Chapter 17
Scientific Reasonableness and the Pragmatic Approach to the Unity of Science

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We are the heirs of three hundred years of rhetoric about the importance of distinguishing sharply between science and religion, science and politics, science and art, science and philosophy, and so on. This rhetoric has formed the culture of Europe. It made us what we are today (...) But to proclaim our loyalty to these distinctions is not to say that there are 'objective' and 'rational' standards for adopting them.
(Richard Rorty 1980, pp. 330–331)

17.1 Introduction

The question of the unity of science is one of the most important issues that has concerned the modern philosophy of science from the beginning. The idea of Unified Science was so important for the Viennese neo-positivists that, from 1933 until its dissolution in 1938, the Vienna Circle edited a collection called *Einheitswissenschaft* with publications of several of the most significant members of the neo-positivist stream. Moreover, already in the USA, the journal *Erkenntnis* changed the name of its number 8, 1939–1940, into *The Journal of Unified Science*, and, finally, an *International Encyclopedia of Unified Science*, that survived until the late 1960s of the past century, replaced both of them. Twenty monographs were published in this *Encyclopedia* in two volumes, from 1938 until 1969; among

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them Hempel's *Fundamentals of concept formation in empirical science*, 1952, and Kuhn's *Structure of Scientific Revolutions*, 1962.

Rudolf Carnap's phenomenalism in *Der logische Aufbau der Welt*, 1928, and Vienna-Circle's physicalism in the 1930s faced the philosophical justification and explanation of the unity of science, that constituted the main aim of the *scientific world-view*, the philosophical programme proposed by Carnap, Hahn and Neurath in the 1929 foundational document of the Vienna Circle: *Wissenschaftliche Weltauffassung-Der Wiener Kreis*. The neo-positivist concern with the unity of science was intended to undermine Wilhelm Dilthey's distinction between natural sciences and humanities, the so-called *Geisteswissenschaften*. Carnap's *Aufbau* particularly was a philosophical monument erected from a logical positivist perspective in favour of the unity of science.

Verstehen vs. *Erklären* (Understanding vs. Explanation) was the contraposition between *Geisteswissenschaften* on the one side, and natural sciences on the other. This contraposition based on the assumed impossibility for the *Geisteswissenschaften* to show an empirical success comparable with the impressive success that the natural sciences allegedly were able to show since the outset of the Scientific Revolution. Thus, in spite of the intended purposes of Diderot and D'Alembert's *Encyclopedie*, 1740, to defend the Unity of Culture, a large tradition supporting this contraposition arose with Johann Gustav Droysen's *Grundriss der Historik*, 1850, Max Weber and Wilhelm Dilthey. The discussion was continued in the second half of the twentieth century by Georg Henrik von Wright, Hans Georg Gadamer and Jürgen Habermas, among others.

The Understanding/Explanation controversy, i.e. the debate *Geisteswissenschaften* vs. *Naturwissenschaften* arose as a reaction of the German historian and social philosophers against the positivist view prompted by Auguste Comte in the philosophy of sciences, by Herbert Spencer in the methodology of sciences and by Adolphe Quetelet in the statistical-sociological sciences. Quetelet, Comte and Spencer broke away from a bi-millenary tradition of Unified Western Culture that not contemplated any substantial difference between humanities and natural sciences, no privileged access to reality was warranted by the natural sciences. In this tradition natural sciences did no enjoy any special status in the whole of culture. Thus the question is whether this break with the tradition of Unified Culture in Europe, and the subsequent reaction introducing a fundamental distinction between humanities and natural sciences, was justified.

In this paper I am going to proceed in three steps. Firstly, I will point to the fact that Carnap's approach to the Unity of Science – a view according to which it was legitimate to give up the contraposition between *Naturwissenschaften, Psychologie* and *Geisteswissenschaften*-, grounded on the philosophical mistake that it was possible to provide a sound explanation for the *foundation* of the whole science on a unique firm basis. Moreover, the subsequent Vienna-Circle's physicalist attempt to save the situation was reasonably rejected by contemporary philosophers of science like Popper and Fleck, and, some years later, by the stream of methodologists that doubted the existence of a neutral empirical basis, and in general by the post-positivist epistemologists.

My second argument will be a more philosophical one: I will treat the question of whether the Unity of Science can be rescued by mimetically applying the method of natural sciences to social sciences and humanities. In order to answer these questions I will scrutinize the alleged overwhelming success of classical science. In particular I will point to the so-called threefold breaking-off of determinism. Moreover I will deal with the question about whether or not the natural sciences do harbour some kind of privileged and exclusive method for the access to reality. I will argue that, as contemporary physics shows, every form of scientific creativity, let us call it *induction, abduction* or *preduction*, only provides with means that allow us to deal *fallibly* with Nature.

