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R.I.S. interview with Jim Rosenau[1]

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R.I.S. Jim, can we begin by asking you to sketch for us how your personal background—your early experiences and education—influenced your research and writing, if at all?

J.R. You know, I'm not sure. On the one hand, I strongly believe that our scholarly work is a product of who we are, of our values and temperaments, but at the same time I have a hard time tracing a link between my early years to the intellectual orientations that mark my subsequent work. No one in my immediate or extended family was an academic. My father and brother were businessmen, my mother and sister housewives. It was a close-knit family, but ideas and curiosity were not the glue that held it together. In fact, my father wanted me to go into business and was not happy when I chose an academic career.

But despite the conventionality of my upbringing—we were quite middle class, perhaps even upper middle class—I had a special sort of schooling. Then it was called 'progressive education'. I spent ten years at the Lincoln School of Teacher's College of Columbia, a school established by the Rockefellers to enable John Dewey to implement his theories. Upon graduation I spent a year at the University of Wisconsin and might have flunked out had I not been drafted in 1943. Then, after three years in the army spent mostly in England, I enrolled in Bard College, a small college above Poughkeepsie on the Hudson River, which was organized around similar notions of education as the Lincoln School. The fact that I never took an exam until those qualifying for the Ph.D. at Princeton years later tells you something about how different my high school and college education was, how it stressed creative thinking rather than passing exams. Perhaps that has something to do with why my work today is marked more by conceptual formulations than by disciplined inquiry. Yet, this connection feels less experiential and more like an after-the-fact interpretation. Indeed, I was not a serious student until I got to Bard after the war. In high school I was the class athlete and at our fiftieth reunion one of my classmates remarked 'Who could have ever guessed that Jim Rosenau would become an intellectual!'

R.I.S. Is that why you wrote, in one of your early essays, that the external world had little impact upon your thinking? Why then did you drift into International Relations as opposed to the humanities or other social sciences?

J.R. Those are big questions. Again I'm not sure. I majored in history at college but more through a process of elimination than any driving concern about the human condition. Those were special years in higher education. All of us were

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driven to make up for the time we spent in the military, as if graduation from college would allow us to begin to think about what we wanted to do in life.

R.I.S. Did you not have a sense that what was driving you on was the kind of 'never again' spirit that brought people into international relations during and after the First World War?

J.R. That makes sense, I suppose, but again only as a retrospective interpretation and not what I recall as a conscious motivation. I had vague notions of making the world a better place, a kind of naive idealism that led me to organize the first International Weekend at Bard, a gathering of foreign students from all over the East that subsequently became an annual event, even a tradition, at the College. It was also an idealism, if that is the right word, that led me to enroll in 1948 in the School of Advanced International Studies (SAIS) in Washington DC (before, incidentally, it became affiliated with Johns Hopkins University and was replaced by a fancy building on Massachusetts Avenue). Still I can't say that at the time I knew who I was or where I was headed even as I then went on to get a Ph.D. at Princeton. In retrospect it all seems like a meandering, or perhaps a restless search, which didn't come into focus until after I had drifted into teaching.

R.I.S. How much did luck play a part in shaping your career? You had the privilege of working with Eleanor Roosevelt and then Dean Acheson. Could you tell us about these experiences?

J.R. At Bard they had what was called a 'field period', in which students had to find a job in the 'real' world for six weeks during the winter. Since Bard was twenty miles up the Hudson from Hyde Park, in January 1948 I started working as an editor of Franklin Roosevelt's personal letters (the ones that belonged to the family and not the Presidential Library). I lived on a farm across from Mrs Roosevelt's home and stayed on for nine months while also commuting to the College. I was the first person to take FDR's letters out of the boxes in which his wife and mother had collected them. My job was to identify the persons mentioned in the letters and write the connective tissue that pulled the letters together into a coherent story. The result was my first publication, an edited book, *F.D.R.—His Personal Letters, 1905–1928.*[2] I made at least one huge error in the book. In 1917, when he was in England, FDR wrote a letter in which he mentioned running into Winston Churchill. What an irony and what a forecast, I thought (and wrote): here were the two World War II leaders starting their relationship in 1917. When the book came out a reviewer noted that I had made up a story, that there were two Winston Churchills at the time, that FDR had met the one who was a famous novelist!

