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TWO DOGMAS OF STRUCTURAL REALISM. A CONFIRMATION OF A PHILOSOPHICAL DEATH FORETOLD

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SUMMARY: Twenty years ago John Worrall offered an alleged non-standard viable form of scientific realism under the name *structural realism*. Structural realism was supposed to be both an alternative to standard scientific realism and a viable form of realism. The central questions addressed in this paper are what I call the two dogmas of structural realism: the idea that there is structure retention across theory change, and the idea that theoretical structures describe the world. Arthur Fine proclaimed that scientific realism was dead. I claim that Worrall's attempt to bring scientific realism back to life has failed.

KEY WORDS: scientific realism, no-miracle argument, pessimistic meta-induction, theoretical explanations, limiting cases

RESUMEN: John Worrall ofreció hace veinte años una forma no típica, supuestamente viable, de realismo científico a la que llamó *realismo estructural*. Se suponía que éste era una alternativa al realismo típico y una forma viable de realismo. Las cuestiones principales que discuto en este artículo son lo que denomino los dos dogmas del realismo estructural: la idea de que hay retención de estructura en el cambio teórico, y la idea de que las estructuras teóricas describen el mundo. Arthur Fine proclamó que el realismo científico había muerto. Yo afirmo que el intento de Worrall de resucitar el realismo científico ha fracasado.

PALABRAS CLAVE: realismo científico, argumento del no milagro, metainducción pesimista, explicaciones teóricas, casos límite

1. *Introduction*

The Colombian Gabriel García Márquez, who in 1982 was awarded the Nobel Prize for Literature, published in 1981 a novella entitled *Crónica de una muerte anunciada*. Translated into English as *Chronicle of a Death Foretold*, this novella is allegedly based on a real event that occurred in 1951. The brothers of Ángela Vicario announce their intention to kill Santiago Nasar for having dishonoured Ángela, who was returned to her parents by Bayardo San Román just few hours after the wedding.

"Realism is dead", proclaimed Arthur Fine twenty-six years ago in a conference on scientific realism held at the University of North Carolina. Arthur Fine begins his paper "The Natural Ontological

Attitude" (1984) with these three words. Amazingly, he announced the death of scientific realism at what was allegedly the high point of realism. Indeed, Hilary Putnam, of whom Jarret Leplin (1984a, p. 1) claimed that he inaugurated a new era of interest in realism with his declaration that realism is the only philosophy that does not make the success of science a miracle, had already published in 1981 *Reason, Truth and History*, in which he presented his then widely-defended form of realism, *internal realism*. Karl Popper also published in 1983 his influential *Realism and the Aim of Science*.

Unlike García Márquez's novella, nobody killed realism. The death of realism was due to natural causes. Fine considered that (standard) scientific realism was dead because the arguments in favour of realism provide no rational support for belief in realism. Indeed he claims that it is misleading to suggest that the only way to account for the success of the scientific enterprise is on a realist basis. Fine (1984, p. 84; 1986, p. 113) resorts to Larry Laudan, whom he states has shown that realism cannot be used to explain the success of science.

Fine's (1984, p. 84) interest, given that realism is dead, is to identify a suitable successor. This is also the purpose of John Worrall (1989). The difference between Fine and Worrall is that whereas the former (1984, p. 84) aims to present a viable non-realist position, a decent philosophy for post-realist times, Worrall wants to present under the name *structural realism* what might be called a decent non-standard viable form of scientific realism.

Two decades have passed since Worrall offered the first contemporary formulation of structural realism. This is a good occasion to analyse its place and viability in the epistemological debate in contemporary philosophy of science. But this paper is not intended to present the history of structural realism. Ladyman (2007) has already done this. My article is a critical contribution centred on what I affirm to be the two main dogmas of structural realism.

2. *Structural versus Standard Scientific Realism*

Let us assume that *standard* scientific realism is the view that currently-accepted theories are attempted descriptions of a reality lying "behind" the observable phenomena, and that what legitimises the assumption that these descriptions are at least approximately accurate is the empirical success of these theories. Thus if the main argument for scientific realism is that our present theories are so successful that they cannot have become so by chance, then, according to Worrall (1989, summary and p. 100), scientific realism faces a serious problem —that there have been very successful theories in the past which are now regarded as false. And in Worrall's view this constitutes the main argument against scientific realism.

The most powerful argument in favour of scientific realism is the no-miracle argument. Ilkka Niiniluoto (1999, p. 436), for instance, explicitly recognizes that scientific realism has been defended by an abductive no-miracle argument. Presented by Hilary Putnam —"The positive argument for realism is that it is the only philosophy that doesn't make the success of science a miracle" (1975, p. 73), and "the typical realist argument against idealism is that it makes the success of science a *miracle*" (1978, p. 18)— the no-miracle argument affirms, in Boyd's words, that "If scientific theories weren't (approximately) true, it would be miraculous that they yield such accurate observational predictions" (1984, p. 43).

Kepler and Galileo have already explicitly used the no-miracle argument to support the truth of the Copernican theory. Indeed, Johannes Kepler claimed in his *Mysterium Cosmographicum* that Copernicus "did not only deduce all the movements that have been registered since the most remote antiquity. He also predicted the future movements, and, although not with absolute precision, any case much better than Ptolemy, Alphonse and all the others" (1596, p. 75). On the next page, Kepler affirms: "Copernicus's principles, which constantly account successfully for so many phenomena, cannot be wrong" (p. 76).

Similarly, Galileo Galilei writes in *Considerazioni circa l'opinione copernicana*:

> He [Copernicus] began to investigate what the system of the world could really be in nature, no longer for the sole convenience of the pure astronomer, whose calculations he had complied with, but in order to come to an understanding of such a noble physical problem; he was confident that, if one had been able to account for mere appearances by means of hypotheses which are not true [Ptolemy's one], this could be done much better by means of the true and physical constitution of the world. Having at his disposal a very large number of physically true and real observations of the motions of the stars (and without this knowledge it is wholly impossible to solve the problem), he worked tirelessly in search of such a constitution. [. . .] through long sense observations, favourable results, and very firm demonstrations, he found it so consonant with the harmony of the world that he became completely certain of its truth. Hence this position is not introduced to satisfy the pure astronomer, but to satisfy the necessity of nature. (1615, p. 74)

> It is true that it is not the same to show that one can save the appearances with the earth's motion and the sun's stability, and to demonstrate that these hypotheses are really true in nature. But it is equally true, or even more so, that one cannot account for such appearances with the other commonly accepted system [Ptolemy's system]. The latter is undoubtedly false, while it is clear that the former, which can account for them, may be true. Nor can one or should one seek any greater truth in a position than that it corresponds with all particular appearances. (1615, p. 85)

The most powerful argument against scientific realism is the so-called pessimistic meta-induction presented by Larry Laudan (1981), according to which the rejection of successful theories in the history of science should lead us to expect that even our finest current scientific theories may themselves be abandoned.

