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ÉVORA STUDIES IN THE PHILOSOPHY AND HISTORY OF SCIENCE

VOLUME TWO

FROM ONTOLOGY TO STRUCTURE

caleidoscópio

M. DE PAZ & J. PRÍNCIPE (EDS.)

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RUA DE ESTRASBURGO, 26, R/C DTO. 2605 -756 CASAL DE CAMBRA
TELEF. (+351) 21 981 79 60 FAX (+351) 21 981 79 55
caleidoscopio@caleidoscopio.pt
www.caleidoscopio.pt

The Genesis of Mathematical Knowledge: Cognition, History, Practices (P12-HUM-1216)

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From ontology to structure

Prefácio

O presente volume contém os resultados do sétimo Simpósio Internacional de Évora sobre Filosofia e História da Ciência e da Tecnologia, intitulado “Estruturalismo: Raízes, Pluralidade e debates contemporâneos” (2016). Esta série de simpósios começou em 2009 e terá a sua oitava edição em Janeiro de 2018. O tema estruturalista do sétimo simpósio tem conexões naturais com o tema do nosso segundo simpósio, “Os savants-philosophes” (2010), uma vez que considerações estruturalistas estavam presentes nas reflexões epistemológicas de savants-philosophes como Henri Poincaré ou Pierre Duhem. Ele também está relacionado com o sexto simpósio “A priori, experimentos de pensamento e atomismo” (2015), dado que a atomística foi um tema recorrente na filosofia da física durante a segunda metade do século XIX.

O presente trabalho reflecte uma colaboração ibérica, implicando a sua organização investigadores das Universidades de Évora e de Sevilha. Esta *joint-venture* é feita num espírito de cooperação intelectual, ideal que Hermínio Martins – o notável pensador da tecnociência e do Portugal contemporâneo e um dos fundadores destes Simpósios – nos deixou a todos nós. Tal como nas obras de Martins sobre filosofia da ciência, o nosso sétimo simpósio uniu estudiosos das ciências naturais e sociais e o primeiro ensaio deste volume, da autoria de Olivier Darrigol, mostra claramente como o estruturalismo, na sua pluralidade, é um tópico comum a diferentes domínios do conhecimento e da cultura.

As abordagens estruturalistas reavivam a guerra, característica do pensamento grego antigo, entre os partidários da matéria e os “amigos das idéias”, mas abandonando as questões metafísicas e ontológicas, ao favorecerem os aspectos metodológicos do conhecimento, ao se focarem “nas funções que constituem e constroem os mundos da cultura humana.[1]” É difícil dar uma definição de estrutura e de estruturalismo que abrace todas as variedades dessa tendência de análise de formas simbólicas humanas. Uma concepção muito geral de estrutura, proposta por Darrigol no primeiro ensaio deste volume, é aquela que vê uma estrutura como um sistema de relações genérico e abstracto; por abstracto entende-se que a natureza dos *relata* é indiferente e por genérico quer-se significar que a mesma estrutura é compartilhada por uma multiplicidade de objetos. Uma estrutura é um ‘sistema’ na medida em que ela é uma forma de organização amadurecida na qual uma totalidade se constitui (ou seja, um todo que é mais do que a soma de suas partes, tendo a interconexão entre as suas partes um carácter orgânico ou holístico). Na perspectiva dinâmica favorecida por Jean Piaget, uma estrutura consiste em operações, relacionadas entre si com base em “coordenações”; no mais alto nível de organização, uma estrutura é representada como um grupo de transforma-

[1] Ernst Cassirer (1945) ‘Structuralism in modern linguistics’, *Word*, p. 114.

ções sujeita a regras de invariância (a estrutura algébrica denominada grupo é um arquétipo matemático desse conceito). Uma estrutura é constituída por vários níveis, e o desenvolvimento do conhecimento, da análise de estruturas, vai revelando níveis sucessivos, "níveis de objetividade", cada vez mais abstractos e gerais[2]; A parte "superior" de uma estrutura tem freqüentemente um carácter tácito ou oculto, devendo assim a análise estruturalista ter uma eminente dimensão (meta-) crítica, aspecto este que é particularmente óbvio nas humanidades e ciências sociais ou na história do pensamento matemático.

O *tournant linguistique* kantiano (promovido por Wilhelm von Humboldt), que abarca uma pluralidade de visões de mundo, facilitou a aplicação do estruturalismo na linguística (Roman Jakobson, Nikolai Troubetzkoy), tendo o vínculo entre estrutura, indivíduo e evento se tornado fundamental, evitando-se assim o formalismo abstracto e valorizando-se os aspectos activo e produtivo das estruturas, bem como a dialéctica entre estrutura e história. Este *tournant* também promoveu a omissão do sujeito, como é o caso da análise de Michel Foucault sobre as formações discursivas, os "epistemai".

Na tradição anglo-saxónica de filosofia da ciência, particularmente em física, o recorrente debate metafísico sobre o realismo também tem alguns vínculos com o estruturalismo (por exemplo, nas obras de John Worrall, James Ladyman, etc.), surgindo novas formulações de realismo feitas em termos de estruturas (ôntico, epistêmico), nas quais se reivindica uma genealogia que remonta às concepções dos "savants-philosophes" (por exemplo, Poincaré). As abordagens estruturalistas também foram desenvolvidas para abordar várias outras questões, por exemplo, sobre as relações entre as teorias e a sua estrutura modular (Darrigol).

Na filosofia da matemática, as perspectivas formalistas e estruturalistas distanciam-se das intuicionistas e avaliam o aspecto técnico desta disciplina. Aquelas tornam-se fundamentais para a mudança na forma como a matemática foi concebida e feita cerca de 1900. Por um lado, as idéias e os métodos pioneiros de Richard Dedekind, Felix Klein e David Hilbert levaram ao surgimento da álgebra abstracta e ao uso de métodos axiomático-estruturais em topologia e em muitos outros ramos da disciplina (por exemplo, nas mãos de Bourbaki). Por outro lado, elas suscitaram a reflexão da parte de filósofos como Ernst Cassirer, e muito depois desencadearam discussões interessantes sobre a natureza da própria matemática.

O objectivo do sétimo simpósio foi o de reconhecer a pluralidade e a especificidade das metodologias estruturalistas, de estabelecer vínculos entre elas e de enquadrá-las através da sua história, considerando os campos das ciências exactas, naturais, sociais e humanas, ou mais amplamente na esfera da cultura. O simpósio teve duas secções principais: 1) estruturalismo nas ciências matemáticas e naturais e 2) estruturalismo nas ciências sociais e humanas. Os textos que agora publicamos correspondem a uma selecção dos trabalhos apresentados na primeira secção.

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[2] Karl-Norbert Ihmig (1999) 'Ernst Cassirer and the Structural Conception of Objects in Modern Science: The Importance of the "Erlanger Programm"', *Science in Context* 12 (4), 513-529.

No ensaio que abre este volume, Olivier Darrigol começa por fornecer uma visão histórica geral das abordagens estruturalistas que emergiram em áreas bastante diversas, desde a linguística até à matemática; na segunda parte de seu ensaio, ele considera as obras científicas e epistemológicas de quatro luminárias da física do século XIX e início do século XX - James Clerk Maxwell, Hermann Helmholtz, Henri Poincaré e Pierre Duhem, identificando as estruturas mais importantes presentes nessas obras e a sua função nas teorias físicas elaboradas por eles.

Os dois ensaios subsequentes apresentam duas propostas pós-Kuhnianas sobre a estrutura das teorias físicas, as quais dão realce às relações inter-teóricas e se afastam do modelo hempeliano nomológico-dedutivo. José Luis Falguera e Xavier de Donato-Rodríguez oferecem uma visão geral da abordagem denominada Estruturalismo Metateórico, para a qual uma teoria empírica é um objeto muito abstracto, dado por diferentes tipos de conjuntos de modelos e conjuntos de modelos de modelos. Esta sua apresentação usa a linguagem intuitiva, preterindo o formalismo seco, e leva em consideração o contexto histórico das discussões epistemológicas. O ensaio de João Príncipe diz respeito à recente proposta de Darrigol de uma estrutura modular das teorias físicas, a qual resulta de um diálogo interno entre o historiador e o filósofo. Uma teoria é construída, aplicada, comparada e comunicada pelo estabelecimento de relações funcionais de dependência parcial entre módulos e pela adaptação mútua entre módulos sem que eles se fundam entre si. Os módulos são eles próprios teorias com diferentes graus de generalidade, desde o mais abstracto até àquele mais elementar que fornece a conexão entre símbolos, esquemas e percepção.

Os dois últimos ensaios são sobre o estruturalismo em matemática. José Ferreirós parte da idéia básica de que a matemática é acima de tudo um trabalho conceptual. A sua perspectiva evita, assim, compromissos ontológicos que têm caracterizado algumas abordagens estruturalistas recentes em filosofia da matemática. Ferreirós segue a posição de Solomon Feferman, baseada na centralidade dos conceitos matemáticos, posição que é sintetizada numa lista de dez teses. A objetividade do conhecimento matemático não se funda aqui na noção de objeto mas sim na interacção entre diferentes estratos do conhecimento matemático. Finalmente, o ensaio de María de Paz explora uma maneira de relacionar a filosofia de geometria de Poincaré com sua filosofia de física, considerando o que ela define como a abordagem estruturalista-metodológica de Poincaré.

Esta publicação e o sétimo simpósio sobre o estruturalismo só se tornaram possíveis devido ao apoio de várias instituições. Agradecemos, em primeiro lugar ao IHC-CEHFCi e à Universidade de Évora pelo apoio institucional e financeiro. Também estamos em dívida com a Junta de Andalucía, que financiou o projeto de pesquisa "A Génese do Conhecimento Matemático: Cognição, História e Práticas" (P12-HUM-1216), possibilitando a cooperação entre a Universidade de Sevilha e a Universidade de Évora. Finalmente, gostaríamos de agradecer a todos os participantes pelas suas contribuições e pelas discussões estimulantes que ajudaram a criar um ambiente intelectual frutífero.

Foreword

The present volume contains the results of the seventh Évora International Symposium in Philosophy and History of Science and Technology, entitled “Structuralism: Roots, Plurality, and Contemporary debates” (2016). This series of symposia started in 2009 and will have its eighth edition in January 2018. The structuralist theme of the seventh symposium has natural connections with the theme of the second symposium, “The *savants-philosophes*” (2010), since structuralist considerations were present in the epistemological reflections of savants-philosophes like Henri Poincaré or Pierre Duhem. It is also related to the sixth symposium, “A priori, thought experiments, and atomism” (2015), atomistics being a recurrent theme in the philosophy of physics during the second half of the nineteenth century.

The present work reflects an Iberian collaboration, implying scholars from the universities of Évora and Sevilla. This joint-venture is inspired by a spirit of collegiality corresponding to the ideal that Hermínio Martins – remarkable and prolific thinker in such diverse subjects as technoscience and contemporary Portugal, and one of the founders of the symposia – has left to us all. In the spirit of Martins' works in philosophy of science, the seventh symposium united scholars from both the natural and social sciences, and the first text of this volume, authored by Olivier Darrigol, shows clearly how structuralism, in its plurality, is a common issue to different domains of knowledge and culture.

The structuralist approaches retake the war, characteristic of ancient Greek thought, between the partisans of matter and the 'friends of ideas', although they abandon the metaphysical and ontological issues, thus favouring the methodological aspects of knowledge, focusing on ‘those functions which constitute and build up the worlds of human culture.’[1] It is difficult to give a definition of structure and of structuralism that embraces all the varieties of this trend of analysis of human symbolic forms. A very general conception of structure, proposed by Darrigol, is the one that sees a structure as an abstract, generic system of relations; abstractness means that the nature of the *relata* is indifferent and genericity means that the same structure is shared by a multiplicity of objects. A structure is a “system” inasmuch as it is a mature form of organization that composes a totality (i.e. a whole that is more than the sum of its parts, the interconnection among its parts being organic-like or holistic). In the dynamical perspective favoured by Jean Piaget, a structure consists in operations related to each other on the basis of “coordinations”; at the highest level of organization, a structure is represented as a group of transformations subjected to invariance rules (the algebraic structure named group being a mathematical archetype of this concept). A structure has a multi-level constitution, and the development of structural knowledge and analysis shows in-

[1] Ernst Cassirer (1945) ‘Structuralism in modern linguistics’, *Word*, p. 114.

creasingly higher levels of abstraction, or 'levels of objectivity'[2]; the 'upper' part of a given structure having frequently a tacit or hidden character, structuralist analysis has an eminent critical – or meta – dimension, an aspect which is very obvious in the social sciences or in the history of mathematical thought.

The Kantian "linguistic turn" (promoted by Wilhelm von Humboldt), embracing a plurality of worldviews, facilitated the application of structuralism in linguistics (Roman Jakobson, Nikolai Troubetzkoy), where the link between structure, individual, and event became fundamental, avoiding abstract formalism and assessing the active and productive aspect of structures, as well as the dialectic between structure and history. This "turn" also promoted the omission of the subject, as it is the case in Michel Foucault's analysis of discursive formation through the 'epistemai'.

In the Anglo-Saxon tradition in philosophy of science, particularly in physics, the recurrent metaphysical debate about realism also has links with structuralism (e.g. for John Worrall and James Ladyman), leading to new formulations of realism in terms of structures (ontic, epistemic), claiming a genealogy which goes back to the conceptions of the 'savants-philosophes' (e.g. Poincaré). Structuralist approaches have also been developed in order to tackle issues regarding the relations among theories and their modular structure (e.g. for Darrigol).

In the philosophy of mathematics, formalist and structuralist perspectives distance themselves from intuitionistic ones and assess the technical aspect of this discipline. They have been central to the changes in the way mathematics was conceived and done from around 1900. On the one hand, the pioneering ideas and methods of Richard Dedekind, Felix Klein and David Hilbert led to the emergence of abstract algebra and the use of axiomatic-structural methods in topology and many other branches of the discipline (e.g. in the hands of Bourbaki). On the other hand, they inspired philosophers such as Ernst Cassirer, and much later they triggered interesting discussions about the nature of mathematics itself.

The aim of the seventh symposium was to recognize the plurality and the specificity of structuralist methodologies, to establish links among them, and to frame them through their history, in the fields of the exact, natural, social and human sciences, or more broadly in the sphere of culture. The symposium had two main sections – 1) structuralism in the mathematical and natural sciences and, 2) structuralism in the social sciences and humanities. The texts that we now publish correspond to a selection of the papers presented in the first section.

In the essay that opens this volume, Olivier Darrigol begins by providing a historical overview of the structuralist approaches that have emerged in quite diverse areas, ranging from linguistics to mathematics; in the second part of his essay, he considers the scientific and epistemological works of four luminaries of nineteenth-century and early twentieth-century physics – James Clerk Maxwell, Hermann Helmholtz, Henri Poincaré, and Pierre Duhem, identifying the most im-

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[2] Karl-Norbert Ihmig (1999) 'Ernst Cassirer and the Structural Conception of Objects in Modern Science: The Importance of the "Erlanger Programm"', *Science in Context* 12(4), 513-529.

portant structures present in these works, and their function in the physical theories elaborated by them.

The following two essays present two post-Kuhnian proposals for analyzing the structure of physical theories, far from the deductive nomological Hempelian model and with emphasis on inter-theoretical relations. José Luis Falguera and Xavier de Donato-Rodríguez offer an overview of the approach named Metatheoretical Structuralism, for which an empirical theory is a very abstract object given by different kinds of sets of models and sets of sets of models. Their presentation favors intuitive language over dry formalism and takes into account the historical context of the epistemological discussions. João Príncipe's essay concerns Darrigol's recent proposal of a modular structure of physical theories, as the result of an inner dialogue between the historian and the philosopher. Theories are built, applied, compared and communicated by establishing partial dependency relations between modules and by the mutual adaptation of modules without fusion. Modules are themselves theories with varying degrees of generality from the most abstract to the most elementary that provide the connection between symbols, schemes and perception.

The two last essays concern structuralist approaches to mathematics. José Ferreirós main aim is to respect the basic idea that mathematics is above all conceptual work, and thus he offers a perspective avoiding the ontological commitments that are characteristic of recent structuralist approaches in philosophy of mathematics. As a point of departure he takes Solomon Feferman's position, based on the centrality of mathematical concepts, as elaborated in a list of ten basic theses. Thus, he develops a particular view about the objectivity of mathematical knowledge not founded on the notion of object but on the interplay of the different strata of mathematical knowledge. Finally, María de Paz's essay explores a way of connecting Poincaré's philosophy of geometry with his philosophy of physics, by focusing on what she defines as Poincaré's structuralist-methodological approach.

We acknowledge the support of several institutions which made possible the seventh symposium on structuralism and also this publication. In the first place, we are very grateful to the IHC-CEHFCi, and to the Universidade de Évora for their institutional and financial support. We are also indebted to the Junta de Andalucía, which funded the research project "The Genesis of Mathematical Knowledge: Cognition, History and Practices" (P12-HUM-1216), and thereby enabled the cooperation between Universidad de Sevilla and Universidade de Évora. Finally, we would like to thank all the participants for their contributions and for the stimulating discussions that helped create a fruitful intellectual environment.

Contributors

Olivier Darrigol is a Research Director at CNRS in the SPHere research team, and also a Research Associate at UC-Berkeley. He is the author of several books concerning the histories of quantum physics, electrodynamics, hydrodynamics, and optics. His latest book (*Physics and necessity*, Oxford University Press, 2014) is a historical-critical analysis of attempts to prove the rational necessity of some physical theories.

José L. Falguera is an Associate Professor of Logic and Philosophy of Science at the University of Santiago de Compostela (Spain). His main research interests are in Philosophy of Science (epistemological, semantic and ontological problems). He has published *Lógica Clásica de Primer Orden* (with C. Martínez-Vidal; 1999) and a number of articles and book chapters in international venues.

Xavier de Donato Rodríguez is a Professor in the Faculty of Philosophy of the University of Santiago de Compostela. He previously was a researcher at the Institute of Philosophical Investigations of the UNAM (Mexico City) and a Professor at the Metropolitan Autonomous University of Mexico. He has been visiting scholar at Brown University (USA) and studied his PhD degree in Philosophy of Science with a fellowship at the University of Munich. He has published a number of articles and book chapters mainly on philosophy of science, epistemology, philosophy of mind and argumentation in international venues.

João Príncipe is a Professor at the Physics Department of the University of Évora and researcher at the IHC-CEHFCi. He is the author of two books about the philosopher António Sérgio and Portuguese cultural history. After his thesis on the French reception of statistical mechanics, he wrote several articles, in a perspective of historical epistemology, concerning Maxwell, Boussinesq, Poincaré and Duhem.

José Ferreirós is a full professor of Logic and Philosophy of Science at the Universidad de Sevilla, and a member of the Institute of Mathematics (IMUS) there. He has authored several books on the history of mathematics and the philosophy of mathematics, most recently *Mathematical Knowledge and the Interplay of Practices* (Princeton University Press). He was also founding member and the first president of APMP, the Association for the Philosophy of Mathematical Practice.

María de Paz is currently a post-doctoral Fellow at the Department of Logic and Philosophy of Science, at the Universidad de Sevilla (Spain). Her PhD dissertation resulted on the book *Henri Poincaré: Del Convencionalismo a la Gravitación* (London: College Publications). She has also co-edited (with Robert DiSalle) the volume *Poincaré, Philosopher of Science. Problems and Perspectives* (New York: Springer) and several papers on Poincaré and 19th-century history and philosophy of science.

The unnamed structuralism of four nineteenth-century philosopher-physicists

Olivier Darrigol

CNRS: UMR SPHere[1]

Structuralism is commonly believed to have emerged in the twentieth century, first in linguistics and in mathematics, then in anthropology, psychology, literary criticism, and other human sciences, with a surge in the 1960s. The word is also used to characterize a variety of the semantic approach to physical theory and a variety of realism in today's philosophy of physics. Although there are many varieties of structuralism, they all share a focus on *structure* qua self-contained, abstract, generic system of relations. Abstractness here means that the nature of the relata is indifferent; genericity means that the same structure is shared by a multiplicity of objects. This minimal definition of structuralism is adopted here, for it is well adapted to a study of interdisciplinary exchanges in a historical perspective. It implies a kind of cohesion and holism, because in a given structure the meaning of a term is entirely defined by its relations with other terms and because any term is related to any other term through a chain of relations (otherwise the structure would divide itself into several independent substructures). In some varieties of structuralism, "structure" may have additional connotations including rigidity, agency, dynamism, or analogy with organisms. The most pervasive structuralist qualifications nonetheless remain abstractness and genericity.

The word "structuralism" received the meaning just defined in the 1920s.[2] In earlier times the word "structure" rarely had its modern structuralist meaning. It usually referred to the way an object is constructed (concretely or metaphorically), with no intended abstraction of the structure from its object(s).This lexical observation raises two questions: How did the word "structure" acquire its structuralist meaning? Did structuralism exist before it was so named? The answer is not easily given because it involves the consideration of a number of different sciences and

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[1] E-mail: darrigol@paris7.jussieu.fr. The following abbreviations are used: *BB*, Akademie der Wissenschaften zu Berlin, mathematisch-physikalische Klasse, *Sitzungsberichte*; *MSP i*, *The scientific papers of James Clerk Maxwell*, ed. William Davidson Niven, 2 vols. (Cambridge, 1890), vol. *i*. All translations are mine, except those of Poincaré's texts, which are taken from Henri Poincaré, *The foundations of science* (New York, 1913); *HWA i*, Hermann Helmholtz, *Wissenschaflische Abhandungen*, 3 vols. (Leipzig, 1882, 1883, 1895), vol. *i*; *SHPMP*, *Studies in the history and philosophy of science*; *SHPS*, *Studies in history and philosophy of science*.

[2] The words "structuralist" and "structuralism" had earlier been used in psychology to characterize the approach of Wilhelm Wundt and his disciples, as opposed to "functionalism." However, Wundt's structures did not have the modern structuralist meaning. See, e.g., Jared Sparks Moore, *The foundations of psychology* (Princeton, 1921), 27-28.

their intricate relationships, and because the importance of abstract structures in a given discipline is not easy to assess objectively. To some extent, structure is in the eyes of the beholder: looking intently and carefully, one could find structure in any science since the scientific approach demands generality and since generality is about shared systems of relations. In order to avoid this difficulty, we must focus on overtly structuralist practices and statements. This still leaves us with a huge corpus of potentially relevant sources. Having considered only a few of them and being unfamiliar with most of the relevant fields, all I can offer is tentative, fragmentary answers.

The first part of this essay is an inquiry into the origins of the structuralist meaning of the word "structure." The answer necessarily involves the detection of structuralist practices in the fields considered. It turns out that in some fields, most evidently in mathematics, structuralist tendencies and approaches existed well before the name existed, although in others the reverse scenario prevailed. The second part of this essay deals with the special case of nineteenth-century physics. Although no physicist in this period employed the word "structure" as we would now do in similar circumstances, it is shown that four major figures of nineteenth-century physics and its philosophy, James Clerk Maxwell, Hermann Helmholtz, Henri Poincaré, and Pierre Duhem all defended varieties of structuralism. They did so more insistently than other philosopher-physicists of this period, and their reflections were deeply interconnected: Helmholtz and Poincaré drew much on Maxwell, and Duhem much on Helmholtz. The comparison of their approaches, as is argued in the conclusion, reveals different conceptions of the historical import of structures.

It is of course not enough to describe structuralism as a historical fact, in physics and elsewhere. We also want to understand its cognitive advantages. In the case of nineteenth century physics, we will see that structures were used as material or tools for theory construction, that they were meant to limit the surplus content of theories and bring them closer to experience, and that they permitted a variety of realism in Poincaré's case. In the conclusion, I will briefly indicate why these virtues do not contradict the self-contained, abstract character assumed in my definition of structures.

1. Structures defined

In today's sciences and in their philosophy, the word structure often refers to a system of relations between terms, wherein the nature of the terms is indifferent. As was just said, a structure in this sense has two essential characteristics: It exists abstractly and independently of its intended object (if there is any); and it can be shared by various objects, in which case the objects are said to be isomorphic. This definition of structure is mostly a twentieth-century novelty. In the nineteenth century, structure referred to the manner in which an object is constructed or organized, in accordance with the Latin root *struere*. This still is the usual dictionary definition. No separation of the structure from its object is hereby intended; no special attention is paid to the sharing of structures.

Received definitions

Dictionary definitions of the eighteenth and nineteenth century typically cited "the structure of a building" for the original use, "the structure of an organism" for a concrete analogical use, and the "structure of a discourse or of a sentence" for an abstract use. Although the abstract use has structuralist potentialities, these are not brought out. One late nineteenth-century French dictionary, conceived by the linguist Adolphe Hatzfeld, includes an abstract, holistic definition of structure as "l'arrangement des parties d'un tout." In 1926, André Lalande's influential *Vocabulaire* for philosophy similarly gives "disposition des parties qui forment un tout" as a first definition of structure; but he innovates in distinguishing two uses of the word in psychology: "combinaison des éléments que manifeste la vie mentale, considérée à un point de vue relativement statique" (sense A); and "par opposition à une simple combinaison d'éléments, un tout formé de phénomènes solidaires, tels que chacun dépend des autres et ne peut être ce qu'il est que dans et par sa relation avec eux" (sense B). Sense A plausibly refers to the older "structuralist psychology" of Wilhelm Wundt and his disciples; sense B explicitly refers to the more recent gestalt psychology ("Cette idée est le centre de ce qu'on appelle théorie des formes (*Gestalttheorie* et spécialement *Gestaltphychologie*)"), although its Viennese originator Christian von Ehrenfels and his Berlin followers had hardly used the word *Struktur*. It could easily pass for a structuralist definition and was indeed often cited by later structuralist thinkers. It is not clear, however, that Lalande meant the mutual relations between interdependent phenomena (*phénomènes solidaires*) to define them completely; possibly he meant only that these relations necessarily contributed to their definition.[3]

We will later see that the structuralist meaning of "structure" was already in the air when Lalande's *Vocabulaire* appeared. It was not so in the nineteenth century. Consider, for instance, how two prominent physicists used the word in the nineteenth century. Maxwell studied the mechanics of "framed structures," the "struc-

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[3] Adolphe Hatzfeld and Arsène Darmesteter, *Dictionnaire général de la langue française*, 2 vols. (Paris, 1890-1893); André Lalande, *Vocabulaire technique et critique de la philosophie*, 2 vols. (Paris, 1926), vol.2, supt., 1059. Lalande thanked the Swiss psychologist Édouard Claparède (one of Piaget's mentors) for the information. He translated *Gestalt* as "structure," in conformity with usage in early French and English texts on gestalt psychology. For instance, the definition of Webster's *New international dictionary of the English language* (Springfield, 1910) reads: "a structure or system of phenomena, whether physical, biological, and psychological, so integrated as to constitute a functional unit with properties not derivable from its parts; as, in music, a chord or a melody; also the pattern or figure assumed by such a system."

ture of [material] bodies," the "structure of the retina," the "internal structure of molecules," and the "molecular structure of liquids." Helmholtz most frequently used the word *Structur* in physiology, referring to the structure of organs; in other domains he rather used the German alternative *Gebilde* where English speakers would have used "structure." Stepping into the twentieth century, we encounter "structure" in Pierre Duhem's *La théorie physique, son objet et sa structure*, published in 1906. As will be explained in a moment, the word is there used, possibly for the first time, with the intention to promote a structuralist view of physical theory.[4]

What about "structure" in nineteenth-century mathematics? The word rarely occurred until when, late in the century, there started to be much talk about "the structure of a group," or the "relations of structure of a group," as defined by the "structure constants" in the case of Lie groups and by the list of products reducing to the identity in the case of finite groups. Élie Cartan's dissertation of 1894 had the title *Sur la structure des groupes de transformations finis et continus*, the structure of a group of transformations being defined as that which does not depend on the nature of the transformations and remains unchanged through *isomorphism.* Starting in 1899, Poincaré abundantly used this terminology in his own writings on Lie groups and groups of transformation.[5]

As for the word isomorphism, from the Greek for "same form," its main scientific use in the nineteenth century was for the chemical isomorphism Eilhard Mitscherlich discovered in 1819 and according to which chemically similar salts tend to crystallize in the same form. In the last third of the century, it began to be used for isomorphism between groups, defined as it still is today as a one-to-one correspondence for which the image of the product of two elements of the group is the product of the images. In his *Theory of groups* of 1897 William Burnside accompanied the definition with the remark that two isomorphic groups are truly the same group when "abstractly considered." But he did not use the word "structure" in this context. Cartan and Poincaré did.[6]

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[4] James Clerk Maxwell, *MSP* 1, 603; *MSP* 2, 275, 276, 549, 463, 549; Hermann Helmholtz, *Handbuch der physiologischen Optik* (Leipzig, 1867), on 19, 65, 192; *Wissenschaftliche Abhanlungen*, 3 vols. (Leipzig, 1882, 1883, 1895), vol. 2, pp. 32, 146, 273, 607; Pierre Duhem, *La théorie physique, son objet et sa structure* (Paris, 1906).

[5] Élie Cartan, *Sur la structure des groupes de transformations finis et continus* (Paris, 1894); Henri Poincaré, "Sur les groupes continus," *Transactions of the Cambridge Philosophical Society*, 18 (1899), 220-255; "Sur l'intégration algébrique des équations linéaires et les périodes des intégrales abéliennes," *Journal de mathématiques*, 9 (1903), 139-212.

[6] Eilhard Mitscherlich, "Über die Kristallisation der Salze in denen das Metall der Basis mit zwei Proportionen Sauerstoff verbunden ist," Akademie der Wissenschaften zu Berlin, *Abhandlungen* (1818-1819), 427-437; Camille Jordan, *Traité des substitutions et des équations algébriques* (Paris: Gauthier-Villars, 1870), 56; Felix Klein, *Vorlesungen über das Ikosaeder und die Auflösung der Gleichungen vom fünften Grade* (Leipzig, 1884), 7-8; William Burnside, *Theory of groups of finite order* (Cambridge, 1897), 22. More exactly, what is now called an isomorphism was called a "holoedric isomorphism" because the old isomorphisms were not necessarily bijective.

With this group-theoretical exception and a few others to be given soon, "structure" retained its ordinary meaning through the nineteenth century. How did the newer meaning of structure as an abstract system of relations come to pervade common and scientific parlance?

Natural history

In order to answer this question it is tempting to first consider the case of natural history since, as was mentioned, dictionaries have long included "the structure of an organism" or the "structure of an organ" as sample uses of the word "structure." Although this employment of the word does not necessarily imply the structuralist abstractness and genericity, the idea of the same (sub-)structure being shared by different organisms or by different organs naturally occurs in comparative anatomy, which is as old as Greek philosophy. Genericity came to the fore when a few botanists and anatomists of the eighteenth century emphasized the uniformity of design of living organisms. For instance, in the fourth volume of his *Histoire naturelle* (1753) Georges Louis Leclerc, Comte de Buffon wrote:

> The reader will decide whether this hidden resemblance is not more marvelous than the apparent differences, whether this constant conformity of design from man to quadrupeds, from quadruped to cetaceans, from cetaceans to birds, from birds to reptiles, from reptiles to fish, etc., in which the essential parts such as the heart, the intestines, the spine, the senses, etc. are always found, does not indicate that in creating animals the supreme Being wanted to employ one idea only and vary it in all possible manners at the same time, so that man might admire both the magnificence of the execution and the simplicity of the design.

Similarly, in his *Traité d'anatomie* (1786), Félix Vicq d'Azyr pondered:

> Is not this [sharing of more or less hidden clavicular bones by all quadrupeds] clear evidence of the ways of Nature, which constantly seems to operate according to a primitive and general model from which she departs but with regret and of which traces can everywhere be found?
>
> Nature thus seems to follow a type or general model, not only in the structure of the diverse animals but also . . . in the structure of their different organs; and we do not know what is more worth our admiration: the abundant variations of forms, or the constancy and the kind of uniformity that a keen eye discovers in the immense extent of her productions.

Although Vicq d'Azyr here uses the word "structure," it is the word "type" that conveys the structuralist idea of genericity.[7]

[7] Georges Louis Leclerc, Comte de Buffon, *Histoire naturelle, générale et particuliére, avec la description du cabinet du Roi*, vol. 4 (Paris, 1753), "L'asne," 377-436, on 381; Félix Vicq d'Azyr, *Traité d'anatomie et de pathologie, avec des planches colorées représentant au naturel les divers organes de l'homme et des animaux*, 2 vols. (Paris, 1786), vol. 1, pp. 9, 12.

At the turn of the eighteenth and nineteenth centuries, Wolfgang Goethe based his "morphology" of plants and animals on the comparison of their forms (*Gestalt*). Goethean form was a holistic, ill-defined concept, implying irreducibility to mechanical elements, and eluding Linnean principles of classification. It purported to be the proper basis of a scientific approach to the biological phenomena that Newtonian mechanical reduction would never capture. Goethe thereby shared Buffon's and Vicq d'Azyr's belief in the structural unity of nature, a precondition for scientific studies. For a given group of animals, vertebrates for instance, he assumed the existence of a general "type" of which the individual species only were continuous variations. The aim of his comparative anatomy was to identify this type, which could then be used dynamically: a given species could continuously evolve within a type, by adjustment of the relative size of the different organs or bones under environmental pressure. Goethe also conceived, in his "Metamorphosis of plants," that the various parts of plants evolved into each other in a form-preserving manner. He used the word *Structur* in its ordinary sense, and his basic concepts of form, type, and metamorphosis were too vague and too fleeting for him to be associated with a well-defined variety of structuralism. In particular, he did not clearly express the idea that form, type or structure were defined by the mutual relations of parts. His, Buffon's, and Vicq d'Azyr's emphasis on comparison and their faith in the existence of biological archetypes nonetheless had a structuralist flavor.[8]

Charles Darwin abundantly used the word structure in his *On the origins of species* (1859), in the usual sense of the build-up of an organism or an organ. In his theory, the structure (and habits) of animals and plants evolve through the combined effect of structure-changing mutations and selection of the structures best fitted to the environment. Structure is never quite the same even between two individuals in the same species, and the partial sharing of structure between the individuals of different species reflects common ancestry. Darwin called this shared portion of structure "generic characters." Unlike Goethe's purely idealist types, these characters received a historico-empirical justification through the evolution process. Although, they have the genericity required for a structuralist notion, Darwin did not truly consider them as abstract systems of relations. Moreover, what he called structure was specific to a given individual, and what could be generic was only some component of a structure.[9]

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[8] Wolfgang Goethe, *Sämtliche Werke*, 40 vols. (Stuttgart, 1902-1907), vol. 39: *Schriften zu Naturwissenschaften*. Cf. George Wells, *Goethe and the development of science 1750-1900* (Alphen aan den Rijn, 1978); Stéphane Schmitt, "Type et métamorphose dans la morphologie de Goethe, entre classicisme et romantisme," *Revue d'histoire des sciences*, 54 (2001), 495-521; Georgy Levit, Petra Reinhold, Uwe Hoßfeld, "Goethe's 'comparirte Anatomie': Die entscheidende Grundlage für die Begründung der Jenaer theoretischen Morphologie, Medizin und Veterinärmedizin," *Deutsches Tierärzteblatt*, 63, (2015), 1729-1733.

