SCIENTIFIC REALISM IN THE PHILOSOPHY OF SCIENCE AND INTERNATIONAL RELATIONS

CANDIDATE: Georgios Evangelopoulos

THESIS SUBMITTED FOR THE DEGREE OF

Doctor of Philosophy

AT THE

London School of Economics and Political Science

Department of International Relations

January 2013

SIGNED DECLARATION

I confirm that all the work presented in this thesis is my own.

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Georgios Evangelopoulos

ABSTRACT

This thesis sets out to challenge the assumption widely held among IR scholars that Scientific Realism (SR) is the definite and final interpretation of realism. The introduction of SR into IR as the latter's proper meta-theory has been the incentive for very intense debates about both meta-theoretical and theoretical IR issues.

I argue that IR has uncritically adopted the strongest version of SR. This can be seen by comparing the different versions of SR and their anti-realist alternatives - as these have developed in the Philosophy of Science literature - to the version of SR which was introduced into IR. It is Critical Realism (CR), however, a version of SR that originated with Roy Bhaskar, which has dominated the SR debate in IR. This development has had negative consequences with respect to the quality of the argumentation about realism in IR. This notwithstanding, a positive implication of this situation is that IR scholars who belong in various traditions of thought have criticized SR from different theoretical angles and thus shed light on many of its shortcomings.

I elaborate on the comments that have been made on meta-theoretical as well as theoretical issues and come up with my own conclusions about SR and CR. In this framework, I also deal with two special issues which have arisen from this debate's problematique: the question about whether reasons can be causes, which lies in the foundations of Wendt's 'constitutive explanation', and the challenge of 'meta-theoretical hypochondria', according to which the extensive concern with meta-theory takes place at the expense of theorizing real-world political problems. Last, I show, by a way of a novel contribution, that Wendt's

latest undertaking, of a ‘quantum social science’, although compatible with SR, suffers inconsistencies and misunderstandings in terms of its methodology, metaphysics, use of quantum mechanics, and application to IR.

This thesis is an interdisciplinary study, which draws upon the Philosophy of Science, IR and Physics (namely Quantum Mechanics), in order to scrutinize the use of SR and CR into IR along with its implications for both IR meta-theory and IR theory.

CONTENTS

ACKNOWLEDGEMENTS

I am deeply indebted to Dr. Spyros Economides and Professor Christopher Hill whose supervision, continuous support and constructive feedback and criticism have been not only vital for the completion of this thesis, but also deeply inspiring and exemplary in their commitment. Working under their supervision has been a unique intellectual and human experience, which I have enjoyed immensely. This thesis, which was completed under extremely difficult circumstances, would not have seen the light of day were it not for their emotional support during the last three years, which have been extremely trying for myself and my family. Of course, despite the above, the usual disclaimer that I alone assume responsibility for the views expressed in this work applies.

I also wish to extend my deepest gratitude to Dr. Panagiotis Papoulias, Professor Kimberly Hutchings, Professor John Gray, Professor Nicos Mouzelis, Dr. Aristidis Arageorgis, Mr. Miltiades Theodosiou, Mr. Athanasios Samartzis and Ms Eleftheria Papoulia for their generosity and willingness to discuss parts of my work with me and come up with invaluable comments on them. Their enthusiasm for philosophy has been inspirational and has helped me overcome various difficulties in the process. It is no exaggeration to argue that without the help of my dearest friend, Dr. Panagiotis Papoulias, in every aspect of my work, and without the emotional support of another great friend and scientist, Professor Charalambos M. Moutsopoulos, I doubt if I could have made it all the way through. Professor Eleftherios N. Economou, a physicist par excellence, has helped me understand better quantum mechanics, which I take on board in the last chapter of this thesis, so my gratitude and deepest appreciation to his person can never be overstated.

Professor Chris Brown is the IR theorist with whom I feel the most intellectual affinity, although I have never had any personal contact with him during my studies at the LSE. Through his lectures and, most importantly, through his books and writings, he has taught me what International Political Theory actually means and how important it is to the future of IR as a field of study. First and foremost, however, I have learnt from Professor Brown that one must relate research in IR Theory to real problems in international political practice. Therefore, I would like to express on this occasion my intellectual debts to him.

Professors Kevin Featherstone, William Wallace, Margot Light, Loucas Tsoukalis, Elias Mossialos, Konstantinos Arvanitopoulos as well as Dr. Peter Wilson, Dr. Carsten Holbraad and Dr. Andreas Gofas have always stood by me by generously offering any help I needed during my doctoral studies, so it is self-evident that I owe them a lot. My PhD research fellows and now very promising IR academics, Dr. Marjo Koivisto, Dr. Mireille Thornton, Dr. Jill Stuart, Dr. Kirsten Ainley, Dr. Annika Bolten, Dr. Stephanie Carvin, Dr. Antoine Bousquet, Dr. Douglas Bulloch, Dr. Robert Kissack, Dr. Tim Oliver, and, first and foremost, Dr. Felix Berenskoetter, have all been sources of inspiration to me and their different but productive approaches to IR and FPA helped me broaden the horizon of my knowledge of these two fields of inquiry.

I wish to thank the Department of International Relations of the London School of Economics for the financial support it has endowed me during one year of my doctoral studies as well as for the ‘Gregory Tzirakian PhD

Scholarship', which I held for three years and helped me to go on with my studies by surmounting great economic obstacles. My sincere and deepest gratitude extends to Mr. Gregory Tzirakian, the donor for the above scholarship.

During all these years I have cherished and deeply appreciated the support of many close and dear friends. These are Professor Dimitrios Kioupis, Professor Lina Papadopoulou, Professor Nicandros Bouras, Professor Leonidas Xanthis, Professor George Tsakos, Professor Gregorios D. Siourounis, Professor Stavros G. Papastavridis, Dr. Karolos Papoulias (President of the Hellenic Republic), Dr. Vasilios Alevizakos, Dr. Spyridon Kotsovilis, Dr. Vassilios Bogiatzis, Dr. Giorgos Galanis, Dr. Georgios Ouroumpeis, Dr. Panagiotis Charitos, Mr. George Karagiannis, Mr. Antonis Lavrantonis, Mr. Manolis Kanakis, Ms. Xanthi Karadima, Mr. Petros Raptis, Mr. Vlassios Leonardos, Mr. Dimitrios Kontis, Mr. John Korfiatis, Mr. Dimitris Pipinis, Mr. Georgios Tsakalos, Mr. Kostas Christopoulos, Mr. Anastasios Tzitizikos, Mr. Phaedon Kydoniatis, Mr. Demetrios Raptakis, Mr. Konstantinos Rokas, Mr. Vassilios Gavrilis, Mr. Alexandros Mountanos, Mr. Petros Ochtaras, and Mr. Orestis Papagiannis.

Finally, I would like to express my deepest thanks to my beloved wife Evangelia, my daughter Ellie, my mother-in-law Kalliopi as well as to my invaluable sisters, Sophia and Penelope. I am a feminist, by definition, as one can easily understand! All these women have stood by my decisions and supported me with the utmost of their strength and affection. Any endowment of my gratitude and deepest appreciation to them can never be enough.

It is to the memory of my parents, Leonidas and Ellie, as well as to my wife Evangelia to whom I dedicate this thesis.

LIST OF ACRONYMS

ASIR	Agents, Structures and International Relations
CR	Critical Realism
EFPA	European Foreign Policy Analysis
EU	European Union
ES	English School
FPA	Foreign Policy Analysis
IBE	Inference to the Best Explanation
IR	International Relations
NGO	Non-governmental Organization
NMA	No Miracle Argument
NOA	Natural Ontological Attitude
PMI	Pessimistic Meta-Induction
QCD	Quantum Chromodynamics
QCH	Quantum Consciousness Hypothesis
QD	Quasi-Duhemianism
QM	Quantum Mechanics
SQUID	Superconducting Quantum Interference Device
SR	Scientific Realism

SSR	Structural Scientific Realism
STIP	Social Theory of International Politics
UA	Underdetermination Argument

1

INTRODUCTION

1.1 The Problem

Alexander Wendt, nowadays one of the most well known theorists of International Relations (IR), first attracted the attention of the IR community when he published a paper on the agent-structure problem in IR, where he introduced Scientific Realism (henceforth SR) as a meta-theory for IR (Wendt 1987). He further elaborated on the ideas firstly presented in the aforementioned article and twelve years later came up with his *Social Theory of International Politics* (henceforth *STIP*), a *grand* constructivist theory of IR, which also relies on SR as its meta-theoretical background (Wendt 1999). Both in the aforementioned as well as in the subsequent and more recent works of his, which concern his 'quantum social science' project, Wendt has used SR as the meta-theoretical bedrock of his elaborations.

Colin Wight, a strong believer in the virtue of science, after publishing a number or papers on the philosophy of science and IR, attempted to provide a 'science of IR' in his book, *Agents, Structures and International Politics* (2006) (henceforth *ASIP*) (Wight 2006). Being a critical realist, Wight has endorsed Critical Realism (henceforth CR), the Bhaskarian version of SR, as the proper meta-theory for IR. Many IR critical realists, like Kurki, Patomäki, Joseph, Jessop, et al. have followed his example in accepting CR as the meta-theoretical background of their substantive theoretical studies in IR.

All the above pose the following questions: what are actually SR and CR, and how did they come to be imported into IR? Therefore, I decided to devote one chapter of my thesis to highlight the SR debate in the philosophy of science and a second one to a meticulous philosophical analysis of the Bhaskarian theory of ontology, namely CR, before I go on to discuss the implications that the introduction of both SR and CR into IR have for the latter's meta-theoretical and theoretical considerations. There are, of course, a few IR scholars like Kratochwil (2000), Smith (2000), Chernoff (2002, 2007, 2009a, 2009b) and Jackson (2001, 2008a, 2009, 2011) who, equipped with a solid philosophical background, have aptly criticized SR and CR. Nevertheless, I have not found in the relevant bibliography any work in which "Scientific Realism in the Philosophy of Science" (Chapter 2) and "Scientific Realism as developed by Roy Bhaskar" (Chapter 3) are examined separately and in detail prior to the critical study of the SR debate within IR, which I discuss in "Scientific Realism in International Relations" (Chapter 4). The present thesis bridges this bibliographical void.

Moreover, given that Wendt's latest intellectual undertaking concerns the very ambitious programme of building a 'quantum social science' that relies on SR as its meta-theory (its ontological basis being the wave-particle duality in Quantum Mechanics in this case), I also devote a separate chapter to the discussion of that project, titled "Alexander Wendt's Quantum Social Science and International Relations" (Chapter 5).

To sum up, this thesis is an inter-disciplinary study, which draws upon Philosophy of Science, IR and Physics (namely Quantum Mechanics), in order to scrutinize the use of SR and CR in IR, along with its implications for both IR meta-theory and IR theory.

1.2 The Argument of the Thesis

In Chapter 2, I will examine SR as an autonomous pursuit in the philosophy of science. This is doubly important to IR: first, whereas SR has repeatedly set the stage for rich and vigorous discussions, in the meta-theoretical context of IR it has been treated rather casually; second, it is necessary to show that any appeal to SR, in order to bolster the scientific credentials of an IR meta-theory, cannot avoid incurring all the associated costs.

The chapter initially examines the main tenets of SR. For the sake of exposition, I follow Psillos (1999, 2009) and present IR through the prism of three theses: metaphysical realism, semantic realism and epistemic realism. I examine metaphysical realism first, since therein lies the major presupposition of SR. Then I present SR's semantics, according to which theoretical terms should be taken literally and not fictionally or metaphorically. Finally, I examine the epistemological thesis of SR, which claims that science can and does deliver theoretical truth, very much in the way it delivers observational truth.

Having discussed the main tenets of SR, I then show how SR needs all three theses to defend itself against anti-realism. It should be noted that metaphysical realism on its own is not enough. First of all, a metaphysical commitment to the existence of the external world does not necessarily entail semantic or epistemic realism. This allows for double semantic standards between observational and theoretical statements, while keeping to SR's ontological commitments. One way to formulate this alternative position is instrumentalism.

According to instrumentalism, theoretical commitments can be abandoned in favour of the observable consequences of a theory. Instrumentalism therefore rejects the *semantic* thesis of SR. However, while acknowledging the importance of this position, I argue for the indispensability of scientific theories and of theoretical terms to science. Following Sellars (1963) and Quine (1960), I insist that theoretical discourse serves explanatory and systematic purposes. The appeal to the semantic thesis of SR, then, according to which scientific terms should be taken literally, is necessary for a proper realist position.

Moreover, the metaphysical thesis of SR on its own does not seem able to account for historical change in science. A systematic divergence between what there is in the world and what scientists claim there is, would, if substantiated historically, undermine SR's robust sense of objectivity. Such a formulation of the matter construes the realism/anti-realism debate in *epistemic* terms. I argue that the epistemological thesis of SR, according to which scientific theory can and does provide true knowledge, seems necessary to counter any doubts on science's objectivity. Consequently, proper realism may be legitimately taken to require all three SR theses.

Still, putting the metaphysical and the epistemic theses in the foreground does not necessitate SR: a realist could agree that scientific practice discovers real entities, but could still deny the literal truth of the theories which describe them. This type of entity-realism ties in more with experimental practice and side-steps the question of theoretical truth. However, I argue that this position would be difficult for a realist to adopt since it is theories that both provide the realist with knowledge of any properties these entities have and allow for the interpretation of experimental results. Overall, while I find the focus on scientific practice to be an important contribution of entity-realism, I argue that it does not ultimately address the issues of the SR debate.

Having defended my formulation of SR, I then turn to an examination of the arguments for and against it. First, I discuss the arguments used by realists. The single most important argument, the 'No-Miracle Argument' (NMA), employs the empirical success of science to justify SR via a defence of the epistemic thesis. I trace the argument's origins to the work of Smart (1963), Maxwell (1962, 1970) and Putnam (1975a, 1978). They all argue that empirical success does not just happen ('miraculously') but occurs because theories are true and because the unobservable entities they posit exist.

I focus on Putnam's argument specifically. I construe it as an abductive philosophical argument, which appeals to 'inference to the best explanation' (IBE). I show how it can defend adequately the epistemic thesis of SR against anti-realist objections, by offering an overarching empirical hypothesis as the best explanation of the success of science, without the need to appeal to any *a priori* distinctive philosophical method. Moreover, Putnam's NMA is capable of addressing the objections put forward by sceptics like van Fraassen. However, I conclude that, in order for NMA to provide a successful defence of the epistemic thesis and thus support SR, it must ultimately appeal to the metaphysical thesis.

The arguments against SR are two: the Underdetermination Argument (UA) and the Pessimistic Meta-Induction (PMI). They challenge SR's epistemic credentials. The former holds that theories always have empirically equivalent rivals and doubts the realist's belief in unobservables, because empirical evidence alone is deemed incapable of fixing theory-choice. The latter holds that the historical record shows that, *contra* SR, science is marked by failures and current science should not be relied on, since theories which have in the past been considered to be true, are now taken to be false.

I first present UA in detail and show that before one examines the epistemic issues raised by UA, a stand should be taken on the metaphysical thesis. This is where PMI comes into relevance. The most promising defence against this argument is to engage in historical case studies and attempt to reconcile realism with the historical record. This can be done by identifying not only stable components that survived disproved theories, but also ones which are often instrumental for the demise of the theory that they had been part of. Again, though, even if such case studies were provided, they could never be conclusive. Moreover, if one chooses to go down the path of examining all past and present scientific theories, then one risks contextualizing SR's theses to such an extent that one may compromise the metaphysical thesis.

In this connection, I discuss Mäki's 'doubly local scientific realism', and argue that the conceptual flexibility that Mäki's view entails makes for a very watered-down version of realism (Mäki 2005). Although it is too early to judge this 'new-generation' SR, I find that at present this watered-down SR lacks the appropriate vocabulary to provide the argumentative structure one would like to see and evaluate. Furthermore, I also find that the outcome of the realism/anti-realism debate has been that *one should no longer go uncritically for an adoption of the standard global conception of SR.* This is of utmost relevance, since one of the central arguments I make in the thesis is that the manner in which SR has been imported and used in IR meta-theory does not appreciate adequately the grave theoretical implications SR imposes on any strong, global realist construal of IR. The standard brand of SR mainly adopted by IR meta-theory is considered to be a definitive and conclusive version of realism; this is mistaken, since many issues in SR remain unresolved, whereas different interpretations of realism abound. Picking the strongest version among them rather uncritically as the definite and final interpretation of realism and importing it in IR meta-theory is a risky business.

As a response to the issues which I discuss in this chapter, realism and anti-realism have both almost fragmented. SR must be refashioned. Realists have to choose what they are realists about. I go on to present briefly both positions which aim to weaken realism, as well as their alternatives, but under no circumstances do I exhaust the issue; such a task would be beyond the scope of the present work. My aim is merely to indicate that there are a variety of realist theories available.

In conclusion, I examine the fact that even the context itself of the realist debate generates controversy, due to the idealized conceptions of 'science' it harbours. The role that an idealized conception of science plays within the realist/anti-realist debate is brought out in detail. I note how the latter might dissolve if a more refined conception of science were to be used. The adoption of an unreal, 'mythological' conception of science is uncritical and permeates much of the work in IR meta-theory – notably Alexander Wendt's and Colin Wight's. Consequently, the aforementioned works inherit all the problems that go with such an adoption.

In Chapter 3, I start by arguing that although the influence that SR exerts on the philosophy of natural sciences remains uncertain, its impact on IR meta-theorizing has been noteworthy during the last few years. However, as already said, the complexity of SR is not fully recognized within the existing body of work of IR. The latter accepts as SR the particular kind of SR that is developed by Roy Bhaskar, which is also known as CR (Bhaskar [1975] 2008).

Therefore, before I move on to examine the alleged usefulness of SR to IR as a meta-theory on the grounds of which one can build 'legitimate' IR Theories – something which I will discuss in Chapter 4 of the thesis – it is necessary to examine the particular SR developed by Bhaskar, especially in terms of its ontological assumptions – hence my selective and precise use of Bhaskar's extensive writings.

In the second section of the third chapter, I present Bhaskar's conception of science as a social product directed towards an independently existing objective world. This allows me to present the distinction between the transitive and intransitive dimensions of scientific practice. In particular, I explain in what sense one may speak of transitive *objects* of knowledge.

In the third section, Bhaskar's philosophy of science will be set against two rival accounts concerning the nature of scientific *objects*: Humean classical empiricism and Kantian transcendental idealism. For Hume, experience exhausts all knowledge of the world possible for us; the world is but a surface, with no underlying structure. For Kant, on the contrary, there is an underlying structure, but this structure is imposed upon the world by man's cognitive activity; it is not an objective feature of the world itself. Bhaskar claims that both these rival philosophies of science are committed to a common *ontology*, which he calls ‘empirical realism’. I will show that the crux of this ontology is the concept of ‘empirical world’, i.e. of the world that is the object of a possible experience. For Bhaskar, empirical realism leads inescapably to a rejection of the intransitive dimension of science.

In the fourth section, I will present Bhaskar’s alternative philosophy of science, for which he uses the label ‘transcendental realism’. There, I am going to distinguish three levels of analysis: at the level of *epistemology*, I will show that transcendental realism is a theory about the objects of scientific practice; at the level of *methodology*, I will show that transcendental realism makes uses of ‘transcendental argumentation’; at the level of *ontology*, I will show that transcendental argumentation leads to a particular conception of what the world must be like for scientific practice to be possible.

Bhaskar makes use of two separate transcendental arguments. The first one focuses on the conditions of possibility of sense-perception, while the second examines the conditions of possibility of experimental activity. Therefore, in the fifth section, I will present a novel codification of Bhaskar’s sense-perception argument, which has the structure of a *reductio ad absurdum*. This transcendental argument leads to a categorical distinction between the level of the ‘empirical’ and the level of the ‘actual’, which also allows Bhaskar to claim that ‘events’ are to be categorically distinguished from ‘experiences’.

In the sixth section, I focus on Bhaskar’s most famous argument, which purports to establish the intransitivity of the world’s structure. *Contra* Hume, Bhaskar claims that the world should not be viewed as consisting of events and constant conjunctions of events. The world is not a surface, but has an underlying structure. However, *contra* Kant, this structure is not a mere product of human cognitive activity, but an independently existing feature of the world in itself. Through his transcendental argument which focuses on the notion of experimental activity, Bhaskar claims to have shown that causal laws of nature are categorically distinct from the regular sequences of events that occur by way of experimentation.

Bhaskar's second transcendental argument makes a substantive claim about the nature of causation. Natural causation is to be understood as the operation of some 'generative mechanisms' that produce the experienciable actual phenomena of the world, on the one hand, but which are to be categorically distinguished from them, on the other. For Bhaskar, what science seeks to accomplish is to acquire knowledge of these enduring generative mechanisms. It is this move from natural causation to generative mechanisms that I examine in the seventh section of the chapter in focus.

In the eighth section, I consider Bhaskar's next move from generative mechanisms to the 'natural tendencies of things'. This examination will allow me to focus my attention on the specific nature of Bhaskar's ontology, which is thoroughly, though not explicitly, Aristotelian in character. As I will claim, Bhaskar's philosophy has the paradoxical feature of presenting itself as a novel philosophy of science, which makes use of Kantian methodology (i.e. transcendental argumentation) in order to arrive at a non-explicit, yet undeniable, version of Aristotelian ontology of substances: a *thing* (Bhaskar's equivalent of Aristotelian *substance*) is to be understood as *a natural agent* endowed with particular *tendencies* (Bhaskar's equivalent of Aristotelian *habitus*) that empower it to naturally perform specific real activities.

Returning to the philosophy of science, I will show that, according to Bhaskar, to state a causal law is to describe a generative mechanism; but to describe a generative mechanism is to describe the natural tendencies of a thing. Thus I will explain what Bhaskar means when he talks about a 'normic' conditional, which is how a statement of a natural law is to be properly understood. The distinction between experiences, events and generative mechanisms leads to a further ontological distinction of the levels of reality: the empirical, the actual and the real.

To sum up, in this chapter I will show how Bhaskar begins with an inquiry in the field of philosophy of science and how he comes up with a full-fledged ontology, which purports to provide us with a richer and deeper image of the world that goes further and beyond *observable patterns of events*. However, this robust new ontology also leads to a novel understanding of the nature of science. Thus I will next return to the point of departure of this chapter, for it will then become understandable why Bhaskar's meta-theoretical principles can have a profound impact on the way working scientists understand the subject-matter of their field. Having examined Bhaskar's take on SR, which, as I mentioned before, is the prevailing form of SR found in the discipline of IR, I will turn my attention to the issue of how SR is used in IR theory and what this use entails.

In Chapter 4, I will examine how the concept of SR and its associated claims about theory, knowledge, and reality have been employed in IR. After a short Introduction to the chapter in focus, I will set up some theoretical caveats for the discussion of SR in IR, by clarifying which of its aspects I am going to illuminate more than others and why. Next, I will turn my attention first to

Wendt and then to Wight, in order to highlight the two different versions of SR in IR that have been widely discussed within IR, with CR being the one to have dominated the IR meta-theoretical discourse during the last years. This will be the start of my critical examination of the SR debate in IR.

I will focus on Wendt in order to prove that there is a thread of thought concerning SR which penetrates his paper about agents and structures in IR (Wendt 1987) as well as his next intellectual endeavours, namely his *STIP* (Wendt 1999), his paper on the alleged inevitability of the world-state (Wendt 2003), his paper on the state as a person (Wendt 2004), as well as his first publications on 'quantum social science' (Wendt 2006a, 2010) – the latter I discuss in the last chapter of the thesis. I will argue that Wendt has imported into IR the strongest of all the versions of SR that I have discussed in Chapter 2. Specifically, he has imported the version that relies heavily on the following three premises: first, there is a world out there which exists independently of our ability to know it or not; second, mature scientific theories must be taken at their face value; third, the world consists of both observable and unobservable entities. Wendt embarks upon SR mainly because he needs the notion of 'unobservables', so that he can theorize about structures in his attempt to describe the relationship between the behaviour of the states as units of the international system and the international system itself (with all its identities, powers, constraints, etc.). I will underline that he does not want to use the notion of 'structure' instrumentally, therefore he takes refuge in SR and claims that structures are entities either observable (e.g. material forces) or unobservable (e.g. culture, ideas); he thus prioritizes ontology over epistemology and uses the meta-theoretical framework of SR in order to build his own social theory of international politics upon it. I will further show that Wendt makes this meta-theoretical choice simultaneously with another choice, that of the agent-structure explanatory theoretical scheme, in order to reject both the alleged individualist methodology of neorealism as well as the holistic methodology of the world-system theory as being unable to explain sufficiently the production, reproduction and change of the morphology of international politics. In other words, I will show that Wendt's theoretical fabric consists of levels which are intrinsically connected in a way that one cannot change any of them without affecting the others.

In the second subsection, I will deal with Wight who, being a critical realist, endorses the Bhaskarain version of SR, namely CR (Wight 2006). As I will point out, he launches an attack against the 'epistemic fallacy' in IR, by rejecting the assumption that reality corresponds to the knowledge we have about it. Wight prioritizes ontology over epistemology and is all for epistemological relativism ('anything goes') and judgmental rationalism (that is, in any given case we can choose between competing theories). A lot of interesting consequences (e.g. SR is rather at odds with the idea of a 'correspondence theory of truth') as well as intriguing questions emerge from these premises. One such question, for instance, is whether Wight's epistemological relativism is simply limited to empirical research, in the sense that epistemology does not or should not play any role in our ontological and methodological considerations, a subject matter which, as I will show, is still contested. Moreover, I will highlight the fact that Wight believes that the meta-theoretical choices are ultimately political.

I will close this second section by presenting and critically discussing the positions of opponents of SR and/or CR, such as Kratochwil (2000), Suganami (2002, 2006), Chernoff (2002, 2007, 2009a, 2009b), Lebow (2011), Jackson (2001, 2008a, 2011) and others. A first general conclusion from this discussion is that many distinguished IR scholars who got involved in the SR debate in IR cannot distinguish between CR and SR. A second is that Wendt's theory is more worthwhile in its theoretical than in its meta-theoretical substance.

In section 3 of the chapter in focus I will deal with two important issues which have surfaced during the SR debate in IR.

The first one is the question about whether reasons can be causes, too. According to Hollis and Smith, reasons cannot be causes, since in *Explaining and Understanding International Relations* (Hollis & Smith 1990) they endorse a strict distinction between explaining and understanding in the study of international politics. On the contrary, Wendt thinks that we need a 'via media' between explaining and understanding and that the 'constitutive explanation' that he suggests, and which takes causes to be real, should complement 'causal explanation'. If this is the case, then reasons can be causes. I will undertake a philosophical analysis of this issue but at the end I will reach the conclusion that the reasons vs. causes debate is still open-ended.

The second issue has to do with what Fred Halliday (2000) used to call 'meta-theoretical hypochondria' in his attempt to criticize some IR theorists' extensive concern with ontological, epistemological and methodological considerations, at the expense of dealing either with theoretical problems which arise from actual politics or with empirical research. My conclusion is that interest in meta-theory does not amount to a waste of time, and IR theorists should be aware of their meta-theoretical choices, since the latter determine, at least to a certain extent, the content of those IR theories which they underpin. However, one should never forget that the subject matter of IR consists in the study of real-world political problems, which are inextricably connected with the daily agonies and hopes of ordinary people for a better life.

The last section of Chapter 4 summarizes in the form of a list the arguments of the chapter. The overall list consists of twenty two items but, for the time being, I will point out only two of them: first, I agree with Chris Brown's argument that CR can do the IR discipline service only if it can invigorate an interest in Marxist Studies in IR (Brown 2007); second, I call, along with Jackson, for a 'pluralist science of IR' (Jackson 2011), in which I would, nevertheless, also like to include – as Onuf suggests – language turn theories and speech act theories (Onuf 1989, 2012).

One may legitimately think that, after the adoption of SR by IR and its implications for the meta-theoretical considerations within the field of IR have been illustrated and criticized in chapter 4, nothing more could be said on this issue. However, both adherents to and opponents of Wendt's attempts to construct a methodological 'via media', which could combine positivist and interpretivist approaches to the study of international politics, have been challenged anew by his "*Social Theory* as Cartesian science: an auto-critique from a quantum perspective" (Wendt 2006a). The purpose of his new research programme, which has been inaugurated in the aforementioned paper and is still in progress, is to frame a new 'quantum social science', which I discuss in the fifth chapter of the thesis.

I will show that Wendt intends to base his 'quantum social science' project on his 'quantum consciousness hypothesis' (QCH), i.e. on the assumption that human consciousness can be explained through quantum mechanics. This assumption, furthermore, seems to be related to panpsychism. He argues that consciousness is a macroscopic quantum mechanical phenomenon in the sense that it behaves as both a wave and a particle, in compliance with the wave-particle duality of quantum mechanics. In this case, QCH aspires to resolve the so-called 'mind-body problem', which is one of the most difficult in the history of both philosophy of mind and science. Wendt was led to this idea from his belief that the adoption of the word-view of classical physics by social sciences limits their capabilities to fully explain, and even possibly forecast, the complex social and political phenomena taking place on the national and international levels. The solution to this deadlock is, in his view, the development of a 'quantum social science' which should be based on his QCH, which holds that the behaviour of individuals may be explained with the use of the aforementioned wave-particle duality of quantum mechanics. The aforementioned explanation presupposes the connection of two distinct physical concepts, namely the 'wave-function collapse' and the 'interaction with the environment'. Another point that arises from this discussion and is worth mentioning is that Wendt does not use quantum mechanics as a *metaphor*; instead, he uses it *literally* in order to provide a completely new and much more comprehensive social theory.

Wendt's interest in the study of 'collective consciousness' was apparent for the first time in his paper, "The state as a person" (Wendt 2004). In my view, this paper marks a turning point for Wendt's intellectual inquiries into the notion of *collective consciousness*, since he takes on board the relevant notions of *supervenience* and *superorganism.* Wendt has thus coined the concept of QCH in order to further explore and elaborate the notion of the *collective consciousness* of the state.

I will show that, in order to achieve this goal, Wendt has more recently invoked another theoretical tool; that of the 'holographic hypothesis', which is based on the 'holographic principle' of Astrophysics (Wendt 2010). The main point of his argument is that consciousness does not end with us but collective consciousnesses are also possible. Wendt believes this is feasible, if the 'holographic hypothesis' is applied when studying the relationship between individuals, the state and the international system. Such an application would consider individuals to be simple points in the international system, with each of them bearing on its own mind or having 'deposited' in its own memory all the information needed for the recreation of the whole international system. This transcends the distinction between individuals and societies and puts the classical level-of-analysis problem of IR Theory (Singer 1961) on another basis.

At this point, I will argue that Wendt's 'quantum social science' is compatible with SR, since it is anchored on the ontological duality of 'wave' and 'particle', and prioritizes this ontology in its methodological and epistemological considerations, according to the demands of the Bhaskarian version of SR. Furthermore, the answer to the objection that Wendt's 'quantum social science' belongs to the positivist methodological tradition – him having thus abandoned his proclaimed 'via media' – is that Jackson's 'pluralist science of IR' allows for a 'combination' of SR as a meta-theory with positivism as its methodology. Then I will devote a great deal of Chapter 5 to the discussion of the methodological, scientific and metaphysical pitfalls associated with the aforementioned position.

Regarding the methodological issues of Wendt's 'quantum social science', I will make clear that the invocation of weak *analogical* arguments, in the context of which aspects of human and social life are compared to aspects of the behaviour of quantum matter, is for many reasons erroneous. It is erroneous, amongst others, first, because the evaluation of an argument that is based on *analogical methodology* must take into account all known relevant similarities and dissimilarities, which clearly does not happen in the case under consideration. Second, it is erroneous because a genuine explanation cannot be exhausted by a mere reduction to the familiar, as we have learnt from the work on scientific explanation produced by Salmon (1992). Furthermore, I will argue that Wendt's reduction of the consciousness problem to that of the mind-body problem, and his attempt to solve both of them through the use of quantum science, might make sense only in the framework of a *metaphor*, and not when the analogy is taken *literally*.

In order to come to terms with the scientific pitfalls of quantum physics that are relevant for Wendt's 'quantum project', I will first present some basic notions and aspects of quantum mechanics. Then I will go on to criticize twelve points included in Wendt's paper on 'quantum social science' (Wendt 2006a). By doing so, I will show that Wendt misconceives many notions of quantum mechanics and misconstrues several physical phenomena. Finally, I will also clarify the context of the 'holographic principle' in Astrophysics, which has been coined by Susskind. I will do so in order to prove that there are neither similarities nor analogies between the 'holographic principle' and Wendt's 'holographic model'.

I am going to deal with the metaphysical weaknesses of Wendt's project through the examination of the mind-body problem. While one cannot, of course, ignore the amount of work going on in neuro-science about the mind-body issue, and the fact that this is hugely ambitious for them, let alone a social scientist like Wendt, I will argue that Wendt's quantum project lacks specific sound metaphysical underpinnings. First, it exemplifies some conceptual confusion about its metaphysical assumptions concerning matter. Second, its metaphysical assumptions concerning the relation of mind to matter remain promiscuously eclectic. This latter point can be deducted from the bibliography used by Wendt for the grounding of his argument as to how consciousness is related and reduced to the mind-body problem.

In the following section, I will examine the importance of the above shortcomings for Wendt's project regarding the creation of a new science of IR. This section is divided into two parts. In the first part, I will present and critically discuss Penrose's theory of consciousness as developed in his book, *The Emperor's New Mind* (Penrose 1989), and its sequel, *Shadows of the Mind* (Penrose 1994a). This is crucial for the consideration of Wendt's quantum project, since he based his QCH on Penrose's work on consciousness. I will conclude that Wendt makes two mistakes. On the one hand, his selection of Penrose's idiosyncratic consciousness hypothesis is eclectic, since he ignores the vast relevant bibliography on the subject, while, on the other hand, he misunderstands and misconstrues the basics of the theory itself.

In the same section, I will also discuss the implications of Wendt's QCH and his 'holographic model' for his new IR Theory. I will argue, among others, that the latter has not reformulated the level-of-analysis problem in a more comprehensive and 'productive' mode. Furthermore, his new quantum project does not help us understand any better than the already existing theories of IR and FPA how *collective consciousness* and, consequently, *collective identity* emerge.

My general conclusion is that, as long as some facets of quantum reality – in particular that of quantum measurement – remain a *mystery*, they cannot solve another *mystery*; that of consciousness. Therefore, Wendt's new quantum social science project, which is assumed to be based on some kind of quantum collective consciousness, is erroneous.

1.3 Conclusion

Alexander Wendt has introduced the strongest version of SR into IR as its proper meta-theory in order for him to be able to theorize about structures, the latter being one of the two poles of the agent-structure explanatory scheme he uses in his *STIP*. Contrary to Wendt, Colin Wight defends a rather idiosyncratic version of SR, the Bhaskarian CR, which is not very highly acclaimed by philosophers of science – this is clearly implied by a careful look at the SR debate in the philosophy of science literature. I will show why and how these two developments have had serious implications for the orientation of both meta-theoretical and theoretical debates within IR.

A lot of distinguished IR scholars have taken issue with SR and/or CR and, as I will also show, they have provided us with critiques of differing quality.

However, what is important is that some of these critiques have been launched against meta-theoretical considerations proper while others against the substantive IR theories which the former underpin.

The aim of this thesis is thus the critical discussion of the introduction and use of SR and CR into IR.

2

SCIENTIFIC REALISM

IN THE PHILOSOPHY OF SCIENCE

2.1 Introduction

This chapter is devoted to a critical exposition of the concept of scientific realism and its associated set of claims about theory, knowledge, and reality. As a view about science and scientific practice, scientific realism (SR) should not be confused with realism in philosophy of language, in philosophy of mathematics, or in political realism and neorealism. Scientific realism is a mature, autonomous pursuit in the philosophy of science, drawing mostly from the natural sciences and their history.

As will be described in more detail in Chapter 4, SR has been appealed to several times in the more recent meta-theoretical debates of IR. Although in the meta-theoretical context of IR, the scientific realist doctrines have been treated rather casually (if not simple-mindedly)[1], in truth SR has repeatedly set the stage for varied, rich and vigorous discussions, with numerous arguments, counter-arguments, and extensively developed strategies over realism in science. These have revealed that realism is neither as casual and straightforward a position as it seems, nor a conception that is easy to dismiss. Consequently, and mostly in order to defend itself, scientific realism has grown, from quite modest philosophical beginnings, to be the dominant position in mainstream Anglo-American analytic philosophy of science. Unavoidably, this

[1] This claim will become apparent from the analysis in Chapter 4.

sophistication has transformed the vivacious realism debate of the 1980s into an industry of endless exchanges of apparently scholastic arguments and rebuttals, where the application of more and more advanced technical tools from logic and semantics is deemed to be a *sine qua non* for genuine philosophical progress.[2]

As a result, no treatment of the realism issue can be both brief and exhaustive. Hence, although I shall try to go over the most important issues in as much detail as possible, still I shall not be able to cover all the areas currently under discussion. The reader should keep in mind throughout (and hopefully, confirm for herself by the end of the chapter) that SR is a vastly complicated issue, lively at times, and often repetitive in the argumentative forms employed in its defence, though always appealing, at least to the extent that one enjoys displays of skill in argument more than the drawing of definite conclusions. The reader coming from IR, especially, should be prepared for a discussion rather more philosophical in kind than she is used to. It should be apparent, however, that no matter where one comes from, appealing to SR is not a choice that should be made lightly: as will be seen, realism remains very controversial in the philosophy of science and one cannot 'borrow' from realist doctrines in order to bolster the scientific credentials of one's own discipline without incurring all the associated costs; and, as the IR scholar especially should appreciate by the end of this chapter, those costs can be quite high.

My next task will be the clarification of the doctrine of SR. Once this task is over, I will present the familiar arguments found in the literature, first those defending SR as a reasonable philosophical position (section 3.1), and then

[2] Of course, this phenomenon has not gone unnoticed; cf., for example, Rescher (1987): *xi*. The authors of a more recent work do not hesitate to talk of 'realist *ennui*' (Callender & Magnus 2004).

those questioning the coherence of SR as a philosophical view about science and its historical development (section 3.2).[3] This will produce some unavoidable repetitions, since I must come back to the core SR doctrines from different points of view in my examination of how the adherents of SR adapt their position to every new round of criticism. In the next-to-last section of this chapter, I mention briefly the various weakening strategies which less committed realists have pursued as well as the alternatives to SR currently on offer. Finally, in section 5, I summarize my findings and I offer my own conclusions. All along, I attempt to portray SR fairly and in the best possible light as the complex position its adherents take it to be; however, this should not mislead the reader: my conclusions will mostly be critical.

2.2 What is Scientific Realism?

SR is, essentially, a doctrine about ontological commitment to scientific posits and theoretical entities.[4] The core idea lies in the conviction that mature science, and the posits of mature science, are generally true in an ontologically demanding sense; scientific theories and hypotheses refer to real entities (whether the middle-sized entities of common sense or unobservable entities), forces, and relations; theoretical claims should be taken literally, potentially referring to a mind-independent reality whose details science itself studies.

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[3] Throughout these sections, I follow Psillos (1999). This is a deliberate strategy, considering that among the SR defenders, I have found Psillos' attitude to be the most representative and the most coherent; his treatment of the issues seems to me to be the most comprehensive one. Psillos tries to cover all the issues, never hesitates to draw detailed distinctions, pursues the realist stance single-mindedly, and seldom allows anything to pass without comment. However, our reliance on Psillos' work does not imply endorsement of his views, as will be seen in the course of the chapter.

[4] For brief (though interesting and at times comprehensive) overviews, see Blackburn (2002), Devitt (2005), Giere (2005), Glymour (1992), Lipton (2005), Psillos (2000a), or Wylie (1986).

Most scientific realists ally themselves closely with philosophical naturalism.[5] Metaphilosophically, this entails that realists tend to prefer empirical arguments –no different, at least in principle, from the kinds of arguments scientists put forward– and broadly naturalistic theories of meaning, reference and knowledge. Last but not least, most scientific realists take it that 'abduction', namely 'inference to the best explanation' (henceforth IBE), is a legitimate and reliable method to identify the best (potential) explanation of natural phenomena.

The debate over SR developed principally in the past thirty years. Historically, SR became the dominant philosophy of science as a reaction to Logical Positivism. The logical positivists construed scientific theories and hypotheses largely as instruments of calculation, or as convenient systems through which the scientists summarize the empirical regularities of observable entities and processes. Apparently, unobservables were deemed to be too 'metaphysical' for positivist strictures. Instead of taking them to refer to actual unobservable entities and processes, the logical positivists preferred to explain them away either instrumentally or reductively (as ultimately referring only to observable entities and events). However, this instrumentalist-reductive stance soon came to be challenged: under the guidance of such figures as Karl Popper[6], Grover Maxwell[7] and J. J. C. Smart[8] in the 1960s, the philosophy of science took the so-called 'realist turn', adopting science as a guide to truth,

[5] E.g. Devitt (2005), Psillos (1999). For discussion, see Rosenberg (1996), section 1, and Wylie (1986). Of course, there are exceptions: Niiniluoto (1999) simply abandons naturalism and offers normative accounts instead; Bhaskar (1979) turns to transcendentalism. A very interesting evaluation of naturalism in the philosophy of science is Worrall (1999). Complementary though constructive criticism can be found in Fuller (1991a).
[6] Popper (1965).
[7] Maxwell (1962).
[8] Smart (1963).

both ontologically and epistemologically. Out of the ensuing exchange of ideas, there developed a complex, multidimensional philosophical debate, with sophisticated arguments for and against SR. The current criticism of SR stems, mostly, from sceptically oriented empiricists, constructivists, verificationists, and other so-called ‘anti-realists’. The central issue today has been over how to interpret scientific theories in order to understand the progress of science.

However, defining SR is not an easy task. As Chernoff notes,

> Over the years, SR has taken vastly different forms, including inferential realism (abductive inference to the best explanation), fiduciary realism (where credibility is at the core), bivalence realism (according to which all statements are either true or false), entity realism (restricting SR to claims about theoretical entities and excluding claims about theoretical truth), theoretical-causal realism (restricting the positing of theoretical entia to those having causal status), and referential realism (according to which all entities in scientific theories have genuine ontic status). (Chernoff 2002: 191)[9]

Amid all these varieties of SR, one can nonetheless discern a family of views concerning truth, explanation, reference and progress, with their own metaphysical, epistemological and semantic dimensions. Realists disagree among themselves about which of these views should be given priority over others (which allows anti-realists themselves to multiply by negating selectively the different realist theses); all of these views, though, take it for granted that

[9] In note 6 of the same paper, Chernoff points to Patomäki & Wight (2000) for “half a dozen forms of nonpolitical realism”, to Putnam (1982) for multiple ones, Haack (1987) for nine, and Devitt (1991): 302-3 for “about a dozen”!

realism about truth and reality should be preferred over any anti-realist, constructivist counterparts.

In this section, I shall try to elucidate carefully the basic doctrines, which go under the name of SR. Following Psillos (1999), I shall adopt the idea that "going for realism is going for a *philosophical package* which includes a naturalised approach to human knowledge and a belief that the world has an objective natural-kind structure" (Psillos 1999: xix). The presence of such loaded terms as the above should already alert the reader that SR is not to be appraised on the basis of locating argumentative fallacies or inconsistencies on the level of detail; on the contrary, SR seems to be a cluster of interlocking philosophical theses, making up an apparently coherent whole which, perhaps, would be better understood if seen as the expression of a *stance*, or even of a '*picture*'. The guiding thread seems to be a second-order one; namely that, when it comes to broader philosophical issues in science, realism is the best explanation we have for the success and coherence of the scientific enterprise.

SR, as a distinct philosophical approach, consists of three theses:

(a) *The metaphysical thesis*: The world has a definite and mind-independent structure.

(b) *The semantic thesis*: Scientific theories should be taken at face-value. They are truth-conditioned descriptions of their intended domain, both observable and unobservable. Hence, they are capable of being true or false. The theoretical terms featuring in theories have putative factual reference. So, if scientific theories are true, the unobservable entities they posit populate the world.

(c) *The epistemic thesis*: Mature and predictively successful scientific theories are well-confirmed and approximately true of the world. So, the entities posited by them, or, at any rate, entities very similar to those posited, inhabit the world. (Psillos 2000a: 706-7)

Viewed as a set of commitments, the above theses project onto science the privileged position of providing access to a mind-independent reality, populated by the posits of our best scientific theories (or entities similar to them), which do not depend for their existence and nature on the cognitive activities and capacities of our minds. Therefore, SR defends a picture of science which at one and the same time embodies a heavy-going metaphysics (perhaps of natural kinds), a referential semantics, and a rather simplistic (though not simple-minded), both axiologically and pragmatically, truth-centred notion of scientific cognition.

Be that as it may, it ought to be understood that these dimensions of realism are only separated for the sake of exposition. Admittedly, each thesis seems to connect independently with the others in many significant ways; the definition of realism, however, presupposes all three. It would really not do to talk simply of 'mind-independent entities' without emphasizing that science provides us with the rational means to describe and discover the properties of those entities; that would be the *metaphysical thesis* without the all-important epistemic construal, which allows us to rely on the methods of science. Similarly, defining SR as the position which takes the theoretical and empirical statements of a science as 'approximately true', could be paraphrased away as merely the *metaphysical thesis* via the disquotational properties of the term 'true' ("S is true iff S"). Equally, however, one should not make much of the notion of 'truth' itself: neither a *theory* of reference nor a *specific* account of truth is presupposed in the definition of SR[10], only 'truth' in a loose semantic sense which may be cashed out in many different ways, even deflationary ones[11]. As we shall see in section 2.2, the purely semantic notion of 'truth' in SR is largely a settled issue: all the parties accept that theoretical claims may be taken as meaningful and capable of being true or false, without adopting a specific account of truth by doing so; the debate, therefore, is not over semantics *per se*, but over how ontology and epistemology impinge on the semantics. At bottom, SR is a *metaphysical* doctrine, with commitments which constrain the realist's understanding of science, scientific statements and scientific practice. Nonetheless, having adopted SR, one may go on and offer a construal of 'truth' in detail; indeed, the adoption of SR seems to be compatible preferably with a *non-epistemic* notion of truth, which "implies two things: first, that assertions have truth-makers; and second, that these truth-makers hinge ultimately upon what the world is like" (Psillos 1999: xxi). On this basis, a realist can oppose a non-realist over the definition of truth and the appropriate construal of reference, since the non-realist will usually reject such non-epistemic notions and restrict truth to matters of warranted assertibility and ideal justification instead.[12]

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[10] For details, see the excellent accounts of this matter in Devitt (2005): 770-1, Stein (1989): 50-1. Mäki (2005): 236 is also very clear about this matter.

[11] For example, see Horwich (1998).

[12] Having said all this, one would hardly expect any realist to conflate the semantic and the metaphysical (non-epistemic) notions of 'truth'; no well-informed and sophisticated scientific realist would find it helpful to ignore (at least) twenty years of literature (mostly by Hilary Putnam and Arthur Fine; see e.g. Leplin 1984b, or Psillos 1999, chapter 10) over this matter, nor would it make sense to repeat such a conflation all over again unless there were some original ideas in the offing. Nonetheless, Bhaskar (1993) & (1994) has done exactly this; with no original ideas, oblivious to all the arguments and the hard-won distinctions, he goes on to offer a 'solution' to epistemic issues by simply identifying the epistemic and the metaphysical (literally non-epistemic; he calls it 'ontological') conceptions of truth. What he comes up with is 'alethic truth' (which could be translated as 'true truth'): "… a species of ontological truth constituting and following on the truth of, or real reason(s) for, or dialectical ground of, things, as distinct from propositions, possible in virtue of the ontological stratification of the world and attainable in virtue of the dynamic character of science" (Bhaskar 1994: 251). I won't venture

I will now attempt to unpack the above theses one by one.

2.2.1 The Metaphysics of SR

Even though SR is not principally a philosophical doctrine about knowledge or being, its basic presupposition lies in a metaphysical ontology, which has quite a lot to say about both knowledge and being. This is why one would be well advised to understand first SR's specific brand of metaphysical realism before going on to assess its associated semantic and epistemological doctrines.

SR's metaphysics involves the claim of mind-independence. How is one to understand this? It has absolutely nothing to do with traditional idealism and phenomenalism, the notion that the world consists of mental entities, be they ideas or actual and possible sense-data. On the contrary, SR's metaphysics rests on a materialist (if not physicalist) construal of the entities posited by scientific theories. Thus understood, idealist or phenomenalist doctrines are repudiated *a priori*.

However, what might be *prima facie* surprising, is that SR's metaphysics gives up on common-sense realism as well —in the sense that, according to SR, common-sense realism (the belief that everyday objects have an independent existence) is *a priori* and fundamentally inadequate (if not misguided, since science is taken to correct the 'unreflective' judgements of common sense) as a form of philosophical realism about the world which science reveals. By focusing on the *theoretical* terms and the *theoretical* mechanisms of a theory, SR does not merely entertain the view that the empirical adequacy of a theory somehow mirrors real entities and processes; rather, SR's metaphysics includes

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into any explanation for this curious phenomenon; instead I will only point to the devastating critique (though, of course, not a very original one, since all these matters have been exhaustingly debated before for years) of Groff (2000).

claims about *un*observable entities as much as about observable ones. As a result, it imposes further requirements on the interpretation of 'mind-independence' than mere empirical adequacy: common-sense realism is implied by SR, but not *vice ver*sa. Assuming the existence of reality, 'mind-independence' extends to the *unobservable* entities posited by scientific theories (i.e., for the most part, these entities do exist and do enjoy the properties science attributes to them); the kind of reality we have assumed is, after all, the one science depicts, and, ultimately, it is the way science depicts it. Therefore, we may not know directly what *kind* of reality we have assumed, what exactly it is capable of doing (or even what it actually does), but, for SR, 'common-sense realism' does not (and cannot) exhaust our accounts of it. *This* is SR's specific brand of realism.[13]

> Thus, SR assumes not only the existence of an external reality, but also a definite philosophical kind of reality, one which seems to encapsulate a robust sense of objectivity: the real –whatever it is– is independent of the cognitive activities and capacities of our minds. It is in this sense that SR's metaphysics shapes our notions of knowledge and being: SR does not lay claim to a specific theoretical picture of reality; it only commits itself to a philosophical notion, according to which the kind of reality that exists is knowable scientifically and it is (approximately) as current and 'mature' science describes it (i.e. what we have called the epistemic thesis). What is relevant to the concerns of the ontologist, therefore, is the scientifically and theoretically describable. The emphasis is throughout on science: "If our

[13] For more details on the connection between common-sense realism and SR, see Clarke and Lyons (2002) and Devitt (1999). An interesting common-sense defence of realism is Kitcher (2001a).

> best science is not our best guide to our ontological commitments, then nothing is" (Psillos 1999: 70).

Accordingly, the SR version of 'mind-independence' abstracts drastically from scientists' practices (techniques, limitations, norms), in order to trace out what is taken to ground them and make them what they are —namely, what actually makes them *true*. The epistemic means and the conceptualisations used to know reality are free to change and evolve; still, according to SR's core metaphysics, through these changing and evolving practices, the mind-independent world emerges as the philosophically relevant arbiter —even though it is via scientific theories, and scientific theorising in general, that we discover and map out this world.

2.2.2 The Semantics of SR

If science is taken to be our ultimate arbiter of truth, it goes without saying that at least one of the main aims of science is to offer true (or approximately true) claims about how the world is. So, what kind of reality is science taken to describe? What kind of structure does the metaphysical thesis allow that there exist? Here, SR has very little of detail to say, although what it does say intends to capture all that is metaphysically important. Except from pointing out the unique, unobservable, natural-kind structure posited by scientific theories, SR resolutely insists that this structure, however in its details, exists objectively and

> independently of humans' ability to know, verify, recognize that [it does]. Instead of projecting a structure onto the world, scientific theories, and scientific theorizing

in general, discover and map out an already structured and mind-independent world (Psillos 1999: xx).

This way of developing the metaphysical thesis does not concern itself with the types of entity that exist (material, mental, etc.)[14], but rather with what is involved in *claiming* that they exist: "… the world is the way the scientific theory *–literally understood–* describes it to be" (Psillos 1999: 70, emphasis added); "*Science aims to give us, in its theories, a literally true story of what the world is like*" (van Fraassen 1980: 8, emphasis in the original). One might interpret this as an expression of "the idea that science is in the truth business" (Lipton 2001: 347; see especially Lipton 2005), or perhaps, of a view which "erects current science into a metaphysic and ties scientific realism too closely to that metaphysic" (Musgrave 1996: 21). Either way, the metaphysical thesis, in its essence, dictates a *literal* treatment of scientific theory. At this point, the need for a literal interpretation of theoretical claims implies that the metaphysical thesis has to be supplemented by a *semantic thesis*: matters of ontological commitment are seen to be the springboard for an elaboration of SR's semantics (the propositions of scientific theories are to be taken at face value).

However, talk of unobservability makes it painfully obvious that scientific theorizing is not ontologically innocuous: science assigns to the world both observable and unobservable features. According to SR, this has repercussions for our interpretation of theoretical claims in science: *semantic realism* (Psillos 1999: xx) is put forward as the correct stance toward the claims of scientific theories. Semantic realism is an 'ontologically inflationary' philosophical

[14] For a recent elaboration of this point, see Psillos (2005).

treatment of theoretical terms: “Understood realistically, the theory admits of a literal interpretation —an interpretation in which the world is (or, at least, can be) populated by a host of unobservable entities and processes” (ibid). In other words, the semantic thesis expresses SR’s prior ontological commitment: theoretical claims, both about observable and unobservables, are neither metaphorical nor meaningless, neither instrumental nor limited to the behaviour of observables — theoretical assertions have truth-values, are descriptive, and reiterate the realist’s metaphysical commitments, only this time in the context of how one should interpret words. A true assertion has ‘excess content’, which cannot be fully captured by any reference to observable entities and phenomena.

Therefore, according to SR, the issue of what makes a theoretical assertion true should involve the notion of (putative) factual reference: one should associate SR with the view that *truth* is a non-epistemic concept, so that “assertions have truth-makers… and…these truth-makers hinge ultimately upon what the world is like” (Psillos 1999: xxi). Crucially, this is why the semantic thesis is *not* a simple paraphrase of the metaphysical one. Contrary to appearances, defining semantic realism by using the terms ‘refer’ and ‘true’ and ‘literal interpretation’, on its own does oblige one to take the claims of scientific theories the way SR actually intends us to do, i.e. as expressions of essential, metaphysical commitments on our part about the ontological status of the world. For instance, taking their cue from the philosophy of language, where the simple ‘disquotational scheme’ of “‘such-and-such’ is true if such-and-such” transforms all truth-claims into sentences which do not mention truth, the non-realists may deflate any metaphysical commitments SR entails by translating them away ‘disquotationally’: i.e., as Devitt (2005: 772) remarks, the mere usage of the terms ‘refer’ and ‘true’ could “also be seen as exploiting only the disquotational properties of the terms with no commitment to the robust correspondence relation between language and the world”. Furthermore, alluding to the explanationist strategy for SR, he continues: “The realist argument should be that success is explained by the properties of unobservables, not by the properties of truth and reference” (ibid).

Indeed, there is a long, so-called ‘anti-realist’ tradition in philosophy, which denies the logical independence of the assertion that an entity exists from the issue of what constitutes evidence for the *truth* of that assertion:

> The truth of an assertion is conceptually linked to the possibility of recognizing this truth… If an assertion cannot be known to be true, or if it cannot be recognised as true, then it cannot possibly be true. (Psillos 1999: xxi-xxii)

Namely, this tradition takes it that the evidential basis for the truth of an existence-claim, on the one hand, and the semantic relation of designation (i.e., factual reference), on the other, should not be distinguished in the way that SR suggests. ‘Merely’ asserting the existence of something should not be taken as enough by itself to commit one to SR’s ‘robust sense of objectivity’. Anti-realists vehemently reject any notion of a ‘non-epistemic’ conception of truth[15]: rather, what is issued as existing involves a suitable set of conceptualisations and epistemic conditions (e.g., being ideally justified or warrantedly assertible) on our part. The fulfilment of a suitable epistemic/conceptual condition should

[15] The standard candidate for which is the correspondence theory of truth or even a causal theory of reference. See section 3.2.

be enough, if one is to commit oneself to the truth of a theoretical claim; putative factual reference is not needed. Semantically, this translates into the conflation of the evidence-conditions of an assertion's meaning and its truth-conditions: it cannot happen that a theory is epistemically *right* (it meets the relevant epistemic condition) and yet *false* — anti-realism precludes (*a priori*) this possibility of divergence by advancing an epistemic conception of truth.

This conception of truth, according to which the truth of an existence-claim is conceptually linked with the fulfilment of (ideal) epistemic conditions, usually arises from distrust towards theoretical terms. For example, since the meaning of theoretical terms is not given directly in experience, one might think that these terms are semantically suspect. SR's semantic thesis is designed to deflect the grounds for such suspicion; ultimately, SR's epistemic and semantic concerns are parasitic on metaphysical ones; SR, as a doctrine, is about the world itself and not merely our account of it. For that reason, the reader is well-advised to keep in mind that the semantic thesis plays a supportive role to SR's metaphysics: SR neither stands nor falls with it. Semantic realism is a doctrine about truth, but "no doctrine of truth is constitutive of metaphysical doctrines of scientific realism" (Devitt 2005: 771). Indeed, this is why SR is not, at least essentially, a doctrine about unobservables *per se*: rather, theoretical claims about both observables and unobservables should obey the same semantical standards. Claims about unobservables rest on contingent grounds: *our* (epistemic or pragmatic) difficulties to observe 'directly' certain objects of theoretical science. Unsurprisingly, therefore, scientific realists insist on the truth –conditions of an existence-claim, as the appropriate semantic standard for how to interpret it.

The observable/unobservable distinction should not have any significant semantic relevance.[16]

A brief perusal of the writings of major non-realists in the current literature[17] should be enough to make it clear that semantic realism is no longer a matter of debate: theoretical claims, like "photons exist", are taken by both sides (realist and anti-realist) to have a literal interpretation, according to which the claim is taken at face value, and can be either true or false.[18] It is far from clear, however, what the 'literal' interpretation of the claim commits us to, philosophically: it may well be that one admits that the claim has 'excess content' — i.e. content which cannot be paraphrased away as a claim solely about observables — but does this have any bearing on whether realism is the correct attitude to science? There does not seem to be any *prima facie* incoherence in granting both the semantic and the metaphysical theses, and yet still be sceptical or agnostic towards scientific theories. After all, merely admitting that the theoretical claims of science can be true or false certainly does not commit one to believe the stronger claim that current theoretical claims may indeed be *true*.

At this point, the *third* aspect of SR, the epistemic thesis, is relevant.

