

ADAM PRZEWORSKI:
CAPITALISM, DEMOCRACY AND SCIENCE

Interview with Adam Przeworski

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Training and Intellectual Formation: From Poland to the United States

Q: How did you first get interested in studying politics? What impact did growing up in Poland have on your view of politics?

A: Given that I was born in May of 1940, nine months after the Germans had invaded and occupied Poland, any political event, even a minor one, was immediately interpreted in terms of its consequences for one's private life. All the news was about the war. I remember my family listening to clandestine radio broadcasts from the BBC when I was three or four years old. After the war, there was a period of uncertainty, and then the Soviet Union basically took over. Again, any rumbling in the Soviet Union, any conflict between the Soviet Union and the United States, was immediately seen in terms of its consequences for our life. It was like this for me until I first left for the US in 1961, right after the Berlin Wall went up. One's everyday life was permeated with international, macro-political events. Everything was political.

But I never thought of studying politics. For one thing, in Europe at that time there really was no political science. What we had was a German and Central European tradition that was called, translating from German, "theory of the state and law." This included Carl Schmitt and Hans Kelsen, the kind of stuff that was taught normally at law schools. That was as much political science as there was. It was not a distinct academic discipline in Poland. So I never thought of studying politics per se.

Q: What did your parents do?

A: Both of my parents were physicians. My father, whom I never knew, was conscripted into the Polish Army in 1939 and was eventually captured by Russians. He was killed in Katyn, in the massacre of Polish officers by the Russians, at about the time I was born. My mother could not work as a doctor under the Nazi occupation – she was baking cakes – but resumed her profession after the war.

Q: Where did you do your undergraduate studies and what did you study?

A: I entered the University of Warsaw in 1957 to study philosophy. In the European system at that time, you entered into a 5-year program, and the first degree you received was a Masters. I then discovered, as did many of my colleagues, that because I was in the Department of Philosophy and Sociology, I could get a double degree in Philosophy and Sociology if I took a few more courses. So, I ended up getting a Masters in Philosophy and Sociology from Warsaw. Only later, when I came to the US, to Northwestern University, did I study political science.

Q: What did you read at the University of Warsaw?

Before World War II, Poland had two very strong intellectual traditions in the social sciences. One was logical positivism. The so-called Vienna Circle was, in fact, a Vienna-Lwow-Warsaw Circle, and several eminent logicians were Polish.¹ That was a very strong tradition. The other tradition was a predominantly German idealist, right-wing historicist tradition.²

After the war, although Marxism became an obvious new influence, positivism retained a strong presence. There was a debate in the journal *Philosophical Thought* (*Mysl Filozoficzna*) between Marxists and positivists, which the Marxists were losing. Then Stalinism took control of the country, and, in 1948, the debate was solved by so-called “administrative measures.” The journal was closed, and all the positivists were expelled from the university. Yet, unlike what happened in other Soviet-occupied countries, they were not killed but were sent to edit works of Plato, Aristotle and so forth. But with the end of Stalinization, in 1955 or so, the repression decreased, and the same debate resurged.

It was an excellent debate that was carried out in an atmosphere of true intellectual conflict and gave rise to very interesting developments. If you want to trace the real origin of

¹ The Vienna Circle was a group of philosophers and scientists organized in Vienna under Moritz Schlick, and that met regularly from 1922 to 1932. Their approach to philosophy came to be known as logical positivism, which holds that philosophy should aspire to the same sort of rigor as science.

² A historicist approach holds that concepts and truths can be understood only in relation to the context of a historical period.

Analytical Marxism, it's in Poland in 1957.³ Why? Basically, positivists were saying to Marxists: "What do you mean by 'long-term interests'? What are these things you call 'classes'? Why would classes pursue long-term interests?" And the Marxists, who were no longer protected by "administrative measures," had to find an answer to these questions. So I entered the university at a fascinating time of real ferment.

The program I entered reflected these broader trends. During Stalinism, the University of Warsaw's Department of Philosophy was closed and replaced by a Department of Dialectical Materialism. The Department of Sociology, which dated to the 1870s, was closed and replaced by a Department of Historical Materialism. Then, in 1957, the year I entered the university, these Departments of Dialectical Materialism and Historical Materialism were closed, and a new department of Philosophy and Sociology, which reflected the influence of both positivists and Marxists, was opened. The program itself consisted of two years of mathematical logic and lots of philosophy of science. This was due to the influence of the positivists. It also included a very systematic and traditional Central European course in the history of philosophy taught by Marxists, people whose names you would recognize today: Leszek Kolakowski, Bronislaw Baczko. It was an excellent program.

Q: Why did you choose to go to the United States in 1961 to pursue advanced studies at Northwestern University?

A: The story goes like this. First of all, Poland was a pretty closed country. So we all grew up in an atmosphere where we wanted to see other things, to get out. And by a complete accident, I met a Northwestern University professor, R. Barry Farrell, in Warsaw. He appeared at a meeting of a student group where we would discuss regularly in English. He invited me for lunch and, out of a blue, asked if I wanted to go to the United States to study political science. I don't remember if I had the smarts to ask him what political science was: I didn't know what it was. But even if he had asked me if I wanted to work on a ship sailing around the world, I would have said "yes."

³ Analytical Marxism is a variant of Marxism that holds that Marxist theory should conform to "normal" scientific methods and that this should involve the development of micro-foundations.

I was twenty years old, and I would have gone anywhere to do anything. So, that's the way it happened. It was by pure accident that I landed at Northwestern.

Q: What did you study at Northwestern?

A: Northwestern at that time was one of the first “behavioral departments” in the country. The faculty included Richard C. Snyder, who did international relations; Harold Guetzkow, the first person to start simulating international systems; and Kenneth Janda, who was one of the first people to do empirical, comparative research. Northwestern’s political science department had a kind of mystique. But most of these people were not very good. To be frank, I think I learned next-to-nothing. I was too well educated to have learned much there. I remember that the opening course was a standard introduction to political science, with the first part dedicated to “what is science?” and the second part to “politics.” I thought the teacher’s knowledge about the philosophy of science was abysmal. I got myself in trouble several times because of that. I was not a disciplined graduate student since I read what I wanted rather than what I was told to read, basically a lot of “social Freudians.”

To be fair, I took an interesting course on economic development from Karl de Schweinitz, an economic historian.⁴ And I took a course on research design from Donald Campbell, a psychologist.⁵ That served me well the rest of my life. I’ve learned it’s a very tricky business to design empirical studies. Those are the two courses from which I learned something in graduate school.

Q: Could you discuss your dissertation work?

⁴ Karl de Schweinitz, Jr. is author of *Industrialization and Democracy* (New York: Free Press of Glencoe, 1964).

⁵ Donald T. Campbell is a scholar renowned for his work on research design. His works include: Donald T. Campbell and Julian C. Stanley, *Experimental and Quasi-Experimental Designs for Research* (Boston: Houghton Mifflin Co., 1966); Thomas D. Cook and Donald T. Campbell, *Quasi-Experimentation: Design and Analysis Issues for Field Settings* (Boston: Houghton Mifflin Co., 1979); and Donald Thomas Campbell, *Methodology and Epistemology for Social Science: Selected Papers* (Chicago: University of Chicago Press, 1988).

A: I did two years of coursework at Northwestern. Then I passed my qualifying exams and went back to Poland with a dissertation topic on the impact of party systems on economic development. I took a job as a sociologist at the Polish Academy of Sciences. Since getting out of Poland was very difficult, both politically and financially, I never thought I would be able to defend my American thesis, so I was simultaneously writing a second dissertation in sociology, which I planned to defend in Poland. However, one day the person who had invited me to study at Northwestern, Barry Farrell, wrote to say that Northwestern had agreed that I could defend my dissertation in Warsaw. There were some American professors visiting Warsaw, and he told me that they could constitute a committee for my dissertation defense. So I had six months to finish my American thesis, which I did.⁶ To the best of my knowledge, it was the first empirical study of the relationship between political institutions and economic development.

Q: At this point, were you thinking of working permanently in Poland?

A: I thought of staying in Poland. But in 1967 I was invited to the University of Pennsylvania for a semester. Since 1964 I had been involved in a collaborative international project called “International Studies of Values in Politics.” It was a local politics project, based on surveys in the United States, Poland, Yugoslavia, and India, that was headed by a group of people at the University of Pennsylvania, notably the late Philip Jacob. We were at the stage of analyzing data, so I was invited to the University of Pennsylvania to teach a couple of courses and help with the data analysis. Then, when I was in Pennsylvania, I got another invitation, to go to Washington University in St. Louis for a semester. I accepted this invitation, and then, during the spring of 1968, while I was at Washington University, there was a student demonstration in Warsaw that was very heavily repressed. There were many arrests, and my friends advised me not to go back.

Q: Why were you advised not to return to Poland? Were you in trouble with the government?

⁶ Adam Przeworski, “Party System and Economic Development,” Ph.D. Dissertation, Northwestern University, 1966. In addition to receiving a Ph.D. in political science from Northwestern University in 1966, Przeworski received a Ph.D. in sociology from the Polish Academy of Sciences in 1967.

A: The year before I went to the University of Pennsylvania I had taught an introduction to sociology course at the University of Warsaw, and, after the government crackdown in 1968, some of my students became eminent dissidents. A Brazilian friend, who was a communist in exile in Warsaw, Pedro Celso Cavalcanti, made a special trip to Berlin to call me and tell me not to come back, because twenty-eight of my forty students were in jail. I was also in trouble in Poland because, in 1963/64, I had participated in a little study group that researched who paid the cost of industrialization in Poland under Stalinism from 1948 to 1955. The clear conclusion was that the workers had paid the cost. Our study showed that the communist party that claimed to rule the country as the crystallization of the dictatorship of the proletariat had actually exploited workers. Obviously, this was not something the Party liked. So, I couldn't go back to Poland. But I couldn't stay in the US either, because I had visa problems. By complete accident, I ended up in Chile.

Q: Why Chile?

A: In Poland I had a Chilean student, Pablo Suarez, who eventually returned to Chile and invited me there to work. This was really the only opportunity that I had. I didn't have any money, I didn't have a job, I couldn't go back to Poland and I couldn't stay in the US. Eventually, this invitation didn't work out. But I was still interested in going to Chile, and I accidentally met Glaucio Soares, who was then the director of FLACSO (Facultad Latinoamericana de Ciencias Sociales – Latin American Faculty in the Social Sciences) in Santiago. When he found out that I was interested in going to Chile, he invited me, and I went in the Fall of 1968.

Q: Did you work on Chile while you were there?

A: No, I didn't work on Chile at all. I was still working on the survey data project at the University of Pennsylvania. And I was writing a comparative politics methodology book with

Henry Teune of Penn.⁷ But during that first stay of six months in Chile my wife and I fell in love with the country. So I got a grant from the SSRC (Social Science Research Council) and we went back to Chile in 1970/71. By that time, I was actually working with a Chilean collaborator on the extensions of suffrage in Western Europe and Latin America. But I never completed that project. Indeed, I am doing it now. My wife, however, did write a doctoral dissertation on Chilean economic history.

Q: Eventually you got a permanent position at Washington University.

A: Yes. I got a permanent position at Washington University in 1969. Then, in 1972, when I was spending a year in France, I got an offer from the University of Chicago. I went to Chicago in 1973 and stayed there twenty-two years.

Q: Were you ever a tenure-track assistant professor?

A: I think I was. But I didn't know the system very well, and tenure was almost automatic at that time. As a matter of fact, when I was going to Chile in 1972, I think, and I needed some travel money, I went to see the head of the Latin American Studies Committee at Washington University, the late sociologist Joseph Kahl, and asked him for money. He said, "Are you tenured or are you not tenured?" I asked, "What does that mean?" Well, it turned out that I was not tenured. But I went to the University of Chicago with tenure.

Research on Social Democracy, Regime Change and Development

Capitalism and Social Democracy

⁷ Adam Przeworski and Henry Teune, *The Logic of Comparative Social Inquiry* (New York: Wiley, 1970).

Q: The first substantive topic you worked on was social democracy. You published a series of articles and two books—*Capitalism and Social Democracy* and *Paper Stones*— on this topic.⁸ What motivated your interest in social democracy?

A: I was a Marxist, and I was trying to make political sense of social democracy. My question was, why was there no revolution in the West? Marxism offered a theory, that I thought was generally reasonable, which said that in industrialized countries there should be a revolution supported, if not led, by an organized working class. Yet the obvious observation was that there was no revolution and there probably wouldn't be one. I was trying to figure out why not.

I was also very influenced by Chile and its history of socialism. I was living in Chile in 1970/71, the first year of Allende's government,⁹ and this made me think about the feasibility of a strategy of gradual transformation of capitalist society. The Allende experience raised the question: Is it a viable strategy for socialists to compete in elections and enact reforms that have majority electoral support? This question led me to turn to Europe, to see what happened historically with the project to achieve socialist reforms in Europe.

My research agenda on social democracy evolved. Initially, around 1970, I was interested in studying the extension of the suffrage from the perspective of "the legalization of the working class," the title of a French book: I no longer remember the author. I was interested in why elites who enjoy voting rights are willing to extend these rights to others, and, in turn, why workers were willing to use these voting rights and work within the system rather than attempt to destroy it, a topic that became recently fashionable among economists. Eventually, my thinking evolved from a more narrow focus on the extensions of the suffrage and the decision of early socialists to participate in electoral politics to a broader understanding of social democracy. In this broader perspective, I thought two questions needed to be answered. One concerned socialist parties and the electoral process: why did socialists decide to struggle for the suffrage and use it for

⁸ Adam Przeworski, *Capitalism and Social Democracy* (Cambridge: Cambridge University Press, 1985); Adam Przeworski and John Sprague, *Paper Stones: A History of Electoral Socialism* (Chicago: University of Chicago Press, 1986).

