

# Cave! Alius Draco: There Was a Sixth Dragon!

Written by Ronen Palan

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RONEN PALAN, SEP 21 2012

I was asked by e-IR's editors to represent, to the extent that I can, a contemporary take on regime theory from the perspective of the late professor Susan Strange. Susan had few kind words to say about regime theory. I cannot claim to have privilege knowledge of Susan's views thirty years on, but if I were to hazard a guess, I would say that she would not have mellowed her original criticism one bit. Interestingly, her contribution to the famous volume proved to be her best cited article. She expressed very well, with great clarity, succinctly yet comprehensively, the five dragons that have bothered, then, and ever since, the growing phalanges of skeptics of regime theory.

Susan's article was not a simple critique of certain failings of a new theory. I believe she chose her title carefully. Her message was intended as a warning to her American colleagues and friends: You are entering the land of dragons, she felt, don't go there! Thirty years on it seems to me that there is a sixth dragon, a late bloomer who remained cocooned in the early 1980s in its dragon egg. Once hatched, it proved to be most significant dragon of all: It was a dragon named *the triumph of form over substance* – and it was the one that Susan was probably most worried about – although she did not refer to it explicitly. The problem that I have with regime theory, then and now, is not that it is a bad tool of theoretical investigation. On the contrary, it was a spectacularly successful tool of the wrong type of investigation

The core question that regime theory sought to answer – which had become subsequently the core 'problematic' of the so-called liberal tradition of IR and IPE scholarship, were the determinants of cooperation among states in the international sphere. The idea then, as it is now, was to try and identify some general rules that reveal the innermost dynamics of inter-state cooperation. Leaving aside the inherent folly of such enterprise, particularly puzzling to me is the reason a very talented group of scholars that Jerry Cohen (2008) dubbed the American school of IPE have opted, collectively, to dedicate so much of their time and intellectual efforts to questions of form: Why do states cooperate, and under what conditions, and correspondingly, dedicate less time and effort on questions of substance, whether or not the regimes are any good? And if not, how can they be replaced by better ones. Neutral observers would probably think that it should have been the other way around: international relations theory should have been about the content of the cooperative (or not) transnational relationships, with only a few nerdish specialists concerned with the determinants of cooperation.

Let me present an example. It could be argued that the international financial architecture pre-2007 was a typical regime in Krasner's sense. It consisted of "institutions possessing norms, decision rules, and procedures which facilitate a convergence of expectations"(see Krasner 1982). Fine. But when a crisis of global proportion such as the one that we are experiencing these days erupts, how many scholars or policy-makers have turned to regime theory, or indeed, to the so-called liberal theory of 'preference formation' for an answer — any answer? When the crisis became visible around August 2007 people wanted to know what the causes of the crisis were. Were they imminent to the financial system itself, to laxity of regulation and oversight, or were they exogenous to the financial system. People were keen to know what to do about the crisis. They also wanted to know what sort of regulations were needed to avert future crises? And once (and if) principles of regulations were agreed upon, how to get from A to B: How to set up a new international financial regime. But does regime theory provides answers, or at the very least a useful analytical framework, that may help address any of the above questions? I do not think so. The theory has no analytical content; it is about form, it is not about substance. Would Susan Strange have anything to say about any of the above questions? I believe she would have been writing frantically these days, but she would not have turned to

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regime theory for help or guidance.

I am not the only one who feels that the current global economic crisis is one among three that humanity is facing during this global age, and possibly the least dangerous and the more manageable among the three. The two others are the ecological crisis and the impending demographic crisis. They are all related. All three would require the setting up of a complex but flexible set of international regimes that could handle such vast challenges. I might be a bit harsh in my judgement, but I cannot see how regime theory can provide possible answers or guidelines to handle such challenges. Regime theory belongs to a genre of academic enterprise that, in the words of Bourdieu and colleagues, are '[d]estined to dazzle rather than to enlighten' (Bourdieu, Passeron and De Saint Martin 1965), 3). As such it must count as a great success – otherwise, we would not devote time discussing it thirty years on. But it does not really help when it comes to the serious challenges the world is facing today.