The central question addressed in John Worrall's seminal paper is whether there is any reasonable way of adopting some sort of realism in science while also recognizing the strength of the pessimistic meta-induction. Worrall's proposal (1989, p. 112) is *structural realism* (SR), a position inspired by Henri Poincaré, which would be potentially capable of both capturing the intuitions underlying scientific realism and avoiding the pessimistic meta-induction.

Worrall's intention was to defend a new form of realism that could be accepted both by anti-realists, as an alternative to standard scientific realism, and by realists, as a viable form of realism. Structural realism should declare on the realism debate as typically concerned with the truth of the theories and the reference of theoretical entities. Scientific realism should no longer be the epistemological doctrine claiming that theories are approximately true descriptions of reality, descriptions that make use of theoretical entities which refer empirically. Since structural realists would admit that standard scientific realism is unviable, then if scientific realism is to be saved, it should not deal with theories or entities, but with structures. Thus truth should be claimed about structures, not about theories or entities. Structural realism does not relinquish the thesis that science provides truths about the World, but contrary to standard scientific realism, it declares that scientific truths are only truths about structures. Worrall claims:

> SR takes it that the mathematical structure of a theory may globally reflect reality without each of its components referring to a separate item of that reality; and that the indication that the theory does reflect

> reality is exactly the sort of predictive success that motivates the *no miracles argument*. This may seem like a hand-waving sort of realism to some, but it is arguably the strongest form of realism compatible with the history of theory-change in science. (2008, p. 290)

Worrall's argument in favour of his new form of realism, structural realism, is grounded in the alleged historical fact that there is continuity in the shift from a theory to a new one, and that what constitutes the element of continuity is not the carrying over of the successful empirical content and/or the full theoretical content of the older theory into the new one. Continuity is of *form* or *structure*, not of content. This claim, asserts Worrall, had already been made and defended by Poincaré:

> Poincaré used the example of the switch from Fresnel to Maxwell to argue for a general sort of syntactic or structural realism quite different from the anti-realist instrumentalism which is often attributed to him. This largely forgotten thesis of Poincaré's seems to me to offer the only potential way of both underwriting the "no miracles" argument and accepting an accurate account of the extent of theory change in science. (Worrall 1989, p. 117)

According to Worrall (1989, pp. 119–120), if we restrict ourselves to the level of mathematical equations, then there is complete continuity between Fresnel's and Maxwell's theories: Fresnel's equations are fully encompassed by Maxwell's theory. This example of the history of physics appears to exhibit cumulative growth of structures and it speaks in favour of *structural* realism.

That science preserves the world's structures had indeed been claimed earlier by Poincaré, according to whom the equations of the physical theories

> express relations, and if the equations remain true, it is because the relations preserve their reality [ces équations expriment des rapports et, si les équations restent vraies, c'est que ces rapports conservent leur realité (1902, p. 174)]. They teach us now, as they did then, that there is such and such a relation between this thing and that. [...] The true relations between these real objects are the only reality we can attain. (1905a, p. 161)

And in Poincaré 1905b, he insists that the value of science does not lie in the fact that it helps us know the true nature *of* things, but in that it helps us know the true relations *between* things: "when we ask what is the objective value of science, that does not mean: Does science teach us the true nature of things? But it means: Does it teach us the true relations of things?" (1905b, p. 130, english version) Moreover, Poincaré points clearly to the idea of structures retention across scientific change:

> At the first blush it seems to us that the theories last only a day and that ruins upon ruins accumulate. Today the theories are born, tomorrow they are the fashion, the day after to-morrow they are classic, the fourth day they are superannuated, and the fifth they are forgotten. But if we look more closely, we see that what thus succumb are the theories, properly so called, those which pretend to teach us what things are. But there is in them something which usually survives. If one of them has taught us a true relation, this relation is definitively acquired, and it will be found again under a new disguise in the other theories which will successively come to reign in place of the old. (1905b, p. 139)

Poincaré concludes with the following expression of structural realism:

> The sole objective reality consists in the relations of things whence results the universal harmony. Doubtless these relations, this harmony, could not be conceived outside a mind which conceives them. But they are nevertheless objective because they are, will become, or will remain, common to all thinking beings. (1905b, p. 140)

Interestingly, the first explicit reference to the philosophical view that for Poincaré our knowledge is about structures is made by Jules Vuillemin, who in his "Préface" (p. 18) to Poincaré 1902 attributes to the French physicist "que nous ne connaissons pas les objets, mais leurs relations, que notre connaissance est donc celle des structures et non de qualités". [We do not know the objects themselves, but only their mutual relationships. Thus our knowledge is about structures, not about qualities.][1]

Following Worrall, James Ladyman presents in following terms the main theses of structural realism:

[1] No minimal reference to Poincaré's structural realism can be found in Joseph Larmor's "Introduction" to the 1905 English translation of Poincaré's book.

> Since there is (says Worrall) retention of structure across theory change, structural realism both (a) avoids the force of the pessimistic meta-induction (by not committing us to belief in the theory's description of the furniture of the world) and (b) does not make the success of science (especially the novel predictions of mature physical theories) seem miraculous (by committing us to the claim that the theory's structure, over and above its empirical content, describes the world). (1998, pp. 409–410, and 2007)

In the following I shall address both the idea that there is structure retention across theory change (Section 3), and that theory's structures describe the world (Section 4). I call them the two dogmas of structural realism. Actually these can also be considered two dogmas of standard scientific realism. Indeed, according to Stathis Psillos, one part of the realist strategy is "to show that there is continuity in theory-change and that this is not merely empirical continuity; substantive theoretical claims that featured in past theories and play a key role in their successes (especially novel predictions) have been incorporated (perhaps somewhat re-interpreted) in subsequent theories and continue to play an important role in making them empirically successful" (2009, p. 72). Moreover, Psillos's *dynamic* image of science amounts to claiming that "theories improve on their predecessors, explaining their successes, incorporate their well-supported constituents and lead to a more well-confirmed (and according to current evidence, truer) description of the deep structure of the world" (2009, p. 83). Since Psillos's form of scientific realism denies that there are any principled limits to knowledge of Nature, so that even the deep unobservable structures of the world are knowable, it becomes for him evident both that theories capture the structures of the world and that there is continuity or convergence in theory-change in form of incorporation, preservation or retention of theoretical substantive claims. What makes Worrall's structural realism different from Psillos's standard scientific realism is the way that SR develops these typically realist theses.