As for Dean Acheson, a couple of years later I was writing my Princeton dissertation about him as a target of Congressional hostility when the Politics Department was asked to provide a graduate student to assist him in preparing for monthly meetings with those with whom he had closely worked during his years as Secretary of State. The meetings were sponsored by J. Robert Oppenheimer, then the director of the Institute for Advanced Study, who had met Acheson on a vacation and, I gathered, found him despondent over his treatment at the hands of Joe McCarthy and others in the Senate. Oppenheimer sought to help him regain perspective by convening the monthly meetings. There were some ten people at the weekend sessions in Princeton, including George Kennan, Dean Rusk, Averill Harriman,

[2] New York: Duell Sloan & Pearce, 1949.

McGeorge Bundy, and Herbert Feis. Their deliberations turned out to be the basis for Acheson's book *Present at the Creation*, a title that I later used to describe the experience as one in which I was present at the creation of *Present at the Creation*.

R.I.S. What were your judgements of 'the great and the good' who you came into contact with?

J.R. I was twenty-three years old and impressed that here I was, little Jimmy Rosenau, associating with some distinguished people. So I was bound to think highly of them, but that was not hard to do, as Mrs. Roosevelt and Mr. Acheson were very special and extremely good to me. I could talk about each of them at length, but I'm not sure how relevant it would be to our purpose here.

R.I.S. Given your contact with Eleanor Roosevelt, does it surprise you in retrospect that this did not trigger a more explicitly normative direction in your work?

J.R. No, it doesn't, strange as that may seem. At the time I was still very much under the influence of my father, a good but very conservative man who was one of that small stratum of Americans in the '30s and '40s who could not stand FDR. I can illustrate this with a true story. In order to take the job with Mrs. Roosevelt I had to have a car so that I could eat at local restaurants. But cars were scarce in 1948 and so I had to borrow my father's. That put him in a difficult choice situation: between helping his son *and* the Roosevelts, or depriving me and not helping his nemesis! He resolved it with this comment the morning I took off for Hyde Park: 'The guy died three years ago and now he's taking my car'.

More seriously, at that time I didn't know I was going to be a scholar. I had no sense of myself as an intellectual, as one who could accumulate a corpus of systematic knowledge, much less strong normative commitments. I was a young man with no clear-cut sense of direction. So yes, I did luck into two unusual situations, but to answer your earlier question, I don't feel luck played a part in shaping my career, else I would have become a historian based on those two situations.

R.I.S. Were you planning to be an academic?

J.R. I wanted to teach, or at least this seemed preferable to entering the business world when one of my SAIS classmates told me her father, the Chair of the History and Political Science Department at the New Jersey College for Women (later Douglass College of Rutgers University), had a sudden opening in May of 1949 and asked her if she knew anyone who might want the job. He had had a resignation and was desperate for somebody to fill in. So I went to New Brunswick and got a job on the spot, which I held for twenty-one years. At the outset I was one of two political scientists in the joint Department (the other was Neil McDonald, who became my mentor and who taught me more about political science than I learned at either SAIS or Princeton!). I taught all kinds of courses, bur mainly IR. I've always felt that the social scientist I've become is in some big way a function of having been in a small department and teaching all over the political science map—introductory courses on Africa, the Soviet Union, Plato, American government, and lots else. To the extent my work has been characterized by intellectual breadth; I've long been convinced that the breadth had its roots in the wide scope of my teaching during those early years. That is less likely today because now we recruit people as specialists for narrow, specific, positions.

In the early '50s the academic world as we know it today was just beginning to take shape. It may sound strange, but back then I didn't know much about the

academic profession (for better or worse, I didn't have the benefit of graduate student scuttlebutt since I only went to Princeton for classes while teaching in New Brunswick). I learned about the profession on the job, mostly by waiting eleven years before being tenured. I probably set a record in our profession in this regard: for five of those years I was an Instructor and an Assistant Professor for the other six. For much of that time I didn't have a notion of knowledge building as an enterprise; I only knew I wanted to help young minds come alive. So basically I was a teacher first and a scholar second, and now I think of myself as a scholar *and* a teacher. I see the two as intimately related. The old saying that one has to publish or perish is misleading. I'd say one has to communicate or perish!

I taught my first IR course in the fall of 1949—on the day that Harry Truman announced the Soviets had detonated an atomic weapon—and I've been teaching ever since. Last year I completed my fiftieth year on a university faculty!

