**ΠΑΝΤΕΙΟ ΠΑΝΕΠΙΣΤΗΜΙΟ ΚΟΙΝΩΝΙΚΩΝ ΚΑΙ ΠΟΛΙΤΙΚΩΝ ΕΠΙΣΤΗΜΩΝ**

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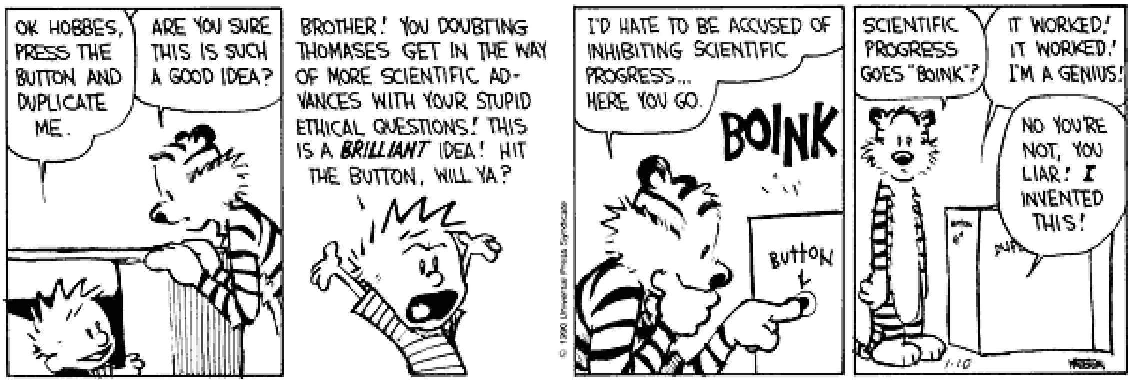
**Κατεύθυνση:** Διεθνείς Σχέσεις και Στρατηγικές Σπουδές.




**Διπλωματική εργασία με θέμα:** Το ζήτημα της επιστημονικής προόδου στη Θεωρία των Διεθνών Σχέσεων.

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Table of contents:


**Abstract:** .... i

Introduction: .... ii

Chapter 1: .... 1

Defining Science: .... 1

Characteristics of the Natural Sciences: .... 1

The formation of a theory: .... 3

Criteria of Theory Choice in the Natural Sciences: .... 5

From natural sciences to social sciences: .... 7

The academic discipline of International Relations: .... 8

Connecting science with philosophy of science: .... 9

Chapter 2: .... 12

**The influence of positivism in the evolution of science:** .... 12

The starting point for the demarcation problem: .... 14

From positivism to Popper: .... 15

From Popper to Lakatos: .... 17

The validation of new theories through scientific research: .... 19

The importance of new (novel) knowledge: .... 20

Kuhn and the importance of paradigms: .... 21

Incommensurability: .... 25

Chapter 3: .... 27

IR and Progress: 27
The concept of progress: 27
Metatheory and Scientific Progress: 29
Kuhn's Promise of Progress: 30
Lakatos and progressive research programs: 31
Explanation and Scientific Progress: 35
A Model of Scientific Explanation: 37
The explanation of events: 37
The explanation of laws: 39
Dimensions of Explanatory Progress: 41
Explanatory progress through historical research: 41
Explanatory progress through theoretical research: 43
Measuring progress between research programs: 44
Scientific Progress and Its Critics: 45
Chapter 4: 49
Democratic peace becomes "science": 49
The Study of Democratic Peace in International Relations: 50
Debate over Democratic Peace Hypotheses: 53
Theoretical critiques over the democratic peace: 54
Methodological Critiques: 57
Neorealism : 59
Conclusions: 63
References: 65

# Abstract:

**This dissertation** is trying to explore whether there can be scientific progress in the International Relations Theory. Scientific progress is examined in the context of social sciences and their inherent limitations, compared to the natural sciences. As accumulation of knowledge and theory choice criteria are thoroughly examined through the spectrum of contemporary philosophy of science, it is ascertained that scientific progress can be observed in research areas of the International Relations Theory such as the democratic peace and neorealism. The democratic peace research program more specifically, is based largely on quantitative methods and remains one of the most robust generalizations that has been produced to date by this research tradition.



**Key words**: social sciences, paradigm, research programme, scientific progress, democratic peace.

# Introduction:

Science has been evolving and developing since the dawn of time. Every systematic occupation with an academic discipline has as an objective to be categorized as science. Given the empirical nature of natural sciences, it has always been more troublesome for the social sciences to obtain a "scientific" status. The most prominent way to examine the scientific status is to evaluate whether there occurs any scientific progress, meaning if older theories are substituted by new. This dissertation will investigate whether there can be scientific progress in the International Relations Theory.

In the first chapter, I will engage with the matter of objectivity of science. Insomuch IR theory contains moral claims and other subjective elements; it becomes highly impossible to develop universally accepted conclusions about the correctness of answers. Hence, there is a rising opinion that the empirical theory produced by IR theory should be analogous to that of natural sciences-objective and free of moral judgments. I will also engage with the critical matter how social sciences are similar and different from natural sciences as well as how old theories get replaced by newer theories. In order to achieve that, there need to be a presentation of the character of natural sciences.

In the second chapter, I will examine what the philosophers of science call the demarcation problem, meaning the quest for a set of criteria to separate science from science from non-science. Considering Lakatos's theory, it is noteworthy to mention that the process of scientific research is understood through a series of research programs. The most crucial advantage of using the Lakatosian account to assess scientific progress is that it avoids the dangers of Popper's falsifiability. The acceptance or rejection of a theory does not depend solely on one test, rather than the ensemble of theories that form a research program. Considering Kuhn's milestone concept, the paradigm alongside with Lakatos's research program, is a contemporary way to assess the total development of the field while simultaneously evaluating areas of the field.

In the third chapter, I will deal with the interweaving relationship between philosophy of science and scientific progress. Philosophy of science is employed to establish that the science has been advancing and that the academic discipline is a work in progress. Especially in the IR, philosophy of science has been offering ways to bring an empiricist character in the field.

In the fourth chapter, I will argue that the area of democratic peace, as well as neorealism could be said to fulfill the criteria of scientific progress, as set by the prominent philosophers of science. Democratic peace studies have actually shown that a concrete body of knowledge in IR is possible.

# Chapter 1:

## Defining Science:

**Science** is an activity-organized in a systematic manner-whose purpose is to generate and accumulate knowledge. The term science was considered only for the scholarly divisions who engaged with empirical observation till relatively recently. The latest definition of science connotes also the body of respectable knowledge which can be taught, including this way the social sciences. The most often-used example in this dissertation concerning science and progress is the succession of Einstein’s Theory by Quantum Mechanics.

Einstein's Theory of Relativity and the development of Quantum Mechanics led to the replacement of Einsteinian physics with a new paradigm in physics which contains two parts that describe different types of events in nature (Τζέμος, 2013). From a relative theory of Einstein where positions and speed could be found as data through the Lorentz transformation, the modern physics through quantum mechanics and the superposition of a quantum (for example an electron) state that the quantum exists partly in all its particular theoretically possible states simultaneously until measured and observed-which sounds extremely like the experiment with Schrodinger’s cat.

## Characteristics of the Natural Sciences:

In order to determine whether IR and the social sciences are genuinely parallel to the natural sciences, and therefore within the scope of scientific methods, there needs to be specified what natural sciences are comprised of (Chernoff, 2007).

First, (**NS1**) science is based on observation that uses human senses and the senses are generally reliable, although not entirely. Hence, precautions like repetition must be taken under consideration so as to correct for possible receptive error. Second, (**NS2**) this demands that the world actually displays regular patterns of behavior. Third, (**NS3**) the regular patterns can be quantified. Fourth, (**NS4**) scientific theories involve causal explanations and causal mechanisms. Fifth, (**NS5**) by creating different conditions artificially, investigators can draw conclusions about the world external to the experiments. Sixth, (**NS6**) scientific theories employ two different types of terms: "observation terms" that refer to entities directly observed and "theoretical" terms that do not refer to what is directly observed. Seventh, (**NS7**) the investigation is "objective" in the sense that the behavior investigators observe is the same whether it is being observed or not; that is, the observation does not alter the behavior. Eighth, (**NS8**) the domain is "objective" also in the sense that different investigators would be able to make the same observations; what is observed does not depend on the personal traits, religion, nationality, or ideology of the investigator. Finally, (**NS9**) reliable, though not necessarily infallible, predictions will be justifiable from the theory.

**In sum:**

**NS1** Science is based on sensory observation, and the senses are generally reliable, although not entirely.

**NS2** The previous characteristic requires that the world display regular patterns of behavior.

**NS3** The regular patterns of NS2 can be quantified.

**NS4** Scientific theories involve causal explanations and causal mechanisms.

**NS5** By creating different conditions artificially, investigators can draw conclusions about the world external to the experiments.

**NS6** Scientific theories employ two different types of terms, "observation terms" that refer to entities directly observed, and "theoretical terms" that do not refer to what is directly observed.

**NS7** The investigation is "objective" in the sense that what the behavior investigators observe is the same whether it is being observed it or not; that is, the observation does not alter the behavior.

**NS8** The domain is "objective" also in the sense that different investigators would make the same observations; what is observed does not depend on the personal traits, religion, nationality, or ideology of the investigator.

**NS9** Reliable (though not necessarily infallible) predictions will be justifiable from the theory.

## The formation of a theory:

People observe with their senses one specific event, for example a ball rolling off the edge of a table, and notice that it is followed by another specific event, such as the ball falling to the floor. This sequence is watched over and over again until a regularity is postulated. Under some conditions this does not occur. For example the ball does not fall to the ground when the ball is on a table in a ship travelling through outer space. There are then initial conditions for the regularity to hold, like being near to Earth, which must be taken under consideration. The time it takes for the ball to drop to the floor should be measured and use the measurements to state the regularities in quantitative terms.

It is also expected that a truly scientific enquiry must be objective, meaning, observer-independent in at least two specific ways. One is that it is believed that the investigators' observations provide them with knowledge of how nature functions. Consequently, the sequences of events observed would happen the same way if the investigators were not there to observe it. The ball rolling off of the edge of the table would fall to the floor in the same amount of time whether or not there was someone watching and measuring. A second form of objectivity that is expected of science is that if the conditions are replicated by another investigator, they will produce the same results.

After a regularity is formulated and quantified, the scientist takes several further steps. One is to ask why the regularity holds, that is, what causal mechanism there exists. The causal mechanism is what reaffirms the scientist that the many times the first event is followed by the second event was not mere coincidence. Gravitational force may be given as an explanation. Secondly, the scientist will research for other regularities to add to the first to produce explanations of more complex phenomena that have been observed. Some of the new regularities might help to explain the initial regularities. A system of laws is hence created, each with causal explanations.

Once a regularity is observed and formulated, scientists want to know if it is true as stated, true but in a more limited form, or not true at all. They try to create the proper conditions to find out. Then they observe what happens. Similarly, there are also cases where two different regularities or complexes of regularities are offered by different investigators and only one can be true. What scientists want are particular observations under particular conditions. Thus they might want to create the right conditions and observe what happens. This leads to experimentation in the natural sciences. The system of regularities with initial conditions and causal explanations are often regarded as the core of a scientific theory.

A traditional element of scientific theories is that when they are fully formulated with their descriptions and causal explanations, they will require the use of theoretical terms. In other words, some of the propositions in a theory require terms that do not refer to observable entities (Wight, 2002). Some use terms that do not clearly refer to observable phenomena but are abbreviated ways of saying things that could be rephrased in purely observable terms, though they would require much more effort and explanation to do so. Sentences of that sort are either true or false. Then there are statements that use terms that appear to refer to things that human beings could, even in principle, never observe.

For example, the explanation of why objects with mass attract one another is a result of an unobservable gravitational force (Chernoff, 2007). While the existence of gravity is posited to explain various events that we observe, the force itself is not observable. The term "gravitational force" is thus a theoretical term. There is no way to revise or rewrite the important statements about gravity that could say the same things in purely observational language. Some philosophers of science hold that all scientific theories have such terms and that they are to be sharply distinguished from observational terms like "pen," "table," and "floor."

In order to test a theory, one must do the following (Spegele, 1980) :

1. State the theory being tested.
2. Infer hypotheses from it.
3. Subject the hypotheses to experimental or observational tests.

4. In taking steps two and three, use the definitions of terms found in the theory being tested.
5. Eliminate or control for perturbing variables not included in the theory under test.
6. Devise a number of distinct and demanding tests.
7. If a theory is not passed ask whether the theory flunks completely, needs repair and restatements or requires a narrowing of the scope of its explanatory claims.

## Criteria of Theory Choice in the Natural Sciences:

There are many criteria that scientists have been said to use by historians of science and many that have been recommended to scientists by philosophers of science (Chernoff, 2007). Eight of the most widely discussed criteria are:

1. *Internal consistency*
2. *Coherence*
3. *Simplicity*
4. *Corroboration/Range*
5. *Falsifiability*
6. *Concreteness*
7. *Fecundity*
8. *Methodological conservatism*

*Internal consistency* is the requirement that all statements of the theory are mutually compatible; one must not be able to derive any formal contradictions from any combination of statements within the theory. *Coherence* is the requirement that the propositions of the theory fit together not just in a way that avoids formal contradiction but creates a meaningful entity. *Simplicity* requires that a superior theory will, compared to its rivals, have fewer laws or laws that postulate fewer entities or causal mechanisms that are less complex. *Corroboration/Range* is the breadth of different events and kinds of events that can be inferred from the laws of the theory. (The idea of *explanatory power* is sometimes associated with range and sometimes with a combination of simplicity and range, which tend to work against one another.) These first four are the most widely accepted through-out the history of science.