[9] Charles Darwin, *On the origin of species by means of natural selection, or the preservation of favoured races in the struggle for life* (London, 1859).

Sociology

By definition, sociology deals with large groups of people and their interrelations, irrespective of the identity of these people. It therefore deals with structures in a fairly abstract sense. Talk about "social structures" or the "structure of society" is pervasive in early sociology as well as in socialism and Marxism, in analogy with the structure of living organisms. For the English polymath Herbert Spencer, the model to follow was Darwin's evolution theory, in which the adaptation of structures to the environment played a central role. Toward the end of the century, the Belgian sociologist Guillaume de Greef and the French sociologist Émile Durkheim adopted Spencer's analogy, with a twist: whereas for Spencer the social organism was reducible to an aggregate of human components in a given physical environment, for de Greef and Durkheim this organism had a specific structure of interrelated "social facts." While this structure still responded to biologically and environmentally defined functions, it also had laws of its own, to be traced to social contracts for de Greef, and to be investigated by empirical methods for Durkheim. These two authors combined the biological metaphor of structure with Auguste Comte's invention of "sociology" as a genuine science with its own object and methods. They were pioneers of what is now called structural functionalism. In their theories, social structures acquired a structuralist meaning and the meaning of the word "structure" implicitly took a modern turn.[10]

Mathematics and philosophy

Although late nineteenth-century sociology used the word structure with a structuralist meaning, it did not formally redefine structure to suit its structuralist purposes. Such formal redefinition first occurred in mathematics. We already saw that the structuralist use of "structure" entered mathematics in the later nineteenth century, in the limited context of group theory. This does not mean that structuralism did not exist earlier in mathematics. On the contrary, the idea of systems of relations existing independently of their concrete or intuitive object is as old as

[10] Herbert Spencer, *The principles of sociology*, vol. 1 (London, 1875); Guilllaume de Greef, *Introduction à la sociologie*, 2 vols. (Paris, 1886-1889); *Sociologie générale élémentaire* (Bruxelles, 1895), leçon 19: "Structure générale des sociétés"; *La structure générale des sociétés*, 2 vols. (Paris, 1907-1908); Émile Durkheim, "La science sociale selon de Greef," *Revue philosophique*, 22 (1886), 658-663; *De la division du travail social: étude sur l'organisation des sociétés supérieures* (Paris, 1893); *Les règles de la méthode sociologique* (Paris, 1895). Unlike de Greef, Durkheim used the word "structure" sparingly; he preferred "organism" and "organs." Cf. Ferdinand Tonnies, "The present problems of social structure," *The American journal of sociology*, 10 (1905), 569-588; Peter Corning, "Durkheim and Spencer," *The British journal of sociology*, 33 (1982), 359-382; Jonathan Turner, "Durkheim's and Spencer's principles of social organization: A theoretical note," *Sociological perspectives*, 27 (1984), 21-32.

mathematics itself. This idea is intimately bound to analogy, which can be defined as the sharing of systems of relations; and it is the main source of generality in science as well as the nerve of much mathematical reasoning. The Polish mathematician Stefan Banach is reported to have said: "Good mathematicians notice the analogies between theories and between methods of proof. The very great ones see the analogies between analogies." This is why structuralist ideals frequently occurred in the history of mathematics, for instance in Euclid's *Elements*, in Gottfried Wilhelm Leibniz's philosophy of mathematics, in George Boole's algebra, in Hermann and Robert Grassmann's theory of quantity, in projective geometry, in Felix Klein's Erlangen program, in Georg Cantor's set theory, in the late nineteenth-century arithmetization of geometry, in the concept of interpretation of a system of axioms by a model, or in David Hilbert's axiomatic program.[11]

An extreme form of structuralism emerged in the early1910s with the publication of the *Principia mathematica* by Bertrand Russell and Alfred North Whitehead, an ambitious attempt to reduce all mathematics to a symbolic logic. The second volume, published in 1912, contained the definition of *relation-numbers* as classes of equivalence of isomorphic relations (just as an ordinary number is a class of equivalence of equipotent sets). This may be regarded as a generalization of group structure, which was defined as the class of groups isomorphic to the same group. In his *Mathematical philosophy* of 1819, Russell renamed the relation-numbers as "structure" (using quotation marks for the technical sense):

> We may say, of two similar relations, that they have the same "structure." For mathematical purposes (though not for those of pure philosophy) the only thing of importance about a relation is the cases in which it holds, not its intrinsic nature.
>
> What we define as the "relation number" is the very same thing as is obscurely intended by the word "structure"--a word which, important as it is, is never (so far as we know) defined in precise terms by those who use it.

Russell went on with a broader philosophical discussion, starting with the remark:

> There has been a great deal of speculation in traditional philosophy which might have been avoided if the importance of structure, and the difficulty of getting behind it, had been realised.

In his opinion, philosophers had in vain assumed a distinction between phenomenal world and noumenal world, because we have access only to the common structure of the two worlds:[12]

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[11] Stefan Banach, cited in Stanislas Ulam, "Marian Smoluchowski and the theory of probabilities in physics," *American journal of physics*, 25 (1957), 475- 481, on 477. On the prehistory of structures in mathematics, cf. Nicolas Bourbaki, *Éléments d'histoire des mathématiques* (Paris, 1960), 29-39.

[12] Bertrand Russell and Alfred North Whitehead, *Principia mathematica*, 3 vols. (Cambridge, 1910, 1912, 1913), vol. 2, 303-346; Russell, *Introduction to mathematical philosophy* (New York, 1919), 59-61. Cf. Paolo Mancosu, Richard Zach, and Calixto Badesa, "The development of mathematical logic from Russell to Tarski, 1900–1935," in Leila Haaparanta (ed.), *The History of Modern Logic* (Oxford, 2009), 318-471, on 421.

> In short, every proposition having a communicable significance must be true of both worlds or of neither: the only difference must lie in just that essence of individuality which always eludes words and baffles description, but which, for that very reason, is irrelevant to science.

As Russell may have known, there was a growing structuralist tendency in contemporary philosophy. In his *Substanzbegriff und Funktionsbegriff* of 1910, the neo-Kantian philosopher Ernst Cassirer argued that in mathematics and in physics substances had gradually been replaced by functions or systems of relations. For instance, Cassirer regarded Richard Dedekind's new arithmetic as an attempt to identify the "logical structure of the pure theory of numbers" or as "the construction of a new 'object' which in its structure is devoid of any arbitrariness." He generally saw the new mathematics as the study of "the structures of classes of relations," in agreement with what he had read in Russell's *Principles of mathematics* (1903). He rejected the empiricist view according to which concepts are generated by abstracting common properties from a class of (similar) objects, and instead recommended the formal strategy of "investigating, in their specific relational structure [*Relations-Struktur*] the connections and relations [*Zusammenhänge und Beziehungen*] on which the systematic composition [*Verknüpfung*] [of the given] rests."[13]

As can be seen from these citations, Cassirer abundantly used the world structure to refer to abstract systems of relations. It did not occur in Russell's *Principles* of 1903, although, as we just saw, Russell gave it its first formal definition in his *Mathematical philosophy* of 1919. There is no mention of Cassirer in the latter book; Russell may have just generalized the meaning of structure already found in group theory.

Among the early readers of Russell's *Mathematical philosophy* was the astronomer Arthur Stanley Eddington, whom Paul Dirac once called "the fountainhead of relativity in England." In his *Space, time and gravitation* of 1920 – a wonderfully deep and yet non-technical exposition of general relativity – Eddington abundantly used the world "structure" as an abstract, mathematical system of relations. As the source of this usage, he cited Russell and his aforementioned exploitation of "structure" to define the true object of science. In Eddington's eyes, relativity theory, when properly understood and developed, was all about structure:

> The relativity theory of physics reduces everything to relations; that is to say, it is structure, not material, which counts. The structure cannot be built up without material; but the nature of the material is of no importance.

> In regard to the nature of things, this knowledge [provided by the theory of relativity] is only an empty shell, |a form of symbols. It is knowledge of structural form, and not knowledge of content.

[13] Ernst Cassirer, *Substanzbegriff und Funktionsbegriff: Untersuchungen über die Grundfragen der Erkenntniskritik* (Berlin, 1910), 37, 52, 48, 256; Russell, *Principles of mathematics* (Cambridge, 1903).

In Eddington's theory of the early1920s and in the associated philosophy, the basic relational structure was the differential manifold of spacetime, the attached tensors, and an affine connection. This structure expressed the necessity to map phenomena through arbitrary coordinates with no pre-established concrete meaning, and the necessity to compare (through the connection) the local (affine) structures at different points of the manifold. Then it was the mind, in its predilection for permanence, that selected, among the tensors that could be derived from the connection, those able to represent metric properties and energetic properties. There were no pre-given rulers to define the metric, and no pre-given substance to define the energy. Everything boiled down to systems of relations properly filtered out by the mind:[14]

> Our whole theory has really been a discussion of the most general way in which permanent substance can be built up out of relations; and it is the mind which, by insisting on regarding only the things that are permanent, has actually imposed these laws on an indifferent world. Nature has had very little to do with the matter; she had to provide a basis –point-events; but practically anything would do for that purpose if the relations were of a reasonable degree of complexity.

Among physicists, Eddington pioneered the structuralist use of "structure," even defining the "world-structure" as the basic object of physics. In a moment we will see that he was not the first structuralist in physics, nor the first to derive the basic structure(s) of the world from a priori principles of intelligibility. He was peculiar, however, in his belief in the complete necessity of these principles. Whereas the structuralism of post-Kantian philosopher-scientists and neo-Kantian philosophers went along with relativized and empiricized versions of the constitutive a priori, Eddington's served as a basis for a strictly rationalist foundation of physics or at least (in 1920) of the non-quantum part of it.

In 1921, the Swiss mathematician and Esperantist René de Saussure (a brother of the linguist) published a volume entitled *La structure de la réalité*. His philosopher colleague Charles Werner summarized his views as follows:

> By "structure" of reality, M. de Saussure means what is left of things when they have been stripped of their proper qualities and of their activity, the rigid frame that supports all the rest, which could be called the skeleton of the real. And he defends the thesis that the structure of reality is of geometric, rather, meta-geometric nature. It is therefore by means of geometric schemes that he represents the structure of the principal elements of reality, from time and space to the intellect and soul, giving in the end the complete schemes of

[14] Arthur Stanley Eddington, *Space, time and gravitation: An outline of the general theory of relativity* (Cambridge, 1920), 195, 197, 201; Paul Dirac, "Recollections from an exciting era," in Charles Weiner (ed.), *History of twentieth century physics* (New York, 1977), 109-146, on 115. Cf. Thomas Ryckman, *The reign of relativity: Philosophy of physics 1915-1925* (Oxford, 2005), Chap. 7. Eddington saw himself as completing the structuralist move he detected in Hermann Weyl's *Nahegeometrie* (in which a connection was needed to compare lengths at different locations). However, Weyl rarely used the word "structure" and did not care to define it or to comment on it. In his *Raum·Zeit·Materie* (Berlin, 1918), one finds "mathematische Struktur" (p. 17), "metrische Struktur" (p. 120), and a few more standard uses of *Struktur*, for instance for the atomistic structure of matter.

the structures of mind and matter. One important conclusion of his is that God, as a pure essence, has no structure and therefore does not belong to the world even though he is in contact with the world.

As I have not seen the book, I cannot decide whether the author's "structures" derived from his interest in the grammatical structure of a universal language, from Russell's logical structures, or from Eddington's geometric world-structure. In any case, his sweeping structuralism is not likely to have had much influence, considering the rarity of his book. I mention it only as witness of the rise of the structural usage of "structure" in the 1920s.[15]

In 1928 the German-born philosopher Rudolf Carnap, then a member of the Vienna circle, published *Der Logische Aufbau der Welt*, in which he made Russell's "structure" the cornerstone of a logicist foundation of all science. He introduced the word and the concept as follows:

A special kind of relational description will be called *structural description* [*Strukturbeschreibung*]. The latter leave unnamed not only the properties of the individual elements of the domain but also the relations that exist between these elements. In a structural description, only the "*structure*" of the relations is given, that is, the collection of all their formal properties.

A little further we read:

[The structural description] is *the highest degree of formalization and de-materialization* . . . Our thesis that scientific propositions concern only structural properties would thus mean that *scientific propositions deal with mere forms, without saying what the elements and the relations of these forms are.*

How could such abstract logicism extend to the science of concrete objects like persons or villages? Carnap replied:

Here is the essential point: *The science of the real* [die Realwissenschaft] *must admittedly be able to distinguish between* [persons and villages]; it does this mainly through labeling [*kennzeichnung*] by means of other constructs [*Gebilde*], *but in the end the labeling is done through mere structural description*.

How exactly Carnap meant to achieve a fully structural description and whether he succeeded in this task need not be considered here.[16]

At any rate, "structure" entered the manifesto of the Vienna circle, a short text written by Hans Hahn, Otto Neurath, and Carnap in preparation to the September 1929 meeting of the Verein Ernst Mach in Prague:

A scientific description can contain only the *structure* (order form [*Ordnungsform*]) of objects: not their 'essence'. What unites men in language are structural formulae [*Strukturformeln*]; in them the content of the common knowledge of men presents itself.

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[15] René de Saussure, *La structure de la réalité* (Neuchâtel et Genève, 1921); Charles Werner [report of the 16th meeting of the *Philosophes de la Suisse romande* on 12 June, 1921], in *Archives de psychologie*, 18 (1921), 175-176, on 175. Two years earlier R. de Saussure had published *La structure logique des mots dans les langues naturelles, considérée au point de vue de son application au langues artificielles* (Bern, 1919).

[16] Rudolf Carnap, *Der Logische Aufbau der Welt* (Hamburg, 1928), 13, 15 (his emphasis).

> Subjectively experienced qualities - redness, pleasure - are as such only experiences, not knowledge; physical optics admits only what is in principle understandable by a blind man too.

This statement followed a characterization of the circle's approach as empiricist, positivist, logicist (based on *Logistik*), universal (covering all sciences), and unitary. It led to a general "theory of constitution" (*Konstitutionstheorie*) in which structure was the central concept. The most cited influences were Ernst Mach and Ludwig Wittgenstein for the anti-metaphysical crusade; Russell and Whitehead for the new logicism; Mach, Helmholtz, Poincaré, and Duhem for structural, relation-based tendencies in mathematics and physics. With Russell's *Mathematical philosophy*, the manifesto probably was the most influential source for the new, structuralist meaning of the word "structure."[17]

Linguistics

As was mentioned, the dictionary definitions of "structure" have long included its metaphorical use in expressions such as "the structure of a sentence." The implied structure is grammatical. Grammar being concerned with the rules of construction of the words in a sentence independently of their meaning, it has the abstractness required in structuralism. It may also have the required genericity, when it comes to the ideal of a universal grammar or to comparative grammar. Universal grammars have long been dreamt of, from the *Grammaire générale de Port-Royal* (1660) to Noam Chomsky's generative grammar. We have already encountered Robert Grassmann, who believed the "philosophical grammar" of a *Formenlehre* could emerge from a comparative study of languages; and René de Saussure, who tried to identify the universal grammatical "structures" of natural languages in order to justify and improve artificial languages such as Esperanto. Generic grammatical structures also concerned the Scottish philosopher Adam Smith in his theory of language formation (1767), and the poet-philosopher Friedrich Schlegel in his comparison of Sanskrit with other languages (1808). Schlegel abundantly used the word "structure" and closely associated it with comparison, in analogy with comparative anatomy:

> The decisive point which will shed light on the whole topic is the internal structure of languages or the comparative grammar, which will give us entirely new insights into the

[17] *Wissenschaftliche Weltauffassung der Wiener Kreis* (Vienna, 1929), 16. The historical reduction of geometry to *Relationsstrukturen* is described ibid. on 20. Luitzen Egbertus Jan Brouwer's, *Die Struktur des Kontinuums*, a lecture given in Vienna in 1928 and published in 1930, is mentioned as the intuitionist option for the foundations of arithmetic, ibid. on 21. The communications at the *Tagung für Erkenntnislehre der exakten Wissenschaften* held in September 1929 in Prague also mentioned "structure": Otto Neurath, "Wege der wissenschaftlichen Weltauffassung," *Erkenntnis*, 1 (1930), 106-125, on 119; Carnap, "Bericht über Untersuchungen zur allgemeinen Axiomatik," *Erkenntnis*, 1 (1930), 303-307, on 305.

> genealogy of languages just as comparative anatomy has illuminated higher natural history.

Schlegel's inspiration clearly came from Goethe, who based his anti-Newtonian morphological studies on comparative anatomy. Also Goethean was Schegel's dynamic understanding of structure in his genealogy of languages. His word choice, however, differed from Goethe's: what he called *Structur* could correspond to Goethe's *Typus*, *Gestalt*, or *Bauplan*. This difference is an evident consequence of the grammatical context.[18]

In the years 1907-11 in Geneva, Ferdinand de Saussure taught a new linguistics based on studying the relations between linguistic signs. Saussure's signs implied both a phonic (or written) *signifiant* and a conceptual *signifié*; but they were divorced from any concrete referent. He distinguished between *langage*, which is a complex, heteroclite faculty involving physical, sociological, and psychological components; and the *langue*, which is "a whole in itself and a principle of classification" and can be autonomously studied through the mutual relations and oppositions in the system of signs. His motivation, the ideal of analyzing language synchronically and independently of its concrete functions, had nothing to do with the life sciences or the social sciences, and the word structure did not occur in his writings, although one of his English translators (much) later rendered *langue* as "linguistic structure."[19]

In contrast, in the late 1920s the Prague circle of linguistics defined *la langue* as "a system of means of expression appropriate to an aim" or as a "functional system." The aim or function being relevant both to the synchronic and to the diachronic study of the *langue*, the Prague circle rejected the Saussurian separation of these two aspects. They nonetheless accepted the priority of synchronic analysis, which they conceived in structural terms as Saussure recommended. Possibly as a consequence of the organicist connotation of the word "function," they abundantly used the words "structure" and "structural" to refer to the mutual relations of the elements of the *langue*. In the collectively written *Thèses* that introduced the first volume of their *Mélanges linguistiques* (1929), they promoted "the structural comparison of related languages" and praised the comparative method for "its

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[18] Antoine Arnauld et Claude Lancelot, *Grammaire generale et raisonnée contenant les fondemens de l'art de parler, expliquez d'une maniere claire et naturelle ; les raisons de ce qui est commun à toutes les langues, et des principales differences qui s'y rencontrent ; et plusieurs remarques nouvelles sur la langue françoise* (Paris, 1660); Adam Smith, "Considerations concerning the first Formation languages," appended to *Theory of moral sentiments*, 3rd ed. (Edinburgh, 1767); Friedrich Schlegel, *Über die Sprache und Weisheit der Indier: ein Beitrag zur Begründung der Alerthumskunde* (Heidelberg, 1808), 28. Cf. Stephen Land, "Adam Smith's 'Considerations concerning the first formation of languages'," *Journal of the history of ideas*, 38 (1977), 677-690.

[19] Ferdinand de Saussure, Cours de linguistique générale, ed. by Charles Bally and Albert Sechehaye [from lectures given in 1906-1911] (Lausanne and Paris, 1916). The English translation is Roy Harris's (London: Duckworth, 1983). Cf. Jean-Marie Benoist, The structural revolution (London, 1975); Thomas Pavel, The feud of language: A history of structuralist thought (Oxford, 1989).

ability to reveal the laws of structure of linguistic systems and of their evolution"; they defined "the structural principle of the phonological system" according to which "the sensorial elements of the phonological elements are less essential than their mutual relations" and they characterized the phonological system by "specifying the relations between phonemes, namely, by drawing the scheme of structure of the given language"; they emphasized the "internal structure (reciprocal relations of the elements)" in the classification of the kinds of denominations in a given language.[20]

The *Thèses* of the Prague linguists were written as a contribution to the first congress of Slavic philologists held in Prague in October 1929. In September of the same year in Prague, the Verein Ernst Mach had held a *Tagung für Erkenntnislehre der exakten Wissenschaften* in connection with the simultaneous meetings of the German physical society and the German mathematical society. The Prague linguists are likely to have attended this event, for which the manifesto of the Vienna circle was written. Possibly, they imitated the way "structure" was used in this circle; their usage may also have derived from organicist analogies, as was just mentioned.[21]

One member of the Prague linguistic circle, the Russian émigré Roman Osipovich Jakobson called *structuralism* the tendency, in any mature science, to extract autonomous structures and investigate their internal dynamics:

> If we wanted to characterize briefly the kind of thinking currently governing science in its most varied manifestations, we could not find a more fitting expression than *structuralism*. Each set of phenomena handled by today's science is thought of not as a mechanical assemblage but rather as a structural unit, a system; and the fundamental task is to discover its intrinsic laws, both static and dynamic. What is at the center of scientific concerns today is not any external impulse or influence but rather the internal conditions for evolution; not genesis as a mechanical operation but function.

Another member of the circle, the Russian prince Nikolai Sergeyevich Trubetzkoy, echoed this view in 1933:

> Today's phonology is characterized mainly by its structuralism and by its systematic universalism . . . The present period is characterized by the tendency of all scientific disciplines to replace atomism by structuralism and individualism by universalism (in the philosophical sense of these terms, of course). This tendency can be observed in physics, chemistry, biology, psychology, economic science, etc. Today's phonology is therefore not an isolated case. It belongs to a broader scientific movement.

Trubetzkoy explicated the biological analogy that underlay the new phonology:

> To define a phoneme is to specify its place in the phonological system, which is possible only if we take into account the structure of this system . . . Phonology, universalist by

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[20] "Thèses," in *Mélanges linguistiques dédiés au premier congrès des philosophes slaves*, 1 (1929), 6-29, on 9, 10, 11, 12. Cf. Émile Benveniste, "«Structure» en linguistique," in Roger Bastide (ed.), *Sens et usages du terme structure dans le sciences humaines et sociales* (The Hague, 1962), 31-39.

[21] On the two congresses, cf. *Mélanges*, ref. 20, on 5 (*avant-propos*); *Wissenschaftliche Weltauffassung*, ref. 17, introduction.

> nature, deals with the system as an organic whole, whose structure it studies . . . In applying the principles of phonology to many different languages in order to bring out their phonological systems and in studying the structure of these systems, one soon notices that certain combinations of correlations exist in the most diverse languages, whereas others never occur . . . A phonological system is not the mechanical sum of isolated phonemes but an organic whole of which the phonemes are members and whose structure is subjected to laws.

The expression "organic whole" reminds us of living organisms. This analogy had been popular in post-romantic linguistics in the nineteenth century, for instance in Wilhelm von Humboldt's *Kawi Werk*, and it had a Goethean flavor; it was foreign to the Geneva school, who favored a more mechanistic view of structure.[22]

Anthropology

Famously, Jakobson befriended Claude Lévi-Strauss at the École Libre des Hautes Études in New York during World War II. Inspired by the new phonology, young Lévi-Strauss developed his structuralist analysis of kinship, in which the correlations between kinship units played a role similar to the interrelations of phonemes in structural linguistics. He later extended the structuralist approach to a comparative study of myths, which was the cornerstone of his structural anthropology. His variety of structuralism was extremely influential, in part due to the literary success of *Tristes tropiques* (1955).[23]

Mathematics, again

Structures also played a central role in the project of a few French mathematicians launched in the mid-1930s under the fictitious authorship of Nicolas Bourbaki. Their ambition was to reunify an increasingly diversified mathematics under

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[22] Roman Osipovich Jakobson, "Romantické všeslovanství-nová slavistika" [Romantic panslavism – new slavic studies], *Čin*, 1(1929), 10-12, cited in Patrick Sériot, *Structure and the whole: East, west and non-Darwinian biology in the origins of structural linguistics* (Boston, 2014), 248; Nikolai Sergeyevich Trubetzkoy, "La phonologie actuelle," *Psychologie du langage* (Paris, 1933), 227-246, on 233; Wilhelm von Humboldt, *Über die Kawi–Sprache auf der Insel Java, nebst einer Einleitung über die Verschiedenheit des menschlichen Sprachbaues und ihren Einfluss auf die geistige Entwickelung des Menschengeschlechts*, 3 vols. (Berlin, 1836-39). Cf. Benveniste, ref. 20, pp. 35-36. On the last point, cf. Sériot, "L'origine contradictoire de la notion de système : la genèse naturaliste du structuralisme pragois," *Cahiers de l'ILSL*, 5 (1994), 19-56.

[23] Claude Lévi-Strauss, *Les structures élémentaires de la parenté* (Paris, 1949); *Tristes tropiques* (Paris, 1955); *Anthropologie structurale* (Paris, 1958). Cf. David Aubin, "The withering immortality of Nicolas Bourbaki: A cultural connector at the confluence of mathematics, structuralism, and the Oulipo in France," *Science in context*, 10 (1997), 297-342, on 309.

the generic concept of structure, which they informally defined as a set equipped with relations (between the elements of the set) and axioms about the relations. The term "structure" first occurred in discussions of the group in 1936: "The object of a mathematical theory is a structure organizing a set of elements." Bourbaki later emphasized that the structures "applied to sets of elements whose nature is *not specified*." He liked to remind the reader that arithmetic numbers and operations applied to any objects and thus offered the prototype of a structure. As a paradigm of structure, he often cited group structure, which implies only one relation of composition between the elements of a set and three axioms for this relation.[24]

When asked about the origin of the word choice "structure," André Weil (the initiator of Bourbaki) could not truly remember. He did not exclude an effect of familiarity with the linguistic concept of structure (he knew the structural linguist Émile Benveniste). It could also be that he extended a usage that already existed in group theory: as was mentioned, since the late nineteenth century additional axioms were said to provide "structure" to a group and "isomorphism" were defined as structure-preserving transformations between two groups. Or it could be that he had read Russell and Carnap. The novelty in Bourbaki's program was not their notion of structure *per se* but the idea of making it the foundation of a unified mathematics. His *Éléments de mathématique* implemented this program through the progressive construction of a hierarchy of structures in the most abstract possible way, any intuitive introduction of a given structure being regarded as interference with the purity and rigor of demonstration.[25]

Bourbaki's structuralism sometimes interacted with structuralism in the human sciences. For instance, André Weil wrote a mathematical appendix to Lévi-Strauss's *Structures élémentaires de la parenté* (1849). From the late 1940s, Jean Piaget drew on Bourbaki's structures to develop his cognitive psychology. Structuralism prospered in many fields through the 1960s, and then started to decline under criticism for its alleged rigidity. Today it survives in attenuated forms in the human sciences; it remains foundational in mathematics; and it has a few avatars in the philosophy of science. The semantic approach to physical theories, for instance, defines theories as classes of models or structures. The variety of this approach defended by Joseph Sneed and his disciples is called structuralist for its

[24] Bourbaki discussion quoted in Liliane Beaulieu, *Bourbaki: une histoire du groupe de mathématiciens français et de ses travaux (1934-1944)* (Ph.D. thesis, Université de Montréal, 1989), 317; Nicolas Bourbaki, "The architecture of mathematics," *The American mathematical monthly*, 57 (1950), 221-232, on 225-226. Cf. Aubin, ref. 23. Besides the informal idea of structure as a set with relations and axioms, Bourbaki had the ambition of a formal definition of structure, which appeared in 1957 only. Since, if we believe Leo Corry, this definition played little or no role in the other volumes of Bourbaki's treatise and in mathematics in general, it is not discussed here. Cf. Leo Corry, "Nicolas Bourbaki and the concept of mathematical structure," *Synthese*, 92 (1992), 315-348.

[25] André Weil, *Souvenirs d'apprentissage* (Basel, 1991), 120. Cf. Aubin, ref. 23, 309 (Aubin also mentions the Front populaire's "réforme des structures" as a possible source). Russell's definition of structure (adopted by Carnap) differs from Bourbaki's (a Russell structure is a class of equivalence of Bourbaki structures) and it derives from more primitive logical axioms.

structured set-theoretical framework and for its emphasis on intertheoretic relations. There are varieties of "structural realism," defended by John Worrall and James Ladyman for instance. All these authors use the word structure with its structuralist meaning as a self-contained system of relations.[26]

From this brief survey of the emergence of new meanings and new employments of "structure," it should be clear that with rare exceptions the word structure was not used in its modern structuralist guise before the late nineteenth century. The first explicit definitions of structure as a self-contained system of relations appeared around 1920 and they seem to belong to Russell, to the Vienna circle, and to the Prague linguistic school. Structure and the associated concept then spread through other human sciences, with a culmination in the 1960s. It also served in mathematics as the foundation of the Bourbaki project in the 1930s. Although there were later interconnections between the mathematical and human-science varieties of structuralism, mathematical structuralism seems to have risen independently of structuralism in the human sciences.

To which extend did the concept of autonomous relational structure precede its being called "structure" in various sciences? Although the word was abundantly used in the life sciences for the structure of organisms and organs and although Buffon, Goethe, and Darwin had structuralist ideas, they did not convey them through the word "structure." In early studies of the grammar of languages, both the concept and the name naturally occurred at least since the eighteenth century, and the Romantic poet Schlegel married them. In early sociology, the "structure" was often used by analogy between society and organism, but without the structuralist meaning. The word implicitly conveyed structuralism when sociology, in de Greef's and Durkheim's approaches, became inherently structuralist. In general linguistics, the concept preceded the name since it inhabited Saussure's lectures, a few years before the Prague circle used the word. In anthropology, literary criticism, and history, the concept and the name appeared in conjunction.[27] In mathematics, the concept was omnipresent since the origin of mathematics; the name entered group theory in the late nineteenth century; Russell gave the first formal definition of a mathematical structure in 1919, with philosophical consequences for Carnap and the Vienna circle; it became pervasive when Bourbaki decided to

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[26] Weil, in Lévi-Strauss, ref. 23 (1949), appendix; Jean Piaget, *Introduction à l'épistémologie génétique,* 3 vols. (Paris, 1950); Joseph Sneed, *The logical structure of mathematical physics*; JohnWorrall, "Structural realism: The best of both worlds?" *Dialectica*, 43 (1989), 99–124; James Ladyman, "Structural realism," in *The Stanford encyclopedia of philosophy* (Spring 2014 Edition), Edward N. Zalta (ed.), URL = <http://plato.stanford.edu/archives/spr2014/entries/-structural-realism/> . On Weil and Piaget, cf. Aubin, ref. 23, 302, 311, 317-320.

[27] The case of psychology is peculiar. In the nineteenth century, Wilhelm Wundt abundantly referred to "mental structures" in his theories. This is why his approach is traditionally called structuralist, even though it is not structuralist in the modern sense. In contrast, the *Gestalt* approach inaugurated in the 1890s by Christian von Ehrenfels is usually opposed to psychological structuralism despite its structuralist flavor (emphasizing the holistic aspects of structure).

found *la mathématique* on the concept of structure. In physics, the name occurred as early as 1906 in Duhem's *La théorie physique*; Eddington promoted its Russellian definition in 1920; it is commonly used for the mathematical structures employed in physical theories; and it is all over the structuralist variety of the semantic approach in contemporary philosophy of physics. Did physics have a concept of abstract structure before it had the name? The following is an answer to this question.

2. Structures in nineteenth-century physics

In the early nineteenth century, the French astronomer Pierre-Simon de Laplace presided over the best mathematical physics of his time. In his grand-unified theory, the world was made of discrete point-like molecules interacting in pairs through central forces. The molecules belonged to ponderable matter or to one of the imponderable fluids associated with light, electricity, magnetism, and heat. Owing to similarities between interactions among molecules of different types, there were analogies between different sectors of the theory. For instance, the Poisson equation

$$\Delta\varphi + 4\pi\rho = 0$$

between the potential φ and the density ρ applied equally well to gravitation, electricity, and magnetism. We could say that the same structure (what we would now call abstract potential theory) applied to three different domains of physics. In this statement, however, structure does not quite have its structuralist meaning, for the identity of structure remains tied to the uniform ontology of the theory: the structure is not thought independently of its object, and the nerve of theory construction is not the structure itself, it is the ontology.[28]

In the 1820s and 1830s, the Laplacian ontology of molecular fluids and matter gradually collapsed. Ethereal vibrations replaced the luminous fluid, molecular vibrations or agitation the caloric fluid, Amperean currents the magnetic fluids; and Michael Faraday rejected the electric fluids in favor of a pure field conception. The grand Laplacian unity was lost and was to be replaced by a more structural kind of unity.[29]

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[28] Cf. Robert Fox, "The rise and fall of Laplacian physics," *HSPS* , 4 (1974), 89-136; John Heilbron, *Weighing imponderables and other quantitative science*. Supt. to *HSPS*, 24:1 (Berkeley, 1993).

[29] Cf., e.g., Peter Harman, *Energy, force, and matter: The conceptual development of nineteenth century physics* (Cambridge, 1982).