⁹ Salvador Allende, Chile's president from 1970 to 1973, was head of a leftist coalition of parties that sought to introduce radical reforms through democratic means. He was overthrown in a military coup led by General Augusto Pinochet in September 1973.

reformist goals? The second concerned economic strategy: why were the socialists willing not to nationalize the means of production once they had the power of government?

Q: What are the main conclusions you drew from this research?

A: The central thing I learned was that reformism was a rational strategy for workers. It was in the interest of workers to support capitalist democracy. An electoral victory of pure workers' parties was not historically feasible, because the assumption that manual workers in industry and transportation would one day become the overwhelming majority of the population in industrializing countries was mistaken. That meant that socialist parties could not win elections solely by representing workers; they could only win by acting as a catch-all, multi-class party. To achieve this, they had to broaden their appeal beyond the specific interests of workers

The second thing I learned, working with Michael Wallerstein,¹⁰ is that workers face a trade-off between the goals of income distribution and economic growth and, under certain conditions, the optimal strategy for workers in the long run may be to limit their distributional claims. By exercising wage restraint, workers induce capitalist to invest, which causes the economy to grow. Hence, workers end up ahead. So, the social democratic strategy of class compromise had a rational basis.

Q: Were there any particular authors you were arguing against in your work on social democracy?

A: I was arguing against an entire socialist tradition—from Lenin to Trotsky, Lukacs, and Luxemburg—that saw social democrats as traitors. That was the main target of my polemic. More pointedly, there is a passage that Marx wrote in 1850 in *Class Struggles in France* that

¹⁰ Adam Przeworski and Michael Wallerstein, "The Structure of Class Conflict in Democratic Capitalist Societies," *American Political Science Review* Vol. 76, No. 2 (June 1982): 215-38.

says that the combination of private property and universal suffrage is impossible.¹¹ This phrase, which Marx repeats in other works, was my target. It was obvious that private property and universal suffrage could exist together, but it was far from clear why. The leftist tradition—radical socialism of every variety—said basically that if the combination of private property and universal suffrage is possible, it’s because social democrats are “traitors.” My view was that social democrats were not traitors. Rather, they did as well as they could under the circumstances. My position is captured in Engels’ phrase that “ballots became paper stones,” which I use as the title of one of my books.¹² Engels came to hold the position that universal suffrage is in fact an effective instrument for advancing workers’ interests and that it was no longer necessary to build barricades, because ballots could be used to win office. The power of elected officers, in turn, could be used to transform capitalist societies.

Q: To a large extent, then, your research focused less on the origins of democracy, including why suffrage was granted and extended, than on the workings of capitalist democracy.

A: I did address the question, why is suffrage extended? My hypothesis was that it was a response to a revolutionary threat. The extension of the suffrage was often preceded by violent mobilizations. For example, in 1867 the mob climbed the fences of Hyde Park in London. I saw the suffrage as a conservative device, in the British sense, to calm down a revolutionary threat. But I didn’t think very much about how democracy itself comes about. I was sort of bewildered by democracy. I didn’t grow up under democracy, so, for me, it was an alien object. Nothing about democracy was obvious. The question of how democracy works puzzled me. It still puzzles me.

¹¹ Karl Marx, *Class Struggles in France, 1848 to 1850* (Moscow: Progress Publishers: 1952). The passage from Marx’s work is cited in Adam Przeworski, *Capitalism and Social Democracy* (Cambridge: Cambridge University Press, 1985), pp. 133.

¹² Adam Przeworski and John Sprague, *Paper Stones: A History of Electoral Socialism* (Chicago: University of Chicago Press, 1986).

Transitions to Democracy and the Stability of Democracy

Q: A second substantive area of your research has been the issue of transitions to, and the stability of, democracy.

A: I started thinking about transitions to democracy in a systematic way in 1979. I was an original member of the O'Donnell/Schmitter/Whitehead project.¹³ We met for the first time in 1979 at the Woodrow Wilson Center in Washington, D.C. I remember that I really didn't know what the whole project was about. Philippe Schmitter, a close friend, said, "Participate, you'll have something interesting to say," but I remember it was extraordinarily painful for me to find something to say. Eventually I did write a paper.¹⁴ But I really didn't know which body of theory and experience was relevant to the question of transitions to democracy. I don't think anybody else did either.

In terms of theory, about three days into the meeting in Washington, it struck me that no one had mentioned either Barrington Moore or Seymour Martin Lipset.¹⁵ Of the forty people in the room, at least thirty taught Moore and Lipset in their courses. I raised this point and said, "Isn't that strange?" I think we understood that the theories of Moore and Lipset were too deterministic. We were trying to strategize transitions to democracy, which meant that we thought some courses of action could be successful under particular conditions while other would not be. Contrary to Moore, the prospects for democracy were not determined by what happened to agrarian class structure two centuries ago. Nor, contrary to Lipset, did we think the prospects for democracy were determined by the level of development. In terms of cases, we looked at past cases of democratization. But we were not certain whether they were relevant.

¹³ Guillermo O'Donnell, Philippe Schmitter and Laurence Whitehead (eds.) *Transitions from Authoritarian Rule. Prospects for Democracy* 4 Vols. (Baltimore, Md.: The Johns Hopkins University Press, 1986).

¹⁴ Adam Przeworski, "Some Problems in the Study of the Transition to Democracy," pp. 47-63, in Guillermo O'Donnell, Philippe Schmitter and Laurence Whitehead (eds.), *Transitions from Authoritarian Rule. Comparative Perspectives* (Baltimore, Md.: The Johns Hopkins University Press, 1986).

¹⁵ Seymour M. Lipset, "Some Social Requisites of Democracy: Economic Development and Political Legitimacy," *American Political Science Review* Vol. 53, N° 1 (1959): 69-105; Barrington Moore, Jr., *Social Origins of Dictatorship and Democracy: Lord and Peasant in the Making of the Modern World* (Boston: Beacon Press, 1966).

Q: That was a good ten years before the collapse of communism in Eastern Europe. When did you first sense that something big was going to happen in Eastern Europe?

A: In June of 1986. Now why? Well, in Poland in August of 1980, there was a strike followed by a massive mobilization. The Solidarity movement was created in three weeks in September of 1980. Sixteen million people became members of the movement. It was the biggest explosion of a social movement in history. As a result, the whole system was on the brink. Then, on December 13th of 1981, we had what I saw as a Latin American style coup d'etat, led by General Jaruzelski. I read that event through Marx's perspective on France in the 1848-51 period. Namely, I saw it as proof that the Communist Party was incapable of ruling the country and had been driven to seek protection through the military. At that time, I wrote a little piece called "The Eighteenth Brumaire of General Jaruzelski."¹⁶ For me, the question was whether the military could maintain the system when the party had failed to. The military did so initially with a fair amount of repression. Still, there was much popular unrest and intermittent strikes in the first part of the 1980s. The military used a stop-and-go strategy: they would repress, step back and seek reconciliation, and then they would repress and step back again. July 22 was the national independence day of Communist Poland; that's when the communists established control following World War II. And the government would always declare an amnesty that day. The joke in Poland in the early 1980s was: "What happened on July 22, 1982? There was an amnesty. What's going to happen on July 22, 1983? There will be an even larger amnesty." That is what everybody thought. But, by 1985, the government saw that its strategy simply wasn't working, and it decided not to arrest striking workers. I got a whiff that they may be giving up.

In June of 1986, I was in Warsaw and I went for a walk, as I often did, with a friend who was a prominent communist reformer, Jerzy Wiatr. He told me, "We are beginning to think we can have elections at the local level to open things up a bit." And I said, "If you're going to have elections, you're going to lose," to which he replied, "You know, it doesn't matter so much *if* we win or lose, but *what* we will lose." And I thought, wow!

¹⁶ Marx wrote a book on France called *The Eighteenth Brumaire of Louis Napoleon* in which he analyzes the process leading up to the establishment of a dictatorship led by Louis Bonaparte, Napoleon Bonaparte's nephew, in

Q: What did he mean by “what we will lose”?

A: He meant whether the rulers were going to lose their lives, their jobs, or just elections. I thought this was strange. Gorbachev had come to power in Russia in 1985, and the Russians started talking about economic reforms. I don't know why, but after the conversation with my friend in Warsaw, I plunged into reading Russian economists' debates about economic reforms. One of the first strong intuitions I had was there can be no end to these economic reforms once they are started; it was a slippery slope. When you start doing what Gorbachev and his team of reformers were planning to do, namely introduce some sort of price mechanism, there is no way to justify the rest of the communist economic model. Once you take that first step, you have to go forward. It's like the bicycle theory: if you don't keep going, you fall. By 1987, I became persuaded that something important was underway in Eastern Europe.

I am staking a claim here. In 1984, Huntington, the great theorist of transitions to democracy, wrote an article saying that transition in Eastern Europe is not possible.¹⁷ In 1989, Juan Linz wrote something similar and published it in 1990.¹⁸ In 1988, I was at a congress in Brazil, and I talked about transitions to capitalism in Eastern Europe. I was shouted out of the room and accused of being a traitor, an idiot, a class enemy, and everything else.

Q: Your work on transitions to democracy was distinguished by its use of game theory to analyze strategic choices formally. Why did you turn to game theory at a time when it was not a common tool in the study of democratization?

A: I was extremely struck by the degree to which Polish communists thought strategically. By that time I was going to Warsaw very often, and it was clear to me that the communists were

December 1851. The “Eighteenth Brumaire” refers to November 9, 1799 in the French Revolutionary Calendar, the day the first Napoleon Bonaparte had made himself dictator by a coup d'état.

¹⁷ Samuel Huntington, “Will More Countries Become Democratic?” *Political Science Quarterly* Vol. 99, N°2 (Summer 1984): 193-218.

strategizing very carefully, even though they made a lot of mistakes. Indeed, whether you went to Spain in the mid 70s or Poland in the mid 80s, over drinks, people analyzed politics in strategic terms. This doesn't mean that everybody knew everything, that everybody could anticipate all the consequences of their choices. But I was struck from the beginning that people were thinking strategically. I started to think, maybe I'll put myself in their shoes, try to understand the situation strategically, model it, and then see what I come up with.

The decision to use game theory probably stems from my general methodological inclination to build a logically coherent argument and use formal tools to ascertain whether the argument is, in fact, logically coherent. That's why Philippe Schmitter invited me to participate in the Wilson Center project. When Philippe said, "You'll have something to say," it's because he thought I would probably think about transitions to democracy in different terms than he and Guillermo O'Donnell did. José María Maravall, a close friend of mine, was recalling the Wilson Center conference recently, and he told me, "When you started talking, I thought you were from a different world. Then you went to the chalkboard and started drawing these boxes and arrows. I had no idea what it was all about." Now he uses game theory himself. So I think my use of game theory was due to a combination of my methodological inclinations and my strong intuition that the political actors involved in transitions to democracy thought strategically.¹⁹

Q: What do you think your game theoretic analysis of transitions to democracy added to the work by Guillermo O'Donnell and Philippe Schmitter?²⁰

A: Let me answer in a roundabout way. I was at a conference in the mid-1970s with Fernando Henrique Cardoso. And Fernando Henrique was doing one of his dependency theory things.²¹

¹⁸ Juan J. Linz, "Transition to Democracy," *The Washington Quarterly* Vol. 13, N° 3 (Summer 1990): 143-64.

¹⁹ For Przeworski's reflections on the literature on transitions to democracy, see Adam Przeworski, "Democratization Revisited," *Items* (SSRC) Vol. 51, N° 1 (March 1997): 6-11.

²⁰ See especially Adam Przeworski, *Democracy and the Market. Political and Economic Reforms in Eastern Europe and Latin America* (New York: Cambridge University Press, 1991), Chapter 2. Guillermo O'Donnell and Philippe Schmitter, *Transitions from Authoritarian Rule. Tentative Conclusions about Uncertain Democracies* (Baltimore, Md.: The Johns Hopkins University Press, 1986).

There were interests; then interests organized into classes and fractions; classes and fractions made alliances, and so on. I asked him, “How do you know that out of these classes and fractions, you’re going to get these alliances?” He replied, “Oh, Adam, you are asking for empty formalisms.” Well, I didn’t think those were empty formalisms, because the way alliances emerge from a structure of interests is not obvious. It could be that only one alliance is feasible, that several are feasible, or that none is feasible. So, we need some tools to find out what alliances are possible. I saw game theory as a tool that allows us to determine what kind of outcomes we should expect under particular conditions, under particular structures of interests.

Specifically, one of my discoveries was that if all the major actors involved in potential transitions to democracy have complete information about each other’s preferences, then under the assumptions with which we described the situation, a transition would never occur. This means you have to start worrying about who knows what. Does the regime know about the opposition or does the opposition know about the regime? What difference does this make? To answer these questions, you need tools, formal tools. I think Philippe and Guillermo, in their *Tentative Conclusions about Uncertain Democracies* volume,²² sort of threw their hands up in the air and just said things are uncertain in transitions to democracy. But these transitions are not as uncertain as they thought. There was more structure, and, hence, more information about regime transitions that they could have utilized.