Many other criteria have been proposed and debated[1]. The next four listed are more associated with particular philosophers or approaches than the first four and are subject to more debate. However, they are included here because they have, nevertheless, been widely adopted. *Falsifiability* is the requirement that we be able to imagine certain experimental outcomes or other specific conditions that, if we encountered them, would lead us to give up the theory[2]. *Concreteness* is the requirement that the theory represent reality in a direct way. This criterion is questioned by various philosophers, especially instrumentalists. *Fecundity*, which has been proposed relatively recently, is the goal of a theory leading us to consider events that we have not previously thought about. A theory that rates highly on this criterion will lead us to new ideas and to look in new places for observations, which in turn will lead us to propose hypotheses or laws. *Methodological conservatism* is the requirement that a new theory fit as closely as possible with the older theory it is replacing.

The preceding are the most widely discussed. But we should note three others. In the social sciences there has been considerable debate over the claim that theories must be value-free. Some have used their position on this question as a requirement for a good theory. So naturalists will insist on the proposition that a good theory

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[1] Political scientists like Vasquez (1998) and King et al. (1994) have defended falsifiability, consistency, concreteness and breadth or range (Chernoff, 2004). Some of the other criteria cited by philosophers of science are: fecundity, degree of corroboration, methodological conservatism, i.e., minimal change relative to previously accepted theory, etc. While many criteria have been advanced by various authors, the most universally accepted criteria are: internal consistency, coherence, simplicity and explanatory power. Studies of the history of science seem to reinforce the notion that these four are invoked by scientists.

[2] The methodology based on falsifiability was structured on the deductive rule of inference, modus tollens (Chernoff, 2004). Given a bold theoretical conjecture, one should find observational consequences (set up appropriate experiments) and ascertain whether the expected consequences follow. This process has been interpreted both as a demarcation principle and as a basis for scientific methodology.

must be *value-free*, at least in the sense that moral or other subjective propositions not be a part of the theory.

## From natural sciences to social sciences:

The positivist[3] account of science does not epistemologically, methodologically or ontologically provide an accurate model of the actual practices of scientists (Wight, 2006). The practicing scientist does not seek for constant combinations of observable events, but rather is involved in a process of modelling hypothetical mechanisms and inferring their necessary existence from their effects within emergent structured systems. Constant combinations are not laws, but a potential manner of their identification; their value is epistemological not ontological. This accents the creative and social aspect of science; scientific understanding is seen to consist in the move from a base in sensory perception and partial understanding to uncover unobserved and hypothesized entities, powers, structures and systems. This is achieved through the use of metaphors, analogies, similes, models and conjectures, and so forth, the role of which is to infer from the known the unknown. Once this non-positivist account of science is accepted, any argument about the possibility of a science of society that bases itself on positivism is bound to be misleading (Friedman, 1997). If positivism cannot be assumed the correct account of the method of natural science, the question of naturalism versus anti-naturalism must be re-examined.

In the natural sciences, the easiest phenomenon to study and observe is what is referred to as a "closed system", which is a system without external interferences. When new levels of systems occur with the combination of particulars into systems, the study becomes trickier even for the natural sciences. This interference, "noise" as called in Quantum Mechanics devalues positivism in the study of society. Societies are clear to people ; social forms are a necessity to act in a social framework. Those social forms in their turn authorize their autonomy and their reality, as along as every form of social activity assumes their existence.

[3] Positivism will be examined thoroughly in the following chapter.

In this framework, I must establish, as it is the object of study of the social sciences, the ontological difference between society and people. Society is a structure and people are conscious agents. Society entails relations and rules that offer governance in a stable manner. Moreover, society can be empirically observed in all its material glory.

## The academic discipline of International Relations:

Part of the academic community has argued that the social sciences work in a way parallel to the natural sciences, and the scientific method could be applied to IR (Chernoff, 2007). The use of scientific methods to test IR claims was strongly proposed by J. David Singer, Melvin Small, and their colleagues involved in the Correlates of War (COW) project[4]. In the 1960s they argued that many of the IR issues debated at that time were essentially the same as those debated centuries earlier, since neither side had defeated the other. This is a situation that is not common in the natural sciences. As mentioned in the introduction, the prevalent theories of physics in the mid-19th century were challenged by Feynman and his followers. Principles of Quantum Mechanics were soon accepted and remain in full effect in physics till this day. When the Einsteinian view was challenged in the mid-20th century it was not challenged by a centuries-old theory but by a very new one, namely the theory of Quantum Mechanics. Investigators examined the evidence, applied widely accepted standards of scientific reasoning and criteria of theory choice, universally rejected the old theory, and embraced the new theory. This is in fact a clear example of scientific progress.

The discipline of IR needs to be confronted like the other sciences and in its future evolution IR will face the likely methodological threats. IR needs to produce a

[4] The **Correlates of War** project is an academic study of the history of warfare. It was started in 1963 at the University of Michigan by political scientist J. David Singer. Concerned with collecting data about the history of wars and conflict among states, the project has driven forward quantitative research into the causes of warfare. The Correlates of War project seeks to facilitate the collection, dissemination, and use of accurate and reliable quantitative data in international relations. Key principles of the project include a commitment to standard scientific principles of replication, data reliability, documentation, review, and the transparency of data collection procedures.

consistent conceptual system which can generate in its turn novel explanations and making them known. Moreover, knowledge should be relevant to the settlement of contemporary issues. Obsessed with the current phenomena, IR lacks acceptable foundation and derogates history.

## Connecting science with philosophy of science:

Having to confront with the impossibility of putting an end to the science question within social sciences and especially IR by turning to the philosophy of science, what should finally be done still remains an actual matter (Elman C. and Elman M, 2003). Since there cannot be definite answer to the question of what science is by alluring to consent in philosophy, one option is that each becomes a philosopher of science, and to spend time and academic efforts trying to resolve difficult and abstract issues about the status of theory and evidence and the confinements of epistemic certainty. Nevertheless this option is not viable. It would be like suggesting a physicist to become a philosopher of physics so as to elucidate the nature of physics and its scientific substance. This also misrepresents the relationship between philosophical debates and scientific practice; practicing scientists have an adequate working definition of what it signifies for something to be "scientific," but this "is less a matter of strategy than of ongoing evaluative practice," conducted in the course of everyday knowledge-producing activities (Taylor, 1996 :133). As it is not desired for a physicist to research about the scientific status of physics rather than producing knowledge, IR scholars should prefer to generate knowledge about world politics. A philosophical foundation is researched and used in a wider framework to portray a set of philosophical and methodological principles that implicate to resolve questions raised by social science theory.

So as not to become philosophers of science, perhaps the current practice should be continued: deploying philosophical aids while on an "ongoing evaluative practice" of one another's scholarship about world politics(Elman C. and Elman M, 2003). For one thing, the grandiloquent power of an appeal to science within IR, as within other scholarly fields that have received a "science question" from their precursors (Steinmetz, 2005a), depends on a claim— perhaps implicit—that the criteria classified as scientific are in fact the kinds of knowledge-production practices that, if adopted, will ascertain IR as a science. In essence, at least, this is an allegation that can be appraised, and more importantly, it is an allegation that can be true or false. Whether it is true or whether it is false has enormous consequences for whether there should be an engagement in the specific course of action. While the lack of consensus among philosophers of science should give up the idea that any given knowledge-production practices are *uniquely* scientific, it is still entirely plausible to ground claims to scientific status in more stable philosophical arguments, and thus to move beyond the merely tactical use of a term such as "science."

According to Larry Laudan I argue that philosophers of science ought to shift their attention to "the question of reliable knowledge" and quitting any attempt to define the limits of scientific practice (Laudan, 1996: 222). But this proposal is only feasible within a academic field not as dominated by the science question as IR has historically been. Whether the philosophy of science is itself a science remains a much less demanding question than the question of whether the study of world politics is or can be a science.

Hence, the best response to the fact that the science question cannot be simply resolved by a turn to philosophy is to *replace* the narrow definition(s) of "science" circulating in the field with a definition that simply cannot be used by partisans of any single approach to the study of world politics as part of an effort to render their opponents' claims unworthy of serious consideration. What should be avoided, as a field, is disdainful caricatures of one another's work as "storytelling" and the accompanying characterization of those approaches as "unscientific" and hence not worthy of intellectual engagement. Instead, a principle of charity (Blackburn 1994, 62) is called for: treat other arguments about world politics as serious attempts to generate knowledge.

In order to craft a sufficiently broad definition of science, it is important not to replicate the errors and weaknesses connected with the disciplining deployments that are criticized. As such, it is unlikely that an acceptable definition of science can be produced by looking for fundamental "rules of inference on which" the "validity" of "scientific research depends" (King, Keohane and Verba 1994). The reason is simple: different kinds of empirical research in IR comply to different "rules of inference," and some reject inference itself in favor of (for example) thick description or structural overdetermination or discourse analysis. Hence, making some set of "rules of inference" the criterion for scientific status simply replicates the same disciplining move under the guise of advancing a putatively neutral set of methods and techniques. Arguably, *any* attempt to specify universal rules and procedures is doomed to collapse into a disciplining move, since there are no rules so universally agreed upon that their adoption would be uncontroversial. The commonality of "science" in IR, then, cannot be sought in rules or procedures for handling evidence or evaluating claims.

Perhaps the common element animating a broad definition of science can be found not in the supposed methods of science, but in the *goals* of science. Colin Wight suggests that "what distinguishes scientific knowledge is not the method of knowledge acquisition, nor the immutable nature of the knowledge produced, but the aim of the knowledge itself," which he takes to be the "explanatory content" of scientific knowledge (Wight, 2006: 61). Defining science in this way seems promising, as long as the precise definition of "explanatory" is allowed to vary so as to encompass a variety of approaches to explaining phenomena in world politics. Unfortunately, Wight promptly goes further in specifying a sense of "explanatory" that excludes more than a few ways of studying world politics:

What signifies scientific knowledge out from other forms of knowledge is that it tries to go beyond appearances and provide explanations at a deeper level of understanding. This implies that the scientist supports that there is a world beyond the appearances that helps explain those appearances (Wight, 2006: 18).

This may be the most important contribution of a broad and pluralistic definition of science: to cure IR of its constant envy of other fields of academic inquiry by stressing the important conceptual work on the matter of science that has already been done within the social sciences themselves.

# Chapter 2:

## The influence of positivism in the evolution of science:

**The most influential school** of philosophy in the early 20th century was logical positivism, which argued for both foundationalism and empiricism (Chernoff, 2007). Its representatives were for most of the part defendants of the traditional concept of science, but with two noteworthy divergences. According to them, knowledge must have an empiricist character and must be constructed through logical assumptions. Under the light of revolution in physics, mathematics and logic appearing at the beginning of the 20th century, they contended that the new methods of acumen would offer an arrangement of knowledge to be evolved through a framework. The main goal was for philosophy to progress in the way that physics and mathematics offered, which depended upon eradicating many traditional questions asked by philosophers.

The roots of the traditional demarcation problem in the philosophy of science renege to the early 20th century logical positivists of the Vienna Circle (Jackson, 2008). The trend at this time in theories was racial and national destinies. The logical positivists, having to defy this trend, sought to elucidate a foolproof way to differentiate between a scientific and a non-scientific statement. Besides being an interesting intellectual puzzle, the scientific status of a claim was also an ambitious political and social problem: it mattered a great deal whether a condemnation of the received wisdom about sexuality, time, space, or governmental authority should be considered scientific and hence worthy of respect, or unscientific and hence intellectually valueless (Moses and Knutsen 2007, 38–39; Lakatos 2000, 22–24). The logical positivists' major criterion for distinguishing a scientific from a non-scientific claim was *verifiability*, which supported that a claim could only be scientific if all of its terms could be checked or confirmed through an empirical examination. The verifiability criterion would exclude claims involving *entelechy* in biology, 'historical or *self-unfolding of absolute reason* in history, because they were not verifiable—but were instead mere metaphors without cognitive content (Hempel, 1965b :237).

One of the most important figures in this school, Rudolf Carnap, eventually claimed that philosophy was the logical investigation of the many possible formal languages (Carnap, 1937). Logical positivists and logical empiricists emphasized the understanding of language at the center of their enterprise, believing that the vast amount of wasted effort over the centuries had been a result of a misunderstanding of how language works and how ideas and meanings are conveyed.

Logical positivists strongly supported the idea of a single method for all sciences (Chernoff, 2007). They believed that all scientific knowledge is developed within the same framework of enquiry, observation, and analysis. The Vienna Circle included several social scientists who carried the scientific project into sociology. While they did not discuss the field of IR, the application of methodology of physics and formal logic to sociology would apply equally to IR.

The claim that accepted theories in both the natural and social sciences must be regarded as fallible will come as no surprise (Chernoff, 2008). Logical positivists sought foundations of certainty for scientific reasoning, but later in the twentieth century foundationalist views became extremely rare. Many other philosophers whom associate with the most optimistic goals for scientific certainty did not view the basis of natural science as infallible. For logical positivists, once the protocol sentences are accepted they remain accepted and become foundations on which scientific theory and knowledge are built. But this view was surrendered half a century ago by many philosophers, including influential allies of the Vienna Circle like Carl G. Hempel (1965), who argued that even ‘‘observation reports’’ are fallible. The reports of our sense are always subject to at least some small degree of uncertainty (especially if we are dreaming or hallucinating); Hempel thus rejected the logical positivists’ foundationalist view of scientific knowledge.

Progress in science and human knowledge would then be possible, according to logical positivists, when scholars stopped wasting time on fields that deal with inherently unanswerable questions. In the view of logical positivists it was essential that it should be possible to state a precise and rigorous rule for separating meaningful questions from the meaningless (Chernoff, 2007). As noted, after years of discussion, the key seemed to be that the unanswerable questions and the attempts to answer them were, in a literal sense, meaningless. Questions in logic, natural science, and some philosophical questions were meaningful. Questions in metaphysics, ethics, religion, and elsewhere were not. The core task was then to find out how to separate meaningful statements in the languages of philosophy and science from the meaningless ones. As noted, logical positivists argued that what makes a statement meaningful is that it can be verified. Logical positivists defended "the verification criterion." They came to identify the meaning of a statement as its method of verification (Carnap 1959, 69–70).