Ladyman (1998, p. 410) points out that there is a fundamental question about the nature of structural realism, in respect to which Worrall's paper is allegedly ambiguous. To wit: is structural realism an epistemological or a metaphysical position? Some realist philosophers of science, for example Anjan Chakravarty (1998, p. 398), Elie Zahar (2001, p. 38) and Steven French and James Ladyman (2003, p. 31), view Worrall's structural realism as a strict epistemic form claiming substantive knowledge of relations only, i.e. that everything we know is the structure of the objective world, whereas the ontological content is unknowable. Moreover the question has been raised of whether epistemic structural realism can address the problem of ontological discontinuity. According to Ladyman (1998, p. 411) only a metaphysical or ontic form of structural realism would solve the question of giving an acceptable realist response to the problems posed by scientific revolutions. Contrary to epistemic structural realism, ontic structural realism emphasizes that structures are not the only thing we can know, but they are the ontological substratum. Other realist philosophers like Stathis Psillos disagree. According to him (2009, pp. 130–135) the distinction between an epistemic and an ontic form of structural realism amounts to establishing a difference between a *restrictive structural realism* (RSR), according to which there is something other than structure in the world which cannot be known, and an *eliminative structural realism* (ESR), the view that there is nothing other than structure in the world, i.e. that structure is all there is. Since Psillos denies that there are any principled limits to knowledge of Nature, he affirms that RSR fails to be realist enough. But ESR would fail too. The reason is that "it implies the wrong ontological thesis that structures require no individuals in order to exist and the wrong epistemic thesis that they can be known independently of [...] individuals which instantiate them" (Psillos 2009, p. 135). As we will see at the end of Section 4 below, this is the same type of criticism as presented by Chakravarty and van Fraassen.

3. *Theoretical Explanations, Limiting Cases and Retention of Structures across Theory Change*

Is preservation of structure a general feature of theory change in mature science? This is the question on the response to which the viability of structural realism depends. Worrall's view on the Fresnel-Maxwell case is the following:

> This particular example is in fact unrepresentative in at least one important respect: Fresnel's equations are taken over completely intact into the superseding theory —reappearing there newly interpreted but, as mathematical equations, entirely unchanged. The much more common pattern is that the old equations reappear as *limiting cases* of the new —that is, the old and new equations are strictly inconsistent, but the new tend to the old as some quantity tends to some limit.
>
> The rule in the history of physics seems to be that, whenever a theory replaces a predecessor, which has however itself enjoyed genuine pre-

> dictive success, the "correspondence principle" applies. This requires the *mathematical equations* of the old theory to re-emerge as limiting cases of the mathematical equations of the new. [. . .] But the principle applies *purely* at the mathematical level, and hence is quite compatible with the new theory's basic theoretical assumptions (which *interpret* the terms in the equations) being entirely at odds with those of the old. I can see no clear sense in which an action-at-a-distance force of gravity is a "limiting case" of, or "approximates", a space-time curvature. Or in which the "theoretical mechanisms" of action-at-a-distance gravitational theory are "carried over? into general relativity theory. Yet Einstein's equations undeniably go over to Newton's in certain limiting special cases. In this sense, there is "approximate continuity" of *structure* in this case. [. . .] But the general applicability of the correspondence principle certainly is not evidence for full-blown realism —but, instead, only for structural realism. (Worrall 1989, p. 120)

James Ladyman (1998, pp. 414–415) views as uncontroversial Worrall's claim that there is retention of structure across theory change. There are indeed, Ladyman argues, many examples of mathematical structural continuity in physics. And Anjan Chakravarty insists that

> Proponents of SR suggest two mechanisms: either mathematical equations survive intact from one theory to the next, or more commonly, old equations are limiting cases of newer ones [. . .]. The notion of preservation of structure from one theory to another has been called the "correspondence principle". (1998, pp. 398–399)

Theoretical physicists indeed acknowledge the existence of "correspondence principles". For instance, Misner, Thorne and Wheeler (1973, pp. 412–413) claim that correspondence principles contribute to maintaining the unity of physics. The correspondence between a new theory and its predecessor is such that "(a) [it] gives one the power to recover the older theory from the newer; (b) can be exhibited by straightforward mathematics; and (c), according to the historical record, often guided the development of the newer theory" (pp. 412–413).

Moreover, among philosophers of science, standard scientific realists, principally Karl Popper, have supported Albert Einstein's *dictum* (1917, §§ 22, 29) that the most important aim of a scientific theory is to point to the establishment of a more comprehensive one, in which it survives as a limiting case. And theoretical physicists have also followed Einstein's view (Rivadulla 2004b, § 1, § 2).

Not every case where structural realists might view structure retention is an instantiation of the application of some correspondence principle. The existence in different theories of similar structures points to the fact that inter-theoretic relations are much more complex than the recognition of limiting cases suggests. I can distinguish at least two different situations: one is when we are dealing with theoretical explanations, another is truly that of limiting cases.

3.1. Theoretical Explanations

In many circumstances physicists are faced with the need to provide *theoretical explanations* of phenomenological, quasi-empirical and even theoretical laws. According to Rivadulla 2005 (pp. 166–167), physical constructs like facts, laws, hypotheses, etc., receive a theoretical explanation when they can be deduced mathematically from the framework of other physical constructs. This view is inspired by Albert Einstein's philosophical reflections on the methodology of theoretical physics; for instance Einstein stated why Newton's theory was committed to providing the theoretical explanation of Kepler's phenomenological laws of planetary movement:

> Newton's object was to answer the question: is there any simple rule by which one can calculate the movements of the heavenly bodies in our planetary system completely, when the state of motion of all these bodies at one moment is known? *Kepler's empirical laws of planetary movement*, deduced from Tycho Brahe's observations, confronted him, and *demanded explanation*. These laws gave, it is true, a complete answer to the question of how the planets move round the sun: [...] But these rules do not satisfy the demand for causal explanation. They are logically independent rules, revealing no inner connection with each other. (Einstein 1927, p. 254; my emphasis.)