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[16] Indeed, it is not clear what exactly the observable/ unobservable distinction implies for defining scientific realism: "A coarse-grained sense that can be given to scientific realism is to say that it asserts the reality of *unobservable* entities: there are genuine facts that involve unobservable entities and their properties. But note the oddity of this way of putting scientific realism. I do not, of course, doubt that there are unobservable entities. But isn't it odd that the basic realist metaphysical commitment is framed in terms of a notion that is epistemic, or worse, pragmatic?" (Psillos 2005: 395). Psillos goes on to offer his own view of the reasons for which the scientific realism debate has focused so much on the observable/unobservable distinction. Later on, he goes so far as to say that "the issue of (un)observability is really spurious when it comes to the metaphysical commitments of realism" (ibid: 396).

[17] For example, those of Arthur Fine, Larry Laudan, and Bas van Fraassen. The term 'non-realists' in the text has been chosen instead of 'anti-realists' because, in this case, only van Fraassen counts as a genuine anti-realist. See section 4.

[18] For further details, see section 3.

2.2.3 The Epistemology of SR

The need of SR for a third thesis, the epistemic thesis, should be already apparent to anyone who harbours doubts about the possibility of science to deliver truth. We may believe in a 'mind-independent' world; we may also admit that theoretical discourse neither reduces to an instrumental interpretation nor limits itself to talk about observables; nevertheless, why should we accept the far stronger claim that science does (or even can) provide the 'royal road' to truth and knowledge of how things actually are?[19]

At this point, perhaps a realist ought to be satisfied with invoking *possible* empirical conditions that would warrant attributing *some* measure of truth to theories. However, in that case, it is far from straightforward to see how such a position would issue in rational or warranted belief in the unobservable entities posited by science (and the assertions made about them). For that reason, SR puts forward the epistemic thesis: the thesis expresses the realist's full commitment to science as a successful, rationally well-founded enterprise: to a scientific realist, it makes hardly any sense to believe in an objective, 'mind-independent' world, about which scientists make detailed existential and descriptive claims, and then go on and doubt the existence of the entities they posit and the truth of what they say of them. On the contrary, according to this third dimension of SR, science can and does deliver theoretical truth no less than it can and does deliver observational truth.

However, this does not mean that SR, as a philosophical position on science, is necessarily uncritical of its current (or past) form. An argument for the realist interpretation of scientific theories is not *ipso facto* an argument for *believing* in

[19] See van Fraassen (1998).

current (or any) science. Rather, the epistemic thesis concerns itself with *reasons*: do we have reasons to doubt that a mature and empirically successful science delivers truth? It goes without saying that we cannot simply declare *a priori* that science has been, or has to be, successful in truth-tracking; why, this may happen even by accident. Instead, we should take time to go carefully over its history and all it currently asserts. The epistemic thesis expresses the scientific realist's understanding that, in the end, reasons for doubt do not exist:

> [The epistemic thesis] of scientific realism intends to stress that it is *reasonable*, at least occasionally, to believe that science has achieved theoretical truth. In other words, the third realist thesis implies that there is some kind of *justification* for the belief that theoretical assertions are true (or nearly true), where this justification comes primarily from the ampliative-abductive methods employed by scientists. (Psillos 1999: xxi)

If reasons for doubt do not exist, we may as well commit ourselves to what the current scientific theories assert; this does not mean, though, that we commit ourselves to *everything* that current theories assert. Indeed, that we do not (and should not) do so may be taken as the natural outcome of our critical and detailed acquaintance with current and past science: having understood the theoretical statements of scientific theories as expressing genuine propositions does not require us to provide a blanket endorsement of the claims of science, since not even scientists themselves adopt such an undifferentiated attitude. Indeed,

> [S]cientists themselves have many epistemic attitudes to their theories. These attitudes range from outright disbelief in a few theories that are useful for predictions but known to be false, through agnosticism about exciting speculations at the frontiers, to a strong commitment to thoroughly tested and well-established theories. The realist is not less sceptical than the scientist: she is committed only to the claims of the latter theories. […] In brief, realism is a cautious and critical generalization of the commitments of well-established current theories. (Devitt 2005: 769)

Thus, one must not lose sight of the general philosophical issues at stake: SR is not about blind trust to science and scientists' claims (which would amount to scientism), but about the conviction that well-confirmed and predictively successful scientific theories should be treated as actual detailed descriptions of *true facts*. Equally, SR is not so much about the epistemic scope of science as a human, social enterprise, as about the philosophical significance of the products of scientific theories in terms of referring to *facts*. Indeed, "even if all the theories we ever come up with are false, realism isn't threatened" (Psillos 2000a: 708).

By treating science as capable of providing access to how things really are, scientific realists limit their attention to scientists' argumentative strategies for securing well-confirmed predictions; in this way, their true focus is on the factual descriptions provided by scientific theories, on the finished and well-founded products of science, and not on how scientists might actually go about establishing those products. Hence, the label of 'epistemic' in the 'epistemic thesis' should not mislead the reader into thinking that, somehow, in passing from the metaphysical to the epistemic thesis, scientific realists have gradually turned their attention to the scientific practices themselves. On the contrary, the focus on scientific practice seems, as it did above for the semantic thesis, to serve the dictates of the metaphysical doctrine itself: to the extent that SR necessarily employs a distinct metaphysical picture of a 'mind-independent' world to understand scientific theories, scientific practice is, equally necessarily, perceived as producing these theories by accommodating itself to the ontological features (constraints) of this world.

Therefore, one might be tempted to call the label of 'epistemic' a misnomer; indeed, Psillos himself comes to the same conclusion when hard-pressed to explain exactly how much weight should one assign to the metaphysical thesis when defining SR:

> Perhaps it was unfortunate that I called the last dimension of scientific realism 'epistemic'. I was carried away by sceptical anti-realist attacks on realism. I would now call it: the *factualist* thesis. (Psillos 2005: 396)

The point, then, is that SR, even when concerned with actual scientific practice, restricts itself to how the practice contributes to the tracing out of *true facts*. Indeed, one should not forget that SR is designed as a "ready-made philosophical engine" (Fine 1986a: 177) to interpret *mature* and *successful* science[20]; inevitably, this 'engine' takes in the sophisticated, complex, social and material traditions of inquiry we call science, and outputs a rather homogeneous collection of putative factual descriptions, produced by an 'army' of single-minded intellects advancing on truth. Admittedly, this is a caricature

[20] "If science does succeed in truth-tracking, this is a *radically contingent fact* about the way the world is and the ways scientific method and theories have managed to 'latch onto' it" (Psillos 2000a: 713).

of science as much as of SR; nevertheless, it certainly chimes well with another claim from a prominent scientific realist, a claim which one might equally well characterize as a caricature of science in these post-Enlightenment days: in response to the interpretation of SR as the combination of semantic thesis and an axiological thesis (along the lines of "science aims for true theories"), Psillos laments that, were we to restrict SR to this characterization, then "all the excitement of the realist claim that science engages in a cognitive activity that pushes back the frontiers of ignorance and error is lost" (Psillos 2000a: 708).

2.3 For and Against Scientific Realism: The Arguments

Not unreasonably, philosophical worries about the commitments of SR abound. As noted above, one is well advised to keep the issues of the ontological, the semantic, and the epistemic dimensions of SR distinct. Even though one may be ontologically committed to the existence of an external world, it is optional to be a semantic, or an epistemic realist. Clearly, belief in a mind-independent world does not entail that one can have true knowledge of this world; it is perfectly coherent for a metaphysical realist to insist on adopting a strong instrumentalist stance when interpreting current scientific theories, even the 'mature' and successful ones. This might allow for double semantic standards (one for observational statements and another for theoretical ones), without however bringing any obvious damage to a scientific realist's ontological commitments. So, what decides the appropriate stance of a realist? Are all realists forced to adopt SR if they are to honour their ontological commitments?

Instrumentalist claims have developed mainly in response to the above dilemma. Apparently, for an instrumentalist, ontological commitments have no normative bearing on a realist's theoretical commitments: it is entirely reasonable to take the meaning of scientific theories to be fully captured by what they say about the observable world; theories should be seen as instruments for the organization, classification and prediction of observable phenomena. Consequently, serious attempts were made on the part of instrumentalists to provide further support for this idea.

Historically, this position was taken to locate the terms of the debate in the interpretation of the theoretical statements versus the observational ones.[21] Using mostly arguments from logical analysis and mathematical logic, Craig's theorem (Craig 1956) was called upon to argue that theoretical commitments in science were in principle dispensable; they could be eliminated without loss in the deductive connections between the observable consequences of the theory.

This was eventually formulated as "the theoretician's dilemma" by Carl Hempel[22]: given Craig's theorem, the instrumentalist's reasoning developed roughly as follows: if theoretical terms were supposed to have no factual reference, then their truth-conditions would not obtain; without truth-conditions, a full explication of scientific theories would allow that one dispenses with theoretical terms entirely. And if this could actually be achieved, then no further question would arise over the factual interpretation of a theoretical statement (or over any commitment to irreducible unobservable entities).

A careful analysis of the reasoning above shows that two assumptions generate the dilemma if accepted uncritically: on the one hand, a sharp distinction between the theoretical language and the observational language has

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[21] In addition, the observable/unobservable distinction took center-stage in the definition of SR. See note 16.
[22] Hempel (1958).

been assumed without argument; on the other, scientific theories are conceived as establishing solely a deductive systematisation of observable phenomena. Both assumptions are highly implausible; this became apparent in the 1960s, when both the sharp dichotomy and the rather narrow view of theories were severely challenged.

Following Pierre Duhem[23], a holistic view was put forward, according to which no observational term is devoid of theory (in other words, all observation is 'theory-laden'). A scientific theory's terms function in interconnection with the rest of the language of science, and so none is anchored to the world of fact alone. In this sense, all observation terms have what one might call a 'theoretical' component; observations no longer represent the 'ultimate' data of science. Unless one conventionally stipulates what is to count as an 'observation term', the distinction between 'observation terms' and 'theoretical terms' is impossible to maintain. As Maxwell (1962) pointed out (among many others), in practice the sharp distinction presupposes a uniform use of terms which simply cannot exist. Hence, strictly speaking, there can be no purely observation terms, and theories do not represent merely the scaffolding of knowledge, to be used and then dismantled; rather, theoretical language and observations form an organic whole of interacting associations.

In addition, no theory has the restricted scope assigned to it by the adherents of instrumentalism: theories are multi-purpose tools, if tools they are; they allow for grand generalisations of observable phenomena, based on inductive arguments and inductive systematisations. They establish connections among observables, connections which go beyond mere deductive systematisations.

[23] Duhem ([1906] 1954).

Moreover, theories help us determine *what* happened by postulating an explanation of *why* it happened: they do not provide solely for empirical adequacy or correct verifiable predictions; they also characterize *explanatory* success. Therefore, one cannot simply identify the explanatory power of a theory with its predictive power. As such, scientific theories and theoretical terms are *indispensable* to science. Once these points were realized, there followed a series of indispensability arguments by Sellars (1963) and Quine (1960) (among others), which suggested that theoretical terms were indispensable for any mature and successful system of laws to organize and explain why observables obey the empirical laws they do.

Consequently, the instrumentalist interpretation of theoretical discourse in its original form (either as the non-assertoric treatment of theoretical statements, or as disguised talk ultimately about observables) proved very hard for anti-realist philosophers to hold on to. Thus, theoretical discourse came gradually to be taken as irreducible and meaningful by all sides of the debate; giving up the attempts to paraphrase theoretical statements into observational discourse, and admitting that theoretical terms are indispensable to a 'mature' and successful science, allowed philosophers to reach a kind of consensus, at least on this matter.[24] As the alert reader will have noticed, however, the instrumentalist challenges above were mainly to the semantic thesis of SR; they had very little to say on either the metaphysical or the epistemic thesis. Indeed, this is why semantic realism is no longer a matter of contention (as noted in section 2.2.2). Nevertheless, mere assent to a non-instrumentalist interpretation of scientific theories does not warrant or compel the title of realism. Mere opposition to

[24] For an excellent account of how this came about, see Psillos (1999), chapters 1-3.

instrumentalist semantics does not entail commitment to realist metaphysics, either. Hence, once again I have come full circle back to the unclarity which I began with: in what sense is one's realism *scientific realism*? The extent of a scientific realist's philosophical commitments is not yet sufficiently clear.

Granted, one can take SR's thrust to lie in fully subscribing to the *metaphysical* thesis; its other dimensions may be construed as further developments of it, or even supportive of SR's ontological kernel. The major philosophical worry, then, is whether SR, by committing itself to such a strong thesis from the beginning, has rendered itself immune to the empirical claim that, historically, science has not been devoid of misses in its record of *truth-tracking*; indeed, this worry has been the source of current anti-realist criticism.

It goes without saying that scientific realists allow for the possibility of a divergence between what there is in the world and what scientists claim there is. As noted in section 2.2.1, scientific realism does not entail uncritical acceptance of current science. It is felt, however, that weakening our commitment to the accounts of current science compromises the robust sense of objectivity scientific realists are after. This robust sense of objectivity is taken to be necessary if a number of anti-realist or social constructivist views about science are to be blocked. Therefore, the terms of the debate have focused on our stance toward the accounts provided by science: if one grants that a mature and predictively successful scientific theory is assertoric and ontologically inflated, why should one not take it as generating true (or approximately true) beliefs and knowledge about the world? Is it not irrational to keep an agnostic or sceptical stance toward theories in that case?

Apparently, putting the matter in this way construes the realism/ anti-realism debate in *epistemic* terms; namely, this time any anti-realist challenge has to be to the *epistemic thesis* of SR. Psillos puts it thus:

> [I]s there any strong reason to believe that science cannot achieve theoretical truth? That is, is there any reason to believe that, after we have understood the theoretical statements of scientific theories as expressing genuine propositions, we can never be warranted in claiming that they are true (or at least, more likely to be true than false), where truth is understood realist-style? (Psillos 2000a: 713-4)

Inevitably, once we assent to both metaphysical and semantic realism, it does indeed seem hard to abstain from adopting the scientific realist's epistemological rationale: it seems entirely reasonable to grant that the scientific enterprise (construed as pursuit of truth) should give rise (at least sometimes) to genuine knowledge of the world. SR's epistemic realism entails, therefore, 'epistemic optimism', according to which, science not only aims at truth and does the best it can in order to achieve this aim, but also that this aim is in principle achievable —the ampliative-abductive methods of science can produce theoretical truths about the world and deliver theoretical knowledge. In this sense, SR is 'presumptuous' (Psillos 2000a: 707): the *modest* claim, that "there is an independent and largely unobservable-by-means-of-the-senses world, which science tries to map" (ibid) should not be enough for SR; what one should aim for is

> the more *presumptuous* claim…that, although this world is independent of human cognitive activity, science can nonetheless succeed in arriving at a more or less

> faithful representation of it, enabling us to know the truth (or at least some truth) about it. (ibid)[25]

It is significant to make clear that this in no way assigns to philosophers of science the task of deciding which scientific theories should be heeded and which should not, any more than it allows them to distinguish the absolutely true theories from the absolutely false ones in current science:

> [I]t seems obvious that what realism should imply by its presumptuousness is not the implausible thought that we philosophers should decide which scientific assertions we should accept. We should leave that to our best science. Rather, presumptuousness implies that theoretical truth is achievable (and knowable) no less than is observational truth. (Psillos 2000a: 714)

Indeed, Psillos, instead of pressing home the ontological aspects of SR, opts for the epistemically strong claim, that "[T]he ampliative-abductive methods of science are *reliable* and can confer justification on theoretical assertions" (ibid), since nothing less can establish the sought-after connection between the mere possibility that certain epistemic conditions "warrant attributing some measure of truth to theories —not merely to their observable consequences, but to theories themselves" (Leplin 1997a: 102), and the guarantee that this possibility may be actualised *in fact*. Anything else than that, simply

> fail[s] to guarantee that this possible connection may be actual (a condition required for the belief in the truth of a theoretical assertion), [whereas] any attempt to give

[25] Psillos notes that the terminology of 'modesty' and 'presumptuousness' is borrowed from Wright (1992): 1-2.

> such a guarantee would have to engage the reliability of the method which connects some empirical condition with the truth of a theoretical assertion. Hence, the defence of the rationality and reliability of these methods cannot be eschewed. (Psillos 2000a: 714)

Again, this makes it quite plausible that the debate over SR should be formulated in *epistemic* terms. These stronger claims epistemologically distinguish SR from both contemporary versions of empiricist philosophies of science (which deny the possibility of rationally justifying belief and/or knowledge about unobservables)[26] and neo-Kantian, constructivist views (which deny the possibility of epistemic access to a mind-independent world)[27]. Consequently, it is no wonder that many realists have chosen to go for the stronger version as above.[28]

Nonetheless, one should not forget that the epistemic dimension of SR is just that: *one* of its dimensions, not the entire position.[29] SR does not consist in one single doctrine; therefore, the reader is advised to keep in mind SR's status as a family of interconnected theses, and hence, that the metaphysical and the semantic theses are always (and crucially) lurking in the background, even though the debate over SR takes place in epistemic terms. That this has further epistemological significance can be seen in the philosophical diversity of SR itself. As will be noted in section 2.3, realists take some interest in actual scientific practice, although they usually adopt a rather *a priori* realist attitude

[26] For example, van Fraassen (1980); see especially his (1998).
[27] Explicit examples here are more difficult to find, though Kuhn (1970) might be understood as one such; see Boyd (1983), section 4, Devitt (1984) or Hoyningen-Huene (1993).
[28] For example, Boyd (1983), Devitt (2005), Leplin (1997a), Psillos (1999), chapter 4, (2000a), Trout (1998), chapter 3. It is unfortunate that the otherwise learned and deep paper of Chernoff (2002) misses exactly this point (Chernoff 2002: 193) when he claims that Leplin (1997)'s 'modest' version of SR is representative of realism.
[29] This point is strongly made in Devitt (2005): 770-1.

in ontologically interpreting it. However, conjoining the metaphysical thesis with the epistemic thesis while downplaying the semantic one, allows a realist to accept that scientific practice discovers real entities and processes (unobservable or not), yet deny the *literal truth* of the theories in which descriptions of these entities and processes are embedded. Indeed, doubting that any current theory provides correct descriptions and thus sidestepping the question of theoretical truth, has been amply defended by Hacking (1983) and Cartwright (1983); it might even be that such a position, at least *prima facie*, with its emphasis on entities which may transcend theories (and theory-*change*) comes up more realist than standard SR. Furthermore, this kind of realism ties up more clearly with actual experimental practice, where manipulation of entities seems more decisive than any corroboration of scientific theory in the abstract. Indeed, thanks to such positions, there has been a renewed interest in the philosophy of experiment, the philosophy of models which scientists use in their endeavours, and a gradual shift toward scientific practices as the primary focus of the philosophy of science.[30]

For this reason, such positions have drawn quite a lot of attention from both realists and anti-realists. Much discussed has been Hacking's conception of realism, which distinguishes between *entity realism* and *theory realism* (Hacking 1983: 27). The one is about theory-transcendent entities, and the other about the truth-valuedness of scientific theories, respectively. Hacking claims that it is no great departure from the spirit of scientific realism to deny theory realism while asserting entity realism. This refocuses attention from realist interpretations of theoretical *discourse* to realist conceptions of scientific

[30] See, for example, Franklin (1986), Galison (1987), Gooding (1990), Gooding et al (1989). For the turn to actual scientific models, see Giere (1998) and Morgan & Morrison (1999).

practice, specifically of the laboratory practice. Experimental practice would be inexplicable, it is claimed, not if certain *theories* were not accepted as true, but if specific unobservable *entities* did not really exist; experimenters are taken to be dealing with laboratory phenomena caused by real entities, and this provides them with good reasons to be realists about theory-transcendent entities instead of theories. In any case, it may be (and it usually is the case) that experimenters are dealing with entities common to several theories and models, so it is not always clear which theory (if any) should be credited with the honorific of 'true':

> [O]ne can believe in some entities without believing in any particular theory in which they are embedded. One can even hold that no general deep theory about the entities could possibly be true, for there is no such truth. (Hacking 1983: 29)

> Even people in a team, who work in different parts of the same large experiment, may hold different and mutually incompatible accounts of electrons. That is because different parts of the experiment will take different uses of electrons. Models good for calculations on one aspect of electrons will be poor for others. Occasionally a team actually has to select a member with a quite different theoretical perspective simply in order to get someone who can solve those experimental problems...There are a lot of theories, models, approximations, pictures, formalisms, methods and so forth involving electrons, but there is no reason to suppose that the intersection of these is a theory at all. (Hacking 1983: 264–5)

> We are completely convinced of the reality of [e.g.] electrons when we regularly set out to build —and often enough succeed in building— new kinds of device that use

> various well-understood causal properties of electrons to interfere in other more hypothetical parts of nature… We design apparatus relying on a modest number of home truths about electrons, in order to produce some other phenomenon that we wish to investigate. (Hacking 1983: 266)

After all, theoretical views undergo change; whereas, posited entities (e.g. electrons), if real, survive theory-change. In addition, experimenters *do* things with these entities: they do not just detect and measure them; they also manipulate them. Hence, "engineering not theorizing is the best proof of scientific realism" (ibid: 263).

Is this a coherent position for a realist to adopt? It seems quite difficult to put theories aside when, at the same time, it is only theory that can provide the realist with knowledge of any properties those entities may have.[31] On the other hand, these properties do come with the theoretical presuppositions of the experimenters; why should one identify them independently with theories? Are the approaches of all groups involved in a modern experiment tuned, as it were, to each other, so as to form a theory?[32]

Perhaps the truly radical aspect of entity realism is the attention it devotes to experimental practice instead of abstract scientific theory. Indeed, a lot of non-trivial work seems to be indispensable for successful experimental processes and results.[33] Standard SR, however, usually involves an implicit 'filtering out' of the details of scientific practice, since the experimenters' methods belong to a context perhaps too 'raw' and unrefined for rational consideration. For

[31] See Clarke (2001), Gelfert (2003), Harré (1996), Morrison (1990), Nola (2002), Resnik (1994).
[32] See Franklin (1986), Galison (1987).
[33] See Collins (1985), Gooding (1990), Gooding, Pinch & Schaffer (1989) and the references in the previous note.

example, according to SR, genuine understanding of putative facts about electrons ultimately requires the elaboration of theories, which extend far beyond what is being used and needed in laboratory life.

Nonetheless, it should be noted that even if experimenters need some theoretical description of the entities they manipulate, this does not entail that they use *comprehensive* theories about them. In the final analysis, what the relative independence of experimentation from theory should make us accept is only that experimental manipulability bestows the right to *causal* talk. In its turn, talk about causes sets essential constraints upon further theory construction. And it is the final product of this process —theory— that realism should concern itself with and reason about. Entity realism, therefore, cannot be fully divorced from theory realism —or so standard SR claims. Once again, I have come full circle to the idea that standard SR requires the full panoply of the metaphysical, the semantic and the epistemic theses.

2.3.1 Arguments for SR

Primarily with the defence of the epistemic thesis in mind, current realists have focused on justifying their realism by appealing (not unreasonably) to the *empirical success* of science. Consequently, several so-called 'explanationist' strategies for SR have been developed and refined over the last few decades, based on arguments from the empirical success of science. Amongst these arguments one of the most highly regarded is the IBE of the empirical success of science, the so-called 'No-Miracle Argument'.[34] This argument has

[34] Boyd characterizes it as "the argument that reconstructs the reason why most scientific realists are realists" (Boyd 1983: 49).

undergone quite a lot of refinements[35], been roundly criticised by Laudan (1981) and others (see section 3.2), and yet is constantly reformulated and reaffirmed. It is usually traced back to three variants, as follows:

(i) Smart's[36] 'No Cosmic Coincidence Argument',

> Is it not odd that the phenomena of the world should be such as to make a purely instrumental theory true? On the other hand, if we interpret a theory in the realist way, then we have no need for such a cosmic coincidence: it is not surprising that galvanometers and cloud chambers behave in the sort of way they do, for if there are really electrons, etc., this is just what we should expect. (Smart 1963: 39)

(ii) Maxwell's[37] argument from the empirical virtues of realistically interpreted theories,

> As our theoretical knowledge increases in scope and power, the competitors of realism become more and more convoluted and ad hoc and explain less than realism. For one thing, they do not explain why the theories they maintain are mere cognitively meaningless instruments are so successful, how it is that they can make such powerful, successful predictions. Realism explains this very simply by pointing out that the predictions are consequences of the true (or close true) propositions that comprise the theories. (Maxwell 1970: 12)

[35] See, for example, Putnam (1978), Boyd (1984), (1985), (1990), Hacking (1983), Devitt (1991), Ladyman (2002), chapter 7, Leplin (1997a), Psillos (1999).
[36] Smart (1963).
[37] Maxwell (1970).

and (iii) Putnam (-Boyd)'s[38] 'No Miracle Argument' (henceforth NMA), which is arguably the most famous attempt at a purely realist explanation of the success of science:

> The positive argument for realism is that it is the only philosophy of science that does not make the success of science a miracle. That terms in mature scientific theories typically refer (this formulation is due to Richard Boyd), that the theories accepted in a mature science are typically approximately true, that the same terms can refer to the same even when they occur in different theories – these statements are viewed not as necessary truths but as parts of the only scientific explanation of the success of science, and hence as part of any adequate description of science and its relations to its objects. (Putnam 1975a: 73)

Note that the three arguments do not address the same camp of anti-realists: Smart and Maxwell are concerned to rebut semantic instrumentalists, hence they defend the semantic thesis, whereas Putnam argues for both the semantic and the epistemic theses; he wants to defend both the claim that theoretical statements may genuinely refer (and so they can neither be merely instruments, nor be reduced to non-theoretical facts), and that the theories themselves are approximately true (a logically stronger statement). Note also how Smart talks of *cosmic coincidence* and Putnam of *miracles*. On this point, both agree that SR does not allow for either cosmic-scale coincidence or miracles: observable phenomena do not just *happen* to be, and do not just *happen* to be related to one another in the way suggested by the theory. It is *because* theories are true and *because* the unobservable entities they posit exist that the phenomena are, and

[38] Putnam (1975a), (1978).

are related to one another, the way they are. This agreement should not mislead one, however, into thinking that the argumentative structures they use are the same. As Psillos notes,

> [...] Smart's 'no cosmic coincidence argument' relies on primarily intuitive judgements as to what is plausible and what requires explanation. It claims that it is intuitively more plausible to accept realism over instrumentalism because realism leaves less things unexplained and coincidental than does instrumentalism. Its argumentative force, if any, is that anyone with an open mind and good sense could and would find the conclusion of the argument intuitively plausible, persuasive and rational to accept —though not logically compelling. (Psillos 1999: 73)

Putnam's argument, by contrast, is not a 'general' philosophical argument, and certainly not *a priori*. Specifically, it is a philosophical argument of the abductive kind, i.e. it appeals to IBE. As such, it assumes that abduction is a reliable method of inference. However, the argument makes this assumption on rather naturalistic grounds: it takes it that IBE is the scientists' own method of producing approximately true theories, and since, typically, these theories have been arrived at by means of IBE, IBE is reliable.[39] For that reason, the scientific realist feels entitled to appeal to abduction and specifically IBE when invoking realism as the best explanation for the empirical success of science. I shall discuss in section 3.2 how this appeal to IBE is central in the defence of realism –especially when put forward in a naturalistic spirit.

Maxwell, on the other hand, differs from both Putnam and Smart: Maxwell focuses on the *epistemic virtues* which realistically interpreted theories are

[39] Indeed, Putnam (1975a) and Boyd (1973) had already argued that IBE is how scientists form and justify their beliefs in unobservable entities.

taken to enjoy, virtues such as simplicity, lack of ad hocness, and explanatory power, and then submits that statements lacking such virtues are less likely to be true than those that have them. Furthermore, as Psillos explains in some detail[40], Maxwell's argument involves an appeal to Bayesian probabilities, specifically to the 'prior probability' (reflecting the initial plausibility ranking) of competing hypotheses for science's success. Arguing on the basis of epistemic virtues of theories, and assuming that realism is better supplied with epistemic virtues than anti-realism, Maxwell infers that scientific realism has much higher prior probability than instrumentalism. Thus, he attempts to offer grounds for Smart's "primarily intuitive judgements as to what is plausible and what requires explanation" (Psillos 1999: 71), reaching the same conclusion as he: one should choose realism over instrumentalism. Roughly, then, whereas Smart's argument relies on seemingly *a priori* plausibility judgements (which may turn out to be distinctively philosophical), Putnam's proceeds rather naturalistically (capitalizing on the abductive-ampliative methods scientists already use). In that sense, "Maxwell's argument is the 'bridge' between Smart's a priori argument and the subsequent Putnam-Boyd naturalistic version" (Psillos 1999: 74).

However, taking into account that semantic realism seems to be contested no longer, plus the fact that current, sophisticated forms of anti-realism dispute exactly Maxwell's assumption (of realism as being better supplied with epistemic virtues than anti-realist contestants)[41], it is prudent to focus on Putnam's No Miracle Argument for SR. Indeed, Smart's and Maxwell's

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[40] Psillos (1999): 74-5.

[41] As Psillos takes pains to make clear, the modern, sophisticated anti-realist (van Fraassen, for instance) adopts a position which "…starts at precisely where Smart's and Maxwell's arguments stop", Psillos (1999): 77.

arguments, as originally formulated, seem to beg what is currently the main question: they assume that once semantic realism has been established, *the epistemic thesis* (that we have reasons to believe that our scientific theories are successful in truth-tracking) *can be granted without further argument* –almost as if belief in the truth of our theories were inevitable and rationally compelling. Furthermore, they do not assign adequate weight to *novel* predictions[42]: one should put extra stress on the fact that "only on a realist understanding, novel predictions about phenomena *come as no surprise*" (Psillos 2000a: 715). In other words,

> Since false theories can issue correct predictions, the realist needs some way to locate instances where he can be sure that truth is responsible for success. Since the prediction of built-in results allows the possibility that a theory is false but still successful, it seems the realist would be better off arguing that the real miracle is success where the result played no part in a theory's development; where the result is *independent* of the theory. (Rich 1999: 512)

Fortunately, NMA seems designed to defend exactly what is currently the main focus of debate, namely the *epistemic thesis* of SR, and it can do so in a non-question-begging manner: NMA can contribute to the defence of realism as an overarching *empirical hypothesis* supported by the fact that it offers the *best* explanation of the success of science, while at the same time rejecting any appeal to an *a priori*, distinctive philosophical method. Finally, NMA can be reformulated in such a way as to incorporate the highly theory-dependent dimension of scientific and experimental enquiries, and thus can be called on to

[42] Leplin (1997a) is the latest realist book-length attempt to make sense of novelty.

address the worries of current selective sceptics (such as van Fraassen). In this version, though still an abductive argument, NMA has a different explanandum: scientific methodology itself. Perhaps the best reformulation along these lines is Boyd's:

> According to the realist, the only scientifically plausible explanation for the reliability of a scientific methodology that is so theory-dependent is a thoroughly realistic explanation: scientific methodology, dictated by currently accepted theories, is reliable at producing further knowledge precisely because, and to the extent that, currently accepted theories are relevantly approximately true. (Boyd 1990: 223)

This kind of argument also seems perfectly in line with the epistemic optimism of realism, so let us examine briefly how exactly NMA supports the *epistemic thesis*. The main issue for the realist is to provide rational grounds to believe that scientific methodology leads not only to correct predictions and experimental success, but also to (approximate) truth and access to the real. Most scientific realists[43] adopt an IBE-based reading of NMA, generalising from the realist understanding of successful instances of IBE-explanatory reasoning in science, to a grand abductive argument for the reliability of IBE itself as the way to secure truth. The common pattern throughout is that to believe in the reliability of abductive reasoning is reasonable, since it tends to generate approximately true theories. Therefore, to address the defence of the *epistemic thesis* one proceeds in three steps:

[43] Boyd (1981), (1983), Devitt (2005), and Psillos (1999), (2000a). For complementary accounts see Putnam (1978): 18-9, Leplin (1984): 203, 205, 217, and Musgrave (1988): 232-4, 239.

(I) Firstly, they adopt an empirical-naturalistic approach: the realist takes it that the problem is ultimately empirical, not a distinctively philosophical one; accordingly, an empirical hypothesis seems to be in order. Realism is put forward in exactly this manner: "[The] defence of realism cannot be a piece of *a priori* epistemology, but must rather be part and parcel of an empirical-naturalistic programme which claims that realism is the best empirical hypothesis of the success of science" (Psillos 2000a: 717).[44] Thus, treating the empirical success of scientific theories as a 'radically contingent' and 'experimental' fact, it is suggested that realism accounts in the best way for that fact: "According to the distinctly realist account of scientific knowledge, the reliability of scientific method as a guide to (approximate) truth is to be explained only on the assumption that the theoretical tradition which defines our actual methodological principles reflects an approximately true account of the natural world" (Boyd 1983a: 71).

(II) Second, having assumed the (no longer disputed) *semantic thesis*, they identify how scientific methodology achieves approximate truth and instrumental reliability: realists suggest that, typically, reliable theories are arrived at via *abductive reasoning*, so that abduction is not only a legitimate scientific method but also a reliable one –it produces approximately true theories. Therefore, the theories that are implicated in the (best) explanation of the *instrumental* reliability of first-order scientific methodology should be accepted as (relevant approximately) true. This is, of course, an empirical

[44] Psillos attributes the originality of this move to Putnam (1978): 19.

claim: “This conclusion is not meant to state an *a priori* truth. The reliability of abductive reasoning is an empirical claim, and if true is contingently so” (Psillos 1999: 80).

(III) Finally, in the same naturalistic spirit as before, the realist may employ the same methods used by scientists themselves to provide an argument for the hypothesis of realism (provisionally taking any apparent circularity as a non-vicious one): since abductive reasoning tends to generate approximate truth (i.e. typically, the successful theories have been arrived at by means of IBE), IBE is legitimated as a cogent manner of reasoning, and hence, *instances* of such reasoning should be taken to be reliable, i.e. leading to truth. In that case then, NMA can go through successfully as an instance of IBE, a sort of ‘meta-abduction’: one supports, via IBE, the *global* hypothesis of realism as the best explanation for the success of science –(I) above– by appealing to local and particular instances of IBE-explanationist strategies for securing truth –(II) above. “[S]uccessful instances [of IBE] provide the basis (and the initial *rationale*) for this more general abductive argument” (Psillos 1999: 79): If particular empirically successful theories are approximately true, and have been arrived at (by scientists) via IBE, then, realist philosophers, using IBE, may safely conclude that the empirical adequacy and success of science is best explained by the fact that science achieves truth. Therefore, science *can* deliver theoretical truth –and the *epistemic thesis* can be safely and rationally asserted.

Note how in (III), IBE is used twice: once for successful scientific reasoning, and a second time for NMA itself. Note also that NMA is not simply a generalisation over scientists' use of IBE; rather, it serves the interests of a broader realist epistemology of science:

> Although itself an *instance* of the method that scientists employ, NMA aims at a broader target: to defend the thesis that Inference to the Best Explanation… is reliable. […] So, what makes NMA distinctive as an argument for realism is that it defends the achievability of theoretical truth. (Psillos 1999: 79)

Both of these features have drawn severe criticism from non-realists: the double appeal to IBE has been charged to be viciously circular, since by already employing IBE, NMA presupposes what is to be shown, i.e. that IBE is a reliable inferential method ("…the objection does alas appear to show that the No Miracle argument preaches only to the converted: it has no probative force for those who are not already inclined to use inference to the best explanation", Lipton 2001: 349). Furthermore, why should one take it that standard scientific reasoning and explanatory power have any evidential weight in the practice of philosophy itself?[45]

In defence, scientific realists appeal to naturalism and try to deflect both charges by admitting the fallibility of the argument on both counts: indeed, IBE could turn out to be unreliable, but realists have no reason to consider it unreliable, and, in any case, this does not concern NMA but IBE independently of the former, whereas the debate is over NMA (Psillos 1999: 85-6); NMA is no worse than attempts to defend *modus ponens* and inductive rules (ibid: 89);

[45] For standard formulations of both objections, see Fine (1986a), (1986b), (1991), Laudan (1981), van Fraassen (1980). Lipton (2001) and Douven (2001) give an excellent evaluation of the circularity objection, both pro and con. The interesting aspect of Lipton's evaluation is that the NMA reasoning "is the drawing of a *general* moral from the prior commitment to the truth of *specific* theories", (351, emphases ours). The implications of this remark are examined in the final section of this chapter.

and thirdly, NMA is not an *a priori* argument, it works in a non-foundational and naturalistic context, where judgments are informed by highly theory-dependent "backgrounds" and no *a priori* justification is intended (Boyd 2000). Finally, and most importantly, although NMA is explicitly based on the *particular* instances of explanatory reasoning in science to defend the *more general* claim that science provides access to truth, this does not allow for unrestricted application of the argument to empirical success. On the contrary, application of the argument to predictive success *simpliciter* is widely — and, in my view, rightly— regarded as unsound.[46]

An objection recently put forward[47] and "even harder to shake" (Lipton 2005: 1267), is that the NMA itself is fallacious: it ignores "adverse base rates" (Howson 2000), a statistical fallacy to which we are often quite prone. This is a sophisticated and complex objection:

> The miracle argument trades on the intuition that most false theories would have been unsuccessful. (Here one is considering the set of possible theories, not just those that have actually been formulated.) The intuition is correct, but the inference to truth is fallacious. Of course if all false theories were unsuccessful, then all successful theories would be true. However, from the fact that most false theories are unsuccessful, it just does not follow that most successful theories are true. One way to see this is to start not with falsehood, but with success. Given the constraint of success, we know that a true theory is one possibility; but given the underdetermination of theory by data, we know that there are also many false theories which would have enjoyed that same success. Most false theories would not meet the constraint, but many would, so alas it looks as though most successful

[46] Cf. Musgrave (1988): 231.
[47] See Callender & Magnus (2004), Lipton (2005).

theories are false! Success may be a good test in the sense that it has a low false positive rate, since most false theories are unsuccessful. However, that is not enough to show that most successful theories are true. ... What seems to be going on is that the miracle argument encourages us to assess the reliability of empirical success as a test for truth by estimating its false-positive rate (the chance that a false theory is successful), a rate we rightly judge to be very low. However, we ignore how incredibly unlikely it would be that, prior to testing, a given theory should be true. This has the effect of hiding from our view all those other theories that would be just as successful even though they are false. (Lipton 2005: 1267-8)

In a sense, then, NMA re-expresses the realist's *global* commitments to science as a road to truth, only now supported, in a biased way, via *local* considerations. Once again, therefore, it should not be forgotten that realist commitments rest above all on the *metaphysical thesis* —neither NMA nor the *epistemic thesis* stand or fall simply on their own; they are part of a philosophical 'package' which has to be compared *in toto* against competing empiricist or constructivist packages.[48]

2.3.2 Arguments against SR

There are two major challenges to scientific realism: the *Underdetermination Argument* and the so-called *Pessimistic Meta-Induction*. Both arguments challenge SR's epistemic credentials. The first begins from the claim that theories always have empirically equivalent rivals, i.e. empirical evidence is never enough on its own to determine the 'one true theory'; the second begins from an historical record of theories ultimately rejected, despite apparently enjoying at one time just

[48] Cf. Devitt (2005): 774.

the same kind of success which, according to the realist, indicates truth in current theoretical counterparts.

Although this pair of arguments has a venerable history (going back at least to Duhem and Poincaré), it would be better understood if placed in the context of the current debate over the *epistemic thesis* of SR: they both aim at securing epistemic implications, namely that belief in theory is never warranted by the evidence, and even if it were, the historical record of science shows that theoretical, scientific knowledge is neither cumulative nor progressive.

The Underdetermination Argument against realism is motivated by a strong scepticism toward belief in unobservables. It should be noted that it has no one clear source[49], although responding to it has been of continuous interest to realism's defenders[50]. The argument springs from the observation that more than one theory can accommodate the very same body of empirical evidence; the conclusion drawn is a doctrine of underdetermination of theory by all possible evidence: empirical evidence alone is deemed incapable of fixing theory-choice. A very simple and powerful formulation belongs again to Boyd:

> Call two theories *empirically equivalent* just in case exactly the same conclusions *about observable phenomena* can be deduced from each. Let *T* be *any* theory which posits unobservable phenomena. There will always be infinitely many theories which are empirically equivalent to *T* but which are such that each differs from *T*,

[49] Here it is worth mentioning Duhem (1906), (1908), and Quine (1960), (1975), among others. The argument in its modern form belongs to the Princeton philosopher Bas van Fraassen in his famous 1980 book, *The Scientific Image*. Van Fraassen calls his view *Constructive Empiricism* (see section 4). Cf. van Fraassen (1976) or (1980), chapter 3. Current debates include Douven (2000), Kukla (1994a), Laudan & Leplin (1991), Leplin (1997b), (2000), Okasha (1997).

[50] Bergström (1984), Boyd (1973), Devitt (2002), Earman (1993), Glymour (1980), Hoefer and Rosenberg (1994), Kukla (1994a), (1996b), (1998), chapter 5 & 6, (2000), Ladyman (2002), chapter 6, Laudan (1990), Laudan and Leplin (1991), Leplin (2001: 397), Newton-Smith (1978), (2000a), Psillos (1999), chapter 8, Stanford (2001), Wilson (1980).

> and from all the rest, in what it says about *unobservable* phenomena (for formalized theories, this is an elementary theorem of mathematical logic). Evidence in favour of *T*'s conception of unobservable phenomena ("theoretical entities") would have to rule out the conceptions represented by each of those other theories. But, since *T* is empirically equivalent to each of them, they all make exactly the same predictions about the results of observations or experiments. So, no evidence could favour one of them over the others. Thus, at best, we could have evidence in favour of what all these theories have in common –their consequences about "observables"– we could confirm that they are all *empirically adequate* —but we could not have any evidence favouring *T*'s conception of unobservable theoretical entities. Since *T* was *any* theory about unobservables, knowledge of unobservable phenomena is impossible; choice between competing but empirically equivalent conceptions of theoretical entities is underdetermined by all possible observational evidence. (Boyd 2002)

The core idea of the argument, then, lies in the straightforward attribution of indefinitely (possibly infinitely) observational equivalent rival theories to a successful theory. The upshot is the lack of sufficient *rational* grounds for preferring any theory to its rivals.

However, as Boyd himself notes, this ignores the fact that the theory and its postulated rivals could still yield different observational predictions when supplemented by appropriate *auxiliary hypotheses* (about the initial conditions, the instruments in the laboratory, the mathematical formalism, etc.), in which case there could be observational evidence favouring one over the other. Indeed, this is part of ordinary scientific practice: scientists routinely supplement theories with well established auxiliary hypotheses in order to obtain observational predictions from them. Moreover, as Laudan and Leplin (1991) argue, these auxiliary assumptions vary over time: they are both defeasible and augmentable. Therefore, what counts as the set of observational consequences of a theory also varies over time. So it is not entirely clear what one should take for the extent of the argument's application: what should count as 'observations' and what as 'theoretical statements'? This is why Boyd concludes that

> … it is probably best to think of the underdetermination argument as applying, not to "small" theories, but to "total sciences", large-scale conceptions of the world that might represent the total scientific conception of the world at a time. Such a conception would already contain all of the auxiliary hypotheses which were legitimate by its lights, so the problem just mentioned does not arise. In this revised form the underdetermination argument says that — whatever our best scientific conception of the world may be at any given time — we will never have any evidence that it embodies knowledge of unobservables. (ibid)

If that were the case, then the key word in all this would be 'unobservables': the underdetermination argument would seem to rely on a strict dichotomy between observables and unobservables, since empirical equivalence is taken to entail *different* theories with the *same* observational consequences. However, it has been argued that such a distinction is more of a matter of degree rather than an absolute, ontological dichotomy.[51] Moreover, it may well be that in the future the different theories will be reduced to (or replaced by) a third, more general theory which is empirically viable but not underdetermined (has no conceivable rivals); or at some point, there are successful observational predictions which allow one

[51] Cf. Maxwell (1962), the analysis of this paper in Creath (1985), and Kukla (1998), chapters 8 & 9.

to reject the rival theory. So, actually proving underdetermination may require hard theoretical work in the context of actual scientific practice. However,

> it is probably very difficult to provide actual examples of genuine underdetermination. If this can be done at all, it is surely a task for scientists rather than philosophers. But it seems unreasonable to expect scientists to concern themselves with such a task. Scientists are expected to contribute to the growth of science. They are not supposed to provide empirically equivalent alternatives to it. Hence, the thesis of underdetermination will perhaps never be more than a plausible conjecture. (Bergström 1984: 354)

Both objections certainly make sense as part of a defence of realism, especially if one takes into account the *metaphysical* dimension of SR: it has already been stressed repeatedly in the previous sections, that, according to SR, the ontological status of an entity should not be determined by its epistemic role in our theoretical accounts —observational status included. The metaphysical thesis should be accepted or rejected before one examines the epistemic issues raised by the underdetermination argument. With the metaphysical thesis in the background, the observational/theoretical distinction appears to be too anthropocentric and too shifting (theory- and technology-dependent) to provide cogent grounds for rejecting the realists' attitude: first of all, one might counter that the special epistemic role of the senses, on which the argument's emphasis on observation is based, derives from the fact that they are the only detectors we have built in to our bodies; furthermore, what counts as 'unobservable' can be revised (and refined) through the use of instruments and procedures whose justification is theory dependent.

This last point is crucial for the defence of realism if one focuses on *extra-experimental* standards for theory assessment, such as explanatory virtues or grounds for inductions to new experimental cases. It is certainly not the case that every theory-choice has to rely on experiment —else there would be a 'crucial' experiment every time a scientist makes a non-empirical judgment. Therefore, if underdetermination covers both rival theoretical claims and auxiliary hypotheses, then scientists have to appeal to *extra-experimental* standards to adjudicate between competing empirically equivalent theories, e.g. the role of explanation as an evidential standard.[52] Thus, by rehabilitating the notion of what counts as evidence for a theory (so as to go beyond mere observational predictions) knowledge of 'unobservable phenomena' is possible.[53] The problem, then, with the underdetermination argument lies with its excessively limited and unrealistic construal of 'scientific evidence' in actual scientific practice: we want theories that predict, but also *explain*, in a balanced and homogeneous manner, both observable and unobservable phenomena.

Summing up: the above considerations do not amount to a straightforward refutation of the underdetermination argument, but they certainly point to the proper context for its assessment; namely, the broader realist understanding of theory-choice both in current practice and in past history. If one has already adopted an instrumentalist, anti-realistic construal of theories, the underdetermination argument seems an attractive and cogent objection to the realist position. Closer attention to actual scientific practice, however, shows that knowledge about unobservables may sometimes be obtained, on the grounds that

[52] For more details, see the excellent account in Boyd (2002).
[53] See Boyd (1983), Lipton (1993), McMullin (1984). Another evidential standard may be the indirect manipulation of 'unobservables' (Morrison 1990, Nola 2002); indeed, I have already mentioned (in section 3) entity-realism (Hacking 1983) as one such line of thought.

‘evidence’ covers more than simple predictive success; it also provides explanatory power, which takes one beyond observations and instrumental successes.

Nonetheless, this alone is not enough to establish the credentials of such a strong position as SR; the above remarks show that, at least in this case, realism may tally better with ‘official’ scientific practice (where, occasionally, one seems to be able to choose, from a pool of empirically equivalent theories, the one theory which is more epistemically warranted than the others), but they certainly do not make realism reasonable across the spectrum. Indeed, being able to choose among empirically equivalent theories would have no bearing on the SR position, if it came about that one have no reasons to take seriously SR’s epistemic commitments. Inevitably, this brings me back to the debate over the *epistemic thesis*, specifically over the manner according to which one should understand science’s aims and development.

At this point, the main obstacle to the realist position is the historical record of science itself: the available historical evidence can be taken to show that scientific theories have been repeatedly overthrown as false, despite enjoying explanatory and predictive success (at the time); therefore, one might (meta-)inductively argue that belief in the realistic interpretation of current or even future theories is unwarranted. This inductive reasoning (from past referential and truth-tracking failures of science, to lack of truth and referential success of current theories), is the so-called Pessimistic Meta-Induction (henceforth PMI). The argument generally contrasts old theories, which even though they were accepted, and the evidence for them was quite persuasive, nevertheless, mostly turned out to be incorrect in the unobservables they posited. From this, the anti-realist concludes that with regard to the theories we currently accept, we should believe that (probably) most of them are likewise incorrect in the unobservable entities they posit.

PMI's main proponent has been Larry Laudan, who has repeatedly pointed out that

> [b]ecause [most past theories] have been based on what we now believe to be fundamentally mistaken theoretical models and structures, the realist cannot possibly hope to explain the empirical success such theories enjoyed in terms of the truth-likeness of their constituent theoretical claims. (Laudan 1984: 91-2)

Indeed, Laudan's own 1981 article "A Confutation of Convergent Realism" has become the *locus classicus* for the Pessimistic Meta-Induction (Laudan 1981). Comparing a variety of past theories to current ones, its central point aims to discredit the realist belief that current theories converge (even approximately) to how the world really is. Consequently, the realist's explanation of science's empirical success (for example, the No Miracle Argument for SR) cannot stand up in the face of historical facts.

Note that in order to motivate the argument, Laudan takes for granted (at least provisionally) the common ground of both realists and anti-realists, namely the point of view of our current theories as a standard of comparison for understanding and evaluating past theories. However, the argument trades on the realist view of our current theories in order to conclude that the realist view is untenable. Perhaps there is some tension here.[54] For this reason, Laudan's PMI should be roughly reconstructed as a *reductio* (Lewis 2001, Psillos 1996):

1. Assume that success of a theory is a reliable test for its truth.
2. So most current successful scientific theories are true.
3. Then most past scientific theories are false, since they differ from current successful theories in significant ways.
4. Many of these past theories were also successful.
5. So successfulness of a theory is not a reliable test for its truth.

To substantiate this claim, Laudan cites the following long list of theories as evidence: the crystalline spheres of ancient and medieval astronomy, the humoral theory of medicine, the effluvial theory of static electricity, the 'catastrophist geology', the phlogiston theory of combustion, the caloric theory of heat, the vital forces theories of physiology, the electromagnetic ether, the optical ether, the theory of circular inertia, and the theories of spontaneous generation (Laudan 1981: 33). All of these are currently taken to be fictions. From this list of examples (which, Laudan says, "could be continued *ad nauseam*"[55]), he concludes that a theory may be empirically successful without being approximately true**.** Therefore, the realist's focus on empirical success is radically weakened: how can truth-likeness explain success if even empirically successful theories turned out to be false?

[54] See Leplin (1997): 141-5.
[55] Laudan (1981): 224 in Balashov & Rosenberg (2002).

It goes without saying that realists have roundly criticized this argument on many grounds.[56] The range of the defence is quite astonishing, varying from a straightforward denial of the empirical successfulness of Laudan's examples[57], to the elaboration of quite intricate causal theories of reference[58]. In between, one may find a sophisticated eclecticism in what may be realistically taken as true in the examples of Laudan.[59] This diversity is not surprising, considering that the pessimistic meta-induction is said to be a "hard blow" to the explanationist defence of realism (Psillos 1999: 101), "the most powerful argument against scientific realism" (Devitt 2005: 784), and "among the niftiest arguments for scientific anti-realism" (Lange 2002: 281).

The best defence against the argument has focused on reconciling the historical record with realism. Engagement in historical case studies[60] usually involves showing that the abandoned theoretical components are not essential for the explanatory and predictive success of past theories: the realists should be selective in what they are realists about, both in terms of theories (only 'mature' theories should count) and of theoretical constituents (only those that have been retained in our current theories should be taken into account). If the theoretical components that survive theory-change are those that are responsible for the abandoned theories' successes, then the realist should attempt to identify

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[56] Boyd (1983), Carrier (1991), (1993), Cummiskey (1992), Devitt (1984), Hardin & Rosenberg (1982), Kitcher (1993), chapter 5, Ladyman (2002): 236-52, Lange (2002), Leplin (2004), McMullin (1984), (1987), Psillos (1996), (1999), chapter 5, Worrall (1994b).
[57] McMullin (1987); Worrall (1994b).
[58] Cummiskey (1992); Kitcher (1993); Leplin (1979). Causal theories of reference trace their origins to Kripke (1972) and Putnam (1975c, 1983b). See also Devitt (1981). This turn to causal theories of reference does not preclude older, so-called 'descriptivist' theories of reference; on the contrary, Psillos himself opts for such a one (1999: chapter 12). See also Niiniluoto (1999).
[59] Kitcher (1993): 140-9; Psillos (1999), chapters 5 and 6; Worrall (1989).
[60] For interesting realistic reconstructions, or reconstructions which argue for SR, see, for example, De Regt (2005), Kitcher (1993), Achistein (2002), Psillos (1994), (1999), chapter 6, Saatsi (2005), Trout (1994), Worrall (1994b). For corresponding criticism, see Chang (2003), Stanford (2003b).

those theoretical constituents of abandoned scientific theories, separate them from others that were more like speculative extras, or '*idle posits*', and did not play an explanatory role in predictions, and, finally, demonstrate that those components which made essential contributions to the theory's empirical success were retained through theory-change.[61] Psillos, for one, acknowledges the argument's force while trying to learn from it and adapt his position accordingly:

> Laudan, realists should say, has taught us something important: on pain of being at odds with the historical record, the empirical success of a theory cannot issue an unqualified warrant for the truth-likeness of everything that the theory says. Insofar as older realists have taken this view, they have been shown to be, to say the least, unrealistic. Yet, it would be equally implausible to claim that, despite its genuine success, everything that the theory says is wrong. The right assertion seems to be that the genuine empirical success of a theory does make it reasonable to believe that the theory has *truth-like constituent theoretical claims*. … [I] suggest that the best way to defend realism is to use the generation of stable and invariant elements in our evolving scientific image to support the view that these elements represent our best bet for what theoretical mechanisms and laws there are. (Psillos 1999: 109)

Devitt, too, admits that

> Scientific Realism already concedes something to the meta-induction in exhibiting *some* scepticism about the claims of science. It holds that science is more or less

[61] This has been the line of argument developed in detail in daCosta and French (2003), Kitcher (1993), Ladyman (2002), and Psillos (1999). See Bishop (2003), Bishop & Stich (1998), Douven & Van Brakel (1995), McLeish (2005) and especially Stanford (2000), (2003a) and (2003b) for incisive criticism.

> right but not totally so. It is committed only to well-established theories not exciting speculations. It leaves room for a theoretical posit to be dismissed as inessential to the theory. … In the light of history, some scepticism about the claims of science is clearly appropriate. The argument is over how much, the mild scepticism of the realism, or the sweeping scepticism of the meta-induction. (Devitt 2005: 784)

However, both Devitt and Psillos find that, if realism is to accommodate Laudan's historical examples, the features of the realist position should change:

> How should realists circumscribe the truth-like constituents of past genuinely successful theories? I must first emphasize that we should really focus on the specific successes of certain theories… (Psillos 1999: 109)

> Settling the argument requires close attention to the historical details. (Devitt 2005: 785) … [This] may be manageable. For, the anti-realist must argue that the historical record shows not only that past failures are extensive but also that we have not improved our capacity to describe the unobservable world sufficiently to justify confidence that the accounts given by our current well-established theories are to a large extent right. This is a hard case to make. (ibid: 788)

Note that both answers presuppose realism in order to make sense. They amount to prescriptions for a certain sort of historiographical analysis. Does this mean that the scientific realist should really stop arguing and do 'realist' history instead? Should the debate be over realist historiography instead of realism? This is unclear, but it would certainly be a serious change of focus: instead of grand claims and grand theories, one is advised to go local ("we should really focus on the specific successes of certain theories…"), and procceed case-by-case, to uncover those theoretical constituents which "really fuel the derivation" (Laudan & Leplin 1991: 462). What is needed are careful case-studies that will:

- Identify the theoretical constituents of past genuine successful theories that made essential contributions to their successes; and
- Show that these constituents, far from being characteristically false, have been retained in subsequent theories of the same domain.

> If all kinds of claims that are inconsistent with what we now accept were essential to the derivation of novel predictions and in the well founded explanations of phenomena, then one cannot possibly appeal to their truth-likeness in order to explain empirical success. Then, Laudan wins. However, if it turns out that the theoretical constituents which were essential are those that have 'carried over' to subsequent theories, then the 'pessimistic [meta-] induction' gets blocked. Settling this issue requires detailed study of some past theories that qualify as genuinely successful. (Psillos 1999: 110-1)

It is important to mention at this point the fact that arguments *contra* SR have been repeatedly drawn from the history of science; Kuhn's (1970) and Hanson's (1958) arguments are two of the most notable. Today the original arguments seem to be considered mostly peripheral to the debate; they seem to have been assimilated to the objections coming from the camp of sociology of science and of scientific knowledge (see section 5). In any case, both Kuhn and Hanson argue from historical examinations of the growth of science to the conclusion that neither method nor observation supports the realist conception of scientific growth: theory-dependence affects scientific knowledge to such a degree that nothing is left for realists to hold onto when theoretical perspectives change (Kuhn's scientific revolutions). Boyd (2002) calls their challenges the 'first version' of the 'Neo-Kantian Challenge' to SR (under the 'second version', he places the strategies and the alternatives to SR I shall discuss in section 4 of this chapter) and offers a cogent critique of this line of argument, treating their arguments as calling for a different theory of reference of theoretical terms than the one Kuhn and Hanson adopted. The reader should realize, however, that neither Kuhn nor Hanson have especially the doctrines of SR in mind; one could claim that their true aim is more 'foundational' in the philosophy of science: they (especially Kuhn) challenge the realists' understanding of scientific practice itself, i.e. the terms and the setting of the realism debate, not any specific realist thesis or argument alone. As such, their challenge addresses not the realists' understanding of the facts, but what actually counts as a 'fact' in the first place. I shall have occasion to return to this approach in section 5, when I offer my own conclusions. At any rate, as Boyd (2002) himself notes, this line of argument easily fits in with anthropological relativism and social constructivism, neither of which addresses the specific arguments in favour or against SR *per se* (NMA or PMI).