Maxwell

The first major proponent of structural unity was James Clerk Maxwell. Approving Faraday's rejection of electric and magnetic fluids and taking his field conception seriously, Maxwell explained the behavior of Faraday's lines of force by analogy with the stationary flow of an incompressible fluid through a porous medium of variable viscous resistance. In this analogy, the electric field relation $\mathbf{D} = \varepsilon\mathbf{E}$, the magnetic field relation $\mathbf{B} = \mu\mathbf{H}$, and the electrokinetic relation $\mathbf{j} = \sigma\mathbf{E}$ were counterparts of the equilibrium relation $\mathbf{u} = k\mathbf{f}$ of the fluid, wherein $\mathbf{u}$ denotes the velocity of the fluid, $\mathbf{f} = -\nabla P$ the force density resulting from the pressure *P*, and $-k^{-1}\mathbf{u}$ the retarding viscous force of the porous medium. The incompressibility condition $\nabla \cdot \mathbf{u} = 0$ (or its variant $\nabla \cdot \mathbf{u} = \omega$ in the presence of a fluid source of density ω) then yield the basic equations of electrostatics, magnetostatics, and stationary currents: $\nabla \cdot \mathbf{D} = \rho$, $\nabla \cdot \mathbf{B} = 0$, $\nabla \cdot \mathbf{j} = 0$. For instance, the first of these equations, together with the electric counterpart $\mathbf{E} = -\nabla\varphi$ of $\mathbf{f} = -\nabla P$, yields the Poisson equation $\Delta\varphi + 4\pi\rho = 0$.[30]

Again we could say that electrostatics, magnetostatics, and electrokinetics here share a common structure; and we could also say that this sharing is explained by the common picture of a resisted flow. Yet there is a major difference with the Laplacian situation. For Laplace, the shared picture of molecules interacting through central forces is an ontology: it purports to be a faithful representation of all matter. In Maxwell's case, the resisted-flow analogy is purely formal and does not at all indicate that something is truly flowing in the described phenomena. Maxwell insists on this point:

> By referring everything to the purely geometrical idea of the motion of an imaginary fluid, I hope to attain generality and precision, and to avoid the dangers arising from a premature theory professing to explain the cause of the phenomena.

Maxwell regarded his "illustrations" or "physical analogies" as a via media between pure formalism and a preferred "physical hypothesis":

> The first process . . . in the effectual study of [electrical] science, must be one of simplification to a form in which the mind can grasp them. The results of this simplification may take the form of a purely mathematical formula or of a physical hypothesis. In the first case we entirely lose sight of the phenomena to be explained; and though we may trace out the consequences of given laws, we can never obtain more extended views of the connexions of the subject. If, on the other hand, we adopt a physical hypothesis, we see the phenomena only through a medium, and are liable to that blindness to facts and rashness in assumption which a partial explanation encourages. We must therefore discover some method of investigation which allows the mind at every step

[30] James Clerk Maxwell, "On Faraday's lines of force," Cambridge Philosophical Society, *Transactions* (1856), also in *MSP* 1: 155-229, Part I. Cf. Norton Wise, "The mutual embrace of electricity and magnetism," *Science* 203: 1310-1318; Darrigol, *Electrodynamics from Ampère to Einstein* (Oxford, 2000), 139-147.

> to lay hold of a clear physical conception, without being committed to any theory founded on the physical science from which that conception is borrowed, so that it is neither drawn aside from the subject in pursuit of analytical subtleties, nor carried beyond the truth by a favorite hypothesis.—In order to obtain physical ideas without adopting a physical theory we must make ourselves familiar with the existence of physical analogies.

Maxwell's idea of shared illustration or physical analogy comes close to the modern idea of shared structure and isomorphism inasmuch as it does not imply a shared ontology. However, Maxwell insistence on the merits of an intuitive picture does not square with the modernist idea of structure.[31]

The circumstances of electromagnetic theory forced Maxwell to take a further step toward abstraction. His resisted-fluid picture only worked for electricity and magnetism taken separately and statically. For the electromagnetic interactions discovered by Christian Ørsted and Michael Faraday he did not have a picture; he only had Faraday's field-based rules. In order to express these rules mathematically, he relied on the formal distinction between "force" and "flux," won by abstraction from the resisted-fluid analogy. The vectors $\mathbf{E}$ and $\mathbf{H}$ are "forces" because they are the counterparts of mechanical forces, and the vectors $\mathbf{j}$, $\mathbf{D}$, and $\mathbf{B}$ are "fluxes" because they are the counterparts of liquid fluxes. From a formal point of view, the fluxes are used to form surface integrals, and the forces are used to form line integrals (defining a work). By the Thomson-Stokes theorem relating the integral of a vector on a circuit and the surface integral of its curl on a surface bounded by the circuit, the curl of a force should be a flux. Maxwell used this rule as a constraint in building the electromagnetic equations. From Ampère's relations between electric current and magnetic force, he got

$$\nabla \times \mathbf{H} = \mathbf{j}\,.$$

From Faraday's rule of the cut lines of force he got

$$\nabla \times \mathbf{E} = -\frac{\partial \mathbf{B}}{\partial t}\,.$$

In both cases, the curl of a force is equated to a flux. Maxwell further introduced the force $\mathbf{A}$ such that $\mathbf{B} = \nabla \times \mathbf{A}$, which enabled him to rewrite the induction law as

$$\mathbf{E} = -\frac{\partial \mathbf{A}}{\partial t}\,,$$

in conformity with Faraday's intuition that the electromotive force resulted from the temporal variation of the "electro-tonic state" of the medium.[32]

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[31] Maxwell, ref. 30, on 156, 159. Cf. Jordi Cat, "On Understanding: Maxwell on the methods of illustration and scientific metaphor," *SHPMP*, 32 (2001), 395-441.

[32] Maxwell, ref. 30, Part II.

Maxwell's distinction between force and flux was only a first example of what he called "the mathematical classification of physical quantity." His *Treatise* of 1873 had a long "Preliminary on the measurement of quantities," in which he arranged physical quantities according to their dimension (Fourier), to their continuous or discontinuous character, to the scalar/vector distinction (Hamilton), to the force/flux distinction he had himself invented, and to topological properties. While he was composing the treatise, he reflected on the merits of such classification. His thoughts can be found in the texts of two conferences he gave in 1870, one for the London Mathematical Society, the other for the British Association for the Advancement of Science.[33]

In his address to the mathematicians, he emphasized the resulting economy of time:

> It is evident that all analogies of this kind depend on principles of a more fundamental nature; and that, if we had a true mathematical classification of quantities, we should be able at once to detect the analogy between any systems of quantities presented to us and other systems of quantities in known sciences, so that we should lose no time in availing ourselves of the mathematical labours of those who have already solved problems essentially the same.

As examples of such classifications, he gave those detailed in the *Treatise*. He also introduced the terms *convergence*, *curl*, and *concentration* for the operators $-\nabla\cdot$, $\nabla\times$ and $-\nabla^2$ formed from the gradient operator ∇ (nabla); and he drew field archetypes for which these quantities had a local extremum. In general, Maxwell did not introduce a symbol without accompanying it with a simple geometrical or mechanical illustration. While he emphasized the benefits that physics drew from the mathematical classification of quantities, he also reminded his audience that mathematics could benefit from imagined physical contents.[34]

The symbiotic development of mathematics and physics is the central theme of Maxwell's address to the mathematical and physical sections of the British Association:

> If the skill of the mathematician has enabled the experimentalist to see that the quantities which he has measured are connected by necessary relations, the discoveries of physics have revealed to the mathematician new forms of quantities which he could never have imagined for himself.

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[33] Maxwell, *A treatise on electricity and magnetism*, 2 vols. (Cambridge, 1873), §§1-26. Cf. Peter Harman, "Mathematics and reality in Maxwell's dynamical physics," in Robert Kargon and Peter Achinstein (eds.), *Kelvin's Baltimore lectures and modern theoretical physics: Historical and philosophical perspectives*, 267-297 (Cambridge, 1987); Darrigol, "Models, structure, and generality in Clerk Maxwell's theory of electromagnetism," in Karine Chemla, Renaud Chorlay, and David Rabouin, *The Oxford handbook of generality in mathematics and the sciences* (Oxford, 2016), 345-358.

[34] Maxwell, "Remarks on the mathematical classification of mathematical quantities," Mathematical Society of London, *Proceedings* (1870), also in *MSP* 2, 257-266, on 258.

Maxwell meant that physics borrowed from mathematics the arithmetic needed for the measurement of quantities while the classification of the various kinds of quantities and the resulting mathematical constructs proceeded from physical ideas.[35]

In structuralist terms, Maxwell regarded mathematics as a reservoir of structures constraining the relations between physical quantities, and physics as an incentive for the mathematician to invent new structures. He did not, however, wish the structures to be thought in a purely abstract way, and he rather had them be locally illustrated in a physical or geometrical manner.

Let us return to the history of electromagnetism. Maxwell's structural approach of 1856, based on the distinction between flux and force, did not fully satisfy him. He wanted a physical mechanism to explain electromagnetic forces and the induction law. This he found in 1862, by assuming that the magnetic field corresponded to molecular vortices in a mechanical medium. This is the famous model later nicknamed "Maxwell's honeycomb" by Duhem. Consistency requirements for the dynamics of this medium led Maxwell to the system

$$\nabla \cdot \mathbf{D} = \rho \,,\ \nabla \cdot \mathbf{B} = 0 \,,\ \mathbf{j} = \nabla \times \mathbf{H} - \frac{\partial \mathbf{D}}{\partial t} \,,\ \mathbf{E} = -\nabla \varphi - \frac{\partial \mathbf{A}}{\partial t} \,,\ \mathbf{B} = \nabla \times \mathbf{A} \,,$$

and to the electromagnetic theory of light. He had no illusion about the reality of his mechanical model:

> The conception of a particle having its motion connected with that of a vortex by perfect rolling contact may appear somewhat awkward. I do not bring it forward as a mode of connexion existing in nature, or even as that which I would willingly assent to as an electrical hypothesis. It is, however, a mode of connexion which is mechanically conceivable, and easily investigated, and it serves to bring out the actual mechanical connexions between the known electro-magnetic phenomena; so that I venture to say that any one who understands the provisional and temporary character of this hypothesis, will find himself rather helped than hindered by it in his search after the true interpretation of the phenomena.

As Maxwell more briefly explained in a contemporary letter to his friend Peter Guthrie Tait, "The nature of this mechanism is to the true mechanism what an orrery is to the solar system." In his *Treatise* of 1873, he underlined that an infinite number of distinct mechanisms were able to produce the same connections between two parts of a mechanical system.[36]

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[35] Maxwell, "Address to the Mathematical and Physical Sections of the British Association," *British Association report*, also in *MSP* 2, 215-229, on 218.

[36] Maxwell, "On physical lines of force," *Philosophical magazine* (1861-62), also in *MSP* 1, 451-513, on 486; Duhem, ref. 4, on 134; Maxwell to Tait, 23 Dec. 1867, in Harman (ed.), *The scientific letters and papers of James Clerk Maxwell*, 3 vols. (Cambridge, 1990-2002), vol. 2, p. 337; Maxwell, *Treatise*, ref. 33, §531. Cf. Daniel Siegel, *Innovation in Maxwell's electromagnetic theory: Molecular vortices, displacement current, and light* (Cambridge, 1991).

These remarks made it desirable to develop an alternative approach in which broader structural considerations determine the field equations. This Maxwell did in 1865 in his "dynamical theory of the electromagnetic field." There he simply assumed that the magnetic field was a hidden motion connected to the electric currents by some unknown mechanism. This assumption in itself requires the Lagrangian structure of the field equations: they can be derived by writing Lagrange's equations for a Lagrangian given by the magnetic energy expressed as a function of the currents (regarded as generalized velocities) and their positions. In particular, the induction law $\mathbf{E}_{\text{ind}} = -\partial\mathbf{A}/\partial t$ becomes Lagrange's equation for the generalized force $\mathbf{E}_{\text{ind}}$ and the generalized momentum $\mathbf{A}$. Maxwell thus imposed the Lagrangian structure on the electromagnetic field. The kind of structure here differs from the ones encountered in his classification of physical quantities, since it concerns the theory as a whole, and not how the components of a given formula fit together.[37]

Again, the implied structure was not as abstract as it would be in a purely structuralist approach. Whereas modern physicists content themselves with the formal expression of a Lagrangian, Maxwell and William Thomson wanted a concrete, physical interpretation of the quantities entering Lagrange's equations: they defined the generalized forces through their work, and the generalized momenta through the impulsive forces needed to bring the system to a given state of motion. If, as Maxwell once proposed, the electromagnetic field theorist could be compared to the bellman pulling the ropes of a belfry without knowing or seeing its mechanism, he could feel the Lagrangian structure through his muscles. Moreover, Maxwell did not regard the Lagrangian approach as the last word in the electromagnetic theory. He appreciated its solidity and its neutrality, but he still hoped that someday physicists would discover a plausible mechanism for electromagnetic field processes.[38]

To sum up, Maxwell had the idea of structures existing independently of any fixed, concrete substratum, and he used it to unify, construct, and consolidate his theories. But he had a natural dislike for purely abstract structures. He believed that at least for some type of minds, the association of a structure with a concrete picture could support and guide our thinking. In order to be fully alive in the world of theories, a structure needed the flesh of a concrete paradigm.

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[37] Maxwell, "A dynamical theory of the electromagnetic field," Royal Society of London, *Philosophical Transactions* (1865), also in *MSP* 1, 586-597. Cf. Jed Buchwald, *From Maxwell to microphysics: Aspects of microphysics in the last quarter of the nineteenth century* (Chicago, 1985), chap. 1.

[38] Maxwell, "Thomson and Tait's Natural Philosophy," *Nature* (1879), also in *MSP* 2: 776-785, on 783-784. On Thomson's conception, cf. Crosbie Smith and Norton Wise, *Energy and empire: A biographical study of Lord Kelvin* (Cambridge, 1989), 270-273, 390-395.

Helmholtz

The great German polymath Hermann Helmholtz considered himself both an empiricist who sought to ground every science on experimental facts and a Kantian who believed in a priori necessary conditions for any empirical knowledge. As a result, the secondary literature tends to be divided in two camps: those who see him as a demolisher of the Kantian system, and those who see him as an enemy of narrow empiricism. In reality, he all along believed experience to be the ultimate source of knowledge (so too did Immanuel Kant); and his philosophical position evolved from a loosely Kantian idealism to a moderate rationalism based on empirically refutable but cognitively necessary structures. I will document this evolution firstly with regard to mechanical reduction, and secondly with regard to the status of geometry and numbers.

When, in 1847, Helmholtz wrote his famous memoir "On the conservation of force" (here meaning energy), he believed "the comprehensibility of nature" to imply the reduction of every (physical) phenomenon to the action of pairs of material points through central forces. The similarity of Helmholtz's deduction of this picture with the transcendental deduction operated by Kant in his *Metaphysische Anfangsgründe der Naturwissenschaften* (1786) has led some commentators to see the young Helmholtz as a follower of Kant's transcendental deduction of the principles of Newtonian mechanics. The similarity is however imperfect and the result of the deduction coincided with the not quite defunct Laplacian foundations of physics, which owed nothing to Kant. Also, Helmholtz did not regard his deduction of the Laplacian scheme as the sole foundation of energy conservation. He believed he could derive the same scheme from a commonly accepted empirical fact: the impossibility of perpetual motion.[39]

From the reduction of any closed physical system to material points and central forces acting in pairs, Helmholtz deduced the conservation of the sum of what we would now call the total kinetic energy of the material points and the total potential energy of the central forces. In domains of physics in which the desired reduction had already been done, for instance for gravitation, electrostatics, and magnetostatics, the corresponding formulae directly implied the conservation of energy as well as a macroscopic expression of the conserved energy as a function of measurable quantities. In domains for which such reduction was not yet available (for instance electromagnetism), Helmholtz nonetheless assumed its possibility and verified that the known macroscopic laws (the expression of electromagnetic forces and the law of electromagnetic induction) complied with energy conserva-

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[39] Hermann Helmholtz, *Über die Erhaltung der Kraft, eine physikalische Abhandlung* (Berlin, 1847), introduction. Cf. Fabio Bevilacqua, "Helmholtz's *Über die Erhaltung der Kraft*: The emergence of a theoretical physicist," in David Cahan (ed.), *Hermann von Helmholtz and the foundations of nineteenth-century science* (Berkeley, 1993), 291-333; Peter Heimann, "Helmholtz and Kant: The metaphysical foundations of *Über die Erhaltung der Kraft*," *SHPS*, 5 (1974), 235-238.

tion. In such cases, the energy principle played the role of a structural constraint on the form and interrelation of macroscopic laws, irrespective of any explicit mechanical reduction.

In later writings on energy conservation and in his later contributions to theoretical physics, Helmholtz increasingly favored the macroscopic, phenomenal approach to energy conservation and rarely mentioned the ideal of reduction to central forces acting in pairs. Around 1870, he based his electrodynamics on an extension of Franz Neumann's electrodynamic potential of a system of currents (their sign-reversed energy), which served to express electromagnetic forces and electromotive forces of induction through spatial and temporal variations of the potential. He still believed that thorough mechanical reduction remained possible, and he cited Maxwell's honeycomb of 1862 as positive evidence for this possibility (although he was even less inclined than Maxwell to take this model seriously). At the same time, he believed Neumann's potential to be sufficient for constructive purposes.[40]

Helmholtz also knew that Maxwell had succeeded in writing his field equations in Lagrangian form, thus establishing the possibility of a mechanical reduction without exhibiting them. In the 1880s he became convinced that the principle of least action, from which Lagrange's equations follow, should be made the basis of all physics. He first showed, in 1884, that the equations for the thermodynamics of reversible processes were analogous to Lagrange's equations for a certain kind of mechanical systems, which he called "monocyclic systems." Late in his life, in 1892, he gave his own Hamiltonian formulation of the equations of electrodynamics, and he extended this formulation to include the coupling of electromagnetic field with ionic vibrators and derive the anomalous dispersion of electromagnetic waves in the optical domain.[41]

Helmholtz did not completely give up the idea of reduction to central forces acting in pair. He believed that at the most fundamental level of mechanical explanation, the Lagrangian of the system should be composed of a purely kinetic part (the sum of the kinetic energies of material points) and a purely potential part depending on the spatial configuration only. In order to generate the more general forms of the Lagrangian needed in thermodynamics and electrodynamics, he introduced hidden motions at the fundamental level and then eliminated the corresponding coordinates to get the more general form of the effective Lagrangian in

[40] Cf. Buchwald, "Electrodynamics in context: object states, laboratory practice, and anti-idealism," in Cahan, ref. 39, 334-373; Darrigol, "Helmholtz's electrodynamics and the comprehensibility of nature," in Lorenz Krüger (ed.), *Universalgenie Helmholtz. Rückblick nach 100 Jahren* (Berlin, 1994), 216-242.

[41] Helmholtz, "Studien zur Statik monocyklischer Systeme," *BB* (1884), also in *HWA* 3, 119-202; "Über die physikalische Bedeutung des Princips der kleinsten Wirkung," *Journal für die reine und angewandte Mathematik* (1886), also in *HWA* 3, 203-248; "Das Prinzip der kleinsten Wirkung in der Elektrodynamik," *Annalen der Physik* (1892), also in *HWA* 3, 476, 504; "Elektromagnetische Theorie der Farbenzerstreuung," *BB* (1992), also in *HWA* 3, 505-525. Cf. Darrigol, ref. 30, on 258, 320, 423-425.

terms of the empirically accessible coordinates. That said, all he needed to know from a constructive view point was the Lagrangian form of the equation of motion:[42]

> I believe the general validity of the principle of least action to be sufficiently established so that it can have a high value as a heuristic principle, as a leading thread in our striving to formulate the law of new classes of phenomena. In addition, this principle has the advantage of condensing, for the investigated class of phenomena, all the relevant conditions in just one formula, thus offering a complete overview of everything essential.

Helmholtz regarded his last theory of anomalous dispersion as a glaring example of this heuristic power of the principle of least action:[43]

> Instead of starting with Maxwell's equations, I have preferred to integrate the additional interactions [caused by the ionic vibrators] in the form of the principle of least action that I developed for electrodynamics, because this prevents us from overlooking necessary counter-actions in the rather intricate play of forces and because this significantly diminishes the number of independent assumptions of dubious validity.

When, in the few weeks separating Heinrich Hertz's death from his own, Helmholtz had to comment on Hertz's attempt to found all physics on the motion of connected mechanical systems involving hidden masses, he politely distanced himself from such constructive projects:[44]

> English physicists like Lord Kelvin in his theory of vortex atoms and Maxwell in his assumption of a system of cells with rotating content . . . have obviously been more satisfied with such explanation as with the mere general representation of the facts and their laws that is given by the systems of differential equation of physics. I must admit that I have so far preferred the latter form of representation and have thus felt I was on a firmer footing. Yet I would not emit any fundamental objection to the way of physicists as prominent as the three named ones [Hertz, Kelvin, and Maxwell].

To sum up, Helmholtz moved from a post-Laplacian reductionist ideal to a principle-based ideal in which the Lagrangian structure was required for any physical theory. Even though he still cared to show that sufficiently general forms of the Lagrangian were compatible with the *possibility* of the former kind of reduction, he strongly believed that the construction of physical theories should directly be based on the principle of least action. This evolution of Helmholtz's theoretical endeavors resembles Maxwell's move from the honeycomb model of the electromagnetic field to the Lagrangian form of the field equation. However, Helmholtz extracted the structural essence of the principle of least action better than Maxwell had done, because he did not try to concretize the various terms of Lagrange's equations and because he insisted on the sharing of the Lagrangian structure by all the major theories of physics. For example, he showed that Lagrange's equations implied similar reciprocity relations in various domains including acoustics, op-

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[42] Helmholtz, ref. 41, *HWA* 3, 210.

[43] Helmholtz, ref. 41, *HWA* 3, 508.

[44] Helmholtz, "Vorwort," in Heinrich Hertz, *Die Prinzipien der Mechanik in neuem Zusammenhange dargestellt* (Leipzig, 1894), vii-xxii, on xxi.

tics, thermodynamics, and thermoelectricity. He praised the inventor of the principle of least action, Pierre Louis de Maupertuis, for having anticipated this generality, although he of course rejected Maupertuis's theological motivations and praised Joseph Louis Lagrange and William Rowan Hamilton for giving formally complete and metaphysically neutral expressions of the principle.[45]

Structuralist tendencies can also be found in Helmholtz's reflections on the foundation of geometry. In the late 1860s, after encountering color space and visual space in his physiological optics, he tried to determine the empirical facts underlying ordinary geometry. The usual axiomatic, synthetic approach dealt with ideal figures instead of concrete objects and could easily be contaminated by unreliable intuitions. In order to avoid this pitfall, Helmholtz opted for an analytic approach in which the points of space were given in a continuous, differentiable manifold of three dimensions. Like Bernhard Riemann, who had introduced this concept in his own reflections on the foundation of geometry, Helmholtz regarded the manifold as a generic structure shared by different kinds of space including the space of color, visual space, ordinary space, or the space of sounds. His aim was to find an empirical justification for the additional structure provided by the Euclidean metric on the manifold. For this purpose, he observed that the measurement of (ordinary) space depended on the existence of freely mobile, rigid bodies. In modern terms, he assumed the existence of a continuous group of displacements of rigid bodies with the proper number of degrees of freedom. Focusing on the algebra of infinitesimal displacements (now called the Lie algebra of the group), he found that the only meaningful choice was the one that left a positive definite quadratic form of the coordinate differentials invariant. In other words, space necessarily had the local Euclidean structure of a Riemannian manifold. As Helmholtz assumed the free mobility of *finite* rigid bodies, the Riemann curvature of this manifold had to be a constant. As he also required space to be infinite and as he originally overlooked the possibility of negative curvature, he concluded that Euclidean geometry resulted from the empirical fact (*Thatsache*) of the existence of freely mobile rigid bodies. He soon modified this conclusion when mathematical readers informed him of the case of constant-negative curvature, which is a model of Lobachevskian geometry. In Helmholtz's final statement, the existence of freely mobile rigid bodies leaves us the choice among all geometries of constant curvature (Euclidean, spherical, or Lobachevskian). Only experience can decide between these various options.[46]

Helmholtz scholars disagree on the precise status of the hypothesis of freely mobile rigid bodies. Some see it as purely empirical, and others as a Kantian con-

[45] Helmholtz, ref. 41, *HWA* 3, 209.

[46] Helmholtz, "Über die Thatsächlichen Grundlagen der Geometrie," Naturhistorisch-medicinischer Verein zu Heidelberg, *Verhandlungen* (1866), also in *HWA* 2, 610-617; "Über die Thatsachen, die der Geometrie zum Grunde legen," Königliche Gesellschaft der Wissenschaften zu Göttingen, *Nachrichten* (1868), also in *HWA* 2, 618-639. Cf. Darrigol, *Physics and necessity: Rationalist pursuits from the Cartesian past to the quantum present* (Oxford, 1994), chap. 4, and further reference there.

stitutive principle. I think it is something in-between, namely, a basic condition for the comprehensibility of nature (the measurability of space). This condition precedes any notion of space and yet does not necessarily apply to natural phenomena: only experience can tell us to which extent (at which scales) space is principally measurable. What matters for our present purpose is not so much the precise status of the premises but the hierarchy of structures that Helmholtz implicitly introduces: firstly a bare manifold, secondly a fibered manifold with a local Lie Group structure, thirdly the subcase in which the Lie Group is Euclidean, and fourthly the sub-subcase of constant curvature. Independently of Sophus Lie's contemporary researches, Helmholtz had the basic idea of a Lie algebra and its exponentiation. But he did not quite see it as a universal group structure. The structural way of thinking is more apparent when he sees the subclass of Riemannian geometries of constant curvature as the generic concept of space in which physicists must select a special value of the curvature to get the physical space. That said, in Helmholtz's reflections on geometry the extraction of mathematical structures was only an implicit byproduct, whereas it came first and foremost in Riemann's and Lie's studies of the space problem.[47]

Having unveiled the "empirical fact" from which the axioms of geometry derive, Helmholtz tried to do the same for arithmetic in his "Zählen und Messen" (counting and measuring) of 1887. By analogy with Kant's association of numbers with the internal intuition of time, Helmholtz introduced ordinal numbers through an empirical fact of internal perception: our ability to order successive events. In his definition, ordinal numbers are arbitrary signs whose purpose is to fix in our memory the temporal order of acts of consciousness. Helmholtz then defines the addition $a+b$ inductively through $a+(b+1)=(a+b)+1$, knowing from Hermann Grassmann that the axioms of arithmetic (transitivity of equality, associativity and commutativity of addition, and compatibility of addition with equality) follow from this definition.[48]

For the sake of the history of structuralism in mathematics, it should be noted that Hermann Grassmann and his brother Robert strongly rejected any empirical definition of number or mathematical concepts in general. Their definition of quantity was purely formal:

> A **quantity** [*Gröse*] is everything that is or can be the object [*Gegenstand*] of thinking, in so far as it has only one, and not several values. The **connection** [*Knüpfung*] **of two quantities** is every placing together or binding of these quantities that is accessible to human thought, in so far as it has only one, and not several values.

[47] Cf. Joël Merker, *Sophus Lie, Friedrich Engel et le problème de Riemann-Helmholtz* (Paris, 2010).

[48] Helmholtz, "Zählen und Messen, erkenntnistheoretisch betrachtet," in *Philosophische Aufsätze, Eduard Zeller zu seinem fünfzigjährigen Doctorjubiläum gewidmet* (Leipzig, 1887), also in *HWA* 3, 356-391. Cf. Darrigol, "Number and measure: Hermann von Helmholtz at the crossroads of mathematics, physics, and psychology," *SHPS*, 34 (2003), 515-573.

This definition could apply to any object of thought and encompassed all mathematics as well as logic. This is why Hermann identified mathematics with a *Formenlehre*, and Robert called the entire realm of exact thought a *Grösenlehre* [sic] in homage to Leibniz's *Sciencia de magnitudine*. The Grasmanns' mathematics was as structural as Bourbaki's would later be, except that the combination of symbolic quantities, not the concept of set was the foundation of all mathematical constructs. Helmholtz lost this structural or formal purity by appealing to facts of consciousness.[49]

Having defined ordinal numbers (*Zahlen*), Helmholtz uses them to define the cardinal number (*Anzahl*) of a set of stable objects by counting. When the counting is applied to objects similar in some respect (for instance all of the same mass), the result is a concrete number (*benannte Zahl*). Lastly, Helmholtz defines a physical quantity and its measurement through an operation of comparison (call it the concrete equality) and an operation of composition (call it a concrete addition). For instance, masses can be compared through a balance, and they can be added by mere aggregation. For the quantity to be measurable, the concrete equality and the concrete addition must satisfy the same axioms as the corresponding axioms of arithmetic. Further assuming the divisibility of quantities and the Archimedean property (implicitly), Helmholtz defines the measure of the quantity as the concrete number of units it contains, plus the number of subunits contained in the residue, and so forth. The result of measurement thus is a fractional or decimal number, with a number of decimals depending on the desired precision.[50]

Helmholtz did not try to construct a mathematically precise concept of quantity, as Poincaré and Otto Hölder would later do. It remains true, however, that he conceived general structural requirements applying to any physical quantity, partly reflecting the axioms of arithmetic, partly formalizing the idea of successive approximation. In his view the general idea of measurement, together with the concept of number, induced a formal quantitative structure, just as the idea of space measurement induced the structure of Riemannian (constant-curvature) geometry. Helmholtz was intensely aware of the structural character of his concept of quantity. This is seen in his concluding statement, in which he summarizes the successive abstractions that enable us to extract numbers from a physical system:[51]

> When we form the concept of a class, we resume in it everything that is alike in the objects which belong to this class. When we conceive a physical relation [*physisches Verhältnis*] as a concrete number, we have also removed from the concept of the units of the class every difference that belongs to them in reality. Units are objects which we consider only as elements of their class, and the

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[49] Hermann Grassmann, *Lehrbuch der Arithmetik für höhere Lehranstalten* (Berlin, 1861); Robert Grassmann, *Die Formenlehre oder die Mathematik* (Stettin, 1872), 7-8 (his emphasis).

[50] Helmholtz, ref.48.

[51] Helmholtz, ref. 48, on 391. This empiricist view of structures generated by abstraction may be contrasted with Cassirer's later view, discussed above.

> expediency of which only depends on there being such exemplars. In the quantities that are built from them, there remains only the most accidental of differences, that of number [*Anzahl*].

Helmholtz's quantitative structure is inherently physico-mathematical: it implies arithmetic on the mathematical side, and the possibility of measurement on the physical side. This raises the question of the relationship between mathematics and physics. In Maxwell's view, the mathematicians' arithmetic help physicists structure the physical world, and the physicists suggest new kinds of mathematical quantities to the mathematicians. In the Grassmann brothers' view, mathematics is strictly autonomous and should never owe anything to physics. Its structural quality derives from the total lack of concrete reference. In the empiricist view of the mathematician Paul du Bois-Reymond, whom Helmholtz praised in his essay, mathematics is essentially generated by abstraction from the physical world. Its structural quality results from the process of abstraction through which similar relations are observed in different sets of objects. In Helmholtz's view, mathematical axioms have an empirical origin, both for geometry and for arithmetic (though not necessarily for all mathematics): they reflect our ability to measure and to count. Yet they cannot be seen as merely resulting from a process of abstraction from the concrete world; they reflect an ideal of the comprehensibility of the world. This ideal shares the a priori character of Kant's transcendental apparatus; but it has neither its rigidity nor its apodictic truth. The extent to which the structures apply to the physical world is a question that only experience can decide: for instance the transitivity of equality is a first test for the possibility of a quantity. There is no expectation that quantitative structure should automatically apply to the entire world of experience. Structure is the formal expression of a tentative form of comprehensibility.

Poincaré

In the first course he gave from the Sorbonne chair of *Physique mathématique et calcul des probabilités* in 1887-88, Henri Poincaré expounded no less than five optical theories based on an elastic ether. He justified this pedagogically odd choice as follows:

> The theories proposed to explain optical phenomena by the vibrations of an elastic medium are very numerous and equally plausible. It would be dangerous to confine oneself to one of them; one would thus be prone to a blind and therefore misleading confidence in this theory.

Poincaré then showed that the received optical theories could be made to share the same system of equations, with proper adjustment of the boundary conditions and proper redefinition of the local displacement of the ether. For instance, the displacement in Augustin Fresnel's theory should be the curl of the displacement in James MacCullagh's theory. What most mattered to Poincaré was the shared structure thus exhibited. The multiplicity of the ether theories reflected their reliance on arbitrary conventions and indifferent hypotheses. Even the ether, Poincaré told his Sorbonne students, might someday disappear from physics. Yet he did not want to teach the structure without its ethereal flesh. For the sake of "clarity," he told his students, "it would always be useful to study a doctrine that relates the equations of a theory to each other." Poincaré's attitude was here similar to Maxwell's: illustrations according to Maxwell could not be taken too seriously because the same illustration applied to different categories of phenomena; and the competing ether theories taught by Poincaré could not be taken too seriously because several different theories could represent the same set of phenomena. For both thinkers, structure was most important but it was most vividly seen through a pseudo-concrete realization.[52]

Poincaré's next course of lectures dealt with Maxwell's electromagnetic theory. The most striking aspect of this theory, in Poincaré's opinion, was its ability to do without a specific model of the ether:

> *To demonstrate the possibility of a mechanical explanation of electricity, we need not preoccupy ourselves with finding this explanation itself; it suffices us to know the expression of the two functions T and U that are the two parts of energy, to form with these two functions the equations of Lagrange and then to compare these equations with the experimental laws.*

On the one hand, Poincaré thought time was not ripe for physicists to abandon the quest for a specific mechanical explanation:

> A day will come perhaps when physicists will not interest themselves in these questions, inaccessible to positive methods, and will abandon them to the metaphysicians. This day has not yet arrived; man does not resign himself so easily to be forever ignorant of the foundation of things.

On the other hand, he emphasized the success of Maxwell's Lagrangian, structural approach:[53]

> What is essential, that is to say, what must remain common to all theories, is made prominent; all that would only be suitable to a particular theory is nearly always passed over in silence. Thus the reader finds himself in the presence of a form almost devoid of matter, which he is at first tempted to take for a fugitive shadow not to be grasped. But the efforts to which he is thus condemned force him to think and he ends up seeing what was often rather artificial in the theoretic constructs he used to admire.