Finally, if I am right, and the ideal of secure science reveals itself merely as a myth of rationalism in our scientific culture, I will propose to replace in the realm of science the requirement of rationality by that of *reasonableness*. Reasonableness is a weaker demand than rationality; it is neither tied to the idea of truth as the aim of science, nor to the existence of a secure and unique scientific method. But it is a guarantee that the justification of our conjectures, decisions, and, if possible, even of our fallible beliefs, is based on critical discussion and argumentation. This way makes superfluous any sharp distinction between natural sciences and humanities, but also any superimposed assimilation of the *Geisteswissenschaften* to the natural sciences, either by the way of the foundation of the different sciences on a common ground, or by the reduction to a fictitious physicalist language, or by a mimetic assimilation of the "superior" method of the natural sciences. To sum up, I conceive of the question of the unity of science as a particular case of the unity of Western culture from a pragmatic viewpoint in contemporary philosophy.

Since the defence of the unity of science amounts to overcoming the idea of the methodological dualism *natural sciences/humanities*, I will be first concerned with the task of surmounting the dualism *via* the neo-positivist foundationalist programme of the Unified Science. This is what I am going to do in the third section. In the next step I will face the question of whether Newtonian mechanics was justifiably the model to follow in social sciences and humanities. The existence of a unique proper method of the natural sciences that should be assumed by the social sciences, in order to be considered properly scientific, will be the main topic of the fifth section. Section 6 finally is a pleading on behalf of reasonableness in the sense given above. It is a consequence of the failure of imposing the idea of Unified Science either by postulating the existence of a common ground for natural and social sciences, or by claiming a methodological reduction of the social sciences to the natural sciences.

17.2 One Culture or Two Cultures: This Was the Question

2.1. The question of whether there was a need to differentiate between natural sciences and humanities arose in Western philosophy mainly as a reaction against the positivist philosophy that defended a monist approach to science and humanities

consisting in a kind of reduction of the social sciences and humanities to the natural sciences.

A *natural* monistic approach to sciences and humanities dominated nevertheless practically the preceding bi-millenary tradition in Western culture. Indeed a certain *unifying encyclopaedism* was the current way of development of the occidental culture. Beginning with Plato and Aristotle in the fourth century B.C., who practiced an encyclopaedic mastering of all the branches of the Hellenic world: from astronomy to ethics, from politics to logic and mathematics, from rhetoric to zoology, etc.

In the fifth century Martianus Capella fixed in his *De nuptiis Philologiae et Mercurii* the seven liberal arts that in the Middle Age constituted the *trivium* (grammar, rhetoric and dialectic) and the *quadrivium* (arithmetic, music, geometry and astronomy). In the twelfth century, Dominicus Gundissalinus (also known as Gondisalvi and Gundisalvo), an important member of Toledo's translation school, extended in his *De divisione philosophiae* the *quadrivium* to encompass physics, psychology, metaphysics, politics and economics, thus conferring to it a more universal character.

In the seventh century Saint Isidoro of Seville compiled in 20 books in his encyclopaedic work *Etimologiae* the different branches of ancient knowledge.

In the twelfth and thirteenth centuries Robert Grosseteste (1175–1253) contributed to philosophy, theology, physics (optics and cosmogony), geometry, and anticipated clearly Galilei's view on the applicability of mathematics to Nature's knowledge: *It is of the greatest usefulness to take into consideration lines, angles and numbers because without them it is impossible to know anything about natural philosophy*, or *All the causes of natural effects have to be given by means of lines, angles and numbers*. His disciple Roger Bacon (1214–1294) devoted also to theology, philosophy, the methodology of sciences – he introduced the expression *scientia experimentalis*, the theory of light and vision, geography, alchemy and astrology, and like his teacher maintained the viewpoint that *It is impossible to know the things of this world unless you know mathematics*, a view that he completed with a declared experimentalism: *Everything depends on experience.* Bacon's *Opus maius* is considered a kind of encyclopaedia as well. Finally *Doctor Universalis* Albertus Magnus (1193–1280) appears as a paradigm of encyclopaedic wisdom. He undertook the synthesis of Plato's and Aristotle's philosophical thought, but he was also a devoted zoologist, a botanist, and a chemist as well.

In the Modern Age grew a tendency to specialization, in a certain way mitigated by the humanist movement in the Renaissance. Indeed, Galilei and Kepler were mainly physicists and mathematicians. But the starter of the astronomical revolution Nicolaus Copernicus was still a typical medieval scholar. He studied theology, canonical law and medicine, was a practising physician, but he was also a philologer, and an economist, besides being a mathematician and an astronomer.

The traditional view of treating all cultural activities at the same level culminated with André-Marie Ampère. Worldwide known as the founder of electrodynamics, he was also an outstanding philosopher of science, as his 1834 *Essai sur la Philosophie des Sciences* shows. In spite of being a contemporary fellow of Comte and Quetelet, Ampère did not vindicate any special or privileged status for the natural sciences.

He proposed in 1830 a division of all our knowledge in two kingdoms: one comprising all truths related to the material world – *cosmological sciences*, the other dealing with human thought and societies – *noological sciences*. Only the object of investigation distinguished both kinds of sciences, since for Ampère the kingdom of human sciences shows structural features compatible with those of the natural sciences. The classification of the humanities or noological sciences is point to point structurally comparable with the classification of the natural or cosmological sciences.