Finally, logical positivists were committed to *the unity of science*. This idea was interpreted in slightly different ways by different people, but it included, at the very least, a scientific method that would apply to any investigation that could be called science. This is a powerful form of naturalism. Most also held that it would include a set of observations that could be used by all sciences. The logic of scientific enquiry would allow these observations to be used to build up more and more complete scientific theories. The most extreme view of the unity of science, which some adopted, held that there would ultimately be a single all-encompassing science that would explain what happens in both the natural and social worlds, and would include physics, chemistry, biology, psychology, economics, politics, and so on.

In general, Kuhn and other historically oriented philosophers of science have shown that a priori grounds for demarcating science from non-science fail to capture the multifarious and pluralistic nature of the scientific enterprise (Spegele, 1980). Many studies of diverse kinds are carried out under the name of science and when one dogmatizes about it, without recognizing the diversity it covers, one is sure to go wrong.

## The starting point for the demarcation problem:

A philosophical solution to the demarcation problem would specify an answer to the great question of how IR would obtain a scientific character. The most outstanding use of IR has been to create a scientific path inside IR. IR structures its arguments in a scientific way, in her effort to become a science. Engaging to a scientific practice $sp_{1....}sp_n$ will lead IR to an indisputable scientific status. Amidst the effort to render this social branch to science, it has been compared to evolutionary biology (Bernstein et al, 2000) or paleontology (Van Belle, 2006) conceiving this way a set of demarcation criteria that could be transferred from one domain to another. If it is functional enough for physics or biology, it should work in IR.

## From positivism to Popper:

From the 1930s onwards the influential logical positivist account of science was challenged 'from within (Kurki, 2008). What replaced the dominance of logical positivism in philosophy of science was the standard positivism of Carl Gustav Hempel and Karl Popper. In his early work on science, Karl Popper (1965, 1968) seeked a demarcation criterion ,that would separate the genuinely scientific from everything nonscientific (Chernoff, 2004). Popper researched a criterion to science, rather than meaning in contrast to logical positivists who researched a criterion of the meaningful. Although there was some common ground between Popper and the logical positivists, he argued against them on a variety of issues for instance, flaws in the verification of the principle of meaningfulness (Popper 1968:35–40), the role of metaphysics (Popper 1968:35–39), and the necessity for universal laws (Popper 1965:288–289).

Popper was heavily influenced by Hume's criticism of induction, as a result of which he denied the acceptability of induction (Hirschman, 1970). This criterion of the established on deduction, he assailed induction. Popper's criterion was falsifiability and its methodology was based on the deductive rule of interference-modus tollens. Given a bold theoretical conjecture, one should find observational consequences (set up appropriate experiments) and ascertain whether the expected consequences follow. This process has been interpreted both as a demarcation principle and as a basis for scientific methodology.

These philosophers of science were ingrained within logical positivism but attacked its excessive reliance on inductive inference (Chernoff, 2004). Popper argued that scientific knowledge does not stem simply from inductive observation but, rather, from deductive testing of hypotheses. Popper accepted that scientists take under consideration many theoretical and conceptual (or metaphysical) pre-conceptions before engaging in empirical testing. He also accepted that verification by empirical testing never proves conclusively a scientific truth, as the logical positivist view of science had assumed. He maintained that by rejecting the logical positivist inductive view of science in favor of a deductive and falsifiability-based model of science -the practice, rationality and progress of science can be justified far more adequately.

According to Popper, a crucial point to a scientific theory is falsifiability: any person can empirically examine a theory and consequently either authenticate or falsify it. Science's main goal is to demand about the claims made and use the falsifiability criterion. Hempel and his deductive-nomological model (DN) model of explanation sums up this method of scientific inference. The DN or covering law model claims that the explanatory and predictive logic of science requires that events (explanandums) should be analyzed through a logically deductive analysis of two kinds of empirical statements, general laws and initial conditions (explanans). Popper argues that to give a causal explanation of an event means to deduce a statement which describes it, using as premises of the deduction one or more universal laws, together with certain singular statements, the initial conditions. This means that to explain something causally we have to describe (a) the universal laws that have been observed and (b) the initial conditions referring to a particular time and place; then (c) deduce the event to be explained.

Observation must acquire a general character in science. Therefore, so as to achieve scientific knowledge there should not be deep ontological assumptions about the nature of observables. In order to obtain a reliable knowledge, scientific analysis must not deviate and make unnecessary contemplating about unobservable entities.

Progress is a matter of conjectures and refutations (ibid). Bold conjectures are offered; refutations are attempted; and if the refutations fail, the conjectures constitute a better basis for knowledge than the refuted propositions that are known to be false. Popper argues that bold conjectural propositions, for which little or no basis exists at the time of their initial proposal, are the core of scientific progress. These conjectures are then subjected to rigorous testing. If they are true, they will stand up to rigorous testing and acceptance of them will advance progress. Weak conjectures are not as likely to move scientific progress forward because they largely overlap the evidence and do not go beyond it, thus failing to add enough of what Popper (1968:41) calls content. They do not reveal powerful new truths. Popper (1965:vii) says that progress is possible in ‘‘our scientific knowledge by unjustified (and unjustifiable) anticipations, by guesses, by tentative solutions to our problems, by conjectures. These conjectures are controlled by criticism, that is, by attempted refutations, which include severely critical tests. They may survive these tests, but they can never be positively justified… even as probable (in the sense of the probability calculus)’’.

## From Popper to Lakatos:

A metatheory is an inquiry at one remove: it is a view of our knowledge of things, as distinct from that knowledge (Chernoff, 2008). One of the most influential statements on the advancement of knowledge is Karl Popper's argument that since science is by definition disprovable, “good” science consists of theories that we attempt to disprove (falsify) but cannot: theories that survive severe tests. (Popper’s famous example points out that one could observe any number of white swans, but this would not prove the hypothesis that all swans are white. However, a single black swan can disprove the hypothesis. This concept is known as falsifiability[5]. If empirical data refutes a theory, the theory must be rejected and a replacement must be found.

[5] According to Lakatos, all scientific theories must meet the minimum criterion of being in principle falsifiable on the basis of publicly available evidence, and social scientists should approach their knowledge claims with that in mind (Chernoff, 2005). Beyond this, however, we should be tolerant of the different standards of inference needed to do research in different areas.

Nevertheless, Lakatos broadened the spectrum of his theory through Popper and both acknowledged that scientists are mostly unwilling to reject their theories. They usually deal the weakened evidence by trying to save them and rewrite part of their theories only to deal with any inconsistencies. Popper and Lakatos both tried to conceive rules for deciding when such repair moves were acceptable. The model of Lakatos of scientific change exceeds Popper ; it switches from an estimation of an individual theory to a series of theories. These series were named scientific research programs (SRPs) and combine an ensemble of essential and fundamental assumptions.

In order to classify a theory as progressive, it must foresee new facts. If this fact does not occur, then a new theory can be only ad hoc and belongs to a degenerative research program. What constitutes a new theory is a troublesome definition: the new theory must conjecture something else further except for the atypical facts employed in its creation.

Lakatos's approach is often contrasted with that of Thomas Kuhn, whose theory of scientific development sees scientific change as being revolutionary and non-rational, consisting of the wholesale replacement of one dominant view of how the world works (a paradigm) by a different one (Polsby, 1998). In contrast to Kuhn, Lakatos rejected the view that a single research program controls a scientific discipline at any given time, or that the decision to reject an old research program and accept a new one was akin to a non-rational conversion involving a leap of faith . He argued instead that research programs should be judged on the basis of rational criteria: their ability to successfully generate predictions of novel facts that are subsequently corroborated with empirical evidence.

For Imre Lakatos this kind of prescriptive way of seeing science is very dangerous, because new ideas might come from demagogues and it's simply not a very pluralistic or open way of speaking about science, to say that it's either the old theory or the new one (Elman C.-Elman M, 2002). So he suggested an alternative way to measure progress in scientific research, and I think that his approach is intuitively appealing for many IR scholars, simply because we try not to throw out our theories with each challenging real-world event but rather we try to improve our theories as a response in order to explain what's happening. Unlike Kuhn, who saw science as dominated by single monopolistic paradigms at any given time, Lakatos's MSRP is far more tolerant in anticipating that at any time a given science will have several scientific research programs. I think this more accurately describes the field of IR. Lakatos envisioned a pluralistic science and he was justifiably worried about the 'destructive effect' that naïve falsification strategies could have on budding scientific research programs.

Yet a Lakatosian view on progress in IR doesn't support relativism; I would take a stand against scholars holding on to 'their' co-existing theories which both fail to explain what's happening in international politics (Schouten, 2009). Lakatos tried to cultivate a theory of scientific method which would be delicate enough to engage with the specific aspects of the present history of science.

## The validation of new theories through scientific research:

Taking a distance from Popper's theory that science should be watched and criticized as a sequence of individual theories, Lakatos supported that scientific branches are best organized as a series of SRPs (Elman C. and Elman M.,2003). SRPs contain four elements: a hard core, a negative heuristic; a positive heuristic and a protective belt from auxiliary hypotheses. Further elaborating, the program's hard core assumptions contain the content of a theory that remain stable and are protected by a negative heuristic. In its turn, the negative heuristic is a set of sentences that suggest that this content is not directly challengeable or testable.

As far as the protective belt of auxiliary hypotheses is concerned, it is the one who is subjected to tests and bears all the changes in order to defend the core (namely, it can be adjusted, re-adjusted or totally replaced). The protective belt is constructed in harmony with the program's positive heuristic. This part contains a set of ideas who give supplementary details, draw conclusions through the use of empirical observations and assess them, introduce new assumptions and implement them on new academic areas; have the ability to modify when anomalies are discovered.

Besides a means for describing competing research programs, the methodology of scientific research offers criteria for comparing and judging in case that innovations or ameliorations in theory progress the theory or not; the so-called problemshifts. Taking a further step, the problemshifts are categorized to intra-program and inter-program. An intra-program change is the one that alters the protective belt. An inter-program change is the one that alters the elements of the hard core, hence moving science from one SRP to another.

These inter-program and intra-program changes don't offer in any case added value. They are both considered as degenerative when seen as ad hoc attempts to rectify the alarming proof. An essential note is to comprehend when a theoretical adjustment is considered ad hoc by grasping the concept of novel facts, which are analyzed in the following section. When a RSP is degenerative, a new theory can only rescue the program from disconfirming evidence. In opposition, when a RSP s progressive, the novel facts can offer and incorporate supplementary content that could not be used up to this point.

Proceeding to the progressive or degenerative element of a problemshift, in this dissertation I will examine three separate concepts of ad-hocness. In ad hoc1, a theoretical change does not cause any new predictions. Ad hoc2 has been employed when none of the new predictions in the theory have been able to verify empirically. Finally in ad hoc3, the auxiliary hypotheses are altered in manners that do not agree with the essence of the positive heuristic. An intra-program problemshift has to comply to all three ad hoc criteria. When an inter-program problemshift is referred, it has to steer away from becoming ad hoc1 and ad hoc2.

## The importance of new (novel) knowledge:

Good science could not be restricted to saving theories. Novel knowledge is highly important because it can create new theories and force scientists to a constant duty of trying to salvage theories. A problemshift should be theoretically progressive and generate novel predictions, because the mere explanation of things is barely enough to be calculated as real science.

The problemshifts constructed to account for disagreeing facts are not faulty, as long as they in their turn generate novel predictions. By any means, problemshifts do not just predict novel facts, but have also to empirically test these predictions. If they are not authenticated, the problemshift is categorized as ad hoc2.

The most direct but limited way to develop background knowledge is that all the facts proposed are established through science at this time. The temporal restriction has been rather draconian and was modified by Elie Zahar's proposition that some well-established facts should be calculated nevertheless in an attempt of a progressive problemshift. The alternative that an older theory reinterpreted by a new theory, converts it into a new fact was too weak to be supported in the long run.

## Kuhn and the importance of paradigms:

In 1962, Kuhn's *The Structure of Scientific Revolutions* transformed the philosophy of science, and intellectual life more generally (Walker, 2010). Yet Kuhn never intended his ideas for the social sciences. In the preface to *Structure,* Kuhn had indeed emphasized how paradigms set the natural sciences apart from the social sciences. Kuhn characterized the social sciences by their fundamental "disagreements" over the "nature of legitimate scientific problems and methods." The natural sciences, by contrast, failed to "evoke the controversies over fundamentals." As a result, Kuhn grew critical of social scientists seeking to "improve the status of their field by first legislating [paradigms and normal science] …They are badly misconstruing my point." Imre Lakatos had a similar reaction to social scientists applying his notion of scientific research programs. He referred to some of these efforts as little more than "phony corroborations" that yield "pseudo-intellectual garbage."

The emergence of paradigm mentalities, as depicted by Kuhn and Lakatos, leads to narrow, rigid, highly specialized, and conservative research approaches that suppress alternatives (Schouten, 2009). For Kuhn, evidence that falls outside the dominant framework is considered incommensurate and can be ignored. When political scientists are guided by paradigm mentalities they hold tightly to both their theory and their method while seeking to insulate themselves from opposing theory and method. They also engage in hostile, zero-sum turf wars when challenged by alternatives. Paradigm mentalities prompt scholars to break into narrow, highly-specialized, esoteric research communities.

Kuhn's paradigm might best be understood in terms of its life-cycle (Walker, 2010). A paradigm is born when a concrete scientific achievement resolves debate over foundations, assumptions, and methods in a scientific field of inquiry. The concrete achievement suspends debate over fundamentals and forges a consensus among scientists. This consensus initiates a period of normal science. Kuhn stated unequivocally that paradigms and normal science can exist only when based upon one or more past scientific achievements, achievements that some particular scientific community acknowledges for a time as supplying the foundation of their practice. Examples of Kuhn's universally recognized scientific achievements include those of Newton, Lavoisier, and Einstein. Kuhnian paradigms cannot be called into being by scholarly consensus alone. Paradigm formation must be anchored by a major scientific achievement that a particular community of scientists finds convincing.

