# Rational Choice and New Institutionalism, a Critical Analysis

by Lorenzo Kamel

In the last few years the field of political science has been dominated, often paralyzed, by a theoretical and methodological debate among proponents of rational choice and new institutionalism approaches. The Hebrew University of Jerusalem has become one of the most vibrant academic centres in this debate. Inspired by a seminar taught by professor Moshe Maor in that university, the following work offers a comprehensive analysis of the main criticism directed at rational choice and new institutionalism theories. It concludes with a discussion of their dominant methodologies.

## Main criticisms directed at the rational choice approach

The foremost purpose of this first section is to focus our interest on the main criticisms directed at rational choice approach, a way of perceiving the reality that Donald Green and Ian Shapiro defined "an empirically worthless but nonetheless highly pretentious theory" (D. Green, I. Shapiro, *Pathologies of Rational Choice Theory*, Yale University Press, New Haven 1996).

Primarily it is important to note that the approach proposed by the theorists of rational choice assumes that there are important forms of political behavior that are the result of choices made with a view to the efficient achievement of given goods. Rational choice explains individual actions and the outcomes they lead to in terms of the course of actions open to individuals, their preferences and their beliefs about important parameters such as others' preferences (H. Ward in D. March, G. Stoker, *Theory and Methods in Political Science*, Macmillan Press, London 1995, p. 69).

Mainstream rational choice theory assumes that individuals are self-interested, implying the idea that all action is fundamentally "rational" in character and that people calculate the likely costs and benefits of any action before deciding what to do.

Even so, the first complication is that actions may lead to various outcomes depending on a random event; or individuals may not know for certain the consequences of their actions. It has been shown that, "granted certain assumptions, individuals choose as if they were maximizing expected

('average') utility, taking into account the various possible outcomes the action could lead to, and the probabilities of their occurrence" (Ward in March, Stoker, *Theory and Methods in Political Science*, p. 79). In my opinion we should accept this clarification. People are psychologically different than the core model assumed. Human actions involve both rational and non-rational elements. Quoting Professor John Scott:

> What distinguishes rational choice theory from other forms of theory is that it denies the existence of any kinds of action other than the purely rational and calculative. All social action […] can be seen as rationally motivated, as instrumental actions, however much it may appear to be irrational or non-rational (J. Scott, *Rational Choice Theory, Understanding Contemporary Society: Theories of the Present*, Sage Publications, New York 2000, p. 2).

One of the fundamental assumptions of rational choice theory is that the primary unit of analysis is the individual decision-maker. This approach has been attacked by those, including Marxists, who believe that social classes/groups are essential:

> Because rational choice theory conceptualizes actions as rational strategies for realizing the preferences of the actor, there is a sense in which it seems to reduce the motives of political actors to self-interest. Yet, as most rational choice theorists would recognize, we have non valid grounds for so privileging self-interest as a motive. Even if an action happens to have beneficial consequences for the actor, we cannot from this fact alone conclude that the actor acted in order to bring about those beneficial consequences, let alone that he did so solely for that reason. Besides, a theory predicated solely on self-interest cannot properly make sense of altruistic actions (M. Bevir, R. A. Rhodes, *A Decentred Theory of Governance: Rational Choice, Institutionalism, and Interpretation*, Institute of Governmental Studies, University of California, Berkeley 2001, p. 14).

Broadly speaking, critics of rational choice theory consider this approach too individualistic, too minimalist, and too focused on rational choices in social action. Moreover, we do not know anything about the provenience of the preferences of people. We do have only some "assumptions". Quoting the analysis of Hugh Ward, rational choice theory "makes implausible assumptions and fails in predictive terms" (Ward in March, Stoker, *Theory and Methods in Political Science*, p. 91).

Rational choice theory implies the idea that preferences are primitive and stable. The Institutional school of economics though denies this assumption, stating that preferences are charged by factors such as advertising.

As noted by Professor Paul Pierson, "Rational choices implies taking slices of history" (P. Pierson, *Politics in Time: History, Institutions, and Social Analysis*, Princeton University Press, Princeton 2004). In this way there is a serious risk of ignoring many important aspects:

The discrepancy between the faith that practitioners place in rational choice theory and its failure to deliver empirically warrants closer inspection of rational choice theorizing as a scientific enterprise (Green, Shapiro, *Pathologies of Rational Choice Theory*, p. 6).

