralists nor among non-structuralists) about which is the most adequate formal elucidation of intertheoretical reduction. Now, if we accept that, whatever the final elucidation may be, an essential component of it is what in Moulines (1984) I called "ontological reduction", then it seems that the proof of theorem is quite direct, given that the ontological reduction can be defined in terms of what I call here "echeloned partial substructures". (Moulines 2011, p. 24, n. 7)

In what follows, definitions offered by Moulines (2011) to specify the notion of echeloned partial substructures are exposed. Afterwards, a proposal will be described in which the definitions of ontological reduction are rethought from this recent refining of structuralist conceptual framework (O'Lery 2018).

2.2.1. Echeloned partial substructures

The notion of echeloned partial substructures relies on the notions of *component, partial substructure and echelon set* which are presented by Moulines in the following way (Moulines 2011, p. 17):

Definition AUX-I. Given $S = \langle A_1, \cdots, A_n \rangle$ a structure with m domains $D_1, \cdots, D_m$ and $n-m$ relationships (with $n > m$) $R_{m+1}, \cdots, R_n$. Given S' a structure, given A a domain or a relationship, A is a *component* of S ($A \widehat{\in} S$) if there exist a I such that $1 \leq i \leq n$ and $A = A_i$.

In other words, A is a component of a structure as long as it can be identified with some domain or relationship of said structure.

Definition AUX-II. S' is a *partial substructure* of S ($S' \widehat{\in} S$) if for every A' that is component of S', there is at least one A such that A is component of S and $A' \subseteq A$.[4]

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[4] The original definition that appears in Moulines (2011) of partial substructure affirms that S' is a *partial*

Meaning that, for a structure to be a partial substructure of another it is necessary and sufficient that all its components (be they domains or relations) identify completely or partially with at least one of the components of the structure to which they are a substructure.

To define the notion of echelon set, Moulines first establishes a set-operation, Θ, consists in applying operations "*Pot*" and "×" (powerset and Cartesian product) to certain previously given sets a finite number of times, always starting with *Pot*. In such a way that:

Definition AUX-III. A is an *echelon set* over $B_1, \cdots, B_m$ if $A \in \Theta\left(B_1, \cdots, B_m\right)$.

Put more simply, A is an echelon set above certain sets $B_1, \cdots, B_m$ as long as A is one of the n-tuples resulting from the operation Θ over $B_1, \cdots, B_m$; that is to say, as long as A is an n-tuple whose elements are one of the sets $B_1, \cdots, B_m$, some subset of $B_1, \cdots, B_m$, or the empty set (given that the operation implies applying the Cartesian product and power set).

Finally, from the above definitions, the notion of partial substructure is defined as:

Definition AUX-IV. S is an *echeloned partial substructure* of S^* $(S \eta S^*)$ if for every S_i that is a partial substructure of S, there is at least one S_k^* that is partial substructure of S^*so that S_i is an echelon set over S_k^* $(S_i \in \Theta\left(S_k^*\right))$.

Meaning, a structure S will be considered an echeloned partial substructure of another S^*, as long as all of its partial substructures can be considered to be one of the elements of the resulting set from the application of the operation Θ over a partial substructure of S^*.

2.2.2. Ontological reduction in terms of echeloned partial substructures

So then, keeping in mind the definitions offered for homogeneous and heterogeneous ontological reduction (Definitions I and II), and this refining of the structuralist conceptual framework through the notion of echeloned partial substructure (Definition AUX-IV), in a recent work (O'Lery 2018) has been offered the first attempt at the reformulation of those definitions of ontological reduction from this last notion. Said proposal of reformulation is detailed below.

In the informal analysis of homogeneous ontological reduction, it was characterized as a type of identity relationship on an ontological level between the domains of the theories that participate in a reduction-relation, which can be total or partial. Just as was said before, said identity relationship is considered total when a basic domain of the reduced theory is identified with a basic domain of the reducing theory. It is partial when a basic domain of the reduced theory is identified with a subset belonging to a basic domain of the reducing theory.

Now, the reduction is a relationship established on the level of the potential models and, in formal terms, every potential model x is a structure ($x = \langle A_1, \cdots, A_n \rangle$ with m domains $D_1, \cdots, D_m$ and n-m relationships (with $n \geq m$) $R_{m+1}, \cdots, R_n$). In this way, it immediately follows that a determined set D_i is a domain in the potential models of a theory if and only if $D_i \widehat{\in} x$. On the other hand, as Moulines has pointed out, any set that is a component of a structure S can be

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substructure of S ($S' \widehat{\in} S$) if every A that is a component of S' is also a component of S. However, in personal conversations Moulines has chosen to modify that definition for the one mentioned in this text given that, when considering the definition of "being a component", his original definition of substructure becomes somewhat strict, and no longer captures the notion of substructure.

considered trivially as a partial substructure of S, given that every set A can be trivially converted into a structure $\langle A, \emptyset\rangle$, where $\emptyset$ is the empty relationship. In this way, every domain D_i that is a component of a potential model x, can be trivially considered to be a partial substructure of x. At the same time, D_i can be trivially considered an echelon set over other domain(s) belonging to another theory, say D_j^*, as long as D_i is one of the ordered pairs having resulted from the operation Θ over D_j^*. In other words, as long as D_i is an ordered pair of which the elements of the pair are D_j^*, or rather a subset of D_j^*, or even an empty set.[5]

In this way, formally speaking, for the identity relationship (total or partial) mentioned previously to exist, the only thing that seems to be necessary about the domains is that $D_i \in \Theta\left(D_j^*\right)$; given that if $D_i \subseteq D_j^*$, then D_i is an echelon set over D_j^*. At the same time, domains D_i and D_j^*, as long as they are components of the models, can be trivially considered to be partial substructures of those models. Thus, the definition of homogenous ontological reduction-relation in terms of echeloned partial substructures could take the following form:

Definition III. Given x y x^* two models such that

$$x = \left\langle D_1, \cdots, D_n, A_1, \cdots, A_m, r_1, \cdots, r_p\right\rangle$$

and $x \in M_p$ and

$$x^* = \left\langle D_1^*, \cdots, D_n^*, A_1^*, \cdots, A_m^*, r_1^*, \cdots, r_p^*\right\rangle$$

and $x^* \in M_p^*$, ρ will be a *homogeneous reduction-relation* of T to T^* iff:

1) For every $x \in M_p$, there is a $x^* \in M_p^*$ such that $x^* \rho x$ and $x \eta x^*$.

However, it has already been mentioned that this first attempt of reformulation of homogeneous ontological reduction with this definition could be objected to as something not completely satisfactory (O'Lery 2018, p. 135). This is due to the fact that it would be too demanding, and in two ways. Firstly, it is too strict because it demands an identity relationship for every component of the models of T. This demand is inferred from the requirement that x must be echeloned partial substructure of x^*; that is to say, that every substructure of x (be it basic domain or not), should be an echelon set over some substructure of x^*.This eventual inadequacy could be rejected by arguing that, though the analysis of ontological reduction is carried out principally in terms of basic domains, the possibility of relationships of identity or correspondence among auxiliary domains is not excluded. In fact, these are the domains (basic and auxiliary) on which relations and/or functions are constructed. Secondly, independent of whether this argument is conceded or not, the definition could be objected to as inadequate insofar as all components (substructures) of a structure are required to be an echelon set over the components (substructures) of another, for the former to be considered an echeloned partial substructure of the latter. And, even specifying that "components" only refers to the basic domains, this could be overstated in that, for ontological

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[5] Considering the definition of echelon set, it would be formally possible that one of the elements of the n-tuple be an empty set. Yet, for the analysis of a particular ontological reduction to fit with this formal scenario, we would need a case of supposed ontological reduction in which one of the domains of the reduced theory does not correspond to any domain in the reducing theory. Although it would certainly be debatable as to whether or not such a case should be considered a genuine ontological reduction, it would still be interesting to analyze how to conceive that relationship.

reduction, be it homogenous or heterogenous, only identity or correspondence is required for at least one of those domains.

Answering these objections, alternatively, a definition could be proposed that more appropriately captures the informal characterization leaving aside the notion of echeloned partial substructure, and instead using the notion of echelon set. This version would imply modifying at least the second condition of the definition (Definition I) given originally by Moulines for this other:

2’) There is an $i,\ j \in \mathbb{N}$ with $1 \leq i, j \leq n$ such that $\Pi_i x$ is the i^{th} base set of x and $\Pi_j x^*$ is the j^{th} base set of x^* and $\Pi_i x \in \Theta(\Pi_j x^*)$.

With respect to heterogenous ontological reduction, regardless of the complexity involved in the correspondence between domains of the reduction relation, it can be captured by a function that takes on the form:

$$\omega : D_i \longmapsto \Theta\left(D^*_{j_1}, \cdots, D^*_{j_n}, A^*_{k_1}, \cdots, A^*_{k_m}\right)$$

In such a way that the definition of the heterogenous ontological reduction in these terms would lead to simply modifying the original condition Definition II.2 with:

2”) There is an $i, j_1, \cdots, j_n, k_1, \cdots, k_m \in \mathbb{N}$, a function ω such that $\Pi_i x$ is the i^{th} base set of x, $\Pi_{j_1,k_1} x^*, \cdots, \Pi_{j_n,k_m} x^*$ are the $j_1, k_1, \cdots, j_n,\ {k_m}^{\text{th}}$ base sets of x^* and $\omega\,(\Pi_i x) = \Theta\left(\Pi_{j_1,k_1} x^*, \cdots, \Pi_{j_n,k_m} x^*\right)$.

In what follows there will be analysis of a specific case—the reduction of classical collision mechanics to classical particle mechanics—in order to evaluate the adequacy of this structuralist analysis for the treatment of homogeneous ontological reductions.

3. The reduction of Classical Collision Mechanics to Classical Particle Mechanics

The specific case that will be considered for evaluating this definition of ontological reduction in terms of echeloned partial substructures is the reduction that can be established between classical collision mechanics and classical particle mechanics. This case, moreover, is one of the ones mentioned by Moulines as an example of completely homogeneous reduction since all the basic domains present in classical collision mechanics are identified with a basic domain that is present in classical particle mechanics (see Moulines 1984, p. 61).

Structuralism has already offered reconstructions of both theories that made it possible to formalize the relationship between the two (Balzer and Mühlhölzer 1982; Moulines 1984; Moulines 1985; Balzer, Moulines and Sneed 1987). In these cases the analysis has been carried out based on a prenewtonian version of collision mechanics that systematizes the works of Christian Huygens, Christopher Wren and John Wallis, among others. And, of course, as Schliesser affirms:

> The papers by Wallis, Wren, and Huygens of 1668 and 1669 that settled on a widely shared and recognized mathematical treatment of the rules of collision which were claimed to have high empirical confirmation and predicted surprising empirical results; this post-Galilean analysis of motion became an autonomous practice

> relatively insulated from metaphysical and theological concerns. This is one reason why Newton singles them out for praise ("the greatest geometers of our times") in the scholium to the corollaries of the laws of motion in the *Principia*. (Schliesser 2011, p. 109)

However, beyond the recognition that Newton himself gave to these researchers, the work of systemization of the laws of collision, especially the one carried out by Huygens, has remained invisibilized by the manuals of that time. As Arthur Bell describes:

> So slowly did the Newtonian system displace the Cartesian, however, that in the English edition of Rohault's *System of Natural Philosophy*, which came out in 1723, Descartes's original treatment of motion and impact is closely followed and we read, for example, on page 78: "When a Body moves any particular way, the Disposition that it has to move that way rather than any other is what we call its Determination". Gravesande's *Mathematical Elements of Natural Philosophy* (1721) was more up to date and contained a summary of Huygens's work on impact without, however, an acknowledgement of the source. This work was dedicated to Newton and is an interesting guide to the scientific heritage of the seventeenth century as it was passed on to the eighteenth century reader. (Bell 1950, p. 116)

It was not until the preface of the third edition of 1747 that the authors prior to Newton were more clearly mentioned and Huygens appears among them:

> Besides those, whose writings are found in these transactions, we have daily testimonies and proofs of the advantage of joining mathematics and experiments together from those celebrated men, Polemus, Defaguliers, Bernoulli, Wolfius, Muffchenbroek, and so many more, that it would be tedious to mention them. To the mathematico-phyfical writings of these we may add what has been left about these things by Galileus, Torricelli, Gulielmini, Mariotte, Huygens and many others, who have wrote about the particular parts of Mathematics, belonging to Physics, and some of which I shall refer to in what follows. But among those, who have illustrated Physics by mathematical demonstrations and experiments, Sir Isaac Newton is to be reckoned the chief, who has demonstrated, in his mathematical Principles of Natural Philosophy, the great use of Mathematics in Physics, in as much as no one before him ever penetrated so deeply into the secrets of nature. (Gravesande 1747, pp. XIV-XV)

Even though manuals like those of Gravesande that were the ones responsible for transmitting 17th century concepts of physical theorizations into the following century (Bell 1950, p. 109), the truth is that, as for a post-Galilean and post-Cartesian analysis of motion relatively insulated from metaphysical and theological concerns, Huygens's 1672 *Horologium Oscillatorium* is the paradigmatic work of this sort before the publication of Newton's *Principia* (Schliesser 2011, p. 109). And, as for analysis of collisions, the same could also be said about his work *De motu corporum ex percussione* (hereafter *De Motu*), which contains the most complete version of Huygens's collision mechanics.

It must be said a reconstruction, in general terms, from within the structuralist framework of Huygens's collision theory would not have significant differences from those reconstructions of collision mechanics already given by structuralism. Thus, the reconstruction offered in *An Architectonic*, is considered to be an adequate starting point for analysis of homogeneous reduction that is proposed here. This reconstruction is the most complete structuralist reconstruction of classical collision mechanics to date. However, in light of the fact that the analysis of ontological reduction implies carefully considering aspects related to semantics and recognizing the importance of Huygens's contribution to the establishment of the collision theory some of the analyses offered in *An Architectonic* could be complemented by seeing them side by side with Huygens's work on collision mechanics.

Thus, we present the analysis of the reduction of collision mechanics to classical particle mechanics offered in *An Architectonic* (Balzer, Moulines and Sneed 1987, pp. 26-34, 255-256). Said analysis comes from, initially, considering the following versions of reconstruction of the two involved theories. Here, it is assumed that classical collision mechanics is the reduced theory and classical particle mechanics is the reducing theory.

In the case of the reduced theory, the potential models x of **CCM** (Classical Collision Mechanics) consist of a set P of particles, a set T of two instants ("before" and "after" the collision, $T = \{t_1, t_2\}$), a function $v : P \times T \rightarrow \mathbb{R}^3$ assigning to each particle $p \in P$ and instant $t \in T$ the velocity of p at t, and the mass function $m : P \rightarrow \mathbb{R}^+$ assigning to each particle a value in $\mathbb{R}^+$:

$$x = \langle P, T, \mathbb{R}, v, m \rangle$$

The law of momentum conservation is expressed by the axiom that is hold in all actual model x of **CCM**:

(1) $\sum_{p \in P} m(p) \cdot v(p, t_1) = \sum_{p \in P} m(p) \cdot v(p, t_2)$

Enunciation (1) affirms that the amount of movement, understood as the mass of a body times its velocity, is the same before and after the collision.

On the other hand, the potential models of the reducing theory, y of **CPM** (Classical Particle Mechanics), consist of: a set P of particles or mass-points; a set T of points of time or instants; a set S of points of space; a function of coordinatization of the time $c_1 : T \rightarrow \mathbb{R}$; a function of coordinatization of the space $c_2 : S \rightarrow \mathbb{R}^3$; a position function $s : P \times T \rightarrow S$ assigning to each particle $p \in P$ and points of time $t \in T$ a point of the space, namely that point of space at which particle p is situated at time t; a mass function $m : P \rightarrow \mathbb{R}^+$ assigning to each particle a value in $\mathbb{R}^+$; and a force function $f : P \times T \times \mathbb{N} \rightarrow \mathbb{R}^3$.

$$y = \langle P, T, S, \mathbb{N}, \mathbb{R}, c_1, c_2, s, m, f \rangle$$

And, in all actual models y de **CPM** the axiom is held that expresses Newton's second law "force equals mass times acceleration":

(2) $m(p) \cdot D^2 r(p, \alpha) = \sum_{i \in \mathbb{N}} f(p, \check{c}_1(\alpha), i)$

To be able to establish the reductive relationship of **CPM** over **CCM**, however, another requirement becomes necessary.

In the reconstruction of classic particle mechanics that has been offered, this additional requirement is presented as one of the special laws of the theory in that, arguably, the law does not apply to all the actual models of **CPM** as is the case of enunciation (2), but only to some special cases. This additional premise is the law of action-reaction which implies conservation of momentum (Balzer, Moulines and Sneed 1987, Chapter IV, *TIV-6-a*, p. 184) and affirms that every force ("action") acting on some particle is counteracted by a force acting on a different particle.

Using these reconstruction proposals for classical collision mechanics and classic particle mechanics as a starting point, below, a summary is provided on Balzer, Moulines and Sneed's analysis and demonstration of a reduction-relation between both theories.

In the first place, the analysis carried out on each of the concepts present in **CCM** enables them to affirm that these concepts can be assimilated with their corresponding concepts in **CPM**. As for concepts "particle" and "instant", both are considered non-theoretical both in **CCM** as well as in **CPM**, thus they take on meanings that are both identical and independent of these theories. As for "mass" and "velocity" which are present in **CCM**, assimilation to their corresponding components in **CPM** requires some additional suppositions. The concept "mass" is considered both **CCM**-theoretic[6] as well as **CPM**-theoretic. In this case, it can be proven that there are pairs of models belonging to **CCM** and **CPM** in which the **CPM** model satisfies, in addition to Newton's second law, conservation of momentum. Just as the authors point out, it seems to go against the possibility that, due to being concepts whose determination presupposes the validity of different laws, they can be considered to be different in terms of their denotation. Lastly, establishing the concept of "velocity" of **CCM** with its counterpart in **CPM** it is a bit more complicated. The velocity function is **CCM**-non-theoretical. The correspondence in this case to this concept will be established introducing a specific connection between both theories that connects the function v ("velocity") of **CCM** with the function s ("position") of **CPM** in such a way that the velocity function must have the same vector values as the first derivative of the position function. This way of resolving the correspondence of the concept "velocity" with its counterpart in **CPM** does not do it justice, in the way that its own authors conceive it, nor in a precise historical treatment of the concept of velocity in **CCM** (which is conceptually independent from position) nor to the disregard of a differential velocity function in **CCM**. Yet, this connection keeps the conceptual disparity that could occur in this case from impeding the laws of **CCM** from being formally derivable from those of **CPM**. The analysis of these concepts shows that the conditions 1_{BMS} and 2_{BMS} can be considered to be satisfied by the general characterization of the reduction-relation (see Section 2). The first of these conditions is fulfilled insofar as it can be established that **CCM** and **CPM** are globally correlated in terms of their conceptual frameworks, and more precisely, that there is a correspondence between the domains and functions present in the potential models of **CCM** and those present in the potential models of **CPM**. The second of these conditions is satisfied by the fact that there are no domains and functions present in the potential models of **CPM** that do not have some counterpart in the potential models of **CCM**, which enables defending that the sense of the reduction-relation is such that **CPM** reduces **CCM**.

In addition, the aforementioned shows that the third condition 3_{BMS} that expresses the derivability requirement is also satisfied. The correspondence between the conceptual framework of both theories, which implies adding a particular connection between the concepts "velocity" (**CCM**-non-theoretic) and "position" (**CPM**-non-theoretic), plus the recognition of the need

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[6] This concludes by showing that there is a method for determining the mass that is **CCM**-dependent, meaning, the determination of the mass of a particle presupposes the validity of the law of momentum conservation, fundamental axiom of **CCM** (See Balzer, Moulines and Sneed 1987, pp. 72-73).

to consider the *actio-reactio* principle, enables them to reconstruct the premises needed to logically derive the law of momentum conservation (fundamental axiom of the **CCM** models) from Newton's second law (fundamental axiom of the **CPM** models) and the law of action-reaction (specialization of **CPM**).

As shown in the characterization of the reduction-relation, said relationship is established in terms of other components of the theoretical nucleus, such as the respective classes of constraints (C) and links (L) of each one of these theories. Even though these components are also involved when establishing the reduction-relation in a precise way, the general classical conditions connectability and derivability are mainly expressed through the relationship that can be established in terms of the classes of its potential models (M_p) and its actual models (M). Moreover, given that our interest here is in ontological reduction, which implies considering the relationship between the respective domains of the theories involved, we will forego the analysis of the reduction-relation in terms of the remaining components of the nucleus.[7]

Therefore, from the first version of homogeneous ontological reduction (Definition I), the following definition can be obtained for the specific case of reduction between classical collision mechanics and classical particle mechanics:

Definition IV. Given x and x* two models such that $x = \langle P, T, \mathbb{R}, v, m\rangle$ and $x \in M_p$(**CCM**) and $x^* = \langle P^*, T^*, S^*, \mathbb{N}^*, \mathbb{R}^*, c_1^*, c_2^*, s^*, m^*, f^*\rangle$ and $x^* \in M_p$(**CPM**); and given Π_i and Π_j two projection functions such that $\Pi_i x$ is the i^{th} set of x and $\Pi_j x^*$ is the j^{th} set of x*, then it can be said that ρ is a *homogeneous reduction-relation* **CCM** to **CPM** iff:

1) For each every $x \in M_p$(CCM) and every $x^* \in M_p$(CPM), $x^* \rho x$
2) $P \subseteq P^*$ and $T \subseteq T^*$**.**

Condition 1) requires that there be an effective reduction-relation between the potential models of respective theories **CCM** and **CPM**. The condition Definition I.2 basically demands that there be at least one basic domain in the potential models of **CCM** that is also a subset of a basic domain present in the **CPM** potential models. This condition is fulfilled insofar as all the basic domains present in the **CCM** potential models, namely P and T, can be considered in such a way that $P \subseteq P^*$ **and** $T \subseteq T^*$**.** The same can be affirmed about the auxiliary domain $\mathbb{R} \subseteq \mathbb{R}^*$**,** even though it bears mentioning that the definition of homogenous ontological reduction (Definition I) as presented by Moulines (1984) does not specify the need of identity or correspondence with respect to the auxiliary domains, as it would be here the class of real numbers $\mathbb{R}$.

On the other hand, as for the functions present in the potential models, it is assumed that:

> Since the rest of relations and functions are constructed out of the base sets, any relationship between the latter will indirectly induce some kind of relationship between the former, which will be a part of the reductive link too. The fundamental problem, then, is to find out a possible way of formalizing the relationship between the base sets in general. (Moulines 1984, p. 58)

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[7] We refer the reader to Balzer, Moulines and Sneed (1987, pp. 255-267, 275-284) for a complete analysis of this.

Certainly, the relations or functions are constructed from the basic domains insofar as each one can be typified by the basic domains; in other words, for each function or relationship, there is a typification that determines the procedure for constructing the set of ordered pairs that results from said function or relationship from a given matrix of sets.

> Intuitively speaking, typifications indicate the set level in which a determined set or relationship has been constructed; they indicate, in a way, their ontological level within the set reconstruction of the theory. This distinction is essential in a conceptual analysis of the theories (to know what type of concepts we are talking about) and also for analysis of global relations between theories, particularly the one I have called "ontological reduction". The typifications are the first thing that one must know about a theory. [...] The typification of a set entity (for example, a relation) with respect to other entities is an indication of how said entity is constructed from the other assumed base entities. To do this, one only needs the set operations of the projection, the power set and the Cartesian product. These three enable us to fix the ontological level of any entity in a given theory. (Moulines 1985, p. 48-49)

Firstly, let us consider the mass function in **CCM** which can be stated as a set of ordered pairs:

I. $m \subseteq P \times \mathbb{R}$

This way of expressing it explicitly contains three typified steps (Moulines 1985, p. 50):

1) It is taken the ordered pair $\langle P, \mathbb{R}\rangle$and it is applied the projection function π_1, π_2 on every member of the pair such that: $\pi_1 \langle P, \mathbb{R}\rangle = P$ and $\pi_2 \langle P, \mathbb{R}\rangle = \mathbb{R}$
2) It is applied the Cartesian product $\pi_1 \langle P, \mathbb{R}\rangle \times \pi_2 \langle P, \mathbb{R}\rangle$; in other words, it is constructed $P \times \mathbb{R}$
3) It is applied power set Pot($P \times \mathbb{R}$); in other words, Pot ($\pi_1 \langle P, \mathbb{R}\rangle \times \pi_2 \langle P, \mathbb{R}\rangle$)

Thus, a typification of the function m is obtained:

II. $m \in Pot\,(\pi_1 \langle P, \mathbb{R}\rangle \times \pi_2 \langle P, \mathbb{R}\rangle)$

The formulation II is equivalent to I, but has more conceptual precision due to clearly indicating how the relationship is constructed from the domains.

Starting with what was stated about the steps of typification, it can be easily noted that the typification for the mass function, now, in **CPM** would take the form:

III. $m^* \in Pot\,(\pi_1 \langle P^*, \mathbb{R}^*\rangle \times \pi_2 \langle P^*, \mathbb{R}^*\rangle)$

Also, according to the correspondence established in Definition V-2 as for the basic domains P and P^* (and taking the correspondence between the auxiliary domains as non-problematic, in this case, the class of the real numbers), the conceptual connection required for the ontological reduction between functions m and m^* could be expressed through a typification of the mass function in **CCM** based on the basic domains of **CPM**:

IV. $m \in Pot\,(\pi_1 \langle P^*, \mathbb{R}^*\rangle \times \pi_2 \langle P^*, \mathbb{R}^*\rangle)$

By showing the correspondence on the level of the typifications of the functions present in the two theories that presumably have a reduction-relation, one can more clearly demonstrate the conceptual connection. In this specific case, the possibility of establishing correspondence of certain concepts from their typification is particularly interesting insofar as, even though the reconstructions that have been made until now on classical collision mechanics have conceptualized the value of quantity of bodies through a "mass" function, the truth is that such term is not used in the prenewtonian systematizations of the collisions. It is well known that Descartes, for example, does not mention "mass" but neither does Huygens. Yet, Huygens supposes some type of "quantification" of the bodies that, independent of what it is called (be it "weight", "magnitude" or any other name), it can be conceptualized by a function whose typification shows ontological correspondence with the mass function of **CPM**. More precisely, it shows ontological correspondence with the domains and the way these domains construct the mass function of **CPM**.

On the other hand, as for the velocity function in **CCM**, an appropriate typification would take the following form:

V. $v \in Pot\,(\pi_1\,\langle\langle P,\ T\rangle \times\ \langle \mathbb{R}, \mathbb{R}, \mathbb{R}\rangle\rangle \times \pi_2\,\langle\langle P,\ T\rangle \times \langle \mathbb{R}, \mathbb{R}, \mathbb{R}\rangle\rangle)$

Also, again, from the established correspondence between the basic domains $P \subseteq P^*$ and $T \subseteq T^*$ (and assuming the identity on the level of the auxiliary domains), the conceptual connection required for the ontological reduction could be expressed by a typification of the velocity function in **CCM** based on the domains P^*, T^* and $\mathbb{R}^*$of **CPM**:

VI. $v \in Pot\,(\pi_1\,\langle\langle P^*,\ T^*\rangle \times\ \langle \mathbb{R}^*, \mathbb{R}^*, \mathbb{R}^*\rangle\rangle \times \pi_2\,\langle\langle P^*,\ T^*\rangle \times\ \langle \mathbb{R}^*, \mathbb{R}^*, \mathbb{R}^*\rangle\rangle)$

It is important to remember that, in the analysis of reduction of **CCM** to **CPM** presented in *An Architectonic*, the correspondence in the case of this concept was established by proposing a particular connection between both theories that connected "velocity" of **CCM** with "position" of **CPM**. Which, even though this approach is useful for making derivability possible, it was not without of controversy. Establishing a conceptual connection for the velocity function, in contrast with what happened with the mass concept where the typification proposed in IV demonstrated ontological correspondence with the mass function of **CPM**, makes the velocity function correspond to a function (the one typified by VI) that is not present among the concepts of **CPM** but that could be constructed from the basic domains of **CPM**, without using the concept of position.

Below, will be analyzed the implications of formalizing the ontological correspondence through typifications such as those formulated in IV (for the concept of "mass") and in VI (for the concept of "speed") when considering the ontological reduction-relation in terms of echeloned partial substructures.

4. Ontological reduction between CCM and CPM

According to what was previously stated, the reduction between **CCM** and **CPM** is a relationship established on the level of its potential models and, in formal terms, every potential model $x \in M_p$(CCM) and $x^* \in M_p$(CPM) is considered a structure, being

$$x = \langle P,\ T,\ \mathbb{R},\ v,\ m\rangle$$

and

$$x^* = \langle P^*, T^*, S^*, \mathbb{N}^*, \mathbb{R}^*, c_1{}^*, c_2^*, s^*, m^*, f^* \rangle .$$

Additionally, as was said, given that every set A can be trivially converted into a structure $\langle A, \emptyset \rangle$, where $\emptyset$ is the empty relationship, the domains P, T and $\mathbb{R}$, insofar as components of x, can be trivially considered partial substructures of the potential models of **CCM**. Also, the same will be affirmed about the domains P^*, T^*, S^*, $\mathbb{N}^*$ and $\mathbb{R}^*$ with respect to the potential models of **CPM**.

At the same time, each one of the basic domains P and T can be considered an echelon set over P^* and T^* respectively given that if $P \subseteq P^*$ **and** $T \subseteq T^*$**,** P can be considered as one of the ordered pairs resulting from the operation Θ over P^* and the same can be said of T **over** T^*. Moreover, assuming the identity of the auxiliary domains, it can also be said that $\mathbb{R}$ it is an echelon set over $\mathbb{R}^*$.

On the other hand, in the case of the functions the following could be argued. In the first place, every one of them constitutes a set of ordered pairs that, insofar as components of the models can be trivially considered to be partial substructures of theses. In the second place, if a typification is conceded like the one formulated in IV, then for the case of the mass function one would have to$m \in \Theta(P^*, \mathbb{R}^*)$, meaning, m is an echelon set over $(P^*, \mathbb{R}^*)$. Also, in the case of the velocity function if the typification formulated in VI is accepted, then we could affirm that $v \in \Theta(P^*, T^*, \langle \mathbb{R}^*, \mathbb{R}^*, \mathbb{R}^* \rangle)$, that is, v is echelon set over $(P^*, T^*, \langle \mathbb{R}^*, \mathbb{R}^*, \mathbb{R}^* \rangle)$.

In such a way that every component of $x \in M_p$ (CCM), be it a basic domain, an auxiliary set or a function, can be considered a partial substructure of x. Moreover, each one of them can be considered an echelon set over at least one of the components of$x^* \in M_p^*$(CPM), those that also are partial substructures ofx^*. This would be enough to formally conclude that $x\eta x^*$, meaning, x is a echeloned partial substructure of x^*, which would fulfill the condition demanded in the definition of homogenous ontological reduction in terms of echeloned partial substructures (Section 2.2.2, Definition IV-1).

From what has been affirmed up to this point, at least two strongly intertwined conclusions can be made. The first has to do with the adequacy of Definition IV in characterizing the intuitive notion of the homogeneous ontological reduction. A possible inadequacy had been mentioned about this definition since it proposes a condition that is too demanding, which it would be to require an ontological connection with all of the concepts of the theory. The analysis of the specific case of the reduction of **CCM** to **CPM**, if we concede what was stated about the ontological connection between concepts that are functions, would show a case of reduction that satisfies this strict condition; given that, every concept of **CCM** (particle, time, mass, velocity) could be formally considered a partial substructure of the potential models of the theory and, simultaneously, echelon set over domains of **CPM**. Nonetheless, the way that concepts like mass or velocity are formally established as echelon set over domains of **CPM (**$m \in \Theta(P^*, \mathbb{R}^*)$ and $v \in \Theta(P^*, T^*, \langle \mathbb{R}^*, \mathbb{R}^*, \mathbb{R}^* \rangle))$ demonstrates their correspondence with more than one domain of **CPM** thus constituting, in Moulines' terms, a heterogenous connection. Moreover, the same could be argued for the case of the basic domain T of **CCM** if the language of **CPM** were to be "translated" from the equation $T = \{\check{c}_1\,(0), \check{c}_1(1)\}$ which would mean considering it to be an echelon set over the basic domain T^* and the auxiliary domain $\mathbb{R}^*$ of **CPM**. This brings us to a second conclusion: the definition proposed in Definition IV not capture only the intuition of strong homogeneous ontological reduction but seems also to capture that of *mixed* strong ontological reduction, where some of the concepts of **CCM** can be identified with domains of

CPM while others correspond, through some function, with more than one of the domains of **CPM**. Of course, accepting that this definition is satisfied in the specific case of the reduction of **CCM** to **CPM** would bring us to reconsider it as a case of mixed ontological reduction and not as completely homogeneous.

5. Conclusions

The purpose of this work was to take a specific case of reduction such as that of classical collision mechanics to classical particle mechanics to evaluate the adequacy of a recent proposal for reformulation of the condition of ontological reduction. What has been considered up until now enables making the following conclusions:

1) The characterization of ontological reduction in terms of echeloned partial substructures enables evaluating the ontological connection between theories, not only on the level of its basic domains, but also on the level of all the concepts that make up its conceptual framework.
2) The first attempt to reform the condition of homogeneous ontological reduction (Definition IV) may not completely be adequate insofar as it would also capture cases of mixed ontological reduction.
3) It is appropriate to wonder, if one accepts the possibility that all the concepts could be evaluated on the level of ontological correspondence, if a specific case can actually satisfy the condition of homogeneous ontological reduction in the strong sense, considering that the correspondence on the level of functions or relationships, if present, seems to always involve a heterogenous ontological connection.

Acknowledgments

Research for this work has been supported by research projects PICT-2018-03454 (National Agency for the Promotion of Research, Technological Development and Innovation-Argentina) and UNTREF-32/19 80120190100115TF (National University of Tres de Febrero). Cordial thanks to Cláudio Abreu for inviting me to contribute to this volume. I would also like to thank C. Ulises Moulines for the careful reading, comments and corrections on an earlier draft of this text.

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The Problem of Explanation in Chomskyan Generative Linguistics. A programmatic Proposal from the Metatheoretical Frame of the Structuralist View of Theories

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1. Introduction

The aim of this article is to address the problem of explanation within the framework of Generative Grammar Theory, particularly in the Chomskyan approach (CHT). As will be shown in the course of the second section, talking about CHT is complex. On the one hand, the designated linguistic theory has gone through important theoretical changes, on the other hand, its explanatory field encompasses different aspects of a vast object: "language". We will try to limit its theoretical complexity and define it according to our reconstructive interests, for which we will focus on Universal Grammar (UG).

In a third section, we will examine in a diachronic way the central purposes of CHT, now circumscribed to its core components (UG), in relation to guiding questions that allow us to locate what the explanandum of the theory is at different moments in time, and what is the proposal to solve those questions: what is the explanatory proposal according to them.

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Abreu, C. (ed.) *Philosophy of Science in the 21st Century. Contributions of Metatheoretical Structuralism*.
Florianópolis: NEL/UFSC, 2023, pp. 61–92.

This development is part of the first proposals of Syntactic Structures (1957) until the so-called 'Minimalist Program' (MP), a theory in force since about 1995. This section will conclude with a list of current problems, which have been added to the traditional agenda of CHT problems, and which will serve as a guide to some current topics on what we will call CHT's "explanatory level problem".

Section 4 focuses on the above-mentioned problem, which we could characterize as follows. Since the object 'language' in CHT is the internalized language (I-Language), sometimes also called "mental language", the development of CHT went hand in hand with the constitution of the vast field of 'cognitive sciences', of which it is now part of. In turn, psycholinguistics was spread into areas of neurobiological studies and/or brain neurology. In addition, CHT's part of this new agenda involved aspects related to the phylogenetic development of language and its research in relation (or in opposition) to animal communication systems. The latter launched a new branch of evolutionary studies towards the evolution of human language. The above involved, in turn, a growth in what is now known as 'biolinguistics'. This growth of disciplinary interrelationships moved the axis of the explanatory question to the question of whether linguistic explanations (always in the field of generativism) could occur autonomously, or whether the explananda of linguistics should have a correlation in an explanatory field of psycholinguistic and computational theories and/or in the relevant branches or subareas of biology.

Contemporary linguists took from Marr (1982) the idea of 'explanatory levels', which he had used in the mapping of visual perception. Thus it was proposed to speak of a computational level (a), which explained the typical linguistic computations of language output generation, and what is the logic of the strategy by which it can be carried out; of a level of representation and algorithm (b) which gives account of how this computational theory can be carried out, and especially, what is the representation for the input and output, and what is the algorithm for the transformation; and (c) level of Hardware implementation: How can the representation and algorithm be realized physically?

Other authors present a more complex organization of explanation in levels (Boeckx and Theofanopoulou 2014). In section (4.2) we draw this proposal.

Then (section 5), we discuss different ways in which the relation of levels of explanation have been carried out, and, finally, we present some problems that have been detected from a perspective in the philosophy of science.

In the following section (6) we concentrate on a very restricted and central area of the linguistic study of language: the morphosyntactic one. Considering it as theory (a sub theory of generative linguistic theory CHT), we place it in the frame of the current MP. We give an account of the principal components of language according to this theory, the relations among them, the conditions of restrictions, and so on.

Among some recent studies which try to relate syntax in MP and the cognitive and neurobiological basis, we selected (Boeckx 2013) and the proposal of explaining how syntax is possible on the foundations of the neurobiological activity of our brain.

The next section (7) proposes as a programmatic aim a reconstruction of the Minimalist CHT (MCHT) using metatheoretical tools of the Structuralist view of science. The idea is to think of CHT as a theoretical net, having the Syntax Theory (SMCHT) as an element. We draw a general picture of the central components, relations, and main laws which characterize this theory. We carried out this task considering the problem of explanation, especially the philosophical proposal about explanation presented in structuralism.

After this general and schematic representation of SMCHT, we will return to the problem of levels of explanation. We are not yet in the position to propose a solution to the problem, but only to point out a direction in which it might be possible to trace a solution; giving a clear elucidation about a central implicated notion: inter theoretical relations; and presenting a metatheoretical option to work in this direction in future studies. Lastly, (section 8) we introduce some final considerations.

2. Chomsky's Theory (CHT). Some introductory remarks

The first difficulty is to characterize CHT, since it constitutes a set of theories (or sub-theories) that can be presented in both a diachronic reconstruction and a synchronic one. From the point of view of a diachronic reconstruction, CHT can be interpreted as a subsequent hypothetical and temporary construction of models, and as their partial progressive replacement, with certain continuities and different theoretical changes. So, it is quite common to speak of the programmatic nature of CHT.

The use of the expression 'Chomskyan program' has been quite common, but also, quite lax from the point of view of the philosophy of science, since, in general, it has yielded little fruit to reconstructive work. We will instead speak of CHT as a sequence of model-theoretical changes into the frame of a macro theory.

On the other hand, from a synchronic perspective, it is important to remark that the linguistic work of Chomsky constitutes a complex theoretical work that tries to account for various different problems. When we talk about CHT, we limit our study to explaining sentence generation as a universal human language faculty, that is, what traditionally conformed to the explanatory elements belonging to syntax, phonology, morphology, and semantics..

As was already said, we believe that the MP is a theoretical sphere that has produced some radical changes in the development of CHT. Precisely, a central problem is the displacement of semantic and phonological components to interface and the central character of morpho-syntactic mechanism in the generation of language. So, in MP the phonological and semantic components are not proper constituents of CHT, but forms of interphase, playing a central role in the externalization of language and processes of linguistic understanding.

So, explaining the mechanism of sentence generation in the MP, implies centrally, giving an account of how the computational morpho-syntactic mechanism of language operates.

3. The problem of the explanation in the Generative Theory (GT)

3.1. Linguistic Explanation in the first steps of GT

The initial linguistic model proposed by Chomsky in *Syntactic Structures* and *Logical Structure of the Linguistic Theory*(a work written in 1955 but published in 1975) briefly consists of a grammatical model, which focuses on the syntactic structure of language. The underlying proposal is that this structure is more or less uniform in all languages, despite some linguistic variety that could be observed in linguistic performance.

In *Aspects of the Theory of Syntax* (1965)—a work that complements other more or less contemporary ones: *Cartesian Linguistics* (1966) and *The Understanding of Language* (1968)—Chomsky assumes a strong philosophical thesis that will be held throughout the theoretical development of his proposals. These ones gradually acquired a greater empirical burden, becoming a substantive naturalistic order. In addition, Chomsky proposes a grammatical model, based on a system of rules which specify the mechanism of sentence production. The theory that emerged was then called 'Standard Theory'.

This theoretical model postulates the phonological, morpho-syntactic, and semantic nature of the universal components of language. The main hypothesis is about the existence of UG which plays an important role in the process of language acquisition and on the production mechanism of well-formed sentences. (Chomsky 1965).

Chomsky (1965) states that knowledge of any language implies the ability to understand an infinite number of sentences. Therefore, it is the scientist's task to give account of the innate linguistic components which provide the basis for language acquisition.

The scientific problems are:

(1) How is the grammar that enables linguistic competence?

If knowing a language means being able to generate and understand phrases and sentences from a language, then:

(a) Which are the elements that constitute this device, needed to produce and understand grammatically correct sequences of words?

(b) Which is the mechanism of such a device?

To these theoretical problems, Chomsky (1965) added a metatheoretical one. He argued that the general theory of language should be capable of describing any knowledge internalized by a native speaker. This would be descriptively adequate insofar as it expressed without difficulty the primary language data. In this way, grammar would be justified by external reasons, that is, by correspondence with linguistic facts, without over generating or under generating constructions that the linguistic data did not present.

But a theory that satisfies the requirement of describing well-formed sentences in any language may not satisfy the condition of explaining how the postulated grammar and its mechanisms of operation became in the speaker's mind, and therefore how it might be acquired. The condition of "explanatory adequacy" was postulated, and it was held that a theory is adequately explanatory if it succeeds in giving reasons for how that knowledge might be acquired. Therefore, while descriptive adequacy would be justified by empirical contrast with the linguistic constructs developed by the speakers, explanatory adequacy is justified insofar as the descriptively appropriate system is preferable to others because it is compatible with the primary linguistic data to which the apprentice of a language is exposed. In this sense, it will be justified on the grounds of internal reasons, that is, because it would account for the specific innate skills that make it possible to acquire language.

Under the schematically presented criterion, many of the Chomskyan theory hypotheses referred to above were proven to be false in the following years, pointing to the explanatory inadequacy of Standard Theory. The main difficulty was that, if the requirement of explanatory adequacy involves evaluating alternative grammars which may be acquired by the child from the data to which he was exposed, the ST model was inadequate given the extension of the expressive apparatus, that is, the numerous transformational rules that the child had to learn, which led to what was called 'the logical problem of language acquisition'. Thus, those hypotheses had to be replaced by a progressive set of changes (Extended Standard Theory and Revised Extended Standard Theory) until a radical change took place, with a new model commonly known as 'Government and Binding' (GB), also known as 'Principle and Parameter' Theory.

3.2. GB theory and the modular model

GB began to develop in the 1980s and it is precisely at this time that Chomskyan's theoretical perspective came to approach the aforementioned naturalism, and that the UG became a vast object of the cognitive sciences. Since then, the naturalization of CHT has led to significant changes in the conception of UG. From the moment that the theory's adequacy requirements were formulated, it was assumed that a theory that aspired to be explanatory should indicate what the linguistic universals are, and express them in the form of concepts, sets, principles, and laws. It should answer the question of what the child's initial assumptions are, that is, what is the specificity of the innate scheme? In the GB model, such universals are anchored in what is called 'mind-brain', and it is postulated that the acquisition of a language occurs according to the underlying, mental-based structure.

To the problems (a) and (b) referred to in the previous point, we now add a central question:

(2) What is the content of the subject's mind-brain which enables the generation of sentences and their understanding?

GB's answer will be: universal linguistics. However, if the intention is to specify which characteristics these linguistic universals possess in GB, the answer will be: the general principles of language, those that are restricted or parameterized to different explanatory areas, 'sub-categories' or 'modules', which operate as different explanatory subsystems of the various aspects of language functioning. In the case of syntax, one of the principles governing the X-Bar module states that 'every syntagma is the projection of a core'.

Even though the 'innate' trait of language was already present in several earlier works (since Chomsky 1965) it is only in Chomsky (1980) that said innatism is characterized as biological heritage. Thus, the content specified in the UG is innate and genetic, properties without which a system could not be called 'language', and the constitution of the UG is "an element of the genotype that traces the course of experience in a specific grammar" (Chomsky 1980, p. 75). This characterization of 'genetic', begins to become present in this period and remains strong until the late 1990s. This led to what was the main hypothesis of the period: the existence of a "unified system of principles that has a fairly rich structure and some open parameters, which are set by experience" (Chomsky 1980, p. 150). In response to the problem identified in (2), the hypotheses formulated by the previous model were naturalized towards what was called a 'Genetic Program' (Longa 2008).

In addition to the above, GB adds the notion and theoretical role of the 'parameters' to the grammatical explanatory component. On the one hand, we have a set of universal 'principles' which are typical of each module of GB theory. These principles, which can be considered as the substantive assumptions of the model, constitute the manual of innate procedures. However, they are activated and shaped according to the experiences that the child was exposed to. Something pre-existing is activated, while it is set to the appropriate position—understandable compatible—with the primary linguistic data: language is parameterized. While the principles determine which maximums characterize a relationship type, the parameters determine which execution options support these maximums. GB assumes that there are some parameters that are set by experience and, therefore, produce variations on the universal principles of language.

3.3. Main hypotheses of the "Minimalist Program". Problems and explanatory proposals

3.3.1. The Faculty of Language and the recursive computational mechanism

In the 1990s Chomsky continued to modify his language program, which took on a more stable form in Chomsky (1995). There are important variations concerning grammatical theory in general and the UG in particular. But, in parallel with theoretical changes in the explanation of language generation, some new problems are introduced to the traditional agenda in the years that follow, including the problem of explaining the phylogenetic emergence of human language. In a sense, the changes that can be observed between TE and GB constituted a deep break between the two, marked by the total abandonment of the explanatory hypothesis which asserted that language consists of rules that form grammatical constructs (or types of clauses). Thus, for example, it is stated that the concept: of 'passive sentence' is nothing more than a 'taxonomic artifice'. However, the changes between GB and Minimalism are of another type, since they do not indicate a discontinuity that would cause a rupture such as the one indicated. On the one hand, the current model shares certain assumptions with the model that precedes it, namely: (i) that there is a component of language in the mind/brain and that it interacts with other systems: a cognitive system (storage) and systems of action (those that access information and use it in different ways); (ii) that the cognitive system interacts with these external, articulatory-perceptive (AP) and conceptual-intentional (CI) systems, through an interface of language representation levels. According to GB, the variation and typology of languages were due to the fixation of parametric values from a universal ensemble, the UG; leading, thus, to a natural resolution of the tension between descriptive and explanatory adequacy.

But, on the other hand, there are important differences: (i) the substantive components of the UG are now a topic of discussion, so for example, certain semantic components that were previously expressed by sub-theories (such as Thematic Theory or The Theory of Ligation) are now functions to be interpreted in the interface of the cognitive system with external systems; (ii) the UG—whose name is practically replaced by that of 'faculty of language' (FL)—is understood as a computational system whose central operation consists of merging minor particles into larger particles; (iii) language, previously understood as that phenomenon from which a succession of states can be identified (the 'initial state' (UG) and the 'final state' (I-language)), is now understood as mere computations. The latter seems to be the most substantial difference, since it realizes the language in a more idealized, more abstract way, with less—or nothing—of grammatical content.

The hypothesis is that "there is a single CHT computational component for human language and only a limited lexical variety. The lexical variation is essentially morphological [...]" (Chomsky 1995, p. 18). "Rather, a state of the language faculty is some accidental product of varied experience, of no particular interest in itself, no more so than other collections of phenomena in the natural world" (Chomsky 1995, p. 17), so "the primary (task) is to demonstrate that the apparent richness and diversity of linguistic phenomena is illusory and epiphenomenal" (Chomsky 1995, p. 19).

However, it should be noted that the guiding questions asked—(1) and (2)—in the previous periods found some adjustment, such as the questions that follow:

(3) What are the general conditions that we expect the human FL to satisfy?

These conditions assume, on the one hand, that FL takes place in a set of cognitive systems of the mind/brain and, on the other hand, that some of the systems would be conceptual in nature and exceed their specificity, such as simplicity, non-redundancy, economy among others.

(4) To what extent is the FL determined by these conditions without any other underlying structure?

This question would lead to the affirmation—or rule out—that the FL is not supported by any other system and, in this sense, that it is a 'perfect system' as long as all its operability—outputs—can 'perfectly' conform to the requirements of external systems—AP and CI.

With all the inaccuracy (4) possesses, it suggests in a very powerful way that the study of FL is a problem placed in biological sciences, and could therefore be formulated as follows: "How can a system such as human language arise in the mind/brain, or for that matter, in the organic world, in which one seems not to find anything like the basic properties of human language?" (Chomsky 1995, p. 10). As will be seen, research into UG mechanisms was normalized, and more and more attention came to be paid to the empirical study of objects in neuropsychology.

In addition, a direct consequence of the loss of specificity of the principles that had once formed the UG is that the genetic burden or endowment required for language growth in the individual was relativized or reduced. Note that, having restricted the scope of grammar to the computational operations of morpho-syntactic traits, the phonological and semantic-intentional aspects of it were left out of the UG. Now, following Chomsky's hypothesis (Chomsky 2005 p. 9), language development is caused by three factors: (i) genetic endowment, (ii) experience, and (iii) non-specific principles of language faculty.

We can agree—albeit partially—with Longa (2008) that there was a change in the explanation, going from the first factor—genetic endowment—towards the third factor, principles being non-depending on data processing language, structural architecture and computational efficiency (see Longa 2008, p. 373-374).

The problem now is to account for the following:

(5) What are the specific features of human genetic endowment regarding language? Which of them has an invariant trait with respect to interaction with the environment? What can be considered a set of restrictions on interaction with the environment? (So, the end result would be something like a 'synthesis'—in the form of a naturalized Kant's dictum.)

It should be noted, then, that in the MP, FL has been deflated so much that it has almost no specific content, except for Merge (the basis of the computational mechanism to which we will refer later). FL, although innate, is no longer genetically given and, by contrast, what was supported in GB, in which "innate" was synonymous with genetics, to which it adds that, much of what was innate, is now assumed to be part of individual development. In addition to the above, the extension of problems in the agenda implies the emergence of new questions:

(6) How did language emerge as a result of human evolution? What are we going to consider "human language"? From what moment in time are we to consider human language? What evolutionary mechanisms explain the emergence of human language?

3.3.2. A Phylogenetic explanation of FL. The new agenda of problems

As regards the topic referred to in the last part of (3.3.1), we can add that, although it mostly focused on the ontogeny of language in Chomsky's early works, the problem of philoxenia of FL was recently introduced in Generativism.

Precisely in the frame of Minimalism, the question about the evolutionary emergence of FL became a main problem. In the frame of this perspective, the central question is: How could the FL appear in the evolution of human beings and *Homo sapiens* in particular?

Even when Chomsky's position towards FL seems to have partly modified in the course of his works, something remains rather unchanged: his objection to the unilateral application of natural selection theory regarding the evolution of FL, which of course places him against the adaptationist program (Fitch 2000; Hauser, Chomsky and Fitch 2002; 2014; Fitch, Hauser and Chomsky 2005; Chomsky 2006; Berwick and Chomsky 2016).

Briefly, Chomsky and his colleagues claim that the theory of natural selection by itself is insufficient or inadequate to account for what they recognize as FL. They might admit that the theory of natural selection does have an explanatory place in the evolution of the trait—or of some part of the trait—, but with the prerequisite to add another theory/ies to the explanation.

In one of his first writings on this issue, Chomsky and his collaborators Hauser, Chomsky and Fitch (2002) talk about the partial separation between FL and communication, introducing a central distinction: faculty of language in a broad sense (FLB), and faculty of language in a narrow sense (FLN), leading us to think of not one, but of two closely related, though different, traits. Let us now briefly explicate both.

FLB is conceived as a system connected to communication abilities that humans share with a broad range of communicating animal species. FLN, on the other hand, is a computational mechanism that allows us to construct unlimited hierarchical structures in syntax from a pool of words ('recursion') together with other internal representations.

For Hauser, Chomsky and Fitch (2002), FLN is unique and strictly restricted to *Homo sapiens*. Since we now have two traits, we have also two lines of inquiry on evolution: one that has to do with external speech and the equipment for sound-communication (see Fitch 2000), and one that has to do with an internal system.

Regarding FLN, Chomsky and colleagues claim two things: (i) that FLN can only be properly found in humans (as syntactic recursion has not been found in non-human forms of communication); and (ii) that FLN is inseparable of FL (no human language lacks recursion).

Moreover, note that for Minimalism, the idea of what counts as properly human in language has been changed: it is now lessened to a recently evolved computational system in our brains and the linguistic fact is now explained by merge and recursion.

When focusing on FLN as an explanandum, not much is needed to explain. Just "some slight rewiring of the brain" suddenly produced is good enough for FLN to appear (see Berwick and Chomsky 2016, p. 67, 79, 164; Chomsky 2000, p. 4; Hauser *et al.* 2014, p. 6).

3.4. Problems and solutions. Levels and areas of explanation in the development of TG. A contemporary reading

Recently, Poeppel affirmed:

> It is also fair to say that the traditional four (and now five) leading questions that have always formed the basis for the generative research program as formulated by Chomsky have had a profound effect on research in cognitive neuroscience, although most often implicitly:
>
> (1) What is it that one knows as the native speaker/listener of a given language? That is, of course, the domain of linguistics.
>
> (2) How is this knowledge acquired? Language acquisition and psycholinguistics are at the center of this area of inquiry, as is developmental psychology more broadly.
>
> (3) How is this knowledge put to use, or processed online? Here, psycholinguistics and computational linguistics are the dominant fields.
>
> (4) How is this knowledge implemented in the brain? This issue has been addressed by the research called, variably, neurolinguistics, cognitive neuroscience of language, or neurobiology of language.
>
> (5) What is the evolutionary history of the computational language system? Paleoanthropology, genetics, and comparative ethology have played important roles in dressing this more speculative question. (Poeppel 2017, p. 155–156)

These five key questions show, on the one hand, the central problems that need to be accounted for today, and on the other hand, the field or discipline in which this task would be achieved. We can also point out that there exists a relationship between the successive order of problems and the change of the theoretical-disciplinary perspectives in which they unfold in the theoretical development of the Chomskyan proposals. While (1) is resolved in the field of discipline limited to linguistics (in the period which goes from the first Chomskyan proposals up to the 1980s); (2) is inserted in the field of psycholinguistics and, although it was already established as a problem in the 1970s, it will be a central topic of empirical research from the 1980s onwards. It is important to note here that, at this stage, linguistics was already beginning to be considered as one of the major fields of the cognitive sciences. (3) Although Poeppel's formulation is restricted to the MP model, we could look for other formulations about the use and processing of language, which would bring the problem to the very origins of the Chomskyan proposals. The original proposals already show Chomsky's concern with revealing syntactic mechanisms as computational models, although he does not yet think of its psychological or mental basis. In addition, Chomsky intended to give not only an account of sentence generation, but also of language acquisition. The link with evolutionary psychology does appear, giving rise to psycho-linguistics, meanwhile the idea of a computational mechanism was replaced by a complex system of rules.

Finally, since the 1980s concern for the establishment of linguistic structuring mechanisms took the form of syntax modules, which relate to the postulation of principles present in the mind-brain of the individual, and are determined by the genetic endowment that we possess as *Homo sapiens*. With regard to (4), the Chomskyan proposal here has always been open to other theoretical fields, since the proposal of the 1980s. As Poeppel argues, it is part of the studies of neurolinguistics and cognitive neurosciences, fields in which in recent decades various theories have been proposed in this regard.

The theoretical dependence on brain physiology and neural mechanisms, which are needed to account for the problem, is key here, as these theories have grown and varied substantially in recent decades. In addition to this, the empirical methods associated with research have also made it possible to account for aspects which were previously inaccessible to research. Finally, (5) adds a problem to the traditional agenda: the comment on the phylogenetic origin of the FL. Here, the responses of key evolutionary biology, paleontology, archaeology and particularly research into evolutionary genetics reveal central aspects of language evolution.

4. Explanation and explanatory levels under MP

4.1. Marr's Levels

Marr (1982) distinguishes three levels of description in the study of the vision system: a computational level, a representational/algorithmic one, and an implementational one. The computational level characterizes both the goal of the computation (say, the spatial localization of a visual target to apprehend a visual scene) as well as the logic of the strategy by which the computation can be carried out. The level of representation/algorithm specifies how the computational theory can be carried out, and which representations can form the basis for executing the algorithm that transforms input and output (e.g., Lego blocks? Algebraic symbols? C++ data structures?). The implementational level of analysis examines how representations and algorithms can be realized physically, for example, in neural tissue.

"The goal, a high bar no doubt, is to characterize a complex system such as vision or language by being sensitive to the demands of each level of description" (Poeppel 2017, p. 156). If we adopt this distinction and apply it to the study of language, we can say that the computational level corresponds to linguistic theory, while the algorithmic/representational level corresponds to psycholinguistics and computational linguistics, and finally, the implementational level corresponds to the neurobiology of language.

From the perspective of modern linguistics, a theory of language would need to capture these three levels of explanation, as it was remarked by Embick and Poeppel (2015), Poeppel (2012) and Poeppel and Embick (2005). We could also say that Chomsky's perspective itself had acknowledged these levels of giving account of linguistic phenomena even prior to the formulation of MP.

4.2. Levels of explanation from Boeckx's perspective

As Boeckx said the histories of modern linguistics and modern cognitive science are intimately intertwined, ever since the Chomskyan attack on behaviorism in the 1950s. But, in spite of the goals of cognitive science and linguistics to develop biologically plausible accounts of human cognition and biological foundations of the language faculty ("biolinguistics") respectively, the enterprise seems far from being realized. Furthermore, in order to relate the genotype to the phenotype, several intermediate steps will have to be taken, in the form of linking hypotheses.

