Politeja
No. 6(75), 2021, pp. 29-50
https://doi.org/10.12797/Politeja.18.2021.75.02

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THE MEANINGS OF SCIENTIFIC PROGRESS IN THE HISTORY OF INTERNATIONAL RELATIONS – SELECTED CASES

ABSTRACT This article aims at reconstructing and interpreting the meanings of scientific progress present in selected important works within the discipline of International Relations (IR). This research objective stems from the gap in the literature concerning scientific progress in IR, as it is mostly concerned with the evaluation of the progressiveness of particular approaches, paradigms within the discipline. The reconstruction of meanings given by particular IR scholars to scientific progress is conducted only as far as its instrumental for the critique of their approaches and making room for the approaches of the critics. My objective is different – using a method inspired by the history of ideas and the research technique of qualitative content analysis, I will attempt to answer the following research questions: Q1 – How is the category of scientific progress of IR understood by particular scholars? Q2 – What are the contexts of its usage? Q3 – How can we interpret the rationale behind the employment of particular meanings in particular contexts? Q4 – How, on the basis of all cases, can we depict the flow of ideas of scientific progress through the history of IR? The cases selected span the development of IR from World War II to the early 2000s: *Edward Carr's The Twenty Years' Crisis*; Morton Kaplan's texts from the early phase of the second great debate; John Vasquez's *The Power of Power Politics*; and Miriam and Colin Elman's *Progress in International Relations Theory*. On the basis of these cases I will argue that the notion of scientific progress in IR is an essentially contested concept within the discipline. Despite certain similarities in the meaning of the term among the cases – a cumulative notion of scientific progress – all of them are used in a way that is intended to legitimize the approach of a particular author as 'properly scientific'. Another conclusion drawn is that

although differing in kind, all of the cases consider important historical events that do not shape the meanings of progress themselves, but instead create a window of opportunity for particular meanings, as their context.

Keywords: International Relations, philosophy of social sciences, historiography – IR, progress in IR, theory of IR

One may argue that *progress* of the discipline of International Relations (IR) is one of the essentially contested concepts within the field.[1] At least since Waltz's *Theory of International Politics* and his appeal for a proper social science of IR to be founded on the basis of the philosophy of science,[2] the problem of progress has blended with the reading, use, and abuse of the philosophy of science by IR scholars. In the discipline, we have scholars arguing for the progress of IR along Kuhnian lines of normal science within the paradigm,[3] through the lenses of Lakatos' scientific research programs,[4] via conventionalist epistemic communities styled after Duheme,[5] through scientific realism,[6] in protest of the notion that the progress is possible along the normative lines drawn from the philosophy of science,[7] and finally there are those that capitulate before the divergent meanings of progress in IR.[8]

1 For the meaning of the term „essentially contested concept" see: W.B. Gallie, "Essentially Contested Concepts", in idem, *Philosophy and the Historical Understanding*, London 1964, pp. 157-191.

2 K. Waltz, *Theory of International Politics*, Reading 1979.

3 See for example: M. Banks, "The Evolution of International Relations Theory", in M. Banks (ed.), *Conflict in World Society: A New Perspective on International Relations*, Hemel Hempstead 1984; J.A. Vasquez, "The Realist Paradigm and Degenerative versus Progressive Research Programs: An Appraisal of Neotraditional Research on Waltz's Balancing Proposition", *American Political Science Review*, vol. 91, no. 4 (1997), pp. 899-912.

4 See for example: J.W. Legro, A. Moravcsik, "Is Anybody Still a Realist?", *International Security*, vol. 24, no. 2 (1999), pp. 5-55; C. Elman, M.F. Elman (eds.), *Progress in International Relations Theory Appraising the Field*, London 2003; E. Harrison, "The Democratic Peace Research Program and System Level Analysis", *Journal of Peace Research*, vol. 47, no. 2 (2010), p. 155.

5 See for example: F. Chernoff, *Theory and Metatheory in International Relations: Concepts and Contending*, New York 2007; eadem, "Bounded Pluralism and Explanatory Progress in International Relations: What We Can Learn from the Democratic Peace Debate", in A. Freyberg-Inan, E. Harrison, P. James (eds.), *Evaluating Progress in International Relations: How do You Know?*, New York 2007.

6 A. Bennett, "The Mother of All isms: Causal Mechanisms and Structured Pluralism in International Relations Theory", *European Journal of International Relations*; vol. 19, no. 3 (2013), pp. 459-481; C. Wight, "Bringing the Outside in: The Limits of Theoretical Fragmentation and Pluralism in IR Theory", *Politics*, vol. 39, no. 1 (2019), pp. 64-81.

7 See for example: O. Wæver, "The Rise and Fall of the Inter-Paradigm Debate", in S. Smith, K. Booth, M. Zalewski (eds.), *International Theory: Positivism and Beyond*, Cambridge 1996, pp. 149-185; T.C. Walker, "The Perils of Paradigm Mentalities: Revisiting Kuhn, Lakatos, and Popper", *Perspectives on Politics*, vol. 8, no. 2 (2010), pp. 433-451; D.A. Lake, "Why 'isms' Are Evil: Theory, Epistemology, and Academic Sects as Impediments to Understanding and Progress", *International Studies Quarterly*, vol. 55, no. 2 (2011), pp. 465-480.

8 A. Freyberg-Inan, E. Harrison, P. James (eds.), *Evaluating Progress*...

As the reader has probably inferred from the 'see for example' qualification in the footnotes, this review of literature is partial, fragmented, and somewhat subjective. Still, I hope this will be excused on the grounds that the form should not eclipse the content. Nevertheless, what can be revealed as a gap within this, albeit sketchily presented, content, is a virtual lack of works dedicated solely to mapping and interpreting the meaning of progress as employed by particular scholars. Of course, it is partially done in almost all the texts referred to (and probably in majority of the works omitted from the footnotes), but the purpose of such reconstruction is primarily to show the weaknesses of 'others' against whom a particular author is casting his or her own preferred vision of the scientific progress of IR. My goal for this text is different: I will attempt to provide a wide panorama of meanings attributed to the category of scientific progress by particular IR scholars, show why they applied a particular meaning of the term, and finally depict the general flow of the idea throughout the historical development of IR. In the end, what I am omitting from the text is my own vision of how the progress of the discipline should look. There are two reasons for this: First, not to emulate the way the matter has been handled so far within the discipline, and second because the text will be long enough as it is, and my own convictions on the matter have been, and will be, fleshed out in my other texts. In the end, to paraphrase *All silent on the Western Front*: this article is to be neither an accusation nor a confession. It will try simply to present generations of IR scholars and their struggles at important moments of the discipline's history, even though they may have not been able to avoid their own polemical fervor.