Like Helmholtz, Poincaré soon came to regard organizing principles such as the energy principle and the principle of least action as highly efficient tools for

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[52] Henri Poincaré, *Théorie mathématique de la lumière* [Sorbonne lectures, 1887-88)], ed. by Jules Blondin (Paris, 1889), pp. II. Cf. Darrigol, "Poincaré's light," in Bertrand Duplantier and Vincent Rivasseau (eds.), *Henri Poincaré, 1912-2012. Poincaré seminar 2012* (Basel, 2015), 1-50.

[53] Poincaré, *Electricité et optique I. Les théories de Maxwell et la théorie électromagnétique de la lumière* [Sorbonne lectures, 1888] (Paris, 1890), pp. XV-XVI (his emphasis). Cf. Darrigol, "Poincaré's criticism of *fin de siècle* electrodynamics," *SHPMP*, 26 (1995), 1-44.

criticizing and constructing theories. In his Saint-Louis address of 1904 he distinguished between two kinds of physics: the old Laplacian physics of central forces, and the new "physics of principles" of which Clausius's and Thomson's thermodynamics and Maxwell's dynamical theory of the electromagnetic field were the canonical examples. By that time he believed the physics of principles to be winning, although the principles themselves seemed in danger. In particular, the relativity principle, which he had introduced in the electrodynamics of moving bodies, did not square well with even the best electromagnetic theories of the time (Lorentz's and Larmor's). The Palermo memoir he wrote in the following year was an attempt to solve this crisis. After a few corrections, he could prove that the Maxwell-Lorentz equations for the electromagnetic field and the motion of electrons were strictly invariant by what he called the "Lorentz group." He used this invariance to explain the lack of effect of a uniform translational motion of the system with respect to the ether, in harmony with the principle of relativity. He further required this principle and the attached Lorentz-group symmetry to apply to other kinds of forces, gravitational forces in particular. The relativity principle and the Lorentz group thus had a highly structuring power, although Poincaré shied away from redefining space and time on the basis of this group structure.[54]

The latter remark brings us to Poincaré's idea of the role of groups at the interfaces between mathematics, geometry, and physics. In his philosophy, the group structure expresses our inborn ability to conceive the composition of operations of the same kind: "The general concept of group preexists in our minds, at least potentially. It is imposed on us not as a form of our sensibility, but as a form of our understanding." This Kantian form plays a central role in organizing our perceptual experience, and therefore should pervade any physical theory. In particular, our concept of space derives from our ability to combine and compensate displacements of objects and displacements of our body according to a Lie-group structure.[55]

Unlike Helmholtz, Poincaré did not regard the group of displacements as deducible from geometric experience. In his view the definition of the class of rigid bodies was necessarily conventional, and the same geometrical experience could be described by means of different groups as long as the mechanical laws ruling the deformation of a concrete body during its displacements were adjusted to the choice of the group. The Euclidean group recommended itself for its simplicity, its practical convenience, and its historical dominance; even if our concrete geodesy someday happened to detect apparent violations of Euclidean properties, we would be wise to interpret these violations by physical deformations of the geodesic devices. Poincaré held a similar conventionalism with regard to kinematics:

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[54] Poincaré, "L'état actuel de la physique mathématique" [Saint-Louis lecture], *Bulletin des sciences mathématiques*, 28 (1904), 302-324; "Sur la dynamique de l'électron," Académie des sciences, *Comptes rendus hebdomadaires des séances*, 140 (1905), 1504-1508; "Sur la dynamique de l'électron," *Rendiconti del Circolo Matematico di Palermo*, 21 (1906), 121-176.

[55] Poincaré, *La science et l'hypothèse* (Paris, 1902), 90. Cf. Igor Ly, *Géométrie et physique dans l'œuvre de Henri Poincaré* (Thèse, Université Nancy 2, 2007).

in his opinion, the Galilean group had to remain the basis of our definition of space and time despite its incompatibility with natural conventions of optical measurements, and despite its differing from the invariance group of the fundamental equations of physics.[56]

For mechanics and other physical theories, Poincaré's conventionalism was less extreme than in geometry, because it was only in the case of geometry that the theories implied in the conventions of measurement (mechanics and optics) had an external origin. For other physical theories, the basic principles and structures had strong inductive grounding. At any rate, the group structure had to pervade physical theory according to Poincaré. The same group could cover the entire domain of physical experience and structure every fundamental theory at a given stage of physics. This was the case of the Euclidean group in pre-relativistic physics, and of the Lorentz group in relativistic physics. For Poincaré, mathematical physics was all about uniform, homogenous behavior in which phenomena could be regarded as combinations of similar (infinitesimal) elementary phenomena. It therefore was the realm of group theory.[57]

In arguing the necessity of group structure in theoretical physics, Poincaré elevated Helmholtz's earlier reflections on the foundations of geometry to a higher philosophico-mathematical plane. Similarly, his discussion of number and quantity had strong affinities with Helmholtz's "Zählen und Messen." Like Helmholtz, Poincaré assumed the existence of a successor $a+1$ for any number *a*; he defined the addition $a+b$ inductively through $a+(b+1)=(a+b)+1$; and he derived its commutativity and associativity also by induction. Whereas Helmholtz cared to justify the existence of successors by internal experience, Poincaré rather regarded it as an innate mental faculty of which we become aware through experience. What mattered most to him was the resulting possibility of mathematical induction, in which he saw the source of any generality in mathematics. Induction was "the prototype of the synthetic a priori judgment" or "the affirmation of the power of the mind which knows itself capable of conceiving the indefinite repetition of the same act as soon as this act is once possible." The group structure, of which arithmetic gives us a first infinite example, proceeded from the same kind of inductive generalization.[58]

Poincaré then introduced the idea of a (measurable) continuum in a manner similar to Helmholtz's definition of measurable quantities, but with different intentions. Whereas Helmholtz meant to specify the conditions under which a quan-

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[56] Cf. Michael Friedman, "Poincaré's conventionalism and logical positivism," *Foundations of science*, 2 (1995), 299-316; Michel Paty, *Einstein philosophe: La physique comme pratique philosophique* (Paris, 1993), 250-263; Gerhard Heinzmann, "The foundations of geometry and the concept of motion: Helmholtz and Poincaré," *Science in context*, 14 (2001), 457-470; Scott Walter, "Hypothesis and convention in Poincaré's defense of Galilei spacetime," in Michael Heidelberger and Gregor Schiemann (eds.), *The Significance of the hypothetical in the natural sciences* (Berlin, 2009), 193-219.

[57] Poincaré, ref. 55, 162-165 (mechanics vs. geometry), 181-188 (elementary phenomena).

[58] Poincaré, ref. 55, on 23-24.

titative structure derived from concrete equality and concrete addition, Poincaré wanted to construct the mathematical continuum. In the latter view, idealized measurement provides the mathematician with the "occasion" to build the continuum in a rigorous, arithmetic-based manner, for instance through Dedekind's cuts: "The mathematical continuum . . . has been created from bits and pieces by our minds, but it is experience that has provided the occasion."[59]

In general, for Poincaré experience provides the occasion to develop the mathematical notions of number and continuum. However, mathematical rigor is incompatible with the vagueness of experience. Mathematics must be grounded on a priori faculties of the mind such as the possibility of indefinitely combining similar objects (of thought). Experience is the occasion, and the mind is the architect. In this view, much of our mathematics is motivated by proto-quantitative aspects of experience. The generic character of these aspects implies the structural character of the resulting mathematical constructs. In turn, the empirical motivation of these constructs explains the success of mathematics when applied to the physical world. What remains unpredictable is the extent of this success. Structures motivated by grossly quantitative experience need not apply to finer experimentation. It all depends on how much homogeneity and self-similarity there truly is in the world:

> For this [group composition], all the operations must be alike. In the opposite case, it would evidently be necessary to resign ourselves to doing them effectively one after another, and mathematics would become useless. It is then thanks to the approximate homogeneity of the matter studied by physicists, that mathematical physics could be born. In the biological sciences, we no longer find these conditions: homogeneity, relative independence of remote parts, simplicity of the elementary fact; and this is why naturalists are obliged to resort to other methods of generalization.

Poincaré's position is again similar to Helmholtz's: The (quantitative) comprehensibility of nature implies its subsumption under certain mathematical structures; but only experience can tell us how far the subsumption can be pushed. The main ways in which Poincaré still departs from Helmholtz are his higher insistence on rigorous, autonomous mathematical constructs and his amplification of the conventional elements of any application of a mathematical structure to the physical world.[60]

Conventionalism easily degenerates into nominalism, which is the doctrine that scientific concepts are but names arbitrarily imposed on an inherently amorphous nature. In this view, promoted at the turn of the century by Édouard Le Roy in the *Revue de métaphysique et de morale*, science does not tell us anything about nature; it is only a rule of action. Poincaré soon replied to Le Roy in the same journal in 1902. In the contemporary foreword to *La science et l'hypothèse*, he noted:

> Some people have been struck by this character of free convention recognizable in certain fundamental principles of the sciences. They have wished to generalize beyond measure,

[59] Poincaré, ref. 55, on 35.

[60] Poincaré, ref. 55, on 187-188.

> and, at the same time, they have forgotten that liberty is not license. Thus they have reached what is called nominalism, and have asked themselves if the savant is not the dupe of his own definitions and if the world he thinks he discovers is not simply created by his own caprice. Under these conditions science would be certain, but deprived of significance.

Against this view, Poincaré argued that despite all conventions, despite indifferent hypotheses, and despite all what today's philosophers of science would call surplus content, physical theories contained relations that reflected genuine empirical regularities and survived replacement by better theories:[61]

> If this were so [as imagined by the nominalists], science would be powerless. Now every day we see it work under our very eyes. That could not be if it taught us nothing of reality. Still, the things themselves are not what it can reach, as the naive dogmatists think, but only the relations between things. Outside of these relations there is no knowable reality.
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> Without doubt, at first blush, the theories seem to us fragile, and the history of science proves to us how ephemeral they are; yet they do not entirely perish, and of each of them something remains. It is this something we must seek to disentangle, since there and there alone is the veritable reality.

The undying content of theories, their real import, is what Poincaré called the *rapports vrais* (true relations or ratios). It is not easy to see precisely what he meant by this phrase nor what kind of realism he thus expressed. What is certain is that he had in mind relations independent of the more contingent elements of physical theory, in one word: a structure. What is also certain is that the qualification "true" meant conformity with the external, to us given world. But where should we locate the *rapports vrais* in a given theory? Should we identify them with the mathematical structure shared by all the formulations of a given theory, for instance the shared systems of equations in the various optical theories available in the 1880s? Even though Poincaré occasionally suggested so much,[62] this does not work too well, for at least two reasons. Firstly, the mathematical structure, no matter how much we have stripped it from all surplus content, does not connect to the empirical world without arbitrary conventions. A change of convention may imply a change of mathematical structure, just as in the case of geometry a different convention of space measurement leads to a different group of displacements. Secondly, even supplemented with the necessary conventions of measurement, the mathematical structure cannot possibly be an exact reflection of nature because the experimental predictions of the theory can have only approximate validity. At best we know that to some approximation the structure correctly represents the phenomena. The precise delimitation of the domain of validity must

[61] Édouard Le Roy, "Science et philosophie," *Revue de métaphysique et de morale*, 7 (1899), 375-425, 503-862, 708-731; 8 (1900), 37-72; Poincaré, "La valeur objective de la science," *Revue de métaphysique et de morale*, 10 (1902), 263–293; ref. 55, on 3-4, 6. For a thorough discussion, cf. João Príncipe, "Sources et nature de la philosophie de la physique d'Henri Poincaré," *Philosophia scientiae*, 16 (2012), 197-222, on 209-215.

[62] Poincaré, ref. 55, 189-190.

await the availability of a more fundamental theory (for instance, we only know the domain of validity of rays optics from the deeper theory of wave optics). Once a deeper theory is known and used to assess the domain of the original theory, we are free to use the structure of either theory within this domain. This is one more symptom of underdetermination of the mathematical structure.

Poincaré does not fully address these difficulties. We can only imagine possible answers, based on hints found here and there in his texts. The first difficulty can be avoided by associating the *rapports vrais* to a doublet including the mathematical structure and the conventions of measurement. As long as the empirical consequences are well defined and as long as they are experimentally verified, the doublet is true to the real world and it will retain its value in the future evolution of physics. To preserve the structuralist flavor of the *rapports*, it might be better to associate them with the class of all such doublets that have the same empirical content. This is pretty much what Poincaré is doing when he considers a variety of ether theories that all lead to the same empirical predictions, or when he tells us that the same physical geometry can be described by different groups with proper adaptation of the conventions of measurement. This view presupposes the existence of a level of convention-free empirical facts for which the comparison of empirical predictions is unambiguous. This is why, in his reply to Le Roy, Poincaré distinguished between the "crude facts" of observation and the more economically expressed but convention-dependent "scientific facts."[63]

As for the approximate character of the *rapports vrais*, it is a problem only for those who expect physical theories to give perfectly accurate predictions in perfectly well-circumscribed domains. In real life, the predictions are approximate and the frontiers of the domain of application are blurred. We may still define the *rapports vrais* of a theory by a triplet including the mathematical structure, the conventions of measurement, and an estimate of the range of validity of its predictions. As was mentioned, this estimate can only be known a posteriori, when a deeper theory is known. There is no reason to assume that Poincaré meant the *rapports vrais* to be defined in a non-retrospective manner. On the contrary, he often used this expression to suggest that a theory anticipated structural features of a later improved theory. For instance, at the turn of the century he tells us that Lorentz's electromagnetic theory is "the one which best explains the known facts, the one which illuminates the greatest number of *rapports vrais*, the one of which most traces will be found in the final construction."[64]

If we neglect these subtleties, we find in Poincaré an amplification and an extension of the structural tendency introduced by the two physicists he most admired: Maxwell and Helmholtz. For Poincaré physical theories imply generic mathematical structures such as arithmetic, the continuum, and groups structure

[63] Poincaré, ref. 61. The distinction is rough, and it was duly criticized in Duhem , ref. 4, on 242-247. A modular view of the comparison between theories would here be more adequate: cf. Darrigol, "The modular structure of physical theories," *Synthese*, 162 (2008), 195-223.

[64] Poincaré, ref. 55, on 205.

created by the human mind in order to capture the regularity of phenomena. Besides these structures of global cognitive significance, there also are special structures attached to specific theories. These may be defined synchronically as systems of relations shared by empirically equivalent theories and independent of the superfluous imagery they often carry with; they may also be defined diachronically as the undying structural core of successful theories. Ironically but predictably, the thinker who most insisted on the necessary conventional elements of any theory is also the one who most insisted on invariant, objective structures in physical theory.

Duhem

Pierre Duhem introduces his *La théorie physique, son objet et sa structure* (1906) with an analogy between theory and instrument. In order to know how to use an instrument properly, Duhem tells us, we pull it apart and analyze the various parts and their configuration. He goes on:

> I have applied a similar analysis to physical theory. I have first tried to determine its *object* in a precise manner. Then, knowing the aim of Theory, I have examined its *structure*; I have successively studied the mechanism of each of the operations through which it is constituted; I have indicated how each of them contributes to the object of the Theory.

Further in his book, Duhem defines theory as follows:

> *A physical theory is . . . a system of mathematical propositions, deduced from a small number of principles whose purpose is to represent as completely and exactly as possible a set of experimental laws.*

The mathematical propositions are relations between a small number of magnitudes [*grandeurs*] represented by (real) numbers and associated with simple properties of the systems under consideration. The principles are themselves hypothetical propositions, or relations between magnitudes, from which all other propositions of the theory can be deduced by strictly logico-mathematical means.[65]

For Duhem, there are two kinds of grandeurs: the genuine *quantities* for which a concrete equality and a concrete addition can be exhibited in Helmholtz's manner, and the *qualities* for which a concrete ordering exists but no concrete addition

[65] Duhem, ref. 4, on 2, 26 (his emphasis). On Duhem's life and philosophy, cf. Stanley Jaki, *Uneasy genius, the life and work of Pierre Duhem* (The Hague, 1984); Anastasios Brenner, *Duhem, science, réalité et apparence* (Paris, 1990); Rober Ariew, "Pierre Duhem", *The Stanford Encyclopedia of Philosophy* (Fall 2014 Edition), in Edward N. Zalta (ed.), URL = http://plato.stanford.edu/archives/fall2014/entries/duhem/ , and further reference there. One might be tempted to think that Duhem had the social structures of his Bordeaux colleague Durkheim in mind when he used "structure" in his title; Durkheim sparing use of "structure" and Duhem's analogy with the structure of instrument do not encourage this speculation.

exists. For instance, temperature is a quality because we can determine whether a body is warmer than another but we cannot add temperature differences in any concrete manner. As long as the concrete ordering satisfies the axioms of a relation of partial order, we may still associate a real number with a quality, except that this number depends on a specific, arbitrary scale. This is what is concretely achieved by a thermometer. Unlike Helmholtz and more like Ernst Mach, Duhem criticized the Cartesian tendency to reduce every quality to a quantity, and recommended, for the best economy of representation, to preserve a small number of qualities in the set of primitive magnitudes of the theory.[66]

The principles of the theory are, taken separately, purely mathematical relations. They are neither consequences of higher metaphysical theory, nor direct expressions of experimental laws. In the first case, one would fall into the historically frequent error of confusing a representation with an explanation; in the second case, one would be a naive empiricist unable to reach a sufficient level of generalization. In both cases, dogmatism would settle in. Duhem also disliked the British indulgence in mechanical illustrations. He understood that for Thomson, Maxwell and their followers the illustrations were just illustrations and did not carry any metaphysical weight, but he considered them as superfluous and even harmful in the process of theory of construction. He compared the evolution of physics in the previous centuries to a rising tide on an inclined shore: the water front oscillates and yet keeps progressing on average. The oscillations are the effect of perishable metaphysical assumptions or unnecessary illustrations; the net progress is what would be left to physicists, if they were as sober as wished by Duhem.[67]

Duhem accompanies his reflections on the nature of theory with reflections on the nature of experimentation. He understands that even simple experiments of physics require, in the statement of their results, a considerable theoretical equipment. For instance, the measurement of an electric current through a galvanometer appeals to the laws of electromagnetism and mechanics. Moreover, Duhem argues that the different principles of a theory cannot be tested separately and that only the complete theory with all its principles and deductions, has a well-defined empirical content. He is therefore very far from giving to the theoretical magnitudes and their mutual relations a direct empirical significance. On the contrary, he insists that any powerful theory has to introduce magnitudes and relations that have no empirical counterpart.[68]

In order to get a more precise idea of Duhem's concept of physical theory, one must examine his own theoretical production, especially his *Traité d'énergétique* of 1911, since he himself regarded *La théorie physique* as a kind of introduction to this treatise.[69] Thermodynamics and thermochemistry being his main domains of interest, he wanted a theory encompassing these two fields as well as transport

[66] Duhem, ref. 4, Part 2, Chap. 1.

[67] Duhem, ref. 4, Part 1; p. 58 (tide).

[68] Duhem, ref. 4, Part 2, chaps. 4, 6; p. 340 (theoretical magnitudes and relations).

[69] Duhem, *Traité d'énergétique ou de thermodynamique générale* (Paris, 1911), 3n.

phenomena and mechanics (he left the more difficult case of electrodynamics for future developments). His basic strategy was firstly to identify the simple magnitudes of the theory at a macroscopic level (position, pressure, temperature, etc. for each infinitesimal element of each simple substance regarded as a continuum), secondly to impose kinematic constraints, thirdly to define the energy variation during virtual changes of states compatible with constraints, and lastly to derive the statics and dynamics of the system from principles regulating virtual and real changes. This works a little like Jean le Rond d'Alembert's foundation of mechanics on the principle of virtual works and on d'Alembert's principle, except that the evolution of systems now includes non-mechanical variables. Duhem defined state and motion in a neo-Aristotelian manner, as a set of independent magnitudes and their time-derivatives, and advised physicists against attempts to reduce any change of state to the mechanical motion of invisible entities.[70]

With this brief survey of Duhem's conception of physical theory, we may now decide how much what he calls the "structure" of a physical theory reflects a structuralist view. Firstly, his theory rests on two basic mathematical structures, metric structure (for quantities) and ordinal structure (for qualities), both leading to the representation of physical magnitudes by numbers. Implicitly, he associates two symmetries with these two structures: symmetry through changes of the basic units (dimensional invariance) for the quantities, and symmetry through changes of scale for the qualities. Secondly, Duhem has a highly abstract conception of theory and its principles. To a concrete physical system he associates "an abstract mathematical *scheme*," and he opposes the intimate structure of concrete bodies, which eludes us, to the "structure of the mathematical scheme," which is the sole object of our reasoning and which is perfectly known to us since it is a mental construct.[71] He rejects mechanical explanation; he advises against mechanical illustration; he asserts the impossibility of direct empirical interpretations of most propositions of a theory; and he sees beauty in the purely algebraic character of the relations between the various propositions of the theory. Thirdly, Duhem notes the important role of analogy in theoretical construction, to be regarded as useful transport of structure and not to be confused with illustration.[72] Fourthly, Duhem illustrates the global interconnection of theoretical propositions by comparing physical theory to an organism:[73]

> Physical science is a system that must be considered as a whole; it is an organism of which a given part cannot function without implying the most remote parts in various degrees.

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[70] Cf. Gérard Maugin, *Continuum mechanics through the eighteenth and nineteenth centuries* (Dordrecht, 2013), Chap. 10; Stefano Bordoni, "Unearthing a buried memory: Duhem's third way to thermodynamics," *Centaurus*, 54 (2012), 124-147, 232-249.

[71] Duhem, ref. 69, on 1, 50.

[72] Duhem, ref. 4, on 152-154.

[73] Duhem, ref. 69, on 308.

Fifthly, Duhem integrates several theories, including (macro-)mechanics, thermodynamics, transport theory, and thermochemistry, in a single, homogenous theoretical structure based on formal rules for the energy variations under virtual changes of state.

To some extent, Duhem structuralism reminds us of Maxwell's and Helmholtz's. He shares their emphasis on numerical structure and on analogies. His justification of the numerical structure and his ambition to embrace all physics (of his time at least) in a general frame also recalls Helmholtz, who was the physicist Duhem most admired. However, Duhem's structuralism seems more strict and rigid than that of his forerunners. He evacuates Maxwell's illustrations; he avoids Helmholtz's mechanical reduction even in its most abstract form; he relegates the atomic constitution of matter, which both Maxwell and Helmholtz defended, to the rank of metaphysical speculation. Whereas Maxwell, Helmholtz, and Poincaré sought a fruitfully destabilizing interplay between micro- and macro-structures, Duhem sought maximal stability in purely macroscopic structures. To be true, Duhem was too good a historian not to see that theoretical structures could undergo radical changes in the evolution of physics; however, in the physics of his time he did not see any reason to alter the basic outlook inherited from (macroscopic) rational mechanics and thermodynamics. Even though he had a sophisticated understanding of the relation between theory and experiment, he did not anticipate that the constructions he deemed superfluous (atomistic theories) or against commonsense (relativity theory) could turn out to be more adequate representations of the empirical world.

3. Conclusions

In the first section of this essay, we found that at least in several cases, mathematics and linguistics for instance, structuralist approaches preceded their being characterized in terms of abstract "structures." This is especially evident in the case of mathematics, for which the concern with abstract relational structures existed since Greek antiquity and yet was not named so until Cassirer and Russell promoted a modern definition of "structure" in the 1910s. The quest for abstract generality being often regarded as inherent in the definition of mathematics, the ancient origins of mathematical structuralism can hardly surprise us. In physics, the concreteness and complexity of the objects of study seem to contradict abstract generality. The ideal of a universally quantitative and mathematical physics, despite its Cartesian roots, was not commonly accepted until the late eighteenth century, and its first implementation was more constructive than structural. Yet, in the early twentieth century, Cassirer regarded the evolution of physics in the past century as the gradual demise of substance(s) in favor of relational "structures."

The second section of this essay confirms Cassirer's insight in the case of four luminaries of nineteenth-century and early twentieth-century physics: Maxwell, Helmholtz, Poincaré, and Duhem. In his electromagnetic theory, Maxwell relied on two kinds of structures, a classification of physico-mathematical quantities according to their combinational properties, and the Lagrangian structure for the fundamental equations of the theory. The modern name "structure" is here justified since Maxwell provided relational definitions and proceeded from them in a combinatorial or algebraic manner, and since his structures went along with a notion of isomorphism. In the classification case, the structure was shared by different sectors of the electromagnetic theory, in the Lagrangian case it was shared by any possible mechanical model of the theory. That said, Maxwell and most of his British colleagues had a strong dislike for purely abstract structure and believed in the cognitive and heuristic importance of concrete models or "illustrations" for the structures they encountered. Maxwell's structuralism was a pragmatic reaction to a frustrated mechanism. Not knowing how to build a simple, complete mechanical model of the electromagnetic field, he understood the merit of structures expressing the possibility of a mechanical model without a specific model to be known.

Helmholtz similarly opted for a compromise between mechanical reduction and pragmatic efficiency in theoretical construction. In his later years, he considered that the Lagrangian or Hamiltonian structure best served this purpose, not only for electromagnetism but also for all the principal theories of physics. However, he departed from Maxwell in the way he justified this structure. Whereas Maxwell adduced the possibility of a clock-like mechanism with contact action only, Helmholtz adduced the possibility of a reduction to central forces acting on pairs of material points. The latter picture was a remnant of the young Helmholtz's Kantian expression of the comprehensibility of nature. In later years, Helmholtz privileged a more empiricist view of the comprehensibility of nature, based on the possibility of counting and measuring objects. This possibility generated the quantitative structure of physics in general, as well as the locally Euclidean structure of geometry understood as the art of measuring space. Helmholtz's structuralism was more pronounced than Maxwell, for he preferred bare structures to the British illustrated structures, and because his structures were more universal than Maxwell's.

Poincaré admired both Maxwell's and Helmholtz's variety of structuralism. With Maxwell's he shared the interest in illustrated structures. For "clarity" and for the sake of mathematical imagination, he liked to see a structure through a model, no matter how superfluous, arbitrary, and unrealistic the model might be. He nonetheless understood the merits of "the physics of principles" in which general principles such as the energy principle, the principle of least action, or the relativity principle imposed a structure on all the fundamental theories of physics. He developed Helmholtz's implicit association of mathematical group structure with conditions of measurability. More broadly, he traced mathematical physics and its pervasive reliance on Lie groups to an assumed uniformity of the physical word. The relevant groups were largely conventional for geometry, less so for complete physical theories. In any case, the true undying content of a theory, what he called the "rapports vrais," was not the mathematical (group) structure itself but the conjunction of this structure with conventions of measurements and an (a posteriori) estimate of the domain of validity. The *rapports vrais* thus expressed an elusive form of structural realism in which no mathematical straightjacket could capture the truth of a theory.

Duhem shared with Helmholtz and Poincaré the idea of a quantitative structure associated with the measurement or the ordering of magnitudes. The main difference was his inclusion, among the simple magnitudes of the theory, of "qualities" that had an ordinal structure only. The intensity of a quality could still be expressed by number in a given scale, so that Duhem's theory remained expressible in terms of relations between numbers representing the various magnitudes, all derivable from a small number of principles of the same kind. Thus defined, the theory was a purely symbolic structure, devoid of ontological import and not to be directly related to sensorial observation. The symbolic structure could only be tested as a whole, because the interpretation of any experiment generally implied the entire structure. The magnitudes never had direct sensorial meaning: they could be related to experiments only through the relational structure to which they belonged. Unlike Maxwell, Helmholtz, and Poincaré, Duhem rejected even the most moderate, structural kind of mechanical reductionism and resurrected the Aristotelian qualities in a move away from mechanism. He believed he could embrace all the physics of his time in an energeticist framework, in analogy with the structures of d'Alembert's dynamics and of macroscopic thermodynamics.

Maxwell, Helmholtz, Poincaré, and Duhem all were structuralists in their appeal to abstract, universal, relational structures and they all agreed that these structures were essentially mathematical and partly dictated by the demands of measurement. Yet they disagreed on the choice of the global structure: whereas Maxwell, Helmholtz, and Poincaré required the Lagrangian structure, Duhem had his own energeticist structure. Our four luminaries also disagreed on the origin and motivation of the structures. For Maxwell, these were abstracted from the pure mechanism expressed in the British "matter and motion" program. For Helmholtz, they had Kantian origins in the reducibility to centers of forces and in constitutive principles of measurability. For Poincaré, they were in part the product of broad inductions from experience, in part the expression of the mind's innate capacity to express regularities. For Duhem, they partly derived from arithmetic and geometric "commonsense," but they also resulted from a long series of trials and errors with the aim of a "natural classification" in mind.

The structuralism of our four theorists also differed in the amount of freedom they allowed in the construction of theories. While Maxwell never departed from mechanical reducibility, he and his British colleagues felt free to choose the kind and level of the basic mechanical entities: they could be macroscopic bodies, invisible atoms, a perfect ethereal liquid, a hidden mechanism, and a few other figments of the Victorian imagination. Helmholtz favored a more sober version of the same openness: he required the Hamiltonian structure of all major theories and at the same time he ardently supported invisible entities such as molecules, atoms, ions, and the ether as long as some empirical laws seemed to require their existence. Poincaré was a pluralist who defended the competitive exploration of multiple, mutually incompatible world pictures and theories. His conventionalism naturally accommodated such multiplicity, and his structuralism allowed communication between the multiple options. Unlike his predecessors (Ludwig Boltzmann excepted), he admitted that mechanical reducibility might not extend to the smallest scales and he easily accommodated quantum discontinuity in the last years of his life. Duhem completely rejected mechanical reducibility, since he did not tolerate the atomistic assumptions needed to conciliate thermodynamic irreversibility with mechanism. This anti-mechanism did not make him more open to theoretical change. He meant his energetics to be the most stable possible frame for contemporary physics. He believed that his economic, commonsense, anti-metaphysical approach would spare physicists the backwards oscillations in the rising tide of progress; and he did not perceive any credible threat to his program in the contemporary rise of a new physics of atoms, electrons, radiations, quanta, and relativity.

According to the anti-structuralist reaction of the 1970s, structuralism should be condemned or at least deeply altered in order to accommodate historical change; structure being essentially a synchronic concept, derived from the comparison of simultaneously existing systems. It is not clear that this criticism fairly applies even to the varieties of structuralism that prospered since Jakobson employed the word in 1929. For instance, the structuralism of the Prague circle of linguistics was explicitly a reaction to the purely synchronic character of the structures of the Geneva school. For them, both the definition and the evolution of structure depended on the functions of language and therefore could not be treated separately.[74] Now, if we look at the given examples of proto-structuralism in nineteenth and early twentieth-century physics, we do find some rigidity: Maxwell and Helmholtz hoped that the Lagrangian and metric structures would forever constrain the laws of physics; and Duhem meant his energetics to stay. They were right in some sense, since today's physicists still require the Lagrangian structure at a formal (pre-quantization) level of their most fundamental theories and since the modern theory of out-of-equilibrium processes share many features of Duhem's theory.[75] They were wrong in another sense: the metric structure and Lagrange's equations of motion are no longer believed to apply to the most fundamental processes in nature; and Duhem's energetics completely ignores any microphysics.

We may advantageously follow Poincaré's hints for conciliating the permanence and the displacement of structures. As long as a given structure serves to express *rapports vrais* in an approximate manner in a given domain of experience, this structure should subsist in superseding theories at least in an asymptotic or regional manner: it should remain valid in some limit or in some sub-domain of the

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[74] In his structuralist psychology, Jean Piaget similarly insisted in the ability of structures to transform themselves: cf. Jean Piaget, *Le structuralisme* (Paris, 1968).

[75] Cf. Maugin, ref. 70.

superseding theories. This constraint, which has been called a *correspondence principle* after Niels Bohr, played an essential role in the construction of the theories that displaced the great theories of the nineteenth century, namely, relativity theory and quantum mechanics. So did too the group-theoretical symmetry considerations emphasized by Poincaré. The structuralism of nineteenth-century physics not only helped construct its main theories, but it also prepared the construction of future theories.

This importance of structures in constructing empirically effective theories may seem at odd with the definition given at the beginning of this essay. If a structure is a self-contained system or relations, how could it serve to construct anything? If the relations connect abstract terms, how could the structure tell us anything about the empirical world? Let us first address this second difficulty. The abstractness of a relational structure means only that its definition does not depend on the nature of the terms connected by the relations; it does not mean that the structure cannot be applied to or controlled by empirical phenomena. On the contrary, with the exception of mathematics, structuralism usually goes along with stronger empiricism, because the relations of the structure are believed to be more empirically significant than ontological assumptions about the terms. In the case of physics, much of the structure results from demands of measurability and uniformity, regarded as preconditions for the success of a quantitative science. When it does not, as is the case for the Lagrangian structure, it represents a move away from an empirically uncontrollable submechanics of the universe and toward a more direct expression of empirical phenomena. More broadly, structuralism dissolves our naive belief in substances devoid of empirical significance, as emphasized by Cassirer and by Gaston Bachelard.[76]

How exactly do structures connect to empirical phenomena? They do not do so by direct empirical interpretation of some of the terms of the relational structure, as logical positivists would have it. They do so through pre-interpreted substructures that enable us to imagine models of concrete experiments. In general, physical theories have a modular structure, which is the name I give to a second-order structure ruling the articulation of theoretical modules or substructures within and between theories. The modular structure evolves in the course of the life of theories, and it plays an essential role in the construction of new theories. In particular, this concept explains the constructive power of structures: even though they are in a sense self-contained, they can enjoy modular connections with other structures. The construction game is then seen as an art of combining, embedding, and grafting partial structures.[77]

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[76] See Cassirer, ref. 13; Gaston Bachelard, *La formation de l'esprit scientifique* (Paris, 1938).

[77] Cf. Darrigol, ref. 63, and João Principe's contribution to this volume.