R.I.S. Can we move on to consider some of your academic influences? We'd like to tease out some of the key sources of your early work.

J.R. Dick Snyder's work on foreigh policy decision-making may have been the prime source. He was teaching in the Princeton Department when I started there in 1950 and his seminar was an eye-opener! It was broadly social scientific in the topics covered, from the impact of culture to the nature of societal institutions, and among his foci was organizational behaviour. Snyder argued that foreign policies were the product not only of external and domestic forces, but also of the norms and practices of the policymaking organization itself. He later put this perspective into writing in what turned out to be a seminal work in the field. It may seem self-evident today, but it was pioneering when it first come out in 1954.[3]

R.I.S. What about the 1954 article of Snyder's which broke with what had gone before?

J.R. It enabled analysts to break with conventional notions of power as the bases of foreign policy, and instead to tease out the underlying dynamics by reconstructing the world as it is perceived, experienced, and evaluated by those who make policy decisions. That was so different from where the IR field was at the time. Where most analysts and teachers followed Morgenthau and Wolfers by treating all states as billiard balls, as impelled by their national interests to react similarly to the same external stimuli, Dick identified multiple sources of behaviour, which in turn led to a stress on the national interest as a subjective rather than an objective assessment. For Snyder the national interest of a country was nothing more than what its duly constituted decision-makers perceived it to be—which meant that states were not billiard balls, that they reacted varyingly to the same stimuli, depending on how their officials defined their interests. All of this may seem obvious today, but in the early '50s it was new, even radical. And it caught on: the transformation was quite stunning. If you look at any of the foreign policy or IR texts from 1954 to the present, the basic premises conform to the Snyderian perspective. More than that, his emphasis on the policymaking organization as a source of a country's foreign conduct anticipated bureaucratic politics by more than a decade.

[3] Richard C. Snyder, Henry W. Bruck, Burton M. Sapin, *Decision-Making as an Approach to the Study of International Politics* (Princeton, NJ: Foreign Policy Analysis Project, 1954), subsequently reprinted in Richard C. Snyder, Henry W. Bruck, and Burton M. Sapin (eds.), *Foreign Policy Decision-Making: An Approach to the Study of International Politics* (New York: Free Press, 1962).

Some years later, I published an article critical of the decision-making approach on the grounds that it had spawned only one major dissertation,[4] a kind of Freudian article in which the son turns on his intellectual father. All these decades later, however, my article seems excessively harsh. It failed to allow for the possibility that enormous intellectual impacts can occur without direct by-products, that a seminal formulation can serve as the basis for many subsequent inquiries without ever being acknowledged as such. I'm convinced that is the case with Snyder's work. Now we treat the processes of decision-making as central variables without being aware of our intellectual indebtedness to him.

R.I.S. Can we extend the story beyond Snyder and talk about your role in the scientific revolution that swept through the American International Relations academy in the late 1950s and 1960s?

J.R. That story has its roots in another person who had a major impact on my intellectual development, my wife Norah. In 1956 she entered a Ph.D. program in psychology at NYU. Her first course focused on the philosophy of science. It was my first systematic introduction to the subject. She would come home every Tuesday night and tell me about these stimulating lectures on issues neither of us had ever encountered. So I learned about the philosophy of science, not so much from Snyder as from Norah. Its rigorous methods for advancing understanding made so much sense. More than that, it was exhilarating, mind-boggling. I'm sure it seems absurd today both to you and your readers—who have been trained in this stuff—but there I was at thirty-three unaware of the existence of a rich literature on the philosophy of science. Out of these Tuesday-night encounters came an aspiration to apply scientific methods to the study of foreign policy. My departure from Morgenthau and classical realism followed quickly thereafter, and so did my joining the ranks of the few behaviouralists in IR.

R.I.S. Did you think that Morgenthau wasn't scientific?

J.R. Most certainly! If you read the first chapter of his text—which he added to its second and subsequent editions as a defence against his critics—Morgenthau argues that realists know why officials do what they do because of the objective order of the social world. No methods of inquiry are set forth. One only needs somehow to grasp the prevailing objective conditions. The theme of an objective reality pervades the chapter, and in no way did he allude to the philosophy of science even though he used the science label to describe his insights. But the behavioural revolution took a radically different stance. It stressed that the way to find out about the world is to gather data systematically, test it against previously generated hypotheses, and then see whether a consensus forms around the meaning of the findings. To the extent consensuses form, they amount to intersubjective rather than objective knowledge. I am still driven by the aspiration to contribute to emerging intersubjective consensus around the questions I investigate.