Based on Einstein's views, in Rivadulla 2005 I claim that "earlier or later physical constructs like empirical facts, phenomenological formulae, theoretical laws and even theories themselves become explained by more general laws and theories. When a physical construct can be deduced mathematically in the framework of a more general construct we affirm that it has received a *theoretical explanation*" (p. 169). There are indeed in the methodology of mathematical physics many interesting cases of theoretical explanations. For instance, the different theoretical explanations of Joseph Stefan's empirical law of the radiation of black bodies. Through Adolph Wüllner's book, *Die Lehre von der Wärme vom Standpunkte der mech-*

anischen Wärmetheorie (Leipzig 1875), Joseph Stefan became acquainted in 1879 with the experiments that John Tyndall conducted in 1865. According to these experiments, the radiation of a platinum wire at 1473 K is 11.7 times higher than at 798 K. Joseph Stefan observed that 11.7 is approximately $(1473/798)^4$, therefore concluding that the total radiation emitted by a black body is proportional to T^4. This is why the expression $E \propto T^4$ is known as *Stefan's radiation law*. But since this formula was purely an empirical or phenomenological law, to use Einstein's words, it *demanded explanation*.

Two independent theoretical explanations were given of Stefan's radiation law. The first one came only a few years later, as Ludwig Boltzman offered a derivation of Stefan's law in his article "Ableitung des Stefan'schen Gesetzes betreffend die Abhängigkeit der Wärmestrahlung von der Temperatur aus der elektromagnetischen Lichttheorie", published in *Annalen der Physik* in 1884. Assuming a gas of electromagnetic radiation enclosed in a volume V, a combination of the first principle of classical thermodynamics with Maxwell's thermodynamic relation and the relation between the pressure of a gas and the total density of its radiation energy, allowed Boltzmann to obtain the differential equation $dE/E = 4dT/T$, whose solution[2] agrees with Stefan's radiation law. Thus I claim that Ludwig Boltzmann provided a theoretical explanation of Stefan's empirical law.

The other independent theoretical explanation of Stefan's law proceeds directly from Planck's radiation law, according to which the total energy density radiated by a black body over all frequencies is given by

$$E = \int_0^\infty E(\nu)d\nu = (\pi^4/15)T^4.$$

A further interesting case of theoretical explanation is Johann Balmer's empirical formula. Balmer, concerned with the spectra of atomic elements, found empirically in 1885 a formula for the distribution of the spectral lines of the hydrogen atom. There not being available at the time any theory of the atomic structure of matter, Balmer's formula *demanded explanation*. This was first given in the framework of Bohr's 1913 model of the hydrogen atom. Indeed, from this theoretical model, a formula is mathematically deducible which, for the value $n = 2$ of Bohr's quantum number,

[2] The complete derivation of Stefan's law by Ludwig Boltzmann is given in Longair 1984, pp. 174–175.

agrees with Balmer's original formula. With no reference to the correspondence principle, Bohr's *theoretical atomic model* provides a theoretical explanation of Balmer's empirical or phenomenological formula. A further, more sophisticated theoretical explanation of this empirical formula was given indirectly by Erwin Schrödinger's wave mechanics. Indeed the so-called *Schrödinger's radial equation* allows us to deduce Niels Bohr's formula of the total energy of an electron, thus also providing a *theoretical explanation* of Bohr's semi-classical atomic theory. Another interesting case is Bose-Einstein's statistical quantum-mechanical account of Planck's radiation law (Rivadulla 2005, pp. 175–177).

What these cases of theoretical explanation share is that none of them uses mathematical limits. It is indeed striking that in the case both of Bohr's hydrogen atomic model and in Schrödinger's wave mechanics the formula of the total energy of electrons is *nearly* the same. Some Worrallian structural realists might see here a case of structure retention across theory change. I prefer to emphasize the fact that the more empirically and theoretically progressive Schrödinger theory has been able to provide us with a theoretical explanation of some aspects of the very restrictive semi-classical Bohr's atomic model. The same applies to the relation of Bose-Einstein's statistical quantum mechanics to Max Planck's own account of the radiation law of black bodies.

Theoretical explanations are remarkable because the explanations they provide render previously available explananda superfluous. Indeed, if Newtonian celestial mechanics gives a theoretical explanation of Kepler's planetary motion laws, then Kepler's contribution is dispensable; indeed it has historical value, and probably Newton's contribution is unthinkable without Kepler's. But, on the other hand, it is also conceivable that Newton would have developed his celestial mechanics even had Kepler himself not existed. And the same applies to the other cases of theoretical explanation. Schrödinger makes Bohr's atomic theory superfluous; Bose-Einstein's quantum statistical mechanics makes Plank's own radiation law dispensable, etc. Thus, from the point of view of theoretical explanations in the methodology of physics, structure retentions across theory change are contingent.

3.2. Limiting Cases

Acknowledging the existence of limiting cases offers strong support to the idea of the rationality of science. In Rivadulla 2004b, I claim that: "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" (2004b, p. 418). This is on the understanding that the existence of limiting cases constitutes a sufficient, but not necessary, condition for the rationality of theory change. For instance, in the transition from ancient geocentric astronomy to the Copernican model, it is impossible to talk about the former being a limiting case of the latter. Thus not every theory change incorporates limiting cases situations. Does this mean that the transition from geocentric to heliocentric astronomy was irrational? No, of course it does not. In such a case, the so-called *predictive balance*, i.e. the weighting of the predictive capacities of the competing theories, is what permits scientists to make a rational choice.

Let us return to the question of the structure retention across theory change. Does the existence of limiting cases also support the particular form of scientific realism, which is structural realism, as Worrall believes? Does the existence of limiting cases endorse the structural realist thesis of continuity across theory change? On my view, no, it does not.