Kuhn's normal science can be characterized by the effort to fit small pieces into a large and complex puzzle (Polsby, 1998). The normal scientist's task involves an intense concentration on the small pieces, while ignoring the bigger theoretical picture:

For Kuhn, periods of normal science must be progressive and based not merely on a promise of answers, but "in the actualization of that promise …by increasing the extent of the match between those facts and the paradigm's predictions. Most periods of growth are characterized by the dominance of one paradigm and the practice of normal science. For Kuhn, this constitutes "the most efficient mode of scientific practice." The normal science that follows scientific achievement is characterized by slow and steady growth in knowledge (Neumann and Waever ,1997).

When confronted with major anomalies, Kuhn's normal scientists simply ignore them (Walker, 2010). According to Kuhn, I argue that no part of the goal of normal science is to awaken new kind of phenomena; indeed those that will not fit in the box are often not seen at all. By somehow ignoring anomalies, scientists can commit to their exclusive attention to effectively solving pieces of the paradigmatic puzzle.

Given scientists' strong commitment to the maintenance of their paradigm, they are frequently hostile toward alternatives. In periods of normal science, scientists do not invent new theories [and] they are often intolerant of those invented by others. Kuhn's normal scientists rigidly adhere to their paradigm even when it is discredited by the evidence.

At first Kuhn's work was hailed by proponents of "scientific" or "behavioral" political science as providing (among other things) an explanation for the persistent tendency of political scientists to talk past each other: they have been operating within different "paradigms" of political inquiry; and so long as no authoritative single paradigm emerges, they will continue to do so, and political science will remain in its present "backward" condition (Ball, 1976). But then, as Kuhn has shown, the natural sciences themselves have at times been similarly unsettled. And this was taken by some political scientists as a sign of hope. Political science had, they believed, undergone its own "revolutionary" phase (lasting two decades, three centuries, or 2,500 years, depending upon how one measures it), but was now about to become a "normal" or "mature" science in Kuhn's sense.

Political science should rather strive, as one commentator put it, to "attain a multiparadigmatic condition" (Beardsley, 1974, p. 60). Or, to put it in Kuhn's terms: political science should remain in its present "immature" state.

Although Kuhn has popularized the term "paradigm," he did not coin it. Our word "paradigm" derives from the Greek *paradeigma,* meaning model, pattern, or exemplar. In the *Republic*, for example, Plato speaks of his ideal polity as a "paradigm (παράδβιτμα) laid up in heaven" (592 b). The first to speak of paradigms in the natural sciences was the eighteenth-century philosopher Georg Christoph Lichtenberg, for whom a paradigm was an accepted standard model or pattern into which we attempt to fit unfamiliar phenomena; and when we have done so we say we have "explained" or "understood" them. It is in this sense also that Wittgenstein

(1968) spoke of paradigms and (Austin, 1970: 202) of "thought-models." This earlier use of the term is rather more precise and restricted than Kuhn's original (1962) use of "paradigm" as a portmanteau concept enclosing exemplary scientific achievements, theories, successful experiments, *Gestalten,* and world-views, among other notions (Masterman, 1970). Wanting to control such terminological inflation, Kuhn now (1970a, 1974) prefers to speak instead of exemplary theories, or "exemplars" for short.

The first advantage—alluded to already—is that political science can now dispense with its pseudo-Popperian, or "dogmatic," conception of falsiflability.[6] That is, the outmoded and untenable view that a theory can be tested directly against facts which are wholly independent of the theory under test can stay in the past, and which, if they do not correspond, require us to abandon the theory as falsified.

Political scientists have used the Kuhnian paradigm in three fairly distinct ways in talking about political inquiry, but in all of these ways, the term paradigm is used more or less interchangeably with conceptual scheme, theory, framework or some other way of characterizing formulations (Stephens, 1973).

The three uses of the Kuhnian paradigm can be summarized as follows :

First, the term paradigm may be applied to the historical development of the discipline by looking at assumptions and ways of studying politics prevailing at a given time, and then speculating on why there was a paradigm change.

Second, the criteria for assessing theoretical formulations may merely be relabeled as criteria for assessing paradigms.

Third, Kuhn's original criteria may be used to argue that one formulation is the paradigm which political scientists have adopted, or should adopt.

[6]Pseudo-Popperian because Popper never was—despite many social scientists' misreading of him—a dogmatic falsificationist; he is, rather, a methodological falsificationist, though not a "sophisticated" one (Lakatos, 1970).

## Incommensurability:

Work (for example, Feyerabend 1962; Kuhn 1962) that aims to refute the possibility of social scientific progress makes use of arguments that seek to undermine the idea of progress in both the natural and social sciences (Chernoff, 2004). Chief among these arguments are those that are concerned with the principles of incommensurability of paradigms and radical underdetermination of theory by data. Postmodern critics of progress in international relations and the social sciences have made extensive use of these arguments, among their various lines of attack. They contend that no progress has been made in the field of international relations because the competing paradigms are incommensurable. In what follows, it will be shown that this argument appears unfounded given that, if there are incommensurable paradigms in international relations, they are generally taken to be those of realists and liberals. But the behavior of these two groups in the democratic peace debate does not conform to Kuhnian expectations.

Lakatos (1978:4) characterizes Kuhn's view as an affirmative answer, n contrast with his own negative answer, to the question of whether there should be an agreement that a scientific revolution is just an irrational change in commitment. In case Kuhn is verified, then there is no explicit demarcation between science and pseudoscience, no distinction between scientific progress and intellectual decay, there is no objective standard of honesty'' (Lakatos 1978:4).

The incommensurability of paradigms thesis is generally believed to undermine traditional accounts of science that conceive of theory choice as objectively or at least as intersubjectively valid (Chernoff, 2004). The outlines of Kuhn's philosophy of science, including the incommensurability of paradigms thesis, are well known. Kuhn (1962) rejects the logical positivist and logical empiricist accounts of science that dominated the middle of the twentieth century by arguing that they were entirely disconnected from the actual history of science. Theory choice is not a regular feature of scientific activity. Science proceeds quite dogmatically within a dominant paradigm that includes methodological rules and procedures for testing hypotheses. The dominant theoretical exemplars have various anomalies, which are not ordinarily taken as falsifying them. Moments of crisis do occur when the activities of normal science give way to the genuine choice of a new theory and paradigm. However, according to Kuhn, the choice is not made on the basis of philosophical, rational, or logical grounds of the sort specified by logical empiricists. Rather, the outcomes of the choices are driven by sociological factors. By Kuhn's account, rationally grounded changes from one paradigm to a better one are impossible; hence the ordinary notion of scientific progress collapses. Progress can occur within a circumscribed sphere only, within a single research paradigm.

According to the incommensurability thesis, when a particular field exhibits a lack of consensus, it may be the result of the impossibility of evaluating contending theories against one another. When consensus reigns, it is for sociological reasons and because of rational progress.

# Chapter 3:

## IR and Progress:

**An outdated view of IR** states that IR concerns mainly the relationships between states. IR is hence confined to the area of international politics. This view could have described accurately the Anglo-American IR but since the scholarly division has broadened its range, keeping the political element at its core. After the ending of the Cold War, a newfound interest was discovered and spread to other areas that interlace with IR. The international system of the contemporary IR is a more complex construct than it used to be, apart from the politico-military dimensions that are innate to this scientific branch: an economic dimension, a sociological and a historical one. IR has been making progress in order to entail all these dimensions and organize them in an appropriate manner. Despite the efforts to illuminate many concepts of the IR, the concept of international system is still obscure mainly because it is such a wide concept.

## The concept of progress:

The problems of observable phenomena in the social sciences are determined outwardly, have society as a median and their historic context is completely different to natural sciences. Political science is connected to history, thus the history of political science must immerse in the history of society. Political science is progressive as far as it can face the possibility of the ever changing empirical problems. In a more abstract way, political science is suggested to progress rather than vertically. Taking under consideration the vertical progress of natural science and that the research traditions are chosen and rejected in a rational way, there is more than one kind of progress.

Scientific progress is normally portrayed as a series of stable, rational choices between competing theories, research programs, or research traditions within a discipline (Dryzek, 1986). Progress and rationality of political science constitute two different things, as opposed to other sciences, where they tend to combine them. The progress of a scholarly division in general is comprised of a series of categorical and stable rational choice among research traditions or research programs. Political science can also entail rational comparisons between research traditions, always taking under consideration that any of the sort comparison should include the social context. It is the very nature of this social context-intrinsic to the discipline-which construes the difficulty to observe when it comes to political analysis. Thus, progress in its normal conception is not feasible. Notwithstanding, the concept of progress in political science can be reconsidered as a scientist makes efforts to comprehend the possibilities and obstacles of empirical observation. This reconsideration is a direct determinant for the handling of political inquiry. More specifically, the existence of diversity in research traditions is the main factor leading to progress. Traditionally, science progresses through cumulation of knowledge, where theories are formed, corroborated and finally included to the "body" of theory that has been repeatedly verified. Reflecting upon the Popperian view, the only way to progress is by falsifying the previous theories. A succeeding theory is then verified, as long as it can endure the constant falsification test. The falsification on its own possesses a progressive character, as the solution for a failed falsification test is a new and improved theory.

The crucial point of scientific activity is problem solution. Those problems I reckon have two fundamental forms; they are either empirical or conceptual. A problem is of the empirical king when a fact in the natural world is out of the ordinary and requires further information to be explained. A problem of any other kind is characterized as conceptual: when a theory has internal inconsistencies or used obscure analytical categories, or does not clarify the potential of empirical verification. This could be ascertained through an example that is used in this dissertation. The internal progress of Quantum Mechanics entails the doubt of the relativity of the position of an electron, as well as the rejection of the previous

Einsteinian theory, which was found to face internal inconsistencies.

## Metatheory and Scientific Progress:

Even those working political scientists who loudly declare their indifference to philosophy of science are inevitably using methodological toolkits based on prior, if unconscious, choices about what it means to achieve and to measure progress (Chernoff, 2007). Understanding those toolkits gives IR practitioners a better grasp of the potential and the limits of their selected methodologies, and a greater appreciation of the alternatives. While political scientists have often shown an interest in evaluating the state of their discipline, most have relied on partial and subterranean criteria. The international relations subfield is no exception.

Recent assessments identify theoretical developments in a variety of research areas, and rate those that have proved most and least useful to the study of international relations. They also question why some theoretical orientations—notably neorealism, dependency, and world systems theory—have become less popular, while others—such as rational choice, historical institutionalism, and constructivism—have received increased support. However, in identifying better theories, and describing the successes and failures of IR research programs, these field surveys rarely address whether there is a pattern to the fate of specific research agendas, or explain why particular theories of international relations have waxed or waned. More importantly, almost none of the recent appraisals adequately engage the question of what measures should be used to determine whether various theoretical moves are progressive.

There is a strong tendency in the subfield to engage in metatheoretic exercises without metatheory; to evaluate theoretical aggregates without using suitable or even necessary toolkits (Elman C. and Elman M, 2003). A good example is the assessment by Jeffrey W. Legro and Andrew Moravcsik of contemporary theoretical developments in realism, an analysis that, although it is cloaked in metatheoretic terms of art, overlooks pertinent epistemology. Legro and Moravcsik argue that "the specification of well-developed paradigms around sets of core assumptions remains central to the study of world politics," and accordingly they describe and evaluate realism as a metatheoretic unit.

The main points of Lakatos, specifically his support of tolerance and diligence enable IR theorists to observe and allow the existence of rival RSPs. I argue that as long as a scholarly division is found often competitive, this competition which causes progress should be mitigated by the fact that different research traditions are able to exist side by side. Moreover, an IR scholar should commit to his theory and be available to support his theory's correctness.

The emergence of paradigm mentalities, as depicted by Kuhn, leads to narrow, rigid, highly specialized, and conservative research approaches that suppress alternatives. For Kuhn, mentioned in the following section, evidence that falls outside the dominant framework is considered incommensurate and can be ignored.

## Kuhn's Promise of Progress:

Kuhn's scientists typically accept the confines of the dominant framework in exchange for small but steady increases in knowledge (Walker, 2010). To do otherwise is to risk isolation and obscurity, if not hostility. Loyalty to the paradigm will be rewarded with the gradual cumulation of knowledge. This implicit contract is one where scientists trade autonomy and theoretical breadth for a degree of security and narrow advances in knowledge. This promise of cumulation—and its realization—drive the conservative but progressive elements that characterize mostly Kuhn's paradigms.

For Kuhn, this implicit contract serves as a constitutional dictate that regulates scientific practice by issuing rewards and punishments in the form of acknowledgements or obscurity. This constitutional dictate is forged by some stunning scientific achievement. Without this scientific achievement, there is no consensus among scientists, are no paradigms, no scientific research programs, and is little chance of science progressing in the orderly and incremental fashion promised by disciplined inquiry as laid out by Kuhn and by Lakatos.

The stringent demands of consensus-forging achievements may be the reason why

Kuhn remains skeptical of their relevance to the social sciences (Stephens, 1973). Some argue that applying Kuhn helps lend legitimacy and status of science to the academic pursuits in political science. But as Kuhn noted, such efforts in the absence of a scientific achievement are misguided at best and fraudulent at worst.

Kuhn is pleased with the fact that the observable phenomena are defined on their own by the same paradigm, which contains testable theories. Kuhn argues for a more elaborate concept of progress in science. Quite particularly, progress from science to non-science happens with diction and imposition of a paradigm. An acceptable idea among academia was that the developing professionalization of the scholarly division can be announcing progress. When a paradigm is conclusively proved, progress is apparent and undoubtable. Amidst his reviewers it was considered that paradigms are completely incommensurable and incomparable. Consequently, the choice of a paradigm from another still remains an irrational matter among scientists. Even his defendants admit that the criteria of paradigm choice are kind of vague.