How and why has form overshadowed substance in contemporary international relations scholarship? How did we get from there to here? My own take on these changes is founded no doubt on somewhat amateurish forensic analysis, and a brief one to boot. I view regime theory in the context of three important, if somewhat parochial conversations, that combined help turn a discipline that was supposed to be practical and policy-oriented into a very academic pursuit in Bourdieu's sense of the word.

## **Three Conversations and the Rise of Regime Theory, or How A Good Question Turns Sour**

The modern discipline of International Relations emerged during the mid 20<sup>th</sup> century, in the shadow of, and some would argue, in response to the advent of a nuclear holocaust. There was, of course, a rich tradition or even traditions of thought that resonate with modern international relations themes and discussions; but before the Second World War, there were only few Chairs and positions in a discipline called International Relations, and they were relatively new then as well. Von Treitschke, one of the forefathers of the theory of *realpolitik*, translated later as 'political realism,' called his famous two-volume study quite simply, *Politics* (Treitschke 1916). Still one of the best expositions of what today we call 'realism', Heinrich Meinecke's study of Machiavellism made no reference to international relations as such (Meinecke 1962). It was left to German refugees like Hans Morgenthau (Morgenthau 1967), Georg Schwarzenegger (Schwarzenberger 1949) or Henri Kissinger (Smith 1986) to develop these ideas and adapt them to the new conditions of post WWII cold war.

Having convinced themselves that an 'anarchical' state system is extremely violent and dangerous, survivors of the Second World War saw danger everywhere: "War lurks in the background of international politics" so wrote E.H. Carr (Carr 1951, 109). International relations theorists had a duty to develop the, 'scientific' study of violence, a 'science of peace and war' wrote Raymond Aron (Aron, 1966, 6). There was a need for a scientific approach to war and violence, a science that may help humanity avoid the cataclysmic catastrophe that will unleash in the age of nuclear weapons. The new science was concerned less with the content of the argument 'among states' (as if states or nations are capable of arguing with each other) as war as policy by other means was no longer a rational or viable option. The study of peace and war in nuclear age moved away, therefore, and justifiably in my view, from question of substance, what the argument might be about, to the question of the determinants of decision-making process. Put simply, it was as important to know under what conditions governments, policy-makers or tyrants may end up persuading themselves that it was worth their while to push that dreaded nuclear button, as much as why they may feel entitled or forced to do so. Good theories of individual and collective decision making processes, including, group psychology, and bureaucratic decision-making were highly sought after, and for a good reason.

The second and important stage in the development of modern international relations theory is traceable to late 1960s and early 1970s. The prospects for nuclear holocaust were receding somewhat by that time, giving way to economic problems such as the oil crisis, the collapse of the Bretton Wood agreement, stagnation and inflation, or stagflation, and so on. International Relations scholars began to pay greater attention to the economy, and to what Bruce Russett called, economic thinking (Russett 1968) as well. Famously, the subject of standard economics that Russett refers to inhabits a world of scarcity. When confronted by scarcities, s/he has to be make choices which requires evaluation of utility, worth and (some would add, at least in the original formulation of Malthus, morality as well). Standard economics adopted the principle of marginalism as an analytical procedure to help simulate the

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calculative rationality of individual choice. In the words of Russett, 'Economic theory is at heart a set of assumptions and deductions about rational choice on the part of individuals and organizations. It is relevant whenever actors have determinate goals but limited means of achieving those goals, and hence must allocate scarce resources' (Russett 1968, 5).

The reader may notice a minor slippage in Russett's description of economic thinking: he overlooks the famous cross-over problem between micro and macro economics. Standard economics had acknowledged the problem it has had in combining micro-economics, the analysis of individuals' decision-making processes, with macroeconomics, the economics of organisation and of the economic environment. In fact, micro-economic foundations are applicable only to individuals and not to organisations. Not surprisingly, some very eminent economists expressed their scepticism about the use of economic thinking in international relations (Eichengreen 1998; Kindleberger 1981) to no avail. Helen Milner (Milner 1998) and Robert Putnam (Putnam 1988), among others, argue correctly, in my view, that rationalist theories of decision-making processes are about individuals, and can best be applied to the behaviour of politicians in the study of international relations theory, and not as theories of complex organisations such as states.