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Metatheoretical structuralism:

Empirical theories as abstract objects[*]

José Luis Falguera[**] and Xavier de Donato-Rodríguez[***]

University of Santiago de Compostela

0. Introduction

In 1971 appeared Joseph D. Sneed's seminal book *The logical structure of mathematical phys*ics. This date points to the birth of a new approach in philosophy of science, known as metatheoretical structuralism (henceforth MS). The book soon attracted the attention of Wolfgang Stegmüller, who already was a prestigious researcher in analytic philosophy and more specifically in the neopositivist philosophy of science. Stegmüller was then interested in the so-called 'Ramsey statement' (Ramsey 1929), and Sneed's book had two chapters related to the Ramsey view. Stegmüller realized the relevance of Sneed's book and he launched, from the Ludwig Maximilian University of Munich, a research program incorporating authors such as Wolfgang Balzer and C. Ulises Moulines. Along the seventies, eighties and nineties in the past century the four mentioned authors, and some others, published a great deal of papers and books developing MS, especially in English, German and Spanish languages. Some of the papers were published in journals of philosophy of science and analytic philosophy of high impact. The milestone of this approach, as it develops its most significant aspects, is the book entitled *An Architectonic for Science. The Structuralist Program,* co-authored by Balzer, Moulines and Sneed (1987).

In this paper we will try to show that, according to MS, an empirical theory is *a complex and very abstract object which has structures as its very elementary components*. Each of these structures can be considered (introduced) as a set-theoretic structure, in the sense of a possible realization or potential model of the Tarskian formal semantics. The idea of taking potential models as elementary structures to account for an empirical theory can be traced back to the Stanford

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[**] ORCID ID:orcid.org/0000-0003-2631-1563

[***] ORCID ID: orcid.org/0000-0002-8464-9960

School of philosophy of science (the most representative member of which was the recently deceased Patrick Suppes). Reconstructing actual empirical theories in this way resembles the method of Bourbaki's program for reconstructing mathematical theories, although of course the reconstruction of empirical theories requires considering components which are not present in mathematical theories.

In a few words, MS considers an empirical theory (in an idealized synchronic sense of an elementary empirical theory) to be a very abstract object given by different kinds of sets of models and a set of sets of models. In what follows we aim to provide an intuitive account of these different sets which constitute an elementary empirical theory according to MS. We will also examine and point out the differences between MS and the more classic and frequently considered approach, the so-called statement view, according to which an empirical theory is a group (or a set) of statements represented by some of them which express scientific laws taken as axioms of a deductive system (in other words, a set of propositions represented by some of them which are considered to be scientific laws and are taken as axioms of a deductive system). Given this, MS considers that each empirical theory has associated an empirical claim assessed as true/false (or adequate/inadequate or correct/incorrect, depending on the values that one assumes as the most appropriates). A consequence of this is that an empirical theory is not the kind of entity that can be true/false (adequate/inadequate, correct/incorrect), but only its associated empirical claim is what may be assessed in this sense.

Finally, we will also present a very schematic and intuitive idea of what is for MS a complex empirical theory. We will consider the nature of a theory from an idealized synchronic point of view (as exemplified by Classical Particle Mechanics, Genetic Theory, or Generative Grammar, when they are considered in an idealized synchronic view for a particular period of time), or from an idealized diachronic point of view attending to its development across time by a given scientific community, since the theory is authored until it is rejected (examples of this are the evolution of Classical Particle Mechanics and Simple Equilibrium Thermodynamics across time as considered in *An Architectonic*).

1. Origins and developments of MS

As it has already been mentioned, MS started its developments in 1971. That year J. D. Sneed published *The logical structure of mathematical physics*, which combines set theory axiomatization of physical theories with a peculiar version of the Ramsey treatment for theoretical terms. We can find the precedents of this semiformal analysis of empirical theories in Suppes' approach, as he used naïve set theory and models (the Tarskian structures inherited from Tarskian semantics).[1]

[1] See Suppes (1957,1960, 1962, 1967, 1969, 1970 and 1974).

Stegmüller, in 1973, published *Theorienstrukturen und Theoriendynamik*. In this contribution he presented Sneed's ideas in a more didactical way, together with a reconstruction of Kuhn's and Lakatos' proposals on scientific development. In fact, Stegmüller – a reputed German researcher in analytic philosophy and more specifically in the neopositivist philosophy of science[2] – had started a research project based on Sneed's proposal about the synchronic and diachronic structure of empirical theories, and the analysis of theoretical terms. C. U. Moulines and W. Balzer stood out among the collaborators in this project since the very beginning. Sneed himself was invited to Munich by Stegmüller in 1975 and spent some time there working with them in the project. It was precisely that year when Balzer introduced – in a formal way – the notion of *theory-element* as the most elementary notion of theory. In 1987, as a result of the aforementioned collaboration, Balzer, Moulines and Sneed published *An Architectonic for Science*, the most important and ambitious volume of MS, which provides an extended presentation of the MS' program as it was developed until then. In the meantime, an important number of papers as well as some books were published by the authors mentioned above and some others working *in* or *about* the MS' program.[3] Besides, since the end of the 80s and up to now the publications related to MS continued thanks to Balzer, Moulines, Sneed and some younger researchers based in Germany, Mexico, Argentina, Colombia and Spain. Although the program is probably less known now among the researchers of other countries,[4] it has been well appreciated by some relevant philosophers of science out of the MS' program such as T. S. Kuhn (1976), P. Feyerabend (1976) and N. Cartwright (2008).[5]

Let us point out that, in order to clarify several different problems in philosophy of science, MS is faced, in the first instance, with the question of elucidating *what an empirical (or factual) theory is*: i.e., what the identity of an empirical theory is; how an empirical theory can be satisfactorily characterized. Previously to the MS' answers to these questions, there have been other proposals. Let us see, in a simplified way, the main answers that are available in the literature. To discuss the kind of ontological consideration on an empirical theory which lies behind these proposals, we will start providing some ontological distinctions.

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[2] Previously, Stegmüller had published a very good presentation and analysis of neopositivist proposals on different kinds of theoretical terms (taxonomic, comparative and metric terms) and on the problem of the cognitive content and meaning of theoretical terms (see Stegmüller 1970).

[3] See Diederich, Ibarra and Mormann (1989).

[4] See Diederich, Ibarra and Mormann (1994), and Abreu, Lorenzano and Moulines (2013).

[5] Nancy Cartwright said: ''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). Cartwright was using the expression "The German structuralist" for what here is called "metatheoretical structuralism" (MS). It seems to us that the latter is a better denomination for the approach, given that its beginnings are to be found in the United States of America (see the above mentioned 1971's book by Sneed) and it has been developed in other countries out of Germany, though in this country it has had the most important influence.

2. Some ontological distinctions

Following a procedure such as that adopted by E. Zalta (1983 and 1989) we are going to assume that there are two different kinds of objects or entities: *concrete* and *abstract* (as disjunctive categories).

Concrete objects are objects in the actual world. Assuming that *Existence* is a property[6] just for these kind of objects, we can say that *concrete objects* can be of two different types: *individuals* and *properties*.[7] *Existence*, in this sense, is different of any existential or particular quantifier (of a determined level or type) applied to a variable (of the appropriate level or type); for example, $\exists x$ when it is used to determine the variable x in an open formula. Technically, using Zalta's difference between *exemplify* and *encode, exemplify* is the kind of connection between an actual object and a property appropriated for it; for example, yourself and being human: you *exemplify* the property of being human, where you and being human – as the latter is considered now – are both concrete objects, the first a particular and the second a property. In this sense, concrete objects of any level *exemplify* properties – of the appropriated immediate superior level – and, in particular, exemplify *Existence*, but never *encode* properties. When one object encodes a property is because the initial object is abstract; for example, 5 *encodes* being a natural number and being odd, Sherlock Holmes encodes being a detective. But any abstract object, in addition to encode one or more properties, *exemplifies* properties; for example, 5 *exemplifies* being a non-Existent, Sherlock Holmes *exemplifies* being a fictional character created by Conan Doyle.

Concrete objects include individuals and properties. First, we find *concrete individuals – or particulars* – (as the pen used by someone, the laptop of one of the authors of this paper, the chair that one of us is using for writing it, yourself – the reader –, etc.). Note that *some concrete individuals are complex*, because each of them is *composed by* other concrete individuals -with their concrete properties- of lower levels: a delimited real gas is (allegedly) composed by real molecules; you – the reader – are composed by a head, two arms, two legs, … Obviously, a complex concrete individual can be composed by different individuals that make up another bigger individual and might be themselves complex.

Then we also have *concrete properties* (as 'being black' for the shoes that one of the authors is using while writing this paper; or 'being a laptop' for the item the same author is using while writing this paper). It is not usual, in the philosophical tradition, to speak of concrete properties and distinguish them from abstract properties; in fact, many philosophers associate properties with universals and, hence, consider them as abstract. Following Zalta (1983 and 1988), we prefer to differentiate between concrete and abstract properties. Each concrete property is charac-

[6] In fact, there are several properties for *Existence*, each of a different level or type (types ≥ 1) according to the immediate inferior level or type of the objects to be applied.

[7] Properties, here, include relations of any kind and type.

terized by exemplifying the (appropriate type of) *Existence* property; hence each concrete property is a property in the actual world.

Abstract objects fail to exemplify *Existence*, because they are not in the actual world. An abstract object has to encode some property, and, as it has been said, it exemplifies some properties (clearly a lot of negative properties; for example, 5 exemplifies being a not-horse, and exemplifies being a not-building, etc.). We can say that also *abstract objects* can be of two different types: *individuals* or *properties.*

Abstract individuals – or particulars – (as '7' or 'π' –the numbers, not the numerals; any novel -not the printed copies- as *The Great Gatsby*, *Ulysses,* or *Lord of the Flies*, or fictional individuals as Sherlock Holmes, Don Quixote, or Mary Poppins). There also are some abstract individuals which are *complex*, because each of them is *composed by* abstract individuals of lower levels – with their properties encoded and exemplified; for example, an ideal gas is composed by ideal molecules; an ideal simple pendulum is composed by an ideal bob and a massless string. A complex abstract individual can be composed by other individuals (abstract or concrete) where all of them is of the same kind (for example, the set members of a family, the set of natural numbers), or of different kinds (for example, we will see that, according to MS, an empirical theory is a complex abstract object composed by individuals of different kinds).

Finally, we also find *abstract properties* (as 'being odd' for '7', 'being detective' for Sherlock Holmes; 'being a hobbit' for Bilbo Baggins, 'being an ideal pendulum' for an appropriate item). These examples are abstract properties in Zalta's sense of "encoding a property". Note that 'being detective' for a concrete real detective is a concrete property, not an abstract one, in Zalta's sense of 'exemplifying a property".

3. Main approaches of what a theory is, previous to MS

Let's look at some of the main proposals in the philosophical literature about what an empirical theory is and what ontological status it does have.

1. The traditional way to identify an empirical theory is to consider it as a list of (type) statements (i.e., assertive sentences of a language with their contents). This way to account for them has a long tradition and has been assumed by the classical views of philosophy of science since the 1920s with its institutionalization in some universities: neopositivism and Popperian school. But in fact, a large part of the metascientific literature of any time (until today) is assuming explicitly or tacitly this point of view on empirical theories. Particularly famous is the version given by neopositivists in the last proposals of this conception: the so-called 'standard view'. Sometimes (specifically in the case of neopositivists) it is considered that the list of statements (for an empirical theory) is

underlying a deductive axiomatic structure represented by some postulates (theoretical axioms + rules of correspondence) which could be paraphrased in a formal logico-mathematical language, mainly first-order logic extended with identity and mathematical expressions. Two aspects are behind this proposal: one, the consideration that an empirical theory has to respond to the ideal of a deductive axiomatic structure, which is to be traced back to the paradigm of Euclidean geometry in *The Elements* (more than 23 centuries ago) and that has been present as an ideal not only in the scientific projects of knowledge (mathematical or empirical) but also in philosophical and theological projects (think for example in the case of Spinoza); the other is the idea of paraphrasing a linguistic formulation of an empirical theory, after reconstructing its deductive axiomatic structure by using a formal logico-mathematical language. The most important issue is the axiomatic structure which each empirical theory would had to preserve. The paraphrase in a formal logico-mathematical language was considered interesting more for a rigorous and precise analysis of the theory in question than for scientific practice. Anyway, though it is true that in the first years of the neopositivist approach the formal logico-mathematical language considered appropriated for this task was one for the classical first order logic, later, and clearly since 1950s, given the limits of this formal language, there was accepted any other deductive formal logico-mathematical language which was necessary for a particular empirical theory (specifically for its theoretic part, because for the observational part a classical first order logic was considered enough): modal logics, second or higher-order logics, mathematical theories, …).[8] For Popper (1963/1972, ch. 3), empirical theories are also lists of statements with a deductive axiomatic structure, but he thought that the axiomatic structure of empirical theories and, hence the reconstruction of them, is not a relevant philosophical task because it easily entails feeding an instrumentalist interpretation of empirical theories which is opposed to his critical realism.

Ontologically, an empirical theory in this approach is, apparently, a concrete object or better several concrete objects which are instances of a list of type statements. Nevertheless, it could be considered an abstract entity because of its being a *type*; in any case, it is not clear what kind of abstract entity is a type. It is true that for many neopositivists assuming nominalist positions the only possibility would be to consider each empirical theory as a concrete object or as a collection of concrete objects. The problem for identifying an empirical theory with just a concrete object given by a list of statements is that it is usual to recognize different linguistic formulations, different list of statements, as *expressing the same theory*. The problem for considering it as a collection of several concrete objects, each of them given by a particular list of statements, is to establish the identification conditions for the different linguistic formulations. We

[8] See in this sense Carnap (1956).

don't reject that an alternative answer(s) is(are) available for the nominalist, but it seems to us that there is not at this moment a clearly satisfactory one.

Anyway, there have been neopositivists assuming abstract objects in some sense. The clearest position in this line is that of Carnap (1950), which accepts abstract objects for a linguistic framework, if the linguistic framework is pragmatically useful for some purpose and has linguistic expressions with a commitment to abstract objects. Nevertheless, for Carnap to accept abstract objects is not a metaphysical (or external) position in the sense of being committed to abstract objects as entities really existing in the actual world, but a mere internal acceptation in the context of a given linguistic framework. From this view, it would be possible to assume that an empirical theory is an abstract entity expressed by several lists of statements. Again, the problem is to establish the identity criteria among different lists of statements expressing the *same* empirical theory. Behind this issue there are problems related to how to ensure translations between two different linguistic formulations which are supposed to express the same theory. An alternative to translations, and to establish the common content, would be to consider some extensional way to determine the abstract object which is an empirical theory for any of its linguistic formulations. It seems to us that the semantic accounts of empirical theories (MS among them)[9] are answers in this direction, on which we will focus in what follows.

Popper (1972, ch. 3 and 4) would easily accept that an empirical theory is an abstract object expressed by different linguistic formulations, because he didn't adopt a nominalist position. In fact, he accepted that cultural products are abstract entities.

2. Since the 1950s some philosophers have defended that an empirical theory is a group of scientific models (in the limiting case: a scientific model). Some of them are Hutten (1954), Hesse (1960), Black (1962), Achinstein (1965, 1968), Suppe (1967), McMullin (1968), Harré (1970), Giere (1984, 1988, 1999), Cartwright (1983). Ontologically the question is to establish what kind of entity a scientific model is and what different kinds of entities there are for different kinds of scientific models. Attending to these considerations, the answer is not clear, as it becomes evident by looking to the different answers formulated by the philosophers mentioned in this paragraph, except in the case of Giere. In fact, this philosopher assumes that a scientific model is an abstract object. However, the problem of the ontological status of scientific models has recently been rethought in the literature, without any agreement on the issue. Some adopt a nominalist position (see Toon 2010, 2012) or apparently nominalist (see Godfrey-Smith 2006, 2009; Barberousse and Ludwig 2009; and Frigg 2010a, 2010b, 2010c), others adopt a realist position about scientific models as

[9] Besides MS, some other semantic accounts are: Stanford School of Philosophy of Science with Tarskian possible realizations and models, van Fraassen with models as state-spaces, Suppe with models as structures given by phase-spaces, etc.

abstract objects (see Giere 1984, 1988, 1999, 2004, 2009; Psillos 2011; and Donato and Falguera 2016), and there is even a philosopher that advocates for a dual ontological status, as an abstract object and a (possible) concrete object[10] for some kind of models which he calls fictional models (see Contessa 2010).

3. Kuhn (1970 and 1971), trying to clarify the notion of scientific paradigm, used the neologism of "disciplinary matrix". If we assume that "disciplinary matrix" is a new expression for empirical theory (as Kuhn himself said[11]), then an empirical theory is presented for him as an entity composed by some symbolic generalizations (with open content), categorical models (analogical models and ontological models[12]) and applications or problems considered to be successful (which are called "exemplars" by Kuhn). Hence, ontologically an empirical theory would be a mix entity for Kuhn: a complex entity which integrates different kinds of components, some of them clearly concrete as the symbolic generalizations and other which could be considered concrete or abstract. Nevertheless, it seems to us that the complex that an empirical theory is, in this account, has to be consider also an abstract object, and part of this is assumed by Kuhn when he explains that a disciplinary matrix is a matrix because it is composed of elements ordered in several ways (see Kuhn 1962/1970, pp. 182 and ff.). The idea of ordered elements entails that the complex is a structure where the order of the elements of which that complex is composed is important. And such a structure must be an abstract object.
4. The semantic tradition of philosophy of science comes to consider a theory as an extensional entity in terms of models (or related structures). In this regard, Stanford School –and specially Suppes (1967, 1969, 1970)–: a class of formal models; van Fraassen (1970, 1972, 1980): a class of state-spaces or a formal model; Suppe (1974, 1989): a class of phase-spaces; Giere (1979/1984, 1988, 1999, 2004, 2009); Dalla Chiara and Toraldo de Francia (1974, 1976); Przelecki (1969); Wojcicki (1977, 1999); Da Costa and French (1990, 2003); etc. We can integrate these different proposals by telling that a theory, in terms of the semantic tradition, is a class of (formal) structures (in the limiting case: a formal structure).

   It is particularly famous Suppes' slogan: *a theory is a class of models* (in the Tarskian sense of "model"). In order to account for the class of models which a theory is, Suppes defines it using a set-theoretic predicate giving the conditions

[10] Contessa (2010) speaks about possible concrete objects, where some of these are individual concrete objects in our sense, and hence actual objects, and other are mere possibilia.

[11] The rejection of Kuhn to use the expression "empirical theory" at that moment was due to the usual association at those times with the standard view of the empirical theories. (See Kuhn 1974, p. 500 and ff.)

[12] There is not any kind of realist view behind the speak of ontological models by Kuhn. He is speaking of ontological models for those that a scientific community takes seriously to represent parcels of the world. Though not because these parcels of the world are in fact as the corresponding models represent, but because these scientific models provide promising ways to solve the empirical problems considered and related with those parcels.

which the models of that class have to adapt to. The idea is that a specific linguistic formulation of an empirical theory is not the important thing; what should be regarded as important are the possible realizations (or potential models) that are governed by the conditions of the theory. For the Stanford School, these conditions are: (i) a type structure for the possible realizations with its type domains, type properties, and type relations (some of which can be type functions); (ii) formal characterizations for each of the concepts corresponding to the variables for the domains, properties and relations; (iii) some laws establishing the way that the concepts are restricted according to the theory. Structures which are governed by (i) and (ii) are the possible realizations (or potential models) [Rp or Mp] for that empirical theory [T]. Structures that also are governed by (iii) are the models [M] for [T], where $M \subseteq Mp$. According to the Stanford School, an empirical theory – and also a formal theory – T is a complex structure given by Mp (i.e., the possible structures or potential models which are appropriated to ask whether they are or they are not restricted by the laws of T or restrictions (iii)), and M (i.e., those possible structures or potential models which are in fact restricted by the laws of T or restrictions (iii) for T); that is, $T = \langle Mp, M \rangle$.

Suppes' characterization of an empirical theory is not one in which it is a concrete object, but an abstract one. But one thing is to say that a theory is an abstract object, and another is to say that, because of this, it must be a Platonic entity, that is, an eternal or atemporal entity existing in a sort of Platonic, non-spatial heaven. We saw that for Popper, at least some abstract objects could be cultural products. However cultural products have a beginning and, most probably, an end. Because of this, they are temporal. And they are temporal because they are authored or conceived by someone(s). It seems clear that a novel, an argument, a musical work, and many other cultural products are temporal in this sense. They depend on someone(s), but also on some way to express them (a text, an oral or written argumentation, a music sheet, …) and without the way to express one of them, it loses its objectivity, and hence it ceases to be an object, any kind of object.[13] Zalta (1983, 1988) analyses some of these kinds of abstract objects which are authored. And recently Thomasson (1999) has spoken about some of them – specifically for works of fiction and for fictional characters – calling them "abstract artifacts". Henceforth we will use this expression to refer to them.

Suppes' characterization of an empirical theory uses a (set theoretic) predicate to name the class of models which identifies that theory. According to this procedure, it would be easy to think that, although an empirical theory is not a list of statements, it could be identified with the intensional content of that predicate, and not with the class of models (or extension) for that predicate. In this case, that object is an intensional entity; if you want, *a concept* (or a *sense*, in

[13] Giere (2004, p. 747, n. 7) is advocating for an interpretation which is closer to that we are assuming here.

the Fregean usage of this term). This is not Suppes' account for an empirical theory (and neither of other characterizations in the semantic tradition). But in some way, it seems that the extensional characterization of an empirical theory requires of an *intensional* object, and that this one determines the class of models or extension for the predicate. In fact, the conditions which define the predicate could be considered that are the content, and so the theory itself. We want to insist that this is not Suppes' point of view, but perhaps it is not totally opposed to it. You can see that, if it were accepted, the intensional entity which we identify as an empirical theory would also be an abstract object. Anyway, if we are considering it, it is because it is closer to our point of view and to introduce it now may facilitate our comments below about the point of view we advocate for.

Nevertheless, coming back to the Stanford School account for an empirical theory we find that Suppes' proposal was criticized by Adams – one of Suppes' disciples – because it doesn't allow differentiating an empirical theory from a mathematical theory. Adams (1959) proposed that, in order to establish what an empirical theory is, it would be necessary to consider structures that are not determined by conditions of the kind before mentioned: (i), (ii) and (iii). The problem is that these, as considered in Suppes' proposal, are mere formal conditions –i.e., just providing set-theoretic and other mathematical conditions for the variables of scientific concepts, without any indication about the material content of those concepts. Regarding this, it is important to keep in mind that these formal conditions make it possible to consider, among the possible realizations (potential models) and even among the models, systems whose base domains are any set of entities –i.e., sets of abstract entities (such as mathematical entities)– in addition to these other systems formed by sets of concrete entities. In this way, the rest of conceptual variables of these possible realizations and models would not be sets of values for concrete entities. According to this, we have to incorporate to the formal characterization of systems for the theory a way to refer to the problems or empirical systems or intended applications the theory tries to account for, knowing that they are chosen by the scientific community working with that theory. Hence, they are not identified just thanks to conditions (i) and (ii). Nor, in fact, has to be assumed that they assure conditions of the kind (iii). Intended applications for an empirical theory are identified pragmatically (by the members of the scientific community). Anyway, from the point of view of Adams, the intended applications for an empirical theory stand for concrete systems which have to be conceived according to *all* the conceptual apparatus of that theory. This means that they have to be conceived according to conditions (i) and (ii) for that theory. With this in mind, an empirical theory would have three components: its potential models (Mp), its models (M) and its intended applications (I); i.e., T=⟨Mp, M, I⟩. Obviously, with this extensional identification of an empirical theory it is an abstract entity again, though even more complex than the one corresponding to the initial Stanford School's characterization. For contrasting an empirical theory, so understood, the important thing would be to show that $I \subseteq M$ is true, where $I \subseteq M$ is the empirical assertion for T; i.e., that all the set of intended applications is a subset of the models (intuitively, that the conditions of the theory account for all the intended applications). Note that, in this characterization of an empirical theory T, it is not an entity which can be true or false, but there would be a claim associated to T – the empirical assertion $I \subseteq M$ – which could be true or false. The empirical meaning of T would consist to inquire whether $I \subseteq M$ is true or false.

However, a new problem results of such a characterization of an empirical theory. It assures that the empirical theory so characterized results justified independently of the empirical systems it is created to account for (the self-justification problem);[14] so that, it is like a trick to assure the empirical assertion; i.e., that $I \subseteq M$. This was the reason why a new characterization in this line needed to be pursuit, if the problems wanted to be overcome. The solution came with MS, to whose characterization of an empirical theory we dedicate the next section.

4. MS' answer(s) on what an empirical theory is and the corresponding ontological status

We must start by saying that for MS an empirical theory must be considered from different perspectives. According to this, from the synchronic perspective it is possible to establish how it is statically; i.e., how it is when it is considered relatively to a moment of time of its development. It is also possible to characterize it in a diachronic perspective, and in this case with two options: one is kinematic, attending to the structure of change with the time; the other is dynamic, attending besides to the motivations, causes or forces of change across time.

In a static sense, it is possible to find less and more complex theories. For MS, the most elementary theory in the static sense is called "theory-element". A complex empirical theory in the static sense is conceived as a group of these "theory-elements" with a hierarchical relationship among them (like an inverted tree; see figure 1) from a basic theory-element, in such a way that any theory-element different from the basic one is maintaining a relation of specialization to some other theory-element, that is, to someone different in the group of the previous ones, and so on until we arrive at the basic theory-element. This complex theory is called "theory-tree". It is a relationship of specialization because going down in the theory-tree from the basic theory-element to a terminal theory-element each lower theory-element incorporates new conditions (particularly new scientific laws) in addition to the conditions of the upper theory-elements. In this sense, a theory-tree is

[14] See Díez and Moulines (1997, Ch. 10, Section 3).

not a deductive structure because the scientific laws of a lower theory-element are not deduced from the scientific laws of an upper theory-element in the theory-tree.

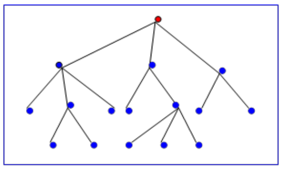

Fig. 1. THEORY-TREE
Red point = Basic theory-element.
Blue points = specialized theory-elements

In the static sense, it is normal to find different theory-trees which have connections among them thanks to bridge laws or intertheoretical laws. A group of these theory-trees is called a "theory-holon".

Standard MS in a diachronic perspective has only proposals for a kinematic consideration understood as a group of theory-trees with the same basic theory-element, each theory-tree representing a moment (or a period of time) in the development of the complex empirical theory, in such a way that it is changing some special theory-elements over time (sometimes appearing new special theory-elements over time, and in some occasion disappearing some special theory-element over time). This group of theory-trees is called "theory-evolution". So, intuitively, a theory-evolution is a changing or living theory-net, to use a metaphor purported by authors of *An Architectonic* (Balzer, Moulines, and Sneed 1987, 205).

The above introductory comments should be enough to understand that for MS the expression of "empirical theory" is a polysemic one. For the present purposes, we only need to see what MS considers a theory-element to be, because any other notion of empirical theory considered by MS is a complex of theory-elements. In this sense, it is good to return to the end of the latter section. There we say that Adams' characterization of an empirical theory was to a certain extend inadequate, but we also mentioned that there was a solution in terms of MS to the problems detected there. The presentation of how a theory-element is it will be fairly good in order to show how those problems are solved, and why any MS characterization of what an empirical theory is (in any of the mentioned perspectives, and for any complexity) is an ideal good proposal. Once this is admitted, it will be easy to show the ontological status of a theory-element, and then to extend the consideration to any more complex empirical theory corresponding to the kinds of theories distinguished by MS.

Adams' characterization of an empirical theory has the problem of self-justification, as we saw above. From the point of view of MS several new aspects must complement Adams' account in order to obtain a satisfactory characterization of the most elementary unity of empirical theory. First, given the characteristic concepts of each theory –i.e., these primitive descriptive concepts which are present in the scientific laws of the theory, it is important to distinguish between theoretical and non-theoretical concepts. This distinction is relative to each empirical theory (it is not absolute for all the concepts of science, or better for all the descriptive terms of science as in the neopositivist approach with the observational/theoretic distinction). The new distinction is based on a criterion related to the specificity of the concept for the theory in question. Because of this, if the empirical theory at issue is T, we distinguish between T-theoretical and T-non-theoretical terms. According to this distinction, it is possible to establish that, for each potential model for an empirical theory T, there exists just a substructure which has the T-non-theoretical concepts as its unique conceptual apparatus.[15] Each of these sub-structures of a potential model is called a "partial potential model"; and the class of them for T is called its "class of partial potential models", Mpp. It is a new component of an elementary empirical theory according to MS, which changes the way to understand the intended applications, with respect to Adams' view.

This new class for T, the class Mpp, allows us to consider the intended applications of T as systems conceived as members of Mpp; i.e., Mpp is the class of all possible intended applications, and I is a set of actual intended applications chosen by the appropriate scientific community (so that, I⊆Mpp; assuming that I is not a unitary set, that there are many intended applications for T). This novelty seems to entail that what has to be shown is not that all the intended applications are models – not that I⊆M – (what is impossible given that now I⊆Mpp, M⊆Mp, and any member of Mpp is a substructure of some member of Mp), but that there are theoretical expansions (one or more) of each of the intended applications – i.e., potential models (one or more) for each intended application – that satisfy the three kinds of conditions defining the class of models for T thanks to the set-theoretical predicate: (i) to be structures with the considered type structure; (ii) to be structures accommodated to all the characteristic concepts according their characterizations; (iii) to be structures which the scientific laws of T give account for. But this proposal is not less problematic than Adams' proposal. It is easy to find potential models which are theoretical expansions of each of the intended applications such that each of these potential models complains with the scientific laws for T. The

[15]Given a theory T, if $x_i=\langle D_1, \ldots, D_k, n_1, \ldots, n_m, t_1, \ldots, t_s\rangle$ is such that x_i is a potential model of T [$x_i \in$Mp] where $D_1, \ldots, D_k$, are its base domains, $n_1, \ldots, n_m$, are the sets of values for its T-non-theoretic conceptual variables, and $t_1, \ldots, t_s$ are the sets of values for its T-theoretic conceptual variables, then the result of cutting $t_1, \ldots, t_s$ of x_i is a partial potential model. For each potential model exists just a partial potential model as a result of omitting its T-theoretical sets of values. There exists a function ***r*** of cutting T-theoretical sets of values of each potential model of a theory T and gives us a partial potential model y_i of T, where $y_i=\langle D_1, \ldots, D_k, n_1, \ldots, n_m\rangle$; i.e., ***r*** is a function such that ***r***: (Mp)→(Mpp). The relation between a member of Mp and the correspondent member of Mpp is formal; it doesn't have to be understood as if the potential models for a theory were temporally previous than the partial potential models for that theory.

self-justification problem is not solved just because the intended applications are considered T-non-theoretical substructures of potential models.

However, that is not the only complementary aspect to Adams' characterization of (the most elementary) empirical theory according to MS. There are two further aspects which are essential. One, the second complement to Adams' proposal, is to understand that the different possible solutions for the problems which a theory aims to account for are not possible isolated solutions for scientific practice. So, they must be interconnected by some constraint-conditions (for example, if in two problems considered by Classical Mechanics there is the same particle it is supposed that the mass value for that particle has to be the same according to this theory; but not necessarily according to the Relativity Theory). This kind of constraints are nor usually explicit in the texts introducing an empirical theory, but they are known within the practice of the theory, though they are recognized explicitly for a theory when they are problematic for a rival theory. These constraints are conditions of compatibility between potential models of a theory or possible solutions of a theory for any possible intended application, and they select combinations of potential models. So that these constraint-conditions for an empirical theory determine a class of combinations of potential models (technically: if GC is this class for our T and $\wp(\mathrm{Mp})$ is the power-set for Mp, then $\mathrm{CG} \subseteq \wp(\mathrm{Mp})$ [such that $\varnothing \notin \mathrm{CG}$; $\mathrm{CG} \neq \varnothing$; and for all x if $x \in \mathrm{Mp}$ then $\{x\} \in \mathrm{GC}$]). Clearly, GC is another component for an elementary empirical theory according to the MS.

Finally, the other component, the third complement to Adams' proposal, is given by intertheoretical principles or bridge-laws that connect some possible solutions (or some potential models) of a theory with possible solutions (or potential models) of other theories. Bridge-laws for an elementary empirical theory, T, have to be considered as conditions allowing the selection of some potential models of T. Let us represent it by GL. This is the last component to introduce according to the MS.

Attending to the previous comments, an elementary empirical theory (T) according to MS – called a "theory-element"– is a complex object with the following components:

- a class of potential models (Mp): structures which are suitable to the conceptual apparatus [T-theoretical and T-non-theoretical concepts] of T.
- a class of models (M): these are potential models which satisfy the scientific laws of T.
- a class of partial potential models (Mpp): structures which are suitable just to the T-non-theoretical conceptual apparatus [T-non-theoretical concepts] of T.
- a global constraint (GC): a class of *admissible compatible combinations of potential models* of T.
- a global intertheoretical link (GL): a set of *potential models* of T, which are linked to potential models of other theories (by bridge-laws).
- and a set of intended applications (I): I is a set of intended applications; they are representations of –a certain way of carving conceptually up– parcels of the

world, such that the theory T aims to understand and explain, or tries to provide good predictions.

According to the considerations given above, we may conclude:

1. A theory-element is a structure of the kind: T=<Mp, M, Mpp, GC, GL, I>,
2. where Mp, M, Mpp, GC and GL are determined formally through different kinds of conditions and I is established pragmatically – not formally – by the scientific community using T.
3. It is a structure of sets of structures.
4. So that, it is *a very complex abstract object.*
5. It is an abstract individual (or abstract particular).
6. Obviously, other less elementary notions of empirical theory for MS, as the above mentioned, correspond to even more complex abstract entities, with theory-elements as their components.

As in the case of Suppes' approach, we can assume that an elementary empirical theory is an intensional entity (a content). It would be given by the different kinds of conditions which determine the formal extensional components of a theory-element, T, for MS, and for some way to refer to the intended applications of T. It seems to us that the usual way to refer to intended applications is by demonstrative propositions about paradigmatic problems (or exemplars, in Kuhn' sense). And these demonstrative propositions are expressed (explicitly or tacitly) in the (oral, written, etc.) presentations of the empirical theory at issue (for example, by expressions "*this is (an exemplar of) a kind of problems (or intended applications) for T*" [for a particular problem (or intended application)]). If this proposal is right, we can also say that an elementary empirical theory is, as an intensional entity, an abstract object which determines another abstract object, the extensional theory-element as it is proposed by MS. We can say that the second is the reference determined by the content, which is the first (in an internal sense of reference to a representational framework; assuming, in an extended way, the account of ontology given in Carnap 1950). Of course, this latter interpretation is not the standard one in MS. However, it seems to us that, at least, it is not against to it.