R.I.S. Did the commonplace objection, that the methods of the natural sciences cannot be applied to the social world, strike you as a legitimate criticism of the behaviouralist project?

J.R. No, that never struck me as a fair criticism. I always felt that while the natural and social worlds differ in key respects, the differences only mean that social

[4] James N. Rosenau, 'The Premises and Promises of Decision-Making Analysis', in J.C. Charlesworth (ed.), *Contemporary Political Analysis* (Free Press, 1967), pp. 189–211. The dissertation was by Glenn D. Paige, later published as *The Korean Decision* (New York: Free Press, 1968).

scientists have to start over more often, since their findings can alter the behaviour of those they study. I still believe that (a belief that is endlessly reinforced by the numerous times those in the physical sciences have had their findings upended and had to start over), albeit my work is no longer as rigorous or quantitative as it once was. At the same time, looking back on my early championing of the scientific method and the behavioural revolution, I can get quite embarrassed by how excessive my enthusiasm now seems. Norah used to talk about how vigorously I rattled the tambourine for science. I can illustrate this with an anecdote reflective of the extremism of the converted. When I moved to Ohio State in 1970, I was assigned an office underneath the University's football stadium and I used to say to anyone who would listen, 'They broke the secret of the atom under the Chicago University stadium and we're going to break the secret of human affairs under the Ohio State stadium!' That tells you a lot about the naivete and depth of my early commitment to the behavioural revolution.

R.I.S. What reactions did the revolution provoke among traditionalists, both in the US and outside?

J.R. You know, there was some annoyance and derision, but mostly I recall our being ignored. US analysts in the foreign affairs field just went about their business, usually oblivious to the methodological implications and flaws on which their inquiries rested. There was more of a reaction in Britain. In fact, Steve [Smith] told me a few years ago that Hedley Bull had me, and not Mort Kaplan, in mind when he wrote a widely cited article condemning the behavioural revolution.[5]

S.S. That's right. Bull told me the story on a train. He wrote his piece in early '65 and based it on a mimeographed copy of your 'pre-theory' article;[6] but since it wasn't published until 1966, he couldn't actually cite it and went after Kaplan instead!

R.I.S. Did you ever cross swords with Bull or any other traditionalist in public?

J.R. I remember one occasion when I was abruptly confronted by Fred Northedge while giving a lecture at LSE. It led to the best response to a question I've ever given in public. Fred had interrupted to say that my presentation of the pre-theory was stupid, and I replied that I had always contended that bad theory was better than no theory and that I was perfectly prepared to amend that to say that stupid theory was better than no theory. I then proceeded to refer frequently to my stupid theory and I had the feeling of growing support on the part of the audience. As for Hedley, we were both at Princeton's Center of International Studies at the time he wrote that anti-behavioural piece, but I recall our getting along fine. I didn't know he had such a negative reaction to my article until Steve told me the story of his train conversation.

R.I.S. When did the scientific method begin to take hold of the mainstream?

J.R. It depends on what one means by the 'mainstream'. I think the publication of David Easton's *The Political System* in 1953 can be regarded as the initial move toward science in the discipline as a whole. That book sensitized a lot of political scientists to the foundations of what they were doing. It might seem egotistic, but I

[5] Hedley Bull, 'International Theory: The Case for a Classical Approach', *World Politics*, Vol. XVIII (April 1966).

[6] James N. Rosenau, 'Pre-Theories and Theories of Foreign Policy', in R.B. Farrell (ed.), *Approaches to Comparative and International Politics* (Evanston: Northwestern University Press, 1966), pp. 27–92.

like to think that in the IR field two large compendia of articles I edited[7] helped reorient the subfield toward the virtues of scientific inquiry, as did my pre-theory and several subsequent articles.[8]

R.I.S. More specifically, can you tell us about the genesis of your 'pre-theory' article?