Superficially, there appeared to be some good synergies between marginalism in economics, and the decision-making studies of strategic studies. Marginalism appears to offer a useful methodology that could place international relations on surer and more scientific footing. Marginalism introduced discrete mathematics formulations, graphs, and some basic probabilistic methods that could impress decision makers. Marginalism and neoclassical are known otherwise as psychological theories (Commons 1959 [1924]), and hence they could introduce various techniques from psychology, such as game theory, herd behavior theories and possibly experimental economics to the decision-making process in international affairs as well. It was the genius of Kenneth Waltz to combine marginalism with an entirely incompatible theory, Durkheim's theory of social structures, to produce the theoretical foundations of neo-realism (Waltz 1990).

Standard economics is a non-conflictual theory. *Homo economicus* inhabits the world of scarcities, but standard economics is not particularly concerned with the political implications of conflict. It is concerned with coordination problems of complex economies, and the transmission of information about needs and desires of consumers to producers and service providers, and vice versa. Markets therefore, are seen as information systems: free flow of information about individual's needs and desires for material goods is likely to yield better and more efficient allocation of resources. The key aim of standard economics was the achievement of Pareto optimality in the allocation of scarce societal resources. Stage two in the development of modern international relations theory too advantage, then, of the justifiable concerns of strategic studies with the decision-making processes to introduce theories of decision-making in other spheres of international relations. We end up, as a result, with theories that are supposed to simulate efficient allocation of material resources applied, now, to a different sphere altogether: a conflict-ridden sphere, yet performing their tasks in complete abstraction.

## Stage Three: Enter Institutionalism

Russett was a bit economical with his thinking about economics. He acknowledged only one strand of school of economics which he took to be the exclusive representative of economic thinking. As it happens, it was a school already in decline at the time. Economic theory of organisations was pulling at that time already in the direction of what became known as New Institutional Economics (NIE). This had become the third stage in the conversation that gave rise to regime theory.

NIE is associated primarily with the works of Ronald Coase, Oliver Williamson and Douglas North. NIE is methodological individualistic in orientation, with strong connections to standard economics. Founded on the seminal work of Ronald Coase (Coase 1937), NIE introduced transaction cost analysis to explain the persistence of bureaucratic organisations such as large firms in market conditions. But as Richard Phillips notes, that original case study that Coase learned and generalised upon, General Motors reasons for acquiring the Fisher Body Company, was patently wrong (Phillips 2013). Nevertheless, transaction cost economics survived and became a typical strategy of expansion from a methodological individualist core in economics to include an ever widening web of social institutions. The idea was simple but compelling, institutional analysis is required to understand the various costs that

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the simple Walrasian bid model ignored. Markets do not exist in isolation; indeed, markets alone do not function very well at all. Markets are embedded in social institutions.

It is interesting to note, as an aside, Barry Buzan's (Buzan 1993) thesis that that methodological individualist theories of the state system (or neorealism) are entirely consistent with, and in fact, assume the embeddedness of the 'system of states' in broader cultural habitus. Buzan follows exactly the same logical steps that have led standard economics in the direction of NIE. But as far as I can tell, he pursues this line of argumentation independently of the development in economic theory at the time. Only that the cultural habitus that is described by the 'English school' as an 'international society' is, on the one hand, incredibly limited in conception, centred as it were on diplomatic relations between states (as if they were unitary actors), and on the other hand, contains unsupported normative biases about the existence of some 'community of states'.