Boeckx sketches a research program that intends to link some levels of explanation (following Marr's perspective). From his view, it is necessary to consider these levels in the connections among Linguistics, Psychology and Neurosciences: the levels of the genome, connectome, dynome, cognome, and phenome. Clearly, from his point of view it is also necessary "to view the linguistic en-

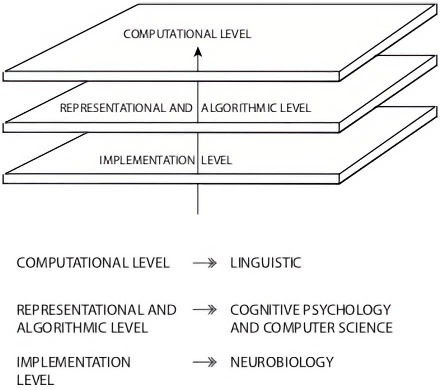

Figure 1: The figure represents the three levels of Marr with the corresponding interpretations of Linguistics, Cognitive Psychology and Computational Theory and Neurology.

terprise as firmly rooted in our knowledge of the brain and its operations (specifically oscillations)" (Boeckx and Theofanopoulou 2014, p. 403).

The connections among the already named levels are shortly described by Boeckx and Theofanopoulou as follows:

> […] the connectome will have to be related to the "dynome" (Kopell, Gritton, Whittington, and Kramer 2014) linking brain connectivity with brain dynamics, specifically brain oscillations. In turn, the dynome will have to be connected to the "cognomen" (Poeppel 2012), understood as the comprehensive list of elementary mental representations and operations. Finally, the cognome will have to be related to the phenotypic level (phenome). This last step shows how elementary mental representations and operations are able to provide the basis for the richness and diversity that has captured the attention of so many students of language and cognition until now. (Boeckx and Theofanopoulou 2014, p. 405–6)

Considering these levels and the relations among them, Boeckx and Theofanopoulou remark that the work in linguistics has mainly taken place at the level of the phenome, or at the border between the phenome and the cognome. But they are also of the opinion that the constituents of the cognome will almost turn out generic so that they will not be specific to a particular cognitive domain.

> We think that this has the (so far) under-appreciated consequence of bringing into contact with two linguistic traditions, one ('Chomskyan') seeking to reduce linguistic complexity to a set of elementary primitives, and the other ('Cognitive') seeking to account for linguistic processes in terms of general 'cognitive' mechanisms. The challenge ahead is to marry these two traditions, showing that elementary primitives used by Chomskyan linguists to explain various linguistic phenomena can be understood in terms of generic processes that in turn can be translated into

> what we know about how the brain works (dynome, connectome, and ultimately genome). (Boeckx and Theofanopoulou 2014, p. 407)

Boeckx seems to be optimistic about the possible results of the Program. Because, in spite of the difficulties that linking linguistics and biology imply, many studies have been carried out in this direction. They include research in different fields (phonology, morphology, semantic, and syntax).

5. The relations of levels in terms of the classic linguistic approach

5.1. The problem of levels relation: How to relate linguistics, cognitive science, and neurobiology

We have already summarized Marr's proposal of levels of explanation, as well as how his idea was transferred to linguistic studies, and also the more complex view of Boeckx. Among other linguists working in the frame of generativism, it is also frequent to speak about two global levels of description or explanation (they are not, of course, synonyms) corresponding to the computational-representational (CR) theories and the neurobiology (NB) theories respectively. The first denomination includes the set of cognitive science—including linguistic theories—and NB refers to the study of the structure and function of the brain; how brain structures and different forms of brain activity underpin perception and cognition.

In the following treatment of the problem, we will maintain our assumption about the three levels in consideration: (a) the computational level: linguistic theory; (b) the algorithmic/representational level: psycholinguistics and computational linguistics and (c) the implementational level: the neurobiology of language. But regarding the problem of how they are related two levels will be generally distinguished: CR and NB, because the central literature in the field of linguistics is treating the controversies while at the same time maintaining a distinction between these two levels.

The central questions will be: What are plausible linking hypotheses between the two domains? What form can they (or must they) take? Can cognitive neuroscience of speech and language processing move beyond a purely correlational state of affairs? (see Embick and Poeppel 2015, p. 364).

There are different ways to think about the possible relations of these levels. Poeppel and Embick (2015) are at first against reduction:

> We argue instead that computational-representational (CR) theories that are the foundation of language research invite not reduction to the biological infrastructure that is described by the neurosciences but rather conceptual change and ultimately unification—subsequent to the identification of linking hypotheses between these domains of study. (Embick and Poeppel 2015, p. 357)

There are also some other parts of the text in which they make explicit assertions against reduction:

> Our goal is to highlight the idea that some crucial explanatory force in understanding language will come from understanding neurobiology. Thus, we do not see any point to attempting to achieve reduction of CR to NB, or vice versa (whatever that would mean). Rather, our goal is to outline a framework for unified inquiry into language, in which CR and NB are investigated together and on equal epistemic footing. (Embick and Poeppel 2015, p. 359)

Regardless of their defense of no reduction between these two levels, it is clear that what they understand as language, and consequently as "theory of language", is just reduced to a level of cognitive representation and computations: "We assume that language comprises a set of representations (e.g., 'morpheme') and computations (e.g. 'concatenation') whose formal properties are the object of research in (psycho)linguistic theory as currently practiced" (Embick and Poeppel 2015, p. 358), to which they immediately add: "We assume, moreover, a view of language that takes it to be part of the natural sciences, in which questions of biology, in general, and neurobiology, in particular, play a prominent role –in principle if not in practice" (Embick and Poeppel 2015, p. 358).

The first assumption—(a)—implies a linguistic position that excludes the phonological and semantic interfaces of language, of language and thinks of language as a syntactic mechanism that basically consists of elements and operations that are describable in psycholinguistic and computational ways. That's the reason they always speak about a CR level: "Theories of the (psycho)linguistic type, which make specific claims about the computations and representations that constitute grammar and aspects of language use, fall under the general heading of 'Computational-Representational' (CR) Theories" (Embick and Poeppel 2015, p. 358). The aims of these author´s perspective is clear: "When we speak of potentially unifying CR and NB theories [...] the goal is to understand why language has the cognitive—that is to say, computational and representational—properties that it has" (Embick and Poeppel 2015, p. 359). The second assumption—(b)—is another central epistemological option: to consider language as an object of natural science and neurobiology which, in turn, have a "prominent role" in giving an account of language. Both assumptions are compatible with Chomsky's view of language in Minimalist theory, although he would not abandon the idea of linguistic theory autonomy.

Embick and Poeppel propose a central question as regard the possible relations of CR and NB levels: How it is possible to map the brain in different computations with the output of language? In this frame, they consider three possible ways: (i) a Correlational way; (ii) an Integrated way, in which neurobiological data provide crucial evidence for choosing among a representational and linguistic level; and (iii) an Explanatory way, in which properties of neurobiological data explain why the algorithmic/representational and the computational level are the way they are.

According to the proposal, Correlational Neurolinguistics means that "CR theories of language are used to investigate the NB foundations of language. Knowledge of how the brain computes is gained by capitalizing on CR knowledge of language" (Embick and Poeppel 2015, 260). It is also made explicit that:

> Correlational neurolinguistics in essence, is the idea that computational theories of language can be used profitably as a basis for exploring the brain, and that using CR theories in this way will tell us how the brain represents and computes language, or what NB structures and response patterns correlate with the representations and

> computations posited in the CR theory. (Embick and Poeppel 2015, p. 361)

Whereas Integrated Neurolinguistics means that "CR neurolinguistics plus the NB perspective provides crucial evidence that adjudicates among different CR theories. I.e., brain data enrich our understanding of language at the CR level" (Embick and Poeppel 2015, 260).

Further information on what the author understands by the heading "Integrated Neurolinguistics":

> [...] situations in which information derived from the types of variables that are examined in neuroscience prove decisive in selecting from competing CR-theoretical options. In particular, we hypothesize that there might be scenarios in which the types of information made available by 'typical' methods employed in (psycho)linguistics (i.e., patterns of linguistic phenomena in the languages of the world, or behavioral data of different types, such as lexical decision times, judgments, or eye movements) underdetermine a choice among different and competing CR theories. (Embick and Poeppel 2015, p. 361)

Finally, about Explanatory Neurolinguistics it is said: "(Correlational + Integrated Neurolinguistics) plus something about NB structure/function explains why the CR theory of language involves particular computations and representations (and not others)" (Embick and Poeppel 2015, p. 361). It is also emphasized in many parts of the referred text that an explanatory way of relationships is seen as a programmatic way, and it is very far from the present state of affairs.

5.2. A possible way of the relation between cognitive science (including linguistics) and neurobiology. Problems and intended solutions

5.2.1.

While considering a possible line of thought in this programmatic way, some central problems arise in the enterprise of addressing the relation between the primitives of cognition (here speech, language) and neurobiology. One of them is the "ontological incommensurability problem" (OCP). Poeppel and Embick (2005) suggest that the primitive elements of the two domains cannot be mapped onto each other at all given the current formulas. Cognitive sciences, including linguistics and psychology, provide a detailed analysis of the ontological structure of various domains (this is what we call the 'human cognome', i.e., the comprehensive list of elementary mental representations and operations), and neurobiology provides a growing list of available neural structures, but the primitive elements of the two domains cannot be mapped onto each other at all given the current formulation of the parts list. The authors affirm this view:

> There are no equivalence relations, no isomorphisms, no easy mappings from the theoretical infrastructure of the cognitive sciences to that of neurobiology. On the assumption that one is adopting a non-dualist research strategy, it is entirely unclear how to link the two approaches (beyond stating some relatively gross correlations). (Embick and Poeppel 2015, p. 364)

They also affirm:

> On such a view, the concepts that form the basis of cognition and neurobiology are impossible to align in principle. If that is the case, then there is no opportunity for identifying plausible isomorphisms and certainly no opportunity for reduction (if that were a goal). (Embick and Poeppel 2015, p. 365)

The second problem is the so-called 'the Granularity Mismatch Problem' (GMP). In the context of trying to give explanations of how the properties of the brain account for perception and cognition—and not just description, a big issue is whether cognition is investigated at the right granularity, if the goal is to develop linking hypotheses to neurobiology. Poeppel and Embick (2005) state the problem in these terms:

> The problem, which we call the Granularity Mismatch Problem (GMP), is that there is a mismatch between the 'conceptual granularity' of the elemental concepts of linguistics and the elemental concepts of neurobiology and cognitive neuroscience (which are, relative to the corresponding linguistic primitives, coarse-grained). (Poeppel and Embick 2005, p. 106)

Immediately, on the same page of the text, the Granularity Mismatch Problem is defined as follows:

> Linguistic and neuroscientific studies of language operate with objects of different granularity. In particular, linguistic computation involves a number of fine- grained distinctions and explicit computational operations. Neuroscientific approaches to language operate in terms of broader conceptual distinctions. (Poeppel and Embick 2005, p. 106)

According to Poeppel and Embick's perspective linguistic research comes at the questions of interest with a toolbox that permits a very high 'resolution'. An area of research as conceptually straightforward as word recognition approaches the issue by incorporating subtle features of phonology, the statistical adjacency relations between sounds, the role of morphological structure, the role syntactic categories may play for lexical access, nuanced theories about the semantics of words, theories exploiting the predictive nature of language processing, etc. to understand aspects of facilitated or impeded lexical processing, and so on. That is to say, even a process as allegedly straightforward as recognizing a spoken word interfaces in a variety of ways with phonology, morphology, syntax, lexical semantics, compositional semantics, and aspects of discourse (see Embick and Poeppel 2015, p. 366). In contrast, neurobiological research on language attempts to address much broader questions. Typical issues that are investigated in imaging studies, for example, include whether syntax is localized or where the mental lexicon is localized. This is, to be sure, not a problem that cannot be overcome.

Also, the techniques available to the brain sciences investigate the issues in a manner commensurate with what can be measured and analyzed; while the techniques available to the cognitive sciences attempt to capture generalizations about speech and language processing at the highest possible 'conceptual resolution'. The practical consequence of such a granularity mismatch is that issues at very different levels of representation end up being addressed. This is not intrinsically bad, but it is limiting in that it fails to move the field forward in developing mechanistic linking hypotheses between brain structure and function and the organization of this particular cognitive system. In recent years, this has been changing in a positive direction in the context of the ever-closer relations between theoreticians, cognitive scientists, and neurobiologists applying state-of-the-art techniques (see Embick and Poeppel 2015, p. 365).

5.2.2.

Considering that there are close relations between perception, cognition and neurobiology at present, the position adopted by the named authors is to turn to unification, following the perspective of unification or consilience (Chomsky 2000). In this view, the central question is: what steps need to be taken to unify these different domains? The answer is given through an analogy that illustrates the relationship between chemistry and physics at the beginning of the 20th century:

> Chemistry was, contrary to intuition, not reduced to the principles of physics. Rather, the conceptual structure (ontology) of physics had to change to adjust to the new insights coming from the (putatively higher order) area of chemistry. Provocatively, a similar situation might present itself in the relationship between the brain and cognitive sciences (Chomsky 2000). Perhaps what are considered elementary functional units in neurosciences will change by considering more closely what the demands of perception and cognition are. (Embick and Poeppel 2015, p. 365)

Once again, the authors choose the strategy outlined by Marr and others:

> The computational level of analysis, provided by linguistics, psychology, and aspects of computer science, must be linked to the implementational level of analysis, neurobiology. What was suggested by Marr is that an intermediate (algorithmic) level of description specifies the representations and computations that are executed by the implementational circuitry to form the basis of the computational level of characterization. If this strategy is on the right track, current research should in part focus on the operations and algorithms that underpin language processing. The commitment to an algorithm or computation in this domain commits one to representations of one form or another with increasing specificity and also provides clear constraints for what the neural circuitry must accomplish. (Embick and Poeppel 2015, p. 366)

As a conclusion of the sketched position, a prerequisite to developing principled linking hypotheses between (linguistic) cognition and neurobiology is the definition of the parts list, or the cognomen, paying special attention to computational constituents. The central point is that "the set of primitive operations and representations will allow the formulation of linking hypotheses that translate more seamlessly to computational neuroscience" (Embick and Poeppel 2015, p. 361).

5.3. Some problems in Embick and Poeppel's approach of the mapping problem

According to the philosophy of science, there are many problems in how linguistics classic papers treat the relation between levels. There are no clear characterizations on those referred relations. For example, we can pay attention to some of the theoretical approaches to 'correlation':

> The problem is that one cannot simply 'draw lines' between the categories provided by each domain and expect such an attempt at 'alignment' to withstand any serious scrutiny. The 'mappings' between domains are at best correlational. Such correlations are, to be sure, positive results. Identifying the 'neural correlates' of any perceptual or cognitive function is often the key research goal (consider, e.g., the vigorous search for the neural correlates of consciousness). (Embick and Poeppel 2015, p. 359)

So 'to correlate' the CR level with the NB level seems to mean that an entity of the CR level can be correlated with a neural entity that serves as its material support. Correlation is possible through mapping. The problem is more complicated if we attempt to understand whether correlation is a relation between categories or entities.

Looking at the GMP there is further information on what the author understands by the heading "Integrated Neurolinguistics": it is evident that from many philosophical perspectives "concepts" as primitives of a discipline cannot be considered equal to "objects" or "conceptual distinctions". The perspective is even worse when we pay attention to paragraphs like this one: In the words of Poeppel and Embick:

> [...] this set of differences in detail of analysis is collectively referred to as the Granularity Mismatch Problem (GMP). In highlighting the GMP, we are not asserting that neuroscience itself is coarse-grained; to the contrary, spectacular progress has been made in identifying the brain's structures and operations. But these accounts of neural structure and function do not connect with (or map to) the objects employed in CR theories: it is difficult to establish CR/NB linking hypotheses because in general the study of how the brain computes what it computes in language is at present too coarse to link up meaningfully with the distinctions made on the CR side. (Poeppel and Embick 2005, p. 106)

Note that once again concepts and objects are considered equivalent; but there are also references to methodological problems, which are relative to the hypothesis of potential CR and NB links, and the difficulties of comparing the studies in these fields.

Referring to the Ontological Incommensurability Problem, Embick and Poeppel affirm:

> The first is that CR and NB theories have different types of primitives, i.e. distinct ontologies, making any attempts at directly linking the two domains prima facie problematic, if not incoherent. [...] To illustrate, consider that the CR-theories and NB-theories are each advancing in their own terms, such that each has developed its own inventory of primitive objects and elementary operations on these objects. (Embick and Poeppel 2015, p. 362)

Note that we can find the same problems in relation to the GMP, because there are no distinctions between terms and entities, and we come and go directly from the semantic field to the ontology of theories.

Additionally, there are also many problems when we try to determine if the relations (correlations and other ones) are laid out between disciplines, methodological ways of proceeding in disciplines, theories, categories or vocabulary of theories and so on. Thus, these statements are not precise enough and obstruct the full comprehension of very important philosophical problems like the relation between theories or disciplines (or levels of explanations corresponding to disciplines).

6. Central components of FLN and levels of explanation.

6.1. The theoretical frame

At the heart of the matter in the Minimalist program is Chomsky's claim that we must distinguish between the machinery for externalization and sound production (which is tied to communication) and the "central processor" which underlies that machinery. In a nutshell, we must distinguish between FLB and FLN.

By dividing FLB in two, now Chomsky refers to the three traits related to FL: (1) a sensorimotor component; (2) a conceptual-intentional system of inference (both 1 and 2 have to do with FLB); and (3) FLN: a computational system that builds hierarchically structured expressions with systematic interpretations at the interfaces with the other two internal systems (see Berwick and Chomsky 2016, p. 11).

While focusing on FLN, Chomsky emphasizes the role of structures and syntactic objects within the mechanisms of performance and economy of the mind. Language did not begin as its externalization, but as a trait related to internal thought (see Berwick and Chomsky 2016, p. 74).

In the minimalist frame, there is an operation that is considered as the central determinant of human language: Merge. It is taken to be the basic combinatorial operation in syntax originally postulated by Chomsky (1995). There, the author affirms that the computational system generates structures (S) that are a set of syntactic objects (SO_1, SO_n), which are then interpreted by the interface systems.

> In the LF interface, Σ can only be interpreted if it consists of a single syntactic object. Clearly, then, the computational system must include a [...] procedure that combines syntactic objects already formed. A derivation converges only if this operation has been applied as many times as to leave us with a single object [...] The simplest operation of its kind is one that takes a couple of syntactic objects (SO_i, SO_j) and replaces them with a new combined syntactic object SO_{ij}. (Chomsky 1995, p. 165)

Thus, considering (1), it is stated that syntactic objects are lexical items (a), "complexes of features, listed in the lexicon", and (b) "is the recursive step" (Chomsky 1995, p. 191). The recursive operation that forms K will include K onwards but no longer α and β.

(1) (a) Lexical items;
  (b) $K = \{\gamma, \{\alpha, \beta\}\}$, where α, β are objects and γ is the label of K.

These syntactic objects consist of parts, so they are 'structured' objects, and those parts are nested into each other, so they determine 'hierarchies'. Throughout his work, Chomsky argued that the sentences of a language are not strings of words ordered according to a transitional probability, but that they constitute greater structures when being organized into cells (or categories) that respect a hierarchical order. In MP, this idea is accentuated by the postulate (b). Although earlier works had defended the necessary recursiveness of a grammar, this model attributes greater explanatory character to this property. It is important to point out that, in this explanation, the main capacity of the mechanism producing linguistic structures is to embed phrases within other sentences, generating sentences with different types and degrees of nesting of smaller structures into larger ones. Recursion then allows us to construct an infinite number of combinations from a finite number of elements.

Since Chomsky (1995) and even in the most recent texts (see, for example, Berwick and Chomsky 2016), the operation has two variants: one is to combine two lexical elements, which is usually called 'external merge'. The other, 'internal merge', makes it possible to explain the phenomenon by which certain objects are 'heard' in one place but 'interpreted' in another, a phenomenon traditionally known as 'displacement'. Anyway, these objects are nothing more than computations, mental objects, therefore, they do not sound or mean anything, but, as a result of the interface with the AP and CI systems, they each will receive a reading.

Boeckx agrees with Chomsky's suggestion and refers to 'unbounded combine' as Merge, defining it as follows:

(2) Merge (simpliciter) $=_{\text{def}} \alpha \oplus \beta \rightarrow \{\alpha, \beta\}$ (Boeckx 2013, p. 467)

Poeppel (2012) also emphasized that the character of "unbounded combination" results to be important because of the nature of the elements manipulated by the operation: "whereas concepts impose selectional restrictions (valency requirements) that limit combinations, lexical items don't[...]. Once such selectional constraints were lifted, concepts clothed as lexical items could combine across modular boundaries" (Poeppel 2012, p. 467).

So, taking elements of combination as pre-lexical ones, a set of elementary particles are to be combined by the operation Merge. They will be labeled relating these ones with a conceptual background. Now, we will have lexical items (a linguistic object) connected to conceptual elements (cognitive objects). This operation is due to Spell-Out. A set of combinations being carried out by Merge would converge in the phonological and semantic interfaces made possible by Spell-Out.

Boeckx presents the idea in this way:

> The picture that emerges from the considerations in this section is that the structural backbone known as natural language syntax boils down to a merge operation (unbounded combination), made possible by the lifting of valency constraints imposed by concepts (lexical items as edge feature carriers), and made usable (i.e. structured) by a cyclic transfer mechanism (phasal spell out). Syntax, then, essentially takes the form of a structural rhythm, a sequence of merge applications interleaved with spell-out points: Merge steps–Spell-Out, Merge steps–Spell-Out, Merge steps–Spell-Out. (Boeckx 2012, p. 470)

In the same way, Boeckx (2013) and Edwick and Poeppel (2015) propose to conceive syntax as an unbounded merge operation that is regulated by the cyclic applications of a process called "Spell-Out". Spell-Out-regulated Merge amounts to an iterative application of a generic combinatorial operation (set-formation), coupled with a periodic forgetting of material already combined. The elements (lexical items) can be freely combined, meanwhile, an active memory buffer ("phase edge" and "phase complement" in the recent generative literature) plays a role in the process, and the right balance between a process of combination (Merge) and a process of deactivation (De-Merge or Spell-Out) takes place.

The other theoretical term in the MP approach concerns the "head" and "complementizer". They are functional syntactic relations that would make possible the legibility of semantic interpretations of sentences.

To go further on the topic of Merge and Spell-Out, we can introduce now Boeckx's considerations of this subject:

> [...] the structure of Cyclic Spell-Out put forth in Chomsky (2000) and subsequent work combined with Marantz's (2008) suggestion that 'light' (phasal) categories label roots, yield an asymmetric Head-Phrase schema (with the head corresponding to the phase head, and the phrase to the so- called phase complement) that does not require any new syntactic operation (or pre-syntactic lexical information) to generate sets that are, for the external systems interfacing with syntax, more structured than sets generated by bare Merge.
>
> (3) $\{\alpha, \{\alpha, \beta\}\} = \{x, \{\sqrt{\alpha}, (\beta)\}\}$
>
> x = Phrase Head/Cyclic Node
> $\sqrt{\alpha}, (\beta)$ = Phrase Complement/Transferred Material.
> (Boeckx 2013, p. 469)

So, the combination of Merge and Spell-Out gives rise to nested structures, that is: products of merge are embedded inside phases (see the notion of 'phase head complement' in Chomsky (2000)). Following the presented approach, it can be said that syntactic hierarchy is reduced to a hierarchical organization of Merge—and Spell-Out- rhythms.

6.2. Morpho-syntax. Possible relations into the neurobiological level

In the last section, we presented how, from the generative perspective, the linguistic level explains the generation of language in the area of syntax. As we have already said, we can consider this level as a computational level (following Marr). Now, we can inquire into the problem of accounting for the language mechanism at the representative, algorithmic, and implementational levels. As Poeppel wrote:

> Modern neuroimaging research is now sensitive to distinctions made in language research. Specifically, distinctions between linguistic subdomains now form the basis of many if not most neuroscience experiments. Typical studies seek to identify the 'brain basis of syntax', or the 'regions underlying semantics, or the 'cortical network supporting phonology', and so on. In fact, since the late 1980s, such studies have dominated the literature and have added substantial new insights to our understanding of brain organization. (Poeppel 2017, p. 163)

It is important to remark on the distinction between the morpho-syntactic component of language and its brain basis, as well as the interphases of language. It is now frequent to speak about neurologic studies which give the basis to syntax, semantics, or phonology as separate phenomena (distinct explanandum) that are to be explained according to other levels of support. The same "separation of explananda" is usually found in the frame of evolutive biolinguistics (especially in Berwick and Chomsky 2016).

In the case of syntax (our focus of attention), Boeckx (2009; 2013) and Boeckx and Theofanopoulou (2014) argue that syntactic computation boils down to a rhythm: an interleaving of Merge and Spell Out applications. From this assertion, it is suggested that this level of description could be put in correspondence with brain-level descriptions that assign a crucial role to rhythms.

The literature on brain rhythms shows that flexible frequency control of cortical oscillations enables computations required for working memory (Dipoppa and Gutkin 2013). Through the eyes of a linguist, this information can be seen as the biological basis of elementary syntactic computation. The role of memory allows us to explain the process of accessing lexical items and merging them. Boeckx suggests thinking of maintaining a memory in terms of the syntactician's memory buffer, and memory clearance as Spell-Out. He also suggests that: "if one is willing to decompose specific linguistic operations like Merge in more generic terms, one can already take advantage of the existing literature to translate these operations in terms of neuronal dynamics" (Boeckx and Theofanopoulou 2014, pp. 416-417).

Poeppel agrees with Boeckx in considering that syntax has a structural rhythm and that we can use a bridge of mind/brain thinking to the well-studied rhythmic properties of the brain.

> In the hands of Gierer and Meinhardt (1972) RD systems have been used to account for many important types of pattern formation and morphogenesis observed in development. RD systems consist of a short-range autocatalytic substance, the activator, and of its long-range antagonist, the inhibitor. (Boeckx 2013, p. 470)

Boeckx argues that:

> [...] the 'translation' of syntactic structure into rhythmic structure could be just what is needed to move from the computational level to the algorithmic level, and from there to the implementational level (using Marrian terminology), considering that rhythms are intrinsic to neuronal activity, the way neuronal ensembles organize themselves. (Boeckx 2013, p. 471)

The authors agree with the evidence given by numerous studies demonstrating that the brain generates a range of oscillations (alpha, beta, gamma, delta, and theta oscillations have figured prominently in the literature), and that these rhythms interact with one another by influencing (modulating) the phase and/or the amplitude of the oscillations. Then, couplings give rise to a hierarchical organization of brain rhythms that underlies many aspects of human cognition. This element can be considered the "syntax of the brain"—as Boeckx (2013) called it following Buzsáki (2010)—which provides a general algorithmic substrate, a coding mechanism, for natural language syntax (see Boeckx 2013, p. 472-473).

Another important contribution from this perspective is the defense of a new model of brain areas-function presuppositions. Boeckx argues that brain models should be far more distributed, and less cortico-centric than the classical Broca-Wernicke-Lichtheim-Geschwind model. According to him, if we assume that the cognitive signature of Merge-based syntax is a cross-modular concept formation, then the 'neuronal workspace' model appears to be the right kind.

Boeckx (2013) claims that the neuronal workspace model emphasizes the role of distributed neurons with long-distance connections forming a 'global neuronal workspace'. In addition to cortical areas, they emphasize the central role of the thalamus, which acts as a modulator of oscillatory synchrony, regulating network interactions and shaping the communication between various brain areas.

> The recruitment of the thalamus in the context of language would, then, have been required to achieve the nesting of high frequency oscillations inside lower frequency oscillations, much like the more sparsely distributed (less frequent) phase heads were required to embed the (frequent) instances of merge, organizing these into constituents. Put another way, thalamic activity would correspond to a periodic sampling of cortical (fronto-parietal) activity ("cyclic spell-out"). Once synchronized, the fronto-parietal network would then act on other specific regions (including Broca's area, but also subcortical structures) to arrive at more detailed linguistic representations (including, ultimately, linear order [...]. (Boeckx 2013, p. 474)

Going back to the distinction between FLN and FNB, we must remember that in the context of the Minimalist Program, FLN is proposed to think of a specific domain of operations with a process of basic elements as the "primitives" of linguistic explanation. Another thing is the broadly construed faculty of language (FLB) that encompasses a range of mechanisms that can contribute to other domains and even function in other species (Hauser, Chomsky and Fitch 2002). As Poeppel argues:

> This area of study has been tremendously controversial, with some researchers arguing vigorously for specialized neural mechanisms and others arguing as vigorously for general mechanisms that are not restricted to language. The difficult part of these debates—from the neurobiological point of view—is that the domains under consideration are quite broad relative to the mechanisms one attempts to study as a neuroscientist; that is to say, 'language', considered as an undifferentiated, monolithic cognitive domain, is arguably not domain-specific, since it draws on memory, attention, and other cognitive and perceptuo-motor capacities (cf. FLB). (Poeppel 2017, p. 160-161)

Following the translation of Poeppel's view, we believe it is an adequate strategy to divide FLB and FLN in order to "cut out" some specific aspects of human language, as they deviate from the general mechanisms of language, which are more proximal to other species. Without this division, the determination of morpho-syntactic mechanisms of human language reduces the explanandum when speaking of a "big object to be explained: language". If we reduce this object to the level of linguistics, the studies on the neurobiological underground (the physical support) of language generation seemed to be an adequate method of research.

7. The structuralist approach to the problem of explanation. Some possible relations regarding to the notion of "level of explanation" applied to syntaxis's field in the MP

7.1. An explanation from the conceptual perspective of Structuralist theories

7.1.1. General considerations

The problem of scientific explanation has received numerous treatments in contemporary epistemology. Recent developments on the topic in the Structuralist View of Theories try to overcome certain problems that arose in the traditional models of explanation, in particular, those related to inferential positions of Hempel and Oppenheim (1948) and Hempel (1966); causalist model of Lewis (1973; 1986) and Salmon (1984; 1989; 1992; 1998; 2002a; 2002b); unificationism of Friedman (1974) and Kichter (1981; 1989; 1993).

The Structuralist View of Theories (Sneed 1971) and (Stegmüller 1973) presented a metatheoretical instrument that was very fruitful to the elucidation of empirical theories in different fields. A sophisticated version of this philosophical position was stated in Balzer, Moulines and Sneed (1987), and later a great number of productions were carried out within its framework. The topic of explanation was systematically introduced and developed by Bartelborth (1996a; 1996b; 1999; 2002); while several other authors have contributed to its advance and diffusion: Forge (2002), Díez (2002a; 2002b; 2012) and Moulines (2005).

From this perspective several applications to various fields of empirical science have been developed, but, however, the treatment of scientific explanation has not been applied to the field of Linguistics yet. The purpose of this section is to focus on this topic in relation to recent developments in Chomsky's Theory (CHT), particularly on the theory's syntax component.

7.1.2. Scientific Explanation as Model-Theoretic Embedding

From the perspective of the structuralist view of theories, the main idea regarding the notion of "explanation" is that explaining a phenomenon (the phenomenon qua: the data model we want to explain) involves embedding it into a nomic pattern within a theory-net, i.e., the phenomenon is embedded into some particular branch of the net that acts as explanans through its law(s) together with initial conditions.

From this perspective explanandum and explanans are certain kinds of models/structures, the former being the data model we want to explain, the latter the theoretical model, defined by certain laws and, when needed, initial conditions, which we use in order to explain the former.

At the top of the theory-net structure we have a basic theory element with the most general law of a theory-element T, which is usually a guiding principle for research under a particular program (Moulines 1982). Given the generality of its law, the set of intended applications of the theory (I) for that basic theory element is very large, almost unrestrictive.

As we said, other theory elements in the net connect with the basic theory-element through the relation of specialization. In any given specialization, no deduction is involved because new additional information is included progressively as we go down the theoretical structure. The more specialized the theory element and its special law(s) are, the more restrictive the respective set I results.

Note that in this basic idea about explanation as nomological embedding, some of the Hempelian conceptions are preserved, only those additional conditions will be required: embedding must be broadened or ampliative (with T-theoretical terms) and specialized (see Díez 2014).

In the vocabulary of any theory (T) structuralism differentiates between terms that are strictly dependent on T, that is, every determination of its (qualitative/quantitative) extension requires the application of T, from terms which can be determined independently of T. Structuralists usually call the former "T-theoretical terms", and the latter "T-non-theoretical terms". This distinction is not absolute but relativized to particular theories. That is, a given concept can be theoretical in one theory and non-theoretical in another. As a healthy norm to avoid circularity, the explanandum of T should be determined through T-non-theoretical terms while the explanans involves the incorporation of T-theoretical terms. This is just the condition: all explanations must include new T-theoretical concepts.

The second referred condition is that nomological embedding, even when it is ampliative, cannot be explanatory without specialization.

What matters is that if all laws of the top of the net is a schematic general principle, they only tell us the kind of entities and the scheme of nomological connections that account for the phenomena, but they don't specify any particular nomological connection. So, we need laws that appear lower down in the net and specialize the general principle. Therefore, the general laws can be tested through one of the net- branches ending in a specific law, since they are the specific bottom laws where the specific empirical information lies (see Díez 2014, p. 1425).

Thus, we must consider that, in order to have a real explanatory approach, the second condition should be fulfilled too: to be explanatory, the embedding has to make use of a special law with its additional restrictions, together with the one(s) in the upper levels of the theoretical hierarchy.

7.2. Structuralist perspective of explanation and levels of explanation. The case of the syntaxis field in the MP

To continue with the preceding perspective on explanation, the perspective on explanation which was drawn immediately before, we need to build a theory net of CHT. This task compels us to place some restrictions and to make some limits in a very broad set of problems in linguistics. Also, the first restriction will be to concentrate on the problem mentioned as (1) in section 3.1 and the answer offered by the MP.

CHT must take account of FLB, that is, the components and mechanism of it which enable the production of the units of what is understood by human language. Note that we are leaving aside a set of problems such as the problem of acquisition; the problem of the evolutionary genesis of language (and the theories giving account of the problem). We concentrate on a general theory CHT which tries to explain how a human being produces a set of well-formed phrases and sentences of a language as an output of some hypothesized background "mental language" (input). This last expression is important here, because the theory assumes: a) that human language is something particular and unique; b) that language is not centrally a way of communication, but a mechanism which is somewhat internalized as a cognitive or mind component (I- language in the classic terminology of Chomsky's work since the 1980s).

Having separated the components of FL, we have (1) a sensorimotor component; (2) a conceptual-intentional system of inference (both 1 and 2 have to do with FLB); and (3) FLN: a computational system that builds hierarchically structured expressions with systematic interpretations on the interfaces with the other two internal systems (see Berwick and Chomsky 2016, p. 11).

In view of these remarks, to provide a metatheoretical representation of the reduced theory, we will restrict CHT to MP, as the only theory to be considered in the frame of the development of CHT. Then, we will build a theoretical network of MP (MPN) as a tree-network, whose basic element (BMP) contains the basic central vocabulary of the theory, the general theoretical terms and fundamental laws.

The net also has a set of specifications of that element, elements being in peripheral nodes, which contain special laws that explain different aspects of the generation of human language.

We can remember that according to MP language is a structural succession of finite components (lexicon) generated by a computational mechanism, whose output consists of phrases or sentences of human language. This computational mechanism involves a set of elements (lexical items) and a set of operations, the most important of which is: Merge.

Also, the computational mechanism is governed by certain rules, principles, or constraints. Some of the central topics that MP must account for can be framed in the following questions: What is the structural mechanism of language? Which are the rules, principles or constraints in the device of the mechanism that allow outputs (phrases or sentences) to be identified as belonging to one language (that is, entities that a competent speaker of a language recognized as such)?

As was already said, according to MP, the computational mechanism of a language interacts with two levels of interface: the sensorimotor interface and the conceptual-intentional interface. The output of the computational mechanism interfaces with a phonetic and phonological manifestation, and at the same time, the expression has got a linguistic manifestation which consists of a linear succession of words. In addition, the phenomenon of semantic interpretation is a process that requires a unit, such as a lexeme, a phrase or a sentence, to mean something. That is, the mechanism involves a relationship between the morpho-syntactic structure and the conceptual-intentional interface, in general terms of the extra-linguistic reference.

We can interpret the morpho-syntactic component of the theory as the primary one. Other hand we have the two interphases as phenomena or set of phenomena to be explained (explanandum) and the construction of theoretical postulates to account for them. Saying this, we can consider the syntactic component of MP as the basic theory element of the net (MPB) and another two theory elements: MPP and MPS respectively as the theory element "Phonology" of MP and the "Semantic" theory element of MP.

In this way we are acknowledging that the general law or laws established in MPB have to be satisfied in MPP and MPS. We also acknowledge that there would be some special laws proper to these theory-elements, with some theoretical terms being specific to them.

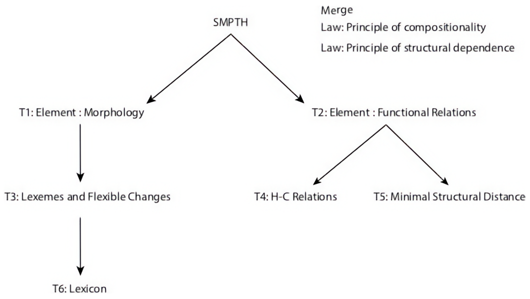

Figure 2: Theoretical Network of Syntactic Theory in the MP.

MPB assumes that all syntax of any language contains a set of elements: the set of lexemes. Syntax theory also postulates the existence of a basic relation: merge. As presented before, in the first phase of building, merge enables the construction of complex lexemes from simple ones. On the other hand, we have another operation, called "spell out" which is always a relation between lexemes and elements of MPP and MPS respectively. In the case of the elements of MPP, the relation will take place between a lexeme and a set of sounds, so that some concatenation of lexemes will be actually a viable sound-representation of a "lexical item" of a language. In contrast, the derivation would "crash". In the case of the elements of MPS, the relation will be performed between a lexeme and a concept, so that some concatenation of lexemes will be viable as a conceptual label for a "lexical item" of a language. In contrast, the derivation would "crash".

As we have anticipated, the operation merge not only contributes to the concatenation of elements and components of grammar (simple and complex lexemes) but also to compound phrases. Then the cyclic and recursive application of merge would make it possible to build sentences from a set of phrases. In this procedure some restrictions are made. One of them is the so-called "principle of structural dependence". It can be stated thus: "In all languages the relationship that endures among the different components of a sentence depends on a hierarchical structure" (and not on the sequence in which they appear). This theoretical principle corresponds to functional relationships: the determination of structural features of the sentence components as core, complementizer.

7.3. A programmatic way of giving a philosophical understanding of the problem of levels of explanation

We think that the problems referred to in (5.3) can be clarified from the perspective of the philosophy of science. The first decision we have to take is whether to follow Marr's three levels applied to the field of linguistics, as drawn at the beginning of (5.3.1), or whether to adhere to the two levels proposed by Poeppel and Embick (2005) and Embick and Poeppel (2015). If we opt to follow Marr's view as applicable to the field of Linguistics, this last field ought to be independent from cognitive-psychology and computer theories (despite their possible relations of course) and then, we would also have to consider a neurobiological field. If we follow the latter, we will make the same mistake as Poeppel and Embick did: they confused the topic of disciplinary fields of cognitive-science with the topic of giving an account of some explanandum related to a discipline. If we follow Marr's view, the linguistic enterprise would explain the typical linguistic computations of the generation of language outputs, and the logic of the strategy by which it can be carried out; while the psychology and computer sciences would give account of representations for those inputs and outputs, and the algorithm for the transformations. Note that if we include linguistics in cognitive sciences, we are concerning with a discipline classification; but if we pay attention to linguistic explanation as a level of giving account of an explanandum, and the cognitive psychology and computer theory as a representational and algorithmic level of explanation of the same explanandum, we are making another distinction.

So, our first decision is to be loyal to Marr's perspective, accepting the introduced idea that we are speaking of an explanandum (a phenomenon or set of phenomena to be explained) which has to be the same to be examined in terms of different theoretical levels of explanations (we will clarify this idea in what follows).

The first thing we have to make clear is that the linguistic theory we are concerned with, the MP, has an explanandum: language as I-language, and in particular, the generation or out-puts from I-language. Otherwise, FLN's entities and relations or operations allow the generation of syntactic and morphological well-formed outputs. The linguistic explanans that accounts for this explanandum was presented in 5.1.

The second step is to consider a cognitive theory whose explanandum must be the same, although the components of the explanans will not coincide. Note we are speaking of theories. Now the elements, relations, functions and other theoretical entities will belong to cognitive-psychology and computer-science. With the statements being presented in the explanans, an explanation of the linguistic explanandum has to be drawn as a representative and algorithmic account of it.

Then, we have the neurobiological theory which accounts for linguistic explanandum, but from its own perspective of research: its own entities of examination, functions, relations and all the vocabulary unique and proper of neurobiology.

We introduce an analogy taking as an example the description of a biology object as a fly. If we tried to describe a fly based on direct observation, we would describe its anatomic elements: segments, legs, flies and so on. If we looked at the insect through a microscope, we would describe the tissues, cellular structures, and so on. If we described it from the point of Chemistry and Physics, we would speak of molecular structures, atoms, the chemical element the composition of its parts, etc. But these levels of description (we refer to description, not of explanation) focus on the same object: the fly.

Different theories describing or giving account of a phenomenon will develop their theoretical explanans, that is: the terms, models and statements usually used to give account to any object in the domain of its explicative relevance.

In the representative level of cognitive psychology and computer science, as well as in the level of neurobiology there are theories—compatible with the linguistic MP—which has theoretical terms. These terms—according to the structuralist approach of theories—are specific to these theories and cannot be determined by external factors. But there are other terms whose meaning and use values do not depend on the theory in question, but from other theories related to them (the non-theoretical terms). One of the advantages of this view is that "theoretical" and "non-Theoretical" terms are not determined by the correlations to an abstract or empirical level of phenomena. Instead, they are determined by the theory itself, or by other theories. This strategy allows us to think of the relations of theories in a broader sense: theory-holoms.

The set of intended applications of the theories under consideration is also a central problem. There is an enormous set of linguistic phenomena, a great part of which has been excluded and concentrated in the manifestation of some phenomenon: the outputs of any language from the syntactic perspective of our theory. This is also a set of applications which depends on people using the theory: they are "intended applications". The last statements have at least these implications: (a) we are not speaking of "phenomena" as bare empirical phenomena, but as "modulated phenomena" which is consistent with the theory; (b) this modulated phenomenon is not at all a theory-dependent entity, because we are considering a theory as a set of theoretical terms and non-theoretical ones, that is, terms depending on other theories, thus we have phenomena in the sense of a set of entities that are part of a useful field of science (in certain contexts of scientific development); (c) In the field of each level of explanation, we will find some central general statements (laws) concerning the generalizations made from the corpus of the theory; (d) Assuming the structuralist perspective, we will have to consider the set of "intended applications" which is determined by the state of affairs in a field of science, and also by the decisions taken by the practitioners of the scientific community (using Kuhn's expression) who belong to this scientific field.

In summary, (1) we recognize one explanandum: the one belonging to MP (in our case The

Syntax component of MP); (2) we follow Marr's proposal of three levels of explanation (we have just referred to them) according to different theories situated on these levels; (3) According to the Structuralist view of theories we refer to "Theories" (theory-elements, nets or holons depending of the extension of "theory" we are dealing with). We recognize some phenomena researched by the theory T (Mpp), a set of vocabulary (T-theoretical and T-non theoretical ones) and other auxiliary one (Mp), a set of models (central laws of the theory T) and some Intended applications (I).

Having MCHT theory as a net, and the Syntactic component as a basic theory- element of that net, it is now clear to determine the explanandum and the explanans in an explanation in the limits of SMTH theory.

Now that we can speak of "levels of explanation" from a determined cognitive psychology, and a determined computer theory as the representational and algorithmic level, our task consists of enunciating which theory we are speaking about. The same task is to be carried out with neurological theories. Then, the following task will be to give account of its respective components.

This endeavor should take into account "compatibility" or "commensurability" among them. Much literature is available on this topic. We will not refer to it, we are simply offering a sketch of the aspects that ought to be considered "levels of explanation" in metatheoretical terms, with the intention of overcoming the difficulties found in the proposals coming from the linguistic literature about the problem. Instead, we want to show some strategies that are necessary to carry out our task: (1) we have to clarify which are the theoretical terms that intervene in the different explanans of theories of the two named levels: (2) a tactic to "correlate" these terms. We use that word here, but it is very important to establish if we would speak on "correlations" of terms, "inter-definitions" of terms, "mapping" terms of different theories, and so on.

Note that we are always speaking about terms. A theory, as such, must give account of a set of phenomena in the frame of conceptualization and definitions introduced by this theory. Other means of achieving this task are considered inviable. We can find theoretical approaches to any object of knowledge. The entities postulated to analyze a scientific object of research (in our case the linguistic objects, and their relation with the domain of application) would be determined by the theory elements drawn by the theory.

The last paragraph brings special remarks on the role played by the terms of a theory, because most articles coming from linguistic approaches to relations between theories place direct attention on the ontologies of theories. We are not saying that this topic is not relevant to the philosophical analysis of theories, but in our opinion the ontological topic will not to be directly available without considering a semantic approach to ontology (we could also add a pragmatic one, but it would be the topic of another text).

Then, we have to think of intertheoretical relations. So, a central problem in this metatheoretical frame will be to determine which inter theoretical relation would be set up among the net theories under consideration. Keeping this purpose in mind it is important to consider the "intertheoretical links", also called "Global Links" (Balzer, Moulines and Sneed 1983). A general proposal to extend the field of intertheoretical relations from relations between theory-elements to relations between nets was carried out by Balzer, Moulines and Sneed (1983) and Moulines (1992), among others.

Since this is a programmatic proposal, we leave, for the time being, an open path to find a metatheoretical proposal for the problem of "level of explanations" with the aim of explaining such a complex phenomenon as human language through the perspective of MP.

8. Final Considerations

At the beginning of the article, we had expressed a central aim: to address the problem of explanation within the framework of Generative Grammar Theory, particularly in the Chomskyan approach (CHT).

We began by making a historical account of what were the main tasks of CHT, intending to clarify the CHT's explanandum and the explanans in different periods of Chomsky's linguistic theory up until the present proposal: MP. We have carried out this enterprise in order to show the different problems and the different theoretical corpus that was proposed to solve them. We also wanted to show how the agenda of problems was growing so that the proper linguistic ones (phrase and sentence generation, grammaticality, and so on) had to be extended to a new field of sciences: the cognitive sciences; and then, we explained how this area of study came to be interconnected with biological sciences (especially genetics, neurology, evolution theory).

How the study of language was carried out and how the object 'language' was conceived in generativism were central topics. One first decision has to do with the opposition to behavioristic approaches in linguistics, and to choose to determine 'language' as I-language. Language is not a set of manifestations of phonological or semantic nature, and it is not communication either, but rather a part of the mind-brain, "language of thought" as it was called many times.

Thus, explaining 'language' deals with theoretical tools (terms, hypotheses, laws) which constitute the explanans of a set of the problems formulated from that conception of language (some of which have changed and other ones which have remained in the present agenda).

The problem-solutions perspective of research—very typical in Chomsky's perspective—had to face methodological aspects of explanation, but also metatheoretical aspects (as the one related to explanatory adequacy) and at present times the so called: "explanatory level problem". The problem was put in terms of whether generativist linguistic explanations could be carried out in an autonomous way, or whether the explanans of linguistics should have theoretical relation to psycholinguistic and computational theories, as well as to some central branches of biology as neurology.

We have taken into consideration the proposal suggested from Marr (1982) about 'explanatory levels'; and another more complex view especially developed in Boeckx and Theofanopoulou (2014). After presenting these perspectives, we concentrated on the works of Poeppel and Embick (2005) and Embick and Poeppel (2015) to analyze the way in which these authors proposed the relations of levels (reduced to cognitive and neurological ones). We have made some critical remarks on these proposals intending to show the difficulties from the perspective of the philosophy of science.

Afterwards, we concentrated on a specific sub-theory of MP: the syntactic theory. First, we looked at it from a theoretical view, and then as a theory-element of MP conceived as a theory-net (following the metatheoretical structuralist view). We presented its central components, relations and law-statements. After this presentation, we summarized some information about the neurology of the brain which has led some authors to think about the relations between linguistics (as part of cognitive sciences) and neurobiology.

Finally, we intended to shed some light on the so-called "explanation level problem" beginning with an elucidation of how 'explanation' can be conceptualized from the philosophical perspective of structuralism. Then, we went back to Poeppel and other's perspectives of relations between cognitive and neurobiological results of studies in order to: (1) offer some criticism regarding the problems that emerge within that view (2) design a strategy based on building big areas of theories (holom theories) and inter theoretical relations among them. The elucidation of "explanation" and "levels of explanations" taking 'theories' as central entities was also of key importance. One of our aims for future works in this area is to take the syntactic theory as part of MP and to carry out the programmatic strategy sketched in this work.

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Theoricity and Testing

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1. Introduction

In the classical conception of scientific theory held mainly within the framework of logical empiricism, the distinction between theoretical and observational concepts played a central role. In the 1970s, several authors began to support the idea that such an absolute distinction should be replaced by a distinction relative to a theory, which would leave aside observability and deal with theoricity by focusing on the different role that concepts play in a specific theory. One of the most influential proposals was made by Joseph Sneed (1971), who can be considered the founder of metatheoretical structuralism (Balzer, Moulines, and Sneed 1987), who proposed to replace the theoretical/observational distinction by the T-theoricity distinction. This second distinction leaves aside observability to pay attention only to the way in which the concepts of a theory are determined. T-theoretical concepts would be those that can only be determined by appealing to the theory T, whereas T-non-theoretic concepts can be determined independently of T.

This distinction was extremely fruitful within the framework of metatheoretical structuralism in reconstructing scientific theories. Specifically, it allows us to clearly show how, when testing scientific theories, it is indispensable to appeal to other underlying theories that allow us to determine

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Abreu, C. (ed.) *Philosophy of Science in the 21st Century. Contributions of Metatheoretical Structuralism*.
Florianópolis: NEL/UFSC, 2023, pp. 93–115.

the concepts of the "empirical basis" of T independently of T itself. Even with the outstanding conceptual sophistication that philosophers of science have achieved in the move from the naïve theoretical/observational classical distinction to the theoretical/non-theoretical relatively-to-a-theory distinction, there remains a metatheoretical knot which must be disentangled:

i. The identification of theoretical and explanatory terms;
ii. The identification of non-theoretical and theory-testing terms.

Santiago Ginnobili and Christián Carman (2016) were concerned with showing that the T-theoreticity/T-non-theoricity distinction proposed within metatheoretical structuralism does not coincide with the explanatory/non-explanatory role that concepts have in a theory (i.e., they argued against the first of the above-mentioned identifications). Following this path, in this paper, we will deal with the second identification. We will do three things: (i) Introduce an account of theory testing inspired by the structuralist metatheory, and with it, a new distinction between the concepts that can be used to test a theory (which we call T-testing concepts) and those that cannot (T-non-testing); (ii) argue that the T-testing vocabulary of a theory T does not necessarily match its T-non-theoretical vocabulary; and (iii) provide a few examples to show this, and how the different distinctions interact in explanation and testing.

To do all this, we will proceed as follows. In the second section, we present a brief history of the conceptual identification between theoricity/explainability, on the one hand, and non-theoricity/testing-capability, on the other. We also review how the structuralist metatheoretical program has served to sophisticate and specify the notion of theoricity, emphasizing the most recent contributions that allow us to disaggregate this notion from that of explanatoriness. In the third section we introduce the distinction of T-testability and argue that it coincides neither intensionally nor extensionally with the distinction of T-theoricity nor with that of T-explainability. In addition, we will show how this new distinction allows us to better explain how the testing of a theory works. To illustrate this, in the fourth section, we present two cases of theories that contain terms that are T-non-theoretical and T-non-testing at the same time: natural selection theory and cladistics. The fifth section will introduce some nuance to the considerations introduced before. We will distinguish between strongly T-testing concepts (what we focus on in this article) and weakly T-testing concepts. Section 6 adds some general philosophical considerations and consequences of our approach. Finally, we draw some conclusions.

2. Structuralism and the Refinement of the Theoricity Criterion

The received, syntactic or classical view in the philosophy of science established a very influential distinction within scientific language, between the observational vocabulary, whose sentences are composed exclusively of observational concepts, and the theoretical vocabulary, whose sentences contain at least one theoretical concept.[4] This distinction allows, in the classical conception, to

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[4] If they contain only theoretical concepts, those sentences are said to be "pure", or "theoretical" proper. In the classical metatheory, laws contain only these kinds of sentences. If a sentence contains both theoretical and observational terms it is called "mixed". Mixed sentences act as a bridge between the theoretical laws and the observational terms describing observed "facts", i.e., they give theoretical concepts an empirical meaning. See below for more on this.

account for the way in which theories are tested (through the hypothetical deductive method) and for how explanation works (through the nomological deductive model of explanation). The idea, specifically, is that the introduction of theoretical concepts makes it possible to more adequately *explain* the behavior of certain phenomena described through observational concepts, and that observational concepts make it possible to *test* the theoretical statements made within the framework of a theory.

It is straightforward to understand why the classical philosophers of science, being empiricists, considered that the theoretical/observational distinction performs this dual work. In Carl Hempel's words: "[s]cientific systematization is ultimately aimed at establishing explanatory and predictive order among the bewilderingly complex 'data' of our experience, the phenomena that can be directly 'observed' by us" (Hempel 1958, p. 41). In his view, those "empirical facts", "data of our experience" or "phenomena that can be directly observed by us" must take the form of *observational statements* (i.e., statements that refer only to observable entities). Laws, on the other hand, may contain concepts that refer to non-observable (i.e., theoretical) entities. Conversely, these laws and theories must be tested by comparing their observational consequences with the (observational) statements we accept, based on our observations. Thus, the empiricist account of both explanation and theory testing depends crucially on the distinction between these two supposedly independent vocabularies, which would have distinct functions in science: observational vocabulary would serve as an independent *judge* for our theories, while theoretical vocabulary would be used to *explain* the data we collect by observing the world that surrounds us.[5]

The theoretical/observational distinction has been widely criticized, primarily by "historicist" philosophers of science, e.g., Feyerabend (1962), Hanson (1958) and Kuhn (1962). These authors formulated their criticisms in different ways, but the common thread among them is the position that all observation (and observational language) is *theory-laden*. Even our most immediate experience is already "conceptualized". So, according to them, there is no theory-independent firm basis of observational statements from which we can judge the adequacy of our theories. Furthermore, the "conceptual scheme" we use to "organize" the sense data is not fixed/universal. We can learn to see things in a certain way. Scientific education, these critics would say, consists in large part of educating our perception (Hanson 1958). But, if theories can teach us to see what we use to test them, then there is a risk that theory testing becomes circular. In other words, if the concepts of the empirical basis of a theory T are loaded by T, then every test of T will have to presuppose this theory, in order to conceptualize the "empirical" phenomena used to build the sentences with which we test it.

These objections pushed philosophers to abandon the observational/theoretical distinction, but the problem of circularity remained. In the paper in which he abdicates the standard conception of scientific theory, Hempel (1970) proposes an alternative solution, by modifying the theoricity criterion in two aspects. Firstly, he made the distinction relative to particular theories (i.e., a term may be theoretical in one theory and non-theoretical in another). And secondly, by replacing the idea that the empirical basis of theories must be described in *observational* terms with the condition that it be described using only *previously understood* (and therefore interpreted) terms.

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[5] Strictly speaking, in the classical metatheory, observational and theoretical *statements* perform those functions. Concepts, by themselves, do not explain or act as judges of anything. We will continue to speak of explanatory, explained, testing, etc. concepts, in an indirect sense, by considering the roles that the statements (or theoretical structures) formulated with them play.

The (now called) *antecedent* vocabulary of a theory T may thus be loaded with theory and not be observational, but the theories with which it is loaded must be *temporally* prior to the formulation of T—and hence, applicable independently of T.

Similarly (although independently, since he had not read Hempel's paper), Sneed (1971) proposed the T-theoretical/T-non-theoretical distinction. Like Hempel's, the structuralist criterion is relative to particular theories. Also, like Hempel's, it postulates that T-non-theoretical terms have to be *prior* to T. The difference lies in what Sneed understands by "prior". According to him, what matters is not *temporal* precedence, but rather *operational* precedence.[6] That is, T-non-theoretical terms are those that can be operationalized or determined independently of T, while T-theoretical terms are those that *require* using the laws of T to be operationalized. As we shall see below, the possibility of independent operationalization is what guarantees that theories can be tested against something independent of themselves, avoiding the circularity issue.

More precisely, a term *t* is T-non-theoretical if and only if there exists a determination method for it that does not presuppose T; a term is T-theoretical if and only if *every* determination method for it presupposes T. Put simply, *determining* a term means finding out its denotation. A *particular* determination is said to be "T-dependent" if it uses the laws of T to find out the denotation of *t*. A determination *method* for a term *t* is a way of systematically determining the denotation of *t*. A determination method will presuppose a theory T just in case every one of its instances (every particular determination following this method) is T-dependent, since the whole procedure will thus presuppose that T is true/empirically adequate/justified.

The structuralists have also developed a second criterion of theoricity, sometimes referred to as the "formal" criterion (Balzer 1983; 1985; Balzer, Moulines and Sneed 1987; Gähde 1983). To avoid confusion, we will call the terms that are T-theoretical in this second sense "T-determinable" (the reason for this will be clear in a moment), reserving the term "T-theoretical" for the first criterion. A term *t* is T-determinable if and only if there exists a determination method for it that presupposes T (i.e., a T-theoretical determination method). That is, T-determinable concepts are those that *can* be operationalized by using the laws of T. T-non-determinable concepts are those that cannot—i.e. those that *have to* be operationalized by appealing to something outside the theory in question (for some application examples see Section 4, as well as Balzer, Moulines and Sneed (1987, pp. 73-78)).