However, it is clear the detailed historical case-studies described by Devitt and Psillos should be required *anyway for the defence and explication of SR doctrines, irrespective of PMI*, since SR itself is put forward as an *empirical* hypothesis to explain the success of science: how else is one supposed to *test* realism if not by applying it in the analysis of actual historical episodes? As an answer to PMI, though, realist historiography seems quite weak: case-studies of historical events, realistically reconstructed, do not seem to have any special relevance to the anti-realist arguments *per se*; one could take them as realist 'just-so' stories, as 'retrospective realist' exercises, and offer anti-realist construals instead.[62] Moreover,

> the conservative policy of a retreat to believing only the least common denominator across the history of science seems perverse, since in many cases our current theories seem strongest precisely where they diverge from their predecessors. (Lipton 2005: 1266)

In a sense, even the interest to undertake such case-studies would seem to presuppose SR itself, rather than the opposite: uncovering formal and/or conceptual similarities and continuities between past and present theories, on the level of theoretical invariants and unobservable entities, can certainly not dispel any serious anti-realist doubts on its own. Perhaps not even 'success' should be taken as narrowly as both realism and anti-realism take it: it is perfectly conceivable that the major criteria for a theory's successfulness are not (or not only) empirical/observational; they may very well have more to do with the cultural and the ideological background of the times than actual empirical confirmations.[63]

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[62] 'Retrospective realism' comes from Andrew Pickering; see Nelson (1994), note 7. On the artificiality of realist stories, see Kukla (1996a), Nelson (1994), Stanford (2000), Psillos (2001b) and especially Stanford (2003).

[63] Indeed, this position (specifically, that the criteria have been —and are— mostly aesthetic and pragmatic) has already been well-argued by McAllister (1993): "Philosophers who debate the plausibility of realism —realists as well as antirealists— frequently assume without second thought that theories which were in history counted as successful were judged to be so on the criteria reconstructed and prescribed by today's philosophers of science. In particular, since most of the participants to the current debate concur in considering degree of observational success to be one of the most notable properties of a theory, they tend to assume that scientists have judged theories —and judged certain theories to be successful— by checking their degrees of observational success." (ibid: 206)

Moreover, it is not entirely clear whether Psillos realises the full implications of the move he is suggesting: Psillos's and Devitt's programmatic claims seem to compromise SR as a *global* doctrine about science. As Mäki (2005) notes, this localization turns scientific realism

> into a more selective account sensitive to the nature and performance of particular theories or disciplines. This has resulted in a shrinking scope for realism: those parts of science which do not fit with some canons of realism are being excluded and given over to anti-realist interpretations. (Mäki 2005: 231)

In other words, if one should go local and try out the '*divide et impera*' move (Psillos 1999: 108) on past and current scientific theories, why not do the same for SR *itself*? In other words, why go for such strong commitments as the *metaphysical* and the *epistemic theses*, if we can be selective in our ontological commitments, and focus only on specific domains of science (e.g. accept a realist construal for biology and astrophysics, but reject one for high-energy physics and cosmology)? Why not be equally selective in our ideological/metaphysical commitments too?[64]

Psillos's move, admittedly a reasonable one, and consistent with his *a priori* philosophical commitments for a global conception of SR, would seem to deflect successfully the PMI-argument's power against the *epistemic thesis* of SR, at the cost of watering down the *metaphysical thesis*: the realist's actual ontological commitments turn out to be so case-sensitive and historicised,

[64] See Fine (1991), who argues against the coherence of even such a position, and the response of McMullin (1991). That the standard methods for defending SR are not appropriate for the whole of science (e.g. historical science seems to be an exception) has been argued by Turner (2004), and his argument has been evaluated favourably by Carman (2005). Of relevance here is also the distinction of wholesale/retail arguments for SR in Callender & Magnus (2004). I come back to this distinction in the final section of this chapter.

epistemically and ontologically so dependent on the *present* understanding of science, that they no longer seem strong enough to sustain a general, metaphysical, and supposedly time-independent doctrine such as SR; in practice, taking theories 'at face value' turns out to be either useless or trivial. (What would such a contextualized realist be *in general* a realist about?) This is a serious problem for the *referential* claims of SR, since there can be no referential invariance of the supposed 'natural kinds' retained through theory-change, if there are no entities that 'stay put', so to speak.[65]

Yet Mäki (2005) for one, while accepting all anti-realist criticisms (ibid: 235) as well as the above realist programmatic aims (ibid: 236), still does not give up on the *global* scope of realism (or anti-realism). Strange as it may sound, Mäki himself puts forward 'doubly local scientific realism' in order "to make the concept of realism itself sensitive to the specific characteristics of various branches of science" and in this way contribute "to a sort of re-globalization", "to re-globalize realism by going local in a more penetrating manner than has been done by selective realists" (ibid: 232). At least *prima*

[65] Carrier (1991) and (1993) rejects the No Miracle Argument in favour of an alternative construal which avoids commitment to any *specific* 'natural kind'-account of referential invariance: he suggests 'kind-structure', the 'underlying structure of likeness and difference in nature' (1991: 34) as the *non*-referential element retained through theory-change. In any case, referential invariance, if developed for the purpose of countering PMI, seems to have "its epistemic priorities all wrong. For, *we know far less about reference* … than we know about what exists. In light of this, the rational procedure is to let our view of what exists guide our theories of reference rather than let our theories of reference determine what exists" (Devitt 2005: 785-6). For "interesting recent arguments to the conclusion that the realist is playing an illegitimate semantic game in trying to salvage her realism by tailoring theories of reference to ensure referential invariance" (Saatsi 2005: 518, note 9), see Bishop (2003), Bishop & Stich (1998), Douven & Van Brakel (1995), McLeish (2005), Saatsi (2005), and Stanford (2003a) and (2003b). In any case, it is not unusual for scientific realists themselves to criticise each other exactly on this point, e.g. Psillos (1997) criticises Kitcher (1993). In addition, it is not clear how the referential demands of scientific realism tally with the naturalistic approach adopted by realists —after all, the 'arch-naturalist' W. V. Quine defended 'referential indeterminacy' and the 'inscrutability of reference'! See McGowan (1999).

facie, ‘doubly local scientific realism’ seems to be the ultimate, so to speak, in watered-down realism and anti-realism:

> *Doubly local scientific realism (and antirealism)*: Some scientific disciplines or their parts at a particular time invite a realist$_1$ or realist$_2$ or realist$_3$ or … interpretation while others may invite an antirealist$_1$ or antirealist$_2$ or … interpretation. In other words, the adherence to realism is a function of kinds of scientific units [R_i] and of kinds of realism [x_i]: $R_1(x_1)$, R_2 (x_2), …, $AR_k(x_k)$, … (ibid: 233)

What’s more, Mäki’s SR does not invoke any of the familiar arguments from success, pessimistic meta-induction, or underdetermination (ibid: 235); he does not take approximate truth or reference as the core concerns of SR (ibid: 236)[66]; he talks of standard SR as defined by “features that are characteristic of the physics-based attitudes that have dominated the debates over scientific realism” and restricted by uses which are “outcomes of conversational trajectories that no community of philosophers collectively and deliberately decided to follow” (ibid).

These are impressive claims; however, even if accurate, one might wonder that if this is what realism should become, what exactly is there then to object about, in global or general terms. Surely, such conceptual flexibility in the understanding of SR cannot sustain any global or general anti-realist argument –*or a realist one, either*. Mäki has foreseen such an objection:

[66] “[R]ather they are derivative concerns. I take realism to be primarily an ontological doctrine. Semantics and epistemology are important but not constitutive of the core concept of realism” (Mäki 2005: 236).

My first response to this objection would be the reminder that there is a sense in which certain popular formulations of realism (and antirealism) already are discipline-relative: realism has characteristically been formulated with one discipline, usually physics or its representative parts, implicitly or explicitly in mind. Thus, those popular formulations are themselves already relative to discipline. My second response is a concession. The formulation of local versions of realism $R_1(x_1)$, R_2 (x_2), …, should be guided by some general realist background intuitions that are not up for grabs (it is another, and perhaps disputable, matter what exactly those intuitions are). So, there is a sense in which some general notion of realism precedes – and constrains – the formulation of its specific versions. The challenge would be to identify the generic realism that all such realisms should share to qualify as *realisms*. The good news is that the endeavour of formulating those local versions of realism may inform us about the proper formulation of a generic realism that is able to accommodate them all. (ibid: 235)

It is too early to judge matters over this 'new-generation' SR: obviously, at present this watered-down kind of SR lacks the appropriate vocabulary to provide the argumentative structure one would like to see and evaluate. It should be noted, nevertheless, that when all has been said and done, standard SR has been more or less *dissolved*: the realism/anti-realism debate has not had a straightforward resolution, but has been replaced by the search for new programmatic aims, new projects and new concerns. *That one should no longer go uncritically for the standard global conception of traditional SR with its associated construal of truth, success and explanation, is a fact which cannot be emphasized enough.*

Arguably, the dissolution of standard SR is a (direct or indirect) consequence of PMI. So what *do* the above mean for PMI, then? Perhaps PMI itself should also shed its global status and be understood in a very specific manner, tailored for a specific philosophical target, namely the contention that an 'empirically successful' theory should be deemed 'probably approximately true'. This connection of success and truth has been at the heart of the realist's intuition of the No Miracle Argument (the best explanation of success of science is the approximate truth of its theories). Perhaps PMI should be seen primarily as an argument developed to undermine NMA, and should be appreciated solely in that context, and not beyond it.

In any case, the jury is still out on the pessimistic meta-induction, and it is not clear what the verdict is going to be; indeed, it is becoming increasingly apparent that the grounds for this sort of anti-realist argument are not as strong as it first appeared, and even the cogency of the whole reasoning might be disputed: the argument itself may turn out to be actually fallacious[67]:

> The series of theories [in Laudan's examples] is not a set with independent members from which we have taken a random sample, for the series is the output of a process designed precisely to convert false theories into true ones. In this context, the fact that past theories have been false does not provide a reason to believe future ones will be false as well. Indeed, one might even argue that the history of science to which the pessimistic induction appeals points in the opposite direction. … Science is in the business of learning from its mistakes, and finding out what does not work may be an indispensable guide to finding out what does work. So one might go so far as to argue that we have more reason to believe some of our current

[67] See Callender & Magnus (2004), Lange (2002), Lewis (2001), Lipton (2005).

theories in light of our extensive knowledge about how various earlier alternatives to them have failed, than we would if, by some miracle, the current theory had been the first one in its domain, a theory without a history. (Lipton 2005: 1266-7)

In other words, the 'inductive base' of the meta-induction, the time-dependence of the 'inductive base', even the statistical conceptions implicit in its formulation, may all be doubted (Lewis 2001).

Consequently, at this point things get rather more technical and conceptually intricate than usual, in relation both to the statistical treatment (e.g. Douven 2005) and the historiographical details of the relevant episodes (e.g. Achinstein 2002). Contentious claims about truth-likeness, Bayesianism, formal theories of confirmation, formal notions of approximate truth, even the status of normative epistemology seem to be called for to keep the debate going.[68] For this reason, one must consider Worrall's suggestion that both NMA and PMI might better be called "considerations" for and against SR, and not arguments *per se* (Worrall 1989: 101). Indeed, such suggestions motivate further opposition to realism, though the alternatives have not been any less intricate or metaphysical or less strongly committed to elaborate *a priori* distinctions (e.g. between what's observable and what's unobservable, and between acceptance of a theory and belief, both of which can be found in van Fraassen's '*Constructive*

[68] This is especially apparent in the review symposium of Psillos (1999) in *Metascience*: while examining NMA and PMI, the reviewers call for (new or better developed) formal accounts of confirmation (Redhead 2001) or truth-likeness (Bueno 2001), and only one of them (Lipton 2001) seems to doubt the cogency of the whole enterprise (on the grounds that realism as such adds nothing to the evidence for the truth of scientific theories which has not been obtained already by the scientists themselves; instead, it *presupposes* it). For appeals to naturalistic epistemology, see Devitt (1991) and (2005): 787; for Bayesianism, Douven (2005); for truth, Niiniluoto (1999); Smith (1998); Weston (1992). All of these appeals are contestable, of course, and have been contested. It certainly seems ironic, though –albeit neither unscientific nor unreasonable mind– that supposedly naturalistically-conducted debates (over how scientific evidence should be understood — realistically or instrumentally) turn out to harbour seemingly irresolvable, *a priori* contrasts very close to the surface.

Empiricism'). All the parties seem to disagree over what counts as evidence, what is the right methodology to pursue the matter, and how much weight should be accorded to explanatory reasoning. Perhaps the disagreements cannot be resolved by the import of evidence and further argument: "[N]o considerations of fact or logic … can –or should– persuade proponents of either side [of the debate] to switch … [so that] the differences between realists and antirealists are irreconcilable" (Kukla 1998: 153-4).[69] Arthur Fine's '*Natural Ontological Attitude*' seems to rest on a similar motivation. I will now turn to a brief examination of these radical alternatives.

2.4 Weakening Strategies and Alternatives to SR

The lack of clarity, the tensions and the inconclusiveness implicit in the above have certainly had an impact on the proponents of both SR and anti-realism. Both realism and anti-realism have fragmented: some realists have tried to weaken realism, while non-realists themselves have opted for entirely new, alternative positions. *Structural realism*, *entity-realism* and Putnam's so-called '*internal realism'* (Putnam 1981, 1983a), all belong to the camp of weakening

[69] Kukla (1998) and Niiniluoto (1999) seem both to have been written in the wake of similar realizations. At least Kukla admits as much (quotation in the text; also, just before evaluating the underdetermination argument: "It's a pretty close repetition of what I've had to say about dozens of arguments on both sides of the realism debate: they are question-begging and redundant. I apologise for being tedious. But my material demands it", Kukla 1998: 89) and tries to provide reasons for an epistemology which actually accepts such 'irreproachable irreconcilabilities'; Niiniluoto, on the other hand, acknowledges the tension, but tries obliviously to carry on the debate as if it's business as usual: he offers formal accounts of concepts, puts forward an ontological picture, and avoids arguing in any depth for either! Clearly, under these conditions, there is *no need* to resolve the debate: one may simply offer pictures and accounts of already decided (*a priori*) realist views and beliefs, label them 'critical' and go on to carry implications (arguably, this describes quite well how SR has been received in IR). Thirty years ago, one might have proceeded in this cavalier manner without raising comments (Bhaskar ([1975] 2008) is a clear example of this attitude); today, however, one may legitimately talk of *ennui* and *déjà vu*, and abandon hopes for any real progress coming out of the realism debate; see Callender & Magnus (2004). Similar realizations can be found as far back as in Levin (1984) and Murphy (1990), and as currently as in Callender & Huggett (2011) and Lipton (2005).

strategies (though Putnam, as usual, is not entirely easy to place exactly). Worrall's epistemic structural realism (Worrall 1989) and the later ontic structural realism proposed by French and Ladyman (French & Ladyman 2003), Bas van Fraassen's constructive empiricism (van Fraassen 1980) and Arthur Fine's 'Natural Ontological Attitude' or NOA (Fine 1986a), belong to the alternatives. Since these positions are quite elaborate philosophical viewpoints that engage fully with all of realism's philosophical commitments, in order to do them full justice I would have to approach them as the stand-alone positions they are meant to be and to develop them extensively and independently of the debate on SR. However, this is beyond the scope of the present work, and as such my exposition on them will be rather brief. Every position above has something going for it, but they all differ, like entity-realism does, from the core commitments of SR in their own specific and often unique ways[70].

Structural Realism (defended e.g. by Worrall 1989) holds onto the mathematical structure of a theory, namely it claims that scientific knowledge is restricted only to these structural aspects of reality which are encapsulated in mathematical structure. Therefore, one may note that the truly radical aspect of this version of structural realism is the attention it draws to the fact that today's scientific theories are also (and indispensably so) intricately *structured mathematical* theories, and not a mere collection of physical concepts. This fact had not been appreciated enough in the realist literature; also actual experimental practice had been underestimated before entity-realism's call for attention to it.

[70] The manner in which entity-realism (the position that gives up on theory-realism in favour of belief in the reality of the entities which experiments take to be part of the causal web of the world) differs from SR was referred to in section 3 of the present chapter.

In other words, Worrall's epistemic Structural Realism gives up on its specific ontological-conceptual commitments (the intrinsic nature of the entities themselves)[71] and tries to combine the lessons from the pessimistic meta-induction, which was induced by anti-realists, and the no-miracle arguments that I discussed above. In that respect Ruetsche notes that:

> The Structural Realist takes both lessons to heart by espousing a realism about the form of structure of our best theories, a realism which explains their empirical success; these forms or structures inoculate themselves against pessimistic inductions by carrying over to successor theories. (Ruetche 2011: 347)

Ladyman questioned Worrall's epistemic Structural Realism because of the latter's failure to address the issue of ontological discontinuity that has dominated the debate between realists and antirealists. Ladyman (1998) proposed a radical form of ontic structural realism, which challenged the metaphysics and the semantics of scientific realism and claimed that structures are the only things that exist. Yet, this form of structural realism does not allow for unobservable entities and, as a consequence, dismisses the role that the discovery of these entities might have in the change of structures. The discussion regarding the content of structural realism and its foundations is still commencing, since structural realism aspires to offer a solution for overcoming the earlier dilemmas that have been dominating the discussion between realists and anti-realists (or empiricists). In his attempt to interpret quantum field theory

[71] For exposition, see Bueno (2001), Chacravartty (2004), Ladyman (1998), and Worrall (1989) (perhaps Carrier 1991 is also relevant here). For criticism, see Cao (2003a, 2003b), McArthur (2003), Psillos (1995), (1999), chapter 7, (2001a), Redhead (2001) and (2007). For the latest defence, see Worrall (2007). Redhead (2001)'s comments are especially useful for understanding the meaning of the term 'structure' (a task already undertaken in current philosophy of mathematics, e.g. in Shapiro 1997).

(Cao 2003a, 2003b), Cao develops a kind of constructive structural realism *vis-a-vis* the development of Quantum Chromodynamics (QCD) (Cao 2010).[72] Dawid develops what he calls 'consent structural realism' in order to deal with the fundamental challenges posed to ontology by string theory (Dawid 2007),[73] which dominates our efforts to unify all natural forces (Weingard [1988] 2001). Finally, Ruetsche (2002a, 2002b, 2011) argues in favour of taking "coalesced structures seriously, notwithstanding the role idealizations play in constituting those structures" (Ruetsche 2011: 349). This is because of the strong possibility that those "structures might persist in future theories, a possibility lent some plausibility by the role played by renormalization, group techniques, devised to accommodate those structures, in shaping physicists' quantum field theories" (ibid: 349). In other words, Ruetsche challenges previous concepts about the notion of structure and defies previous forms of scientific and structural realism.

Putnam's internal realism (Putnam 1981) transforms the concept of truth itself, proposing a perspectivalism instead of the external-world correspondence built into realism itself; while NOA gives up on both realism and anti-realism, charging them both with unreflecting fixation on certain unrealistic, unreasonable 'pictures' of science, and advising replacing theoretical

[72] By examining the history of the evolvement of QCD and in particular the interaction between theoretical conceptualizations and experimental "observations" of quarks and gluons in the events of naked charmed particles and three jets, Cao proposes a more sophisticated form of structural realism, namely constructive structural realism. The basic assumptions underlying his theory are: "i) the physical world consists of entities that are all structured and/or involved in larger structures; ii) entities of any kind can be approached through their internal and external structural properties and relations that are epistemically accessible to us." (Cao 2010: 6)

[73] The consent structural realism proposed by Dawid (2007) is a more modest form of structural realism and is based on the assumptions that (a) one can accumulate knowledge about structures and that (b) one can gradually approach the one true structure implied by string theory. This form of structural realism attempts to do justice to the observation of real entities and the role they play in structuring our theories. Furthermore, this form of structural realism, also, takes into account the structures through which one accesses knowledge, the use of which can make the world observable to us. According to Dawid: "Thus emerges an intricate compound of mutual dependence between observational and structural aspects of reality" (Dawid 2007: 41).

commitment with a non-special ('natural') attitude toward science — one that is minimal, deflationary ('quietist') and explicitly local. It advises the careful examination of particular scientific claims and procedures, and cautions against any general interpretive agenda toward science, in a rather anti-philosophical, Wittgensteinian spirit. It certainly goes against the spirit of the entire realism/anti-realism debate as it has traditionally been conducted — a fact which, of course, has been noted (and roundly criticised) by scientific realists[74].

Van Fraassen's constructive empiricism, by contrast, redefines anti-realism itself, while trying to honour the core intuitions of the empiricist, anti-realist position, in a way that is fitting for our times. In this spirit, van Fraassen replaces empiricism's distinction in terms of observational and theoretical terms and predicates with one between observable and unobservable *entities*. Further, his constructive empiricism takes empirical adequacy (not truth) as the goal of science. As a result, when it accepts a theory, it accepts it as 'empirically adequate', i.e. in an instrumentalist-like manner. This involves commitment to working within the framework of the theory but eschewing belief in its literal truth. At least *prima facie*, both distinctions —observable/unobservable and belief/commitment— seem to fly in the face of the realist attitude toward scientific theory and practice.[75]

Putnam's views have been out of the debates for quite a long while now. Putnam himself soon gave up on them and turned his back on the realism/anti-realism debate in order to pursue other, less 'metaphysical', philosophical interests. Structural Realism, as was manifested in the brief discussion above,

[74] For criticism, see Kukla (1994b), Musgrave (1989), Psillos (1999), chapter 10.

[75] For criticism and defence, see Alspector-Kelly (2001), (2004), Fine (2001), Giere (2005), Gutting (1983), Kukla (1995), (1998), chapter 8, Leeds (1994), McMichael (1985), Musgrave (1985), O'Leary-Hawthorne (1994), Psillos (1999), chapter 9, (2000a), (2000b), Rosen (1994), Teller (2001), van Fraassen (1994).

has drawn a lot of attention and has developed into a position with its own associated literature of sustained criticism. Arthur Fine's and Bas van Fraassen's positions have grown complex enough to deserve a whole chapter in themselves. Both are the subjects of ongoing discussion and their associated literature has grown so vast that a more detailed investigation of their work is beyond the scope of this thesis.

Nonetheless, I have already reached the limits of standard SR, the specific *rationale* of which should by now be clear.

2.5 Criticism and Conclusion

What ought to be clear by now is that standard SR can no longer be accepted in its entirety as it is. SR has to be refashioned, in order to accommodate the empirical failure of otherwise successful theories, as well as the discontinuity of conceptual and ontological commitment, so evident in the history of science. Realists have to be selective in what they are realists about: the literal reading of entire theories ('taking theories at face value') cannot be sustained as it is. All this was discussed in the analysis of the implications of the PMI-considerations: the global, uncritical version of SR just doesn't stand up to historical scrutiny, either as an explanatory device for the success of scientific theories, or as a philosophical interpretative approach to science. Indeed, what I ended up with was, at best, Mäki's 'doubly local scientific realism' or something close to it:

> In seeking to be informed about the sciences, doubly local scientific realism subscribes to principles such as the following: the applicability of realism to a unit

> of science is a matter that has to be decided locally, case by case; the information needed for such decisions has to be acquired from local sources, by way of detailed empirical examination of such units; the identification of the proper units of science and the proper contents of realism is a locally interactive and empirical matter; any claim about science more generally or more broadly has to be based on local philosophical inquiries, in a bottom-up matter. (Mäki 2005: 234)

Therefore, in the process of assessing SR, one is to conclude that the truly important level of the realist's philosophical analysis lies at that of the *detailed* mechanisms and the *specialized* theoretical elements of *conceptually intricate, formally sophisticated, fully mathematical*, scientific theories. As I elaborated in section 3, *the methodological and the technical complexities of this level can hardly support a grand-theory type of narrative*[76] *such as standard SR.* They certainly don't point directly to any kind of a realist interpretation, either. On the contrary, as has been repeatedly argued by post-Kuhnian approaches to the philosophy of science[77], such a 'piecemealist'[78] approach would take one directly out of the context of the realism debate and into examining the manner in which the context itself has been set up. Indeed, our own suggestion shall be that the context of the realism debate itself generates controversy, because of the idealized conceptions of 'science' it harbours.

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[76] The word 'narrative' should not be taken as pointing implicitly to any postmodern type of critique, although there are affinities to postmodern critiques in what follows. However, 'Postmodernism', it should be kept in mind, is a very ambiguous term in the philosophy of science nowadays: it may encapsulate anyone from Quine (Murphy 1990) to Feyerabend (Preston 1998); mostly, the term comes up (unjustifiably) when discussing the Sociology of Scientific Knowledge programmes and similar stances (see Gal 2002 for a sober analysis of these in relation to realism). On the other hand, I take this occasion to explicitly distance myself from any deconstructionist versions of postmodernism: I have found them totally unhelpful in evaluating the realism debate; see, e.g., Parusnikova (1992).

[77] Fuller (1989), Gal (2002), Galison (1988), Miller (1987), Roth (1996), Rouse (1981), (1987), (1991a), (1991b). Interesting realist treatments of these matters are Boyd (1992), (2002), Giere (1998), Kitcher (1993), Nola (1994), Radder (1993).

[78] This term is borrowed from Rouse (1991): 141, who credits it in turn to Dudley Shapere.

I shall come back to these remarks shortly. For the time being, I would like to prepare the ground by making three points.

First, the reader should note how the realists' commitment to the complex theoretical machinery of current science does seem to be, at least *prima facie*, rather precarious. Current theories have many mathematically equivalent formulations, and vary according to the purposes for which each is used. Each one also employs a great range of incompatible models, according to the experimental and calculational context of its use.[79] In addition, engaging with mathematically intricate theories requires a certain understanding of the philosophical significance of the mathematical apparatus used; in other words, significant problems in the philosophy of mathematics and applied science are bound to crop up sooner or later. Indeed, it is not clear how realists can avoid the debates over the so-called 'indispensability argument' in the philosophy of mathematics (Colyvan 2001), which is implicitly involved in the reconstruction of the ontological claims of a 'true' mathematical theory; cf. Resnik (1995). Scientific realists systematically neglect the fact that, if empirical success is taken to provide confirmation of a theory's ontological hypotheses (the realist's assumption), then, since our current empirical theories are heavily mathematised, this could be taken as confirmation of their mathematical existence assumptions. One can see Leng (2005), who goes as far as to argue "that it is scientific realists who should be most concerned about the issue of Platonism and anti-Platonism in mathematics... The question of mathematical ontology comes to the fore… once one considers our scientific theories," (ibid:

[79] This fact has given rise to the so-called 'model theoretic' approach to scientific theories; see the essays in Giere (1998) and Morgan & Morrison (1999). This approach should be distinguished from the formal, semantic view of scientific theories, according to which theories are construed as models of their linguistic formulations; see Chakravartty (2001) for a comprehensive review of the latter in relation to SR.

65). The same issue in anti-realism (van Fraassen's version, to be specific) has already received extensive treatment in the context of nominalism and fictionalism in the philosophy of mathematics; see Bueno (1997) & (1999), and Dicken (2006). In any case, van Fraasen himself was very well aware from early on that, his own philosophy of science at least, had to face issues in the philosophy of mathematics:

> I do not really believe in abstract entities, which includes mathematical ones. Yet I do not for a moment think that science should eschew the use of mathematics, nor that logicians should, nor philosophers of science. I have not worked out a nominalist philosophy of mathematics –my trying has not yet carried me thus far. Yet I am clear that it would have to be a fictionalist account, legitimizing the use of mathematics and all its intratheoretic distinctions in the course of that use, unaffected by disbelief in the entities mathematical statements purport to be about. (van Fraassen 1985: 303)

Second, and more troubling, is the fact that SR seems to 'put all its eggs in one basket': everything that is philosophically important is taken to hang on the metaphysical and epistemological status of the interpretation of theories, and *present* theories at that; with current theories as points of reference, the realist supposedly recognizes what has been retained and what not, employs them as standards of comparison in order to circumscribe his/her commitments, and restrict them according to the 'story' suggested by the realist reconstruction of historical facts. Realists, then, give a picture of their activity as one of re-description and re-interpretation both of scientific facts and scientific practice; after having visited, so to speak, the scientific laboratory, they re-state, on their terms, what the scientists tell them they found. It is no wonder, therefore, that SR functions primarily as a template for understanding things, not merely evaluating them; as such, it should be characterised as a guideline for re-describing and re-conceptualising scientific practice in specific (realist) terms, e.g, as

> the doctrine that the sort of evidence that ordinarily counts in favour of the acceptance of a scientific law or theory is, ordinarily, evidence for the (at least approximate) truth of the law or theory as an account of the causal relations obtaining between the entities quantified over in the law or theory in question. (Boyd 1973: 1)

The right to impose such a picture on the 'facts', is, ultimately, what the debate is all about. The realist commitments themselves, however, seem to have been taken for granted in this endeavour all along: Why should 'our *current* theories' —our *current* points of reference— be given a realist interpretation in the first place?

This, among other things, would imply that scientific inquiry always converges on a single interpretation and that, for the philosopher, no alternatives should be taken seriously when a scientific community has reached a consensus over one theory. However, not only is it the case that the convergence on a single interpretation of scientific inquiry is often debatable, but it is also the case that philosophy has a lot to contribute to the debate when examining alternatives. This is because unconceived alternatives may be of significance still. As Stanford argues:

> [I]f the historical evidence confirms that past practitioners have indeed routinely failed to conceive of well-confirmed alternative hypotheses … that were sufficiently serious as to be actually accepted by later scientific communities, then we have every reason to believe that there are similar alternatives to our own contemporary scientific theories that remain *presently* unconceived, even if we cannot specify or describe them further. This challenge to scientific realism enjoys several advantages over the traditional pessimistic [meta-]induction, but perhaps the most important is that the problem of unconceived alternatives concerns the *theorists* rather than the *theories* of past science: even if contemporary scientific theories sometimes enjoy empirical successes arguably unprecedented in their scope and character, this offers us no reason to suppose that today's scientists are any better at conceiving of the full range of theoretical possibilities confirmed by this evidence than were the greatest scientific minds of the past. (Stanford 2006: 123, emphasis in the original)

Furthermore, our most accurate and fruitful scientific theories still await plausible 'realist' readings. Take quantum mechanics, for example (Ruetsche 2002a, 2011; Wessels 1993), quantum field theory (Ruetsche 2002b), or even the general theory of relativity (Belot 1996); both require extensive reconstructive analysis in order to uncover exactly what (if anything!) a realist commits oneself to. This is not merely a 'philosophical' problem: scientists themselves do not know exactly what kind of a 'realist' claim the theories make on reality (if any!), and for a full theory of quantum gravity (which we presently lack), these interpretative issues are deemed to be important (Callender & Huggett 2001). Nonetheless, this does not entail that a realist construal of our current theories is indispensable or even preferable. On the contrary, it is far more probable that our current (incompatible with each other) theories, if realistically understood, are actually *false*. I shall have occasion to revisit the full implications of this situation in Chapter 5, when I examine Alexander Wendt's ideas for a so-called 'quantum social theory'.

Moreover, it is even less plausible that (a) one coherent picture of the natural world in all its complexity arises once 'the dust settles down' and that (b) the philosopher need only observe that picture. For instance, it is well-known that our two most successful and 'mature' physical theories (general relativity theory and quantum field theory) do not currently cohere with each other; they are actually mutually inconsistent both in what each one predicts for the other's domain, and in the conceptual resources each one employs. Still, this has not discouraged realists from taking mutually inconsistent theories at face value, even if only in each one's respective domain of application (Brown 1990).

Even if SR, as a global, 'grand-theory'-type of philosophical picture, seems to be no longer motivated (as noted, the picture has to be applied with care, piecemeal, and after detailed acquaintance with the theory's technical and conceptual apparatus), *the picture has to be there*. Such passive acceptance, though, is perplexing: Should it not be *first* the realist who should argue against the passive acceptance of our current theories and confront them with alternatives, in order to test their cogency? What better reason to take a theory 'at face value' than playing it against alternatives? The realist is after all committed to the view that there is a coherent ontology underlying current *theory*; not that an ontology corresponds to current *epistemology* (the epistemic processes followed by scientists and the justifications and/or descriptions they offer)*, which seems open to the vagaries of scientific practice*. The realist's focus of interest should be ontology. One would not expect the realist merely to trust in the results of a 'consensus', even if this consensus is a well-respected scientific one. *It may be wrong, however cogent the epistemic practice*. Certainly, as noted, the realist does not accept scientific claims uncritically; but this is merely lip-service *if no realist challenges them*, either.

In addition (and this is my third point), one cannot help but notice how organically connected the three dimensions of SR have shown themselves to be in practice: once the *semantic thesis* has been settled, and one accepts that scientific claims can be true and have existential purport, the *metaphysical thesis* is ushered in to cash out what the semantics entails; truth and existence are given a non-epistemic construal, which of course motivates our need to rely on the *epistemic thesis*, securing thus our commitment to science as supportive of belief in the truth of theories. But this in turn requires an *even stronger* reading of the *metaphysical thesis* than the one I began with: a commitment to a reality intelligible *in a particular way*, pictured as ontologically constrained by *referentially invariant entities* or *structures*, *natural kinds* or *mathematical forms* which somehow survive through theory-change and allow us to claim that 'science converges to the real'. Worrall, for example, unhesitatingly takes it that, "in our more enlightened, post-Cartesian dualism-times" (Worrall 2007: 153)[80], SR's metaphysics should be more accurately formulated as:

> There exists a structured reality of which the mind is a part; and, far from imposing their own order on things, our mental operations are simply governed by the fixed laws which describe the workings of Nature. (ibid: 153)

[80] Attributing the formulation to Elie Zahar (2001: 86).

This metaphysical position has certain epistemological implications for Worrall's 'structural scientific realism' (hereafter: SSR, since 'structural realism' was renamed 'structural scientific realism' in Worrall 2007):

> Not only is this structured reality partially accessible to human discovery, it is reasonable to believe that the successful theories in mature science – the unified theories that explain the phenomena without *ad hoc* assumptions – have indeed latched on, in some no doubt partial and approximate way, to that structured reality, that they are, if you like, *approximately true*. (ibid: 154)[81]

The metaphysics, however, becomes more and more detailed: from step one, the reality is 'structured', the mind has a specific 'whole-part' relation to it, and scientific laws 'describe the workings of Nature'. Although reasonable when put in context (realism), for an anti-realist these claims are all far too demanding, too strong, to take them seriously as *defining* the terms of the realism debate.[82] This positive feedback, however — fleshing out the metaphysics in order to accommodate the epistemology — might conceivably go on and on, the realist position becoming more and more complicated with every pass: indeed, the complexity of the current arguments and counter-

[81] It is worth repeating at this point that the correspondence or semantic view of truth, which is assumed by SR as the way in which a theory mirrors reality is negated by Worrall ("But there is no reason why the way in which a theory mirrors reality should be the usual term-by-term mapping described by traditional semantics. Indeed, as I have remarked several times already, if we are talking about an epistemically accessible notion then it cannot be!" (Worrall 2007: 154)). Instead, "SSR [Structural Scientific Realism] in fact takes it that the mathematical structure of a theory may globally reflect reality without each of its components necessarily referring to a separate item of that reality. And it takes it that the indication that the theory does reflect reality is exactly the sort of predictive success from unified theories that motivates the No Miracle Argument" (ibid: 154). This position, which I personally find sophisticated and more close to the *nature* of the more advanced physical theories (e.g., general relativity, QM, QCD, quantum field theories, etc) is first and foremost due to Worrall's profound reading of Henri Poincaré's, *Science and Hypothesis* ([1905]1952), where Poincaré developed "a classic account of the No Miracle Argument" (ibid: 132).
[82] Cf. the criticisms in Giere (1998).

arguments seem to demand it *if the doctrines of SR are to be kept alive*. Does that mean that SR (or anti-SR, for that matter) does not even amount to a stable position?

This last question brings me back to my previous remarks on the context of the realism debate. One wonders whether something might have gone askew with the *setting* of the realism debate itself. Could it be that both the realist and the anti-realist have implicitly adopted a certain view of science (of the theories, the laws, the mathematical apparatus, and the experimental practice) which strongly biases the discussion in favour of the realist/anti-realist philosophical divide, which naturally produces the controversy? It is entirely possible that the realism debate has been founded on a set of dichotomies which only produce questions, with neither the provisions nor the conceptual resources to supply answers by themselves. Perhaps the realists should widen their scope, and not merely 'go local' in their research habits, but also give up their faith in the strong, 'global' claims entirely. Indeed, as it has already been discussed, Arthur Fine has repeatedly argued for his NOA by construing the realism debate as fixated on certain 'pictures' which are not naturally found in scientific practice; thus, the realists and anti-realists beg the question even before the debate begins. Admittedly, questions such as the above are difficult ones to state precisely, let alone argue cogently about. However, they have already been much debated in the literature (mostly over the coherence of the reasoning behind the No Miracle Argument[83]). They concern deep issues of

[83] Fine (1986a), (1986b), Laudan (1981). But see also Giere (1998), chapter 4.

philosophical method, namely naturalism, methodological pluralism, and the place of metaphysics in philosophical (and scientific[84]) debate.

I would like to conclude my survey of SR in the philosophy of science by suggesting that, when all is said and done, the realism debate is largely self-sustained and self-generated; specifically, the features which produce the realism/anti-realism controversy lie in the idealized conception of ‘science’ which both realists and anti-realists have uncritically adopted: it seems to me that this conception imposes a gap and then tries to bridge it via (dis)ingenuous technical tools and evasion of the truly relevant conceptual issues. This conception of ‘ideal science’ consists in the pure, ‘mythological’ notion, the one found in textbooks, research reports, and popularizations. This notion adopts hastily the portrayal of scientists as going after *truth* and *extra-human realities*, instead of parsing the scientific practice in more natural and weaker terms, such as e.g. those of *intelligibility* and *credibility*. In the latter picture, scientists try to *make sense* of the physical and social phenomena, producing theories and ideas not in order to reveal the structure of a pristine, static and ideal world, but in order to make those phenomena intelligible, accessible, and open to tractable, credible and reliable, theoretical and mathematical treatments. Hence, the strong, ambitious realist conception neglects outright the concrete character of *doing* science, in favour of the ‘phenomena’ and the ‘pictures’ which scientists themselves provide in order to order and explain them. This realist stance is not as innocent as it sounds, since in practice it amounts to a strongly selective guideline for understanding science, its history and sociological aspects, as well as how one should talk about theory: as a result,

[84] In the context of methodological worries, see the excellent reconstructions of actual historical scientific debates in Chang (2001), Achinstein (2002) and Stein (1989).

the realists either dismiss the heterogeneity of science, its historicity and social dimensions as irrelevant, or use these very same features to motivate the realist quest for Truth and Nature, via a cleaner, more abstract and ultimately unreal conception of science[85]. This is unfortunate, since in this way, SR seems to underplay the radical character of truly novel developments which do not fit the realist model (e.g. mature theories which are non-visualizable and essentially mathematical — quantum mechanics — or extremely successful though made up of cookbook-style techniques — quantum field theory).

It seems to us, therefore, that this restricted and selective attention to scientific practice is unreasonable; by contrast, the kind of wider focus that was hinted at above, which does not allow for easily drawn, sharp, unambiguous distinctions, between the public, social, human factors (epistemology), and 'Nature' (ontology), provides for a more natural point to found an inter-disciplinary (and consistently naturalistic) philosophy of science. In that case, theoretical entities, both observable and unobservable ones, would work essentially as part of the scientist's mechanisms of *making sense* of Nature, not in the sense of instruments or as (approximate) truth-claims, either, but as *distinctive and heterogeneous techniques of achieving intelligibility of the phenomena, credibility (in the scientific community) and reliability (of any technology that results)*. These aims impose their own constraints on scientific practice, which may rhetorically (and innocuously) be glossed over as 'the quest for truth and the inner workings of nature', but which have very little to

[85] See, for example, Boyd (1992) and Kitcher (2001b); especially relevant to the latter are the criticisms of Roth (2003).

do with any specific ontological picture such as the one SR projects onto science.[86]

The distinction between realism and anti-realism in that case would start to blur: theories would be functioning neither as an instrument to calculate experimental outcomes, nor as adequate representations of phenomena, but as part of an interlocking and mutually supporting set of techniques which work towards both of these goals at the same time: in other words, instrumental and representational adequacy are always *goals for a theory to aim for*, *achievements*, not the starting point of understanding theory. Indeed, embodying both representation and calculation, a theory allows for the means to further the goals of research itself, since, on the one hand, by pressing home the instrumentalist aspect of theory as opposed to its representational function, theory may propel itself forward by taking advantage of any development in the calculational techniques; while, on the other hand, favouring realism and the function of representation over instrumentalism, opens up new possibilities and new theoretical ideas which the calculation will have to catch up to[87].

This way of dividing the *goals* of theory into realistic portrayal and calculational adequacy — instead of trying to characterize the *properties* of theory in only one of these ways — seems to exclude the anti-realists' fixations as unmotivated as well: the anti-realist has to make room for both goals, not to accommodate the one in terms of the other, as e.g. van Fraassen's constructive empiricist attempts to do, when he distinguishes between 'commitment' to a

[86] Stein (1989) has offered a far better construal of this line of thought than one could afford here: Stein, examining historical episodes from science, gives a cogent argument for how neither realism nor instrumentalism make a difference in scientific practice; on the contrary, insistence on either soon outgrows its usefulness in producing successful theories.
[87] Arguably, this is exactly what has happened to the frontiers of research in high-energy physics, such as string theory.

theory's predictive function and 'belief' in the explanatory pictures it provides. Blackburn puts the point eloquently:

> The problem is that there is simply no difference between, for example, on the one hand being animated by the kinetic theory of gases, confidently expecting events to fall out in the light of its predictions, using it as a point of reference in predicting and controlling the future, and on the other hand believing that gases are composed of moving molecules. There is no difference between being animated by a theory according to which there once existed living trilobites and believing that there once existed living trilobites….What can we do but disdain the fake modesty: 'I don't really believe in trilobites; it is just that I structure all my thoughts about the fossil record by accepting that they existed'? (Blackburn 2002: 127-8)

As such, the realist's concern of separating epistemology from ontology would look largely derivative and unmotivated: it crops up after all the hard work of securing intelligibility and credibility has been done, adding nothing to the theories themselves (neither bolstering the evidence nor offering rational grounds for taking them more seriously than the scientists themselves), it cannot help in scientific debates in deciding to which theory one should attribute truth, it cannot contribute in historiographical pursuits, and, finally, it draws attention from all the interesting questions about science, such as: Are scientific laws mere 'generalizations' or part of the scientist's mechanisms of making sense? How do models work? What is the role of mathematics? And so on.

To all these questions, the realist reacts by invoking 'truth' and 'capturing reality'; I would suggest that such reactions undervalue by far the complexity of the questions here: SR, if it is to stand for an intelligible and credible agenda for answering them, has to do a lot of work first, work which any scientist would probably find unproductive and unmotivated: it would have to go beyond merely admitting that our current interests and perspective influence any reconstruction we might offer; it would have to give a cogent description of what exactly those interests are in SR's case, and examine how it separates the social from the natural, if it doesn't simply take the separation for granted from the beginning. The realist stories, which try to capture "what, in retrospect, prove to be the pivotal points at which a successful direction of development was initiated, whether or not their importance was appreciated at the time, while suppressing the peripheral or abortive" (Leplin 1997a: 68), are no longer enough, unless what the realist aims for, ultimately, is to partake in "all the excitement of the realist claim that science engages in a cognitive activity that pushes back the frontiers of ignorance and error" (Psillos 2000a: 708).

The full development of these thoughts would distract the discussion too much. However, I would wish to register my dissatisfaction with the, in my view, unreasonably strong, global claims of realism on the one hand (Worrall's SSR being the version of SR which I am in more sympathy with for it makes more modest and more sophisticated claims), and my agreement with the suggestion that SR rests on the uncritical adoption of an unreal conception of science, on the other. This latter conception has not been constructed by philosophers, although a lot of philosophy has been put into it. One might say that this 'mythological' conception of science springs from scientists' natural reactions. Nevertheless, this does not absolve the philosophers from uncritically adopting it instead of critically examining it.

The 'mythological' conception of science originates in physics and natural science. Furthermore, it seems that it has also been guiding the self-understanding of social science. IR, especially, does not seem to be an exception; on the contrary, the Third Debate has been largely enacted with a naïve conception of science at the background. For that reason, it would be interesting to examine how SR has been received in the field of IR; specifically, in the works of IR theorists Alexander Wendt and Colin Wight. In my view, the strong commitment to the 'mythological' conception of science[88] and the simplistic treatment of SR in IR presenτ major shortcomings to IR meta-theoretical considerations. This is going to be the focus of the next two chapters. In chapter 3 I will deal with Bhaskar, the arch critical realist, on whose work a huge part of IR meta-theory is mainly built upon. In chapter 4 I will focus exclusively on the ways SR has been conceived, explored and deployed within IR Theory.

[88] Wendt (1999: 39): "[I am] a strong believer in science… I am a 'positivist'". Since Wendt was also a scientific realist at that point, 'positivism' points to his commitment, at the time, not to any specific positivist doctrines about unobservables, but to what I have referred to here as 'mythological' science in general.

3

SCIENTIFIC REALISM AS DEVELOPED BY ROY BHASKAR

3.1 Introduction

There is a long debate concerning the question whether debates in the field of philosophy of science have a bearing on the development of actual scientific practice and theorizing. The philosophy of science is a philosophical discipline which has the practice of science as its object. Its aim is not only to understand the evolution of science and scientific change, as is the case of the history of science, but also to provide an understanding of what science is and what can count as scientific work. Philosophy of science, therefore, seeks not only to understand the practice of science, but moreover to both justify and criticize it.

However, philosophy of science has often been regarded with contempt on the part of working scientists, who often claim that the philosophers' work is ultimately irrelevant to the actual business of scientific practice.[89] This claim can be summarized in the following terms: science can and will continue on its way to the extension and deepening of our knowledge and mastery of the world, without needing either the guidance or the control of the philosophy of science.

While this attitude might be considered dominant in the domain of the natural sciences, it is far from universally accepted in the field of the social

[89] For more on this view, which is held by many leading working scientists, look for instance at Steven Weinberg's *Dreams of a Final Theory* (Weinberg 1994).

sciences. The latter, in their search for an understanding of the real requirements of their practice both in themselves and in relation to the natural sciences, have often and customarily turned to philosophy in general and to the philosophy of science in particular. This specific interest in philosophy and meta-theoretical investigation in general has often been regarded both as a distinctive feature and as a flaw of the social sciences.

In the previous chapter, I have examined SR as a robust albeit problematic philosophy of science with its own metaphysics, semantics and epistemology. While the influence of SR in the field of the natural sciences remains uncertain, its influence on the social sciences in general, and on the discipline of IR in particular, can hardly be overestimated. Actually, one might claim that SR has been the touchstone of the meta-theoretical debates that have dominated the discipline during the last twenty years or so.[90]

How can this interest on the part of IR in the philosophy of science in general, and in SR in particular, be accounted for? This question will be discussed in detail in Chapter 5 of the thesis, where I will closely examine the influence and the bearing of SR on IR. A brief, though schematic, answer can be formulated in the following terms: the interest of IR theorists in SR is the result of a revolt against a particular image, or some would say an 'ideology', concerning what an IR theory should be like in order for it to be regarded as scientific, or even meaningful. This attitude has been both acclaimed, as a sign of scientific seriousness and theoretical rigour, and criticized, for being an impediment to the production of actual substantive work.

[90] This is true mainly due to the works of Alexander Wendt (1987, 1999), Heiki Patomäki (2002) Colin Wight (2006) and Milja Kurki (2008).

In the previous chapter, I have been able to testify to the richness and complexity of the debates on SR, especially in the field of the natural sciences. SR is far from being a uniform position; actually, it would be better to describe it as a set of meta-theoretical presuppositions or 'prejudices' in the positive Gadamerian sense of the term[91], that seek to justify themselves in the light of internal and external criticism. It is this feature of SR, as discussed in the context of the philosophy of science, which provides it with its distinctive dynamic and vibrant character.

Now, it should be stated from the very beginning that this kind of complexity and lack of uniformity is absent in the debate on SR as discussed in the context of IR. Actually, in the context of the meta-theoretical debates in the field of IR, what counts as SR is the particular brand of SR developed by the British philosopher Roy Bhaskar (Bhaskar's version of SR is also known as Critical Realism). While Bhaskar's Critical Realism (CR) is in conformity with

[91] As regards the notion of 'prejudice' in the positive, Gadamerian, sense of the term, see Hans-Georg Gadamer's *Truth and Method* (1975). It is there where one reads: "Rather, a person trying to understand a text is prepared for it to tell him something. That is why a hermeneutically trained mind must be, from the start, sensitive to the text's quality of the newness. But this kind of sensitivity involves neither 'neutrality' in the matter of the object nor the extinction of one's self, *but the conscious assimilation of one's fore-meanings and prejudices. The important thing is to be aware of one's own bias, so that the text may present itself in all its newness and thus be able to assert its own truth against one's fore-meanings*" (ibid: 238). Furthermore, cf. *Gadamer in Conversation – Reflections and Commentary* (Palmer 2001: 43-44), where Gadamer in a reply to a relevant question of Carsten Dutt, clarifies that: "[…] The idea that authority and tradition are something one can appeal to for validation is a pure misunderstanding. Whoever appeals to authority and tradition will have no authority. Period. The same thing goes for prejudgements. Anyone who simply appeals to prejudices is not someone you can talk with. Indeed, a person who is not ready to put his or her prejudices in question is also someone to whom there is no point in talking. One time, at the beginning of his career, Heidegger made use of the term *Vorurteilsüberlegenheit* [one's superiority over prejudgement] as a corrective measure. This concept includes the capacity for conceding the correctness of the argument of the other person, and in cases where one does not know enough and one has trust in the better knowledge of the other person, one recognizes his or her 'authority' in the matter. "*All our learning is based on this. Good judgement [a positive form of prejudgement] is a faculty one uses in taking action or in claiming to know something*, and of course it is not something one acquires through book reading" (ibid: 44). The kernel of Gadamer's argument lies, however, in Dutt's next comment: "[…] *Your true target is really a form of historical consciousness which has been the guiding consciousness of the modern humanities and social sciences, insofar as its insights into the historicity of its objects has been paired with a blindness to its own inextricable involvement in that history*" (ibid: 44-45).

several views and intuitions of other versions of SR, such as the ones I examined in the second chapter of this study, it departs from them in numerous ways.

The dominance of Bhaskar's philosophy in the meta-theoretical debates of IR renders necessary a careful and scrupulous examination of his main philosophical positions. For it is an undeniable fact that Bhaskar's version of SR has been introduced in IR without having been previously submitted to critical scrutiny from a philosophical point of view, thus leading to an often unreflective endorsement of his views. Thus, Bhaskar's positions tend to be unequivocally adopted as philosophical truths which an IR theorist should either accept or reject, in which case the rejection of Bhaskar's views would be equated with a rejection either of philosophy altogether or, more simply, of philosophy's relevance in IR.

3.2 Two Dimensions of Scientific Practice

Bhaskar's version of SR is developed in its most systematic form in his seminal work *A Realist Theory of Science* (Bhaskar [1975] 2008).[92] Bhaskar's main goal is to establish science as a social product directed towards an objective world.

According to Bhaskar, science is a product of human activity. It is always situated in a specific time and place in the context of the historical evolution of human society. Science is a work of man as a social and historical being; some would even say that science is man's most distinctive or noblest work. Though science has no existence independently of man, since it is made by man and the

[92] Chris Brown claims that *A Realist Theory of Science* is "by common agreement, the ur-text of critical realism" (Brown 2007: 410).

locus of its existence is the human mind, it is directed upon objects that exist wholly independently of man. The objects of science, the various things, properties and laws *of* which we seek knowledge, exist in themselves in complete independence of man's efforts to acquire knowledge of them. They can interact with men, they can change men and can be possibly changed by men; but their being is distinct from the being of man.

Therefore, one should distinguish two aspects or dimensions of science: first, the social dimension of science, which establishes science as a historical product of the human society; second, the objective dimension of science, which consists of the independent objects whose knowledge is the ultimate aim of all scientific endeavour: the objective world or nature that science struggles to render manifest. According to Bhaskar, two separate kinds of objects correspond to these two dimensions of science: the 'transitive' and the 'intransitive' objects of science. The 'transitive' objects of knowledge

> are the raw materials of science – the artificial objects fashioned into items of knowledge by the science of the day. They include the antecedently established facts and theories, paradigms and models, methods and techniques of inquiry available to a particular scientific school or worker. (Bhaskar [1975] 2008: 21, cf. 194-195)

Two things are to be noted here. First, that science does not grow out of a direct, non-mediated interaction with nature, but rather out of antecedent scientific practice. There is no leap from non-science to science, no "spontaneous production of knowledge" (ibid: 24), but rather a continuum of transitive, temporary, fallible and socially constructed practices. The materials of the scientist are no immediately given ‘data’, but previous scientific knowledge and practice. As Bhaskar remarks, “[K]nowledge depends upon knowledge-like antecedents” (Bhaskar [1975] 2008: 22). Second, the transitive dimension of science consists of transitive *objects* of science. But in what sense does it make sense to speak of transitive objects of science? Bhaskar’s position is somewhat ambiguous on that point.

I will try to make it clear by taking an example of a transitive object of knowledge, e.g. a particular scientific theory, for instance, Newtonian mechanics. When one learns the theory, one learns *two* things: the theory itself and the reality that the theory purports to describe. The theory itself is a social and historical construct: it was produced by men in a definite historical moment, in the context of a particular society, etc. The theory is formulated in a particular natural or artificial language, and makes sense only in that language. This theory is something to be learnt in itself and in that sense it is an *object* of knowledge. But when one learns the theory, then, in the case that this theory is *true*, one also learns something beyond the theory itself: one acquires knowledge of the reality that the theory purports to describe. The same holds in the case of a photograph, a picture, or a map. Suppose one gets hold of a map. If the map is exact or *correct*, then the object of knowledge is, besides the map itself, the spatial region that the map *represents*. There is, of course, an obvious difference between a map and an actual spatial region: the map is of paper, 2-dimensional, man-made; the region is physical, 3-dimensional, and not man-made. But, in the case that the map is correct, there is also an identity: when one looks at the map, in the case that one understands the map, one learns not only the map, but the region the map represents. In that case it makes sense to speak of two different *objects of knowledge*: the man-made, socially constructed, transitive ones and the independent, objective, intransitive ones.

The existence of the 'transitive' and the 'intransitive' dimensions of science is the real nexus of Bhaskar's argument since, according to Bhaskar:

> Any adequate philosophy of science must be capable of sustaining and reconciling both aspects of science; that is, of showing how science, which is a transitive process dependent upon antecedent knowledge and the efficient activity of men, has intransitive objects, which depend upon neither. In other words, it must be capable of sustaining both (1) the social character of science and (2) the independence from science of the objects of scientific thought. (Bhaskar [1975] 2008: 24)

3.3 Two Rival Philosophies of Science

Bhaskar seeks, first of all, to establish the objectivity of the intransitive dimension of science. In this effort, he sets his own philosophy of science against two rival accounts concerning the nature of scientific objects[93].

The first rival philosophy of science that Bhaskar takes into consideration is *classical empiricism* whose main representative is David Hume. Classical empiricism is usually interpreted as an epistemology, that is, as an account of what can be known rather than as a philosophy of science. But, according to Bhaskar, classical empiricism's epistemology leads directly to a philosophy of science and indirectly to a defective ontology. Classical empiricism's main thesis is that all knowledge comes from experience: experience exhausts all knowledge of the world possible for us. What is apprehended in experience is

[93] A short presentation of Bhaskar's ontological scheme is to be found in the entry "Ontology" written by Bhaskar himself for *The Blackwell Dictionary of Modern Social Thought* (Bhaskar 2003: 442-443).

an individual or atomistic event, e.g. that *x* is the case. The world available to us is nothing other than a series of events presented to us in the context of discrete experiences. Then, what is the purpose of science, according to classical empiricism? It is to establish connections between events, that is, to show that an event of type A is constantly followed by an event of type B. Therefore, the purpose of science is to establish *constant conjunctions* of events according to Hume's term, or *regular sequences of events* as Bhaskar preferably calls them. The world is reduced to events and to the possible connections of those events.

According to Bhaskar's imagery, the world for classical empiricism may be viewed as a 'surface', that is, without any depth, with no *underlying structure*. Events are like points on this surface linked to one another with discrete lines. According to classical empiricism, the ideal of science would be to establish links between all the points on this surface. It is important to note that this surface exists only in the human mind because the events that make up this surface exist solely as phenomena of experience. For classical empiricism, trying to discover what lies behind those phenomena would only be an attempt to return to some kind of dogmatic metaphysics.

The second rival philosophy of science that Bhaskar takes into consideration is transcendental idealism, which is represented by Immanuel Kant and the neo-Kantian philosophers. According to transcendental idealism, nature is not reduced to atomistic events. Behind the events there is an order, a structure or, to use Kant's term, a lawfulness (*Gesetzmäßigkeit*). However, this structure is not a property of the objects themselves given to experience. It is rather to be understood as a product of the human mind, which in its cognitive activity *imposes* an order upon phenomena. For transcendental idealism, knowledge is not acquired through the passive state of mere observation. Knowledge is an *activity* through which the human mind organizes the data of experience into models of the world. The world ceases here to be seen as a surface. However, the depth - the underlying structure - that the world is provided with is not to be viewed as a property of the *world itself*, but rather as a product of human cognitive activity.

Now, there are two ways to interpret the nature of this cognitive activity: either as an activity that seeks knowledge as an end in itself (in the spirit of Plato or Aristotle)[94] or as an activity influenced by the various interests and dependencies that shape human striving in the world in general (in the spirit of Hegel or Habermas).[95] But, if cognitive activity is shaped by particular interests and viewpoints and if the structure of the world is something imposed upon the world by cognitive activity, then it follows that for transcendental idealism, properly interpreted, "structure becomes a function of human needs; it is denied a place in the world of things" (Bhaskar [1975] 2008: 28).

The common element that connects both rival philosophies is their commitment to a particular ontology that Bhaskar describes as *empirical realism*. Empirical realism is the view according to which the principal category upon which any account of reality should be founded is the category of *experience* or *experience ability*. For both classical empiricism and transcendental idealism, no knowledge can exist of what exceeds the limits of

[94] On Aristotle's analysis of knowledge as a natural human end in itself, see Aristotle, *Metaphysics*, Book I, 1. On his theory of theoretical knowledge as man's highest good, see Aristotle, *Nicomachean Ethics,* Book X, 7-9. See also Richard Kraut (1991), especially Ch. 1 & Ch. 3 and, also, Thomas Nagel (1972). On Plato's views, see T. H. Irwin (1995), Ch. 20, §§ 233-234. For a comparison between Aristotle's and Plato's views on the subject, see A. W. H. Adkins (1978).