Institutions are viewed broadly in NIE literature as shared cognitive belief systems of interacting individuals. Social institutions are considered, in Douglas North's famous definition, as 'the rules of the game in a society or, more formally, are the humanly devised constraints that shape human interaction' (North 1990, 3). The rules that North has in mind are often 'politically determined rules that are imposed "top down" on economic agents by the polity' (Grief 2006, 8). Transaction cost analysis postulated that certain institutions could increase the efficiency of market performance. Although it is worth bearing in mind that the efficiency postulate was relaxed, however, in later studies. Douglas North writes for instance, 'In Structure and Change in Economic History' (1983): 'I abandoned the efficiency view of institutions. Rulers devised property rights in their own interests and transaction costs resulted in typically inefficient property rights prevailing. As a result it was possible to account for the widespread existence of property rights throughout history and in the present that did not produce economic growth' (1983, 6). Institutions survive because they provide anchor of stability in an otherwise unstable world. In fact, any observable form of social stability, or any social order and patterning, is ipso facto product of an institution.

John Williamson argues that the wider sphere of neoclassical markets can be divided into four spheres: L1: embeddedness, which consists of informal institutions, customs, traditions, norms and religion, L2 institutional environment: formal rules of the game –esp. Property, L3 Governance: play of the game and especially contracts, and L4 resource allocation and employment (Williamson 2000). Now, Williamson believes that not all levels exhibit a degree of regularities and patterning, and hence institutionalist theory is ultimately limited. But the trends in NIE was to build up from L4 towards L1, although the road is long and winding.

The broad context of the development of NIE theory sheds light on the development of regime theory. Robert Keohane, for instance, understood regime theory as an application of transaction cost theory to international institutions. One problem with the analysis was pedagogical. As Martha Finnemore notes: 'Incommensurable definitions mean that despite similarities in labelling, these approaches—all called institutionalist—have little in common' (1996, 325). What Keohane calls, institutions, are considered by institutionalists as organisations, or 'going concerns.' Not surprisingly, perhaps, Keohane make use of transaction cost analysis not as a theory, but by analogy. He simply asserts that international institutions are employed to reduce transaction costs.

It is also important to bear in mind some more parochial institutional considerations that played a role in the development of regime theory as well, namely, that regime theory emerged at a particular time and in a particular place, where and when important conversations were taking place, particularly at Stanford in the departments of Sociology and Political Science. I am referring to the rise of certain variants of historical institutionalism associated with the works of, among others, Richard Scott, John Meyer, Brian Rowan, Paul DiMaggio and Walter Powell.

Richard Scott notes the emergence of a distinct school of institutional theory that 'emphasized that institutionalized belief systems constitute a distinctive class of elements that can account for the existence and/or the elaboration of organizational structure' (Scott 1987, 497).

Scott identifies the main contours of the theory, which are reminiscent of Krasner's regime theory:

1. 'the new formulation stresses the role played by cultural elements-symbols, cognitive systems, normative

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- beliefs-and the sources of such elements’.
2. organizations do not necessarily conform to a set of institutionalized beliefs because they “constitute reality” or are taken for granted, but often because they are rewarded for doing so through increased legitimacy, resources, and survival capabilities
  3. with less attention devoted to process, more can be given to the nature of the belief systems themselves
  4. there is the recognition that, in modern, rationalized societies, the forms and sources of social beliefs and other types of symbolic systems have themselves become more rationalized: folkways and traditions and customs give way to laws, rules, and regulations; and elders’ councils and other forms of traditional authority are replaced by the nation-state (Scott 1987, 497-8).

Meyer and Rowan, in particular, sought clearly to elevate norms to the level of ‘variable’ in political analysis: ‘When norms do play causal roles in theories of bureaucratization’, they complained, ‘it is because they are thought to be built into modern societies and personalities as very general values, which are thought to facilitate formal organization. But norms of rationality are not simply general values. They exist in much more specific and powerful ways in the rules, understandings, and meanings attached to institutionalized social structures’ (Meyer and Rowan 1977, 342).

The parallels with Krasner’s notion of regime are obvious. His regime theory factored in some of Meyer and Rowan’s variables as well to suggest that more informal forms of organisations do exist, but these are not institutions in the sense that North or others refer to. It is possible to understand Krasner’s regime theory also in Meyer and Rowan’s sense, as indication of the powers of informal rules, conventions and norms in shaping state behaviour—in other words, as a constructivist theory.