## Lakatos and progressive research programs:

The rationality of any choice among competing research traditions in political science must, then, be contingent upon time and place and a given set of socio-political circumstances (Dryzek, 1986). Empirical problems in the social sciences are externally defined, socially mediated, and historically situated in the way those of natural science are not. The history of political science must therefore be bound up with the history of society. Political science is an historical discipline. But the choice among research traditions in political science is no less rational for being contingent. Chronic flux in the rivalry of research traditions in political science should not be attributed to irrationality, mob psychology, or any supposed immaturity of the discipline. Hence political science can be a rational discipline. But it cannot be progressive in the same sense as the natural sciences, for stable and definitive choice among research traditions is impossible. Still there is a sense in which political science can progress, though only on a reconceptualization of progress. Stating the thesis at its boldest, political science is progressive to the extent that its ability to cope with contingency in the character of its empirical problems grows with time. This adaptive capacity is enhanced to the degree that a large number of potentially useful research traditions exist. Metaphorically, political science can be said to progress laterally rather than vertically. The vertical progress of natural science, a succession of research traditions chosen and eventually discarded on a rational basis, is not the only kind of progress that can occur. A capacity to cope with contingency may be illustrated by the fortunes of the Hobbesian research tradition. The Hobbesian ontology contains self-interested individuals and pressing absolute material scarcity of total resources relative to the subsistence needs of entire populations. Hobbes' own work never disappeared completely. Indeed, it has been one of the staples of political philosophers since the seventeenth century. On the other hand, little internal progress in the Hobbesian tradition is evident in the centuries following his death in which scarcity, at least in the Western world, was held at bay by continuing expansion of frontiers in the New World.

A similar illustration of an ability to deal with contingency can be found in the history of classical macroeconomics. After the general demise of the classics in the wake of the Great Depression, the tradition's flame kept burning only inside the walls of the University of Chicago and in a few isolated outposts elsewhere. The inflation of the 1970s yielded empirical problems which the tradition could address, and its flame spread like wildfire throughout the academy and into government. Again, the initial 1970s applications, in this case monetarism and rational expectations, were no improvement on pre- 1940s classicism, for they shared an inability to explain large-scale involuntary unemployment. Olson (1982) both pays homage to the classical tradition and constructs a clear advance within it, for his theory can explain involuntary unemployment and its anomalous coexistence with inflation. Olson traces stagflation to the political power in stable societies of distributional coalitions such as cartels and labor unions. His concurrent extension of the research tradition into empirical problem areas traditionally of more concern to political scientists than economists, such as social and political rigidities and the determinants of public policy is further evidence of renewed progressiveness.

When it came to the institutional theory, it proceeded roughly as a Lakatosian would suggest. It restated the core of the realist research program; identified and emphasized anomalies facing realism; proposed a new theory to resolve those anomalies; specified a key observational implication of its theory; and sought to test hypotheses based on that theoretical implication, searching for novel facts.

Among the IR scholarship, theorists tend to rely on the methodological concepts of Lakatos. He has confirmed part of the Popperian theory; nevertheless his alterations are the ones who signified his importance. According to Lakatos I argue that the scientists compare within the framework of a research program-which entails the theory, the necessary alterations while anomalies occur, supported by a series of heuristic rules. Therefore, the positivists' conception of the units put to the test should be criticized and grasp the concept that a sole theory is not able to compare to another theory alone, rather than in the framework of a research program.

In reality, when a scientist discovers an anomaly -meaning a result unacceptable to the current theory- he will not reject right away the falsified theory. Instead he will resume to the experiment and check their available means to reconsider what could have wrong. Even if the observations are unable to comply with the current theory, still the scientist will not reject it. Most of the time, he will suggest minor alterations. This altered theory will at some point come across an anomaly and will be further altered. Ultimately, the scientist is making an effort to save as much as possible of his theory by altering it. In order for a science to progress, it must not be restricted in the constant threat of falsifiability.

A progressive research program uses a powerful set of principles about how the world works to explain past events and to point our attention to events that we have not previously observed (Elman C. and Elman M, 2003). These may be future events, which obviously have not been previously observed, or they may be past events that were never interpreted as relevant for the discipline at hand; investigators might not have understood their connections to and role in larger patterns. Feynman's laws of

QED formed the hard core of the Quantum Mechanics research program and constitute a very powerful theory. Their power stems from their ability to withstand centuries of observations, without being falsified and without having to be modified in ways that undercut the hard core.

There is also a directive to keep testing the research program—the positive heuristic—and a directive to avoid violating the hard core—the negative heuristic (Nickles, 1987). The heuristics give us ways to evaluate one theory against another as other accounts of science do. But they also tell us something about how to revise a theory and how to replace one theory by another within the same research program. Lakatos thus says that one research program is to be preferred over a rival not only if the theory it contains is better than rivals, for example by having greater content or a simpler form, but also if the research program revises the theory in the proper way, in accordance with the heuristic rules. One research program is to be preferred over another if it follows the heuristic guidelines and points us in the direction of new facts to be discovered. If small adjustments are made in the hard core in order to keep the research program in line with new observations, then the research program becomes narrower and degenerates.

Let consider an example in IR that some authors maintain is progressive and which will be mentioned thoroughly in the next chapter( Ray 2003;Chernoff 2004). The monadic hypothesis (democracies fight wars less than nondemocracies), and the dyadic hypotheses (pairs of states that are both democratic fight less often than other pairs of states), were previously discussed. It has been noted that there has been powerful empirical support for the dyadic hypothesis, which has led IR theorists to search for explanations. After considering the plausibility of those explanations, some scholars considered some of the implications (Siverson 1995; Bennett and Stam 1998). One implication might be that, if democracies make decisions in this different and more cautious way than non-democracies, then they would be less likely to fight wars that would incur large losses; thus they would be less likely to lose wars or suffer large casualties. These hypotheses were tested and found to be true. This is one reason to think about the democratic peace research program as progressive, in Lakatos's sense. This is not something that scholars would have been likely to think of or to take measurements of before the debate on democratic peace advanced as far as it did.

In means of understanding, I should also examine a degenerative research program. Suppose a scholar looks at the many cases in which a large state defeated a small neighbor in battle and advances the principle that the state with the larger economy will always win in a war. This could be considered as the hard core of a hypothetical research program. This statement has considerable range and great content. It tells us with great simplicity something about all wars. Eventually a the anomaly is encountered—Vietnam, with a small economy, defeated the United States. A supporter of the "large economy theory" then tries to save the theory by arguing that the principle holds unless there is an ocean in between the states at war in which case the smaller economy will win. This does help to make the theory consistent with the observations, but it reduces the con-tent considerably and does not add any insights. It is especially unclear why the presence of an ocean would give the weaker economy an advantage.

Lakatos' writings generate compelling criteria to create an articulated scientific theory. A superior theory is part of a research program that is progressive. In particular this means that any modifications to the research program belong to the protective belt and not to the hard core, and it indicates to new facts and phenomena. Lakatos, in his theory, was consistent with the idea that real progress in science is feasible. In contrast to the critics who reject the traditional account of science, Lakatos developed a view that said there are philosophically defensible criteria that permit to evaluate and compare the merits of different units of appraisal, namely research programs, and they in their turn allow genuine scientific progress.

## Explanation and Scientific Progress:

The demarcation question had been given prominence, and answered influentially, by Karl Popper in the 1930s. The rationality question was pushed to the fore by Thomas Kuhn in the early 1960s (Ball, 1976). It was Kuhn's work that drew Lakatos's attention and motivated his work in the philosophy of science from the mid-1960s on. (Previously, Lakatos had worked only in the philosophy of mathematics.) Kuhn argued in essence that scientific change—at least during times of "revolution"—was not rational. Lakatos rejected this conclusion, and drew on Popper's earlier work to attack Kuhn, effectively fusing answers to the demarcation and rationality questions in a single argument (Polsby, 1998). Popper had argued that what made a theory scientific was its falsifiability. Lakatos's strategy was to adopt this demarcation criterion but to shift the unit of appraisal from the individual theory to *sequences* of theories. It is a succession of theories and not one given theory which is appraised as scientific or pseudoscientific, Lakatos asserted. Lakatos labeled such a succession of theories a "research program." He argued that research programs could be judged as either "progressive" or "degenerating," and that, on this basis, rational decisions between rival programs could be made.In this way, Lakatos meant to refute Kuhn's influential thesis concerning growth and change in scientific knowledge.

For Lakatos, progress in science is measured by the degree to which the series of theories leads us to the discovery of novel facts (Elman C and Elman M, 2003). Vibrant research programs that, over time, explain new facts, and in addition overtake lagging programs, are rationally preferable to the latter. Lakatos illustrated this understanding of scientific rationality with the story of Einstein's overthrow of the Newtonian paradigm:

*Einstein's theory is better than—that is, represents progress compared with—Newton's theory anno 1916 ... because it explained everything that Newton's theory had successfully explained, and it explained also to some extent some known anomalies and, in addition, forbade events like transmission of light along straight lines near large masses about which Newton's theory had said nothing but which had been permitted by other well-corroborated theories of the day; moreover, at least some of the unexpected excess Einsteinian content was in fact corroborated (for example, by the eclipse experiments).*

# A Model of Scientific Explanation:

Through this dissertation, I have established the empirical difficulties that IR (and as a matter of fact all of the social sciences) could encounter in order to achieve scientific progress. Therefore, it is necessary to use a model so as to organize a concept of progress through explanatory processes. The model of explanation employed in this section would be labelled as positivist by most scientists, including Popper. But its utility is so great that it is widely employed even by anti-positivists social scientists. This model is comprised of four elements: theories, laws, initial conditions and events, though the most crucial are laws and events. When taking under consideration this model, a law or event is analyzed in case something expected occurs under the existent circumstances. When an explanation is assembled, information that offers solid foundation to explain a phenomenon which has already or will occur, is necessary.

## The explanation of events:

An event is a concrete occurrence or happening at a particular place and time. To explain an event one must appeal to one or more laws, since without knowledge of regularities and recurring patterns in the world, we would have no reason to expect particular happenings at particular times. Event-explanation also requires a description of the conditions or the setting in which the event occurred. A particular event is explained by showing that it is the kind of event one would expect in the concrete circumstances that prevailed, given known laws.

A simple example illustrates this logic. Suppose we wish to explain the observed rise of mercury in a thermometer at a particular place and time. To do so, we must adduce information that enables us to see this event as something that was to be expected in the circumstances that prevailed. For example, we might point to the thermometer’s immersion in hot water (initial conditions) and cite the regularity that mercury expands when heated (a law). The regularity tells us that we should expect the mercury to rise in any thermometer that is heated. The initial conditions establish that this particular thermometer was, at the given time and place, heated. The law and the initial conditions together offer conclusive reasons (through the logic of deduction) for believing that the mercury in the thermometer did in fact rise at this time. Insofar as the mercury's rise is expected, it is explained.

The positivist model yields two distinct research strategies for the explanation of events. The first is a generalizing strategy, according to which researchers treat the event to be explained as an instance of a certain *type* of event, which is then shown to accompany or follow regularly from conditions of a specified kind. The example of the thermometer immersed in warm water, where the rise in mercury is explained by showing it is just the kind of event one would expect under the circumstances, illustrates this explanatory approach. An example in the study of world politics is Jack Snyder's account of why the Cold War's end was peaceful. He focuses on the Soviet Union and argues that "a state's foreign policy is shaped by the myths it holds about how to achieve security." Expansionist myths (which hold that a state's security is enhanced by aggressive expansion) are held in check in democracies, but can "run rampant" in polities that feature logrolling among highly concentrated interest groups. The state's domestic political structure is, in turn, shaped by the timing of its industrialization.

The second strategy is a particularizing one, in which the researcher explains an event by detailing the sequence of happenings leading up to it. In this approach, which aims at accurate historical reconstruction, there is no attempt to place the phenomenon in question into a larger class. The event is explained as the end-point of a concrete historical sequence, not as an instance of a particular type. In Hempel's words, this type of explanation "presents the phenomenon under study as the final stage of a developmental sequence, and accordingly accounts for the phenomenon by describing the successive stages of that sequence." Like a generalizing or covering-law account, a particularizing or reconstructive explanation necessarily relies upon laws, but these are component laws rather than covering ones, in that each pertains to only a segment of the pathway leading up to the event to be explained. Of course, each component law can be considered a covering law for the segment of the pathway that it explains, but the event itself is explained by the *sequence* of happenings leading up to it, and typically this sequence is not law-governed. (If the sequence is law-governed, a single covering law can be constructed to explain the outcome.) Like any sound positivist explanation, reconstructive accounts explain by showing that the event in question was to be expected in the circumstances in which it occurred.

These two strategies, and the explanations they yield, are equally positivist. Each fully meets the requirements of positivist explanation. Yet they lead to quite different ways of framing questions in research. In the generalizing or covering-law approach, an event is identified, and the researcher asks: what is this a case of? The logic of inquiry proceeds "outward" from the consideration of single events to the analysis of classes of events. In the particularizing or reconstructive approach, the researcher asks of the event to be explained: from what historical pathway did this event emerge? Inquiry then moves "inward" to a detailed reconstruction of the actual sequences, causal conjunctions, and contingencies that led, step by step, to the outcome in question.

Note that the law or laws used in an event-explanation must be theoretically-grounded. That is, we must be able to show that the laws can be derived from deeper regularities and structures, that they identify the operation of causal capacities and do not simply represent accidental conjunctions of event-types. Often, in explaining an event, the scientist will not only cite the relevant covering or component laws, but will suggest or point to the theoretical warrant for them.