Both kingdoms are identically structured. The kingdom of cosmological sciences incorporates the classification of all mathematical, physical and natural sciences known at the beginning of the nineteenth century. The classification of the noological sciences proceeds in two sub-kingdoms: the kingdom of the noological sciences properly, and the kingdom of the social sciences. The first ones encompass both the philosophical and the dialegmatic sciences, whereas the second ones encompass both the ethnological and political sciences. Each one of these four sciences is subdivided in two branches, and these in two more, etc., so that there result sciences of first, second and third order, among which we today identify: psychology, metaphysics, ethics, literature, pedagogic, ethnology, archaeology, history, military art, social economics, politics, logic, methodology, elementary ontology, natural theology, theodicy, etc.

Ampère (1834, pp. 14–17) explains not only the legitimacy of the noological sciences, but also the intricate relationships existing between both kinds of kingdoms:

> How can we not see the analogy that exists between the mathematical sciences and the sciences relative to the inorganic properties of bodies? And between these sciences and those dealing with organic beings, and between the latter and the study of the human faculties? And finally, is not the connection of such study with the study of the language of monuments, letters and the fine arts also evident, and of these to the social sciences? (p. 17)

2.2. An opposite stance was prompted by Quetelet, Comte and Spencer, who broke with the traditional view of the Unified Western Culture, thereby promoting a radical monist view of the Unified Science that not only was the root of the neopositivist doctrine of the Unity of Science, but also motivated the dualist reaction of the contraposition humanities/natural sciences, and the understanding/explanation controversy.

In fact, Comte defended the rejection, that later constituted a fundamental point in the Vienna Circle, of metaphysical thought, and the mimetic application to sociology of the mathematical approach to mechanics as well. Spencer maintained on his side the view of the existence of a method for the discovery of truths characteristic of all sciences. And Quetelet finally applied the probability calculus to the study of social phenomena, thus giving birth to mathematical statistical investigations. *Mundum numeri regunt* was Quetelet's motto.

Auguste Comte's *positive philosophie* was an attempt of encyclopaedic classification of all sciences, from mathematics to *social physics*, pointing to their respective interrelations, and to the hierarchic *unity* of the whole science as well, from which theology and metaphysics are completely discarded. The distinctive

character of Comte's positive science is the rejection of the search of essences, first and final causes. But Comte introduced as well the term *sociology* in the sense of *social physics*, to wit (Comte 1825 [1854, p. 150]) as

> a science that has as its proper object the study of social phenomena, to be considered in the same spirit as astronomical, physical, chemical and physiological phenomena, that is to say as subject to natural, invariant laws the discovery of which is the main aim of its research.

In a similar direction Herbert Spencer undertakes a classification of sciences, from Mechanics, Physics and Chemistry to Astronomy, Geology, Biology, Psychology, Sociology. The importance of Spencer's *monist* approach to the Unity of Science lies in the fact that *the same scientific method* is assumed to be shared by all the sciences: *induction*. Thus sociology has the same inductive character as biology and psychology, and it consists (Spencer 1876, pp. vii–viii) of

> the empirical generalizations that are arrived at by comparing different societies and successive phases of the same society.

Adolphe Quetelet was not a philosopher. But he had implicitly assumed a philosophy of science consisting in the application of a quantitative method for the discovery of the laws of social human behaviour. Quetelet called first this science *social mechanics*, whose aim was the discovery of conservation principles, or natural laws, in human affairs, in analogy to physical mechanics. In 1835, *Sur l'homme et le development de ses facultés*, he changes its name and indirectly assumes Comte's expression of *social physics*. Indeed as Lottin (1912, p. 382) claims,

> One of the characteristic features of Quetelet's spirit is his tendency to find for the human species laws *analogous* to the ones that govern the physical world. Imbued with Laplace's *celestial mechanics*, Quetelet sought to found an *analogous science for those phenomena relative to the human species*; he wanted to create a *social mechanics* for the study of the laws governing the *social system*.

Nevertheless, one important point must be taken into account. Contrary to Comte, who rejected the application of probability calculus to sociology, Quetelet makes of this calculus the fundamental tool of his scientific activity, thus creating the science of statistical sociology. The question is that the application of probability to the investigation of social phenomena is as old as the doctrine of chances itself is. And here lies the origin of the so-called *science of theoretical statistics*. From its very beginning (Cf. Rivadulla 1991, 1995), the doctrine of chances was applied to the calculation of probabilities of life (Christian and Lodewijk Huygens, Leibniz, Halley, etc), to the calculation of life insurances (Jan de Witt), and even to justice (Nicolas Bernoulli, *De usu artis conjectandi in iure*, 1709). This was the ground on which it based the extraordinary extension of the application of mathematical probability to every kind of social researches. Déparcieux, Lacroix, Condorcet, Laplace, who in the Introduction to his *Théorie analytique des probabilités*, pp. LXX–LXXII, proposed to apply to the moral and political sciences the method used in celestial mechanics, were some of the most significant theoreticians preceding Quetelet, and the expressions *Political Aritmetick* (Petty 1960), *Social mathematics* (Condorcet, 1793) and *Social mechanics* (Quetelet 1831) form a chain that concluded with Comte-Quetelet's *Social physics*.