Seen in this light, Krasner’s edited collection stood at the very juncture of the new development in international political economy – the application of different strands of NIE thought to the field of International relations. Krasner’s regime theory is seen conventionally to have played a critical role in conversation between realists, neorealists and the liberal wing of international relations theory. I view things somewhat differently. To my mind, Krasner’s regime theory stood at an important juncture in the evolution of the ‘American school of IPE’ – between two strands of institutionalism, rationalism and constructivism. It broached and gave us a foretaste of ideas and concepts that would subsequently dominated the discussion in international relations: institutions, cognitive behaviour, norms, rules and so on.

Either way, it begs the question: What is wrong with that? Why should Strange, or myself, object to the introduction of the more sophisticated variants of NIE, or the debates among the various strands of institutionalism, to international scholarship under the guise of regime theory. They seem, if anything, to add to the richness of international relations scholarship.

## From Substance to Form

There is nothing wrong with the application of economics or institutional theories to international relations. But the application of these theories has been flawed, and that has to do with the substance of economic thinking. Standard economic theory is seen today primarily as sophisticated methodology and as a technique of modelling behaviour because there was such a broad consensus about its aims and goals that it is not talked about. It is more or less understood by all economists that the economic system must provide for the material needs of society. The fulfilment of material needs require considerable amounts of planning, forecasting and ideally, efficient mechanisms for allocation of scarce resources. Standard economics is not a critical theory in the sense that it does not seek to challenge the basic institutional structures or patterns of modern society. But it was always a political economy: it provided a theory and rationale for policy making in complex societies, including the argument for limited government intervention in the economy. Last but not least, standard economics is in essence non-conflictual theory. It recognises that resources are scarce but understands economics as a shared enterprise of coordination among different subjects.

Economic theories discuss forms and procedures of decision-making in the context of an implicit substance: they are

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the coordination problems of resource allocation in complex societies. When such theories are transposed from economics to international relations the shared goal that underpins these theories disappears. International relations is not about the provision of the material needs of society, or at the very least, the provision of material needs is only one, among a number of its stated goals. Furthermore, international relations assumes a world of conflict between entities that are territorially bounded. When sets of theories that were designed for one purpose are employed for another, in completely different conditions, something is lost in translation. In international relations, economic theories have become theories about the processes of decision-making that individuals make, applied willy-nilly to organisations, but lacking ultimate rationale for doing so.

The results are theories of form without substance. Regime theories are theories about the coordination problems that states are facing with no particular reasons or cause for coordination besides some vague notion that those states join regimes have a reason for doing so. Regime theory supposedly tells us about the impact of coordination, but has little to say about the substance of the regime as such. The constructivist interpretation of regime theory suffers from similar fate. Meyer and Rowan's theory of norms is a concept that was reworked from pragmatic political philosophy and the Thorstein Veblen's idea of 'habit of thought.' At core, the attachment to norms and habits of thought is not instrumental or cognizant, but operates at another level. In Frederick Will's formulation, norms have manifest aspect and implicit or latent aspects. The latter is crucial to the guiding function of norms (Will and Westphal 1997). Meyer and Rowan assumes the power of such latent aspects in their theory of organisation. But states do not have a latent or implicit aspect to their 'personality'; they do not have a 'subconscious' field that may guide their conscious behaviour. The concept simply does not make sense when applied at state level. The attachment to norms can be explained, then, only instrumentally by reference and by analogy to transaction costs. At the state level, the concept introduces an ambiguity because we can never be sure what the concrete motivations for states are to abide by certain international norms.

The result is a theory that suffers from the five dragons that Strange wrote about. Let me quote:

The five counts (or "dragons" to watch out for) are first, that the study of regimes is, for the most part a fad, one of those shifts of fashion not too difficult to explain as a temporary reaction to events in the real world but in itself making little in the way of a long-term contribution to knowledge. Second, it is imprecise and woolly. Third, it is value-biased, as dangerous as loaded dice. Fourth, it distorts by overemphasizing the static and underemphasizing the dynamic element of change in world politics. And fifth, it is narrow minded, rooted in a state-centric paradigm that limits vision of a wider reality (Strange 1982, 479).

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