I agree that it is reasonable to say that Newtonian mechanics is a limiting case of relativity theory when $v/c \rightarrow 0$, i.e. when bodies move with velocities which are much smaller than light speed in open space, and that classical mechanics is also a limiting case of quantum mechanics when Plank's $\hbar \rightarrow 0$. But although both cases offer good reasons for believing in the rationality of science, they are not enough to create belief in realism, not even in structural realism. Worrall's main mistake is to believe that there is structure retention across theory change. What follows is an immanent criticism of the main thesis of structural realism.

There is a wide variety of situations where correspondence principles allegedly apply. For instance, special relativity claims that the kinetic energy of a particle of mass m, expressed as the difference between its total energy and its energy at rest, is given by the formula $E_{relkinetic} = \gamma mc^2 - mc^2$, where $\gamma = (1 - v^2/c^2)^{-1/2}$. If we compare this formula with the expression of the classical kinetic energy, $E_{claskinetic} = mv^2/2$, then we observe that there is no similarity between both formulae that could allow us to conclude that there is some kind of structure retention when we pass from Newtonian to relativistic mechanics. It is true indeed that in the correspondence limit $v/c \rightarrow 0$, the relativistic kinetic energy reduces to the classical value of kinetic energy:

$$\lim_{v/c \rightarrow 0} E_{relkinetic} = mv^2/2.$$

This permits the affirmation that Newtonian mechanics constitutes a limiting case of special relativity theory, and seems also to suggest that there is structure (equation, relation) retention across theory change.

But the application of the correspondence principle is based on the assumption that the limiting case $v/c \rightarrow 0$ is equivalent to $c \rightarrow \infty$. Indeed both expressions are used without distinction in relativity theory. But this use is made only in order for the correspondence principle to apply mathematically. Physically it is inconceivable, since one of the principles of relativity theory is that of the constancy of the value of c, which is very small compared to the dimensions of the Universe, and which by no means approaches infinity. Thus it is important to distinguish between what we claim when we say that Newtonian mechanics constitutes a limiting case of relativity theory, and how we ought to interpret this claim. The claim is that special relativity theory approaches Newtonian mechanics in empirical situations of small velocities compared to the light speed in empty space. The interpretation is merely an instruction to employ Newtonian mechanics instead of relativity theory in empirical situations of small velocities compared to the light speed in empty space.

The process of obtaining Newton's *Universal gravitation law* from the general relativity theory shows peculiarities that are different from the case discussed above. Indeed (Rivadulla 2004a, chap. IV, § 2.2), in the limiting case $v/c \rightarrow 0$ we first obtain in general relativity the value $d^2x^i/dt^2 = (c^2/2)(\partial g_{00}/\partial x^i)$ for the acceleration equation of a moving particle, where i denotes the spatial indices 1, 2, 3 and g_{00} is the only necessary component of the metric tensor. Then we combine this value with the formula $d^2x^i/dt^2 = -\partial\phi/\partial x^i$, expressing in Newtonian mechanics the value of acceleration in terms of the (Newtonian) gravitational field potential ϕ. This is all we need now in order to express g_{00} in terms of ϕ, i.e.: $g_{00} = -2\phi/c^2$. Further mathematical work allows us to obtain Newton's gravitational law.[3]

[3] Landau and Lifshitz (1951, §§ 10–7 and 10–8) obtain the *Newtonian limit of relativity theory* by comparing the expression of the non-relativistic action for a particle in a gravitational field, that contains the Newtonian gravitational potential ϕ, with the expression of the action of a free particle in relativity theory. This allows

Worrall, and Misner *et al.* emphasize that the correspondence principles apply purely at the mathematical level, or that the correspondence between the newer and older theories can be exhibited by straightforward mathematics. But as to the correspondence between relativity theory and Newtonian mechanics, what matters is that *in general relativity no forces and potentials exist.* Thus the question is: How can the Newtonian law follow from general relativity? The answer is that the deduction *has been forced* in the way just described: we have profited from obtaining in general relativity an equation of the acceleration of a moving body, and we have inserted into it the value of the acceleration of a moving body in a Newtonian gravitational potential. In other words: unless we *contaminate* general relativity with Newtonian mechanics, it is not possible to obtain the Newtonian Gravitational Law from general relativity alone. To do this is of course legitimate, since it is an instantiation of the application of the deductive way of reasoning in theoretical physics. But this deduction should not mislead us into thinking that general relativity and Newtonian mechanics *share* a common structural law: *the universal gravitational law.* Indeed, *this is not a formula of general relativity*, since general relativity, which is a geometrical theory of gravitation, does not recognize the existence of (gravitational) forces and potentials.

Let me now point out that not every operation of taking $c \to \infty$ produces a meaningful Newtonian limit of relativity theory. Indeed, if in *Einstein field equations* $R_{\mu\nu} - \frac{1}{2}g_{\mu\nu}R = \frac{8\pi G}{c^2}T_{\mu\nu}$ —where $R_{\mu\nu}$ is the curvature tensor, $g_{\mu\nu}$ is the metric tensor, $R = R^{\mu}_{\mu}$ is the curvature scalar, G is Newton's gravitational constant, and $T_{\mu\nu}$ is the energy-momentum tensor— we take the limit $c \to \infty$, we obtain $R_{\mu\nu} - \frac{1}{2}g_{\mu\nu}R = 0$, i.e. an empty universe, a space void of matter and energy. In this case no structure could be preserved in the transition from relativistic to Newtonian mechanics, because the empty space can be by no means the Newtonian limiting case of general relativity theory.

In the above-mentioned cases, the Newtonian limit of special and general relativity theory, the assumption is made that the limiting case $v/c \to 0$ is equivalent to the limiting case $c \to \infty$. But this possibility is *physically forbidden* by relativity theory, both special and general. A similar situation pertains in quantum mechanics, both semi-classical and standard. A first example is the conservation of the

to obtaining for the interval ds the value $ds = [c - (v^2/2c - \phi/c)]dt$. Taking the limit $c \to \infty$ further mathematical work allows to deducing Newton's gravitational law.

angular momentum $L = n\hbar$ of an electron. When the quantum number n grows, $\hbar$ must decrease. In the limiting case $\hbar \to 0$, n must go to infinity, and then quantum physics becomes classical. This is precisely what Bohr's correspondence principle claims. Another example is the derivation of the *Hamilton-Jacobi* equation of classical mechanics from Schrödinger's Equation, after taking the limit $\hbar \to 0$ (Messiah 1959, chap. VI, § 4). In each case it is obvious that when in any quantum-mechanical equation we take $\hbar = 0$, this equation automatically ceases to be an equation of quantum mechanics. Particularly in the second case, it is thus illegitimate to affirm that classical and quantum mechanics share Hamilton-Jacobi's equation.