2.3. In this situation Johann Gustav Droysen (1808–1884) proposed to separate the aims of natural sciences from those of history. The aim of the first ones was *explanation*, whereas history, and by extension all humanities or *Geisteswissenschaften*, were committed to *understanding*. Understanding the *intention*, i.e. the search for *intelligibility* of human actions, was opposed to provide *causal explanations* of natural phenomena. Here roots the controversy *dualism* vs. *monism* in the methodology of scientific Western thought.

Droysen's view paved the way for a long and fruitful stream of thinkers in social philosophy, among which stand out Wilhelm Dilthey's sharp distinction between *Naturwissenschaften* and *Geisteswissenschaften* (humanities, history, sociology, according to Carnap 1928, §23) in his *Einleitung in die Geisteswissenschaften*, Leipzig 1883 (1922), Hans Georg Gadamer's development of interpretative hermeneutics -as Georg Henrik von Wright (1971, pp. 29–31), asserts, "In explicit opposition to positivism's idea of the unity of science, hermeneutic philosophy defends the *sui generis* character of the interpretative and understanding methods of the *Geisteswissenschaften*", von Wrights viewpoint of *understanding* as "a prerequisite of every explanation, whether causal or teleological", and Jürgen Habermas's modern *theory of communicative action*.

17.3 The Neo-Positivist Approach to the Unity of Science

3.1. The idea of the Unity of Science is, together with the doctrines of the foundation of knowledge and the meaninglessness of metaphysics, one of the three main theses of logical positivism. This idea was proclaimed – "Als Ziel schwebt die *Einheitswissenschaft* vor" – as fundamental in the programmatic document of the Vienna Circle, published in 1929 on occasion of the First Congress on the Epistemology of Exact Sciences, to be hold in Prague. Rudolf Carnp's *Intellectual Autobiography* (1963, p. 52. My italics, A. R.) reminds us of this:

> In our discussions, chiefly under the influence of Neurath, the principle of the unity of science became one of the main tenets of our general philosophical conception. This principle says that the different branches of empirical science are separated only for the practical reason of division of labor, but are fundamentally merely parts of one comprehensive unified science. *This thesis must be understood primarily as a rejection of the prevailing view in German contemporary philosophy that there is a fundamental difference between the natural sciences and the Geisteswissenschaften* (literally 'spiritual sciences', understood as the sciences of mind, culture, and history, thus roughly corresponding to the social sciences and humanities). In contrast to this customary view, Neurath maintained the monistic conception that everything that occurs is a part of nature, i.e., of the physical world. I proposed to make this thesis more precise by transforming it into a thesis concerning language, namely, the thesis that the total language encompassing all knowledge can be constructed on a physicalist basis.

Rudolf Carnap's *Aufbau*, 1928, intended proposal was to present a *rational* and *logical reconstruction* of the real process by which the concepts of science and of

every day life are formed. Carnap's main thesis is that any concept – object, in his terminology – is constituted by direct application of Russell's class and relation logic to the domain of the *immediately given*. The result is a *constitutional system* of all concepts (objects), independently of the scientific branch to which they may belong: natural sciences, psychology, cultural sciences. Following, if all concepts of science can be ordered in a unique constitutional system in a way that they are also reducible to the fundamental concepts of the system, and all scientific sentences are also reducible step by step to sentences about these fundamental concepts, then we have here the philosophical explanation of the unified character of science. Reducibility to sentences about fundamental concepts of the constitutional system meant *verifiability*.

The second step took place with the transition from *Aufbau*'s *phenomenalism* to *physicalism* in the very beginning of the thirties. As Rudolf Carnap (1934, pp. 248–249) recognizes, both expressions *physicalism* and *unified science* were proposed by Otto Neurath in the Vienna Circle. According to this new view, the unity of science grounds on the possibility of expressing all scientific sentences in the *physicalist* language. As Carnap (1963, p. 52) claims in "Physicalism and the Unity of Science", in his *Intellectual Autobiography*, "the total language encompassing all knowledge can be constructed on a physicalist basis". In this language are expressible the propositions of biology, but also the sentences of sociology and psychology are translatable into the physicalist language (Cf. Carnap 1931, 1932). Neurath (1931, pp. 408, 422), for whom sociology, *social behaviourism*, is taken in a wide sense, encompassing economics, ethnology, history, ethics, law insofar as they are free of metaphysics, shares also with Carnap the idea of the formulation of all sciences in the physicalist language. Indeed in 1935b, p. 6, he claims

> All sciences must be capable of formulation in the universal language of physics. There is no room, in this respect, for the distinction between natural sciences and sciences of the spirit. Psychology studies the behaviour of human beings that is intersubjectively describable in physical language, i.e. *behaviourism*. Sociology studies the behaviour of human groups, i.e. *social behaviourism*.

Neurath (1935b, p. 7) concludes

> These different scientific disciplines, unified in the same language, associated so as to carry out universally valid predictions, constitute *unified science*.

3.2. The idea of an *encyclopaedia of unified science* had been proposed 1 year earlier by Neurath (1935a) during the *Prager Vorkonferenz der internationalen Kongresse für Einheit der Wissenschaft*, which he (1935b, p. 54) also refers to in the following terms:

> The best model of our scientific ideal cannot be the 'system', but only the *Encyclopaedia* methodically elaborated with the means of the modern logic of science.