Q: Who were you talking to in terms of formalizing your game-theoretic analysis of transitions to democracy? At that time there was no game theoretic literature on transitions.

A: I was talking to nobody. I didn’t have interlocutors. But, even though what I was proposing was novel, I think a lot of people got persuaded. They found some of the reasoning useful. In 1986, I published a piece in which I used some ideas from Thomas Schelling’s work to shed light

²¹ Cardoso is one of the founders of dependency theory, a theory that emphasizes the importance of external determinants of the development prospects of poor countries. His most widely read book is Fernando H. Cardoso and Enzo Faletto, *Dependency and Development in Latin America* (Berkeley: University of California Press, 1979).

²² Guillermo O’Donnell and Philippe Schmitter, *Transitions from Authoritarian Rule. Tentative Conclusions about Uncertain Democracies* (Baltimore, Md.: The Johns Hopkins University Press, 1986).

on when supporters of the incumbent, authoritarian regime would start jumping ship.²³ And I remember people found that useful. Even Juan Linz found it useful. They were listening.

Q: Subsequently, some people criticized your game theoretic analysis of transitions for not being formal enough.²⁴ What's your assessment of these critiques?

A: My model was crude and rudimentary for three reasons. One, there wasn't as much game-theoretic work on politics twenty-five years ago. Two, my skills were not good enough to do it better. And three, I just wanted enough of a tool to understand, to my satisfaction, what was going on. I wasn't writing a game theory article. I knew that there were hard-liners, reformers, and the opposition, and that was good enough for me. I didn't think I needed more.

Q: You followed up your game-theoretic research on transitions to democracy with some statistical work on transitions to, and the stability of, democracy.²⁵

A: By 1990 we had quite a few new democracies, and the question that appeared on the political and intellectual agenda was "consolidation," a term I do not like to use. So I started asking the same question everybody else did, namely, "Now that we have these democracies, are they going to be successful? Are they going to survive or not?" I posed the general question, "What are the conditions under which democracies survive and under which they die?"

The fact that we had many new democracies meant that we had enough cases of transition to democracy to start thinking statistically about democratization. Even though we

²³ Adam Przeworski, "Some Problems in the Study of the Transition to Democracy," pp. 47-63, in Guillermo O'Donnell, Philippe Schmitter and Laurence Whitehead (eds.), *Transitions from Authoritarian Rule. Comparative Perspectives* (Baltimore, Md.: The Johns Hopkins University Press, 1986).

²⁴ See, for example, Scott Gates and Brian D. Humes, *Games, Information, and Politics: Applying Game Theoretic Models to Political Science* (Ann Arbor, MI: University of Michigan Press, 1997), Chapter 5.

²⁵ The main relevant works are: Adam Przeworski and Fernando Limongi, "Modernization. Theory and Facts," *World Politics* Vol. 49, N° 2 (1997): 155-83; and Adam Przeworski, Michael E. Alvarez, José Antonio Cheibub and

never realized it, we used to be extremely Bayesian²⁶ in our approach to studying contemporary democratization. In 1979, we had only three cases of transitions to democracy on which to build our beliefs: Portugal, Greece, and Spain. As a result, every new case of transition to democracy changed our minds about the causes of democratization. Our beliefs were very unstable. Every case mattered, because there were extremely few. That was how we were learning about democratization. By the beginning of the 1990s, I started thinking we now have enough cases of new democracies that we can begin to develop statistics on them.

Q: One of the central findings of your research was that the level of economic development explains the survival of democracies, as Lipset had suggested in 1959,²⁷ but it does not account for the emergence of democracies. Your thesis about an asymmetric pattern of causation has been questioned by several authors, who argue that even your own data do not support it.²⁸

A: There is no doubt that the probability that a democracy survives increases with per capita income. You can control it for everything from the kitchen sink to the grandmother's attic. That relationship will survive anything. It's monotonic, and it's strong, unbelievably strong. I have no shred of doubt about that.

With regard to whether transitions to democracy are more likely as countries become more economically developed, let me say the following. When Fernando Limongi and I first studied this issue in our 1997 *World Politics* article,²⁹ we did not find any significant relationship between transitions to democracy and the variables we considered. When we were writing the

Fernando Limongi, *Democracy and Development: Political Institutions and Well-Being in the World, 1950-1990* (New York: Cambridge University Press, 2000), see especially Chapter 2.

²⁶ Bayesian statistics is based on a view of probability that hinges on the personal degree of belief an individual holds that an event will occur. This view is contrasted to a classical approach to statistical inference, which relies on a frequentist view of probability.

²⁷ Seymour M. Lipset, "Some Social Requisites of Democracy: Economic Development and Political Legitimacy," *American Political Science Review* Vol. 53, N° 1 (1959): 69-105.

²⁸ Carles Boix and Susan Stokes, "Endogenous Democratization," *World Politics* Vol. 55, N° 4 (July 2003): 517-49.

²⁹ Adam Przeworski and Fernando Limongi, "Modernization. Theory and Facts," *World Politics* Vol. 49, N° 2 (1997): 155-83.

book—*Democracy and Development*—and refining the data, we found a little curvature in there, that is, we found some evidence of a relationship between economic development and transitions to democracy.³⁰ But we didn't pay much attention to this relationship, in part because we couldn't pay attention to it statistically due to the way we were estimating it. Then Boix and Stokes questioned our findings.³¹ Now there is a whole bunch of papers that argue that democratization becomes more likely as countries develop economically. But they all incorrectly specify the statistical model. It turns out that regimes transitions do not follow first-order Markov process: what I mean is that the probabilities of transition depend on past history, not only on the current conditions. Once one introduces past regime history into any statistical specification, the relation between development and democratization vanishes.³² It is simply not true that as countries become more developed they are more likely to become democracies.

Q: Do you have a hunch about why the impact of income is not the same under democracy and authoritarianism. That is, why does the level of income have such a powerful effect on the survival of democracies but not on the survival of dictatorships?

A: I have hunches. I think that democracy becomes more stable in more developed societies because as people become wealthier, too much is at stake in attempting to subvert democracy. Intense political mobilization is risky in general, and in wealthy democracies it is even more risky, because people have too much to lose. For example, if the American presidential election of 2000 had occurred in a country with one-third the income of the United States, it would have ended in a coup d'état or a civil war, as it did in Costa Rica in 1948 under very similar circumstances. These outcomes did not occur because people in the United States have too much to lose. They eventually said, "Well, we are going to be governed by a government that probably

³⁰ Adam Przeworski, Michael E. Alvarez, José Antonio Cheibub and Fernando Limongi, *Democracy and Development: Political Institutions and Well-Being in the World, 1950-1990* (New York: Cambridge University Press, 2000), Chapter 2.

³¹ Carles Boix and Susan Stokes, "Endogenous Democratization," *World Politics* Vol. 55, N° 4 (July 2003): 517-49.

³² Adam Przeworski, "Economic Development and Transitions to Democracy: An Update," unpublished manuscript, Department of Politics, New York University, August 23, 2003.

stole the election, has no legitimacy, and that we don't like. But so what? We will survive. We have our homes, our cars and our TVs. So, why bother? There is too much at stake to go into the street and build barricades or whatever." In less developed countries, by contrast, more is at stake. This is why democracies survive in wealthy countries.

Q: But why doesn't the same mechanism work for dictatorships?

A: If I am correct that wealthy dictatorships are stable, the kind of mechanism I have just described may indeed be at work. Namely, when you become a Taiwan, South Korea, or maybe even an East Germany, or a Spain during Franco's time, the systems function. People are eating, and turning against the system becomes dangerous. It's always dangerous, but maybe it becomes too risky because there is too much to lose. Now, rich dictatorships do eventually fall. But my claim is they don't fall because of the income level, they fall because of the accumulation of random hazards. For example, dictatorship fell in Taiwan not because it became wealthy, but because Taiwan needed the support of democracies in its geopolitical struggle with China. I think dictatorship fell in Spain, first, because the founding dictator finally died and, second, because Spain wanted to get into the European Community and couldn't get in as a dictatorship. I think dictatorship fell in East Germany because dictatorship fell in the Soviet Union. I think dictatorship fell in Venezuela in 1958—this was the fourth wealthiest dictatorship that ever fell—because the United States stopped supporting Jiménez.³³ So, dictatorships eventually die. But they die for idiosyncratic reasons, not because they have become developed.

Q: You have said that we have a pretty good understanding of why democracies break down, yet we still lack a good understanding of why dictatorships break down. Might part of the reason for this gap in our knowledge be that people have spent far more time studying democracies than dictatorships?

A: Yes. We currently do not do a good job distinguishing one dictatorship from the next. This poses a problem for how I have been studying transitions to democracy. I have studied the question statistically by assuming that the fall of a dictatorship is equivalent to the emergence of a democracy. But very often dictatorships fall and are replaced by other dictatorships. So, we need to distinguish among dictatorships, allow for the possibility that the outcome of the fall of a dictatorship is another dictatorship, and then re-estimate the model. Then I think we will know more. At the moment, I am working on this issue with a former graduate student, Jennifer Gandhi. We wrote a paper together, and she wrote a whole dissertation on the issue of institutions under dictatorship.³⁴ For some reason, the literature decided a long time ago that institutions under dictatorship are merely window dressing. Ultimately, it's the individual or collective dictator who decides. Take Friedrich and Brzezinski. In the introduction to their book on dictatorship,³⁵ they say, "We are not going to bother with constitutions and institutions. They don't matter." The broader literature does much the same. There is a very good book by Brooker, sort of a review of the literature on dictatorship.³⁶ However, the word "law" or "institutions" does not even appear in the index. Juan Linz has worried a lot about types of dictatorships.³⁷ The problem is that his classification is not operational. I cannot reproduce it. Juan knows, because all of history is stored in his brain. But I am a great believer in reproducible classifications, and I do not know what observables I would have to consider to reach the same conclusions Juan does.

So Jen Gandhi and I posed the question, "Is it true that institutions do not matter under dictatorship?" And we are finding consistently that institutions actually matter a lot under

³³ Marcos Evangelista Pérez Jiménez was the head of Venezuela's military dictatorship from December 1952 through January 1958.

³⁴ Jennifer Gandhi and Adam Przeworski, "Dictatorial Institutions and the Survival of Dictators," unpublished manuscript, Department of Politics, New York University, 2002.

³⁵ Carl Friedrich and Zbigniew Brzezinski, *Totalitarian Dictatorship and Autocracy* (Cambridge: Harvard University Press, 1965).

³⁶ Paul Brooker, *Non-Democratic Regimes: Theory, Government and Politics* (New York: St. Martin's Press, 2000).

³⁷ See Juan J. Linz, "Totalitarianism and Authoritarian Regimes," pp. 175-411, in Fred Greenstein and Nelson Polsby (eds.), *Handbook of Political Science* Vol. 3, *Macropolitical Theory* (Reading, Mass.: Addison-Wesley Press, 1975). Reprinted with a new introduction as Juan J. Linz, *Totalitarian and Authoritarian Regimes* (Boulder: Lynne Rienner, 2000).

dictatorship. They affect all kinds of policies and outcomes. I think dictatorships are by far the most understudied area in comparative politics. We need to start thinking about it.

Q: There is a notable contrast between your 1991 book, *Democracy and the Market*, and your 2000 book, *Democracy and Development*.³⁸ In 1991, you critiqued Lipset and Barrington Moore for offering history without people, and you emphasized the importance of focusing on strategic actors. Yet your 2000 book could be characterized as correlations without people. There seems to have been a shift in perspective and a loss of a sense of politics in your work.

A: This is completely fair. The intent of the 2000 book was to clear up some of the mess in the empirical literature. We told ourselves, “Let us get the best facts we can, conduct robustness tests, and then decide what we should believe.” We were programmatically repressing any theoretical findings, and we were deliberately saying, “We don’t want to theorize, we don’t want to hang these facts on our theoretical assumptions. We want to be purely inductive, purely frequentist.” Let’s first establish the facts, then we can think about how to explain them. I’ve recently published two pieces addressing why it is that democracy survives in developed countries.³⁹ You have to write a very complicated model to figure that out. I think I have a story that explains this now. But addressing these kinds of questions and introducing micro-motivations and strategic decisions is a different task from what we proposed in the 2000 book. My idea was to see what the facts are that need to be explained, then explain them. I get articles by economists all the time that say “here is a stylized fact” and then propose some really complicated model to explain it. I frequently respond to such articles by saying “there is no such fact.” So, I didn’t want to write models until I knew what I wanted to explain.

Determinants of Development

³⁸ Adam Przeworski, *Democracy and the Market. Political and Economic Reforms in Eastern Europe and Latin America* (New York: Cambridge University Press, 1991); Adam Przeworski, Michael E. Alvarez, José Antonio Cheibub and Fernando Limongi, *Democracy and Development: Political Institutions and Well-Being in the World, 1950-1990* (New York: Cambridge University Press, 2000).

³⁹ Adam Przeworski, “Democracy as an Equilibrium,” *Public Choice*. “Democracy as an Equilibrium.” *Public Choice* 123(2005): 253-273. Benhabib, Jess and Adam Przeworski. “The political economy of redistribution under democracy.” *Economic Theory*(2005)..

Q: Another major topic you address in *Democracy and Development* concerns the political determinants of economic development.