J.R. My memory is clear on that, partly because it was the one article that Norah got ecstatic about—she described it as important and durable, and it turns out she was right—but also because it originated on a blackboard in a classroom. Having tried to wax eloquent about scientific methods in my introductory IR course for several years subsequent to Norah's NYU seminar, I realized I had to demonstrate more concretely what being scientific about IR might entail. Somehow I decided the size of a country was important, as was the degree of its economic development and the degree of its political accountability, with the result that I put on the blackboard a scheme illustrating the operation of these variables in different types of countries under varying conditions. I did that for a couple of years in the early 1960s. I then received my first invitation to write a paper for a conference at Northwestern. A number of luminaries in the field were also scheduled to give papers and I was so flattered to be in their company that I took the assignment very seriously and decided to organize the paper around the typology I'd been using in class for so long. That history of the article still sticks in my memory because it makes the point that teaching and research are essentially the same—the former involves communicating to known others and the latter to unknown others, but they both consist of sharing ideas, which is why I insist that we have to communicate or perish!

R.I.S. One of the criticisms of the 'pre-theory' article involves its lack of explicitness about the direction of causation? How would you respond to this now?

J.R. I haven't really thought about the article for a long time (though its premise that action stems from the confluence of five clusters of variables located at five levels of analysis continues to underlie my way of thinking about why actors do what they do), but my reaction is to reject the criticism. The article contains a theory, a crude one to be sure, but nonetheless a theory from which hypotheses are derived in terms of how eight types of actors are subjected to different combinations of the five types of variables. The hypotheses were set forth in footnote 45 and they generated three Ph.D. dissertations. I've often cited this reaction to footnote 45 because it was in sharp contrast to a case study I had written a few years earlier that produced neither reactions nor citations.[9] The contrast between three dissertations and an unnoticed case offers a quintessential example of my deep conviction that the name of our game is to sustain the knowledge-building process through provoking others to undertake further work.

R.I.S. After 1966 a whole set of other concerns, new agendas, emerge out of your work. Did you think of yourself as a theoretical entrepreneur, launching new ideas and research programs for others to follow?

J.R. Yes, I did, and I still do. I've always felt good about how my development of the issue-areas concept in the pre-theory article anticipated the literature on

[7] James N. Rosenau (ed.), *International Politics and Foreign Policy: A Reader in Research and Theory* (New York: Free Press, 1961; revised edn., 1969).

[8] These were collected together in James N. Rosenau, *The Scientific Study of Foreign Policy* (New York: Free Press, 1971; revised and enlarged edn., Frances Pinter Publishers, 1980).

[9] James N. Rosenau, *The Nomination of 'Chip' Bohlen* (New York: Henry Holt, 1958).

international regimes and how my work on linkage politics[10] anticipated Keohane and Nye's notion of complex interdependence as well as Kissinger's use of the label (though he conceived of it more in terms of issue than institutional linkages). So I see my task as that of stimulating thought. It doesn't matter much if I'm wrong, but it matters whether others react. Steve Smith and I once clashed on this readiness to be wrong by trying out new concepts. He argued I was responsible for what I wrote, whereas I contended I was not responsible for how others reacted. I remember Steve being very upset on this issue, perhaps because my concept of adaptation had given him headaches when he organized his dissertation around it.

R.I.S. Can we ask you about your work on comparative foreign policy in the mid-1970s? Lurking behind the question is the frank admission in one of your pieces that you felt the CFP (comparative foreign policy) research program was not leading to cumulative knowledge.

J.R. That's a good question. It is true that I've raised questions about how much cumulation the CFP community has accomplished. And it is also the case that my work in the last two decades has dealt only peripherally with foreign policy phenomena and focused more on global issues and their micro sources. So it might be said that I abandoned CFP. That feels wrong. I did not turn *against* the enterprise; rather I like to think I turned *away* from it—not out of boredom or laziness, but partly because my methodological training did not keep up with the strides in the field and partly because I rattled the tambourine so hard I created expectations that exceeded what could be accomplished in just a few years. Had I been a sophisticated methodologist maybe it would have been different, I don't know. But the CFP community continues to flourish and reinvigorate itself, so perhaps my earlier judgment was premature.

R.I.S. Would you call yourself a positivist then, and would you still call yourself a positivist?

J.R. No to both questions. During the behavioural revolution that was not a term used by American political scientists. It is my impression that the notion of positivists and positivism was first brought into IR dialogues by you Brits who in recent years have come to use it as a derogatory term for IR practitioners in the US. Recently I wrote a paper in which I called myself a pre-post positivist[11] partly because it feels descriptive and partly to spoof those who have knee-jerk reactions when confronted with quantitative data and systematic inquiries. In short, I have no need to think of myself as a positivist since that implies that findings are objective. For me it makes sense to view all empirical data and interpretations of them not as generated by value-free inquiries, but as stemming from value-explicit models and frameworks. Inevitably our values shape the way in which we gather and interpret our data.