Carnap (1934, p. 261) supported Neurath's view of an *encyclopaedia of unified science* that culminated in 1938 with the publication of an *International Encyclopaedia of Unified Science*. In the very first pages of this *Encyclopaedia*, Neurath (1938) claims that this *Encyclopaedia* continues the work of the

French Encyclopaedists, but also improves the work of Comte, Spencer, Mill.[1] Nevertheless, from a theoretical viewpoint no new ideas in relation to the Unified Science appear in Neurath's 1938 paper. Indeed he repeats on p. 20 the view that

> An encyclopedia (in contradistinction to an anticipated system or a system constructed a priori) can be regarded as the model of man's knowledge.
> An encyclopedia and not a system is the genuine model of science as a whole. An encyclopaedic integration of scientific statements,. . ., is the maximum of integration which we can achieve.

Co-operation of scientists makes it possible that the *Encyclopaedia* achieves its aim of integration of the scientific disciplines, i.e. the unification goal.

3.3. Nevertheless in spite of the enthusiasm displayed by Neurath and Carnap the implementation of the unity of science and the realization of the *Encyclopaedia* was not very successful. The *Journal of Unified Science* did not survive its first number, and the *International Encyclopaedia of Unified Science* only published two volumes in 32 years, 20 monographs in total, very valuable each one from a philosophical viewpoint, but with scarcely any relation to the announced aim of the *Encyclopaedia*.

The ideal of Unified Science encountered many problems indeed. From the very beginning this ideal was tied to the problem of the foundation of knowledge. In his Intellectual Autobiography, Carnap (1963, p. 50) claimed:

> I believed that the task of philosophy consists in reducing all knowledge to a basis of certainty. Since the most certain knowledge is that of the immediately given, whereas knowledge of material things is derivative and less certain, it seemed that the philosopher must employ a language which uses sense-data as a basis.

Moreover one of the most characteristic theses of logical positivism, that has mainly contributed to its presence in the history of modern philosophy, the thesis of the elimination of metaphysics, which also partially depended on the thesis of the foundation of knowledge, was simply abandoned, once the unreasonableness of conclusive verification was recognized. [Carnap's later replacement of conclusive verification by gradual confirmation, leading to *inductive logic*, an enterprise in which Rudolf Carnap wasted most of his precious time, was finally abandoned as well as unviable by contemporary philosophers of science.]

The proper foundation of knowledge, which according to Carnap (1931, p. 437) constituted also an urgent task of physicalism, has been one of the most criticized ideas in the post-positivist philosophy of science. Carnap's implementation of the foundationalist programme in *Aufbau* was intelligent but enormously artificial. And when *phenomenalism*, charged with nearly insurmountable philosophical problems, was abandoned on behalf of physicalism, and the reducibility to the fundamental concepts anchored in the elementary experiences that constituted the immediately given was transferred to reducibility or translatability to protocol sentences, the unanimity about the form and function of the protocol sentences disappeared from the bosom of the neo-positivist family. The fictitious character of the protocol sentences was a big handicap for the credibility of the whole enterprise, and the ambiguity, artificiality and vagueness of the *physicalist language* [for instance Carnap 1938, pp.

46–48, does not use any longer this expression, but *physical language*: "That sub-language of the language of science, which contains – besides logic-mathematical terms – all and only physical terms", where with "physical terms" Carnap understands "those terms which we need – in addition to logico-mathematical terms – for the description of processes in inorganic nature". Together with *biology in wider sense*, that constitutes the whole of the rest of the science, and which includes psychology, the social science, the humanities and the history, Carnap considers that the basis of a unity of language is given] did not either contribute to the feasibility of the whole enterprise.

If the troubles and indecisions in the neo-positivist house were not enough, the thesis of the foundation of knowledge, what he later connected to *the bucket theory of mind*, was thoroughly attacked from outside by Popper since his *Logik der Forschung*, 1935 on. Ludwik Fleck (1935, p. 64) declared as well the *complete sterility* of the whole *Erkenntnis* 1 and 2 neo-positivist deal. Norwood Hanson's thesis of the *theory-ladeness of observational language*, and the post-positivist rejection of the existence of a neutral observational language, contributed finally to the definite abandonment of the philosophy of logical positivism.

The difficulties with the realization of unified science must have been so big, that Joergen Joergensen's (1951, pp. 82–83) scepticism on the viability of the whole enterprise in the second volume of the *Encyclopaedia* does not leave any room to doubt:

> Strictly speaking, the thesis of physicalism cannot be considered proved until the reduction to the thing-language of the total number of the concepts of the natural and social sciences is made, which means, of course, never.
> As regards the question of the reduction of scientific theories to a few or even a single deductive system, the prospects are, in the opinion of logical empiricists, much darker than where the question of the reduction of concepts to the physical thing-language is concerned. Not even all physical laws can at present be included in a single deductive theory, and the prospects for a derivation of biological from physical laws -let alone a derivation of psychological or sociological laws from the physical plus the biological laws- are distant, although not hopeless.