The general method I am employing in the article is greatly inspired by the history of ideas in two widespread general meanings of the term. I view both the precise reconstruction of the meaning of an idea as used by a particular scholar, and the understanding of an idea on the basis of the context within which it was employed as complementary interpretative moves. I use the research technique of qualitative content analysis, which requires that I precisely state the research questions that will guide my reading of particular texts and my interpretation of all of them as a group. Hence the following questions: Q1 – How is the category of scientific progress of IR understood by particular scholars?; Q2 – What are the contexts of its usage?; Q3 – How can we interpret the rationale behind the employment of particular meanings in particular contexts?; Q4 – How, on the basis of all cases, we can depict the flow of ideas of scientific progress through the history of IR?

The last thing to do before I turn my attention to the cases is to introduce them and give reasons for their selection. The *Twenty Year's Crisis*[9] by Edward Carr, although he himself called it a 'period piece', is one of the best-recognized texts in the discipline. Its notoriety is based on the notion of first great debate in IR, and the crowning of realism as a core of the discipline after World War II. As for Kaplan, I could limit myself to *System and Process*, yet his later texts from the turn of the 1950s and 1960s

[9] See: M.A. Kaplan, *System and Process in International Relations*, New York 1957; idem, "Is International Relations a Discipline?", *The Journal of Politics*, no. 23/3 (1960), pp. 462-476; idem, "Problems of Theory Building and Confirmation in International Relations", *World Politics*, no. 14/1 (1961), pp. 6-24.

develop his ideas about the science of IR and its progress. On the other hand, his article from 1966 is not analyzed because of its polemical character as a direct response to H. Bull's famous *International Theory: The Case for a Classical Approach*. The texts chosen represent the idea of scientific progress employed by the chief proponent of the scientific approach, which after the turn of the 1950s shaped mainstream IR. In the case of Vasquez, the author himself states in the introduction to *The Power of Power Politics* (one subject of my analysis) that the parts of the book concerned with theory and philosophy of science were unchanged from its previous edition (1983). Thus, in a way, the idea of progress present in Vasquez's book has similar roots to Waltz's dictum about the philosophy of science, and as such gives us insight into the early stages of the reception of said philosophy into IR. In case of the tome edited by the Elmans, the issue was whether to analyze it as a whole, to include the introduction by the editors and their text that is their direct contribution along with the other essays, or the Elmans' part as distinct from the rest of the contributions. I lean towards the second option: I present Elmans' argument on Lakatos and his research programs, and discuss the other texts when needed to assess the meaning of the whole tome in its historical context.

THE DIALECTIC OF PROGRESS BETWEEN UTOPIA AND REALISM

For Carr, the genealogy of science lies in its social purpose. Although he differentiates between science taken over by this purpose and science as an impartial search for objective facts and their explanation, social purpose is his preferred starting point when analyzing the social science of IR: 'Our first business, it will be said, is to collect, classify and analyse our facts and draw our inferences; and we shall then be ready to investigate the purpose to which our facts and deductions can be put. The processes of the human mind do not, however, appear to develop in this logical order. The human mind works, so to speak, backwards. Purpose, which should logically follow analysis, is required to give it both its initial impulse and its direction'.[10] He then quotes Engels, who attributes more agency as far as scientific progress is concerned to society's technical need than to '10 universities', a picturesque albeit imprecise unit of measurement. So is Carr an exponent of Marxist sociology of knowledge, where the progress of science is understood in terms of progressiveness of the class that conducts it? Actually, he is not. He uses his discussion of the purpose of science to introduce the distinction between natural and social sciences: *In physical sciences, the distinction between the investigation of facts and the purpose to which the facts are to be put is not only theoretically valid, but is constantly observed in practice*. (...) *In the political sciences, which are concerned with human behavior, there are no such facts. The investigator is inspired by the desire to cure some ill of body politic. Among the causes of the trouble, he diagnoses the fact that human beings normally react to certain conditions in a certain way. But this is not a fact comparable with the fact*

[10] E. Carr, *The Twenty Years' Crisis, 1919-1939*, London–Basingstoke 1981 [1939], p. 2.

that human bodies react in a certain way to certain drugs. It is a fact which may be changed by the desire to change it.[11]

Thus, Carr presents himself as antinaturalist, although in his writing the distinction between social and natural sciences lies in volume, rather than in kind. He needs this point of reference however, for the composition of the first part of the dichotomy, utopism, and to frame it as a historically necessary phase of (social) science, its infancy. However, the second phase of science, realism, which completes the dichotomy attributed to Carr, by no means constitutes the adult science, although it is in itself the sign of its progress. To make this clearer, let us turn to Carr's descriptions of both phases.

He describes the first stage, utopism, as follows: *During this stage, the investigators will pay little attention to existing 'facts' or to the analysis of cause and effect, but will devote themselves whole-heartedly to the elaboration of visionary projects for the attainment of the ends which they have in view – projects whose simplicity and perfection give them an easy and universal appeal*.[12] It is precisely this kind of utopism that Carr sees as a quality of the IR thought contemporary to him. His utopists include Norman Angell, Alfred Zimmermann, and Woodrow Wilson. Their preoccupation with international morality and normative and deductive modes of thinking, their insistence on the pacifying effects of international law and organizations, and their belief in the power of world public opinion, are all for Carr instances of utopian science more preoccupied with its social purpose, the abolition of war, than with cold-hearted, fact-driven analysis. This leads us to the second phase of the development of IR, realism.

Carr, alongside Hans Morgnethau, is considered the chief classical realist of IR. He describes that stance attributed to him: *Representing a reaction against the wish-dreams of the initial stage, realism is liable to assume a critical and somewhat cynical aspect. In the field of thought, it places its emphasis on the acceptance of facts and on the analysis of their causes and consequences. It tends to depreciate the role of purpose and to maintain, explicitly or implicitly, that the function of thinking is to study a sequence of events which it is powerless to influence or alter. In the field of action, realism tends to emphasise the irresistible strength of existing forces and the inevitable character of existing tendencies, and to insist that the highest wisdom lies in accepting, and adapting oneself to, these forces and tendencies*.[13] I would argue that the most common misinterpretation of Carr and his impact on the discipline lies here. He paints realism both as an approach to the acquisition of knowledge and as a 'practical philosophy' guiding political action. If we focus only on the first meaning of realism presented here, we will be left with a notion of science that is very plain. I would argue that this is the reason why Carr's thought is interpreted in simplistic dualist terms – he found IR infantile and utopist, so he countered it with the 'cold, scientific logic' of realism, and hence progress in the discipline has been made. Furthermore, his approach to science, dubbed 'realism', was

[11] Ibid., p. 3.