[95] On Hegel, see Neuhouser (2003), especially Ch. 4-6. On Habermas' views on the subject, see Habermas (1973), (1984), (1987); Rehg (2009), Ch. 4 & Ch. 5.

possible experience. But, apart from the *object of which* there is experience, the notion of experience presupposes necessarily the *subject for which* there is experience.[96]

This necessary implication of subjectivity in the notion of experience combined with the belief that all statements about reality should be limited to the domain of what can be given in the context of a possible experience, lead inescapably to the common predicament of modern philosophy: reality should always be described in relation to the knowing subject or, in other words, *no reality can be thought of that is independent of man*. This predicament is illustrated in the positions of three of the most prominent modern philosophers vis-à-vis the objectivity of the external world. For Berkeley, there is no external world; reality exists only as *an idea of the knowing subject.*[97] For Hume, the reality of the external world is something that one cannot help but believe in, however, for this belief one can have no rational grounds; thus, one is inescapably led into *scepticism.*[98] For Kant, the reality of the external world is something which cannot be known by way of philosophical argument. However, this world is, first, a world whose structure is imposed by human cognitive activity: *the world's structure is not an essential property of the world itself.* Second, this world is only a small portion of the world at large; the external world of whose reality we can have philosophically grounded knowledge is the world in so far as it is capable of being given in the context of

[96] It is worth mentioning here that the following argumentation, which is based on the concepts of subject and subjectivity, constitutes an original reconstruction of the Bhaskarian empirical realism and is not to be found as such in the Bhaskarian *corpus*.
[97] On Berkeley's immaterialism, see Winkler (1989); Pitcher (1977); Pappas (2000).
[98] On Bhaskar's interpretation of Hume, see Bhaskar [1975] 2008: 24-7, 37, 40-2). On Humean scepticism, see Fogelin (1994: 209-237).

a possible experience; of the world which exceeds the limits of possible experience we can have no knowledge whatsoever.

Bhaskar remarks that the crux of this ontology that defines reality in relation to the knowing subject is the concept of the *'empirical world*', of the world that is the object of a possible experience. A necessary consequence of empirical realism, which as I have discussed, characterizes both classical empiricism and transcendental idealism, is a denial of the intransitive dimension of science: the world of which science seeks knowledge is not independent of man; on the contrary, it can only be described in relation to man and his cognitive activity.

3.4 Transcendental Argumentation: From Philosophy of Science to Ontology

Bhaskar sets himself to provide an alternative philosophy of science that fully acknowledges the intransitivity of the world that science seeks to discover. He calls this philosophy of science 'transcendental realism'. For transcendental realism, the objects of knowledge:

> (…) are neither phenomena (empiricism) nor human constructs imposed upon then phenomena (idealism), but real structures which endure and operate independently of our knowledge, our experience and the conditions which allow us access to them. (Bhaskar [1975] 2008: 25)

Bhaskar's aim is to demonstrate a fact about the world: that it exists wholly independently of human cognitive activity. In this sense, Bhaskar seeks to establish an *ontological* claim. However, his point of departure is a questioning that concerns the objects of scientific activity; in other words, he begins with an investigation in the field of the philosophy of science. This blending of the philosophy of science with ontology is manifest in the methodological strategy that he follows in order to establish his thesis on the independent existence of the objects of scientific research. He presents a *transcendental argument*, that is to say, an argument whose aim is to demonstrate a truth about the world starting from a questioning about what the world should be like for scientific activity to be intelligible.

There is no necessity that there is scientific knowledge. We can easily think of a world without man or any form of intelligent life. A world could have existed that would have never become the object of scientific investigation. But *there is* scientific knowledge or at least scientific research. Yet, this fact is not necessary; its negation is possible. But given that it is an actual fact, it has to be possible.[99] For only what is possible can be a fact, for there are no impossible facts. Now, if there is scientific knowledge, or at least scientific research, one may ask: how is scientific research possible? In other words, what must be the case for scientific research to be possible? Or, to use Kantian terminology, *what are the conditions of possibility of scientific research*?

One may think of many types of answer to this question. For scientific research to be possible, *man* himself should be in a certain way. Therefore, there are certain *anthropological* or *psychological* conditions of possibility of scientific research. Furthermore, *society* should be in a certain way. Thus, there are *sociological* or *historical* conditions of possibility for scientific research. A Habermasian or a neo-pragmatist like Richard Rorty would add that there

[99] "It is not necessary that science occurs. But given that it does, it is necessary that the world is in a certain way. It is contingent that the world is such that science is possible. And, given that it is possible, it is contingent upon the satisfaction of certain social conditions that science in fact occurs. But given that science does or could occur, the world *must* be a certain way" (Bhaskar [1975] 2008: 29).

should exist a specific *normative framework* for science to be possible. So, certain *ethical* conditions of possibility of scientific research should also be recognized. But, what is the most important for scientific research to be possible is that *the world itself* must *be* in a certain way. Therefore, there can and must be recognized certain *ontological* conditions of possibility of scientific research.

To sum up the argument so far: Bhaskar argues that there is no necessity for the occurrence of science. Nevertheless, since science does occur, science is possible. But for science to be possible, certain conditions have to be met. There are many different kinds of conditions for the possibility of science; moreover, there must be certain ontological conditions for this possibility. Therefore, one might seek to discover some truths about what the world is like through a research on the ontological conditions of possibility of scientific research. This is what a transcendental argument is. It is an argument that purports to establish a truth about the world through an investigation on the conditions of possibility of cognitive activity at large or scientific activity in particular.

Finally, it is worth mentioning that the notion of a transcendental argument is completely missed by Andrew Collier in his *Critical Realism: An Introduction to Roy Bhaskar's Philosophy* (Collier 1994). According to him, a transcendental argument is simply an argument that searches the conditions of possibility of an actual phenomenon. Therefore, a transcendental argument would explain what must be the case for an actual phenomenon to take place. He says that the form of a transcendental argument is the following: "from something that is actual, to a more fundamental 'something' that grounds its possibility" (ibid: 20). Now, this is *not* what a transcendental argument is. This is just what a simple explanatory argument is. Suppose it rains, and someone asks "How is this possible?". The argument that would explain the conditions of possibility of raining would not be a transcendental argument. It would simply be an explanatory argument, i.e. a simple *explanation*. A transcendental argument is something extremely more specific than that. It is an argument that demonstrates a truth about objective reality through an investigation on the conditions of possibility of cognitive activity. Thus, Kant in the 'Transcendental Deduction' of his *Critique of Pure Reason* purports to establish a truth concerning the structure of nature through an investigation on the conditions of possibility of empirical knowledge. Therefore, through his 'Transcendental Deduction' he establishes that the subjective conditions of possibility of experience are at the same time necessary laws of nature. He summarizes his point in his famous 'supreme principle of all synthetic judgments': "the conditions of the possibility of experience in general are at the same time conditions of the possibility of the objects of experience, and on this account have objective validity" (Kant 1999: 283).

3.5 The Intransitivity of Objects of Sense-Perception

After having established his methodological strategy of transcendental argumentation, Bhaskar embarks upon establishing his account on the intransitivity of the objects of scientific enquiry. He presents two different transcendental arguments. The first one focuses on the conditions of possibility of sense-perception, while the second one, which is by far the most important, focuses on the conditions of possibility of experimental activity.

The transcendental argument that focuses on the case of sense-perception runs as follows: what has to be established is that the object of sense-perception exists wholly independently of the subjective act of perceiving or the sense-perception itself as a mental entity. As Bhaskar claims, “the intelligibility of sense-perception presupposes the intransitivity of the object perceived” (Bhaskar [1975] 2008: 31). In order to establish this thesis, his argument is based neither on a particular philosophy of mind nor on any kind of metaphysical presuppositions. Instead, Bhaskar examines the conditions of possibility of certain forms of our cognitive activity.

His argument, which he exposes in an extremely condensed (and rather ambiguous) form, could be reformulated as a *reductio ad absurdum*:

- Suppose that the object perceived does not exist independently of sense-perception itself;
- Then the object of sense-perception should be construed as a product of the act of perceiving itself existing solely as a mental representation or idea.
- Now, it is an actual fact that men can perceive an object in different ways. For instance, one can look at an object from a different angle and see it differently, or one may acquire a certain information on the object which leads one to perceive the object in a different way;
- But if the object perceived is a mental creation of the perceiving subject, then this change in the way of perceiving the object cannot be interpreted as a *different* perception of the *same* object.
- Instead, one would have to accept that what is really happening in the case of a change in the way one perceives a thing is that the perceiving subject

creates *two (or more)* distinct mental representations through its perceptual activity. One does not see the same object in different ways. On the contrary, it should be interpreted as creating two different mental objects.

- This means that perceptual change, the idea that the same object is seen in different ways, should rather be viewed as a mere illusion.

At this point comes Bhaskar's transcendental claim that in order to warrant the possibility of perceptual change, one should acknowledge the independent existence of the object of perception.[100] The aforementioned represents a move from an examination of the conditions of intelligibility or possibility of a practice of *ours* - in this case: change in the way we perceive a thing - to a claim concerning the nature of the real world. In order to be able to make sense of something that *we* do in the context of our cognitive activity, we are led to a claim about external reality.

It is important to note the *hypothetical* character of Bhaskar's argument: the world must be interpreted as independent from our cognitive activity, *if* perceptual change is to be understood as a case of different perceptual representations of the same object, rather than a case of a creation of two different mental objects. Yet, Bhaskar offers no argument to establish that perceptual change should be understood in the first way rather than in the second one. It seems as if one were left with a choice between two *prima facie* equally plausible interpretations of our cognitive activity. If one accepts the

[100] This codification of Bhaskar's transcendental claim in which I make use of the *reductio ad absurdum* method is an original one for it is not to be found in this form in any of the philosopher's or his own critics' and commentators' texts. This way of presenting it will hopefully help in 'demystifying' its inner structure and logic by making it first of all comprehensive. Some of the critics of his book *ARTS* believe that its author fails to ground his transcendental claim but arguably they are not right (cf. Caroline Whitbeck's review of this book in Whitbeck 1977: 114-118).

first interpretation, one is forced to admit a particular account of the nature of reality; the same holds in the event that one accepts the second interpretation. Different interpretations of our cognitive capacity have different *ontological commitments*. Then what is the reason to prefer one interpretation to the other? Bhaskar's account seems to be that the first interpretation is faithful to the way working scientists understand their own activity, that it is "consistent with our intuitions" (Bhaskar [1975] 2008: 33), while the second one would force scientists to abandon crucial aspects of the image they have of their own work.

Based on his argument concerning the conditions of intelligibility of sense-perception, Bhaskar draws a categorical distinction between two different kinds of entity: experiences, which belong to the domain of the empirical, and objects perceived, which belong to the domain of the actual. It was shown that for classical empiricism the objects of scientific research are reduced to events and sequences of events. Bhaskar's transcendental argument purports to demonstrate that *even if* one admits that the objects of scientific research are events and sequences of events, those events *should be categorically distinguished* from the experiences through which one has cognitive access to them.

Before moving to the examination of Bhaskar's argument that focuses on the conditions of intelligibility of experimental activity, it is helpful to take note of how Bhaskar's position is situated vis-à-vis the other philosophers that he takes into consideration. *Contra* Berkeley, he establishes that the objects of sense-perception are categorically distinct from the acts of perceiving. *Contra* Hume, he believes that the independent existence of the objects of sense-perception can be demonstrated by way of philosophical argument, rather than be accepted as a matter of mere faith. Further, Bhaskar is in agreement with Kant's critique of Hume's vision of the order of reality. He agrees with Kant that the world should not be viewed as a surface consisting of events and constant conjunctions of events. The world and the objects of the world have an order, an underlying structure. But *contra* Kant, he wishes to establish that this structure is not a simple product of human cognitive activity, but a property of the world itself. This claim is the *demonstrandum* of his famous transcendental argument that analyzes the conditions of intelligibility of experimental activity: the structured nature of the world and the intransitive character of this structure.

3.6 The Intransitivity of the World's Structure

Hume believed that a causal law is nothing other but a constant conjunction of events. A law of nature is simply the *general fact* that an event of type A is constantly followed by an event of type B. For Hume, this regular sequence of events is simply what a natural law is. But, how can one establish the existence of a particular natural law? The obvious answer would be that one could establish it through scientific research. And what is the most distinctive feature of scientific research? Hume's answer is again obvious: experimental activity.

It is at this point where Bhaskar's next transcendental argument begins. Bhaskar makes the rather trivial remark that an experiment does not exist by itself. An experiment is not a natural happening; instead, it is something produced by men. The aim of an experiment is, initially, the co-production and then, the observation of a specific regular sequence of events. If this particular regular sequence of events occurred by itself, independently of any human interference, there would be no need to make an experiment. What renders experimental activity necessary is precisely the fact that the sequence of events under question does not normally take place by itself. Therefore, one may say: without human interference, the sequence of events whose occurrence a scientific inquiry seeks to testify does not take place. Here comes the next step in the argument:

(1) Following Hume, if one equates a causal law with a particular regular sequence of events;

(2) if no causal law can be scientifically identified without a proper antecedent experimental procedure;

(3) if the goal of an experimental procedure is the occurence of a particular regular sequence of events;

(4) if the occurrence of the particular sequence of events cannot take place without an experimental procedure;

(5) if in every experiment man acts as a causal agent, that is, if every experimental procedure involves human activity;

(6) if what exists as a result of the activity of some object, some person, some force or some law is a direct or indirect product or effect of that object, person, force or law;

Then, it follows that:

(7) all causal laws identified by science are the direct or indirect products or effects of man.[101]

This conclusion follows naturally from the premises: the regular sequence of events produced by an experiment exists as a result of human activity; therefore

[101] This 'codification' of Bhaskar's argument is original since it can be found neither in any of his own works nor in his critics' publications.

it is a direct or indirect effect of product of man. Furthermore, the regular sequence of events produced by an experiment does not take place naturally. If it did, there would be no need for an experiment. Therefore, it can only take place after the occurrence of an experiment which means that it can only exist as a result of human activity. Therefore, it is necessarily a direct or indirect effect or product of man. Now, if a natural law were a sequence of events, then the sequence of events produced by the occurrence of an experiment would be a natural law. Therefore, the natural law would be a result of the conduction of the experiment and, thus, should be considered as a direct or indirect effect of man. Finally, if no natural law may be scientifically recognized without the antecedent taking place of an experiment, then it follows that every natural law that is recognized by science is the direct or indirect effect of man.

What one has here is again a case of a *reductio ad absurdum*. In order to avoid the absurd conclusion that the laws that govern nature are made by man, Bhaskar claims that Hume's account of natural causation should be completely abandoned in favour of the following two theses:

(a) A *causal law* is not the same thing as a *regular sequence of events*. "There must be an ontological distinction between them" (Bhaskar [1975] 2008: 33).

(b) A causal law continues to operate independently from any experimental activity, that is, "outside the contexts under which the sequence of events is generated… Causal laws endure and continue to operate in their normal way under conditions, which may be characterized as '*open*', where no constant conjunction or regular sequence of events is forthcoming", given

> the fact that "outside astronomy, *closed systems*, viz. systems in which constant conjunctions occur, must be experimentally established" (ibid: 33).

Berkeley's primordial mistake was to identify events and experiences. Now, one can see that Hume's primordial mistake was to identify regular sequences of events and causal laws. If one accepts Hume's account of natural causation, one is forced to consider that causal laws are products of man, which is an absurdity. To avoid this absurdity, one should admit that the causal laws of nature are categorically distinct from the regular sequences of events that occur by way of our experimental activity. A regular sequence of events is to be understood as *an instance or a manifestation* of a certain causal law and not as the causal law itself. Furthermore, in order to make sense of experimental activity, one should admit that a causal law does not cease to operate outside the context of an experiment. An experimental framework should rather be construed as a particular instance of the operation of a causal law, in the context of which a particular law of nature becomes "actually manifest and empirically accessible to men" (ibid: 46).

What one has here is another case of Bhaskar's transcendental argumentation. Bhaskar moves from an investigation of the conditions of intelligibility of an activity of ours, viz. experimental activity, to a claim concerning the world itself. For experimental activity to be properly intelligible, one should assume, *contra* Hume, that causal laws are not constant conjunctions, that is, regular sequences of events. A causal law is something over and above a regular sequence. But, what exactly is a causal law?

Analyzing the nature of natural causation is the next step in Bhaskar's argument.

Before moving to the analysis of Bhaskar's account of causation, it is important to sum up what Bhaskar has accomplished so far. Bhaskar has presented two transcendental arguments, viz. two arguments that establish propositions about what the world is like from an examination of the conditions of possibility or intelligibility of two cognitive activities of ours: sense-perception and experimentation. These two arguments allow him to establish several ontological claims, namely that:

(1) events are categorically distinct from experiences;

(2) the objects of experience exist independently of experience;

(3) a causal law continues to operate outside the context of an experiment;

(4) a causal law "presupposes a 'real something' over and above and independent" (ibid: 49) of a regular sequence of events.

3.7 From Natural Causation to Generative Mechanisms

Starting from an investigation on the philosophy of science and more specifically on an investigation concerning the objects of scientific knowledge, I have presented a preliminary account of Bhaskar's ontology, on which I will now elaborate further.

Bhaskar's use of the concept of 'ontology' is of seminal importance for construction of his theoretical framework. According to Bhaskar: "The answer to the transcendental question 'what must the world be like for science to be possible?' deserves the name of ontology" (ibid: 23). This means that for Bhaskar ontology is the discipline that investigates a particular set of conditions of the possibility of scientific knowledge: the conditions that have to do with what the world must be like for science to be possible. Given Bhaskar's use of the term ontology, it becomes obvious that ontological questions can only be approached by use of transcendental arguments. This agrees with Kant's conception of ontology. But to understand this point, one has to take a closer look at Kant's critical project.

Traditional metaphysics was divided into two parts: 'general metaphysics' and 'special metaphysics'. General metaphysics investigated what it is to be a being, viz. what it is for something to exist. After the 17th century, general metaphysics took the name 'ontology', from the Greek word '*ov*' which means 'being'. Special metaphysics was divided into three parts: psychology, which investigated what it is to be a rational subject in general, cosmology which investigated what is to be an object in general, and theology which investigated the ultimate cause of being in general.[102]

Now, Kant's aim was the deconstruction of metaphysics as a theoretical discipline in so far as metaphysics is an attempt to acquire knowledge through reason of what cannot be an object of a possible experience. His critical project was divided into two parts: a deconstructive part, named 'Transcendental Dialectic', where the three branches of special metaphysics are shown to lead inescapably into antinomies and incoherence and are first abandoned as theoretical disciplines and then transformed into objects of moral investigation; and a constructive part, named 'Transcendental Analytic', which does not eliminate ontology or general metaphysics, but rather transforms them into an investigation on the mind's contribution to the constitution of the empirical

[102] See Heidegger (1997: §§1-2).

world. This transformation of traditional ontology into an Analytic of the Understanding is accomplished precisely through the use of transcendental arguments. In this way Kant manages to demonstrate that nature's most general laws are but the most general categories of mind. The proof of this is the object of his Transcendental Deduction and his Analytic of Principles. Three things are to be noted here:

(1) Bhaskar recognizes transcendental argumentation as the sole methodology for arriving at a philosophical ontology. Actually, he even describes ontology as an answer to a 'transcendental question', as he calls it. From this point of view, he can be considered a Kantian concerning his conception of what is ontology. At the same time, one should notice that this Kantian conception of what ontology is appears in conflict with his *stricto sensu* ontology in itself, which is thoroughly, though not explicitly Aristotelian, as I will explain later.

(2) While the aim of Kant's philosophy is to wholly discredit any attempt to transgress the limits of possible experience, Bhaskar positively affirms that philosophical ontology must go beyond the limits of what is given in experience, reflecting upon the 'real basis' of experienceable events.

(3) While Kant transforms ontology into an Analytic of Understanding in which the mind's *a priori* categories are shown to be nature's constitutive most universal laws, Bhaskar's transcendental argumentation purports to prove the existence of an 'ontological realm' wholly independent of the human cognitive activity.

In order to provide more depth and thickness to his ontology, Bhaskar elaborates on his account on natural causation.

Proposition (4) in section 6 above indicates that a causal law presupposes a 'real something' over and above a regular sequence of events. Through the logical analysis of this proposition, one could arrive at a deeper understanding of the Bhaskarian nature of causation. Moreover, one could comprehend the latter in the following manner:

(1) There is something that is beyond events and regular sequences of events. *Contra* Hume, the world is not a surface. Beyond events, there is an underlying structure.

(2) This 'something' is real. *Contra* Kant, it is neither a product of the mind's cognitive activity, nor something imposed by man upon reality, nor a figment of imagination.[103]

(3) This 'something' that subsists beyond the events is *operative*; it is something *active*.

[103] It seems as if, according to Bhaskar, causal laws for Kant are imposed by the mind upon phenomena. This is a complete misunderstanding of Kant's theory of causation. According to Kant's 'Second Analogy of Experience' of the *Critique of Pure Reason*, "all alterations occur in accordance with the law of the connection of cause and effect" (Kant 1999:304). This proposition, which is a general principle of nature in its formal sense, that is as of nature as the lawful totality of objects of possible experience, is demonstrated through a transcendental argument whose aim is to establish how an objective law of nature derives from a set of subjective *a priori* conditions of experience. It is in this sense that one might say that the *general principle of causality* described in the Second Analogy of Experience might be considered as a general law imposed upon nature by the cognitive subject. But what may be true of the general principle of causality is not true of the more particular laws of nature discovered by the natural sciences. This claim testifies to a confusion between nature in its formal sense (*natura formaliter spectata*) and nature in its material sense (*natura materialiter spectata*) and is accordingly a conflation of the idea of a pure science of nature, which investigates the *a priori* conditions of a physical world, and the empirical science of nature, which investigates the particular laws of nature (cf. Kant 1997: 47-65). Kant agrees that the general principle of causality is a necessary law of nature (in its formal sense) that can be known wholly *a priori.* Nevertheless, he flatly rejects the idea that the particular laws of nature, such as the 2nd law of thermodynamics or the orbit of Uranus could be equally known in an *a priori* way; on the contrary, their knowledge necessarily presupposes empirical research. Accordingly, while one might affirm that the general principles of nature in its formal sense are "imposed" upon it by the cognitive subject, this idea should be flatly rejected in the case of the empirical laws of nature. In the same way that man does not impose upon nature the existence of a mountain but rather discovers this existence empirically, man does not impose upon nature the 2nd law of thermodynamics or the orbit of Uranus, but rather discovers them through empirical research and theorizing. Thus, Bhaskar is wrong in assuming that his demonstration of the intransitivity of natural causation in the material sense goes against Kant's ideas.

(4) The products of the activity of this 'something' are the events; this something '*generates*' the events.

(5) This 'real something' that is over and above events and independent of them, and which, by its activity, generates the events, must be thought of as a *generative mechanism*.

Thus, one arrives to one of Bhaskar's most famous concepts: the concept of a 'generative mechanism'. Bhaskar's transcendental analysis has established the existence of something real beyond events, whose activity and operation generates the events. Bhaskar notes:

> The world consists of mechanisms not events. Such mechanisms combine to generate the flux of phenomena that constitute the actual states and happenings of the world. They may be said to be real, though it is rarely that they are actually manifest and rarer still that they are empirically identified by men. They are the intransitive objects of scientific theory. They are quite independent of men – as thinkers, causal agents and perceivers. They are not unknowable, although knowledge of them depends upon a rare blending of intellectual, practico-technical and perceptual skills. They are not artificial constructs. But neither are they Platonic forms. For they can become manifest to men in experience. Thus we are not imprisoned in caves, either of our own or of nature's making. We are not doomed to ignorance. But neither are we spontaneously free. This is the arduous task of science: the production of the knowledge of those enduring and continually active mechanisms of nature that produce the phenomena of the world. (Bhaskar [1975] 2008: 47)

It is the continuous and enduring operation of these normally invisible mechanisms that produces the actual events of which we have experience. The generative mechanisms of nature are in a state of continuous activity. This activity, however, is not always realized. The reason for this is that the activity of each mechanism is often impeded by the activity of other mechanisms. The world can be seen as an arena in which generative mechanisms in constant operation struggle to produce their natural effects. Thus, as Bhaskar notes, a mechanism may be active but may not be realized or actualized. And it may be actualized, but its actualization may pass unnoticed by men. Therefore, men have empirical access to only a small portion of the total activity of the world's mechanisms. Nevertheless, what sciences seek to accomplish is to acquire knowledge of these enduring mechanisms of nature. For this to happen, there needs to be in place the transitive dimension of science in its social and historical aspects.

Bhaskar insists on the fact that all objects of scientific inquiry have a transitive dimension. As I have discussed, constant conjunctions, which are the indications of the operation of a generative mechanism, can be normally observed only in the context of experimental activity. And it is quite evident that the setting up of an experimental framework presupposes a certain stage of social and historical evolution.

But, even at a more basic level, the level of experience, there are sociological and historical conditions for scientific inquiry. As Bhaskar notes, "experiences and the facts they ground are social products" (ibid: 57). All experience presupposes a theoretical-conceptual framework that mediates the empirical apprehension of the world. There is no experience without concepts:

without concepts we can see nothing; according to Kant's famous dictum: "intuitions without concepts are blind" (Kant 1999:193-194). The conceptual equipment of man is not given naturally but it is rather constructed through the historical evolution of mankind and society.

3.8 From Generative Mechanisms to the Natural Tendencies of Things: The Domain of the Real

Having established the intransitive existence of generative mechanisms of nature and having demonstrated the necessity of the transitive dimension of scientific research, Bhaskar then moves to the final step of his argument: the analysis of how a generative mechanism of nature should be understood.

Bhaskar has purportedly proved that the world consists of generative mechanisms, which exist independently of man. These mechanisms operate continuously, even though they are not always realized and are rarely experienced. Moreover, it is the operation of such mechanisms that produces the actual events of reality, some of which we experience. When we set up an experimental framework, what we seek is the isolation of a generative mechanism so that we can observe what its operation is, when it is exercised unimpeded by external interferences. Therefore, the statement of a causal law, which is the ultimate aim of scientific theorizing, is nothing other but a description of the operation of a generative mechanism.[104]

But what is a generative mechanism of nature? We know that is something that is beyond the regular sequence of events that we can observe through experimentation. Further, we know that it is something operative, efficacious,

[104] "It is normally only in the laboratory that these enduring mechanisms of nature, whose operations are described in the statements of causal laws, become actually manifest and empirically accessible to men" (Bhaskar [1975] 2008: 46).

active, and that its operation produces events. At this point, Bhaskar makes a crucial claim: this something must be primarily thought of as an *agent*, and more particularly as a *causal agent*. Therefore, a generative mechanism must be thought of in relation to the concept of a *thing*. Bhaskar here is asking us to construct the notion of an *active thing*, viz. of a thing endowed with the power to act. When we think of a generative mechanism, we should think of a thing which acts: "For a generative mechanism is nothing other than a way of acting of a thing" (Bhaskar [1975] 2008: 51).

It is at this point that Bhaskar moves to his neo-Aristotelian account of philosophical ontology.[105] A generative mechanism is the way of acting of a thing. There are things endowed with *powers*. The things are naturally active; they operate. More specifically, they operate in a continuous manner. They continuously exercise the powers that they have. However, these powers are only occasionally realized, because their operation is impeded by the operation of other things. The way of acting of a thing, *what a thing does*, is determined by the *nature* or *essence* of the thing. What science seeks to discover is the nature of things, which by their continuous activity and interaction produce the

[105] For the Aristotelian origins of his ontology of things (whose Aristotelian equivalent is the notion of a *substance* or *ousia*) and tendencies (whose Aristotelian equivalent is the notion of *nature* or *physis*), Bhaskar's gives only a minor hint in footnote 34 (Bhaskar [1975] 2008: 50) of his book, where he refers to E. Anscombe's and P. Geach "*Three Philosophers*" (1961), and more specifically to the chapter on Thomas Aquinas. Nevertheless, despite the undeniable affinity of his ontology with the Aristotelian ontology, Bhaskar almost never makes explicit reference to Aristotle's thought. On Aristotle's ontology of substance, see Irwin (1988: §§10-12; pp. 199-275); also Politis (2004: 190-259). On Thomist ontology, see Ch. 1 in "Metaphysics: A Theory of Things" of Stump (2003). The philosophical community has not focused on this relation between Bhaskarian and Aristotelian ontology, with the exception of the review by R.J.B. in the *Review of Metaphysics (*1980: 619-620), where it is claimed that Bhaskar's "interpretation of the ontological conditions for science is thoroughly and unequivocally Aristotelian, although the author unexplainably avoids making this identification explicit. (Aristotle is mentioned only once, and then in a footnote). Bhaskar's mode of argumentation is transcendental, but the resulting brand of realism is Aristotelian". This relation must become the object of philosophical research in order for Bhaskar's philosophy to be understood in its best light; Milja Kurki, who considers Aristotle's theory of causation in her own works about causation in IR, has already worked to this goal in Kurki (2006), Kurki (2007), and, foremost, Kurki (2008).

actual phenomena of the world. As Bhaskar notes, "[S]cientists attempt to discover the real essences of things a posteriori and to express their discoveries in real definitions of the natural kinds" (Bhaskar [1975] 2008: 214).

Everything is endowed with certain powers determined by its nature, by what kind of thing it is. According to Bhaskar these powers are to be understood in continuous operation: they naturally exercise themselves. They do not need to be triggered in order to get exercised. Bhaskar asks us to imagine of a thing as a natural causal agent that acts naturally. It is for this reason that Bhaskar asks us to think of a thing's powers as 'tendencies'; that is, as potentialities which by nature tend to actualize themselves and which would efficiently actualize themselves if they were not impeded by the contrary operations of other causal agents. A thing by its nature operates, but only rarely does its operation get actualized and even more rarely does its actualization gets perceived by men.

Bhaskar can now describe what it is to state a causal law. The statement of a causal law is, as has been argued, a description of the operation of a generative mechanism. It is now clearer what it is to describe the operation of a generative mechanism; it is to describe the *tendencies* of a thing, i.e. what a thing tends to do intrinsically *by its nature*. A thing by its nature has its proper form of operation, its proper activity. This operation is exercised even if it does not get actualized.

Suppose that the nature of a thing determines it to exercise an activity that we may call ψ'ing. Now, the thing will continually exercise this activity, that is, it will continually be ψ'ing, even if this ψ'ing does not get actualized. If the conditions are favourable it will actualize itself, and if the conditions are favourable for us we can be able to perceive the ψ'ing. But in itself the activity of ψ'ing is exercised continuously. Now, (a) if to state a causal law is to describe a generative mechanism, and (b) if to describe a generative mechanism is to describe the natural tendencies of a thing, then to state a causal law is to describe the natural tendencies of a thing. Therefore, the statement of causal laws implies the use of a distinct kind of conditional statements which describe the exercise of the natural tendencies of things. Bhaskar calls this kind of conditional statements 'normic' conditionals. Normic conditionals:

> do not say what would happen, but what is happening in a perhaps unmanifest way. Whereas a power's statement says A would ψ in appropriate circumstances, a normic statement says that A really is ψ'ing, whether or not its actual (or perceivable) effects are counteracted. They are not counter-factuals, but *transfactuals*; they take us to a level at which things are really going on irrespective of the actual outcome. To invoke a causal law is to invoke a normic conditional. (Bhaskar [1975] 2008: 51)

Bhaskar's analysis leads to a further distinction concerning the levels of reality. At the most immediate level, what we have is *experiences*. But, these experiences can only be accounted for by reference to *events* in the actual world. But now, one can see that events can only be accounted for by reference to a more basic level: the level of generative mechanisms, of causal agents, of things and of their natural tendencies to operate. Accordingly, one can divide reality into three domains. The most basic level is the domain of the *real*. The latter includes things which, by the specific nature that they have, tend to act in certain ways in order to perform an activity that is proper to them. At the next level, there is the domain of the *actual*, which is the level of events. The domain of the actual is produced by the domain of the real, just as events are produced by the tendencies of things, viz. by generative mechanisms. Things naturally act in a continuous way, but their tendencies only get actualized in relation to the activities of other things. The level of the actual is the level of the realization of the tendencies of things, and it is only a subset of the level of the real. Then, there is the least basic level: the domain of the experiences of the events that take place in the actual domain. This is the domain of the empirical, which is again a subset of the domain of the actual, and an even more limited subset of the domain of the real.[106]

3.9 Conclusion

Through his use of transcendental argumentation, Bhaskar has arrived at the point of providing a full-fledged ontology. He began with an inquiry on the objects of scientific research. He claimed the existence of two dimensions of scientific knowledge: the transitive one, which is man-made and socially constructed; and the intransitive one, which is wholly independent of man.

Then, he tried to specify the nature of the intransitive dimension of science. He asked how one should think the world might be like in order for our cognitive activities (sense-perception and experimentation) to make sense. Through this research he managed to provide us with an ontology in his sense

[106] Here I adopt the following diagram which appears in Bhaskar's *ARST* (Table 1.1, Bhaskar [1975] 2008: 56):

	Domain of Real	Domain of Actual	Domain of Empirical
Mechanisms	√		
Events	√	√	
Experiences	√	√	√

of the term, i.e., through an investigation on the ontological presuppositions of the intelligibility of scientific practice. Thus, he has claimed that the objects of scientific research are neither experiences nor events nor regular sequences of events. *Contra* Berkeley, he claimed that there are events beyond experiences. *Contra* Hume, he claimed that the world has a structure beyond regular sequences of events. *Contra* Kant, he claimed that this structure is a feature of the world itself and not something imposed upon the world by the cognitive subject. This structure of the world, which accounts for the regular sequences of events that one observes through experimental activity, is to be understood in the form of generative mechanisms producing phenomena. These generative mechanisms are to be understood as the natural tendencies of things, which by their nature tend to act and which are continuously exercised. Moreover, they do so even if they are only actualized under appropriate circumstances and even if they are perceived only at very limited circumstances. "The world consists of things, not events" and science "is concerned essentially with what kinds of things they are and with what they tend to do" (Bhaskar [1975] 2008: 51). Things and their natural tendencies are "the real basis" of the regular sequences of events that men co-produce in their experimental activity.

Bhaskar has moved from epistemology to philosophy of science and then to ontology. The latter, however, has deep consequences in terms of the way one should understand scientific practice. As a consequence, one returns to the point of departure of the present chapter, which is the relation between philosophy of science and scientific practice. Bhaskar's philosophy claims to offer a different image of the world than the one presented by the prevailing philosophies of science. He alleges that his image of the world is richer, deeper and goes further and beyond observable events and patterns. If the world is understood in a different way, then science, whose aim is the understanding of the world, has to be construed differently as well. According to Bhaskar, science has to search far deeper than atomistic observable events; it should embark upon a quest of the world's operative but unobservable underlying structure, which constitutes the causal generative mechanisms that produce the phenomena of the observable world. Therefore, it is easily understandable that the adoption of Bhaskar's meta-theoretical principles would have an impact on the way working scientists understand the subject-matter of their own field.

Overall, in this chapter, I presented, dissected and critiqued Bhaskar's CR, with special focus on its ontological assumptions. This was an endeavour that had to be undertaken prior to the examination of the alleged usefulness of CR to IR as a meta-theory on the grounds of which one might build 'legitimate' IR theories. This was necessary, because what counts as SR in the context of the meta-theoretical debates in the field of IR is mainly (albeit not exclusively) Bhaskar's CR. It is now on the subject of the relationship between CR, SR and IR that I will turn my attention to.

4

SCIENTIFIC REALISM IN INTERNATIONAL RELATIONS

4.1 Introduction

This chapter examines how the concept of Scientific Realism (SR) and its associated set of claims about theory, knowledge and reality have been employed in the field of International Relations. As noted in the second chapter of the thesis, SR is primarily a view about ontology. Consequently, it is important to keep in mind that in this context the word 'realism' has its own distinctive use and it should not be confused with the same word in political realism and neorealism.

Attention to SR was drawn when Wendt (1987), in the context of offering priority to ontology over epistemology, put forward the doctrines of SR as a meta-theory for IR. Wendt wanted to develop a non-positivist social science of IR; consequently, he used SR in order to provide the foundation for challenging the positivist orthodoxy in IR. His ideas were most fully articulated in his *Social Theory of International Politics* (Wendt 1999). Subsequently, by bringing into the discussion well-worn debates from the philosophy of the social sciences, other authors have supported or criticized his position.[107]

Intriguingly, Wendt's discussion of SR has been judged to be "required reading for any student of international relations, or political science for that matter" (Krasner 2000: 131). Despite the fact that I would not like to flatly dismiss this claim, I will argue that Wendt's discussion puts forward SR as part

[107] For example, Alker (2000), Jackson (2011), Krasner (2000), Patomäki & Wight (2000), Smith (2000), Wight (1999), and others.

of the articulation of a substantive meta-theoretical agenda. The cost of doing this is that it distorts the *genuine* SR position and that it abuses SR's status as a *philosophy* of science whereas, in fact, SR does not represent unequivocally a 'real scientist's own philosophy', nor do scientific realists themselves prescribe that it should be taken as such. On the other hand, one may argue that such a distortion and abuse may well be justified if Wendt's ultimate aim is neither objective exposition nor accuracy (after all, in his 1987 paper, Wendt mentions *absolutely none* of the arguments against SR, not even the standard ones[108]), but instead, a programmatic strategy of *inspiration*, so to speak, against both world-system theory and neorealism. Such a line of argument would hold that Wendt aims for a new meta-theoretical doctrine, which will motivate new ways of studying IR. Indeed, Wendt's emphasis on the ontological-realist features which, in his view, an IR meta-theory should have if debates are to move forward, has given further impetus to those perspectives in IR that privilege ontological questions over epistemological ones (the latter tending to end up in social constructivism). A focus on ontological matters is taken to point beyond the alleged theoretical, methodological and praxiological cul-de-sacs in which current IR theory finds itself. Choosing ontology does not imply the defence of a specific ontological framework within which one should operate; on the contrary, as Patomäki & Wight note,

> It is not simply a scientific ontology we mean here, as in theoretical disagreements over whether states are the most important actors, for example. What we mean by

[108] See section 3.2 of the previous chapter.

> ontology is a philosophical ontology; an inquiry into which is logically prior to the development of any scientific or social ontology. (Patomäki & Wight 2000: 215) [109]

The distinction between *philosophical ontology* and *scientific ontology*[110] is apparent in the following extract by Bhaskar:

> Two senses of the term can be distinguished: either (a) the branch of philosophy concerned with the nature of existence or being as such, apart from any particular existent objects (philosophical ontology); or (b) the entities posited or presupposed by some particular substantive scientific theory (scientific ontology). (Bhaskar 2003: 442)

Thus, one ought to take special note of the context in which the SR doctrines are developed. Since Wendt brings SR into debates with their own dynamic and logic, perhaps one should not expect that his promotion of SR should amount to anything more than the offering of guidelines to navigate amid contradictory meta-theoretical IR debates.

Consequently, one might claim that a full and just critique of Wendt *et al.* should not restrict itself to the arguments and counter-arguments for SR, as these presented in the second chapter, but should have a deeper understanding of *how specifically SR is meant to be taken up in the field of IR meta-theoretical debates*. It goes without saying, however, that such a perspective on SR's critical analysis runs the risk of turning SR into a series of self-conscious examinations of IR as a distinct discipline as well as an object of study. Such considerations would inevitably move one too far from the evaluation of SR

[109] Patomäki & Wight point to Bunge (1996) for further elaboration of this point.
[110] Two interesting papers on ontology are Michel (2009) and Jackson (2008b).

itself. As a consequence, some crucial caveats are in order. The development of these caveats will be the focus of the next section of the chapter.

4.2 Some Theoretical Caveats

The discussion in this section of the chapter will put section 4.3 in perspective, by explaining that the subject matter of the present chapter is not (nor, more importantly, should it be) the evaluation or the reform of the IR meta-theoretical framework in which the discussion about SR in IR takes place. Instead, I am concerned with the evaluation of SR as this doctrine is found in this area of study; in other words, my aim is to examine its credentials, its coherence and, above all, its alleged indispensability in the IR meta-theoretical discussion.

First, it is important to understand in detail what exactly SR is opposed to in the context of the IR meta-theoretical framework. Since, for example, Wendt (1999) meta-theoretically rejects anti-realism, materialism, post-positivism, empiricism and instrumentalism, it can be argued that these opposing viewpoints deserve an adequate characterization (Chernoff 2002: 190). However, for the purpose at hand (i.e. the evaluation of the introduction of SR into IR), an elaborate and fully detailed survey will not be necessary. The reason for that, as I am going to discuss in section 4.3 of the present chapter, is that SR (especially the Bhaskarian version of it) is not introduced into IR as merely one philosophical doctrine amongst other, more or less philosophical, positions. Instead, it is introduced primarily as an *instigator to action* — SR is meant to guide *empirical research* and motivate *political action*, not only to elaborate philosophical argument. For instance, it is indicative of the role that some critical realists envisaged for SR in IR theory that Kurki has recently expressed her disappointment at a surfacing tendency for the depoliticisation, fragmentation and de-concretisation of critical and philosophical IR research. She argues that despite all its sophistication, the IR meta-theoretical and philosophy of science debate has not offered any alternatives to either the theoretical or the political/practical concerns of IR. This is an important failure, not least because we are in the midst of a period which is characterized by a multiple crisis of the national and international economic and political system. As such, a "specialization in meta-theoretical inquiry has brought an interest in a rather narrow and 'technical' set of philosophical questions at the expense of broader political questions embedded in philosophical analysis" (Kurki 2011: 136).

Furthermore, one should not overlook what is taken to be SR's most significant feature when brought into IR: the fact that SR harbours its own "enormous potential for those who wish to construct an account of the influence of unobservable structures … on behavior" (Smith 1996: 36-7). In other words, a secondary dimension of the defence of SR focuses on the key-word 'unobservable' —a first order theoretical matter— and (as I will show) this is where the true rationale for introducing SR lies;[111] that is, one does not merely defend a philosophical doctrine or an attitude, but one also defends the *reification* of specific first-order theoretical terms in IR theories (states, systems etc.) *whose employment and interpretation are dependent on meta-theoretical choices and assumptions.*

According to scientific realists, the decision on how to answer a meta-theoretical question ("Should we use unobservables in our theories?") should

[111] Chernoff (2002), an otherwise excellent critical account of Wendt's SR in IR, seems to have totally missed this dimension.

not be conflated, at least *prima facie*, with the first-order theoretical question of whether specific suggested entities actually exist (and hence are fruitful or not). This literally absolves one from carrying out a detailed examination of SR-alternatives in IR (at least for the purposes of this chapter)[112], since bringing SR into IR also serves ontological questions (the ontological status and the identity of specific unobservables) and not merely methodological agendas. Instead, one should focus on the concept of 'unobservable' itself, and ask whether the use of 'unobservables' as a theoretical term legitimates (or even motivates) the adopting of SR (section 4.3).

For reasons of completeness, nevertheless, I shall briefly examine the fact that the two theories that SR opposes are social constructivism and methodological positivism. The former encapsulates quite straightforward ideas about how agents create the social world, how language determines social ontology (reality) and epistemology (knowledge), and how a hermeneutical stance accords better (or not?) with the rule-based nature of social/international structures. The latter (opposed by both constructivism and realism) is a rather complex meta-theoretical position, which combines realist and anti-realist elements[113] with classical logical positivist ones. Positivists are usually committed to objectivism, empiricism and naturalism; occasionally to behaviourism, too. For my purposes, I will use the more or less accurate description of methodological positivism proposed by Brglez.[114]

[112] See Chernoff (2002) for part of the task that is side-stepped here; he argues that Wendt has misrepresented his opponents' views.

[113] Again, Patomäki & Wight (2000): 215-9 provides adequate grounds for the identification of positivism with anti-realism (a fact well-known in the philosophy of science, though apparently not in IR).

[114] According to Brglez: "With a substantial amount of simplification (perhaps even unjustified about various positivistic methods, methodological strategies and research designs) methodological positivism *inter alia* assumes at the level (or the discourse) of the philosophy of (social) science an epistemological belief in foundations (objectivity), derived either from

Having said that, it is important to acknowledge in advance that whether in choosing opponents or in accumulating defenders, SR has been more or less received into IR under certain quite strong assumptions, which go unquestioned in the relevant literature and which actually dominate the current evaluation of SR doctrines. These assumptions make the reception of SR in IR meta-theory seem easy, natural and perhaps even inevitable. First in the hierarchy of these assumptions is the idea that the study of world politics and international relations should strive for the status of a 'science' akin to natural science: IR should be approached as a 'scientific' subject. I ought to mention these assumptions, however briefly, since they constitute the basic framework that facilitates the transfer of SR philosophy from the natural to the social sciences. Furthermore, if any discussion of SR in IR in this chapter is going to be fruitful for IR meta-theory itself, these assumptions must be put in the proper perspective.

The end of the Cold War defined a new intellectual space for scholars to challenge existing theories of international politics. Constructivists (e.g. Nicholas Onuf, Friedrich Kratochwil, Alexander Wendt) drew on established sociological theory in order to demonstrate how social science could help IR scholars understand the importance of identity and norms in world politics. Consequently, IR theorizing usually begins – although this was not the case in the past – with the demand for a social theory or a road map that offers a

empirical experience (senses) or from human rationality (logic). Such an account of foundations at the level (discourse) of social theory leads epistemologically towards an inductive or deductive search for scientific laws (based ontologically on constant conjunction of events), and at the level of empirical analysis towards the correspondence theory of truth. At the same time, methodological positivism inter alia ontologically assumes an instrumental use of theoretical concepts and models ('as if they were true') at the levels of social and IR theory, and axiologically strictly separates 'is' from 'ought' in empirical research." (Brglez 2001: 343) scientific account of the social world, upon which rests the theoretical understanding of international politics. A social theory should flesh out an ontology and an epistemology in the form of a scientific account, i.e. it should contribute to the study of IR the way ‘ontologies’ and ‘epistemologies’ are supposed to do in the context of a ‘scientific’ discipline. Such theories are to be used to analyse the origins, development and consequences of norms and cultures and are to be presented in “the form of most general social scientific propositions” (Holsti 1991:166).

In addition, by taking it for granted that a scientific study of IR is possible, IR theorists do not fail to respond to methodological questions in their attempt to elaborate the meta-theoretical foundations of their research:

> These kinds of questions, which do not refer directly to specific empirical explanatory problems in the way that substantive theories do, are stuff of meta-theory and meta-theoretical concepts such as ‘agency-structure’, ‘micro-macro’ and ‘time-space’. … In the social sciences substantive theories aim to generate new empirical information about the social world, whereas meta- or sensitizing theories and concepts are concerned with general ontological and epistemological understandings; meta-theories and meta-concepts are designed to equip us with a general sense of the kinds of things that exist in the social world, and with ways of thinking about the question of how we might ‘know’ that world. (Sibeon 2004: 13)

Hence, overall, a strong attachment to a rather simplistic and traditional notion of science is assumed throughout the whole debate about science in IR. This allows for peculiarly uncritical attempts to adapt philosophical notions from the natural sciences to the meta-theoretical study of IR. Characteristically, in this context SR serves as a platform for ontological-oriented theorizing by assuming that SR's conceptual strengths in the philosophy of the natural world are transported intact into the philosophy of the social world.

At this point, a number of important questions arise: Should one doubt the above assumptions? Should one beware of 'scientism' in mainstream IR? Are the defenders of SR confusing the scientistic with the scientific? All too often, philosophical models of science, which are based primarily on physics, have dominated the study of social phenomena. Still, the positivist conception of science has never been devoid of harsh critiques. The several *Methodenstreiten* throughout the very long history of positivism as *the* guiding philosophy of social sciences have always focused on the incompatibility of the very nature of positivism with the nature of the social world.[115] Dilemmas such as the 'epistemic fallacy', the problem of induction, the alleged irreducibly open nature of the social world, the necessity of ceteris paribus clauses, value-ladenness and the theory-ladenness of observation, inevitably and inherently haunt any positivist study, including that of IR. Indeed, the contrast implicit in this picture is the one that gave rise to the Explanation/Understanding divide[116].

One could justifiably argue, however, that such epistemological divides are quite alien to the concept of standard SR itself. If anything, it is the ontological divide which is liable to raise doubts: namely, an SR interpretation of the natural world does not extend necessarily to the social world itself; hence, the standard scientific realist arguments nesting in the philosophy of science are not necessarily suitable for the specific debates in the social sciences. Even if social

[115] See among others: Dilthey (1988); von Wright (1971); Adorno *et al*. (1976); and Habermas (1978).
[116] See Hollis & Smith (1990). An interesting critical discussion of the Explanation/ Understanding divide can be found in Wendt (1998).

studies should emulate some standard conception of 'science' and, additionally, even if SR is transferred to the philosophical debates by resource to the methodology of such a 'science', one can still not avoid the brute fact that the referent of a scientific theory of the natural world would still be *non-intelligent,* whereas the referent of a social theory (an agent) would be *intelligent*.[117] Again, at the very least, this ontological division calls for a radical re-conceptualization of SR before implementing the interdisciplinary leap of bringing it into the realm of the social sciences; the methodological troubles of social inquiry being only tangential to the coherence of SR per se.

Wendt *et al*.'s starting point, though, seems to lie in the conviction that scientific research done in the SR spirit is the best way to get to know the world. Moreover, in so far as the conceptual system that Wendt himself builds can provide a new, fertile pathway to further IR theoretical studies, then SR itself would be legitimated as the appropriate meta-theory. The circularity and the insularity that these assumptions impose on the discussion are quite troubling.

Once again, doubts crop up at this point: should these features motivate one to put on hold any close examination of the reception of SR in IR? One should emphasize that our understanding of SR in IR is determined by the meta-theoretical choices that have shaped the relevant debate. Therefore, one legitimately wonders if one should examine one's credentials before proceeding to analyze the reception of SR itself.

However, it is worth stressing at this point that the aims of this chapter do not allow either for the doubting of the specific meta-theoretical context of the

[117] See Hollis (1992) for a more detailed elaboration of this point.

reception itself[118] or for the doubting of its (characteristically old-fashioned) conception of scientific inquiry. The conception of scientific inquiry that has been adopted by IR is old-fashioned for a number of reasons. To name only one, Kratochwil is right when he argues that:

> Historians and philosophers of science in the last generation have also fundamentally changed the conception of science. The emphasis has decisively shifted from simple 'tests' and the discovery of universal laws, to a concept of science as a *practice*. Instead of logic and the logical positivist speculation about the nature of 'true' statements which were part of, for example, Popper's 'Third World', we have come to realize that knowledge production has an important practical and historical dimension that needs to be reflected upon." (Kratochwil 2006: 6)[119]

Furthermore, SR has been received as part of the post-positivist agenda.[120] As such, it would be inappropriate to cast doubts on the conceptual credentials of the framework which considers international politics to be a subject for scientific study, while examining the way IR has itself received the doctrines of SR. This is a far broader matter, which goes beyond the discussion of SR which I undertake here. Consequently, a critical examination of the conceptual

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[118] The wider context is, of course, the context of the so-called third debate in IR. It has been going on between positivism and post-positivism since the late 1980s and it is over whether positivist theories of IR are valid, and, if not, what can be an alternative.

[119] Kratochwil points to the works of Fuller (1991b) and Knorr-Cetina (1981, 1999), which "all focus on the process of knowledge production instead of on 'ontology' or field-independent epistemological categories" (Kratochwil 2006: 6). One might add Collins (1992), Giere (1998), and Pickering (1995), along with my own reflections on the concept of science as practice (which encapsulates the dimension of the historicity of science) in the second chapter of the thesis.

[120] More precisely, critical realists consider CR – the Bhaskarian version of SR – to be part of the post-positivist agenda, because they consider CR incompatible with positivism. On the contrary, Wendt's version of SR belongs to the same agenda only in the sense that it constitutes a 'via media' between positivism and interpretivism by attempting to bridge these two *prima facie* incompatible methodologies.

framework which considers international politics to be a subject for scientific study has to be added to those ‘external’ matters, which touch on the evaluation of SR only tangentially (e.g. the Explanation/Understanding debate). This does not mean, though, that, for instance, SR cannot play a crucial role in the ‘reasons vs. causes’ debate. Obviously, choosing SR as the appropriate meta-theory for IR strongly biases the methodology towards causal explanations. This aspect of applying SR to IR will be referred to again in sections 4.3 and 4.4. Nevertheless, the aforementioned issues should not be taken to be definitive for the evaluation of SR in IR meta-theory.

In any case, the wide diversity of the authors who defend SR and Wendt’s specific employment of SR’s doctrines, plus the rather enthusiastic welcome that Wendt’s discussion of SR has received in IR, is a good reason to carefully examine and analyze Wendt’s interpretation of SR in this chapter. As an important offshoot of this assessment, I will go on to provide a far more detailed critical analysis of Wendt’s recent turn to Quantum Theory in Chapter 5.

My arguments in this chapter will appear as follows: In the next section (4.3) I present the SR debate in IR. In the first subsection of section 4.3, I deal with Wendt’s understanding and distinctive use of SR in the context of IR; in the second, I will examine how SR is deployed in the work of Colin Wight who, inspired by Roy Bhaskar and his *paradigm* of Critical Realism, attempts to reform IR discussions accordingly. In the third subsection, I intend to discuss arguments for and against SR which have been put forward by other IR scholars who, being familiar with Wendt’s and Wight’s works, have got involved in the relevant debate by offering their own, often very differing, approaches to the SR problematique.

In section 4.4 of this chapter I try to come to grips with two important issues, which have either directly or indirectly (in the case of the second issue) been raised by the SR debate in IR. Namely, in the first subsection of section 4.4, I deal with the causes vs. reasons issue, particularly in the light of SR's contributions to the discussion of causation in IR, whereas in the second subsection, I critically present what Fred Halliday used to call 'meta-theoretical hypochondria'. The chapter finishes with the presentation of my conclusions in section 4.5.

4.3 The SR Debate in IR

Alexander Wendt and Colin Wight have been the main protagonists in the SR debate in IR and I will devote a substantial part of my discussion to the critical examination of their positions. It seems to me that Wendt should have precedence over Wight since the former was the first to acquaint IR scholars with the SR problematique; therefore, I begin with a discussion of his work.

4.3.1 Wendt on SR

In this section, I will illustrate how Wendt's IR Theory has evolved with regards to the study of SR within IR, from the appearance of his paper on "The agent-structure debate in International Relations Theory" (1987) onwards. The goal of my analysis is to highlight the main thread of Wendt's successive reflections on SR and other relevant philosophical issues that pertain to the four papers of

Wendt's, which I intend to discuss here.[121] Notwithstanding the significance of the philosophical issues per se, in what follows I will also explore how these meta-theoretical assumptions underpin the Wendtian IR Theory. The reason for doing this lies in the fact that one must always bear in mind that in Wendt's theorizing "meta-theoretical and theoretical matters are necessarily connected" (Guzzini & Leander 2006a: xvi).

Wendt (1987) introduced SR into IR in his attempt to resolve the agent-structure problem in IR Theory and thus come-up with a new IR Theory, which could supersede the existing ones. He was motivated by his disappointment with the inappropriateness of Waltz's neorealism (Waltz 1979) and Wallerstein's world-system theory (Wallerstein 1974) to provide a comprehensive *structural* explanation of the roles of the state and the international system *as mutually dependent entities*. He argues that the weakness of both theories is due to the individualist and structuralist ontologies of neorealism and world-system theory respectively. Wendt summarizes the problem as follows:

> […] neorealism and world-system theory share a common, underlying approach to the agent-structure *problem*: they both attempt to make either agents or structures into primitive units, which leaves each equally unable to explain the properties of those units, and therefore to justify its theoretical and explanatory claims about state action…Notwithstanding their apparent aspiration to be general theories of

[121] One could also add Wendt (1992) to this list. However, his reflections on anarchy do not include any new ideas about SR and this is the reason that I have not included him. What is really striking is the context of the following statements at the end of this paper, at least when they are read in retrospect and one bears in mind the direction that Wendt's theorizing has followed after its publication: "Neither positivism, nor scientific realism, nor post-structuralism tells us about the structure and dynamics of international life. Philosophies of science are not theories of international relations." (Wendt 1992: 425)

international relations, the individualist and structuralist ontologies of neorealism and world-system theory preclude the development of such theories. In contrast, a structurationist approach to the agent-structure problem would permit us to develop theoretical accounts of both state agents and system structures without engaging in either ontological reductionism or reification” (Wendt 1987: 349)

In order to frame such a *structurationist* approach to the agent-structure problem, Wendt was in need of a meta-theory, which could conceive of *unobservable* entities in such a way that would enable us to see state agents and system structures as mutually dependent and co-constitutive – that is, liable to mutual reproduction or even change – according to the development of their interrelationship over space and time. Wendt’s attraction to SR started with his interest in the notion of the *unobservable*, as the latter presented itself in the relation between agent and structure. According to Onuf, Wendt was intrigued on how an unobserved ‘thing’ called structure had ‘observable effects’ (Onuf 2012: 25); even though, one might point out that system theorists also did this, i.e. they used to attribute ‘observable effects’ to unobserved or unobservable structures. Given the above, it seems that, while unobservables are not a problem for “working scientists who take unobservables for granted”, they are a problem for positivist philosophers (ibid). Wendt’s embracing of this idea led to the “immediate if unplanned effect of unleashing an increasingly rancorous debate over the discipline’s philosophical grounding” (ibid.).

Indeed, by drawing upon the literature of the philosophy of science and especially – however, not exclusively – on Bhaskarian CR, Wendt’s SR legitimates the ascription of ontological status to unobservable entities like generative mechanisms. The conclusion to the third chapter of the thesis, which refers to the notion of generative mechanisms, is relevant here. There I argued that: “This structure of the world, which accounts for the regular sequences of events that one observes through experimental activity, is to be understood in the form of generative mechanisms producing phenomena. These generative mechanisms are to be understood as the natural tendencies of things, which by their nature tend to act and which are continuously exercised. Moreover, they do so even if they are only actualized under appropriate circumstances and even if they are perceived only in very limited circumstances. ‘The world consists of things, not events’ and science ‘is concerned essentially with what kinds of things they are and with what they tend to do’ (Bhaskar [1975] 2008: 51). Things and their natural tendencies are ‘the real basis’ of the regular sequences of events that men co-produce in their experimental activity.” (see page 150 of this thesis).

Furthermore, Wendt claims that another crucial characteristic of SR is the prioritization of ontology over epistemology. He argues that empiricists subordinate ontology to epistemology, for “what exists is a function of what can be known *experientially*” (Wendt 1987: 352), whereas scientific realists accept abductive (or retroductive) *inference* as justified, “if the entity in question can produce observable effects, or if its manipulation permits us to intervene in the observable world” (ibid: 352). Following Wendt, SR assumes that the unobservable entities, which are theoretical terms of mature scientific theories, do in fact exist. This is because scientists themselves consider them to exist and, as a consequence, organize their experiments in accordance with their theories:

> Thus, the fact that we can use theories about the (unobservable) internal structure of atoms to build nuclear weapons which can destroy cities is a good reason for the realist to believe that such structures exist, as we understand them today. (ibid: 352)

The above thesis is compatible with structuration theory, for the latter legitimates the scientific consideration of unobservable and irreducible social structures within its own framework. This holds because the structuration theory "conceptualizes agents and structures as mutually constitutive yet ontologically distinct entities" (ibid: 360). In other words, agents and structures are co-determined in the sense that "social structures are the result of the intended and unintended consequences of human action, just as those actions presuppose or are mediated by an irreducible structural context" (ibid: 360). It is obvious that SR underpins the application of structuration theory in IR, as this is demonstrated by the fact that Wendt uses the notion of *unobservables* in order to speak of structures.