The structuralist T-theoricity distinction is a notable sophistication over previous accounts and, more importantly, it has been applied in the reconstruction of countless scientific theories from different scientific disciplines. However, in the standard structuralist account, there remained a link between T-non-theoretical/T-explained/T-testing vocabularies, on the one hand, and T-theoretical/T-explanatory/T-non-testing vocabularies, on the other. This identification is similar to the one present in the classical observational-theoretical distinction: T-theoretical concepts are introduced to explain the behavior of phenomena described by means of T-non-theoretical terms,

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[6] In many cases, these two distinctions will in fact coincide since the vocabulary that is temporally previous to some theory T will also tend to be operationally independent from it. However, it is possible for a researcher to, *at the same time*, recognize a phenomenon to be explained and give an explanation for it. More importantly and frequently, as Hempel (1970) himself notes, there are cases where some *term(s)* are previously available, but the meaning of the concepts they denote change, making the application of the criterion very difficult in practice (how much conceptual change is necessary to consider that we are not dealing with the same concept?). With Sneed's criterion these problems do not arise.

and T-non-theoretical concepts allow the theory to be tested. In more technical terms, the global empirical basis of a theory coincides with its global *explanandum*.[7]

One clear instance of this identification can be found in the following passage by Díez:

> We will call T-testing, or T-non-theoretical, or T-empirical vocabulary, that part of the characteristic vocabulary of the theory T used in the description of the "T-data", that is, of the "phenomena" of which the theory wants to account for (explain/predict) [...] We will call T-explanatory, or T-theoretical, the characteristic vocabulary of T that is not T-testing vocabulary, that is, the concepts used in the formulation of the laws of T, which cannot be determined/measured without presupposing the validity of any of these laws. (Díez 2012, pp. 68-69, our translation)

Let us begin with the identification of the first two respective vocabularies (theoretical and explanatory, non-theoretical and explained). Although structuralism *per se* does not provide an account of scientific explanation, there are some conceptions of explanation that take the structuralist metatheory into consideration (Bartelborth 1996; Díez 2013; Forge 2002), since knowing how theories are logically structured is extremely relevant for understanding how they explain phenomena.

Take for example Díez's account, known as *ampliative, specialized embedding*. Succinctly, in this view, *explananda* are conceptualized as data models of the form $DM = \langle D_1, \ldots, D_n, f_1, \ldots, f_i \rangle$ where the Ds are domains of objects and the fs are relations and functions over those domains. Explaining a phenomenon means embedding its DM representation into a theoretical model TM, such that: (*i*) DM is a substructure of TM (i.e., TM contains every domain, relation and function in DM, plus some others); and (*ii*) TM satisfies some theoretical laws, which restrict the possible interpretations of the concepts, and in that way make some of the *explananda* phenomena expected. The details of this proposal, which include some additional criteria to distinguish between adequate and inadequate explanations, do not matter here. What matters to us is that, according to Díez's initial view (Díez 2002), every new concept that TM introduces to account for DM must be T-theoretical.[8] Thus, as one can readily see, the T-theoretical concepts are the ones that play explanatory roles (and in that sense can be called "T-explanatory"), while the T-nontheoretical concepts figure exclusively in the *explananda* of theory T (and therefore can be called "T-explained").

This identification of the T-theoretical and T-explanatory vocabularies was put into question by Ginnobili and Carman (2016), also in structuralist terms. These authors argued, firstly, that the T-theoretical status of a concept could change over time without its explanatory role changing. For example, scientists could find some new ways of operationalizing "mass" and "force" independently of classical mechanics. If this happened, "mass" and "force" would become T-non-theoretical for classical mechanics but their explanatory role over the movements of particles would remain the

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[7] The notion of "global empirical basis"—proposed by Pablo Lorenzano (2012)—would be analogous to the notion of "empirical basis" used in the classical conception, in the sense that it is a concept relative to a theory in general and not to its particular applications. And, in the same sense, the "global *explanandum*" of a theory should be understood as a way to speak of the systems whose behavior theory intends to explain, and not to refer to particular explanations (Ginnobili and Carman 2016).

[8] Later on, and partly due to discussions with Ginnobili and Carman (see below) he weakened this point, and only demanded that at least one new concept be T-theoretical (Díez 2013).

same within that theory. The only reason to think otherwise would be an *a priori* identification of T-theoretical and T-explanatory vocabularies. Secondly, and perhaps more importantly, these authors showed that there are clear and convincing examples of theories that, to account for their global *explanandum*, expand conceptually by appealing to *both* theoretical and non-theoretical concepts, in some cases, and *only* to non-theoretical concepts in others. In both cases T-non-theoretical concepts can figure as part of the T-explanatory concepts of the theory.

In the following sections we go deeper into Ginnobili and Carman's path and show that, in the same way that the distinctions between T-theoretical/T-non-theoretical and T-explanatory/T-explained concepts are not the same, the T-testing/T-non-testing distinction is also independent of both of them.

3. Theory Testing and T-Testing Vocabulary

In this section, we provide a sketch of an account of theory testing inspired by the structuralist metatheory (and which, we believe, is implicit in a lot of structuralist practice), that will allow us to characterize more appropriately the distinction between T-testing and T-non-testing concepts, and to show how this distinction is not identifiable with the other two distinctions introduced above.

Put simply, a particular test of a theory consists in a pair of determinations of a term t, one of which is T-dependent and the other is T-independent. In other words, to evaluate a theory one must determine (at least) one term by using a determination method that presupposes T and another that does not presuppose T. The test is said to be successful if both determinations result in the same value for t (or in a value that is close enough given some standard of approximation), and unsuccessful otherwise.[9]

To illustrate this in a simple manner, consider the following example. In classical genetics (CG),[10] the non-theoretical level is comprised of (among other things) the traits of the organisms of the breeding population, and their proportions. That is, one can determine the frequencies of traits in a population without applying the laws of classical genetics. For instance, if the trait being investigated is the height of a particular species of plant, then there exists a determination method that does not involve using the laws of CG (e.g., using a ruler) for determining plant height.[11] On the other hand, determining gene and genotype frequencies *requires* applying the laws of CG. That is, the *only way* of knowing that an organism has a given genotype, or a population a given distribution of genotype frequencies, is to apply CG itself.[12] In a typical case of application, one begins with a set of trait frequencies for two subpopulations at t_1 (say, a distribution of plant heights); one then postulates a given genetic architecture (*loci*, allele-types, etc.) and establishes what

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[9] This would correspond to what, in section 5, we will call a *strong* test. There may be other types of tests. See below for more on this.

[10] Structuralist reconstructions of CG, and treatments of the issue of T-theoricity there, can be found in Balzer and Dawe (1997) and Lorenzano (1995).

[11] In this particular case, the trait looks "observational" in the sense that measuring something with a ruler does not seem to use any theory. But we could have chosen a trait that requires some more independent theorizing to be determined (e.g., blood type, which does satisfy mendelian inheritance).

[12] Or at the very least, this was the only way of knowing that at the time Mendel proposed his laws, and at the beginning of the twentieth century.

the trait frequencies would look like at t_2 if that were the case—i.e., one performs a CG-theoretical determination of trait frequencies. Lastly, one measures plant heights at t_2—i.e., performs a CG-non-theoretical determination of trait frequencies—and sees if the value coincides with the CG-theoretically determined value. If it does, then the postulated values for genotype frequencies are taken to be adequate, if it does not, then they are discarded. It is also easy to see that, in the first case, this would not only confirm that a given genotypic architecture is present, but also represent a confirming case for CG. On the other hand, if the values did not coincide, then we would be in front of a Kuhnian unresolved puzzle, which, if failed to be subsumed after repeated attempts—e.g., after postulating different possible genetic architectures—, could become an anomaly.

To put our account in enunciativist terms, testing a theory would involve making a derivation for *both* the theoretical and non-theoretical determination methods.[13] That is, instead of thinking of theory testing as an instance building an argument and then “observationally” checking whether the conclusion holds, the picture would look more like building two arguments:

$$\begin{array}{l} L_1, \dots, L_n \\ C_1, \dots, C_m \\ \hline E_1 \end{array} \qquad\qquad \begin{array}{l} L^*_1, \dots, L^*_i \\ C^*_1, \dots, C^*_j \\ \hline E_2 \end{array}$$

Where $L_1, \dots, L_n / L^*_1, \dots, L^*_i$ are (two different sets of) laws, one of which includes laws of T and the other of which does not, $C_1, \dots, C_m / C^*_1, \dots, C^*_j$ are (different sets of) initial conditions, and E_1/E_2 represent the occurrence or non-occurrence of the same phenomenon. This would further illustrate the holistic character of theory testing (i.e., theories are never tested against “raw” experience, but only against a background that includes other theories). Also, as in the classical account, our perspective assumes that the laws of the theory perform the function of connecting independent areas of our experience. In our case, the laws perform this function within the T-dependent determinations that use them.

With all this in mind, it is straightforward to see which part of the vocabulary of a theory is relevant for testing it. If testing a theory consists in determining a concept both theoretically and non-theoretically, then the concepts that can be used for that purpose are those that can be determined in both ways. In terms of the distinctions introduced above, a concept will be said to be T-testing if and only if it is T-determinable and T-non-theoretical. On the other hand, a concept will be T-non-testing if it is either T-theoretical or T-non-determinable. This distinction is interesting because, as we will show in the next section with concrete examples, some concepts are both T-non-theoretical and T-non-determinable (and thus T-non-testing). Put more simply, that there are cases where some part of the empirical vocabulary of the theory is not relevant for testing it. Consequently, even though the T-theoretical vocabulary is always T-non-testing, the theoricity distinction does not collapse with the testability distinction because the vocabulary that is used to test a theory may be a proper subset of the empirical (non-theoretical) vocabulary. To put it graphically, the relations between T-(non)-theoretical, T-(non)-explanatory and T-(non)-testing

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[13] If we equated determining a concept with making an inference, which is doubtful. At least some accounts of determination methods would not agree here. For example, Roffé (2020c) recently proposed that determination methods should be thought of as algorithms, and following an algorithm and making an inference are two different things.

vocabularies would look as follows.[14]

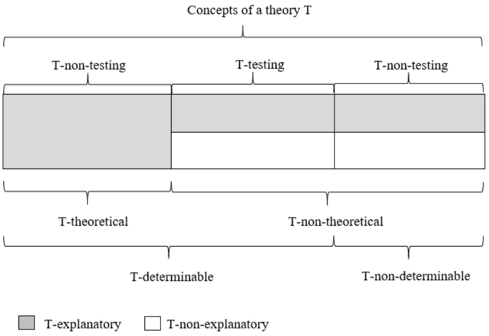

Figure 1: Relations between the three distinctions.

Before moving on to the examples just mentioned, it is worth making some technical precisions to our proposal. In the standard structuralist account (e.g., the one presented in Balzer, Moulines and Sneed 1987) the T-non-theoretical language of a theory T is represented as a class of models M_{pp} or "partial models", which results from eliminating the T-theoretical concepts from the class of "potential models" M_p (which represents the entire language of the theory). The intended applications (I) of the theory are presented as a subset of the M_{pp}. If Ginnobili and Carman are right, then I is not a subset of M_{pp} (since the representation of the global *explanandum* phenomena may not contain all the T-non-theoretical terms). Rather, one may introduce a class of models M_e as a substructure of M_{pp}, containing only the explained vocabulary, and I will be a subset of M_e. In that way the distinction between T-non-theoretical and T-explained vocabularies is captured formally. In our case, if we call M_d the class of models that contains the denotations of the T-determinable concepts (another substructure of M_p), then the class of T-testing models (call it M_t) can be obtained as the substructures of M_p that contain the concepts present in both M_{pp} and M_d. Visually:

$$\begin{tikzcd}
 & & M_e \arrow[r, "\subseteq"] & I \\
 & M_{pp} \arrow[ur, "\sqsubseteq"] \arrow[dr] & & \\
M_p \arrow[ur, "\sqsubseteq"] \arrow[dr, "\sqsubseteq"'] & {} \arrow[r, phantom, "\sqcap"] & M_t & \\
 & M_d \arrow[ur] & &
\end{tikzcd}$$

Figure 2: New proposed landscape of the structuralist classes of models.

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[14] It may surprise the reader that we include a space for T-explanatory and T-non-determinable concepts (which must then also be T-non-theoretical, since all T-theoretical concepts are T-determinable, see section 5). That is, we reserve a space for concepts that the theory adds to account for its *explananda*, and that cannot be determined by the theory itself. The next section will also give some examples of this.

4. Cases

In this section we present two theories as cases of application of our distinction, which contain terms that are at the same time T-non-theoretical and T-non-testing, namely, natural selection theory (NST from hereafter) and cladistics (CLAD from hereafter). Before going into that, a brief consideration of the strategy we will employ may be useful going forward. The key for upholding that those terms are T-non-testing will be to defend that they are T-non-determinable. The *prima facie* obvious way of arguing that a term t is T-non-determinable would be to show that, for any arbitrary model of the theory, even if the denotations of all the other terms are known, the laws of the theory would still not allow us to infer a univocal denotation for t, and hence that there would always be at least two possible values for it that render the laws true. However, things are more complicated than that, because a concept (e.g., a domain D) might figure as part of the signature of another concept (e.g., a function $f : D \to X$). In such cases, it would be logically impossible to know the extension of f but not of D. Hence, the way to argue that a concept c is T-non-determinable will be to show that, for any arbitrary model of the theory, even if the denotations of all other terms *that do not depend logically on* c are known, the laws of the theory would still not allow us to infer a univocal denotation for t.[15]

In what follows, we provide informal (or semi-formal) reconstructions of NST and CLAD and argue mostly informally about the T-non-determinability of the terms in question. This will be enough for, at least, giving some plausibility to our theses. A fuller, more formal and more rigorous examination of the cases below would be desirable, but falls outside the scope of this writing (the respective formal reconstructions of both theories can be found in Ginnobili (2012; 2018) and Roffé (2020b; 2020d)).

4.1. Natural Selection Theory

Ginnobili and Carman (2016) appeal to NST to show that explanatory conceptual ampliation does not always appeal exclusively to theoretical concepts. Here we will appeal to the same theory to show that not all NST-non-theoretical concepts are NST-determinable, and therefore, that there are NST-non-theoretical terms that are NST-non-testing.

The nature and structure of the theory of natural selection have been an important topic of discussion in the philosophy of biology. For the point we want to discuss we will rely on Ginnobili's structuralist reconstruction of it (Ginnobili 2010; 2011; 2016; 2018).

Darwin proposed the theory of natural selection to explain how populations of organisms evolve adaptively. That is, how they acquire traits that allow them to succeed in the environments in which they live. For example, the explanation of how certain birds have acquired a coloration pattern that camouflages them in their environment is as follows: In the past, there was a population of birds with different colors of plumage. Those that were more similar to the place where they fed were longer-lived, because predators visually confused them with the background, and left more offspring that in turn carried that trait. Generation after generation, new variations emerged that increased the resemblance of the pelage with the environment and these were spread by

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[15] A concept c_1 is logically dependent of another c_2 if and only if it is *logically* impossible to know the extension of c_1 without knowing the extension of c_2 (for example, because c_1 is a function that has c_2 as a domain).

having a greater reproductive success, until we reached the current population (Darwin 1859, pp. 84-85). This historical explanation (in the sense that it appeals to changes over many, many generations) appeals to iterations of what Ginnobili considers the fundamental law of the theory, which schematically states that in a given generation, those organisms that carry certain traits will have greater reproductive success.

Here we will not include the debatable details of the reconstruction offered. Suffice it to point out that, according to Ginnobili (2016, pp. 18-19), the fundamental law of the theory would be the following (we modified slightly it to fit our terminological uses):

[(The trait t_1 is more effective than trait t_2 in performing function f in environment e → organisms that possess t_1 are fitter than those who possess t_2 in e) and t_1 and t_2 are inheritable] → the organisms that possess t_1 will be more successful in differential reproduction than those that possess t_2 in e.

Where t_1 and t_2 are two variants of a trait-type T—e.g., 1 m and 1.10 m for the trait-type of the length of the neck of a giraffe—, f is a particular biological function—e.g., reaching the higher branches of trees—and e is a particular environment—e.g., the African savanna in a period of scarceness.

Although we are not going to present the full formal reconstruction of NST (for that see Ginnobili (2012; 2018)), it is useful for the purposes of our work to list of some of its fundamental concepts.

- O is a set of organisms of a given population to which NST is applied.
- T (=$\{t_1, t_2, \ldots t_n\}$) is a set of trait-variants of the same trait-type.
- E is the set of different environments, and e is a distinguished individual (a particular environment) within it.
- *DESC* is a function that assigns a trait to a particular organism.
- H (heritable traits) is a subset of T. Ginnobili (2012; 2018) introduces it from a definition and not as a primitive concept.
- F is a set of (biological) functions, and f is a distinguished individual within it.
- *EFEC* is a 4-ary relation that establishes an order in the effectiveness with which a pair of traits perform a function. $EFEC(t_1, t_2, f, e)$ symbolizes that t_1 is more effective than t_2 in performing f in environment e. It is a comparative concept, not a metric one.
- *FIT* (fitness) establishes an order among of different kinds of organisms of a generation in the particular environment in which they are found. It is also a comparative, non-metric concept.
- *RS* (reproductive success) establishes an order among of different kinds of organisms of a generation in the particular environment in which they are found. Ginnobili (2012; 2018) does not introduce it as a primitive concept, but rather as being defined from the mathematical language presupposed by the theory.

What this theory explains is why certain types of organisms in a population have greater reproductive success than others. The intended applications of the theory, therefore, are organisms in a population that differ in the possession of a trait and that differ in their reproductive success in a given environment. Thus, e, E, O, T, RS and $DESC$ allow us to describe the global *explanandum* of the theory (M_e in the previous section). The explanation consists in pointing out that organisms that possess a certain trait, which performs a function more effectively, improve their fitness in that environment, thus improving, if the trait is heritable, their reproductive success. The concepts with which the intended applications are conceptually enriched, and are thus T-explanatory, are: *f*, *F*, *H*, *EFEC*, *FIT*.

Ginnobili and Carman (2016, following Ginnobili 2011) argue that this is at least a case of mixed conceptual extension, because the functional attribution, the effectiveness with which a function is fulfilled, and the heritability of the trait can all be determined outside of NST. They even argue that this could be a case of purely non-theoretical explanatory conceptual extension, arguing that the concept of fitness could be considered as NST-non-theoretical, since its different specifications can be determined independently of the theory. For our purposes it is not necessary to discuss this point. What we must ask ourselves is which of the non-theoretical concepts appearing in the fundamental law of NST are NST-determinable, and consequently, serve to test NST, i.e., are NST-testing.

The most obvious prediction that can be made with the theory has to do precisely with the determination of its *explanandum*. That is, with predicting or retrodicting which kinds of organisms will have greater reproductive success. Reproductive success (as a comparative concept) can be ascertained in an NST-theoretical way and can also be determined in a non-theoretical way (since one can simply count the number of organisms of each kind that are present in each generation). Therefore, it is a T-testing concept. Even when it is not a traditional way of testing the theory, it is possible to think of NST-theoretical determinations of heritability. If we had all the other concepts of the law determined we could find out whether a trait is heritable or not by determining whether the trait affects the reproductive success of its possessors. The same can be said of the effectiveness with which the function is performed. Having all the other concepts of the law determined, we can establish—perhaps in a non-deductive way—which variant of the trait best fulfills its function by determining the reproductive success of its possessors.

What about the rest of the concepts? There remain *e*, *E*, *f*, *F*, *O*, *T*, *DESC*, and *FIT* to consider. Let us leave aside the question of the determinability of the concept of fitness, which would involve a discussion beyond the scope of this article.

The functional concepts *f* and *F* are also somewhat controversial, and the answer depends on the account of functions one adopts. For instance, some people have argued that the function(s) of a trait is just the effect(s) that it has been selected for in the past (Millikan 1989; Wright 1973). Ginnobili's reconstruction is incompatible with this approach since the theory contains functional notions, and consequently that definition of function would be circular. Here, we will assume this reconstruction to be adequate. If that is the case, then the notion of function (more specifically, the concept that states that a certain trait fulfills a certain function) would be NST-non-determinable, and consequently, NST-non-testing. There is no way to find out from NST what the function of a *feature* is. This is knowledge that comes from physiological and behavioral studies. It is important to note that this does not depend on reconstructive decisions made. As we presented the conceptual framework, what we must say is that the distinguished element *f* cannot be determined from NST (i.e., we cannot know which, out of all the possible functions of the trait, is the relevant one). However, we could have introduced a specific concept for functional attribution. In that case we would say that it is impossible to determine that concept. The point is that, as we said, it is not possible to perform the functional attribution only by considering NST.

Moving on to other concepts that can be found in the law, the terms that represent the environment, particularly *e* (the actual environment), is clearly T-non-determinable. Firstly, as the vocabulary is presented above, it is a domain in *EFEC*, *FIT* and *RS*, so the question would be if knowing what the organisms and their (heritable) traits are, as well as what the function at stake is, would be sufficient for establishing what the selective environment consists of. And the answer is obviously no. For instance, knowing that in a particular application of NST there is a population of giraffes, some with longer and some with shorter necks (both heritable) and that the relevant function is feeding, does not permit us to infer that the environment is one in which the leaves are high and the food is scarce. It might have consisted in any number of other scenarios. For instance, it might have been the case that giraffes with longer necks could submerge their heads deeper in the water and catch algae or fish better, or that they could see farther away to find food sources, etc.[16]

Another clear case is the function that assigns traits to organisms (*DESC*), which allows us to partition the population into varieties and consequently, to predict differences in reproductive success among these varieties. One could argue that knowing what the relevant traits are and which is more effective, and seeing which particular organisms are having greater reproductive success could allow us to assign traits to organisms. But this has at least two problems. Firstly, the law only states that one *kind* of organism has greater reproductive success than the other, not that every individual organism of one kind has greater reproductive success than every individual organism of the other kind. However, *DESC* assigns traits to individual organisms. Secondly, and more importantly, *RS* implicitly contains *DESC* in its definition, since the kinds of organisms that have differential reproductive success are defined as groups of organisms that share the possession of a common trait (which presupposes *DESC*). Therefore, *DESC* can in no way be determined from NST for a particular organism.

Finally, *O* and *T* are also not very clear cases of T-non-theoretical and T-non-testing concepts. Not because they could be T-determinable. They clearly are not, since almost every other concept in the theory depends on them (has them as a domain), and thus without having both of them determined we cannot determine almost anything else. This is a common characteristic of the most basic domains of theories (think, for example, about the set of particles in classical mechanics). In that way, they are clearly T-non-testing. What is doubtful is whether they should be considered T-non-theoretical. We will expand on this in the next section.

If this analysis is correct, NST would be a case where none of the above distinctions is coextensive. There are explanatory NST-non-theoretical concepts (which are not part of the global *explanandum*) and NST-non-theoretical concepts which are not determinable and therefore are not part of the testing basis of the theory.

4.2. Cladistics

Even though Darwin's most well-known development is NST, it is not the only theory he proposed, nor the one he considered to be his most important contribution. While the iteration over many generations of NST explains the presence of adaptations, it does not account for the possession of certain *structurally* similar (and sometimes functionally very dissimilar) traits that biologists call *homologies*. A famous example is the tetrapod limb, which has approximately the same bones, arranged in approximately the same pattern, in a wide variety of species (for example, in humans,

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[16] These considerations would also hold even if *EFEC*, *FIT* and *RS* did not have *e* as a domain (i.e., if we had chosen to somehow present the vocabulary and the laws in a different way).

bats, whales and moles), even though they serve for widely different purposes in each (grabbing, flying, padding and digging).

Prior to Darwin, one popular explanation was that organisms were structurally similar in this sense because God created every species (or at least every vertebrate) parting from a common plan or *archetype* (see for example Owen 1847; 1849). Darwin realized that the archetype was not an idea in the mind of a God, as Owen had thought, but an actual ancestor (Darwin 1872, p. 384). Thus, the fact that organisms share homologies is indicative of, and indeed *explained by*, the fact that they share a common ancestor. At some point in time, some subpopulations of this ancestor became reproductively isolated and their traits evolved independently by adapting to the local environments, but preserving, however, the general structure of the ancestor's trait. In this way, the evolution of life on Earth can be depicted by a tree that parts from a single root species and subsequently divides into the rich diversity of species found today.

Moreover, even at that time, it was obvious to biologists that some organisms share some homologies among themselves that they do not share with other organisms, and that this pattern is nested. For instance, all spider species share some homologies that scorpions do not; and in turn, spiders and scorpions share many homologies that shrimps do not. The darwinian explanation for this is that the most recent common ancestor of spiders is not an ancestor of scorpions (i.e., spider species diversified among each other later than they did with the ancestor of all scorpions), and the same goes with spiders + scorpions and shrimps. In other words, a particular tree (a subtree of the entire tree of life) explains the particular (nested) *distribution* of homologies among these species.

However, not everything is as easy as it may seem from the paragraphs above. Many times, homologies (in the sense of structurally similar traits) do evolve independently in separate lineages, i.e., *convergence* does occur in nature.[17] And this, in turn, can obscure what the phylogenetic relations between a set of species are. Consider for example the following very simplified example:

Species/Character	C_1	C_2	C_3	C_4
S_1	1	1	1	0
S_2	1	1	0	1
S_3	0	0	1	1
S_4	0	0	0	1

Table 1. Example data matrix (homology distribution scheme) for four species S_1-S_4 and three homologous characters C_1-C_4 , each with two alternative states codified by 0 and 1.

Here, according to characters C_1 and C_2, S_1 and S_2 share a common ancestor that is not an ancestor of S_3 and S_4 (and vice-versa, since the latter two share state 0). However, according to C_3, S_1 and S_3 have an ancestor that is not an ancestor of S_2 and S_4 (and vice-versa). C_4, on the other hand, seems to suggest that S_2, S_3 and S_4 have a common ancestor that is not one of S_1. In other

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[17] This is sometimes expressed by saying that *primary* homologies—i.e., structurally similar traits—are not always *secondary* homologies—traits inherited from a common ancestor—(see for example Blanco, Roffé and Ginnobili 2020; Pearson 2010; 2018; de Pinna 1991; Roffé, Ginnobili and Blanco 2018). Here we stick to the terminological choice of using the term "homology" for primary homologies, though we intend nothing of weight with this.

words, we have (among others) the following three possible hypotheses of relatedness:

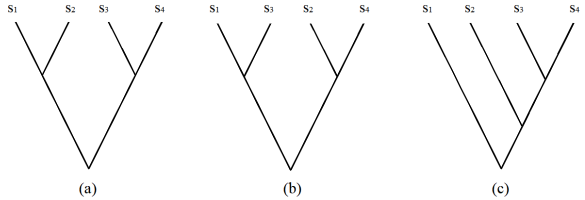

Figure 3: Three of the fifteen possible trees for 4 taxa.

The question is, how do we choose among them? How do we decide which character state(s) are the convergences and which are not? In Darwin's time there was no systematic procedure for doing this. This is what can be achieved with the methods of cladistics.

In the cladistic methodology, each character is mapped into each tree to see how many evolutionary changes one would need to postulate to account for the currently "observed" distribution. The score or *length* of a tree is simply the sum of the changes needed to account for every character under consideration. In the example from Figure 3 above, the length of tree (a) is 5, while the lengths of (b) and (c) are both 6, making (a) preferable to them—one out of all the possible optimal character mappings is shown in Figure 4 below.

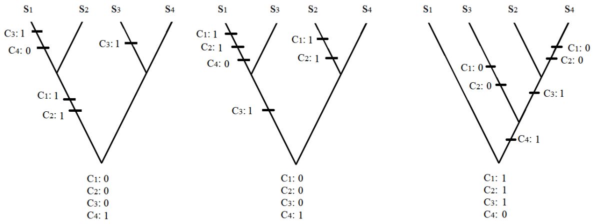

Figure 4: Character optimizations for the three trees shown in the figure above.

Notice that, despite first appearances, character C_4 is actually uninformative for phylogenetic purposes, since it accommodates equally well in every possible tree. In trees (a) and (b) it suffices to postulate that the root ancestor had state 1 and that its state changed in the branch leading to S_1 (and we could have even done the same in tree (c)).

The tree that is inferred as the actual one, among all possible trees, is simply the minimum length tree (or one of them, if there is more than one). There are many complications to the general sketch just presented (for more complete presentations of the theory the reader may see Kitching *et al.* (1998) and Wiley and Lieberman (2011)) but the above will be enough for our purposes.

Moving on to how to reconstruct this theory, its concepts can be formally represented in the following way.

- T, a set of taxa (the set of species whose phylogenetic relations will be inferred).
- A, a set of trees, such that each tree is a graph containing the members of T as leaves, and satisfying some formal properties (each tree is directed, acyclic, rooted, etc.). Note that A is univocally defined for each T.
- $a_R \in A$, a distinguished individual (a particular tree) that represents the actual evolutionary history (the one we wish to infer as the correct one).
- C (= $\{C_1, ..., C_j\}$), a set of characters, such that each character might be further thought of as a set of states (i.e., $C_i = \{s_1, ..., s_i\}$).
- $DESC$, a function that assigns a state for each character C_i to each taxon in T. Note that T, C and $DESC$ together comprise the data matrix shown above in table 1.
- LEN, a function that takes an input tree and a character assignment and computes the length of the tree.

Given all this, the fundamental law of CLAD would simply state that, given a set of taxa T, of characters C and an assignment $DESC$, the actual evolutionary tree a_R is among the minimum length (LEN) trees (once again, this is a semi-formal and very simplified version see Roffé (2020d)).

As said above, what cladistics explains is the "observed" distribution of homologies (i.e., shared character states) among a set of taxa. Thus, the CLAD-explained vocabulary would consist of T, C and $DESC$. To account for this distribution, the theory extends that vocabulary with a set of trees, a function to compute the length of such trees and the actual tree. This is the CLAD-explanatory vocabulary.

Of course, the explained concepts T, C and $DESC$ are CLAD-non-theoretical; the data matrix is typically built previously and independently to the beginning of the phylogenetic cladistic analysis (in fact, we will argue that it is always built that way).[18] The actual evolutionary tree a_R is CLAD-non-theoretical as well. Even though in the usual cases of application it cannot be determined independently of CLAD because the relevant evolutionary events are in the deep past, this is only an empirical limitation not a conceptual one. There are, in fact, applications to experimental phylogenies where the actual tree is known independently (for more on this see Hillis *et al.* (1992) and Roffé (2020b)).[19]

A and LEN have a more confusing T-theoricity status. At first glance, one might think that these are the T-theoretical concepts of the theory. However, they are both *defined* functions. For instance, given a set of taxa, the set of trees is automatically determined as the set of all possible graphs with certain mathematical characteristics. Thus, the determination of trees and lengths does not seem to presuppose the fundamental law of CLAD, and the extensions of both can be determined for any possible set of taxa and assignments, even if the law were false (the resulting minimum-length tree did not coincide with the actual one).[20] In that sense, even if their definitions

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[18] See Roffé (2020a) for a fuller discussion of this point in the context of dynamic homology.

[19] This characteristic is common of many other theories. Some particle trajectories may also be in the past, for example, and thus only be determinable CM-theoretically. That does not mean that trajectories are theoretical for classical mechanics, since there are other applications in which trajectories can be determined non-theoretically. That a concept is T-non-theoretical does not mean that it should be determinable independently if T *in every application* of the theory. This point, however, tends to confuse philosophers of systematics (and systematists themselves) more than physicists, since actual trees are *almost always* in the past.

[20] If, however, one included the definitions as part of the fundamental law, this would not hold. This does not seem like a very reasonable option though.

are part of the theory, they seem to be CLAD-non-theoretical, and we would again be in presence of purely T-non-theoretical conceptual extension.

Going back to the main subject of this paper, to answer which of these concepts are T-non-testing, we need to examine which are T-determinable. The most obvious T-testing concept is a_R. As said above, it can be determined independently of the theory, and it can be determined with it by finding the minimum length tree and applying the fundamental law.

A and *LEN* can be determined simply by applying their definitions. If this counts as a CLAD-theoretical determination is, once again, doubtful, and we will not discuss it here in greater length.

One case of a clearly T-non-theoretical and T-non-testing concept is the function *DESC*, the assignment of states to the terminal taxa. That it is T-non-testing stems from the fact that it is T-non-determinable. One could think that there are some applications in which having the data matrix and a_R determined does induce a unique assignment of states to the terminal taxa. For instance, given a character C_1 comprised of two states, 0 and 1, and the following tree:

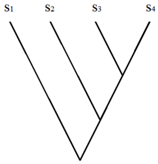

Figure 5: Actual tree which could be thought to (but does not) result in a univocal assignment of states to taxa.

One might think that the only possible interpretation is the one that assigns 0 to S_1 and S_2 and 1 to S_3 and S_4. However, the reverse is also possible, also with a minimum length of 1. And it is easy to see that the same procedure (inverting the 0s and the 1s) can be done for each possible assignment, obtaining another one that yields the same length for every possible tree. Since every change (from 0 to 1 and from 1 to 0) counts the same, the corresponding trees will have the same length.[21] Furthermore, this is not just a change of codification/scale. Even if one fixes in advance what a 0 and a 1 mean (for example, the absence and presence of a given morphological structure, respectively) one cannot know solely by applying CLAD which taxa have the feature and which do not.

Finally, *T* and *C* are as organisms and traits in NST, they are the most basic domains of CLAD and one cannot do anything with the theory without them (such as determining assignments, lengths, trees, etc.). Therefore, they are CLAD-non-testing as well.

In the next two sections, we consider other ways of testing theories, and consider some additional general points and suggestions, and finally, in Section 7, we draw some conclusions.

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[21] If there are more than two states, the procedure is analogous. Things start getting more complicated when one allows for non-uniform cost transformation schemes, but this is almost never done in phylogenetic practice, so we keep the uniformity assumption for simplicity.

5. Weak T-determination

In Section 3, we characterized a test of a theory as a pair of determinations of a term t, one of which is T-dependent and the other is T-independent. This would actually correspond to the strongest way in which a theory can be tested, which is when the theory makes a single theoretical prediction (i.e., a univocal T-theoretical determination) for the denotation of a term t. In that case, as we said above, we simply compare the predicted value (the T-theoretical determination) with the "observed" one (the T-non-theoretical determination). However, theories sometimes make predictions that are weaker than that, in the two following ways.[22]

Firstly, theories sometimes make "disjunctive" predictions, in the sense that they establish that the denotation of a given term t must be either d_1 or d_2 or d_3 or... So long as the set of values in that disjunction is a subset of all the possible interpretations of t (alongside the rest of the concepts) the theory is putting a restriction on what can happen and is thus making a prediction in some sense. Hence, we can characterize a term t as *weakly* T-determinable if, given the denotation of every other term that does not depend logically on t, the laws of the T restrict the possible interpretations of t. More precisely, if we take a concept c and the set C of all its possible interpretations in the $\mathrm{M_p}$, and the set ∂ consisting of the class of actual models M "cutting of" (the denotation of) the concept c, then c will be weakly T-determinable if and only if there is an $m = \langle D_1, ..., D_i, R_1, ... R_j, f_1, ..., f_k, c_x \rangle$ such that (*i*) $\langle D_1, ..., D_i, R_1, ... R_j, f_1, ..., f_k \rangle \in \partial$, (*ii*) $c_x \in C$, (*iii*) $m \in \mathrm{M_p}$, and (*iv*) $m \notin \mathrm{M}$.[23]

An example would be the concept *DESC* in cladistics, presented in the previous section. What our argument about inverting the 0's and 1's showed is that, given the denotations of all the other terms, there will always be at least two ways of assigning states to the terminal taxa which will make the actual tree be an optimal tree. However, this does not imply that *every* possible assignment will make the actual tree optimal. The set of assignments that do this will, in many cases, be a subset of that. In other words, there will be some possible assignments that the laws of CLAD rule out. Thus, *DESC* can be said to be strongly CLAD-non-determinable but weakly CLAD-determinable.

With this in mind, one could also think of introducing the notion of *weakly* T-testing concepts, as those that are T-non-theoretical and weakly T-determinable. A test using a disjunctive T-theoretical determination for the relevant term t could be called a weak test. Note that a weak test would still consist of a non-theoretical and a (weak) theoretical determination for some concept, so our conception of testing is still adequate. Also note that strong tests (those in which the theoretical prediction is univocal) are usually preferred to weak tests, since they allow stricter testing of theories (in Popperian terms, a theory that makes a single non-disjunctive prediction it has more potential falsifiers).[24]

There is a second relevant phenomenon, which we can also characterize as a weaker version of testing. Some theory could only put (disjunctive) restrictions on the denotations that a set of concepts can take, but not univocally determining the denotation that each particular concept must have. For a very simplified example consider a theory with the following law:

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[22] We thank José Díez for bringing these two phenomena to our attention.

[23] Again, we thank José Díez for his help in formulating this condition.

[24] In the same way, a test involving a finite disjunctive prediction would be preferrable to one in which the relevant concept can have an infinite number of values (while still being a subset of the values in $\mathrm{M_p}$)

(L) If the patient has fever and a sore throat, then she has the flu.

From the fact that Alice has a fever and a sore throat, one can determine that she has the flu. Thus, "having the flu" would be a strongly T-determinable theoretical concept. However, from the fact that she has the flu and has fever, one cannot establish that she must have a sore throat, because L only gives a sufficient condition for having the flu, not a necessary one. Thus, having a sore throat would not be strongly T-determinable in this fictitious theory. However, note that from the fact that Alice does not have the flu one can establish that she does not have both fever *and* a sore throat. This is a disjunctive restriction on the possible interpretation of both concepts that does not univocally determine the denotation of either of them.

What is interesting about our analysis from above is that making the concept of strong testing explicit allows us to subsequently characterize these (and possibly other) weaker senses of testing as well.

6. General Considerations

Before moving on to our conclusions, we can draw three additional considerations, to illustrate how these developments of metatheoretical structuralism can also be relevant to certain general discussions within the philosophy of science.

The first concerns theory testing. Since its inception, the central theme of structuralism has been the explication of the structure of scientific theories, and the way in which it changes over time. Undoubtedly, the reconstruction of theories and their links is relevant to an understanding of how the testing of theories is carried out. Structuralists usually state (informally) that the standard conception of theory testing, the HD method, must be sophisticated and improved, but by no means discarded. Some of the things that are typically said to need revision are the dependence of the HD method on the observational-theoretical distinction, the fact that it confuses theory testing with hypothesis testing, and the fact that what is tested in a deductive hypothetical way is not the fundamental law of a theory, nor any special law, but the empirical assertion.[25] However, a better account of testing has not yet been worked out in detail. To undertake this task, it will be fundamental to keep in mind the way in which the T-theoricity and T-determinability distinctions interact in testing, the proposed distinction between T-testing and T-non-testing concepts and the correct presentation of the global empirical testing basis.

The second general point concerns the criterion of demarcation. Since the beginnings of professionalized philosophy of science in the early twentieth century, the refutability or testability of scientific theories has been discussed as a criterion of adequate factual knowledge. In some cases, to distinguish it from pseudoscientific or metaphysical theories (Popper), and in other cases as a criterion of cognitive significance (logical empiricism). Since then, we have learned that the discussion in its beginnings was somewhat naïve, and that the criteria provided by the classical philosophers of science turned out to be too restrictive (almost all interesting science turned out to be pseudoscientific/metaphysical/cognitively meaningless). However, the questions these philosophers posed may still be relevant today. In particular, one may wonder if there are (or could exist) theories that, by their very conceptual constitution, are impossible to test. This question is

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[25] Which is a factual statement that states that a certain empirical system can be subsumed under one of the lines of specialization of a specific theory-net.

still interesting and could be relevant for those who want to continue to discuss the demarcation criterion in a more sophisticated way. The discussion carried out in this paper could help identify what a theory without a global testing basis (i.e., one without T-testing concepts) would consist of.

Take for example the case that is usually presented as an example of spurious unification: *What God wants to be the case is the case* (Kitcher 1881, p. 528). Kitcher's intuition is that the reason why this statement fails as a law has to do with the fact that it does not provide a genuine unification, and that this has to do with the stringency of the explanations provided. Díez (2002; 2014), continuing previous attempts to deal with explanation from a structuralist point of view (Bartelborth 1996; 2002; Forge 1999; 2002), deals with the question of spurious unification by pointing out the extreme malleability of abstract principles that lack special laws through which to increase their empirical content. For example, if we were to take the second principle of classical mechanics by itself, without the additional restrictions imposed by special laws, something similar to what happens with the principle above would occur. It would be possible to apply it trivially to any case we could imagine (the second principle, considered in isolation, is empirically unrestricted, as Moulines (1982), points out).

The discussion we carried out above allows us to collaborate with the characterization of what is wrong with the spurious statement provided. Let us take a more specific version of the given spurious law: *Organism x has trait y because God created it that way*. This statement has as its concepts *x possesses trait y* and *God created x with trait y*. We think it is quite intuitive to hold that the first concept would be T-theoretical (it would be impossible to determine what God wants independently of this law) and that the second would be T-non-theoretical, since it is possible to determine the possession of traits independently of the law. Note, however, that (in the absence of criteria restricting the divine will) it would not be possible to determine the possession of the trait from the law. This leads to the concept of trait possession being T-non-determinable (both strongly and weakly). The testing basis of the theory would be empty. And in this particular sense, the theory would not be testable (it would not be possible to theoretically ascertain the value of the non-theoretical concept), even when it has non-theoretical terms. This, which seems quite intuitive, had not been pointed out in the discussions regarding the spurious character of this type of statements. And it could collaborate with the elucidation of the sense in which the statement is irrefutable.

Additionally, and independently of the discussion about demarcation, this can be useful to collaborate with the structuralist approach to explanation. The standard characterization of special law given, for example, *Architectonic* (Balzer, Moulines and Sneed 1987, p. 170), is extremely weak (i.e., a law is a special law of itself). This discussion allows us to give an extra requisite that makes genuine special laws increase the empirical content of the theory, so that genuine testing becomes possible (the point made by Díez). The requirement (or one of the requirements) would be that what special laws must achieve is to provide procedures that allow theoretical determination for the non-theoretical concepts that appeared in the fundamental law. In other words, they must make some T-non-theoretical concept(s) T-determinable, thus allowing the theory to have at least one T-testing concept.

Finally, we can return to the issue of the status of the most basic domains of theories (such as organisms, particles, etc.). Although this is a more specific structuralist discussion, it does have wider implications for more general philosophical discussions (e.g., a lot of effort has been, and continues to be, devoted to understanding what an organisms is). As shown in the examples above, in many (if not most) cases, these concepts will turn out to be T-non-testing for their respective theories. Remember that a concept *c* will be T-determinable when, knowing the denotations of all other concepts that do not depend logically on *c*, the laws of T allow us to infer (either a univocal or a disjunctive) denotation for T. However, since almost every other concept will contain them as domains, then no other denotations will be known, and nothing will be inferable. This, however, is merely a conjecture that needs fuller examination in more applied work. What is doubtful, however, is if these concepts fit into our new category of T-non-theoretical and T-non-testing, because the former can be uncertain.[26]

7. Conclusions

Having gone a long way, we can now be more explicit about what was said in the introduction. The classical philosophers of science intended to account for the role that concepts had in explanation and in testing with a single distinction, by appealing to what, because of their empiricist attitude, they considered key: observability. Within the framework of metatheoretical structuralism, the question of the role of observation in science was separated (not because it is unimportant or because it has no role) from that of better understanding the functioning of the independent testability of scientific theories. A more sophisticated and fruitful distinction than the classical observational-theoretic one was then proposed, paying attention to theory-dependent and theory-independent criteria of determination.

However, the idea that independent testability was the key to understanding the role that concepts have in explanation persisted, as Ginnobili and Carman (2016) have argued. Even within structuralism itself other proposals emerged, which provided additional distinctions to Sneed's original one. This allowed these authors to have a more sophisticated account of both explanation and testing, and of the different roles that concepts can play in these. This did not imply a criticism of the T-theoricity distinction, but rather, an establishment of its proper scope and role, a condition of possibility to establish its usefulness.

In that vein, Ginnobili and Carman (2016) proposed the T-explainability distinction and argued that it does not coincide neither extensionally nor intensionally with that of T-theoricity. To hold this, they showed there are theories that conceptually extend their intended applications

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[26] It is debatable whether the T-theoreticity distinction applies to all concepts of a theory, since some concepts of some theories do not seem to be determinable through theoretical determination methods at all (neither T-theoretical nor T-non-theoretical). The most debated case is the concept of *particle* in classical mechanics, for there is no explicit theory that allows the application of this concept. Moulines (1991, p. 224) has argued that *particle* could come from a very elementary implicit theory that provides such application criteria. Falguera (2006, p. 76) has doubted that such a theory exists, suggesting that it is a concept that, being non-theoretical in classical mechanics, depends in some sense on its laws, since we consider particles to be those entities that follow the laws of classical mechanics. He characterizes such concepts as T-non-theoretical but with "theoretical charge" of T. Ginnobili, Carman and Lastiri have suggested instead that the distinction does not apply to such concepts since their application does not appeal to the laws of any theory as is usually the case (Ginnobili 2018, pp. 147-50; Ginnobili, Carman and Lastiri 2008). Such concepts seem rather to be used to refer to the domain of intended applications of the theory. This could be the case of concepts such as *O* in NST (which does not always apply to organisms in the strict sense). If this were the case, the T-theoreticity distinction would not establish a partition between the concepts of a theory. Some concepts might be neither T-theoretical nor T-non-theoretical in T. We leave the question aside for the sake of simplicity.

with non-theoretical concepts. However, we can make the conjecture (partly reasonable and partly based on the fact that we know of no case that refutes it) that every T-theoretical concept is T-explanatory.

In this paper we have tried to show how a third distinction, that of T-determinability (which could already be found in the standard literature of structuralism), interacts with T-theoricity in the testing of a theory. We have also attempted to show that not every T-non-theoretical concept is T-determinable. This allowed us to introduce one additional distinction, T-testability, which permits us to talk more precisely about the global testing basis of a theory.

Additionally (although this was not our main goal), we have shown that not every T-explanatory concept is T-determinable. The picture of the situation, then, is quite complex (see Figure 1 in Section 3). As is often the case, the theoretical or metatheoretical frameworks that we propose to account for certain phenomena are usually too simple in their origin (because it is rational to begin by thinking that the phenomena we want to account for are simple) and tend to become more complicated and sophisticated as time goes by, in the development of the program.

Acknowledgments

This work has been financed by the research projects PUNQ 1401/15 (Universidad Nacional de Quilmes, Argentina), UNTREF 32/19 80120190100217TF (Universidad Nacional Tres de Febrero, Argentina), PICT-2018-3454 y PICT-2020-SERIEA-01653 (ANPCyT, Argentina), UBACyT 20020190200360BA (Universidad de Buenos Aires, Argentina) and PIUNAHUR-2021 *La incorporación de la perspectiva de género en la enseñanza de la biología en el marco del Profesorado Universitario en Biología de la UNAHUR*. We would like to thank José Díez for his valuable comments on previous versions of this paper.

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Scientific Explanation as Ampliative Specialized Embedding: New Developments

José Díez[1]

1. Introduction

As is well known, the first detailed, dominant account of scientific explanation was elaborated by Hempel and collaborators in the 1950s and 1960s. According to that Hempelian account (Hempel and Openheim 1948; Hempel 1965), to explain a phenomenon consists, roughly, of deductively or inductively inferring it from antecedent conditions and nomological regularities that connect those conditions to the explanandum. To explain consists of making the explanandum *nomologicall y expectable* from antecedent conditions. In deterministic explanations (such as the specific deviation in the orbit of a satellite explained by the near pass of an asteroid with a specific mass and trajectory, and the law of gravitation and Newton's Second Law) the expectability is "total", and the correspondent inference is *deductive*, thus we have the Deductive-Nomological (DN) model for deterministic explanations. In indeterministic explanations (such as Peter's respiratory problems explained by his smoking two packets of cigarettes daily for the last thirty years and the statistical, non-accidental regularity that the majority of such heavy smokers end up with respiratory problems) the inference is "partial", and the correspondent inference is *inductive*, thus we have the Inductive-Statistical (IS) model for indeterministic explanations.

This Hempelian model suffered from a series of deficiencies which made it soon criticized. The main trouble came from different kinds of counterexamples that proliferated in the literature and that challenge the necessity or the sufficiency of Hempel's conditions. At the general level, the most discussed counterexamples challenge the sufficiency of Hempel's analysis: there are

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Abreu, C. (ed.) *Philosophy of Science in the 21st Century. Contributions of Metatheoretical Structuralism*.
Florianópolis: NEL/UFSC, 2023, pp. 117–135.

nomological inferences which satisfy Hempel's conditions that clearly do not qualify as possible[2] explanations. Famously, these include the following cases (some of these examples are deterministic, some indeterministic, but one can easily find both kinds in all of these cases). Symmetries: one can nomologically infer the height of a tower from its shadow and the inclination of the Sun using some laws of optics; but this is not a possible explanation of the height of the tower, that is to say it does not explain why the tower has the height it has. Forks: one can nomologically infer a storm from the sudden drop of a barometer reading in the area, together with the non-accidental regularity that almost always a storm follows such a barometer reading drop; nevertheless, it is clear that the drop of the barometer reading does not explain why the storm occurs. Time reversal: one can nomologically infer the existence of one's parents from one's own existence and certain biological laws, although our existence of course does not explain our parents' existence. Irrelevances: one can nomologically infer that John is not pregnant from his ingesting birth control pills and the non-accidental generalization that nobody who takesbirth control pills gets pregnant; but surely John's non-pregnancy is not explained in this way.

Together with these and other counterexamples that affected the general sufficiency of the analysis, other examples challenged the necessity of Hempel's conditions for indeterministic cases. As we have seen, according to this model an indeterministic explanation is a valid/strong inductive argument, but in valid inductive arguments the premisses make the conclusion highly probable (as in the example of Peter's respiratory problems), which excludes as non-explanatory cases in which the premisses increase the probability of the conclusion but do not make it very high. In the famous paresis case, it seems to be a good explanation of the Major's paresis that he contracted syphilis and did not get treatment, and that it is a non-accidental regularity that 25% of untreated syphilis sufferers develop paresis. Here the explanans makes the explanandum *more* probable (than without it), but not *highly* probable. Therefore, it seems that to (implicitly) be a valid inductive inference is not a necessary condition for probabilistic explanations.

Besides these general insufficiencies that may affect any scientific area, the Hempelian model received additional, specific criticisms from philosophers of particular scientific fields, who argued that, no matter how well the nomological expectability model could work in some areas, it does not work in their specific field, with large areas in biology, psychology and social sciences being the main objectionable examples of such non-Hempelian realms they adduced.

The Hempelian account, suffering from these and other problems (see Salmon (1989) for a detailed analysis), was thus soon assessed as inadequate. These problems motivated the development of alternative proposals, mainly unification (Friedman 1974; Kitcher 1983; 1991) and causal (Scriven 1975; Lewis 1986; Salmon 1984; Humphreys 1989; Woodward 2003; Strevens 2008) models, including the recent new mechanicism among the later (Mahamer, Darden and Craver 2000; Glennan 2002; Bechtel and Abrahamsen 2005), and the statistical relevance model for the specific case of probabilistic explanations. The general idea of the causal account is that to explain a phenomenon consists in referring to (the contextually relevant part of) its causal history. The new-mechanistic account is a specific version of causalism, in which causal explanations are construed as specifications of parts together with their properties, functions and causal interac-

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[2] The difference between possible and actual/good explanations is that in the latter, the premisses of the inference are true/correct/accepted, but the conceptual analysis provides conditions for both: what the analysis provides are the conditions for being a possible explanation, i.e., for being considered a good candidate for a scientific explanation; if in addition the explanans is true/correct, the explanation is "factually correct".

tions that give rise to the phenomenon. Causalism, though, seems, for the well-known Humean reasons, to be metaphysically dubious to strict empiricists, who in general favoured unification accounts that try to fix Hempel's counterexamples while preserving empiricist strictures, that is, which avoid primitive metaphysical notions, such as causation, that are, according to them, as unclear as the notion of explanation we are trying to elucidate, if not more so. The main intuition behind unification approaches is that to explain a phenomenon consists of reducing the number of principles and assumptions necessary to account for or predict it.

The causal and unification alternatives, though, have their own problems. With regard to causalism, although causation is arguably sufficient for explanation, it is hardly necessary since one may find bona fide explanations in different scientific fields whose causal nature is far from clear (several social sciences, areas of psychology, functional biology, quantum mechanics and relativistic mechanics, among others; see Ruben (1990) for a summary). As for unification, it is neither necessary nor sufficient. Unification does not always have explanatory import, for there may be merely descriptive or phenomenological unificatory laws on which the expectations/predictions do not have any explanatory import (e.g., Kepler's laws). On the other hand, some bona fide explanations can hardly qualify as unifying; for instance, Newton's gravitational explanation of free fall on the Earth's surface cannot (taken alone) qualify as unifying, but this does not undermine its explanatory value (which may, of course, *increase* after incorporating it into the whole unifying Newtonian theory).

Recently, Díez (2002; 2012a; 2012b; 2014), elaborating on some ideas from Sneedian structuralism (see Balzer, Moulines and Sneed 1987; Bartelborth 1996; 2002; Forge 2002), has argued that it is possible to fix counterexamples to Hempel's account without moving to either causalism or unificationism, but sticking closely to Hempel's strictures. Díez claims that his neo-Hempelian approach can qualify as a general theory of scientific explanation that is applicable all across scientific practice and may later be supplemented with additional (causal manipulativist, causal mechanistic, unifying, etc.) features in specific fields.

2. A neo-Hempelian account of scientific explanation as ampliative specialized embedding

Ampliative specialized embedding (ASE) preserves the core of Hempelian nomological expectability, though formulated within a model-theoretic framework with the notion of *nomological embedding*. The basic idea is that explaining a phenomenon consists of (at least) embedding it into a nomic pattern within a theory-net (see Díez 2014 for details). Now explanandum and explanans are certain kinds of models or structures: the data model DM = $\langle D_1, \ldots, D_n, f_1, \ldots, f_i \rangle$ one wants to explain, and the theoretical model, TM = $\langle D_1, \ldots, D_m, g_1, \ldots, g_j \rangle$, which must involve at least the same kinds of objects and functions (but can introduce new ones: more on this crucial point soon), and which is defined by the satisfaction of certain laws. In the Classical Mechanics (CM) Earth-Moon case, for instance, the explanandum is the data model that represents the Moon's actually measured spatiotemporal trajectory around the Earth, and the explanans model is the mechanical structure including masses and forces and satisfying Newton's Second Law and the law of gravitation. We explain the Moon's trajectory when we embed it in the mechanical system, that is, when we obtain the Moon's kinematic trajectory from the mechanical model. "Embedding" here means (if we simplify and leave idealizations aside for now) that the data model is (or is isomorphic to a) part of the theoretical model. In a Mendelian Genetics case, the explanandum model describes certain transmission of phenotypes, e.g., for peas, and the explanans model includes genes and satisfies certain genetic laws. The transmission of traits is explained when one embeds the specific known pattern of transmission of traits into the theoretical model, that is, when the observed phenotype distribution is identical to (or isomorphic to a) part of the full genetic model. The basic idea is that if the explanation succeeds, then we find that the data we wish to explain as part of the theoretical model defined by certain laws. That is, if things behave as those laws stipulate, then we should find some results at the data level; and when the actual data coincide with the expected results, embedding is establishedand the explanation succeeds.

As made apparent by the last sentence, this account preserves the *nomic expectability* idea. The embedding provides the expectability part; for, if the embedding succeeds, one may "expect" to find the explanandum data as part of the theoretical model. This expectability, though weaker than Hempel's inferentialism as it does not demand that explanations must be logical inferences *stricto sensu* (so it is not subject to the "explanations are not inferences" criticism), nevertheless also has room for both deterministic and probabilistic (including low probability cases, if needed) explanations, depending on whether the regularities that define the explanans models are deterministic or probabilistic. The nomic component comes from the fact that the theoretical model that embeds the explanandum model is defined by the theoretical structure which satisfies certain laws (understood merely as non-accidental generalizations). As Díez (2014) emphasizes, this sense of nomological explanation is quite modest, meaning just that the explanans model satisfies certain non-accidental generalizations, no matter how ceteris paribus, local, or domain restricted they are. On the other hand, the explanandum data model involves only T-non-theoretical entities,[3] so it is measured without using the laws that define the theoretical model, that is, the T-explanandum model is determined/measured independently of T-laws, which guarantees that the intended embedding is not trivial and may fail.

This general idea is augmented with two additional conditions for the embedding to fix the problems encountered by classical Hempelianism. For the nomological embedding to be explanatory, it must be *ampliative* and *specialized*. As our mechanical and genetic examples illustrate, the explanans model must include additional ontological (in metaphysical terms) or conceptual (if one prefers a more epistemic formulation) machinery, with respect to the explanandum model. In the mechanical case, the explanans includes, together with kinematic properties, new dynamic ones, namely masses and forces, which behave with the former properties as the mechanical laws establish. In the genetic case, the explanans model includes, together with the phenotypic properties, new genetic ones, genes or factors that behave with the former properties as the genetic laws establish. This ampliative character of these embeddings is what explicates their explanatory nature, compared to other embeddings that lack explanatory import. In the Keplerian case, for instance, we also have nomological embedding, that is, expectability based on non-accidental regularities; but this embedding does not qualify as explanatory because the embedding model (defined by Kepler's laws) does not introduce any new conceptual/ontological apparatus with respect to the

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[3] As is usual in structuralism literature, a concept/entity is T-theoretical iff it *can only* be measured/determined by making use of T-laws (e.g. masses and forces in CM); and it is T-non-theoretical otherwise, i.e., if it can be measured/determined without using T-laws (although it *can* also be measured/determined using T-laws) (e.g., length and duration in CM).

embedded model: both models include only kinematic properties. The same applies to the genetics example: if we take purely phenotypic statistical transmission regularities as defining models that embed certain phenotypic data, again we have nomological embedding with no explanatory import. Nomological embedding without ontological/conceptual ampliation is not explanatory.

The second additional condition is that the ampliative laws used to define the explanans model must be "special" laws, and not merely schematic or programmatic principles. This distinction originates in Kuhn's difference between "generalization-sketches" and "detailed symbolic expressions". Kuhn's distinction is famously expressed in the following passage:

> […] generalizations [like $f = ma$…] are not so much generalizations as generalization-sketches, schematic forms whose detailed symbolic expression varies from one application to the next. For the problem of free fall, $f = ma$ becomes $mg = md^2s/dt^2$. For the simple pendulum, it becomes $mg\text{Sin}\theta = - md^2s/dt^2$. For coupled harmonic oscillators it becomes two equations, the first of which may be written $m_1d^2s_1/dt^2 + k_1s_1 = k_2(d + s_2 - s_1)$. More interesting mechanical problems, for example the motion of a gyroscope, would display still greater disparity between $f = ma$ and the actual symbolic generalization to which logic and mathematics are applied. (Kuhn 1970, p. 465)

This Kuhnian idea has been elaborated in detail by meta-theoretical Sneedian structuralism with the notions of *guiding principles* and their *specializations* in a *theory-net* (see, e.g., Balzer, Moulines and Sneed (1987; 2000), for several examples).