3.4. In spite of the difficulties of the neo-positivist approach to the unity of science, a new attempt to rescue the encyclopaedic project of the Vienna Circle is being undertaken nowadays by Shahid Rahman, John Symons and others. To this respect they haven launched a new series: "Logic, Epistemology and the Unity of Science" *in the spirit of* Diderot and Neurath. Rahman and Symons (2004, p. 7) summarize this project in the following terms:

> we see our work, inspired by the Vienna Circle's promotion of the scientific attitude in philosophy, as a part of a very traditional, though radical, Enlightenment project. Like Diderot and D'Alembert before them, Neurath's vision of the Encyclopedia is a cooperative and ambitious enterprise. We would like to see our series continue this tradition. This is why we take Otto Neurath as the inspiration for this series and look to his initial statement of the Encyclopedia of the Unified Sciences as our model.

The inspirers of the renewed unified-science-movement are well acquainted with the difficulties the original project had to face, some of which I have presented in

point 3.3. above. But they are also aware of the new means available for the development of the renewed project. I welcome indeed the project, and in the encyclopaedist spirit of co-operation, I claim, from a pragmatic viewpoint, that the unity of science can be defended as a part of the unified western culture.

17.4 Was Newtonian Mechanics Justifiably the Model to Follow for Social Sciences and Humanities?

4.1. A close glance at the situation of modern physics shows that the methodological dualism *Naturwissenshaften* vs. *Gesiteswissenschaften* grounds on a deep mistake about the alleged privileged relationship of the mathematic-experimental sciences to reality.

Although the suspicion of the privileged position of these sciences in the realm of Western culture goes back to Galilei's work, there is no doubt that it reaches the climax with Newton. Already Bernard Fontenelle (1657–1757) in his *Eloge de Sir Isaac Newton*, read before the Académie des Sciences de Paris on November 22nd 1727, claimed that Newton was considered by his contemporaries as their chief and master, and that his *philosophy* was adopted in the whole of England. The French astronomer Alexis-Claude Clairaut (1713–1765) and D'Alembert, among others, qualified in the middle of the eighteenth century Newton's work as revolutionary. Moreover Laplace, Euler, d'Alembert and Lagrange contributed with the development of analytical mechanics in the eighteenth century to the enrichment and consolidation of Newtonian mechanics.

From a philosophical viewpoint the most important contribution to the popularity of Newtonian mechanics was Pierre Simon Laplace's (1749–1827) scientific determinism of his mechanistic world-view. In the Introduction to his *Essai philosophique sur les probalilités*, 1819, Laplace claimed that

> An intelligence that at any given moment knew all the forces that animate nature and the mutual positions of all the entities that compose it, if moreover this intelligence were vast enough to submit its data to analysis, it could embrace in the same formula the movements of the largest bodies of the universe and those of the lightest atom; nothing would be uncertain for it, and the future and the past would be present before its eyes.

According to Popper (1982a, p. 6)

> The fundamental idea underlying 'scientific' determinism is that the structure of the world is such that every future event can in principle be rationally calculated in advance, if only we know the laws of nature, and the present or past state of the world.

Since a deterministic world demands a deterministic theory, Newtonian mechanics would thus be a deterministic theory that truly represents the world.

4.2. The question is that for many reasons Newtonian mechanics is unable to accomplish this task. As Popper (1982a, p. 7) claims, "Newtonian mechanics does not entail 'scientific' determinism". First of all, because of the *problem of three or more bodies*. As Bruns already in 1887 proved, it is impossible to find out an analytical general solution to the problem of the determination of the movement of

three particles of non-negligible masses m_1, m_2, m_3 that attract themselves according to Newton's gravitational law. The problem is not integrable and shows a chaotic behaviour. This problem can be considered the origin of the studies of the so-called *deterministic chaos*. As a consequence, since Newtonian mechanics was not even able to provide analytical solutions to the Three-Body Problem in celestial mechanics, it is no right to consider it a deterministic theory. [By the way, deterministic quantum-mechanical Schrödinger's equation also lacks of analytical solutions for multi-electronic systems.]

The second shortcoming "deterministic" classical mechanics was faced with was due to its inapplicability to mass phenomena. Indeed it was meaningless to compute the motion equation for each one of a huge number of interacting constituents. Thus in order to deal with mass phenomena it was inevitable to resort to statistical analysis. At the end of the nineteenth century Ludwig Boltzmann (1844–1906), James Clerk Maxwell (1831–1879) and Josiah Willard Gibbs (1938–1903) developed the classical statistical mechanics as an application of probability to mechanical mass phenomena of discernible particles. When it was necessary to deal with indiscernible particles in the realm of quantum physics, Enrico Fermi (1901–1954), Paul Dirac (1902–1984), Satyendra Nath Bose (1894–1974) and Albert Einstein (1879–1955) developed quantum statistical mechanics already in the twentieth century.

Finally, the development of modern theoretical physics shows further shortcomings of classical mechanics. Indeed, relativity theory extends its applicability domain beyond that of Newtonian mechanics to objects with velocities comparable to that of light in the open space, and to very intensive gravitational fields, like in the proximity of black holes, where for instance time collapses. This means that in the proximity of massive bodies time flows slower than in other circumstances.