I now come to the implications that the above statements have for IR theorizing. Regardless of whether or not structuration theory provides a suitable solution to the agent-structure problem, SR challenges the dichotomy between the methodological orthodoxy of empiricism, which speaks in the name of 'science', and the 'un-scientific' approaches to IR offered by hermeneutics and critical theory. Although not an IR Theory in itself, SR aspires to be a meta-theory upon which one can build different kinds of IR Theories, in the frameworks of which one searches not only for causes in the Humean sense, but also for generative mechanisms. The existence of the latter can be inductively inferred by the observation of the emerging phenomena/effects, which these causal mechanisms produce.

To sum up, in his 1987 paper on agents and structures in IR, Wendt made two basic claims. The first claim consists in that his structuration theory provides a way to combine a focus on structures (ideas, norms, culture, etc.) with a focus on agents. This explains how the international system can be produced, reproduced and even changed due to the mutual reconstitution of its agents and structures, which is not self-referential, but which allows agents to undertake actions that can change the existing structures. As a consequence, an explanation is now on offer as to why anarchy in the international system cannot be properly explained only by its reduction to states' behaviours and the distribution of power between them (as Waltz allegedly argues). Moreover, the same line of thought could offer an explanation as to why the structure of the international system cannot be conceived by its reduction to the principles which organize the world economy, namely global capitalism (as Wallerstein argues). The second claim holds that SR prioritizes ontology over epistemology and methodology and allows for the study of unobservables (structures), namely of many social phenomena "which traditional empirical social scientists have been forced to ignore" (Ringmar 1997: 271).

Wendt's *STIP* (1999) is a follow-up to his 1987 paper and consists of two parts. In the first part, titled "Social Theory", in order to frame the *meta-theoretical* background of his IR Theory, the author embarks upon the philosophy of natural and social sciences. In the second part, titled "International Politics", Wendt unfolds his *social theory* of international politics.

No matter how strange it may seem to the IR scholars who are not familiar with the corpus of Wendt's works, it is real politics that have inspired Wendt to develop an IR Theory which is consistent in all its levels. In particular, he has been triggered by his belief that neorealism - perhaps even more so than realism - is flawed, in the sense that it leaves little room for changing the structures and the functioning of international politics. He claims that one should take on board the fact that the end of the Cold-War has proved that change is more possible than neo-realists used to believe in the past (or even today). Consequently, through his *STIP*, Wendt went on to frame a constructivist approach to the study of international politics that provides for the possibility for change of the morphology of the international political landscape.

Indeed, power politics is assumed to be the overwhelming practice in international politics and realism is considered – especially by the majority of mainstream American IR scholars – to be the theory that best describes that practice. As a consequence, Wendt argues that one has to grasp "the link between the 'common sense' in international affairs (mostly realist materialism) and the way it not only understands but also constructs the actual state of world politics" (Guzzini and Leander 2006a: xviii). In order to achieve this goal, Wendt challenges realism. In order for his challenge to be successful, Wendt puts the philosophical underpinnings of realism into the limelight. He grapples with the meta-theoretical assumptions of IR Theory by grounding the building of a substantive IR Theory on a scientific realist meta-theory. In doing so, Wendt puts forward a theoretical fabric whose levels are intrinsically connected in a way that "changing one can affect all the others" (Guzzini and Leander 2006a: xviii).

Wendt's aims become even clearer if one pays greater attention to his statement that the subject of *STIP* is "the ontology of international life" (Wendt 1999: 370). The understanding of the ontology of international life is a prerequisite for the study of international politics, for "even the most empirically minded students of international politics must 'do' ontology, because in order to explain how the international system works they have to make metaphysical assumptions about what it is made of and how it is structured" (ibid: 370).

Wendt then goes on to propose a scientific realist meta-theory on which he bases his new substantive theory of the international system. The latter is differentiated from Waltz's neorealism in that it views the international system in idealist and holist terms instead of materialist and individualist ones. Wendt defends an *idealist* or *social ontology*, so that he can attack the dominant materialist ontology of mainstream IR theories such as, primarly, neorealism and, secondarily, neoliberalism. This materialist ontology gives priority to "material forces, defined as power and interest, and bring in ideas only to mop up residual unexplained variance" (ibid: 371). However, it is important to note that Wendt seems in fact not to endorse the 'ideas all the way down' thesis all the way. Instead, he defends a thin constructivist approach, which is compatible with the notion of a 'rump materialism', according to which the distribution of capabilities (i.e. geographical and resource constraints) and the composition of capabilities (i.e. technology) count as material phenomena (Wendt 1999: 109-113). Therefore, Gofas (2007: 66) argues that "Wendt introduces the notion of 'rump materialism', in order to remind us that "constructivism should not proceed 'as if nature did not matter' (Wendt 1997: 111)." Among the examples of ‘rump materialism’ which Wendt provides is that of the German invasion in Poland in 1939, which “was caused largely by ideas, but the material advantage enjoyed by the Germans was an important factor about the situation: given their aggressive intentions, it made it easier, and therefore more likely, for the Germans to invade” (Wendt 2000: 166). All these are effectively summarized by Wendt himself who writes that:

> The key is to reclaim power and interest from materialism by showing how their content and meaning are *constituted* by ideas and cultures. Having stripped power and interest explanations of their implicit ideational content we see that relatively little of international life is a function of material forces as such. It therefore makes more sense to begin our theorizing about international politics with the distribution of ideas, and especially culture, in the system, and then bring in material forces, rather than the other way round. (Wendt 1999: 371)

Nevertheless, I am cautious over the claim that the relationship between the material and the ideational forces is clear-cut in Wendt’s ‘rump materialism’. As I have mentioned previously, Wendt does not seem to endorse completely the ‘ideas all the way down’ thesis. Indeed, one cannot be sure if he stands for ‘ideas all the way down’ or for ‘ideas *almost* all the way down’, as Steve Smith (2000) has shown by quoting many of Wendt’s contradictory statements on this issue.

Next, Wendt turns his attention to an exemplification of the ‘agent-structure problem’. Namely, he examines the question of how the ideas held by state agents relate to the ideas that make up the structure of the international system. As it is known, methodological individualism, which is mainly expressed in rational choice and game theoretical approaches, holds that “social structures supervene on the properties of independently existing, preconstituted agents, like states" (ibid: 371-72), but cannot see "that agents might be *constituted* by social structures, that the nature of states might be bound up conceptually with the structure of the states system" (ibid: 372). Therefore, Wendt proposes a structurationist approach to the problem about how ideas and state agents are interrelated. He argues that it is only through the application of the structurationist approach that one can conclude that ideas 'act' upon states but are also 'acted upon' by them. As Wendt puts it:

> …the ontology of international life that I have advocated is "social" in the sense that it is through ideas that states ultimately relate to one another, and "constructionist" in the sense that these ideas help define who and what states are (Wendt 1999: 372)

Wendt's constructivism takes international system to be a social fact, which allows for the possibility of social learning in the absence of which one cannot conceive the reflexivity in the social world.

It is now time to deal with the meta-theoretical issues that underlie, underpin and, to some extent, determine Wendt's substantive IR theory. Traditional constructivists used to claim that constructivist ontology is incompatible with positivist epistemology (which is taken to be the mainstream epistemology in natural sciences); instead, only interpretivist and post-positivist epistemologies are considered to be compatible with constructivism. Wendt, on the contrary, believes that even post-positivists are "tacit realists" (ibid: 373) in their empirical research, since they test *falsifiable* conjectures of a kind similar to those which are used by positivists in their own work; indeed, for him what really matters is that "the epistemic authority of *any* scientific study, whether using interpretive or positivist methods, depends on publicly available evidence and the possibility that its conclusions might in some broad sense be falsified" (ibid: 373). Therefore, Wendt suggests that 'causal explanation' should be complemented with 'constitutive explanation'. Such a development could provide us with a 'via media'[122] as the appropriate way of theorizing about causation in IR. The latter would, in its turn, make constructivist ontology (based on a scientific-realist meta-theory) compatible with both positivist and post-positivist epistemologies.

This brings up the discussion on questions of causation, which are addressed by Wendt's constitutive explanation. According to Wendt, "causal questions inquire into antecedent conditions or mechanisms that generate independently existing effects; this is generally what we want to know when we ask 'why' something happened or 'how' a process works" (ibid: 373). On the contrary, "constitutive questions inquire into the conditions of possibility that make something what it is or give the causal powers that it has, and as such they are interested in relationships of conceptual, not natural necessity; this is what we want to know when we ask 'how is X possible?' or, simply, 'what is X?'" (ibid: 373).

Wendt, furthermore, is explicit in that natural and social scientists alike should ask causal as well as constitutive questions. Nevertheless, the extent to which a constitutive explanation could be useful for the study of political events

[122] Gofas calls Wendt's 'via media' an 'epistemological Westphalia', since by introducing this term Wendt attempts to transcend "current disciplinary divisions between causal and non-causal forms of theorising" (Gofas 2007: 67); the same term is also used by Wendt himself in a different context (2006a: 216).

of major importance to the historical development of the international system is questionable. In order to address this issue, Wendt gives the following example:

> Gorbachev's New Thinking was a deep, conceptual reassessment of what the US-Soviet relationship 'was'. It was constitutive theorizing, at the lay level, and based on it Soviets were able to end, unilaterally and almost overnight, a conflict that seemed it had become set in stone. It may be that objective conditions were such that the Soviets 'had' to change their ideas about the Cold War, but it does not change the fact that in an important sense those ideas *were* the Cold War, and as such changing them by definition changed the reality. (ibid: 375)

Admittedly, Wendt has at times attempted to explain the actual difference which a scientific realist's view of the social world makes for empirical research. The above example testifies to his intention to do exactly that. Similarly, Ringmar (2007) refers to Shapiro & Wendt (1992) where the two authors examine the politics of consent from a scientific realist perspective, by taking on board the existence of unobservable structures. Strict empiricists usually ignore the existence of such unobservable structures and, hence, cannot come up with a satisfactory explanation of consent. As a consequence, "empiricists can study neither how political agendas are manipulated nor the processes through which people's preferences and identities are shaped" (Shapiro and Wendt 1992: 200-6, quoted in Ringmar 2007: 274). In other words, due to the existence and the impact of unobservable structures, agents cannot be taken at their own estimation because they might be victims of false consciousness, something for which empiricists cannot account for.

By analogy, I argue that sometimes a state does not behave according to the distribution of power and wealth in the international system, because there are one or more unobservable structures at work which impact upon this state's behaviour and thus co-determine it.

Following Shapiro and Wendt, Ringmar argues that in similar cases, one needs "to make a hypothesis regarding what things *would have been like* if only structural power had not been present, and then measure the difference between this condition and the one presently at hand" (ibid: 274).

As already mentioned, constitutive theorizing, which Wendt encourages, facilitates the reflective thinking on structures, which through their interactions with agents make up various social facts. This opens up the possibility for initiatives to be undertaken on behalf of the agents (individuals, states, NGOs and radical political movements alike) in the direction of changing the morphology of the current international political landscape. That given, the dialogue between IR and the fields of Political Theory and Normative IR should be intensified, since the perspective for changing the structures of the international systems presents us with normative questions and practical political challenges of the first order.

In this vein, Wendt claims that this dialogue between IR and the fields of Political Theory and Normative IR has until recently "been kept very limited by the domestic orientation of most Political Theory and the marginalization of normative questions in IR by Realism" (Wendt 1999: 376-77).

This might hold with respect to the direction that IR studies have followed mainly in the USA, but not in Europe. Furthermore, even in the USA we have excellent examples of theoretical works where IR encounters classical and modern Political Theory, Moral Philosophy and Ethics, Legal Theory and Human Rights Theory.[123] Furthermore, there are also recent publications where FPA and EFPA are fertilized through their encounter with Political Theory and Human Rights Theory. Suffice it to mention only Christopher Hill (2007), and Lisbeth Aggestam and Christopher Hill (2008), where the authors, in order to theorize about foreign policy-making in multicultural societies in general and in EU in particular, first come to grips with questions about whether we have different kinds of multicultural societies, what their specific characteristics are, why and how have the latter emerged and, finally, how they do influence the national foreign policy-making of the states within which such multicultural societies have developed.

I would like to make one more point before completing my account of *STIP*: since the possibility for the emergence of an 'anarchical society' is clearly provided by Wendt's theory, one may trace connections and affinities between the latter's 'thin constructivism' and the work of some theorists who belong to the *English School* of IR theory. Wendt and most English School scholars seem both to share a state-centric view of the international system and to assign to ideas – although with differing degrees of emphasis - a role in the shaping of world history. Furthermore, although from different perspectives the notion of anarchy in the international system also interests them both - for instance, Wendt (1992) was clearly a response to Bull ([1977] 2002). Suganami (2001) argued that hypothetical causal narratives of the kind that Wendt produces may assist historical research of the kind we encounter with in some works of the

[123] An example of the attention paid to human rights within European IR Theory is Vincent (1986). Moreover, Michael Donelan ([1990]1992), Richard Tuck ([1999] 2001), Chris Brown (1992), (2002), (2010), John Rawls (2001), Kimberly Hutchings (1999) are only but a few of the works which belong to what one nowadays terms International Political Theory.

English School, while insisting that progress - beyond Wendt or the English School - in historically and theoretically based IR scholarship cannot be achieved “unless we engage in meta-historical reflections or critical investigations into the nature and function of historical enquiry and representation” (Suganami 2001: 419).

To sum up, the first part of *STIP* presents a constructivist meta-theory (based mainly on the Bhaskarian CR),[124] while the second half discusses notions such as identity, norms and culture, along with those of anarchy in international politics, which are closely related to various other streams of work in IR.

Moving on to the next major step in the evolution of Wendt’s manner of doing IR Theory, I should like to reiterate that, according to Guzzini and Leander, the update of Wendt’s IR Theory came “in the version of a theory of history” (Guzzini & Leander 2006a: xvii), which can be found in his paper, “Why a World State is Inevitable” (Wendt 2003). Guzzini and Leander argue that Wendt in that paper favours a *teleological* approach to History, in which politics and metaphysics are interrelated. On the one hand, I agree with them that this work highlights a turning point in the development of Wendt’s IR Theory, insofar as it explores the idea that *collective entities*, such as the world state, might constitute kinds of *self-organizing systems*. On the other hand, unlike Guzzini and Leander, I would not support the idea of a new “theory of History”, but rather of a new ‘developmental theory of the international system’. Nevertheless, in order to be fair to Guzzini and Leander, I must admit that with the introduction of the notion of self-organizing systems, Wendt

[124] Chris Brown and Kirsten Ainley speak of “a clear, albeit at times somewhat anodyne, version of constructivist epistemology” (Brown & Ainley 2007: 50). However, I don’t think Wendt’s meta-theory is so anodyne, since it has important implications for substantive IR Theory.

aspires to *historicize* his social theory of international politics (*STIP*); that is, he aspires to put it *into motion* and thus further differentiate it from the through and through scientific theory of neorealism.

It is interesting to note that it is Ringmar who first made this point, when, after the publication of Wendt's 1987 paper and before the publication of the *STIP*, he argued that

> He [Wendt] tries, in a word, to put the neo-realist picture *into motion*, to historicize it, to move it closer to actions and thought and to human life as we know it. Yet, in the final analysis, Wendt is also a social scientist and *not* a historian, and while he may try to historicize neo-realism, there are inevitably limits to any such enterprise (Ringmar 2007: 285).

Given the above, one might say that Ringmar foresaw, to a certain extent, the development of Wendt's technique of IR theorizing towards a certain direction that emphasizes notions such as personification, self-organizing systems, collective consciousness, etc. (for instance, see Wendt 2003, 2004, 2006a).

In fact, Wendt takes on board the notion of emergence and argues that self-organization theory provides a scientific basis for a teleological explanation of the way the international system develops. The self-organization theory challenges the neo-Darwinian theory of evolution, but only a few IR scholars are today familiar with the former. Be that as it may, "in the social sciences more generally the idea of self-organization has been around since the 'spontaneous order' tradition of the Scottish empiricists, and is getting considerable attention today" (Wendt 2003: 492).[125] Wendt has thus enriched the concept of 'constitutive explanation', which has been coined in his 1987 paper and further developed in his *STIP*, with the notion of *a spontaneous order* which may emerge in a self-organizing system "as a result of the interactions of elements following purely local rules" (ibid: 498). By applying the above ideas to IR Theory, Wendt concludes that the emergence of a world state is, at the end of the day, inevitable. He holds it to be inevitable since, given the alleged end-directedness of the historical development of the international system, a global monopoly on the legitimate use of organized violence must also be inevitable. However, it is important to point out that natural scientists, as well as biologists who work on the 'self-organization theory', might reject the above as not being a direct implication of the latter. Moreover, Wendt himself admits that much of the work produced by self-organization theorists is not teleological to the extent that "many self-organization theorists might vigorously reject any such reading of their approach" (Wendt 2003: 492). In the context of his approach, however, Wendt considers that agency is at both the micro- and macro- levels absolutely consistent with his teleological explanation of the final development of the international system to a world state:

[125] For instance, Cornelius Castoriadis' philosophical system is based on the notions of *autonomy*, *emergence* and *self-organization*. Castoriadis and the Chilean biologist, neuroscientist and philosopher Francisco Varela have been mutually influenced by each other's work. They were both interested in the notions of *autonomy* and *autopoiesis* (which means self-creation, self-construction in Greek); see their discussion on this issue in chapter 4 ("Life and Creation") of Cornelius Castoriadis' book, *Postscript on Insignificance – Dialogues with Cornelius Castoriadis* (2011: 58-73), where the notion of *emergence* in physical sciences and biology is closely related to that of *autopoiesis* and *autonomy* in social and political theory. Moreover, Friedrich Kratochwil, in his critique of Wendt's *Social Theory of International Politics* refers to Varela and his influence on Luhmann, when arguing that "constructivists can be found not only in cultural sciences. Indeed, some of its most prominent exponents have been biologists (Maturana and Varela 1992) who, in turn, influenced an entirely new system in sociology (Luhman 1984, 1997)" (Kratochwil 2006a: 22).

> Human agency matters all the way down, but it is increasingly constrained and enabled by the requirements of universal recognition. (Wendt 2003: 491)

The above brief presentation of the main ideas of his 2003 paper highlights a number of issues. Firstly, it highlights that "it is through the process of collective identity formation that Wendt sees the development of a world state as being inevitable" (Brown & Ainley 2009: 76). Secondly, it shows Wendt's attempts to do justice to his SR assumptions, since ontology retains its priority over epistemology and methodology. This is deduced, amongst others, from his emphasis on the possible unobservability[126] of the structures and boundary conditions that are supposed to generate end-directedness in a system. However, given that ontology comes first, one is faced with the hard epistemological problem of "how we can know whether a world state is inevitable before the system gets there?" (Wendt 2003: 503).

A crucial comment to be made here is that, according to Karl Popper, in order for a statement to be so powerful as to claim the status of a scientific conjecture, it must have a low probability of verification and the probability of the statement being verified is always smaller the more specific the statement is; and vice versa (Popper [1963] 1996). Therefore, I strongly doubt that a statement as general as the one which holds that one day the international system will inevitably develop into a world-state due to its alleged end-directedness feature could have a claim to the status of a scientific conjecture.[127]

[126] Here unobservability is a great advantage, as no-one can refute Wendt's assertions of telos/end-directness.

[127] In addition, one could not help underscore that in both his 2006a and 2010 papers Wendt appears to have abandoned the idea of the inevitability of the world state.

In 2004 Wendt published his paper “The State as Person in International Theory” (Wendt 2004),[128] which brought him closer to his latest proposal for a ‘quantum social science’ (Wendt 2006a, 2010). In his 2004 paper, which marks his shift to the investigation of the notion of state’s ‘collective consciousness’, he considers the state to be a kind of a ‘superorganism’ with its own ‘collective consciousness’. As I discuss in chapter 5, it is exactly this search for a comprehensive concept of ‘collective consciousness’, which has led Wendt to develop his ‘collective quantum consciousness hypothesis’; the latter being one of the cornerstones of Wendt’s ‘quantum social science’.

When it comes to the simple, albeit very important, question about how this new way of theorizing about ‘the state as a person’ may be of some help to the empirically-oriented IR scholars, Wendt is unable to provide any concrete suggestions or empirical research proposals which differ from the existing ones. He justifies his new theoretical work by invoking the rather trivial argument that states should be persons for “states help bring order, and yes, even justice to the world, and if we want to have states then it is better they take the form of persons rather than something more amorphous, because this will help make their effects more politically accountable” (Wendt 2004: 316).

To conclude, one should bear in mind that Wendt’s appeal to notions such as ‘self-organization theory’, ‘collective consciousness’ and lastly that of ‘collective quantum consciousness’ (the latter being part and parcel of his ‘quantum social science’ project) exemplifies how the meaning of the concept

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[128] For a critical examination of Wendt’s reflections on the state as a person, one can read the rest of the papers which co-constitute the “Forum on the state as a person”, which has been organized by the *Review of International Studies*: Jackson (2004a), Jackson (2004b), Neumann (2004) and Wight (2004).

of 'constitutive explanation' has been enriched and developed within Wendt's IR theorizing over the years.

4.3.2 Wight on SR

Colin Wight is a critical realist who is very interested in and very well exercised about social science and philosophy of science issues. His *Agents, Structures and* International *Relations* (2006) (henceforth *ASIR*) has been until recently – that is, until when Patrick Thaddeus Jackson came up with *The Conduct of Inquiry in International Relations* (2011) – the only book after Wendt's *STIP* to deal extensively with meta-theoretical issues. However, *ASIR* is not exhaustive in the discussion of such issues. On the contrary, by considering the long debated IR agent-structure issue as instrumentally useful for explaining the development of the international system, Wight delves into the deep waters of sociological theory in order to find a solution.

In the first chapter of *ASIR* Wight launches an attack against the 'epistemic fallacy' in IR, which consists in the assumption that *what is can be reduced to what is known*. Wight claims that positivism is mistakenly equated with science. SR and the crucial issue concerning the affinities between natural and social sciences, which allegedly legitimate the search for a common research methodology, are also discussed in the framework of the first chapter. In the next three chapters the author is concerned with the examination of the agent-structure debate, first as this has unfolded in sociological theory, and then as it has been introduced into and developed within IR. In the final three chapters Wight focuses on the epistemological and methodological issues that are raised by the consideration of the agent-structure problem. I will argue that Wight, like Wendt, aspires to develop a scientific IR Theory.

To reach the aforementioned goal, Wight draws heavily upon Bhaskar's CR,[129] whereas, on the contrary, Wendt's SR is not as exclusively Bhaskarian. Moreover, Wendt does not clearly designate himself as a critical realist. Talking of Bhaskar, one may notice that Bhaskar is not very prevalent in the philosophy of science literature. In the second chapter of this thesis, I have synopsized the current debate about SR in the philosophy of science, without being, however, able to trace any reference to Bhaskar. Monteiro and Ruby support the argument that "Bhaskar does not feature prominently in the PoS [philosophy of science] literature" and "indeed, the infrequent references to his work in the PoS are highly sceptical of Critical Realism. See: Ruse (1981), Gunnell (1995), Kivinen and Piiroinen (2004)" (Monteiro and Ruby 2009: 31, fn. 24).

In the same vein, Fred Chernoff argues that SR is not a form of CR but "CR can be conceived of as a very specific, though quite unusual, sort of SR (perhaps as Mormonism can be seen as a specific sort of monotheism)" (Chernoff 2007: 400) and he is right in further claiming that "the vast majority of scientific realists in the philosophy of science would sharply disagree with the core 'critical' tenets of Bhaskar's critical realism (as most monotheists would disagree with the core of Mormonism)" (ibid).

Wight and Joseph argue that "CR is a very specific development of SR within the human sciences" (Wight & Joseph 2010: 2) and that "it is vital to differentiate SR from CR" (ibid: 4). Something which is really surprising,

[129] However, more recently, Wight and Joseph have argued that "one could extend the notion of CR beyond the *narrow* confines of Bhaskar's particular framework" (Wight & Joseph 2010: 2, emphasis added).

however, is that Wight and Joseph appear to ignore the complexities of the ongoing SR debate within the philosophy of science, when they argue that

> SR is a philosophy of science that arguably is the dominant approach now taken within the philosophy of science. In fact the most serious contender to SR is Bass Van Fraassen's 'constructive empiricism', but this approach is fighting a rearguard defence of an empiricist account of science; something that seems to most to be a lost cause (ibid: 4)

Contrary to the above, Chris Brown uses SR and CR as synonyms:

> Colin Wight's attempt to distinguish between the two positions, associating Bhaskar specifically with critical realism, is plausible but seems to me not to tally with general usage, at least in the field of IR (Brown 2007: 409, fn. 1).

To conclude on this issue, I am aligned with Chernoff and make a distinction between these two terms in my thesis, because although Bhaskar's CR is in conformity with several views and intuitions of other versions of SR, such as the ones I have critically presented in chapter 2, it also departs from them in numerous ways.

The next step in this discussion is to point out that Wight does not equate science with positivism. He believes that the declared incommensurability of differing epistemologies and the consequent lack of any 'dialogue' between them is due to the fact that IR theorists - mainly but not exclusively of a positivist vein - used to either prioritize epistemology over ontology or even completely reduce ontology to epistemology. On the contrary, Wight gives ontology a priority over epistemology (that is, we should care more about what there is than how we know it), since he argues that it is in the *historically* and *socially* driven ontological assumptions of the different IR paradigms where the real differences between these paradigms lie. Thus, ontology is primary, whereas epistemology and methodology are merely derivative, and not the other way around. The inherent logic of this position is that since the differing ontologies (for instance, those of Marxism and post-modernism) can be debated, the possibility of a dialogue between the different IR paradigms should not be excluded, meaning that the different IR paradigms are finally not incommensurable and all equally subject to judgmental rationalism. Wight is then an ontological realist ("there is a reality independent of the mind(s) that would wish to come to know it" (Wight 2006: 26)), an epistemological relativist ("scientific realism can be understood as an endorsement of Paul Feyerabend's methodological claim that 'anything goes'" (ibid: 25)), and a methodological pluralist, because he qualifies judgmental rationalism as the appropriate methodology for his IR theorizing ("despite epistemological relativism, it is still possible, in principle, to choose between competing theories" (ibid: 26)).

A critical point should be made here which concerns a subtle but decisive difference between SR and CR with regard to a 'correspondence theory of truth'.[130] It is noticeable that one finds in the philosophy of science clever examples of why one must be skeptical about the SR's endorsement of a notion of truthfulness. It is true that even strict realists among the philosophers of science, like Michael Redhead, identify shortcomings in the way SR justifies the

[130] Suffice it to remind the reader at this point that "correspondence to reality – both in its observable and unobservable elements – is the scientific criterion for theory-choice. The goal of scientific theories is not only empirical reliability, but truth about (correspondence with) all aspects of the world" (Monteiro & Ruby 2009: 31).

notion of truthfulness. Redhead claims that, although he has nailed his colours "to the mast as a realist", still "there are a number of issues that still need attending to" (Redhead 1995: 16). One of the four issues he quotes is as follows:

> Realism seems to require some adequate notion of truthlikeness or verisimilitude. Given that our theories are often discarded, can one nevertheless make sense of approach to the truth? This is a very thorny technical problem in philosophy of science that hinges on the question: What is a theory really about? For example, consider an astronomical theory that predicts the number of planets P and the number of days in the week D. Let one suppose that it gets both these numbers wrong, but gets P + D right. Should this count in assessing whether the theory has got closer to the truth? This is a question to which no totally satisfactory answer has been given. Intuitively P + D is not an interesting or significant quantity to get right, but how do we rule it out on purely logical grounds? (ibid: 19).

Having said that, one notes that the IR scientific realists who draw exclusively on the Bhaskarian notion of SR (namely CR) [131] may be at odds with the idea of a 'correspondence theory of truth'. The reason is that CR distinguishes itself from other versions of SR in that "it explicitly recognizes that the social nature of knowledge means that science will sometimes get it wrong and that scholars must therefore maintain a critical stance toward truth claims (Wight 2006: 39)" (Monteiro & Ruby 2009: 31, fn. 24). The above conclusion does not come without epistemological implications, for

[131] See, for instance, Wight & Patomäki (2000), Patomäki (2002), Kurki (2006, 2008) and Wight (2006).

> critical realism thus promises to combine relativism at the epistemological level (making for pluralism by allowing all sorts of approaches, theories, paradigms, research traditions, etc., to operate side-by-side within a discipline) and realism at the ontological level (continuing to view scientific knowledge as getting at the way the world really works, independent from our efforts to know it). (Monteiro & Ruby 2009: 31, fn. 24)

In a critical mood, Käpylä and Mikkola argue that the attempt to combine the fallibility of human knowledge with the 'getting things right' attitude based on correspondence-like concepts of truth is problematic. Their insistence on this position is evident in their conclusion where they write that "this is the case even when Critical Realism endorses a more relaxed view of (what we called) 'resemblance theory of truthlikeness' that tries to overcome the problems of correspondence theory" (Käpylä & Mikkola 2010: 32).

However, if those IR critical realists who object to a correspondence theory of truth were aware of Worrall's SSR, they would have probably been less reluctant to accept any kind of theory that entails the notion of correspondence to reality. Indeed, as I have claimed in the second chapter (page 109) of the thesis, according to the moderate metaphysics of SSR, "there exists a structured reality of which the mind is a part; and, far from imposing their own order on things, our mental operations are simply governed by the fixed laws which describe the workings of Nature" (Worrall 2007: 153). At the epistemological level the latter implies that "not only is this structured reality partially accessible to human discovery, it is reasonable to believe that the successful theories in mature science – the unified theories that explain the phenomena without *ad hoc* assumptions – have indeed latched on, in some no doubt partial and approximate way, to that structured reality, that they are, if you like, *approximately true* (ibid: 154).”[132] Due to the analysis above, one can clearly see to what misconceptions about key-notions of SR the uncritical adoption of SR by IR may lead.

Next, there is a need for a second argument on Wight’s epistemological relativism to be put forward, since Wight’s endorsement of Feyerabend’s famous methodological phrase, “anything goes”, must be further explained:

> It is only when epistemology swallows up everything, Wight explains, that that phrase connotes a denial of judgmental rationality. Once, however, we separate ontology from epistemology, the phrase takes on a different sense. It means that although any individual research question may be judged rationally in terms of the strongest argument, it is impossible to say in advance which methods apply. Instead of methodological appropriateness being determined a priori by some epistemological foundation, what makes a method appropriate or not is the ontological nature of the object under study. The appropriate methods, therefore, must be decided case by case. (Porpora 2007: 308)

I think this clarification with respect to the meaning of Wight’s epistemological pluralism is of some significance, for other critics (cf. Kořan 2007: 325-26) conceive that Wight’s position with regards to epistemology is that the latter is simply reduced to empirical research. If this were the case, one need not have any concerns about *if and how* epistemology could progress in the future. However, the aforementioned is an ambiguous position to hold. When Wight

[132] This critique also applies to Monteiro and Ruby who, in their attempt to show that SR cannot serve as the founding meta-theory of IR, seem to not be aware of SSR and all the relevant discussion I have presented in the second chapter. Thus they go on to argue that SR “hinges on accepting a highly contested claim: that science grants us access to a mind-independent world” (Monteiro & Ruby 2009: 32).

argues that "which structure does indeed dominate human practices at any given time or place is an empirical question" (Wight 2006: 299) or when he writes that "without taking some things as given, no research would ever get off ground" (ibid: 249), he implies that it is ontology which defines methodology case by case; not that epistemology does not or should not play any role in ontological and methodological considerations. This is exactly the reason why Wight (2006: 249) quotes Hollis in arguing that

> The proper conclusion is that epistemology has to go along way around, visiting arguments about the historical particularity of all ways of searching into and discovering truth but then returning with renewed determination to transcendental questions of how knowledge is possible. (Hollis 1994: 259)

It must be admitted, though, that one cannot but comment that Hollis is so general in his instructions on how epistemology could recover its role in the future that the charge of absolute epistemological relativism in the case of Wight's epistemology has not been yet encountered in a satisfactory way.

Concerning the concept of ontology in Wight's meta-theory, it is worth mentioning that he is interested in two different kinds of *ontology* (which I have already mentioned in the Introduction to this chapter): "*philosophical* ontology, which concerns the relationship between the researcher and the world, and *scientific o*ntology, which concerns the catalogue of things that are taken to exist and hence available to serve as objects of scholarly research (Patomäki & Wight 2000)" (Jackson 2008a: 341).

One should also add that Wight's ontology is an emergentist one in that it is similar to that of Wendt (2003), since they are both Bhaskarian in their origin.

Ontological emergentism holds that at each layer of a stratified reality, properties emerge which are unique and irreducible to the more fundamental entities of the underlying layer.

From the aforementioned discussion of epistemology and ontology as these are both meant within Wight's meta-theoretical framework, one concludes that Wight rejects Wendt's 'via media', for the former is not interested in 'bridging' positivist and post-positivist methodologies. Moreover, Wight argues that the explaining versus understanding debate (Hollis & Smith 1990) belongs to the realm of methodology rather than that of epistemology. What is valuable for Wight's theory in the discussion above, is that the distinction between these two terms is based on ontological considerations about the social world (Wight 2006: 256). Last, what I would like to repeat and emphasize further is that Wight endeavours to reclaim scientific realism from the grip of positivism, given that positivism in the eyes of even his opponents is equated with science.[133]

All the above constitute a brief discussion of the meta-theoretical considerations which constitute the bedrock of Wight's IR theorizing about structures and agents.

Now, I will discuss in more detail the substantive IR theory which Wight unfolds in his *ASIR*. A difference of the outmost importance between Wendt

[133] Similar comments have also been made by Douglas Porpora and Chris Brown, respectively: 1) "In IR as in other social sciences, Wight finds, science and positivism are so equated that challenges to positivism are construed as challenges to science. Thus, as we critical realists know, even positivism's opponents think that in rejecting a positivist approach to human behaviour, they simultaneously are denying the purview of science" (Porpora 2007: 307), and 2) "One of the most important questions to be answered by any of the social sciences, including International Relations, is whether or not it is possible to study society using the methods of the natural sciences, suitably adapted. Those who believe it is are sometimes, and quite wrongly, referred to as positivists, even though they have little to do with August Comte or Ernst Mach; so pervasive has this usage become that that adherents of a scientific approach have partially adopted it, often describing their approach as neopositivist" (Brown 2007: 412).

(1999) and Wight (2006) is that the former used social theory as a platform for exploring important questions such as the mutually constitutive effects of interactions between states, the social structure of anarchy and the issue of structural change. In contrast, Wight placed the agent-structure issue at the center of his social theoretical project. Indeed, in order to study it, Wight brings on board a great deal of sociological theory, ranging from Emile Durkheim, Max Weber and Antony Giddens' 'structuration theory' to Margaret Archer's 'morphogenetic approach'. However, contra Wendt, who relies on Giddens' structuration theory in his attempt to tackle the aforementioned problem, Wight separates CR from structuration theory and recovers "the analytical role of social relations, especially those of some material nature" (ibid: 310). Thus, Wight does not defend the 'ideas all the way down' thesis but rather believes that there is a strong irreducible material basis in some kinds of social facts or phenomena.[134]

The employment of Wight's emergentist ontology along with the aforementioned *social relational* approach[135] to the study of phenomena which emerge at different levels (or layers) of the international system "allows for incorporating the material dimension, inter-subjective actions, social relations and pure subjectivity into a realist account of social ontology" (Kořan 2007: 324-25); in other words, according to Wight, there are the aforementioned "four planes of activity". Next, Wight distinguishes three types of agency:

[134] This is probably due to the fact that Wight and other critical realists are 'faithful' to the Marxist origin of the Bhaskarian ontology.

[135] Moreover, it is very interesting that even in the Philosophy of Physics and Geometry, *relationalism* is recently enjoying renewed attention among philosophers and physicists who concern themselves with ontological questions about the relationship between space and matter. For instance, *relationalists* deny that space is ontologically prior to matter and seek to ground all claims about the structure of space in facts about actual and possible configurations of matter – see Belot (2011).

intrinsically subjective, inter-subjective and structure driven. Given the aforementioned, it is rather a matter of the empirical research to judge which of these three types of agency prevails (one notices that epistemology seems to be reduced to empirical research, a point which is debatable as I have already argued). Given also that agency is "embedded within, and dependent upon, structural contexts" (Wight 1999: 109), but not reducible to structure, none of these four levels of analysis is the most appropriate one for the study of the agent-structure problem in IR *in general*. Consequently, since each time one should choose the appropriate level of analysis in accordance to the *specifics* of the case under examination, one cannot come up with a *definite* solution (one that holds for all cases) of the agent-structure problem in IR.

Kořan reveals the following 'flaw' in the logic of the above 'emergentist solution' to the agent-structure debate, which seriously challenges SR's assumption about the independent existence of the observed entities and has, also, further serious epistemological implications:

> By asserting that behaviour bears an imprint of structure while it is the agent alone who decides to act, it indeed might follow that it is but an empirical question of the relative autonomy of the agent. Yet, Wight himself is ambivalent on this issue. At one point he insists that 'social objects are as potentially impervious to the wishes of individuals as are objects of the natural world (…) [h]ence it is misleading to think that [its] importance depends solely upon whether they enter the minds of lay actors' (p. 271). On the other hand, Wight explicitly denies this view (p. 222) and argues that the extent to which structures are actualized 'will depend upon the action, reaction and interaction of specific agents' (p. 220). In both cases, whether the structure dominates over agency, and whether it independently exists at all, is

> not the researcher's 'empirical question' but a matter of agential (in)abilities, and thus solely a subjective factor. This poses a serious challenge to an attempt to reconcile structuralism and agency under the realist roof, unless we treat the 'structural' emergent layer merely as an epistemological/heuristic device. (Kořan 2007: 325)

On the other hand, Brown and Ainley welcome the fact that, thanks to Wight, one can avoid getting lost in the realm of abstract theorizing about the agent-structure problem and instead use only empirical analysis in order to establish the causal significance of *particular* agents and *particular* structures in *particular* situations:

> This is anathema to theorists such as Waltz, who want to simplify the study of IR, but does seem to be the most reasonable way to think about agents and structures, and, very importantly, it pushes us back towards real-world problems, and away from those more abstract questions that are best dealt with by philosophers of science rather than scholars of International Relations (Brown & Ainley 2009: 70)

I rather agree with Brown and Ainley that the solution to the agent–structure problem which Wight suggests brings one back to real-world politics (and this is really important since IR's subject matter is primarily real-world politics, and not sociological theory or philosophy). However, what is highly problematic is not only the uncritical introduction of abstract theoretical terms belonging to other disciplines into IR, but, first and foremost, the fact that IR seems to follow a trend that has also been torturing other social and political sciences. This trend consists in the extensive concern with the philosophical foundational problems of these sciences, at the expense of the development of substantive theories. For instance, Nicos Mouzelis, who criticizes the existence of this same trend in sociological theory in his book, *Back to Sociological Theory* (1991), devotes a whole chapter ("Philosophy or Sociological Theory?") to its discussion and argues, among others, that

> […], it remains possible as well as desirable to distinguish between a predominantly philosophical and a predominantly theoretical-sociological analysis…Even if sociological theory must always entail philosophical presuppositions, it does have its own autonomy and logic. Conflating and thus abolishing the distinction between epistemological/ontological and sociological-theoretical issues proper therefore destroys the latter's specificity and relative autonomy – resulting in analyses which, despite the numerous insights and brilliant comments they provide on particular theories, often lead to misleading or inadequate conclusions. (Mouzelis 1991: 12-13)

The above should also hold for IR in the sense that, if one substitutes IR for sociological theory, then one gets a concrete depiction of the state of IR after it has been invaded by meta-theoretical concerns.

Furthermore, in this abstract Mouzelis replies to those who, like Wight and Joseph, argue that one of the many good reasons that the social sciences must engage in philosophical reflection is that they need to assert their scientific status, because they are not so developed as the natural sciences, which "are often said to have only fully matured when they left philosophy behind and established themselves as autonomous modes of knowledge generation" (Wight & Joseph 2010: 5).[136]

Taking a step further, I would like to argue that, by taking literally Wight's suggestion that one should take refuge in empirical research if one wants to see how the relationship between agent and structure is shaped in each different case, one risks concluding that there is no need to theorize about agents and structures at all. My point, however, is that although Wight and Wendt have heavily drawn upon both the philosophy of science and social theory in order to build their own IR theories, they have put more emphasis on the former than on the latter. Therefore, Mouzelis may be right when he argues that the constituent elements of each of the aforementioned "four planes of activity", which are suggested by Wight, are very poorly theorized because

> Wight's emphasis on ontology and his lack of emphasis of the theoretical tradition that Parsons initiated in the post-war period – a tradition which theorizes in a rigorous and logically *coherent* manner such basic concepts as values, norms, institutions, etc… – has led him to an *ad hoc* type of theorizing. (Mouzelis 2008: 226, fn. 3)

Therefore, it is my opinion that those IR scholars that are interested in the agent-structure debate should continue theorizing about it by drawing upon sociological theory. One could, for instance, take on board Mouzelis (2008), and

[136] The assertion that natural scientists believe that they do not need philosophy of science in order to have their work done is not very accurate, since nowadays one observes an increased interest of physicists in the philosophy of science and especially in the ontological problems which are posed my modern physical theories, such as QM, relativity, quantum field theory and string theory. Thus, one could argue that physicists have become more interested than they have ever been before in the works produced by philosophers of science on the foundations of scientific theories and vice versa (see, among others, Healey 2009, Callender & Huggett 2001, Kuhlmann et al. 2002, Dieks 2006, 2008, etc.).

his most recent reflections on the agent-structure issue.[137] Mouzelis' work reveals that apart from Parsons, Giddens, Bhaskar, Habermas and Archer, there are also important – albeit not so well known beyond the borders of their own science – sociologists, like Alexander and Lockwood, whose works should be carefully considered in the context of the study of the agent-structure issue. In the second chapter of the thesis, I have gone for a rather detailed discussion of SR as it has been deployed in the philosophy of science literature. I considered that to be a necessary task given the eclectic and uncritical use of SR in IR. I could have done exactly the same by providing an analytical discussion of the agent-structure debate within sociological theory in a separate chapter, however such an endeavour would have been a distraction from my work, since the subject matter of this thesis is SR and not the agent-structure debate *per se.*[138] However, one should bear in mind that the discussion of the agent-structure issue in sociological theory is far richer and more sophisticated than it has been presented within Wendt's and Wight's works. Moreover, my emphasis on Anglo-Saxon sociological theory is not accidental; the purpose of thinkers who belong to this tradition has been to clarify conceptual tools and to construct new ones *by following criteria of utility rather than truth* (this may also be useful to a pragmatist turn in the study of the agent-structure debate within IR):

[137] See, in particular, "Part V: Towards a non-essentialist holism" of Mouzelis (2008).

[138] For a very clearly written overview of the agent-structure debate in IR (which is accompanied by a good bibliography to start with), see chapter 4 of Brown & Ainley (2009). To this I would add Ainley (2006), an excellent piece of work where the author is not caught up in perplexing meta-theoretical debates but, on the contrary, relates her work in international political theory to real problems in international political practice and, by emphasizing agency, explores alternative views of it. Thus, she comes up with outlining "models of agency as sociality and responsibility as a social practice, arguing that these models both better describe the way we talk about and experience our social lives, and also often offer significant possibilities to broaden the scope of international justice and enable human flourishing" (Ainley 2006: 3).

> Viewing pragmatism in the manner of C. S. Pierce – i.e. as a method for the clarification of concepts by showing how they are or can be used – one might argue that sociological theory, as developed by Parsons and others*, has as its major task to clarify current conceptual tools and to construct new ones by following criteria of utility rather than truth*. Adhering to this orientation, I have kept clear of the type of ambitious theorizing that purposes to provide substantive universal propositions either in the form of 'laws' and contextless generalizations, or in the form of philosophical analysis on the ontological nature of the social, the possibility of social knowledge, the constitution of the subject, and so on. (Mouzelis 1995: 8-9).

This way of theorizing leads to the development of middle-range theories, as opposed to grand theories of the type Waltz, Wallerstein and Wendt provide. It indicates a possible re-orientation of IR theorizing; this may happen if one seriously considers that middle-range theories come up with certain research proposals for empirical test/falsification of the utility of their conceptual tools, in contradistinction to the fact that neither Wendt nor Wight have yet generated any empirical (note: not empirici*st*) research programme in the field (cf. Griffiths, Roach & Solomon ([1999] 2009: 156-57)).

Going back to the discussion of agent and agency in *ASIR*, I would like to draw attention to how Wight addresses the question about whether a state can be conceptualized as a person in its own right. As Porpora claims, given that "we know from Davidson that the mark of agency is intentionality and from Anscombe that intentional action can be understood as behaviour done for reasons" (ibid: 311), one could further argue that states seem to behave for reasons and should, therefore, be personified. However, Wight – and I sympathize with his view – disagrees with the personification of the state by counter-arguing that reasons and intentions imply a background of mental states and this, in its turn, implies that the state should be taken to be a 'superorganism' with 'collective consciousness'. This is a conceptualization with which he cannot come to terms. On the contrary, Wendt has put on the table and defended, first, the idea of 'superorganism' with 'collective consciousness' (in order to argue for the allegedly inevitable development of such a 'superorganism' as the international system to a world state – see Wendt 2003) and, second, the idea of the personification of the state (Wendt 2004). Wight takes issue with Wendt's concept of 'collective consciousness' and proposes a different one, that of 'collective intentions'. He does not, however, elaborate further on this concept (cf. Porpora 2007: 312). In Wight's own words:

> Wendt's adherence to 'state-as-agent' thesis, however, is an endorsement of structuralism at the level of the state. As Bhaskar puts it, 'nothing happens in society save in virtue of something human beings do or have done. What this means in relation to theories of the state is captured nicely by Bob Jessop, who argues, '[i]t is not the state which acts: it is always sets of politicians and state officials located in specific parts of the state system'.[139] In Wendt's theory states not only take the place of persons, but actually are persons. That is, states play the role of human beings for Wendt. But states, even if they are agents, are not persons. On, the contrary, states are institutional structures constructed by human beings. (Wight 2006: 187-88)

[139] See Jessop (1990: 367).

To put it in a nutshell, Wight argues that the state should be seen as an ensemble of structures rather than an individual agent. Brown and Ainley summarize his argument along the following lines and make the additional point that FPA should have much sympathy with Wight's conceptualization of the state:

> He [Wight] emphasizes that the way that state power is actualized depends on the actions of agents – individuals and groups – located in groups within the ensemble of the state (Wight 2004: 279). So, for Wight, the state is not an agent in itself, and certainly not a person, but rather a body that facilitates the exercise of power by the agents within it, a position with which foreign policy analysis (FPA), … , should have much sympathy. (Brown & Ainley 2009: 76)[140]

Making three further comments will complete my discussion of ASIR:

First, Wight neither provides us with certain examples of how his various accounts of structure and agency could lead to certain empirical research questions, nor tells us what kinds of normative or analytical conclusions we could arrive at through the realization of these accounts. Bjola is right when he wonders about "what kind of norms, institutions, or social identities follow from the conceptualization of structure or agency along the lines suggested by Wight" (Bjola 2007: 317) and poses the following plausible questions:

> Does an account of structure as 'law-like regularities' entail a configuration of interests and identities that is analytically or normatively different from a view of structure as 'relations of difference'? Similarly, does a conceptualization of agency as 'positioned-practice places' favor different outcomes than a conceptualization of

[140] Indeed, though FPA should avoid disaggregating so much that the state loses all definition and the assumption of agency qua state.

> agency as 'freedom of subjectivity'? If the answer is 'yes' in both cases, then the key questions (how and why) are left unanswered by Wight. (ibid)

In the same vein, Lawson in his book review of *ASIR* argues that "if the better probing of empirical puzzles is a core dimension of Wight's agenda, it is a shame that he has contributed relatively little to them here" (Lawson 2007: 774-75) and expresses his fear that "Wight's extensive underlabouring" may "become a means for meta-theoretical bunkerists to spend even less time on the empirical front-line" (ibid).

Second, it sounds rather odd that Wight, towards the end of *ASIR*, claims that he does "not intend to outline a general theory of international relations" (Wight 2006: 294), although this is exactly what he appears to have done throughout the whole book (Floyd 2007: 188). The logic behind this argument is rather simple: given that in the Bhaskarian version of SR it is ontology which determines both epistemology and methodology, Wight's commitment to ontological realism suggests that he aspires to build a general theory of international relations upon it.

Third, Wight assigns to science an emancipatory role and a practical task; that is, Wight aspires to provide a scientific IR that will help us understand and analyze the fabric of the international system properly. Such an endeavour might make this system's shortcomings surface and thus encourage and facilitate action in order to improve the way the international system works nowadays. However, what differentiates Wight from Wendt is the former's strong emphasis on his argument that philosophical choices are finally political. Onuf is clear with regards to this feature of Wight's *ASIP*:

> Yet Wight's conviction that philosophical choices are finally political gives the book a polemical feel; it was soon followed by an exchange between Wight and Kratochwil that left some readers wondering if their differences warranted so much posturing.[141] (Onuf 2012: 25)

I will conclude my discussion of Wight by drawing attention to the following three features of CR: first, the goal of science is to describe how things really are, and, second, there is a world out there, which is independent of our minds and our ability or inability to know it. Third, this world is inhabited by both observable and unobservable entities, which are the objects of science. The existence of unobservable entities is inferred from their observable effects; therefore, unobservable entities are described with reference to their effects. Finally, causation is real, and not simply the product of human thought (on the contrary, according to any traditional empiricist, we are free to talk about the existence of a 'lawlike regularity' but not about the existence of a 'cause') (cf. Brown & Ainley 2009: 58).

4.3.3 Critique – Opponents of SR

Turning to the critics of SR, Buzan believes that "in line with the current fashion in IR Theory, it is the philosophical side of Wendt's theory that has

[141] I suppose that Onuf refers to an exchange Kratochwil and Wight had had about epistemological issues in 2007; Kratochwil (2007a) had kicked off with his plea for "a pragmatic approach to theory building", Wight (2007a) replied by arguing that "inside the epistemological cave all bets are off", Kratochwil (2007b) came back with a paper having the telling title, "Of communities, gangs, historicity and the problem of Santa: replies to my critics" (his other critics have been Lebow and Suganami) and Wight (2007b) closed this rather "vicious circle" – besides the sophisticated arguments which both sides have put on the table - with the equally telling title, "A response to Friedrich Kratochwil: why shooting the messenger does not make the bad news go away!".

attracted the most comment" (Buzan 2006, xv). This explains the fact that the majority of the attacks which have been launched on *STIP* focus on SR, namely the meta-theoretical bedrock Wendt has built his substantive IR Theory upon. In other words, there are two distinct trends as to the critical examination of Wendt's *STIP*. On the one hand, those IR scholars who are genuinely committed to the study of SR usually "believe that progress in IR Theory can only be achieved by getting the whole enterprise onto a firmer foundation in philosophy of knowledge" (Buzan 2006, xvi). On the other hand, those who consider the debate on the philosophical foundation of IR to be not so decisive for overall progress in IR Theory, focus more on the theoretical rather than the philosophical aspects of Wendt's *STIP*. I will focus on the critics of SR/CR as an IR meta-theory, without, nevertheless, neglecting to track the implications of the possible strengths and weaknesses that SR might entail for the development of substantive IR theories. Namely, I shall be examining some of the main IR critics of SR/CR such as Suganami, Kratochwil, Jackson, Chernoff and Lebow, with a view as to how they see SR/CR as both an IR meta-theory and a basis for first-order IR Theory building.

Friedrich Kratochwil, a leading constructivist with a very wide and solid philosophical background, attacks Wendt's SR on many fronts. The most crucial issue he raises is whether or not Wendt's version of realism is compatible with constructivism as a meta-theoretical orientation. According to Kratochwil, Wendt seems to neglect the *social* component of SR, which consists in science being conceived "as a communal practice rather than as a simple finding and 'testing' of theories" (Kratochwil 2000: 36). This is even more surprising since Bhaskar, upon whose work Wendt relies, sees *talking and* *debating scientific issues* to be a part and parcel of the process through which scientific progress is achieved.

Furthermore, as discussed elsewhere in the thesis, the three basic premises of SR are the following:

1) the world is independent of the mind and language of individual observers;
2) mature scientific theories typically refer to this world;
3) even when it is not directly observable. (Kratochwil 2000: 37)

Kratochwil sees no problem in considering "unobservables" to be (natural or social) entities, since "that there are 'unobservables' in every theory is hardly controversial, strict empiricists excepted" (ibid: 38). Moreover, Kratochwil holds, that "hardly anyone – even among the most ardent constructivists or pragmatists – doubts that the 'world' exists 'independent' from our minds" (ibid: 38). The problem is how we can come to know this independently existing word. According to Kratochwil, one should take on board that this world cannot be recognized and conceived in its totality in a pure and direct fashion, i.e. without any 'description'. We do not know, he goes on, whether "what we recognize is already organized and formed by certain *categorical* and *theoretical* elements" (ibid: 38, italics are mine). This leads us directly to the Kantian notion of "thing in itself". A "thing in itself" remains unrecognizable and unknowledgeable unless it is brought under a description, something which brings us to the following two significant questions:

> First, whether this 'naming' is indeed a function of the congruence of our concepts and the 'things', as this provides the yardstick for 'truth'; or, second, whether 'truth' is a matter of the conditions governing the justifiability of assertions rather than a matter of the 'world' (ibid: 38)

In his attempt to address the above two questions, Kratochwil argues that "things in themselves" are knowledgeable only in the context of the social practices that make the things into what they are called or referred to; consequently, 'truth' is not a matter of the world. Given that Kratochwil questions "the existence of 'truth', along with 'science' as method of arriving at it" (ibid: 43), one comes to conclude that the search for a foundational meta-theory in IR is infeasible and meaningless. As a further consequence, SR is also incompatible with the kind of constructivism Kratochwil defends. Next, in order to avoid the accusation of relativism or even nihilism (although such an inference would be unjustifiable, given that the aforementioned critique concerns and applies only to a very dogmatic epistemology and concept of science), Kratochwil takes refuge in a wider conceptualization of science, in which the use of the notions and rules of logic (bivalence or rational principles, deduction, etc.) are not sufficient to help one grasp the reality in all its aspects. After all, Kratochwil argues, the "finality and legitimacy of a judgment coincide only in logic" (ibid: 44). In his attempt to provide a solution to this deadlock, he brings ethical principles into play that interpret the question of decidability as one of a fair procedure, which provides an honest approach to scientific debates as well as an honest judgment about the existence or not of scientific progress. I think this is a remarkable point inasmuch as Kratochwil reminds us that even the late Popper moved towards accepting that purely logical criteria are not sufficient to guarantee truth and scientific progress. Instead, a conduct of rational discussion among scientists that should be based on certain ethical rules must be introduced.[142]

This is exactly the *social component* of the scientific progress, which I have mentioned at the beginning of my presentation of Kratochwil's perspective of science; it is in the lack of this feature that Kratochwil identifies one of SR's major shortcomings.[143]

Kratochwil's piece shows clearly how differently – sometimes even idiosyncratically – Wendt's SR is conceived by his critics. Two examples should suffice to make this point more eloquently:

First, in Wendt's seminal (1987) paper, where SR is introduced in the study of international relations for the first time as the proper meta-theory for the concerns of IR, the examination of the arguments *against* SR is rudimentary, if not entirely absent. This makes for a lop-sided presentation that obscures both the strong claims SR makes, as well as the process in which its conceptual resources can be employed to defend SR from anti-realist attacks. Perhaps this poor portrayal of SR explains why a lot of Wendt's critics and commentators (e.g. Suganami; see my second example) find SR unnecessary, even though they accept most of the traditional SR claims.

The second example I have in mind is where Suganami (2002) criticizes Wendt, based on nothing more than personal intuitions and opinions, instead of an actual critical analysis. This leads to a variety of rather simplistic claims. For

[142] Look at Kratochwil (2000: 39). A similar point to Kratochwil's, however not identical, is made by Lebow when he argues that "Popper,…, ultimately came to understand science as an ethical practice and rejected altogether the idea that philosophy could provide truth warrants" (Lebow 2011: 1219).

[143] For a reply to the Kratochwil's critique of Wendt, from the perspective of CR, see Morgan (2002). Morgan argues that Wendt's defence of SR "is relatively untouched by Kratochwil's critique, since that critique is constructed in terms of empiricist conflations" (Morgan 2002: 118).

instance, Suganami claims that "Of course, all our scientific assertions can be construed *realistically*, that is, as involving ontological commitment on the part of those who make such assertions" (Suganami 2002: 27, italics are mine), but this cavalier attitude toward SR misses the fact that the realism debate is exactly about *how* to cash out assertions such as the one above; that is, explaining what 'realistically' in fact means. In the same vein, Suganami writes that "scientific assertions can be said to depict the world as it is independently of our knowledge claims only if they are *not wrong*, and we can never be sure which, if any, scientific assertions are of this sort" (ibid: 27). The aforementioned extract seems to indicate that Suganami has not heard of 'approximate truth claims' and the fact that the wrongness of a *scientific* theory comes in degrees. Suganami concludes that

> In the end, I wish to remain agnostic as regards the real (that is, knowledge-independent) existence of theoretical entities invoked in the currently best explanation of nature. In fact, I do not understand why it is necessary to go beyond simply acknowledging that a particular entity is invoked in the currently best scientific explanation and that implicit in the idea that this entity *explains* nature is the supposition that it exists independently of our knowledge. (ibid: 27-8).

A careful examination of the aforementioned claim reveals that it begins with Suganami being a constructive realist, but ends up with Suganami as a full-blooded scientific realist. Similarly, he insists that he remains agnostic with respect to the existence (or not) of unobservable entities, when he takes issue with the notion of causation in Wendt's IR theory, in order to argue that all we need to do "is to point to the causal potentials of collectively held ideas, to which the discussion of the reasonableness or otherwise of the belief in the knowledge-independent existence of certain subatomic particles would seem to be irrelevant" (ibid: 30). With regards to this argument I am aligned with Gofas' criticism "that remaining agnostic on the subject is a luxury we cannot afford" (Gofas 2007: 70), mainly because "*the criterion we invoke for establishing the reality of unobservable entities is decisive for the question of where does causality reside*" (ibid: 70).

