The Rise and Decline (and Rise?) of Systemic Theories in International Relations

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1-. International relations (IR) theory has suffered a restructuring among several lines over the past two decades. The gradual but uninterrupted decline of systemic theories - *primus inter pares* in the discipline since the 1970s- is one of those. (1) This decline was accompanied by a rise of those approaches that privilege domestic politics as the place to look for answers. For reasons I will develop below, such an intellectual step was logical, expected, and *partially* appropriate. (2) While the current state of affairs should not be seen as immutable and a systemic comeback is plausible, the truth is that domestic politics, and non-systemic approaches in general, are well entrenched in a semi-hegemonic position. In this essay I will explain the reasons behind the aforementioned shift, assess its consequences, and advance some hypotheses on the future of systemic theories of IR.

2-. Born between the interwar period and the dawn the Cold War world, IR was created with the explicit objective of explaining the causes of war –particularly great wars, understood under the lenses of the two devastating conflicts of the first half of the 20th century. Since then, IR scholars have struggled to respond to the main challenges –or what they perceive as the main challenges- in world politics. (3) This "duty" to explain the world drives theory to follow the patterns of change in international politics, which, as they develop, suggest new *problématiques* and novel ways to approach them. In important ways then –although, as discussed later, this is not the whole picture- (4) a sociology of inquiry is needed to better understand some of the key transformations in IR theory -e.g. the shift from systemic to domestic theories.

Systemic approaches (5) made their meteoric rise under the shelter of K. Waltz's Neorealism. (6) They were created as a tool for a particular time with particular problems. (7) This was a world in which the primary preoccupation was how to manage the bilateral relationship between the United States and the USSR so that it would not en up in World War III. There were certainly other interests in the discipline, but this one outweighed all the rest. A Cold War context made systemic theories very appropriate. Needless to say, the bipolar conflict had been in place a long time before Waltz's path-breaking *Theory of International Politics*. (8) The essential point is, however, that Neorealism proved to be very successful in explaining the basic patterns of interest in this particular period of the history of IR –i.e. dynamics of polarity, relevance of nuclear weapons, consequences of anarchy and its relationship with war and cooperation, *inter alia*- in a more parsimonious and convincing way than the discipline had ever been able to do.

The IR community recognized this "Copernican turn", as Waltz defined it, as progress and systemic approaches were established as mainstream, maybe even as “normal science.” Anyone trying to explain something in international politics had to reckon with the system. This was true for realists (see the work of Gilpin, Walt, and Grieco) but also for scholars with a line of inquiry that differed substantially from Waltz’s (see Keohane’s *Cooperation after Hegemony* for a good example).

3-. A dramatic event that shakes the bases of an academic discipline is sometimes needed to motivate scholars to devise new lines of inquiry and surpass research programs that appear to be losing heuristic power. This is what the fall of the Soviet Union did with Neorealism, and systemic approaches in general. (9)

Structural realism was in many ways, and problematically so, a theory *for* the Cold War. Its discussion on nuclear weapons, bipolarity, uncertainty, and superpower dynamics seemed to be too tied to a specific historical context. (10) The inability of neorealism, or any other systemic theory for that matter, to foresee –or even explain- the disappearance of the bipolar world –a systemic change *par excellence*-supposed a hard blow to its appeal. (11) Both the fall of the USSR and the subsequent appearance (or uncovering, once the Cold War veil was lifted) of new “themes” in international politics -IPE, civil wars, the role of leaders, the democratic peace, *inter alia*- opened a fertile camp over which to argue for the need to “go beyond systemic theory.” (12)

I argued supra that this was an appropriate move (or partially appropriate). But the reasons implicitly inferred up to know -failure in predicting events and a crisis in the IR community (in a Kuhnian sense)- cannot support this claim. The other face of the coin is that the thorough self-examination of the 1990s also responded to internal problems of systemic theories as research programs. For example, in the 1980s the discipline was stuck in the mud of absolute vs. relative gains debate, a degenerative discussion from a Lakatosian perspective. (13) Visible problems of heuristic power were calling for a *partial* move beyond the system. This was the real cause for the shift, and the best argument to characterize it as “appropriate”. The exogenous shock (fall of the USSR) had the role, not at all minor, of opening a window of opportunity for dissenting scholars.

Helen Milner was one of the most eloquent advocates for this turn. Her argument, in short, was that “systemic theory simply cannot take us far enough” (Milner, 1992). The assumption that anarchy was the principal variable defining states preferences and the primacy of a straight causal line from the system to the state and then to policy-making was excessively simplistic, Milner argued. How could the discipline solve this quagmire? By studying domestic politics to understand states’ preferences and, consequently, the differing patterns of conflict and cooperation in international politics. (14) As Milner contended: “…cooperation may be unattainable because of domestic intransigence, and not because of the international system.” (15)

A reaction against systemic theories was not exclusive to the liberal trenches. Following this turn toward domestic politics, some realist scholars directed their efforts at the incorporation of domestic variables as a way to add complexity to systemic models that they saw as too crude. In his *From Wealth to Power,* F.