## The explanation of laws:

A law is a regularity, or repeating pattern, that describes a causal relationship between two or more factors. Like events, laws are to be explained by fitting them into larger patterns, by appealing to other, possibly higher or more encompassing, laws. All positivist explanation, whether of events or laws, is thus "nomic," or reliant on laws. Hempel concludes that "all scientific explanation ... seeks to provide a systematic understanding of empirical phenomena by showing that they fit into a nomic nexus." Science teaches us that when, say, this thing over here moves in one way (say, a tree falls), that thing over there will move in another (say, the car parked most unfortunately under the tree is crushed). We can learn more about what to expect from falling trees by observing them in yet more conditions. In this way, the positivists argue, science builds up a "nomic nexus" (an interconnected set of claims showing things of the world moving this way and that, and doing so by showing *why* we should expect these movements to be interconnected as they are.) To explain something, for the positivist, is just to fit it into the nomic nexus: when we come upon the car to find it flattened after a big storm, we say, well, yes, this is just the sort of outcome we should have expected—a flattened car—given that the tree fell on it.

A theory is a description of causal structure within a bounded domain of activity. It posits "basic entities and processes" governed by "basic theoretical laws, or theoretical principles." A theory consists of "substantive paradigmatic claims ... about what types of things exist and the manner of their existence." It is a representation, partly idealized, of a bounded domain of entities and of interconnections among them. "A theory is a picture, mentally formed, of a bounded realm or domain of activity. A theory is a depiction of the organization of a domain and of the connections among its parts." Theories convey causal information.

Theories have truth value, but because they are complex structures or networks of statements, they cannot be judged simply "true" or "false." Some of the statements that a theory comprises are true, and others, being idealizations or distortions of one type or another, are not. Different theories thus have different degrees of truth, or *verisimilitude*. In Popper's formulation, "a theory T1 has less verisimilitude than a theory T2 if and only if (a) their truth contents (or their measures) are comparable, and either (b) the truth content, but not the falsity content, of T1 is smaller than that of T2, or else (c) the truth content of T1 is not greater than that of T2, but its falsity content is greater." Lakatos, following Popper, characterizes the verisimilitude of a theory as its "truth-content minus falsity-content." Theories explain laws by showing them to be the products of an underlying causal structure. In Hempel's characterization,

Theories are usually introduced when previous study of a class of phenomena has revealed a system of uniformities that can be expressed in the form of empirical laws. Theories then seek to explain those regularities and, generally, to afford a deeper and more accurate understanding of the phenomena in question. To this end, a theory construes those phenomena as manifestations of entities and processes that lie behind or beneath them, as it were.

Laws explain other laws in two schemas: genus-species and partwhole. "Some regularities are simply species of others. For example, the regularity captured by the law, 'Wood floats on water' is a species of the regularity captured by the law, 'A solid body with a specific gravity less than that of a given liquid will float in that liquid.' The relata of the genus regularity are more comprehensive that the relata of the species." In the part-whole example, a regularity is "a *manifestation* of several other regularities." For example, "the regularity captured by the statement, 'Car radiator crackings are regularly connected with certain (specified) conditions of temperature, radiator content, etc.' is explained by showing it to be a manifestation or convergence of several other different kinds of regularities (some concerning the behavior of water at the freezing point, others accounting for the brittleness of the metals that radiators are made of, and so on)."

## Dimensions of Explanatory Progress:

*Scientists do not just develop theory; they also explain history.*

## Explanatory progress through historical research:

Because Lakatos defines a scientific research program as "a series of theories," his methodology of scientific research programs equates scientific progress with *theoretical* progress, failing to acknowledge even the possibility that scientific knowledge can grow through historical research (Elman C. and Elman M, 2003).

What deserves emphasis here is that the apparatus of Lakatos's methodology of scientific research programs is incapable of grasping the advances in knowledge that this research program has generated. While Lakatos's concern is exclusively with theoretical progress, the Alvarez hypothesis is a purely historical one: it asserts that a meteorite hit the Earth 65 million years ago and that this impact caused a mass extinction. Its chief rival is the "volcanism hypothesis," which attributes the extinction to massive volcanic activity. The debate is not theoretical; it does not involve the pitting of one series of theories against another, which is the only kind of debate Lakatos's methodology of scientific research programs can comprehend. Knowledge of this episode has been gained by using existing theories and laws, and acquiring a more precise characterization of the initial conditions and the event itself. In other words, scientists have developed better, more accurate, more complete explanations of the various aspects of the complex K-T episode because they have acquired more and better historical knowledge. In terms of the explanatory model outlined above, we conclude that explanatory progress on this topic has resulted from research directed toward the concrete particulars of the event in question, rather than on the theories and laws that are drawn upon in pulling together the overall explanation.

For a program to be as purely historical as this one, of course, researchers must have a good deal of reliable theory at hand. Otherwise, the improved knowledge of initial conditions they generate would not lead to improved explanations. K-T extinction researchers rely upon well-confirmed theories in physics, biology, and geology to construct their explanatory accounts. By comparison, theories in the study of world politics are weak, and research programs are unlikely to be as one-sidedly historical as the one on the K-T mass extinction. Instead, they will typically include a significant theoretical component. But that does not necessarily make these programs simply a *series of theories*, which is the only kind of research effort that concerns Lakatos. Many programs—for example, those concerning the "long peace," the end of the Cold War, the democratic peace, and ethnic conflict—will measure their progress not just in terms of theory-building, but on the historical side of the ledger as well, generating more accurate and appropriate descriptions of the relevant historical conditions. To the extent that these improved descriptions contribute to more powerful or more detailed explanations, they constitute scientific progress.

## Explanatory progress through theoretical research:

This example of theoretical progressivity offers insight into the role and implications of "as-if assumptions" in theory-building. Scientific theories are built on assumptions that are partly true and partly false (Elman C. and Elman M, 2003). The parts that are true supply the theory with its explanatory leverage; the components that are false (the "as-if" assumptions) determine the conditions under which the leverage can be used. For example, in the ideal gas model, the gas is said to behave *as if* the molecules occupy no volume and have no interactions. These are idealizations. They are useful because they lay bare the essential workings of a gas, showing us how to conceptualize a gas's temperature in terms of the energy of the molecules, its pressure in terms of the force with which the molecules hit the walls of the container holding it, and so forth. The idealizations also restrict the model's range of applicability: while the ideal gas law explains well the behavior of a gas where few molecules occupy a large box, it accounts poorly for the behavior of a highly compressed gas, where the molecules are crammed together. Hence the idealizations are effective and constructive. This comes as no surprise. If a small amount of gas occupies a large volume, its molecules will on average be widely separated, and they will not much affect one another's motion through attractive and repulsive forces. Under such conditions, Boyle's Law offers a good explanatory account. On the other hand, if a gas is highly compressed, the interactions between the molecules become significant. In this case we need a more detailed equation, like the van der Waals, to explain behavior. The theory's explanatory power increases as its false assumptions are "relaxed"— that is, as the assumptions' distorting, idealizing, or simplifying effects are removed. At each step in the process, it is the assumptions that are true that carry the explanatory burden.[7] To the extent the theory remains false, its range and power are restricted.

Lakatos defines the "positive heuristic" of a theoretical research program as the "instructions" that scientists follow in making relatively simple and unrealistic models more complex, realistic, and powerful. Lakatos defines model as a set of initial conditions (possibly together with some of the observational theories) which one knows is *bound* to be replaced during the further development of the program. The positive heuristic sets out a program which lists a chain of ever more complicated *models* simulating reality. The example he gives, paralleling the one just described concerning the kinetic theory of gases, is drawn from Newton's work.

## Measuring progress between research programs:

Exploring why scientists have switched their allegiance from one research program to a rival program has been a major bone of contention among philosophers and historians of science (Elman C. and Elman M, 2003). Lakatos believed that shifting allegiances from one research program to another was a rational process in which scientists chose the progressive program over its degenerating rival. Thomas Kuhn, on the other hand, regarded this process of shifting loyalties and commitments to be largely grounded in psychological and sociological factors, making these shifts primarily nonrational. Kuhn referred to these shifts by scientists as revolutionary science, leading to completely different terms, conceptual definitions, and problems to be investigated.

Unlike the criteria for choosing between competing theories, philosophers and historians of science have largely agreed upon the criteria for measuring progress within a single research paradigm. Lakatos and Popper, for example, both emphasize the ability of successive theories to predict novel phenomena. Both

7 An instrumentalist, being uninterested in explanation, is uninterested in a theory's truth. After all, if predictive capacity is all that matters, the truth or falsity of a theory's assumptions are irrelevant. Milton Friedman, "The Methodology of Positive Economics," in Friedman, *Essays in Positive Economics* (Chicago: University of Chicago Press, 1953). But if we are interested in *explanation* as well as prediction, the truth of theories becomes an issue.

Popper and Kuhn emphasize increased generality, precision, and simplicity as indicative of intra- programmatic progress. Lakatos's concept of progressive shifts within a research program includes, among other things, increased generality.
Eliminating logical inconsistencies as well as resolving methodological problems such as inappropriate statistical tests are measures of intra- programmatic progress for both Kuhn and Laudan.

Scholars in international relations appear to be in agreement, as well, regarding measures of progress within a single research program. In the past, for example, Rudolph Rummel, J. David Singer, and Harold Guetzkow argued that their respective research programs were progressive because, among other reasons, they tested more precise, more general, or simpler propositions as research in their programs evolved. More recently, Kugler and Organski and Bruce Bueno de Mesquita have claimed that their research programs were more progressive than rival programs because their respective programs met Lakatosian standards for progress while rival programs did not. Elman and Elman are among the first scholars to clarify and define Lakatos's measures precisely.

Positivism involves applying scientific methods to evaluate specific theoretical predictions. Both positivist research programs (such as the power transition program) and nonpositivist programs involve making empirical claims about reality. But positivist research programs test their empirical claims employing scientific methodology, whereas nonpositivist programs do not.

## Scientific Progress and Its Critics:

First, Lakatos fails to specify criteria for delineating the various elements of a scientific research program (Elman C and Elman M, 2003). He tells us that a scientific research program comprises a “hard core,” a “positive heuristic,” a “negative heuristic,” and a “protective belt of auxiliary hypotheses.” And though Lakatos characterizes each of these elements in general terms, he offers no insight into the process by which an analyst can non-arbitrarily specify them for particular research programs. In his historical reconstructions of scientific growth and change, Lakatos simply stipulates the constitutive elements of various research programs. He makes no attempt to justify or explain his judgments. For example, he asserts that the hard core of the Newtonian program consisted of "the three laws of dynamics and [the] law of gravitation." Lakatos offers no discussion of Newton's views on space and time, which were central to the Newtonian framework and considered by Newtonians to be as "irrefutable" as any of Newton's laws of motion. Nor does he justify including the third law of motion, which nineteenth-century Newtonians were prepared to abandon in their efforts to assimilate work on electromagnetism to the underlying mechanistic paradigm. The reader is given no guidance on the question of determining the adequacy of any particular characterization of a research program's hard core or heuristics. Indeed, we are given no indication that Lakatos gave serious thought to this central problem.

Second, although Lakatos makes the prediction of "novel facts" the very condition of the progressivity of a research program, his discussion of novel facts is riddled with ambiguities and inconsistencies. A *novel fact* is defined differentiated as: a future event, a new interpretation of an old fact, a phenomenon not explained by rival theories, and a phenomenon not used as guidance in extending one's own theory. The practicing scientist who wishes to use Lakatos's methodology of scientific research programs is left in the dark not only with regard to the question of identifying novel facts, but more fundamentally with respect to the underlying logic of judging claims to progressivity made on behalf of any particular research program. Lakatos clearly understood that he had not worked out this aspect of his methodology of scientific research programs; one of the reasons that we have at least four definitions of novel fact is that he kept revising his account to cope with deficiencies of earlier versions. But he never resolved the question in a real way, leaving open the critical issue of just what logic of confirmation or corroboration Lakatos thought scientists should affirm in their efforts to predict novel facts.

Third, and finally, Lakatos is guilty of a severe selection bias in the aspects of science he takes under consideration in articulating and defending the methodology of scientific research programs. Scientific inquiry has two sides, theoretical and historical. Theoretical research aims to characterize fundamental entities and their relationships, enduring causal structures, and the like, in abstraction from particular circumstances. Historical analysis is focused on past and present states of affairs: actual events, located in space and time, featuring observable (namable) entities and structures. The bias in Lakatos's work is that his concerns are drawn exclusively from the theoretical side of science. Indeed, he defines a scientific research program as a *series of theories*. But many research programs are heavily historical; for example, the massive research program on the so-called K-T extinction event that occurred about 65 million years ago has generated nearly 3,000 publications to date.

Despite these difficulties, Lakatos's methodology of scientific research programs remains a useful departure point for discussions of progress in international relations, for two reasons. The first is widely appreciated: to make comparative judgments in any scientific debate, we need to look at research programs in dynamic profile, rather than in static snapshots. Lakatos's rejection of "instant rationality" is convincing. A second reason for appealing to Lakatos's work is rarely articulated: if we are to understand and judge scientific progress, a commitment to epistemological realism (or "metaphysical realism," in Popper's terminology) is necessary. Lakatos articulates a relatively clear and straightforward epistemological realism, not only in his essay framing methodology of scientific research programs, but in the wider corpus of his work. His views on this topic are valuable in sorting through debates over progress in science, particularly when discussion turns to the nature or structure of scientific theory.

*If researchers in a discipline or subfield are, over time, producing more explanations, or better explanations, or both, we can consider that area of research "progressive."*

The debate concerning the proper methods to use in studying international relations is an old one, going back to the early post-World War II years when behavioralists of the scientific school argued that this field and other social sciences could replicate the successes of the physical sciences (Chernoff, 2007). Many authors have disputed this view, using two general arguments. One states that the natural sciences show progress, but the social sciences do not. The second contends that neither the social nor the natural sciences exhibit true progress. With respect to the first sort of criticism, philosophers of social science like Charles Taylor (1985), Daniel

Little (1991, 1998) and James Bohman (1993) propose that the social sciences are inherently unlike the natural sciences; all agree that whereas the natural sciences are predictive, the social sciences are not. They hold that views of scientific progress such as those of Charles Sanders Peirce, Pierre Duhem, Karl Popper, Imre Lakatos, and others cannot be applied to the social sciences. Little (1991), for example, argues that social scientific laws are too complex; they contain too many variables. Furthermore, the social sciences exhibit phenomenal regularities, not underlying, governing regularities.