Most theories are hierarchical net-like systems with laws of different degrees of generality within the same conceptual framework. Often there is a single fundamental law or guiding principle 'at the top' of the hierarchy and a variety of more special laws that apply to different phenomena. Fundamental laws/guiding principles are kind of "programmatic", in the sense that they establish the kind of things we should look for when we want to explain a specific phenomenon, and the general scheme that specific laws must develop. It is worth emphasizing that general guiding principles, taken in isolation, without their specializations, say empirically very little: they are too unspecific to be tested in isolation. In order to be tested/applied, fundamental laws/guiding principles have to be specialized ("concretized" or "specified") by specific forms that, in the Kuhnian sense referred to above, specifying some functional dependences that are left partially open in the laws further up in the branching system.

The resulting structure of a theory may be represented as a net, where the nodes are given by the different theory-elements, and the links represent different relations of specialization. For instance, the theory-net of CM has Newton's Second Law as the top unifying nomological component, i.e., as its fundamental law or guiding principle (Moulines 1979; Balzer, Moulines and Sneed 1987), which can be read as follows:

CMGP: For a mechanical trajectory of a particle with mass m, the change in the quantity of movement, i.e., $m \cdot a$, is due to the combination of the forces acting on the particle.

This **CM** guiding principle at the top becomes specialized down, opening up different branches for different phenomena or *explananda*. This branching is reconstructed in different steps: first, symmetry forces, space-dependent forces, velocity-dependent forces, and time-dependent forces; then, e.g., the space-dependent branch specializes into direct and inverse space-dependent forces; the direct space-dependent branch in turn specializes into linear negative space-dependent forces, etc., while the inverse space-dependent branch specializes into inverse-square space-dependent forces, etc.; then at the bottom of every branch we have a completely specified law that is the version of the guidingprinciple for the specific phenomenon in hand: pendula, planets, inclined planes, etc. (Kuhn's "detailed symbolic expressions").

The theory-net of **CM** looks (at a certain historical moment) as follows (only some, simplified, terminal nodes are shown here, which suffices for my present goals, for a complete presentation see Balzer, Moulines and Sneed (1987); at the bottom, in capitals, there are examples of phenomena explained by the branch):

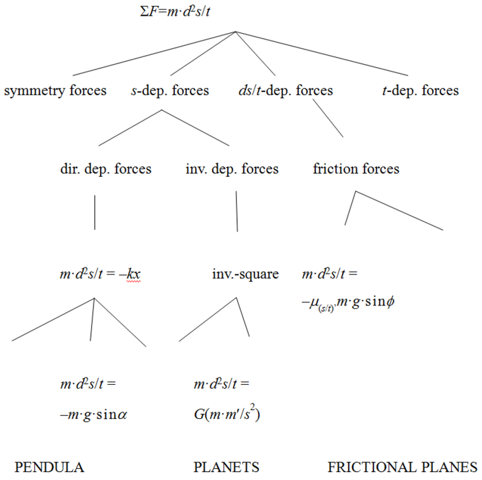

Figure 1

Now we can spell out the second additional condition for an embedding to be explanatory, namely, that the embedding must be *specialized*. The basic idea is that among the (non-accidental) generalizations that define the explanans model, at least one must be a specialization, i.e., the explanans model cannot be defined exclusively using general guiding principles. The reason for this is the programmatic character, and its corresponding empirical weakness, of these fundamental laws when they are not accompanied by their specializations. Given this empirically restricted character of guiding principles, if you only use one of them in the explanans, without a specific specialization, then the embedding becomes trivial, empirically void. Take, for instance, Newton's second law, $\Sigma F=m\cdot d^2s/t$, alone, without any specific systematic constraint on the kind of functions, f, that we can make use of, no matter how crazy these functions could be, then, as Díez points out, "with just some mathematical skill we could embed any trajectory" (2014, Section 2.2); even for weird trajectories, such as that of the pen in my hand moved at will: with enough purely mathematical smartness one could determine the f_1, f_2, ... that embed it. Or take what can be considered the general guiding principle of Ptolemaic astronomy: "For every planetary trajectory there is a deferent and a finite series of nested epicycles (with angular velocities) whose combination fits the trajectory". As has been proved (Hanson 1960), any continuous, bounded, periodic trajectory may be so embedded. The moral, then, is that for a nomological embedding, even an ampliative one, to be properly explanatory and not merely an *ad hoc* trick, the explanans model must be specified using some special law in the sense referred to here.

One could then object that we might find bona fide explanations only in highly developed unified theories of a net-like structure, which seems counterintuitive since, as we noted above, there are bona fide explanations that can be quite isolated. This is true. However, although it is also true that the notion of special law is particularly clear in relation to a general principle in the framework of a theory-net, it is not true that special laws can exist exclusively within a highly developed theory-net. The law for harmonic oscillators, for instance, is a special law no matter when it was discovered or whether it was integrated into a bigger, unified theory-net (in several theories, some special laws are formulated even before a general guiding principle is explicitly formulated, see, e.g., Lorenzano (2006) for the case of Classical Genetics).

3. Applications

When ASE was proposed, the main examples referred to were from physics, namely Classical Mechanics and Thermodynamics. ASE, though, is not confined to physical theories, inasmuch as outside physics we also find ampliativeness and specializations. It is of special interest that in the last few years ASE has been applied successfully to several biological cases, for biology was, according to opponents of the classical Hempelian account, a field where Hempelian nomological explanations could not be found. This is not the place to respond to the main objections against nomological explanations in biology (see, e.g., Beatty 1995; Mitchell 1997; 2003), it suffices my present goals to proceed via exemplification, by mentioning the successful applications of ASE to theories belonging to biological studies. The main ones are as follows.

The first example is Natural Selection (NS) (Díez and Lorenzano 2013; 2015), commonly understood as a theory that aims at explaining why a specific (hereditary) trait (e.g., giraffes' long necks, moths' black wings, etc.) spread—or become reduced—in a given population within a specific biological context. As we know, the standard evolutionary explanation is, roughly, that the trait in question spreads (becomes reduced) because in that biological context it facilitates (hinders) the performance of a function that is beneficial for differential reproduction. Here the general guiding principle is something along these lines:

A transmissible trait t that in acontext C facilitates the performance of an adaptive function/behaviour f that is beneficial for differential reproduction, increases, *ceteris paribus*, its frequency in the population in C.

This has the unspecific or schematic nature that we have seen is characteristic of guiding principles: they need to be further specialized in order to be applicable to the explanation of specific phenomena. In this case, the specializations specify, for the trait/explanandum in question, the function that is beneficial for differential reproduction that is facilitated by the trait in the given context. For giraffes' long necks, for instance, the function is food supply. For moths' black wings, the function is escaping predators. And so on and so forth. The family of such specifications or specializations has the structure of a theory-net, with different specifications grouped in different layers. For instance, the functions may facilitate survival, or mating, or fecundity, etc. Survival functions may more specifically facilitate food supply (e.g., the case of long necks), or escaping predators, etc. Mating functions may, more specifically, facilitate sexual attraction, or compete with sexual rivals, etc. Escaping predators may in turn specialize into escaping by running, or escaping by camouflaging (e.g., the moths' black wing), etc. And so on and so forth. As becomes apparent, the explanation of every trait appeals in its explanans to new elements, in this case a function that is beneficial for differential reproduction, that are nomologically (i.e., non-accidentally) connected with the trait in a specific way, namely, the trait facilitates the performance of the function in the biological environment. This brief summary suffices to show how adaptive explanations proceed via introducing "new stuff" and nomologically connecting it to the explanandum via non-accidental generalizations (no matter how local, in this case) of a "specific" (not merely schematic) form.

This example comes from macrobiology, but the same pattern may be found in microbiology, for instance, in allosteric theory (Alleva, Díez and Federico 2017a; 2017b). Monod-Wayman-Changeux allosteric theory (MWC) focuses on a particular regulation of biochemical activity: allosteric regulation (Monod, Wyman and Changeux 1965). Here what we want to account for is the specific pattern (hyperbolic, sigmoidal, etc.) of protein activity that varies with the quantity of substratum present. The characteristic MWC explanation consists of relating the biological response to modifications in the spatial structure (conformation) of the protein. MWC was initially constructed mainly for enzymes and haemoglobin, but later the activity of other important biological proteins, such as transmembrane receptors or membrane channels, was similarly explained. The complete details go beyond the scope of this paper, but the main idea is as follows. Such "oligomeric" proteins are constituted of certain sub-units: its protomers. Protomers present "sites" where the substratum or ligand can "bond". MWC aims at explaining the change in oligomeric protein activity in terms of the binding state of the sites on their protomers. Depending on these factors, the protein may be in one of two conformational states: "tense", with a low affinity for substrates; or "relaxed", with a high affinity. The nomological, non-accidental regularities postulated establish when proteins are in one or other of these states, how they can change from one to the other depending on the states of their protomers, and how the protein activity changes depending on the proportion of the states.

In this case, the formal guiding principle is too complicated to be presented here, but what we have seen suffices to grasp the main idea: the velocity of protein activity depends on the states of the sites of their protomers, whether they are bound, and how these protomer states interact and change. Again, this is a very schematic principle that becomes specialized when applied to different phenomena. The specializations specify, roughly, the kind of interaction between protomers (cooperative or non-cooperative) and the kind of ligands that bind to them (homotropic, heterotropic, etc.). Given this and the postulated nomological effect in terms of states, affinities, etc., a specific activity pattern/curve may be explained in each case.

So, MWC explanations are of the ASE form. The given explanandum, i.e., a particular activity curve, is explained by attributing to the phenomenon a series of additional structures, entities and properties (protomeric subunits, their sites, whether they are bound or not to ligands, their states and transitions, etc.) that are nomologically connected to each other and to the explanandum (activity velocity) in a specific/specialized way. The fact that in this case the explanation *also* has a mechanistic reading does not undermine my point. First, it is not the case that all aspects of such explanations may be mechanistically interpreted (Alleva, Díez and Federico 2017a). But, second and more importantly, even if they might be (or if in other cases they may be), this would be perfectly compatible with ASE. ASE proposes that all explanations proceed via ampliative and specialized embedding, but it is not incompatible with some (or even many) explanations being mechanistic. ASE is the general form of scientific explanations, compatible with some explanations being in addition mechanistic (or causal yet not mechanistic, or unifying, etc.). The problem could arise if all explanations were mechanistic (or causal) in which case it could then be argued that ASE is dispensable; but as the long debate on scientific explanation shows, this is far from being the case.

I will be briefer with my last two examples. The first comes from Classical Genetics (Díez and Lorenzano 2018; 2023). As is well known, Classical Genetics (CG) explains transmission patterns of traits or phenotypes from parents to offspring through reproduction in terms of "factors" or "genes" that are related to phenotypic traits and which are combined and transferred in reproduction according to certain nomological regularities. This general explanatory strategy is expressed by the CG guiding principle, roughly:

> The statistical communality of characteristics/phenotypes between parents and progeny is due to (i) the presence in parents of factors/genes, (ii) the transmision of factors from parents to progeny, and (iii) a determining relation between specific factors and specific characteristics, so that there is a specific correspondence between distributions of factors and distributions of characteristics.

This guiding principle was not explicitly formulated but rather was an implicit assumption in the explanatory practices of CG (Lorenzano 2000). This formulation makes its characteristic schematic nature apparent, for it leaves open the specific distributions of genes in parents, the associated phenotypes, and the specific form of the correspondence between genetic and phenotypic distributions. Every specific application of this general scheme specifies these parameters for the explananda in question. We do not need to enter into more detail to see that this explanatory practice follows the ASE pattern: a specific distribution of parents' phenotypes in progeny (e.g., pea colour, or Drosophila wings) is explained by postulated additional entities (genes) that are nomologically (i.e., non-accidentally) related to phenotypes in a *specific* manner such that we can successfully embed the explanandum into the ampliative, specialized explanans.

My last example comes from a specific area in ecology research: the one that explains how the dimension of a population in a given niche evolves (Díez and Suárez 2023). What is characteristic of this explanatory practice, such as the Lotka-Volterra predator-prey model, or the explanation by Coyte, Schluter and Foster (2015) of the stability of the microbiome, is that the changes in population dimension are explained by the interaction of two factors: an intrinsic growing ratio, and an extrinsic effect of the population's interaction with the environment, possibly including other populations. This, again, may be formulated as a schematic guiding principle of the form:

> For a species with a relative density X in an environment, the net change in this value over time is due to the combination of the internal growth ratio of the species, *f*, and the limiting role played by its interaction, *g*, with other species in the environment.

This schematic principle can be summarized by the equation "$dX/d_t = f$ (X) + g (...)", where X corresponds to the density of the relevant species, f to the intrinsic growth function, and *g* the "relation-to-the-environment (including other species) density-affecting" function. All these are open parameters that become differently specified for different explananda (e.g., the specific kind of mathematical function for growth; whether the interaction with other populations is competitive, cooperative or mutualistic; the number of species; etc.), giving rise to different concrete equations for different cases, e.g., the equations of Lotka-Volterra "$dX/d_t = -Xm + bhX$" or Coyte, Schluter and Foster "$\mathrm{dX/dt} = X_i \left(r_i - s_i X_i + \sum_{j=1, j\neq i}^{S} .a_{\mathrm{ij}} X_j \right) \; i = 1, \ldots, S$". It is apparent that these explanations also follow the ASE pattern: population dynamics is explained by appealing to additional stuff, such as internal growth, the presence of other populations, and certain properties of the niche, all connected *in a specific manner* via nomological, non-accidental regularities expressed by specific differential equations.

These examples of explanatory practice in biological research following the ASE pattern illustrate how ASE applies outside physics, and also that ASE is not subject to the criticisms addressed to classical Hempelianism with regard to its inapplicability to biological explanations. It is myclaim (that I cannot defend here) that ASE also fares better than its unificationist or causalist (including mechanicistwithin the latter category) accounts as a *general* account of scientific explanation. Admittedly, its gain in generality is at the cost of certain losses, for ASE is quasi-minimalist in the sense that it does not specify substantive conditions (i.e., causes, unification, etc.) for explanatoriness, but simply bears witness to the conditions that are in place in explanatory practices (ampliativeness, specialization) that distinguish explanatory from non-explanatory embeddings. This, though, is not non-elucidatory radical minimalism (such as minimalist theories of truth—e.g., Horwich (1990)—or of representation—e.g., Suárez (2015)), for it goes beyond mere platitudes and poses strong constraints on explanatoriness.

4. T-Explanatoriness, T-theoreticity and T-testability

In the initial presentations of ASE (Díez 2014; 2012a; 2012b) there was an implicit (and in some cases explicit) identification of T-theoretical concepts with the concepts that provide the explanatory import in T-explanations; and of T-non-theoretical concepts with the concepts that conceptualize the empirical phenomena T aims to explain or account for, i.e., T-testing base. These identifications, between T-theoretical and T-explanatory concepts, and between T-non-theoretical and T-explained/T-testing concepts, have been questioned from within the structuralist school by Ginnobili and Carman (2016), and by Roffé, Bernabé and Ginnobili (2021).

First, it is fair to acknowledge that such identifications were present in the initial work, and quite explicitly so in some of it, asin the following passage:

> We will call *T-testing*, or *T-non-theoretical*, or *T-empirical* vocabulary, that part of the characteristic vocabulary of theory T that is used in the description of the "T-data", that is, of the "phenomena" the theory aims to account for (explain/predict) [...] We will call *T-explanatory*, or *T-theoretical*, the characteristic vocabulary of T that is not T-testing vocabulary, that is, the concepts used in the formulation of the laws of T that cannot be determined/measured without presupposing the validity of some of those laws. (Díez 2014, pp. 68-69, my translation)

Let us consider both identifying relations in turn. Ginnobili and Carman (G&C) focus on the first identification and openly dispute it; or to be more precise, they dispute one of its directions. Assuming the ampliative character that ASE attributes to explanations, they accept that T-theoretical concepts (the concepts introduced by T that cannot be determined without using T-laws) have explanatory import, that is, the explanans makes use of them in embedding the explanandum. They question, though, the other direction, namely, that all the concepts that T-explanations introduce in the explanans and which are not present in the explanandum, are T-theoretical. They have two arguments against this: one conceptual and the other empirical.

The conceptual argument is that, even if as a matter of fact, extensionally in all known cases the explanatory concepts introduced by the explanans are T-theoretical, being T-explanatory does not intensionally imply being T-theoretical. The reason, they argue, is that, depending on the contingent evolution of science, some concepts that are T-theoretical at a given time *t*, might become T-non-theoretical at a later moment *t'* if a method of determination that is independent of T is discovered, but this would not destroy the explanatory character of the concept in question as used in T.[4] They do not provide an example, but rather refer to this as a possibility accepted by standard structuralism. They refer to a passage in Balzer, Moulines and Sneed (1987) where the authors accept that the concept of mass could be determined independently of the laws of classical mechanics, for instance if it were possible to count the number of atoms in a body. Actually, since different atoms may have different masses, the method would be a little more complicated, maybe (one might propose) counting the number of basic particles that constitute the atoms. I do not think this would work either, for different basic particles have different masses, so we would have to have determined beforehand the mass of the basic particles. Be this imaginary example as it may, let us for the moment concede, for the sake of the argument, that at least there seems to be no contradiction in a concept being T-theoretical at *t* and becoming T-non-theoretical at *t' > t*. Two comments are in place.

First, we have to be sure that we are dealing with *the same* concept. The same word does not always express the same concept. For instance, both classical and relativistic mechanics (RM) use the word 'mass', but it is commonly accepted that the concepts expressed by the word as used in CM and in RM, namely the concepts "mass_{CM}" and "mass_{RM}", are not the same concept. So, the fact that there are RM modes of determining mass does not imply that after the emergence of RM the *classical concept of mass* became CM-non-theoretical.

Secondly, even when they are one and the same concept, it is not necessarily the case that the new modes of determination change the theoretical status of the concept, in particular when intertheoretical relations, such as reduction, come into play. For instance, "mass" was Classical-Collision-Mechanics (CCM)-theoretical at a moment previous to the development of CM, and one may argue that the CM and CCM concepts of mass are indeed one and the same (if one does not accept this—and to me it is far from uncontroversial—then the previous comment about different concepts expressed by homophones applies). Do the new CM methods of determination that do not involve collisions, e.g., by use of a dynamometer, imply that the concept of mass ceases to be CCM-theoretical? I do not think so. CCM is reducible to CM, which means roughly that (if the concepts are the same, as we are assuming) the content of CCM "is part of" the content of CM, which I take as making room for us to consider the same concept as being intuitively T-theoretical in both theories. It is true that, in order to spell this intuition out in precise formal terms, we should

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[4] The "as used in T" is crucial, for the relativization to a theory is also essential for explanatoriness. The concept "pressure" is explanatory in mechanics, but may not be in thermodynamics.

modify the standard, simple definition of T-theoreticity by including qualifications when the methods of determination come from other theories with which T has particular intertheoretical relations (such as reduction). I am not going to try to provide such a modified criterion of T-theoreticity here, but I think that what is said suffices to make it plausible that, for a given T-theoretical concept, *c*, the appearance of a new, T-independent method of determination does not necessarily make the concept T-non-theoretical.

So, it is not that easy to make both precise and plausible the idea of a T-theoretical concept changing its theoretical status. I conclude that this first conceptual argument by G&C against all T-explanatory concepts being T-theoretical, is less persuasive than it could initially seem. This, though, still leaves open the possibility of finding T-explanations, part of whose ampliative apparatus is not T-theoretical. This brings me to their other, factual, argument: extensionally speaking, as a matter of fact, there are cases of theories whose explanations introduce in the explanans concepts that are not theoretical in the theory in point. G&C mention four candidates: the theories of Natural Selection, of the Common Origin, of Population Genetics and of Merton's Anomy. With regard to our present concerns, since in a sense I basically concede this point, I will just refer to the first example.

Following Ginobilli (2012), and similarly to my summary above, G&C take Natural Selection (NS) as explaining why a certain trait spreads as it does in a species in an environment through the trait being heritable and having more or less efficiency in performing a function whose better/worse performance improves/decreases differential reproduction. According to them, the concepts that NS introduces in the explanans of its NS-explanations are "function", "efficiency (of a function)", "heritable" and "aptness", and they argue that at least the first three are clearly NS-non-theoretical. Which functions are in place (food supply, escape from predators, attract sexual partners, etc.) is something clearly determinable without assuming the truth of the theory. And the same happens with the efficiency of a trait in performing a function; and with a trait being heritable or not. I concede this point and accept that a theory, T, may have ampliative explanations in which the ampliation uses concepts that are not T-theoretical but that come from other theories, such as "function". Actually, the first complete version of ASE (Díez 2014), and as a consequence of an exchange with G&C, already qualifies the previous versions and accepts that in cases with complex intertheoretical relations (such as NS), it maybe the case that the conceptual ampliation of the explanans uses concepts introduced by other theories. Nevertheless, Díez (2014) still demands that, *if* the explanation is an explanation of T, then at least some of the explanatory concepts introduced by the T-explanans must be T-theoretical. So, in a sense, it is still true that there is no T-explanatoriness without T-theoreticity (although other non-T-theoretical concepts may intervene).

G&C, though, resist this move and press the point further, claiming that there may even be theories in whose explanations, despite being ampliative, there is no T-theoretical concept at all; that is, theories that describe the explanandum with some T-non-theoretical concepts, and that explain such explanandum nomologically connecting the explanandum concepts with new concepts, but ones that are also T-non-theoretical (determinable without T):

> If we are right, and Wallace's theory, common origin theory, and even Darwinian theory of natural selection, are cases of genuine explanations in which there is conceptual explanation without appealing to any T-theoretical concept, perhaps we should further weaken the requirement, ceasing to demand that there be conceptual

> ampliation with at least one [T-]theoretical term, and simply asking that there be conceptual ampliation (pure, mixed or non-theoretical). This would not imply any loss for the ampliative embedding account of explanation. (Ginnobili and Carman 2016, p. 83)

Note that this means that the theory in question has no T-theoretical concepts, but is still explanatory. With regard to the examples, I very much doubt that NS is a good case, as I do not think that the concept "aptness" is NS-non-theoretical. But some of the other examples, if correct, would suffice for the point G&C wish to make. With regard to Wallace, apparently his adaptive theory explains without a notion of aptness, and restricts all intended applications to cases of survival (Ginnobili and Blanco 2010; Ginnobili 2011). If this is right, this could be such a case, although of a theory with restricted applicability or explanatory scope (actually, in some sense equivalent to part of the more general NS theory). As for the theory of common origin (CO), it explains certain homologies postulating common ancestors, with the explanatory concepts being introduced by the explanans "heritability" and "ancestry" (Blanco 2012), which according to G&C are clearly CO-non-theoretical. Again, if this is the case, we have another exemplary case.

There seem to be, then, theories that provide bona fide explanations, that actually meet the pattern of ampliative specialized embeddings, but in such a way that none of the concepts introduced in the explanans are T-theoretical: they are new with respect to the concepts used to describe the explanandum, but these new concepts are not introduced by T-laws, they are determinable by other theories. How does this state of affairs affect ASE? I agree with G&C that it does not affect the core of the proposal, which is (specialized) ampliative embedding, regardless of the concepts that the explanans introduces all being T-theoretical, or some being T-theoretical and others not, or even in some very special cases none at all being T-theoretical. Actually, if I already accepted cases in which the ampliation is partially T-theoretical and partially T-non-theoretical, then once I assume that T-explanatoriness may come at least in part via T-non-theoretical concepts, I should have no problem with cases in which the ampliation is fully T-non-theoretical. If a little T-non-theoreticity in the ampliation does not corrupt T-explanantorines, then full T-non-theoreticity should not corrupt it either. Of course, one could *impose* the presence of T-theoretical concepts in the analysis of T-explanatoriness; but without independent reasons, this would be an ad hoc move. Thus, if close inspection confirms the existence of these cases, I must withdraw the demand that some of the concepts introduced by the T-explanans must be T-theoretical. The situation could be plural: very often all are T-theoretical, more seldom some are T-non-theoretical, and rarely none is.

What remains is, perhaps, some terminological oddity. If the explanation is a T-explanation, that is, *offered by/within theory T*, then how come the explanans does not include any concept introduced by T? In what sense can we then talk of an explanation *given by T*? The answer to the first question is clear: these T-explanations do not include any T-theoretical terms because T has no T-theoretical terms. That is, in these rare cases, the theory *does not*, contrary to what is usual, *introduce* its own notions; but it is nevertheless explanatory by borrowing additional concepts from elsewhere and combining them with the explananda concepts through non-accidental generalizations. The explanatory work can then be done by T *without T introducing its own terms*. This is unusual but, I concede, not conceptually inconsistent. As for the second question, I must admit this somewhat odd terminological consequence. Or perhaps not that odd: these explanations can still be called "T"-explanations, explanations *provided by T*, in the sense that it is T that provides the non-accidental generalizations that combine the explananda concepts with the new concepts, even if those new concepts *are not* introduced by T.

A last, important, issue is how to distinguish, in these cases, among the T-non-theoretical concepts, those that are descriptive of the explananda and those that are explanatory. When the explanatory concept is T-theoretical, there is an independent manner of identifying it, namely, the fact that it cannot be determined without T-laws. But if there are explanatory concepts that are T-non-theoretical, how do we then draw the explanatory/non-explanatory line among T-non-theoretical concepts? I think that there is no formal answer to this question and that the answer is essentially pragmatic: T-explananda concepts are those used by T-scientists to describe the phenomena they want to account for. And the T-explanatory concepts are simply the rest of the concepts used by T, that is the rest of T-non-theoretical concepts since in these extreme cases all T-concepts are T-non-theoretical. And of course, if T uses a concept and the concept is not used to describe an explanandum, then the only option is that T uses it as part of its ampliative, explanatory machinery.

This far, I have addressed the first equivalence, "T-theoretical ↔ T-explanatory"; more precisely, its right-to-left direction. The opposite direction, that is, that all T-theoretical concepts are T-explanatory, is not questioned by G&C, and I stick to it strongly. Although maybe not strictly incoherent, I find it unconceivable for there to be a case in which a theory *does introduce* T-theoretical concepts but nevertheless T-explanations *do not use them* in their explanans. There are no known cases of this kind, and I bet none will be found.

I will conclude with some brief comments with respect to the second equivalence: "T-non-theoretical ↔ T-testable". The idea was that phenomena described, and identified/measured, T-non-theoretically provide both the explananda of the theory and its testing basis: the empirical facts determined independently of T and relative to which T checks its predictions. Nevertheless, if we reject part of the first equivalence, we might need to revise this second one: if the direction "T-explanatory → T-theoretical" in the first equivalence is not true, *and we assume* that (*) T-testing concepts are T-non-explanatory, then the direction "T-non-theoretical → T-testing" in the second equivalence cannot be true. For instance, if the concepts "function", "efficiency" and "heritable" are both NS-non-theoretical and T-explanatory, then NS is not tested by making predictions about functions, their efficiency or about heritability; but as it happens, it is tested with regard to traits spreading in a particular manner.

The other direction of this equivalence, "T-testing → T-non-theoretical", though, seems uncontroversial, and conceptually so. The reason is that for a prediction to be conceptually a test, it must be possible that the prediction fails. And, in order for the prediction to be fallible, the fact theoretically predicted must be T-independently identifiable in order to check whether it coincides with the theoretical prediction, which in turn implies that such a testing fact must be described with T-non-theoretical concepts, for then it can be identified/measured T-independently, and thus possibly fail.

This explication assumes that a T-testing fact must be both T-theoretically predicted and T-non-theoretically determinable, which implies that the T-non-theoretical concepts used to describe the testing facts must be both determinable T-dependently (on some occasions) and determinable T-independently (on some other occasions); that is, there must be methods to determine their extension that depend on T (for the testing fact to be T-predicted) and other methods for determining their extension that do not depend onT (for the fact to be T-non-theoretically identifiable).

This is, for instance, what happens with space/distance and time/duration in CM: we can measure them CM-independently (e.g., distance via triangulation) but also CM-dependently (e.g., calculating/predictinga spatiotemporal trajectory from a given force and mass).

In their contribution to this volume, Roffé, Bernabé and Ginnobili (R&A) discuss the relation between a concept being T-non-theoretical, being T-testing (i.e., used in describing T-testing facts) and being T-determinable. In their work, R&A first revise some of the G&C cases and provide a new alleged case of a theory whose T-explanatory concepts are T-non-theoretical, namely cladistics, as reconstructed in (Roffé 2020a; 2020b); and second, they extract consequences from the fact that some T-non-theoretical concepts are not T-determinable (remember that *c* being T-determinable does not mean that *every* method of determination of *c* is T-dependent—this would equate it to being T-theoretical—but just that *some* method is).

First, it is worth noting that R&A reject assumption (*) above, namely that T-testing concepts are not T-explanatory. They claim that "a concept will be T-non-testing if it is either T-theoretical or T-non-determinable" (Section 3), but this does not imply (*). Actually, in the graph just below the quote, they explicitly make room for concepts that are both T-explanatory and T-testing at the same time. I think this is controversial. In fact, this is in tension with my comments above about the pragmatic distinction among T-non-theoretical concepts between T-explanatory and T-non-explanatory ones. I said that, since some T-non-theoretical concepts may be explanatory, the question arises as to how identify such T-non-theoretical yet T-explanatory concepts, and I proposed a pragmatic answer: T-non-theoretical T-non-explanatory concepts are those chosen by T-scientists to describe the phenomena they want to "account for", and T-non-theoretical T-explanatory ones would be "the rest" (if any). If "account for" refers to both a testing basis and explainable phenomena, then this answer assumes that explanatory concepts are non-testing. If R&A deny this, then they should provide a different answer to the question. If they do, perhaps the remaining issue would be merely terminological. If we stipulate that every concept that can be determined T-independently (i.e., every T-non-theoretical concept) is T-testing, and since T-non-theoretical concepts can be T-explanatory, then of course there may be T-testing concepts that are also T-explanatory. In such a case, I still think that we should distinguish two subtypes of T-testing concepts: those "merely testing" and those which are also explanatory. I will not pursue this issue further here.

I will conclude with a comment on the second part of the quote, that is, their claim that if a concept is not T-determinable then it is not T-testing. R&A understand "*c* is T-determinable" as the denotation/extension/values of the concept being (fully) specifiable by determination procedures using T-laws, and then they argue that sometimes T-non-theoretical concepts are not T-determinable in this sense. As an example, they refer to some natural selection concepts and cladistic concepts. For instance, according to R&A, the concept "function" (i.e., what function a trait has) is not NS-determinable, since, given all the other concepts that are determined, NS-laws do not determine the function of a trait unequivocally: there may be different candidate functions that are all NS-compatible with the determined valued of the other concepts plus NS-laws. The same happens, they argue, with other NS concepts, such as "environment". And with cladistic concepts.

Even if I think some of the examples are controversial, I will not dispute their claim and concede that there may be theories (the ones mentioned, or others) whose laws do not (fully) determine the denotation of a concept—given the determination of the rest of the concepts. What I do want to contend is the conclusion they extract and which is expressed in the quoteabove: if a T-non-theoretical term is not T-determinable (in the mentioned sense), then it cannot be considered a T-testing concept. Here, I strongly disagree. I accept that, among T-non-theoretical concepts, some may be T-determinable (in R&A's sense, i.e., "fully" determinable) and others may not be. But this does not mean that those which are not (fully) T-determinable are *not T-determinable at all*. This is quite implausible; for if, despite everything, they are T-concepts, it is because they are involved in T-laws, and such laws must have *some* constrictive effect on their extension. Even if (given determined values for the rest of the concepts) T-laws do not fully or unequivocally determine the values of a T-concept, for sure they must at least restrict, i.e., eliminate, certain values as possible (in structuralist jargon, and roughly expressed, the laws must at least eliminate some potential models as not being actual models).

Let us use 'fully/univocally T-determinable' for R&A's sense, and 'weakly/partially T-determinable' for this second, weaker, sense. What I reject is that only T-non-theoretical fully T-determinable concepts may be considered T-testing concepts, as R&A initially claim. If T-laws restrict the values of a T-non-theoretical concept, even if not univocally, but only partially, it is still possible to make predictions involving the concept that are *fallible* (even if the prediction over the concept values is disjunctive, at least some values are excluded), and this is all we need to consider a concept to be T-testing. So, T-testing concepts are T-non-theoretical concepts that are fully *or* weakly T-determinable. In the last part of their contribution to this volume, and as a consequence ofour exchange on this topic, R&A concede this point and agree to talk of two senses of testability: "T-testing" and "weak T-testing", corresponding to the two senses (fully/weakly) of being T-determinable. To me, weak testability conceptually suffices for testability; that is, testability is, *conceptually*, weak testability, although of course *pragmatic* considerations make it advisable to go for strong testability when possible. In this regard, standard structuralism would not need a reform. It is unclear to me whether R&A would end up accepting this, although I tend to think that they would not, since their paper seems motivated by the idea that weak or partial T-determination does not suffice for testability (see the mentioned quote above claiming that if a concept is not T-(fully) determinable then it is not T-testing). If this is what they finally continue to think, then I would strongly disagree.

5. Concluding remarks

In this chapter I have summarized the main aspects of the account of scientific explanation as ampliative specialized embedding (ASE). ASE preserves the core of Hempel's nomological expectability idea, but substantially reforms his model by substituting logical inference by model-theoretic embedding and adding two crucial conditions, namely, conceptual/ontological ampliativeness and nomological specialization. I have presented new applications of the account to four biological theories, a field traditionally claimed not to be suitable for nomological explanations. I have also discussed some criticisms addressed at the relations between T-theoreticity, T-explanatoriness and T-testability that initial versions of ASE presupposed: I accept some of these criticisms and modify ASE accordingly. These modifications, though, do not affect the main aspects of the account, nor its applicability. It is my claim that these new developments confirm, overall, that ASE fares well as a general account of scientific explanation, and in any event better than its rivals.

Acknowledgments

José Díez formally acknowledges the support of the following Catalan and Spanish research grants (2021-SGR-00276, PID2020-115114GB-I00, and CEX2021-001169-M, funded by the Ministry of Science and Innovation MCIN, Spain).

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Structuralism as a Resource to Detect and Solve Some Current Philosophical Problems

Daniel Blanco[1]

1. Introduction

The structuralist view of theories is a pertinent tool to address some current philosophical problems. I am not saying that the use of this metatheoretical perspective is indispensable for us to find all and every answer in the philosophy of science (not even for the ones I mention here), but rather that at least some important questions from that realm are perfectly manageable from this perspective, and even can be faced and resolved rather easily. Also, and given its inherent formalist standpoint, perhaps its approach to the matter is preferable to the alternatives, every time one values precision and clarity as virtues. Then, if I succeed, this article could be an incentive to dive into crystalline structuralist waters.[2]

Specifically, I briefly introduce three philosophical cases that can be successfully dealt with using structuralist tools. All of them can be initially addressed using only the rudiments of one (important) chapter from the structuralist manual (Balzer, Moulines, and Sneed 1987). Most assuredly, we would need more to get the complete picture and a definitive solution to each one

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[2] To this date, several works have been published—in articles and anthologies—that have to do with the fertility of the approach (see Diederich 1989; Diederich, Ibarra and Mormann 1989; 1994; Abreu, Lorenzano and Moulines 2013; Balzer and Moulines 1996; Balzer, Moulines and Sneed 2000; Díez and Lorenzano 2002; and also the 2014 volume 79 of number 8 of *Erkenntnis*). In writing this contribution, I had in mind a reader who is not necessarily an expert in that view at all.

Abreu, C. (ed.) *Philosophy of Science in the 21st Century. Contributions of Metatheoretical Structuralism*.
Florianópolis: NEL/UFSC, 2023, pp. 137–153.

of these problems, but I limit the study to that early phase of analysis, which I think would be enough to carry out my purpose.

These three philosophical problems are (1) the acknowledgment of rivalry between theories and within a particular theory; (2) some aspects of what has been called "revolutionary science"; and (3) the search and exclusion of circularity in empirical theories. Almost every case study we are about to consider stems from the evolutionary biology realm, but of course, it could be extended—*mutatis mutandis*—to many others.

The contribution is organized as follows: firstly (Section 2), I will succinctly introduce how our metatheoretical approach suggests the way scientific theories should determine some terms of its vocabulary following a procedure that, when seriously taken, allows us to detect and/or face precisely the philosophical problems we are about to describe. In this very section, I will show the quotidian nature of this procedure while working under the program. Secondly (Section 3) I briefly introduce the mentioned problems and how the former procedure can help us to find their respective solutions/clarifications. The reader will notice that the first two situations are frequently referred to both by historians and philosophers of science. The third one, while not as common as the former ones, is very important insofar as it has put in check an entirely new discipline within evolutionary biology/systematics: cladistics. Finally, I offer the conclusions.

2. A quick look at structuralism

2.1. Structuralist normal science

Structuralism was born to explicate scientific theories from a semantic/model-theoretic stance using formal tools, initially through the work of one disciple of Patrick Suppes, Joseph Sneed (1979). These formal reconstructions of theories are seen by structuralists themselves from two perspectives:

1) The reconstruction is a goal in itself, maybe *the* goal. It is a response to the motto "what can be said, can be said with clarity"[3] that lies behind the elucidatory function of the philosophy of science. Elucidative tasks are especially relevant and necessary when one takes into account that few concepts are so misused in the philosophy of science (and in science) and at the same time so frequently employed than 'theory". Then, to clarify it

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[3] More precisely: "Philosophy aims at the logical clarification of thoughts. Philosophy is not a body of doctrine but an activity. A philosophical work consists essentially of elucidations. Philosophy does not result in 'philosophical propositions', but rather in the clarification of propositions. Without philosophy, thoughts are, as it were, cloudy and indistinct: its task is to make them clear and to give them sharp boundaries. [...] Everything that can be thought at all can be thought clearly. Everything that can be put into words can be put clearly" (Wittgenstein 2001, 4.112-4.116). As is well known, this was one of the main objectives of the Carnapian program. Carnap himself wrote in his *Aufbau*: "We too, have 'emotional needs' in philosophy, but they are filled by clarity of concepts, precision of methods, responsible theses, achievement through cooperation in which each individual plays his part. [...] We feel that there is an inner kinship between the attitude on which our philosophical work is founded and the intellectual attitude which presently manifests itself in entirely different walks of life; we feel this orientation in artistic movements, especially in architecture [...] We feel all around us the same basic orientation, the same style of thinking and doing. It is an orientation which demands clarity everywhere, but which realizes that the fabric of life can never quite be comprehended" (Carnap 2003, pp. xvii-xviii).

is imperatively desirable. Structuralism takes a stand on what a scientific theory is, and the formal apparatus it provides can identify its components with singular precision. Also (and for reasons that exceed us), it reconciles two visions of scientific enterprises usually seen as incompatible: the quest for precision that distinguished logical positivism and, among others, the genidentical or historical changing character of theories so emphasized by Kuhnian theses (Kuhn 1962; Stegmüller 1981; 1983).

2) The second perspective emphatically claims that the usefulness of the reconstruction goes well beyond the formalization; though those additional benefits are invariably based on it. Therefore, and far from being a sterile ground in other respects, the formal reconstruction acts as a platform from where a multitude of problems in the philosophy of science can be successfully addressed. As a result, even if at first glance you cannot see the additional advantages resulting from a formal reconstruction of a particular theory, you might discover what the approach has to say regarding many metatheoretical debates, including both generic and more local ones.

It is this second issue that I want to stress here. We are about to see how even what constitutes the first step in reconstruction is sufficiently powerful to deal with the problems that occupy us here. For now, the main point is that structuralism deals with them explicitly or implicitly in normal work. Again, and surprising as it might be, *it is routine*, it happens *every time* the approach is applied. Let us analyze what this means.

2.2. Structuralism and the empirical basis of theories

When Professor Pablo Lorenzano taught his students (myself one of them) about structuralism, he used to advise on what to do first when one is about to reconstruct a particular theory T. He often said, following Sneed (1979, p. 297), that pragmatically one starts where one can, and not always where one should. Without a doubt, he was right. However, ideally, the first thing one should do is to specify what T speaks about, and what its vocabulary is.

Then, in the second step of our reconstruction, if faced with an explanatory theory, it is necessary to identify what elements of that vocabulary help us to determine what T intends to explain. In empirical theories, these explanatory targets of T are chunks of reality, parts of nature. (As I shall show in the next subsection, this does not mean that a metaphysic alignment on the part of structuralism has taken place.) Often, this goes hand in hand with the reason why the theory was originally conceived, that is, as an answer to a problem in the realm of experience (Blanco 2011b). Structuralism claims that each scientific theory has something like "a world of its own". This "world" is characterized as a subset of terms within the vocabulary of T. This means that once you list the terms of T, it is possible to distinguish between two kinds of them. The structuralist criterion for this discrimination has to do with the answer you can give to the following question regarding every term in that vocabulary: "Is it possible to determine it without the application of T?"[4] Of course, there are two possible answers:

1) If it is not possible, then that term belongs to the set of what structuralists call "T-theoretical terms". It is so called precisely because of its dependence on T at the time of its determina-

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[4] To determine a term is to assign one or more numbers, through a measuring method of calculus, if the term is quantitative; or if it is qualitative, with the application to an object.

tion: for every determination of the term, T and its laws are presupposed and applied. T is indispensable to do the job.

2) If it is possible, then, that term belongs to the set of what structuralists call " T-non-theoretical terms". T and its laws are not needed for its determination. T is dispensable to do the job (Kamlah 1976; Sneed 1979, p. 33; Balzer and Moulines 1980; Moulines 1985; Balzer, Moulines and Sneed 1987, Chapter II).[5]

In every empirical theory, what its users intend to explain, that is, the *explananda* of T, should be linked with this last set, and not with the former one. Notice that these *explananda* could be determined using the laws of T, but the fact that one can determine them without T is what makes its testing possible insofar while testing, you compare the results obtained thanks to the application of T with results obtained *from a different source* that is considered to be reliable by the users of T. If not, there would be no control for T, it would be controlling itself, and would be valid in every intended application for bad/vicious reasons. While dealing with explanatory scientific theories, we need to provide guarantees that this kind of problem is not present every time we test them.

This is why the set of T-non-theoretical terms can count as the ontology for T, it is its empirical basis (a basis that is available before the discovery of T) and is considered as unproblematic data (or undisputed "facts") by the users of T, these being borrowed from the mentioned trustworthy or undisputed "different source" (we will return to this in the next subsection).

As can easily be inferred, this set of T-non-theoretical terms is closely related to the domain of application of T, which, as I mentioned, is what counts as "the first motor" that led scientists to invent/discover T. Sufficed to say that, for the Sneedian perspective, this pragmatic issue is as essential for the identity of T as its formal content.[6]

Therefore, the terms involved in the *explananda* of T should be T-non-theoretical. Usually (always?), the rest of the vocabulary (the T-theoretical terms) has explanatory power for those *explananda* (for more on this, see Bartelborth (1996; 2002), Díez (2002), Moulines (2004) and Lorenzano (2005)). Note that this distinction helps us to separate the problem to be attacked from what we use to solve it, and once this is done, the task can continue by exploring the following steps to attain the reconstruction of T. Let me say more about the origins of those T-non theoretical terms.

2.3. The sources of T-non theoretical terms

In the earlier subsection, I mentioned that what counts as data to be explained by any explanatory theory T, does not (should not) come from T, but from a "different" source. But, what is that

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[5] Between 1983 and 1986, Wolfgang Balzer developed a second criterion for theoreticity, the so-called "formal" one (Balzer 1985; 1986; Balzer, Moulines and Sneed 1987, pp. 73-78). This new criterion has no relation with what is presupposed by T and involves an existential and not a universal quantifier. Briefly, Balzer says that a term is T-non-theoretical *if there exists any* possibility for its determination that is independent of T. In this contribution, we do not follow Balzer on this.

[6] Suppes' first doctoral student, Ernest Wilcox Adams (see Suppes 1994a, p. 201; 1994b, p. 5) suggested that introducing the entities to what scientists intend to apply a theory, should be included in the semantic reconstruction of theories. Surely a lot in the world might satisfy the demands of the formal axioms, but the users of a theory only have some portions of that great set in mind while applying it (Adams 1959, pp. 257-259). Sneed strictly followed Adams on this (Suppes did not).

"different" source? Is it the world itself? "Not necessarily", is the usual structuralist answer. Why? Because the T-non-theoretical terms probably are determined through the application of other previous-underlying theories.[7]

This leads to two new important issues within this approach. First, we can identify connections between different theories in science that help to transfer inter-theoretically information between each other. T inherits its T-non-theoretical terms from other theories.[8] In short, while determining the *explanandum* of T, we do not need to presuppose T, but we do presuppose the previous theories needed for their determination (see Moulines 1984; 1985; Diederich 1989, pp. 364-365).

Note that this introduces a connection between two metatheoretical realms: philosophy of science underlines the importance of the history of science insofar as any structuralist reconstruction leads us to look in history for one or more theories that are indispensable to determine the terms the theory under study explains. That is, this creates a meaningful link with the history of science that cannot be underestimated: as we will see, one can resolve a controversy on an alleged circular explanation by introducing an earlier theory that determines what T means to explain. (This relationship with history is not linear, but we can safely claim that if a set of terms was available before T; then we can consider it as T-non-theoretical.)[9] One can take this as a new convenience for their marriage (see Giere 1973; McMullin 1974) or for the consideration of both as "Siamese twins".

The second consequence becomes clear once we note that what *counts as* "the world" for T, might not be the same for another theory, T'. This is because what counts as a T-non-theoretical term in T could be T-theoretical in T' (see Sneed 1979, p. 252). This distinction is related to the theory we are dealing with. A term is not T-theoretical or T-non-theoretical in an absolute sense but in a particular context.

This gives us a clue to the answer we are looking for. What we can consider the world while dealing with a particular theory might well be loaded with another previous theory. Then, the primary source of T-non-theoretical terms (the uncontroversial data for T) is another different theory. If that can be extended to all the not-theoretical terms of every theory in science is something about which structuralists are, in principle, agnostics (see Díez 2012, pp. 110-113; Falguera 2003; 2012).

It is time now to see how this simple tool can bring light to common issues in the philosophy of science.

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[7] A lot of what counts as data for scientific theories is obtained through the use of instruments, and sometimes (not always, perhaps nor even in the majority of cases) these instruments are built following one or more theories. This has new philosophical consequences for some topics we address here (the relationship between incommensurability and relativism, for example). For space reasons, I sidestep this issue but see Bueno (2012), Díez (2012, pp. 104-113), Jaramillo Uribe (2012), and Lorenzano (2012).

[8] Even when it was first proposed by Sneed (1979), this idea was present in intuitions made by prior classic philosophers of science (see Hempel 1970; Lewis 1970). Note the curious coincidence in the date of these sources. While visiting Argentina in March 2009, Sneed was asked by José Díez whether he was aware of Hempel or Lewis's papers (both appeared in 1970, while he was working on his book published in 1971). Sneed's answer was "not at all".

[9] Even when the independent determination of any given term of T does not imply its historical precedence, a previous determination for that term does imply its independence from T. (There is an interesting conversing case regarding mass and Classical Particle Mechanics, see Moulines (1991, pp. 236-238)).

3. Dealing with philosophical problems in science

3.1. On rivalry in science

The first example has to do with empirical commensurability between and within theories (Kuhn 1962; Feyerabend 1962), and in the following, we will address briefly both kinds of cases (see Stegmüller 1975; 1976).

3.1.1. Rivalry between different theories

It is possible to identify competition between any two theories, by the comparison of what counts as the "part of the world" each one of them intends to explain. You have genuine rivalry if the intersection of both sets is not empty. The bigger the extension of that intersection, the more ferocious the competition would be. Note that, paradoxically, the core of the competition is an agreement about what we have in "the world" for each of them to explain. The greater the agreement, the more difficult their coexistence would be. There is an intentional component here: to have a rivalry, the users of both theories must intend to apply them to the same domain (the very same elements belong to both sets, being then mutually co-extensive), or at least that there is overlapping, that is, that the intersection of both sets of intended applications is not empty. In the first case, we have total empirical equivalence between both; in the second, only partial empirical equivalence.

For example, it has been repeatedly defended that the theory of natural selection is a rival of natural theology in the sense that the respective users of both intend to explain the same portion of nature: adaptations. However, it is perhaps true that the former theory forbids the existence of organic structures that would be perfectly accepted in the latter. To use Darwin's example, traits that while useful for a different species, result to be harmful for the one that bears them (Darwin 1859, p. 159). Still, the intersection of the two sets of terms involved in the determination of the *explananda* of both theories is not empty, though probably not co-extensive, and then we have genuine rivalry between them (see Ospovat 1980a; 1980b; Blanco 2008; 2011; Caponi 2011a; Ginnobili 2014).

Following with one additional Darwinian case, it has been underscored that the antagonism between the theory of the *Vertebrata* Archetype coined by Richard Owen and the theory of common descent has to do with their coincidence on what they want to explain: the presence of homologies (see Blanco and Ginnobili 2020). Here, there are reasons to think that their respective sets of intended applications are coextensive with each other, given the fact that Darwin did not question any of the procedures proposed by Owen to detect homologies. On the contrary, he used them. I will return to this point in Subsections 3.2 and 3.3.[10]

A third case of this kind of rivalry can be found in the scope of the domain of application of the theory of natural selection between Alfred Wallace and Darwin. The second one, more pluralist,

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[10] Using present standards, it would be hard to consider nineteenth-century natural theology (and its inherent creationism) as a scientific theory. Perhaps we should consider it as a proto-theory or something of the sort. However, it would be anachronistic to do so, insofar as Darwin himself considered it as a scientific attempt (Darwin 1859, pp. 194, 203, 355, 356, 372, 393, 471, 473, 474, 478). Interestingly enough, Darwin does not say the same about Owen's theory of archetypes, objecting to its scientific status because of its link with religion (Darwin 1872, p. 383).

opened the door, between others, to one Lamarckian theory to explain the origin of some features, while the first one used to be skeptical of that, remaining "loyal" to a more universal application of the theory of natural selection in nature. While Darwin himself intended to limit the set of applications of the theory of natural selection, Wallace intended to broaden it. A debate between both scientists was the obvious result. Note that this discussion takes place within the intentional realm. One should not confuse an intended application with a successful one, the latter being decided in the empirical arena.[11] Of course, and it happens very often in scientific practice, one can explain the failure through several recourses, preserving the applicative intentions about the seemingly failed application, despite the evidence against it.

As a result, when reconstructing any two theories, we can see the coincidence (or not) between their domain of application and infer from it the level of their competition. Surely, it is not always the case that these disputes end in an all-or-nothing verdict, which would mean a total replacement of one theory for the other. Sometimes, what happens is a mere redistribution of the domain of application of those theories. As it is determined intentionally, the domain of application of any theory is not fixed but is an open set, where elements can enter or leave through time.[12]

Depending both on the context in which this rivalry takes place and the caliber of the theories at stake, the result can be one of what Kuhn called "scientific revolution" on the one hand (I will deal with this in Section 3.2), or an "internal" debate between each other, which we shall see next.

3.1.2. Rivalry within one and the same theory

A less obvious kind of rivalry can also perfectly be explicated using structuralist tools: the competition between heterogeneous theoretical elements of the same theory. This is possible thanks to the hierarchical perspective that arranges those elements in a multi-level array. As we go down the hierarchy, through the elements located in the lower stratified portions of the theoretical structure, typically, we find that their respective domain of application gets increasingly more restricted, since the theory gains in empirical content.[13]

The more important difference with the former kind of rivalry is that it explicates the discussion between scientists under *the same* research program. Even when the resolution of that discussion might be challenging and complex, the triumph always takes place within one single theory/paradigm/program and does not lead to any revolutionary event at all. On the contrary, in the

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[11] Typically, structuralism associates certain empirical claims to every element of the set of the intended application, such as (for every x belonging to that set) "x behaves as the laws of T predict". That sentence can be true or false, but that does not mean that the theory is true or false, but only that that part of the world ("x") does or does not satisfy the laws of T. When the sentence is true, we are in front of a successful application of T in the sense that "x" does behave as T predicts. Even when closely related, one should not confuse the set of intended applications of T with the empirical claims associated with T. We have three sets: the set of potential applications, the set of intended applications, and the set of successful applications. Note how an intended application might well be not successful, and a potentially successful application might not be intended.

[12] Structuralists claim that the intended applications are an essential part of a particular theory. However, this change does not necessarily lead to repetitive revolutions provided that some particular elements remain, that is, its paradigmatic exemplars. This is another way in which a theory might change without a modification in its identity.

[13] For a complete understanding of these issues, it might be necessary to explicate both what a theoretical element is and what lies behind this arrangement. Unfortunately, we leave this aside for space reasons (see Balzer, Moulines, and Sneed, Chapter II).

long run, the program as a whole is strengthening. If this ends up in change, it is an intra-theoretical change.

We find an example of this in another well-known debate between Darwin and Wallace regarding the application of a different portion of (explanatory theoretical terms within) the theory of natural selection: how pervasive is sexual selection in the history of life? Is it the theoretical element of the theory of sexual selection or another one (such as the one related to survival) that should be applied to explain the evolution of this or that trait? Whatever the answer to that question, the theory of natural selection wins insofar as we have found a successful application for it whether by the theory of sexual selection or by the theory of natural selection related to an improvement in survival (or any other one).[14]

Again, and as we can see from the last example, it is not an all-or-nothing debate, but it often ends with a change of belonging of elements in the respective sets of the intended application of those theoretical elements, which are portions of the same gathering theory. As we take the intended applications as part of the identity of theories, this kind of accidental, peripheral change can capture the development of a theory in what Kuhn called "normal science" (see Kuhn 1962; 1976).

Finally, this approach is also relevant to the historiographical issue of priority between Wallace and Darwin, because this kind of application allows us to see what was what both of them discovered with precision, what concepts both contributions share, and what the novelties between both versions are (see Blanco 2016; Ginnobili and Blanco 2019). Note that a conclusion based on this systematic approach can be made independently of both chronological issues and even the opinions of Darwin and Wallace themselves (Wallace 1908, pp. 5-7; Darwin 1958, p. 21).

3.2. The nature of scientific revolutions

The empirical commensurability between competing theories that I mentioned above helps us when thinking about the nature of scientific revolutions. How much of the outgoing paradigm is displaced? How much of the incoming paradigm involves an inheritance of the displaced one? As Kuhnian revolutions take place within a discipline, it is difficult to find one without com- mensurability of some sort between the old paradigm and the new one.[15] Again, it is this commensurability itself is what ignites the very rivalry that ends up in the disruptive event. The first intuition here is that even when surely revolutions have to do with novelties, sometimes they are also linked with the rearrangement of remaining "old" data. Therefore, the notion of revolution o of change of theory/paradigm is not incompatible with some kind of preservation. If we keep

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[14] Many authors, including those that do not share semantic perspectives when addressing scientific theories, have realized that some apparent flaws of the theory of natural selection, such as its alleged tautologicity, are related to the layering of a hierarchical structure (see Díez and Lorenzano 2013). Once they took that into account, they were able to see that the theory of artificial selection, the theory of sexual selection, and the theory of natural selection related to survival are indeed theoretical elements of the same theory (see Endler 1986, pp. 3-15; Tuomi 1981; Brandon 1996, p. 51; Lerner 1959; Gould 1976; Wassermann 1978; Tuomi and Haukioja 1979a; 1979b; Naylor and Handford 1985; Castrodeza 1988, pp. 182-183; Ginnobili and Blanco 2019). Then, a rivalry between any of these three theories is an internal debate that would not end in a revolutionary event.

[15] To talk about incommensurability between theories that have nothing in common is not sound. In such a case, we do have incommensurability, but it is not epistemologically meaningful (see Díez 2012, pp. 72-73). It is trivially true that Chomskian linguistics is incommensurable concerning quantum mechanics.

in mind the structuralist distinction between T-theoretical terms and T-non theoretical terms, two successive theories might be partially commensurable not because part of their vocabulary is theory-neutral as a whole (they might well be loaded with a preceding theory), but because that part of their vocabulary is not loaded with those two theories (see Kuhn 1962; Lorenzano 2012; Díez 2012). It is this kind of neutrality what allow the inter-theoretical preservation of data in the empirical realm (see Kuhn 1983), even when you can have novelty in the explanatory realm. Let us see two examples.

The first case has to do with the so-called "Copernican revolution" (by the way, one of the exemplars used by Kuhn himself). In dealing with the rivalry between ancient astronomy, the Tychonic version of the cosmos, and the Copernican point of view, a discussion has taken place regarding the concept of "planet" and how it could concern the revolutionary character of the rise of modern astronomy. However, it is undeniable that here we have partial commensurability; once we notice that *the same* data (astronomical tables for the apparent trajectory of each "planet") are explained by these three different theories with similar success (see Díez 2012, p. 92).

Severinus Longomontanus, a former assistant of Tycho, and a great defender of his system, wrote a manual in which he introduces not only Tycho's model (its main goal), but also the equivalence in precision to give account of *the same* available data while using any of the three systems (for Saturn's apparent movement, for example; see Longomontanus 1622, pp. 324-326). That means that "the world" was in some genuine sense the same for these theories, whatever the very important novelties introduced by Tycho or Copernicus might be concerning Ptolemy: three mutually exclusive theories share the same phenomena to be explained and this coincidence is the very reason why that (or any other theoretical) competition starts in the first place. The structuralist pro- gram would easily see this partial compatibility of these systems, shedding light on what Kuhn meant when he recognized a revolution here.[16]

The second case has to do, once again, with the link between the theory of common descent and the theory of the Archetype. There is a debt of the Darwinian Theory to Owen concerning the determination of homologies that Owen explained using an ideal archetype. Virtually, there is no change at all between the set of intended applications in both theories and even it is possible to detect the same paradigmatic exemplars, such as the mandibles of insects (Owen 1843, p. 215; Darwin 1872, p. 383) or the different forms of limbs in the group *Vertebrata* (1847, p. 127; 1849, pp. 3-9; Darwin, 1859, p. 200, 434, 479; 1872, p. 383). An exhaustive description of this is beyond our goals (see Blanco and Ginnobili 2020), but more light can be found in this very case, as we shall see in the next section.

As a corollary, probably every so-called revolutionary event in the history of a discipline does not change everything we can find in the old research paradigm (see Laudan 1984).