R.I.S. Given the shared premises of behavioralists you mentioned, it is often unclear—particularly on this side of the Atlantic—to what extent this group was

[10] James N. Rosenau, 'Toward the Study of National-International Linkages', in J.N. Rosenau (ed.), *Linkage Politics: Essays on the Convergence of National and International Systems* (New York: Free Press, 1969), pp. 44–63, and James N. Rosenau, 'Theorizing Across Systems: Linkage Politics Revisited', in J. Wilkenfeld (ed.), *Conflict Behavior and Linkage Politics* (New York: David McKay, 1973), pp. 25–56.

[11] James N. Rosenau, 'Confessions of a Pre-Postmodernist: Or Can an Old-Timer Change Course?' a paper prepared for presentation at the Annual Meeting of the International Studies Association (Minneapolis, 21 March, 1998).

united. Were you conscious of being part of a coherent movement that sought to advance one research agenda?

J.R. Yes, until I moved away from the CFP community, I felt very much part of such a movement. I even proclaimed its existence in an article and two books to which I contributed at the time.[12]

R.I.S. But how about today? Where would you locate yourself in the contemporary IR scene?

J.R. Today the US mainstream remains very state-centric in its approach and I feel way outside it. I find myself more comfortable with those who perceive vast transformations at work in the world. Only a few in my country proceed from this premise, but elsewhere many analysts treat the dynamics of transformation as given. The work going on at Aberystwyth and throughout the UK as well as Europe, Scandinavia, and Asia views the world in this way and strikes me as more imaginative and relevant than the issues preoccupying American IR specialists. It is not surprising, therefore, that I rarely get invited to conferences in the US, but that in the last couple of years I have been invited to comparable occasions all over Europe and in many parts of Latin America and Asia. So I think of myself today as very much an oddball in the profession at home. That's okay. My ego is intact and I'm ready to concede I may be in error in many ways.

My writings in recent years have focused on the emergent epoch and the powerful clashes between globalizing and localizing forces on which it rests. Put differently, a main reason my IR mainstream flows abroad is that it's much more into these clashes. Globalization is not a preoccupation of American IR. I feel strongly that change in the structures and processes of world affairs should be our prime preoccupation, our organizing focus.

R.I.S. Previously you spoke about your early commitment to teaching and science. What do you regard as your mission now? What are your current professional priorities?

J.R. Well, a couple of goals. One is still to alert young people to the idea that human affairs can be studied systematically. Another is to persuade colleagues that it is okay to relax their criteria of parsimony and confront the complexities that sustain world affairs today. And while I no longer rattle the tambourine as I once did, I still see myself as a scientist, as one committed to advancing knowledge through systematic inquiry. This eventually led me, in the later '70s to become more and more of a social scientist and less and less a political scientist. I am now much more comfortable with the literature in sociology and anthropology than that of political science. This shift fortified me to pull apart the state-centrism that is so widely taken for granted. In focusing on the tensions between local and global dynamics I have become ever more sensitive to the complexities that mark our time. And therein lie the links between my earlier and later work: I guess I've always been interested in the boundary-spanning complexities that link the local and the global to each other.

[12] Philip M. Burgess, Charles F. Hermann, and James N. Rosenau, 'The Adaptation of Foreign Policy Research: A Case Study of an Anti-Case Study Project', *International Studies Quarterly*, 17 (March 1973), pp. 119–44; James N. Rosenau (ed.), *Comparing Foreign Policies: Theories, Findings and Methods* (Beverly Hills: Sage Publications, 1974); and James N. Rosenau (ed.), *In Search of Global Patterns* (New York: Free Press, 1976).

And there's a methodological side to this shift. Relaxing my criteria of parsimony led to the creation of my 'anomaly file'—into which I put clippings about events with which my training and experience up through the '70s could not cope. And the anomaly file kept growing, until its overflow provoked me to return to the drawing board and seek to develop a new model of world politics that could handle the seeming anomalies. The result was what I call my turbulence or post-international model, which is developed in what I regard as the best book I ever wrote, *Turbulence in World Politics*.[13] I really believe in that line of argument, and I think my students have found it a useful formulation. But it too is well outside of the American mainstream. A recent edited book on new IR theories[14] made no reference whatever to the turbulence model.