Anyway, adopting the standard proposal of MS for empirical theories or adopting the proposal given in the latter paragraph, an elementary empirical theory is *a very complex abstract object*. Now, we have to remember that empirical theories as abstract objects don't need to be Platonic, atemporal objects. As we said above, they can be understood as abstract artifacts in Thomasson's sense and in a way which we think can be easily captured by Zalta's theory of abstract objects (see Donato and Falguera 2016). This means that an elementary empirical theory is a temporal entity, but not a spatial object. Nevertheless, it is, as an artifact, depending on someone(s) which authored it, and, once it is authored, on some concrete object by means of which it is represented; i.e., it is depending on some material support. Obviously, this point of view is expanded to any empirical theory of any complexity. In this sense, an empirical theory is similar to novels, other stories and arguments. The difference has to do with its peculiar conditions, its intended applications and, of course, the use of the theory to account (to explain, to obtain predictions) for the parcels of the world conceived as intended applications.

5. Conclusions

The only alternative to consider empirical theories as abstract objects is to consider each of them as a list of statements. In any case, according to this view, in fact, it would be better considered each empirical theory as a list of type statements, and a type statement is an abstract object (assuming that a type is not a concrete object). If we are right, this would not be satisfactory solution for a nominalist. However, it has to be rejected, because in any case, intuitively, the same empirical theory can be expressed in different ways (i.e., by different lists of statements). For this reason, it seems to us that MS is probably the best way until now to characterize what an empirical theory is. According to this view, it is a very complex abstract, particular object, but an extensional one. Furthermore, if you prefer our interpretation, which we understood as compatible to a large degree with MS, an empirical theory is also a very complex abstract object, but an intensional one which determines the latter extensional one. The intensional one is a very complex content, namely the content established by the different kinds of conditions for an empirical theory and the pragmatic decision on its intended applications. This content determines <Mp, M, Mpp, GC, GL, I>, which a very complex and abstract extensional entity. In any case, an empirical theory is not an atemporal entity. It has a beginning -it is authored by one or more scientists-, and it probably has an ending. It is a peculiar abstract object: an abstract artifact.

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La structure modulaire des théories physiques

João Príncipe

Universidade de Évora, Instituto de História Contemporânea-CEHFCi

1. L'épistémologie et l'histoire des sciences dans le temps historique

Au cours du siècle qui assiste à l'essor de la révolution industrielle, la discipline appelée la physique connaît elle-même de larges développements, avec une prolifération de domaines et de sous-disciplines, un raffinement croissant de la physique mathématique et des artéfacts expérimentaux. Cela s'accompagne, surtout dans la seconde moitié du siècle, par une pluralité de réflexions épistémologiques. Alors que les philosophes rationalistes continuent la critique de l'empirisme, les savants-philosophes comme Maxwell, Helmholtz, Poincaré, Mach et Duhem, réfléchissent de manière plus informelle, mais plus proche des pratiques savantes, sur plusieurs questions telles que:

i) les processus par lesquels on produit et structure les théories (l'induction, l'analogie et la généralisation, le rôle des mathématiques, la construction du cadre spatiotemporel, la déduction des lois ou principes théoriques en partant de conditions générales de compréhensibilité incorporant un élément à priori, etc.)
ii) l'unité de la physique, le réductionnisme et la hiérarchisation interne (compte tenu de la prévalence de la mécanique et de l'avènement de la thermodynamique et électromagnétisme)
iii) le statut épistémique des théories et leur variation diachronique (le pluralisme théorique et la suspension du jugement, la distinction entre lois et principes, nominalisme versus réalisme, réductionnisme versus phénoménalisme)
iv) le rapport théorie-expérience, le problème de la mesure et la possibilité de vérifier une loi ou un principe théorique ou de réfuter une théorie (holisme).

Au XX^{e} siècle, avec l'avènement de la relativité et de la mécanique quantique, avec celui du logicisme et des tendances formalistes et avec la spécialisation professionnelle de la philosophie des sciences, l'importance de l'histoire de la physique pour la philosophie des sciences a été méprisée au moins jusqu'aux années 1960 (si on oublie le cas français) ; la philosophie analytique a eu un essor lequel s'est accompagné par un déclin des traditions rationalistes qui, inspirées de Leibniz et de Kant, valorisaient la notion de sujet épistémique et l'unité de la raison humaine (Brunschvicg et Cassirer étant probablement les dernières étoiles de cette constellation). Le contexte qui a vu la floraison du positivisme viennois, était marqué par la nouveauté radicale des (r)évolutions récentes des théories physiques ; on y perçoit un certain oubli moderniste pour l'histoire lequel trouva sa couverture idéologique dans la distinction entre le contexte de la découverte et le contexte de la justification ; cela allait de pair avec des problématiques associées au tournant linguistique qui a éliminé le sujet épistémique des discussions (les aspects liés à l'invention scientifique et à ce qui Kant nommait le 'régulateur'), aux travaux de Frege et de Russell et à une exigence de rigueur qui donnait priorité à une reconstruction rationnelle des théories et/ou à une vision très abstraite de celles-ci, laquelle méprisait le langage dite intuitif et lui substituait les énoncés écrits ou traduits en langage logique.

L'histoire est à nouveau placée au centre de la compréhension du changement scientifique avec Norwood Hanson et Thomas Kuhn. L'apport conceptuel des révolutions, l'analogie du gestalt-switch, l'incommensurabilité entre paradigmes et la distinction entre révolution et science normale renouvellent les débats épistémologiques en ouvrant une dimension tacite de l'activité scientifique et une dimension sociologique où l'intersubjectivité n'est plus le résultat d'une structure purement transcendantale commune aux savants ni d'une structure logique ; l'important rôle des exemples types qui illustrent et permettent l'apprentissage d'un paradigme défavorise la conception des théories comme des systèmes déductifs. Les approches postérieures à Kuhn, surtout celles inspirées du constructivisme social, valorisent le local et se méfient des grandes narrations, des structures globales et de la compréhension interne des idées scientifiques.

Au sein de la tradition formaliste en philosophie des sciences, sous l'influence de Frege (qui a souligné la distinction entre sens et référence) et de Tarski (qui a favorisé la conception sémantique des théories - une théorie étant comprise comme une classe de modèles qui constitue son référent ; le système d'axiomes constituant le sens), la tradition nommée structuraliste (associée à l'école de Stanford et à quelques philosophes continentaux), essaie d'intégrer quelques aspects de la pensée de Kuhn dans ses reconstructions rationnelles des structures théoriques, en valorisant des aspects 'pragmatiques' (la notion kuhnienne d'exemplaire) et en essayant de dépasser le modèle déductif (Hempel) : elle s'intéresse aux rapports interthéoriques et elle introduit des notions de termes T-non-théoriques (qui remplacent l'idée d'un langage observationnel neutre) de spécialisation d'une théorie et de réseaux de théories qui peuvent varier diachroniquement ; tout cela il faut l'avouer, en continuant un peu la tradition de la philosophie analytique et du positivisme viennois, en interagissant fort peu avec les historiens et poursuivant, en bonne partie, des problématiques externes à la pratique (et aux réflexions) des scientifiques concrets, situation assez favorisée par une réalité académique qui élimine les liens interdisciplinaires.[1]

On ne doit donc pas s'étonner que les recherches historiques plus récentes sur la physique des XIX^{e} et XX^{e} siècles permettent de jeter un nouveau regard sur

[1] Voir, dans ce volume, l'article de Falguera et de Donato Rodríguez.

quelques problèmes de la philosophie générale des sciences. Un excellent exemple de cette récente interaction entre histoire et philosophie est la réflexion d'Olivier Darrigol sur la structure modulaire des théories physiques, laquelle met les relations inter-théoriques au centre de la compréhension de l'articulation des théories et de leur rapport à l'expérimentation. Dans cette communication, je vous propose un esquisse de lecture de cette réflexion ; cette lecture met probablement en évidence mon penchant néo-kantien lequel se manifestera dans quelques commentaires ; j'ajouterai quelques réflexions concernant un possible lien avec les conceptions des adeptes du structuralisme métathéorique, lesquels ont collaboré dans notre symposium.

2. L'articulation d'une théorie illustrée par l'histoire de l'hydrodynamique

La vision hypothético-déductive de l'explication scientifique est en harmonie avec la perspective fondationnaliste des théories physiques, avec l'idée de leur hiérarchisation, et avec l'idéal réductionniste lequel est intimement lié à l'idée de progrès scientifique.[2] Les théories phénoménologiques sont idéalement susceptibles d'être réduites à des théories plus fondamentales, la thermodynamique étant expliquée par la mécanique statistique, la mécanique classique par la mécanique quantique, etc. L'articulation d'une théorie, qui correspond à la science normale de Kuhn, peut être décrite par une marche déductive idéalisée, ayant par point de départ les lois générales de la théorie et son formalisme. L'archétype est donc donné par la «théorie physique à part entière, avec une structure hypothético-déductive serré et un grand domaine, d'application, à multiples facettes» (Darrigol 2013, 34); l'existence de modèles et de théories ouvertes ne correspond qu'à des stratégies provisoires.[3]

Signalons, avec un peu d'ironie socratique, que si la méthode transcendantale de Kant établit qu'on doit partir du *factum* en remontant à ses conditions de possibilité, la vérité historique est que le *factum*, présenté par l'histoire de la physique qui tient compte des possibilités réelles qui se manifestent dans la construction théorique, montre que l'idéal déductif-nomologique ne systématise pas correctement la variété de la pratique théorique effective.

Une discipline dont le développement diffère radicalement du modèle néo-Hempélien est l'hydrodynamique, laquelle a été le sujet de l'ouvrage *Worlds of Flow* (2005). Après cet ouvrage, Olivier Darrigol a publié l'étude épistémologique

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[2] Le progrès qui résulte de l'invention de nouvelles théories ne connaît que deux modes : la généralisation avec réduction de la plus vieille théorie ou le remplacement tout cours, qui n'est présent que dans les étapes initiales des sciences.

[3] L'accent mis sur les révolutions scientifiques de la relativité et la mécanique quantique a favorisé ce point de vue.

'Pour une philosophie de l'hydrodynamique' (2013); dans ce texte, le point de départ est un récit historique abrégé qui se concentre sur les processus spécifiques par lesquels l'hydrodynamique s'est élaborée. La loi la plus générale du mouvement des fluides, l'équation de Navier-Stokes, a été créée vers 1830, mais l'hydrodynamique n'a pas avancé en cherchant simplement les solutions de cette équation non-linéaire aux dérivées partielles avec des conditions aux limites données. Le processus a été beaucoup plus tortueux – on a bâti une variété de spécialisations de l'équation générale - fluide incompressible non-visqueux avec écoulement laminaire, des ondes liquides dans une approximation linéaire de l'équation du mouvement, l'étude des mouvements tourbillonnaires, étude de l'instabilité des solutions, étude de la turbulence et l'étude des couches limites.

L'appareil explicatif sophistiqué qui s'est constitué a été le résultat de diverses stratégies et méthodes heuristiques, parmi lesquels se trouvent:

i) L'application directe de méthodes mathématiques, l'intégration des équations qui résultent de la spécialisation de l'équation générale, l'étude des symétries des équations de Navier-Stokes, et l'utilisation de méthodes d'approximation.
ii) des heuristiques plus créatives et risquées qui font appel à des intuitions de nature personnelle et plus sujettes à des erreurs et impliquant la vérification a posteriori de la compatibilité de ses conclusions avec les équations générales; ces stratégies souvent impliquent l'importation de concepts et méthodes d'autres théories de la physique.[4]
iii) formation de concepts en partant de suggestions faites à partir d'observations et d'expériences - ceci montre l'incapacité historique d'articuler la théorie à partir de ses fondements théoriques.
iv) La complexité des écoulements observés et l'incapacité de résoudre l'équation de Navier-Stokes a parfois conduit à la construction de modèles spécifiques qui consciemment simplifiaient radicalement certains aspects des écoulements – quelques-unes des hypothèses de ces modèles violaient les propriétés générales de la théorie.

Ces développements ont été conduits par la nécessité de réaliser des applications spécifiques (Darrigol 2013, 27).[5]

Le besoin d'efficacité cognitive, qui traduit pragmatiquement la notion de progrès, confronté à l'incapacité de traiter l'équation de Navier-Stokes dans toute sa généralité, en trouvant des solutions pour des conditions aux limites données, a activé la capacité de construire la description d'un système en éliminant ce qui peut être considéré comme (plausiblement) des détails sans importance pour l'analyse des situations physiques concrètes; dans cette voie, on a élaboré des spécialisa-

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[4] Mais aussi, dans environ la moitié des cas, des concepts forgés par des hydrodynamiciens ont été postérieurement utilisés dans d'autres théories.

[5] «Some kind of flow frequently observed in nature needed to be explained or the functioning of some instruments or devices needed to be understood», (Darrigol 2013, 27).

tions en fonction du type de l'écoulement, spécialisations pour lesquelles ont été forgé des concepts appropriés.[6]

En résumé, le cas d'hydrodynamique illustre la nécessité d'étendre le concept d'explication, afin de refléter la pluralité des stratégies d'articulation d'une théorie. Si on veut harmoniser la philosophie des sciences avec stratégies les plus réussies des savants, il faut admettre que l'unité de la physique est le résultat non du développement d'un idéal euclidien mais plutôt d'une ruse de la raison,[7] c'est-à-dire d'un parcours sinueux.

Un des invariants historiques de ce parcours, lequel Olivier Darrigol met en relief, est la présence d'une structure fonctionnelle modulaire des théories, laquelle se manifeste dans leur application, leur construction, leur comparaison et communication. Le cas de l'histoire de l'hydrodynamique, lequel a sûrement motivé l'analyse proposé par l'auteur dans son article de 2008, correspond à un cas où « le progrès dans la compréhension et application de la théorie a été le résultat de l'invention graduelle de sa structure modulaire » (Darrigol 2008, p. 198).

Commençons donc par une caractérisation générique des théories et des fonctions modulaires, le module d'une théorie étant lui-même une partie organique d'une théorie.[8]

3. Une brève conceptualisation d'une théorie physique

Une théorie scientifique est une structure de haut niveau qui systématise et unifie des régularités empiriques significatives pour les humains historiques – elle est le sommet et l'archétype de l'explication scientifique dès la Grande Révolution scientifique de Galilée, Kepler et Newton. Les grandes théories physiques ont manifesté une certaine stabilité des fondements pendant leur processus d'articulation ; si on définit la structure comme un mode stable et élaboré d'organisation on peut dire qu'une théorie physique est une structure symbolique

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[6] Si on peut s'exprimer métaphoriquement en termes fichtéens, le cas de l'hydrodynamique rend particulièrement clair comment l'auto-imposition de limites (dans le rapport originaire entre le Moi et le Non-Moi) est la stratégie du Moi absolu pour avancer dans la connaissance.

[7] La pratique concrète semble être opposée au but global. Pour unir il faut d'abord séparer, voir fragmenter, assouplir le déductivisme et le réductionnisme ; la ruse de la raison historique a été méditée par Kant, à propos du cosmopolitisme, et par Hegel. Chez Kant, une des idées qui illustrent la ruse de la raison, et laquelle semble contraire à l'esprit d'un kantisme strict, est celle de que la raison doit acquérir historiquement des nouvelles formes successives : voir (d'Hondt 1996, 186).

[8] Voir prochaine note – 'partie organique d'une théorie' signifie qu'elle porte en soi les caractères essentiaux d'une structure.

(réussie) qui est utilisée comme une stratégie humaine pour l'explication, la prédiction et le contrôle des phénomènes naturels de base.[9]

Une théorie est le résultat de l'exercice de l'esprit humain au niveau plus élaboré auquel il travaille et sa construction et sa transmission supposent ce travail mental très élaboré. Comme l'enseignent Aristote et les scholastiques, et aussi Brentano et Meinong, nos actes mentaux impliquent une intention, ils sont dirigés vers un objet, lequel a donc aussi un sens non-linguistique; l'objet que les actes mentaux créent ou visent n'est pas forcément extérieur et individuel; il peut être dépourvu d'existence dans un sens matériel ou de cohérence logique; la présentation à l'esprit d'un objet ne signifie pas un jugement sur sa vérité ou sa fausseté, il peut se présenter comme une hypothèse - *Annahme* - et être accompagné par la suspension du jugement ou d'un statut de *als ob*, c'est-à-dire de fiction; il y a des types très divers d'objets qui se présentent à l'esprit ou qui résultent de l'activité de l'esprit et qui peuvent venir à la conscience et être conceptualisés; certains sont des choses individuelles et localisées, mais d'autres sont des opérations à caractère non-figuratif, certains sont les relations entre des objets, des situations et des motifs complexes, des suggestions vagues mais aussi des maximes, etc.[10]

Une théorie physique est, sur le plan linguistique, un exercice exquis lequel présuppose l'écriture, la capacité à être moulé en texte dans un mélange de langage naturel (une langue comme le français) et de langage mathématique; contrairement à un poème ou à un roman, elle peut être réécrite de différentes façons et par différents auteurs, sa structure essentielle étant conservée; cela rend plus raisonnable de supposer que sa compréhension et sa transmission implique un type particulier d'intersubjectivité lequel rappelle l'existence d'un sujet épistémique, existence sur laquelle Piaget a beaucoup insisté.

Par un exercice d'abstraction, une théorie peut être décrite et analysée comme étant une formation discursive autonome et refermée sur elle-même et ayant un caractère relativement statique. Mais une théorie est aussi un processus de construction complexe lequel mobilise une variété de capacités et de structures non-linguistiques. Beaucoup de concepts théoriques font appel à notre structure per-

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[9] Jean Piaget attribue aux structures considérées en général des caractéristiques qui sont partagées par les grandes théories – totalité, les lois permettant d'assimiler amples classes de phénomènes, dynamisme (lequel se manifeste dans le devenir de l'articulation de la théorie) et d'autoréglage vu que la théorie se développe sans sortir du cadre imposée par ses propres lois, (Piaget 1969, 17-19). La possibilité de décrire une théorie en faisant abstraction de son évolution diachronique est le résultat de son auto-réglage associée à la permanence des lois et de définitions de base, par exemple sur le type potentiel et général de systèmes qui leur sont associés. Cette stabilité ne semble pas être aisément assimilé au caractère non-contradictoire d'un ensemble d'axiomes ; des systèmes assez fragmentaires (par exemple les principes de l'équilibre du levier) peuvent être formalisés, d'après la logique des ensembles, d'une manière qui conduit à une impasse dynamique et qui cache les rapports avec des principes généraux, voir, par exemple la formalisation ou modèle des principes du levier dans (Andreas et Zenker 2013).

[10] L'idée, d'origine platonicienne, d'une échelle ou escalier de Formes, sur laquelle ont écrit Hermann Weyl et António Sérgio, est une variante de ces méditations. Sur Meinong voir : (Courtenay 2010).

ceptuelle, à des schématismes (Kant), au langage naturel purifié par des préoccupations de rigueur logique, et aussi à des systèmes catégoriaux inspirés par des principes métaphysiques généraux (par exemple, le variable doit être expliqué à partir de l'invariant ou, souvenons-nous, les principes leibniziens de raison suffisante, de continuité et de l'harmonie préétablie); le langage exprime, traduit et façonne la vie mentale dans laquelle se présentent aussi des objets souples et très élaborés, parmi lesquels on trouve des principes actifs et générateurs; en quelque sorte cela a été dit par Kant en posant le synthétique a priori; cela a aussi été illustré par les réflexions de Poincaré sur le principe de l'induction et sur le concept de groupe mathématique de transformations.[11]

Ayant à l'esprit toute cette richesse de niveaux, prenons comme point de départ la caractérisation d'une théorie physique proposée par Olivier Darrigol, qui identifie des composants fonctionnels clés.

Une théorie physique présuppose un univers symbolique de base; il comprend d'abord la scène sur laquelle les phénomènes sont représentés à travers de concepts, lesquels doivent avoir une traduction mathématique;[12] parmi les grandeurs associées à ces concepts, quelques-unes doivent être mesurables.

Parmi les virtualités utiles de cet univers on a la possibilité de définir les individus, leurs configurations et leurs interactions, ainsi que leur évolution (mouvement, transformation). Aux entités postulées, dont certaines peuvent être inobservables, correspondent des faisceaux stables de quantités mathématiques, lesquelles surgissent dans les relations fonctionnelles ou statistiques qui représentent les lois de la théorie.[13] Pour une partie de ces quantités des processus de mesure idéale

[11] Le caractère ontologique des objets de la perception et/ou des entités et lois posées par des systèmes catégoriaux et par des principes métaphysiques, est question de croyance, et varie avec le 'goût' du sage (qui leur associe des convictions variées: réalisme, nominalisme, suspension du jugement). Le système catégoriel implicite dans l'usage d'une théorie physique comprend toujours les catégories de substance (ou invariable) et attribut, celles d'individu, de relation et de système individuel, lesquelles sont associées, avec beaucoup de souplesse, à des représentations mathématiques. Contrairement à la tendance croissante de la sophistication des outils mathématiques, les systèmes catégoriaux, comme les langues naturelles, sont plus durables (et restent invisibles/tacites pour les scientifiques) ; leur importance structurelle n'émergeant que dans les temps révolutionnaires (ou dans les analyses des philosophes). Cette richesse et complexité d'aspects et de niveaux, met en cause l'adéquation, la question de la référence au sens de Frege, d'une reconstruction rationnelle des théories sous la base de l'approche de la théorie naïve des ensembles, ou de l'approche inspiré de la notion Bourbakiste de structure. En particulier le rôle de principes heuristiques et/ou régulateurs, au sens kantien, difficilement formalisables, peut être méprisé.

[12] On suppose que les phénomènes (perçus par l'observation directe ou par l'intermédiaire d'instruments) sont la manifestation de situations, des évolutions ou des changements du système, dont la constitution peut contenir des inobservables à part entière.

[13] Ainsi, un intérêt particulier est conféré à l'invention et à la formalisation de la notion de fonction, au calcul infinitésimal, aux différents types d'espaces qui généralisent l'espace physique à trois dimensions de la géométrie analytique - espaces à n dimensions, espaces de fonctions et de fonctions de fonctions, des espaces topologiques mais aussi structures algébriques (groupes,

sont connus. La postulation d'entités (des individus, des systèmes) qui sont soumises à des lois permet une compréhension unitaire et causale des processus et des équilibres admissibles. La causalité revient à une structure légaliforme, dont le concept évolue au fil du temps historique - de l'hypothèse d'un déterminisme strict, dont le paradigme est l'équation différentielle avec des solutions univoques et stables, jusqu'aux systèmes plus chaotiques ou gérées par de principes statistiques, qui demandent des considérations sur la stabilité et l'approximation.

Parmi les systèmes considérés par une théorie, un certain sous-ensemble - que Darrigol appelé schèmes interprétatifs - permet de générer des plans directeurs (*blueprints*) d'expériences idéales; ce sont les schémes interprétatifs qui fournissent le pont entre la théorie et l'expérience et, en même temps aident à définir le domaine concret d'application d'une théorie :

> [Un schème interprétatif] comprend un système donné de l'univers symbolique, ainsi que d'une liste des grandeurs ou quantités caractéristiques qui satisfont les trois propriétés suivantes. (1) elles sont choisies parmi ou dérivées des quantités (symboliques) qui définissent l'état de ce système. (2) Au moins pour certaines d'entre elles, des procédures de mesure idéales sont connues. (3) Les lois de l'univers symbolique impliquent des relations de nature fonctionnelle ou statistique parmi elles. En général, les grandeurs schématiques ne représentent qu'une infime fraction de ce qui pourrait être conçu dans l'univers symbolique. .... Les schèmes interprétatifs ne sont pas formés automatiquement à partir de l'univers symbolique. Les lois de l'univers symbolique en eux-mêmes ne déterminent pas le processus de filtrage qui donne les schèmes. Aussi, l'univers symbolique n'impose pas la distinction entre les quantités mesurées et théoriques. En général, la classe des schèmes interprétatifs augmente dans le temps ainsi que la gamme d'applications de la théorie (Darrigol 2008, 202) ou (Darrigol 2014, 350-351).

Les méthodes d'approximation et les considérations de stabilité sont particulièrement importantes pour juger les implications des lois sur les schèmes interprétatifs (Darrigol 2014, 248). Une bonne partie de ces schèmes illustre la pertinence empirique de la théorie, puisque les applications de la théorie sont basées sur des schèmes concrètement réalisables ; mais une partie de ces schèmes sont purement imaginaires, permettant la réalisation d'expériences mentales (Darrigol 2008, 199). La réalisation concrète d'un schème interprétatif s'appuie en général sur des actes de mesure et mobilise un savoir-faire avec une composante tacite ; elle est dirigée par le schème interprétatif (qui fonctionne peut-être comme un archétype ou un modèle au sens où Max Black en parle[14]), lequel peut lui-même avoir une structure modulaire, en appelant à des modules élémentaires qui finissent par avoir un rapport stable avec des structures de la perception.

Avec le développement dans le temps historique, la classe des schèmes interprétatifs d'une théorie augmente au fur et à mesure que les applications de la théorie augmentent ; quelquefois une partie des quantités ou des entités qui restait purement symboliques acquièrent une figure schématique, devenant des 'êtres

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treillis, corps, etc.); dès l'époque de Galilée les mathématiques intéressant la physique mathématique ont eu un énorme essor.

[14] Voir 'Models and Archetypes', chap. 13 de (Black 1962).

d'expérience' : c'est le cas des électrons qui étaient des entités symboliques dans la théorie de Hendrik Lorentz au moment où on a interprété les rayons cathodiques comme étant des électrons en mouvement (Darrigol 2008, 203).

4. La fonction modulaire[15]

Comme nous l'avons vu dans le cas de l'hydrodynamique, le développement de la théorie a été fait par le développement de sa structure modulaire, par construction successive de modules spécialisés, qui sont eux-mêmes des théories avec un domaine plus restreint. Dans la formation d'une théorie on aperçoit aussi l'existence de modules, qu'on peut nommer modules de définition ; ceux-ci proviennent d'autres théories préalables qui aident à définir son univers symbolique et son horizon conceptuel, un processus d'importation plus ou moins directe de structures de base. Ainsi la géométrie euclidienne, considérée en tant que géométrie physique, fixe la scène des phénomènes spatiaux et permet de se figurer des processus idéaux de mesure des distances et fournit en même temps la notion de solides idéaux; toutes les théories de la physique dite classique incorporent la géométrie euclidienne comme un module.

Un deuxième exemple : puisque les notions de pression et de travail proviennent de la mécanique, la thermodynamique a un module mécanique avec une fonction de définition; la même chose est vraie de la théorie cinétique et de la mécanique statistique. Cependant dans le cas de la thermodynamique, laquelle est une théorie phénoménologique, le module de mécanique permet la schématisation interprétative, alors que dans le cas de la mécanique statistique le module mécanique sert à construire l'univers symbolique mais il n'est pas sensé d'être accessible par voie expérimentale. Un module ayant fonction de définition, pour parler la langue de Kant, a une fonction constitutive.

Considérons un autre rapport modulaire. Si on considère l'histoire de la théorie cinétique des gaz et de la mécanique statistique de Maxwell-Boltzmann, celles-ci ont été proposées comme permettant la réduction de la thermodynamique à une autre théorie plus fondamentale – en effet, la thermodynamique est un module approximant de la mécanique statistique ; la mécanique statistique est postérieure à la thermodynamique, et elle sert à préciser les limites de validité de la thermodynamique. Dans ces cas, un paramètre caractérisant une certaine classe de schèmes interprétatifs peut devenir très grand ou très petit de manière que les rapports entre quantités schématiques peuvent être déduites d'une autre théorie plus simple (le module approximant). Un autre exemple : la mécanique dans les théories de l'éther mécanique du XIXe siècle est un module réducteur de ces dernières, car la mécanique y est détournée de son domaine original pour être utilisée dans la cons-

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[15] Voir la section 'modules defined', (Darrigol 2014, 351-354).

truction de l'univers symbolique des théories de l'éther élastique.[16] Enfin, les manuels universitaires de physique exploitent depuis longtemps une des relations modulaires les plus communes et importantes – pour des classes choisies de schèmes interprétatifs, sous des conditions particulières imposées aux quantités caractéristiques, surgissent des modules spécialisés – songeons aux chapitres de électromagnétisme, en commençant par l'électrostatique.

Les modules permettent de saisir la structure et l'articulation entre symboles, schèmes et expériences. Olivier Darrigol a identifié ou moins cinq fonctions modulaires et a donné des exemples de structure modulaire dans des théories à part entière, dans des théories ouvertes (cas des premières théories quantiques, par exemple celle de l'atome de Bohr) et dans des modèles (que veulent prévoir le comportement de systèmes complexes, comme dans le cas de l'atmosphère).

Dans un élan généralisateur, nous pouvons admettre comme méthode d'analyse de l'état et de l'évolution d'une théorie physique la détermination exhaustive des théories B qui sont des modules fonctionnels d'une théorie donnée A. Dans une présentation vaguement formalisée, nous dirons que la caractérisation de la structure modulaire d'une théorie A est faite par une détermination de tous les triplets (B M_i A) signifiant que la théorie B est un module fonctionnel du type i de la théorie A ; cette détermination n'est pas purement logique mais est historiquement inspirée. La nature constructive/génératrice de ces structures est clairement indiquée dans l´énoncé suivant: il y a (aura) des théories B et des rapports modulaires du type i tels que la théorie A se développe en intégrant (en créant) les Bs ou les parties organiques de B lesquelles ont la fonction modulaire du type i par rapport à la théorie A. Il est raisonnable d'admettre que les rapports modulaires M_i sont des relations structurelles fondamentales existant en petit nombre (et que les ensembles des A et des B ont une petite cardinalité). Mais ce dernier petit nombre est assez grand pour engendrer une structure globale assez différente d'une taxonomie, avec sa typique structure ramifiée. Le changement temporel de ces systèmes de rapports, en particulier l'articulation modulaire croissante des théories majeures, représente l'évolution structurelle de la physique.

Le rôle que le module B joue par rapport à A est défini par l'analyse historique en tenant compte des pratiques et des jugements des acteurs historiques qui construisent ou critiquent les théories; ce critère méthodologique qui apporte à l'analyse épistémologique une dimension de pluralisme et de diachronie, ne doit pas être confondu avec une position relativiste du type "everything goes".[17]

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[16] Les rapports entre mécanique quantique et mécanique classique sont plus compliqués car si la mécanique classique est un module approximant de la mécanique quantique, la première est, d'après Bohr, indispensable pour définir les schémas interprétatifs de la mécanique quantique.

[17] Si nous nous limitons à des paires (BA) vérifiant le lien ternaire ci-dessus, il convient de souligner que ces relations binaires ont un caractère souple et peu formalisé - elles ne sont pas des relations réflexives, c'est-à-dire - il est faux et sans intérêt, que (A Mi A) . Dans le cas le plus courant, elles ne sont pas des relations symétriques - si (A Mi B), alors il est faux que (B Mi A) ; cependant il peut arriver qu'à un moment de l'histoire une relation et la relation symétrique

5. La modularité des théories et les problématiques de la philosophie générale des sciences

L'article de 2008 jette une nouvelle lumière sur un ensemble de questions générales qui sont associées à la topique, aux questions centrales, de nature générale, de la philosophie des sciences depuis l'époque des savants-philosophes comme Maxwell, Mach, Poincaré, Duhem, en passant par la glorieuse époque des débats entre Popper, Kuhn, Lakatos et Feyerabend, et en allant jusqu'à l'époque où une vision plus fragmenté des théories a été thématisée (citons l'ouvrage de Nancy Cartwright *How the laws of physics lie* et les travaux sur la priorité de la modélisation). Ces questions ont en bonne partie motivée la réflexion d'Olivier Darrigol et il propose des réponses novatrices.

Ici, j'esquisserai quelques unes des réponses en considérant les questions suivantes : i) le pluralisme épistémologique et théorique, ii) le rôle de la physique mathématique, iii) les questions inspirées par les réflexions de Pierre Duhem sur (l'absence) les expériences cruciales et sur le holisme qui empêche de falsifier, par voie expérimentale, une théorie ; iv) la comparaison entre théories en compétition et la question de l'incommensurabilité, v) les théories et les modèles.

i) La structure modulaire d'une théorie ne peut pas être identifiée comme étant a priori unique à un certain moment historique, car elle dépend de la conception des théories par les savants en situation, le regard rétrospectif de l'historien pouvant identifier plusieurs rapports interthéoriques alternatifs et synchroniques – par exemple, à la fin du XIX[e] siècle, le réductionnisme mécaniste a été remplacé, chez certains savants, par l'idée de réduction de la mécanique à l'électromagnétisme (Kuhn, 1978), (Darrigol 2008, 205). L'articulation de la structure modulaire d'une théorie peut être un processus relativement long et le pluralisme théorique (Maxwell, Poincaré, Boltzmann) est une attitude défendable pendant les phases de cette articulation (Darrigol 2008, 213). L'exploration d'une variété de différents univers symboliques a des avantages heuristiques et la communication entre sous-cultures de savants, ayant des engagements thématiques divers (Holton), devient possible par le fait qu'elles partagent quelques modules et par le fait de l'existence de savants pratiquant la traduction entre théories (Poincaré), (Darrigol 2008, 213).

ii) La construction de l'appareil interprétatif de la théorie présuppose et mobilise l'outillage de la composante symbolique, laquelle est essentiellement mathématique (l'Univers symbolique se fait en partant de produits cartésiens de l'ensemble des nombres réels ou des imaginaires et des espaces de fonctions qui peuvent leur être associés); donc, comme l'affirmaient déjà Maxwell, Poincaré et, de manière très frappante, Duhem les théories donnent un rapport médiat avec le monde de la perception, ou si on veut avec le monde réel des sens –

coexistent : si la relation considérée est celle de réduction inter-théorique, il peut y avoir compétition entre différentes visions réductionnistes.

les théories sont des exemples de notre création de formes symboliques (Cassirer) ; le caractère médiat et symbolique des schèmes interprétatifs résulte de l'omniprésence des mathématiques. Se sont celles-ci qui sont utilisées dans la comparaison des théories, dans l'obtention de modules approximants et de modules réducteurs. La compréhension historique et philosophique des théories n'a pas besoin, en général, de reconstructions logiques très élaborées (leur écriture en langage formelle) et les tendances axiomatiques qui se sont manifestées en physique mathématique (Hilbert) ne le semblent exiger non plus.

iii) Les lois physiques, peuvent être classées en trois types : les lois fondamentales dont la validité est absolue pour les systèmes de l'univers symbolique ; les lois empiriques qui résultent d'une généralisation de données numériques obtenus par des expériences qui réalisent concrètement une classe de schèmes interprétatifs ; les lois phénoménologiques qui sont le résultat de l'application des lois fondamentales à des classes de schèmes interprétatifs (la déduction de ces lois en partant de la théorie, exhibe leur caractère exact ou approchée – cas de la loi du pendule pour des petits angles d'oscillation) ; dans une théorie phénoménologique toutes les lois fondamentales sont aussi des lois phénoménologiques, ces lois établissant des rapports entre des quantités schématiques ; il faut signaler que les lois phénoménologiques qui sont vraies dans le cadre de l'univers symbolique de la théorie ne peuvent pas, en soi et de manière indépendante, être considérées 'vraies' mais tout simplement 'adéquates', puisque les expériences mobilisent des schèmes, des modules, des sous-modules et tout une connaissance pratique (know-how) de nature tacite. Cela est encore plus vrai pour le cas des lois fondamentales des théories non-phénoménologiques, dans lesquels des quantités et des entités sont postulées sans être schématisées ; cependant l'histoire montre comment le progrès théorique s'est fait en partant de ce genre de théories par un processus de purification de l'univers symbolique et de schématisation d'entités et de quantités inobservables, comme le montre la réussite des théories atomiques au tournant du XX[e] siècle, moment historique où des adeptes de théories phénoménologiques, comme Ostwald ou Duhem, voulaient bannir ce genre de théories (Darrigol 2008, 218-219).