3.3. On circular explanation

Although the specific problem I now address is philosophical in nature, it has been initially stressed by experts in the scientific discipline in question itself: cladistics (Hennig 1966). Shortly, cladistics is an approach to systematics in biology that the majority of contemporary scientists consider to be the best one for several reasons. One of them is its efficiency regarding the reconstruction of

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[16] Note another related issue: just as incommensurability can be used against realism, data preservation in revolutionary events seems to support it. I thank C. Carman for providing Longomontanus' reference.

the parental lines through grouping biodiversity, the two realms (systematics and parenthood) that Darwin was convinced should be tied to each other (Darwin 1872, p. 369). Therefore, they believe that the ideal outcome of cladistics is a hypothetical tree that intends to be the actual reconstruction of the familiar tree between the involved groups (potentially, all the groups of all living beings that live or ever lived on our planet). In a nutshell, the final and herculean task of the community of cladists is to offer the ultimate tree of life that, if correct, reflects—no more, no less—the history of life. Again, classification and the specification of parenthood become two sides of the same coin, and this is a tenet for some important portion of this community. From this perspective, the application of cladistics methods results to be an explicit recognition of the relevance of Darwin's thoughts on the topic, and the materialization of its ideal of what biological classification should be. However, the relation between cladograms and the actual evolutionary history is precisely what is at stake in an internal debate within the community. The problem we are about to introduce is not but a derivative of that controversy.

That being said, what is the problem? As I mentioned, it is a byproduct of a more general debate regarding the role evolution plays in the analysis, and it is related to how one can recognize/determine homologies in nature. How cladists do their (often, very complex) task is beyond my goals. However, it is necessary to deal with some key parts of the initial procedure to introduce what the problem is about.

To take into account that the theory of common descent, so clearly linked to cladistics, is today the usual/natural explanation for the occurrence of homologies, leads us to demand that those determinations should be done without the assistance of the theory of common descent. This has been stressed by several authors, including those who were not fond of cladistics methods, such as Ernst Mayr. He, together with Peter Ashlock, wrote:

> Relationship among species and higher taxa is indicated by the existence of homologous characters, but there is considerable uncertainty about what homology is and how it can be established [...] When Darwin discovered common descent as the cause of homology, it became possible to adopt a more rigorous definition than the ones suggested by [his] forerunners, and yet, 12 years after the publication of the Origin, *there is still considerable argument over the definition of homology. The problem is how to avoid a circular definition*. [...] Simpson's analysis of the problem (1961, pp. 69-93) is particularly enlightening. He points out [...] the phenomenon of identical twins. Two siblings are not identical twins because they are so similar; rather, they are so similar because they are identical for having been derived from a single egg cell. The establishment of an unambiguous definition of homology is thus the first step in the analysis. (Mayr and Ashlock 1991, pp. 142- 143, my emphasis. See also Wood 1995; Padian 2007, p. LXXXVIIɪɪ; Griffith 2007; Rosenberg and Neander 2009, p. 309.)

However, together with the triumph of cladism as *the* systematic methodology, most of the enormous literature that discusses this topic has seen the light in the context of cladistics analysis. The intuition is nonetheless the same: the historical explanation (common descent) cannot be what is at stake at the time of the determination of similitudes that are explained by that historical circumstance.

There is not a homogenous position within cladists on how genuine (or serious) this problem is. Two groups can be identified in this community: those who consider it as a valid restriction that should be respected (pattern cladists), and those who do not. Again, space reasons prevent us from elaborating on this.[17]

However, structuralism leaves us no way out, and I think it is quite simple to see what position it should take in that internal debate. If we determine the *explanandum* of T only by the application of T, how could it be possible to do non-circular testing of T? If that is the case, we lack the needed guarantee for genuine testing. Structuralism is not exactly a straight solution to the problem, but it offers what I esteem to be the clearer available view on the topic as long as it helps us to see a problem where not everybody sees it, and, as I shall show, gives the key clue about the direction where we should be looking for a solution. So far, the main point is to recognize that if the only method to determine any two traits as homologies is using the theory we then apply to explain the occurrence of those specific traits, then, we end up in the undesirable scene of circular (vicious) explanation. Therefore, our verdict is that the problem is real, and requires a solution.

Sneed and his colleagues have seen that these problems are a serious threat to the empirical nature of theories, and the same tool we are describing serves as a way to detect the presence of this circularity problem.[18] Also, structuralism leads us to look at previous theories, in this case, in the realm of comparative anatomy that were useful for the diagnosis of homologies by the time *On the Origin of Species* was first published (Darwin 1859).

The way Darwin established parental links was by taking into account what has been known since the works of Owen as "homologies" (1843; 1847; 1849), a set of (some kind of) similarities between living beings that can be found even in very different living forms within *Vertebrata*. Since 1859, the theory of common descent has been considered the best available scientific explanation for the presence of those similarities, but, previously, Owen himself had provided one with his inventive theory of the Archetype.

Santiago Ginnobili, Ariel Roffé, and I have recently entered into a discussion using these tools (Blanco 2012; Roffé, Ginnobili and Blanco 2018; Blanco, Roffé and Ginnobili 2020; Roffé 2020b). We showed both the relevance of structuralism in dealing with this issue and why the theory of common descent as it was introduced in Darwin's main work is not circular.[19] However, Owen's procedures for the diagnosis of homologies are not enough today (see Remane 1952). But that does not prevent us from taking into account that whatever other protocol is used in making these determinations, it should not be loaded with the explanatory theory (common ancestry) for their occurrence. Even in the case that we need to apply a theory to get the determination of homologies, that theory is neither the theory of common ancestry nor the Archetype one.

For that reason, the normal practice of structuralism helps us to avoid making mistakes that

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[17] The problem of circular explanation in evolutionary biology, specifically in the theory of common descent has bogged down philosophers (and scientists interested in philosophy of science) for several recent decades, and the sources are too numerous to be quoted here. I offer a short list: Roffé (2020), Brower (2000; 2019), Pearson (2018; 2010), Roffé, Ginnobili and Blanco (2018), Lorenzo (2015), Wagner (1989; 2014), Pearson (2010), Rupke (2009), Amundson (2005), de Pinna (1991), Aboitiz (1988) and Brady (1985).

[18] I think we can consider it as a by-product of Sneed taking distance ourselves from the classical observational-theoretical distinction (see Putnam 1962; Bar-Hillel 1970; Suppe 1979, pp. 68-69; Stegmüller 1981, p. 92, 94, 106; 1984, pp. 235-236).

[19] This is true at least for this reason. For discussions about tautologicity (to which structuralism can also be of help), see the sources mentioned above, in n. 14.

might lead to this kind of circularity, and this is the case whether there is an explicit an ongoing discussion on the topic or not regarding the theory in question. In other words, to have at least this portion of structuralism in mind from the starting point can genuinely help to do better science.[20]

4. Conclusions

1) Structuralism has been conceived as a formal tool for the explication of scientific theories from a semantic point of view. Even when it comes to satisfying an important necessity in the philosophy of science (what is the nature of scientific theories, what are their structures, etc.), its fertility can be seen in the fact that it provides us with a platform from which one can address significant philosophical problems. Rivalry within and between theories, the nature of scientific revolutions, and circular explanations are only three of them.

2) As I succinctly showed, the T-theoretical and T-non-theoretical distinction together with a hierarchical view of theories help to shed light on specific philosophical issues that ignited specific debates within the community, such as:

- What do two rivalry theories (e.g., the theory of common ancestor and the theory of the archetype; ancient astronomy and the Tychonic or Copernican Systems) have in common?
- Can some particular type of rivalry between theories (e.g., the theory of natural selection related to an improvement in survival and the theory of sexual selection) be seen as a competition within the same theory (e.g., the theory of natural selection)?
- What is the nature and scope of scientific revolutions (e.g., how much of Owen's comparative anatomy works remains in Darwin's theory of common descent)?
- How does our conception of revolution in science affect our position regarding scientific realism?
- Are these two scientists (e.g., Darwin and Wallace) the co-discoverers of that theory (e.g., the theory of natural selection)?
- Does this theory (e.g., the theory of common descent) offer a circular explanation?
- From which previous theory does this one (e.g., the theory of common descent) get the data (e.g., the determination of homologies) it is about to explain?

Both the elucidation of these problems and the provision of a compass that leads to their solution are side effects of the routine application of this approach.

3) For those interested in formal approaches as well as for those interested in pragmatic issues within science, to dive into crystalline structuralist waters is worth the effort.

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[20] I strongly believe that the teaching of scientific theories and the way textbooks are written would also benefit from this approach.

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Organizing Nursing Knowledge from Metatheoretical Structuralism's Viewpoint

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1. Introduction

In this article we will show the potential of metatheoretical structuralism to aid in the recognition of the specificity and epistemic relevance of nursing knowledge. Nursing is a discipline with high importance within health systems in the whole world, but because of different socio-historical reasons its specific knowledge is often considered to have a low epistemic value, closer to an "art" or to the type of "natural" skills for caring like the skills parents use to care for their children (more specifically a mother's care, as women are still the vast majority of professional nurses). That kind of knowledge is not considered to have a scientific nature, as it is usually learned spontaneously: caring, love and empathy sometimes seem to be elements with higher importance than scientific theories in shaping nursing knowledge. This situation causes specific phenomena that are especially relevant to take into account in Latin America (even though it is not exclusive of this region). Wages of nursing professionals are, generally speaking, very low. In many countries, University graduates coexist with other nurses educated in very short courses (about three years). A very high number of nurses proceed from low income households, and so on.

One of these phenomena is specially relevant for our discussion: medical doctors usually consider nurses as a kind of low-level technical support to their own practices. Within the hegemonic

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Abreu, C. (ed.) *Philosophy of Science in the 21^{st} Century. Contributions of Metatheoretical Structuralism*.
Florianópolis: NEL/UFSC, 2023, pp. 155–179.

medical worldview, nurses are relegated only to unpleasant care-actions like keeping the patient's hygiene, giving injections, and so on. This worldview, as is widely recognized in nursing literature (for a detailed account of these phenomena during the COVID-19 pandemics in Argentina, see Federico, Pérez and Senones (2021)), denies nursing's specificity as the science of care. Scientific caring is a holistic issue, and cannot be reduced to the keeping of the patient's biomedical variables within certain thresholds, but must also include elements from the social sciences and specific nursing scientific knowledge.

As we said, there is a vast literature in nursing journals discussing these phenomena. One of the main strategies to show the scientificity of nursing knowledge has been to organize its specific body of knowledge in a systematic fashion through metatheoretical efforts (see Alligood 2014). Although these epistemological efforts made by nursing theorists are very interesting in themselves for philosophers of science, nurses theorists are pursuing this complex endeavor because highlighting nursing's systematicity (see Hoyningen-Huene 2013) seems to be a proper way to defend the epistemic value of nursing knowledge, and in doing so, a way to achieve better social (and economic) positions.

In this article we will begin by analyzing the most important (and most accepted) effort for systematically organizing nursing knowledge due to nurse theorist Jacqueline Fawcett (2005), namely, her "structural holarchy", as nowadays is considered, within the nursing community, the mainstream proposal (Alligood 2014).

After that, we will present the alternative from metatheoretical structuralism for organizing scientific knowledge (see Balzer, Moulines and Sneed 1987), which combines, in a refined way, contributions by the most important philosophers of science of the 20th Century, including, among others, logical empiricists and Thomas Kuhn.

Then, we will present Dorothea Orem's self-care framework as an exemplar of a structuralist reconstruction of a mainstream nursing theory. The reconstruction will show the advantages of structuralism in highlighting the epistemic elements of the nursing knowledge.

Finally, we will state that metatheoretical structuralism provides a better tool to achieve the nurses' objectives. On the one hand, it is rigorous and precise. On the other hand, if nursing wants to be recognized as having the same epistemic value as traditional sciences, it may be a good strategy to show that it has the same structure as these sciences do. As structuralist philosophers have built a huge body of work reconstructing many theories of traditional sciences (see Diederich, Ibarra and Mormann 1989; 1994; Abreu, Lorenzano and Moulines 2013), a comparison would be done easily, as long as the structuralist program incorporates in the future rational reconstructions of nursing knowledge as part of its program.

2. Organizing nursing knowledge: Jacqueline Fawcett's structural holarchy

There are many professions which have as their main objective to provide care, but nursing is supposed to be the one which provides it through scientific foundations. It is central to the nursing discipline to analyze such scientific foundations and to organize them in order to highlight its structure (pointing out its similarity with traditional sciences' structure) with the objective of defending its epistemic status. Risjord (2010) among others, stated the critical need of the nursing discipline to show the specificity of its knowledge as the right strategy to avoid naive but ubiquitous views of nursing as a technique, or, even worse, as "an art based on empathy" as its core component. Defending the scientific nature of nursing is not (only) about the self-esteem of its professionals, but about giving it the right place and role in health institutions (avoiding the role of mere assistants of medical doctors) and getting proper subsidies to grow the body of knowledge of the discipline. In this sense, even when the problem is metatheoretical in its nature, the social consequence is very concrete. It seems to be a very important and interesting program to support nursing by providing specific tools to get their knowledge properly organized, and to be able to propose tentative answers to such questions as: which are nursing's specific epistemological units of analysis (theories, models, paradigms)? Which properties do those units have? How do they work in the dynamics of nursing knowledge growth? Are these units the same as in the traditional sciences (such as physics, chemistry or biology)?

According to Hernández Conesa "the meditation about the principles which rule the knowledge, exercise and way of intervention of nursing care in contemporary culture is one of the unpostponable discussions" (2006 p. 3). So, in this article we will analyze nursing's mainstream organization model, due to nursing theorist Jacqueline Fawcett. Such analysis will allow us to show that metatheoretical structuralism can do the trick much better, and under such basis we will defend the pertinence of extending structuralism's reconstructing program to the nursing discipline. Hernández Conesa herself highlighted "Sneed and Stegmuller's structuralist program's" (2006, p. 7) great potential for philosophy of nursing, but not much work has been done to apply it up to now.[3] Fawcett's structural holarchy for organizing nursing knowledge has been widely adopted within the nursing community (see Alligood 2014), but, even with its very sophisticated philosophical framework, it has not been capable yet of convincing health and education managers of the importance of nursing theory. Let's start with its analysis in order to find its strengths and weaknesses.

According to Fawcett,

> The *Structural Holarchy of Contemporary Nursing Knowledge* is a heuristic device that places the five components of contemporary nursing knowledge into a holarchy based on level of abstraction. [...] With the exception of empirical indicators, the components of the structural holarchy are composed of concepts and propositions. Empirical indicators measure concepts. (Fawcett 2005, p. 4, her emphasis)

Fawcett's structural holarchy organizes nursing knowledge in a stratified structure going from the abstract on top to the empirical in the bottom.

The most abstract component is called the "metaparadigm", and it consists of "the global concepts that identify the phenomena of central interest to a discipline, the global propositions that describe the concepts, and the global propositions that state the relations between or among the concepts" (Fawcett 2005, p. 4).

The metaparadigm acts as "an encapsulating unit, or framework, within which the more restricted [...] structures develop" (Eckberg and Hill 1979, p. 927). It must be taken into account that "The concepts and propositions of a metaparadigm are admittedly extremely global and provide no definitive direction for activities such as research and practice" (Fawcett 2005, p. 4).

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[3] For a brave but flawed attempt to apply metatheoretical structuralism to nursing knowledge, see Salas Iglesias (2003) and Federico (2020) for its critical analysis.

It is interesting to see that Fawcett took such a notion from Margaret Masterman's criticism of Thomas Kuhn's *The Structure of Scientific Revolutions*.

> The idea for a component of knowledge called the metaparadigm arose in discussion of the multiple meanings Kuhn [...] had given to the term paradigm. Masterman (1970) pointed out that one meaning reflected a metaphysical rather than scientific notion or entity and labeled that meaning as the metaparadigm. (Fawcett, 2005, p. 4)

Masterman's work, compiled in the philosophy of science classic *Criticism and the Growth of Knowledge* (Lakatos and Musgrave 1970) was the main cause of Kuhn's abandoning of the very concept of paradigm which lead him to fame, as Masterman claimed to have found 21 different meanings of that concept: an unacceptable ambiguity in a mostly formal field of philosophy. After Masterman's accusation, Kuhn wrote his "1969 posdata" to *Structure* where "paradigm" was disambiguated to "disciplinary matrix" (for science's epistemological unit of analysis) and "exemplar" (for a successful case of problem resolution within the disciplinary matrix, see Giri and Giri (2020)). Masterman's article was very influential in philosophy because of her sharp criticism of Kuhn's ambiguities, but her propositive side (an improved alternative to Kuhn's paradigm framework) didn't achieve such success. However, it is there where Fawcett took inspiration for her structural holarchy. "Metaparadigm", holarchy's top concept, is defined by Masterman in a very large footnote:

> For when [Kuhn] equates 'paradigm' [in *The Structure of Scientific Revolutions*' first edition] with a set of beliefs (p. 4) , with a myth (p. 2), with a successful metaphysical speculation (p. 17), with a standard (p. 102), with a new way of seeing (pp. 177-121), with an organizing principle governing perception itself (p. 120), with a map (p. 108) and with something which determines a large area of reality (p. 128), it is clearly a metaphysical notion of entity, rather than a scientific one, which he has in his mind. I shall therefore call paradigms of this philosophical sort metaphysical paradigms or metaparadigms. (Masterman 1970, p. 65)

A metaparadigm, then, "[...] is concerned with general issues of the subject matter of a discipline [... that is] what a discipline is concerned with" (Kim 2000, p. 28). Even when it is the most abstract epistemological unit, the metaparadigm is of critical importance because it "[...] specifies the wide limits of the phenomenon under scrutiny in a discipline, for example, to separate nursing of other disciplines, like medicine, the clinic exercise of physiology or sociology" (Alligood 2004, p. 32). Fawcett had defined the metaparadigm of any discipline as "a statement or group of statements identifying its relevant phenomena", and claimed that through an extensive review of nursing literature of the time she found conclusive "evidence supporting the existence of a metaparadigm of nursing" (Fawcett 1984, p. 84).

For Fawcett the metaparadigm of nursing is made up of four concepts: Human Beings, Environment, Health and Nursing. "The discipline of nursing is concerned with the patterning of human health experiences within the context of the environment" (Fawcett 2005, p. 6) and "the discipline of nursing is concerned with the human processes of living and dying, recognizing that human beings are in a continuous relationship with their environments" (Fawcett 2005, p. 6).

Beneath metaparadigms, in a lower level of abstraction there lie the "conceptual models" or simply "models".[4] Florence Nightingale (1859) is supposed to have created nursing's first model, but the very concept of "model" started to be used explicitly after the 1970s (as in most traditional sciences, but with a different sense). According to Fawcett,

> [...] a conceptual model is defined as a set of relatively abstract and general concepts that address the phenomena of central interest to a discipline, the propositions that broadly describe those concepts, and the propositions that state relatively abstract and general relations between two or more of the concepts. (Fawcett 2005, p. 16)

For Fawcett, each conceptual model "gives direction to the search for relevant questions about the phenomena of central interest to a discipline and suggests solutions to practical problems. Each one also provides general criteria for knowing when a problem has been solved" (Fawcett 2005, p. 17).

The term "conceptual model", according to the author, is synonymous with the terms "conceptual framework", "conceptual system", "paradigm", and "disciplinary matrix". Fawcett states that a conceptual model:

> [...] provides a distinctive frame of reference—"a horizon of expectations" (Popper 1965, p. 47)—and "a coherent, internally unified way of thinking about [...] events and processes" (Frank 1968, p. 45) for its adherents that tells them how to observe and interpret the phenomena of interest to the discipline. Each conceptual model, then, presents a unique focus that has a profound influence on individuals' perceptions. The unique focus of each conceptual model is a characterization of a possible reality. (Fawcett 2005, p. 17)

Each model (in Fawcett's sense) provides different ways of seeing the metaparadigm's concepts: as in Kuhn's paradigms, two persons with different conceptual models seem to be in different worlds. Every nurse shares their metaparadigm (by definition), but not necessarily their conceptual models.

Conceptual models address the phenomena identified by a metaparadigm and, therefore, incorporate the most global concepts and propositions in a more restrictive yet still abstract manner. Each conceptual model, then, provides a different view of the metaparadigm concepts: "Thus, although conceptual models address all of the concepts representing the subject matter of the discipline, as identified in the metaparadigm, each metaparadigm concept is defined and described in a different way in different conceptual models" (Fawcett 2005, p. 17). In order to illustrate her point, Fawcett mentions that:

> Neuman's Systems Model (Neuman and Fawcett, 2002) focuses on preventing a deleterious reaction to stressors, whereas Orem's (2001) self-care framework emphasizes enhancing each human being's self-care capabilities and actions. Note that Neuman's conceptual model does not deal with self care, and Orem's does not focus on reactions to stressors. (Fawcett 2005, p. 17)

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[4] Actually, the second component of the structural holarchy of contemporary nursing knowledge is the "philosophy", but here we are only interested in discussing epistemological units of analysis, so we will not take that component into account in our analysis of the holarchy.

Beneath models, going further in the path of lower abstraction, the structural holarchy has "theories", which can be of a more general nature (derived directly from conceptual models, called "grand theories") or a more restricted one (called "middle-range theories").

Regarding the use of the word "theory" in the nursing discipline: "Scholars have used many different terms to refer to theory [...]. Among those terms are atomistic theory, grand theory, macro theory, micro theory, middle-range theory, mid-range theory, practice theory, praxis theory, and theoretical framework" (King and Fawcett 1997 *apud* Fawcett 2005, p. 17). So, the need of making some conceptual refining seems critical inside of the special philosophy of nursing. Fawcett made a great effort in this direction. According to her, theories are "one or more relatively concrete and specific concepts that are derived from a conceptual model, the propositions that narrowly describe those concepts, and the propositions that state relatively concrete and specific relations between two or more of the concepts" (Fawcett 2005, p. 18). One of the functions of a theory, Fawcett states, "is to narrow and more fully specify the phenomena contained in a conceptual model. Another function is to provide a relatively concrete and specific structure for the interpretation of initially puzzling behaviors, situations, and events" (Fawcett 2005, p. 20).

Regarding the relation between theories and models (in her sense), Fawcett claims that:

> The distinction between a conceptual model and a theory should be made because of the differences in the way that each is used –if one is to know what to do next, one must know whether the starting point is a conceptual model or a theory. [...] conceptual models and theories differ in their level of abstraction. A conceptual model is composed of several abstract and general concepts and propositions. A grand theory or a middle-range theory, in contrast, deals with one or more relatively concrete and specific concepts and propositions. (Fawcett 2005, p. 23)

In summary, grand theories are derived directly from conceptual models. These theories can serve as starting points for middle-range theory development. Alternatively, sometimes it is possible to derive middle-range theories directly from the conceptual model, without an intermediate grand theory.

Fawcett proposes the following rule as a guideline for distinguishing between conceptual models and theories:

> [...] if a given work is an abstract and general frame of reference addressing all four concepts of the metaparadigm of nursing, it is a conceptual model. If the work is more concrete, specific, and restricted to a more limited range of phenomena than that identified by the conceptual model, it is a grand theory or middle-range theory. (Fawcett 2005, p. 24)

There is another crucial distinction between models and theories: the way to test them is completely different:

> Four steps are required before a conceptual model can be tested, albeit indirectly. First, the conceptual model must be formulated; second, a middle-range theory must be derived from the conceptual model; third, empirical indicators must be identified; and fourth, empirically testable hypotheses must be specified. In contrast, only three steps are required for empirical testing of a middle-range theory.

> First, the theory must be stated; second, empirical indicators must be identified; and third, empirically testable hypotheses must be specified [...]. A conceptual model cannot be tested directly, because its concepts and propositions are not empirically measurable. More concrete and specific concepts and propositions have to be derived from the conceptual model; that is, a middle-range theory must be formulated. Those more concrete concepts then must be operationally defined and empirically testable hypotheses must be derived from the propositions of the theory. (Fawcett 2005, p. 24)

Grand theories are wide of scope, and are composed of concepts and propositions with lower abstraction (i.e., more specific and concrete) than those present in conceptual models, but more abstract than those present in middle-range theories: Middle-range theories are close to specific phenomena, and so they are able to describe, explain and/or predict them. They also point to certain ways to manipulate phenomena and allow testing grand theories and even models by linking them to empirical predictors (i.e., data). Actually, the most developed nursing knowledge can be regarded as middle-range theories. According to Orlando:

> [Middle-range theories] are composed of a limited number of concepts and propositions that are written at a relatively concrete and specific level. Nurse's activity is an example of a middle-range theory concept [...] such as instructions, suggestions, directions, explanations, information, requests, and questions directed toward the patient; making decisions for the patient; handling the patient's body; administering medications or treatments; and changing the patient's immediate environment. (Orlando 1961, p. 60)

Although it is not a component of Fawcett's structural holarchy, a critical element of middle-range theories is the "empirical indicator", as it allows empirical testing. Fawcett defines empirical indicators:

> [...] as a very concrete and specific real world proxy for a middle-range theory concept; an actual instrument, experimental condition, or procedure that is used to observe or measure a middle-range theory concept. The information obtained from empirical indicators is typically called data.
>
> The function of empirical indicators is to provide the means by which middle-range theories are generated or tested [...]. Nurses have developed a plethora of empirical indicators in the form of research instruments and nursing practice tools. (Fawcett 2005, p. 21)

So, empirical indicators are critical in testing middle-range theories as they "[...] are directly connected to theories by means of the operational definition for each middle-range theory concept" (Fawcett 2005, p. 21). However,

> [...] there is no direct connection between empirical indicators and conceptual models [...] or the metaparadigm. Consequently, those components of the structural holarchy of contemporary nursing knowledge cannot be subjected to empirical testing. [...] Rather, the credibility of a conceptual model is determined indirectly through empirical testing of middle-range theories that are derived from or linked

> with the model. [...] Similarly, the metaparadigm cannot be empirically tested, but should be defendable based on dialogue and debate about the phenomena of interest to the discipline as a whole. (Fawcett 2005, p. 22)

As we said before, Fawcett also found in nursing knowledge middle-range theories that were not derived from grand-theories, as Pamela Reed's Self Transcendence theory (1997). The structural holarchy implies a dynamic of nursing knowledge growth where top-abstract units are created first in a theoretical effort and then bottom-concrete units are derived in an attempt to provide empirical use to such efforts. However, the presence of bottom middle- range theories created out-of-nowhere seems to be a critical counterexample to this idea of science dynamics. Besides, it provides good reasons to avoid investing in theoretical nursing research, as middle-range theories are the only ones capable of being applied in the field: why lose time and money in theorizing about abstract top-level models if the only interesting and useful stuff are the bottom-level theories? Why not leave abstract speculations to philosophers?

Another important issue is that many of the most important middle-range theories used in nursing are not nursing-specific, but are imported from more traditional disciplines such as biology, anthropology or sociology. All these reasons are practical causes for which Fawcett's holarchy fails in providing nursing with good reasons to defend its epistemic status.

3. Metatheoretical Structuralism's notion of Theory-Net

The structuralist way of organizing scientific knowledge is also based on the Kuhnian concept of "paradigm" (or, after 1969, "disciplinary matrix"). This is why linking Fawcett's intuitions with the structuralist program is relatively easy.

According to Kuhn, disciplinary matrices have, among other components, laws (in Kuhnian jargon "symbolic generalizations"). But not every law has the same hierarchy, as there are general (or schematic) generalizations and specific laws. For example, in Classical Mechanics:

> [...] generalizations [like Newton's second law] are not so much generalizations as generalization-sketches, schematic forms whose detailed symbolic expression varies from one application to the next. For the problem of free fall, $f = ma$ becomes $mg = md^2s/dt^2$. For the simple pendulum, it becomes $mg\mathrm{Sin}\theta = -md^2s/dt^2$. For coupled harmonic oscillators it becomes two equations, the first of which may be written $m_1d^2s_1/dt^2 + k_1s_1 = k_2(d + s_2 - s_1)$. More interesting mechanical problems, for example the motion of a gyroscope, would display still greater disparity between $f = ma$ and the actual symbolic generalization to which logic and mathematics are applied. (Kuhn 1970, p. 465)

This idea was elaborated in metatheoretical structuralism (for a detailed account see Balzer, Moulines and Sneed 1987; Díez and Lorenzano 2002) through the concept of theory-nets. The nomic components of a disciplinary matrix in a certain historical moment can be presented hierarchically in a theory-net where the most abstract unifying component (i.e., Newton's second law, $f = ma$, in the example of classical mechanics) lies on top, and every step down introduces new nomological constraints (for example, space or velocity dependence). At the bottom of the network there are specific laws, where the guiding principle takes a specific form for a specific phenomenon (in the example, pendula, free fall, harmonic oscillators or gyroscopes).

It must be taken into account that in metatheoretical structuralism the relation between top and bottom nomic components is not one of (deductive) "derivation" (as happened in traditional positivist philosophy of science, which inspired Fawcett's metatheoretical structures, see Risjord (2010) and below), but of "specialization" (see Balzer, Moulines and Sneed 1987). In the structuralist sense of specialization, bottom laws specify functional dependencies that are left partially open in the top laws. Another consequence of this relation is that top-abstract laws cannot be tested in isolation, as they must be "concretized" in order to be part of experiments. This means that bottom-concrete laws are the ones actually empirically tested (as they are the ones that have more empirical information). General laws are only indirectly tested through the bottom laws. Sometimes, bottom laws are blamed for failed predictions, in order to protect top-general laws.

In metatheoretical structuralism, as in every semantic (or model-theoretical) conception, a theory is not a mere linguistic entity. The most basic component of a theory is a class of models, in the formal sense—as "a system or structure which intends to represent [...] a 'piece of reality', built from entities of different kinds that realize a kind of statements, [...] or, more precisely, the statements are true in such systems" (Díez and Lorenzano 2002, p. 28). However, the main difference between structuralism and other semantic conceptions is that for structuralism a theory must not be identified with a class of structures (or models), but with a series of classes of structures hierarchically organized. Each class of structure is called a "theoretical element", and the whole series is called a theory-net. A theory consists, usually, of a hierarchical network of theoretical elements.

The simpler kind of structure capable of being considered a proper formal elucidation of the intuitive concept of scientific theory is the theoretical element, which can be identified, roughly, with an ordered pair of the following elements: on the one hand, a "theoretical core" which expresses the conceptual resources at different levels and the nomic constraints (i.e., laws) which, according to the theory, rule the phenomena of interest and, on the other hand, a set of "intended" or "intentional applications" (i.e., the empirical systems to which the theory is meant to be applied, the ones allegedly ruled by its laws). We do not need to enter into all the formal structuralist apparatus in detail here, but in order to account for an explananda (e.g., a planetary trajectory in Classical Mechanics), an explanatory theory introduces T- theoretical terms (e.g., mass and force in Classical Mechanics) and postulates non-accidental connections between these theoretical terms and the T-non-theoretical ones mentioned above (e.g., mechanical laws. For more details in this topic, see Balzer, Moulines and Sneed (1987)).

Some examples of scientific theories actually can be reconstructed with just a theoretical element. However, this is only true for the simplest theories. Usually, individual theories must be considered as an aggregate of several (sometimes a lot of) theoretical elements. These aggregates are the theory-nets. This reflects the fact that many theories have laws with different degrees of generality within their conceptual frameworks, from the top-general ones to the bottom branches' more concrete and restricted ones.

The relation between the theoretical elements of a network is one of "specification" or, in the structuralist jargon, of specialization, which is a non-deductive, reflexive, antisymmetric and transitive relation (see Balzer, Moulines and Sneed 1987).

In summary, for metatheoretical structuralism, a theory-net is a set of theoretical elements linked through a specialization relation. A theoretical element that is not a specialization of another theoretical element is called a "basic theoretical element". Even when in principle it is possible that theory-nets possess many different shapes, every case reconstructed so far is tree-shaped, with a single basic theoretical element in the top and different branches all the way to the terminal specializations.

It is possible to see that both metatheoretical structuralism and Fawcett's schemes share the idea of hierarchical structures where the more general-abstract elements lie on top and the more concrete-empirical elements lie in the bottom. However, the structuralist framework has many advantages over Fawcett's holarchy.

In the first place, structuralism was applied for analyzing several branches of factual sciences, including the most traditional ones, like physics, chemistry, biology, and social sciences (for a compilation of structuralist metatheoretical exemplars, see Diederich, Ibarra and Mormann (1989; 1994) and Abreu, Lorenzano and Moulines (2013)). This serves not only as a proof of its fruitfulness: if nursing wants to defend its epistemological status, it seems to be a good strategy to show that the structure of its knowledge is analogue to those of the most respected and traditional sciences. This can only be properly done using the same tool applied to such traditional sciences.

In the second place, semantic conceptions of theories (unlike traditional syntactic conceptions) characterize theories as "extralinguistic entities", which allow them to identify theories with different linguistic formulations. This is a great advantage as the specific formulation of a theory does not affect its content (see Lloyd 2006). For this reason, theories reconstructed axiomatically in the first order logic (as traditional positivist philosophy of science recommended) are generally very simple and highly schematic (see Díez and Lorenzano 2002), while those reconstructed for the semantic family can be far more complex. In the third place, it is necessary to explain the fact that scientists want their theories (even the more abstract ones) to be applied to explain, predict and describe empirical phenomena. This is captured, in metatheoretical structuralism, by the notion of empirical assertion. This relation is described in different flavors by different members of the semantic family. Structuralism shares with historicist conceptions awareness about the existence of many essential elements which are irreducibly pragmatic and historically relative, and so they cannot be treated in a purely formal way. However, while the historicists deny it, structuralism states that those elements can be treated by rigorous conceptual analysis. This is why structuralism claims to have recovered the best of both classical and historicist philosophies of science (see Díez and Lorenzano 2002). As structural holarchy is all made of linguistic propositions, it is not able to grasp those pragmatic and historically relative (i.e., not possible to formalize) elements of real scientific practice.

4. Analyzing Fawcett's structural holarchy of contemporary nursing knowledge from metatheoretical structuralism's viewpoint

In this section we will analyze critically Fawcett's structural holarchy, in order to identify and describe with higher precision its epistemic elements: metaparadigms, models, general theories, middle-range theories. Let's begin with the first one, from top to bottom.

Masterman (1970), after finding the 21 different senses of "paradigm" in Kuhn's *Structure*, condensed them in three core senses: the metaphysical sense that Fawcett used in her structural holarchy (see Section 2), a sociological sense (of shared habits within a scientific community) and an artifactual sense (of an instrument for solving puzzles, see Federico (2020)). As Masterman noticed (and Kuhn agreed in his 1969 posdata), none of these senses are equivalent with the traditional senses of "theory", which were of a linguistic nature.

The metaphysical notion of paradigm, applied to nursing knowledge, is supposed to have propositions shared by every nurse in the world. No matter where she was educated, every nurse is supposed to share interest in caring, human beings, health, the environment and of course, the relations between those elements. Badillo Zúñiga, Ostiguín Melénez and Bermúdez González (2013) and also Siles González and García Hernández (1995) stated that it is not necessarily true that every nurse share their metaphysical commitments, and that the effort that Fawcett did in including the metaphysical sense of paradigm in her structural holarchy would have been more fruitful if she would have chosen the sociological sense, as metaparadigms seem to only have the function of delimiting the nursing discipline from other ones.

When Fawcett describes metaparadigms, as seen before, she claims them to be composed of (metaphysical) propositions, from which the following components of the structural holarchy (i.e., models) are derived (through logical deduction). A sociological sense of paradigm would do the job of delimiting the field of interest, but would not be composed merely of propositions (Kuhn's philosophy as presented in *Structure* is not of a linguistic nature as the received view one which seem to be implicitly in the back of Fawcett's thoughts), and so it will not allow nursing theorists to "derive" conceptual models from there. This may be seen as a disadvantage, but philosophically, the idea of "deriving" conceptual models from metaphysical propositions is no longer attainable (see Grünbaum 1988). The relation of specialization is much clearer and more fruitful in order to explain the links between general abstract statements and lower level concrete statements. So, there is no need for nursing philosophy to attach itself to the concept of metaparadigm in order to explain the particular nature of the nursing scientific community. As Kuhn himself stated, "Scientific communities can and should be isolated without prior recourse to paradigms; the latter can then be discovered by scrutinizing the behaviour of a given community's members" (Kuhn 1970, p. 176).

Then, in order to isolate nursing's scientific community, it seems to be a better strategy to see what every nurse has in common in their behaviour rather than to see what they have in common in their metaphysical assumptions, even if that doesn't fit in the holarchy's linguistic structure. This strategy would let Fawcett's conceptual models as the top hierarchical component in the structural holarchy.

According to Fawcett, each conceptual model "is a distinctive professional nursing perspective that stipulates rules, or guidelines" (Fawcett 2005, p. 36), and "the expectation is that the nursing model enhances understanding of the phenomena of interest to nursing" (Fawcett 2005, p. 57). "Conceptual models" are allegedly equivalent to Kuhn's disciplinary matrixes, but, according to Fawcett, they are also equivalent to Popper's "horizon of expectations", Frank's "coherent, internally unified way of thinking about [...] events and processes" and Laudan's "research traditions" (see Fawcett 2005, pp. 36-37). There is a sense in which these concepts capture an intuition about shared values, methodologies, ways of thinking about phenomena, etc., but clearly they are not equivalent concepts, which make Fawcett's proposal very imprecise. This is a consequence of formulating a philosophical theory based on many antagonistic authors. Doing the job with a single well-developed metatheoretical stance will certainly avoid such dangers.

Fawcett's refining attempt of her concept of "conceptual model" made great efforts to pair it especially with Laudan's (1981) concept of "research traditions". That concept allowed Fawcett to explain the non-directly-testable character of conceptual models:

> Research traditions are not directly testable, both because their ontologies are too general to yield specific predictions and because their methodological components, being guidelines or norms, are not straightforwardly testable assertions about matters of fact. Associated with any active research tradition is a family of theories. [...] The theories [...] share the ontology of the parent research tradition and can be tested and evaluated using its methodological norms. (Laudan 1981 *apud* Fawcett 2005, p. 36)

According to Fawcett, the content of a nursing model is presented in the form of abstract and general concepts and propositions and provides general guidelines for nursing. Therefore to test a model it is necessary to "derive" (deductively) one or more theories from there. Once again, Fawcett faces a critical issue here, in taking her "models" as equivalent to Laudan's research traditions. As it happens with Kuhn's paradigms (or disciplinary matrixes), they are not wholly linguistic epistemological units of analysis as they are not the sort of things composed only by "concepts" and "propositions". They have epistemic values, ontologies and methodologies, and are not only propositions about the phenomena of interest that would eventually allow a nurse to derive lower-level propositions. Both Kuhn and Laudan have developed their historicist frameworks of philosophy of science in opposition to traditional philosophies which had as their core unit of analysis "theories", defined as a collection of axiomatically ordered propositions about certain phenomena. By pairing her linguistic models with Kuhn's disciplinary matrixes and Laudan's research traditions, Fawcett is basically comparing apples with oranges.

On the one hand, nursing models must be related to the metaparadigm:

> It also seems reasonable to expect the relational propositions of the nursing model to link all four metaparadigm concepts [i.e., care, health, environment and human beings]. This may be done in a series of propositions that reflect linkage of two or more metaparadigm concepts, or it may be accomplished by one summary statement encompassing all four metaparadigm concepts. (Fawcett 2005, p. 54)

On the other hand, at least one theory must be able to be derived from the model:

> The credibility of a nursing model cannot be determined directly. Rather, the abstract and general concepts and propositions of the nursing model must be linked with the more concrete and specific concepts and propositions of a middle-range theory and appropriate empirical indicators to determine credibility. The resulting conceptual-theoretical-empirical system of nursing knowledge then is used to guide nursing activities, and the results of use are examined. Thus credibility of nursing models is determined through tests of conceptual-theoretical-empirical systems of nursing knowledge. (Fawcett 2005, p. 55)

It should not be expected that a model will account for all the phenomena of interest in nursing: "One should not criticize a nursing model for failing to consider, for example, problems in self-care abilities when the model emphasizes the nurse's management of stimuli to promote adaptation" (Fawcett 2005, p. 57).

However, Fawcett does have some intuitions about hierarchy and testability of propositions going in a right direction (even with several imprecisions). Structuralism, as we will show soon, does capture such intuitions in a precise way.

On the one hand, the idea that a "conceptual model" determines a domain or gives account of a more or less heterogeneous set of phenomena is understood in structuralism by characterizing the basic theoretical element of a theory-net, particularly through the notion of pretended application.

On the other hand, the intuition that a model provides a "guideline" or "heuristics" of what to research for and how to work is precised through the notion of "fundamental law of the same theoretical element" or "guiding principles".

Guiding principles are "programmatic" as they tell us the kind of things we should look for when we want to explain a specific phenomenon, and they provide a unifying nomological factor, but taken in isolation (i.e., without their specializations, something that rarely happens in real science), empirically they say very little. As explained in Section 3:

> This peculiar epistemic status of general guiding principles has the consequence that, after a failed prediction, one may change the general principle but one can also try to fix the anomaly by modifying only the specific law. A succession of different theory-nets preserving at least the top theory-element constitutes the evolution of a single theory over time (Kuhn's normal science). (Alleva, Díez and Federico 2017, p. 3)

Thomas Kuhn's synchronic notion of paradigm (or disciplinary matrix) can be properly captured and elucidated through the structuralist notion of theory-net. This can be useful to build linkages between Fawcett's holarchy and structuralism's theory-nets, as Fawcett herself assimilates her models to disciplinary matrices (and then to Laudan's research traditions). Kuhn's "symbolic generalizations" are captured by structuralism's "fundamental laws", while "paradigmatic exemplars" are captured by "successful applications" (i.e., applications that work as typical exemplars for other applications of the theory of interest).

Once again, the idea that the testability of Fawcett's models is done via less abstract propositions (which are components of the theories previously derived from such models) seems intuitively right. However, the idea of deductively deriving bottom level propositions from top level propositions is no longer attainable in philosophy of science (see Díez and Moulines 1997). Besides:

> Note that [in metatheoretical structuralism] the top-to-bottom relation is not one of implication or derivation, but of specialization in the structuralist sense (Balzer, Moulines and Sneed 1987): lower laws are specific versions of higher ones, i.e., they specify some functional dependencies that are left partially open in the laws above them in the branch. It is worth emphasizing that the difference between top general guiding principles and the other laws has epistemic import. Top general principles cannot be empirically tested "in isolation"; they can be tested, and eventually falsified, only through one of their specific versions for a specific phenomenon. (Alleva, Díez and Federico 2017, p. 3)

The specialization relation provides to the laws in a theory-net a "logical congruence" that Fawcett demands to the epistemic components of a structural holarchy, which seems to be another point of compatibility between her proposal and structuralism:

> [...a] step of evaluation of a nursing model considers the logic of its internal structure. Logical congruence is evaluated through an intellectual process that involves judging the congruence of the model author's espoused philosophical claims with the content of the model. In addition, the process requires judgments regarding congruence of the world view(s) and category(ies) of nursing knowledge reflected by the model. (Fawcett 2005, p. 54)

According to Fawcett,

> Grand theories and middle-range theories are derived from nursing models. Thus, the extent to which the nursing model leads to generation of theory should be judged. The need for logically congruent conceptual-theoretical-empirical systems of nursing knowledge for nursing activities mandates that at least some theories be derived from each nursing model. The expectation, therefore, is that the abstract concepts and propositions of the model be sufficiently clear so that the more concrete concepts and propositions of grand theories and middle-range theories can be deduced and testable hypotheses can be formulated. (Fawcett 2005, p. 55)

Theories, in Fawcett's proposal, allow nursing practice and research, and are constantly tested in everyday work. "The ultimate goal of theory development in such professional disciplines as nursing is the empirical testing of interventions that are specified in the form of predictive middle-range theories" (Fawcett 2005, p. 444). This is allegedly achieved through less abstract concepts and propositions:

> The concepts of a theory are words or groups of words that express a mental image of some phenomenon. They represent the special vocabulary of a theory. Furthermore, the concepts give meaning to what can be imagined or observed through the senses. They enable the theorist to categorize, interpret, and structure the phenomena encompassed by the theory. The propositions of a theory are declarative statements about one or more concepts, statements that assert what is thought to be the case. (Fawcett 2005, p. 443)

As seen before, Fawcett describes two kinds of theories: grand theories and middle-range theories (see Section 2). Only the latter can be directly tested:

> The relatively abstract and general nature of grand theories means that their concepts lack operational definitions stating how the concepts are measured, and their propositions are not amenable to direct empirical testing. In essence, then, evaluation of the testability of a grand theory involves determining the middle-range theory-generating capacity of a grand theory. The criterion of testability is met when the grand theory has led to the generation of one or more middle-range theories.
>
> The relatively concrete and specific nature of middle-range theories means that their concepts can have operational definitions and their propositions are amenable to direct empirical testing. Consequently, an approach called traditional empiricism is used to evaluate the testability of middle-range theories. That approach requires the concepts of a middle-range theory to be observable and the propositions to

> be measurable. Concepts are empirically observable when operational definitions identify the empirical indicators that are used to measure the concepts. Propositions are measurable when empirical indicators can be substituted for concept names in each proposition and when statistical procedures can provide evidence regarding the assertions made. The criterion of testability for middle-range theories, then, is met when specific instruments or experimental protocols have been developed to observe the theory concepts and statistical techniques are available to measure the assertions made by the propositions. (Fawcett 2005, p. 444)

Under structuralism, Kuhn's "exemplars" are understood as successful applications, delimiting the set of intentional applications. The process of problem-solving (or puzzle- solving) can be described as the process through which the users of a theory introduce additional constrictions to the basic theoretical core of a theory-net, in particular to the fundamental laws, in order to obtain successful applications of the core. Those successful applications are empirical assertions linked to the terminal specializations which "pass the test of experience" (i.e., "corroborated", in popperian jargon, or "confirmed", in carnapian jargon).

Here we provided an account on how the notion of theory-net allows us to better understand the links between Fawcett's models, theory and experience. Now we are going to precise, under metatheoretical structuralism, the difference between Fawcett's grand theories and middle-range theories.

Our intuition is that a middle-range theory is a (little) theory-net in itself when it is not linked to any general theory (or to a conceptual model), or, if linked, it is a part of a wider theory-net. In the first case, middle-range theories are networks with few theoretical elements with a basic theoretical element without laws of a high degree of abstraction. In the second case, the middle-range theory's network is integrated to a network of many elements and its basic theoretical element does have laws with a high degree of abstraction which often allows unifying the theory to other theories, in a big network compatible with Fawcett's idea of a conceptual model. One illustrating example is Orem's Self-Care framework (see Rempenning and Taylor 2003), which we will reconstruct in the next section. Fawcett characterizes it as follows:

> Orem has referred to her work as the Self-Care Deficit Theory of Nursing, the Self-Care Nursing Theory, and a general theory of nursing. In various explications of the structure of her work, Orem [...] has stated that the general theory of nursing is a conceptual framework or conceptual model or frame of reference that contains three parts –the Theory of Self-Care, the Theory of Self-Care Deficit, and the Theory of Nursing Systems. The concepts and propositions of the general theory of nursing are written at the level of abstraction and generality usually seen in a conceptual model. (Fawcett 2005, p. 223)

The very concept of "middle-range theory" was originally defined by sociologist Robert K. Merton (1968). It is suspicious that Fawcett never mentions such a point, even when she uses that concept extensively along her work.

> According to Merton, whose discussion of middle-range theory is congruent with the way the term is used in the nursing literature, middle-range theories are the theories that fall between the working hypotheses that are an essential part of

> conducting research and the all-inclusive systematic efforts to develop a unified theory [of the discipline]. (Lenz *et al*. 1995, p. 2)

A series of articles analyze the mertonian notion of "middle-range theory" via structuralist tools (Lorenzano and Abreu 2010; Abreu 2012; 2014; 2019; 2020). Lorenzano and Abreu claimed that:

> The network structure that, according to the structuralist conception, generally is adopted by theories, can also help us to understand that attribute of middle-range theories that Merton had in mind [...]. If this claim is considered in the historical development of the discipline, from certain theoretical networks with different basic theoretical elements representing middle-range theories, it would be possible to propose more general theoretical elements which encompass them as specializations of the middle range theories' basic theoretical elements (and their specializations), as it happened with Galilean mechanics and Kepler's laws when they were unified to newtonian mechanics. (Lorenzano and Abreu 2010, pp. 490-491)

Just as Fawcett, Merton thinks of his proposal of middle-range theories with a positivistic flavor, where the deduction relation between propositions comes attached. About this, Lorenzano and Abreu stated that:

> [...] according to Merton, from the assumptions (or laws) of middle-range theories "specific hypotheses are logically derived and confirmed through empirical research". This position [...] is a clear example of what [Alberto] Coffa called "deductive chauvinism", who, by coining such denomination "made more than inventing a clever term. He recognized—and directed his sharp attention—to a dominant and often unconscious tendency to force philosophical concepts and theories through a deductive mold" (Grünbaum 1988, p. xv). This is a typical tendency in the classical conception of theories. Instead, the structuralist conception is not limited, neither in its treatment of the relations of the series of classes of structures hierarchically organized with which, in any case, would identify a theory, nor in the theories and the susceptible to empirical contrastation "specific hypothesis", by this "deductive chauvinism". (Lorenzano and Abreu 2010, p. 490)

Below we will present a structuralist reconstruction of an example of a "model" according to Fawcett, and its application in nursing: Dorothea Orem's Self-Care Deficit Framework. We hope that by showing the said exemplar, the advantages of the structuralist tools for analyzing nursing theories that we introduced in this section will become clearer.

5. The Self-Care Deficit Theory of Nursing in the light of Structuralism

The Self-Care Deficit Theory of Nursing or Self-Care Nursing Theory (SDTN), was shaped between 1958 and 1980. It's founded by four sub-theories (Orem 2003a; Banfield and Orem 2014): the Self-Care Theory (SCT), the Dependent-Care Theory (DCT, which may or may not be explicit), the Self-Care Deficit Theory (SDT) and the Nursing Systems Theory (NST). Orem

(2003a) states that both the theory (referring to the whole model) and its conceptual structure are accepted as part of the practical science of the theoretical nursing. Next we will present the concepts and the structure of the theory as Orem proposed it.

6. The theoretical network of Self-Care Deficit Theory of Nursing

Orem (2003a; 2003b) developed the following concepts of SDTN:

1. The nursing variable "nursing agency" which refers to the power to provide care.
2. Two patient variables: (a) "self-care agency", the capacity of individuals to meet their ongoing requirements necessary for life, health and well-being and (b) "therapeutic demand for self-care", that is, the actions to be taken to meet the requirements for self- regulation of one's own human functioning.
3. The concept "self-care deficit" has as its referent a state where the self-care agency is unbalanced due to the conditioning effects of health or health-related factors in meeting the therapeutic demand for self-care.
4. "Self-care" refers to the regulatory actions of people who are aware of and invest efforts to meet their therapeutic demand for self-care.
5. The "nursing system" refers to the actions taken by nurses who activate their nursing agency to know the values of the patient's self-care agency, their therapeutic demand for self-care and the relationships between these, and with this knowledge act to ensure that the patients' therapeutic demand for self-care is met, that self-care agency is protected and that the exercise or development of self-care is regulated.
6. The concept of "basic conditioning factors" refers to the internal or external conditions that affect the current or future values of self-care agency and the therapeutic demand for self-care.
7. The "universal self-care requirements", which are common to all human beings at all stages of the life cycle that must be adjusted to each individual's age, developmental status, environmental factors and health status, and other conditioning factors. When these adjustments have been made, the requirement is said to be particularized for the individual. Eight universal self-care requirements have been described that practitioners consider to have validity. Orem (2003a) distinguishes "developmental requirements" and "health deviance" requirements for those with pathology.

Succinctly, SDTN proposes that people have the capacity to care for themselves, even in times of illness. People can restore their health and well-being through self-care, understood as the ability of all people, according to Self-Care Theory (SCT), to meet their regulatory and developmental needs (universal requirements and basic factors). Self-care is a process that is learned from childhood to adulthood in contact with other members of society. It is only when the individual is unable to meet her demand for self-care, according to the Self-Care Deficit Theory (SDT), that the health system appears, assessing the needs of the subject, now a patient, and providing the care that the subject is unable to provide for herself. This is developed in the Nursing Systems Theory (NST).

Inside of Orem's framework the health system is not invasive, only supplementary to the unmet needs of the individual, so three ways of intervention or systems are distinguished: the fully compensatory, the partially compensatory and the educational support. The health system, particularly the nursing system, will act until the subject's self-care capacities, state of health and well-being are restored.

Taking into account these concepts and applying the structuralist instrumental (Balzer, Moulines and Sneed 1987), the top abstract general statement that is believed to work as a heuristic guide, like a law, when applying SDTN, is a statement that proposes how individuals do to maintain their state of health and well-being:

> [...] therapeutic self-care needs at any point in time describe the patient or environmental factors that need to be kept stable within a range of values or made to reach that range for the good of the patient's life, health, or well-being. (Orem 2001, p. 238)

But this statement does not make explicit the totality of concepts that the author uses. In the task of conceptual elucidation, i.e., of clarification and subsequent reconstruction of this statement, it is found that the concepts actually used are many more, namely, for every person:

$$\alpha(t) = k.e(t)$$

Where "α" assigns to a person according to factors and requirements, at a time "t", a care action (delimited by the care agency). For this purpose, with the determined conditioning factors, at a given time, a state of requirements is previously assigned to a patient, e.g., glycemic state, state of maturity, state of nutrition, etc. Among these states are the "desired states" or "set points" known as "*Esp*", those that indicate health or well-being. Each of these, whether biological, psychological or social, are determined by the respective theories of each disciplinary field.

The factor "e" (demand for care) is the (modulus of the) value of the state in which the person is. It is determined as the difference between the current state of the assessed requirement minus the value of the desired state. Thus, "e" identifies the amount of care (self-care or demand for care) that an individual needs.

Finally, "k" (proportional factor) relates the care action to the deviation from the state in order to keep the requirement states stable within a range of values or make them reach that range for the good of life (Orem 2003b) and its value indicates the intensity of system action, which could be drastic or mild. The concept "k" is not explicit in the theory and is generally not quantified, but it is necessary to achieve dimensional consistency, e.g., for the "maintenance of sufficient water intake" when the subject is dehydrated, "k" relates the variable "blood pressure" to "fluid intake into the body" and determines, for example, the intensity of the serum flow if the patient's hydration is carried out in the health system. It thus makes it possible to relate care actions to states in a particular way, so that the actions are compatible and proportional to the variable to be adjusted in order to restore the patient's self-care.

Presented under this formalism, the law states that: the care actions that a subject needs at each moment of his life depend on "k" and the demand for care "e", which the subject experiences, in such a way that these actions allow her to maintain or, if necessary, return to the ideal state of health or well-being, "*Esp*".

The legaliform statement presented here takes up Orem's intuition when she proposes her notion of "therapeutic demand for self-care". According to the Structuralist analysis of SDTN, it is understood that there is another sub-theory that is concealed, or not made explicit in the nursing literature, of a more general nature, with this law, and which we propose to call "Care Theory" (CT). CT would correspond to the basic theoretical element of the network, the more general-abstract element.

However, according to SDTN, a person who exercises self-care and is in a healthy state is not the same as one who does not. Only in the latter case and if the person cannot satisfy her demand for self-care, will the nursing staff intervene. Next, we will proceed to demonstrate how Orem's framework is applied.

The research that will be reconstructed is that of Sousa *et al.*—"Testing a Conceptual Framework for Diabetes Self-Care Management" (2004)—, a research case which was pointed as relevant by Fawcett (2018) herself.

7. A case of nursing practice as a successful application of SDTN

The proposal of Sousa *et al.* (2004) aims to analyze the self-care of diabetic patients, evaluating how certain individual and "environmental" factors impact self-care.

Among the intervening factors are: gender, age, race/ethnicity, education (formal), type of diabetes, duration of diabetes, household income and social support. Self-perception of health and knowledge about diabetes were among the requirements noted. The study was developed in a population of 141 adult subjects, of both genders and different races, with insulin- dependent diabetes mellitus (Type I and II), discharged from a clinic in the southeastern United States.

Now, according to the elucidatory proposal, the "care actions" that a subject needs, at each moment of her life, are given by the function $\alpha(t) = k.e(t)$ and, in turn, the "care demand" is calculated in the patients from the "current state δ" (which is assigned to a patient, according to the determined conditioning factors, a state of requirements) of those patients and the wellbeing or expected state.

The 141 patients, due to their condition of insulin-dependent diabetics, present a "δ" far from the state of wellbeing, which indicates a "demand for care" since $e = |\delta - Esp| \neq 0$, which will yield a specific "care action", since $\alpha(t) > 0$ and, therefore, the subjects are in an "unhealthy" situation, ruling out the "healthy" situation.

According to this SDTN reconstruction proposal, there are two ways in which unhealthy subjects return to the initial state, "*Esp*": the subjects have self-care capacity, situation (1), or they do not, situation (2). The first of the situations corresponds to the Self-Care Theory (SCT). While the second of the situations corresponds to the Self-Care Deficit Theory (SDT). The result of the study yielded that one variable significantly affects diabetes self-care management positively: the older the age, the greater the diabetes self-care management, suggesting that over time subjects may have developed greater self-care skills, which is consistent with other studies (Hartweg 1993; Anderson, Fitzgerald and Oh 1993). Those with higher education also had greater knowledge of diabetes.

Regarding the impact of the requirements, which are those variables that can be manipulated according to the present elucidation, self-perception of health had a significant effect on self-care agency. Individuals who rated their health as good or excellent had better self-care agency, as did caucasians, those with longer duration of diabetes and individuals with more education about the disease, which reflects that greater knowledge of diabetes allows learning and mastering specific skills that are fundamental for its monitoring and management (Sousa *et al.* 2004).

Based on the above, SDTN allows us to evaluate that in the situation of "demand for care", it is the elderly with more education, with more knowledge of the disease and with a better self-perception of their health (variables understood as the "factored" and "requisitioned" state ∂ of the patients), who achieve self-care optimally, situation (1), since $\alpha(\mathrm{t})$ is proportional to $|e|$ and $\alpha(t) \subseteq SA$, "SA" being understood as the set of self-care actions. Thus the "self-care agency" satisfies the "self-care demand" that the subject needs.

Younger patients with lower education, who are also those who present less knowledge of the disease and worse self-perception of their health, (variables understood as the "factored" and "requisitioned" state "∂" of the patients), do not achieve self-care and, therefore, are in situation (2), since $\alpha(t) < |e|$, which indicates that the subject cannot satisfy her "self-care demand", i.e. there is a "self-care deficit".

When there is a deficit, the individual's self-care therapeutic demands are greater than those of her self-care agency. The individual cannot perform all the necessary personal care actions to attend her needs, and the individual must have the assistance of others (Sousa *et al.* 2004). This is the moment when health systems must act, which is theorized in Orem's Nursing Systems Theory (NST), one of the lower level theoretical elements in SDNT's network.