To conclude these reflections, I present a case that in my view constitutes a clear refutation of the structural realist doctrine of structure retention across theory change. In the year 1900, Lord Rayleigh was seeking the solution to the problem of black body radiation. He assumed a photon gas enclosed in a recipient containing N_ν stationary electromagnetic waves with average energy $< E >= k_B T$, where k_B is Boltzmann's constant. Since, by application of the equipartition theorem of classical statistical mechanics, the total energy of the gas is $E = N_\nu k_B T$, and the number of oscillation modes is $N_\nu = 8\pi\nu^2/c^3$, then the energy density radiated by a black body becomes $E(\nu, T) = (8\pi\nu^2/c^3)k_B T$. This formula is known as the *Rayleigh-Jeans law.*

Now this formula reappears when in Planck's radiation law we take the mathematical limit $\hbar \to 0$, which for a structural realist is a confirmation of the idea of structure retention. The problem is that if we integrate over all frequencies, then the total energy density clearly diverges:

$$E = (8\pi k_B T/c^3)\int_0^\infty \nu^2 d\nu = \infty.$$

Physically this implies that for high frequencies the Rayleigh-Jeans Law is empirically completely contradicted. This fact is known as the *ultraviolet catastrophe*, so named by Paul Ehrenfest in 1911.

It is obvious which consequences this last case has for the doctrine of the structure retention across theory change. If the structural realist view on structure retention were true, then the empirically refuted Rayleigh-Jeans law should be part of *Planck's radiation law*, and so Planck's law would be wrong. But I have also affirmed that Planck's radiation law follows mathematically —i.e. it receives a theoretical explanation— from the framework of Bose-Einstein's statistical quantum mechanics. Thus the refutation chain would extend too far.

The limiting cases just analysed show that there is no general structure retention across theory change. They show that structural scientific realists are wrong when they claim that there is a kind of continuity between theories separated by a scientific revolution, i.e. that there are some links connecting together two sometimes contradictory theories. Thus I do not see how structural realism can avoid the force of pessimistic meta-induction.

Talking of limiting cases indeed justifies the *pragmatic decision* to make use, for instance, of Newtonian mechanics in cases of small velocities compared with the speed of light in open space. In general, the existence of limiting cases in physics constitutes an authorization of a new theory to deal with the more simple models of an older theory. This justifies as rational the choice in mature (i.e. successfully predictive, according to Worrall 1989, p. 120) science of the theory from which in limiting cases any older less successfully predictive theory can be obtained. Since instrumentalist philosophers of science assume this view as well, it is not true that retention or preservation of structures, in the limited conditions we are allowed to talk about, furnish us with reasons for exclusively believing in scientific realism.

In many respects, my viewpoint is close to van Fraassen's alternative to *structural realism*, so-called *empiricist structuralism*, which is intended to provide a reasonable account of the stability maintained through scientific theory change (van Fraassen 2006, abstract, and p. 305). Against Worrall's view that what is retained through theory change is *structure*, van Fraassen (2006, pp. 303–304) asks if there is really an objective difference in nature as opposed to our representations of nature: "Is it not a little embarrassing to start with the thesis that what is preserved through scientific revolutions is the structure attributed to nature, and then to have to identify structure by noticing what has been preserved?" I adhere to van Fraassen's account (2006) on the success of past mature sciences, that *any superseding theory does not explain how essentially correct the old theory was about the underlying structure of nature*. Instead, the explanation of the past successes of the older theories takes place "by implying approximately the same predictions for the circumstances in which the older theories were confirmed and found adequately applicable. Thus the past empirical success can now also be counted as an empirical success for the new theory" (2006, p. 298). Neither is the success of science a miracle for him, "because in any theoretical change both the past empirical success retained and new empirical success *were needed as credentials* for acceptance" (van Fraassen 2006, pp. 298–299).

Indeed van Fraassen's criticisms of structural realism are not immanent because they proceed from a viewpoint that is grounded in a philosophical stance completely opposed to that of realism, both standard and structural. As a matter of fact, van Fraassen's position on this issue is an answer to the question on *how can we account for the continuous increase in the empirical success of science*?, which is fundamentally different from the realist one, to wit: *what explains the empirical success of science at all* (2006, note 29). Van Fraassen's criticisms come from outside, but his basic position is, from an epistemological point of view, as legitimate as the realist ones.

To finish my reflections on the relevance of limiting cases to the case of structural realism, I briefly address the question of whether it might be reasonable to demand more than partial retention of structure. Worrall claims himself that "it is important for a defensible realism to establish a way in which successive theories in mature science have indeed been at least quasi-accumulative. And I claim that only the structuralist can successfully establish such an account" (Worrall 2007, p. 147). In my view, it is nonsense to imagine a situation in which only a part of Newton's gravitation law is preserved in relativity theory or only a part of Hamilton-Jacobi equation is retained in quantum physics, whether standard or Bohmian. Physicists would face a bizarre situation if confronted with the problem of identifying a given equation as a part of an already available law. Quasi-accumulation is simply nonsense at the theoretical level. At the empirical level, quasi-accumulation of successes would amount to assuming that only a part of the empirical successes of the previous theory is accounted for by the new theory. Then the question would be in which sense the new theory might be better than the older one. Thus my answer is that only full structure retention deserves attention. This section has been devoted mainly to this issue.

4. *Realism about Structures and the Ontic Form of Structural Realism*

Ladyman's argument on behalf of an ontic or metaphysical form of structural realism is based on the alleged fact that, even where there is mathematical continuity across theory change, there is not always ontological continuity. But contrary to Worrall, Ladyman (1998,

p. 415) does not relinquish his commitment to the ontology of scientific theories. The cases of radical ontological discontinuity indeed undermine standard realism, according to which the laws of nature belonging to approximately true theories demonstrate how the unobservable entities and processes described by the theories must behave. Moreover there is the case of underdetermination, which admits of different approaches: sometimes we have different formulations of a theory, and this prevents us from being realists, because we do not know which version is true about the world. Sometimes the problem lies in the fact of whether some entities, for example electrons, are individuals or not.