Moreover, I do not think that Suganami has properly understood some crucial components of CR. For instance, with respect to *causation*, which consists in *constitutive explanation*, Suganami claims that "in short, a causal relationship has to do with mechanistic coming about or intentional bringing about of an effect-event (under a relevant description), whereas a constitutive relationship has to do with making a particular description of something logically possible" (Suganami 2006: 67). This description of *constitutive explanation* is inaccurate, as can be seen by the analysis on *generative mechanisms* in Chapter 3. However, because it is an issue which is important for the study of structures in IR, it is important to highlight the main differences between a *causal relationship* and a *constitutive* one. Yalvac (2010: 170) quotes Sayer (2000: 11) in noting that "Bhaskar makes a distinction between three levels of reality: the actual, the empirical and the real". Moreover, "given the above distinction, events are caused by structures coming into different 'contingent relationships' with each other 'to co-determine the occurrence of events, thus breaking the one to one relationship between structures and events found in the closed system of scientific experiments'" (Yalvac 2010: 170).[144]

As mentioned above, the aforementioned distinction between the ontologies of empirical realism, constructivism and SR applies to the study of structures in IR. The *empirical realism* of both *realist and neo-realist IR theories* enables us to consider states as "observable objects (or units) with directly given material interests that they seek to advance as relatively unified subjects against other states (units)" (Jessop 2010: 187). The *social constructivism* of *IR constructivist theories* does not only take on board that "social practices are constitutive of social relations, but also commits the 'epistemic fallacy' in assuming that reality corresponds to the knowledge we have about it" (ibid: 187). Then, SR makes the difference in that

> It…endorses a modified social constructivist position in recognizing that the social world is always-already meaningful but qualifies this through the claim that there are complex material as well as discursive mechanisms that shape the variation, selection and retention of ideas, concepts and practices. This implies that, while all ideas are equal, some are more equal than others. (Jessop 2010: 187)

If one were a critical realist and would like to make a general comment on the nature of causation in terms of CR and its ramifications for IR theory, one would have to argue, along with Kurki, that

[144] A similar, perhaps even clearer, description of the ontology of SR, from the perspective of CR, is given by Jessop: "In contrast, SR is committed to a depth ontology that distinguishes in general terms the levels of the real (causal mechanisms that include tendencies, counter-tendencies, capacities, liabilities, etc.), actual (the product of the interaction among a plurality of mechanisms in specific conditions) and empirical (observations concerning the actual and/or the real)" (Jessop 2010: 187).

> In the case of causation, critical realism, through its particular non-positivist insistence on the ubiquity of causal forces in natural and social spheres, challenges positivist assumptions on science and causation in IR and directs IR theorists towards more holistic, reflective and methodologically pluralistic causal theories. It also forces post-positivists away from simplistic rejection of the language of science and causality and, in fact, points out that many of their analyses provide a way forward for IR theoretical causal analysis. (Kurki 2007: 377-78)[145]

Returning to Suganami, one might claim that, despite the fact that his comments on SR are based on a misunderstanding of its premises and an ignorance of the context within which it has been deployed in the philosophy of science literature, he does succeed in his critique of the use that Wendt makes of SR in defending the belief in the reality of the state. Suganami argues that it is unnecessary to invoke SR in order to defend the argument that states really exist, since in order to do that all that Wendt needs to do is to point to the "causal potentials of collectively held ideas" (Suganami 2006: 64)[146]

Moreover, Suganami is right in arguing that Wendt in his attempt to *prove* the existence of a constitutive relationship between social structures and the agents' identities and interests - and thus *disprove* both *individualism* (the alleged method of neorealism) and *holism* (the alleged method of the world-system theory) as appropriate methods for IR theorizing - comes up with nothing more than a number of *causal* narratives. His concluding comment that Wendt "could not have done otherwise" (ibid: 69) is not to be bypassed, given

[145] On the issue of causation in terms of CR and its implications for IR theory, see also Kurki (2006) and Whitham (2012) but, first and foremost, Kurki (2008). Furthermore, for a defence of the need for a multicausal analysis in the study of foreign policy, which is basically rooted in CR's ontology, see Eun (2012).
[146] However, one may argue that this applies just as much to the idea of the state itself, as to that of international society, etc.

that Wendt has not yet provided us with a substantial empirical research proposal, which could lead to "novel verifications" of his own theory of causation. Furthermore, according to Suganami (ibid: 71), another weak spot in Wendt's theory is that it is permeated by an ambivalence as to whether it will concede to individualism, via the claim that unlike rocks, human beings are intentional agents that "exist partly in virtue of their own thoughts" (Wendt 1999: 181), or not. Overall, it can be said that on the one hand Suganami has failed in his critique of SR from a philosophical point of view but, on the other, he has made sharp-witted comments on SR's application to substantive IR theorizing.

Stefano Guzzini and Anna Leander have not grappled with SR very systematically. Nevertheless, they have made the valid point that Wendt's version of positivism - a "sophisticated positivism" as Wendt himself designates it - shares, notwithstanding Doty's claim about the opposite (Doty 2000), almost nothing with positivism (as it is usually understood in IR), apart from the assumption that there is in principle no difference between the natural and social sciences. This "sophisticated positivism" holds that all observation is theory-laden and scientific theories are tested only against other scientific theories and not against the world itself; a situation which renders the search for secure foundations of knowledge impossible (Wendt 2000: 73). It is important here to single out the claim by Kratochwil (2006) who has aptly observed that Wendt's idea of SR seems not to be compatible with the aforementioned position according to which one cannot test scientific theories against the existing world (Guzzini & Leander 2006c: 79, fn. 4). However, Guzzini and Leander argue that "Wendt's project is best understood as a grand synthesis than a *via media*" (ibid:

91), since it makes divergent positions (e.g. positivism and interpretivism) *complementary*, "by reconfiguring them under a new encompassing framework" (ibid: 91).[147] Therefore, it is even more interesting that they underscore that "it comes as no surprise that Wendt's present project is to use the idea of *complementarity in quantum theory* – a post-positivist natural science, as it were – as a basis of social science" (ibid: 91). It is obvious that Guzzini and Leander believe that *the idea of complementarity* is a permanent feature of all versions of the Wendtian IR Theory. As already mentioned, I will deal with Wendt's ambitious new project towards 'a quantum social science' in chapter 5 but, for the time being, I cannot help but highlight the fact that quantum mechanics is by no means a "post-positivist natural science". Even more so, to compare Wendt's encompassing IR Theory with the way Einstein embedded Newtonian physics, as Guzzini and Leander do (2006c: 73), can be at best described as a bad type of science fiction.

Furthermore, Jackson (2008a) has some important points with regard to the distinction that Wight makes between philosophical and scientific ontology.[148] In the presentation and discussion of Jackson's stance vis-à-vis Wight's CR, which follows in the next few pages, I come back to some philosophy of science issues which I have already discussed in chapter 2. This is necessary in order to show that, although Jackson (2008b, 2011) and Wight (2006) deal extensively with philosophy of science in their works, they don't grasp the complexity of the

[147] Contra Guzzini and Leander, Milan Brglez claims that, due to his SR, Wendt "rescues" positivism. If he is right, the problem lies in that SR is then incompatible with "thick constructivism" and other post-positivism alternatives. At the end, Brglez argues that, "because of (his) scientific realism, IR theorists are better off engaging (not ignoring) him (Wendt)" (Brglez 2006: 354).
[148] In case one is interested in a very detailed analysis and critique of SR by Jackson, they can read chapter 4 ("Critical realism") of Jackson (2011); cf. Jackson (2008b).

relevant discussion as it has developed in the relevant bibliography. This is something that has crucial consequences for the introduction of SR/CR in IR.

First, Jackson argues that one can ascribe to only one of these two ontologies, instead of the whole package. To put it differently, if one does not want to accept both philosophical and scientific ontologies, one can either accept Wight's ontology of agents and structures without accepting his philosophical ontology of a mind-independent reality, or the other way around. Second, as has already been discussed, Wight's position is against the 'epistemic fallacy' of reducing what is to what is known (based on the assumption that all we take to exist must be knowable to the human mind). In Wight's view, "statements about being cannot be reduced to, and/or analyzed solely in terms of, statements about our knowledge of being" (Wight 2006: 28). Jackson (2008a) points out that by arguing the above, Wight is led to conclude that the existence of unobservable entities (like quarks or social classes) is "not dependent upon their specification in any theory" (Wight 2006: 32). However, one should bear in mind that, as it stems from my discussion in chapter 2, according to the epistemic thesis of either SR or SSR (Worrall's moderate version of SR), only entities (both observables and unobservables) which are posited by mature and predictably successful scientific theories, or, at any rate, entities very similar to those posited are taken to inhabit the world. In other words, the existence of entities is dependent upon their specification in a theory (in contradistinction to what the Bhaskarian/Wightian SR holds).

This verifies the claim of the previous subsection according to which Wight prioritizes ontology over epistemology, since he does not commit to any particular set of posited entities but, instead, it is the empirical research and subsequent evidence that in each distinct case decides about the existence or not of the posited entities. Jackson also makes the point that if the posited entities are not taken as objectively existent, as philosophical realism demands, then they could be simply regarded as "useful analytical postulates", since in this case one could leave the question of their existence aside (Jackson 2008a: 342-3).

Wight provides three reasons as to why key theoretical entities and generative mechanisms should be taken to be real and not merely useful:

(1) SR corresponds to scientific practice better, since scientists organize their experiments by putting them to the test; in Wight's own words, "Scientific realism makes intelligible what scientists do" (Wight 2006: 24). Wight has made the same point in his "manifesto for SR in IR" where he argues that "the starting point for understanding SR is to realize that it is, first and foremost, an account of what scientists actually do in their practice" (Wight 2007: 382). Chernoff rightly counter-argues that "this is quite far from a philosophical doctrine of SR" (Chernoff 2007: 401). Jackson's comments that he is not sure "why the observation that scientists probe and test their theoretical postulates necessarily entails realism" (Jackson 2008a: 343) are in the same vein, given that even anti-realist approaches such as causal conventionalism (Chernoff 2005) can make intelligible what scientists do. Moreover, he holds that "even if it were incontrovertibly the case that scientists treated their theoretical posits *as* if they were real, this would say nothing whatsoever about whether those theoretical posits were in fact real – or about whether scientific progress in some sense *depended* on the search for real-but-unobservable entities and mechanisms" (Jackson 2008a: 343). A proper examination of the above

statements necessitates extensive argumentation in terms of analytic philosophy and goes well beyond the scope of this thesis. Anyway, there are four initial points to be made on the above.

First, Wight's thesis is a rather controversial one. Were it for a realist like Psillos (1999), he would have claimed that if realism were about science practice, then it would have been an empirical view and could be therefore falsified. However, other philosophers of science, like Giere,[149] argue that realism is in fact about science practice and scientists are realists in practice. One may counter-argue that even this view is problematic, because the practice underdetermines the philosophical stances. Therefore, it may probably be best to argue about realism directly. This can be done by arguing, for instance, that taking theories to be true is the best explanation of their success. What an IR critical realist like Wight might need here is an account of *success* concerning IR theories as well as an analysis of existing theories that are successful.

Second, I would like to draw attention to my presentation of both SR and its alternatives in the second chapter of this thesis. The arguments presented there allow me to claim that apart for Chernoff's conventionalism – which draws on Duhem ([1908] 1969) - there are also other anti-realist alternatives to SR. Consequently, it is uncertain why one would have to choose Chernoff's conventionalism instead of one of its alternatives.[150] I personally believe that those social scientists, including IR scholars, that are interested in the philosophy of science, but do not adhere to the positivist epistemology, are

[149] I have referred to his works (Giere 1998, 2005) in chapter 2.
[150] In Ellie Zahar's *Poincaré's Philosophy: From Conventionalism to Phenomenology* (2001), "Appendix IV: Ramseyfication and Structural Realism", which is written by Worrall and Zahar (Zahar 2001: 236-251), contains the conventionalist ideas on which Worrall has mainly based the argumentation which he has presented in his most recent and updated work on SSR (Worrall 2008).

rarely enchanted by the role Mathematics play in the structure of scientific theories. Moreover, they do not investigate its philosophical significance and, as a consequence, have never paid any attention to Poincare's *Science and Hypothesis* ([1905] 1952), as, on the contrary, Zahar and Worrall (2001) have done. Instead, they usually (and almost unavoidably) draw on the *general* philosophy of science literature, and do not try to follow *technical* philosophy of science works on QM, General Relativity or Quantum Field Theories. The latter, however, presupposes an understanding of the mathematical frameworks of these theories, and since this is the case, it is not accidental that it is in this kind of works – see, for instance, Cao's *From Current Algebra to Quantum Chromodynamics* (2010) and *Ruetsche's Interpreting Quantum Theories* (2011) – where one can trace a rather strong influence of Worrall's SSR. At this point, one may plausibly wonder about the value of familiarity with the advanced knowledge of Mathematics and Physics which is required in order to understand technical philosophical works which are concerned with the foundations of physical theories that are at the very forefront of both theoretical and empirical research in natural sciences. The answer is that normally one should not be preoccupied with them. However, such a preoccupation becomes necessary, from the moment that some philosophically inclined IR scholars, like Wight, suggest that IR should endorse SR as its meta-theory, *since it describes more accurately the way scientists work* (*and the way social scientists ought to work*). One must point out that other IR scholars, like Jackson, disagree and argue that Chernoff's conventionalism could probably explain better than SR how in fact scientists work. With respect to the above, I am uncertain on whether philosophy of science can explain better than science itself how scientists work.

However, if one finally (be it right or wrong) resorts to the philosophy of science in order to address this question, then one must be familiar with *technical* philosophical works such as the aforementioned ones. If this is not the case, it is difficult to have a comprehensive picture of what SR or SSR or different anti-realist alternatives *stand for*.

Third, I think Chernoff makes a good point when he argues that "Wight's preference that scientists hold 'realist beliefs' about the unseen world of theoretical entities is an example of what Arthur Fine calls 'motivational realism'; *it neither implies nor is implied by SR*" (Chernoff 2007: 402).

Fourth, I must accept that CR seems compatible with the type of entity-realism, which ties more with experimental practice and avoids the question of theoretical truth. However, in chapter 2, I argued that this position would be difficult for a realist to adopt, since it is theories that provide the realist with knowledge of any properties these entities have and allow for the interpretation of experimental results.

(2) Taking theoretical entities to be real motivates further empirical investigation, in the sense that "if scholars are only using theoretical terms, like 'structure', instrumentally, then 'there is little need for them to make clear how to use the term' since the only issue is whether the 'postulated term helps explain/predict the phenomena' and not whether the term accurately captures anything really existing (Wight 2006: 122)" (Jackson 2008a: 343). My first comment is that this issue belongs to the huge discussion about the *status* of theoretical entities in both SR and its anti-realist alternatives and is almost identical with the one I have made just a few lines above. My second comment

is that Jackson rightly uses the following good example from IR theory, in order to argue that even philosophical non-realists assign specific meanings to their terms. According to Jackson:

> …it would be a stretch to argue that, for instance, a philosophical non-realist like Waltz doesn't make clear what he means by 'structure'; the fact that Wight spends many pages arguing with Waltz's conception of structure would seem to support the point. So while it is clear that a philosophical realist has to conduct investigations into their posited theoretical entities, it is not clear that a non-realist would not do so – albeit on different grounds, such as the demand for logical clarity and precision. (Jackson 2008a: 343)

However, Wight is so convinced of the solidity of this argument because if it does not hold, a number of questions remain unanswered. Specifically:

> How can we put theory into practice if our theoretical posits do not exist? What does not exist cannot do anything; possess causal power, bring about change, or be changed. And why should we bother to expose theoretical assumptions underlying practice if we deny the existence of those same assumptions? (Wight 2007: 381)

There are a number of points which can be made on the aforementioned. If prediction is what really counts, then it is not clear why theoretical assumptions should be true. So the realist needs to argue that a) explanation counts and b) explanation requires truth. Anti-realists, on the other hand, argue differently. According to them, we need theories not because we want true pictures, but because we need systematization and classification. Even theoretical assumptions can be criticized from this perspective for being more or less useful, plausible or convenient. The conclusion is that more argument is needed here in defence of realism.

Here is one more example of the extent to which Wight emphasizes this point:

> At the heart of the SR account of science is the view that the entities postulated by mature scientific theories (electrons, genes, viruses, dark matter, black holes etc.) are believed by scientists to be real. If scientists did not believe them to be real how do we explain attempts to split atoms, provide cures for viruses, or the search for dark matter? (Wight 2007: 382)

If one wishes to take a more critical stance on Wight than the one I have taken, one could argue that the above thesis is a descriptive one, and as such, it might be wrong. Realism is a view about *what* exists as well as *how* that exists (mind-independently, etc.); the latter question being definitely applicable to natural sciences. Now, the problem of what mind-independence in the social sciences consists of is a serious one,[151] but I think there are ways to construe it. For instance, Psillos, in his paper "Scientific Realism and Metaphysics" (2005), provides us with a framework within which such a construal becomes legitimate. He argues that there are two ways of conceiving of reality; either as comprising all *facts* or as comprising all and only *fundamentalist* (meaning *irreducible*, *basic*) facts. Psillos argues that SR should be committed to the *factualist* view of reality (not to the *fundamentalist* one), an assumption which

[151] For example, if diplomats do not believe in the existence of an international system, as Mrs Thatcher did not believe in 'society', it has major consequences.

he relies upon in order to develop a series of arguments before his final conclusion that SR "is independent of physicalism and non-Humeanism and that the concept of truth is required for a sensible understanding of the metaphysical commitments of SR" (Psillos 2005: 385). I believe that, for a realist, it is within this very theoretical framework that the examination of the issue of mind-independence in social sciences might become more reasonable and feasible.

Furthermore, following Wight again, one notices that

> It is not only the practice of science which validates a realist treatment of theoretical terms. For we use this knowledge to manipulate these entities in a practical sense. (ibid: 383)

This is a crucial argument that needs further elaboration. As the argument stands, it might make sense in physics and perhaps psychology, but if it is to make sense in IR it is going to need development, defence as well as some examples from IR itself. What are the relevant 'social experiments'? And again, how broad are the conclusions from these 'social experiments'? Do we conclude the truth of the relevant part or just the existence of the relevant entities? These are three decisive questions which must be addressed in order for the above argument to be evaluated within the field of the social science of IR. Otherwise, without these clarifications, Wight seems to waver between the existence of entities and the truth of theories.

It is worth noticing here that all the above comments I have made on Wight's statements with respect to the status of the entities of scientific theories, are comments to some of which a 'traditional', so to say, scientific realist (not a critical realist) could have presumably ascribed to.

To sum up, Wight's emphasis on his argument that one should take theoretical entities to be real is not well justified, since it needs further elaboration. The latter is not an easy task, for things are rather unclear with Wight's CR. On the one hand, it endorses the scientific realist assumption that theoretical entities should be considered to be real, which is compatible with the other scientific realist assumption that there exists an independent world out there. On the other hand, CR's commitment to the idea of rejecting the 'epistemic fallacy' of reducing what is to what is known, along with his heavy prioritizing of ontology over epistemology, 'force' one to conclude that Wight's epistemic-relativism leads to a truth-relativism. Indeed, as I have clarified in the former subsection, where I presented the basic features of Wight's *ASIR*, CR endorses a truth-relativism. Finally, the following passage from Wight's "manifesto for SR" is most illuminating:

> Given the historically specific nature of knowledge, we have to accept the fact of epistemological relativism; the view that all beliefs are socially produced and that neither truth values, nor criteria of rationality, exist outside of history. 'Whenever we speak of things or of events, etc. in science we must always speak of them and know them under particular descriptions, descriptions which will always be, to a greater or lesser extent, theoretically determined.'[152] But epistemological relativism in this sense does not imply judgemental relativism, and we can, and do, make rational choices between competing knowledge claims. (Wight 2007: 386)

[152] Bhaskar [1975] 2008: 249.

(3) Only if theoretical entities (e.g. such structures as 'social classes' or 'global capital market') are taken to be real, can social science unveil their impact on the formation of oppressive social relations and thus fulfill its emancipatory role. Jackson (2008a) wonders if this reason is of any importance to those IR scholars that, like himself, do not assign a socio-political role to any IR theory or meta-theory, because they "do *not* believe that the goal of social science is to contribute to human emancipation, or *even to carve out a special place for human beings by identifying 'properties best reserved for human agents*' (Wight 2006: 206)?" (Jackson 2008a: 344, italics are mine).[153] Although I consider myself to belong in the same (Weberian) tradition as Jackson and, consequently, would not like to assign any politically progressive/emancipatory role to social sciences, I must admit that critical realists are not completely wrong in their argument that meta-theoretical choices are not inconsequential but, on the contrary, have political connotations and implications for the substantive IR theories proper (see, for instance, Wight 2006, Kurki 2009, 2011 and Kurki & Sinclair 2010). These implications are overestimated, but they should not be totally ignored. One moderate and balanced argument about the alleged progressive political implications of critical realists' emphasis on ontology is due to Kurki. Kurki argues that statements of the kind that critical realists "point out that positivist assumptions about like-law regularities reify the social world, making present social systems natural" (Kurki 2009: 450) maybe rather problematic a reading, for "there is no singular political interpretation of positivism". However, it is telling that she adds that "yet, this does not mean

[153] For a reply to Jackson (2008a), see Wight (2008), although, strangely enough, this time Wight is as not analytical in his counter-arguments as usual.

there is no politics arising from positivism, or that the political charge of positivism has not been important for positivists" (ibid: 450).

Richard Ned Lebow (2011) provides a critique of SR, which is developed from a different perspective to that of Jackson's. Lebow's difference comes, first, from his anxiety about whether SR can tell us anything about the nature of human behaviour, i.e. the *human condition*,[154] and, second, from his anxiety about whether social entities differentiate themselves from the natural ones in terms of substance.

Lebow is correct in his argument that the end of Cold War has brought about the dethroning, on the one hand, of neorealism from its position as the overwhelming IR theory and the emergence, on the other, of classical realism and constructivism. Classical realism has undergone a renaissance after the collapse of communism because of the hegemonic behaviour of the world's leading nation-state and the ethnic, cultural and religious conflicts that have recently surfaced in many places throughout the world. Constructivism is also reinforced in the IR system of ideas because of the consensus within the IR community that the ideas of liberty and human rights have played a decisive role in undermining the fabric of the former Soviet Union and that questions of identity appear to be central to many post-Cold War conflicts. This view is supported by liberalism and the *English School*, which claims that ideas are important for the shaping of international politics. Despite the above, it is constructivism which is mainly associated with the aforementioned theoretical arguments, for constructivism probably possesses more appropriate tools in explaining the *formation* of collective identities than any other relevant rival

[154] See Hanah Arendt's, *The Human Condition* (1999), where the author reflects on problems of human agency and political action.

theory. This is the reason constructivism has been given more credit for its ability to accurately explain current international affairs than that given to liberalism and the English School.

Lebow reminds us that classical realism "is rooted in the tragic understanding of politics associated with the writings of Thucydides, Machiavelli and Hans J. Morgenthau" (Lebow 2011: 1220) and makes the following acute statements with regards to the basic features of classical realism:

> Like Greek tragedians, classical Realists tend to regard history as cyclical; efforts to build order and escape from fear-driven worlds, while they may succeed for a considerable period of time, ultimately succumb to the destabilizing effects of actors who believe they are too powerful to be constrained by law and custom. Classical Realism stresses sensitivity to ethical dilemmas and the practical need to base influence, wherever possible, on shared interests and persuasion. (ibid: 1220)

Lebow (2008) offers a constructivist theory of political order and IR, which is based on theories of motives and identity formation that derived from the ancient Greeks and insists that the role of agency cannot be properly understood through the singular study of the agent-structure relationship (which is underpinned by SR in Wendt and Wight). On the contrary, he claims that "philosophers and social scientists have attributed an array of often contradictory traits to human beings, and at least as far back as Thucydides, more sophisticated thinkers have suggested that we are a grab bag of conflicting tendencies" and that "circumstances appear responsible – at least to a degree – for determining which tendencies come to the fore" (ibid: 1226).[155]

In his most important objection to SR, Lebow deploys his ideas about the nature of social 'facts' along the lines of the Weberian tradition to which he adheres. For Lebow:

> Social 'facts' are reflections of the concepts we use to describe social reality, not of reality itself. They are ideational and subjective, and depend on other equally subjective concepts, never making contact with anything real in the sense that temperature does. Even the existence of reasonably precise measures for social concepts – something we only rarely have – would not make them any less arbitrary. (Lebow 2011: 1227-28)[156]

Next, Fred Chernoff, who has a solid background in the philosophy of both natural and social sciences, as is evident in the coherence and clarity of his argumentation in his paper, "Scientific Realism as a Meta-Theory in International Politics" (2002), argues that

> Wendt offers two sets of considerations motivating adoption of SR. His foremost is roughly: 1. Meta-theory should not rule out substantive theories we might

[155] It should be, however, considered whether recent works in cognitive psychology, such as Pinker (2011), could be taken on board by IR theorists who attempt to explain and understand agents' actions in international politics. In the aforementioned book, Pinker argues that, contrary to popular belief, over millennia and decades, humankind has become progressively less violent. He attributes this to the power of progressive ideas; modernity and its cultural institutions make us better people and reduce violence within and between societies. Pinker's argument in this book is contested - see, for instance, the book-review by Micale (2012) - but I think that the ideas it brings into the discussion about violence, including warfare, are of interest to IR scholars and may constitute a good future research program in IR. In any case, given our greater capacity for mass murder and evident willingness to implement it on occasions, one may doubt whether it is meaningful to argue that humankind has become progressively less violent.

[156] One may note here that these social concepts become real in the sense that we act as if they were facts.

> otherwise accept. 2. SR has no such effect, but anti-SR does. 3. We should, therefore, accept SR as our meta-theory. This paper has tried to show that the argument is unsound because both premises are false. The first premise is false because we should have some a priori criteria for a properly formed theory, which should at least include requirements like "internal consistency". The second is false because SR does require the assumptions be "realistic", and avowed instrumentalists need not accept this requirement. (Chernoff 2002: 205).

However, in order to do justice to Wendt's argument that SR is compatible with methodological pluralism – which implies that SR does not rule out differing substantive IR theories – I must say that the first of Chernoff's points is rather unjust; namely, there is no reason to conclude that Wendt is willing to accept any IR Theory that does not fulfill the criteria of logical coherence, internal consistency, etc. Chernoff's second point is valid although, as I have discussed in the second chapter of the thesis, the debate between scientific realists and anti-realists requires a rather complex and sophisticated argumentation as to what it means for their assumptions to be 'realistic'.

Chernoff continues as follows:

> Wendt's ancillary argument motivating SR is that IR is capable of more progress that has been exhibited because there is unnecessary disagreement over epistemology. He seems to reason as follows: 1. Agreement on foundational questions aids progress in the discipline. 2. Most IR scholars accept SR, even if they do not acknowledge it openly. 3. It follows from 2 that there is less disagreement among IR scholars about ontology than about epistemology. 4. Therefore, a shift from epistemological to ontological foundations aids progress. This second argument is invalid because premise 3 does not follow from premise 2.

> Even if IR scholars accept SR, it does not follow that they share ontological views, since SR merely states connections between a theory and its ontological commitments; it does not commit IR theorists to any particular ontology (because it does not motivate adoption and any specific substantive theory). (ibid: 205)

Here Chernoff makes a simple, albeit crucial argument, which I would like to emphasize once more since I have the impression that it is very often overshadowed by the discussion over the specifics and technicalities of both SR and its alternatives: *SR does not commit IR theorists to any particular ontology, thus enabling the adoption of differing substantive theories*. What does this mean in practical terms? Which entities' existence is going to be tested empirically, at the end of the day? From the point of view of the kind of conventionalism that he defends, Chernoff (2007) questions Joseph's (2007) emphasis on the value of theoretical entities' *emergent properties* in terms of their ability to help us explain what we observe around us:

> The particular emergent properties postulated by, say, one version of structural Marxist theory differ from those postulated by other Marxist variations and by non-Marxist theories. Which, then, are the 'real' emergent properties? If all competing theories 'talk about' certain structures and properties, they are not thereby real. We often have minimal grounds for selecting one theory over the rivals and is a questionable move to insist, as scientific realists do, that the entities referred to by one theory exist, while all others do not. (Chernoff 2007: 405)

Chernoff makes a number of interesting comments on various aspects of SR. One of them is about the validity of the application of the No Miracle

Argument[157] in the social sciences, which he rejects since he argues that the No Miracle Argument is fit for the explanation of scientific progress of only the natural sciences (Chernoff 2007: 404).

Finally, Chernoff (2002) qualifies Quasi-Duhemianism (henceforth QD)[158] for a possible meta-theory of IR but I am not going to present QD here, since the aim of this chapter is not to provide any alternative to SR meta-theory, but only to analytically discuss the pros and cons of the introduction of SR into IR. For the time being, suffice it to say that I am aligned with Jackson who goes for a 'pluralist science of IR'. The latter "posses the challenge of dealing with bodies of *warranted knowledge stemming from philosophically incompatible methodologies*" (Jackson 2011: 210, italics are mine) and relies on the key-concept of the 'translation' of each one of these methodologies into the languages of the others:

> Instead, the implication of methodological pluralism is that between different bodies of warranted knowledge we have the ongoing challenge of *translation*: literally, the task of making claims comprehensible to speakers of other methodological languages…methodological pluralism sets up a variety of contentious conversations and efforts to appreciate the insights of alternative ways of producing knowledge while avoiding the temptation to universalize our own methods of conducting scientific inquiry." (ibid: 210)

[157] I have discussed the No Miracle Argument (NMA) in Chapter 2.

[158] In a number of successive papers Chernoff comes back to the issues of SR and QD (Chernoff 2007, 2009, 2009a). However, the basic line of his argumentation about SR and QD is the one he has presented in his 2002 paper. To cut a long story short, I agree with Chernoff that "the merits of Wendt's work are to be found in the substantive theory" (Chernoff 2002: 205), not in its meta-theory.

It is questionable, however, whether one needs such an extensive argumentation on meta-theoretical issues in order to claim that methodological pluralism is the right approach to the study of IR and to acknowledge that each methodology must understand what the alternative methodologies really do and predictably result to. Other IR scholars have come to the same conclusion in a much more direct way, namely by just observing the developments in real-world politics and invoking the appropriate theories in order to capture different aspects of complex political phenomena. For instance, Christopher Hill and Michael Smith adopt methodological pluralism as one of the three methodological assumptions they make in order to study EU's international relations, when they argue:

> *It follows that a methodological pluralism is therefore required when seeking to explain and understand the EU's international relations*. No one approach, whether broad-brush as in realist, rationalist and constructivist, or more specific, as in geo-politics, intergovernmentalism or 'expectations', comes near being adequate by itself. The usual problems then arise of how to relate diverse, and possibly incommensurable middle-range, theories to each other, but these are inevitable in any attempt to do justice to complexity. (Hill & Smith 2011b: 8)

Hill and Smith not only defend methodological pluralism but also put on the table the inevitable question about how one could "relate diverse, and possibly incommensurable middle-range, theories to each other"; which is exactly the problem of the 'translation' of one methodology to another that Jackson has also put forward.

As for Jackson's 'pluralist science of IR', Onuf wants it to be *even more* pluralist, in the sense that Jackson concedes ground at positivism at the start of

The Conduct of Inquiry in International Relations (2011) by aspiring to science as a model of scholarship. Onuf argues that the obvious reason this *should not have happened* is positivist resistance to the language turn. Indeed, no social theoretical framework today "can dispense with the nuts and bolts of language to hold it together and still be inclusive" (Onuf 2012: 26); for him, it is noticeable that "language and speech have no entries" in Jackson's index (ibid: 26). Had Onuf claimed the opposite, he would have renounced his own major contribution to the field, namely his constructivist IR theory (Onuf 1989). There are, of course, many other well-known IR theorists who have spelt out their disagreement, albeit on different grounds, with the adoption of science as a model of scholarship for IR. Two such theorists are Hans Morgenthau (1946)[159] and Hedley Bull (1966).[160]

With regard to the development of CR as a science of IR, Chris Brown (2007) argues that one should not ignore the political roots of Bhaskar's work. In fact, Bhaskar ([1975] 2008) reacted against the disillusionment with science in the 1960s and 1970s – a symptom of which was the decreasing interest in the 'scientific Marx' of *Capital* that the radical intellectuals of that epoch have shown. The latter is, according to Brown, "the ur-text of critical realism" (Brown 2007: 410). With this work Bhaskar aimed to put "new life into the materialist approach to social theory, and to combat the idealist leanings of 1960s radicalism" (Brown 2007: 414). Brown finds that "the kind of Marxism that Bhaskar and his followers wished to re-legitimate in the face of criticisms of 1960s romantics is hard-headed and a little cold-blooded" (ibid: 416). Although not a Marxist himself, Brown calls for more works like that of Joseph

[159] For a reconsideration of Morgenthau's work, see Williams (2007).
[160] For a defence of the scientific approach to IR, see Kaplan (1966) and Nicholson (1985), (1996) and (1999).

(2007), with the thought that they may lead to a genuine revival of historical materialism in IR studies. He makes this point when arguing that

> Reading Joseph, it is clear what we are being offered – critical realism is not simply a negative movement saying what is wrong with existing theories, but provides the positive basis for another and – allegedly – better theory: namely Marxism (Brown 2007: 415).[161]

There are, indeed, some Marxist IR scholars who use CR as the meta-theoretical foundation for their work, in the sense that they combine aspects of SR with elements of Marxist theory, in order to refine the latter and to reawaken the interest of the IR community in the utility of Marxism for the study of the international.[162] Indeed, Marxism could be probably fertilized through CR, due to the latter's emphasis on ontological depth. This could lead to the integration of the economic theory of *Capital* and the Marxist theory of historical materialism. Such a development might provide us with a Marxist theory of the international, namely an IR theory in the categories of capitalist economic evolution, which is now lacking (cf. Yalvaç 2010).

For the above reason, though not a Marxist myself, I agree with Brown's suggestion that we need a resurgence of interest in Marxist studies within IR theory. Furthermore, I strongly agree with him that if the effect of CR is to

[161] Wight and Joseph replied to Brown (2007) and distinguished SR/CR from Marxism (Wight & Joseph 2010: 3-4; cf. Lebow 2011: 1221). They are right to argue that SR/CR is not reducible to Marxism because, among other reasons, SR "draws on a wide, and differing, range of intellectual resources that go well beyond Bhaskar" (Wight & Joseph 2010: 3). That notwithstanding, I am aligned with Yalvaç in that "CR is compatible with diverse theoretical positions as a philosophical project, *but it is most compatible with Marxism*" (Yalvaç 2010: 185, emphasis added).

[162] See, for instance, Jessop (2001), (2002), (2007), (2010), Joseph (2002), (2007), (2008), (2010a), Yalvaç (2010), etc.

"revitalize debates over epistemology and ontology it will do the discourse no service" (Brown 2007: 416).

In terms of the discussion on the role of science within IR theory, now, I would also like to call attention to John Lewis Gaddis' paper, "History, Science and International Relations" ([1996] 2002). Starting from the idea that historians and social scientists suffer from 'physics envy' or any other kind of methodological 'envy', he comes to demonstrate that even 'hard sciences' such as geology and paleontology are 'historical sciences', "whose strength lies in explaining in great detail where we are and how we got there, *but whose pretensions to forecasting are confined to much less specific observations about the overall framework within which certain known processes will occur*" (Gaddis [1996] 2002: 39, emphasis added). Thus, among others, he emphasizes the importance of rediscovering the importance of the *historical narrative*, which may well serve as "a kind of bridge between the 'new' hard sciences of chaos and complexity and the 'old' social sciences" (ibid: 45). He does this in order to argue that "by sticking with narratives, the historians who never bought into the 'old' social sciences in the first place have achieved something rather remarkable: *they have come out on the cutting edge of a revolution by persisting in a reactionary stance*" (ibid: 45, italics are mine). When he reaches the end of the road he argues that a rediscovery of narrative by social scientists "could move us back towards the pre-professional era when intelligent people could comfortably involve themselves in, and learn from, multiple disciplines without being regarded as dilettantes" (ibid: 45).

I believe that we need both approaches for the study of IR; that is the scientific one (whether it confines itself to positivism or accepts what is meant by Jackson's spacious 'pluralist science of IR') as well as the non-scientific ones (which include the historical narrative). As a consequence, I think that IR scholars and social scientists in general, may 'envy' Physics the way philosophers 'envy' Mathematics. Indeed, as Gian-Carlo Rota, a distinguished mathematician and philosopher has argued in his paper, "Mathematics and Philosophy: The Story of a Misunderstanding" (Rota 1990), the attempts of certain philosophers in the twentieth century to mimic the language, the method and the results of Mathematics have harmed Philosophy. Even more so, according to Gian-Carlo Rota, one could go as far as to say that the aforementioned attempt resulted from a misunderstanding of both Mathematics and Philosophy and has, in fact, harmed both disciplines.

To conclude, I would argue that the fact that SR has been uncritically introduced in the study of IR as its proper meta-theory - the examination of the arguments *against* SR as a philosophy of science has been rudimentary, if not entirely lacking - has led to an extensive but perplexed argumentation about SR within IR. Things are even more unnerving when some IR scholars cannot distinguish between SR and CR. The result is that the debate over SR as the proper IR meta-theory is not productive and, even worse, cannot help the building of substantive IR theories in a substantial manner. This does not, however, devalue completely the general consideration of and reflection on meta-theoretical issues.

4. 4 Two important issues raised by the SR debate in IR

During the SR debate in IR a series of interesting issues have surfaced, the majority of which I have discussed in the second section of this chapter. Here, I will discuss two of them which are of a quite different nature from one another: the first one is the causes vs. reasons issue, which belongs to the subject matter of the philosophy of language while the second one has to do with the so-called ‘meta-theoretical hypochondria’, from which IR theorizing allegedly suffers.

4.4.1 Causes vs. Reasons

The introduction of the constitutive explanation (which asks questions of the kinds “how-possible?” and “what?”) as a complement (not an alternative) to causal explanation (which ask questions of the kinds “why?” and to an extent “how?), inevitably poses the question about whether reasons can be causes. In his paper, “On constitution and causation in international relations” (1998), Wendt argues that it is wrong to take that material conditions (pace natural world) imply causal theorizing whereas ideas (pace social world) imply constitutive theorizing. This is because he suggests that *ideas*, in the form of *reasons*, can be *causes* (as in rational choice theory) and clarifies that “in saying that reasons can be causes I am taking one side in a debate about what remains a controversial issue; for an opposing Wittgensteinian view see…” (ibid: 107, fn. 18). Steve Smith’s comment on this point is that he is concerned that “an *idealist* account seems best suited to exactly this alternatively Wittgensteinian account, rather than the one that stresses the ultimately causal nature of the social world” (Smith 2000: 153, emphasis added). Smith defends the distinction between explaining and understanding (Hollis & Smith 1990) on the basis that explaining is the appropriate epistemology to the natural world, whereas understanding is the appropriate epistemology to the social world. As a consequence he also defends the distinction between causes and reasons.

Furthermore, Smith makes the valid point that, contrary to Wendt, two other leading constructivists, Kratochwil and Onuf, consider reasons to be only constitutive. Kratochwil argues "that there is a crucial difference between causal explanations in the world of observational facts and that of intentions" (Kratochwil 1989: 25), while "Onuf's version of constructivism, like Kratochwil's, stresses the rule-based nature of social life, and paints a far more nuanced conception of the nature of regulative and constitutive rules than does Wendt" (Smith 2000: 159-60).

Now, if one wants to deal in some considerable depth with the reasons vs. causes issue, one should first address the question: Can the reason why one acts be a cause of his acting? It would seem that this depends on the answers to two different questions: (a) Are actions caused? (b) Might the reason one gives for the way one acts, actually *be* the cause of one's actions? It is possible that the first question belongs to behavioural science; the second one, however, calls for an analysis of the logical status of the notion of explanation. One wants to know whether giving reasons is equivalent to producing *causal* explanations.[163]

Two approaches have crystallized from the vast philosophical literature on this subject. At one extreme, one might hold, like Blackburn (see 1994: 321), that intentional behaviour is explained teleologically by reference to an agent's reasons (beliefs, intentions, wants):

At the other end, one might simply identify, like White does ([1968] 1977: 17), reasons with causes, subsuming both of them under the concept of events

[163] It is clear that if we want to know why someone acts as they do, surely we need to know both their intentions and the pressures on them from outside circumstances. The latter is what John Vincent, following Burke, called 'the empire of circumstance' (Vincent 1994; cf. Hill 1996b).

related by causal laws. In this case, reasons are assimilated to causes because of the similarities between the two kinds of explanation of action:

The first viewpoint — reasons must be distinguished from causes — stands in the tradition of the hermeneutical distinction between the explanation (*Erklären*) of the natural sciences and the understanding (*Verstehen*) of the social sciences. We find its best expression in the philosophy of the later Wittgenstein, at least as developed in the works of his more 'orthodox' followers, G.E.M. Anscombe, Peter Winch, and, more recently, P.M.S. Hacker (see Glock 1996: 75).

In the social sciences, Peter Winch's *The Idea of a Social Science and its Relation to Philosophy s*tands out for insisting that the notion of cause is very different in the natural from what it is in the social sciences. According to this – standard – interpretation, Wittgenstein himself eschewed a uniform, nomological paradigm of causation,

> which so fascinates us when philosophizing. We are tempted to subsume all cases under this one paradigm, despite the fact that, as we all know (even if we often need to be reminded of the fact), there are countless correct uses of 'cause' and related causative verbs that do not fit this special paradigm. Specifying a cause is one category, itself logically diverse, of explanation of change. Specifying a reason, a person's reason, for believing something or for doing something, is another. (Hacker 1996: 158)

This distinction, however, has been severely criticized by Davidson (1963), who occupies the contrary position. Davidson claimed that the existence of a reason is a mental event, causally linked to the acting; otherwise, acting and the reason for which the action is performed would be loose and separate. The reason that explains an action, then, is the one that was causally efficacious in prompting the action. Davidson concludes that although we explain action by reference to reasons, these are causes that are identical with (though not reduced to, but possibly supervenient on) neurophysiological phenomena.

Apparently, the picture that motivates Davidson is a causal conception of the mind according to which, mental phenomena are the inner causes of outward behaviour (see Glock 1996: 75).

Wittgenstein's distinction stands at odds with this picture. His emphasis on rule-following and his painstaking examination of the justificatory role that reasons play in our lives – that "reasons come to an end somewhere" – is an original attempt to draw the philosophers' attention to the *normative*, *social* and irreducibly *consensual* character of the practice of reason-giving. Contrasting reason-explanations with causal ones, Wittgenstein elaborates on the alternative, contextual paradigm he puts forward throughout the *Philosophical Investigations*. Ultimately, one is invited to acknowledge the utter *vagueness* of the philosophical idea of cause, as well as the *diversity* of our actual use of this notion in practice when giving explanations of one's actions.

Indeed, later developments in the philosophy of mind seem to have confirmed Wittgenstein's insights. The invention of the metaphysical notion of supervenience and the dogma of anomalous monism, despite being offered as further elaborations of the metaphysics of cause and action, have only produced more heat than illumination.

The 'utility' of the above analysis for IR theory is debatable. Nevertheless, Wendt (2000) in his reply to Smith (2000) insists that reasons can be causes.

This means that consensus over the way one ought to address this philosophical problem, which underlies the difference between constitutive and causal theory and as a consequence has implications for both IR theory and empirical research, is not to be expected anytime soon.

4.4.2 'Meta-theoretical hypochondria': True or an exaggeration?

The perplexed and at times tedious philosophical argumentation which makes for the arcane[164] language which is used when debating SR as an IR meta-theory brings me to the last issue I will be addressing in this chapter: the current extensive concern with epistemological, methodological and ontological issues makes one wonder whether this happens at the expense of dealing with other more important IR issues which surface in the international political landscape, or not. Fred Halliday (2000: 247) used Cilfford Geeertz's term of 'epistemological hypochondria' to sharply criticize this penchant for meta-theorising which characterizes the works of a good number of modern IR theorists. The term refers to an "obsession with method and with questioning philosophical issues at the expense of getting on with a job of actually looking at the world or looking at societies" (ibid).

While Halliday recognizes the importance of having a view on the epistemologically related issues, he thinks that "to spend the majority of your time and the majority of your journal space, discussing meta-theory,

[164] Reus-Smit defends the validity of meta-theory but not the arcane language in which 'meta-theoretical bunkerists', as George Lawson (2007: 775) aptly names them, take refuge in order to make their arguments unintelligible by other IR scholars who work in different areas of interest: "Nowhere here do I defend the cultivated obscurantism that enchants small but diverse pockets of our field, in which ornate language is deployed as a badge of membership more than a medium of communication" (Reus-Smit 2012: 526).

epistemology, the critic of ethnocentric assumptions and all the rest of it is a misuse of time" (ibid).

His critique becomes harder by arguing that such issues lie outside the realm of IR, not least because none of the questions posed by these issues "can be worked out solely or mainly through international relations" (Halliday 2000: 247).[165]

Some years earlier, William Wallace (1996) had launched an attack on the alleged trend towards over-theorizing in IR, which is presumably being served by IR theorists who live in the 'ivory towers' of the academe ('monks'). For Wallace, these academics are enchanted by the fetishism of doing theory for theory's sake. This has led them to the nurturing of scholasticism in IR theorizing. He accuses them of forgetting that "more, and more detailed, empirical research should be one of first priorities, guided by theoretical assumptions but intended to inform – and so modify – theoretical assumptions" (Wallace 1996: 314). As a consequence, Wallace claims, IR has "become too detached from the world of practice, too fond of theory (*and meta-theory*) as opposed to empirical research, too self-indulgent, and in some cases too self-righteous" (ibid: 316, italics are mine).[166] Wallace prefers 'technocrats' to 'monks'; that is, he prefers scholars who, on the one hand, retain a *relative* autonomy from the policy universe in order to remain uninfluenced of the current modes of thinking and are thus able to produce academic work which could come up to the high standards the IR academic community has itself set

[165] He expressed similar views in Halliday (1996: 324).
[166] Wallace wrote this paper just before the end of the Third Debate, so his charges for over-theorizing were mainly directed against critical theorists and post-structuralists. However, given that he does not appear to have changed his mind on this issue from the publication of his 1996 paper until now, I think I am entitled to assume that his argument holds even for the current debate on IR meta-theorizing, which, this time, arises from philosophy of science considerations.

for the assessment of the works produced under its auspices. These same scholars, on the other hand, deal with real-world political problems and are willing to get actively engaged with governments or other policy-making oriented institutions or relevant policy boards.

Although he may not be aware of the meta-theoretical assumptions of his own preferred model of empirical inquiry (cf. Reus-Smit 2012: 4), there is a great deal of truth in Wallace's claim that IR has moved a long way off from its point of departure. As Halliday (1996: 318) liked to remind us, "no crisis, no discipline" and, as E.H. Carr himself stressed, "IR like all academic subjects emerged as a distinct *academic* discipline because of a particular crisis in modern society, in this case of relations between states" (Carr [1961] 2001: 8 - 9). According to E. H. Carr again, "no science deserves the name until it has acquired sufficient humility not to consider itself omnipotent, and to distinguish the analysis of what is from aspiration about what should be" (ibid: 9); theoretical complacency may constitute a great danger to the IR's future as a discipline.

This means that in the case of a political science such as IR, it is rather odd, to say the least, that a great number of papers which are published in IR journals deal with theoretical issues that are of no relevance to the study of real-world political phenomena. Indeed, nowadays a large amount of work in academic IR is not meant to meet the needs and challenges of real world politics, either on the level of the political practice or on that of theory (something that many advocates of the current orientations in IR theorizing dangerously underestimate). Furthermore, it is not only true that very important currents of thought in IR were born within critical historical circumstances, but also that sometimes these currents of thought have implications for the nature and quality of international relations that last for long periods (even until today). For instance, due to "a fine combination of history and theory" (Brown & Ainley 2009: 37), Christopher Hill (1989) has demonstrated that the Second World War did not bring about "the death of liberal internationalism" and the "triumph of realism". What it did bring about is the allowance of "fewer wild swings of the pendulum between abstract social engineering and cynical power reductionism" (Hill 1989: 327)

Hill's suggestion points to a very stimulating way of doing IR Theory with the 'assistance' of History, since it depicts the evolution of political ideas and theories in accordance to the historical development of real-world politics.

However, coming back to the initial question, one must closely examine Steve Smith's reply to Wallace, in which he argues that "agreeing with Wallace means that academics will run the risk of having to work within the agenda of the policy community, of being unable to stand back and examine the moral, ethical and political implications of that choice" (Smith 1997: 515). At the same time, he also criticizes Wallace's view of theory as being only explanatory and problem solving oriented, instead of being concerned "either with understanding the world or with emancipation" (ibid: 513-14). That given, I would be more sympathetic with the views Hill has expressed on another occasion on the issues of the substance of 'academic IR' and the challenge posed to it by the "siren song of policy relevance", because I find them more balanced and moderate than the ones expressed by Wallace and Smith on the same issues. This is due to fact that Hill's views ascribe to IR scholars a task that strikes a fine balance between avoiding 'academic snobbery' while at the same time preserving "a distinct sense of purpose" (Hill 1994: 19).

Furthermore, with regard to the place of theory in the 'academic IR', Hill claims that "the academic comparative advantage, which applies to International Relations no less than to any other subject, is a long time-perspective and a concern with fundamental causation" (ibid: 20) and that "the nature of state, the causes of war, the problem of rationality in foreign policy, are all subjects which deserve extensive reflection in their own right and not simply in the margins of breathless analyses of Maastricht, the Gulf or Boris Yeltsin" (ibid: 21).

Without denying the reasonableness of the converging – although from different perspectives – views of Halliday and Wallace, one could challenge the common view shared by many IR scholars according to which the lack of practical relevance of some large part of IR theory is due to the rapid recent growth of IR meta-theorizing.

For instance, Christian Reus-Smit claims that

> It is a commonplace to bemoan *our* field's lack of practical relevance, and to blame this sorry situation on our penchant for ever-more extensive theorizing over the analysis of real-world phenomena (Reus-Smit 2012: 525).

However, I think it is an exaggeration to speak of "our field's lack of practical relevance." Instead, I would go for the rather more moderate expression that there is a lack of practical relevance of some large part of IR Theory, since I believe one should not ignore that there is a considerable amount of theoretical work which has nothing to do with philosophy of science considerations and is of substantial practical relevance.[167] On the other hand, in order to do justice to Reus-Smit's claim above, one should take into account that he bases his judgment on the observation that "the most comprehensive survey of IR scholars reports that 85 per cent of respondents believe that there is a gap between the kind of research we produce and what the policy community finds useful, and roughly half of these think the gap is widening" (ibid: 526-27).

Furthermore, Reus-Smit argues "first, that the theory versus practical relevance thesis rests on assertion more than evidence, and second, that it misconstrues the problem" (ibid: 525), in the sense that

> IR's status…is undermined not by excessive theorizing, but by a series of other disciplinary handicaps; by IR's marginal interest in the nature of politics as a distinctive form of a social action, by the dissipation of the field's early practical intent, by the persistent bifurcation of explanatory and normative inquiry, and, symptomatic of these problems, by the virtual extinction of the figure of international public intellectual. (ibid: 526)

Although I do not endorse every single point of Reus-Smit's argument, I feel aligned with him when he argues that "theory, even abstract meta-theory, can aid practical knowledge" (ibid: 539). First of all, he is right in arguing that meta-theoretical reflexivity, along with clarity of purpose, logical coherence, consideration of alternative arguments, and the provision of good reasons

[167] I think that the discussion about SR as a meta-theory of IR is not so important as other ideas and programmes which are already under discussion within substantive IR Theory; for instance, I find much more interesting and maybe fruitful the attempt to bridge constructivism and Realism which has been undertaken by Barkin (2010). Barkin bases his main argument on the crucial and very often bypassed observation that "Realism is a substantive theory of international politics, constructivism is not; constructivists are making a general point about the nature of knowledge in the human sciences, i.e. that it is reflexive and intersubjectively created, and this general point may or may not be compatible with one or other variety of Realism - there is no hostility between the two camps" (Brown 2012: 861-62).

(empirical evidence, corroborating arguments, textual interpretations, etc.) are necessary and sufficient conditions in order for a theoretical work in IR to be considered worthy of assessment and evaluation. In particular, he indicates that if epistemological assumptions determine or, at least, affect the questions we ask, then, if we are conscious of these assumptions, we will also be conscious of the limitations they pose to the kinds of questions we are 'allowed' to ask. Likewise, if ontological assumptions affect how we see the social universe that surrounds us and which we are also integral parts of, determining what does exist within this universe allows us not to neglect to take under consideration the things that matter. As Reus-Smit says, "the oft-heared refrain that 'if we can't measure it, it doesn't matter' is an unfortunate example of epistemology supervening ontology, something that meta-theoretical reflexivity can help guard against" (ibid: 533). It is obvious, however, that many IR scholars choose a theoretical model, or even frame their own, without being aware of the epistemological, ontological and methodological assumptions which underpin it and which have implications for the kind of work they can produce within this certain theoretical and meta-theoretical framework. Moreover, I now come back to a point I have already made in the second subsection of this chapter, according to which IR scholars tend to forget that every distinct IR meta-theory is, as Kurki (2009) points out, 'politically charged', in the sense that it has its own political connotations and implications, which determine (that is, enable or constrain) the context of the substantive IR theory it underpins. For instance:

> Critical realism's insistence on the language of material reality and causality, for example, and its suggestion that social life exists as a stratified, layered reality

> beyond the 'observable' could be seen as 'politically charged'. These argumentations are widely utilized by critical realists to reject idealist and relativist strands of argumentation, seen as suggestive of political conservatism…Simultaneously, the language of layers and depth ontology is used to reject the atomistic image of society as constituted by autonomous individuals espoused by many positivist liberals. (Kurki 2009: 445).

The initial question about whether considerations of philosophy of science are of value to substantive IR theorizing can be reformulated as "Is IR a scientific field so that it should be grounded on philosophical foundations or not?" Nuno P. Monteiro and Keven G. Ruby, in their joint paper, "IR and the false promise of philosophical foundations" (2009a), have first raised this question and *International Theory*, the journal in which the above paper was published, organized a symposium on the topic, "Who needs Philosophy of Science, anyway?"[168]

The elaboration on important questions such as the alleged scientific nature of IR and the feasibility or not of coming up with a single IR foundational meta-theory, which have been both extensively debated in the framework of the aforementioned symposium and about which I have discussed in section 4.3.3 of this chapter, cannot be encountered comprehensively within a single chapter. I have only provided some indicative arguments on why I am an advocate of the view that there is no need to search for a foundational meta-theory of IR and I am all for a 'pluralist science of IR', in which differing epistemologies and their

[168] Milja Kurki (2009), Patrick Thaddeus Jackson (2009), Fred Chernoff (2009), Raymond Mercado (2009) and James Bohman (2009) have all contributed to this symposium by submitting their critiques of Monteiro's and Ruby's paper to the *International Theory*; the debate has been closed by a reply from Monteiro and Ruby (2009b) but this is certainly not the last word spoken on the question.

consequent methodologies bring different aspects of the observed complex political phenomena within sight.

I would conclude that although the charge of 'meta-theoretical hypochondria' advanced against modern IR theorizing has a good deal of truth, one should not forget that any IR theory relies upon a certain meta-theory and that the substance of the former is dependent, at least to a certain extent, on the latter's assumptions. What matters to every IR scholar is to be conscious of the meta-theoretical assumptions of his substantive theoretical or empirical work.

At the end of the day, theory and meta-theory are only human intellectual products, which are characterized by different degrees of abstraction and which are supposed to help us capture the complexities of political life as that is deployed on the national and international levels:

> Academics quite properly travel the highways and byways of methodology, theory and history in their efforts to explain political action. But it is the elected government and its primary institutions that citizens look to in the first and last instances to secure and improve their lives, and which should be held to account. When political scientists neglect this fact, and their own role in society, they risk aridity. (Hill 1991: 247)

Real-world politics are inextricably connected with real people's daily agonies for the benefit of their own well-being and future.

4.5 Conclusion

In this chapter I have critically discussed the introduction of SR into IR. I have primarily focused on the works of the arch-scientific realists Wendt and Wight.

I build my argument by taking on board comments on certain aspects of SR, which were suggested by both proponents, as well as, most crucially, opponents of SR. I have tried to use a rigorous, analytical approach to the SR problematique and to avoid generalized expressions about the prons and cons of the adoption of SR by IR. The conclusions I arrived at are the following:

1) SR was introduced into IR as its proper meta-theory, which is assumed to underpin substantive IR theories.
2) Unfortunately, this introduction was not accompanied by any examination of the arguments for and against SR, which have been deployed in the relevant discussion about both SR and its anti-realist alternatives in the philosophy of science literature. On the contrary, the standard brand of SR which is imported and used in IR meta-theory has been considered to be the definitive and conclusive version of realism. This is mistaken, however, since the introduction of the standard brand of SR into IR has led to the neglect of all the shortcomings that characterize SR as it has developed in the philosophy of science.
3) Furthermore, the miscomprehension of certain concepts and premises of SR has increased due to the inability of some critics to differentiate between SR and CR (the latter is a version of SR that relies heavily on Bhaskar's work, which, however, is rarely discussed in the SR debate in the philosophy of science).
4) Wight, but not Wendt, endorses almost the entire corpus of the Bhaskarian CR and thus becomes a typical critical realist, followed by important IR theorists, like Patomäki, Joseph, Kurki, Jessop, etc. In IR, if one refers to SR, one means more often than not CR.

5) SR is based on the premise that there is an independently existing world, which is knowable to us. This world consists of observable and unobservable entities. The theoretical entities, which are provided by mature scientific theories, are taken to be true.
6) Following Bhaskar, CR distinguishes philosophical ontology, which concerns the relationship between the researcher and the world, from scientific ontology, which concerns the catalogue of things that are taken to exist and, hence, are available to serve as objects of scholarly research.
7) CR prioritizes ontology over epistemology without committing IR theorists to a certain set of entities. This leads to an epistemological pluralism and to a judgmental rationalism.
8) Wendt's SR is compatible with positivism; moreover, Wendt puts forward the constitutive explanation, which is complementary to the causal explanation and constitutes a 'via media' and a 'bridge' between positivist and post-positivist methodologies. In contrast, for critical realists, CR is not compatible with positivism. Wendt's constitutive explanation rejects the dichotomy between reasons and causes, in that one can accept that reasons can be causes. For this reason, the reasons vs. causes debate is of central importance to Wendt's SR.
9) Wight argues that the explaining versus understanding debate belongs to the realm of methodology rather than that of epistemology. Furthermore, he argues that if something in this debate is of value to his own theory is that the distinction between these two terms is based on ontological considerations about the social world. Therefore, the reasons vs. causes

debate, which arises from the explaining vs. understanding debate, is not crucial to CR.