A: I have had a life long interest in this question that goes back to Poland and my years as a graduate student at Northwestern. The communist regime in Poland legitimized itself by saying it was going to produce development. The communists said they were offering a shortcut to modernity. In Poland, we had doubts about that claim. Was it true that dictatorship was necessary for economic development? Or was it just a propaganda line of the communist regime? The same issue surfaced in the United States, where Karl de Schweinitz and Walter Galenson both published pieces in 1959 that basically said “We are democrats, but maybe we have to face the hard fact that in poor countries you need dictatorship to mobilize resources for development.”⁴⁰ That was the question I tackled in my dissertation, and it has been an issue I have thought about the rest of my life.

After 1990, the broad question of the impact of political regimes on development became relevant again. We wanted to know not only whether the new democracies that had emerged were going to survive, but also what kind of economic results they were going to produce. American discourse on the matter had changed. Whereas the standard line had been that democracies were not good for development, now the official American propaganda line was that democracies would produce great development. Meanwhile, a literature had accumulated. I reviewed this literature with a former student of mine, Fernando Limongi, and found it bewildering.⁴¹ The most bewildering part was that no study before 1982 showed that democracies grow faster, yet no studies after 1982 showed that dictatorships grow faster. And since a change in ideology had also occurred at that time, I thought this pattern in the literature was peculiar. So I decided to study the question seriously, statistically, and with good data.

⁴⁰ Karl de Schweinitz Jr. “Industrialization, Labor Controls, and Democracy,” *Industrial Development and Cultural Change* Vol. 7, Nº 4 (1959): 385-404; Walter Galenson, *Labor and Economic Development* (New York: John Wiley and Sons, 1959).

Q: What conclusions did you reach?

A: It is clear that democracy, at an aggregate level, does not affect the rate of growth of total income. There are some people, Robert Barro, for example,⁴² who claim that if you measure democracy in continuous terms, you find a curvilinear relationship between democracy and development. But I think there is also a curvilinear relationship between dictatorship and development: non-democratic countries with medium levels of income also have high rates of growth. If you plot rates of growth by per capita income, you will see that they reach a maximum, and then start declining. So I think economists like Barro are spotting a pattern that is independent of democracy. I think that regimes, at an aggregate level, have no impact on development.

The Holy Grail of this whole quest, and I am still active in it, is to find political institutions that are effective for development. This program has been unsuccessful so far. There is a literature that uses subjective measures of institutions, such as the security of property rights, independent judiciary, transparency, corruption, and so on. These measures all cover the recent period. If you do a cross-section, you find that these institutions correlate with economic growth. It always works. But when you try to reproduce these results using observables instead of subjective measures, you can never get any results. So even at this more disaggregated level, we still can't find any effect of institutions on growth. There is a large literature that says "institutions matter," but then the question becomes "which institutions?" And we don't know. Maybe institutions matter, but we really don't know which. I am still at it, collecting data that go farther back in history.

The Concept of Democracy

Q: In the statistical research you conduct in *Democracy and Development*, you use what you call a "minimalist conception" of democracy as a system in which rulers are selected without

⁴¹ Adam Przeworski and Fernando Limongi, "Political Regimes and Economic Growth," *Journal of Economic Perspectives* Vol. 7, N° 3 (Summer 1993): 51-69.

⁴² Robert J. Barro, *Determinants of Economic Growth: A Cross-Country Empirical Study* (Cambridge, MA: MIT Press, 1997).

violence by competitive elections. Moreover, you have argued explicitly for such a minimalist conception.⁴³ Why have you adopted this view?

A: People have very high expectations of democracy. I start from an understanding of democracy as a system in which rulers are elected and are subject to re-election, that is, they can be removed by a vote of a majority of citizens. I sought to understand, through inductive and deductive thinking, what it is reasonable for us to expect from democracy. As we have discussed, statistical results show that we shouldn't expect economic development from democracy. But should we expect that decisions will be rational, in a sort of 18th century way? Again I say "no." Should we expect accountability? Well, we know that elections are a very dull instrument of accountability. They are certainly not sufficient to ensure accountability. Should we expect that democratic governments produce equality? Here I think the puzzle is still open. Why is it that democracies don't equalize incomes more? I think we should expect such equalization, but I don't think we see it. So, to the question, "What should we expect democratic governments to generate?" I respond: development no, rationality no, accountability little, equality perhaps.

What can we expect with some certainty from democracy? We should expect that people are not going to kill each other, nor that would be killed by governments. That is why I go back to Popper and Bobbio⁴⁴ and say, "Democracy is a system that keeps us from killing each other; and that's good enough." I came to this conception of democracy as a result of the 1973 coup against Allende in Chile. I realized how important democracy is and that any policy that may undermine democracy is irresponsible, because it may lead to mass murder. My minimalist view of democracy really dates from that experience. We leftists had an ambivalent attitude about democracy. We used democracy if it advanced our goals and dismissed it if it didn't advance our goals. But in 1973 I realized that democracy is a value to be defended above all else. That was a major transformation in my thinking.

⁴³ Adam Przeworski, "Minimalist Conception of Democracy: A Defense," pp. 23-55, in Ian Shapiro and Casiano Hacker-Cordón (eds.), *Democracy's Value* (New York: Cambridge University Press, 1999).

⁴⁴ Karl Popper, *The Open Society and its Enemies* (London: Routledge & Kegan Paul, 1945); Norberto Bobbio, *The Future of Democracy* (Minneapolis: University of Minnesota Press, 1984), pp. 156.

Q: Currently, under the umbrella phrase the “quality of democracy,” there seems to be a move away from a minimalist conception of democracy. Is this productive?

A: It’s extremely productive. This question is related to the methodological controversy about dichotomous versus continuous measures of democracy.⁴⁵ It seems to me that the right way to think about this is as follows. There are some countries we cannot think about as democratic, and comparing whether Pinochet was more democratic than Videla,⁴⁶ or whether Stalin was more democratic than Hitler, makes no sense. These regimes were clearly dictatorships, and they have a score of zero. Now, that doesn’t mean we cannot say that one country is more democratic than another. This is where I use pregnancy as an analogy. Somebody can be one month pregnant, two months pregnant, and so on. We can make distinctions. So I am extremely sympathetic toward endeavors to assess the quality of democracy.

The problem with such efforts is that it is very hard to devise satisfactory measures of the quality of democracy; and one has to be very careful doing it. One has to be very careful because this phrase, “the quality of democracy,” is becoming a geopolitical instrument of the US government and of international financial institutions, which use it to force an institutional and political agenda on various countries. In this regard, there is an outburst of effort to try and rate “good governance.” But, what does good governance mean from the point of view of the government of Kenya or Indonesia? It means that the US government says, “We are going to give you hundreds of millions of dollars if you do this to your political system.” And many of the people who are advocating such agendas have no idea what they are doing.

It might be different if such policy decisions were based on solid research, if we really knew what works and what doesn’t. Then I would be hesitant, but sympathetic. But we don’t know. Let’s say we introduce an independent judiciary. What does an independent judiciary produce in Ecuador? I read a little piece on this topic, and the conclusion was that an independent judiciary makes judges cheaper to bribe. When judges lack independence from politicians, you have to bribe a politician, and that politician has to share the bribe with other politicians so they will back him up. But reforms that increase the independence of the judiciary

⁴⁵ David Collier and Robert N. Adcock, “Democracy and Dichotomies: A Pragmatic Approach to Choices About Concepts,” *Annual Review of Political Science* Vol. 2: 537-65 (Palo Alto, Cal.: Annual Reviews, 1999).

may only make it cheaper to bribe judges because foreign firms can just pick up one judge after another, thereby cutting out the politicians. We don't have an idea of what works and what doesn't. The debate about the quality of democracy has to be conducted with a greater understanding of its political consequences.

What bothers me also is that many of these initiatives hide an ideological agenda. Take, for example, Freedom House's ranking of countries.⁴⁷ They rate countries according to whether people are free to do things. So the United States ranks close to the top. Americans are free to form political parties, they are free to vote. But they don't form political parties, and half the population doesn't vote, even in presidential elections. I find this idea of freedom as an abstract potentiality divorced from the ability to exercise it to be ideologically tainted and unconvincing. Rosa Luxemburg once said "The problem is not to be free, but to act freely." In this spirit, we should be asking how many parties are there, what do they propose, how often do poor people compete and get elected, etc.? But that is not what Freedom House does. I see Freedom House as a product of American ideology.

Q: How would you go about studying the quality of democracy?

A: The first thing I would look at is the access of money to politics. This is what really differentiates democracies. When Lenin says, in a letter to Hungarian workers in 1919, "Bourgeois democracy is just a specific form of bourgeois dictatorship," he has the following mechanism in mind. Democracy is a universalistic system, sort of a game with abstract, universalistic rules. But the resources different groups bring into this system are unequal. Now, imagine a basketball game played between people who are seven feet tall and people who are short like me. The outcome is clear. We are playing this game between people who can spend a lot of money at it and people who can't. I think there was a real grain of truth in the work by

⁴⁶ President Jorge R. Videla was president of Argentina during 1976-81 in the context of a military regime.

⁴⁷ Freedom House publishes two annual indices of all countries of the world, one on political rights and another on civil liberties. The data can be accessed at <http://freedomhouse.org/index.htm>

Miliband on the empirical Marxist theory of the state.⁴⁸ Namely, when money enters politics, economic power gets transformed into political power, and political power in turn becomes instrumental to economic power. This is what we are witnessing in many countries. If I were to try to measure the quality of democracy, that's where I would hit first, on all the rules and practices that regulate the access of money to politics.

Research on Methodology

Q: In addition to your substantive projects, you have also written about methods, especially in the earlier years of your career. Why have you had this interest in writing about methods?

A: There are probably two reasons. First, several times I started tackling substantive problems only to discover that the available methods didn't work, that they couldn't serve to answer the question. As a result, I would start getting involved in methodological issues. I never really did methodology for methodology's sake. But I do have to admit that I sometimes ended up writing methodological pieces without going back to the substantive problem. That's true of the book about systems analysis that I co-authored in 1975,⁴⁹ which grew out of a project on the extension of the suffrage. In that case, I never went back from the methodological issues to the substantive problem.

The second reason was that, when I left Poland, I didn't want to study Poland, I didn't know enough about the US to study the US, and I didn't want to study Latin America because I was not a Latin American. So I had to figure out what I could do, and methods was one of those things. It was only in the early 1970s that I said to myself, "Why are you doing all this methodological stuff if what you really care about are substantive questions?" That's when I started doing substantive things again.

⁴⁸ Ralph Miliband, *The State in Capitalist Society* (New York: Basic Books, 1969).

⁴⁹ Adam Przeworski and John Sprague, *Systems Analysis for Social Scientists* (New York: Wiley, 1975).

But I have continued flirting with methodological issues. I recently co-authored a piece with a former student, James Vreeland.⁵⁰ We wanted to know what impact the IMF has on economic growth. But when we started thinking about this, we concluded that there was really no statistical model that did what we wanted to do. So we ended up writing a methodological article as a byproduct of the substantive article.⁵¹

Also, I have to admit that I find methodological work intellectually pleasing. I like working on methods because I like logical puzzles.

Q: Your best-known methodological work is your book with Henry Teune, *The Logic of Comparative Social Inquiry*.⁵² What do you see as its main contributions?

A: The book's main theoretical contribution, which originates from Polish sociology, is that comparative politics is not about comparing, but rather it is about testing hypotheses across countries. What we are involved in when we conduct "comparative research" is testing general hypotheses under different historical conditions.

Another contribution concerns the generation of data that are comparable across countries. We were focusing on surveys, and at the time people believed that you ensured comparability by translating, as accurately as you could, questionnaires from one language into another. We found that when you asked people if there were any conflicts in their community in the United States, they would say, "yeah, there are three: over water, over schools, and over this road." But when you asked this question in India and translated the word "conflict" to the closest word in Hindi, people would say "No, no. In this community we live in peace, we don't kill each other." Why was this? Because in the Indian understanding there was nothing between the extremes of peace and harmony and mutual killing. The notion of limited, regulated conflict was not in their conceptual apparatus. I came to the conclusion that it did not work to translate

⁵⁰ Adam Przeworski and James Raymond Vreeland, "The Effect of IMF Programs on Economic Growth," *The Journal of Development Economics* Vol. 62 (2000): 385-421.

⁵¹ Adam Przeworski and James Raymond Vreeland, "A Statistical Model of Bilateral Cooperation," *Political Analysis* Vol. 10, N° 2 (2002): 101-112.

⁵² Adam Przeworski and Henry Teune, *The Logic of Comparative Social Inquiry* (New York: Wiley, 1970).

questions literally, that there was no cross-national equivalence, which is the technical term we were concerned about. Teune and I developed what we thought was a clever way of controlling the meaning of different scales across countries.

Q: This book, published in 1970, is still used in many graduate courses. Given that the field of methodology is supposedly ever changing, this is somewhat surprising. Why do you think that is the case?

A: Yes, the book is still being printed; it's still alive. Why is this so? I think it was a good book. I think we really set things straight. There are a lot of things in this book that I do not believe now. For example, the stuff on research design, about most similar and most different systems designs, was wrong. I changed my mind on some of these things. But I think the central thesis, that comparative research is fundamentally about testing hypotheses under different historical and geographical conditions, provides a tie to the general enterprise of social science. I also think we offer some useful advice regarding the specific pursuit of information under different historical and geographical conditions.

Q: If you were to rewrite *The Logic of Comparative Social Inquiry*, in what other ways would it be different?