There are also some important empirical criticisms related to the rational choice approach. Indeed one of the most strong criticism of the rational choice approach involves not the theory in itself, but how it is used. These critics were well presented by Donald Green and Ian Shapiro. They noted that a theory does not account adequately for observed phenomena. The desire of rational choice theorists to generate a universally applicable model of politics leads them to evade and to ignore contrary evidence. To sum up "the developer of a theory does not even attempt to test it empirically" (Green, Shapiro, *Pathologies of Rational Choice Theory*).

Rational choice theory is appropriate and functional to explain such phenomena as the market, but it becomes a failure when it claims to act as a hegemonic theory and when it claims to explain not only the economic field, but also all social behavior of man[1]. Moreover I think that we should take in account that even when people want to act rationally, they sometimes do not have all the information to choose from. In other cases they have this information, but they do not have the tools to use it (i.e. "bounded rationality").

Ultimately let us conclude our examination of the rational choice approach with a summary outlook provided by Hugh Ward. He pointed out "four modes of criticism of rational choice theory" using the following path: a) the internal critique of rational choice "heretics" who wish particularly to emphasize bounded rationality; b) the sociological critique, which centers on the way rational choice theory appears to play down social structure and holistic modes of explanation; c) the psychologists' argument that individuals often do not act rationally in the standard sense and are motivationally and psychologically

[1] In this context it may be interesting to hint briefly to the so-called game theory, at present dominant in political science. Game theory "deals with situations where others' choice of strategy affects your best choice and vice versa" (Ward in March, Stoker, *Theory and Methods in Political Science*, p. 66). This is a technique of analysis related to the field of rational choice, that arose in the context of mathematics, but found extensive applications in many disciplines including economics and sociology. A game is a formal representation of configurations of the reality in which two or more players have to act strategically, i.e. in an environment that is modified by the actions of other participants. Two or more players must choose one of two or more courses of action. The game can be zero-sum, or not. Zero-sum game can be exemplified by the case in which a defined resource should be divided among the various actors; it describes a situation in which a participant's gain or loss is exactly balanced by the losses or gains of the other participant(s). R. B. Myerson, *Game theory: analysis of conflict, Harvard University Press*, London 1997.

complex; d) the critique from mainstream political science, based on the implausibility of the assumptions made and the predictive failure of the model.

In conclusion we can assert that rational choice theory is a useful set of research methods for the tool-kit of all political-scientists. Even so "its status is more akin to those of statistical techniques that are appropriate for certain types of data; *it is not a stand-alone paradigm for understanding the whole of the political sphere* [emphasizes added]" (Ward in March, Stoker, *Theory and Methods in Political Science,* p. 93).

**Main criticisms directed at the New Institutionalism**

The theme of "institutions" and "institutionalist" approaches is as old as social thinking. In some ways, the idea of institutionalism and the analysis of the way institutions affect our society can be found already in the writings of Greek philosophers.

In the last two decades, the theme has been "rediscovered" and re-launched within each of the major social science disciplines. This most recent emphasis has been labeled and acknowledged as an approach of its own, generally identified as "New Institutionalism".

New institutionalism focuses on pervasive influence of institutions on human behavior through rules, norms, and meanings for behavior or regulative, normative and cultural-cognitive frameworks. It recognizes that individual choices are influenced by the societal context.

"New Institutionalism" is often contrasted with "Old Institutionalism". From the point of view of the older institutionalism, new institutionalism tries to explain institutional change as merely another instance of utility maximization. Old institutionalism, on the contrary, seeks to articulate reasons for institutional change in terms of social and political volition.

It is often said that new institutionalism is at its weakest when trying to explain the genesis and transformation of institutions. Moreover:

> The new institutionalism attracted so much attention because the label marked "a disposition to oppose the political science mainstream" and reflected "the shift of ground by some of those interested in the state". Indeed, the study of institutions could be new only to the advocates of American behaviouralism or European state theory who had deliberately downgraded its importance. It had always remained part of the political science mainstream (R. A. W. Rhodes, *Theory and Methods in Political Science*, Macmillan, London 1995, p. 54).

Until the Fifties there was a clear dominance of the institutional approach within political science. Quoting Vivien Lowndes: "Institutionalism *was* political science". The "behavioral revolution" changed all that, in fact new institutionalism was born out of a reaction to the behavioural revolution.