[12] Ibid., p. 5.

[13] Ibid., p. 10.

vague enough that the next generation of scholars, behaviouralists, could highjack it and propose, building on their predecessors, a way forward for the discipline in an orderly, cumulative fashion.

The above reading of Carr is flawed, however. It focuses on the dichotomy of utopism-realism, while ignoring the more wholesome thought of the author. First, as I pointed out above, it drops the second, practical meaning of realism present in *The Twenty Years' Crisis*. That is important, since if we consider Carr a realist in both meanings, we can arrive at an interpretation where his purpose as an author has more to do with the state of world politics contemporary to his writing than to the state of IR as a science. His involvement with the scientific side of things is predicated mostly on the important role it played in the practice of world politics at the time. He thus naturally criticizes it for its naivety, as a realist should, but actually dedicates the majority of his book to the critique of particular aspects of political practice of the Western world. He links this practice with IR scientific thought, but he sees this linkage more as two phenomena predicated upon the same causal root – the ideology of liberalism. One might argue that precisely this ideology, with its preoccupation with human rationality, best exemplified by the rationality of scientific progress, is what rendered the following passage from Carr's book less well-known in the discipline, or at least more overlooked: *But there is a stage where realism is the necessary corrective to the exuberance of utopianism, just as in other periods utopianism must be invoked to counteract the barrenness of realism. Immature thought is predominantly purposive and utopian. Thought which rejects purpose altogether is the thought of old age. Mature thought combines purpose with observation and analysis. Utopia and reality are thus the two facets of political science. Sound political thought and sound political life will be found only where both have their places*.[14]

This is the proper statement of the goal of scientific progress in social science according to Carr. It is not realism, but a synthesis of both utopism and realism. Without purpose, social science becomes stale and uninteresting: Without the 'cold hearted logic of scientific method' it becomes naïve. Hence both utopism and realism are transitory stages of the development of IR, and the mature practice of this science (and every other social science) is a synthesis of these two opposing approaches. This is the ideal of science, present in Carr's writings, that progress should lead to. Now why did the discipline 'overlook' this dialectical notion of scientific progress?

The first answer is placed within the general historical context of the publication of Carr's book. Although written just before World War II, I would argue it gained notoriety only after the war was over.[15] It was a time of need for general answers that would put the tremendous human tragedies of the conflict into perspective. Within this climate of a search for 'lessons learned', the fine details of Carr's analysis, his dialectic notion of scientific progress, was to an extent overlooked. 'Bad utopists' of the interwar period could have been blamed for the intellectually bereft theories of world

[14] Ibid.

[15] See Carr's introduction to the second edition, ibid., pp. 9-10.

politics that added to the causes of war.[16] The second reason has more to do with the context of the development of IR as a discipline itself. As mentioned above, realism as a phase of the development of science was vague enough that it could be taken over by the proponents of the scientific approach to IR in the late 1950s and early 1960s. This will become clearer after the casebelow – here it suffices to say that the vague notion of science and the role of theory therein, present in classical realism, not only that of Carr, was to blame.[17]

ROUND AND ROUND THE PROGRESS GOES

To reconstruct the meaning of scientific progress present in Kaplan's thought, let us begin with fragments: *If theorizing stops – rather than starts – only with simple models, there will, of course, be no progress and even no really operational knowledge*;[18] *One reason for this gap lies in the belief of the author that international politics, and social sciences generally, is so poorly developed that the construction of a precise deductive system would be more constrictive and misleading than enlightening, that, at this stage of development, some ambiguity is a good thing*.[19] Thus Kaplan is persuaded that the progress of IR is a thing that is happening, and from this, he draws certain conclusions about possible qualities of theory (models) that are attainable at this particular level of the development of IR. It is all the more interesting if one considers how throughout *System and Process* Kaplan goes to great lengths to present the theories (models) he explains in the book as conceptual frameworks, not as proper theories as in the natural sciences. Yet, however we label the content of his book, Kaplan is considering it as a part of the tendency, present in IR at the time, to reduce great amounts of data about world politics to a relatively small number of coherent propositions.[20]

Let us now turn our attention to the nexus between the theory development and discipline's progress present in Kaplan's thinking: *Some research problems may seem to demand more or less theory. And it would seem more fruitful to see what speed turns out best for particular kinds of tasks by applying our insights to these problems, then to engage in a methodological debate, which in practice may produce only verbal acrobatics.* (...) *We*

[16] This simplistic view of the first debate was questioned at the turn of XX century by the so-called revisionists in the historiography if IR. See for example: M. Kahler, "Inventing International Relations", in M. Doyle, G. Ikenberry (eds.), *New Thinking in International Relations*, Boulder (CO) 1997; B. Schmidt, "Lessons from the Past: Reassessing the Interwar Disciplinary History of International Relations", *International Studies Quarterly*, vol. 42, no 5 (1998), pp. 433-457; L.M. Ashworth, "Did the Realist-Idealist Debate Really Happen? A Revisionist History of International Relations", *International Relations*, vol. 16, no. 1 (2002), pp. 33-51.

[17] Such interpretations can be found in: N. Guilhot, "The Realist Gambit", in idem (ed.) *The Invention of International Relations Theory*, New York 2011, pp. 128-161.

[18] M. Kaplan, *System and Process*..., p. 35.

[19] Ibid., p. 218.