A: That's easy to answer. I now believe that counterfactuals play the crucial role in comparative thinking. What we want to know in comparative research, in the social sciences in general, is what would have happened had a particular unit, say country, been observed in a different state of the causal variable, under different "treatment." The trick is to find reasonable ways to inform such counterfactuals, to use what we can observe to inform the hypothetical states we do not observe. Take the impact of colonialism, the topic of a dissertation I am currently directing, by Sunny Kaniyathu. It is obvious that when Adam Smith thought that colonialism was ruinous for the colonized territories, he assumed that these territories would have developed had they not been colonized. Later Marxists thought the same. In contrast, Marx and J.S. Mill thought

colonialism was conducive to economic development because they assumed that otherwise these territories would have remained stagnant. Hence, the answers depend on the counterfactual one assumes. Which, then, are the correct counterfactuals? How do we choose among them? So if I were to write a comparative methods book today, it would be selection bias driven.⁵³

Q: In terms of selection bias, do you find King, Keohane, and Verba's discussion of the issue useful?⁵⁴

A: I think it's an excellent discussion, though, to my taste, the issue of counterfactuals is underemphasized. King knows what he is doing and understands the importance of the problem. But their formulation of the issues rushes too quickly to statistics, without going through philosophical problems entailed in counterfactual thinking.

Core Ideas and Their Reception

Q: What are the ideas you most like or consider to be your best ideas?

A: What good ideas do I think I've had? I think my idea of class compromise was a great idea. I like the way Wallerstein and I conceptualized the idea of structural dependence of the state on capital. I like the whole idea of electoral trade-off and the disintegration of the working classes as it enters into electoral politics. But I am not sure it worked empirically. We expected much more of a decay of the socialist parties than we found in *Paper Stones*.

I think that my understanding of democracy as a set of rules for processing conflicts in a peaceful way, that entail a particular kind of uncertainty, and that allow groups to make certain inter-temporal trade-offs, is a good one. I'm very much attached to this idea of democracy enabling inter-temporal trade-offs.

⁵³ Selection bias is a systematic form of error that derives from the study of a non-random sample.

From my methodological work, I think the idea that comparative politics is about testing hypotheses across countries is a good one.

Q: What do you think about the reception of your work? Are there any ideas that you think have been unfairly neglected?

A: I have been lucky in that when I thought I had a good idea, it found echoes. Sometimes things I didn't even think were particularly brilliant found echoes. These ideas were not strictly speaking original. You can always dig back somewhere and find somebody who said something like this. But for me they were original; and they were received as such.

I do think, however, that two of my methodological ideas were neglected. The idea that comparative research is testing general hypotheses under different historical conditions never took off.⁵⁵ I think quite a few people do approach comparative politics in this way. But open any comparative textbook, and you're going to find that the first or second sentence of it says that comparative politics is about comparing countries. Also, the suggestion about how to generate data that are comparable across countries has not been picked up, until the recent work by Gary King.

Q: Are there any things you wrote that you think were fundamentally misinterpreted?

A: For some reason, the analysis of the process of economic reform in the fourth chapter of *Democracy and the Market* has been read as a sign of my support of radical neoliberal reforms. I can't quite figure it out. This is an obvious misinterpretation.

⁵⁴ Gary King, Robert O. Keohane and Sidney Verba, *Designing Social Inquiry. Scientific Inference in Qualitative Research* (Princeton, NJ: Princeton University Press, 1994).

⁵⁵ Adam Przeworski and Henry Teune, *The Logic of Comparative Social Inquiry* (New York: Wiley, 1970).

The Research Process

Q: Turning to the research process itself, how do you formulate research questions? From where do you draw inspiration?

A: What typically happens is that I find that there is something I don't know, that we collectively don't know, or about which we collectively hold beliefs that are not mutually consistent. If I feel that the issues involved are politically important, then I am likely to start thinking about them. Basically, I get motivated by politically important problems that are intellectually puzzling. For me, research is a normatively and politically driven matter.

Q: Does reading political theory, the classics, inform your research?

A: Reading classics of political theory is extremely important to me. It's a source of hypotheses, historical information, and great ideas. I believe that few of the basic problems are new. If you read Aristotle, you will find the agenda of American political science pretty much laid out. Obviously, historical conditions have changed, and you can now ask all kinds of detailed questions that are not raised in the classics. Also, the classics often contain vague intuitions and not formulations that can be actually researched. Still, they are an immensely important source of knowledge and intuition.

I interact with a group of political philosophers on a daily basis. I've been teaching a course for years with Bernard Manin, an historian of political thought and the author of a great book on the theory of representative government.⁵⁶ We teach this course together; he teaches about Rousseau and I do models, he talks about Condorcet and I do models. These authors are a very important source for me.

When I came to this country in the 1960s, typically the same people taught political philosophy and comparative politics. In fact, most jobs in comparative politics were cast as jobs in political philosophy and comparative politics. The same person would teach "From Plato to

⁵⁶ Bernard Manin, *The Principles of Representative Government* (New York: Cambridge University Press, 1997).

NATO,” as it was called at that time, and “Introduction to Comparative Government.” That relationship between comparative politics and political theory became disassociated. Today I think we ignore political philosophy. Students of comparative politics are not introduced to big questions anymore, as vague as the intuition behind them may have been. And I think that the cost is that students are more and more narrow.

Q: You have characterized yourself as a “methodological opportunist.”⁵⁷ Could you describe your general approach to methods?

A: I am averse to methodological controversies, which I distinguish from technical issues. Everyone wants to know about methodology of comparative politics, and I constantly get invited to engage in methodological controversies. David Laitin is running such things all the time, Robert Bates, too. But I avoid such controversies. I do think things have to be technically right. If you’re doing theory, you have to do rigorous theory. If you’re doing statistical analysis, you should be doing good statistical analysis. You need craftsmanship in both. I think craftsmanship is enormously important, but I don’t have a methodological religion.

I don’t think everything should be done with game theory, or with statistics, or with structural analysis, or with stories. Methods are tools, and some methods are good for some questions and other methods are good for other questions. I am driven by substantive questions, and I try to answer them as well as possible. This leads me to use different methods.

There is another reason I don’t think it is productive to get involved in abstract discussions about which is a good method and which is a bad method. As Kuhn suggested, people imitate exemplars rather than being persuaded by methodological preaching.⁵⁸ I have always believed that giving good examples works better than persuading with abstract ideas. So, if I want to persuade people that something is a good method, I use it in my research.

⁵⁷ Adam Przeworski, Contribution to “The Role of Theory in Comparative Politics: A Symposium,” *World Politics* Vol. 48, N° 1 (October 1995): 16-21.

Q: Yet you do seem to consider yourself a scientist.

A: Yes, I am a scientist. I believe that logical coherence and empirical falsifiability are essential criteria of science. What you say has to cohere logically, and it has to have observable consequences that can be shown to be true or false.

Models and Economics

Q: The tools of formal theory and game theory figure prominently in your work. At what stage in the research process do you begin modeling? What are you trying to accomplish with your models?

A: What normally happens is that I start thinking about a causal chain. For example, you have a society with a particular per capita income, income distribution, and degree of inequality. This society also has political institutions that determine how decisions are made. One way to start to model the society is to focus on the decisive political actor and this actor's income location. That's a classic model. Then you might ask, "What is going to happen to this society over time if it starts off poor and unequal? And, alternatively, what is going to happen to the society if it starts off poor and equal? How will income inequality and political institutions change in these different scenarios?" I immediately find that I can't sort out this kind of problem without writing down some symbols. I'm not smart enough to think about this causal chain without formalizing it.

Many years ago my friend Jon Elster taught me that informal, deductive arguments don't work. Some people are geniuses; you give them assumptions, and they can tell you the conclusions. When you do the mathematical model to check their conclusions, you see that they are right. I've known people like this, but that sort of informal deduction is beyond my capacity. Mathematics, somebody once wrote, is a tool for the stupid. Smart people know what consequences are implied by the assumptions. But I find it too confusing. So I start writing down

⁵⁸ Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1970).

symbols quite early into the process. Very often these symbols never appear in print. I do it just to clear my mind. My daughter, who knows more mathematics than I do, thinks I start modeling too early, that I don't think enough before I plunge into mathematics. She is probably right, because when you start formalizing there is a misfit between your intuition and the formalization, and the resulting model sometimes doesn't answer what you thought it would answer. But I have to formalize to clear my mind. I don't know how to think otherwise.

Q: Formal theorizing has not been part of the tool kit of comparativists until recently. How did you learn this way of thinking so early?

A: As a seventeen year old student in Poland, I was exposed to two years of tough, rigorous mathematical logic. I was taught to think deductively. That helped me when I came across Luce and Raiffa's book on game theory.⁵⁹ There was almost no training in formal theory when I was being educated as a political scientist. My greatest challenge has always been to keep up with my students. I always fear that I am just not capable of learning the new stuff. But my prior exposure to mathematical logic took away the fear of anything with symbols in it. In the end, it's just a matter of time, of allocating the time to keep learning new things.

Q: In the process of formalization, of building a model, are you able to achieve new insights and reach surprising conclusions?

A: Sure. I have found lots of surprising deductive results. For example, when I was working on my model of transitions to democracy, I came to the conclusion that if the hard-liners and reformers inside the non-democratic regime and the opposition to the regime all know everything, there will be no transition. I didn't see this until I wrote the model for it.

When you build a model you are not necessarily going to achieve results that are surprising in terms of your main intuition. Instead, the payoff very often comes in corollary,

lateral conclusions that you didn't think about. For example, I have been working on a model of the survival of democracy as a repeated conflict over wealth distribution.⁶⁰ I was trying to show that the probability that a democracy will survive increases at higher income levels. In the process, I discovered that poor countries cannot redistribute much income under democracy. That was completely surprising; I did not think about or anticipate it.

You do get surprises in modeling. But perhaps most often you just realize that your ideas are incoherent. I have been working on a model relating political accountability to economic growth with Jess Benhabib for more than a year now and it just does not want to cohere. We fix one argument and immediately discover that it is inconsistent with another. I think we have it right now, but in the process we discovered that many published models of "predatory state" are simply incoherent.

Q: In addition to using formal tools in theorizing, you've often drawn on the work of economists. When did you start reading economics?

A: Since about 1972. I was teaching a course on the Marxist theory of the state, a topic that had generated a great explosion of interest at the time. In 1969/70, there was an exchange between Miliband and Poulantzas,⁶¹ and the literature was evolving every year as new works appeared. I came to the conclusion that the Marxist theory of the state made no sense, because Marxist economics made no sense. During this time there were several critiques of Marxist economics and several theorems that showed that Marx's claim about the declining rate of profit under

⁵⁹ R. Duncan Luce and Howard Raiffa, *Games and Decisions: Introduction and Critical Survey* (New York: Wiley, 1957).

⁶⁰ Adam Przeworski, "Democracy as an Equilibrium." *Public Choice* 123(2005): 253-273.

⁶¹ Ralph Miliband, *The State in Capitalist Society* (New York: Basic Books, 1969); Nicos Poulantzas, "The Problem of the Capitalist State," *New Left Review* N° 58 (1969): 67-78; Ralph Miliband, "The Capitalist State: Reply to Nicos Poulantzas," *New Left Review* N° 59 (1970): 53-60.

capitalism was false. Elster, John Roemer and I came to the conclusion that the economic model underlying Marxist theories of the state made no sense.⁶²

That's when I decided to bite the bullet and learn some neo-classical economics. I was aided in the process by the fact that Michael Wallerstein, who had reached the same conclusion as I, was a student in my class. He went to the economics department and did their whole graduate program in economics. He basically taught me the rudiments of neo-classical economics. Since then I have been reading more and more economics. Today I read more things by economists than by political scientists, because a lot of economists do political science now. I recently published a textbook on political economy in which the main thesis is that you cannot do political economy unless you know economics.⁶³

Statistics and Data

Q: What role do statistics play in your research?

A: Things end with statistics. I do not turn to statistics until after I have learned enough history and achieved a clear set of hypotheses with prima facie plausibility that follow logically from some assumptions. Then I turn to statistics to see if the hypotheses are true or false. But let me emphasize an important point: I don't regard my statistical observations as anonymous "data points." In *Democracy and Development*, we studied 130 countries. Yet I can give you at least a half-hour story of the history of one hundred of them. I really do believe that you have to know the history of these places before you do statistics.

⁶² For Przeworski's assessment of Marxist theories of the state, see Adam Przeworski, *The State and the Economy Under Capitalism* (New York: Harwood Academic Publishers, 1990).

⁶³ Adam Przeworski, *States and Markets: A Primer in Political Economy* (New York: Cambridge University Press, 2003).

Q: One of the distinctive things about your work is that you largely produce your own data sets, whereas economists and political scientists, too, tend to download from the Internet data sets created by someone else. What are your criteria concerning data sets?

A: Economists are, by and large, careless about the data they use, especially the political data. I am a purist about data. First, I think that data carry in themselves theoretical and sometimes ideological baggage. With regard to the data on political regimes that I used in my collaborative work on democracy and development, we first defined very explicitly what we meant by democracy and what we did not mean by democracy. Only then did we start to collect data. We discuss our methodology in some detail.⁶⁴

Second, I very much believe that the data we generate should be reproducible by others on the basis of observations. Somebody who has the same information I do and who knows the rules I used to produce my data should be able to arrive at the same conclusions. I believe that results have to be reproducible from observations and rules.