Sousa *et al.* (2004) provide some guidelines on how the health professional should remedy such a deficit, in order to avoid complications derived from diabetes, betting on a "support-educational system". This assessment is made taking into account that $|e| > \alpha(t)$, meaning that the deficit is greater than the self-care that can be performed by patients, but also that such deficit is not large enough for a drastic intervention, since $\alpha(t) \subset (SA \bigcup DA)$, where $SA > DA$, indicating that self-care actions (SA) can, by strengthening them, be greater than the required dependent-care actions (DA). Therefore, the authors suggest that the nursing professional should intervene through an individualized education program (Sousa *et al.* 2004).

Returning to the structuralist proposal of theoretical network (Section 3), we show here that like Classical Mechanics, Orem's framework can be reconstructed as a theoretical network. The SDTN theoretical network presents the nomic components ordered by degree of abstraction. The highest part of the network presents the most abstract component, what we call the Care Theory (CT), whose fundamental law takes the form of $\alpha(t) = k.e(t)$ and which presents a great unifying power. In the lower parts of the network appear the more specific components, which explain how the professional should proceed in the face of the patient's indicators (factors and requirements), what Orem calls Nursing Systems Theory (NST). In order to determine the extent of care that the health system will provide to the patient, namely "fully complementary system" (FCS), "partially compensatory system" (PCS), or "support-educational system" (SES), the practitioner must first assess the patient's self-care capacity, assessing whether the data presented are subsumed under the specialized law, which takes the form $\alpha(t)$ is proportional to $|e|$ and $\alpha(t) \subseteq SA$ of Self-Care Theory (SCT), or are subsumed by the specialized law, which takes the form $\alpha(t) < |e|$of Self-Care Deficit Theory (SDT).

The SDTN theoretical network presents the tree-like form of Figure 1.

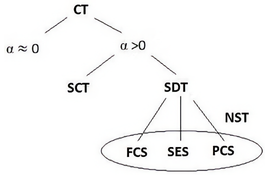

Figure 1: SDTN's theory-net

Basically, our reconstruction shows that a fundamental piece of nursing knowledge as Orem's SDTN has a structure that is similar to most of the traditional scientific theories reconstructed by structuralism. It also shows that the structure of the theory can be shown clearly with a formal tool, which can accommodate with high precision every element of the theory in both its more abstract general elements and its more concrete, empirically field-applied ones, which have a non-deductive relation of specialization.

8. Final considerations

Risjord (2010) points out that in the mainstream tradition in nursing philosophy, the one started by Fawcett, middle-range theories are defined as testable concrete theories in contrast with grand theories. However, Fawcett's understanding of what a theory is based on the viewpoint of the received view, an abandoned viewpoint within professional philosophy of science:

> The received view of theory suggests a particular image of the sciences. Understood as an axiomatic system, a theory is structured like a pyramid. The fundamental laws (axioms) are at the apex. These entail a larger number of middle-range theories. The large body of observation statements is at the base, supporting the whole edifice. (Risjord 2010, p. 110)

The alternative way of conceptualizing a theory, continues the author, arose from the critique of the received view:

> The "semantic conception of theories," of which [Frederick] Suppe was a prominent proponent, emphasized the use of modeling, especially causal modeling, in scientific understanding. In the philosophy of science, this view has helped to illuminate the way that underlying mechanisms explain complex phenomena. (Risjord 2010, p. 141)

Generally speaking we agree with Risjord's point, but with a small dissent. There are many different schools within the semantic conception of theories, and Suppe's is certainly not the most developed one (but it is the one that has been previously applied in nursing philosophy, see Suppe and Jacox (1985), Suppe (1993) and Lenz *et al.* (1995)). In this article we have pointed out that Fawcett's intuitive insights can be captured best by another school within the semanticist family, namely structuralism, as Hernández Conesa (2006) has already noticed.

Lorenzano and Abreu have already stated that the kind of mertonian structures (namely middle-range theories) which are so important within both sociology and nursing (see Alligood 2014)

"[...] are in better tune with the semanticist family in general, and with the structuralist conception in particular, than with the classic conception of theories (i.e., the received view conception)" (Lorenzano and Abreu 2010, p. 489).

We have already established (see Section 3) that structuralism is a fruitful program for rational reconstruction of scientific theories. Our elucidation of Fawcett's hierarchical proposal for organizing nursing science (Section 4) shows that nursing structure is not far away from the structure of traditional sciences which has already been successfully reconstructed by structuralist philosophers.

Finally, we showed that SDTN has the characteristic structure of unified theories that can be reconstructed as a single theory-net (Section 5), with a structure similar to other unified theories such as Newtonian mechanics, Thermodynamics, Classical Genetics and others. The structuralist reconstruction of Orem's framework is consistent with Fawcett's insights. However, the reconstruction refines Fawcett's intuitions about the theories that establish Orem's framework and assigns a different level of generality to each of them, shedding light on the existence of an implicit theoretical element (CT) which is necessary in order to unify the whole framework.

The reconstruction of of Orem's framework confirms our hypotheses about the use of the notions of "paradigm", "models" and "theories" in nursing. Namely that in nursing, "models", "theories" and "paradigms" are compatible with "theoretical network" and "theory" (general or mid-range) with specialized parts of the network.

The reconstruction also shows the need for specialized elements so that SDTN can be tested: in Fawcett's words, that there is no direct connection between "empirical indicators" and conceptual models, hence the need for mid-range theories. However, there is room to discuss the place of the notion of "middle-range theories" in the network. That inquiry falls outside of the scope of the present article, but will be the object of future work.

Acknowledgments

This work has been possible because of fundings: PUNQ 0990/19 (National University of Quilmes), PICT 2018-3454 (National Agency for the Promotion of Research, Technological Development and Innovation- Argentina) and UNTREF 32/19 80120190100115TF (National University of Tres de Febrero).

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The Structure of the Medical Clinic

César Lorenzano[1]

1. Introduction

The philosophy of medicine has developed belatedly. Only in 1976, the Philosophy of Science Association of the United States in its Biannual Meeting, asked in the title of a Symposium that is in itself a definition "What does the philosophy of medicine have to do with the philosophy of science?". The Symposium intended to discuss non-traditional questions within the philosophy of science (such as sociobiology or the philosophy of technology), and other more usual topics (The book edited by Frederick Suppe and Peter Asquith (1977), its Part II is precisely entitled: "What does Philosophy of Medicine has to do with Philosophy of Science?"). That very same year, was published in Spanish perhaps the first analysis of medicine as a theoretical discipline, establishing its scientific character and structure (see Lorenzano 1977). The present article continues this line of research—a program that has been developed for more than 30 years—analyzing the structure of what constitutes the core of the medical knowledge, the medical clinic. It uses for this purpose a modified version of the structuralist conception of theories characterizing exemplars of the clinical theory, instead of models—as is done in the standard version. The reasons for this modification lie in the fact that it is not a mathematical theory—as physical theories are—, and that from the beginning its elements exhibit an undoubted empirical interpretation, so that presenting it by mathematical structures such as models and sets, and then interpreting them, would be an unnecessary redundancy. Let us add that the characterization of the exemplars—taking this term in its Kuhnian sense (Kuhn 1962)—is done by means of diagrams, so that their comprehension is not hindered by the complexities of the formal language of sets and models.

We will present—in an imaginary, but no less plausible situation—a physician M who examines patient K. We will follow the steps of his diagnosis—determining its epistemic structure—such

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Abreu, C. (ed.) *Philosophy of Science in the 21st Century. Contributions of Metatheoretical Structuralism.*
Florianópolis: NEL/UFSC, 2023, pp. 181–188.

that this structure can be seen as the one used by the members of the medical community when they diagnose any other similar case.

2. Dr. *M* in his office

Mr. *K* had a winter flu-like illness about a week ago, and he is recovering. But in the seventh day he woke up with fever, elevated pulse rate, moderate to severe respiratory distress and a painful paroxysmal cough, with rusty or bloody expectoration. When he goes to the doctor's office, he presents spontaneous chest pain, which Dr. *M* attributes to the proximity of an internal lesion of the disease. With his fingers he percusses the area, feeling a dull sound—dullness—in contrast to the sonority of the rest of the thorax. When he auscultates, he hears a tubal murmur at that site, instead of the usual soft crackling and air intake. At this point, *M* makes a presumption of pneumonia. The chest X-ray corroborates the diagnosis, which is reaffirmed when bacteriological analysis of the sputum shows the presence of pneumococcus, the usual agent of pneumonia.

Dr. *M*—and *K*, consequently—are fortunate. The rest of the clinical examination is normal, so the evolution of the disease will be as is expected. There are no complications, and it will be cured after a few days of treatment. Before the antibiotic era, a week after the onset of the disease, occurred an episode termed crisis. Then happened that the fever went down and the patient improve and cure, or the patient get worse and died.

M knows that no patient is the same as another—of the same age, the same previous organic and immunological condition—; their lung lesions are not in the same place, don't evolve in the same way and don't respond in the same way to antibiotic treatment.

However, *M* can diagnose and predict the patient's evolution—his prognosis. If we analyze the process by which he infers the nature of *K*'s disease, we find no traces that he deduces it based on a general law of pneumonia, which, moreover, is absent in the textbooks.

So, if there are no general laws, and no deductive process with which he identifies—diagnoses—the disease, how is it possible, based in what epistemic structure, if there is any?

In what follows we will try to answer these questions, central, as I think, to understand the special language of science, its elements and structure, as well as the inferential form it engages.

My bet is that we are faced with a paradigmatic case of the use of a theory—which I will call, for lack of a better name—a clinical-infectious theory, to which other scientific theories will resemble when they are used to characterize the furniture of the world they delimit, and to which they try to predict its behavior.

3. The analysis of language

After narrating this story, which coincides, albeit simplified, with *K*'s clinical history, I will analyze its lexical structure. When we begin the analysis of the clinical history, we notice that it describes what are termed the patient's signs and symptoms: what is observed, and what the patient reports. Words such as fever, cough of certain characteristics, dull percussion, tubal murmur are words that describe what emerges from the exploration of *K*'s body—the signs of the disease. Symptoms are what *K* tells he experiences, the fatigue, the chest pain, the feeling of exhaustion and sickness—although *M* can also perceive it.

As we can see, this is not an empiricism of sensations; the basis of scientific knowledge has to be objective—intersubjective—, and sensations are subjective. Neither is an empiricism that speaks of macroscopic objects and qualities. It is much closer to a basic language described in the early seventies by Hempel (1973) as prior knowledge, and by the structuralist conception (Sneed 1971; Stegmüller 1973; 1974; 1979; Balzer, Moulines and Sneed 1987) as non-theoretical, coming from a prior theory.

Secondly, to this basic language describing what we will term the semiological disease is added another, by which *M* hypothesizes about what lung alterations—organic in general—cause *K*'s symptomatology. To say that *M* thinks it is pneumonia is equivalent to state that he thinks that in the area of the thoracic pain, where henoticethe dullness and the tubal murmur, the air in the lung is replaced by an exudate of certain characteristics, determining what he calls pulmonary condensation, and that is the cause of the semiological disease. This language, different from the semiological one, refers to alterations of at least two specialized disciplines, anatomic pathology and physiopathology.

It has the characteristics of been theoretical, since it is introduced to explain the semiological disease, and it is only relevant if it is causally involved in a defined clinical disease.

We note that both theories—semiology and anatomy-physio-pathology—evolve together following stages stablished by a law-like statement, a prediction that in medicine is termed prognosis.

Finally, in another twist to causality in medicine, in this specific case, since it is an infectious disease, it is established that pneumococcus is the microorganism that causes the lung alterations, which in turn causes the signs and symptoms of *K*. If there are no microorganisms, it would not be an infectious disease.

4. The structure of the medical diagnosis

Based in these distinctions, we analyze the structure of the medical diagnosis, if we considered *K*'s disease a paradigmatic case of pneumonia, and in general of all the diseases. As we have observed, three clearly differentiated stages were followed in *K*'s diagnosis:

i. the finding of the signs and symptoms that classify the patient as a case of a semiological disease;
ii. the hypotheses about the lung alterations, as well as the microbial causal agent;
iii. the subsequent corroboration of the previous hypotheses, and the evolution of the disease—its prognosis.

These stages correspond to three successive characterizations that we term as non-theoretical (semiological); theoretical (the hypothesis that he will presumably present a pulmonary condensation), and finally actual (if the hypotheses are corroborated and the disease evolves as predicts (prognoses) a pneumonia evolutionary law-like statment, that relates all the elements that we find in *K*'s clinical history, and states that signs, symptoms and lesions evolve together towards cure or death.

I would like to point out that the categories of non-theoretical, theoretical and actual, do not characterize mathematical models, but cases—examples—of the microbial theory of diseases, whose structure is shown below by means of diagrams.

4.1. *K* as a non-theoretical exemplar

The diagram (Figure 1) reads as follows. *K* in the time from t_1 to t_2 goes from a previous state—influenza—to serious signs and symptoms.

Figure 1

The diagram describes *K*'s semiological disease in its most general aspects, without specifying the signs and symptoms he has, so that it is suitable—by its generality—to describe any semiological disease, not only *K*, and not even pneumonia.

4.2. *K* as a theoretical exemplar

In this description *K*—or any patient—does not only have certain signs and symptoms *S*. Dr. *M* assumes—hypothetically—that he also presents anatomo-physiopathological *AFP* alterations closely related to *S*, in which the pneumonic bacillus can be found. These facts will have to be corroborated by specific studies.

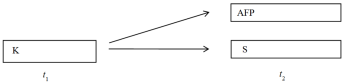

Figure 2

4.3. *K* as an actual exemplar

To be an actual exemplar, *K* must satisfy two conditions. The first is that the organic alterations stated by *M* are corroborated by means of complementary studies—in this case, X-rays, and bacteriological analysis. The second one requires that the non-theoretical and the theoretical elements must evolve as is predicted by *M*.

M knows that before the discovery of antibiotics, a week after the onset of the disease, happened that patients start sweating, and fever and decay and disappear—a process termed crisis—, and a month later also disappears the dullness and tubal murmur. This was a legitimate instance of corroboration of the diagnosis, since only pneumonia evolves in this way.

But *M* belongs to the generation of physician for which antibiotics are so natural as airplanes or cell phones. His prognosis is different. He thinks that crisis should occur two or three days after the administration of antibiotics. If the patient cures quickly with penicillin or equivalents as is expected, it corroborates the diagnosis: it was a pneumonia.

If it does not evolve as expected, it could be another disease that simulates pneumonia, and the presence of the microorganism something totally incidental, since pneumococcus is a regular inhabitant of the organism, which under certain conditions causes the disease to which it gives its name.

If both conditions are satisfied, K is indeed a pneumonic patient. In a diagram:

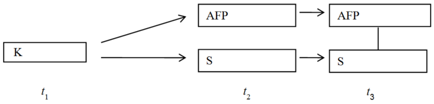

Figure 3

The diagram (Figure 3) shows that S—signs and symptoms—and *AFP*—anatomy-physiopathological alterations—evolve from onset to the end of the disease at t_3, as is predicted. M predicted crisis or death of K within a week. In turn, the cure can be total, or with some scar traces of scarring, sequela of the disease. Pneumonia, unlike many other diseases, does not generally lead to chronicity, another possible evolution. With the appearance of antibiotics the evolution changes, and the patient cures in a few days.

The successive schemes, shown in their most general form, are apt to describe the structure of every medical diagnosis, and consequently of every case—exemplar—of the medical clinic.

If we ask what an exemplar of the clinical theory of diseases is—knowing that the clinic synthesizes the totality of medical knowledge—the answer is similar—but not identical—to the answer of the structuralist conception of theories, in its most general form.

"x is *CTD* exemplar if it is non-theoretical, then theoretical, and finally actual"

The exemplar so fall clinical disease are those who's structure is the same we find in K; i.e. non-theoretical exemplars, related to theoretical exemplars by evolutionary laws that when predicts successfully how they evolve, constitutes actual exemplars.

We will advance a little more in my characterization of the diseases.

A disease is the group of its exemplars, whose members are its actual cases. Who groups them is the medical epistemic community, for the purpose of naming them with the same term, with the consequent linguistic economy that derives from this generalizing maneuver. Dr. M proceeds in no other way when he diagnoses K as suffering from pneumonia. It also includes fictional cases—such as this one—with which the community instructors relate exemplary experiences mainly for teaching purposes.

We will note that this characterization coincides with an old medical proverb that states that there are no diseases, but sick people.

At this point, it is time to look at the consequences of the lexical and structural reconstruction of K disease and its generalization to the other cases of disease. We did it by the procedure of simplifying its elements, eliminating the characteristics that particularize them, and mentioning it with a generic term, for example, signs and symptoms or even more synthetic, a letter S, instead of tubal murmur, dullness, fever, etc., or *AFP*, instead of anatomy physiopathology, or in its more extensive version that speaks of exudate, and microorganisms. As is evident, we don't identify a theory by its models—as is usual in semantic conceptions—, avoiding the problems of relating mathematical entities with the physical systems of factual sciences, and which have not been satisfactorily solved since Plato.

Our strategy avoids these drawbacks, reducing the reconstruction to its factual exemplars—cases—, in which invoking its structure has no greater ontological or lexical difficulty than expressing that the structure of the human body consists of head, trunk and limbs, defining structure as a system in which there are elements and relations between them.

In this conception, a theory consists only of structurally characterized specimens, and the general word that encompasses them—disease—has no other content than that of these specimens, and refers exclusively to them, not to a supposed abstract entity termed "disease".

5. The use of a theory

Under this epigraph I am going to refer to the cognitive mechanism that M employs when he diagnoses K.

In short, M compares K with other cases of pneumonia that he knew previously, and to which K, in his symptomatology, resembles. That is why M thinks that K will have organic lesions similar to those of other cases, and that he will evolve in a similar way.

It is from this perception of similarities with K's semiological and clinical structures, and not with his phenomenal features, such as his weight, hair color, height, length of nose—that M infers that K suffers from pneumonia. He did not need any general law to deduce the disease of K. It is for this reason that in textbooks we do not find general or more specific laws about diseases. It is enough to know specimens—actually, the more of them that are known the better—to extend that knowledge to other specimens, diagnosing them.

In the reconstruction, we found that the evolutionary axiom fulfills the requirements of relating the different elements of the theory to each other and predicting their evolution, as do the laws. However, its form differs from the legal form statements that speak of all in all times and places—something that is unthinkable from nominalism, since "all" refers to a Platonic entity. We will say that this axiom describes a fundamental similarity of the actual exemplars, that of their behavior, which is verifiable from exemplar to exemplar, and which always varies in some degree.

These structures allow M, without producing any scientific innovation, to diagnose K, in what we will call applied science.

If M did not find the pneumonic bacillus present, nor any other already known, he could investigate the presence of a new germ specific for those lesions, and attribute the disease to it, also following the parameters established by Robert Koch in the paradigmatic cases of carbuncle and tuberculosis, discovering with them a new disease—basic research.

Again, to do so, he did not need nothing but to know the procedures done to identify specimens of different diseases and use similar ones to obtain similar results.

6. Synthesis

We create a fictional case—that of Dr. M and a patient K—to present the structure of the exemplars of the medical clinic.

We analyze that it consists of non-theoretical elements (coming from a previous discipline, semiology) and theoretical elements (coming from anatomy-physio-pathology). With them we characterize non-theoretical exemplars (only with these elements), theoretical exemplars (if theoretical elements are added to the previous ones), and finally, actual exemplars (if they comply with a statement that predicts their evolution).

It is about the relationship between two theories, one semiological, the other anatomy-physiopathological and microbiological, which constitute in their relationship the clinical theory.

Let us add that they speak of *K*—or any other human being—, and therefore, of spatiotemporally situated systems. All biomedical knowledge (of so much current transcendence due to the importance of its discoveries) possesses this characteristic, and is therefore physicalist in this sense, as Otto Neurath (1931; 1983) wanted it to be. However, he differs from the latter in that the structures describing *K* pneumonia—and any other disease—are not only non-theoretical. As we saw, it includes both non-theoretical and theoretical elements, which in turn can be independently corroborated. This last circumstance means that its existence—its realism—is not questioned, and that instrumentalism is not the epistemology that best suits medical knowledge.

Regarding the epistemic mechanisms involved in the applied or basic research of *M*, they are those typical of Wittgenstein's nominalism of similarities (see Wittgenstein 1958; Goodman and Quine 1947; Bambrough 1966; Rodríguez-Pereyra 2000). Similarities that do not constitute a new universal, because the exemplars differ to some degree in their structures and are not predicated of a single initial exemplar but of the totality of the actual ones, which provide the network of resemblances that *M* uses to investigate new exemplars that he must treat in his medical practice.

In the epistemic reconstruction of the medical clinic, we simplify and generalize *M*'s the very detailed clinical history, to stablish the structure of the specimens without indicating specific signs, symptoms or organic alterations, putting in their place those generic terms, and even letters that synthesize them. Finally, we arrive to a general structural description of specimens, suitable for the description not only of clinical infectious diseases, but of all diseases.

Our metatheoretical bet is that the scheme can be suitable to exhibit the structure of any exemplar of any factual theory, extending nominalism and physicalism to the totality of factual knowledge.

As we showed, biomedical knowledge doesn't need general laws. The statement that correlates signs and symptoms with organic lesions in the evolutionary process of the disease—its most basic axiom—, which can be written in a law-like traditional style don't need any other reference than the singular experiences from which it originates.

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Laws, Models, and Theories in Biology Within a Unifying Structuralist Interpretation: A General Explication and an Account of the Case of Classical Genetics

Pablo Lorenzano[1]

1. Introduction

Three metascientific concepts subject to philosophical analysis are law, model and theory. Throughout the twentieth and twenty-first centuries, three general conceptions of scientific theories can be identified: the "classical (or received)" view, the "historical (or historicist)" view and the "semantic (or model-theoretic)" view.

For the *classical view*, in its most general approach, theories should be represented as sets of statements deductively or axiomatically organized. Laws, on the other hand, are an essential component of these: they constitute the axioms by means of which they are metatheoretically represented (Carnap 1939; 1956; 1966). In the beginnings of the classical view, models were conceived as marginal phenomena of science (Carnap 1939). Subsequent authors (Braithwaite 1953 and Nagel 1961) strive to incorporate the models, and recognize their importance, into the framework of this classical view.

Historicist philosophers of science, with their alternative notions to the classical concept of theory (*pattern of discovery* in Hanson (1958), *ideal of natural order* in Toulmin (1961), *paradigm* or *disciplinary-matrix* in Kuhn ([1962] 1970; 1970; 1974a), *research program* in Lakatos (1969; 1970; 1971) and *research tradition* in Laudan (1977)), shine light on a conception of the laws that

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Abreu, C. (ed.) *Philosophy of Science in the 21st Century. Contributions of Metatheoretical Structuralism*.
Florianópolis: NEL/UFSC, 2023, pp. 189–258.

diverges from the classical one. In addition, at the same time alternative proposals to the classical one are developed, which highlight the function of models in scientific practice (Achinstein 1968; Hesse 1966; Harré 1970) and investigate the role analogies and metaphors play in the construction of models (Black 1962; Hesse 1966) or of other components, linked to these, raised by historicist philosophers, such as exemplars (Kuhn [1962] 1970; 1970; 1974a; 1979).

In the current day and age, where the importance of models in scientific practice(s) is emphasized, the "*semantic view*"—which deals with the subject matter of models within the framework of a general conception of scientific theories—has been imposed as an alternative to the classical and historicist views of scientific theories.[2] Meanwhile, the "*model views*" of science—which deal with questions of the relationship between models and experience and between models and general theories independently of a general metatheory of science (Cartwright, Shomar and Suárez 1995; Morrison 1998; 1999; Cartwright 1999; Suárez and Cartwright 2008)—have been developed. Even more recently, the "*model-based science*"—which grew out of the community of philosophers, epistemologists, logicians, cognitive scientists, computer scientists, and engineers, working in different aspects of what is known as "model-based reasoning", with special focus on hypothetical-abductive reasoning and its role in scientific rationality (Magnani, Nersessian and Thagard 1999; Magnani and Nersessian 2002; Magnani and Li 2007; Magnani, Carnielli and Pizzi 2010; Magnani 2013; Magnani and Casadio 2016; Magnani and Bertolotti 2017)—has been proposed.

According to the semantic view of theories concepts relative to models are much more fruitful for the philosophical analysis of theories, their nature and function, than concepts relative to statements. The nature, function, and structure of theories can be better understood when their metatheoretical characterization, analysis or reconstruction is centered on the models that they determine, and not on a particular set of axioms or linguistic resources through which they do it.[3] Therefore, the most fundamental component for the identity of a theory is a *class* (set, population, collection, family) *of models*, so thata theory can be characterized in the first place for defining/determining the class, set, population, collection or family of its models. That is to present/identify a theory means mostly presenting/identifying its characteristic models. With the emphasis on models, one might think that not only can the term, or the concept, of "law" be dispensed with,[4] but also that the issue of laws should not be discussed. However, models must be identified in some way. And in the "*semantic view*" this is usually done through the laws or principles or equations of the theory to which they belong (thus, models would constitute the semantic or model-theoretic counterpart of such laws or principles or equations). On the other hand, even though for "model views" models do not form part of, and/or are independent or "autonomous" with respect to, theories (in some usual, encompassing sense of the term), they

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[2] "Over the last four decades the semantic view of theories has become the orthodox view on models and theories" (Frigg 2006, p. 51).

[3] This idea has been developed in different particular ways, giving rise to different approaches, variants or versions, which despite their differences constitute a family, the *semantic family*. For a characterization of this family, and of some of its members as well as a reference to many of them, see Lorenzano (2013b), and Ariza, Lorenzano and Adúriz-Bravo (2016) and Section 4.1 below.

[4] For skeptical positions about any notion of law and the substitution of the term "law" by other notion, such as "(fundamental) equations" or "(basic) principles", see Cartwright (1983; 2005), Giere (1995) and van Fraassen (1989). In fact, Carnap himself had already considered the possibility of dispensing with the term "law" in physics (Carnap 1966, p. 207).

would also be represented, or would contain, or would be identified, by means of principles, equations or laws, although not universally.

The aim of this article is to present the explication of these concepts, and of their relationships, made within the framework of Sneedian or Metatheoretical Structuralism,[5] and of their application to a case from the realm of biology: Classical Genetics. The analysis carried out will make it possible to support, contrary to what some philosophers of science in general and of biology in particular hold, the following claims: a) there are "laws" in biological sciences, b) many of the heterogeneous and different "models" of biology can be accommodated under some "theory", and c) this is exactly what confers great unifying power to biological theories.

To begin with, the structuralist explication of the concepts of law, model and theory will be presented successively, which will be preceded by an introduction to the subject, followed by its application to Classical Genetics.[6]

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[5] See Balzer, Moulines and Sneed (1987) for a complete and technically precise presentation of this metatheory, and see Díez and Lorenzano (2002), Moulines (2002) or Kuipers (2007) for a concise presentation of it.

[6] The analysis of Classical Genetics is based on Lorenzano (1995; 2000; 2002). The expression "Classical Genetics" is ambiguous. Sometimes it refers to all that was done in genetics prior to the development of molecular genetics and in contrast to it, i.e. to what was done in the so-called "classical period", a period that would cover the period from 1900 to 1939 (Dunn 1965). Sometimes it refers to what was done in genetics, in a shorter period of time, namely from 1910 to the late 1920s or early 1930s, primarily by Thomas Munt Morgan and his disciples and collaborators Alfred Henry Sturtevant, Calvin Blackman Bridges and Hermann Joseph Muller—later known as "the *Drosophila* Group", "the *Drosophila* School" or "the Columbia School". And sometimes it refers to one of the theories developed in the larger or shorter "classical period", which, besides "Classical Genetics", is also known as "Mendelian Genetics", "Transmission Genetics", "Classical Transmission Genetics", the "Theory of Gene Transmission", "Formal Genetics", "Gene Theory" and the "Theory of the Gene". We use here the expression "Classical Genetics" in this last sense of one theory, which we make precise through our analysis. We believe that the designation of this theory as "Mendelian", as is customary, among other things, in many genetics' textbooks, is historically erroneous, since formulations—such as the so-called "Mendelian laws"—and developments never made by Gregor Mendel are thus attributed to him. (For an structuralist analysis of Mendel's "hybridism", see Lorenzano 2022; and for an analysis of these, as well as of other questions related to the history of genetics—such as that of the supposed "rediscovery" of Mendel at the beginning of the century—, see Lorenzano (1995), and the bibliography cited therein). On the other hand, we agree with Kitcher (1984), Weber (1998) and Waters (2004), who have argued that the expressions "Transmission Genetics", "Classical Transmission Genetics" or the "Theory of Gene Transmission" do not refer to all that was done in genetics in the "shorter" classical period, i.e. to all types of research carried out during that period, to all answers for all kinds of questions that classical geneticists asked, to all theories developed by classical geneticists. It refers rather to a theory of inheritance developed during that period, which was—and still is—accepted as providing explanations, even satisfactory ones, of some regularities or patterns of inheritance (inheritance patterns), i.e. answering one (still important) type of questions that were raised during that period, corresponding to one type of research carried out by classical geneticists. According to Kitcher (1984), the theory of *gene transmission*, which addresses the family of *pedigree problems*, constitutes the "heart" of "classical genetic theory", while out of this theory grow other "subtheories", like the theory of *gene mapping*, which addresses questions about the relative positions of loci on chromosomes, and the *theory of mutation*, which tackles the question of how to identify mutations. Another name that sometimes appears in the literature is "Chromosome Theory of Inheritance" (or "Chromosome Theory of Mendelian Inheritance"). This name refers both to the attempts made at the beginning of the 20th century by Sutton and Boveri and to those made by Morgan and his collaborators ten years later to link the theory of inheritance with cytology. Thus, the chromosome (Mendelian) theory of inheritance would form a theory that includes, together with a theory

Next, the relevance of the previous analysis to the issues of the existence of laws in biological sciences, the place of models in theories of biology, and the unifying power of biological theories will be stressed.

Finally, the article will conclude with a discussion of the presented analysis.

2. The Concept of Law from the Point of View of Metatheoretical Structuralism

2.1. Introduction

In English the term "law" (in Old English *lagu*, in Medieval English *lawe*; in Latin *lex, legis*, in French *loi*) has an old Teutonic root *lag*, "lie", what lies fixed or evenly. It is usually used either alone, in the singular or plural form, "law(s)", or in the phrases "law(s) of nature", "natural law(s)", "law(s) of science" and "scientific law(s)". Although in Western culture the ideas of natural law, in the juridical (legal) and ethical (*ius naturale*, *lex naturalis*) sense—i.e., laws or moral precepts, in the approximate sense of those that are held to be either a divine mandate or intuitively obvious to all or with the ability to arrive at them by reasoning from obvious and indisputable premises, i.e., not based on legislated law, but on reason, divine command or moral instinct, and which are common to all nations—, and of *law of nature*, in the sense of the natural sciences, they go back to a common root, here we will only consider laws of nature in the scientific sense.

Despite having a long history going back to a time when people thought of nature as obeying the laws of its Creator in a similar way as individuals obeyed the laws imposed by their monarch, the expression "law(s) of nature" in the latter sense has only very rarely been used in the philosophical discussions of classical antiquity, as well as in theological discussions throughout the Middle Ages and the Renaissance. However, it is frequently used in the natural philosophy of the seventeenth century and by the end of that century it became common currency in scientific discussion and has remained so ever since.

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of inheritance, its interrelations with another body of biological knowledge, namely cytology. According to Darden and Maull (1977) and Darden (1980; 1991), the chromosome (Mendelian) theory of inheritance would be an example of an "interfield theory", whereas the theory of inheritance would be an example of an "intrafield theory". The expressions "Transmission Genetics" and "Classical Transmission Genetics" have an ambiguous meaning, since it is sometimes used in the sense of an intrafield theory and sometimes in the sense of an interfield theory. We might call the theory discussed here "Theory of Gene Transmission" (following Kitcher 1984), but also "Theory of the Gene"—following Morgan's (1917; 1926) usage—, although, and according to Vicedo's (1990a; 1990b; 1991) suggestion to distinguish between the formal concept of gene (which lacks any specification about its nature) and the biological concept of gene (which interprets the formal concept in cytological terms), the expressions "Formal Gene Theory" or "Formal Genetics" could also be used. By this we refer to the theory of inheritance developed by Morgan and his collaborators, without including the essential links that this theory has with other theories, especially with cell theory. We also believe that it is methodologically more appropriate to first reconstruct both theories—gene theory and cell theory—as two distinct and separate theories and then to investigate their intertheoretical relations or links. As a final note, for the first systematic exposition in a book of the theory referred to here, see Morgan *et al.* (1915); while Sinnott and Dunn (1925) may be considered the *first textbook of genetics in the Kuhnian sense*, inasmuch as it contains, with pedagogical goals, a clear and actualized—in comparison with Morgan *et al.* (1915)—exposition of the principles of genetics, paradigmatic applications of them (or 'exemplars'), as well as problems to be solved by the student.

In addition, the following two aspects should be taken into account. On the one hand, natural philosophers of early modernity, and especially mathematicians, had at their disposal an alternative, well-established and highly differentiated terminology, which they could and often did use to refer to natural regularities. Among these expressions, we find following: *regula*, *axioma*, *hypothesis*, *ratio*, *proportio*. On the other hand, the expression "law(s) of nature" does not necessarily denote a single concept or a concept with precise limits. Moreover, the concept of law of nature, like many of our concepts—whether they are everyday, scientific or metascientific—, may be considered as an "open" concept—both extensionally, i.e. as an open set, and intensionally, i.e. that doesn't possess conditions that are jointly necessary and sufficient for its application; or whose conditions are necessary but not sufficient; or sufficient but not necessary; or that they constitute a disjunction of conditions neither necessary nor sufficient, but whose instances of application share a certain "family resemblance" (Wittgenstein)—or as a "cluster" concept—having associated with it a cluster of criteria, of which only *the majority* must be satisfied by any instance (prototype theory)—, even though these conditions or criteria may change historically.

Thus, in the uses already established in the 17th century of the concept of law of nature, such as those of Descartes and Newton, the connection between laws of nature and God, as creator and lawgiver, was explicit. The secularization of the concept of the law of nature occurred at different times in Europe. In France, towards the end of the 18th century, with the French Revolution, Laplace was already able to argue that God was "an unnecessary hypothesis". In the German-speaking countries, Kant thought he could base the universality and necessity of Newton's laws no longer on God or nature, but on the constitution of human reason. While in Britain, despite the legacy of Hume, discussions continued as to whether the laws of nature were expressions of divinity until the Darwinian Revolution, but the secular interpretation of Darwin's "law of natural selection" finally prevailed there. This secularized version of the laws of nature has dominated the philosophical understanding of science ever since.

In scientific as well as in philosophical literature many authors speak not just plainly about *laws*, but about *natural* laws, or laws *of nature*, on one hand, and about *scientific* laws, or laws *of science*, on the other hand, too. Such expressions, besides, are commonly used as if the expressions belonging to one pair were interchangeable with the expressions belonging to the other pair, i.e. as if they were synonymous or had the same meaning. However, we consider it convenient to distinguish the first pair from the second one, since they correspond to different approaches or perspectives (e.g., Weinert 1995). The first pair corresponds to an approach of an *ontological* kind—corresponding to how things themselves are—, while the second one corresponds to an approach of an *epistemic* kind—centered in what we know.

Some philosophers have argued that a philosophical treatment of laws should be given only for the laws of nature and not for the laws of science. While others consider it more appropriate to refer to the laws of science than (only) to the laws of nature, because, in any case, it is the laws of science that would provide important keys to understanding what a law of nature is.

In what follows when we speak about laws, we will be talking about *scientific* laws, or laws *of science*, and not about *natural* laws, or laws *of nature*.[7]

At least as of 1930 the problem of what a law—i.e. the problem of finding the necessary and

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[7] For a more extensive discussion about the nature of laws as well as an analysis of natural laws, within the framework of Metatheoretical Structuralism, see Forge (1986; 1999) and Lorenzano (2014-2015).

sufficient criteria or conditions which a statement should satisfy in order to be considered or in order to function as a law—is discussed.

According to the classical view (Hempel and Oppenheim 1948), a *law* is a true *lawlike* statement that has the following properties: it is universal, with an unlimited or at least unrestricted scope of application; it does not refer explicitly or implicitly to particular objects, places, or specific moments; it does not use proper names; and it only uses "purely universal in character" (Popper 1935, Section 14-15) or "purely qualitative" predicates (Hempel and Oppenheim 1948, p. 156).

Despite successive and renewed efforts there is not a satisfactory adequate set of precise necessary and sufficient conditions as a criterion for a statement to be considered a "(scientific) law".[8]

The discussions in the field of general philosophy of science have also been held, and have taken place, in the special field of philosophical reflection on biology and its different areas such as classical genetics, population genetics, evolutionary theory and ecology, among others.

Some philosophers of science and of biology—partly based on, and partly supported by, evolutionists like Mayr (1982; 1985) and Gould (1970; 1989)—deny the existence of laws in biology in general, and in genetics in particular. Two main arguments have been put forward against the existence of laws in biology. The first one is based on the alleged locality or non-universality of generalizations in biology (Smart 1963); the second one is based on the alleged (evolutionary) contingency of biological generalizations (Beatty 1995).

At least three responses to these arguments can be found. The first one consists in submitting them to a critical analysis. This approach is chosen by Ruse (1970), Munson (1975), and Carrier (1995), among others. The second one is to defend the existence of laws, or principles, in biology but arguing that they are non-empirical, a priori. This strategy is followed by Brandon (1978; 1981; 1997), Sober (1984; 1993; 1997) and Elgin (2003). The third one is to defend the existence of empirical laws, or principles, in biology but arguing for a different explication of the concept of law or of non-accidental, counterfactual supporting, generalizations (Schaffner 1993; Carrier 1995; Mitchell 1997; Lange 1995; 2000; Dorato 2005; 2012; Craver and Kaiser 2013). Our proposal will be of this third kind. But in such a manner that it will allow us to consider "theoretical pluralism", "relative significance" controversies and some kind of contingency as not exclusive of biology (agreeing with Carrier (1995) on this) and to better understand the role played by different laws or lawlike statements of different degrees of generality in biology (capturing some of the points made by Ruse (1970) and Munson (1975)) as well as the "a priori" component pointed out by Brandon, Sober and Elgin.[9]

With respect the existence of laws in genetics in particular—and taking into account the classical proposal of differentiating between two types of genuine laws: on the one hand, laws of unlimited, unrestricted scope or *fundamental laws* and on the other, laws of limited, restricted scope or *derivative laws* that would *follow* from more fundamental laws (Hempel and Oppenheim 1948, p. 154)—, we must distinguish the claim that there are no laws in genetics at all, which is hardly tenable given at least the so-called "Mendel's *laws*", and the more asserted and discussed claim that there are no fundamental and/or general nomological principles in genetics. And, in a similar way as we did in the case of laws in biology in general, our position with respect to the denial of the

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[8] See Stegmüller (1983) and Salmon (1989) for an analysis of the difficulties of the classical explication of the notion of scientific law.

[9] For a more detailed discussion of the two first kinds of responses, see Lorenzano (2006b; 2007b; 2007c; 2014-2015) and Díez and Lorenzano (2013; 2015).

existence of fundamental laws in genetics will be of replacing the classical understanding of such laws and of the more restricted laws by a different one, which will allow us to better understand the role played by different laws or lawlike statements of different degrees of generality in genetics.

For their part, the historicist philosophers of science, on the way to conform and/or expound, or expand on, their conceptions about the development of science, with their correlative alternative notions to the classical concept of theory (such as *patterns of discovery* in Hanson (1958), *paradigm* or *ideal of natural order* in Toulmin (1961), *paradigm* and *discipline-matrix* in Kuhn ([1962] 1970; 1974a; 1977), or *research program* in Lakatos (1969; 1970; 1971), or *research tradition* in Laudan (1977), or *field* in Shapere (1974; 1984)), let emerge a certain conception about laws different from the classical concept of law, either with the same terminology (Toulmin 1953; Hanson 1958, Lakatos 1969; 1970; 1971; [1974] 1978; Shapere 1984c) or with a different one (Kuhn ([1962] 1970; 1974a; 1976; 1977; 1981; 1983a; 1983b; 1989; 1990), who, besides speaking of "laws", speaks of "symbolic generalizations").

One of the aspects shared in their stance related to the classical concept of law is that universality is a too demanding condition. This is a point already made many years ago by Toulmin for physics:

> Any one branch of physics, and more particularly any one theory or law, has only limited scope: that is to say, only a limited range of phenomena can be explained using that theory, and a great deal of what a physicist must learn in the course of his training is connected with the scopes of different theories and laws. It always has to be remembered that the scope of a law or principle is not itself written into it, but is something which is learnt by scientists in coming to understand the theory in which it figures. Indeed, this scope is something which further research is always liable to, and continually does modify. (Toulmin 1953, p. 31)

And what is valid for physics may also be valid for biology. This means that the alleged critique to biological generalizations for their non-universality doesn't even hold for the generalizations of physics. Therefore, biological generalizations should not be "doomed" because they lack of universality. What matters is not strict universality but rather the existence at least of non-accidental, counterfactual supporting, generalizations, which we take as uncontroversial present in biology, though generally more domain restricted and *ceteris paribus* than in other areas of science such as mechanics or thermodynamics.

A second aspect shared by the historicist philosophers of science is the acceptance of "laws"—or whatever they are called—of different degrees of generality within a "theory"—or whatever they are called. Thus, e.g., we have in Lakatos the most general "laws" as part of the *hard core* of the *research programme*, while the less general ones constitute the "'protective belt' of auxiliary hypothesis" (Lakatos [1974] 1978, p. 4); and what Kuhn calls "symbolic generalizations"—but also "generalization-sketches" (Kuhn 1974a), "schematic forms" (Kuhn 1974a), "law sketches" (Kuhn [1962] 1970; 1970; 1974a; 1974b; 1983a) or "law-schema" (Kuhn [1962] 1970)—and their "particular symbolic forms" (Kuhn [1962] 1970, 1970; 1974a; 1974b) adopted for application to particular problems in a detailed way. In both cases, the most general "laws" have a sort of a-priori (or analytical) "flavor" inasmuch as they are *irrefutable* by a methodological decision for Lakatos and because they are not directly tested (or applicable) for Kuhn. And in Kuhn more clearly than in Lakatos the relation between the two types of "laws" is not of a deductive kind—even though he does not delve into the analysis of the nature of the logical relationship between them.

As many philosophers of biology and of physics, we also accept a broader sense of lawhood that does not require non-accidental generalizations to be universal and with no exceptions in order to qualify as laws.[10]

This minimal characterization of laws as counterfactual-supporting facts is similar to the one defended in Dorato (2012), and it is also compatible with some proposals about laws in biology in particular, such as the "paradigmatic" (Carrier 1995) and "pragmatic" (Mitchell 1997) ones.

Whether one wants to call these non-accidental, domain restricted, generalizations "laws" is a terminological issue we will not enter here. What matters is, tagged as one wills, that these non-accidental generalizations play a key role in biology in general and in genetics in particular. We will show that in the case of Classical Genetics (**CG**). But, before that, we will present in the next subsection the structuralist explication of the concept of law. In particular, we will present the structuralist explication of the two kinds of laws, of the most general ones—even though without universality and with modal import—and of the less general ones within a theory, and of the type of relationship between them.

2.2. The Structuralist Concept(s) of Law

Within the structuralist tradition, when dealing with the subject of laws, discussions, even from their beginnings with Sneed (1971), though not with that terminology, focus on those scientific laws which, starting with Stegmüller (1973), are called "fundamental laws" of a theory.

However, accepting the problems for finding a *definition* of the concept of a law, when the criteria for a statement to be considered a fundamental law of a theory are discussed within the framework of Metatheoretical Structuralism, one tends to speak rather of "necessary conditions" (Stegmüller 1986, p. 93), of "*weak* necessary conditions" (Balzer, Moulines and Sneed 1987, p. 93), or, better still, only of "symptoms", some even formalizable (Moulines 1991, p. 233), although

> in each particular case of reconstruction of a given theory, it seems, as a general rule, to be relatively easy to agree, on the basis of informal or semi-formal considerations (for example, on its systematizing role or its quasi-vacuous character), that a given statement should be taken as the fundamental law of the theory in question. (Moulines 1991, p. 233)

On the other hand, Metatheoretical Structuralism draws a distinction between the so-called *fundamental laws* (or *guiding principles*) and the so-called *special laws*. This distinction, which will be developed later, elaborates the classical distinction between two kinds of laws with different degrees of generality in a different way as well as the Kuhnian distinction between the symbolic generalizations and their "particular symbolic forms" adopted for application to particular problems in a detailed way.[11]

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[10] Some philosophers of biology admit this, i.e. the existence of laws or some sort of non-universal and non-exceptionless lawlike generalizalizations, at least in some areas of biology, such as ecology (Weber 1999; Cooper 1998; 2003; Colyvan 2004; Colyvan and Ginzburg 2003; Lange 2005), evolutionary theory and classical genetics (Weber 2004).

[11] On the other hand, the expressions 'fundamental law' and 'special law' are not used here in Fodor's sense (Fodor 1974; 1991)—the former for laws of basic or fundamental sciences, the latter for laws of special sciences—but rather in the sense used by structuralists, i.e. for different kinds of laws within a theory.

Very briefly, five criteria can be mentioned as necessary conditions, *weak* necessary conditions or "symptoms" for a statement to be considered a fundamental law/guiding principle in the structuralist sense:

1) *Cluster or synoptic character*. This means that a fundamental law should include "*all* the relational terms (and implicitly also all the basic sets) and, therefore, at the end, *every fundamental concept* that characterize such a theory" (Moulines 1991), "several of the magnitudes", "diverse functions", "possibly many theoretical and non-theoretical concepts" (Stegmüller 1986), "almost all" (Balzer, Moulines and Sneed 1987), "at least two" (Stegmüller 1986).[12]
2) *Applicability to every intended application.*. According to this, it is not necessary that fundamental laws have an unlimited scope, apply every time and everywhere and possess as universe of discourse something like a "big application", which constitutes an only one or "cosmic" model, but it rather suffices that they apply to partial and well-delimited empirical systems: the set of intended applications of the theory (Stegmüller 1986).[13]
3) *Quasi-vacuous character*. This means that they are highly abstract, schematic, and contain essential occurrences of T-theoretical terms, which in structuralist sense are terms whose extension can only be determined through the application of a theory's fundamental law(s)[14] so that they can resist possible refutations, but which nevertheless acquire specific empirical content through a non-deductive process known as "specialization" (Moulines 1984).
4) *Systematizing or unifying role*. Fundamental laws allow including diverse applications within the same theory since they provide a guide to and a conceptual framework for the formulation of other laws (the so-called "special laws"), which are introduced to impose restrictions on the fundamental laws and thus apply to particular empirical systems (Moulines 1984).[15] It is clear that the distinction between fundamental and special laws is relative to the considered theory.

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[12] It is clear that the consideration of this criterion, in any of its versions, must take into account that it is strongly dependent on the respective language used, i.e. on the respective formulation of a theory, since it is only in relation to it that a term can be considered primitive, basic or fundamental. On the other hand, it is interesting to note a difference in this criterion between the classical conception of laws and theories and the structuralist metatheory. According to the former, the descriptive (non-logico-mathematical) concepts occurring in laws, as axioms or postulates of a formal axiomatic system (Hilbertian or Frege-Hilbert type), are, typically, theoretical concepts (in the classical sense) and, thus, fundamental laws are *theoretical laws*, formulated by means of pure theoretical statements, containing only theoretical terms (or concepts). Whereas here, typically, but not necessarily—as in the case of reversible thermodynamics (see Moulines 1984)—fundamental laws contain both **T**-theoretical and **T**-non-theoretical (in the structuralist sense) terms (or concepts), and if described classically, they would be described as *mixed* statements.

[13] The validity of laws can be regarded as *exact*—and thus as *strict* or non-interferable laws—or, rather, to the extent that they usually contain not only *abstractions*, but also various *idealizations*, as *approximate*, as already pointed out by Scriven (1959) and more extensively by Cartwright (1983)—and so as non-strict or *interferable* laws, and compatible with various specific treatments of this situation, such as those referring to ceteris paribus clauses (Cartwright 1983), "provisos" (Coffa 1973 and Hempel 1988) or "normicity" (Schurz 2009).

[14] For more on the structuralist **T**-theoretical/**T**-non-theoretical distinction, see Section 3.2.

[15] By saying it in a model-theoretic way, fundamental laws determine the whole class of models of a theory, while special laws determine only some of them, which constitute a subclass of the class of models.

5) *Modal import*. Fundamental laws express non-accidental regularities, are able to give support to counter-factual statements (if they are taken "together-with-their-specializations" within a theory, in the sense that we will introduce later of theory-net), even when they are context-sensitive and with a domain of local application, and that, in its minimal sense, instead of attributing *natural necessity*, *necessity of the laws* is attributed, and, in that sense, they should be considered as *necessary in their area of application*, even when outside such an area it doesn't need to be that way (Lorenzano 2014-2015; 2019; Díez and Lorenzano 2013; Moulines 2019).

Fundamental laws/guiding principles are "programmatic" or heuristic in the sense that they tell us *the kind of things* we should look for when we want to explain a specific phenomenon. But, as said before, taken in isolation, without their specializations, they say empirically very little. They can be considered, when considered alone, "empirically non-restrict". In order to be tested/applied, fundamental laws/guiding principles have to be specialized ("concretized" or "specified"). These specific forms adopted by the fundamental laws are the so-called "special laws".

It is worth emphasizing that the top-bottom relationship established between laws of different levels of generality is *not* one of implication or derivation, but of *specialization* in the structuralist sense (Balzer, Moulines and Sneed 1987, Chapter IV): bottom laws are specific versions of top ones, i.e. they specify some functional dependencies (concepts) that are left partially open in the laws above. That is the reason why they are called "*special* laws" instead of "*derivative* laws" like in the classical view of laws, according to which the laws with a more restricted or limited scope are assumed to be logically derived or deduced from the fundamental laws. Actually, "special laws" *are not derived* or *deduced literally* from the fundamental laws (at least are not derived or deduced *only* from them) without considering some additional premises. Formally speaking, the specialization relation is reflexive, antisymmetric and transitive, and does not meet the condition of monotonicity.

When the highest degree of concretization or specificity has been reached, i.e. when all functional dependencies (concepts) are completely concretized or specified, "*terminal* special laws" are obtained. This kind of special laws, proposed to account for particular empirical situations, can be seen as particular, testable and, eventually, refutable hypotheses to which to direct "the arrow of *modus tollens*" (Lakatos 1970, p. 102).

2.3. Laws in Classical Genetics (CG)

Classical Genetics (**CG**) is a theory about the hereditary transmission, in which the transmission of several traits or characteristics is followed from generation to generation of individuals. It talks about *individuals*—sets of individuals or populations that make up "families", that is, populations connected by bonds of marriage, parentage or common descendent—, and of certain *traits* or *characteristics possessed by them* (their "appearance")—what is called their "phenotype"—, individuals that *mate* and produce *progeny*, which also possess certain *traits* or *characteristics* (phenotype) and where numerical ratios, proportions or relative frequencies (r_j's) in the *distribution* of those characteristics in the progeny are distinguished.

The connections between its different components, i.e. its different objects and functions, can be graphically represented in the following way (see Figure 1), where the objects are represented by rectangles and the functions by arrows:

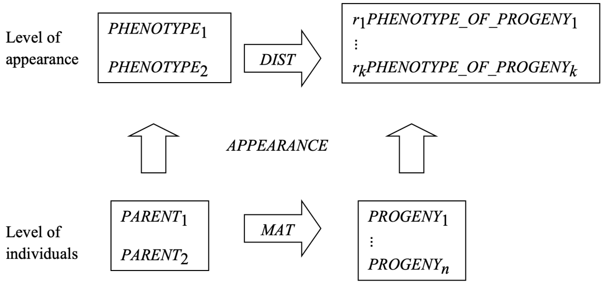

Figure 1

Classical Genetics (**CG**) intends to account for *biological systems*—constituted by *individuals* (that make up "families") *possessing* certain *traits* or *characteristics* (their phenotype) that *mate* and produce *progeny*—that show certain *patterns of inheritance*, which **CG** conceptualizes as *distribution of characteristics* from parental individuals toprogeny, expressed by certain proportions, numerical ratios or relative frequencies.

Examples of *cases* of characteristics *distributions* in *specific biological systems* are the following:

1) For the case of the color of pea seed albumen: Pea plants belonging to the first filial generation, with yellow seed coats, which are self-fertilized and produce offspring having a proportion 3:1 of yellow seed coat (¾) and green seed coat (¼).
2) For the case of the color of four o'clock flowers: Four o'clock plants belonging to the first filial generation,with pink flowers, which are self-fertilized and produce offspring having a proportion 1:2:1 of red, pink and white color flowers.
3) For the case of the color of pea seed albumen together with the form of pea seeds: Pea plants belonging to the first filial generation, with yellow seed coat and round seed form,which are self-fertilized and produce offspring having a proportion 9:3:3:1 of yellow seed coats and round seed form ($^{9}/_{16}$), of yellow seed coats and angular seed form ($^{3}/_{16}$), of green seed coats and round seed form ($^{3}/_{16}$), and of green seed coats and angular seed form ($^{1}/_{16}$).
4) For the case of the color of pea seed albumen together with the form of pea seeds and the color of pea flowers: Pea plants belonging to the first filial generation, with yellow seed coat, colored flowers and round seed form, which are self-fertilized and produce offspring having a proportion 27:9:9:9:3:3:3:1 of yellow seed coats, colored flowers and round seed form ($^{27}/_{64}$), ofyellow seed coats, colored flowers and angular seed form ($^{9}/_{64}$), of yellow seed coats, white flowers and angular seed form ($^{9}/_{64}$), of green seed coats, colored flowers and angular seed form ($^{9}/_{64}$), of yellow seed coats, white flowers and angular seed form ($^{3}/_{64}$), and of green seed coats, white flowers and angular seed form ($^{1}/_{64}$).
5) For the case of the length of corn ear: Corn plants belonging to the first filial generation, with neither short nor long ear length, which are self-fertilized and produce offspring with corn ear length that ranges from very short to very long in a transitional continuous way.

6) For the case of the form of fowl combs: Fowls belonging to the first filial generation, with walnut comb form, are crossed among them and produce offspring having a proportion 9:3:3:1of walnut comb form ($^9/_{16}$), of rose comb form ($^3/_{16}$), of pea comb form ($^3/_{16}$), and of single comb form ($^1/_{16}$).
7) For the case of the color of pea flowers together with the length of pea pollen grain: Pea plants belonging to the first filial generation, with purple flower color and long pollen grain length, which are self-fertilized and produce offspring having a proportion 7:1:1:7 of purple flower-color and long pollen grains ($^7/_{16}$), of purple flower-color and round pollen grains ($^1/_{16}$), of red flower-color and long pollen grains ($^1/_{16}$), and of red flower-color and long pollen grains ($^7/_{16}$).

For every specific case geneticists have to postulate *specific* (pairs of) factors/genes of the (parental) individuals—what is called their "genotype"—and the *specific* manner of their *combination* in reproduction (genotype-distribution) that accounts for the *specific* distribution of characteristics (phenotype-distribution) in the progeny.

This means that, in order to account for the distributions of characteristics in the progeny (i.e., for the ratios, proportions or relative frequencies), the following parameters have to be theoretically postulated:

(i) appropriate types and numbers of (pairs of) factors or genes—the genotype—(either one or more),
(ii) the way in which they are distributed in the progeny (as expected or theoretical probabilities, with combinations of factors or genes with the same probability or not),
(iii) the specific relationship in which they are with the characteristics of the individuals—their phenotype—(with complete or incomplete dominance, codominance or epistasis).

Thus, geneticists propose *special laws*, which contain these three types of specifications, plus the assumption (iv) that, given specifications (i) and (iii), the genotype distributions, given by specification (ii), *match* or *fit* (exactly or approximately) the phenotype distributions in the progeny.

The following are the specifications introduced to account for the above examples:

1) (i) these pea plants are heterozygous with respect to factors for seed coat color, (ii) combinations of their factors in offspring are equiprobable, and (iii) factors for yellow seed coat are dominant over factors for green seed coat (Sinnott and Dunn 1925, pp. 40-41, 45-50).
2) (i) these four o'clock plants are heterozygous with respect to factors for flower color, (ii) combinations of their factors in offspring are equiprobable, and (iii) factors for red flower color are incomplete dominant over factors for white flower color (Morgan 1926, pp. 5-7).
3) (i) these pea plants are heterozygous with respect to factors for seed coat color and for seed form, (ii) combinations of their factors in offspring are equiprobable, and (iii) factors for yellow seed coats are dominant over factors for green seed coat and factors for round seed form are dominant over factors for angular seed form (Morgan 1926, pp. 7-10).
4) (i) these pea plants are heterozygous with respect to factors for seed coat color, for flower color and for seed form, (ii) combinations of their factors in offspring are equiprobable, and (iii) factors for yellow seed coats are dominant over factors for green seed coat, factors

for colored flowers are dominant over factors for white flowers and factors for round seed form are dominant over factors for angular seed form (Sinnott and Dunn 1925, pp. 72-73).

5) (i) these corn plants are heterozygous with respect to factors for ear length, (ii) combinations of their factors in offspring are equiprobable, and (iii) the three pairs of factors are for ear length in corn with a cumulative effect (Sinnott and Dunn 1939, pp. 125, 127-129).
6) (i) these fowls are heterozygous with respect to both pairs of factors for comb form, (ii) combinations of their factors in offspring are equiprobable, and (iii) the walnut comb depends on the presence of two dominant factors, one of these genes alone produces the rose comb, the other alone produces the pea comb, the combination of the recessive alleles of these factors produces the single type of comb (Sinnott and Dunn 1925, pp. 91-92).
7) (i) these pea plants are heterozygous with respect to factors for flower color and for pollen grain length, (ii) combinations of their factors in offspring are not equiprobable (purple flower-color and long pollen grains that go in together come out together more frequently than expected for independent assortment of purple-red and round-long), and (iii) factors for purple flower-color are dominant over factors for red-color and factors for long pollen grain are dominant over factors for round pollen grain (Sinnott and Dunn 1925, p. 151; 1939, pp. 192-193).

In the terminology of Metatheoretical Structuralism the result of specifying (i), (ii) and (iii), plus the specific form adopted by the match or fit (iv), should be considered a *special law*; moreover, to the extent that all concepts are completely concretized or specified, each special law should be seen as a *terminal* special law. And such kind of special laws are what are intended to apply to particular cases.