Literature reviews normally disaggregate new institutionalism into rational choice, historical and sociological new institutionalism. These three schools of thought developed simultaneously but independently. What unites them is the fact that all three branches developed in reaction to the behavioural revolution in the social sciences in the 1960s and 70s, and that they all seek to elucidate the role that institutions play in the determination of social and political outcomes:

Rather than taking the functions of political institutions at face value, behaviouralists sought to explain how and why individuals acted as they did in "real life". […] A generation later, rational choice theorists sought to explain politics in terms of the interplay of individuals' self-interest (V. Lowndes in D. March, G. Stoker, *Theory and Methods in Political Science*, Palgrave, New York 2002, p. 99).

By the end of the 1980s a new institutionalism has emerged as a reaction to the "undersocialised" character of dominant approaches in the discipline. New institutionalism is essentially a cognitive model. According to it, cognition within organizations can be understood in terms of cognitive frameworks emerging from organizational fields.

New institutionalism recognizes that institutions operate in an environment consisting of other institutions, called the institutional environment. Every institution is influenced by the broader environment.

Much of the research within new institutionalism deals with the pervasive influence of institutions on human behavior through rules, norms, and other frameworks. However there are some important aspects that it seems unable to explain:

The new institutionalists must explain why some societies, seeing the inefficiency of their own institutions, do not simply imitate the more optimal institutions of their successful peers in order to approximate those rates of economic growth. Instead, income and growth differentials persist over long periods of time, resulting in a more or less stable hierarchy of successful and unsuccessful countries (in terms of economic growth). In this case, new institutionalists invoke "path dependency," but do not specify exactly how it works. We need to know when and how such changes occur to understand this factor (A. Hira, R. Hira, *The New Institutionalism: Contradictory Notions of Change*, American Journal of Economics and Sociology, April 2000).

Critics of traditional institutionalism point to its limitations in terms of scope and method. Where the old institutionalists were disdainful of theory, the new institutionalists are markedly enthusiastic, developing different theoretical projects.

According to Peters, we can count seven types of new institutionalism: normative institutionalists, rational choice institutionalists, historical institutionalists, empirical institutionalists, international institutionalists, sociological institutionalists and network institutionalists.

In contrast to the traditional institutional approach, new institutionalism focuses upon informal conventions as well as formal rules. New institutionalists are concerned to explore how institutional stability is accomplished through human action. Institutions are not "things", as implied in some traditional approaches, but processes (Lowndes in March, Stoker, *Theory and Methods in Political Science,* pp. 98-99).

The new institutionalists concern themselves not just with the impact of institutions upon individuals, but with the interaction between institutions and individuals. They insist on a more autonomous role for political institutions.

However, in my opinion, even if the new institutionalists agree that political institutions are "the rules of the game", they do not provide clear distinctions between institutions and norms in general. The new institutionalists include too many aspects of political life under one category which disguises the variety of interactions and causal mechanisms that occur:

> If the concept of institution "means everything, then it means nothing" – how can political institutions be distinguished from other social facts? […] How can political scientists recognize (and measure) an institution when they see one? On a theoretical level, how can they avoid the traps of reductionism and tautology? (Lowndes in March, Stoker, *Theory and Methods in Political Science,* p. 103).

A common criticism against new institutionalism is that it does not help us to understand organisational change, only stability (K. Thelen, S. Steinmo, *Structuring Politics: Historical Institutionalism in Comparative Analysis*, Cambridge University Press, New York 1992).

Moreover Peters reminded the new institutionalists of the "need for more rigour in conceptualization and then measurement of the phenomena that are assumed to make up institutions" (G. Peters, *Institutional Theory in Political Science: The New Institutionalism*, Pinter, London 1999).

Finally we can conclude that new institutionalism has provided new perspectives and in this sense represents an important "tool" in political science. However, it fails in many explanations and therefore it should be considered only as an important step in view of future developments:

> If we take seriously the new institutionalism's features of path dependence, the importance of context, and group norms, we can hardly maintain an individual actor-based, rational choice-type of model. In modifying the assumptions of rational choice models and introducing the seed of relativity, i.e., the relative uniqueness of situations, the new institutionalism fails in its attempts to improve rational choice models and loses their virtue of predictive power. We are inevitably led to call for an entirely new model of human behavior that incorporates, but goes beyond, the new institutionalism. Future models should explicitly define and delineate the true ultimate sources of institutional change, namely changes in culture, ideas, and social practice, and then we can turn to how they relate to the institutional and preference changes that result from them (Hira, *The New Institutionalism: Contradictory Notions of Change*).