Ladyman's proposal is to shift to a different ontological basis, a shift that consists in taking structure to be primitive and ontologically subsistent. The resulting ontic view of structural realism should deal successfully with the problems of realism with respect to both theory change and underdetermination. French and Ladyman claim in this respect:

> We regard the ontic form of SR as offering a reconceptualisation of ontology, at the most basic metaphysical level, which effects a shift from objects to structures. [. . .] A form of realism adequate to the physics needs to be constructed on the basis of an alternative ontology which replaces the notion of object-as-individual/non-individual with that of structure in some form. (2003, p. 37)

To assume that structures play the role that individuals played in standard realism amounts to claiming that "Instead of beginning with a 'definitely determined entity' which possess certain properties and which then enters into definite relations with other entities [. . .], what we now begin with are the laws which express the relations in terms of which the 'entities' are constituted" (French and Ladyman 2003, pp. 39–40). The question metaphysical structural realism faces is whether there can be structures or relations without relata.

Before addressing this question, let us first consider the issue of whether or not theoretical structures reasonably adapt to the world. According to Worrall:

> On the structural realist view, what Newton really discovered are the relationships between phenomena expressed in the mathematical equations of his theory, the theoretical terms of which should be understood as genuine primitives.

> Is there any reason why a similar structural realist attitude cannot be adopted towards quantum mechanics? [. . .]
>
> The structural realist simply asserts, [. . .] that, in view of the theory's enormous empirical success, the structure of the universe is (probably) something like quantum-mechanical. (1989, pp. 122–123)

But to infer from the empirical success of a theory to its probable truth is to resort to the no-miracle argument, to optimistic meta-induction. Thus the burden of the proof lies in the realist's hands. Let me quote Hume's words in his attack on the methodology of induction in Section IV, Part II, of his *Enquiry Concerning Human Understanding*:

> These two propositions are far from being the same, *I have found that such an object has always been attended with such an effect*, and *I foresee, that other objects, which are in appearance similar, will be attended with similar effects*. I shall allow, if you please, that the one proposition may justly be inferred from the other [. . .]. But if you insist, that the inference is made by a chain of reasoning, I desire you to produce that reasoning. The connexion between these propositions is not intuitive. There is required a medium, which may enable the mind to draw such an inference, if indeed it be drawn by reasoning and argument. What that medium is, I must confess, passes my comprehension; and it is incumbent on those to produce it, who assert, that it really exists, and is the origin of all our conclusions concerning matter of fact. (Hume 1748, p. 33)

If we apply Hume's argument to the issue of structural realism, then it is obvious that it is incumbent on structural realists to present the arguments that might convince us that most structures of physical theories represent relations existing in Nature.

The structural realist can argue, as Worrall does, that in view of the enormous empirical success of our fundamental physical theories, the mathematical equations of these theories describe the relationships subsisting in the World, i.e. he can use the no-miracle argument. But this equals structural realism and standard scientific realism at the justification level. The focus has merely changed —from justifying theories and theoretical entities to affirming structures. But the no-miracle argument is not an acceptable argument for realism. I share the view that it has been refuted by the history of science, and that it would be more reasonable to conceive of theories and/or theoretical models as instruments which are merely intended both to save the phenomena and to deal predictably with Nature (Rivadulla 2006).

Structural realists also claim, as do French and Saatsi, that: "Structural realism [...] is the view that our best theories represent the world 'approximately right', where approximately right is spelled out by a structural version of explanatory approximate truth" (2006, p. 556). Or as Anjan Chakravarty affirms, "Structural realism (SR) holds that most structures of fundamental physical theories correctly mirror relations present in an external, mind-independent reality" (1998, p. 398). Thus according to Chakravarty, structural realism is "*the view that most structures of fundamental physical theories correctly represent relations between objects in the natural world*" (1998, p. 407; my emphasis). Now, if the structural realist conceives of the mathematical structures of our mature theories as true representations of reality, then he faces an unresolvable problem.

Indeed as Madrid-Casado (2009, § 3) has shown, Von Neumann's proven isomorphism between matrix mechanics (MM) and wave mechanics (WM) provides a counterexample to structural realism. From the standpoint of the mathematical theory of representation, since MM and WM are isomorphic, and if they were also isomorphic to the respective structures of reality R_{MM} and R_{WM}, then by composition of isomorphism, R_{MM} and R_{WM} should be isomorphic. If this were so, then R_{MM} and R_{WM} should have the same cardinality. But this is not the case. The former is discrete (corpuscles) and the latter is continuous (waves). Thus either it is wrong to affirm that MM and WM are isomorphic, or it is wrong to say that R_{MM} and R_{WM} are isomorphic. Von Neumann's 1932 theorem proves that MM and WM are isomorphic (this permits the claim that MM and WM are mathematically and empirically equivalent). In consequence, MM and R_{MM} cannot be isomorphic. Neither can be WM and R_{WM}. Madrid-Casado's formalization of this argument is the following: Let A be: "MM and WM are isomorphic"; B: "MM and R_{MM} are isomorphic, and WM and R_{WM} are isomorphic"; and C: "R_{MM} and R_{WM} are isomorphic". Thus, from $(A \wedge B) \rightarrow C$ and $\neg C$ it follows logically that $\neg A \vee \neg B$. But von Neumann's equivalence proof shows A to be true. Consequently B is false. Madrid-Casado's conclusion is that the relation between mathematical models and real systems is not an isomorphism.

The recourse to weaker relationships such as homeomorphism, similarity or homomorphism does not help a great deal. Since R_{MM} and R_{WM} have differing cardinality, they cannot be homeomorphic.

If the claim is that R_{MM} and R_{WM} are similar, it must be stated in which respects corpuscles and waves are similar. Analogously it occurs in relation to homomorphism. Homomorphic structures are not necessarily bijective.

As to the question of the reality of structures posited by mathematical physics, I share Fine's beliefs on relativistic physics:

> Einstein wanted to claim genuine reality for the central theoretical entities of the general theory, the four-dimensional space-time manifold and associated tensor fields. This is a serious business [. . .]. I believe the majority opinion among working, knowledgeable scientists is that general relativity provides a magnificent organizing tool for treating certain gravitational problems in astrophysics and cosmology. But *few, I believe, give credence to the kind of realist existence and non-existence claims that I have been mentioning. For relativistic physics*, then, it appears [. . .] that *most who actually use it think of the theory as a powerful instrument, rather than as expressing a "big truth"*. (1984, p. 92; my emphasis.)