[20] Ibid., p. 9.

have too often seen rejected methods gain new vigor at a new stage of development of a discipline.[21] In this way Kaplan seems to argue that depending on the level of theoretical development of a particular discipline as a science, different methods can be used for its subject matter. The qualities of theory themselves change along with progress – it becomes more and more similar to theory in natural sciences. It is here where 'progress' is hidden as an implicit category central to Kaplan's argument. The more a particular discipline is developed, the more one can expect from its theory – the more far reaching its explanations, the greater the precision of its predictions, its elegance and internal cohesion. Kaplan is attributing this regularity to the state of natural sciences: *There seems to be a hierarchy of biology, chemistry, and physics within the physical sciences, with the degree of theoretical development ascending from one to the next; each science appears bound by the laws of the former as it adds new laws or propositions that distinguish its particular subject matter*.[22]

To sum up the meaning of 'progress' in Kaplan's work, a few words about theory as a structuring device of the discipline of IR should be added. He argues, for example, that in the inter-war period, idealism was dominant in IR, and its main flaw was that it did not look for general knowledge, but for particular solutions to particular problems. This statement bears semblance to Carr's position presented in the preceding case. This quality of idealism led to a situation where knowledge created in the IR of that time was not additive – there was no accumulation. Thus, Kaplan claims there was nothing that could serve as a theoretical core for the budding discipline.[23] In similar way, he claims that in present times (that is the time of his writing), there was a great need for IR theory, a situation caused by the many IR scholars longing for their discipline to be 'properly' scientific, in the way natural sciences are. Unfortunately, claims Kaplan, the state of IR at the time was in almost direct opposition to these longings: The majority of case and historical studies employed its theoretical fundamentals unconsciously, and thus it did not matter whether they were drawn from economics, psychology, or sociology.[24] Hence, the 'realists' that supplanted the utopists after WWII were having problems with the scientific status of their own work. Their realism, claimed Kaplan, was not realistic enough. This in turn helped him to legitimize his own preferred approach to the science of IR. If we were to think about the first great debate as a revolution, the way Kaplan uses the dualist logic of the realist critique of idealism, at least in the simple interpretation present in the discipline at the time, to dispose of classical realists in the next great debate, clearly it would be possible to say that the revolution eats its own children.

From the above presentation of Kaplan's arguments from the end of the 1950s and the early 1960s, a reconstruction can be made. The state of IR as an at least partially independent field of research, yet having strong ties with political science, is unsatisfactory

[21] Idem, *Is international*..., p. 476.

[22] Idem, *Problems of*..., p. 8.

[23] Idem, *Is International*..., pp. 462-463.

[24] Idem, *Problems of*..., p. 6.

from the point of view of theoretical (scientific) progress. The solution to this problem is to create theories (models), simple at the beginning and becoming more complex with time, in the discipline. This is the only hope for IR to become more scientific. In Kaplan's own words: *While it is doubtful that theories in social science, and in international politics in particular, can ever have the power of theories in physical science or be applied with the success achieved by physical scientists in making applications from their disciplines, the present sorry state of social science is no proof in itself that social science cannot attain such predictive power*.[25] Of course Kaplan has no delusions, stating many times that the ideal of natural sciences is unattainable in IR. Yet the sole fact that he brings up this ideal in his texts, which are arguing for stricter and more rigorous building and application of theory, is very significant. Kaplan's narration sketches the picture of IR as a theoretically weak discipline, one which could only be transformed if one were to embrace their propositions about the ways it should be cultivated. It is important to point to the dynamism of his vision – the contemporary (for him) possibilities and level of development of IR did not enable the production of very sophisticated theories, yet, along with the discipline's development, its theories would be refined. Unfortunately, in such an approach, the development of the discipline is rendered as identical to its theoretical progress, and in this way the author forms a vicious cycle. The discipline will have 'better' theory only when it reaches a higher level of development, and this, in turn, will be acquired by means of scientific refinement of present theories, thus making the discipline 'better'. What is unfortunate for this vision, and hence closes the cycle, is the fact that on the present level of development the theory cannot be 'better'.

Kaplan's notion of progress in IR is unintelligible outside of the context of the discipline at large. The late 1950s and early 1960s was the time of controversy between classics/traditionalists and behaviouralists/scientists, arising from the parental discipline of political science. Without delving too deeply into detail, for the sake of my argument the debate can be presented as a controversy over the proper understanding of what science is. Traditionalists were the defenders of an interpretative/understanding/reflexivist theory, and behaviouralists were arguing for explanatory theories. Considering that the debate, more or less won by behaviouralists, did not result in the creation of a traditionalist enclave in the image of political theory (as it did for political science),[26] I will claim that what was at stake was none other than 'disciplinary hegemony': the ability to impose upon the whole discipline a particular vision of what science *is*, and by what method it should be pursued. This does not mean, of course, that in one moment the whole field became behaviouralist, yet tendencies and trends in the mainstream of IR still were, not so long ago, as the (un)famous methodological textbook by King, Keohane and Verba depicts, staunchly positivist.[27] Kaplan, to legitimize

[25] Ibid.

[26] See for example: E. Hauptmann, "Defining 'Theory' in Postwar Political Science", in G. Steinmetz (ed.) *The Politics of Method in the Human Sciences: Positivism and Its Epistemological Others*, Durham 2005.

[27] G. King, R.O. Keohane, S. Verba, *Designing Social Inquiry*, Princeton 1994.

a certain methodological and meta-theoretical proposition, uses the category of progress in a way that is not logically coherent. This lack of coherence is often overlooked, precisely because of the legitimizing (ideological) quality of the term itself, partially stemming from the way his arguments highjacked the earlier logic, however simplified, of the realist critique of idealism.

POWER OF PROGRESS IN USE

The aims that Vasquez set for his work are twofold: to consider how it was possible that the perspective of power politics, attributed to realists, was able to dominate the whole field of international studies, and to inquire into the question of whether such a perspective would be able to adequately explain international phenomena. Thus, his analysis has two dimensions – the descriptive and the evaluative.[28] He also made it clear that he would be using the category of paradigm, taken from T. Kuhn's philosophy of science.

To define the term paradigm, Vasquez draws on the extensive critical literature on Kuhn's thought, but he also does not forget to reach for the works of this author himself. Yet, at the beginning he claims that he would refer to the critique of the work of the author of *Structure of Scientific Revolutions* (1962) and to consider how it influences the possibilities of research on the realist paradigm in IR. In this aspect, he is mainly interested in three problems: 1) how to define the term 'paradigm'; 2) whether Kuhn's description of scientific change is adequate; and 3) how to define a particular paradigm in IR.

Considering the first problem, he begins with introducing a well-known critique of Kuhn's work made by M. Masterman, who thoroughly reveals just how imprecisely Kuhn uses 'paradigm' in his *Structure*. Thus, Vasquez elucidates two understandings of what 'paradigm' is: 1) a certain social construction: *shared constellation which is the basis of classifying an aggregate of scholars as a community*;[29] and 2) a pattern of a solution to a problem: *In addition to providing sets of solved problems, the exemplar is used in scientific education to inform a student about existing unsolved problems or puzzles in the field*.[30] Vasquez is not content with Kuhn's reformulation, in concert with other critics, arguing that if a paradigm is to be used in his own endeavor, it should be reformulated as a concept. In his view, it stemmed from a need to be operationalized in a way that enabled more precision in its use, and this in turn was needed for evaluative purposes. Hence, he argues, he needed his own stipulative definition of the term: *Stipulative definitions are neither correct nor incorrect, since they are not empirical statements; rather, they can be evaluated on the basis of their ability to conceptualize a set of phenomena in a way that clarifies rather than obscures relationships. In this sense, the most useful stipulative*

[28] Ibid., p. 13.