Zakaria argued that anarchy and the distribution of power were not enough to explain the behavior of rising powers. After observing that at the end of the 19th century the US was not as assertive as a structural approach would have predicted, he hypothesized that this was because it did not have the governmental capacity to do so. To solve this puzzle he argued for the incorporation of models of resource extraction and governmental capability to try to get through the Neorealist corset. This was an important intra-realist challenge to a somewhat ossified systemic realism. (16)

The rise of domestic approaches represented a generalized discontentment with the excessive importance given to parsimony and the inflexibility that came with it. Parsimony, which should be no more than a tool in theory building, was placed as a goal in itself, restricting research in a way that went against the discipline's own progress. Those boundaries had to be overcome if we wanted to say something about some of the important issues left unstudied by a focus on the system. Once again, the Cold War world with its apparently clear strategic problems may have seemed more propitious to a highly parsimonious approach to theory building. In a post Cold War world, the costs of parsimony were too heavy. Domestic theories certainly lost in parsimony, but they gained in a more real approach to IR problématiques. This was the primary rationale behind the turn here discussed, and in this limited sense, the shift was appropriate. (17)

4-. It would be nice to unambiguously assert that the fall of systemic theories made IR a coherent and progressive discipline. This, unfortunately, is not the case. The past two decades have seen the formation of a different *ethos* of theory building and discipline development that may end up doing more harm than good to our broader understanding of international politics.

Something not mentioned up to now is the ascent of quantitative and strategic-choice approaches in the discipline. Quantitative approaches gained prominence by the same time that, and related to, domestic theories were supplanting systemic theories. (18) Strategic choice and game theory, following developments in other academic areas -especially economics-, also gained importance in the 1990s under the idea of formalizing theories and going beyond the "isms." There is nothing wrong with these approaches *per se*. Quantitative work has been very important in the empirical development of IR - maybe too neglected in the past. Formal theory, on the other hand, is a powerful and clear tool to build and evaluate theories while avoiding problems of underspecification all too common in the discipline –though, this is only true if one can get through its assumptions. (19)

The problems of this new "methodological bets" are to be found in the costs for the general development of the discipline. The most pressing are the ones related to the idea that theory construction should be a bottom to top affair, and the implicit notion that by building the parts individually we will eventually end up in a progressive accumulation of theoretical knowledge. However, this epistemological decision may well result in the proliferation of particularistic theories of problems ever more sophisticatedly studied, increasingly particular and micro, and *in crescendo* uninteresting. (20) By depending on a kind of magical automatic accumulation of theoretical knowledge we are risking to end up with an even more chaotic and incoherent discipline (more on this in the conclusion).

5-. As said in the introduction, the fall of grace of systemic theories cannot be taken as an irreversible given; it is possible to devise some scenarios in which systemic approaches could make a comeback.

The first one is linked to the relationship between theory and History discussed earlier. The post Cold War world, particularly the 1990s, was a strange period for the discipline. The study of IR has historically dealt with great power politics as its core. The "curious" 1990s came with a certain absence of great power politics, especially due to the overwhelming power position of the US. This goes a long way in explaining the growing emphasis on domestic politics, civil wars, international organizations, *inter alia*, during those years. A partial return of classical great power politics (or the perception of it) -for example under the banner of the rise of China and some other middle powers- might motivate a recasting of systemic theories -particularly for those wanting to study polarity (a *passé* topic in the unipolar 1990s), (21) systemic change and its consequences, etc. (22)

Another plausible scenario would be the success of some of the ongoing projects to make systemic theories more sophisticated and comprehensive by, for example, incorporating domestic variables. A good example is "Neo-classical Realism" (see fn. 16). This research project proceeds from a systemic assumption of the influences of the system (that is, a neorealist basis) but incorporates domestic politics as an intervening variable between systemic pressures and decision-making. Though a rather interesting *proto*-school, Neoclassical Realism is still in its infant stages and has yet to produce work of remarkable characteristics.

Lastly, domestic politics, as should have been expected, were not the panacea for the development of IR theory. There might well be a social exhaustion with the results of domestic and micro-theory –a Kuhnian crisis analogous to the one that discredited systemic theories. This may eventually take IR on unexpected paths.

Nevertheless, if measured by academic output and *Geist*, predicting a comeback of systemic approaches seems a risky bet. The discipline appears to be quite comfortable with increasing its empirical production, formalizing theories towards an *Icarian* "scientism", and avoiding, at its own peril, a "wholist" view of international politics.