10) Wendt has used SR in order to introduce the notion of unobservable entities in the IR vocabulary and thus legitimize the theorization of unobservable structures in the study of the agent-structure issue within IR theory. His aim was to show that neither the alleged individualist ontology of neorealism nor the structuralist ontology of the world-system theory is adequate to explain the formation of international politics. He thinks that the explanatory scheme of the agent-structure relationship can help one explain not only how international politics are shaped and reproduced but also how these can change.

11) In contrast, Wight takes agents and structures to be ontologically strictly independent, providing us with certain typologies of structures and agencies which, however, are not always compatible with one another.

12) Wendt's *STIP* is an important piece of theoretical work. This theoretical gravity is mainly found in its second part, which concerns his social theory of international politics and in which he deploys his own constructivist theory. In it Wendt emphasizes the role of ideas but also acknowledges the limits, which are imposed on them by 'rump materialism'. Per contra, Wight's intention was not to provide a substantive theory of IR but, instead, to argue that IR should become a science without need to resort to positivism.

13) All the above have been contested by eminent IR scholars who have criticized SR and CR on two fronts: first, in terms of their inner logic,

consistency, coherence and comprehensiveness as theories and, second, in terms of their ability to help the building of substantive IR theories.

14) Wendt's 'rump materialism' is contested because, as Steve Smith, among others, has argued, Wendt's ambiguous statements about the relationship between the material and the ideational in his *STIP* allow one to doubt the status of his 'rump materialism'. I argued that, although Wendt's argumentation is not free from inconsistencies and contradictions, his thesis is not 'ideas all the way down' but rather 'ideas *almost* all the way down'; otherwise his constitutive explanation as a via media methodology makes no sense.

15) However, Smith's further point that in both of the above cases the ideational and the material are very different kinds of stuff, and this seems to make naturalism impossible, is at least debatable.

16) According to the majority of their critics, Wendt and critical realists have not succeeded in coming up with certain empirical research proposals and this is considered to be the major shortcoming of both SR and CR.

17) Critical realists and Wendt believe that meta-theoretical choices are 'politically charged' and, as such, have implications for the choice of the substantive IR theory. This is quite a plausible position to hold.

18) More generally, however, the consideration of meta-theoretical issues is of some value, since every IR scholar should be aware of the meta-theoretical premises he uses in order to underpin his own work. This, nevertheless, must not lead to a 'meta-theoretical hypochondria.'

19) If one takes under consideration the deadlocks one reaches when one discusses the meta-theoretical issues which are posed by SR and CR, it is

legitimate to conclude that further *intensive* involvement in this kind of discussions does not merit any progress in IR theorizing.

20) Chris Brown has argued that CR can do the IR discipline service only if it leads to a resurgence of interest in genuine Marxist studies in IR and, for the sake of pluralism of ideas in IR theory, I absolutely agree with that position.

21) I strongly support a 'pluralist science of IR,' which must be more inclusive even than Jackson's, in the sense that it must incorporate theories of language turn and speech act theories (as Onuf suggests). The above theories must be included in the 'methodological apparatus' of IR, even if this implies that, in this case, SR would not be justified branding itself as a science.

22) To pay lip service to the notion of science implies that IR should endorse as legitimate epistemologies all the post-positivist ones, not that positivism should be abandoned. Therefore, in the above broad definition of the 'science' of IR, or whatever else one would like to call it, positivism should retain its important – however not overarching – role. Otherwise, quantitative and formal methods would have to be abandoned along with the questions that seemed to call for these very methods.

Overall, I conclude that the introduction of SR/CR into IR does not stand up to the scientific and critical realists' expectations. Consequently, one may wonder if the study of this issue deserves the intense intellectual labour of many leading IR theorists, proponents and opponents of SR alike. Sometimes, however, the journey towards knowledge might yield more benefit than the answers at the final destination of an intellectual endeavour. Such seems to be the situation with the meta-theoretical debates within IR.

5

ALEXANDER WENDT'S QUANTUM SOCIAL SCIENCE AND INTERNATIONAL RELATIONS

5.1 Introduction

Despite the fact that the use of SR in the Philosophy of Science and IR has been extensively discussed in the previous three chapters of the thesis, one more issue should be added to my examination. This issue is Wendt's quantum social science project and its implications for IR Theory, and emerged when Alexander Wendt (2006a) devoted a significant portion of his reply to the critics of his celebrated *STIP* to a provocative outline and defence of a quantum approach to the social sciences. In this venture he aspires to initiate a new science of IR, as part of a new social science as a whole. Thus Wendt grounds his newly developed theoretical investigations in quantum mechanics, while underpinning them by using a revised SR as their meta-theory.

The aspirations raised by his new research program are well summarized by Griffiths, Roach and Solomon when they write that: "Wendt has drawn on quantum theory to probe the limits of international theory" (2009: 158). Furthermore, they argue that Wendt's "primary aim is to suggest the possibilities of what he calls 'capacity for collective self-consciousness'" (ibid: 158-159), meaning that a quantum social science may offer more sophisticated and contextually enriched methods for studying the formation of 'collective self-consciousness' than the ones we already have at our disposal. They also believe that Wendt has been attracted to quantum theory by its "non-reductionist and non-deterministic" character. The latter led him to envisage a 'quantum social science', in which quantum theory "has to be applied in a systematic manner to world politics" (ibid: 159). As for the epistemological goals of this undertaking, they claim that "in his view, such theory holds important implications for developing a new non-foundational epistemology, which might help to further reconcile critical theory with mainstream approaches" (ibid: 159).

A critical step-by-step presentation of Wendt's argument will help clarify its structure and content as well as its shortcomings. Wendt's starting point is that the mind-body problem – in particular, the account of *consciousness* and *intentionality* – is a fundamental problem for social science that has not, as yet, found any satisfactory solution in the philosophical or scientific literature (Wendt 2006a: 185-189). This prompts him to put forward what he acknowledges to be a 'heretical thought': the limitations of both contemporary philosophy of mind and social science stem from the common underlying assumption that the connection between mind ('ideas') and body ('matter') must be informed by the world-view of classical physics (Wendt 2006a: 183). Thus he advances two claims: First, that the quantum consciousness hypothesis – namely, that "consciousness is a macroscopic quantum mechanical phenomenon" – is true. Second, that the quantum nature of consciousness does have implications for social science (Wendt 2006a: 183-184).

Wendt then goes on to devote the rest of his exploration of the quantum underpinnings of social science on the following tasks: First, he presents a non-technical review of the conceptual innovations of quantum theory (Wendt 2006a: 190-193), then he presents the various aspects of the quantum consciousness hypotheses in connection with a variant of panpsychism (Wendt 2006a: 193-196) and he gives an outline of a quantum model of man that allegedly accounts for both teleology in human action and freedom of the will (Wendt 2006a: 197-199). Finally, he gives a sketch of a quantum model of social systems within which one is supposed to get a glimpse of the collective unconscious, the collective consciousness and human interaction (Wendt 2006a: 200-205).

Wendt is clearly aware that his quantum research program in social science is as complex as it is risky. As a consequence, he is content with merely offering an outline of how a possible course of argument for a quantum social science *might* be formulated. Accordingly, it may appear premature to embark on a detailed assessment of Wendt's quantum turn in focus. Indeed, Wendt does not offer detailed arguments, explanations or testable hypotheses. Consequently, one might regard Wendt's whole approach as a hodge-podge of contentious speculations, the one stacked onto the other. Nevertheless, it is not premature to identify some crucial methodological, scientific, and philosophical shortfalls in the 'quantum Wendt', as this will serve as a cautionary alert to any similar intellectual endeavours.

This is precisely the aim of the chapter at hand. Section 2 identifies a number of weaknesses in Wendt's methodology. Section 3 focuses on physics in an attempt to expose Wendt's misconceived basic concepts of quantum theory and his misconstruing of this theory's 'functioning' in the real physical world. Section 4 argues that Wendt is not explicit in his approach as to the metaphysical principles he uses in order to underpin his program. These three sections (2 to 4) constitute a fairly comprehensive investigation with regards to the methodological, purely scientific and metaphysical weaknesses of Wendt's quantum program. In section 5 I will draw readers' attention to the implications of these results for the way Wendt grounds his 'new IR science' on quantum mechanics. Wendt's new explorations in IR Theory will be exclusively dealt with for this reason. I intend, inter alia, to examine Wendt's idea of the international system as a hologram along with a consequent new approach to the 'level of analysis' problem based on the former. Both have been recently presented in his piece, "Flatland: Quantum Mind and Social Science" (Wendt 2010),[169] a sequel to the "Social Theory as Cartesian Science – An auto-critique from a quantum perspective" (Wendt 2006a). It is in this context where lies my undertaking of proving that *Wendt's latest theoretical achievements do not necessarily presume abandonment, but only a revision of scientific realism as the meta-theoretical framework of his differing versions of STIP*.[170] The last section summarizes the conclusions that can be drawn about the prospects of such a tentatively sketched program. To sum up, what is at stake in this chapter is to discuss Wendt's 'quantum physics' and its extensions to social science and the theory of International Relations (IR). The fact that 'quantum Wendt' has not attracted much attention from IR scholars[171] so far may attribute an added value to the aforementioned venture.

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[169]Wendt in his personal webpage (http://mershoncenter.osu.edu/expertise/spotlight/Wendt.htm) informs us that this piece is the basis for his current project, *Quantum Mind and Social Science*. A preliminary outline of this project is to be found in Wendt (2006b).

[170] Wagner and Gebauer (2008), when referring to Wendt (2006a), talk of a "revised *STIP*" or even a "*STIP* 2".

[171] Wagner and Gebauer (2008) make this point at the beginning of their paper ("Judging by the marked absence of responses to Alexander Wendt's *Auto-Critique from a quantum perspective* thus far, it would seem that the academic community have either gone back to their books and are still there, that it would prefer to ignore Wendt's paper as irrelevant to the academic debate,

5.2 Methodological issues

Wendt does not support his claims that facts concerning human consciousness and human interaction are explainable by appeal to quantum theory via the use of precise and detailed arguments. Rather, he *postulates* that human beings and social systems exhibit quantum features (e.g. are described by wave functions subject to collapse) and tries to assess the explanatory gains from such postulates. The assessment relies on weak *analogical* arguments in the context of which aspects of human and social life are compared to aspects of the behaviour of quantum matter. For instance, the transition from an unconscious state to a conscious one is seen as similar to wave-function collapse and the sharing of meanings and knowledge among members of a society is seen as similar to quantum entanglement. The understanding of the mentioned aspects of quantum systems is, in turn, supported by arguments from *authority*: Wendt cites, here and there, views of theorists he takes to be specialists on quantum physics and its philosophical implications. Moreover, Wendt has recourse to *consistency* considerations only on occasion. He claims, for instance, that one reason to take panpsychism seriously is that "it is consistent with quantum theory" (Wendt 2006a: 195). The methodological pitfalls with such stratagems are both grave and multiple.

To begin with the most obvious, mere consistency does not necessarily entail evidence of truth. What's more, a hypothesis like panpsychism, i.e. "the view that something like human consciousness goes all the way down to the sub-

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or that is simply waiting to see what will happen") and repeat it in their conclusions, as well ("This article began by emphasizing the marked lack of responses Wendt's *Auto-Critique* has drawn so far. Oliver Kessler's article in 2007 aside, there has been no meaningful engagement with his modified *STIP*, a deficit that we hope to address in part here"). However, they do not mention the other IR scholar who has seriously embarked on a critical appreciation of the 'quantum Wendt', namely Keeley (2007).

atomic level" (Wendt 2006a: 195) is trivially consistent with *any* (consistent) physical theory, simply because such a theory does not even talk about consciousness. The 'venerable' history of panpsychism in Western philosophy, to which Wendt himself alludes, offers just another indication of this point.

The issue that characterizes more pervasively Wendt's quantum approach to social science, however, is the deployment and use of analogical arguments. Here, again, the methodological fact to be observed is rather simple: similarities alone do not pave the way to explanation. Suppose some entity X is similar to another entity Y with respect to some feature R and suppose, further, we *do* have an explanation of why Y exhibits R. All this need not necessarily imply that we have an explanation of why X exhibits R, since X and Y may be wildly different entities that simply 'happen' to exhibit a formal similarity.[172] In addition, the evaluation of any argument which is based on an analogical methodology must take into account all known relevant similarities and dissimilarities.[173] Lastly, it is widely accepted in the philosophy of science that genuine explanation cannot be exhausted by a mere reduction to the familiar (see, e.g., Salmon 1992: 14).

Wendt's quantum program does not fare much better either for in this case one cannot even talk about 'reduction to the familiar' since quantum theory is

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[172] The early hydrodynamic interpretations of quantum mechanics present a clear example. The interpretations were based on structural similarities between the equations of Schrödinger's wave mechanics and those governing the flow of a nonviscous irrotational fluid subject to conservative forces (e.g. the equation of continuity). Given that the former equations were supposed to govern *atomic* physics whereas the latter were applicable to the idealization of a *continuous* fluid, these interpretations appeared to attempt to account for the behaviour of atoms by appealing to theories that deliberately disregarded atomicity. See, Jammer (1974: 33-38).

[173] This follows from the *requirement of total evidence* for the applications of inductive logic (see Salmon 2007: chapter 4). Wendt seems to overlook this requirement systematically. For example, as Keeley (2007: 427) persuasively argues, there are significant dissimilarities between human decision processes and wave-function collapse: "Sometimes apparent decisions are nowhere near this decisive. Instead, competing strands of thought continue even into the implementation process, while the decision itself represents a botched compromise by contending parties rather than some definite resolution".

notorious for its interpretive puzzles. We do not, as yet, satisfactorily understand the workings of the quantum world and, accordingly, it is extremely doubtful whether an appeal to such a shaky understanding may shed light on queries regarding consciousness, society, international relations, or other social science puzzles. Wendt is, of course, aware of this problem. He writes:

> However, what the quantum is, nobody knows. Which is to say, even though physicists know how to *use* quantum theory, they do not *understand* it, what it is telling us about the nature of reality. Quantum metaphysics is 'under-determined' by its physics… and as such requires an 'interpretation' of the theory. Philosophers have been intensely debating which interpretation is correct since the 1930s, and show no signs of stopping soon. Over a dozen interpretations now exist, with metaphysics that are not only wildly different, but simply wild. (Wendt 2006a: 189-190)

Nevertheless, Wendt still fails to realize the methodological strictures one has to adopt in the face of this situation. Indeed, in undertaking a project like Wendt's, one has to be clear about both the quantum theory and the philosophical interpretation of that theory one relies upon. On the contrary, Wendt remains eclectic and ultimately unclear on both physical and philosophical grounds (see Sections 3 and 4 below).

Arguments by appeal to authority pose another issue. Such arguments are persuasive only if the authority cited is really an authority on the matter under consideration and there is substantial agreement among authorities on the matter at hand (see Salmon 2007: 118-120). But Wendt appears to violate, at places, each of these conditions. Most serious researchers on quantum theory or the philosophy of quantum theory would not recommend Zukav's *The Dancing Wu Li Masters* (Zukav 1979), even as good popular book in which the experimental findings of quantum theory are clearly described (compare Wendt 2006a: 191 and endnote 19).[174] On the other hand, Wendt, whether intentionally or not, regularly brushes aside disagreements among experts and covers up differences between physical concepts. To illustrate this point, consider the following excerpt about consciousness:

> Although there is disagreement among quantum consciousness theorists about where precisely consciousness is 'located' in human beings, in my view the most plausible answer is in the collapse of our wave functions. This process happens continually as we interact with the environment, providing a basis for our experience of a stream of consciousness…. In other words, the desires and beliefs which the rationalist model of man sees as causing behaviour actually do not exist *until* behaviour takes place – before that point the Self is a superposition of multiple and mutually incompatible desires and beliefs. (Wendt 2006a: 197-198)

In the excerpt above, Wendt begins by acknowledging disagreement among quantum consciousness theorists and then sets aside the disagreement in order to put forward his own conjecture. In the very next sentence, he vaguely associates two distinct physical concepts – namely, 'wave-function collapse'

[174] Moreover, there is no final agreement on whether one can comprehend the conceptual issues of quantum theory without immersing oneself into physical and mathematical niceties. For instance, Albert's *Quantum Mechanics and Experience* (1992) is an authoritative and illuminating presentation of these issues with minimal ('high-school') requirements as to the reader's background knowledge in physics and mathematics.

and ‘interaction with the ‘environment’,[175] before he finally goes on to draw some conclusions regarding human decision-making and behaviour.

Due to the aforementioned methodological aspects, Wendt’s quantum conjectures can, at best, be regarded as *metaphors*. It is worth noticing here that this kind of work is not rare in the literature on quantum consciousness. Atmanspacher assures us that it is rather common:

> There are quite a number of accounts discussing quantum theory in relation to consciousness that adopt basic ideas of quantum theory in a purely *metaphorical* manner. Quantum theoretical terms such as entanglement, superposition, collapse, complementarity, and others are used without specific reference to how they are defined precisely and how they are applicable to specific situations. For instance, conscious acts are just *postulated* to be interpretable somehow analogously to physical acts of measurement, or correlations in psychological systems are just *postulated* to be interpretable somehow analogously to physical entanglement. Such accounts may provide fascinating science fiction, and they may even be important to inspire nuclei of ideas to be worked out in detail. But unless such detailed work leads beyond vague metaphors and analogies, they do not yet represent scientific progress. (Atmanspacher 2006: 8)

The excerpt dissects accurately the profile of the work Wendt has produced so far on quantum approaches to the social sciences.

What is striking in Wendt’s case, however, is that his aim is not to provide one more *metaphor* as to the adaptation and use of both quantum mechanical

[175] Here and elsewhere Wendt seems to confound quantum *state-vector reduction* with *decoherence*.

logic and concepts within the theoretical framework of social science and IR theory, as one would eventually have preferred to consider it.

The above is actually the goal of the venture on which Dimitrios Akrivoulis -not Wendt- has embarked in his own PhD dissertation, *The quantum politics metaphor in International Relations - Revising American Newtonianism* (Akrivoulis 2002), which, according to Ben Wagner and Jonas Gebauer, constitutes "the most complete metaphorical consideration of quantum politics to date", for its author "undertakes a long and detailed study of a paradigmatic quantum shift in the political science" (Wagner and Gebauer 2008: 9-10). Akrivoulis, influenced by Rob Walker's writings in IR Theory (cf. Walker 1991 and Walker 1993), also takes on board Ricoeur's seminal philosophical work in hermeneutics. Thus, considering imagination as a dimension of language he differentiates between the social functions of the Newtonian imaginary (which the 'established' social science is presumably based on)[176] and the social functions of the quantum imaginary. He does all this, in order to conclude, among others, that

> It is here that the fundamental ambiguities of a quantum imaginary could be placed beside, juxtaposed with and contrasted to the ones of the Newtonian imaginary. And here the regressive analysis of meaning that we follow, when examining the positive and negative traits of the Newtonian ideology (integration, legitimation, distortion), could also prove helpful in the exploration of the respective traits of a quantum utopia (challenge, possibility, and escapism). Given the above, it is

[176] There are, on the other hand, those who claim that "for conceptual and empirical reasons" the quest for predictive theory in IR "rests on a mistaken analogy between physical and social phenomena", whereas "evolutionary biology is a more productive analogy for social science". In this vein, they have developed 'hard' and 'soft' versions of this analogy, too. For such an approach, see Bernstein, Lebow, Stein and Weber (2000).

> perhaps not difficult to access that, first, whereas the Newtonian imaginary functioned in an integrating manner establishing a kind of social bond and reaffirming a sociopolitical order that was taken as natural and given, a quantum imaginary would aim at denaturalizing this order by subverting or challenging the given forms of social bond and political relating. (Akrivoulis 2007: 10)

On the other hand, in his attempt "to use quantum mechanics as the foundation of a theory of international politics" (Wagner and Gebauer 2008: 10), Wendt has gone far beyond a mere argument in favour of a new *quantum metaphor*. There is no clearer evidence of this claim's truth than the following:

> If the quantum consciousness hypothesis is true then the elementary units of social life, human subjects, are quantum systems – not just metaphorically or by analogy, but really. (Wendt 2006a: 196)

Or, even more precisely,

> This book project explores the implications for social science of thinking about human beings and society as quantum mechanical phenomena. In the past there has been very limited discussion of this question, but only as an intriguing analogy and thus it had essentially no impact. My suggestion is that man (sic) and society really are phenomena. (Wendt 2006b)

While a *metaphor* is usually based on assumptions of the sort "as if", here Wendt appears to neglect the metaphorical or analogical use of the core elements of his theory, by taking them to be real.

The implications of Wendt’s idiosyncratic –to say the least- interpretation of quantum mechanics for the study of the philosophical foundations of social science and IR, make him the sole representative of ‘strong quantum politics’, whereas Akrivoulis, along with others, are defenders of what Wagner and Gebauer (2008: 11) tend to call ‘weak quantum mechanics’. The scholars belonging to the latter category make use of quantum mechanics in a metaphorical manner when studying international politics, while Wendt calls for a ‘new social science’, a ‘new IR Theory’ based *literally* on quantum mechanics.

Arguably, it was these expectations raised by Wendt’s ‘strong quantum politics’ for radically shifting and reorienting modern IR Theory that led Barry Buzan to call his colleagues to arms by advising them to run back to their books and study not only IR Theory and Philosophy of Science but Quantum Mechanics as well, in order for them to be able to come to grips with this new intellectual challenge presented by Wendt’s latest work:

> The good general Wendt finds here an attack that should disorientate his enemies on both fronts, and force the more energetic and creative of them back to their books for some overdue cross-disciplinary re-education. This is ‘new International Relations’ with vengeance. (Buzan 2006: xvi)

More recently, Simon Curtis and Marjo Koivisto (2010) – in the context of their attempt to reformulate the relationship between Science and History in International Theory with the help of the recent alleged transcendence of the assumption of incommensurability between scientific and historical frames of inquiry – make a reference to the role positivism has had and still aspires to maintain as the basic methodology for both natural and social sciences. In the context of the above inquiry, they conclude that Wendt's quantum social science to be a rather promising new positivist alternative:

> The early positivists thus had faith in the unity of science: a single scientific method to gain access to the secrets of both the natural and social worlds. … In their view, human beings were physical objects located in physical space, and, just like other physical objects, they should theoretically be able to be integrated into the physical sciences, with their behaviour deducible from their material needs. This would allow the predictive capacity (and, in the case of historical knowledge, retroductive capacity), necessary for a 'social physics'. The notion that the activities of human beings can be predicted by reference to their physical needs is an essential element of mechanical approaches to formulating behaviourist science. An interesting recent move that has clear similarities with this tradition is Alexander Wendt's fascination with a quantum social theory of international politics. (Curtis & Koivisto 2010: 438-439)

However, although I appreciate Buzan's pioneering work in IR Theory, which made him an authority in the field and whose claims about IR theory must always be carefully considered, and despite the fact that I do not underestimate Curtis' and Koivisto's rather positive stance vis-à-vis Wendt's latest intellectual endeavour, I still remain, at the very least, sceptical in terms of the potential that Wendt's new approach to social sciences has, if it is to be regarded as something more substantial than a mere metaphor.

In order to adduce further support for this claim, I will now turn from methodological to more 'substantive' theoretical matters.

5.3 Wendt's 'Understanding' of Quantum Physics

As already said, the main thesis in Wendt's paper is that the current approach to Social Sciences (SS) and World Politics (WP) is inadequate and that a radically new mental framework is needed. The latter must be based, literally, on Quantum Physics (QP). That means that consciousness results from the brain being a macroscopic quantum system in a coherent state (at least partly); and treating it as such might shed light in the study of SS and WP.

As the author recognizes explicitly, this thesis is highly speculative, to say the least, in both of its two assumptions: (a) that consciousness is a macroscopic quantum mechanical phenomenon, and (b) that this may have implications for SS and WP.

The arguments advanced by Wendt in support of the second assumption can be reduced to the general statement that the inner workings of the human brain (how the brain becomes mind) influence the way we perceive reality and, as a result, influence how the scientific truths are reached by the collective mental processes of the scientific communities. Such a statement is too general to be persuasive. Although it is recognized as a deep fundamental question, it cannot receive a scientific answer at least until the consciousness formation (or, more generally, the brain/mind question) is resolved. Until then, it cannot get a scientific answer even in connection with much simpler (than the study of SS and WP) frameworks, such as the relation between mathematics and physics or our inability to perceive space with more than three dimensions.

I conclude my critique of the assumption (b) in Wendt's thesis by saying that it is way too premature to argue in favour of the influence of the mind-body problem on the way we study SS and WP. Moreover, I will return to this issue in section 4 as well, where I will examine the metaphysics of the mind-body problem in more detail.

Before turning to the arguments that Wendt puts forward in favour of his first assumption (that consciousness is a macroscopic quantum mechanical phenomenon), it is worth pointing out that I will enter the realm of Physics, where concepts have precise mathematical definitions and statements ought to be subject to falsification by quantitative observations/experiments by the flawless operation of existing technology.

5.3.1 Aspects of Quantum Physics Relevant for Wendt's Theory

Wendt starts by stating that the laws of physics impose constraints on SS; this is, of course, true for any science of higher complexity than physics (chemistry, biochemistry, biology, ecology, SS). Then he asserts that the laws of physics can be either classical or quantum and that the former is inadequate for studying the brain/mind problem, because classical mechanics is too materialist to capture the deep essence of consciousness.

As I will point out further on, the laws of physics are quantum mechanical, not classical; period.[177] Classical mechanics becomes simply an adequate approximation to quantum mechanics only under certain limiting circumstances. The structure and the properties of matter at all scales, from the protons and neutrons to atoms, molecules, living cells, ordinary macroscopic solids and liquids, planets, stars, white dwarfs, etc., are what they are because they obey quantum mechanical laws. The universal physical constant $\hbar$, the

[177] The analysis which follows is based on Feynman, Leighton and Sands ([1965] 1989), Griffiths (2002) and Paul (2008).

‘trade mark’ of quantum mechanics, is a determining factor in the structure and properties of all forms of matter. The density, the compressibility, the thermal conductivity, etc., of the simplest macroscopic system, e.g., of a piece of metal, such as a copper wire, are what they are because of quantum mechanics; their values involve the universal quantum constant $\hbar$. If classical mechanics were true, matter, living or not, would not exist; the whole world would eventually collapse to one or more black hole-like entities.

The widely accepted statement, apparently adopted by Wendt, that the microscopic world is the realm of quantum mechanics, while the macroscopic one is the realm of classical mechanics, is misleading. The correct statement is that classical mechanics fails completely to account for *microscopic motions*, while it is a fully adequate approximation for *macroscopic motions only* (not properties) of both *macroscopic and microscopic entities*. Keep in mind though that as a result of the atomic structure of matter, *microscopic motions determine the structure, the properties and the functioning, if any, of macroscopic objects*. This is the reason the world is at all scales quantum mechanical. Consequently, the statement that the human brain functions by obeying the laws of quantum mechanics is as obvious to a research physicist as the statement that the *properties* of a piece of metal are determined by quantum mechanical laws. What is not trivial is the highly speculative and intriguing idea that parts of the brain are in a *coherent* quantum state and, if this is so, it may have implications for the brain/mind problem.

One may wonder what a *coherent* quantum state is. To provide any reasonable (no matter how imprecise as a result of not using the mathematical language) answer to this question, one needs to present a few basic aspects of the quantum theory. To this end, I will follow Wendt's suggestion to introduce quantum mechanics by contrasting it to classical mechanics, and then I will present my critique of Wendt's views.[178]

In classical physics two fundamental entities are introduced: elementary particles and waves. Elementary particles are local point-like objects, indivisible and discrete (in the sense that their number in any system is obviously a non-negative integer). Particles in their motion follow a trajectory (or an orbit, if you prefer) which is a mathematical line (not necessarily a straight one); by knowing *the position and the velocity* at any point and the total force acting on the particle, then its trajectory is uniquely determined.

In contrast, waves, which exist in a medium (continuous or discrete) and even in vacuum, are non-local (in the sense that they have a non-zero extent in space) and are of continuous nature (a macroscopic classical wave does not consist of a non-negative number of elementary constituents); their motion is governed by partial differential equations. Finally, the waves exhibit the characteristic wavy feature of *interference* (the waves starting from different sources may cancel each other at the same regions of space, while elsewhere they may overstrengthen their sum). The phenomenon of wave interference is easily visible by throwing two stones in the quiet waters of a lake and watching what happens as the two concentric ripples meet each other. However, if we throw a large number of stones randomly, the resulting waves are so complicated and disordered that the interference phenomenon is not visible anymore. For the same reason we cannot see the interference in the light coming out from two different ordinary lamps; the waves arriving from these

[178] However, for a clearer and more detailed introduction to the basics of quantum mechanics see Feynman's description of the two slit *Gedankenexperiment* in "Quantum Behavior", which can be found in Feynman , Leighton and Sands ([1965] 1989: 1-1 – 1-11).

two lamps are disordered superpositions of many uncorrelated waves, so that the interference cannot become visible. In contrast, if we split the beam from a laser into two sub-beams, the interference of light waves becomes clearly visible as alternating dark and bright regions on the screen. If each of two waves is the superposition of many 'ordered' and 'correlated' waves so that the interference phenomenon is observable, we call each wave *coherent*; if each of the two waves is a 'disordered' and 'uncorrelated' superposition of waves as to make the interference unobservable, is called *decoherent*. A coherent wave can become decoherent through interactions with the *environment*; we say then that the wave 'decoheres'.

To summarize: According to classical mechanics, elementary particles are local, indivisible, discrete entities, following a trajectory in their motion; waves are non-local, continuous entities exhibiting the phenomenon of interference.

However, all experimental facts show that:

(a) Actual elementary particles (i.e., the ones existing in Nature) are point-like indivisible, discrete entities which, however, do not follow a trajectory in their motion but they move as waves exhibiting the phenomenon of *interference*. Thus, actual particles have some properties of classical particles and some properties of classical waves. This is the reason they are named wave-particles.

(b) Actual waves (i.e., the ones existing in Nature) move as classical waves exhibiting the phenomenon of *interference*, but they consist of elementary indivisible, discrete units. Thus, the actual waves have some properties of classical waves and some properties of classical particles. It is only reasonable to name them particle-waves.

The conclusion is the so-called *wave-particle duality*, one of the two cornerstones of modern science (the other is the atomic structure of matter).

The main task of physics, following these experimental facts, was to construct a coherent (mathematical) formalism, which will merge together particle and wave features that seem mutually exclusive. Quantum mechanics achieves just that but at a price:

(a) Instead of the familiar concept of trajectory, a wave function is employed for the description of the motion.

(b) The physical information extracted from the wave function is of a probabilistic nature (although 100% probability, i.e. certainty, is not unusual). E.g., if you measure the orientation of the spin of the electron, quantum mechanics may predict that the result will be spin-up with probability, let us say, 70%, and spin-down with probability 30%. Before making the measurement, both eventualities (70% up, 30% down) are present; if the result of the measurement is, e.g., spin-down, from this point on the electron will be in a state with spin-down and the next measurement of its spin will be spin-down with probability 100% (i.e., with certainty).

(c) This feature is the third part of the price paid by merging together seemingly exclusive particle and wave properties. The measurement affects the measured system by forcing it to be immediately after the measurement in a state consistent with the results of the measurement. This is known as the *wave-function collapse* (the term *collapse* is more appropriate for measuring the position; here, however, we are using the term for any measurement). Probably the most striking and counterintuitive aspect of the role of measurement in QM appears in entangled systems. Two systems A and B are said to be entangled if their quantum states are correlated - that is, not separable (see Economou 2012). A famous example of entangled systems was considered by Einstein et al. in the renowned 'EPR paper' (Einstein, Podolsky & Rosen 1935), in which a particle decays into two photons moving in opposite directions and having opposite spins, meaning that A is up while B is down and A is down while B is up. The result of measuring the spin of B will be either up or down with a probability of 50%. However, if the measurement of B is done immediately after A has been found to be down, then the result will be up with certainty, in spite of A and B being separated by arbitrary large distance. This 'spooky action at a distance' led Einstein to question the completeness of quantum theory as representing the physical reality.[179] Needless to say that the *Gedankenexperiment* proposed by EPR was actually

[179] For instance, a very recent and representative example of the ongoing discussion on these issues comes from a paper written by Pussey, Barrett & Rudolph (2012). Its title reads "On the reality of the quantum state". Let us see how they summarize their main argument in the beginning of this publication:

> Quantum states are the key mathematical objects in quantum theory. It is therefore surprising that physicists have been unable to agree on what a quantum state truly represents. One possibility is that a pure quantum state corresponds directly to reality. However, there is a long history of suggestions that a quantum state (even a pure state) represents only knowledge or information about some aspect of reality. Here we show that any model in which a quantum state represents mere information about an underlying physical state of the system, and in which systems that are prepared independently have independent physical states, must make predictions that contradict those of quantum theory (Pussey, Barrett & Rudolph 2012: 475).

In other words, this work is based on two assumptions. The first assumption is that there is a 'real physical state' of a system, which is objective and independent of the observer. The second is that systems prepared independently have independent real physical states. These rather obvious assumptions lead to the following no-go theorem: If the quantum state (ψ) merely represents information about the real physical state of a system, then the experimental predictions obtained contradict those of quantum theory. Therefore, the authors conclude that the wave function (ψ) is real in the sense that it does not represent mere information about an underlying physical state of the system, but is the physical state itself of the system.

The philosophical implications of this paper for scientific realism (SR) are inextricable to the extent that the central conclusion of the paper concerns the very nature of the mathematical skeleton of a fundamental physical theory, such as quantum mechanics. As has been shown in the second chapter of this thesis, this issue belongs to the core of the problematique of SR in the philosophy of science.

performed in 1982 by Aspect et al. (Aspect, Grangier & Roger 1982) and the resulting data were in full agreement with the predictions of QM.

Let me now show why wave-particle duality is of fundamental importance in accounting for the world structure (in the framework of the atomic idea). According to the latter, everything is made up of indivisible, microscopic wave-particles, which attract each other through the action of interactions (which are made of particle-waves). From the level of atomic nuclei all the way to the level of an asteroid there are three types of wave-particles: protons, neutrons (organized into nuclei through the action of the strong interactions) and electrons (trapped around the various nuclei by the action of the electromagnetic interactions to form electrically neutral atoms). Atoms make molecules, ordinary solid and liquid matter, living organism, etc. However, the interactions, as a result of their overall attractive nature, would squeeze all these structures to one or more black-hole-like entities, if left unopposed. What opposes the interactions and prevents their catastrophic squeezing, making, thus, the existence of matter inside and outside us possible? It is just the wave/particle duality! This duality leads to Heisenberg's uncertainty principle, which in turn implies that α wave-particle trapped within a finite volume acquires necessarily non-zero kinetic energy; the latter cannot be smaller than a value proportional to $\hbar^2$ and inversely proportional to the product of the mass of the wave-particle and the volume to the power of two thirds.

The final fundamental conclusion is that the various structures of matter exist, because the squeezing pressure of the interaction is counterbalanced by the expansive pressure of the quantum kinetic energy. This picture of the world can be easily tested quantitatively by comparing the values it produces for various quantities (such as the size of atoms, the length of chemical bonds, the density of various solids, etc.) against the corresponding experimental values; notice that, if we set $\hbar$=0, namely if we return to classical mechanics, the kinetic energy will disappear and the attractive interactions would be left unopposed to squeeze the matter to non-existence.

In spite of the phenomenal quantitative successes of quantum mechanics in passing all experimental tests (and there is a huge number of them), including the daily flawless functioning of millions of devices based on it, still it is not easy to 'swallow' its foundation, i.e., the particle/wave duality, the probabilistic nature of its predictions, and the measurement effect (the collapse of the wave function). Everyone would like to see a formulation compatible with our perceptions which, nevertheless, would incorporate the observed wave/particle duality. Such a formulation has not been achieved and not for the lack of trying.

It is obvious that quantum mechanics is a vast field within Physics and a presentation that does it full justice is way beyond the scope of the present work. For my purposes, nevertheless, the above presentation of the basics of quantum mechanics, albeit incomplete, suffices.

5.3.2 A Critique of Wendt's View of Quantum Theory

I will now return to Wendt's views and present my critique of them, by means of elaborating on a number of very specific shortfalls that Wendt's use of quantum mechanics exhibits.

1. At the start of his brief exposition of quantum mechanics, Wendt makes the following statement:

> Quantum theory is perhaps best introduced by the classical worldview that it overthrew. Like quantum metaphysics, the classical worldview is an *interpretation* of physical theory, in this case classical physics, and as such essentially metaphysical. It makes five basic assumptions: 1) that the elementary units of reality are physical objects (materialism); 2) that larger objects can be reduced to smaller ones (reductionism); 3) that objects behave in law-like ways (determinism); 4) that causation is mechanical and local (mechanism); and 5) that objects exist independent of the subjects who observe them (objectivism?). In philosophy of mind these assumptions are shared by materialists, dualists, and proponents of the linguistic turn alike, and thus by extension by most positivists and interpretivists in social science.
>
> Quantum theory challenges all five. At the sub-atomic level physical objects dissolve into ghost-like processes; wholes cannot be reduced to parts; the world does not behave deterministically; causation is non-local; and objects do not exist independent of the subjects who observe them. Importantly, these findings do not necessarily invalidate the classical worldview at the macro level, since quantum states normally 'decohere' into classical ones above the molecular level, which is why the everyday world appears to us as classical. Decoherence has been a barrier to developing a unified quantum theory encompassing both micro and macro levels, and is a fundamental obstacle to the quantum consciousness hypothesis in particular (see below). But at least at the micro-level the quantum revolution has decisively overturned the claim of the classical worldview to provide a complete description of reality. (Wendt 2006a: 190-191)

My criticism goes as follows:

(a) Atoms and electrons do not dissolve into ghost-like processes. On the contrary, they become 'more real' as quantum science and technology advance: one electron-transistor has been already obtained experimentally; the electron-spin is manipulated so as to produce giant-magnetoresistance (Nobel Prize 2007), a discovery that has already found practical use in modern computers. One-electron spin is controlled by laser beams in order to act as quantum bit (qubit) in quantum information processes; electrons in properly designed metal-insulator interfaces have given rise to the new field of plasmonics; coherent flows of electrons in superconductors exhibit interference and quantization at a macroscopic scale; atoms are cooled down and produce coherent beams exhibiting interference and a host of other impressive effects; atoms have been photographed and manipulated one by one; etc. All these phenomena are natural consequences of the fact that the atomic and the subatomic wave-particles obey quantum laws. We can say that it is quantum mechanics that allows us to test and confirm the reality of these microscopic particles in so many and ever expanding ways.

(b) Wholes *are* reduced to parts. Reductionism is the day-by-day tool of research physicists working within the framework of the atomic idea and employing – of course – quantum mechanics.

(c) The question of determinism in quantum mechanics has two sides: the wave function itself develops in time deterministically (the present determines the future), until a measurement occurs. The measurement is essentially the interaction of the quantum system (which obeys the fundamental uncertainty principle) with an object obeying classical physics, i.e. one that is not subject to the restrictions of the uncertainty principle (Landau & Lifshitz 1977: 2-4). Thus, while measuring very accurately the position of a quantum particle, we have to transfer to it a large amount of momentum so that the uncertainty in the momentum becomes very large as dictated by the uncertainty principle. In general, it can be said that a measurement acts as filter for the various eventualities incorporated in the wave function: the ones which are inconsistent with the outcome of the measurement are eliminated, while the ones which do not contradict with the outcome are allowed to pass to the future. As a result of this fundamental feature of the measurement in QM, the wave function changes discontinuously as to be consistent with the result of the measurement. The prediction of this outcome is in general of a probabilistic nature, for example we can predict that the measurement of the energy of the carbon atomic system being in a hybrid sp^1 state will give this value with such a probability. Of course, there are cases where the combination of wave function and the measurement procedure is such that all the eventualities present in the wave function are allowed to pass. In such cases we can predict the outcome with certainty. This is why research physicists do not doubt the existence of the electron.

(d) Classical physics is only a very satisfactory approximation to quantum mechanics when certain limiting conditions are satisfied. The world is quantum at all levels.

(e) Decoherence is a common wave phenomenon appearing within both the classical formalism and the quantum reality; and it does not prevent quantum mechanics to be a unified theory at all levels, micro-, macro-, mega-.

2. Furthermore, Wendt's reference (2006a: 191) to the "four findings from quantum theory: wave-particle duality, wave function collapse, the measurement problem, and non-locality" needs, in my view, revisions because:

(a) Wave/particle duality is not a finding from quantum theory; it is an undeniable experimental fact that had to be incorporated in any acceptable physical theory and –of course- in the quantum theory.

(b) Wave function collapse and the measurement problem are essentially the same thing; finally, the non-locality stems from the wave-like propagation of wave-particles, i.e. from the wave /particle duality.

3. Wendt states also that

> Wave-particle duality refers to the fact that sub-atomic phenomena have two irreducible and non-equivalent descriptions. Under some experimental conditions they are best described as waves, in others as particles. (Wendt 2006a: 191)

I would like to comment here that quantum mechanics is a complete self-consistent theory, which does not need classical mechanics in order to produce its quantitative predictions. In the cases where quantum mechanics is satisfactorily approximated by classical mechanics, the use of classical mechanics is preferred since the latter reduces significantly the calculational effort.

4. Regarding Wendt's statement that

> Wave-particle duality challenges two assumptions of the classical worldview. One is that science can achieve an integrated, unitary Truth about the world… The other challenge is to the materialist view of matter. (ibid: 191)

I have the following remarks:

(a) Wave/particle duality and quantum mechanics do not challenge the first assumption of the classical worldview. On the contrary, it goes a long way towards making reality what was a dream before. This follows from the fact that quantum mechanics extended the subject matter of physics from macroscopic motions (to which classical mechanics is restricted) to all kinds of microscopic motions and, as a result, to the structure and properties of protons, neutrons, nuclei, atoms, molecules, solids, liquids, planets, stars, dead stars all the way to the universe; it made also possible the emergence of cosmology as a serious scientific subject.

(b) Quantum mechanics is based on the picture that the material world consists exclusively of elementary point-like material particles which nevertheless propagate as waves, while they retain their point-like property all along. By accepting this picture (which does imply the Uncertainty Principle) we obtain a tremendous amount of information and 'understanding' about the world. Here, I find it appropriate to quote the brilliant physicist Richard P. Feynman:

> If, in some cataclysm, all of scientific knowledge were to be destroyed, and only one sentence passed on to the next generation of creatures, what statement would contain the most information in the fewest words?
>
> I believe it is the atomic hypothesis that all things are made of atoms–little particles that move around in perpetual motion, attracting each other when they are

> a little distance apart, but repelling upon being squeezed into one another. In that one sentence, there is an enormous amount of information about the world, if just a little imagination and thinking are applied. (Feynman 2007: 4)

5. Wendt continues the deployment of his ideas by presenting us with the following:

> Wave function collapse refers to the fact that the transition from wave to particle is instantaneous in time and has no apparent physical cause. Such 'quantum leaps' challenge the determinism of the classical worldview, and as such have caused much angst among physicists, with Einstein famously complaining that 'God does not play dice'. But their anomalous character also points toward a possible solution, since wave function collapse is strongly analogous to our experience of consciousness, which involves free will and also does not seem to have a physical cause – an analogy that the quantum consciousness hypothesis will exploit.
>
> The measurement problem refers to the fact that it is impossible to measure quantum phenomena without disturbing them: the process of measurement inevitably leads to a change in the appropriate description of sub-atomic particles. As long as we don't measure them they appear as waves, and as soon as we do as particles. This challenges another basic assumption of the classical worldview, the subject-object distinction, and with it the possibility, even in principle, of true objectivity. In quantum measurement observer and observed initially constitute a single system, rather than two as they are classically. Far from being just a given, the subject-object distinction is now emergent from the process of measurement itself, which makes a "cut" in a previously undivided whole. Within social science post-modernists, feminists, and others have made similar critiques of the subject-object distinction at the macro-level, but generally without a quantum basis. A

> quantum connection would give these critiques additional force, and point toward the necessity of a participatory epistemology in social inquiry. (Wendt 2006a: 192)

However, the wave function collapse, as a result of a measurement, does not imply an instantaneous transition from wave to particle, as Wendt seems to claim in the above passage. The particle *per se* remains unaffected by the measurement; its state of motion as described by the wave function may change abruptly from the wave function before to another wave function so as to be consistent with the results of the measurement. The measurement of the position does not mean that a wave is transformed to a particle. It simply means that the change of the wave function to a small spot (as a result of the position measurement) revealed the particle aspect which existed all along. Wendt's unconventional picture of the elementary particles seems to come from his implicit identification of the measurement in general with the *position measurement;* actually, in almost all cases, we measure other (than the position) quantities (which are more relevant from the observation point of view). (By the way, the connection of the wave/particle duality with some feminists' post-modern critiques of the subject-object distinction at the macro-level might have remained unconceivable, had it not been for Wendt's idiosyncratic representation of quantum mechanics!).

6. Moreover, in the subject of Wendt's arguments about quantum mechanics, I cannot but point to the following statement since, in my view, it contains some misconceptions:

> Findings like these overthrew the classical worldview at the micro-level, but not at the macro, where classical thinking still dominates. The reason is decoherence. As we have seen measuring sub-atomic systems interferes with them, collapsing quantum waves into classical particles. Importantly, this applies not just to measurements in physicists' laboratories, but in nature everywhere. Whenever particles interact they are in effect 'measuring' each other, inducing decoherence. That's why in everyday life we see only material objects, not wave functions; quantum effects quickly wash out beyond the molecular level, leaving classical mechanics as the appropriate description at the macro. (ibid: 193)

First, there are macroscopic systems (consisting of interacting microscopic particles) which are coherent and –as a result- exhibit interference and quantization phenomena. Liquid Helium 4 below the temperature of 2.18º K is one example; electric currents in superconductors are another - there are commercially available devices called SQUIDs (Superconducting Quantum Interference Devices) which are based on macroscopic coherent systems. The reasons that ordinary macroscopic systems decohere is not due to the interactions among their constituent particles. Interaction with the *environment* is the main cause of decoherence. There are also other cases where the direct test of coherence cannot take place. E.g., in the case of ordinary solids, the possibility of the interference test is *a priori* excluded, since they cannot both occupy the same volume at the same time.

7. It is self-evidently incorrect to identify the process of measurement in general with the emergence of decoherence, as Wendt does in the following sentences:

> Quantum brain theorists are trying to bridge the yawning gap between sub-atomic particles and the whole brain in such a way that quantum coherence might be transferred from the former to the latter. The key problem is identifying physical structures in the brain whose properties will insulate particles from measuring (and thus collapsing) each other, while simultaneously enabling them to be entangled (and thus having coherence). (ibid: 193)

8. Furthermore, Wendt states that:

> For this purpose they are using quantum field theory – a generalization of quantum theory that deals with macroscopic phenomena – to model brain activity. (ibid: 194)

It must be clarified with respect to the above that quantum field theory completes quantum mechanics by adding particle properties to classical waves. Thus, quantum field theory is a unifying quantum formalism which treats on equal footing both 'material' (e.g. electrons) and 'interaction' (e.g. photons) particles. It is a convenient formalism for many-body systems.

9. Moreover, Wendt's attempt to connect quantum mechanics with panpsychist ontology lacks clarity. The leap towards which Wendt aims cannot be adequately substantiated by an under-elaborated statement of intent:

> For that we need to replace the materialist view of matter that underlies classical approaches to the mind-body problem with a panpsychist ontology, which is the

view that something like human consciousness goes all the way down to the sub-atomic level. (ibid: 195)

10. Furthermore, in the same page Wendt goes on to argue that:

> The question of how consciousness emerges from matter is therefore spurious, since in some sense it is there all along. What is emergent is rather the *distinction* between consciousness and purely physical matter from an underlying reality that is neither. David Bohm (1980) calls this underlying reality the 'implicate order,' as distinct from the 'explicate' order of physical matter and consciousness. As such, panpsychism might also be described as a 'dual aspect' or 'neutral' monism. (ibid: 195)

The above passage, besides being subject to the claim that it lacks clarity as much as the excerpt in point 9 above, also leads to questions such as "Has an electron consciousness? In what sense? Has quantum mechanics anything to do with these questions?". These questions are never addressed or elaborated, even for the sake of clarifying whether they are worthy of elaboration.

11. Wendt goes on to connect mental processes such as consciousness, or the lack of it, with coherence and decoherence, respectively. This, taken at face value, ought to have one conclude that a Helium 4 nucleus (the so-called alpha particle) has consciousness, since it is in a coherent state:

> But are we then to believe that rocks have consciousness? No, because of decoherence. From a quantum perspective, part of what constitutes life may be the ability to maintain coherence in a multi-particle system. (ibid: 196)

Coherence, or the lack of it, is not the basis for distinguishing living systems from the non-living ones. Both, since they are multi-particle systems held together mainly by electromagnetic forces, are generally in an incoherent state as a result of their energy-level spacing being much smaller than the interaction energy with the environment. This does not exclude the possibility that some particular subsystems of a biologically advanced species may be in a coherent state, at least for a short time. Because they lack experimental or detailed theoretical support, such speculations, which have been proposed in the literature, are not generally accepted by the relevant scientific community.

12. Wendt's 2010 paper represents his version of a 'quantum social science' along the lines that had already been laid out in his earlier work (Wendt 2006a). Were it not for the 'holographic principle', which Wendt invokes here in order to supply his analysis of the international political system with a new theoretical tool, we shouldn't have added anything more to the previous critical comments on Wendt's misunderstanding of quantum mechanics. Although I find a direct 'transplantation' of the 'holographic principle' from Physics into the realm of IR theorising incomprehensible, Wendt argues to the contrary; that is, he argues for its adaptation in IR Theory as a valid scientific – and not merely a metaphorical – analogy:

> The holographic principle does not depend on any particular medium for its realization and as such is highly general. It has been argued by some cosmologists, for example, that the entire universe is a holographic projection, not just metaphorically but literally (for example, Susskind 1995). This puts us in a good position to extend the holographic perspective to IR, where the mechanisms and medium go beyond mere visual perception of physical objects. (Wendt 2010: 290)[180]

One might respond to this claim by taking refuge in Susskind's own words when explaining to the *Scientific American's* audience what this principle is all about:

> *Question:* If I get you correctly, the holographic principle extends the complementary model of a black hole to the universe.

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[180] In fact, in order to clarify things, I should say that Wendt's (2010) enrichment of his earlier 'quantum consciousness hypothesis' with the inclusion of the 'holographic principle', was due to the inspiration he drew from the work of the neurophysiologist Karl Pribram on 'visual perception and the holographic brain':

> The conventional view of visual perception is representational: light refracted of external objects pours via the eyes into the brain, which uses the information in the light to form cognitive representations of the objects. This view is very classical in its assumption that there is a clear ontological distinction between subject and object, and that perception takes place locally, within the brain. *In contrast, the quantum consciousness hypothesis suggests a holographic model of perception, which was first put forward in the 1960s by Karl Pribram, who was trying to explain why we experience objects as 'out there' in the world even though their images are actually on our retinas*. (Wendt 2010: 287, the italics are mine).

The idea that Wendt found so attractive in Pribram's works (Pribram 1971, 1986) is that in the brain, just as within a holographic image, each part contains the whole within it; an interesting, however controversial, scientific image of the functioning of the human brain. The idea of the 'holographic universe', as that has been framed by the distinct works of Pribram and the renowned physicist David Bohm, is also drawn upon by the widely discussed book of Michael Talbot, *The Holographic Universe: The Revolutionary Theory of Reality* (Talbot 2011). However, this work, which explores the metaphysical implications of quantum physics and argues that a holographic model of the universe could explain supersymmetry, various paranormal and anomalous phenomena and can act as the basis for mystical experiences, seems to belong, as many others of the same sort, to the so-called 'quantum mysticism' literature. As such, it probably has no place in any serious discussion about either modern interdisciplinary brain/consciousness studies or the metaphysical implications of quantum theory.

Answer: Yes. Suppose we want to describe some system with enormous precision. To probe with great precision, you need high energy. What's eventually going to happen as you try to get more and more precise is you're going to start creating black holes. The information in a black hole is all on the surface of the black hole. So the more and more refined description you make of a system, you will wind up placing the information at a boundary.

There are two descriptions of reality: either reality is the bulk of spacetime surrounded by the boundary, or reality is the area of the boundary. So which description is real? There is no way to answer it. We can either think of an object as an object in the bulk space or think of it as complicated, scrambled collection of information on the boundary that surrounds it. Not both. It's an incredibly scrambled mapping of one thing to the other thing. (Susskind 2011: 67)[181]

In concluding this section, I would like to stress that quantum mechanics is a physical theory and, as such, it must be judged by its ability to account for as many observations and experiments as possible in all scales of the natural world and the man-made structures. Quantum mechanics is phenomenally successful in this respect, having extended the subject matter of physics from the classical realm of studying macroscopic motions to *almost* everything from

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[181] As Susskind argues, his 'holographic principle' is an extension of the complementary model of a black hole to the universe. The more deeply one conceives the latter, the better he understands the former. The American physicist explains the complementary model of a black hole as follows (Susskind 2011: 66-67): "It turns out that the mathematics of the event horizon of a black hole is very similar to the uncertainty principle. Again, it's a question of 'or' versus 'and'. At a completely classical level something falls into a black hole, something doesn't fall into a black hole, whatever. There are things outside the black hole, and are things inside the black hole. What we learned is that's the wrong way to think. Don't try to think of things happening outside the horizon and things happening inside the horizon. They're redundant descriptions of the same thing. You describe it one way, or you describe it the other way. This means we have to give up the old idea that a bit of information is in a definite place [see "Black Holes and the Information Paradox," by Leonard Susskind; *SCIENTIFIC AMERICAN*; April 1997]". One without advanced technical knowledge of Mathematics and Physics may also consult Chapter 18 ("The Universe as a Hologram") of Susskind's 2004 book, *The Black Hole War – My battle with Stephen Hawking to make the world safe*. On the other hand, a mathematical physicist may enjoy reading Roger Penrose's brilliant exposition of the 'holographic principle' in his masterpiece, *The Road to Reality* (Penrose 2004: 920-923).

the quarks to the Universe. However, the methodology of Physics (controlled experiments, quantitative character, use of mathematical language to derive results starting from a few basic relations) imposes severe restrictions regarding which systems can be studied profitably by physical methods. For instance, physicists have just started to calculate large biological molecules which participate in the structure and the function of cells. Convincing calculations of more complex systems such as a cell are out of reach for the time being. This is obviously more so for the human brain and, certainly, for the structures and functions of the human societies. Any approaches to these subjects using quantum mechanics, including Wendt’s paper, cannot be considered scientific statements/conjectures in the Popperian sense, simply because they are not falsifiable.[182]

5.4 The Mind-Body Problem in Wendt’s Theory

The motivation behind Wendt's embarking on his quantum programme towards a quantum social science originated in the mind-body problem. It is common knowledge that there have been various philosophical angles on this problem since Descartes, whose writings shaped the way the problem is understood in modern and contemporary philosophy. One comes across monistic approaches, whether idealist or materialist (physicalist), dualistic approaches, interactionist or not, functionalist approaches, etc.[183] Wendt does not express *any*, even programmatic, preference among all these. Instead, he claims that all these approaches ultimately suffer from one common shortcoming: they are based on the world-view suggested by classical physics. Then he goes on to indicate

[182] See Popper ([1957] 1997), as well as Chapter 1 (“Science: Conjectures and Refutations”) and Chapter 10 (“Truth, rationality, and the growth of scientific knowledge”) in Popper ([1963] 1996) .

[183] See, for example, Kim (1998).

how, in his view, a quantum approach to consciousness can overcome this hurdle and solve the mind-body problem.

But this is misleading on several scores. To begin with, there does not seem to be a unique world-view bequeathed to us by classical physic.[184] Furthermore, the various quantum approaches to consciousness entertain different philosophical background assumptions as to how the mental is 'related' to the material or the physical. I will now take up these points in turn.

As any serious student of the history of physics will attest, the development of thermodynamics and electromagnetic theory during the 19th century guided physics toward a rejection of the mechanistic world-view that dominated the 17th century Scientific Revolution. In fact, it is normal to maintain that classical physics has bequeathed to us *two* radically different metaphysical schemes. The *atomistic metaphysics* recognizes a plurality of fundamental substances with diachronic identity (continuants), which is seated in their 'primitive thisness'[185] or ascertained by appeal to the continuity of their spatiotemporal trajectories. By contrast, the *plenum metaphysics* recognizes a single fundamental substance ('ether'), which can manifest quality complexes with no diachronic identity (ephemerals). Discreteness and continuity are, respectively, the cardinal images. Particle theories and field theories, with Newtonian

[184] Or, even if there were just one, Wendt's depiction of it would not be entirely accurate. For instance, Wendt (2006: 190) claims that one of the basic tenets supporting what he takes to be "the classical world-view" is that "objects behave in law-like ways (determinism)". Of course, a world bound by laws of nature need not necessarily be a deterministic world. In addition, it has been established that even the world of Newtonian particle mechanics and gravitation is not a safe haven for determinism. On such matters see Earman (1986).

[185] Primitive thisness is meant as a principle of individuation that transcends an entity's properties. The traditional term, derived from Scholastic vocabulary, is 'haecceity'.

mechanics and Maxwellian electromagnetism as the corresponding exemplars, have served to typify the atomistic and the plenum metaphysics respectively.[186]

None of these metaphysical schemes has survived, intact or unaltered, the advent of quantum theories. The notorious 'wave-particle duality', manifested by quantum systems, is actually an attestation of this fact. Of course, this diagnosis of the problem differs radically from the one offered by Wendt:

> Wave-particle duality challenges two assumptions of the classical worldview. One is that science can achieve an integrated, unitary Truth about the world. Quantum theory seems to be true, but its truth requires contradictory narratives … The other challenge is to the materialist view of matter. (Wendt 2006a: 191)

With respect to what Wendt cites as the first challenge, the expression 'wave-particle duality' refers to the capacity of a single quantum mechanical system to manifest behaviour similar to that of *classical* waves or to that of *classical* particles under diverse experimental conditions. The possession of this capacity prohibits the possibility of giving a unified description of quantum entities with *classical* concepts or images. Still, there might be such a unified description that relies on *non-classical* concepts or images. Much of the philosophical work on quantum theories aims at articulating precisely such a description.[187] What's more, the thesis that science can achieve an "integrated, unitary Truth about the world" is not, properly speaking, part of any physical theory, whether

[186] This sketch of the distinction is oversimplified on several scores. For one thing, ether theories have not invariably been embedded into a monistic ontology; dualistic theories (ether plus material particles) abound in the repertoire of classical physics.
[187] Admittedly, this task has not yet been achieved. Moreover, many interpreters of quantum theories do not share the commitment to such a task. Notably, Bohr ([1949] 1983) insisted that the concepts of classical physics are indispensable for the description of experiments on quantum systems.

classical or not. It is, rather, part of a *realist stance* toward scientific theories. And almost every physical theory, including quantum mechanics, is subject to both realist and antirealist readings.[188]

Turning now to the variety of philosophical background assumptions embraced by the various quantum approaches to consciousness, one can readily discern the following distinction (cf. Atmanspacher 2006). Some of those approaches aim at explaining the – at least apparent – interaction between mental and material entities by positing some *direct relation* between the two. In general, this relation might be one of reduction, emergence, or supervenience supporting either a monistic or a dualistic ontology. Other approaches posit *indirect* relations between mind and matter mediated by some third, psychophysically neutral, category of entities. On such approaches, mind and matter are conceived as *dual aspects* or effects of a common underlying reality.