[29] J. Vasquez, *The Power...*, p. 20.

[30] Ibid.

definition of paradigm is one that can utilize most of Kuhn's insights and provide an adequate account of how science proceeds.[31]

The final definition of paradigm that Vasquez uses is: ***The fundamental assumptions scholars make about the world they are studying.*** (...) *What are the fundamental units of which the world is composed? How do these units interact with each other? What interesting questions may be asked about these units? What kind of conceptions will provide answers to these inquiries? By responding to these questions, the fundamental assumptions form a picture of the world the scholar is studying and tell the scholar* ***what is known about the world, what is unknown about it, how one should view the world if one wants to know the unknown, and finally what is worth knowing*** [bold in original].[32] Vasquez argues that one of the major advantages of this definition of paradigm is that it can reduce the ambiguities inherent in Kuhn's usage of the term. This, in turn, on one hand makes it possible to retain some of his more important insights about the specifics of scientific work, while on the other it helps to precisely depict the conditions in which paradigmatic change takes place. What is more, in Vasquez' view, this makes it possible to falsify the central thesis of *Structure*[33]. He is a bit cryptic on this point, but from extrapolation I think the central thesis being falsified in this particular instance is the lack of rationality of scientific change/progress – an accusation often made by rationalistic critics of Kuhn's approach. The other uncertain thing, at this moment of analysis of Vasquez' argument, is specifically which Kuhnian insights he wants to retain and what role they play in his whole argument. Why does Vasquez present his own stipulative definition of paradigm as one so similar to Kuhn's original notion? The answer to this question lies in the analysis of two other points of his argument: the adequacy of Kuhnian description of scientific research, and the problem of its evaluation.

Considering the first of these problems, Vasquez criticizes the vision of scientific progress based on periods of normal science and revolutionary change. However, the critique itself is slightly odd. On one hand he reconstructs Kuhn's argument, but on the other, at on every step of this reconstruction he brings up arguments against it (instead of simply reconstructing and then criticizing). For example, he draws attention to what he perceives as empirical deficiencies of Kuhn's theory, only to dodge the problem by arguing that discussion on this subject and its criticisms does not confirm or falsify anything, but only shows how the issue itself should be investigated further. Finally, Vasquez states: (...) *Kuhn's framework provides a way of asking the major questions of this analysis – Is the dominant paradigm adequate? Is it producing knowledge? Before these last two questions can be addressed, a set of criteria for evaluating paradigms must be developed.* ***Here Kuhn provides little aid*** [my bold].[34] This is this precise moment at which Vasquez and Kuhn finally part ways. First one only needs the internal logic of the description of scientific practice that the term 'paradigm' gives, but without the logic of

[31] Ibid., p. 22.

[32] Ibid., p. 23.

[33] Ibid., p. 25.

[34] Ibid., p. 27.

arational (for some irrational), revolutionary change that stands behind it. In the last fragment, I should present this problem with greater clarity, but for now it suffices to say that this internal logic of description is precisely those mysterious Kuhnian insights that Vasquez wants to employ.

Introducing his argument about the evaluation of scientific research in the context of appraising the whole paradigm, Vasquez does not mention the problem of scientific progress at all. For him, the issue of evaluation is a technical one. He even claims that the criteria used for this evaluation, present in philosophy of science, are widely accepted by its practitioners. The only problem for him in this respect is to determine the logical relations between particular criteria and establishing their proper meaning. When he writes about the controversies over Kuhn's arational vision of scientific progress and the way in which this scholar answers the criticisms of his approach, he presents a quotation from the author of *Structure*, in which Kuhn gives the following standards for evaluation: accuracy, scope, simplicity, and fruitfulness. From this Vasquez reaches the conclusion that Kuhn is in fact *willing to evaluate paradigms by employing the standard criteria used in science to determine the adequacy of theories*.[35] Vasquez's argument after this becomes murky, as he does not clarify what he means by 'standard criteria used in science', but instead turns to another discussion around epistemic problems. What does he accomplish by this leap? He is able to dodge the question of paradigm *incommensurability, which he sees as present in Kuhn's thought, but which he, following Kuhn's critics, dismisses as unimportant and apparent.*[36]

Finally, Vasquez, under the influence of arguments made by critical rationalists, mainly K. Popper and I. Lakatos, assumes the solution to the problem of paradigm evaluation based on the following argument: (1) the purpose of science is to produce knowledge; (2) knowledge itself is a semantic concept; that is, one can determine whether something is known by stipulatively defining what is meant by knowledge and establishing decision-rules on how to employ the word;[37] and (3) what is meant by knowledge is (at least in part) empirical corroboration of the hypotheses.[38] Here Vasquez is skimming above some serious problems that were important for the philosophers of science to whom he refers. First, he does not specify whether the reasoning he employs is widely accepted by the majority of them, or was just proposed by Tolumin, to whom he refers. Second, he encroaches upon the field of semantics, postulating, also rather unconsciously,

35 Ibid., pp. 28-29.

36 Ibid.; It suffices to bring up famous text by Ole Wæver from *Positivism and Beyond* to show why the question of paradigmatic incommensurability and the career the term made was plaguing the IR still in late 1990's.

37 In his place Vasquez gives the following footnote: "For justification for this position in regard to the word truth see Tarski (1949)". This is a great example of how Vasquez builds the legitimacy of his arguments on the authority of the philosophy of science. To attribute to Tarski the idea that, simply put, establishing meaning of the word is an operationalisation of it, is to alter the author's thought beyond recognition. To refer in the process to Tarski's definition of truth is grossly misleading, for his conception is purely a formal one and it states what truth is with nothing resembling any stipulative definition in a way Vasquez understands it.

38 Ibid., p. 30.

its pragmatic and operationalistic version, which itself leads to some important philosophical controversies. Third, in the final part of the quotation, he uses the technical term 'corroboration', which in the context of 'paradigm', which appears in the beginning of the next paragraph, is truly an abomination, considering the magnitude of disagreement between the authors of both terms (of course I have in mind the particular technical meaning of the terms used by Vasquez, the authorship of which can be attributed to Popper and Kuhn). Finally, he uses corroboration without any thought given to the problems concerning its links to another of Popper's concepts, namely verisimilitude.