6-. Going beyond systemic theories –not in the sense of vanishing them, but of relaxing some of their strictures, increasing their sophistication, and trying new approaches- was the necessary thing to do for a methodology that was unable to cope with many of the relevant problems in IR. The turn to domestic and particularistic perspectives brought much needed renovation, indeed. However, the excesses incurred by systemic theorists as a result of an obsession with parsimony and structural effects may now seem analogous (although for the opposite reasons) to a fixation with the particular and micro-level studies in contemporary IR theory. A blind push to obtain ever more data of increasingly micro phenomena puts at risk what we can say about international relations in general. We may, for example, be more much prepared to sophisticatedly answer why a *specific* insurgent group responded in a *specific* way to the level of aggression of a *specific* state, (23) but we may also be losing our interest and capacity to think about the nature of conflict in its most elemental condition. The stakes are too high for the IR community to avoid an honest discussion on how far we are willing to continue on this path.

(1) This essay works with the assumption of a relative decline of systemic apporaches. To argue that they have vanished would be utterly incorrect. For a convincing argument on the inevitability of structural constraints see Jervis's*System Effects*.

(2) Although a change may be welcomed, the results are not always as encouraging as expected (more on this qualification of "appropriate" later).

(3) This does not mean, of course, that there is an exclusive focus on policy or immediacy, It means that in its most basic essence, the idea of the discipline is to be able to provide some answers to the pressing problems in the international system. To give an example, few people would be interested in studying the prospects of war between France and Germany in the 21st century *per se* – though it surely is studied as a historical case that can shed light on other issues-, while this was one of the main topics in the nascent IR discipline.

(4) Social science does not progress only by exogenous shocks, but also for endogenous reasons that cannot be explained by what happens outside theoretical disscusions.

(5) Understood simply as those that privilege the influence of the structure over the behavior of the units.

(6) This type of theories certainly were not born with Waltz; systemic is a much broader category than Neorealism. The important point is that Waltz devised the more convincing type of systemic theory. For simplicity, Waltz' Neorelism will be used here as the epitome and a kind of proxy for systemic theory.

(7) It must be said that the rise of systemic theories also responded to changes in the social sciences in general; for example, the influence of structuralist anthorpoligist Levi-Strauss' work, which Waltz knew well.

(8) Theories of IR before Waltz hosted a diverse group of analysts: Classical realism from the hand of a Hans Morgenthau, Geroge Kennan and Raymond Aron; liberal approaches from a Stanley Hoffman, Robert Keohane and Joseph Nye; Bureacratic Organization and foreign policy from a Graham Allison; and a long *et cetera*.

(9) See R. N. Lebow, "The Long Peace, the End of the Cold War and the Failure of Realism."

(10) See I. Oren's *Our Enemies and US: America´s Rivalries and the Making of Political Science*.

(11) As with its rise the decline of systemic theories was also linked to broader transformations in the world of ideas, to which IR seems to always be a latecomer. From a broad perspective, this phenomenon had started in the 1960s with the work of Foucault, Derrida, Geertz and others.

(12) The end of the immediate preoccupation with bipolarity also gave the opportunity to rethink some long-term historical problems of Neorealism (see Schroeder 1994).

(13) Some of the scholars engaged in this deabate were: Keohane, Grieco, Axelrod, and Mastanduno; cf. Milner (1992).

(14) In another article in *International Organization* (1987) she argues that to understand the way in which states make decisions in the international economy it is not enough to look at anarchy. Her model studies the type of economic links between countries (high or low interdependence) and the influence of interests groups that may pressure the state to make particular decisions; these policy outcomes would have been incomprehensible from a systemic/anarchic stance. According to Milner, there is an important dynamic of preference construction and strategies adopted that are to be found in domestic politics.

(15) See also Putnam (1988) for an interesting effort to move beyond lists of domestic factors and towards a coherent two level theory.

(16) This line of research has been given the title of Neoclassical Realism (see G. Rose 1998). See the work of R. Schweller, J. Taliaferro, A. Friedberg, and T. Christensen.

(17) Systemic theories were also attached to what has been discussed as the "paradigm wars" between realism, liberalism, constructivism, etc. The turn away from them can also be given credit for helping to discredit this unproductive way of theorizing.

(18) This trend was tied to the notoriety of the "democratic peace" project that was, and still is, an empirical enterprise at its core. See Russett and Oneal (1999); cf. Gartzke (2007).

(19) See Wagner, *War and the State,* and Lake and Powell *Strategic Choice and International Relations*.

(20) This is not the nature of all the work in this approach, of course, but just a possible trend of the school as a whole. See Walt's "Rigor or Rigor Mortis" for a sharp, but not always convincing, critique.

(21) For an exception see the work by N. Monteiro on unipolarity. This does not mean that polarity disappeared from the IR map, but it was certainly shrinked as a research question.

(22) Some young scholars on this line of research are: P. MacDonald, J. Parent, D. Kliman and M. Beckley.

(23) See Jason Lyall's "Does Indiscriminate Violence Incite Insurgent Attacks? Evidence from Chechnya" To be fair, Lyall's work attempts to generalize from this specific case –how convincing he is not very clear, however.

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