Dans l'application des théories, les schèmes interprétatifs et leur réalisation concrète, enveloppant en général des mesures, profitent de la structure modulaire, mais mobilisent aussi des théories externes et des connaissances non-théoriques (Darrigol 2008, 207). Un résultat expérimental négatif, disons une prétendue réfutation expérimentale, peut être accommodé en adaptant la ceinture de protection d'une théorie (Lakatos); mais la structure modulaire impose des restrictions très sévères à ces stratégies car elle restreint la forme des schèmes, lesquels utilisent, plus ou moins tacitement, des composants modulaires bien connues et établies. La comparaison entre théories en compétition peut être faite par des expériences cruciales, il suffit que les deux théories partagent les mêmes modules qui permettent de construire des schèmes interprétatifs partagés (Darrigol 2008, 208).

iv) Deux théories en compétition, ayant les mêmes domaines d'application, partagent un ensemble de schèmes interprétatifs et les grandeurs caractéristiques qui sont définies en utilisant des modules communs (c'est le cas dans les étapes avancées des théories de la physique mathématique) ; en conséquence l'incommensurabilité totale entre les deux théories est impossible; en fait on trouve des savants qui sont bilingues et savent établir des ponts entre les deux théories (Darrigol 2008, 208).
v) Les modèles (songeons au modèle de l'atome de Bohr, aux modèles de l'atmosphère, etc.) et les théories ouvertes ne diffèrent que des théories par leur différence en ce qui touche à des qualités ou vertus comme portée (scope), unité structurelle, économie de représentation ou complétude ; mais, comme les théories, ils sont incrustés dans un univers symbolique et ont leurs schèmes interprétatifs ; ils ont une structure modulaire et sont donc en rapport avec des théories plus complètes (Darrigol 2008, 215-216).

6. Analyse modulaire et structuralisme métathéorique

Si on considère, comparativement, la conception du structuralisme métathéorique (SM) (le lecteur peut consulter la présentation faite, dans ce volume par Falguera et de Donato Rodríguez), avec la conception modulaire (CM), on peut faire quelques remarques générales.[18]

D'abord, le SM est une analyse de la structure des théories qui veut fournir une reconstruction formelle et idéalisée des théories et des rapports interthéoriques, en identifiants les différentes composantes des théories (modèles potentiels, modèles potentiels partiaux, modèles) et de leurs rapports; le SM s'inscrit dans la vision sémantique des théories et suit une stratégie de réduction à un langage formelle qui présuppose l'axiomatisation des théories et une nette dichotomie entre sens et référence des formations symboliques (qui oblige à concevoir les théories comme des classes de modèles), ce qui produit en même temps des préoccupations spécifiques de rigueur et des problématiques intrinsèques. Bien que des arguments d'inspiration historique soient présents pour le choix des modèles, il suffit de regarder la bibliographie utilisée dans quelques des reconstructions, notamment celles attentives aux aspects diachroniques, pour reconnaître l'intérêt très relatif pour les études historiques.[19]

[18] Quelques remarques sur ce sujet se trouvent dans (Darrigol 2008, 216, 217).

[19] Un exemple : la discussion sur les rapports entre la mécanique classique et quelques unes de ses 'spécialisations' dans (Casanueva 1993); la notion de force y est considérée est purement 'dynamique,' en oubliant la statique (qui précède historiquement) : le rapport entre les théories du choc et de l'action à la distance (cartésiens versus newtoniens) est passé sous silence (probablement parce que jugé purement métaphysique), etc.

La CM est très attentive à la pratique savante et à l'histoire des sciences et aux réflexions des savants (celles de Helmholtz sur la mesure, par exemple). Dans la CM l'identification des modules (les modules étant de la même nature que la théorie analysée, c'est-à-dire des théories ou des parties organiques de théories) est faite d'après des critères fonctionnelles en profitant de l'analyse historique ce qui implique le respect du jugement des acteurs historiques, et la description des systèmes physiques est associée à l'analyse de l'univers symbolique, des schèmes interprétatifs contenant des grandeurs mesurables ; si on veut y considérer une des préoccupations constantes du SM, qui est celle de la référence, on dira que dans la CM, la possibilité de mesurer et de concevoir ces schèmes assure la référence (le rapport à l'expérience) ; dans la CM, la réflexion épistémologique mobilise les ressources habituellement utilisés par les physiciens : le langage naturel purifié (quelquefois les adeptes du SM parlent de 'langage intuitif'), la connaissance de la physique expérimentale et de la physique mathématique et la réflexion philosophique sur les sciences de la tradition non-logiciste (en étant attentif aux ressources heuristiques de l'approche logiciste, bien sûr), sans craindre un manque de rigueur.

Les textes du SM sont assez difficiles à lire en raison du langage provenant de la logique et en raison de la difficulté de saisir la pertinence des traductions des conditions définissant les prédicats. Mais il faut reconnaître que dans le SM quelques uns des critères qui permettent d'identifier les différentes composantes d'un modèle sont de nature fonctionnelle ; par exemple la dichotomie entre langage observationnel (neutre par rapport aux théories) et langage théorique est remplacée par la distinction entre termes T-théoriques et termes T-non-théoriques (ces concepts servent à donner la description empirique des systèmes auxquels on applique la théorie T), distinction de nature fonctionnelle, puisque les seconds peuvent être des termes théoriques d'une autre théorie. Si on songe à une traduction (partielle) on dirait que ces termes sont présents dans les schèmes interprétatifs, dans les considérations sur la mesure par exemple ; aussi les conditions définissant les modèles potentiels et les modèles potentiels partiaux ont un air de famille avec l'univers symbolique et la présence de modules de définition. L'existence de termes T-non-théoriques ou de modules communs à deux théories qui se succèdent comme deux paradigmes (Kuhn) permet de comprendre comment l'incommensurabilité absolue est plutôt rare.

La CM introduit des fonctions modulaires dès la définition de la théorie, dès la constitution de l'univers symbolique ; l'univers symbolique de la mécanique classique par exemple contient la géométrie euclidienne en tant que théorie physique ; cela montre comment il est très difficile de traduire les modèles partiaux du SM comme correspondant à l'univers symbolique. La structure en arbre invertie du SM peut être vue comme un exemple de structure modulaire où les modules sont des spécialisations de la théorie de base ; le changement des ramifications de ces arbres (et des rapports entre différentes arbres) suggère la compréhension de la diachronie, mais l'exigence d'une reconstruction rationnelle unique semble être la priorité. Pour la CM, les aspects diachroniques doivent être étudiés historiquement ; les rapports interthéoriques doivent être identifiés, mais l'existence d'un catalogue préalable de fonctions modulaires ne doit pas faire supposer qu'il soit achevé.

Pour résumer, la structure modulaire se manifeste dans différents niveaux et contextes de la vie et de l'analyse des théories, la reconstruction rationnelle n'étant qu'un de ces contextes, un contexte surement important pour l'analyse philosophique. Le dialogue entre ceux intéressés par ces questions, philosophes des sciences et historiens avec ouverture aux questions épistémologiques, exigerait peut-être un peu de courage sociologique, de dépassement de barrières institutionnelles et d'auto-réflexion sur les habitus (Bourdieu) théoriques. L'utilisation systématique d'exemples issus de l'histoire des sciences, et l'effort pour traduire en langage intuitif (celui des manuels de physique) les hiérarchies et conditions des distinctions et des prédicats pourrait sans doute aider à approfondir/établir le dialogue entre le SM et la CM.

7. Conclusion

La connaissance des fonctions modulaires permet une riche analyse des diverses théories physiques montrant qu'elles forment synchroniquement un *holon* de théories (en reprenant un concept du structuralisme métathéorique); compte tenu de la possibilité d'extension modulaire d'une théorie, et du changement de rapport modulaire entre A et B (synchroniquement pluriel et diachroniquement mutable) on peut aussi décrire l'évolution d'une théorie et de ses rapports interthéoriques. Par cette démarche philosophique et historiquement bien renseignée, des questions générales de la philosophie des sciences apparaissent sous un nouveau jour.

L'intuition de départ que la compréhension et construction du complexe se fait par la composition fonctionnelle, par l'établissement de relations de dépendance partielle, d'adaptation mutuelle sans fusion (Darrigol 2008, 216), les composants fonctionnels étant plus simples et connus, cela par un processus de composition qui génère de nouvelles structures qui, à leur tour, peuvent être utilisées pour créer de nouvelles relations et structures, qui, en même temps, refont la structure plus globale, traduit et précise l'idée que la structure et le développement des théories est en bonne partie basé sur des rapports inter-théoriques, lesquels fournissent des éléments pour la construction de schémas interprétatifs qui permettent de rejoindre le monde de l'expérience.

Mais, comme le note Piaget, l'analyse structurelle doit être prise comme une méthode parmi d'autres. Les études historiques montrent diachroniquement le manque d'homogénéité des théories physiques, et montrent en même temps comme une théorie et aussi l'ensemble des théories d'une époque constitue un tout organique. Olivier Darrigol dans l'ensemble de son œuvre a été bien attentif à cette richesse, et je finis avec ses mots :

mon approche modulaire est seulement *une* approche qui permet de comprendre des aspects importants de la vie des théories, mais certainement pas tout. Il me semble qu'il faut rétablir la richesse des théories (aspects intentionnels, linguistiques, et imaginatifs) pour donner une idée plus complète des théories et surtout pour mieux comprendre la construction de nouveaux univers symboliques.[20]

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[20] Le texte communication personnelle d'Olivier Darrigol continue comme suit: « Certes dans certains cas l'idée de module réducteur permet de comprendre une bonne partie du processus de construction, mais il n'y a pas toujours de module réducteur (théories à principes) et alors l'expérience et l'intuition jouent un rôle complexe bien difficile à saisir; tout ce qu'on peut dire c'est qu'il y a de la modularité dans le produit final ».

Una aproximación conceptualista al estructuralismo

José Ferreirós

Universidad de Sevilla

A la memoria de Sol Feferman (1928-2016)[1]

El estructuralismo matemático del siglo XX surgió de la maduración del llamado "enfoque conceptual" de una serie de matemáticos alemanes. Ya Riemann había introducido un marcado giro conceptual, muy filosófico, en sus trabajos; es famoso su objetivo de basar cada teoría en un *concepto central*, y su aspiración a determinar las propiedades de los entes matemáticos en función de las *marcas características* de dicho concepto, relegando a una mera consecuencia la posibilidad de encontrar fórmulas que los representen. Esto le llevó a abandonar el viejo estilo de trabajo, centrado en técnicas de cálculo, y a desarrollar nuevos métodos más abstractos, por ejemplo topológicos (en teoría de funciones y teoría de variedades). Por su parte, Dedekind tuvo siempre a la vista esas aspiraciones metodológicas de su amigo y maestro, a la vez que iba elaborando la metodología del álgebra moderna, que en el siglo XX sería llamada *estructural*: cuerpos, ideales, isomorfismos, automorfismos… El famoso "está todo ya en Dedekind" de Emmy Noether apuntaba precisamente en la dirección de esa metodología, y la matemática de Noether ha sido descrita precisamente como "*begriffliche Mathematik*", matemática conceptual. Dicho sea de paso, Cassirer elaboró su 'filosofía de la relación', y su análisis del paso de concepciones de sustancia a concepciones de función, teniendo muy en cuenta el modelo ejemplar de las contribuciones de Dedekind. [2]

En realidad, cabría retrotraer algo más en el tiempo la genealogía de esos enfoques conceptuales y pre-estructurales, recordando que Gauss definió la Matemática como "la ciencia de las relaciones, en la medida en que uno abstrae de cualquier contenido de las relaciones…" Como dejó escrito en un célebre artículo de 1831 (aquél en el que introdujo la idea del plano complejo, la representación geométrica de los 'imaginarios'):

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[1] Conferencia impartida en la Universidade de Évora, Palácio do Vimioso, 4-5 Noviembre, 2016. Agradezco a los participantes en el congreso *Structuralism: Roots, Plurality and Contemporary debates* por sus comentarios. Una versión ampliada, en inglés, ha sido presentada para su publicación.

[2] Sobre Riemann, ver Laugwitz 1996 y Ferreirós 2000. Para Dedekind, hay numerosos trabajos que incluyen Stillwell 1999, Reck 2017.

> El matemático abstrae enteramente de la cualidad de los objetos y del contenido de sus relaciones; se ocupa meramente con la comparación de sus relaciones entre sí. (Gauss 1831)

Pero no es el objetivo de este trabajo hacer una historia o genealogía del estructuralismo matemático. Mi objetivo es filosófico. Buena parte del llamado estructuralismo en *filosofía* de las matemáticas tiene una orientación platonista, que puede resultar un tanto extraña a quien conoce las ideas correspondientes directamente a partir de la práctica matemática, o incluso a través de su surgimiento histórico. Dedekind y otros habrían pensado quizá que estos nuevos enfoques aligeran de ontología el trabajo matemático; no que replantean el problema ontológico alejándolo de los objetos 'simples' para focalizarlo en estructuras completas. Y sin embargo, esto es lo que nos propone el estructuralismo *ante rem* de S. Shapiro, quien no duda en enfatizar el postulado de *existencia objetiva*, "independiente del matemático, su forma de vida, etcétera, e independiente también de si las estructuras son instanciadas en el dominio no matemático" (Shapiro 2008). ¿Es necesario seguir este camino?

Hay alternativas, claro, y muy particularmente el *estructuralismo conceptual* que ha venido proponiendo el gran lógico Solomon Feferman. Argumentaré a favor de este enfoque y trataré de contribuir a perfeccionarlo, sobre todo explicando cómo es posible explicar la *objetividad* del conocimiento matemático sin recurrir a la postulación de objetos independientes.

1. Las estructuras: ¿sistemas relacionales u objetos abstractos?

En la práctica, aprehender estructuras significa elaborar conceptos de estructura, articular nociones generales que describen sistemas relacionales, por medio de un sistema de axiomas que los caracterizan. Esto es lo que suele entenderse por axiomática estructural. Ejemplos prototípicos fueron ofrecidos por Hilbert al trabajar sobre la noción de espacio geométrico, por Dedekind al caracterizar los cuerpos de números y los ideales, o al repensar la teoría de Galois; incluso por Riemann, al acuñar la noción de variedad diferenciable y basar en ella una nueva reflexión –más profunda– sobre las bases de la geometría. Eso es lo que importa en la práctica, como bien lo describe Mac Lane en su artículo de 1996.

Ahora bien, los filósofos están eminentemente preocupados por la naturaleza del conocimiento matemático y por su objetividad. Esto les conduce a menudo por las sendas de la ontología. El *estructuralismo platonista* considera las estructuras como *objetos abstractos* (o incluso, meta-objetos); la plena independencia de dichas entidades sería garantía de la objetividad y la verdad. Pero, ¿es esta una garantía sólida, o más bien una ficción filosófica?

Cuando se habla de platonismo se quiere decir que *hay* objetos matemáticos, los cuales no son físicos sino abstractos, y que son independientes de agentes inteligentes como nosotros, de nuestro lenguaje, pensamiento o prácticas (Linnebo

2013). Creo firmemente, sin embargo, que esta no es la única manera de garantizar la objetividad del conocimiento matemático. Incluso creo que es perfectamente posible seguir hablando de objetos abstractos, pero en términos de un platonismo más liviano (*lightweight*) que no incurriría en excesos ontológicos.

A diferencia del estructuralismo platonista, cabe plantear un *estructuralismo relacional* o relacionista desde el que puede pensarse que la vía estructural reduce de hecho las implicaciones ontológicas de las matemáticas. Eso es lo que parecen haber pensado Gauss, Dedekind, Hilbert y otros, como el mismo Poincaré. En vena similar, Feferman (2009) afirmaba:

> the basic objects of mathematics exist only as thought-objects or mental conceptions, though their source lies ultimately in everyday practices.

El estructuralismo relacionista puede ser armonizado con el habla común de los matemáticos, si se reconoce la *realidad* de los objetos matemáticos en un sentido que es radicalmente diferente de la existencia de los objetos del mundo físico. Lo importante sería aclarar cómo y por qué logramos *conocimiento objetivo* de las relaciones que expresamos hablando de dichos entes abstractos. Pero no hay por qué imaginar que los objetos matemáticos sean independientes de nosotros como agentes inteligentes: basta que sean independientes de *mis* procesos mentales, que sean pues objetivos en el sentido de fuertemente intersubjetivos. He aquí un rasgo kantiano del proyecto que estamos investigando.

Desde esta perspectiva, el platonismo habitual comete el error de poner el carro delante de los bueyes. No piensen que la matemática es objetiva porque estudia objetos puros e independientes, sino precisamente al revés: los objetos matemáticos son reales para nosotros debido al alto grado de objetividad del conocimiento matemático.

De nuevo, el punto clave es que la matemática es TRABAJO CONCEPTUAL; de hecho, es una parte destacada y singular del trabajo teorético que se desarrolla en el pensamiento científico. En dicho trabajo se estudian y clarifican relaciones, pero también sistemas relacionales, y aún más: las interrelaciones entre sistemas relacionales, y así sucesivamente. El caso de la teoría de Galois, cuyo núcleo está en las interconexiones entre cuerpos y grupos ligados a determinadas ecuaciones algebraicas, podría servirnos para ilustrar esas ideas en todo detalle; cada cuerpo, cada grupo, es un sistema relacional.[3] No hay problema en hablar de objetos, en el sentido débil (*lightweight*), pero el tema de los objetos no es primario: parece más bien un subproducto de la psicología humana. Cuando el entramado de relaciones e interrelaciones que debemos considerar es muy complejo, engordar la ontología alivia nuestros procesos de pensamiento. La misma lógica elemental (lógica de predicados de primer orden) es un reflejo de ello, codificando el sentido mínimo de la noción de 'objeto' en un contexto teorético.

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[3] La teoría de Galois se centra en el estudio de las interrelaciones entre ambos tipos de estructuras, extrayendo información acerca de las ecuaciones y los cuerpos de números asociados a ellas, a partir de lo que sabemos de dichos grupos (que Dedekind propuso pensar como grupos de automorfismos).

2. Feferman y sus diez tesis

Solomon Feferman ha presentado en diversos lugares un enfoque que él llamó "conceptual structuralism" (lo elabora en Feferman 2009 y 2014). De hecho, me consta que preparaba en el momento de su muerte un libro que debía llevar el título de *Logic, Mathematics and Conceptual Structuralism* (comunicación personal). Su enfoque quedaba resumido en Diez Tesis que le vi proponer ya en 2008, aunque su origen puede rastrearse hasta mucho antes. En ellas se resume un análisis complejo de las raíces del pensamiento matemático, que personalmente encuentro muy cercano y relevante. Parte de dicho análisis consiste en un cierto tipo de estructuralismo.

Traduzco a continuación las 10 Tesis de Feferman:[4]

1. Los objetos básicos del pensamiento matemático existen sólo como concepciones mentales, aunque su fuente puede encontrarse de múltiples modos en la experiencia común (contar, ordenar, emparejar, combinar, separar y localizar en el espacio y el tiempo).
2. La matemática teórica tiene su origen en el reconocimiento de que esos procesos son independientes de los materiales u objetos a los que se aplican, y de que son infinitamente repetibles –en potencia–.
3. Las concepciones básicas de la matemática son de ciertos tipos de imágenes del mundo [*world-pictures*] simples e ideales, que no corresponden a objetos aislados sino a estructuras, esto es, a grupos de objetos concebidos coherentemente, interconectados por unas pocas relaciones y operaciones simples. Dichas concepciones son comunicadas y comprendidas con anterioridad a ninguna axiomática.
4. Algunas características significativas de tales estructuras surgen directamente de las imágenes del mundo [*world-pictures*] que las describen, mientras que otras características pueden ser menos ciertas. La matemática necesita poco para comenzar, pero una vez empieza, le basta con poco para llegar lejos [*a little bit goes a long way*].
5. Las concepciones básicas difieren en su grado de claridad. Uno puede hablar de lo que es verdadero bajo una concepción dada, pero dicha noción de verdad puede ser sólo parcial. La verdad en sentido pleno sólo es aplicable a concepciones totalmente claras.
6. Lo que es claro en una concepción dada depende del tiempo, tanto para el tiempo individual como históricamente.
7. La matemática pura (teórica) es un cuerpo de pensamiento desarrollado sistemáticamente por refinamiento sucesivo y expansión reflexiva de las concepciones estructurales básicas.

[4] Tomado de una conferencia impartida en San Sebastián el 1 Oct. 2008, a invitación del autor, en el VIII International Ontology Congress. Su título fue: 'Conceptual Structuralism and the Continuum'.

8. Las ideas generales de orden, sucesión, colección, relación, regla y operación son pre-matemáticas; cierto grado de comprensión de las mismas es necesario para comprender las matemáticas.
9. La idea general de propiedad es pre-lógica; cierta comprensión de ella, y de las partículas lógicas, es también un prerrequisito para comprender las matemáticas. El razonamiento matemático es en principio de carácter lógico, pero en la práctica descansa, en buena medida, sobre varias formas de intuición.
10. La objetividad de las matemáticas se basa en su estabilidad y coherencia bajo comunicación repetida, escrutinio crítico, y expansión debida a muchos individuos que a menudo trabajan independientemente. Los conceptos incoherentes, o los que no superan el examen crítico o llevan a conclusiones en conflicto, son discernidos y eventualmente separados de la matemática. La objetividad de las matemáticas es un caso especial de la objetividad intersubjetiva que es ubicua en la realidad social.

Habría mucho que comentar a propósito de estas ideas de Feferman. De hecho, comentarlo todo con calma exigiría más bien un libro.[5] Aquí me centraré en aquello que concierne directamente al estructuralismo.

El enfoque de Feferman y su aproximación al conocimiento matemático es en realidad *agencial* o *agent-based*, al igual que lo es el mío (Ferreirós 2016). En realidad, considero que esta característica es inevitable si queremos realmente defender un estructuralismo *conceptual*. La idea clave, ya lo hemos dicho, es que el trabajo matemático es trabajo conceptual: un estudio de relaciones, sistemas relacionales, interrelaciones entre dichos sistemas, etc. Para entender cómo se elabora *conocimiento* a partir de esa base, es imprescindible hablar de los agentes que comparten dicho conocimiento. Y para lograr dar cuenta adecuadamente de la objetividad del conocimiento matemático, es imprescindible entrar en el análisis de cómo el conocimiento puede ser *compartido* por comunidades de agentes. En el caso matemático, logran compartirlo de una forma *fuertemente intersubjetiva*. Explicar lo que esto quiere decir es un trabajo que está en desarrollo.

Así, hay que vérselas con la cuestión de cómo la lógica y las matemáticas se elaboran sobre la base de las lenguas vernáculas y de prácticas humanas simples (contar, medir, diseñar figuras); esto es, cómo se elaboran sobre ciertas nociones pre-teóricas que surgen de la práctica. Un enfoque conceptualista exige pues, para su desarrollo, entrar en interacción con estudios cuidadosos de las raíces cognitivas del pensamiento humano, y muy especialmente las raíces de nuestras concepciones básicas del tiempo, el espacio y el número. Pero, hablando de las ciencias cognitivas, hay que enfatizar que la cognición humana es un asunto más complejo de lo que abarca normalmente la ciencia cognitiva hoy en día: nuestro conocimiento se elabora sobre elementos semióticos, p. ej. representaciones explícitas tales como los diagramas, los mapas, los numerales, los símbolos numéricos. Entran en juego la cognición expandida y la semiótica, no meramente las neurociencias.

[5] Para la cuestión de cómo conocemos esas ideas pre-matemáticas y pre-lógicas de las que habla Feferman, el lector puede encontrar algunas ideas en Ferreirós (2016).

Cabe distinguir diferentes grados en la relación que se puede discernir entre las estructuras y la experiencia. 1. Algunas estructuras no hacen más que implementar contenidos extraídos de nuestra experiencia, de nuestra relación con el mundo, ciertamente con alguna dosis de *idealización*; esto sucede con la aritmética básica (surgida por reflexión sobre el contar, explotando la posibilidad de operar con símbolos) o con la teoría de grupos básica (inspirada en propiedades de diversas operaciones simples, como medir, transportar y otras). 2. A otro nivel encontramos estructuras que constituyen extrapolaciones de fenómenos del mundo, con un grado mayor de idealización o incluso con ciertas bases *hipotéticas*; tal es el caso de los números reales **R** o de las funciones f: **R** → **R**, o también de los grupos continuos. La práctica de medir conduce naturalmente a las fracciones o los números racionales, pero no a los reales, que no podrían haberse introducido sin plantear ciertos supuestos hipotéticos (principalmente relativos a la continuidad, el continuo espacial). 3. Por fin, hay estructuras que introducen nuevas iteracciones basadas en extrapolaciones a partir de estructuras previas, buscando una clausura de nivel superior, tal como sucede con el universo de los conjuntos **V**.

Si centramos nuestra atención, como hace Feferman, en las estructuras más básicas y centrales, no algebraicas, que son los sistemas numéricos **N** y **R**, además del sistema conjuntista **V**, encontramos que muchos autores las consideran estructuras *rígidas* (ver p. ej. el interesante trabajo de Isaacson 2011). Dicha idea se basa en resultados técnicos de la lógica, concretamente en los teoremas de categoricidad para los sistemas numéricos **N** y **R**, y el resultado de cuasi-categoricidad para **V** que estableció Zermelo. Al ser categóricas o monomorfas, dos modelos cualesquiera de una de esas estructuras son isomorfos,[6] y por ello estaría bien determinada en principio la verdad o falsedad de cualquier proposición relativa a dichas estructuras rígidas. Ahora bien, Feferman (2014, 22) escribe:

> The direct apprehension of these [basic structures] leads one to speak of truth in a structure in a way that may be accepted uncritically when the structure is such as the integers but *may* be put into question when the conception of the structure is less definite as in the case of the geometrical plane or the continuum, and *should* be put into question when it comes to the universe of sets.

Dicho punto de vista no es para nada subjetivo, sino que se basa en el examen y análisis cuidadoso, desde un punto de vista lógico, de los resultados de categoricidad y sus prerrequisitos. Para la categoricidad de **N** basta con un sistema muy débil de lógica de segundo orden, y el resultado ni siquiera explota la impredicatividad de dicha lógica. En cambio, la categoricidad de **R** no puede establecerse sin acudir a la lógica *plena* de segundo orden, con todos los problemas que esto conlleva.[7] Y para lograr la cuasi-categoricidad de **V**, Zermelo recurrió al expediente

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[6] En el caso de **V**, isomorfos hasta cierto cardinal inaccesible. Ver Zermelo 1930.

[7] En otro lugar (Ferreirós forthc.) defiendo que la lógica "plena" o "estándar" (como a menudo se la llama) de segundo orden no es ningún sistema de lógica, sino más bien teoría de conjuntos aplicada. Nótese que esto permite afirmar que hay lógicas de segundo orden perfectamente convincentes.

de emplear la lógica plena de segundo orden también, tratando a la operación conjunto-potencia como si fuera externa o anterior a la teoría de conjuntos. Esto es difícilmente aceptable. Por decirlo de otra manera: como la lógica plena es en realidad teoría de conjuntos aplicada, fundamentar en ella la teoría de conjuntos es incurrir en un círculo vicioso. (Quine advirtió bien este aspecto de la cuestión, aunque de ello sacó incorrectamente la conclusión de que no hay una lógica de segundo orden; sí que la hay, pero sólo si renunciamos a trucos mágicos pretendidamente semánticos. Ver Ferreirós forthc.)

Tratándose de Feferman, que es célebre en relación al predicativismo, una clarificación más resulta imprescindible. Su estructuralismo conceptual puede ser adoptado por aquellos que buscan sólo interpretar la matemática clásica. No hay nada en ese enfoque que nos fuerce a compartir ideas constructivistas, ni nada que nos obligue a eliminar las definiciones impredicativas.

3. Objetividad (¿sin objetos?)

El mayor problema que debe enfrentar cualquier tipo de conceptualismo, en relación a las matemáticas, es el de cómo explicar la objetividad del conocimiento matemático. Pero, precisamente, la propuesta de Feferman de comparar los objetos matemáticos con los objetos sociales no resulta muy convincente: sugerente sí, y quizá aclaratoria, pero no convincente si se pretende una identificación bastante estricta.

En realidad, el estructuralismo matemático siempre ha parecido traer consigo una idea de que la ontología no importa demasiado: si lo que estudia el matemático son estructuras o sistemas de relaciones, parece posible adelgazar al máximo los supuestos ontológicos. Pero muchas versiones del estructuralismo filosófico caminan en otra dirección. Esto se debe a que los filósofos se preocupan, con razón, de la cuestión de la objetividad. Y si se trata de asegurar que podamos hablar de significado e incluso de verdad en relación a las matemáticas, parece inevitable fijar de algún modo esos estratos de realidad a los que se referirían las proposiciones matemáticas. Por ejemplo, Shapiro dice:

> the *ante rem* structuralist holds that, say, the natural number structure and the Euclidean space structure exist objectively, independent of the mathematician, her form of life, and so forth, and also independent of whether the structures are exemplified in the non-mathematical realm. (Shapiro 2008)

Claro está que al decretar la existencia independiente de una realidad platónica del tipo que sea (ya se trate de objetos como los números, o de meta-objetos como las estructuras) obtenemos una fortísima base para afirmar la objetividad de la matemática. Pero, ¿resulta una base convincente? Todos sabemos que ese decreto es contrario a la mayor parte de la ciencia actual, y también a muchas formas de filosofía. ¿No será peor el remedio que la enfermedad? Y sobre todo, ¿no estaremos haciendo las cosas al revés? Esta es de hecho mi opinión: quienes, como Gödel o como Shapiro, siguen la vía del realismo pleno (*heavyweight*), ponen el carro delante de los bueyes. El camino no es explicar la objetividad apelando a una realidad de objetos, sino precisamente a la inversa: indicar por qué tiene justificación hablar de objetos reales –en el sentido de la lógica– explicando cuál es la base de la objetividad matemática.

La matemática establece resultados necesarios acerca de estados de cosas hipotéticos (la idea es de Peirce), esto es, demuestra teoremas acerca de hipotéticas estructuras. ¿Cómo podría una empresa así ser objetiva? Veremos que se debe por un lado a las fuertes raíces cognitivas del pensamiento matemático, y por otro a las enormes restricciones o *constraints* que operan sobre el conocimiento matemático.

No todo es puramente hipotético en matemáticas. Hay enormes diferencias entre considerar dado cualquier número natural, olvidando restricciones empíricas o cuestiones de *feasability*, y considerar dado cualquier número real. La aritmética elemental se puede establecer sobre bases constructivistas, lo cual no elimina el hecho de que idealizamos al no limitarnos a un finitismo estricto. Pero la aritmética de los reales, o el análisis clásico, no son planteables en un enfoque constructivo; exigen mucho más: no hay reglas recursivas que permitan generar los objetos del dominio que se introduce; asumimos *dado* un dominio de objetos que no sólo es infinito y denso, sino que además es *continuo*.[8] He aquí una hipótesis clave de la matemática moderna, por eso decía Peirce que el matemático estudia lo que sucede necesariamente en estados de cosas hipotéticos. La matemática se mueve en el ámbito de lo posible, proyectando lo real sobre un trasfondo de posibilidades.

Piense el lector en dos teoremas matemáticos muy básicos, y pregúntese si los considera igualmente ciertos:

A. Hay infinitos números primos.

B. Toda sucesión monótona creciente de números reales, si es acotada, tiene un límite.

El primero A. se sitúa en el nivel de la aritmética básica y a ningún matemático se le ocurriría ponerlo en cuestión. El segundo B., en cambio, ha sido considerado inválido por matemáticos de primera fila, como Brouwer o Weyl. Prueba empírica, si se quiere, de que hay estratos muy diferentes en la conformación del pensamiento matemático.