From the standpoint of these problems, I am leaning to the interpretation of the above fragment of Vasquez's argument as a peculiar postulate that legitimizes itself upon the inexplicit notion of progress. To put it differently – progress is possible only when there are no Copernican revolutions defined as paradigm shifts, according to the logic of Kuhn's argument in Structure. Thus, Vasquez takes the 'paradigm,' with all its splendor, from Kuhn, but he cannot take it with the logic of change that for Kuhn is inseparably connected with the concept itself. This inability stems form the fact that Vasquez needs not the logic of change, but the logic of progress.

Finally Vasquez claims that, In social science, particularly in International Relations inquiry, the problem of evaluation of paradigms turns not so much on comparing the corroborated empirical content of rival theories and their research program, but on finding any theory with corroborated hypotheses it produces. Since a paradigm is used to produce theories, it is possible to evaluate the adequacy of a paradigm in terms of the corroborated hypotheses it produces.[39] Thus, in the above fragment, Vasquez is postulating nothing other than a rational, or dare I say Popperian, evaluation of paradigm – the term coined by Kuhn in this particular meaning, which could be described as an attempt to mix water with fire. Moreover, he is not clear whether he wants to evaluate a paradigm vis a vis another paradigm or to evaluate a paradigm per se. In light of the perplexing idiosyncrasy of Vasquez's approach, it is hardly a surprise that he evaluates the realistic paradigm as unfitted for international relations. It would also appear at first that he does so without sailing on the murky waters of the problem of scientific progress in social sciences. Yet, what Vasquez's argument does in fact is to create a paradigm-progress-legitimacy amalgam, one that has long since haunted the discipline. The idea, to simplify, is that 'paradigm', as a notion from Kuhnian philosophy, is a social device in which the cumulative progress of (normal) science happens. If you want to legitimize an approach to IR as properly scientific, you need to show how scientifically progressive it is. Thus, the move to label every possible tradition and approach as 'paradigm'.[40] If what you are doing is done within a paradigm, what you are doing is progressive and hence a legitimate scientific practice.

To better understand the extreme eclecticism of Vasquez's notion of progress, it is crucial to invoke the context in which it was created, and the legitimizing value of

[39] Ibid., p. 31.

[40] The practice aptly depicted by the following discussion: P. Feaver et al., "Brother, Can You Spare a Paradigm? (Or Was Anybody Ever a Realist?)", *International Security*, vol. 25, no. 1 (2000), pp. 165-193.

category. The dissertation lying at the core of the analyzed text was created in the first half of the 1970s. Two classics describe the climate of the moment in the following words: As student in the late 1950's and early 1960's, we were taught to look at international politics through 'realist' glasses, which emphasized the ever-present possibility of war among sovereign states. As our earlier work indicates, we soon become uneasy about this one-sided view of reality, particularly about its inadequate analysis of economic integration and of the roles played by formal and informal international institutions.[41] These are the opening words of the introduction to Power and Interdependence (1977), a work that, among others published in the early 1970's, heralded the birth of IPE (International Political Economy) as a subdiscipline of IR. In this time, the subject matter of the discipline was broadened by problems such as economical interdependence or the role of international regimes and hegemonies.[42]

In this turbulent period, the analyzed work of Vasquez was created. From this perspective, it is much clearer why he uses the term 'paradigm'. It helps him to identify what can be called the hegemony in the discipline, that is realism, and at the same time to provide rationalistic criteria for its evaluation that enable presenting the dominant paradigm as laden with problems and anomalies. It is making an appearance that it is only waiting to be swept under by the Copernican revolution instigated by a new way of perceiving the international milieu – neoliberalism. Thus, it is clear why Vasquez is performing his intellectual acrobatics with categories of philosophy of science with respect to progress. The term was supposed to legitimize the new approach, incompatible with realism. It was, however, incompatible with the primary vehicle of Vasquez's argument – the paradigm. The lasting legacy of Vasquez' work is the trinity of paradigm-progress-legitimacy, and the harm done is the lack of meaningful communication or a dialogue that, still to an extent plagues the discipline.[43]

PROGRESS À LA CARTE

In the last case study, I will analyze *Progress in International Relations Theory*, the reader edited by C. Elman and M.F. Elman. They begin claiming that their book: 'investigates how international relations (IR) theorists can equip themselves to determine whether the subfield's work is getting any better; that is, whether it is progressive in the sense of providing cumulative knowledge about hitherto unexplained phenomena'.[44] It is interesting how the authors use the category of progress explicitly, which was not exactly the case in previous texts. They link this category with accumulation of knowledge,

41 R. Keohane, J. Nye, *Power and Interdependence*, Boston 1977, p. 7.

42 On the genesis of IPE in such context see: B. Cohen, *International Political Economy: An Intellectual History*, Princeton 2008.

43 For the basics of the argument about this state of affairs see the positions in passim 8.

44 C. Elman, M.F. Elman, "Introduction: Appraising Progress in International Relations Theory", in eidem (eds.), *Progress in International Relations Theory: Appraising the Field*, Cambridge 2003, p. 1.

thus giving it a decidedly positive meaning. In this vein, they propose three aims for the publication they are editing. The first was to draw the conceptual framework of methodology of scientific research programs (MSRP) created by I. Lakatos. They are arguing that although other authors used references to the philosophy of this thinker, they usually did so in a way that was incoherent, or even distorting the thought of Lakatos. The second aim was to discover whether Lakatosian methodology is fitted for the role of appraisal of IR theory. They claim that an answer is possible because his framework is tested against a wide spectrum of empirical evidence. Their third goal was for their book to contribute to the general debate on the nature of scientific change in social sciences and IR in particular. Nevertheless, they emphasize that their framework was by no means the only one possible, considering that science can progress in various ways. Thus, they do not claim that only the methodology of scientific research programs can describe it satisfactorily.[45]

Due to the limited scope of my study, I will turn my attention to the interpretation and critique of two key issues present in the Elmans' explication of MSRP – the category of new facts and the historical quality of the evaluative dimension of this method. The authors are well-aware of the importance of new facts to MSRP as a whole: *Because predicting new phenomena is such an important part of the methodology of scientific research programs, the definition of novelty plays a crucial role.*[46] The essence of their argument, as I mentioned above, is the importance of the prediction of new facts by a particular program and how it provides for its progressiveness.[47] After the presentation of various definitions of new facts present in Lakatos's thought, and critical appraisal of some controversies that arisen around it, they present the definition, and claim it as their own, from the text of J. Worral: [MSRP] *embodies the simple rule that one cannot use the same fact twice: once in the construction of a theory and then again in its support. But any fact which the theory explains but which it was not in this way pre-arranged to explain supports the theory* ***whether or not the fact was known prior to the theory's proposal*** [bold in original]. From this quotation it is clear that the new fact is one that, from a functional point of view, does not create a vicious cycle – it is not used as an example of explanation in a particular theory, and thus it is not an example of a successful prediction upon its basis.