The second sort of criticism holds that even the natural sciences do not really show progress and so neither, a fortiori, do the social sciences. These arguments are targeted at traditional positivism. They are both descriptive and prescriptive, describing practice in the sciences and arguing that a more traditionally positivist and ambitious notion of certainty is philosophically unwarranted. In the late 1950s through the early 1960s, Norwood Russell Hanson (1958), Thomas Kuhn (1962), and Paul Feyerabend (1962) began to develop positions aimed at undermining the traditional account of the natural sciences (Latour, 1987, Knorr and Cetina, 1993). The implications for the social sciences were obvious, and Kuhn became one of the most oft-cited philosophers of science in the writings of international relations theorists and other social scientists. Scholars argued that the natural sciences are essentially subjective; the process of replacing one theory or paradigm by another, allegedly superior, one can be understood on the basis of sociological rather than rational grounds. Ultimately, scientific progress is an illusion because of the problem of the incommensurability of paradigms. In international relations, various reflectivist and postmodern critics of scientific progress concur with this element of the Kuhnian critique.

# Chapter 4:

# Democratic peace becomes "science":

Democracy's influence on war and other forms of violent international conflict is an exemplar of what political scientists consider to be a theoretically progressive research program (Van Belle, 2006). The science part of Political Science pales in comparison with Palaeontology, most notably in the role that theory plays in shaping the focus of individual empirical research projects and how those empirical studies are interconnected to form research agendas.

When considered in terms of the details of the empirical subject being studied, Paleontology shares several key characteristics with politics. Most notably, both Political Science and Paleontology have to deal with extremely poor qualities of the basic data offered by the subject of study. The data used in the study of politics are best characterized as problematic. They are incomplete, haphazard and biased toward the large and unusual cases. They are subject to wide variations in interpretation and they are difficult to gather. Experimentation in a controlled and replicable environment is limited to the margins of the discipline and almost all of the research efforts tend to be poorly funded. However, these are almost the exact same points that can be made about the data in Paleontology. The data in Political Science are similarly incomplete. Even data as fundamental and well documented as gross national product, so often used in the study of international politics as a measure of a nation's overall economic activity, are only unavailable for about the last 80 years, and to this day, these data can only be roughly estimated for several countries, such as Cuba, North Korea, and many of the former Soviet States. Other data, particularly comparative data used in international relations, are often far less complete and far less reliable than Gross National Product (GNP).

Much of the data available to political scientists are similarly haphazard in their preservation or availability for study. Unless someone happens to have an interest sufficient to justify the effort, most information on most political activity is simply not recorded and what is recorded is seldom the ideal measure for the politically relevant factor of interest. In politics it is the big events that have their details preserved in the historical records to be found by researchers. War may be one of the rarest events in international politics, almost nonexistent in comparison to trades, migrations or mundane diplomatic exchanges, but far more information is available about war than for just about any other type of international event. Further, the bias toward the large defines the data on wars. It is the big wars such as great power wars or system transition wars that are most thoroughly documented.

Gathering data for the study of politics may not always be as physically difficult, but it is time-consuming, and in some ways the bowels of libraries and archives or the offices of government decision makers can be just as unpleasant as July in the badlands of the Dakotas. Generally, the difficulty for political scientists usually arises from the effort that must be invested in insuring that the data gathered is reliable and replicable.

The parallels are not perfect, obviously, but the similarities noted in the nature of the data available create similar obstacles to scientific progress in the two disciplines and some parallel norms have arisen that appear to be responses to these shared challenges, such as data sharing.

## The Study of Democratic Peace in International Relations:

As Chernoff put it in the introduction to his article on theoretical progress in international relations, ''Democratic peace studies show that such a body of knowledge is possible'' (Chernoff 2004, Levy 1994 Elman and Elman 2002, Ray 2003) and others reviewing the democratic peace as a subject of study also draw similar conclusions indicating progress or advancement of the scientific enterprise. This is in no way a full critique or comprehensive overview of the hundreds of articles and books that comprise the democratic peace literature.

The democratic peace research program can be roughly depicted in terms of the claim that democracies do not fight wars against one another. Qualifications to that statement, such as limiting it to mature or stable democracies, can be added or debated, but the basic claim is that amongst the democracies of the world there exists a community free of war and there is substantial empirical evidence of just such a regularity (Levy 1994).

More than a decade ago Jack Levy (1994:452) noted that ''the idea that democracies almost never go to war with each other is now commonplace. The skeptics are in retreat and the proposition has acquired a nearly law-like status.''

Cumulation and consensus in international relations, along with many other fields in the social sciences, have been slow in coming. Three-quarters of a century ago, John Hobson (1926:7) felt the need ''to afford some explanation of the slowness of these sciences in producing any considerable body of larger truths, in the shape of generally accepted laws and principles.'' Democratic peace studies show that such a body of knowledge is possible. Indeed, many authors have touted the successes of research on the democratic peace.

Singer and many like-minded scholars in IR (and in other social sciences) asked why the history of debates in natural sciences like physics has been so different from the history of debates in IR (Chernoff, 2007). One answer regularly defended was that the social sciences failed to follow the example of the natural sciences by focusing only on what is objective .Many writers advocated a reorientation of IR methods to make them more objective and thus more like the natural sciences (Yurdusev, 2003). This would include a focus on using observable behavior as evidence rather than intentions or values. They were often called behavioralists. Their project also sought to make the observations much more precise, which would make it possible for different investigators to conduct the same tests on hypotheses.

An example helps to illustrate how these scholars make their case. One of the key theoretical claims in IR deals with "democratic peace," which generally states that democratic government or lack thereof makes a difference in how war-like states are (Kant 1989; Babst 1964; Babst 1972; Doyle 1983a; Doyle 1983b; Russett 1993). In the 18th century, Kant offered an argument about what would be necessary for the world to become peaceful, which included the condition that all states in the system must adopt republican constitutions. This was based on Kant's ideas of freedom and justice, but not on observations of existing states, because few real republics existed in Kant's era or before. Dean Babst published two papers noting that, in the two centuries since Kant wrote, many democracies have been formed and many wars have been fought, but in all that time no two democracies have ever gone to war against one another (1964; 1972).

Several years later, Melvin Small and J. David Singer argued that rigorous scientific-style tests could settle the matter (1976). They set out to test the claim that democratic regime type makes a difference in war-related behavior. They could not set up experiments like physicists do, since it is impossible to put democratic and nondemocratic states into different sorts of pairings, create potential conflict situations, and observe them to see if there are any patterns in the wars fought (especially patterns indicating that democratic states go to war less often than nondemocratic states). Some natural sciences have similar limitations. For instance, astronomers might test a hypothesis by analyzing older data in a new way. They cannot set up alternative solar systems or galaxies to see how they will behave or conduct laboratory tests to create interesting conditions in the same way that physicists and chemists often can. And those who study botany or evolutionary biology might test a hypothesis or law by looking at data or specimens that have already been collected. What Small and Singer did was look at many cases from the past to see what happened in terms of war and peace.

Small and Singer (1976) looked at a great deal of data drawn from the previous five hundred years of war and peace. In order to do this, they used the COW database, which contains information on every country and includes variables such as what kind of government each country has, what kind of arms each has, whether or not each has engaged in war during any given year, and if so, against whom and for how long. They compared all the democracies in the world to all the nondemocracies and looked to see if democracies fought wars less often. They used general laws to derive a specific observable conclusion through deduction. By using deductive-nomological reasoning they deduced consequences (what one would expect to find) from the view that democracies are peaceful.

Small and Singer then analyzed the data and measured frequencies of war, which allowed them to see if the observed pattern matched the expected results. They concluded that there was no significant difference between the frequency of wars fought by democracies and by nondemocracies. They thus defended a realist position that wars are fought based on power considerations and not what type of governing regime is in place in a given state. They certainly maintained that their results were *objective*, both in the sense that any other scholars using the same data could apply the same mathematical tests and come up with the same conclusions and in the sense that the pattern would be the same whether or not anyone was studying it.

One of the classic examples of this situation is the 1967 Arab-Israeli War. Egypt and Syria were just about to launch a massive attack on Israel when Israeli leaders, seeing the troops massed, decided to strike the first blow in order to minimize the damage of the impending Arab assault. Although the Arab armies still inflicted heavy losses on Israel in the first days of the war, Israel soon reversed the course of the war and captured large portions of Arab territory—the Sinai Peninsula and the Gaza Strip from Egypt, the Golan Heights from Syria, and the West Bank from Jordan.

## Debate over Democratic Peace Hypotheses:

One of the major controversies in international relations during the past half-century has been over claims that the social sciences are capable of producing theories whose structures and functions parallel, at least roughly, theories in the natural sciences. Such claims emanate from behavioralists, positivists, and proponents of the scientific approach (Hempel 1965; Singer 1969; Rummel 1979, 1981; Rosenau 1980; Russett 1993; Bueno de Mesquita 2002) and have been contested by interpretivists, critical theorists, and postmodernists as well as others (Carr 1964; Habermas 1971; Taylor 1985; Ashley 1986; Wyn-Jones 1999). Key traits that scientific disciplines manifest are the accumulation of knowledge as successive waves of inquiry proceed and the development of a consensus on a question that is carefully examined according to scientific methods of inquiry. Critics have argued that the natural sciences are capable of these traits but not such social sciences as political science and the study of international relations (Bohman 1993; Little 1998). Other critics, especially Kuhn and his followers, have contended that even the natural sciences do not exhibit cumulation and an approach to consensus (Feyerabend 1962; Kuhn 1962; Latour 1987; Knorr-Cetina 1993). They hold that competing paradigms are not commensurable. Defenders of a scientific approach to international relations have had difficulty citing examples of areas of study that exhibit these characteristics. Thus, the conceptions of the natural sciences of Popper, Lakatos, Duhem, and Peirce and those of the critics of natural scientific progress, like Kuhn and Feyerabend, are examined below. It is argued here that recent studies of the democratic peace satisfy all these definitions of scientific progress. To start with, however, what is needed is a careful step-by-step examination of the interactions among the critics and defenders of the democratic peace phenomenon.

The central democratic peace hypothesis was first stated at the end of the 18th century by Immanuel Kant (1989). Kant's argument was theoretical rather than empirical; the historical record was sparse given that few states actually fit his definition of a republic or democracy at the time. The idea was not widely discussed until Dean Babst (1964, 1972) published the first of a pair of articles arguing that democracies are more peaceful than nondemocracies, although Babst did not cite Kant. Two prominent quantitative international relations theorists, Melvin Small and J. David Singer (1976), published a critique of this democratic peace hypothesis, while R. J. Rummel (1979, 1981, 1983, 1995, 1997) was the first to publish empirical tests supporting and calling attention to the hypothesis.

## Theoretical critiques over the democratic peace:

Even critics of the democratic peace use similar definitions and methods, or at least they start with these and build their critiques by arguing for the need for adjustments in such techniques (Chernoff, 2004). The realist critique by Farber and Gowa (1997) is an excellent example. These two scholars argue that ''interests'' account for the pattern of dyadic war and peace better than ''regime type.'' However, their argument does not proceed as the Kuhnian incommensurability account would expect, that is, by offering a distinct set of terms or a set of terms that take on different meanings because they are embedded in different theories. Rather, the terms ''democracy,'' ''autocracy,'' ''militarized interstate dispute'' (MID), and ''war'' have the same meanings as they do when used by supporters of the democratic peace phenomenon; both sides in the debate start with the same datasets, generally the Correlates of War (Singer and Small 1972, 1982) and Polity II and III ( Jaggers and Gurr 1995). Even in arguing for their preferred realist variable , Farber and Gowa make use of the same data set for assessing the presence of war as the proponents of the democratic peace do for many of their tests, in particular, the Correlates of War data. When deviating from the definitions used in the consensually agreed-upon datasets, authors accept the burden of defending their deviations. Indeed, when Christopher Layne (1994) criticized the definition of democracy being used in democratic peace studies without offering an explicit definition of his own, the failure did not go unnoticed (see Russett 1995:167).

Even though they use the same 1820–1980 dataset that Russett and other democratic peace proponents use, Farber and Gowa disaggregate the data by dividing them into three time periods: pre-World War I, interwar years, and post-World War II. These researchers show that the democratic peace hypotheses do well in the post-1945 period but poorly earlier, especially in the pre-1914 period. They point out that so many new states emerged in the system after World War II that the results derived by others, based on the aggregated data, are unduly biased by the particular systemic conditions after 1945. For example, in the dyadic analysis of the 165 years from 1816 to 1980, there are 284,602 dyads for which complete data exist. However, even though the thirty-five years following World War II constitute just over one-fifth (21.2 percent) of the number of total years examined, they included nearly two-thirds (65.7 percent) of the dyads. Thus, when the results are aggregated, any factors stemming from the post-1945 system disproportionately affect the statistical tests. Proponents and opponents of the democratic peace are not talking past one another; they are speaking the same language, building upon one another's methods, and developing new results to which the other side is expected to respond.

The latter use alliances as their indicator of ''common interests'' when testing their argument that such interests explain conflict patterns much better than does ''democracy.'' Russett and Oneal point out that alliances are not independent of democracy. Ideology is often a reason for states to choose one another as alliance partners, even though geostrategic reasons can also have a bearing on conflict behavior.

Erik Gartzke (1998) has also argued that common interests more than democratic character are responsible for peace among democracies. In his view, democracies have an affinity for one another that leads them to have overlapping rather than conflicting interests. Hence, the supposed effects of democracy in constraining the escalation of conflicting interests are greatly overstated given that conflicting interests do not exist and, thus, do not need to be constrained. Gartzke contends that all the proposed causal mechanisms used to explain the low incidence of conflict among democracies mistakenly assume that there have been potential conflicts between democracies. Consequently, because there has been little conflict to prevent, there has been no need to escalate. Gartzke (1998:6) uses the following analogy: ''Observations of the democratic peace are not unlike studying incidents of seasickness in Central Asia. There is nothing to report. Still, we cannot then assume that Uzbek culture makes them hearty seafaring folk or that Tadzhik bureaucrates introduce a mysterious regime that makes local villagers immune to the effects of vertigo.'' Democracies' rarely conflicting preferences are what lead to lack of war, not anything about their democratic nature.

