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The unnamed structuralism of four nineteenth-century philosopher-physicists

Olivier Darrigol

CNRS: UMR SPHere¹

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

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, *Wissenschaftliche Abhandlungen*, 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 orga-

nized, 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 *Gestaltpsychologie*)”), 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-

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

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 Abhandlungen*, 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 particulière, 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

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 ‘comparierte 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); Guillaume 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 early 1910s 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 1918, 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

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 word "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 early 1920s 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.

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 aux 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 Schlegel'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, heteroclitic 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

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

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 les 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

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 Entwicklung 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

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*; John Worrall, “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

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 $\mathbf{D} = \epsilon \mathbf{E}$, 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 Laplace 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

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.

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

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

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

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}_{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.

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-

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 monocyclischer 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* (1892), 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-

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-medizinischer 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** [*Größe*] 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 *Größenlehre* [sic] in homage to Leibniz's *Scientia 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

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

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:

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-

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.

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 *rappports 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 *rappports 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.

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 world. 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 con-

junction of this structure with conventions of measurements and an (a posteriori) estimate of the domain of validity. The *rappports 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 exist-

ence. 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

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

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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