En el nivel de la matemática elemental –la parte de la matemática que no depende de hipótesis, p. ej. la aritmética básica–, encontramos una forma de conocimiento que ofrece garantías de certeza. He defendido que nuestra aceptación de los axiomas de Dedekind-Peano se debe a que todos ellos –incluyendo el de inducción completa– son reconocidos como *verdaderos* de nuestra concepción del número natural (ver mi capítulo en Lassalle Casanave & Ferreirós 2016). De he-

[8] Axiomas o hipótesis de continuidad son el de Dedekind: toda cortadura es producida por un objeto del dominio; o el de Bolzano-Weierstrass: toda sucesión de intervalos cerrados y encajados tiene intersección no vacía.

cho, la concepción básica del número natural basta para justificar una formulación en segundo orden del sistema de Peano.[9]

En cuanto a los estratos más avanzados del conocimiento matemático, típicamente *hipotéticos* –basados en supuestos como el axioma de continuidad para **R**, o el axioma de elección, o el axioma de las paralelas de Euclides–, lo primero que hay que decir es que están diseñados para encajar con los estratos más elementales. El sistema de los números reales es una expansión del sistema de los naturales; y los teoremas sobre números naturales que se obtienen a través de un desvío por el dominio de los reales (en la teoría analítica de números) no son por ello menos fiables. Este juego recíproco entre estratos elementales y estratos hipotéticos establece una primera restricción fundamental. Y como he dicho antes, la objetividad de las matemáticas se debe a las *restricciones* que operan en su campo.

Además, por seguir con el ejemplo, el sistema de los reales **R** está ligado con las prácticas de medir, y también interconectado con los números racionales (que son a su vez una expansión de los naturales). Nuevas restricciones y nuevas bases para que toda una comunidad de agentes pueda compartir conocimiento de manera fuertemente intersubjetiva. Podríamos añadir más cosas aún, ya que los números reales constituyen una increíble encrucijada de caminos dentro del dominio de las matemáticas: todo se liga aquí, aritmética y geometría, álgebra y topología. Pero la idea importante es que las *interrelaciones* entre diferentes prácticas y diferentes estratos del conocimiento establecen toda una serie de apoyos y restricciones que se combinan para garantizar la objetividad del conocimiento matemático.

Aun cuando la matemática superior se base en supuestos hipotéticos, estos no son en absoluto arbitrarios. Están anclados en elementos anteriores, que los condicionan, y limitados también por poderosos puentes que ligan diferentes áreas de las matemáticas. Elaborar todo el mapa de las interconexiones en que entra **R** sería una tarea muy difícil. Por ejemplo, sería incompleta si no se indican también los puentes que ligan de nuevo esos números con el estudio de las funciones, y de los espacios de funciones, etc., etc.

En resumidas cuentas, la objetividad del conocimiento matemático es fruto de las intensas restricciones que se derivan de sus raíces cognitivas (cognición básica, prácticas de contar y medir, prácticas de diseñar figuras, prácticas semióticas y simbólicas) y de las interrelaciones entre diferentes estratos del conocimiento, que en ocasiones llegan a ser sumamente densas. En mi opinión, ningún otro campo del conocimiento humano ofrece un ejemplo comparable de intersubjetividad (Ferreirós 2005).

De ahí la objetividad (no platónica, sino intersubjetiva, kantiana) de teorías como la geometría euclidiana o el análisis de variable real, objetividad que es compatible con la presencia de idealizaciones en dichas teorías.

¿Y dónde queda el asunto de los objetos matemáticos? La realidad de los objetos matemáticos se deriva de la objetividad de las teorías correspondientes, y de-

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[9] Sin la magia conjuntista de la lógica 'plena'; pero el sistema, aun siendo incompleto formalmente (Gödel), es semánticamente completo (Dedekind).

pende también de nuestro empleo de la lógica elemental. El marco lógico permite establecer afirmaciones de existencia, predicar sobre las correspondientes 'cosas' y establecer relaciones entre ellas; con esto basta para hablar de objetos. Nótese que, aunque el matemático estudie sobre todo relaciones y sistemas relacionales (e interrelaciones entre éstos), los humanos rápidamente pasamos a expresar los resultados de dicho estudio hablando de objetos: pensamos las relaciones como objetos, y lo mismo hacemos con las estructuras, etc. El marco de la lógica elemental es una expresión de dicha tendencia, como también lo son las lenguas naturales.

Los padres de la lógica moderna –Bolzano, Frege, Peirce– insistieron en que la noción lógica de existencia desborda el marco de las realidades físicas. El símbolo ∃x no tiene su significado limitado a las cosas que pertenecen al mundo físico, que existen en el espacio-tiempo e interactúan entre sí mediante relaciones causales; este campo es lo que Frege llamaba el dominio de lo actual (*Wirklichkeit*, derivado de *wirken*, actuar). El símbolo ∃x puede y debe utilizarse también para otras realidades, y muy especialmente las de tipo lógico y matemático; es lo que Frege llamaba el dominio de lo objetivo (*Gegenständlichkeit*, derivado de *Gegenstand*, objeto). Similares distingos introdujeron en diversos escritos Bolzano y Peirce.

Aquí es donde resulta de alguna utilidad la comparación que hace Feferman entre realidades matemáticas y realidades sociales. El dinero existe, con independencia de si se trata de monedas de oro, billetes de papel, o transacciones informáticas; y su existencia está entre las realidades más decisivas que han influido en la vida humana sobre la Tierra. Pero su forma de ser nada tiene que ver con los átomos o las proteínas, y en especial no existe "independientemente de los agentes, sus formas de vida, etcétera" (parafraseo la cita de Shapiro que dimos arriba). También el matrimonio existe como institución social, con claros efectos sobre nuestra experiencia y nuestra vida; de nuevo, es una realidad que no es independiente de nuestras formas de vida, rituales, sistemas legales, etc. También existen los dioses homéricos en los mitos de los antiguos, como existen Hamlet y Sancho Panza en obras literarias inolvidables; uno diría incluso que la realidad de Sancho Panza es mayor que la de tantos hombres y mujeres que un día existieron en la realidad actual, pero cuyas trazas han sido borradas por el paso del tiempo.

Ahora bien, la realidad matemática no es la realidad de las ficciones literarias, ni siquiera la de los dioses o los santos. Tampoco es satisfactoria, en mi opinión, la comparación con las realidades sociales, por sustanciales en la experiencia humana y fuertemente institucionalizadas que puedan ser éstas. El camino hacia la explicación de esta aparente paradoja no resulta tan difícil de indicar: la objetividad de las teorías matemáticas, de los teoremas, etc., es de una naturaleza bien diferenciada. Cierto que el dinero es, hoy, una realidad casi universal sobre la superficie de la Tierra; pero la realidad de los números, como diríamos ingenuamente, "es mucho mayor". Cuestión de *objetividad*, de intersubjetividad particularmente fuerte, de raíces cognitivas y conocimiento compartido, de restricciones, interconexiones y puentes.

Pero, eso sí, no olvidemos poner los bueyes delante del carro. No al revés. De lo contrario, nos enredaremos en disquisiciones metafísicas sin sentido.

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Convención, Estructura, Hipótesis

El convencionalismo de Poincaré a la luz de algunas consideraciones metodológicas

María de Paz

Universidad de Sevilla

Hypothesen sind Netze, nur der wird fangen, der auswirft.
Novalis

La posición filosófica de Poincaré, conocida como 'convencionalismo' se ha situado siempre en conexión con problemas acerca del estatuto epistémico de ciertos principios, siendo estos problemas causados por el desarrollo de nuevas teorías científicas. En este sentido, es una filosofía situada típicamente en estrecha conexión con la práctica y el desarrollo de la ciencia. Teniendo en cuenta esta perspectiva, el objetivo de este artículo es mostrar un posible camino que conecte la filosofía de la geometría de Poincaré con su filosofía de la física y la mecánica por medio de la noción de estructura, entendida en el sentido del pensamiento estructuralista en matemáticas. Y la idea es hacerlo pensando la filosofía de Poincaré como surgida de su práctica científica y no como una reflexión de segundo orden sobre la ciencia.

Al utilizar la noción de estructura y el pensamiento estructuralista en matemáticas no se trata necesariamente de una idea o concepto tomada estrictamente de Poincaré, sino de algo que nos pueda servir para interpretar o entender su pensamiento, pese a que en algunos aspectos pueda suponer 'traer algo de fuera'.

El artículo se divide en tres partes. En la primera, nos centraremos en proporcionar una descripción básica de lo que entendemos por estructuralismo matemático con el objetivo de que quede claramente caracterizado aquello a lo que nos referimos al hablar de estructuralismo en la obra de Poincaré. Por tanto, no pretendemos en esta parte realizar una discusión filosófica profunda sobre las implicaciones de esta posición dentro de las matemáticas, sino utilizar esta concepción para repensar algunas ideas del sabio francés. Igualmente, no procuramos presentarlo como un estructuralista, en la medida en que este no es el objetivo central, ni como un proto-estructuralista, sino simplemente mostrar que hay algunos rasgos estructuralistas identificables en su pensamiento que pueden llevar a pensar filosóficamente algunas cuestiones de la manera que él lo hizo. La segunda parte mostrará cómo algunos rasgos de estructuralismo matemático en su manera de abordar la geometría se encuentran conectados con su posición filosófica acerca de esta disciplina. La tercera parte plantea si estas ideas que emergieron a partir de la práctica matemática guardan alguna relación con su aproximación filosófica a la física y a la mecánica.

Antes de abordar la cuestión del estructuralismo matemático, queremos esclarecer que no se pretende aquí proporcionar una interpretación unificadora del convencionalismo geométrico y físico-mecánico de Poincaré, cuya separación en base a su dominio de aplicación hemos defendido en otros lugares (cf. de Paz 2016), sino de establecer una conexión entre las posiciones filosóficas que este autor tiene para estas disciplinas.

Aspectos básicos para una comprensión del estructuralismo matemático

Como he mencionado más arriba, este apartado no trata de una reflexión filosófica detallada sobre el estructuralismo matemático, por lo que no discutiré sus versiones filosóficas más famosas, ya sea la *ante rem* o la *in re* o modal.[1] El objetivo es simplemente retener algunos puntos clave que resultan útiles para interpretar algunos aspectos de la obra y el pensamiento de Poincaré como estructurales o estructuralistas.

En su famoso artículo "Structure in Mathematics", Saunders Mac Lane presentaba la noción básica y general de estructura matemática con las siguientes palabras:

> una lista de operaciones y relaciones matemáticas y las propiedades requeridas para estas, normalmente dadas como axiomas y, a menudo, formuladas de tal manera que son propiedades compartidas por un número de objetos matemáticos específicos posiblemente muy diferentes (Mac Lane 1996, 174).

La idea es, por tanto, que los axiomas que describen las propiedades de ciertas operaciones matemáticas definen estructuras, las cuales son marcos relacionales, o sea, sistemas de relaciones para trabajar, en definitiva, para hacer matemáticas. Algunos casos famosos que analizaba en ese artículo son el concepto de grupo, el espacio métrico y el espacio topológico; existe, así, una pluralidad de nociones de estructura.

La estructura proporciona una descripción rigurosa, pues describe axiomáticamente las propiedades comunes a diversos objetos matemáticos, por lo que presenta un método general de aproximación a esos objetos:

> Un objeto matemático 'tiene' una estructura particular cuando aspectos específicos del objeto satisfacen la lista (estándar) de axiomas de esa estructura (Mac Lane 1996, 176).

Esto significaba, además, presentar este enfoque como un método, como una manera abstracta de hacer matemáticas que puede tener varias ejemplificaciones.

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[1] Una discusión sobre estas en relación con Poincaré (y Lautmann) puede encontrarse en Heinzmann 2014.

Y es precisamente el método, como una manera de hacer matemáticas y no como una filosofía de la matemática lo que queremos destacar aquí. Por eso es importante la siguiente afirmación:

> el 'estructuralismo metodológico' […] tiene que ver primeramente con el *método*, más que con las cuestiones semánticas y metafísicas que atañen a las otras [versiones del estructuralismo matemático] (Reck 2003, 371).

La idea es, así, presentar una manera de *hacer* matemáticas que consiste en un enfoque general típicamente conectado con los enfoques axiomáticos (dado que los axiomas definen las propiedades de la estructura) sin preocuparse acerca de la naturaleza de los objetos matemáticos, o sea, sin referencia a ningún tipo de ontología:

> El *estructuralismo metodológico* consiste, entonces, en ese enfoque general ampliamente conceptual […]. Un matemático que es un estructuralista metodológico no estará preocupado con la identidad o naturaleza última de los objetos en los diferentes sistemas estudiados (Reck 2003, 371).

Este desinterés por la ontología propio del método estructuralista se traduce, así, en el enfoque conceptual, caracterizado por el estudio no de objetos, sino de estructuras, que son aquellos sistemas definidos por medio de axiomas o condiciones. Esos sistemas, por consiguiente, no son objetos, sino que serán mejor entendidos como conceptos abstractos (de ahí el 'enfoque conceptual') y, consecuentemente, si hay varios sistemas, habrá entonces varias posibilidades conceptuales. Este enfoque conceptual, que emerge a mediados del siglo XIX, se encuentra caracterizado por varias figuras:[2]

> Ellos [Riemann y Dedekind] trataron de situar consistentemente las teorías matemáticas en el marco general más apropiado, de un modo tal que 'las formas externas de representación' fueran evitadas, se eligieran nuevos objetos básicos y se situase una definición de las propiedades internas características de estos objetos (i.e., un concepto fundamental) al principio de la teoría (Ferreirós 1999, 31).

Por tanto, esta caracterización del trabajo metodológico de Riemann y Dedekind es un ejemplo del enfoque estructuralista. En esta perspectiva, las representaciones concretas solo aparecen al final y no al principio de la teoría, por lo que son vistas, de este modo, como un producto del trabajo conceptual.

Así, podemos decir que los aspectos más destacables del estructuralismo en cuanto método para hacer matemática consisten en el estudio de estructuras y no de objetos y que, dichas estructuras corresponden a conceptos abstractos o nociones generales definidos a partir de ciertas características (ya sean condiciones, propiedades o axiomas) que pueden ser aplicables en diferentes contextos. Y se trata de un método para hacer matemáticas y no de una teoría filosófica sobre la ontología o la semántica de las matemáticas porque la elaboración de estas nocio-

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[2] Stein (1988, 258-259) suma a los nombres de Riemann y Dedekind a los que se refiere Ferreirós, los de Frege, Russell y Whitehead. A estos podrían añadirse otros aún más relevantes en la cuestión del estructuralismo como Hilbert, Hausdorff o Noether.

nes, la elaboración de estructuras es, precisamente, hacer matemáticas y, al mismo tiempo, supone una guía para la investigación matemática (cf. Mac Lane 1996, 181).

Una interpretación estructuralista del convencionalismo geométrico de Poincaré

Una vez caracterizada la noción de estructura de una manera general y entendido el estructuralismo como una metodología matemática y no como una reflexión de segundo orden sobre la misma, veamos si hay un modo de hacer cuadrar estas ideas con la filosofía de la geometría de Poincaré.

El primer problema que se plantea en este punto es que hasta ahora he insistido en un método para la matemática y no en una filosofía sobre ella y, sin embargo, el convencionalismo geométrico es precisamente una filosofía de la matemática (o al menos de una parte de la matemática). Ahora bien, ¿qué ocurre si presentamos esta particular filosofía de la matemática como una consecuencia de la práctica matemática de Poincaré? O sea, ¿puede ser considerada como una consecuencia de su modo de hacer matemática? Por supuesto, dicha presentación no implica en modo alguno que la posición convencionalista sea una consecuencia necesaria o la única consecuencia posible de hacer geometría del modo en el que Poincaré la hizo. Implica, solamente, que es una concepción filosófica que tiene esta práctica en su origen. Esta es, sin duda, la mejor manera posible de resolver esta tensión.

La caracterización estándar del convencionalismo geométrico presenta esta posición como una consecuencia de la aplicabilidad de la geometría a la física o, más bien, al espacio físico:

> Él [Poincaré] defendió el convencionalismo para algunos principios de la ciencia, más notablemente para la elección de la geometría aplicada (la geometría que encaja mejor con la física para una determinada explicación de la realidad). Pero la elección del sistema geométrico no es una convención arbitraria. De acuerdo con Poincaré, elegimos el sistema basado en consideraciones de simplicidad y eficiencia dada la situación general empírica y teórica en que nos encontramos. Junto con las aspiraciones de simplicidad y eficiencia, la información empírica debe iluminar y guiar nuestras elecciones, incluyendo nuestras elecciones geométricas (Folina, IEP).

Esta caracterización está justificada por afirmaciones hechas por el propio Poincaré del tipo:

> La experiencia nos guía en esta elección que no nos impone. No nos dice cuál es la geometría más verdadera, sino cuál es la más *cómoda* (Poincaré 1902, 91).

En este sentido, se trata de una posición filosófica para el conjunto de geometría + física. La caracterización de esta posición que pretendemos presentar aquí es ligeramente diferente. Esto no significa que esta interpretación estándar sea incorrecta, es también la posición de Poincaré. Tan solo, esta parte de su trabajo y de su pensamiento no ocupa el centro de nuestro análisis, pues se trata, de hecho, de dos problemas diferentes, aunque conectados. El primero de ellos es lo que normalmente se denomina como 'el problema del espacio' y se corresponde con la pregunta de qué tipo de geometría corresponde al espacio físico. El segundo problema, que es el que nos ocupa, es el de la pluralidad de geometrías y dada esta pluralidad cuál es su estatuto epistemológico, es decir, se trata de un problema puramente matemático y no de un problema en el que se aborden cuestiones de geometría + física.

Nuestro objetivo es presentar la posición de Poincaré como una posición genuinamente matemática, no como una posición desarrollada para aplicar la matemática al mundo físico; no como una 'filosofía práctica de la geometría', sino simplemente como una filosofía de la geometría, sin tener en vista su aplicación al mundo físico.

Si tomamos en cuenta temas como la teoría de grupos de Lie, a la que Poincaré dedicó intensos esfuerzos (cf. Gray y Walter 1997) y la relación que esta guarda con la noción de estructura, tal y como es señalado por Mac Lane (1996) y otros autores, podemos considerar el enfoque de Poincaré en geometría como situado en la línea estructuralista. De hecho, Poincaré define la geometría como el estudio de un grupo:

> Lo que llamamos geometría no es sino el estudio de las propiedades formales de un determinado grupo continuo; por tanto podemos decir que el espacio es un grupo (Poincaré 1898, 41).

Teniendo en cuenta esta perspectiva, de lo que se trata en geometría, por tanto, es del estudio de ciertas propiedades que definen un grupo y, consecuentemente, la estructura de ese grupo será lo que defina las propiedades del espacio. Poincaré define el espacio como un grupo porque no quiere entenderlo como un objeto matemático, sino como un concepto general. De hecho, afirma que es "solo una palabra que hemos tomado por una cosa" (Poincaré, 2017, xxix).

Así, en geometría, de lo que se trata es de estudiar las estructuras de grupo que están definidas a partir de axiomas que expresan sus propiedades. Pero, ¿qué son los axiomas? La respuesta a esta pregunta es una de las más famosas de Poincaré:

> Los axiomas geométricos no son, por tanto, ni juicios sintéticos a priori ni hechos experimentales. Son convenciones (Poincaré 1902, 66).

Con esta afirmación Poincaré se posiciona filosóficamente respecto al estatuto epistemológico de los axiomas situándolos en un nivel que no corresponde ni al de verdades extraídas de la experiencia ni al de afirmaciones verdaderas en función de nuestras capacidades intelectuales, sino a una suerte de *tercera vía* (cf. Pulte 2000 y de Paz 2014) en la que se sitúan proposiciones que no son ni verdaderas ni falsas, pero que resultan informativas y determinantes para el contenido de nuestra ciencia. Además, el hecho de no ser ni verdaderos ni falsos, sino convencionales implica la posibilidad de que existen axiomas alternativos y, en consecuencia, como los axiomas definen diferentes grupos, habrá varios grupos posibles que respondan a la estructura atribuible a distintos espacios. Esos espacios serán los marcos conceptuales que después intentemos aplicar a la experiencia de una manera también convencional. Pero lo que aquí nos interesa destacar no es esta aplicación en la cual hemos de conjugar la geometría con las leyes de la física, sino el hecho de que los axiomas definen la estructura de grupo y dicha estructura corresponde a un determinado marco conceptual. Como dice DiSalle:

> El espacio […] ya está constituido como un esquema conceptual, empezando con la concepción primitiva de teoría de grupos identificada por Poincaré como la base de nuestro conocimiento espacial elemental (DiSalle 2012, 14).

En consecuencia, las estructuras o marcos conceptuales vienen definidas a partir de axiomas que son, desde la perspectiva de Poincaré, convenciones. Es así como las convenciones definen marcos conceptuales. A partir de la caracterización epistemológica de los axiomas como convenciones, podemos vincular la perspectiva filosófica convencionalista con la aproximación estructural a la geometría, que no es sino un método de *hacer* geometría, de trabajar en esta disciplina: “veamos, pues, actuar al geómetra y tratemos de sorprender sus procedimientos” (Poincaré 1902, 13). Pues en la medida en que existen diferentes estructuras de grupo definidas por diferentes axiomas, Poincaré elabora, a partir de este enfoque, una posición filosófica que permite dar cuenta del estatuto epistemológico de los axiomas sin implicarse en una discusión acerca de su verdad o falsedad. Es decir, al trabajar con el enfoque de grupo, encuentra diferentes posibilidades, por lo que si diferentes grupos son posibles, los axiomas que los definen no podrán tener el estatuto de verdades a priori y, en la medida en que la geometría no es una ciencia que trabaje con objetos empíricos (cf. Poincaré 1902, 164), las afirmaciones que realiza no podrán ser experimentales.

Sin embargo, tenemos que ser conscientes de que esta posición convencionalista para la geometría es solo sobre una parte de la matemática, por lo que cabe aquí preguntarse, si Poincaré sería o no un estructuralista *tout court*, es decir, si su enfoque metódico valdría también para otras ramas de esta disciplina. Cuando exploramos sus afirmaciones en lo relativo a la aritmética y concretamente acerca de la teoría de números, al examinar las cortaduras de Dedekind, afirma:

> Pero contentarse con esto sería olvidar demasiado el origen de esos símbolos; falta explicar cómo se ha ido conduciendo a atribuirles una especie de existencia concreta y, por otra parte, ¿no comienza esta dificultad con los mismos números fraccionarios? ¿Tendríamos la noción de estos números, si no conociéramos de antemano una materia que concebimos infinitamente divisible, es decir, un continuo? (Poincaré 1902, 34).

O sea, con respecto a los números, Poincaré no afirma nada que pueda considerarse claramente estructuralista ni su visión encaja con el enfoque conceptual de otros autores como Dedekind.

En vista del desarrollo posterior de la matemática y el enfoque axiomático y estructural que triunfó en teoría de números, es posible que la visión de Poincaré no resulte adecuada; sin embargo, proceder de una manera estructuralista en alguna rama de la matemática no es incompatible con no utilizar una aproximación estructural para toda la matemática, pues como dice el propio Mac Lane: "nunca fue el caso que toda la matemática refiriese a tales estructuras" (1996, 177).

¿Es posible extender esta interpretación al convencionalismo mecánico?

Una vez examinado el vínculo entre la aproximación metodológica de Poincaré a la geometría y su concepción filosófica de la misma, vale la pena considerar si es posible extender esta conexión y, por tanto, esta interpretación filosófica al convencionalismo físico-mecánico. Partiendo de la idea de que Poincaré utiliza la misma terminología – a saber, la noción de convención – para calificar epistemológicamente tanto los principios de la mecánica como los axiomas de la geometría, cabe preguntarse si hay un vínculo entre estas dos posiciones, pese a que se trate de disciplinas diferentes cuyo estatuto, objeto y estructura Poincaré se encarga cuidadosamente de separar (cf. Poincaré 1902, 162-166).

Pese a esta distinción, sospechamos que hay un elemento metodológico subyacente que puede resultar común, desde la perspectiva de Poincaré, a estas dos disciplinas y, en ese sentido, resulta útil contextualizar históricamente el ámbito en el que surge su concepción física, conocida como la 'física de los principios' (cf. Poincaré 1905, 174 y ss.).

Poincaré describe esta posición como aquella física matemática cuyos principios fundamentales aspiran a proporcionar una descripción general de los fenómenos, sin entrar en descripciones sumamente detalladas de los mecanismos subyacentes. Esta idea se puede enmarcar en un movimiento generalizado de abstracción en la física que comienza con la *Mecánica Analítica* de Lagrange y supone:

> un declive de la justificación empírica *y* metafísica de los conceptos y de las leyes que los combinan. […] Los 'primeros principios' de la mecánica devienen axiomas *formales* de la ciencia en lugar de leyes materiales de la naturaleza (Pulte 2009).

Este enfoque de máxima generalidad en el cual se deja parcialmente de lado la descripción empírica y metafísica subyacente a los principios que estructuran la física matemática puede entenderse también como una forma de hacer física o mecánica, o sea, como un método. Aunque resulta bastante común que las elecciones metodológicas estén conectadas con perspectivas filosóficas, tal y como hicimos en lo que respecta al método geométrico, consideraremos de momento este enfoque como un método propio de una determinada manera de hacer y entender la física. De hecho, Poincaré lo caracteriza como un nuevo modo de hacer física que sustituye el enfoque anterior, conocido como la 'física de las fuerzas centrales' (cf. Poincaré 1905, 171 y ss). Y esta es la forma en que los caracteriza y refiere la transición de uno a otro:

> Renunciamos a penetrar en detalle la estructura del universo, a aislar las piezas de este vasto mecanismo, a analizar una a una las fuerzas que lo ponen en marcha y nos contentamos con tomar como guía ciertos principios generales que tienen precisamente por objeto dispensarnos de este estudio minucioso (Poincaré 1905, 175).

En definitiva, la física de las fuerzas centrales proporciona sus explicaciones en términos de masas y fuerzas en interacción. En cambio, la física de los principios subsume los fenómenos bajo principios generales que son la guía fundamental para comprenderlos.

De este modo, las teorías físicas pueden entenderse como estructuras sofisticadas que son definidas (entre otras cosas) a partir de los principios. La idea de fondo consiste en subsumir varios hechos experimentales o leyes empíricas bajo principios formulados en un abstracto lenguaje matemático que expresa una estructura que puede ser común a varias teorías científicas.

La teoría de Maxwell es un prototipo destacado de esta nueva física matemática porque este autor no se preocupa de los constituyentes últimos de la materia o del éter:

> ¿Qué es el éter, cómo están dispuestas sus moléculas, se atraen o se repelen? Nada sabemos de ellas; pero sabemos que este medio transmite a la vez las perturbaciones ópticas y las perturbaciones eléctricas; sabemos que esta transmisión debe hacerse conforme a los principios generales de la mecánica y esto nos basta para establecer las ecuaciones del campo electromagnético (Poincaré 1905, 127).

El ejemplo de Maxwell le sirve a Poincaré porque la suya es una teoría cuyos conceptos fundamentales (campo, carga, corriente) tienen en esencia un significado macrofísico o macroscópico y no solo porque desconocía hasta qué punto eran aplicables en escalas inferiores, sino – y esto lo más relevante desde la perspectiva metodológica – porque su idea de modelos de éter es la de ilustrar la teoría y no la de responder al cuadro microfísico subyacente. La importancia es la compatibilidad con las ecuaciones, con la forma matemática de la teoría, con sus principios generales. Como describe Giedymin, para la física de los principios el método consiste en

> principios matemáticos abstractos, a menudo sofisticados que son usados para condensar las leyes empíricas o los hechos experimentales comunes a varias teorías (Giedymin 1982, 44).

Esto supone que la misma teoría o estructura es compatible con diferentes modelos físicos. Los principios, por un lado, y el contenido observacional del que la teoría puede dar cuenta, por el otro, son los componentes de la teoría, de tal manera que los principios estructuran la teoría y, precisamente, a la pregunta de qué son los principios, Poincaré proporciona la misma respuesta que había dado con respecto a los axiomas de la geometría: "son convenciones" (cf. Poincaré 1905, 207). De esta forma, tal y como ocurría en geometría, las convenciones definen estructuras, que son, en definitiva, los marcos conceptuales dentro de los cuales realizamos nuestros análisis empíricos. Si pensamos, por ejemplo, en el caso de la mecánica newtoniana, las leyes del movimiento son principios entendidos en el sentido de Poincaré, o sea, convenciones (cf. Poincaré 1902, 110-147). El primer principio (el de inercia) establece el movimiento privilegiado y cualquier alteración de este movimiento de referencia significa que hay una fuerza actuando (en función de la segunda ley del movimiento) y esta puede ser medida (precisamente del modo en que expresa la segunda ley):

> Dentro del marco definido por las leyes de Newton, la investigación de cualquier sistema en interacción puede partir del modelo idealizado más simple y cualquier desviación del comportamiento ideal es informativa (de Paz y DiSalle 2014, xiii).

Gracias a esta concepción de los principios como definitorios de marcos conceptuales, podemos decir que es posible extender la visión estructuralista, al menos en lo que concierne a la perspectiva metodológica, a su filosofía de la física. Pero esto no significa, como hemos señalado más arriba, que el estatuto epistemológico de la física y de la geometría en cuanto a aquello que estas nos permiten conocer del mundo sea exactamente el mismo. La razón es que en las disciplinas físicas el experimento juega un papel mucho más importante que en geometría:

> Entendemos ahora por qué la enseñanza de la mecánica debe permanecer experimental. Solo así podremos comprender la génesis de esta ciencia y esto es indispensable para la comprensión completa de la ciencia misma (Poincaré 1902, 165).

Consecuentemente, existen diferencias entre la geometría y la física, pese a la similitud de su método, y al uso de la noción de convención tanto para los axiomas de la geometría como para los principios de la mecánica.

Consideraciones finales

El objetivo fundamental ha sido establecer una línea que nos permitiera comprender y vincular el convencionalismo geométrico con el físico-mecánico, y esto ha sido posible a través del análisis del método utilizado por Poincaré para aproximarse a estas disciplinas y su caracterización como estructuralista en el sentido de la metodología estructural en matemáticas.

Sin embargo, el título de este artículo además de a estructuras y convenciones, de las que hemos hablado tanto en lo relativo a la geometría como a la física, hace también referencia a hipótesis. Esta referencia no es en modo alguno accidental, no se trata de una tríada aleatoria, sino que, al igual que hay una relación entre el enfoque metodológico estructuralista y la filosofía de la convención, existe también una relación con la noción de hipótesis de la que aquí queremos dejar constancia, pese a que el análisis más pormenorizado de este vínculo quede para otro lugar.

La noción de hipótesis es clave en el marco del pensamiento de Poincaré y de su aproximación a la ciencia y esto queda patente ya desde el título de su primer libro filosófico, *La science et l'hypothèse*. Pero incluso antes de la publicación de esta obra podemos ya constatar la relevancia de una noción que irá ganando peso a lo largo de sus escritos. En 1887 Poincaré publica un artículo titulado "Sur les hipothèses fondamentales de la géométrie". Se trata de un escrito técnico, en el que aborda cuestiones relativas a la teoría de grupos y en el que muestra, por primera vez, la posibilidad de elegir entre diferentes grupos para geometrías de dos dimensiones. Es probablemente en razón de su carácter técnico, desde el punto de vista matemático, por lo que no fue publicado en ninguna de sus obras más filosóficas. Sin embargo, su objetivo es filosóficamente muy relevante, pues se trata de determinar qué tipo de proposiciones son aquellas que se sitúan a la base de la geometría. Y ya desde su título – que sin duda es una clara referencia a la conferencia de habilitación de Riemann de 1854 *Ueber die Hypothesen welche der Geometrie zu Grunde liegen* – califica las proposiciones fundamentales de esta disciplina precisamente como hipótesis. En este artículo no introduce aún la noción de convención, pese a que el vocabulario convencionalista y, fundamentalmente, la posibilidad de elección entre diferentes geometrías se encuentran presentes a lo largo de todo el texto. Ni que decir tiene que el enfoque metodológico es claramente estructural, pues es a partir de la estructura de diferentes grupos como caracteriza las diferentes geometrías.

El objetivo de Poincaré al calificar como hipótesis las proposiciones fundamentales de la geometría es cuestionar su carácter auto-evidente como axiomas. No se trata de verdades a priori, sino que son simplemente proposiciones hipotéticas entre las cuales podemos elegir para formar un grupo u otro y la elección de hipótesis conecta así esta palabra con la noción de convención, las cuales, frente a la auto-evidencia de los axiomas tienen el carácter de ser auto-impuestas y, sin embargo, comparten con los axiomas el poder estructurador, que implica que una vez elegidas, conforman un marco conceptual determinado.

Además, en la introducción de su primer libro filosófico Poincaré explicita el vínculo que existe entre la noción de convención y la noción de hipótesis:

> Veremos así que hay muchas clases de hipótesis […] que otras, por fin, no son hipótesis más que en apariencia y se reducen a definiciones o a convenciones disfrazadas. Estas últimas se encuentran sobre todo en matemáticas y en las ciencias afines. De ellas, estas ciencias toman su rigor. (Poincaré 1902, 2).

A partir de esta cita podemos interpretar que, de manera general, las convenciones son hipótesis (al menos en apariencia). Cuando pensamos lo que esto significa en relación con los principios de la física, lo que se pone de manifiesto es su carácter hipotético, su carácter provisional y no de verdades definitivamente asentadas. Pues los principios, al tener el estatuto de convenciones se caracterizan precisamente por no ser verdaderos ni falsos, dado que son el resultado de grandes abstracciones y generalizaciones y en la medida en que no dan cuenta de los cuadros ontológicos subyacentes, no pueden ser establecidos de manera definitiva para las ciencias de la naturaleza.

De hecho, tratar los axiomas de la geometría y los principios de la mecánica como hipotéticos forma parte de una tendencia general en el siglo XIX cuyo rastro puede seguirse a través de autores como Bernhard Riemann o Carl Neumann:

> La distinción de Newton entre leyes del movimiento o axiomas e hipótesis me parece insostenible. La ley de inercia es una hipótesis (Riemann 1876, 525).

> Tendremos que conceder que para esos principios o hipótesis [de la física…] no puede hablarse de corrección o incorrección, de probabilidad o improbabilidad (Neumann 1870, 12-13).

O sea, tal y como el enfoque conceptual de la matemática representado por la metodología estructural es también un desarrollo del siglo, también lo es la consideración de las proposiciones que se sitúan a la base de las ciencias como hipotéticas. Y es precisamente en la confluencia de estas dos tendencias, donde cabe situar el convencionalismo de Poincaré, tanto geométrico como físico-mecánico.

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