Moreover, if we pay attention to the Newtonian celestial model it can be claimed that it fails in domains *in which it should not fail, if it were true.* These failures must be considered more relevant than mere restrictions of its domain of intended applications. Indeed the Newtonian theoretical model was not only incapable to face old challenges like Mercury's perihelion advance. It was also incapable to give satisfactory answer to some intended applications like the light deflection by the Sun and the gravitational redshift. But what is of most philosophical relevance is the fact that the replacement of the Newtonian theoretical model did happen by means of theories for which the *predictive balance* was overwhelmingly favourable and they were *incompatible* with it. From this it follows that the Newtonian model had to be considered as an inadequate model to deal with gravitational phenomena, and that it is meaningless to claim that it does *represent* reality even approximately.

On the other side, quantum mechanics rejected the classical view that physical magnitudes can be measured with unlimited precision, since Planck's constant constitutes an inferior limit for the precision of every measure. Moreover, the probabilistic nature of quantum mechanics incorporated indeterminism into the subatomic domain.

The threefold break of determinism and the inapplicability of classical mechanics to domains where it should offer fruitful results, if it had been a true representation

of reality, makes evident that the expectancies woke up by it were not completely justified. Thus it does not seem justified the optimism of those like Quetelet and Comte who saw in the natural sciences, particularly in Newtonian celestial mechanics, a model to follow in social sciences and humanities.

What does it look like the issue of the existence of a *scientific method* in the physical sciences, which are the paradigmatic natural sciences?

17.5 Fallible Strategies as Means of Dealing with Nature in the Methodology of Science

Since the beginning of the methodology of science nearly 2,400 years ago, philosophers have been looking for different ways of scientific discovery. Aristotle's well known inductive inference was intended to provide a method of legitimatization of first principles. Plato preferred a different procedure, which can now be identified with Peircean abduction, as it aimed at the postulation of geometrical hypotheses in order to save the appearances presented by movements of the planets. Old astronomy from Plato until Kepler provides excellent examples of abductive procedures for the postulation of astronomical geometrical models.

From a logical viewpoint both induction and abduction are logically illegitimate. And a middle way like Bayesian probable inference has proven also to be untenable (Cf. Rivadulla 2004b). Nevertheless induction and abduction can legitimately be accepted as methodological strategies providing hypotheses that allow to deal fallibly with Nature. The only condition is to assume that scientific *ars inveniendi* is not submitted to rules, i.e. that there is no algorithmic procedure capable to produce secure knowledge. And this amounts to relinquishing to the quest for truth and certainty at theoretical level in science.

Furthermore there is a third strategy, commonly applied by theoretical physicists, but which philosophers of science seem not to have identified yet. I call it *theoretical preduction*, and it consists in a form of anticipative reasoning that starts from accepted results provided by the available theoretical background which are *methodologically* postulated as premises of the inferential procedure.

The products of preductive inference are theoretical models, factual hypotheses and theoretical laws, and they are fundamentally fallible constructs, since they depend on the assumed theoretical available background, which is not known to be true.[2] Many of them can be very successful indeed. Nevertheless it is not unusual that preduced constructs must be rejected as unviable ways of dealing predictably with Nature: Bohr's atomic model, Rayleigh-Jeans radiation law, Helmoltz-Kelvin gravitational collapse model of the stars' energy, etc are good examples of this. Occasionally empirical data do not oblige to the rejection of preduced theoretical constructs, but do put them seriously in jeopardy, for instance the solar neutrinos problem or the temperature of the solar crown for the case of current solar theoretical models.

Preduction and abduction oppose to each other in the methodology of science. Whereas by means of abduction, which is an ampliative inference, the inferred

hypotheses are suggested by available empirical data, by means of preduction the inferred hypotheses are deductively constructed on the basis of the available theoretical framework. The theoretical background is constituted by entire disciplines, like physics, as a whole, insofar their theories and theoretical constructs are assumed to be consistent with each other. Basically *preduction* is an extension of deductive reasoning to the context of scientific discovery.

In conclusion I claim that *induction, abduction and preduction are merely reasoning strategies we use in the methodology of physics, in order to provide predictive hypotheses that allow us to deal fallibly with Nature*. Nothing in the methodology of physics points to the existence of a unique and secure method that could be more or less mimetically assumed as a model to follow by social sciences and humanities. Thus if there is no such method, then it seems that it was neither necessary to plead for a dualist fundamental difference between hard and soft sciences, nor for trying a monistic methodology embodying social sciences and humanities and natural sciences.

17.6 Reasonableness and Rationality in Western Scientific Thought

On August 24, 2006, at the XXVIth General Assembly of the *International Astronomical Union* in Prague, astronomers voted a new definition of the term *planet*. As a consequence of this Pluto was removed from the list of solar planets, and reduced to the new category of *dwarf planets*.

Could not we philosophers follow the example of the astronomers and agree on a definition of (*scientific*) *rationality*? If we did, then we would be in a situation allowing us to determine if some decisions taken by scientists or scientific communities are strictly rational or merely reasonable. But as long as the meaning of *scientific rationality* is not agreed upon, I will have to indicate some features of rationality we are supposed to accept more or less. In order to do this I resort to Rorty (1991, p. 35), according to him

> In our culture, the notions of 'science', 'rationality', 'objectivity', and 'truth' are bound up with one another. Science is thought of as offering 'hard', 'objective' truth: truth as correspondence to reality, the only sort of truth worthy of the name. (. . .) We tend to identify seeking 'objective truth' with 'using reason', and so we think of the natural sciences as paradigms of rationality. We also think of rationality as a matter of. . . being 'methodical'. So we tend to use 'methodical', 'rational', 'scientific', and 'objective' as synonyms.