These are my main criteria regarding data sets. I find some commonly used data sets do not meet these criteria. This is my quarrel with Freedom House, which I also find ideologically loaded. And this is my quarrel with the Polity data set.⁶⁵ Finally, data collection is an extremely messy operation, and, for this reason, you need to run all kinds of logical consistency checks. Very often the data sets are structured in a manner that allows you to do this. For example, if you have “votes by party” and then “total number of votes,” you can do a little check to see if the votes by party add up to the total number of votes. You’d be surprised, because these things often don’t add up.

⁶⁴ See Michael Alvarez, José Antonio Cheibub, Fernando Limongi and Adam Przeworski, “Classifying Political Regimes,” *Studies in Comparative International Development* Vol. 31, N° 2 (Summer 1996): 1-36; Adam Przeworski, Michael E. Alvarez, José Antonio Cheibub and Fernando Limongi, *Democracy and Development: Political Institutions and Well-Being in the World, 1950-1990* (New York: Cambridge University Press, 2000), Chapter 1.

⁶⁵ The Polity project offers annual data on all countries of the world, on regime and authority characteristics. The data can be accessed at <http://www.cidcm.umd.edu/inscr/polity/>

Q: In the 1960s, there was a surge of interest in the social sciences in generating data sets. This interest subsequently faded, and now we are again witnessing a strong interest in data collection. What explains this cycle?

A: It's a good question. I think the observation is correct that there was a major trend toward collecting aggregate data in the mid 1960s and early 1970s, that now there is a new trend in this direction, and that in the meantime not much attention was given to the issue. I really don't know why this is so. The mid 1960s was the age of factor analysis, and we had indicators of everything. That died out because it wasn't very informative. So perhaps the interest in data died out with factor analysis. I don't know. But the appearance of the Penn World Tables, widely used by growth economists since the mid-1980's, was an important occurrence. The Penn Tables gave us economic data at least. That's what convinced me to plunge back into the democracy and development stuff.

Narratives and Cases

Q: If one compares your 2000 book on democracy and development, to your 1985 and 1986 books on social democracy, ⁶⁶ it seems that you have moved away from using historical narratives in your research.

A: I do not think so. For my research on social democracy, I read a lot of writings and biographies of socialist leaders. I was trying to understand how these people saw the world, what choices they faced, and how they anticipated the consequences of their decisions. I thought that if I could put myself in their shoes, then maybe I could figure things out. So I read a lot of history. My method, to the extent I was aware of what I was doing, was almost a Weberian

⁶⁶ Adam Przeworski, *Capitalism and Social Democracy* (Cambridge: Cambridge University Press, 1985); Adam Przeworski and John Sprague, *Paper Stones: A History of Electoral Socialism* (Chicago: University of Chicago Press, 1986); Adam Przeworski, Michael E. Alvarez, José Antonio Cheibub and Fernando Limongi, *Democracy and Development: Political Institutions and Well-Being in the World, 1950-1990* (New York: Cambridge University Press, 2000).

verstehen method.⁶⁷ I was attempting to see the structure of choices from the point of view of the protagonists. The things I wrote had a substantial narrative component.

Then, for the particular question I addressed in *Democracy and Development* I thought I needed statistics. But in the work I'm currently doing on development, I am back to reading biographies of dictators and novels about dictators, which are very informative. I would like to get into Park's shoes and Mobutu's shoes and see why one of them was a developmental leader and the other was a thief.⁶⁸ My current hunch is that developmentalist dictators are those who loved their mothers: obviously this is not something you will learn or be able to test with statistics, but when you read novels and biographies, the pattern becomes uncanny. Note, by the way, that if this is true, counterfactuals entail something we cannot observe, selection on unobservables.

Q: You don't write case studies, as this methodology is conventionally understood. Yet you have published various articles on Poland. What role does Poland play in your thinking?

A: Because Poland is the country I know, it's the case I used for trying out abstract ideas. It is not that easy for me to think abstractly. So, I like to process abstract ideas through examples. Poland is the case I often used for this purpose. Also, when historical events, the rise of the Solidarity movement and the subsequent coup d'état, were happening in Poland, I got involved in studying Poland and writing some papers as political interventions. But, otherwise, it plays no particular role. I have recently been to Latin America more than Poland.

Q: How do you learn about the countries that interest you?

⁶⁷ *Verstehen*, a German word, is usually translated as "interpretive understanding." See Max Weber, *The Methodology of the Social Sciences*, translated and edited by Edward Schils and Henry Finch (New York: Free Press, 1949), pp. 160.

⁶⁸ General Park Chung Hee was the autocratic President of South Korea from 1961 to 1979. Mobutu Sese Seko was dictator of Zaire from 1965 until 1997.

A: I typically go to meetings abroad, where I have friends who grab me at the airport and eagerly say, “Do you know what’s happening?” And they tell me all about what’s happening. Then I go and sit for three days where people are delivering papers on Argentina, Kenya, Poland, or China, and I update. Conferences are a great way to do that. You are force-fed for three days, and you learn a lot. I learn by going to places and talking to people.

Q: Are there any countries you keep up with closely or on a regular basis?

A: To do comparative politics the way I do, that is, without a specific area focus, I have to keep up with and understand the complex realities of at least a few countries. For various reasons having to do with my personal history, I keep up with Argentina, Brazil, Mexico, Spain, France, Poland, South Korea, and Kenya. I visit these countries maybe once every two years, some more often. I also read about them systematically. And I have students there, who send me things that they and other people write. When I go abroad, I never interview people formally. But I do talk to people, including government officials. Many former social science colleagues who participated in the Wilson Center project on transitions to democracy as well as some of my former students are in governments these days, and I certainly talk to them. We meet over dinner. That’s how I keep up. But it’s only keeping up, it’s not the same as doing systematic research.

Q: You must have good language skills.

A: Polish is my native language. I can read and speak French and Spanish rather fluently, and I can get along in other romance and Slavic languages. I read novels in different languages. For example, I just finished a novel in Portuguese.

Non-academic Writing

Q: Some of your writing has been aimed at a broad non-academic audience. Do you make a conscious effort to produce more accessible versions of your work when you are attempting to reach a broader audience?

A: I almost always do. I very often write something technical for a smaller audience, and then, when I am really sure I have something, I write something less technical for a broader audience. I do try to write, from time to time, with the goal of making a political intervention. I wrote a piece that had a lot of echo in *The Journal of Democracy*, on neoliberal fallacies.⁶⁹ I wrote something for the *Boston Review* on democracy and the economy.⁷⁰ Another piece for *The Journal of Democracy*, on why democracies survive, as well as a collaborative volume, *Sustainable Democracy*,⁷¹ were intended as political interventions as much as anything else. Early on I wrote things on Poland that were deliberately politically aimed. I do see myself as a participant, even if marginal and ineffectual, in public life.

Colleagues, Collaborators and Students

Q: At the start of your career, you worked briefly at the University of Pennsylvania and at Washington University. Then you were at the University of Chicago from 1973 to 1995, and now you teach at New York University (NYU). Who were the colleagues you interacted with most closely at these places?

A: At Washington University, I learned an amazing amount from John Sprague. I learned dynamic models and all kinds of other things from him. At Chicago, I was very close to Philippe Schmitter. We always disagreed about basic things, and whenever we were both on a student's

⁶⁹ Adam Przeworski, "The Neoliberal Fallacy," *The Journal of Democracy* Vol. 3, N° 3 (July 1992): 45-59.

⁷⁰ Adam Przeworski, "A Better Democracy, A Better Economy," *Boston Review* Vol. 21, N° 2 (April/May 1996).

⁷¹ Adam Przeworski, Michael Alvarez, José Antonio Cheibub, and Fernando Limongi, "What Makes Democracies Endure?" *The Journal of Democracy* Vol. 7, N° 1 (January 1996): 39-55; Adam Przeworski *et al.*, *Sustainable Democracy* (New York: Cambridge University Press, 1995).

committee, they always suffered immensely. But Philippe and I certainly talked a lot and were friends. He left Chicago in 1982.

Then something very rare occurred at Chicago: we had the crystallization of a group of people who were both personal friends and intellectual interlocutors. This group even had an institution, The Center for Ethics, Rationality and Society, where “ethics” was Russell Hardin, “rationality” was Jon Elster, and I was “society.” It also included Stephen Holmes, Bernard Manin, Pasquale Pasquino, and others. Now almost everybody who was in this group at Chicago is in New York City. We still meet every Monday in the fall, hosted by John Ferejohn. We talk for two hours and then have a dinner. This is really the center of my intellectual life. We are probably somewhat exhausted with each other by now, because it has been a long time. But it is still thrilling and stimulating. It turns out that I interact more with philosophers than anyone else. But I also have some economist friends at NYU from whom I learn a lot, in particular Jess Benhabib. And in the department I talk to Neal Beck, who always finds something wrong with my presentation of statistical results.

Q: This rare moment at the University of Chicago came to an end when most of the group you were in moved to New York City. What was the reason for this exodus from Chicago?

A: We weren’t pushed. It wasn’t because of anything that happened at Chicago. We all left mainly for purely personal reasons. Russell Hardin moved first. Then Jon Elster and subsequently I moved. Jon wanted to move to New York City for personal reasons. I did, too. My wife was working at the OECD in Paris, and for 14 years I was commuting from Paris to Chicago. But then she got a job at the United Nations in New York, and this was a chance for us to live in the same city. Once Jon and I were here, that brought along Holmes, Manin, and Pasquino. They moved in part to be with us and in part because they were attracted by New York City. But it wasn’t anything about Chicago that led us to leave.

I think we all regretted leaving Chicago, because we all cherished that institution. Those were great days. It really was a place committed to the pursuit of ideas. You could walk into the office of the dean and say, “Look, I’ve been sitting on this project for five years. I’m sick and tired of it, and I’m close to finishing but I need some time off..” All the dean would say is “Write

me three pages telling me why you need time off.” And you walked out with time off. The administration was willing to put the money behind the intellectual goals. Chicago was a unique institution. Anyone who was ever at Chicago was smitten with it and has romantic feelings about it.

Q: Another group you’ve been closely involved with is the Analytical Marxism group. What was the basic agenda of this group?

A: The group was dedicated to subjecting Marxism to the scrutiny of the methods of contemporary social science. The idea was to take Marxism and see how much and what part of it holds up when you apply to it the same standards of inference and evidence applied to any other theory. Althusserian Marxism had this nice trick of having its own methodology, its own internal way of evaluating the validity of its theory.⁷² We broke with this approach and said, “No, you have to evaluate Marxism the same way as any other theory. It’s either coherent or incoherent, true or false.” I joined the Analytical Marxism group in 1979 or 1980—I think that was the group’s second year—and I stayed until the mid 1990s, when Jon Elster and I left. I very much enjoyed it and learned a tremendous amount from it. But I eventually left because I thought we had accomplished our intellectual program. We produced some important works that have lasted, including a reader by John Roemer, *Analytical Marxism*, Elster’s *Making Sense of Marx*, my *Capitalism and Social Democracy*, Gerry Cohen’s *Karl Marx’s Theory of History: A Defense*, and Roemer’s *A General Theory of the Exploitation and Class*.⁷³ We ultimately found that not much of Marxism is left and there really wasn’t much more to learn. So I left the Analytical Marxism group mainly for intellectual reasons.

⁷² Althusserian Marxism is a structuralist variant of Marxism that grew out of the work of French theorist Louis Althusser. The two classic texts are Louis Althusser, *For Marx* (London: Verso/NLB, 1968) and Louis Althusser and Etienne Balibar, *Reading Capital* (London: Verso, 1969). For an overview, see Ted Benton, *The Rise and Fall of Structural Marxism: Althusser and His Influence* (London: Palgrave Macmillan, 1984).

⁷³ John Roemer (ed.), *Analytical Marxism* (Cambridge: Cambridge University Press, 1986); Jon Elster, *Making Sense of Marx* (Cambridge: Cambridge University Press, 1985); Adam Przeworski, *Capitalism and Social Democracy* (Cambridge: Cambridge University Press, 1985); G.A. Cohen, *Karl Marx’s Theory of History: A Defense* (Princeton: Princeton University Press, 1978); John Roemer, *A General Theory of Exploitation and Class* (Cambridge, MA: Harvard University Press, 1982).

Q: During all the years you have worked at US universities, have you been in touch with other Polish émigrés?

A: Only with friends from childhood, most of who now live abroad. I never felt comfortable in the Polish culture, which is intensely nationalist, thoroughly Catholic, and highly intolerant. I was brought up as a Catholic, but at a very young age I revolted against both Catholicism and Polish nationalism.

Q: Some people find it strange that you didn't follow the usual trajectory of academics coming from Poland, which is to abandon Marxism and even become virulently anti-Marxist. Instead, you became something of a Western Marxist. Why, in contrast to many other Polish émigrés, did you not reject Marxism?

A: I have to think about this. Let me start by saying a little bit about Western Marxists. In 1978, at the International Sociological Association's (ISA) World Congress in Uppsala, Sweden, there was a big roundtable on development, and I presented a paper entitled "Capitalism: The Last Stage of Imperialism," which was basically turning upside down Lenin's famous argument that imperialism was the last stage of capitalism.⁷⁴ I was arguing, supporting Karl Kautsky, that imperialism is just a way for capitalism to penetrate other countries. Once this penetration has been achieved, capitalism reproduces itself so that you don't need imperialism any more. A Russian on the panel got totally incensed and said, "Vladimir Illich Lenin said, 'Imperialism is the last stage of capitalism.' This guy says 'Capitalism is the last stage of Imperialism.' *Ne vozmozhno*," which in Russian means, "you can't do that." There was general consternation in the room. A Polish Marxist friend took this guy apart and explained to him what I had and had not said. Finally, the Russian concluded that I was '*tsntij Markist*,' or "*their Marxist*," by which he meant a Western Marxist.