The above reconstruction is of course the amiable way of interpreting the explication of new facts made by Elmans (with help of Worral). Unfortunately, even then the category itself is not clear enough, and it does not provide the reader with criteria that are easily operational and can be readily applied to the task of evaluating a particular theory or research program – criteria that were supposed to be presented by authors themselves.[48] The last straw in case of Elmans is when they try to defend the category

[45] Ibid., pp. 5-7.

[46] C. Elman, M.F. Elman, "Lessons from Lakatos", in eidem (eds.), *Progress in International Relations Theory: Appraising the Field*, London 2003, p. 33.

[47] Ibid., pp. 28-33.

[48] The other problem concerns the use of this method of appraisal in social science. The authors of *Progress* are trying to show that it is possible in a separate section of their text. Unfortunately, in my opinion their argument is flawed. This flaw stems from the approach they employ – they do not

of new facts and its practical implementation: *even if scholars continue to disagree over which definition of novelty to use, so long as they make their decisions in full knowledge of the different candidate's strength and weaknesses, and insofar as they are explicit about the definition of novelty that they are employing, there is no reason why the novel fact debate should pose an insurmountable obstacle* [in practical employment].[49] This statement can be read as contradictory to the greater purpose of legitimizing particular programs on the basis of this particular philosophy of science.

Now I will turn to a stylized reconstruction of the conclusions arrived at by the contributors of the volume edited by the Elmans. So, what do authors of the eight articles aiming to use in practice Lakatos's criteria think about the progressiveness of their programs? The authors of four of the texts – R. Keohane and L. Martin, writing about institutional theory; J. DiCicco, J. Levy, considering the power transition programme; J. Ray, evaluating the thesis about democratic peace; and J. Snyder, describing progress in selected programs containing normative elements – claim that all these programs are progressive according to the adopted criteria. The author of the text about operational code, S. Walker, states that, although it develops (and so progresses) in accordance with Lakatosian criteria, it is more fruitful to use conceptual frameworks established by another philosopher of science, L. Laudan. Writing about differences between (classical) realism and neorealism, R. Jarvis also has a problem with applying the criteria of research programs, but he claims that even an attempt at conducting such analysis is a valuable intellectual exercise. It is interesting that two papers written by representatives of opposing camps, A. Moravscik and R. Schweller, the former presenting a liberal research program, and the latter a neoclassical realism program, are the most critical towards the use of this method to evaluate scientific progress. Moravscik tries to persuade that sticking too strictly to Lakatosian categories

confront the issue itself, but instead write about whether or not Lakatos was fond of the idea itself, or whether or not he was prejudiced towards the social sciences (Ibid., pp. 45-50). The Elmans in no way explain the difference between natural and social sciences, and one can infer from this the following: it is their oversight, or they are naturalists and do not think there is any significant difference between said sciences at all, and so they do not see the need to mention it in the proper way. Let us take an example from astronomy. The anomalies in the orbit of a particular planet, in respect to the calculations based on the theory of its movement, can be explained by postulating the existence of another planet – its gravitation is responsible for the supposed anomalies. If the author of such amendment to the theory, in course of devising it, did not observe, via telescope, the responsible planet, but observed it only after he made calculations on the basis of his amendments, and they are what enabled this observation, only then it could be considered as new fact in the meaning of the term used by Elmans. But, analogically, how to search for such new facts in IR? If the sole possibility of observation of social facts rests upon the categories that are inherent to the observer, how is it possible for such categories not to be repeated in a way that excludes the possibility of them being the new facts (one may say that the fact that those categories repeat themselves in this certain way is what enables scientific/systematic observation in social sciences)? The Elmans do not provide answers to any of these questions. Although, depending on the meta-theoretical stance of the reader, or in this instance the interpreter, they can be extremely different. It is a paradox of intellectual history that the issue of facts in social science was mentioned by Carr who, by extrapolation, would probably not be fond of the Elmans' take on the matter.

[49] Ibid., p. 64.

may cause rivalry in a zero-sum game between particular approaches, and this would have a negative impact on fruitful and rigorous synthesis, thus lead to suspension of the progress of his subdiscipline.[50] Schweller, however, criticizing Lakatosian categories, proposes a common-sense approach based on evaluation of practical aspects of a given method.[51]

So, what vision of progress is offered by these texts? Half of the authors see progress in accordance with Lakatos's criteria, two of them do not form a definitive judgment, and the other two are negatively oriented toward the idea itself. One can get the impression of a group that meets, discusses, and then, in spite of a few 'nays', decides that progress exists, pats each other on the back, and gets back to their progressive scientific work. It seems to be a sign of an important problem, which was remarked on by the very author of the method of scientific research programs. His method should be used to evaluate programs seen with a long insight or even only *ex post*.[52] Despite this statement, in this case the method has been used to evaluate current work of the researchers, and, what is more, by the scientists that could benefit from qualifying their approach as progressive or, possibly, denying this quality to that of their rivals.

To make this problem easier to understand, I will sketch the context behind the creation of this publication. At least from the end of the Cold War, in the discipline there was a problem concerning adequacy of explanations present in structural realism/neorealism, which can be perceived as a paradigm (so, a pattern of how to solve a problem, in this case of creating a scientific theory in social science of IR) of scientific theory in the so-called main stream of IR.[53] In the early 1990s, there was a neo-neo synthesis, an attempt to unify neorealistic and neoliberal postulates, which would lead to a new scientific orthodoxy in IR regarding general international politics theory,[54]

[50] Such an opinion is perfectly understandable if one takes into account the syncretic nature of the author's approach. But what is not so clear, and even surprising, is why Moravscik is accusing Lakatos – this very author presented as a progressive program (at least during some time) a program in physics based on wave-particle duality, even when he was pointing to a logical problem of violation of the law of non-contradiction present at its very core. The said volition stemming from the fact that this program was a fruitful synthesis in itself. The other issue is that in his article with Legro, Moracscik is actually doing precisely that – he uses Lakatosian ideas to subsume the new incarnation of realism – a neoclassical one – to his own approach. W. Legro, A. Moravcsik, "Is Anybody Still a Realist?", *International Security*, vol. 24, no. 2 (1999), pp. 5-55.