Gartkze uses UN General Assembly voting as a measure of ''affinity'' or ''similar preferences,'' which some regard as superior to Farber and Gowa's ''alliance'' measure given that there are more cases of states working together without formal alliances. It seems obvious that if democracies have greater affinities for one another and states with greater affinities and similar preferences do not fight, then democracy is still playing a crucial role. Gartzke (1998:11) proposes that, ''regime type similarity actually leads to similar preferences.'' Indeed, the factors that cause democracy, like ''ecological, material, or cultural factors,'' are actually the causes of the shared preferences among democracies. In dealing with this issue of causation, Russett and Oneal argue that democracy has causal effects on UN voting and alliances. But the important observation here is that Russett and Oneal (2001) take up the challenge of ''preference similarity'' and, using Gartzke's idea of UN General Assembly voting as the indicator, they respond to the criticism by testing the democratic peace hypotheses using ''common interests'' as one of the control variables.

## Methodological Critiques:

Spiro (1994) has criticized Russett (1993) and Maoz and Russett (1993) on methodological grounds, especially their analyses of time series data. Russett (1995) responded directly to Spiro. The papers by Oneal and Russett (1999a and 1999b) revised and extended this analysis (for example, by bringing in ''trade'' as an explanatory variable). These time series techniques have also been criticized by Nathaniel Beck, Jonathan Katz, and Richard Tucker (1998) and later by Donald Green, Soo Yeon Kim, and David Yoon (2001) on different grounds. Russett and Oneal responded to these critics in subsequent publications (Oneal and Russett 1999a, 2001; Russett and Oneal 2001) and made adjustments in their analyses suggesting once more the progressive nature of the debate. Let us examine each of these critiques in more detail.

Spiro (1994) argued that Russett's (and Maoz and Russett's) use of the pooled time series method was illegitimate because cases were not entirely independent; peace between the United States and Canada in 1994 is not causally independent of the peace between them in 1993. Russett (1995) replied to Spiro's attack by offering an alternative method, by not using dyad-years as the unit of analysis but instead stable dyadic relationships or ''dyad-regimes'' during which a relation-ship remained stable. The entire two-century period of peace between the United States and Canada became a single case. This revamping of the unit of analysis avoids any charge of inflating the set of peaceful dyad-years with large numbers of observations that are not independent. Russett then presents an alternative analysis of ''war'' for the post-World War II period as well as analyses in which the dependent variable is ''use of force'' and ''disputes of any sort.'' In the case of war, there were 169 dyad-regimes in which no war occurred, and there were zero wars between democracies. In the 1,045 dyad-regimes that did not contain a democracy, there were thirty-seven wars (3.4 percent) among them. Russett (1995:175) regards the objections raised by Spiro as constructive, indicating that the critique ''forced me to devise new tests. The result is that the evidence for the democratic peace is stronger and more robust than ever.''

The second type of criticism was leveled by Beck, Katz, and Tucker (1998). In their 1997 paper in the International Studies Quarterly, Oneal and Russett use logistic regression of a pooled time series of dyad-years by state to argue that ''democracy'' and ''interdependence'' are significant factors in reducing international conflict (rather than just ''democracy'' as in Russett 1993 and Maoz and Russett 1993). Beck, Katz, and Tucker charge that Oneal and Russett encounter problems because time series cross-sectional data, especially those using binary variables, undercut the assumption that the observations are independent. ''Since it is unlikely that units are statistically unrelated over time,'' binary time-series cross-section observations ''are likely to be temporally dependent'' (Beck, Katz, and Tucker 1998:1260–1261). They indicate that corrections need to be made for temporal dependencies and suggest a method for doing so.

Although Beck, Katz, and Tucker (1998:1275) say that ''we were able to replicate Oneal and Russett's (1997) original estimates exactly,'' they add that ''a different picture emerges, however, when we correct for temporally dependent observations using grouped duration methods.'' They find that the dramatic effects are on the ''trade'' variable. In effect, by showing how the data can be reanalyzed, Beck, Katz, and Tucker (1998: 4) offer a remedy to the problem, although, as they admit, the results may prove to be very different. Their alternative method upholds the effects of democracy but not the effects of liberal trade policies.

An interesting example of progress here is Beck and his colleagues' exact replication of the democratic peace results using the same methods, and their acknowledgement that even when the corrections are made, the powerful effects of democracy remain intact. Furthermore, in subsequent papers, Oneal and Russett (1999a:22, 2001:470) acknowledge the criticisms and revise their analyses to meet them. Referring later to their 1997 piece, Oneal and Russett (2001:470) say that ''we did not consider whether there was heteroskedacticity in the error terms, account for the grouping of our data by dyads, or address the lack of independence in the time series'' and add that Beck, Katz, and Tucker ''showed us the error of our ways'' (Oneal and Russett 2001:470).

## Neorealism :

Recent assessments identify theoretical developments in a variety of research areas such as the democratic peace which was aforementioned, and rate those that have proved most and least useful to the study of international relations (Elman and Elman, 2002). They also question why some theoretical orientations—notably neorealism, dependency, and world systems theory—have become less popular, while others—such as rational choice, historical institutionalism, and constructivism—have received increased support. However, in identifying "better" theories, and describing the successes and failures of IR research programs, these field surveys rarely address whether there is a pattern to the fate of specific research agendas, or explain why particular theories of international relations have waxed or waned. More importantly, almost none of the recent appraisals adequately engage the question of what measures should be used to determine whether various theoretical moves are progressive.

Anti-positivists and postmodern writers emphasize the question of what science is and how the study of IR may be viewed as such (Chernoff, 2005). One does not have to look far to see the anti-positivists' use of meta-theory. In his oft-cited essay, 'The Poverty of Neorealism', Rick Ashley (1986: 280) is explicitly critical of certain 'metatheoretical commitments' of neorealism. Ashley points out that neorealists' critique of classical realism includes a conception of what counts as a 'science' and that neorealists argue that classical realism does not satisfy that definition (1986: 260). Classical realists do not meet 'modern *scientific* standards' of theory, specifically because they are 'too fuzzy, too slippery, too resistant to consistent operational formulation'. Referring to (Waltz, 1979: 62–4), Ashley notes that neorealists do not distinguish between subjective and objective aspects of inter-national political life. Gilpin also says that classical realism 'is not well grounded in social theory'. But on the other hand, on the question of naturalism (or what he calls 'positivist' social science), Ashley endorses Giddens's claim that 'there are no particular barriers to the treatment of social conduct as an "object" on a par with objects in the natural world' (Giddens 1974: 4, cited by Ashley 1986: 281).

Kenneth Waltz's *Theory of International Politics* garners the most numerous references as the founding work of the neorealist scientific research program (Walker, 2010). After frequent references to Lakatos and Kuhn, Waltz proclaims that neorealism is the only theory within IR that would be "recognized as theory by philosophers of science." Many followed this line of reasoning and now treat realism as the dominant and exclusive paradigm in IR.

Waltz's response highlights several problems with employing Lakatos in International Relations. First, Waltz assumes (rather than demonstrates) that neorealism has legitimately earned its status as a scientific research program by exhibiting some novel facts. If this were indeed the case, a devotee to Lakatos might justifiably shove aside anomalies, as Waltz attempted. But realism has nothing approaching Lakatos' novel facts, so shoving aside anomalous evidence is unacceptable by Lakatos' standards. Second, by imposing Lakatos' criteria of subsumption, Waltz endorses the idea that science progresses most efficiently when guided by one overarching theoretical framework. Any criticism of a theory becomes pertinent only when it is packaged with a new research program that can subsume its standing rival. This tall order minimizes critical thinking. Finally, Waltz conveniently skirts the issue of "fruitfulness" or progress of his neorealist research program. As noted above, Waltz confides that with neorealism "there's been very little progress." So Waltz's effort to dismiss Schroeder's evidence on these grounds is puzzling and inconsistent with Lakatos's criteria of progress. By ignoring strong anomalous evidence, Waltz's critique of Schroeder exposes the hazards of relying on paradigm mentalities to guide the study of IR.

In a second critique, according to Colin and Miriam Elman I criticize Schroeder for failing to demonstrate a full appreciation of the neorealist research program (Elman and Elman, 2001). In a classic example of stretching the domain of realism to incorporate nearly all state behavior, the Elmans charge that Schroeder underestimates the extent to which his rendition of the historical .

A wide variety of contemporary IR theorists endorse some version of Lakatos's criterion of the 'progressiveness' of scientific research programmes or his notion of 'sophisticated falsificationism' (Chernoff, 2005). A few references to the most influential figures in neorealism, liberal institutionalism and reflectivism should provide a sense of the wide acceptance of Lakatos's methodological ideas.

Most of the prominent current IR theorists endorse a Lakatosian approach to theory-testing and appraisal, though not all of them explicitly discuss each of the major components of Lakatos's philosophy of science. Their adoption of Lakatos's views is sometimes suggested by their use of distinctly Lakatosian terminology, viewing theories in terms of 'research program(me)s', with 'hard core' and 'protective belts' of propositions, even if they do not specifically cite Lakatos.

Among the neorealists, Waltz (1997) directly engages Lakatosian criteria to evaluate neorealism. Waltz says, 'Because of the interdependence of theory and fact, the construction and testing of theories is a more problematic task than most political scientists have thought. Understanding this, Lakatos rejected "dogmatic falsification" in favor of judging theories by the fruitfulness of the research programs they may spawn' (Waltz 1997: 914). Waltz argues that the attacks on neorealism based on the criteria of 'progressiveness' and 'fruitfulness' are erroneous, not because Lakatos is mistaken in advocating such criteria, but rather because the criticisms (by Vasquez 1997, 1998, 2002 and others) mischaracterise both neorealism and the Lakatosian criterion.

Postmodern and constructivist writers make use of the Lakatosian criteria, at least when evaluating naturalist theories. Ashley addresses this aspect of the criterion directly, noting that some neorealists see their theory as part of a 'progressive scientific' discipline (Ashley 1986: 260). Ashley uses the Lakatosian understanding of 'scientific research program' to relate neorealism's commitment to 'statism' as its ‘hard core’ and ‘protective belt of auxiliary hypotheses’, which are derived from an eclectic array of sources (1986: 275–6). The constructivist Wendt says the only criterion of theory choice he endorses is the Popperian and Lakatosian criterion of falsifiability (1999: 89).

*All scientific theories must meet the minimum criterion of being in principle falsifiable on the basis of publicly available evidence, and social scientists should approach their knowledge claims with that in mind. Beyond this, however, we should be tolerant of the different standards of inference needed to do research in different areas.*

And he adopts a Lakatosian conception of the character of theories when he refers to the ‘hard core of research programmes’ (1999: 28).Finally, it is noteworthy that even an avowed opponent of philosophical musing for its own sake, like Valerie Hudson – a self-described ‘IR mechanic’ (Hudson 2001) – is compelled to endorse Lakatos’s methodology. She says, refer-ring to the formulation and testing of foreign-policy analyses, ‘If metatheory cannot be translated into something that would have practical application to this type of endeavor, I admit to having little use for it’ (2001: 1) but later adds, ‘I am an unrepentant Lakatosian’ (2001: 4–5)

# Conclusions:

The quest for knowledge is innate to the human nature. With the constant cumulation of knowledge and evolution of available means, scientific theories were created. What could distinguish scientific knowledge from other forms of knowledge is that it makes a serious effort to offer explanations at a deeper level of understanding. This could suggest that the scientist should try to examine the phenomena beneath the surface. Through the centuries, it has been relatively easier to build and replace scientific theories in the natural sciences, mainly because the empirical data and postulated regularities facilitated towards this direction. Slowly but in a steady pace, the social sciences began to develop and flourish. During the last century they have tried to obtain a more scientific character and this character is manifested via the possibility of scientific progress in an academic discipline.

In the case of the International Relations Theory, the study of IR began as a theoretical discipline. Philosophy of science debates usually result to the philosophy of science principles one can adopt to defend an IR and subsequent social science research. Although the literature concerning the progress in IR is noteworthy, there still has not been adopted a unique way to approach this issue. In this dissertation the three main approaches in philosophy of science have been presented: those of Kuhn, Popper and Lakatos. Despite the contribution of all three as well as Laudan's, Popper for example made it extremely difficult to be supported on account of his constant falsifiability tests in order to achieve progress. Nevertheless a slight tendency towards the Lakatosian account is shown, as it was found more fitting to circumscribe the diversity of IR. Using the Lakatosian account provides the researcher both structure and focus in order to develop knowledge in a rational and accumulative manner.

A research program in IR that is characterized as progressive is that of democratic peace. The democratic peace research program can be approximately described through the hypothesis that democracies do not fight wars against one another. A researcher can either limit the hypothesis to mature or stable democracies or add and debate. Still, the basic hypothesis is that amongst the democracies of the world there exists a community free of war and there is substantial empirical evidence of just such regularity. Democratic peace studies constitute scientific inquiry and exhibit progress. Most usually, research on the democratic peace can evidence accumulation in a way not so different than in the natural sciences. Satisfying a very basic Lakatosian criterion of progress, democratic peace studies are known to have created a debate, initiated by Babst. This debate in its turn has broadened the content on which it focuses, adding novel predictions to this progressive academic area.

To conclude, I support the idea that scientific progress is possible in International Relations Theory. Positivists may have been to extremes with their tests, along with Popper's falsifiability. They underestimated the empirical challenges that are presented in social sciences. Nowadays, the academic area of International Relations has matured enough -it's not all about world politics-and has been subjected to the necessary processes imposed by the philosophers of science. Besides the democratic peace and neorealism, the Theory of International Relations has been able to quantify his data and is ready to obtain a more scientific status quo.

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