This is a good moment to analyse whether the ontic form of structural realism should be preferred to the epistemic one. The answer to this question depends both on the justification of this preference and on the issue of how there could be structures without objects.

French and Ladyman (2003, p. 45) reject the notion that there can be some objects over and above the common structures retained through theory change, since the only non-metaphysical understanding of the nature of such objects, warranted by the physics, is structural. The thesis of metaphysical structural realism is that: "there are mind independent modal relations between phenomena (both possible and actual), but these relations are not supervenient on the properties of unobservable objects and the external relations between them, rather this structure is ontologically basic" (French and Ladyman 2003, p. 46). Thus the lemma of ontic structural realism becomes: *Structure is all there is.*

In the eyes of a non-realist philosopher of science this position immediately provokes the question: How can we know that it is the structure that is ontologically basic? Neither the claim of structure retention nor the no-miracle argument offer compelling reasons. In order to support their viewpoints, it is incumbent on structural realists to present further arguments.

According to French and Ladyman (2003, p. 48), we can avoid metaphysical underdetermination only if we desist from enquiring into what is a hypothetical entity over and above the structural aspects, i.e. if we avoid that which is ontologically non-structural. For instance, we cannot describe the nature of a field without recourse to the mathematical structure of the corresponding field theory. It is the field that is the structure, the whole structure and nothing but the structure. It is the field that metaphysically has structural character, and not our knowledge of it.

Again, in the eyes of a non-realist the question arises of how we can justifiably claim that the structures postulated by our mature contemporary theories really exist, that we can accept them on grounds different from their being empirically successful. But empirical success cannot be taken as an indicator of truth, truth-nearness or probability of truth.

Ontic structural realism has encountered two main contrary positions. One comes from the lines of realism. It is Chakravarty's. The other comes from van Fraassen's empiricism. According to Chakravarty, structural realism and entity realism[4] contain each other: "Advocates of traditional SR often speak as though their interests are confined to the truth of relations. But such relations contain substantive information about entities. [. . .] It is for this reason that we say that ER and SR entail one another; they are, in fact, one and the same position: semirealism" (1998, p. 407). For Chakravarty (1998, pp. 399–401) it would be unintelligible to subscribe to the *reality* of relations without committing to the fact that *some things* are related. In this sense SR must entail ER. Since, moreover, we have to expect that newer theories resemble their predecessors in illuminating causal relations between certain theoretical entities, then ER must entail SR.

For his part, van Fraassen qualifies as the most puzzling aspect of ontic structural realism the insistence that we conceive of the world as not consisting of objects, "even in the very broad sense of bearers of structure which are themselves something other than structure(s)" (2007, p. 55). In van Fraassen's view "what sense does it make to try and conceive of structure that is not structure of something? *Structure of nothing is nothing*, isn't that so?" (2007, p. 60). We could paraphrase van Fraassen's idea in Kantian terms by claiming

[4] Chakravarty's standard characterization of ER (entity realism) is that "entity realism holds that most of the entities referred to in scientific theories are truly existing constituents of the natural world" (1998, p. 406).

that relations, i.e. structures, without entities are empty, and, maybe also by claiming that entities without relations, i.e. entities not embedded in structures, are blind.

Thus my position is both that structural realists do not succeed in arguing that theories tell us about the structures of the world, and that ontic structural realism creates more problems than it solves —that ontic structural realism is part of the problem and not part of the solution.

5. *Summarizing Remarks*

I have focused my reflections in this paper on what I have called the two dogmas of structural realism. I have shown first that not every case where structural realists might talk of structure preservation across theory change is an instantiation of some working correspondence principle. Indeed, the presence in different theories of similar equations points to the fact that inter-theoretic relations are more complex than mere reductions by application of mathematical limits. Many cases of alleged structural retention across theory change are typical instances of *theoretical explanation*. And, as I have argued, from the point of view of the role theoretical explanations play in the methodology of physics, the explananda in the alleged cases of structure retention are contingent.

As to the strict application of the limiting cases, I have pointed to the specific prohibition that $c \to \infty$ and $\hbar \to 0$ in both relativity theory and quantum mechanics. In relativity theory the speed of light in empty space is well known to be finite and constant. No quantum theory is imaginable in which Planck's constant is zero. Moreover it is inconceivable that there could be any structure preservation between a mechanical theory of gravitation, like Newtonian physics, and a geometrical theory of gravitation, like Einstein's general relativity theory, where no forces and potentials can exist. Only by mathematically forcing the relationship between both theories or, as I have also claimed, by contaminating general relativity with Newtonian mechanics, is it possible to create the illusion that both theories share some common equations. This has been my immanent criticism of the idea that the existence of limiting cases demonstrates the existence of structure preservation across theory change.

The empirical refutation of Rayleigh-Jeans law provided us with the second relevant argument against the first dogma of structural realism. As a matter of fact, if structural realists insisted on the alleged preservation of this law in the framework of Planck's radiation law, then this last equation should be considered empirically refuted as well. But since Planck's radiation law is theoretically explained in the framework of Bose-Einstein statistical quantum mechanics, the latter should also be considered empirically refuted, and so forth.

Therefore my position regarding the first dogma of structural realism is that there is no general structure retention across theory change, and no general continuity between sometimes contradictory theories.

As to the second dogma of structural realism, realism about structures, I have claimed that either structural realists have to rely on the very arguable optimistic meta-induction, in which case there are no more reasons for preferring a structural realist view on the truths of structures than a standard realist view on the (approximately) truth of theories, or that structural realists have to rely on the view that structures of mature scientific theories represent real relations in the World. A counter-example provided by Madrid-Casado strongly argues against this second possibility.

Finally, I have not entered the internal dispute inside structural realism on whether an ontic form should be preferred to an epistemological one. But since ontic structural realism tackles the puzzle of ontologically subsistent structures that are relations without relata, I claim that instead of being a part of the solution, ontic realist is a part of the problem concerning structural realism.

Arthur Fine argued that standard scientific realism was dead. Moreover, John Worrall's attempt to bring scientific realism back to life in the form of structural scientific realism has failed. Scientific realism is indeed dead.[5]

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