⁷⁴ V. I. Lenin, *Imperialism: The Highest Stage of Capitalism*, new, revised translation (New York, 1939).

I found myself in these kinds of situations not infrequently. I never thought of communism as an implementation of Marxism. I saw communism as a bureaucratic regime that betrayed the working class. I never had pro-communist sympathies; I was a Marxist opponent of communism. As I mentioned earlier, this got me into trouble in Poland in the mid-1960s when I participated in a study group that criticized the communist party for oppressing workers. I found myself at the extreme opposite side of this coin, so to speak, when, in the early 1990s, I saw that neo-liberal economic policies were not, in fact, an application of neo-classical economic theory. There is no support in neo-classical economics for neo-liberalism. As you can see from these examples, I went to the sources and tried to distinguish theory from ideology. So, I was an anti-communist and also a Marxist.

Q: You have co-authored a lot of publications. Could you discuss the people you have collaborated with and why you seek out collaborators?

A: I'm a collaborator by nature, so there are quite a few. I collaborated with my colleague, Henry Teune, when I was at Penn. I collaborated with John Sprague at Washington University, which drove me crazy. John is the least disciplined person I know, and I am one of the most self-disciplined persons I know. But, still, I had a lot to learn from John, so we wrote another book, joined by an old Chilean friend of mine, Fernando Cortés. I collaborated on a book with Luiz Carlos Bresser Pereira and José Maria Maravall, who both had experience of having been ministers in their respective countries, and I learned from them how to think in policy terms. I am now writing some papers with an economist at NYU, Jess Benhabib, from whom I have learned how to think about economic growth. However, most of my collaborators were my graduate students. I think my main source of learning is teaching. And my main interlocutors throughout my life have been my graduate students. I've always run a sort of natural science laboratory with students who took courses from me, got interested either in things similar to what interested me or in some aspect of projects I was working on, and we collaborated. I have continued to collaborate with some of them after they graduated. Working on *Democracy and Development* with Mike Alvarez, Ze Cheibub, and Fernando Limongi was pure pleasure, on a personal as well as intellectual level.

Collaborators straighten you out. It's more than one mind at the same time. For example, when I was working with Michael Wallerstein, I would be going on about something, and he would look at me with his characteristically sweet smile and say, "Are you certain this is true?" I would immediately realize that I was speaking nonsense. So collaborators are good at tempering your enthusiasm. They are particularly important when you do formal work. Everybody makes algebraic mistakes, and you need people to put things on the blackboard so the other person can check if it is true. Otherwise, you end up making mistakes. This is why formal work is so often co-authored. It is just too hard to do it alone. Finally, collaborators are useful because the amount of work is often too big for one person to handle. If you are collecting data, it's next to impossible to do it all by yourself. It's too time consuming. I am now engaged in yet another massive data collection, again with a group of four graduate students.

Q: How do collaborative projects get started?

A: Typically what happens is that I start talking to somebody and find that they had ideas on the same topic or something original to tell me. And somebody, often it was myself, would say, "Why don't we do it together?" With the project that resulted in *Democracy and Development*,⁷⁵ I basically walked into class and said, "This is what I am about to do. If somebody wants to join, join." Essentially, you look for somebody with whom you get along with personally and who you think is smart, hard-working, and disciplined. If people are undisciplined and don't do their part, you go crazy. By and large, I just really love to collaborate.

Q: How does the actual writing process proceed when you collaborate?

A: In every collaboration, somebody does the first draft, then we talk about it. Somebody else does a re-write, and then it goes back and forth, back and forth. Sometimes somebody writes one

⁷⁵ Adam Przeworski, Michael E. Alvarez, José Antonio Cheibub and Fernando Limongi, *Democracy and Development: Political Institutions and Well-Being in the World, 1950-1990* (New York: Cambridge University Press, 2000).

section, somebody else writes another section, then we merge it, and somebody re-writes it. And it typically goes through many re-writes. With *Democracy and Development*, I wrote the first and last drafts by myself. But that was mainly because it was a book, and we were afraid that if different people drafted different chapters, the style would be uneven.

Q: You have trained lots of graduate students. What's your approach to teaching graduate students?

A: First, I do "train" them. I subject graduate students to a systematic program. What typically happens is that a student says he or she wants to study with me. I ask them what they want to do. Then I ask what they know, and then I tell them, "Here is what you need to learn in order to do what you want to do." These days what they need to learn typically consists of some philosophy, some economics, and quite a lot of statistics. So my students get a systematic training by others.

In addition, I have always taught an introduction to something. For many, many years I taught a course called "Marxist Theories of the State," which evolved into "Theories of the State," and then into "Political Economy." I may not teach this course anymore, because I already published a textbook on the subject.⁷⁶ I don't think I can teach what I've already written. In any case, students typically take this introductory course. I also teach advanced courses, usually about whatever I am working on or about some methodological aspects I think students should learn and cannot get from others. For example, I recently taught a course called "Statistical Methods of Comparative Research," which focused on selection bias.

I don't teach facts. My view is that students should learn facts by themselves, by reading history. But I do force all my foreign graduate students to take an American Government course. And unless they are especially strong-headed and committed, I don't allow them to write about their own country for a long time.

Students acquire all these skills and then they formulate a research project. And I supervise them quite tightly. I usually run a doctoral seminar. One of the things I discovered a long time ago is that graduate students in the US are left alone at the very time that they most

need interaction with their advisers and other students. In the US, graduate students finish their coursework, defend their proposal, their funding typically ends, and then they are on their own. That's when you most need to speak to others, hear others, and learn new techniques you may need to use for your dissertation. So I have always kept some form of interaction framework for advanced students. I always encourage them to participate in seminars, to talk to others, and to present their work.

That's basically my model for training graduate students. I have been at this for a long time, and I think I know how to do it. I may have chaired more dissertations than anybody else in the discipline—the number is approaching fifty. I don't like teaching undergraduates, mainly because one has to motivate them – they have other preoccupations than learning in their lives -- and there is little one can learn from them. But I love training graduate students.

Q: What is your view of the level of interest in politics among graduate students today?

A: The people who entered graduate school during the Vietnam era, the generation of the American cultural revolution, had gone through quite a lot in their lives. They had intense feelings about politics, culture, and society. They usually had done something else, often political organizing, and were going back to school to reflect on their experiences, often seen as failures. Very often they were not teachable, mistrustful of “positivism,” hostile to rigorous method. This was very particularly characteristic of students from Latin America, who just knew that the US was imperialist and did not think there was anything to learn here. But they deeply cared about politics; they studied politics because they wanted to change the world.

Today the situation is different. These kids, and they are kids, who are now in graduate school, by-and-large, have grown up in exceptionally peaceful, prosperous, and non-conflictive times. These students are smart, well educated, and eager to be taught. But they have no passions or interests. And it's not just the Americans. I get students from Bogazici or Bilkent, Turkey's elite private universities, and from Di Tella and San Andrés, Argentina's elite private universities. And they are indistinguishable from the daughters of doctors from Iowa. These kids

⁷⁶ Adam Przeworski, *States and Markets: A Primer in Political Economy* (New York: Cambridge University Press,

absorb education and all the skills easily, but when the moment arrives when they are supposed to start asking questions, they have nothing to ask. They want to be professionals, and they think of their task as writing articles and books, rather than saying something about the world, not to speak of changing it.

Q: What can be done to trigger greater passion in graduate students today?

A: I don't know whether there is any kind of awakening experience. I certainly believe that Americans who study comparative politics in any form or fashion, even if it consists of doing models and statistics, should go somewhere and experience daily life abroad to see what it feels like. But I don't know whether that is sufficient.

The Achievements and Future of Comparative Politics

Findings and Knowledge Cumulation

Q: If you look at where the field of comparative politics was 30 years ago and where we are now, what are the main things we have learned?

A: Let me preface my answer with one caveat. I think that some of the best research in comparative politics is done these days by economists, so I will include them in my answer. Daron Acemoglu and James Robinson, Alberto Alesina, Roland Benabou, Jess Benhabib, Torsten Persson and Guido Tabellini, and many others do excellent work in comparative politics. They typically don't know enough about politics, but they address central questions and get answers. With that inclusion, yes, I think there has been a tremendous accumulation of knowledge.

What have we learned? Ever since Duverger's and Rae's seminal books,⁷⁷ we have learned a lot about the consequences of electoral systems. Cox's book, *Making Votes Count*, is the latest example of it.⁷⁸ We know how the electoral systems interact with social cleavages to produce parties, how they affect the distributions of votes, and so on. We've learned a lot about coalition formation and cabinet formation; there is a formal and an empirical literature on these topics. We understand much more about the legislative process. We've learned a great deal very rapidly in the last few years about ethnic conflict and ethnic peace. We have learned that most of the time ethnic groups live together in peace, and perhaps we are beginning to understand some mechanisms that explain this finding. Finally, I think we understand much more about the processes of regime transitions. I could go on.

More broadly, one test of the advances we have made is that when a student raises a topic with me, most of the time I can say "Read this, read that, here is the literature that says this and that." On various topics, the conclusions do not converge. But at there are bodies of literature on a variety of topics.

Q: Are there any topics on which we have not made significant advances?

A: We still do not know why and when people with guns obey people without them: the determinants of civilian control over the military. We still don't understand political parties very well. This is truly an important topic, which we have neglected. We don't understand why parties come into existence, what mechanisms hold them together, and what the glue of party discipline is. Though we have learned a lot in general about authoritarianism, I also think we know disastrously little about the structure of dictatorships. Perhaps most importantly, in spite of a flood of writing on this topic, we still do not understand how democracy can be compatible with poverty and inequality.

⁷⁷ Maurice Duverger, *Political Parties* (London: Methuen & Co., 1964); Douglas W. Rae, *The Political Consequences of Electoral Laws* (New Haven: Yale University Press, 1969).

⁷⁸ Gary Cox. *Making Votes Count: Strategic Coordination in the World's Electoral Systems* (New York: Cambridge University Press, 1997).

I also think we are not doing well with globalization. I've written something on it recently,⁷⁹ so I was forced to read the literature. I found it deeply unsatisfactory. In particular, the political consequences of globalization are poorly understood. I think the problem, in part, is that we need some kind of methodological breakthrough in this area of research. The methods that are currently used don't do well enough. The findings are disparate, and most of them are based on statistical methods that assume that observations of particular countries are independent. So it is hard to believe the statistical findings. This is a big, important topic. Somehow we are going to have to start thinking differently and pay more attention to the sort of methods that would be appropriate for studying this issue.

Generally, to a large extent because of the availability of data, we know more about the OECD countries than about the less developed ones. But this gap is rapidly closing.

Q: Are there any other methodological problems that are holding back inquiry in comparative politics?

A: To elaborate on my prior answer, studying things in an interdependent world is an open methodological problem. There I think we don't have answers yet. We have this notion of two-level games, for example.⁸⁰ But how do you estimate such models? How do you test hypotheses that countries are interdependent, but in each country there is some conflict. It is very hard. I think globalization is a big, open methodological issue in general.

Another central methodological issue is how to study things historically, how to study history. The new institutionalism contains a potential contradiction when it asserts simultaneously that institutions matter and that they are endogenous. If they are endogenous, then we need to sort out the effects of institutions and of the conditions under which they function. The central methodological problem in comparative politics is selection bias, and,

⁷⁹ Adam Przeworski and Covadonga Meseguer. "Globalization and Democracy," paper presented at the Seminar on Globalization and Inequality, Santa Fe Institute, 2002.

⁸⁰ Robert D. Putnam, "Diplomacy and Domestic Politics: The Logic of Two-level Games," *International Organization* Vol. 42, N° 3 (1988): 427-460; Peter Evans, Harold Jacobson and Robert Putnam (eds.), *Double-Edged Diplomacy: An Interactive Approach to International Politics* (Berkeley, CA: University of California Press, 1993).

while we do have methods for handling this problem, different methods are based on different assumptions and often generate disparate conclusions. This is true of statistical studies of the impact of institutions in general, but it becomes particularly prominent when we study history. If everything is path-dependent, then it makes no sense to speak of the impact of institutions. To identify their impact, we need to think more systematically about counterfactual histories in which different institutions would have existed under the same historical conditions.

Q: You have emphasized the methodological difficulties of addressing complex questions in a rigorous fashion. Another reason why progress on such questions might not be made is that comparativists simply fail to pose big, interesting questions about politics in the first place.

A: What is it that we are not asking? Certainly, we are not asking, “So what does all that we do know add up to?” But we also fail to ask several questions that are researchable with the methods we have. What determines the access of moneyed interests to politics? What is it about our democratic institutions that makes people feel politically ineffective? Why is it that these institutions perpetuate misery and inequality?

There is a saying in my native language, “It is not the time to cry over roses when forests are burning.” And as I talk to people in Argentina, France, Poland, or the United States, I hear that they are burning. People around the world are deeply dissatisfied with the functioning of democratic institutions, in the more as well as in the less developed countries. They see politicians as serving interests of the rich, of corporations. They cannot understand why democratic institutions seem to be impotent in reducing glaring and persistent inequalities. They feel that political parties do not serve as a mechanism of transmission of their values and interests. They perceive that important decisions are made by institutions, often international, over which no one has control.