In all of the above cases, it turns out that the postulated distributions in the progeny of parental pairs of factors or genes involved (specification (ii)) *matches* or fits (exactly or approximately) the characteristics distributions in the progeny (assumption (iv)), given the type and number of factors involved (specification (i)) and the postulated relationships between pairs of factors (genes) and characteristics ((specification (iii))). This means that the proposed special laws are *successfully applied* in the respective cases presented.

So far, we have identified some of the different special laws that have been proposed in classical genetics.

In order to try to identify some fundamental law/guiding principle in classical genetics, the strategy we will use is to ask what all the different special laws of **CG** have in common.

It is worth noting that the key metatheoretical question is not "from what fundamental laws or general principles or equations are all specific special laws of **CG** deduced?", but "what do all special laws of **CG** have in common?"

Answering this question is not only a feasible task; it will also shed light on the relationship between laws of **CG**, and moreover, as we shall see later, on the relationship between models of **CG**, and **CG** as a theory, in the sense of a theory-net, and on the unifying power of **CG** in particular and of theories in general.

One might respond to this question by denying that there is one particular feature (or set of features) that all special laws of **CG** share and argue that the case of genetical laws is analogous to Wittgenstein's games (1953, § 66 and ff.): what ties different special laws together and what makes them belong to **CG** is some kind of family resemblance between them rather than the existence of a fixed set of shared features, providing necessary and sufficient conditions for membership to them.

However, this answer begs another question because we still want to know in what sense the different special laws of **CG** are similar to each other.

It seems unlikely that the desired similarities can be read off from the mere appearance of them, and this is all that the Wittgensteinians can appeal to. Moreover, what matters is not that they are similar to each other in appearance but rather that they share certain structural features: the special laws of **CG** possess the same structure (of the same logical type), meaning that they all are specifications/specializations of one and the same fundamental law/guiding principle of **CG**, respectively. And thus, as we shall see later, they form a theory, or, better, a theory-net, the theory-net to which they all belong.

In specific **CG** applications only specific laws appear, and that is all what we have in standard text-books. However, we would like to suggest that they are specific versions of a general, fundamental law or guiding principle for the application in point. Nevertheless, in contrast to other empirical theories like those that belong to physics such as classical particle mechanics or thermodynamics,[16] the fundamental law/guiding principle of **CG** is not "observed" in the standard literature, but it is only "implicit" there. The Fundamental Law/Guiding Principle of Classical Genetics, implicitly presupposed in specific **CG** applications, reads in an intuitive way as follows:

CGGP: The statistical communality of characteristics/phenotypes between parents and progeny (given by characteristics/phenotypes distributions in the progeny) is due to (i) the presence in parents of factors/genes, (ii) the factors distribution from parents to progeny, and (iii) a determining relation between specific factors and specific characteristics, so that (iv) factors distributions "match"/"fit" (in specific manner to be specified) characteristics distributions.

All interconnected concepts of **CG** can be graphically depicted as follows (see Figure 2, where besides the components already present in Figure 1 symbolic representations appear at the new theoretical level on the top). Specifically, there is a symbolic representation of *factors* or genes—the genotype—, of (probability) *distributions* (α_j's) of those factors or genes in the progeny (*COMB*), and of the function of the way in which (pairs of) *factors* or genes *relate to characteristics* (*DETERMINER*)):

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[16] For an analysis of these theories from a structuralist point of view, see among others Balzer, Moulines and Sneed (1987).

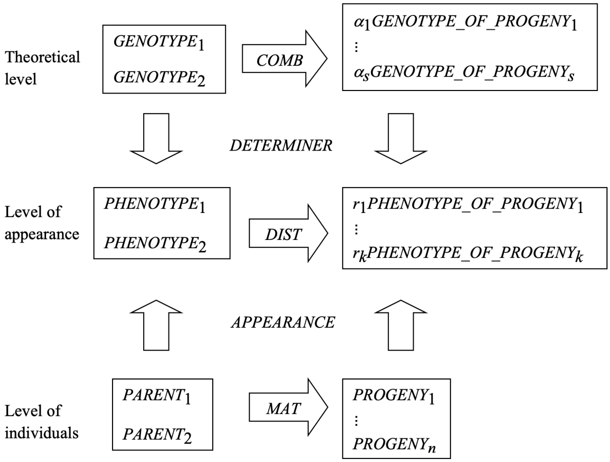

Figure 2

As mentioned before, fundamental laws/guiding principles are programmatic/heuristic in the sense that they tell us *the kind of things* we should look for when we want to apply the theory to a specific phenomenon. In the case of **CG** fundamental law/guiding principle, its heuristic character can be read as follows:

CGGP: When confronted with specific statistical distribution of specific parental characteristics (phenotype) in offspring, look for factors (genes) (genotype) responsible for the characteristics that combine in a specific manner in parents and "match"/"fit" the characteristics distribution in offspring.

As we already mentioned, in every specific case geneticists have to look for specific factors/genes (genotype) and discover the specific manner of their combination in reproduction (genotype-distribution) that accounts for the characteristics distribution (phenotype-distribution) in offspring.

This means that **CG** fundamental law/guiding principle guides the process of specialization, since, as we saw before, in order to obtain special laws that account for the distributions of the parental characteristics in the progeny, (i) the number of pairs of factors or genes involved (either one or more), (ii) how the parental factors or genes are distributed in the progeny (with combinations of factors or genes with the same probability or not), and (iii) the way in which factors or genes are related to the characteristics (with complete or incomplete dominance, codominance or epistasis) have to be specified, and, in addition, (iv) the match/fit of the genotype-distribution with the phenotype-distribution is assumed.

3. The Concept of Model from the Point of View of Metatheoretical Structuralism

3.1. Introduction

The term “model”, and its obsolete form “modell”, in English, comes from the Middle French word “modelle” and the Old Italian word “modello”, which arose during the Renaissance, in the 16th century, and is derived from the Latin term “modulus”, which is a diminutive of “modus”, meaning “manner” or “way”, but also “measure” or “measurement”. This term has since become customary in architecture and art, as well as its correspondingly derived terms “modelle” and “modèle” in French, “Modell” in German, and “modelo” in Spanish, through which the variety of meanings spread rapidly to the present abundance, in which it is used in all kinds of everyday situations (Müller 1983). In these situations, it is used both to refer to the thing “painted” or modeled and to the “painting” or model of some original.

In science it only began to be used towards the end of the 19th century, through the allusion to “mechanical models” or, with different terminology, “mechanical analogies”, proposed and discussed, among others, by Maxwell (1855; 1861), Thomson (1842; 1884), Boltzmann (1902) and Duhem (1906). Similarly, in the context of German physics, the term “Bild”, in singular, or “Bilder”, in plural was usual (Helmholtz (1894), Hertz (1894) and Boltzmann (1905), who discussed the “models” and developed a “Bild conception” of physics in particular and of science in general).

Its use, however, was not limited to the field of physics, but extended to other domains of science, being of central importance in many scientific contexts. Thus, in biology, for instance, it is standard practice to speak about the Lotka-Volterra model of predator-prey interaction or the double helix model of DNA or models organisms or models “*in vivo*” or “*in vitro*”. But neither in colloquial contexts nor in the diverse scientific contexts is the term “model” used in a non-unitary way, but rather it is an ambiguous, multivocal or polysemic expression that expresses more than one concept. As Nelson Goodman puts it: “Few terms are used in popular and scientific discourse more promiscuously than ‘model’ ” (Goodman 1976, p. 171). At the same time, as we pointed out, it must be taken into account that different terms, such as the mentioned “analogy” or “Bild”, have been used to refer to models. The term model applies to a bewildering set of objects from mathematical structures, graphical representations, computer simulations, to specific organisms or objects. Each of these objects seems so different that the idea has been firmly put forward that there is something that can be considered a model. And the means by which scientific models are expressed goes from sketches and diagrams to ordinary text, graphics and mathematical equations—just to name some of them.

In the face of such diversity, it is legitimate to raise the question of whether under the different uses of the term “model”, not only in the empirical sciences, but also in the formal sciences, lies the same notion and/or there are systematic links between them. Suppes (1960) has argued for an affirmative answer to the question of the unity of notion, while Black (1962), Achinstein (1968) and McMullin (1968), among others, have supported a negative one. Falguera (1992; 1994) has proposed a sort of defense of Suppes’ position, within the framework of structuralist metatheory, but doing so by assuming the *representational* perspective of Etchemendy (1988; 1990) in relation to models of formal semantics, rather than the standard *interpretational* one of Tarski (1936). He has also discussed the relationships between some of the different types of models, such as the so-called "*scale* models", "*mathematical* models", "*analogical* models" and "*theoretical* models" (Falguera 1993; 1994).

Being models—in some of their meanings—for many scientists and philosophers of science of central importance in a multiplicity of scientific contexts, a diversity of functions is also attributed to them, such as: enabling the application and testing of theories, helping to construct theories, promoting the understanding of theories and abstract formalisms, contributing to the expansion and transformation of theories, mediating between theories and the world, serving as a pragmatic substitute for theories, enabling the description and preparation of data, being a component of explanations, helping to establish causal relationships between events, enabling the understanding of a concrete object or system, being useful in the classroom as pedagogical aids, helping to construct and evaluate experiments, and representing phenomena.

On the other hand, different authors have proposed different typologies and classifications (neither necessarily exhaustive nor, much less, exclusive) in order to analyze models and to understand their nature and function in science.

In fact, also the questions that have been discussed around the models have been of varied nature—e.g. ontological (what kind of entities are models?), semantic (what is the representational function that models perform?), epistemic (how do we learn with models?) and of general philosophy of science (how do models relate to theories?) (Frigg and Hartmann 2005; 2006; 2012). These questions, while varied in nature, are closely linked.

But although the literature in philosophy of science has concentrated mainly on the so-called "theoretical models" (Black 1962), not everyone agreed with the exact role that models played in the empirical sciences nor with their relevance for them. Neither does everyone agree on their relation with the laws and empirical theories and the eventual need to take them into consideration as components of the latter.

With respect to the philosophical discussion of such models, four temporally consecutive phases can be distinguished throughout the twentieth century (Hartmann 2010) and even so far in this century, since we can be considered to be still in the fourth of the phases. We may agree with Jim Bogen, when he states in the back cover of the book *Scientific Models in the Philosophy of Science* (Bailer-Jones 2009) that "The standard philosophical literature on the role of models in scientific reasoning is voluminous, disorganized, and confusing".[17] And we may also agree that one of the axes already mentioned that would enable the organization of at least part of such literature, and with which the book ends, is what is identified as one of the "contemporary philosophical issues: how theories and models relate each other" (Bailer-Jones 2009, p. 208).[18] Taking this into account, we can characterize the different phases referred to above as follows.

The *first phase* begins at the early 20th century with the analyses of the French physicist and philosopher of science P. Duhem (1906), who contrasts the role and meaning of mechanical models—conceived as graphic, illustrative, visible, tangible or palpable, and as characteristic of what he calls "the English spirit" and the "English school"—with the fundamental theories—which

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[17] To get an overview and delve deeper into various aspects of the philosophical debate on models and modeling, see also Morgan (2012), Weisberg (2013), Gelfert (2016) and Frigg (2023).

[18] For a different account of the relation between models and theories, though in a similar vein to the one presented here—in the sense of stressing the continuing importance of theories and the "partial" autonomy of models respect to theories—, see Morrison (2007; 2016).

he conceives as abstract and characteristic of what he calls "abstract", "imaginative spirits", and of the "French" and "German schools". Although he considers a physics that relies primarily on the use of models to be of lesser value and provisional, he nevertheless achieves a characterization of scientific models that set a course and allows him to assign an objective to the construction of models and a guiding function to models in the research process. However, in his view, physics ultimately aims at general, abstractly formulated principles and theories (Duhem 1906, Chapter 4). In contrast to this, Duhem's British antagonist, N. Campbell (1920), stresses precisely the necessity of analogies as essential parts of theories, even though he does not explicitly use the term "model".

In the *second phase*, corresponding to logical empiricism, models are conceived primarily as marginal phenomena of science. Thus, e.g., R. Carnap writes: "It is important to realize that the discovery of a model has no more than an aesthetic or didactic or, at best, heuristic value, but is not at all essential for a successful application of the physical theory" (Carnap 1939, p. 68). However, later authors, such as R. Braithwaite (1953) and E. Nagel (1961), strive to incorporate models, and to recognize their importance, within the framework of the *classical* (or received) view of scientific theories, even if a purely syntactic-formal treatment of the model concept is shown to be problematic (Psillos 1995).

In the 1960s and 1970s, the *third phase*, coinciding in time with the development of the *historical* (or historicist) conceptions of science (to which we will return later), a number of authors participated in this problematic (Achinstein 1968; Apostel 1961; Black 1962; Bunge 1973; Byerly 1969; Harré 1970; Hesse 1966; Hutten 1954; McMullin 1968; Suppes 1960; 1962). There originate, on the one hand, works that try to reconcile the most strongly formalist and model-theoretic proposals with the diversity of scientific practice (Apostel 1961; Bunge 1973) and, on the other hand, alternative proposals to the views of the logical empiricists are developed, which emphasize the role of models in scientific practice (Achinstein 1968; Hesse 1966; Harré 1970). In connection with this, it is also investigated what role analogies and metaphors play in the construction of models (Black 1962; Hesse 1966) or of other components, linked to these, put forward by historicist philosophers of science, like Kuhn's exemplars ([1962] 1970; 1970; 1974a; 1974b; 1979).

In the *fourth phase*, beginning around 1980, the importance of models in scientific practice (including conceptualization and theorizing) is emphasized. *Model* views of science and the so-called *model-based* science are developed—addressing, among others, the issues of the relationship between models and experience and between models and general theories independently of a general metatheory of science—as well as *semantic* (or model-theoretic) views of science—which address such issues within the framework of a general conception of scientific theories (and to which we will return later). But whether within model views, model-based science or semantic views, there is an attempt to understand not only what models are, but also how they work and even how they are constructed from detailed case studies belonging to different sciences. In addition, the consequences that model-building practice has for other philosophical questions are highlighted, such as realism—linked to the discussion of idealization, approximation and representation in science—,[19] reductionism—even in authors for whom there are no systematic

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[19] See Nowak (1979), van Fraassen (1980; 1987; 2008), Laudan (1981), Sneed (1983), Cartwright (1983; 1989), Laymon (1985), McMullin (1985), Mundy (1986), Stegmüller (1986), Balzer, Moulines and Sneed

relationships between models and theories—,[20] besides to the already mentioned laws of nature and laws of science,[21] and scientific explanation.[22]

In biology the term model is used in different ways, calling "models" different entities, whether equations (e.g. the Lotka-Volterra equations), idealized representations of empirical systems (e.g. as stated by the Hardy-Weinberg law), exemplars (e.g. "Mendelian" or classical account of the inheritance of colour seed peas), organisms (e.g. *Drosophila melanogaster*) or physical objects (e.g. the double helix model of DNA), among other things.

But whether in one sense or the other, there are authors who from the 1950s onwards emphasize the importance of models—especially mathematical or theoretical models—in the biological sciences and try to analyze them outside the framework of the semantic conceptions previously mentioned (Beckner 1959; Beament 1960; Holling 1964; Simon 1971; Schaffner 1980; 1986; Barigozzi 1980; Wimsatt 1987; Fox Keller 2000; Morrison 2002; 2004; Godfrey-Smith 2006; Winther 2006; Weisberg 2007; 2013; Knuuttila and Loettgers 2016). Levins (1966) occupies a central place in the discussion about models and model building in biology. Since then, his proposal about the existence of a three-way trade-off between generality, realism, and precision, such that a model builder cannot simultaneously maximize all of these *desiderata*, has been much discussed (e.g. Orzack and Sober 1993; Levins 1993; Orzack 2005; Odenbaugh 2003; Weisberg 2006; Matthewson and Weisberg 2009). And the role of Kuhnian-type exemplars has also been investigated in the field of biology (Schaffner 1980; 1986; Darden 1991; Lorenzano 2007a; 2008a; 2012; Skopek 2011). On the other hand, other types of models, different from the theoretical ones, such as material models of several kinds, have also been the object of analysis (see, e.g, Griesemer 1990 and Laublichler and Müller 2007, and for organisms in particular, the pioneer works of Burian 1993 and Kohler 1994, and, for an account and updated overview of this growing subject, Ankeny and Leonelli 2020, online/2021, print).

3.2. The Structuralist Concept(s) of Model

As would be expected, being a member of the semantic family, the structuralist view shares with all the other family members the fundamental thesis on the centrality of models for metatheoretical analysis. But, on the other hand, it sometimes differs from other members of the semantic family in its characterization of the precise nature of these entities that are called models, although occasionally it coincides.

(1987), Giere (1988; 1994; 2006), Suppe (1989), Worrall (1989), Swoyer (1991), Brzeziński and Nowak (1992), Mäki (1994), Ibarra and Mormann (1997; 2000), Díez and Falguera (1998), Ladyman (1998), Psillos (1999), Niiniluoto (2000), Chakravartty (2001; 2007), Suppes (2002), Casanueva and Benítez (2003), French and Ladyman (2003), Teller (2004), Morrison (2005), Rueger (2005), Frigg and Votsis (2011) and Frigg (2023).

[20] See Hacking (1983), Balzer, Pearce and Schmidt (1984), Moulines (1984), Balzer, Moulines and Sneed (1987), Bickle (1995; 1998; 2002; 2003), Falkenburg and Muschik (1998), Cartwright (1999), Hartmann (1999), Batterman (2002), Bokulich (2003) and Hartmann (2008).

[21] See, among others, Cartwright (1983; 1999), Giere (1999), van Fraassen (1989), Forge (1986; 1999), Lorenzano (2006b; 2007b; 2007c; 2008b; 2014; 2014-2015; 2019), Chakravartty (2007).

[22] See van Fraassen (1980), Cartwright (1983), Bartelborth (1996a; 1996b; 1999; 2002), Forge (1999; 2002), Elgin and Sober (2002), Díez (2002; 2014), Woodward (2003), Moulines (2005), Lorenzano (2005), Bokulich (2003; 2009; 2011; 2017), Kennedy (2012), Reiss (2012), Díez and Lorenzano (2013; 2015), Ginnobili and Carman (2016) and Lorenzano and Díez (2022).

A model, in its minimal informal meaning, is a system or structure which intends to represent, in a more or less approximative way, a "portion of reality", made up by entities of various kinds, which *makes* true a series of claims, in the sense that in this system "what the claims say occurs", or more precisely, the claims are true in this system.

Models are conceived as systems or structures, i.e. mathematical structures. In the standard version of metatheoretical structuralism, these structures are set-theoretical or relational structures of a certain kind,[23] constituted by a series of basic domains (sets of objects) and of relations (or functions) over them, i.e. as entities of the form: $\langle D_1,..., D_k, R_1,..., R_n\rangle$, where $R_j \subseteq D_{i1} \times ... \times D_{ik}$ (the D_i's represent the so-called "base sets", i.e. the "objects" the theory refers to, its ontology, whereas the R_j's are relationships or functions set-theoretically constructed out of the base sets).[24]

In order to provide a more detailed analysis of empirical science, Metatheoretical Structuralism distinguishes three kinds of (classes, sets, populations, collections or families of) models. Besides what are usually called (the class, set, population, collection or family of) "*theoretical* models"

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[23] In trying to be as precise as possible, Metatheoretical Structuralism prefers the use of (elementary) set theory—whenever possible—as the most important formal tool for metatheoretical analysis. However, this formal tool is not essential for the main tenets and procedures of the structuralist representation of science (other formal tools, such as logic, model theory, category theory, and topology, as well as informal ways of analysis, are also used). Besides, there are also uses of a slight variant of Bourbaki notion of "structure species" in order to provide a formal basis of characterizing classes of models by means of set-theoretic predicates (Balzer, Moulines and Sneed 1987, Chapter I), and of a version of the von Neumann-Bernays-Gödel-type of language including urelements for providing a purely set-theoretical formulation of the fundamental parts of the structuralist view of theories (Hinst 1996). There is even a "categorial" version of Metatheoretical Structuralism that casts the structuralist approach in the framework of category theory, rather than within the usual framework of set theory (see Balzer, Moulines and Sneed 1986; Sneed 1984; Mormann 1996). The choice of one formal tool or another or of a more informal way of analysis is a pragmatic one, depending on the context which includes the aim or aims of the analysis and the target audience. Nonetheless, in standard expositions of Metatheoretical Structuralism, as well as in what is presented here, models are conceived of as *set-theoretical structures* (or models in the sense of *formal semantics*), and their *class* is identified by defining (or introducing) a *set-theoretical predicate*, just as inthe *set-theoretical* approach of Patrick Suppes (1957; 1967; 1970; 2002; McKinsey, Sugar and Suppes 1953).

[24] In a complete presentation, we should include, besides the collection of so-called *principal base sets* $D_1,...,$ *Dj or* $D_1,..., D_k$, also a second kind of base sets, namely, the so-called *auxiliary base sets* $A_1,..., A_m$. The difference between them is the difference between base sets that are empirically interpreted (the principal ones) and base sets that have a purely mathematical interpretation, like the set $\mathbb{N}$ of natural numbers, or the set $\mathbb{R}$ of real numbers (the auxiliary ones). Here, auxiliary (purely mathematical) base sets are treated as "antecedently available" and interpreted, and only the proper empirical part of the models is stated in an explicit way.

On the other hand, in philosophy of logic, mathematics, and empirical science, there have intense discussion about what would be a better way of understanding the nature of sets occurring in the relational structures, and of the models themselves. In relation to sets, according to the standard interpretation of 'sets-as-one' (Russell 1903) or 'the highbrow view of sets' (Black 1971) or 'sets-as-things' (Stenius 1974) sets themselves, though not necessarily their elements which may refer to concrete entities, should be considered as abstract entities, while according to the interpretation of 'sets-as-many' (Russell 1903) or 'the lowbrow view of sets as collections (aggregates, groups, multitudes)' (Black 1971) or 'sets-of' (Stenius 1974) sets do not have to be interpreted that way. For theoretical models, even though they are usually considered as abstract entities, there is no agreement about what kind of abstract entities they are, i.e. what is the best way of conceive them—either as interpretations (Tarski 1935; 1936) or as representations (Etchemendy 1988; 1990), or as fictional (Godfrey-Smith 2006; Frigg 2010) or as abstract physical entities (Psillos 2011). However, due to space limitations, we will not delve into these issues.

or simply (the class, set, population, collection or family of) "*models*"—also called (the class of) "*actual* models" in structuralist terminology—, the so-called (class of) "*potential* models" and (class of) "*partial* potential models" are taken into account.

To characterize these structuralist notions, two distinctions are to be considered: the distinction between two kinds of 'conditions of definition' (or 'axioms', as they are also called) of a set-theoretical predicate, and the distinction between the **T**-theoretical/**T**-non-theoretical terms (or concepts) of a theory **T**. According to the first distinction, the two kinds of conditions of definition of a set-theoretical predicate are: 1. those that constitute the 'frame conditions' of the theory and that "do not say anything about the world (or are not expected to do so) but just settle the formal properties" (Moulines 2002, p. 5) of the theory's concepts; and 2. those that constitute the 'substantial laws' of the theory and that "do say something about the world by means of the concepts previously determined" (Moulines 2002, p. 5).

According to the second distinction, which replaces the traditional, positivistic theoretical/observational distinction, it is possible to establish, in (almost) any analysed theory, two kinds of terms or concepts, in the sense delineated in an intuitive formulation by Hempel (1966; 1969; 1970) and Lewis (1970): the terms that are specific or distinctive to the theory in question and that are introduced by the theory **T**—the so-called '**T**-theoretical terms or concepts'—and those terms that are already available and constitute its relative "empirical basis" for testing—the so-called '**T**-non-theoretical terms or concepts', which are usually theoretical for other presupposed theories **T**', **T**'', etc.

In accordance with the standard structuralist criterion of **T**-theoreticity (originated in Sneed 1971 and further elaborated in detail in the Structuralist program; see Balzer, Moulines and Sneed 1987, Chapter II), a term is **T**-theoretical (i.e. theoretical relative to a theory **T**) if *every* method of determination (of the extension of the concept expressed by the term) depend on **T**, i.e. if they are **T**-dependent, if they presuppose or make use some law of **T**; otherwise, a term is **T**-non-theoretical, i.e. if at least *some* method of determination (of the extension of the concept expressed by the term) *doesn't* presupposes or make use of some law of **T**, if it is **T**-independent.

Now we are in position to characterize these structuralist basic notions:

1) The class of *potential models* of the theory $\mathbf{M_p}$ is the total class of structures that satisfy the "frame conditions" (or "*improper* axioms") that just settle the formal properties of the theory's concepts, but not necessarily the 'substantial laws' of the theory as well.
2) The class of (actual) *models* of the theory **M** is the total class of structures that satisfy the "frame conditions", and, in addition, the "substantial laws" of the theory. If $A_1,\dots, A_s$ are certain formulas ("*proper* axioms" or simply "axioms") that represent the laws of the theory, *models* of the theory are structures of the form $\langle D_1,\dots, D_k, R_1,\dots, R_n\rangle$ that satisfy the axioms $A_1,\dots, A_s$. (And that is the reason why, as it was mentioned before, models may be considered the model-theoretic counterpart of theory's laws.)
3) The class of *partial potential models* $\mathbf{M_{pp}}$ are obtained by "cutting off" the **T**-theoretical concepts from the potential models $\mathbf{M_p}$($\mathbf{M_{pp}} := \mathbf{r}(\mathbf{M_p})$, where **r**, the "restriction" function, is a many-one function such that $\mathbf{M_p} \rightarrow \mathbf{M_{pp}}$). If potential models are structures of type x ($x = \langle D_1,\dots, D_k, R_1,\dots, R_n\rangle$), *partial potential models* $\mathbf{M_{pp}}$ are structures of type y ($y = \langle D'_1,\dots,$

D'_j, $R'_1,\ldots, R'_m\rangle$), where each structure of type y is a *partial substructure* of a structure x.[25] (And let's call a specific structure of type y, with specific instances of the **T**-non-theoretical concepts, a "*data model*" of **T**).[26]

Now, let us identify all these kinds of models in Classical Genetics, starting with data models, and then moving on to potential models first and then to theoretical models that result in a successful application—exemplified with a detailed analysis of the first case of Section 2.1—to end with the classes of potential models, models and partial potential models.

3.3. Models in Classical Genetics (CG)

If the examples of hereditary transmission given above (in Section 2.1) are to be represented in the structuralist format, they should be conceived as *data models* of **CG**. That is, they should be conceived as structures of type y of partial potential models: $y = \langle I, P, APP, MAT, DIST\rangle$, with specific values adopted by the concepts that occur therein each given example:

- the set of *individuals* ($I = \{i_1,\ldots,i_n\}$) that can be proper individuals as well as populations, i.e. individuals that make up "families", that is, populations connected by bonds of marriage, parentage or common descendent; we write $i \in J$ to express that any individual i is in the model;[27]
- the set of *phenotypes* ($P = \{\pi_1,\ldots,\pi_k\}$), where each phenotype $\pi \in P$ has the form $\langle c_1,\ldots, c_k\rangle$, where $c_1 \in C_1,\ldots, c_k \in C_k$ are characteristics of different types and $(C_i)_{i\leq k}$ symbolizes the whole set of *types of characteristics*. As can be easily seen, the actual primitive concept is this set of *types of characteristics*, while the set of phenotypes is a defined one);
- the function of *appearance* that assigns *their characteristics (phenotype)* to individuals—being parental ($APP(i_1) = \pi_1, APP(i_2) = \pi_2$) or progeny ($APP(i_n) = \pi_j$);
- the function of *mating* that assigns their progeny to pairs of individuals ($MAT(i_1, i_2) = \langle i_1,\ldots, i_n\rangle$, where the number n varies according to the parental individuals); and

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[25] A structure y is a *substructure* of another structure x (in symbols: $y \sqsubseteq x$) when the domains of y are subsets of the domains of x and, therefore, the relationships (or functions) of y are restrictions of the relationships (or functions) of x. A structure y is a *partial substructure* of x (also symbolized by $y \sqsubseteq x$) when, besides being a substructure of x, there is at least one domain or relationship (or function) in x that has no counterpart in y. The important thing is that the *partial* substructure y contains less components—domains or relationships (or functions)—than the structure x. Thus, structures x and y are of different logical types. If y is a *substructure* (either partial or not) of x, it is also said, inversely, that x is an *extension* of y.

[26] Phenomena are represented by means of (structures of the type of) partial potential models, while data are usually represented by finite substructures of (structures of the type of) partial potential models. Due to space limitations, we are unable to go into this topic in more detail. For further discussions on this issue, see Suppes (1962), Bogen and Woodward (1988), Woodward (1989; 1998), Ibarra and Mormann (1989), Mayo (1996), van Fraassen (2008), Massimi (2011), Lorenzano (2012).

[27] Classical Genetics is concerned with populations, inasmuch as reliable frequencies of traits in the progeny are not obtained through consideration of a single mate. Generally, the progeny of individual parents will not even exhaust all possible phenotypes. The real carriers of phenotype are individuals, but a population can be defined in terms of individuals in an explicit way as a subset of the power set of individuals in which their members are linked by kinship relations. A population so conceived is called a "family". In that sense, individual is the real primitive concept, and population and family are defined ones.

- the function of *distribution* of *characteristics (phenotype)* that assigns relative frequencies of occurrence of *characteristics (phenotype) in the progeny* to *characteristics (phenotype) of* pairs of parental individuals ($DIST(\pi_1, \pi_2) = \langle r_1\pi_1,..., r_k\pi_k\rangle$, where $\langle \pi_1,..., \pi_k\rangle$ denotes the sequence of the phenotypes of the progeny, $\langle r_1,..., r_k\rangle$ denotes a distribution written in an explicit manner as a k-tuple of numbers, $r_i \geq 0$, $\sum_{1\leq i\leq k} = 1$, where each real positive number r_i is the weight or probability of the phenotype number i that occurs in the corresponding sequence of the phenotypes of the progeny).

Structures of type *y* are used to represent in a model-theoretic, structuralist way those empirical (biological) phenomena that **CG** intends to account for—i.e. empirical (biological) systems, where individuals that make up families possess certain *characteristics* that *mate* and produce *progeny*, which also possess certain *characteristics* whose distributions are expressed in numerical ratios, proportions or relative frequencies—, which also constitute what allows us to test Classical Genetics, that is, its "(empirical) basis of testing".

Let's consider the case of *Peas Seed Color* in more detail, though. The system under consideration is constituted by a set I of individuals (plants or animals in general, peas in this case, parental or offspring). They form the objects involved in this intended application: $I = \{i_1,..., i_n\}$. The considered characteristics only refer to the color of the seed albumen. Thus, P (in general) or π (in this particular case) $= \{c_1, c_2\}$, where c_1 symbolizes the yellow color and c_2 the green color. These are the only characteristics possessed by the individuals of interest in this case: $APP(i_i) = c_1$, $APP(i_i) = c_2$. If we represent the crossing among the parental individuals that give rise to the first filial generation (or F_1) by MAT, we have: $MAT(i_1, i_2) = \langle i_1,..., i_n\rangle$; the same applies to the second filial generation (or F_2): $MAT(i_1, i_2) = \langle i_1,..., i_n\rangle$. If we represent the distribution of parental characteristics in the offspring by $DIST$, we have: $DIST(c_1, c_2) = 1c_1$ in F_1, and: $DIST(c_1, c_1) = \langle 0{,}7505c_1, 0{,}2495c_2\rangle$ in F_2. We can now represent the *model of data* for the case of a monohybrid cross—for the color of the seed albumen—in peas by $\langle I, (C_i)_{i\leq k}), APP, MAT, DIST\rangle$, that expresses what we want to explain, i.e., the relative frequency $0{,}7505c_1$, $0{,}2495c_2$ of yellow and green seed coats, respectively, or (approximately) ¾ of offspring have yellow seed coat and ¼ have green seed coat or a proportion 3:1, as follows: $\langle\{i_1,..., i_n\}, \{c_1, c_2\}, \{\langle i_1, c_1\rangle, \langle i_2, c_1\rangle\}, \{\langle i_1, i_2, i_1,..., i_n\rangle\}, \{\langle c_1, c_1, 0{,}7505c_1, 0{,}2495c_2\rangle\}\rangle$, in F_2. Let's then call such a structure "the *data model* for **CG** of *Peas Seed Color*", or $DM_{\mathbf{CG}}(PSC)$ for short.

And if we now want to represent in structuralist format the different ways in which the given examples of cases of characteristics distributions are accounted for by introducing appropriate types and numbers of factors or genes(the genotype), the way in which they are distributed or combined in the progeny (as expected or theoretical probabilities), and the specific relationship in which they are with the characteristics of the individuals, we should consider these first as "potential models" of **CG** that, by adding the specific match/fit of genotype-distributions with phenotype-distributions, result then in "(actual) models" of **CG** and lastly in successful applications. That is, they should be conceived as structures of type x of potential models: $x = \langle I, (C_i)_{i\leq k}, (F_i)_{i\leq k}, APP, MAT, DIST, (DET_i)_{i\leq k}), COMB\rangle$ that are extensions of the structures of type *y*, which contain, besides specific instances of the concepts occurring in these structures, also specific instances of the following concepts:

- Firstly, the set of *genotypes* ($G = \{\gamma_1,...,\gamma_s\}$), where each genotype $\gamma \in G$ has the form of a finite list of *pairs of allelic factors* $\langle\langle f_{i1}, f_{i2}\rangle,..., \langle f_{s1}, f_{s2}\rangle\rangle$, where for every $I \leq s$ and any two factors f_{i1} and f_{i2} that belong to the same set F_i means that f_1 and f_2 are alleles or allelic factors. $(F_i)_{i\leq s}$ symbolizes the whole set of *types of factors*. As can be easily seen, the actual primitive concept is this set of *types of factors*, while the set of genotypes is a defined one).

- Also, the function of *determining* that maps genotypes into phenotypes such that pairs of allelic factors yields a unique π_i (or c_i), but taking into account that in general some phenotypes may be produced by different genotypes (for each genotype, we have equations of the form $DET_i(\gamma_i) = \pi_i$ (or c_i); and the whole function DET is defined as the tuple of all DET_i in the following way: $DET(\gamma) = \langle DET_1(\gamma),\ldots, DET_k(\gamma)\rangle$, where the last expression under consideration yields some phenotype $\langle \pi_1,\ldots, \pi_k\rangle$ (or $\langle c_1,\ldots, c_k\rangle$).
- Lastly, the function of combination or *distribution* of factors (genotype) that assigns probability distributions of factors (genotypes)in the progeny to factors (genotypes)of parental individuals ($COMB(\langle \gamma_1, \gamma_2\rangle) = \langle \alpha_1\gamma_1,\ldots, \alpha_s\gamma_s\rangle$, where $\langle \gamma_1,\ldots, \gamma_s\rangle$ denotes the sequence of the genotypes of the progeny, $\langle \alpha_1,\ldots, \alpha_s\rangle$ denotes a probability distribution written in an explicit manner as a s-tuple of numbers, $\alpha_i\geq 0$, $\sum_{1\leq i\leq s}= 1$, where each real positive number α_i is the weight or probability of the genotype number i that occurs in the corresponding sequence of the genotypes of the progeny).

Returning to the case of *Peas Seed Color*, the following is hypothesized:

(i) First, there is only one pair of factors or genes involved (that we can symbolize by $\langle f_1, f_2\rangle$).

(ii) Next, the factors or genes combination is equally probable, the parental factors or genes are distributed in the offspring with the same probability; such a function can be in general represented in the following manner: $COMB(\langle \mathrm{a}_1, \mathrm{b}_1\rangle, \langle \mathrm{c}_1, \mathrm{d}_1\rangle) = (¼\, \mathrm{a}_1\mathrm{c}_1 + ¼\, \mathrm{a}_1\mathrm{d}_1 + ¼\, \mathrm{b}_1\mathrm{c}_1 + ¼\, \mathrm{b}_1\mathrm{d}_1)$, where a_1, b_1, c_1, d_1 symbolize any factor or gene, and, in a specific manner, for the crossing carried out (F_2): $COMB(\langle f_1, f_2\rangle, \langle f_1, f_2\rangle) = (¼ f_1 f_1 + ¼ f_1 f_2 + ¼ f_2 f_1 + ¼ f_2 f_2)$.

(iii) Finally, one of the factors (f_1), which is "responsible" for the yellow color of the seed albumen, is *dominant* over the other (f_2), *recessive*, which is "responsible" for the green color of the seed albumen; the determining function can be represented as follows:

$$\begin{aligned} a)\, DET_i(f_2, f_2) &= c_2 \\ b)\, \left.\begin{aligned} DET_i(f_1, f_1) \\ DET_i(f_2, f_1) \\ DET_i(f_1, f_2) \end{aligned}\right\} &= c_1 \end{aligned}$$

If we put all the information of (i), (ii) and (iii) a), b), i.e. the specifications for the **CG**-theoretical concepts, together with the specifications for the **CG**-non-theoretical concepts contained in the structure of type y from above ($\langle I, (C_i)_{i\leq k}, (F_i)_{i\leq k}, APP, MAT, DIST\rangle$), we obtain the following structure of type $x = \langle I, (C_i)_{i\leq k}, (F_i)_{i\leq k}, APP, MAT, DIST, (DET_i)_{i\leq k}, COMB\rangle$: $\langle \{i_1,\ldots, i_n\}, \{c_1, c_2\}, \{\langle f_1, f_2\rangle, \langle f_1, f_2\rangle\}, \{\langle i_1, c_1\rangle, \langle i_2, c_1\rangle\}, \{\langle i_1, i_2, i_1,\ldots, i_n\rangle\}, \{\langle c_1, c_1, 0{,}75c_1, 0{,}25c_2\rangle\}, \{\langle f_1, f_1, c_1\rangle, \langle f_2, f_1, c_1\rangle, \langle f_1, f_2, c_1\rangle, \langle f_2, f_2, c_2\rangle\}, (¼ f_1 f_1 + ¼ f_1 f_2 + ¼ f_2 f_1 + ¼ f_2 f_2)\rangle$. Let's then call it "the *potential model* for *Peas Seed Color* of **CG**", or $PM_{\mathbf{CG}}(PSC)$ for short.

When the different specific potential models of **CG** postulate, in addition, a "match" or "fit" between the distribution of characteristics (phenotype-distribution) and the distribution of factors/genes (genotype-distribution) in the progeny, we obtain the "(actual) models" of **CG**—what is the model-theoretic way of saying the "special laws" of **CG**. And if the "match" or "fit" can be established, they result in successful applications (as they actually do in the treatment of the examples presented in Section 2.1).

For the case of *Peas Seed Color*, if it is required that the structure of type x satisfies, *in addition*, the specific form adopted for the treatment of this case of condition (iv) of the Fundamental Law/Guiding Principle of Classical Genetics (**CGGP**), i.e. the match (or fit) of factors distribution $((¼f_1f_1 + ¼f_1f_2 + ¼f_2f_1 + ¼f_2f_2))$ with characteristics distribution$(\langle c_1, c_1, 0{,}75c_1, 0{,}25c_2\rangle)$, we obtain what we call "the (*actual*)*model* for *Peas Seed Color* of **CG**", or $M_{\mathbf{CG}}(PSC)$ for short.

In a similar way, as in the case of the different laws, one can ask now what all these data models, all these models and all these successful applications have in common. And the answer to these questions is straightforward, using the notions of class of partial models, of potential models, and of models introduced in the previous section.

What all these data models have in common is that they are specifications of partial potential models of **CG** (i.e. **CG**-non-theoretical concepts); what all these theoretical models have in common is that they are specifications first of potential models of **CG** (i.e. **CG**-non-theoretical concepts as well as **CG**-theoretical concepts), and then, by postulating the match or fit between distributions of genotypes and phenotypes, of (actual) models of **CG**; what all these successful applications have in common is that they are specifications of (actual) models of **CG** in which the match/fit between distributions ofgenotypes and phenotypes is established.

We will next identify such classes of types of models in **CG**, starting with the class of potential models, continuing with the class of models, and concluding with the class of partial potential models.

The class of *potential models of classical genetics* ($\mathbf{M_p}(\mathbf{CG})$) is constituted by the total class of structures that satisfy the "frame conditions" (the so-called "*improper* axioms") that just settle the formal properties of **CG**'s concepts, but not necessarily the 'substantial laws' of **CG** as well, and for which it makes sense to wonder if they are actual models of **CG**.

We can put together all **CG**'s basic concepts in one structure x, which thus contains the "conceptual framework" of **CG**: $x = \langle I, (C_i)_{i\leq k}, (F_i)_{i\leq k}, APP, MAT, DIST, (DET_i)_{i\leq k}, COMB\rangle$, and then formulate the "frame conditions" for **CG**'s basic concepts as follows (by means of the introduction or definition of the set-theoretical predicate "being a potential model of *classical genetics*"):

Definition 1

$\mathbf{M_p}(\mathbf{CG})$: $x = \langle I, (C_i)_{i\leq k}, (F_i)_{i\leq k}, APP, MAT, DIST, (DET_i)_{i\leq k}, COMB\rangle$ is a *potential model of classical genetics* ($x \in \mathbf{M_p}(\mathbf{CG})$) if and only if

(1) I is a non-empty, finite set ("individuals": variable i)

(2) $(C_i)_{i\leq k}$ is a non-empty, finite set ("types of characteristics": variable c_i)

(3) $(F_i)_{i\leq s}$ is a non-empty, finite set ("types of (allelic) factors": variable f_i)

(4) $APP: I \rightarrow Po((C_i)_{i\leq k})$ ("appearance": $APP(i) =$)

(5) $MAT: I \times I \rightarrow \mathrm{Po}(I)$ is a partial function ("mator": $MAT(i, i') = \langle i_1,\ldots,i_n\rangle$)

(6) $DIST: Po((C_i)_{i\leq k}) \times Po((C_i)_{i\leq k}) \rightarrow \mathrm{D}(Po((C_i)_{i\leq k}))$ is a partial function ("distributor": $DIST(\pi, \pi') = \langle r_1\pi_1,\ldots, r_k\pi_k\rangle$)

(7) $(DET_i)_{i\leq k}$: $Po((F_i)_{i\leq k}) \rightarrow Po((C_i)_{i\leq k})$ is surjective ("determiner": $DET_i\ ({}_i) = \pi_i$)

(8) $COMB$: $Po((F_i)_{i\leq k}) \times Po((F_i)_{i\leq k}) \rightarrow \mathrm{D}(Po((F_i)_{i\leq k}))$ ("combinator": $COMB(\gamma, \gamma') = \langle \alpha_1\gamma_1,\ldots, \alpha_s\gamma_s\rangle$)

The objects that occur in the predicate *may be interpreted as follows*:

(1) I represents the set of *individuals* (parents and progeny), which constitute populations linked by kinship relations called "families" (see note 3).

(2) $(C_i)_{i\leq k}$ represents the set of *types of characteristics* that constitute the different *phenotypes* $(\pi_1,\ldots,\pi_k)$; each phenotype $\pi \in P$ has the form $\langle c_1,\ldots, c_k\rangle$, where $c_1 \in C_1,\ldots, c_k \in C_k$.

(3) $(F_i)_{i\leq s}$ represents the set of *types of (allelic) factors* that constitute the different *genotypes* $(\gamma_1,\ldots,\gamma_s)$; for every $I \leq s$ and any two factors f_1 and f_2, that f_1 and f_2 belong to the same set F_i means f_1 and f_2 are alleles; each genotype $\gamma \in G$ has the form of a finite list of *pairs of allelic factors* $\langle\langle f_{i1}, f_{i2}\rangle,\ldots, \langle f_{s1}, f_{s2}\rangle\rangle$, where $i \leq s$ and f_{s1} and f_{s2} are members of F_i.

The functions that occur in the predicate *are interpreted as follows*:

(4) APP represents the *appearance* of the individuals—being parental or progeny—, given by *a function that assigns their characteristics to individuals*, symbolized by equations of the form $APP(i_1, i_2) = \langle i_1,\ldots, i_n\rangle$, where the number n varies according to the parental individuals.

$$APP(i_1) = \pi_1, \langle c_1,\ldots, c_k\rangle$$
$$APP(i_2) = \pi_2, \langle c_1,\ldots, c_k\rangle$$
$$APP(i_n) = \pi_j, \langle c_1,\ldots, c_k\rangle \text{ (where } i \leq n, j \leq k).$$

(5) MAT represents the *mating* of the individuals, given by a partial function which represents the transition from the parents to their progeny, symbolized by equations of the form $MAT(i_1, i_2) = \langle i_1,\ldots, i_n\rangle$, where the number n varies according to the parental individuals.

(6) $DIST$ represents the *transition from parental phenotypes to distribution of phenotypes in the progeny* given by relative frequencies (actually, $DIST$ is not a real primitive concept, then it can be defined through MAT and APP; beginning with two parental individuals i_1, i_2, we see the value of $MAT(i_1, i_2)$, i.e., the set of progeny $\{i_1,\ldots, i_n\}$; we determine the value of $APP(i_n)$ for $i \leq n$, i.e, phenotypes that occur in the progeny; we count the total number n of progeny as well as the number m_i of progeny that exhibits a given phenotype and calculate the relative frequency $r_i = m_i/n$ of that phenotype; the list of all relative frequencies obtained for the different progenies is thus the desired distribution of phenotypes in the progeny of i_1 and i_2, that is, the value of $DIST(i_1, i_2)$);[28] the notation $DIST(\pi_1, \pi_2) = \langle r_1\pi_1,\ldots, r_k\pi_k\rangle$, where all r_i are real positive numbers, such that $\sum_{1\leq i\leq k} r_i = 1$, represents a distribution of phenotypes in explicit form.

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[28] For a phenotype $\pi \in P$ and a set of individuals $X \subseteq I$, the *relative frequency of π in X*, $RF(\pi/X)$ is defined in the following manner:

If X is a set of proper individuals, then

$RF(\pi/X)$ = (the number of $i \in X$, such that $APP(i) = \pi$) over

(7) $(DET_i)_{i \leq k}$ represents the *determination* of phenotypes by genotypes given by a function that assigns phenotypes to genotypes (for each genotype, we have equations of the form $DET_i(\gamma_i) = \pi_j$, and, for all genotypes γ, one equation of the form $DET(\langle \gamma_1, \ldots, \gamma_s \rangle) = \langle DET_1(\gamma), \ldots, DET_k(\gamma) \rangle$, where the last expression under consideration yields some phenotype $\langle \pi_1, \ldots, \pi_k \rangle$ (where $I \leq s, j \leq k$)).

(8) *COMB* represents the *transition from parental genotypes to genotypes in the progeny*, assigning to parental genotypes a combination (distributions) of genotypes in the progeny (we have equations of the form $COMB(\langle \alpha_1 \gamma_{11}, \ldots, \alpha_s \gamma_{1s} \rangle, \langle \alpha_1 \gamma_{21}, \ldots, \alpha_s \gamma_{2s} \rangle) = \langle \alpha_1 \gamma_{11}, \ldots, \alpha_j \gamma_{ij}, \ldots, \alpha_s \gamma_{ns} \rangle$ (where all α_i are positive real numbers, such that $\sum_{1 \leq i \leq s} \alpha_i = 1$)).

We can now define the class of (actual) *models of classical genetics* (**M**(**CG**)), which is the total class of structures of type x: $x = \langle I, (C_i)_{i \leq k}, (F_i)_{i \leq k}, APP, MAT, DIST, (DET_i)_{i \leq k}, COMB \rangle$ that satisfy the "frame conditions", and, in addition, the "substantial laws" of **CG** (the so-called "*proper* axioms") as follows:

Definition 2

M(**CG**): If $x = \langle I, (C_i)_{i \leq k}, (F_i)_{i \leq k}, APP, MAT, DIST, (DET_i)_{i \leq k}, COMB \rangle$, then x is a *model of classical genetics* ($x \in$ **M**(**CG**)) if and only if

(1) For any I and P: for any $i, i' \in I$ such that MAT is defined for $\langle i, i' \rangle$ and for any $\pi \in P$, there exist s genotypes $G, \gamma_1, \ldots, \gamma_s$, a function $COMB$, and k functions $DET, DET_1, \ldots, DET_k$, such that:

$$COMB(\gamma, \gamma') = DIST(DET_i(\gamma_i), DET_i(\gamma_i'))$$

The condition of definition, or axiom, (1) formulates in a more formal way the fundamental law/guiding principle of classical genetics (**CGGP**) of Section 2.1, which establishes that a "match" or "fit" takes place between the observed distributions of characteristics (phenotype-distributions) and the distributions of factors/genes (genotype-distributions). Every structure of type x that satisfies it is an (actual) model of **CG**.

In order to achieve a thorough understanding of this law, let us consider two parental individuals with phenotypes π, π', genotypes γ, γ' and the corresponding distributions over phenotypes and genotypes in their progeny: $d_{ph} = \langle r_1 \pi_1, \ldots, r_k \pi_k \rangle$, $d_{ge} = \langle \alpha_1 \gamma_1, \ldots, \alpha_s \gamma_s \rangle$. Consider first the simple case in which DET is one-one. In this case each phenotype π_j comes from exactly one of the genotypes $\gamma_1, \ldots, \gamma_s$. So $k = s$ and we may assume that each π_j is produced by γ_j. The natural notion of fit between the two distributions $\langle r_1 \pi_1, \ldots, r_k \pi_k \rangle$, $\langle \alpha_1 \gamma_1, \ldots, \alpha_s \gamma_s \rangle$ is this. We say that d_{ph} and d_{ge} ideally fit with each other if and only if, for all $j \leq s$: $r_j = \alpha_s$.

However, given that, as already mentioned, some phenotypes may be produced by different genotypes, the situation, in general, is not as simple as that. In these cases, we have to compare the probabilities of all these genotypes with the relative frequency of the phenotype they all produce. Formally, let us introduce, for given parental genotypes γ, γ', and given index $j \leq k$ the set $M(\gamma,$

(the number of elements of X).

For a phenotype $\pi \in P$ and a set of populations $X \subseteq \mathrm{Po}(I)$, the *relative frequency of* π *in* X, $FR(\pi/X)$ is defined in the following manner:

$FR(\pi/X)$ = (the number of elements in the sets $i \in X$, for which $APP(i) = \pi$) over

(the number of elements of elements of X).

$\gamma', j)$ of all probabilities α_i occurring in d_{ge} such that the corresponding genotype γ_i produces phenotype r_j (compare Figure 3). Moreover, let us write $m_j = \sum \alpha_i$, $i \in M(\gamma, \gamma', j)$, for the sum of all those probabilities α_i whose corresponding genotype γ_i give rise to the same π_j with relative frequency r_j.

$$\begin{tikzcd}
 & & M(\gamma, \gamma', j) = \{2, l, q\} & & \\
COMB(\gamma, \gamma') = d_{ge} = \alpha_1\gamma_1, & \alpha_2\gamma_2, \ldots, \arrow[ur] \arrow[dr] & \alpha_l\gamma_l, \ldots, \arrow[u] \arrow[d] & \alpha_q\gamma_q, \ldots, \arrow[ul] \arrow[dl] & \alpha_s\gamma_s \\
 & & d_{ph} = r_1\pi_1, \ldots, r_j\pi_j, \ldots, r_k\pi_k & & \\
 & & r_j = c_j = 2 + l + q & &
\end{tikzcd}$$

Figure 3

In order to fit d_{ph} and d_{ge} we then have to compare each relative frequency π_j with the sum m_j. We say that, in the general case, d_{ph} and d_{ge} ideally fit if and only if, for all $j \leq k$: $r_j = m_j$.

Two distributions of genotypes $\langle \alpha_1\gamma_1, \ldots, \alpha_s\gamma_s \rangle$ and of phenotypes $\langle r_1\pi_1, \ldots, r_k\pi_k \rangle$ *ideally fit* together if and only if:

i) $k \leq s$,[29]

ii) each phenotype π_j arises from one genotype γ_i or more different genotypes γ_i by means of DET_i,

iii) the probability coefficients of the items related in ii) ideally fit with each other.

Taking into account these clarifications about the notion of fit, we can reformulate the general equation $COMB(\gamma, \gamma') = DIST(DET(\gamma), DET(\gamma'))$ of the fundamental law/guiding principle of classical genetics (**CGGP**) in the following ways, either as

$$\sum\nolimits_{\alpha \in M(\gamma, \gamma', j)} \alpha = r_j$$

or, considering also the definition of $DIST$ through MAT and APP, as

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[29] Through this characterization of genotypes (in which it is not required that $s = k$, that is, admitting that various pairs of allelic factors may determine one and the same character) it is possible to include in the model both the *interaction of factors* and the so-called *hypothesis of multiple factors*. Note also that the present formalism captures the phenomenon of *multiple allelism*. This phenomenon is given by the fact that different individuals in a species may have different genotype components from the same factor set F_i. We allow for arbitrary, finite factor sets F_i. So, within a species there may be hundred or even thousands of different allelic pairs that are formed from one factor set.

$$\Sigma_{\alpha \in M(\gamma, \gamma', j)}\, \alpha = \frac{\| PROGENY\_j \|}{\| PROGENY\_1 \| + \ldots + \| PROGENY\_k \|}.$$

The class of *partial potential models* of **CG** characterizes the point of departure for Classical Genetics. It is constituted by that which is intended to systematize, explain and predict. In order to characterize this class, it is necessary to distinguish between theoretical and non-theoretical concepts within **CG**, i.e. between specific concepts of *classical genetics* (or **CG**-theoretical) and non-specific concepts of *classical genetics* (or **CG**-non-theoretical).

A detailed discussion of the application of the **T**-theoreticity criterion to every **CG**'s term (or concept) is beyond the aim of this paper. But we did it elsewhere (Lorenzano 2002), and reached the result that the set of *factors* or *genes* $(F_i)_{i \leq k}$ that may possess different alternative forms, even though they are paired in the individual, called "alleles", the *function* $(DET_i)_{i \leq k}$ *that assigns characteristics to pairs of factors* or *genes*, and the *function COMB that represents the transition from parental factors* or *genes to factors* or *genes in the progeny* are **CG**-theoretical while the rest of the concepts are **CG**-non-theoretical.

We are now able to characterize the class of partial potential models of **CG** through the set-theoretical predicate "being a partial potential model of *classical genetics*" as follows:

Definition 3

$\mathbf{M_{pp}}(\mathbf{CG})$: $y = \langle I, (C_i)_{i \leq k}, APP, MAT, DIST \rangle$ is *a partial potential model of classical genetics* ($y \in \mathbf{M_{pp}}(\mathbf{CG})$) if and only if exists an x such that

(1) $x = \langle I, (C_i)_{i \leq k}, (F_i)_{i \leq k}, APP, MAT, DIST, (DET_i)_{i \leq k}, COMB \rangle$ is a $\mathbf{M_p}(\mathbf{CG})$

(2) $y = \langle I, (C_i)_{i \leq k}, APP, MAT, DIST \rangle$.

4. The Concept of Theory from the Point of View of Metatheoretical Structuralism

4.1. Introduction

The term "theory", in English, appears in late 16th century denoting a mental scheme of something to be done, comes from the Latin word "theoria", which, in turn, comes from the ancient Greek word "θεωρία" (theōría), meaning "to see", "to look", "to observe" and then "knowledge". According to some, the Greek word derives from "Θεωρεῖν" (theorein) which comes from "θεωρός", "one who sees a spectacle", such as festivals of the gods or religious processions, oracles or theatrical scenes; while already in antiquity, some derived the etymology from the first part of the compound "θεωρός" from the word for god ("θεός") (König 1998). Later, the term "theory" is not only used for the observation or contemplation of certain sacred or festive events, but also for the "purely intellectual" consideration of abstract ideas, facts or states of affairs that are not accessible to sensory perception. Hence, since then, it has been customary to contrast "theory" with "experience"—from the Latin "experientĭa"—or with "empirical"—from the Greek "ἐμπειρικός" ("empeirikós"). In turn, in colloquial language, the term "theory" is used either as a vague supposition or pure speculation (without much hold) or as something that has not yet been "tested" (or contrasted); in the latter case, "theory" is used in the sense of "hypothesis" and the expression "hypothetically" is replaced by the phrase "in theory". Sometimes, the term "theory" is used in a (rather pejorative) sense as "mere theory" as opposed to "practice"—from the Greek "πρᾶξις" ("praxis")—or to "*actually functional* practice" (Thiel 1996).[30]

A primary use of the term "theory" is to refer to that (usually very complex) entity that organizes the phenomena of a subject area and describes the basic properties and relationships of the objects belonging to that area, postulating general laws (or principles) for them and making it possible to give explanations and to provide predictions about the occurrence of certain phenomena within that area.

In modern science, the term "theory" refers to scientific theories, and this is the meaning that interests us here.

However, with the constitution and development of different scientific disciplines and subdisciplines, the existence of theories with different levels of abstraction and pursuing different objectives can be recognized. Moreover, unlike what happens in philosophy of science, in what we would call the "meta-scientific-language-in-use-of-scientists" (and even "of those-who-write-textbooks" or "of those-who-dedicate-themselves-to-the-public-communication-of-science") a terminologically precise distinction is rarely made between hypotheses, laws (of different types and levels) and theories.

In the same way as in the case of the concept of law above there has long been discussed the problem of establishing the nature, structure and function of a scientific theory. After decades of discussion, different conceptions coexist, often at odds, of what a theory is, whether there is a theory structure that is shared by all scientific disciplines, and how a theory works.

As was already said, three main philosophical conceptions about scientific theories have been developed during the 20th and the 21st century so far: the "classical (or received)" view, the "historical (or historicist)" view and the "semantic (or model-theoretic)" view.

In all three of these metatheoretical conceptions, we can distinguish three general aspects in the explication of the concept of theory: one referring to the (more) "theoretical" (or "formal")

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[30] This sense is discussed, and rejected, by Kant in his "On the saying: This may be correct in theory, but it does not apply to practice" of 1793, in the following terms in the field that interests us:

> [...] it was not the fault of theory if it was of little use in practice, but rather of there having been *not enough* theory, which the man in question should have learned from experience and which is true theory even if he is not in a position to state it himself and, as a teacher [...]. (Kant 1793, pp. 202-203)
>
> Thus no one can pretend to be practically proficient in a science and yet scorn theory without declaring that he is an ignoramus in his field, inasmuch as he believes that by groping about in experiments and experiences, without putting together certain principles (which really constitute what is called theory) and without having thought out some whole relevant to his business (which, if one proceeds methodically in it, is called a system). (Kant 1793, p. 203)
>
> Now if an empirical engineer tried to disparage general mechanics, or an artilleryman the mathematical doctrine of ballistics, by saying that whereas the theory of it is nicely thought out it is not valid in practice since, when it comes to application, experience yields quite different results than theory, one would merely laugh at him (for, if the theory of friction were added to the first and the theory of the resistance of air to the second, hence if only still more theory were added, these would accord very well with experience). (Kant 1793, p. 204)

part, another to the (more) "empirical" ("applicative" or "testing") part, and the last referring to the relationship between both parts, between the "theoretical" and the "empirical", between the "theory" and the "experience". And one of the main differences between the classical, the historical and the semantic views lies in the central basic ideas they have about the general way of conceiving each of these aspects. On the other hand, it should be said that, despite the use of the singular definite article to refer to the three previous philosophical conceptions, each of them groups a number of versions, variants or approaches, which while sharing certain basic general ideas on these three aspects, differ from each other in the particular way they understand or elaborate these basic ideas.