Of course, both the direct-relation and the dual-aspect scheme draw their ancestry from the history of Western philosophy long before the advent of quantum physics. Descartes and Spinoza, respectively, defended such schemes – to mention just two of the 'greats'. At this point, it is useful to examine how various quantum approaches to consciousness stand vis-à-vis this distinction. On the one hand, Wigner ([1961] 1983) appears to suggest a direct-relation dualistic scheme. On the other hand, Stapp (1999: 159) talks about a 'hybrid ontology' which possesses 'both idea-like and matter-like qualities' and, as a consequence, intimidates a dual-aspect scheme. By contrast, the approach elaborated by Vitiello (1995, 2001, 2002) belongs to the direct-relation

[188] Again, the relevant literature contains numerous arguments to the conclusion that quantum mechanics signals the demise of realism. But these arguments rest on narrow construals of what realism amounts to.

physicalist variety. And the same is true for that proposed by Penrose (1989, 1994a, 1997a and 2011). Lastly, Bohm ([1980] 1997: 209) affirms explicitly the dual-aspect orientation of his approach by writing: "… we do not say that mind and matter causally affect each other, but rather that the movements of both are the outcome of related projections of a higher-dimensional ground".

In conclusion, Wendt's programme towards a quantum social science lacks specific sound metaphysical underpinnings. Its metaphysical assumptions concerning matter conceal a number of conceptual confusions; moreover, its metaphysical assumptions concerning the relation of mind to matter remain promiscuously eclectic.

5.5 The Obstacles Towards a New Science of International Relations

As should be clear by now from the aforementioned analysis, Wendt (2006a) and Wendt (2010) are the first, and so far the only, steps of his new project in development, *Quantum Mind and Social Science.* In that, they represent the direction he intends to follow in the process of actually writing it up.

Wendt seems to think now that his previous two efforts to combine positivist and interpretivist methods of inquiry in social science and IR theory through the means of his 'via media' (Wendt 2000) proved to be less successful than initially hoped. This is mainly due to the remaining unsolved problem of consciousness which is directly connected to that of the mind-body (in the Cartesian dualistic scheme) and comprises the essence of human behaviour as the basis for any reasonable social science. Wendt undertakes the enterprise of building a new quantum social science, not in order to pass this issue by or transcend it, but in order to resolve it via his 'quantum consciousness hypothesis' (QCH).

As discussed in the previous chapter, Wendt's first effort had the 'agent-structure issue' as the point of its departure (Wendt 1987). In order to resolve this 'open problem', Wendt brought SR to the fore, because of the following three assumptions which SR entailed and which he considered to be connected with the purposes of his overall venture: 1) there is a *real* natural and social world out there, independent from us and our ways of knowing it, 2) scientific theories have to be taken at their face value until they become falsified and 3) *reality* consists of both observable and unobservable entities. Due to the third assumption, Wendt felt it was legitimate to bring *ideas* into the existing and inaugurate discussions on *causes* and *reasons* in IR theory, by substituting the respective notions of *explaining* and *understanding* by the new term of '*mutual constitution*', namely making feasible a *synthesis* of the two of them. In this vein, SR gives to ontology a priority over epistemological and methodological concerns, putting the latter aside and functioning as the single meta-theoretical framework able to underpin any IR theory which aims to bridge the opposing epistemological traditions of positivism (the current mean-stream epistemology in the USA) and interpretivism (mainly, however not exclusively, developed in continental Europe). In other words, the so-called 'first Wendt' "focused on structurationism and scientific realism as ways of thinking about IR" (Keeley 2007: 417).

On the other hand, the 'second Wendt', as deployed in his *STIP* and other relevant publications, seems to put an emphasis on the state and the international system, although Wendt himself denies it (Wendt 2000: 174-175), when arguing for his having presented states in *STIP* only as quasi-autonomous, self-organising actors. My impression, however, is that the state in *STIP* "is generally treated as a unitary and intentional actor" (Keeley 2007: 420), something which brings Wendt closer to realist and liberal IR scholars who conduct their empirical research within a positivist epistemology and which simultaneously fills radical interpretivists of various sorts with frustration, since they are unambiguously inclined towards denying any possible reification of the state and the international system. This shift towards an enhanced importance of the state in Wendt's theorizing became apparent with the appearance of his paper, "The state as a person" (Wendt 2004). It is here that Wendt takes on board the notions of *supervenience* and *emergence* in order to approach the state as an *organism* (more accurately as a *superorganism*) with *collective consciousness*. Wendt's most recent 'quantum consciousness hypothesis' is traceable to the aforementioned idea of the *collective consciousness* (of the *state*), which he presumably felt the need to further explore and elaborate. All in all, the 'second Wendt' is an outspoken supporter of a 'middle-ground' constructivism which is based "on an uneasy tension between mental causality and rump materialism that shows itself as a conflict between upward determination of ideas and their downward causation on the material world" (Kessler 2007: 243).

At this point, it is worth referring to Kessler's well presented argument concerning the 'inefficiency' of the 'supervenience hypothesis' to explain/solve the mind-body problem, if only for the sake of its simplicity and clarity:

> According to supervenience, the argument goes, mental event A is supervenient on physical event C (Ego), and Â is supervenient on Ĉ (Alter).
>
> What we are interested in is how A can influence the physical basis of Ĉ as to allow for Â to emerge. But the step from A to Â can only be *explained* in two ways: either we argue that the change from A first requires a change on this physical basis C. Ego's C now causes Alter to change his brain into Ĉ which then leads to Â. Or we argue that A caused Â directly. The first option is unable to explain mental causality, since the real 'cause' is from C→Ĉ, a form of reductive materialism. The second option, that is an explanation in the form of A→Â falls back to ontological dualism.
>
> This inability arises from a genuine conflict between upward determination and downward causation which has not yet been resolved. This is what I regard to be the central problem: by taking mental causality seriously, supervenience arguments are bound to collapse into either ontological dualism, raising again the issue of bridge-laws, or materialism proper. Now, one could argue that this problem might not be a problem at all, since they can both be considered valid for they simply highlight different aspects and simply see different things. This is the dual explanation strategy (Wendt 1995). This avenue is a recurrent theme in Wendt. (Kessler 2007: 256)

As a consequence, Wendt (2006a) develops his QCH as a possible solution to the mind-body problem, via the inspiration he draws from the theoretical work on the 'consciousness issue' which has been produced by Penrose (1994), Hameroff and Penrose (1996) and Hameroff (2001).

There are two main issues with this approach. The first has to do with Wendt's understanding of Penrose's work on the 'consciousness issue' and the insufficient coverage that Wendt gives to the specifics of a theory, which is important for his project. The second has to do with the incorporation of his view that human consciousness is seen as a quantum mechanical phenomenon in that it behaves as both a wave and a particle, in compliance with the wave-particle duality of quantum mechanics, as a gateway to the solution of the problems he tries to tackle in QCH. In the former case, an inaccurate understanding of the specific theoretical work of Penrose's undermines Wendt's project at its roots. In the latter case, a misplaced analogy between the way human consciousness works and Quantum Theory in his QCH undermines the structure of Wendt's project.

5.5.1 Penrose's Theory on the 'Consciousness Issue'

It is remarkable that Wendt, in both his first piece and his most recent pieces (Wendt, 2006a, 2010), is very eclectic with regards to the use of the very wide bibliography produced by Hameroff, Penrose and their critics on the quantum consciousness issue, confining his references only to the following three pieces: Penrose (1994), Hameroff and Penrose (1996) and Hameroff (2001). More indicative is, of course, the absence of any critical approach to Penrose's ambitious plan to ground a theory of quantum consciousness, launched by his book, *The Emperor's New Mind – Concerning Computers, Minds and the Laws of Physics* (Penrose 1989).

The book is an attack on Artificial Intelligence (AI), since its purpose is to demonstrate that the human mind cannot be modeled by a Turing machine or any other mechanical devise. In trying to explore the nature of mathematical creativity, he guides us in the labyrinths of Mathematics (here one finds a good introduction to lambda calculus and to the Mandelbrot set, among others), Mathematical Logic (with an interesting but controversial discussion of Gödel's Theorem, which was bound to cause great debate about its 'consistency' after the publication of the book) and modern Physics (ranging from quantum mechanics and quantum gravity to the discussion of Time's arrow). It is only within the last chapter of the said book that he comes to grips with the idea of quantum consciousness, since he is basically captured by the quest of "where the physics of the mind lies".

This book sparked the appearance of long, detailed and profound reviews on its subject matter, most of which have been positive, in general terms, vis-a-vis his exposition of Mathematics and Physics (Dewdney 1989, Barr 1990, Senechal 1992), but have ignored or been very skeptical of his 'quantum consciousness hypothesis' (Johnson 1990). It is important to cite Johnson at this point, since he first explains, very clearly, how Penrose connects quantum physics with the problem of consciousness and then goes on to aptly criticize it:

> Rather, he [Penrose] seems to believe that consciousness somehow arises from this interaction between the quantum realm and the macroscopic world of everyday existence, where the traditional rules of billiard-ball mechanics hold sway. When we are thinking about a problem, he proposes, the many possible solutions exist simultaneously in quantum superposition, much like the myriad positions of an electron around a nucleus. By considering all the possibilities at once, the brain can deal with incredibly complex problems beyond the power of algorithms. At some point in these quantum cogitations, the probability wave collapses in a sudden flash. We see the truth as the answer to the problem crystallizes like ice forming on a pond. We have an epiphany – contact with Plato's world.

> But here, just as Penrose's story is getting good, it comes to a screeching halt. Try as he might, he can find nothing in neuroscience to suggest that quantum behavior has anything to do with the way the brain works. Small as it is, a single neuron is enormous compared with an atom; its behavior seems best described by the macroscopic laws of chemistry. And even if there were some kind of quantum computer in our heads, what would cause all of its simultaneous calculations (or whatever they are) to collapse suddenly into an insight?
>
> Penrose says the answers to such questions will have to await the completion of his current effort to reconcile quantum theory with general relativity, which explains things such as stars. For most of this century, physicists have tried in vain to combine these theories of the very small and the very large. This joining of the microcosm and the macrocosm into a theory of everything would explain the quantum-mechanical paradoxes that seem to occur when we intrude on the subatomic realm. If Penrose is right, it would also explain the universe inside our heads, revealing how brains intersect with the Platonic world. (Johnson 1990: 48-49)

Penrose tried to reply in detail to his critics by writing a new book, *Shadows of the Mind – A search for the missing science of consciousness* (Penrose 1994a), a sequel to *The Emperor's New Mind*, in which he further developed his argumentation on three levels: First, in order to take into consideration the counter-arguments that appeared after his first book has been published, he rewrote his Gödel arguments and came up with a new one, which, according to him (Penrose 1994b), makes a much stronger case against any computational picture of consciousness. His new Gödelian argument seems to be in agreement with the famous and controversial use by John Lucas of Gödel-type arguments against the computational model of mind (Lucas 1961, 1970), as is clearly stated in another, more recent, paper (Penrose 1997). Moreover, if one finds the presentation of this argument in *Shadows of the Mind* fairly long and involved, I may call attention to another presentation of it by Penrose himself (Penrose 1995), where he set out the argument only in its essentials. Searle has also provided us with his own version of, as he calls it, "Penrose's version of Turing's version of Gödel's incompleteness proof", which Penrose uses to try to prove Lucas's claim (Searle 1995: 96). Second, as concerns the discussion of Physics in *Shadows of the Mind*, the author's view of the quantum state reduction has somewhat shifted from that which he expressed in *The Emperor's New Mind*. Third, after Hameroff wrote to him about his work on microtubules and their role to control the strength of synapses, Penrose felt more comfortable with this news than with his initial thought cast in his first book and based on the idea that nerve signals could really be treated quantum mechanically. In his new treatise he seems to be more confident that quantum effects could actually be happening in the brain and this is pictured in the title and context of Chapter 7 of *Shadows of the Mind* which has the telling title "Quantum Theory and the Brain" (Penrose 1994a). It is here that Penrose induces us to study the role of cytoskeletons and microtubules in correlation with that of quantum coherence as a new means of building up a quantum model of the brain and thus explaining consciousness (relevant are also Penrose 1994c, Hameroff 1994, Hameroff and Penrose 1996, Hameroff 2001).

This new book by Penrose has sparkled a great debate about many different aspects of its context; in fact, mathematicians and logicians (e.g., Feferman 1995, McCullough 1995, Faris 1996, Odifreddi 1997, Franzén 2005), physicists (e.g., Josephson 1994), cognitive scientists (e.g., Klein 1995, Baars

1994, 1995, Bringsjord & Xiao 2000), computer and robot scientists (e.g., Moravec 1995, McCarthy 1995, McDermott 1995, Aaronson 2013, chapter 11), philosophers (e.g., Searle 1995, Chalmers 1995, Maudlin 1995, Copeland 1998, Detlefsen 1998, Shapiro 1998, Berto 2009, Tieszen 1996, 2011) and freelance science writers (e.g., Brown 1994), alike, have all participated in this vivid dialogue through publications in journals, in order to discuss Penrose's version of Gödel's incompleteness theorem (namely its 'soundness' as to its implications that, first, human reasoning cannot be formalized and, second, there is a limitation on what computers can accomplish relative to humans), as well as his interpretation of quantum mechanics and, subsequently, its relevance (or not) to the 'laws of brain' and consciousness. Penrose has replied to the scientists who have submitted commentaries on *Shadows of the Mind* in the framework of a forum organized on the said book by the electronic journal *PRSYCHE* (Penrose 1995).

The first issue at stake of this debate concerns the implications of Penrose's version of Gödel's incompleteness theorem for the capacities of human reasoning vis-à-vis those of computers. With regards to this, one could present an argument in full alignment with Detlefsen according to which, like Lucas' and others' before it, Penrose's argument "fails to establish any inherent asymmetry between the capacities of humans and machines as regards their abilities to evaluate their own beliefs" (Detlefsen 1998: 135). This happens because

> When we say, then, that we can judge our own beliefs to be consistent, we do not mean this in such a way as to imply that the set referred to as 'our beliefs' should

> be taken to include this judgment itself. We do *not* clearly have a capacity to judge sets of our beliefs containing their own consistency judgment to be consistent. The fact that […] various computing machines lack this capacity does not therefore point to a difference between them and us. (Detlefsen 1998: 136)

Many of the aforementioned papers on Penrose's and Lucas' arguments about Gödel's incompleteness theorem are so technical that it is difficult for the general audience to follow the full force of the arguments presented in them. Given the above, it is strikingly interesting to note that many years ago the philosopher J. L. Mackie commented on Lucas' claim in his book, *Ethics: Inventing Right and Wrong*:

> An ingenious argument against determinism, based on a theorem about the necessary incompleteness of a certain kind of mathematical system, has been developed and defended especially by J. R. Lucas. I shall not attempt either to expound or to criticize it here, but shall bluntly state my opinion that what it shows is not that human minds are not deterministic structures, but at most that some human minds are not closed deterministic structures, that certain sorts of provocative input can change the way they work; but we knew that already. (Mackie 1990: 219)

However, I think the most clear-cut and profound philosophical critique of Penrose's new argument about Gödel's incompleteness theorem has been offered by Tieszen (1996, 2011). This critique is demonstrated in a coherent and most comprehensive way in the following passage, where Tieszen first explores Gödel's and Penrose's differing beliefs regarding the nature of *mental* and *brain* processes, and then steadily builds his own argumentation upon this assumption. Here the fallacy of the interconnection of Gödel's incompleteness theorem with Penrose's conjecture about a non-computational (possibly quantum) nature of the human consciousness is evident:

> Gödel once suggested that the argument that mental processes are mechanical is valid if one assumes that (i) there is no mind separate from matter and (ii) the brain functions basically like a digital computer (see Wang 1974: 326). Gödel evidently thought that (ii) was a prejudice of our time that might actually de disproved. It is interesting to compare this with the position of Roger Penrose in his book *Shadows of Mind* (Penrose 1994a; see also Feferman 1996). Unlike Gödel, Penrose denies (ii). He holds that the missing science of consciousness is to be a form of neuroscience that recognizes non-computational brain processes. Penrose thinks that Gödel's incompleteness theorems imply some kind of Platonism. He also uses the incompleteness theorems to argue that there are non-computational procedures for *knowing* mathematical truths, but this is immediately equated with the view that there must be non-computational *brain* processes (Penrose 1994a). It is fair to ask Penrose the question that immediately gets us into a corner: how could the brain be stimulated by abstract objects or, rephrasing it somewhat, what could the relationship between the brain (which for Penrose is not a computer) and abstract objects be? Given what a brain is and given the kinds of physical interactions in which it can be involved, and given that an abstract object is typically understood to be a non-physical object of some kind, it would be a category mistake to suppose that brains could have access to abstract objects. One might expect to address issues about non-computational *mental processes*, where these are not immediately equated with brain processes, but there is no phenomenology of consciousness of abstract objects. There is neuroscience with its "natural attitude" on one side, and

> Platonism on the other, and not the slightest hint how the two could be related. One might therefore think that the missing science of consciousness cannot consist only of neuroscience (see Tieszen 1996 and 2005). (Tieszen 2011: 131)

This is not the place to present and develop the rich and substantial exchange between specialists on such grand and, most importantly, contested scientific and philosophical issues as the above. Suffice it only to cite the second part of the closing paragraph of Faris' piece, since it refers to Penrose's speculations about an allegedly "quantum nature and functioning" of human consciousness:

> Finally, there is the proposal of a link between quantum mechanics and consciousness. Penrose goes out of his way to admit that the evidence for this is practically nonexistent. As he says, his argument requires that "our brains have somehow contrived to harness the details of a physics that is yet unknown to human physicists". So we must take it as vision of a possible future for science. It is hard to see the appeal of this vision. We do not understand either quantum mechanics or the mind, but this does not suggest that one is the solution to the other; most likely each will bring its own surprise. (Faris 1996: 208)

In order to put all the above in a nutshell and give a more acute and compelling criticism of Penrose's 'quantum consciousness hypothesis,' I quote Michael Shermer's relevant words in his regular column, "Sceptic", in *Scientific American*, where he cleverly invokes a good comment made by the physicist Victor Stenger on the issue at stake:

The attempt to link the weirdness of the quantum world to mysteries of the macro world (such as consciousness) is not new. The best candidate to connect the two comes from University of Oxford physicist Roger Penrose and physician Stuart Hameroff of the Arizona Health Sciences Center, whose theory of quantum consciousness has generated much heat but little light. Inside our neurons are tiny hollow microtubules that act like structural scaffolding. Their conjecture (and that's all it is) is that something inside the microtubules may initiate a wave-function collapse that results in the quantum coherence of atoms. The quantum coherence causes neurotransmitters to be released into the synapses between neurons, thus triggering them to fire in a uniform pattern that creates thought and consciousness. Because a wave-function collapse can come about only when an atom is "observed" (that is, affected in any way by something else), the late neuroscientist Sir John Eccles, another proponent of the idea, even suggested that "mind" may be the observer in a recursive loop from atoms to molecules to neurons to thought to consciousness to mind to atoms....

In reality, the gap between subatomic quantum effects and large-scale macro systems is too large to bridge. In his book *The Unconscious Quantum* (Prometheus Books, 1995), University of Colorado physicist Victor Stenger demonstrates that for a system to be described quantum-mechanically, its typical mass (m), speed (v) and distance (d) must be on the order of Planck's constant ($\hbar$). "If mvd is much greater than ($\hbar$), then the system probably can be treated classically." Stenger computes that the mass of neural transmitter molecules and their speed across the distance of the synapse are about two orders of magnitude too large for quantum effects to be influential. There is no micro-macro connection. (Shermer 2004: 38)

One has also to underline at this point that Penrose seems to have neither changed his own views about these issues nor come up with new ideas or any evidence in support of his ‘quantum consciousness hypothesis’ in his most recent writings which appeared after the publication of *Shadows of the Mind* (Penrose et al. 1997, 2011). However, an important feature of *The Large, the Small and the Human Mind* (Penrose et al. 1997) is the inclusion of short critical essays by Abner Shimony, Nancy Cartwright and Stephen Hawking, as well as Penrose's response to his critics in the final chapter of the book. It is my belief that the most acute observations on Penrose’s attempt to connect Gödel’s Theorem to the problem of human understanding are put forward in Shimony's essay. Shimony’s main argument is nicely synopsized by Michael Redhead as follows:

> In his comments Shimony accuses Penrose of trying to ‘climb the wrong mountain’, with his Gödelian argument. Shimony believes the distinctive features of mentality are much more decisively brought out by arguments such as John Searle’s ‘Chinese room’ argument, which shows, says Shimony, that correct computation by an automaton does not constitute understanding, without any need to appeal to complexities of mathematical logic. Shimony goes on to sketch a metaphysics that he calls ‘modernized Whiteheadianism’, in which all physical processes involve an element of protomentality. Penrose, in his reply, admits that some such framework probably expresses best his general point of view. (Redhead 2000: 917)

To summarize, the fact that Wendt builds mainly (although not exclusively) his own new theoretical approach to social science and international politics upon Penrose’s work on quantum consciousness (Wendt 2006b), necessitated an elaboration of Penrose's ideas along with some counter-arguments put forward by Penrose's main critics. Wendt has, so far at least, taken Penrose’s theory, or what he understands as Penrose's theory, for granted and treated it as conclusive. A reason for this may be that Wendt uses popular science books on quantum mechanics in his attempt to understand this physical theory.

However, it is a truism among physicists, mathematicians and philosophers of science, alike, that without both a profound comprehension of the conceptual tools and a technical knowledge of the mathematical ‘skeleton’ of quantum mechanics, any attempt to interpret this theory is bound to be faulty with regards to issues of methodology and metaphysics, as well as with regards to its ability to describe precisely the physical world.

5.5.2 Quantum Consciousness and the Holographic Model in Wendt’s new IR Theory

Having clarified the superficial treatment that Wendt makes of Penrose's theory, it is now time to turn my attention on the use that Wendt makes of the view according to which, human consciousness is seen as a quantum mechanical phenomenon in that it behaves as both a wave and a particle, in compliance with the wave-particle duality of quantum mechanics. Even more importantly, according to Wendt, “consciousness does not end with us – collective consciousnesses are possible, and particularly a conscious state” (Keeley 2007: 425). This is exactly what interests Wendt most of all. He assumes QCH might be true and explores its implications for thinking about human behaviour, society, state and the international system. However, one crucial reservation he shares about QCH concerns *decoherence*, since QCH under its current formulation cannot explain “how can the brain sustain and aggregate trillions of quantum events into one experience of quantum subjectivity” (Wendt 2006b).

Furthermore, Wendt (2010) argues for a ‘holographic model’ of the relationship between individuals and society. He does this in order to avoid the recasting of intersubjective phenomena in terms of individual choices, in which supervenience at the end of the day results. Attempting to transcend the distinction of individual and societies, Wendt takes refuge in the adoption of the ‘holographic hypothesis.’ How do all these things really ‘apply’ to a new conceiving of the structure of the international system? The whole idea is rather simple. Unlike photographs, holograms store all their information in every part of the image. So, if cut into pieces, each piece will contain a smaller, intact and, so to say, ‘inclusive’ version of the original image. Wendt thinks this is also true of the international system and therefore he considers individuals to be simple points of it, with each of them bearing in its own mind or having ‘deposited’ in its own memory all the information needed for the recreation of the whole international system.

An important ramification of the acceptance of the ‘holographic principle’ when analysing the structure of the international system is located in the observation that “what all this suggests, in short, is a completely *flat* ontology for social science, in which state, the international system, and all other collective intentions are *nothing but* individuals acting in material contexts” (Wendt 2010: 295). Individuals are, this way, brought back in, however without the principle of Methodological Individualism (MI), since the latter is not ‘compatible’ with the idea of ‘quantum entanglement’ and its function in producing collective consciousnesses. The ‘level of analysis issue’ has been restated on the basis that “states and other putative corporate actors are not really actors, but holographic projections by individuals” (Wendt 2010: 301), produced during the following process:

> ...Actions taken with reference to the wave function of the international system (for example, invading Iraq) will typically affect more people at greater distances than actions taken to reference to local high school board elections. Thus, in contrast to the vertical imagery of bottom-up/top-down that dominates the current debate, in a flat ontology it might be better to start by replacing ‘level’ with the more horizontal imagery of ‘scale’ (cf. Marston et al 2005), and then thinking about relationships between scales in terms of ‘inside-out’ vs. ‘outside-in’. Neither direction of influence would be causal (in a classical sense, anyway), because there is no real reality beyond the individual to do any causing; thus, on this view it would not make sense to talk about ‘states’ affecting, or being affected by, the international system…So while in a formal sense I am suggesting we abandon the discourse of levels, I am not suggesting that we can currently do without concepts like the state or international system. But these concepts need to be re-conceptualized and used in a way consistent with the fact that they are only virtual realities, not real ones. (Wendt 2010: 303).

There are a number of comments that one may offer with regards to Wendt's aforementioned views:

1. First and foremost, it is important not to forget that the whole project of using the ‘holographic principle’ to the study of the international system, which consequently has led, among others, to a revision of the ‘level of analysis issue’ in IR, is deeply flawed. This is because, as shown previously in this

chapter, the context of the ‘holographic principle’ in Physics is quite different from the one accorded to it by Wendt, with the purpose to ‘adopt’ and ‘apply’ it to his newly developed ‘quantum social science’.

2. Secondly, Keeley puts forward an array of reasonable questions about consciousnesses of putative agents on different levels of the international system, which I fully endorse:

> If we accept the argument for the state as a high-order consciousness, other questions inevitably arise. Does this imply that decision processes visible to us are (on our level) quantum processes of decoherence? Are we to the state as neurons to the mind? Further, as Wendt seems to recognize, there is no inherent need to stop the generation of such consciousnesses with the state. In that case the question of governments arises: why should we accept the state yet reject governments (not to mention other collectives) as consciousnesses? The number of actors in international relations, as well as the levels of analysis to be handled, would be increased significantly. Moreover, it would seem possible for individuals to participate in more than one such consciousness simultaneously. If such higher-order consciousnesses exist, how could we know it? Would they know we exist? What would be the connection between our processes and their thoughts and activity: could we deliberately influence them? What connection, if any, would their purposes have with ours? Would we be at best as flies to wanton boys? Particularly if Wendt wishes to develop his argument in a teleological vein, there could be serious problems for a progressive element in that argument. (Keeley 2007: 427-428).

3. When it comes to the revised 'level of analysis issue' which is based on the 'holographic principle', I am bound to say that, were it correct, it could have been of some help to the study of some interesting political phenomena emerging in the international political landscape such as that of the Europeanization of national foreign policies of the EU Member States. The latter cannot be sufficiently explained with the methods provided by the traditional IR Theory and Comparative Politics but, on the contrary, necessitate the invention and synthesis of new ideas and concepts.[189] Questions of the sort, "How to conceptualize the process (e.g. is it specific to EU Member States?)" or "What is changing and what are the mechanisms and direction of change (top-down from the EU Member States, bottom-up, or socialization)?" (Wong and Hill 2011b: 4) belong, to a certain extent, to the problematic of the 'level of analysis issue'. However, I am afraid that Wendt's new approach to the 'level of analysis issue', though much more ambitious than the previous and well known ones in the rich bibliography of IR Theory on this topic, makes the study of the Europeanization of national foreign policies of the EU Member States much more complicated. This is because it does not provide us with any new analytic conceptual tools suitable to explain different aspects of this complex phenomenon. It seems to me that different FPA middle-range theories that aim to shed light on the different manifestations and epiphenomena of the aforementioned Europeanization process, along with historical studies of the routes each one of these states followed in their attempts to reach this goal, can offer better services in explaining the Europeanization of the national foreign

[189] For this line of argument see Mouzakitis (2011).

policies of the EU Member States[190] than Wendt's analysis of the international system, which is based on the idea of collective quantum consciousnesses of corporate agents and has the individual as its basic unit. For if this very idea of quantum consciousness indeed proves to be flawed (as I have tried to show that it is), the whole fabric falls apart immediately. In a more general note, though grand-theories, such as that of Wendt's, raise great hopes for explaining complex social and political phenomena, it is a commonplace in IR Theory and FPA that "attempts at grand generalizations always bring forth a shoal of objections to single-factor explanations of a complex world" (Hill 2011: 84).

4. The statement that "individuals are back in" makes me wonder whether this statement amounts to anything more than a mere truism, since Wendt seems to have nothing more substantial to contribute to the long enduring discussion of individuals' role in interpreting or even shaping large-scale political events. It is my belief that History and Foreign Policy Analysis (FPA) are much more suitable than Wendt's 'holographic hypothesis' for this goal, in the sense that they offer concrete personified examples of individuals who act and produce important political events on micro- and macro- scale as well as sophisticated methodology, which can be used in the analysis of such phenomena. With respect to the above, it is worth recalling Christopher Hill's claims that

[190] For instance, Hill (1983), Hill (1996a), Wong and Hill (2011a), Manners and Whitman (2000) and Tonra (2001) constitute good examples of the study of the Europeanization of national foreign policies of the EU Member States, with each of these volumes containing both introductions (where methods and goals of the study are presented) and conclusions (in which attempts to categorizing the findings of the papers included in each volume are of considerable help to a comprehensive theorizing of this Europeanization process in different but successive time periods of its development).

> ...Individuals are not simply passive victims of impersonal forces. They are always agents of some kind and in some context, and deserve attention for their views and actions as well as their pain. (Hill 1999: 122) [191]

And that

> While academics tend to discuss IR at the level of the system, once the focus is shifted to agency it is all too easy to be seduced by the fascination of events, the charisma of personalities and the demands for what Churchill called 'action this day'. But beyond all this is the challenge of acting to develop a framework within which particular actions may legitimately be pursued and cooperation may have a chance of holding. (Hill 2011: 97)

It may be much worthier to consider the above acute observations when studying agency in international politics, than Wendt's rather simple reminder of the crucial role individuals play in the development of the international system. For instance, Wendt does not offer anything more in the way of explaining what it actually means for someone to act politically as an individual in the modern complex world, which forms can his actions take and under which presuppositions and circumstances are these actions feasible, which are the probabilities for their success, and so on.

One counter argument which might be put forward is that these questions do not belong to the anxieties and considerations of an IR theorist who aspires to establish a grand theory of international politics which should be based on the

[191] Hill wrote this paper in reference to the war diaries 1939-1945 of Gustave Folcher, a French peasant, on the occasion of his translating them from the French to the English language. Herein he reflects on their importance for the study of History and IR.

idea of the existence of both individual and corporate consciousnesses, and which will be able to explain, by reduction to it, all the political phenomena that gradually emerge from political actions of single and corporate agents who act on different but intersecting political levels, domestic, foreign and international. The questions in focus, the counter argument goes, matter to those IR scholars who engage in empirical research or who are interested in middle-range theories. Even so, the essence of the initial question remains: what can Wendt's idea about quantum consciousness tell us, at least for the time being (when, let us not forget, we have not understood all of quantum mechanics and have understood little on consciousness) which is new about the importance of the individual acting in the political landscape, national and international?

5. Furthermore, it seems important to point out that by focusing once more on ontology (namely, the ontology of the quantum wave-particle duality, this time) and by avoiding any methodological and epistemological considerations – by establishing 'a new non-foundational epistemology' (Griffiths, Roach and Solomon 2009: 159) – Wendt sticks to the meta-theoretical framework of his earlier developed SR (Wendt 1987, 1999). This holds, despite the fact that he has now moderated his previous concern for (unobservable) social structures by according priority to the agent (individual) over the structure (meaning the state, the government, the international system and other corporate units).

Even in the case where one, unlike me, acclaims Wendt's new ambitious theory by considering it to be a kind of 'social physics' and a part of the positivist methodologies tradition (see, for instance, Curtis and Koivisto 2010:

438-439, as cited earlier in this chapter), there does not seem to be a problem of incompatibility between the alleged positivist methodology of Wendt's quantum social science and SR; the latter being this theory's meta-theoretical framework. This is more the case, if we are to adopt Jackson's definition of 'a pluralist science of IR', which is based on methodological pluralism (not methodological *relativism*) and "poses the challenge of dealing with bodies of warranted knowledge stemming from philosophically incompatible methodologies" (Jackson 2011: 210).[192]

6. Another interesting comment to be made on Wendt's 'quantum consciousness theory' is that it implies that "the more mental causality and thus autonomy of 'consciousness' is granted, the more an ontologically based constructivism becomes implausible" (Kessler 2007: 243). Kessler also argues that in the event one can trace affinities between Wendt's turn to quantum and other IR theories, he can't help tracking such resemblances in radical versions of IR theories which either are exclusively Wittgensteinian in their approaches or follow, more generally, the so-called 'linguistic turn'. This happens, he claims, because theories of this sort are interested in speaking as an activity, i.e. showing the indeterminacy of world and inter-subjective objectivity of facts

[192] The obvious question of course, is how this is feasible. According to Jackson,

> Because there is no methodological neutral metalanguage into which we could reliably translate our warranted knowledge-claims and have them be globally understood, methodological pluralism sets up a variety of contentious conversations and efforts to appreciate the insights of alternative ways of producing knowledge while avoiding the temptation to universalize our own modes of conducting scientific inquiry. (Jackson 2011: 210)

Although I do not find the meaning of this passage perfectly clear, especially regarding the ways and 'methods' that these conversations may lead to the described desired goal, what I derive from it is the claim that SR is compatible with the methodological positivism of Wendt's quantum social science; a claim with which I do agree.

"which leads, according to Stefano Guzzini, to a micro and macro feedback loop of description and action" (Kessler 2007: 266). Kessler presumably sees affinities between 'the indeterminacy of world' and the 'uncertainty principle' of quantum mechanics, on the one hand, and the 'inter-subjective objectivity of facts' and the 'quantum measurement problem', on the other. However, as I have explained before, this is not the case, and as such, Kessler's argument is rendered faulty.

7. At the beginning of this chapter I have quoted a statement made by Griffiths, Roach and Solomon, according to which, the primary aim of Wendt's quantum social science project "is to suggest the possibilities of what he calls 'capacity for collective self-consciousness'" (Griffiths, Roach and Solomon 2009: 158-159).

However, as I have repeatedly noted, I seriously doubt whether one should take Wendt's quantum theory of consciousness and its implications for the formation of collective self-consciousness at their face value, at least for the time being and given that the theory is at its infantile stages of development. Moreover, one should also not take Wendt's quantum theory of consciousness and its implications for the formation of collective self-consciousness at their face value, because that would be done at the expense of relevant insights, which bear on collective self-consciousness and are drawn on cognitive psychology and psychoanalysis (due to the latter's attempts to reach the subconscious).

It is my belief that the above point is very important, because collective self-consciousness is crucial when studying the formation of collective identity, a major issue of Wendt's first influential book (Wendt 1999). I discussed this at length in Chapter 4 of the thesis, and as such, nothing more will be added here.

Collective self-consciousness is also essential, for the analysis of foreign policy decision-making, especially when one comes to the theory of *groupthink*, where collective psychology provides us with insightful observations on human behaviour. In his discussion of the psychological factor in the framework of foreign policy decision-making analysis, after calling our attention to the commonsensical but, at the same time, important assumption that "there is a difference, however uncertain, between decision-makers' *psychological environment* (how they perceive the world) and their *operational environment* (events as they happen independent of one person's perception)" (Hill 2003: 111, italics in the original), Christopher Hill then relies upon it and goes on to examine, among others, the role of *rationality* and *irrationality* in the *groupthink*. Suffice it to cite here only his concluding words on this issue:

> Decision-makers at times have no choice but to accept the limits on rationality and bring other qualities into play, some of them using the emotional rather than the calculating side of the brain. Miriam Steiner has argued, in a brilliant and still unsurpassed article, that 'in a world with important nonrationalistic elements, true rationality requires that nonrationalistic capabilities and skills be appreciated and developed side by side with the rationalistic ones' (Steiner 1983: 413) Feeling and intuition are just as vital attributes of decision-makers as thinking and sense-based observation. Thus the overwhelming mass of impediments to rationality are not quite the obstacles they seem; some, indeed, cry out for a-rational, if not non-rational, techniques, and concepts like judgement, leadership, empathy and charisma always imply at the very least a combination of the intellectual and

> emotional sides of personality. Ultimately, both cognitive psychology and psychoanalysis have done a lot to contribute to our understanding of how foreign policy decisions are and should be made. (Hill 1999: 116)

It is true that Karl Popper in his classic, *The Poverty of Historicism*, argues that "the social sciences are comparatively independent of psychological assumptions, and that psychology can be treated, not as the basis of all social sciences, but as one social science among others" (Popper [1957] 1997: 142). However, I cannot help but notice that although psychology should probably not be treated as the basis of all social sciences, cognitive psychology has been such a substantial contribution to the understanding of foreign policy decision-making that it should not be ignored.

8. Finally, on a more general point, in his attempt to defend the methodological pluralism of the English School (ES), Richard Little argues that, albeit tentatively, "some of the more surprising implications of the ES approach to methodology have started to be teased out in Alexander Wendt's most recent work" (Little 2008: 100). However, he goes on, "few if any ES theorists…are likely to engage with Wendt's quantum move. They are much more likely to be sympathetic with the view that epistemological and ontological problems will be resolved in the hurly-burly of doing research" (ibid: 94).

Little seems convinced that Wendt's quantum social science project signals a turning point in the methodological study of social sciences, because of the abandonment of the Cartesian dualism which it entails:

> But in essence, sub-atomic phenomena can be described in two irreducible and non-equivalent ways – as either waves or particles and, as a consequence it is not possible in principle, to know both the position and momentum of a particle at the same time. It follows that the idea of unitary and integrated knowledge is inherently impossible, because we necessarily require conflicting narratives to describe sub-atomic phenomena. Moreover, in providing these narratives, it also has to be accepted that the subject-object distinction breaks down. Quantum physicists, therefore, have opened up a Pandora's Box to reveal, among other things, a post-modern world of alternative realities where the conception of causality breaks down. (ibid: 93)

If one bears in mind my comments on and objections to Wendt's understanding of QM, which I have deployed earlier in this chapter, one can come to understand why I disagree with the Little's conclusions. Given that the description of sub-atomic phenomena does not involve conflicting narratives and the conception of causality does not break down in QM (it becomes probabilistic), I cannot conceive what is meant by "a post-modern world of alternative realities where the conception of causality breaks down".

Furthermore, Little holds that the most important methodological implication of Wendt's quantum social science project is that

> …it has led him [Wendt] to shift his position on methodological pluralism and to reject the attempt to achieve a synthesis of positivism and interpretivism. Instead, he argues that quantum naturalism reaffirms the position that explanations sought by positivists and the understanding pursued by interpretivists are both necessary and complementary ways of viewing the social world. (ibid: 94)

Be that as it may, what is of real interest to me is that Little argues that the above conclusion "might not come of a surprise to the ES" (ibid: 94), because "the founders were, in essence, intuitive or perhaps even unintentional methodological pluralists" (ibid: 94). Although I cannot understand how one can *unintentionally* subscribe to methodological pluralism, let alone *unintentionally* 'serve' it in his theoretical or empirical research, I do agree with Little's point that "the complexity and diversity of the ontological and epistemological assumptions that underpin the ES approach to theory and history have come about, to some extent, *as a consequence of not paying close attention to ontological and epistemological questions*" (ibid: 94, emphasis added). I think that this observation is important since one of the main problematiques of this thesis, as discussed in chapter 4, deals exactly with the question of the extent to which second-order IR meta-theoretical considerations determine the ontological and methodological plurality and sophistication of the first-order IR theories that they are taken to underpin.

Along the same line of argument, Little contends that the methodological pluralism which is implied by Wendt's quantum social science project can also be applied to many working historians (and here one must take into account the impact historians have had on the English School's research agenda):

> Historians almost invariably acknowledge that there are always at least two stories to tell, one from the inside and the other from the outside. They know that they operate from a potentially privileged position because they can endeavour to recover the viewpoint of the historical actors, but also have the luxury of knowing what happened after the event. So, self-fulfilling prophecies and unanticipated consequences, for example, can become crucial elements of any historical

> narrative. Indeed, historians necessarily have to make sense of events in the light of subsequent events. What is distinctive about the ES is not simply the focus on structural constraints, but also the recognition that the impact of structural constraints have to be examined in the light of both the separation and the interaction between facts and values, the relationship between the story told from the outside and the inside, the fact that the actors have a conception of both the past and the future, and the link between actors and analysts. These complexities map quite closely onto Wendt's quantum perspective on methodological pluralism. (ibid: 94)

With regards to the above, I really doubt if we needed "Wendt's quantum perspective on methodological pluralism" in order to conceive the above complexities, which definitely characterize the English School's methodological apparatus. These can be found out directly if one reads carefully the existing bibliography on the English School. Now, if one wants to come to grips with such difficult questions as "What is History?" and "What historians actually do?", one can refer to a number of great works which demonstrate utmost originality (for instance, Carr ([1961] 2001), Collingwood ([1946] 1973)) or strong analytical power (for instance, Evans (1997). Moreover, on the Philosophy of History (which deals with these and other similar questions), one can use Dray (1964) and Walsh ([1977] 2008). As a consequence, one does not need to resort, as Little wrongly suggests, to Wendt's new theory in order to shed light on the above issues.

5.6 Conclusion

The foregoing considerations articulate several lines of criticism of Wendt's (2006a, 2010) preliminary sketch of a road towards a quantum social science. Some pertain to issues of methodology while others to issues of content. As to methodology, I argued that Wendt fails to offer precise arguments in support of his claims. Moreover, the arguments the reader can distill from Wendt's text often fail to be cogent due to violations of well-known criteria that should govern the deployment of the pertinent argument forms. Coming to the main argument of his intellectual venture, which concerns the literal application of quantum mechanics to the study of social science, I argued that Wendt misconceives the context and function of many basic concepts of quantum physics. Moreover, while he also misconceives the context and function of their philosophical interpretations and ramifications, he remains – whether knowingly or not – too eclectic as to the metaphysical assumptions of his intellectual enterprise. Thus, despite his intentions, the results he produces for IR Theory are of minor importance as they do not constitute any scientific progress. Of course, it should be granted that some inaccuracy and vagueness may be due to the fact that Wendt's project is at its infantile stages of development. But any research project, no matter how ambitious or how early on in its development stage it is, must have a clear core and orientation. Otherwise, there are neither heuristic guidelines for the advancement of the programme nor criteria that will distinguish success from failure.

Overall, Wendt's quantum programme shares a feature, which is common to many attempts that appeal to quantum physics in order to 'solve' some thorny philosophical problem and is plainly revealed in Wendt's own words:

> In short, the nature of the quantum is no less a mystery than consciousness…. The quantum consciousness hypothesis suggests that the two mysteries have a common solution, namely that the quantum in effect *is* consciousness, which in some form goes all the way down in matter. (Wendt 2006a: 190)

But no mystery, as long as it remains a *mystery*, can solve another mystery. And the idea that progress will be achieved by reducing the total number of mysteries to be resolved amounts, at this level of generality, to mere *wishful thinking*. True, the strange but fascinating facets of quantum reality – in particular, the miracle of quantum measurement – have been marshaled to resolve several baffling puzzles. But the trick that makes such attempts appealing is simple: from a *miracle* almost anything follows!

6

CONCLUSION

6.1 The Contributions

My aim in this section is to outline some of the contributions that the argument in the present thesis offers in terms of the critique of the introduction and use of SR and CR in IR as well as in terms of other issues which are discussed in the thesis (e.g. SR in the philosophy of science, Bhaskar's CR) and pertain to its problematique.

In Chapter 2, I examined SR as an autonomous pursuit in the philosophy of science and found that the outcome of the realism/anti-realism debate has been that *one should no longer go uncritically for an adoption of the standard global conception of SR*. This is of utmost relevance, since one of the central arguments I make in the thesis is that the manner in which SR has been imported and used in IR meta-theory does not appreciate adequately the grave theoretical implications that SR has for any strong, global realist construal of IR. The standard brand of SR adopted by IR meta-theory is considered to be the definitive and conclusive version of philosophical realism; this is mistaken, since many issues on SR remain unresolved, whereas different interpretations of realism abound. Picking the strongest version among them rather uncritically as the definite and final interpretation of realism and importing it into IR meta-theory is a risky business. I also noted how the adoption of an unreal, 'mythological' conception of science is uncritical and permeates much of the work in IR meta-theory – notably Alexander Wendt's and Colin Wight's.

Consequently, the aforementioned works inherit all the problems that go with such an adoption.

In Chapter 3, I embarked upon a very close reading of Bhaskar's main statement of his particular version of SR, also known as CR. The chapter is of a thoroughly philosophical character, focusing solely on the examination and structuration of a series of philosophical arguments. Such a detailed examination of CR is necessary, because it is this Bhaskarian version of SR, which constitutes the hegemonic meta-theoretical discourse in the SR debate within IR. Failure to do such an examination can lead one to an unreflective stance towards SR's and philosophy of science's place in general in IR. The main original contributions to the understanding of Bhaskar's philosophy offered in this chapter could be summarized as follows:

1) I offer an argument that explains in what sense one can understand Bhaskar's notion of a 'transitive object of science'.
2) I elucidate Bhaskar's distinction of the Humean and the Kantian versions of 'classical empiricism', while explaining, through an original argument based on the notions of subjectivity, experienciability and the 'empirical world', why both versions are committed to a common ontology, which Bhaskar terms 'empirical realism'.
3) I dissolve a very common misunderstanding, one that appears much too often in the meta-theoretical debates of IR, concerning the nature of a *transcendental argument*. This allows me to claim that Bhaskar makes constant use of 'transcendental argumentation' in order to demonstrate the intransitive dimension of scientific practice.

4) I place Bhaskar's philosophy in the context of the history of philosophy, particularly in reference to Berkeley, Hume and Kant as well as Aristotle.
5) I present a novel codification of his first transcendental argument based on the notion of sense-perception.
6) I present an original codification of his second and most famous transcendental argument based on the notion of experimental activity.
7) I explain in a novel way how the notions of structure, generative mechanism and natural tendency of a thing are intertwined.
8) I differentiate in an explicit way three levels of analysis: philosophy of science, methodology and ontology. I make several original claims about Bhaskar's use of the notions of 'ontology' and 'transcendental'.
9) I present a critique of Bhaskar's implicit understanding of Kant's theory of causation.
10) I present the novel statement that Bhaskar makes use of Kantian methodology in order to arrive to an utterly un-Kantian and thoroughly, yet inexplicitly, Aristotelian ontology.
11) I explain in a brief, yet hopefully clear, manner how Bhaskar's ontology of generative mechanisms is to be associated with Aristotelian ontology of substances.
12) I present an explanation of the appeal of Bhaskar's philosophy of science to working social scientists, as Bhaskar's philosophy presents a richer picture of the subject-matter of social science, which goes beyond observable patterns of events.

In Chapter 4, I argued that Wendt has imported the strongest version of SR into IR, a 'move' which is risky since many issues on SR remain unresolved and different interpretations of realism abound (as I have shown in Chapter 2). Unfortunately, those IR scholars who have got involved in the SR debate in IR do not seem be aware of this 'complex situation', at the expense of the quality of the argumentation about SR and CR within IR. I have also shown that many IR scholars who have come to grips with these issues have not been able to distinguish between SR and CR, the Bhaskarian version of SR, which has been introduced by Wight and other critical realists into IR and which has dominated the field. The illumination of this distinction is of some importance – without it being, however, novel – since Wendt and Wight have based their meta-theories on different premises and this is not inconsequential. For instance, although they both prioritize ontology over epistemology, Wight argues that CR is incompatible with positivism, whereas Wendt proposes a methodological 'reconciliation' between positivist and post-positivist approaches to the study of IR. Therefore, a second small contribution I have made in the framework of the Chapter in focus is that I have highlighted the differing implications CR and SR have not only for meta-theory (see the above example) but also for substantive IR theory (for instance, the adoption of the notion of 'constitutive explanation' helps us search, on both the theoretical and empirical research levels, for causes in places which are sidelined by the 'causal explanation', which is mainly used by positivist IR theories, e.g. neorealism). Furthermore, I have emphasized that IR critical realists argue that meta-theoretical choices are 'politically charged' and I agreed with Chris Brown that CR may contribute to the resurgence of historical materialism studies in IR.

In Chapter 5, I came up with a critique of Wendt's 'quantum social science' project, which is also based on SR as its meta-theoretical background. There are very few critiques of this project and this explains why this Chapter is novel with regards to most of its content. I have criticized Wendt's 'quantum social science' with regard to its methodology, understanding of QM, metaphysics, and application to IR. My comments on Wendt's understanding of QM illustrate how Wendt uses QM literally and not metaphorically. Furthermore, nowhere in the IR literature is to be found any critical approach to Roger Penrose's 'quantum consciousness theory', as it is mainly employed in his two successive and widely discussed books, *The Emperor's New Mind*, and *Shadows of the Mind*. Consequently, I discussed Penrose's work on consciousness, as it has been presented in books and academic journals of Philosophy, Physics, Mathematics, Logic, Artificial Intelligence and Biology, in order to show that, despite its sophistication, his theory of consciousness is highly contested. This is crucial because it makes Wendt's 'quantum consciousness hypothesis' problematic, given that the latter relies heavily on Penrose's hypothesis. If one takes on board that this hypothesis lies in the foundations of Wendt's 'quantum social science' project, one understands that in the eventuality that the hypothesis is erroneous, Wendt's theory falls apart. Needless to say that, as a consequence, Wendt's attempt to hinge on 'quantum collective consciousnesses' of states and the international system itself in order to explain international politics fails, too.

6.2 Directions for further research

6.2.1 The agent-structure problem in a nutshell

As I argued in this thesis, Wendt was led to SR due to his effort to resolve the 'agent-structure problem' in IR Theory. In his attempt to grant causal powers to what he terms structure, e.g. something which is unobservable but can, nonetheless, be inferred by its effects, he chose SR as the meta-theoretical background of his work. He did this for the simple reason that one of SR's three basic assumptions is that the world consists of both observable and unobservable entities. However, in both his 1987 paper on the agent-structure problem in IR and his *STIP*, Wendt fails to come up with any *new* theoretical version of the agent-structure model. Moreover, he is absolutely explicit as to the absence of any intention on his part to get involved in any empirical work, which could bring to the fore concrete examples of the relationship between real-worldly agents and structures. He adopts an *agent-structure dualism* scheme, that is, he endorses the view according to which agent and structure are two *distinct* entities, which affect greatly one another. This happens because he takes on board Margaret Archer's social-theoretical work on the agent-structure issue (Archer 1982, 1995), which is based on Bhaskar's CR and therefore admits the aforementioned *dualism* scheme with respect to the enduring relationship between agent and structure. In other words, although Wendt includes Antony Giddens' considerations about the agent-structure problem in his own examination of the same problem, he negates Giddens' view of the *agent-structure* duality (Giddens 1979, 1984). The latter involves seeing agent and structure as two sides of the same coin, meaning that each one of these entities is absolutely dependent on the other and shapes it in a decisive manner;

according to Giddens, the social structure is both the medium and the outcome of social action.

Colin Wight is another thinker who relies heavily on Archer's and Bhaskar's CR. Thus, while, in general, he is aligned with Wendt on the agent-structure issue (meaning that he also endorses agent-structure dualism), Wight, unlike Wendt, offers a typology of structures (as I have shown in Chapter 4), something which Wendt does not do. The main difference between the two thinkers, however, lies in the fact that Wendt is more selective than Wight in his choice of the theoretical model through which he addresses the agent-structure issue. Whereas Wight follows *stricto sensu* the Bhaskarian ontology, methodology and epistemology, Wendt's SR draws on ideas and notions from a richer bibliography on SR, sociological theory and, more recently, science. Moreover, it does this despite the fact that it remains faithful to Bhaskar's CR regarding the choice of its basic ontological assumptions. Heikki Patomäki (2002), Milja Kurki (2007, 2008), David Dessler (1989) and others who also endorse Archer's and Bhaskar's CR, do not distance themselves from the dominant explanation scheme regarding the relationship between agent and structure, which they assume to be that of a dualism.

When examining the agent-structure issue with respect to the existing FPA and European Foreign Policy Analysis (henceforth EFPA), one observes that the theoretical reflections on it are poorer in FPA and EFPA than they are in IR Theory. Nevertheless, one can still find interesting reflections on this issue in Carlsnaes (1992), Tonra (2001), Knudsen (1994), and Wœver (1994). Furthermore, the majority of FPA scholars quote Giddens (1979, 1984) rather uncritically when they refer – usually in a vague manner – to actors and structures; a few of them, with Walter Carlsnaes being the most eminent, rely on the works of Dessler (1989), Archer (1982, 1995, 2000, 2003) and Bhaskar ([1975] 2008, 1979), in order to provide us with their own model of studying it. Indeed, Carlsnaes conceived his own model of the agent-structure relationship which he demonstrates in every FPA paper he writes on this subject (Carlsnaes 1992, 1994, 2002, 2006).

My exposition above is a very brief, albeit neither inaccurate nor misleading, presentation of the agent-structure problematique in the FPA and EFPA literature.

6.2.2 A new approach to the agent-structure problem

It is my belief that a further original contribution to the discussion of the agent-structure problematique could result from a focus which combines sociological theory, IR Theory and FPA. IR is a social science and as such it could be privileged from its contact with and learning from other social sciences and sociological theory. It is also true that the philosophy of science and the philosophy of social sciences are important to the study of the foundations of natural and social sciences, respectively. However, it is a mistake to substitute philosophy of science for sociological theory when dealing with sociological issues. In the same vein, it is implausible to substitute philosophy of science for IR Theory, International Political Theory, or FPA, when studying the agent-structure problem at the levels of the national and the international.

The detailed discussion of the agent-structure issue within the framework of sociological theory may help IR scholars see it in its complexity. With the exception of Giddens, who has coined the *agent-structure duality* explanatory scheme, there are many other distinguished sociologists who have dealt with the same problem at length. One example is Nicos Mouzelis (2008) who draws on Giddens, Parsons, Habermas, Alexander and Lockwood, to come up with a more refined explanation of the complex relationship between agent and structure. *He argues that, while at times the relationship between agent and structures takes the form of a duality, at some other times it is the dualism that characterizes that relationship*. This depends on many factors, one of which is the given location of the agents and the structures in the micro- and macro-scale of the social and political environment. This distinction between duality and dualism with respect to the relationship between agent and structure, and the observation that this relationship sometimes takes the shape of dualism, while other times takes the shape of duality (depending on the location of the actor vis-à-vis that of the structure), *cannot be found in current IR theories about the agent-structure issue and this may make the difference in future works on the same topic*.

Furthermore, one takes the risk of getting lost in endless and rather sterile meta-theoretical discussions about the agent-structure issue, if one loses sight of the kinds of agents and structures one can locate in IR, let alone that some of them may not remain unchangeable or continue existing in the long durée; as Christopher Hill notes, one should, instead, confront the "'first order' questions of what entities, actors, units to admit of in world politics" (Hill 2003: 313). One must investigate who the major players (actors) in international politics are, how important each of them is, and, finally, under which conditions political action can occur" (Hill 2011). Only then can one estimate if change as a result of political action (agency) is possible, how much change is reasonable to expect, and from whom. In other words, it is crucial that one should be able to make concrete empirical research proposals to test his IR theoretical model of agents and structures and their relationship. I would only like to reiterate that the lack of such proposals is probably the most serious shortcoming of the Wendt's and Wight's models in IR Theory.

More generally, in trying to do justice to the relevant meta-theoretical considerations about the agent-structure issue in IR and FPA, Hill argues that

> In one sense the agency-structure debate has been good for foreign policy analysis. It has returned the perennial issues of causation, freedom and determinism to the agenda of International Relations and it has led to a sharper examination of the rather unsophisticated conceptual basis of some foreign policy studies. On the other hand, it has often been presented as the agency-structure 'problem', which by extension should admit a 'solution'. This is not the approach taken here, where the mathematical analogy is seen as inappropriate to the immense political and historical complexities facing all those who wish to understand foreign policy. Rather it is assumed that causation always involves both structures and agencies, and that – as a number of authors have pointed out, following Antony Giddens – the two kinds of phenomena help to constitute each other in a perpetual process of interaction. This means, by definition, that it is impossible to come to fixed conclusions about the limits to agents' freedom of choice or their capacity for impact (two different things). We may analyse the parameters of choice, constraint and change but human beings will always have the 'wiggle room' of specific historical circumstances in which to remake at least some of the world. (Hill 2003: 26)

Given the above, there is great scope of elaboration on the agent-structure problematique in the areas of FPA and EFPA, by, first, using tools of sociological middle-range theories which can account for the observed effects of the relationship between various kinds of observable agents and unobservable structures and, second, by taking on board the specific characteristics of FPA and EFPA (White (2001, 2004a, 2004b), Bretherton & Vogler (2006), Tonra & Christensen (2004), Carlsnaes & Smith (1994), Tonra (2001), Keukeleire & MacNaughtan (2008), Hill (2003), Hill & Smith (2011a), Hill & Smith (2011b), Jørgensen (2004), etc.). For instance, one such tool that may be adopted from sociological middle-range theories is the 'strategic-relational approach', which has been developed by Bob Jessop within the framework of CR (Jessop (2002) and Colin Hay (2002) – see also the debate on the agent-structure issue between Hay (2005) and McAnulla (2005)). The 'strategic-relational approach' has been used as a theoretical tool of FPA, especially when one wants to study how actors meet their environment (which seems to be a reformulation of 'the agent-structure problem' in the FPA language); for instance, see Brighi 2005: 79-83, and Brighi & Hill 2008: 119-122. Furthermore, it has been used by critical realists, like Marjo Koivisto, who relies on the concept of structuration and the 'strategic relational position', in order to account for the state's normative capacities in IR (Koivisto 2012: 46-8).

However, irrespective of how sophisticated the models I am going to use may be, what I will try to bear in mind and never forget is the existence of the 'wiggle room' of which Hill has spoken in the above cited passage. This pragmatic expression in fact denotes the fundamental freedom which human beings always retain to make their own history.

7

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