R.I.S. Does that bother you?

J.R. No, as I say, my ego is intact. But it does suggest a crucial dimension of my mission has not caught on. Maybe some day it will.

R.I.S. What books have you read recently that have made you think deeply about such matters?

J.R. Two come immediately to mind, both by anthropologists. One is Appadurai's *Modernity at Large* and the other is Hannerz's *Transnational Connections*.[15] They are both exciting and creative probes into the underpinnings of globalization.

R.I.S. Which journals do you most enjoy receiving through the mail?

J.R. Whichever journals—and I subscribe to a lot—contain innovative articles on the subjects within my spheres of interest. Yours is one of them, but there are many others. Most journals try to appeal to diverse audiences and thus usually have no more than a few articles I find relevant. So one has to scour widely in the literature. Most of the articles I find interesting end up in the syllabus for my course on 'The Dynamics of Globalization', a syllabus that now exceeds 45 pages.

R.I.S. Do you think that the various post-positivist or critical theories have made a significant impact on the way we think about the world?

J.R. The question is puzzling for me. I've never been clear what critical theory is. Is it just being critical? Has it got a substantive paradigm? Or is it only a methodological perspective? Those questions don't seem to both others, so I must miss the key points about such theories. The same is true for my reactions to postmodern formulations. I've never been able to get a good answer to the question of what I should do if I'm on a dissertation committee and have to evaluate a postmodern dissertation. What criteria do I bring to bear to assess whether it is a good or bad dissertation? Do I go by my gut? Or are there standards that should be applied? I simply don't know, and I think that may be as much a flaw of postmodernism as it is a limitation of mine. On the other hand, I do think we now are in a very different time and are thus obliged to treat the state-centeredness of the world as problematic. We need to be more precise about the epoch in which we live.

R.I.S. Do you know where we are today?

[13] James N. Rosenau, *Turbulence in World Politics: A Theory of Change and Continuity* (Princeton, NJ: Princeton University Press, 1990).

[14] Michael W. Doyle and G. John Ikenberry (eds.), *New Thinking in International Relations Theory* (Boulder, CO: Westview Press, 1997).

[15] Arjun Appadurai, *Modernity at Large: Cultural Dimensions of Globalization* (Minneapolis: University of Minnesota Press, 1996), and Ulf Hannerz, *Transnational Connections: Culture, People, Places*, (London: Routledge, 1966).

J.R. Yes, I think I know. Our world has become torn between integrating and fragmenting dynamics, what I have labelled 'fragmegration'. I believe you can understand any situation in the world today better by viewing it as racked by the tensions between these two contrary forces. Thus it is also a disaggregated world, with ever more numerous centres of authority. I find it both exhilarating and humbling to dare to comprehend the complex, dynamic, and emergent world to which the twenty-first century is heir!

R.I.S. So there isn't a single international system?

J.R. Right. There is, rather, a global system and innumerable local systems, all of which are crowding out what we have long known as the international system. States and their system will doubtless continue doing what they have always done, but their roles and consequences will become meaningful only in a larger, fragmegrative context.

R.I.S. Christianity kept the neomedieval world together. Is there anything holding your disaggregated, fragmegrated world together?

J.R. Just sheer numbers, so many actors that not one of them can undo the underlying structure.

R.I.S. How do you see the relationship between academe and the policy world?

J.R. I neither think nor worry about it. I have an article of faith that what we do collectively in academe somehow, circuitously—in all kinds of ways and through all kinds of routes, through our writings, our students, our students' students, our students and readers who become officials, journalists, and thoughtful citizens—makes its way into the halls of government. It is a faith that allows us to focus on the substantive problems and processes of our discipline. More than that, it allows us to preserve the university for what it is—a wonderful place where ideas flow, cumulate, provoke, get rejected, and occasionally come together into coherent wholes. So I do not worry about building bridges between the academic and policy worlds. Our ivory towers have ample antennae with which to hear the world and ample microphones with which to assist the world.

R.I.S. Looking back over your long career, what are you most proud of?

J.R. Not an easy question. I guess I take pride in having had others, students and colleagues alike, take my ideas seriously, reflect on them, refine them, reformulate them. I even get a kick out of the ideas being criticized and discarded, since that means they were heard and pondered.