This is Rorty's *cliché* of rationality in the *strong* sense of the term. It meets the standards of rationality which any scientific realist would agree on. For instance Karl Popper (1983, pp. 6–7):

> By a rationalist I mean a man who wishes to understand the world, and to learn by arguing with others. (. . .) By 'arguing with others' I mean, more specifically, criticizing them; inviting their criticism; and trying to learn from it. The art of argument is a peculiar form of the art of fighting – with words, instead of swords, and inspired by the interest of getting nearer to the truth about the world.

> I believe that *the so-called method of science consists in this kind of criticism*. Scientific theories are distinguished from myths merely in being criticisable, and in being open to modifications in the light of criticism. They can be neither verified nor probabilified.

In a few words, Karl Popper's lemma about rationality is that *rationality is simply openness to criticism* (*op. cit.*, p. 27). With the particularity that for Popper *scientific method* is critical attitude about theories, hypotheses and conjectures, in clear opposition to verification and probabilistic methods. Thus Popper's idea of scientific rationality fits very well to Rorty's *rationality cliché*.

A general tendency to truth approximation and to the assumed existence of a *scientific method* were, according to Kitcher (1993), the general features of what he labelled the *legend of science*. Although the *legend* reached its climax at the times of logical positivism, Kitcher declares in the *Post Scriptum* of his book that the *legend* is not yet dead, for it was in general right about the features of science. It only needs a metamorphosis. But since according to him truth is the most obvious epistemic goal of science, and rationality is a notion that relates together goals and means, it becomes difficult to see how Kitcher's search of a middle way between rationalism and anti-rationalism could be successful.

Paul Thagard's (2004) view on scientific rationality fits also very well to the standards widely accepted by scientific realists: Besides practical goals, science aims at epistemic goals like truth and explanation. For him

> A person or group is rational to the extent that its practices enable it to accomplish its legitimate goals.
> And
> The occasional irrationality of individual scientists or groups is compatible with an overall judgment that science is in general a highly rational enterprise.

Contrary to these realist viewpoints, my reflections on Sections 4 and 5 above show that the quest for truth and certainty is not the aim of scientific research, since history shows that the *theory is not the space of truth*. Moreover, as it can be seen also from foregoing sections, the existence of a unique scientific method is illusory.

Indeed there are situations in science appearing as undoubtedly rational steps. For instance, as I have argued in Rivadulla 2004a, p. 418: "If we are given two theories, and one of them constitutes a limiting case of the other one, then we are in a privileged situation in order to make a rational choice between them. Indeed the existence of limiting cases in mathematical physics allows one to account for theory change as an intrinsically rational process." Nevertheless the claim that one theory constitutes a limiting case of another one, and that the decision on behalf of the other is strictly rational, has nothing to do with truth or certainty. Astonishingly *scientific realist* Karl Popper (1982b, pp. 29–30. My italics, A.R.) seems to share this point of view as his following words betray him:

> The decisive thing about Einstein's theory, from my point of view, is that it has shown that Newton's theory -which has been more successful than any other theory ever proposed- *can* be replaced by an alternative theory which is of wider scope, and which is so related to Newton's theory that every success of Newtonian theory is also a success for that theory, and which in fact makes slight adjustments to some results of Newtonian theory. *So for me, this logical situation is more important than the question which of the two theories is in fact the better approximation to the truth.*

Together with the use of rationality in strong sense, there is also a *weak* sense of rationality, which according to Rorty (1991, pp. 36–37) means the following:

> In this sense, the word means something like 'sane' or 'reasonable' rather than methodical. It names a set of moral virtues: tolerance, respect for the opinions of those around one, willingness to listen, reliance on persuasion rather than force. These are the virtues which members of a civilized society must possess if the society is to endure. In this sense of 'rational', the word means something more like 'civilized' than like 'methodical'. When so construed, the distinction between the rational and the irrational has nothing in particular to do with the difference between the arts and the sciences. On this construction, to be rational is simply to discuss any topic – religious, literary, or scientific – in a way which eschews dogmatism, defensiveness, and righteous indignation.

Besides the episodes of strict rationality I prefer to talk about *reasonability* instead of *rationality* even in Western science, not only in culture in general. Reasonableness and rationality complement each other indeed, since although what is reasonable is not always rational, what is rational is necessarily reasonable. But once we have given up the search for truth and certainty as aim in scientific inquiry, once we recognize that strictly rational moments are the exception and not the rule in science, reasonableness becomes a most common feature of scientific decisions. Since reasonability is guided by such pragmatic principles like critical discussion, fallibility, intersubjective agreement, predictive success, etc., it is a common feature of decisions both in natural sciences and humanities. This offers an argument on behalf of the unity of Western culture.

Notes

1. More on this point in Rahman and Symons (2004, Section 4).
2. I have already presented the idea of *theoretical preduction* in Rivadulla (2008) and (2009).

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