Regarding the *classical view* it could be said that, although all classical philosophers of science considered theories to be sets of statements organized deductively or axiomatically, not all agreed on the specific way in which this should be understood and clarified (e.g. Popper 1935; Carnap 1956; Hempel 1958). For instance, in the most well-known and developed of Carnap's versions, the "theoretical" or "formal" part is constituted by the formal axiomatic system or "calculus" (symbolized by "T")—which only contain descriptive theoretical terms—; the (more) "empirical" or "testing" part is given by pure observational statements—which only contain descriptive observational terms—; and the relationship between "theory" and "experience" is established through linguistic means, the so-called correspondence rules (symbolized by "C")—which connect theoretical terms with observational terms.

The main representative of the classical conception of scientific theories in biology during the first half of the twentieth century was Joseph Henry Woodger, who aimed to apply and develop the philosophy of the science of logical positivism for the specific field of biology (Woodger 1937; 1939; 1952; 1959; 1965; for his axiomatization of classical genetics, see esp. Woodger 1959). A continuation of Woodger's work in the field of genetics can be found in H. Kyburg (1968), A. Lindenmayer and N. Simon (1980) and M. Rizzotti and A. Zanardo (1986a; 1986b).

In relation now to the general adequacy of the classical view for the analysis of biological theories, we could say that, though scientists like Conrad Hal Waddington (1968-1972) promoted and employed such a view, opinion about the applicability of the classical view to biology was divided among the philosophers. Thus, Morton Beckner (1959) did not accept the classical view, but assumed that its application to biology was, at best, limited and, with its treatment of biological theories as families of models, anticipated to some extent the analysis of such theories later carried out within the framework of the semantic conception. Thomas Goudge (1961; 1967), for his part, was quite explicit in pointing out that, regardless of the applicability that this conception might have in physics, it would not capture important features of biological theories and explanations. On the other hand, Mary Williams (1970), Michael Ruse (1973) and Alexander Rosenberg (1985) were philosophers who during the seventies and eighties sustained the applicability of the classical view of theories to biology. In particular, it has been argued (Thompson 1989) that the two most explicit and detailed attempts to provide an analysis of evolutionary theory within the classical conception are Michael Ruse's (1973) outline of the axiomatic structure of population genetics—which, he claims, constitutes the core of evolutionary theory—and Mary Williams' (1970) axiomatization of the theory of natural selection—which, we would say, contains a few peculiarities regarding the standard presentations and applications of the classical conception of theories. Here we should also mention the analysis of evolutionary theory carried out by Mario Bunge and Gregorio Klimovsky in terms of the classical conception, understanding theories as hypothetico-deductive systems consisting of a set of starting hypotheses, fundamentals, or principles, and all their logical consequences (which include derived hypotheses and observational consequences) (Klimovsky 1994), or as sets of propositions closed under deduction (Bunge and Mahner 1997). Other philosophers, on the other hand, argued in favor of the relevance of this conception for biological theorization, although with varying degrees of caveats and subtleties (e.g., Hull 1974).

As of the 1950s, the classical view on the scientific theories has been very much criticized. There were mainly two kinds of criticisms: a) criticisms of certain aspects of the classical view (e.g. the distinction between theoretical and observational terms), and b) a global criticism which attacks mainly the bases of this conception, proposing an alternative view on science.

Notwithstanding, it is possible to claim that the classical view of theories has been the most adopted in scientific disciplines in the last part of the twentieth century and is still presupposed nowadays in many of them.

The second kind of criticisms to the classical view of theories came mainly from historicist philosophers of science, such as Toulmin, Hanson, Feyerabend, Lakatos, Kuhn, Laudan and Shapere. And as a result of this, a new conception about the nature and the synchronic structure of scientific theories (without this being implied in a strict sense and without it being systematically developed) underlies the majority of diachronic studies and analyses, which is supposed to be closer to scientific practice, as history presents it to us. This new notion is developed in different ways by the so-called *new philosophers* of science. Central to the *historicist view* was the idea that scientific theories—which the historicist philosophers refer to with different terms—are not sentences or sentence sequences, and in a proper sense they cannot be described as true or false (although true or false empirical claims are certainly made with them), but they are highly complex and ductile entities, susceptible of evolving in time without losing their identity.

It may be said that, in the best known and most widespread version of the historical conception, namely, that provided by Kuhn (1970), the (more) “theoretical” or “formal” part of the theories (paradigms/disciplinary matrices) is constituted by the (verbal and) symbolic generalizations, while the (more) “empirical” or “applicative” part is given by the exemplars (or “examples of their function in use”). On the other hand, the link between both parts is established by what Kuhn calls “special (or appropriate) versions” and “particular (or detailed) symbolic forms (or versions or expressions)”, which acquire the symbolic generalizations in order to be applied to particular problems (situations, phenomena). And although Kuhn does not elaborate in detail what the relationship between symbolic generalizations and their particular forms is, as stated before, he makes it very clear that this is not one of logical deduction.

As for the application of Kuhnian views to the field of biology, we already find mentions of Darwin in Kuhn ([1962] 1970), in his argument against the understanding of the history of science in terms of teleological development towards truth (Kuhn [1962] 1970, p. 172) and as an example of one of the “great revolutions” (Kuhn [1962] 1970, p. 180), and to the *Origin of Species* (Darwin 1959) as one of “the classic books in which these accepted examples first appeared” (Kuhn [1962] 1970, p. 20). However, we do not see in it a systematic use and application of the notions of paradigm/disciplinary matrix in the analysis of particular cases pertaining to the field of biology,[31] but a discussion of whether or not a scientific revolution took place—although it is clear that the very notion of scientific revolution presupposes the notion of paradigm/disciplinary matrix, since

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[31] Something similar can be said about Laudan’s consideration of “Darwinism” as a research tradition.

"here we regard as scientific revolutions those episodes of non-cumulative development in which *an old paradigm is replaced in whole or in part by a new one* incompatible with it" (Kuhn [1962] 1970, p. 92; emphasis ours). And the same can be said about other historians and philosophers of science in general or of biology in particular,[32] who, even when they mention other cases such as Gregor Mendel and genetics (Cohen 1985), concentrate mainly on the discussion of, in the words of Mayr (1972), "the nature of the Darwinian revolution". (On the controversies surrounding Darwinian ideas and revolution, see, among other works, Ghiselin (1969; 2005), Hull (1973; 1985), Greene (1971), Mayr (1972; 1977; 1988; 1990), Ruse (1979; 1982; 2009), Oldroyd (1980), Wuketits (1987), Bowler (1988), Burian (1989), Levine (1992), Stefoff (1996), Proctor (1998), Junker and Hossfeld (2001), Herbert (2005), Hodge (2005), Smocovitis (2005). Even before Kuhn's proposals, there are already authors who refer to the "Darwinian revolution" (Judd 1912; Himmelfarb 1959). Sometimes (Maienschein, Rainger and Benson 1981; 1991), even accepting the revolutionary character of Darwin's work, it is questioned whether it implied a "radical", "total" or "absolute" rupture, since both discontinuities and continuities can be pointed out—which is perfectly compatible with Kuhn's thinking—, as well as the difficulty to say, in terms of "paradigms"/"disciplinary matrices", what this change consisted of—which shows, in our opinion, that no satisfactory notion of "theory"/"paradigm"/"disciplinary matrix", with clear criteria of identity, is presupposed).

On the other hand, we should note that the notion of paradigm that eventually historians and philosophers of biology have found most fruitful in carrying out their analyses, to which we have already referred, is that of "exemplar", either arguing that the theories of the biological (and/or biomedical) sciences possess a particular structure distinct from that of physical theories (Schaffner 1980; 1986; Darden 1991) or considering that this is not the case, if they are analyzed within the framework of some version of the semantic conception of theories (Schaffner 1993; Lorenzano 2007a; 2008a; 2012).

Moreover, even if it has not been a particularly privileged field, biology has not been completely alien to the use of Lakatos' concept of research program in its analyses. Thus, for example, it is used by Michod (1981) to analyze the history of population genetics, by Meijer (1983), Van Balen (1986; 1987), Martins (2002) and Lorenzano (2006a; 2013a) to analyze the history of the so-called "classical", "formal" or "Mendelian" genetics, by Denegri (2008) to analyze parasitology, and by Piavani (2012) to analyze the shift from the modern synthesis to the extended evolutionary synthesis.

Shapere's concept of field has also been applied to biology. In particular, the concept of field, and its derivatives of intra-field theory and inter-field theories, have been used mainly by Darden to analyze some biological theories, their development over time, as well as their interrelationships. We would like to mention especially the analysis of the theory of the gene as an intrafield theory, that of the chromosomal theory as an interfield theory (Darden and Maull 1977), and that of the changes that occurred in "Mendelian Genetics" between 1900 and 1926 as changes in the field of heredity (Darden 1974; 1991).

The new notion of theory proposed by the historicist philosophers is, however, so extremely

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[32] A notable exception, although in the field of biochemistry and in a "structuralized" version—that is, passed through the sieve of the structuralist conception of theories, to which we will refer later—, is Lorenzano, C. (1994).

imprecise at times that it ends up blurring almost completely what seem to be correct intuitions. The main motivation positivists or logical empiricists had for developing a formal philosophy of science was precisely to avoid a vague and imprecise metascientific discourse. And much of the controversy that arose after the appearance of the new philosophers was generated by the imprecision and ambiguity of some of its main notions.

The majority of the philosophers of science who were sensitive to the historicist perspective concluded that the complexity and richness of the elements involved in science, escaped any attempt at formalization. It was considered that not only were the formalizations like those made within the classical view of theories totally inadequate to express these entities in all their complexity, but it did not seem reasonable to expect that any other procedure of formal analysis should grasp the minimum elements of this new characterization. This is the antiformalist moral that spread in many metascientific environments after the *historicist revolt*.

However, some of the more recent currents in philosophy of science show that vagueness and ambiguity are not necessary components of the philosophical reflection about science and that at least part of the new elements mentioned by the new philosophers are susceptible to formal reconstruction and reasonable formal analysis. Thus, during the 1970s trust in the viability of the formal or semiformal analyses of science is recovered, at least in some of its areas, particularly in that which refers to the nature of theories.

An example of this is Philip Kitcher's concept of *practice* (1983; 1984), which bears important analogies to Kuhn's *paradigm* and also to other metascientific units such as Shapere's *field* and Laudan's *research tradition*. According to Kitcher, a crucial part in scientists' practice are the patterns of reasoning, explanatory schemata, argumentative patterns or argumentative schemata, which he uses in his analyses of several biological theories, such as Darwin's natural selection and common descent theories (Kitcher 1989; 1993), theoretical or mathematical population genetics, i.e. Fisher's, Haldane's and Wright's genetic trajectories (Kitcher 1993), neo-Darwinian selectionism (Kitcher 1993), different theories from the classical period of genetics (Kitcher 1989; 1993) and molecular genetics (Kitcher 1993), besides in his account of the relationships between classical and molecular genetics (Kitcher 1984).

But most important is the new way of understanding the nature of theories that is now known as *semantic* or model-theoretic *view*, which resumes and continues to develop the work carried out in the first half of the twentieth century by Hermann Weyl (1927; 1928), John von Neumann (1932), and Garrett Birkhoff (Birkhoff and von Neumann 1936), and after the Second World War by Evert Willem Beth (1948a; 1948b; 1949; 1960) and Patrick Suppes (1957; 1962; 1969; 1970; 2002; McKinsey, Sugar and Suppes 1953). This new conception of scientific theories, which, with its different approaches, variants or versions, constitutes an authentic family, becomes established towards the end of the seventies and during the eighties as an alternative to the classical and historicist views.

Among the members of this family, we find the *set-theoretical* approach of Patrick Suppes, whose first version was developed/supplemented by his disciple Ernest W. Adams (1959), and by Suppes himself; the *structuralist* view of theories (also called *metatheoretical structuralism* or *Sneedian structuralism*) of Joseph D. Sneed and his first followers, Wolfgang Stegmüller and his disciples C. Ulises Moulines and Wolfgang Balzer (Sneed 1971; Stegmüller 1973; 1979; 1986; Balzer, Moulines and Sneed 1987; 2000; Balzer and Moulines 1996); the *partial structures* approach of Newton C. A. da Costa, Steven French, James Ladyman and Otávio Bueno (da Costa and

French 1990; 2003; Bueno 1997; French and Ladyman1999); the *state-space* approach of Bas van Fraassen (1970; 1972; 1974; 1976; 1980; 1987; 1989; 2008); the *phase-space* approach of Frederick Suppe (1967; 1972; 1989); the *model-based* proposal of Ronald N. Giere (1979; 1983; 1985; 1988; 1994); the approach proposed by Roberto Torretti (1990); and several "European" versions, such as that of Maria Luisa Dalla Chiara and Giuliano Toraldo di Francia (Dalla Chiara and Toraldo di Francia 1973) in Italy, Marian Przełecki (1969) and Ryszard Wójcicki (1974) in Poland, and Günther Ludwig (1970; 1978) and Erhard Scheibe (1997; 1999; 2001) in Germany.

All members of this family share the "formalistic spirit" of the received view though not the letter: the *classical virtue of conceptual clarity and precision* is a regulative principle for them; nevertheless, they consider that the best way of approaching this ideal is *to make use of all the logico-mathematical instruments which may contribute to* the attainment of this aim. But they adopting a pragmatic stance on this issue, e.g. the version of the semantic conception that will present later, Metatheoretical Structuralism, normally prefers the use of set theory, but other formal tools, such as first-order and higher-order logic, model theory, category theory, and topology, as well as informal ways of analysis, are also used.

On the other hand, some of them—particularly the structuralist view and the partial structure approach—are conscious of the numerous philosophically essential aspects of science which resist to be dealt with in a purely formal way, be it either because we do not have at our disposal the suitable tools for the task (at least not at the present time), or because we encounter elements which are *irreductively pragmatic and historically relative*, like the ones which have been mentioned in the historicist view.

The basic central idea, shared by the different "members", approaches, variants or versions of this family, is that concepts related to models are more useful for the philosophical analysis of scientific theories, of their nature and functioning, than those related to statements, i.e. that the nature, function and structure of theories are better understood when their characterization, analysis or metatheoretical reconstruction is centered on the models they determine, not on a particular set of axioms or linguistic resources by means of which they do so, even when the determination of the models is made by means of a series of principles or laws, which define a class of models (a class sometimes called "set", "collection", "population" or "family").

Within the multiplicity of kinds of scientific models, the semantic conception is centered on the so-called "theoretical models". As previously stated (Section 3.2), a (theoretical) *model*, in its minimal informal meaning, is a system or structure which intends to *represent*, in a more or less approximative way, a "portion of reality", made up by entities of various kinds, which *makes* true a series of claims, in the sense that in this system "what the claims say occurs", or more precisely, the claims are *true* in this system.

Since the notion of model is fundamentally a semantic notion (something is a model of a claim or sentence if the claim is *true* for it), and its most frequent analysis is made by model theory, this new approach which emphasizes the importance of models in the analysis of science is called a *semantic* or *model-theoretic conception*. In contrast, the received view of theories is called *syntactic* because it characterizes theories as sets of sentences or statements and it places general emphasis on the linguistic-syntactic aspects.

It is worth noting that the semantic option neither supposes nor intends to disregard statements (sentences or propositions) or, in general, certain resources or devices or even linguistic formulations. It does not mean that resources or devices of any kind, including linguistic ones, are superfluous for the meta-theoretical characterization of theories. Of course, we need some resource, device or language in order to determine or define a class of models. Nobody intends to deny this. Insofar as the models are determined in an explicit and precise manner in the meta-theoretical analysis, they are usually determined by giving a series of axioms, principles or laws, i.e. through statements. But even when the determination of the models is usually made through a series of axioms, the identity of the theory does not depend on specific resources or specific linguistic formulations. The different resources, devices or linguistic formulations are essential in the (trivial) sense of being the necessary means for the determination of the models, but in a really important sense, they are not, since nothing in the identity of a theory depends on whether the resource, device or linguistic formulation is one or another. It is a misrepresentation to say that, according to the semantic conception, a theory *is* a class of models, in the sense of being *identified with* a class of models or being *identical to* a class of models.[33] The semantic conception claims, rather, that a theory can be characterized in the first place for defining/determining the class, set, population, collection or family of its models, i.e. that a theory is *identified through* its models, *not with* them: to present/identify a theory means mostly presenting/identifying the characteristic models as a family, because it is an essential component of a theory, but not the only one.[34]

Considering now the most general aspects in the explication of the concept of theory, in relation to the semantic conception, the following could be stated. The (more) "theoretical" (or "formal") part of a theory would be constituted (at least) by the class (set, collection, population, family) of models—in general not determined by using first-order logic, but by means of other formal tools, such as higher-order logic, type theory, set theory (either elementary or naïve set theory or axiomatic set theory like Zermelo-Fraenkel or von Neumann-Bernays-Gödel axiom systems), structure species or category theory, or even by using semi-formal or informal tools, and identified (just to mention the most well-known versions of "the" semantic view) by *defining* or introducing *a set-theoretical predicate*, for Suppes, da Costa *et al*. and also normally for the Sneedian structuralism; by *characterizing state* or *phase spaces* governed by certain laws, for van Fraassen and Suppe; or directly *by postulates*, *laws*, *and equations* that appear on scientific texts, for Giere—and given some understanding of models—in the sense of *formal semantics* or *model theory*, for Suppes, da Costa *et al*. and usually for Sneedian structuralism; or as *trajectories* or *points* in *state* or *phase spaces*, for van Fraassen and Suppe; or as model in any *informal* acceptable *sense* of the term, for Giere.

The (more) "empirical" ("applicative" or "testing") part would be constituted by the "phenomena" conceived in one of the following ways: as *models of data* for Suppes, or *intended interpretations* or *intended models* in the modification introduced by Adams in the Suppesian approach; as *intended applications* formally represented as "partial potential models" in Sneedian structuralism; as *partial structures* in the *partial structures* approach; as *empirical substructures*

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[33] Admittedly, some "sloppy" presentations or assertions by members of the semantic family can lead to this misunderstanding of the general characterization of that family. Several authors make an understandable criticism of this identification of a theory *with* the class of its models (see, e.g., Portides 2017). In any case, such a critique would not apply to Metatheoretical Structuralism, which does not carry out such an identification.

[34] Although due to space limitations we cannot deal in depth with the problem, see footnote 22 for indications on the possible compatibility, pointed out as problematic (Thompson-Jones 2006), between the two notions of model as a *truth-making structure* and as a *mathematical model* (and even other notions of model) and an account of scientific representation.

that only contain observable entities in van Fraassen's approach; as *physical systems* that function as *nonproblematic 'hard-data'* for the theory in Suppe's approach; or as *real systems* in Giere's approach.

And bearing in mind that, with the exception of Giere's case, the other versions conceive the "theoretical" and the "empirical" parts as systems or structures of a certain type, the relation between the two would be of a sort of morphism, generally weaker than isomorphism, such as homomorphism, or of isomorphism but between the systems or structures representing the "phenomena" and a part of the systems or structures representing the models (be it a partial structure or substructure or even a partial substructure of them), a relation which is usually called "embedding".

As for the qualification of "at least" concerning that the (more) "theoretical" (or "formal") part of a theory would be constituted by the class of models is due to the following. Sneedian structuralism offers a more detailed analysis of the fine structure of the theories than all other versions of the semanticist family, allowing the identification of a greater number of components of such part of them. Therefore, unlike the other versions of the semantic family, it is not enough to have the class of models in order to have the whole (more) "theoretical" (or "formal") part of a theory.

The semantic view has had an impact in diverse areas of biology, and some of its versions have been applied to them. For example, Suppe (1974) tries to shed light on some philosophical problems related to speciation and taxonomy through the use of his own version of the semantic conception. In addition, attempts have also been made to apply in a systematic way van Fraassen's state-space approach to the analysis of the structure of the theory of evolution, and thus eventually of population genetics (Beatty 1980; 1981; Lloyd 1984; 1986; Thompson 1983; 1986). This, in turn, has motivated the position taken by authors such as Sloep and van der Steen (1987a; 1987b) and Ereshefsky (1991) and the response and/or further developments of Beatty (1987), Lloyd (1987; 1988) and Thompson (1987; 1989; 2007). This variant of semantic conception has also been applied to the analysis of theories of sex and gender (Crasnow 2001) and of ecology (Castle 2001).

Ronald Giere himself, in his introductory book to the philosophy of science (Giere 1979), provides an informal analysis of classical genetics, in the form of a definition of a kind of system—a Mendelian breeding system—and of some of the theoretical hypotheses employing this theory.

We also find the works of Magalhães and Krause (2000, 2006), which make use of the axiomatization *à la* Suppes in an attempt to identify the class of models of the theory of evolution by natural selection and of population genetics.

However, it is the Metatheoretical Structuralism that has produced the greatest number of analyses of particular theories belonging to the biological sciences[35]—even though this fact has

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[35] Just to mention a few, in the field of evolutionary biology, we can see the analyses of the structure of the theory of evolution by natural selection made by Ginnobili (2010; 2012; 2016; 2018), Ginnobili and Blanco (2019), Díez and Lorenzano (2013; 2015), as well as of the theory of common descent made by Blanco (2012) and of population dynamics by Díaz and Lorenzano (2017) and Lorenzano and Díaz (2020); in the field of inheritance and genetic theories (classical, molecular and population genetics), the works of Balzer and Dawe (1986a; 1986b; 1990), Balzer and Lorenzano (2000), Casanueva (1997; 2002; 2003), Casanueva and Méndez (2005), Dawe (1982), Lorenzano (1995; 2000; 2002; 2014) and Méndez (2006); cellular and tissue theories have been the object of structuralist analysis by Asúa and Klimovsky (1987; 1990), as has the theory of excitable

gone largely unnoticed by the international audience, perhaps because most of their works have been published in Spanish or German and not in the contemporary *lingua franca*, English.[36]

Finally, to conclude this overview, we can say that in classical genetics in particular, the analyses have made their historical journey in parallel with the philosophy of science. We have already mentioned authors who have analyzed it within the framework of the received view (such as Woodger, Kyburg, Lindenmayer and Simon, and Rizzotti and Zanardo); others who have done this using some version of the historicist conception (like Van Balen, Martins and Lorenzano) or other "post-classical view" (like Darden and Kitcher); and also those who have done it applying the semantic conception of theories (such as Giere, Dawe, Balzer, Lorenzano, Casanueva and Méndez).[37]

4.2. The Structuralist Concept(s) of Theory

The point of departure of the structuralist explication of the concept of a theory is the recognition that the term "scientific theory" is ambiguous, or better: polysemic, in its pre-systematic use. Sometimes it means just one law (like when one speaks indistinctly of the *law* of gravitation or of the *theory* of gravitation). This sense is not explicated by the structuralist concept of a theory, but by the structuralist concept of a law. Sometimes, the use of the term "scientific theory" corresponds to what is explicated by the structuralist notion of *theory-element*. In this sense, a theory-element is the smallest portion of science that seems to possess all the characteristics usually associated to theories. However, even this smallest sense of theory *cannot be identified with a clas*s (or set or population or collection or family) *of models, although it can be identified mainly through them. Despite the fact that* such a class is the most basic component for the identity of a theory, it is not the only one. A *theory-element*—i.e. the simplest kind of set-theoretical structure that can be identified with, or can be used as a rational reconstruction of, or can be regarded as a formal explication of, a theory (in an informal, intuitive sense)—can be identified, as a first approximation, with an ordered pair consisting of the "(formal) *core*", symbolized by **K**, and the theory's "domain of intended applications", symbolized by **I**: $\mathbf{T} = \langle \mathbf{K}, \mathbf{I} \rangle$.

The *core* **K** constitutes the formal identity of any empirical theory with a certain degree of complexity, which is composed by the ordered classes of *potential models*, *actual models*, *partial potential models*, *constraints* and *links,* i.e. $\mathbf{K} = \langle \mathbf{M_p}, \mathbf{M}, \mathbf{M_{pp}}, \mathbf{C}, \mathbf{L} \rangle$.

In the previous section we already introduced the classes of *potential models*, (*actual) models*, and *partial potential models.*

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membranes by Müller and Pilatus (1982), of neuroendocrinology by Bernabé (2019), and of cladistics by Roffé (2020). For further references, see Diederich, Ibarra and Mormann (1989; 1994), Abreu, Lorenzano and Moulines (2013) and Lorenzano (2023).

[36] For a concise presentation and evaluation of the reconstructions of theories in biology carried out using both the classical view and the different semantic variants, see, in German, Krohs (2004; 2005). For a discussion of theories in biology from other perspectives, but which we believe are to some extent compatible with Metatheoretical Structuralism and the analysis carried out here, see *Biological Theory* (2013), Vol. 7 (June 2013, issue 4).

[37] For an exposition and evaluation of the reconstructions of Woodger (1959), Lindenmayer and Simon (1980) and Balzer and Dawe (1990), see Lorenzano (1995). For a critical commentary on the reconstruction of Rizzotti & Zanardo (1986), see Balzer and Lorenzano (2000). And for one of Kitcher's analysis (1989; 1993), see Blanco, Ginnobili and Lorenzano (2019).

While the innertheoretical relationships between the different models of a theory are represented by the so-called *constraints* **C**, the intertheoretical relationships are represented by the so-called (*intertheoretical*) *links* **L**. They characterize the theory's "essential" relationships to other theories by connecting the **T**-non-theoretical terms with the theories they come from.

Any empirical theory is related to "reality" or "outside world", i.e. to some specific phenomena or empirical systems submitted to some specific conditions, to which it is intended to be applied and for which it has been devised. These empirical systems also belong to a theory's identity because otherwise we would not know what the theory is about, for the class of models contains "all" models, intended as well as non-intended. They constitute what is called the theory's *domain of intended applications* **I**. The domain of *intended applications* of a theory, even when it is a kind of entity strongly depending on pragmatic and historical factors that, by their very nature, are not formalizable, is conceptually determined through concepts already available, i.e. through **T**-non-theoretical concepts; thus, each intended application may be conceived as an empirical (i.e. **T**-non-theoretical) system represented by means of a structure of the type of the partial potential models $\mathbf{M_{pp}}$. All we can formally say about **I** is, thus, that it is a subset of the class of partial potential models $\mathbf{M_{pp}}$.

Theories are not statements, but are *used* to make statements or claims, which then have to be tested. The (empirical) statements (or claims) made by means of scientific theories are, intuitively speaking, of the following kind: that a given domain of intended applications may actually be (exactly or approximately) *subsumed* (or *embedded*) under the theory's principles (laws, constraints, and links), or to put it more precisely, under appropriate structures that satisfy theory's principles (laws, constraints, and links).[38] Normally, in any "really existing" theory, the "exact version" of the so-called *central empirical claim* of the theory—that the whole domain of intended applications may actually be (exactly) subsumed (or embedded) under (appropriate structures that satisfy) the theory's principles—will be strictly false. What usually happens is that either there is a subclass of intended applications for which the empirical claim is true, or that the central empirical claim is, strictly speaking, false but *approximately true*.[39]

Some "real-life" examples of scientific theories can actually be reconstructed as *one* theory-

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[38] It is worth noting that the subsumption or embedding relation, unlike that of isomorphism or similarity, is asymmetric.

[39] For a structuralist approach to features of approximation and a precise formal explication of the notion of the approximative empirical claim, see Balzer, Moulines and Sneed (1987, Chapter VII). On the other hand, Metatheoretical Structuralism acknowledges that, besides approximation, abstraction (also called "Aristotelian idealization") and idealization (also called "Galilean idealization") play an important role at different levels of scientific practice, and, of course, in model(s) construction, i.e. in the construction of potential models, (actual) models and partial potential models. At this point, and considering, among others, the historical and pragmatic aspects of intended applications, it should be clear that, if the problem of the scientific representation of phenomena by means of models were to be posed, for Metatheoretical Structuralism, this kind of representation could not be exclusively explicated by mapping of structures (as thought as constitutive of the semantic view by some authors; see, again, Portides (2017)). Moreover, if we accept in our analysis the standard notion of mathematical structure as set-theoretical structure, together with Etchemendy's representational perspective on model theory, on the one hand, and also accept the structuralist distinction between types of models, in addition to the pragmatic and intentional aspects of representation, we can refer, not only to the "representation of phenomena by means of models", but also to the "representation of theoretical systems by (theoretical) models" and to the "representation of 'empirical' systems by partial potential models". But, insofar as the subsumption or embedding relation is a relation between structures (between theoretical models and those

element, but usually single theories in the intuitive sense have to be conceived as aggregates of several (sometimes a great number of) theory-elements. These aggregates are called *theory-nets*. This reflects the fact that most scientific theories have laws of very different degrees of generality within the same conceptual setting. Usually there is a single fundamental law or guiding principle "on the top" of the hierarchy and a vast array of more special laws—which apply to specific situations—with different degrees of specialization.

Each special law determines a new theory-element. What holds together the whole array of laws in the hierarchy is, first, the common conceptual framework (represented in a model-theoretic way by the class of potential models), second, the common **T**-theoretical and **T**-non-theoretical distinction, and third, the fact that they are all specializations of the same fundamental law.

The theory-element containing the fundamental law(s)/guiding principle(s) is called the "*basic* theory-element" of the theory, i.e. of the theory-net. The other theory-elements of the theory-net are specializations or "*specialized* theory-elements".

When the highest degree of concretization or specificity has been reached, i.e. when all functional dependencies (concepts) are completely concretized or specified, "*terminal* special laws", which determine the most specific class of (theoretical) models, are obtained. The empirical claims associated to the corresponding "*terminal* specialized theory-elements" can be seen as particular, testable and, eventually, refutable hypotheses, which enables the application of the theory to particular empirical systems.[40] In the simplest model-theoretic way of representing these particular empirical claims, they state following: "*data model*" d of **T** can actually be (exactly or approximately) extended to, or subsumed or embedded in, the "*theoretical model*" m of **T**.

The resulting structure of a theory may be represented as a net, where the nodes are given by the different theory-elements, and the links represent different relations of specialization[41] (see Figure 4).

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used to characterize or represent "empirical" systems, namely, partial potential models), such a formal relation should be considered a necessary condition for representation of phenomena, but by no means also sufficient. (For an updated discussion of the problem of representation by models or model-representation, see Frigg and Nguyen (2017; 2020) and Frigg (2023)).Having said this, it should be pointed out that not every representation is considered to be a *scientific* representation and not every scientific representation is considered to be a *model-representation* (and the representational function is not the only epistemic value of (any type of) model (see Knuuttila 2005; 2011)).

[40] This is the model-theoretic, semantic, in particular structuralist version of what has been said in Section 2.2 about the testability and eventually refutability of particular hypotheses/terminal special laws. While in the classical approach of testing the particular hypotheses/terminal special laws are the entities to be tested, in the structuralist approach the "empirical claims" associated to *terminal* special laws are the entities that carry the weight of testing and to which it is able to direct "the arrow of *modus tollens*" (Lakatos 1970, p. 102).

[41] From a formal point of view, a *theory-net* **N** is a sup-semilattice $\langle \overline{\mathbf{T}}, \sigma$, with theory-elements as elements of the set $\overline{\mathbf{T}}$ and the specialization relation σ as the relation between the elements of the set $\overline{\mathbf{T}}$ dominated by a supremum $\mathbf{T}_0$ called *basic theory-element* of theory-net **N** (García de la Sienra 2019).

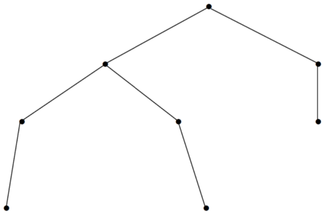

Figure 4

A theory-net **N** is the standard structuralist conception of a theory from a static or *synchronic* point of view. In this sense, a theory is *a complex*, *strongly hierarchical* and *multi-level* entity.

But a theory can also be conceived as a kind of entity that develops over time. A theory in the *diachronic* sense is not just a theory-net, which exists in the same form through history, but a *changing* theory-net, which grows and/or shrinks over time. Such an entity is called a *theory-evolution* **E**. It is basically a sequence of theory-nets satisfying two conditions: at the level of cores, it is required for every new theory-net in the sequence that all its theory-elements are specializations of some theory-elements of the previous theory-net; at the level of intended applications, it is required that the domains of the new theory-net have at least some partial overlapping with the domains of the previous theory-net.

Finally, it can be said that the structuralist view has been proposed to represent not just *intra*theoretical changes that occur in science (by means of the concept of a *theory-evolution*), but also different types of *intertheoretical changes*, such as *crystallization*, *embedding*, and *replacement with (partial) incommensurability*. It is worth noting that the process of crystallization of a theory would allow the treatment of the role of models (in the sense of laws or theory-elements) in the genesis of new empirical theories (in the sense of theory-nets and, later, of theory-evolutions) within the structuralist framework.[42]

4.3. The Theory-Net of Classical Genetics

According to our proposal, and as any other robust unified theory such as classical mechanics or thermodynamics, **CG** can also be better analyzed as a theory-net.

4.3.1. The basic theory-element of classical genetics

The "*basic* theory-element" of **CG** consists of its "(formal) *core*", symbolized by **K**(**CG**), and its "domain of intended applications", symbolized by **I**(**CG**).

The basic core of classical genetics

The basic *core* of classical genetics **K**(**CG**), which constitutes its formal identity, is composed by the ordered classes of *potential models*, *actual models*, *partial potential models*, *constraints* and *links*.

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[42] We believe that, on careful consideration, Metatheoretical Structuralism is unaffected by the worries or criticisms expressed by Love (2010; 2013) about the semantic view, including the criticisms he considers "damning" (like those stated by Halverson 2012; for a response to Halverson from the semantic approach, see van Frassen 2019).

In the previous section we already defined the classes of *potential models* $\mathbf{M_p(CG)}$, (*actual*) *models* $\mathbf{M(CG)}$, and *partial potential models* $\mathbf{M_{pp}(CG)}$.

In a truly complete reconstruction of **CG** we should include the constraints and the links this theory has to other (underlying) theories. However, due to space limitations, we won't discuss them or make them explicit. So, the *basic core of classical genetics* (**K**(**CG**)) will be characterized as follows:

Definition 4

$\mathbf{K(CG)} := \langle \mathbf{M_p(CG)}, \mathbf{M(CG)}, \mathbf{M_{pp}(CG)} \rangle$.

The intended applications and the basic theory-element of classical genetics

The *domain of intended applications* constitutes the class of those empirical systems to which one wishes to apply the fundamental law/guiding principle of the theory. They cannot be characterized by purely formal means. All we *can* say from a formal point of view is that an intended application is a partial potential model, which means that $\mathbf{I(CG)} \subseteq \mathbf{M_{pp}(CG)}$. Members of **I**(**CG**)—to which one wishes to apply the fundamental law/guiding principle of **CG**—are biological systems, characterized in **CG**-non-theoretical terms—i.e. systems represented by structures/data models of type y ($\langle I, (C_i)_{i\leq k}, APP, MAT, DIST\rangle$), where the transmission of several traits or characteristics (phenotype) of certain individuals, that make up a family, is followed from generation to generation, such as the case of peas, genus *Pisum*, investigated by Mendel, of fowls, investigated by Bateson and collaborators, and of the fruit-fly, *Drosophila melanogaster*, investigated by Morgan and disciples.

Now the *basic theory-element of classical genetics* (**T**(**CG**)) can be characterized as follows:

Definition 5

$\mathbf{T(CG)} := \langle \mathbf{K(CG)}, \mathbf{I(CG)} \rangle$.

The empirical claim of classical genetics

Classical genetics (**CG**) assumes that certain empirical systems such as those characterized above, characterized in **CG**-non-theoretical terms, satisfy the conditions imposed by **CG** in the following sense: those are the data of the experience that should be obtained, if reality behaves as **CG** says. This pretension is asserted by the empirical claim of classical genetics, which may be formulated in the following way:

(I) Any given intended system can be, when adding a set of **CG**-theoretical components $(F_i)_{i\leq s}$, $(DET_i)_{i\leq s}$, and $COMB$ to the **CG**-non-theoretical part of the corresponding theory-core ($\langle I, (C_i)_{i\leq k}, APP, MAT, DIST\rangle$), (exactly or approximately) extended to, or subsumed or embedded in, a **CG** (actual) model.

This claim may be trivial if the conditions imposed by the core on the **CG**-theoretical components are weak. But this should not be a reason for rejecting the core as trivial. This core serves as a basic core for *all* the intended applications of classical genetics. Interesting, non-trivial claims may be obtained by incorporating additional restrictions through the so-called "specializations".

4.3.2. Specializations of classical genetics

There are different possible ways of specializing classical genetics. As already advanced in Section 2.3, specializations consist of specifications of

(i) types and numbers of (pairs of) factors or genes ($(F_i)_{i\leq s}$—the genotype $\gamma \in G$—(either one or more),

(ii) the way in which they are distributed in the progeny ($COMB$) (as expected or theoretical probabilities, with combinations of factors or genes with the same probability or not), and of

(iii) the specific relationship $(DET_i)_{i\leq s}$, in which they are with the characteristics of the individuals—their phenotype $\pi \in P$—(with complete or incomplete dominance, codominance or epistasis).

The diverse possibilities of specialization can be partially or totally realized, in an isolated or joint way. One specialization of **CG** in which the three types of specification have been fully realized is denominated *terminal specialization*.These are what we have in specific **CG** applications. As in other robust unified theories, **CG** particular applications to particular empirical systems include specific versions/applications of this "law". Thus, we had a specific version/application of this "law", i.e. a special law, for each type of paradigmatic example presented before.

The Theory-Net of Classical Genetics (**N**(**CG**)) looks as follows—with the "*basic* theory-element" of **CG** at the top and in which are only depicted "*specialized* theory-elements" of **CG** corresponding to the examples given in Section 2.3:

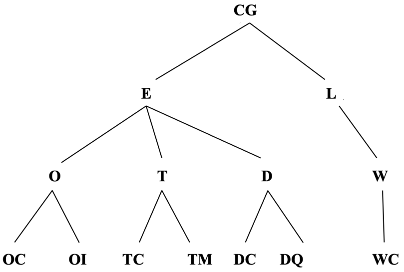

Figure 5

On a first level of specialization of **CG** theory-net we have either that all combinations of factors have equal probabilities (**E**) or that not all the combinations of factors are equally probable, i.e. that "linkage" takes place (**L**). On the other hand, we can further specialize **E**. Thus, on a second level of specialization of **CG** theory-net, it can be considered either that just one pair of factors are involved in the determination of the characteristics, and that there are four different possible combinations of factors (**O**), or that two pairs of factors are involved in the determination of the characteristics, and that there are sixteen different possible combinations of factors (**T**), or that three pairs of factors are involved in the determination of characteristics, and that there are sixty-four different possible combinations of factors (**D**). On a third level of specialization of **CG** theory-net we reach the level of terminal specializations. If **O** is further specialized, we can have either a case of complete dominance (**OC**) (see case 1 from Section 2.3 of the color of pea seed albumen), or a case of incomplete dominance (**OI**) (see case 2 from Section 2.3 of the color of four o'clock flowers). If **T** is further specialized, we can have either a case of complete dominance (**TC**) (see case 3 from Section 2.3 of the color of pea seed albumen together with the form of pea seeds), or a case of multifactorial inheritance (**TM**) (see case 6 from Section 2.1 of the form of fowls comb). If **D** is further specialized, we can have either a case of complete dominance (**DC**) (see case 4 from Section 2.3 of the color of pea seed albumen together with the form of pea seeds and the color of pea flowers), or a case of quantitative characteristics (**DQ**) (see case 5 from Section 2.3 of the length of corn ear). Finally, we also can specialize **L** further. Thus, on a second level of specialization of **CG** theory-net, it can be considered that just one pair of factors is involved in the determination of the characteristics, and that there are four different possible combinations of factors (**W**). As before, on the third level of specialization of **CG** theory-net we reach the level of terminal specializations. If **W** is further specialized, we can have either a case of complete dominance (**WC**) (see case 7 from Section 2.3 of the color of pea flowers together with the length of pea pollen grain).

5. Making Them Explicit: Laws and the Connection of Models to Theories. Discussion on the Basis of Previous Analyses

We would like to discuss now the issues of: a) the existence of laws in biological sciences, b) the place of models in theories of biology, and c) the unifying power of biological theories, in the light of the analyses carried out.

5.1. On Claim a) that there are "Laws" in Biological Sciences

It is worth mentioning that in the literature it has been recognized that there exist certain areas of science where fundamental laws/guiding principles—though maybe with another terminology, such as "basic principles" or "fundamental equations"—occur explicitly formulated in linguistic terms, and sometimes even in an axiomatic or quasi-axiomatic way. Newton's Second Law is an example of that, i.e., of a fundamental law/guiding principle explicitly formulated in linguistic terms, even in an axiomatic way since its first public occurrence, in the first edition of *Principia Mathematica Philosophia Naturalis* (Newton 1687)—although it was mistakenly ranked at the same level of the other two "Axioms, or Laws of Motion": the Law of Inertia and the Law of Action and Reaction.

On the other hand, in the literature of philosophy of science it has also been pointed out that there are other areas of science where fundamental laws/guiding principles do not occur explicitly and clearly formulated in linguistic terms. An example of one of these areas is evolutionary biology and the so-called "Principle of Natural Selection". In biology textbooks (beginning with Darwin's *Origin of Species*) we cannot find nor "observe" that principle formulated in all their generality, abstraction and schematization—although there is an agreement about the fact that a fundamental law/guiding principle "is there" and a lot of discussion about the right and convenient way of identifying and formulating it.[43]

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[43] For a discussion and a proposal on this issue from a structuralist point of view, see Ginnobili (2016) and Díez and Lorenzano (2015).

However, philosophers of science have also pointed out to other areas of science where nothing can be found or "observed" at all that possesses the criteria mentioned in Section 2.2 and that would, therefore, be considered a fundamental law/guiding principle in a plausible way. And precisely the field of Classical Genetics is an example of that. If we consider what is sometimes called a "law" in the area of Classical Genetics, namely, the so-called "Mendel's Laws", it is easy to recognize that neither Mendel's First Law (Law of Segregation) nor Mendel's Second Law (Law of Independent Assortment) or the sometimes mentioned Mendel's Third Law (Law of Dominance or Law of Uniformity) are schematic and general enough to connect all or almost all of the terms of Classical Genetics nor to be accepted by the respective community of geneticists as valid for all applications, with modal import, and as providing a conceptual framework adequate to formulate all the special laws of Classical Genetics. These laws therefore cannot be considered fundamental laws of Classical Genetics. That is to say no such law can be "observed" in the literature of genetics (Smart 1959: 1963; Kitcher 1984).

We grant that, sometimes, we cannot "observe" (explicit linguistic formulations of) general laws (or guiding principles) in the standard presentations of the respective theories, i.e. in the different texts (either journal articles, manuals or textbooks) written by scientists or science teachers.

Nevertheless, this article has argued for the existence of a fundamental law/guiding principle of Classical Genetics that even though not stated explicitly in biological literature, underlies implicitly the usual formulations of the theories, systematizing them, making sense of geneticists' practice, and unifying the different and heterogeneous models under just one theory.

In Section 2.3 a fundamental law/guiding principle in this area has been made *explicit*—against what can be called "narrow inductivism" or "restricted empiricism" in metascience.[44] And it is easy to realize that in the formulated fundamental law/guiding principle we can identify all criteria of fundamental laws/guiding principles indicated in Section 2.2. First, **CG** fundamental law/guiding principle can be seen as a *synoptic* law because it establishes a substantial connection between the most important terms of **CG** in a "big" formula. It contains all the important terms that occur in **CG**, both the **CG**-theoretical ones (the set of factors/genes, the function that assigns characteristics to pairs of factors or genes, and the function that represents the transition from

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[44] Nowadays, it can be considered a truism in philosophy of science that empirical science goes beyond "appearances", "phenomena", or "facts", in order to understand them better. Empirical science postulates, in addition, a realm of entities that are not directly empirically accessible, but they are accepted, *at least* inasmuch as the linguistic frameworks or theories in which they essentially occur are accepted as well (Carnap 1950). Thus, for instance, electric fields and wave functions are accepted, at least inasmuch as the theories of electromagnetism and quantum mechanics, respectively, are accepted. And scientists had good reasons to do so. Let's call that view on science "non-narrow inductivism" (inspired by Hempel 1966) or "non-restricted empiricism" (inspired by Carnap 1956). The analyses—or explications—of (metascientific) concepts, such as *law*, *model* or *theory*, can be considered as forming *interpretative eschemes* or *explanatory models*—in the sense of Hintikka (1968) within epistemic logic, and of Stegmüller (1979) and Moulines (1991; 2002) within philosophy of science—, of a philosophical nature, which propose or exhort us to "see the world" of science in a certain way. And a philosopher of science who uses one of these explanatory models overcomes narrow inductivism and restricted empiricism on a metascientific level in a similar way to what has been said and recommended for the case of the scientist. She/he interprets in a non-narrow inductive or non-restricted empiricist way what scientists do: not because we do not (directly) "see" their "fundamental laws/guiding principles", they are not "(in some sense) there" to be seen. As Goodman says, "We see what we did not see before, and see in a new way. We have *learned*" (Goodman 1978, p. 173).

parental factors/genes to factors/genes in the progeny) and the **CG**-non-theoretical ones, which are empirically more accessible (the set of individuals, the set of characteristics, the function that assigns their characteristics to individuals, the function of mating that assigns a progeny to parental individuals, and the function that represents the transition from parental characteristics to characteristics in the progeny). Second, **CG** fundamental law/guiding principle has been *implicitly accepted as valid in every intended application of the theory* by the respective community of scientists, i.e. by the community of geneticists who accept or use **CG**. In fact, accepting **CG** implies accepting **CG** fundamental law/guiding principle, while rejecting **CG** fundamental law/guiding principle implies rejecting **CG**. And of course, geneticists may not succeed in applying **CG** to particular empirical systems, and may decide to use another theory, with another fundamental law(s)/guiding principle(s). But to the extent that they work with **CG**, they accept as valid, even though just implicitly, **CG** fundamental law/guiding principle. Third, **CG** fundamental law/guiding principle is *highly schematic* and *general* and it possesses *very little empirical content*; such that is, when considered in isolation, irrefutable or "empirically non-restrict" (Moulines 1984) (i.e. it has a "*quasi-vacuous*" character). This is because to test what **CG** fundamental law/guiding-principle claims—namely, that the coefficients of the empirically determined distribution of characteristics and of the theoretically postulated distribution of factors in the progeny, given the also theoretically postulated relation between factors and characteristics, are equal—, without introducing any kind of further restrictions, amounts to a "pencil and paper" exercise that does not involve any empirical work. Nevertheless, fourth, as we would expect in the case of any fundamental law/guiding principle, despite being irrefutable, it *provides a conceptual framework* in which all special laws can be formulated; that is, special laws with an increasingly high degree of specificity and with an ever more limited domain of application, until we reach "terminal" specializations whose associated empirical claims can be seen as particular, testable and, eventually, refutable hypotheses, which enables the application of **CG** to particular empirical systems (its *systematizing* or *unifying* role). And fifth, **CG** fundamental law/guiding principle expresses a non-accidental regularity that is able to give support to counter-factual statements (if it is taken "together-with-their-specializations" within the corresponding theory-net), even when it is context-sensitive and with a domain of local application, and that, in its minimal sense, what is attributed is the *necessity of the models*, and, in that sense, it should be considered as *necessary in its area of application* (i.e. it possesses *modal import*). This means that, when the theory-net of **CG** contains an application with *s* as the relevant specialization of **CG** fundamental law/guiding principle and *i* as the empirical system/application, then, given the constrictions that the specialization *s* determines at the **CG**-non-theoretical level, a certain data model *should* be obtained for the empirical system *i* to which the theory-net of **CG** is intended to apply, i.e. the empirical system *i* to which the theory-net of **CG** is intended to apply *should* behave in a certain way—represented by the corresponding data model. Recalling that any specialization presupposes all that is "above" it in the corresponding branch of the theory-net of **CG**, notably the fundamental law/guiding principle, the counterfactual "if *s* were the case, then *i* (i.e. its corresponding data model), would be the case" is true according to the theory-net of **CG**.

Regarding the place of the so-called "Mendel's Laws" in the theory-net of **CG**, we can say the following. Mendel's Laws, inasmuch as they impose additional restrictions on the **CG** fundamental law/guiding principle, thereby adding information that is not already contained in its highly schematic formulation and restricting its area of application, can be obtained from the fundamental law through specialization and hence must be considered "special laws" of **CG** though not "terminal specializations". This is because Mendel's First and Second Lawsare a kind of "(pure) theoretical laws" (specific versions of the theoretical analogue of the transition of phenotypes), which only establishes what happens at the level of the (allelic) factors or genotypes, through function *COMB*, but doesn't say anything about how to connect such a level with that of "the experience", "the empirical", that is, with the characteristics or phenotypes. On the other hand, the sometimes-mentioned Mendel's Third Law must also be considered a "special law" of **CG** though not a "terminal specialization". Mendel's Third Law, understood either as the Law of Dominance or as the Law of Uniformity, consists in a specification of the function *DET* that establishes a relationship of (allelic) factors or genotypes with the characteristics or phenotypes.

5.2. On Claim b) that many of the Heterogeneous and Different "Models" of Biology Can be accommodated under Some "Theory"

Let us suppose that a theory (a theory-net in the structuralist sense) is not clearly visualized and, nevertheless, certain "laws" or "equations" or (theoretical) "models" are clearly identified, but they cannot be taken as fundamental laws/guiding principles or (theoretical) "models" *of a theory*; they are rather considered "autonomous" with respect to "theories" and do not cover the entire supposed domain of application of the corresponding realm.

This situation could arise under the following two circumstances:

The first occurs when these laws, equations or models are *indeed isolated laws* or *equations* or *models*. This circumstance can occur *in a synchronic* or *in a diachronic way*. Both situations are "entirely compatible" with Metatheoretical Structuralism.

In fact, the law of ideal gases and Ohm's law are mentioned in the structuralist literature (Balzer 1996) as examples of isolated laws. Even though they are not part of theory-nets, they are perfectly conceptualizable in structuralist terms, namely, as theories that can actually be reconstructed as *only one theory-element* (see Figure 6).

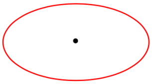

Figure 6

On the other hand, this "isolation" of the theory-elements, in the structuralist terminology (or "autonomy" of the "models", in the "model views" terminology), not only can be systematically and synchronously established, but also may or may not remain diachronically invariable.

If it were the last thing, it could be a case where "a law is in search of a fundamental law/guiding principle, of which it becomes a special law" or, in other words, where "a (theoretical) model is in search of its theory (i.e. its theory-net) to which it can be incorporated" (see Figure 7).

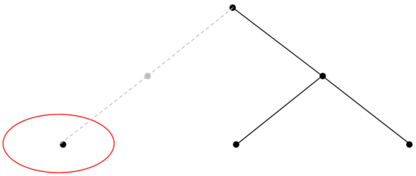

Figure 7

Or it could be a case where “a law, or a (theoretical) model, from which—together with many other things—a theory (i.e. a theory-net) is developed (and, finally, ends up consolidating or crystallizing)” (see Figure 8).

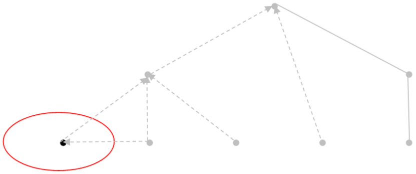

Figure 8

And although this can only be determined retrospectively, all these circumstances, whether it be an isolated law or model, which so remains, or an isolated (or incipient) law or model, which is later incorporated into a theory or from which a theory is developed and ends up crystallizing a theory (theory-net), would be susceptible of being represented by the structuralist metatheory, through their conceptualization as an isolated theory-element (the simplest and smallest notion of theory) or as its incorporation (or reduction, exact or approximate) to a theory-net (as specialization) or as part of a crystallization process, respectively.[45]

Up to this point we have referred to what could take place in general in relation to theoretical models. But, without going deep into the history of Classical Genetics, we could distinguish what happened in relation to the two of the types of models present in a theory according to Metatheoretical Structuralism—models of data and theoretical models. It might be said that the former reached a definitive stabilization, initiated with Mendel’s experiments, with respect to phenomena and experimental techniques, at the beginning of the 20th century.[46] But this stabilization and permanence contrasts with the continuous modification of the theoretical models—i.e. their theoretical extension or expansion, and conceptualization and theorization, trying to explain the former, to account for them, in the articulation of all their concepts in regularities of a certain type—in a process of crystallization, which leads to the emergence of the first paradigm/research program/theory in Genetics, Bateson’s Mendelism, and its subsequent replacement by Morgan’s Classical Genetics (Darden 1977; 1991; Lorenzano 2013).

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[45] For a systematic treatment of this less known metascientific concept, see Moulines (1997, 2011, 2014).

[46] This analysis of (part of the history of) Classical Genetics would be compatible with the so-called “experimental model systems” (Rheinberger 2007).

Another circumstance occurs when the *laws*, or (theoretical) *models*, despite their appearance of "autonomy" from "theories" and from any fundamental law/guiding principle, *are not*, in fact, *autonomous* in a sense that can be made precise as follows. Here we would be faced with cases of laws, or models, which would be special cases of fundamental laws/guiding principles that are not "observed" in their greater generality and schematism, but which, whether or not their existence is accepted in the usual way, "are there", being susceptible of becoming explicit. As it has been argued above, the really different and heterogeneous models (laws) of Classical Genetics can be accommodated under one theory, i.e. one theory-net. Despite the differences and heterogeneity of the models presented in Section 3.1 they can be placed in the theory-net of Classical Genetics.

5.3. On Claim c) about the Unifying Power of Biological Theories

As stated before, what all models of Classical Genetics share is that they appeal to the *same* theory, i.e. theory-net. Such models may differ substantially in their form. In fact, the laws obtained by specialization from the fundamental law/guiding principle do not preserve the logical or mathematical form.

The theory-net of Classical Genetics arises from the specification received of the concepts of (allelic) factors, of *determination* of phenotypes by genotypes and of *transition from parental genotypes to genotypes in the progeny*. In each specific case, specific pairs of (allelic) factors, and specific forms of the functions of *determination* of phenotypes by genotypes and of *transition from parental genotypes to genotypes in the progeny* should be searched for in order to account for the specific *distribution of phenotypes in the progeny*. The different ways in which Classical Genetics can be applied are established by the different special laws of the theory.

The interrelations between different theory-elements allow them to be seen as parts of "something unitary". In other words: the relation of specialization in a theory-net seems to be a guarantee of cohesion, and allows a better understanding of what the valuable unification of bona fide scientific theories consists of.

The unifying power of a theory depends not only on the number of successful applications/models but also (and more prominently) on how heterogeneous such applications/models are. Therefore, the evaluation of the unifying capacity of a theory must take into account the heterogeneity of cases in which it is applied, through the heterogeneity of the different specializations, of the different specifications that the concepts of the theory receive. Classical Genetics applies to a heterogeneity of cases—from the **CG**-"empirical"/non-theoretical level—thanks to the heterogeneous way in which the set of factors or genes $(F_i)_{i \leq k}$, the function $(DET_i)_{i \leq k}$, and the function *COMB*—from the **CG**-theoretical level—are specified. The reason why Classical Genetics is unifying is because it constitutes a collection of theory-elements that deal with different types of cases by subsuming or embedding them in some line of specialization of its theory-net, which is the "multidirectional development" of a common fundamental law/guiding principle.

6. Conclusion

In this article a unifying analysis of the concepts of law, model and theory has been first presented and then applied to Classical Genetics. In this area a fundamental law/guiding principle and special laws as well as its theory-net have been identified and made explicit. Finally, the consequences of the analysis were drawn in favor of the ideas that there are "laws" in biology (special and fundamental laws/guiding principles where a theory-net has been identified), that many of the heterogeneous and different (theoretical) models of biology can be accommodated under some "theory" (in case a theory-net has been identified), and that theory-nets in biology possess unifying power.

What an approach to the theme of the unifying power of science must achieve is not only to show how more cases of those already known are incorporated, but also the association in the same framework of different parcels of the world. This is, we insist, where the true unifying power of theories resides. With its notion of theory-net Metatheoretical Structuralism is the perspective that most clearly captures both the different successful applications/models of a theory and what they all have in common.

Just as the unifying capacity counts as an epistemic virtue when choosing between conflicting theories, the ability to explicate that merit may well count as a virtuous criterion for the choice of metascientific approaches to such theories at the same time. Something similar can be said about the very unifying power of the metatheoretical view. And Metatheoretical Structuralism has shown its unifying power with the unifying analysis (explication) of the (metascientific) concepts of law, model and theory presented here.

On the other hand, the proposal of Metatheoretical Structuralism in general, and the analysis presented here in particular, should be considered as compatible with and/or complementary to the analyses susceptible of being carried out by the "model views", the "model-based science" and to pluralistic accounts of models, as well as to the "practice-oriented" philosophies of science (especially those which do not deny or even emphasize the practice of theorizing). However, Metatheoretical Structuralism, and the analysis presented here, would not be compatible to any "atheoretical" account according to which "there are no theories at all" (in any precise sense of theory) or "there are only practices".

In any case, our analysis proposes or exhorts us to "see the world" of science in a certain way. We hope to have contributed with the present article to the plausibility of such a way of seeing the world of science and to encourage other philosophers of biology to do the same.

Acknowledgments

This paper is based on a previous one (Lorenzano and Díaz 2020), where very similar points are made, but instead of applying the explication of the metascientific concepts of law, model and theory to Classical Genetics, they are applied there to Population Dynamics. Research for this work has been supported by research projects PICT-2018-03454 (National Agency for the Promotion of Research, Technological Development and Innovation-Argentina), FFI2012-37354/CONSOLIDER INGENIO CSD2009-0056 (Spain) and FFI2013-41415-P (Spain).

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