**How do you know your causal argument is true? What are the main formal properties by which your argument might be tested?**

Parallel to the theoretical discussion portrayed above, a methodological discussion evolved in the field of political science, which will be analyzed in the following.

Causal inference is one of the most important forms of reasoning in science. A cause raises the probability of an event. In order to find an objective way for knowing when a causal argument is true we can use many criteria. Professor John Gerring is probably the scholar who analyzed these "tools" most thoroughly. The first criterion pointed out by Gerring for the purpose of our interest is the "*plenitude*". The more comparative reference points one has at his disposal, the better one has the chance to test the veracity of a certain proposition. Moreover, a large sample may also help in specifying a given proposition.

A second aspect is related to "*comparability*". In fact "the problematic status of a case is that in order for it to serve its function, in order for it to be a case *of* something, cases must be familiar to one another in some (though by no means all) respects" (J. Gerring, *Causation, A Unified Framework for the Social Sciences, Journal of Theoretical Politics, Sage Publications*, London 2005, p. 184).

In order to understand if an argument is true we should consider other important aspects such as "*independence*": "If a unit is not independent, no new information about [a variable] is obtained by studying it twice, and no additional confirmation of a [a theory] is obtained by counting it twice" (M. Zelditch, *Intelligible comparisons*, in I. Vallier (ed.), *Comparative Methods in Sociology. Essays on Trends and Applications*, University of California Press, Berkeley 1971, pp. 267–308). "*Representativeness*" refers to comparability between the sample and the population. Unlike problems of comparability, problems of representativeness must remain at the level of assumption. Other important factors are "*variation*" (empirical evidence of causal relationship is covariational in nature) and "*transparency*".

Still following Gerring's work, "*replicability*" represents a fundamental characteristic for a causal argument which is supposed to be true. In fact, a good research should be replicable by future researchers: reliability and replicability may be viewed as two aspects of the same general goal (Gerring, *Causation*, p. 189).

The second part of this analysis is related with the main formal properties by which an argument might be tested. In this sense we should focus our interest on the interesting debate between Kuhn, Popper, Lakatos, Feyerabend and some other important scholars that in the previous decades have developed

studies about the "demarcation" between science and non-science in the previous decades. This demarcation is crucial if we want to detect the "properties" that we are looking for.

Popper, for example, noted that the world is full of verifications of theories/arguments. There is an "incessant stream of confirmations, of observation which 'verified' the theories in question" (K. Popper, *Conjectures and Refutations. The Growth of Scientific knowledge*, Routledge, London 1965, p. 35). However, as he noted, it is easy to obtain confirmations, or verifications, for nearly every theory, if we look for confirmations. Actually a theory which is not refutable by any conceivable event is non-scientific. Irrefutability is not a virtue of a theory, but a vice. Every genuine test of a theory is an attempt to falsify it or to refute it. To summarize, the criterion of the scientific status of a theory is its falsifiability, or refutability, or testability. For the reasons that we have analyzed, I come to the conclusion that I fully agree with the following postulate:

> The problem which I tried to solve by proposing the criterion of falsifiability was neither a problem of meaningfulness or significance, nor a problem of truth or acceptability. It was the problem of drawing a line (as well as this can be done) between the statements, or systems of statements, of the empirical sciences and all other statements – whether they are of a religious or of a metaphysical character, or simply pseudo-scientific. Years later – it must been in 1928 or 1929 – I called this first problem of mine the "*problem of demarcation*". The criterion of falsifiability is a solution to this problem of demarcation, for it says that statements or systems of statements, in order to be ranked as scientific, must be capable of conflicting with possible, or conceivable, observations (Popper, *Conjectures and Refutations,* p. 39).

This position follows in some way an assumption of David Hume: induction cannot be logically justified. In fact observation is always selective. It needs a chosen object, a definite task, an interest, a point of view, a problem.

Far from Popper and "Popperian scholars" was the position of Thomas Kuhn. In his most famous book (T. Kuhn, *The Structure of Scientific Revolutions*, Chicago University Press, Chicago 1962) he came to realize that traditional accounts of science, whether inductivist or falsificationist, do not bear comparison with historical evidence. Kuhn pointed out that a mature science is governed by a single paradigm. It coordinates and directs the "puzzle-solving" activity of the groups of normal scientists that work within it. According to Kuhn, the existence of a paradigm (term that refers to a set of theories, laws and instruments that define a tradition of research in which theories are universally accepted) capable of supporting a normal science tradition is the characteristic that distinguishes science from non-science.