[51] C. Elman, M.F. Elman, "Introduction", in eidem, *Progress in...*, pp. 12-15.

[52] I. Lakatos, "History of Science and its Rational Reconstructions", *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association*, vol. 1970 (1970), pp. 91-136; idem, "On Popperian Historiography", in J. Worrall, G. Currie (eds.), *Mathematics, Science and Epistemology*, vol. 2, Cambridge 1978, p. 204.

[53] Obviously, the criticism of this approach began in the early 80's, as e.g. in one of the first works regarding this subject: R. Keohane (ed.), *Neorealism and Its Critics*, New York 1986.

[54] Two works that can serve as examples are: D. Baldwin (ed.), *Neoliberalism and Neorealism: the Contemporary Debate*, New York 1993 and C. Kegler Jr. (ed.), *Controversies in International Relations Theory: Realism and the Neoliberal Challenge*, New York 1995. Another testimony to this attempt is a chapter of a textbook by S. Lamy, "Contemporary Mainstream Approaches: Neo-realism and Neo-liberalism", in J. Baylis, S. Smith (eds.), *Globalization of World Politics*, Oxford 2008.

but many scholars objected to it.[55] Thus, the second half of the 1990's can also be considered another turbulent period in IR. Under these circumstances, the use of the term 'progress' by the Elmans turns out to be perfectly understandable. With this notion, together with the epistemic authority of Lakatosian philosophy of science, they try to relegitimize the proper scientific character of many mainstream, and thus impugned, views of IR.

CONCLUSION

To conclude let me return to the research questions posed in the introduction – I will first answer three of them on a case-by-case basis, and then ponder the last one.

For Carr, the meaning of progress in IR is a dialectic between utopist and realist phases of this science. Their clash, in his view, should lead to synthesis – a mature social science that stays true to its social purpose but does so without naivety and in relation to facts and logic (the scientific method). The context of Carr's notion of progress is the World War II, which falsified much of the purported knowledge about IR. This led to the particular, simplistic reading of his notion of progress that retained the duality of realism-utopism, but without the synthetic notion of mature science. This can be attributed in part to the qualities of his text itself – the practical, realist need to invalidate utopist thought on the premise that its flaws translated into flaws of political action based upon the premise itself. This, in conjunction with the general historical context of the book, the need to 'learn the lessons' of the second global conflict in the span of 20 years is to be blamed for this simplistic reading.

The notion of progress in Kaplan is a cumulative one. Science is viewed as a process where along the passage of time a discipline matures through the development of more and more refined theories. This process reinforces itself since, as the author claims, the better the theories, the more scientific progress they 'generate'. The reasoning here is somewhat circular, as the postulate for better theories is predicated on the very notion of progress these better theories are supposed to bring about. The context of this notion of progress is the second great debate, and Kaplan is one of the chief proponents of a scientific approach that he posited as one that should supplant the traditionalist approach. The debate itself was a reflection of a similar debate that happened 10-15 years earlier in political science (the parent discipline of IR), one was caused to an extent by the demand for a peculiar quantitative social science by the US Government during

[55] Examples of their criticism can be found in e.g. R.N. Lebow, T. Risse-Kapen (eds.), *International Relations Theory and the End of Cold War*, New York 1995; S. Smith, K. Booth, M. Zalewski (eds.), *International Theory: Positivism and Beyond*, Cambridge 1996; M. Doyle, G. Ikenberry (eds.), *New Thinking in International Relations Theory*, Boulder 1997. In addition, there is also the revisionism in the historiography of the discipline demystifying myths deeply ingrained in its common conscience, represented by such authors as B. Schmidt in *The Political Discourse of Anarchy: A Disciplinary History of International Relations*, New York 1998, or L.M. Ashworth and his works from the turn of the 20th century.

World War II. Kaplan saw such a meaning of progress as legitimizing his own chosen way of conducting social science. He actually borrowed the simplified, dualistic logic form – the debate between realism and idealism – to combat its victor, classical realism.

I would argue that Vasquez employs an idea of progress present in Kuhn's notion of normal science – once the paradigm is established, it enables slow and steady cumulative progress of scientific knowledge. The problem with that is the practice it triggered in the discipline of IR – since everybody wants social science, they are committing to be progressive, and everybody needs a paradigm. This proliferation led to the tribalism of warring schools of thought in IR, a situation very close to the description given by Kuhn to the pre-paradigmatic stage of the development of science sketched in the post scriptum to his *Structure*. The context in which Vasquez uses the term is somewhat complicated: on one hand we have historical events of the collapse of the Bretton Woods system in the global political economy that prompted the interest in this subject matter and translated to the development of international political economy as a subdiscipline of IR, and on the other hand the end of the Cold War, and the alleged inability of the then-dominant (after Waltz' publication of 1979) structural realism to explain it. Finally, we have the powerful move made by the author of Theory of International Politics – the introduction of philosophy of science as a legitimizing basis for IR scholarship, which Vasquez probably wanted to emulate. His take on the problem, employing the Kuhnian notion of paradigm (and the Popperian means to evaluate it), has led to the amalgam of paradigm-progress-legitimization that diversified the discipline at the cost of dialogue among its various practitioners.

The Elmans' introduce their notion of progress closely following the methodology of scientific research programs from the philosophy of I. Lakatos. The notion of new facts that contribute to a particular program's growth is central to their approach. The growth itself should be understood on the cumulative premise as a progress of science within which the particular program is placed. The context within which the book is written is on one hand the end of the Cold War, as was the case for Vasquez's work, and on the other the neo-neo synthesis of the main current of IR and the challenge to it posed by the postmodern/critical approaches gaining momentum at the time. The problem with the Elmans' publication, as we saw in the conclusions of its contributors, is that the vagueness of the concept enables for facile legitimization of one's own program by showing that it is progressive. This, coupled with the high level of sophistication of the method from the philosophy of science, raises questions about its ability to legitimize not only the particular programs analyzed in the book, but also the mainstream of IR that they constitute.

There are some persistent similarities in the notions of progress employed by the analyzed scholars. In one way or another, they all contain a strong cumulative element. They all serve to legitimize the take on the social science of IR of the particular author as properly scientific. Moreover, all the notions have been constituted in the context of certain real-world events. Yet, this is not to say that there was no inner (to IR or science at large) logic in how they were conceived by the authors of the works analyzed in my text. Especially, the introduction of philosophy of science and a legitimizing discursive

device. Still, the differing particulates of the employment of the notion of progress, and the divisive consequences that arose, especially in the last two cases, demonstrates that 'progress' is definitely one of the most contested terms in IR.

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