However, Kuhn's attempt was fiercely attacked by many scholars. Quoting Eileen L. McDonagh, we can summarize in the following way the main criticisms moved toward Kuhn:

> The definition of a paradigm is vague and ambiguous; criteria for paradigm change suggest an irrational basis for science; the definition of "normal science" is an overly conservative and restricted view of the purposes of science; paradigm change is a matter of degree and paradigms are not incommensurable; the question about the total development of science and whether it advances to a greater approximation of truth with each paradigm change is unanswered. (E. L. McDonagh, *Attitude Changes and Paradigm Shifts: Social Psychological Foundations of the Kuhnian Thesis*, Social Studies of Science, Vol. 6, N. 1).

According to Kuhn, scientific development cannot be considered as a uniform process of accretion of knowledge, but a discontinuous change in the vision of the world (the Austrian physicist Ludwig Boltzmann proposed the same concept over half a century before Kuhn). In its mature stage, science proceeds by virtue of its dogmatism, anticipating the results you want and then forcing nature to agree with them. The scientific paradigm is a binding agreement that requires the exclusive.

The most sharp critic of this approach was Paul Feyerabend (Stephen Toulmin wrote that "the distinction between normal and revolutionary science cannot be drawn as sharply as Kuhn believes". S. Toulmin, *Conceptual Revolution in Science*, Synthese, XVII, 1, March 1967). He directed the following critic at Kuhn: "You [Kuhn] use a monolithic ideology in the 'dress' of a story. You are a mystic, an irrational. You are as a sorcerer, Tom, you're immoral".

Feyerabend, concerned about the political implications inherent in the dogmatism of normal science, which misses the importance of competition between research programmes, considered a state of a permanent crisis, leading to a confrontation and the replacement of paradigms, as highly desirable. In Feyerabend's words, "there is no scientific method. Scientists should follow their subjective wishes" (in Chalmers, *What is this thing called Science?*, University of Queensland Press, Hackett 1999).

Moreover, he noted that there is the «suspicion that normal or "mature" science as described by Kuhn is not even a historical fact» (I. Lakatos, A. Musgrave, *Criticism and the Growth of Knowledge*, Cambridge University Press, Cambridge 1970).

An interesting aspect in the controversy between Kuhn and Lakatos is related to the concept of "incommensurability". Kuhn, opposing Popper's point of view, pointed out that the birth of a new paradigm requires a change to be interpreted as a "rupture" because without that break no new order of ideas, no new necessary mindset enters. The new paradigm is incommensurable (the

influence of Wittgenstein should be noted here), namely without communication with who is not "converted" to the new "path".

Lakatos fully rejected the claim of the incommensurability[2]. He insisted that we can objectively compare the relative progress of traditions in competition one with the other: "The history of science has been and should be a history of competition between research programs" (Lakatos, *La falsificazione e la metodologia dei programmi di ricerca*, in I. Lakatos, A. Musgrave, *Critica e crescita della conoscenza*, Feltrinelli, Milano 1976). As noted by Alan Chalmers:

> Kuhn stressed the extent to which workers in rival paradigms "live in different worlds" to such a degree that he left himself with inadequate resources for elucidating a sense in which a change from one paradigm to another in the course of a scientific revolution is a step forward (Chalmers, *What is this thing called Science?*).

Ultimately, it should be interesting to summarize the main critics attracted by Kuhnian thesis quoting some conclusion written by Jerone Stephens. They are related to the field of politics, but, in many ways, they can be extended to many other fields of interest:

> The original formulation of the Kuhnian paradigm contributes very little to the study of politics. About the only result that has occurred is that we now have another way of characterizing a formulation added to our vocabulary but since the criteria originally proposed by Kuhn for distinguishing between inadequate and adequate formulations cannot be sustained, we are back where we started before the Kuhnian paradigm was adopted by political science: we still need criteria for distinguishing political science formulations from each other in order that we can abandon the useless ones and retain those that aid us in the study of politics (J. Stephens, *The Kuhnian Paradigm and Political Inquiry: An Appraisal*, American Journal of Political Science, August 1973).

[2] Kuhn was also accused of relativism as it postulated that the fact that science is the result of a consensus, rather than objective criteria. He, however, denied that he aimed to give a relativistic account of science.