The danger is that unless we keep asking such questions, we leave the answers to demagogues of different ideological stripes. I was struck on a visit to Argentina that the entire political discussion is polarized between neo-liberals, who believe that “the market” is the demiurge of everything, and neo-populists, who believe that the demiurge is “the people,” in its eighteenth century singular.

The entire structure of incentives of academia in the United States works against taking big intellectual and political risks. Graduate students and assistant professors learn to package their intellectual ambitions into articles publishable by a few journals and to shy away from anything that might look like a political stance. This professionalism does advance knowledge of narrowly formulated questions, but we do not have forums for spreading our knowledge outside academia; indeed, we do not talk about politics even among ourselves. It has been decades since professional journals—”professional” is what they are called—published essays on “What is wrong today with the United States, with democracy, or what not?” or on “How to make the world better?” As far as I am concerned, we would be saying more if the *American Political Science Review* were simply closed.

Rational Choice Theory

Q: Given what you have said about training graduate students, it seems clear that you support the incorporation of game theory as a standard tool in comparative politics.

A: I send my students to take game theory courses because I think it is essential, a tool for everybody to have in their pocket, which doesn’t mean that you pull it out in all circumstances. I once had a Chinese student whose father had participated in the Long March and later became a Chinese communist notable. He wrote a dissertation about the Chinese revolution based on intimate knowledge of the case and access to provincial archives that nobody could access before. He did an incredible amount of historical digging. But he also had a game theory model. He was interviewed for several jobs in this country, and at one place he was told that he would have gotten the job if he had not used game theory. This was several years ago, and now, fortunately, that kind of bias is gone. One of the striking things about the job ads in comparative politics over the last two years is that they almost all call for applicants with broad comparative interests *and* methodological training. This is an evolution that is here to stay. I think it’s a trend that is long overdue.

Q: At the same time, you've written critical things about rational choice and game theory.⁸¹

A: Sometimes game theory is a useful tool, but other times it is not. I'm skeptical about game theory in two ways.

First, I am quite willing to believe that sometimes people do not act strategically; I'm not even going to say "rationally," because that is a narrow and very demanding notion. People are not always consequentialists, by which I mean they do not always do things because they look toward the future and see the consequence of their action. People very often have deep beliefs and will not admit anything that is inconsistent with those beliefs. They feel so passionate that they are going to do things regardless of the consequences. I remember that when we were trying to distinguish different strategic types within authoritarian regimes – "hardliners," "reformers," etc. – in the transitions project, Fernando Henrique Cardoso remarked "But do not forget the stupid ones" (*ontos*). More generally, game theory starts with preferences, and we do not know what they are. It works, I think, when there are plausible reasons to impute motivations to particular classes of actors. It makes sense with regard to "consumers," who want to maximize consumption and leisure. It works with "landlords and peasants," "unions and firms." But it fails with "individuals" or "voters": people have so many different motivations that no simple assumption can characterize all of them. In a nutshell, I think that game theory works when it is accompanied by good sociology, when one can make reasonable inferences from positions in some structures of interdependence to motivations of actors who occupy these positions.

Secondly, game theory generates many equilibria and one consequence is that it provides poor theories of history. Dynamic game models typically rely on ad hoc selection of equilibria. Again, sometimes they work and sometimes they do not.

Analytical Narratives and Comparative Historical Research

Q: One attempt to introduce game theory into comparative politics takes the form of analytical narratives, as proposed in the book by Bates et al.⁸² Elster was quite critical of this book in his review in the *American Political Science Review*.⁸³ Do you share Elster's concerns?

⁸¹ Adam Przeworski, "Marxism and Rational Choice," *Politics and Society* Vol. 14, N° 4 (December 1985): 379-

A: I see *Analytical Narratives* as less path-breaking than its authors do, but I am sympathetic to the main intent of the analytical narratives project, which is that case studies should be theoretically informed and theoretically informing. I have nothing against studying cases. I think you can learn a lot by studying Poland or Argentina. But I want to know what general hypotheses are relevant to the particular case studies.

Let me add two further points. First, narrative does not necessarily have to take the form of game theory. Second, when you do case studies, you need to know where your case is located in the broader context of other cases. So, I say, “Do a regression before you do case studies. Then look at cases along the line first. After that, look at some outliers, because they may be illuminating about specific conditions.” Here’s an example. I think Guillermo O’Donnell’s piece, “State and Alliances in Argentina,” is brilliant.⁸⁴ I always give it to my students as *the* country study. Yet Argentina is a unique case. If one does, as I eventually did, regressions of various kinds for the whole world, you find that Argentina is always standard deviations out.⁸⁵ It had, by far, the largest number of regime transitions of all countries. And it had democracies that did not survive when the country was relatively wealthy. In fact, the wealthiest instances where democracy fell are Argentina in 1976, Argentina in 1966, and Argentina in 1962. Argentina was among the ten most developed countries in the world in 1900, but now it’s in the doldrums. It’s the weirdest country in the world. What does this all mean? It means that when you start theorizing on the basis of Argentina you are going to get very little generality. This is why my first principle on political narratives is to locate the case in the broader context.

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⁸² Robert Bates, Avner Greif, Margaret Levi, Jean-Laurent Rosenthal, and Barry Weingast, *Analytical Narratives* (Princeton: Princeton University Press, 1998).

⁸³ Jon Elster, “Rational Choice History: A Case of Excessive Ambition,” *American Political Science Review* Vol. 94, N° 3 (September 2000): 685-95.

⁸⁴ Guillermo O’Donnell, “State and Alliances in Argentina, 1956-1976,” *Journal of Development Studies* Vol. 15, N° 1 (October 1978): 3-3.

⁸⁵ Adam Przeworski, Michael E. Alvarez, José Antonio Cheibub and Fernando Limongi, *Democracy and Development: Political Institutions and Well-Being in the World, 1950-1990* (New York: Cambridge University Press, 2000), pp. 99-101.

Concerning Elster's review of *Analytical Narratives*, I think he was critical for the wrong reasons. Elster has a way of weighing every criticism evenly. His typical critique of papers goes like this, "I have eleven points. Point number one is that on page three you made this mistake. Point number two is that everything you said is badly formulated. Point number three..." So, he has sort of a laundry list approach. I am persuaded that he was right on many historical points; the contributors to *Analytical Narratives* didn't get their history very right. But I don't think Elster grappled with their intent.

Q: Another approach to comparative politics that focuses centrally on cases and history is comparative historical analysis, which is often inspired by Barrington Moore's *Social Origins of Dictatorship and Democracy*.⁸⁶ What is your view of this literature?

A: What bothers me about Barrington Moore is the sense of action at a distance I get from his work. Moore's work has causes that are three centuries ago and consequences that are fifty years ago. What happened in between? I was never persuaded by *Social Origins of Dictatorship and Democracy*. There is no question that it's a beautiful book: broad and erudite. But I was never persuaded by its analysis of causal mechanisms. More generally, I am typically not persuaded by the macro-comparative historical sociology. As John Roemer observed (in the "Introduction" to *Analytical Marxism*), while we want to establish regularities at the macro level, their explanation must be formulated at the micro level: someone must be doing something to bring the macro state about. The macro-comparative historical sociology fails to provide such causal mechanisms.

I do not find this literature very useful as a source of information either. One thing I discovered while trying to collect data is that we don't have good political histories. In this regard, I found macro-historical sociology useless in terms of information. Much of it happens at the level of mysterious actors. Macro-historical books provide very few dates, names and places. They analyze collective actors, like peasants, landowners, and the bourgeoisie, who march

⁸⁶ Barrington Moore, Jr., *Social Origins of Dictatorship and Democracy: Lord and Peasant in the Making of the Modern World* (Boston: Beacon Press, 1966).

through history without dates and places. From the factographic point of view, I find this literature uninformative.

Methodological Standards and Comparative Politics Inside and Outside the United States

Q: During the last decade, methodological issues have been given much attention within comparative politics. What do you think is behind this change?

A: I have a rational choice explanation for this trend. I think Americanists in political science departments, who are more methodologically oriented, started to put pressure on people in other sub-disciplines to beef up their methodological standards. In most departments the methodological development in comparative politics was pushed down the throat of area studies people. Americanists, because they were born and raised in the country they study, don't have to learn the language, history and culture of other societies. So they could spend their time learning theory and methods. In turn, comparativists are in the unenviable situation where they often have to learn both. You have to learn Turkish, the history of Turkey, and so on. And then you also have to learn the theory and methods that Americanists learn. But few comparativists did this, at least among those doing area studies work. At a certain point, I think Americanists revolted, because departments had double standards.

We had a tenure case once at the University of Chicago, of a person who did first-rate research on the Soviet Union. For two years this person sat in regular meetings of a local cell of the Communist Party and saw from the inside how it worked. This research was ethnographically impressive. But it had no question, no method, no conclusion. It was pure ethnography. When the tenure case came up, one of the people we asked for letters was an economist, who also studied the Soviet Union. And he wrote to us saying, "I think what's involved here is whether you want to have one standard or two. We economists abandoned this kind of ethnographic stuff, and we have one standard for everybody. But you may want to have two." He wasn't encouraging us one way or another. He was just saying, "This is what your decision is." And I

think this case illuminates what happened in political science departments across the country. Basically, Americanists said, “We want to have one standard.”

Q: Is this healthy for comparative politics?

A: I think it’s very healthy. I don’t think we have coped with it institutionally, because having a single standard implies that comparativists have twice as much work to do as Americans who only study the United States. The changes are inevitable and beneficial, but costly.

Q: What are the implications of this imbalance for the future of comparative politics?

A: It will mean that, as in the past, foreigners educated in the US will play a key role in the development of the field. If you look at the history of American comparative politics, you will find that many of the eminent comparativists are or were foreigners: Karl Deutsch, Guillermo O’Donnell, Leonard Binder, Juan Linz, Ari Zolberg, the list goes on.

Q: What about the contributions of Americans to comparative politics?

A: Well, let me say something that is going to shock and offend most of my area studies colleagues. I have strong feelings against studying foreign countries. When I lived in Poland and I saw foreigners, mostly Americans, come to Poland and study Poland, I thought these people didn’t have any idea of what they were doing. They were framing their studies in terms of American ideological issues, and thus they did not address problems that we Poles, or Polish social scientists, saw as fundamental. They were just exporting American ideological fantasies.

I am extremely guarded about the American conception of comparative politics as one where Americans go out and study other countries. The field of comparative politics is strange. When Americans study the United States they do American Politics, and when Americans study Brazil, they do comparative politics. Now, I ask myself, “What do Brazilians do when they study

Brazil?" This is not to say that Americans have not done very good work on particular countries. One could go on citing and citing. Sometimes they did studies that were seen as important contributions within the countries they studied: Schmitter's work on corporatism in Brazil as well as Alfred Stepan's book on the military in Brazil are books that Brazilians see as fundamental contributions to the understanding of their country.⁸⁷ But I suspect this kind of work is quite rare.

These days in particular, though it has been true for a very long time, US-trained foreigners are much better at studying their countries than Americans ever will be. I had Argentine, Korean, Chinese, and Brazilian students, who are first-rate social scientists by every criterion. They went back to their countries and do excellent work, better than most foreigners will ever be able to do. There is no reason why studying the world should be a monopoly of the United States. It's not to say that knowledge produced by Americans is not useful to people in other countries. But, at some point, we need to start thinking about the study of comparative politics as an enterprise in which we collaborate, exchange views, and perhaps provide some resources to people studying their own countries, rather than playing this parachuting game.

Conclusion

Q: You have had a long, prolific career, yet you keep on pushing yourself into new areas and learning new things. What keeps you going?

A: To some extent, it is a question of tolerance for pain. Nowadays this is especially true for us older people. All these kids know things that you don't, and there are so many technical gimmicks around that you know you should be using but don't know how. You are never certain if you are still able to learn these things or if it surpasses your abilities. So, every time you plunge into something new, you feel the pain. But, obviously, I like what I am doing, and maybe I don't know how to do anything else. I guess I just like doing research. I also have strong

⁸⁷ Philippe C. Schmitter, *Interest Conflict and Political Change in Brazil* (Stanford: Stanford University Press, 1971); Alfred Stepan, *The Military in Politics. Changing Patterns in Brazil* (Princeton: Princeton University Press, 1971).

political feelings, and a lot of my work is driven by that. I think of myself as making interventions in political debates, and I believe the quality of that intervention matters. So that is obviously part of the motivation to keep going.

Q: What are your research plans for the years ahead?

A: I only have medium-term plans now. The big thing that will keep me busy is what has kept me busy for a long time: democracy, development, and income distribution.

I am engaged in two projects and I am not yet clear how they are related. I want to examine democracy from the perspective of its founders. It is obvious to me that democracy is not what the “founders” in different countries intended it to be and expected it to be. So my question is why. Was the original project unfeasible? Or did things take an accidental turn? As often, I have a political motivation: I want to know why democracy has not generated more economic equality, more effective political participation, and a better balance between order and freedom. Is it inherent in democracy and thus irremediable? What are the limits of democracy: how much equality, how much effective participation, how much liberty can any democratic system generate at its best?

The second aspect of this project entails collecting historical data. For various reasons, I have become persuaded that to make sense of the recent developments, one has to go back farther in history than I have or we have. For example, as I already mentioned, the stability of the post-1950 political regimes appears to depend on their entire regime history. And if one wants to examine the impact of political institutions on development, one cannot jump centuries, assuming that institutions never change. Hence, I want to return to the relation between political institutions and economic development by taking a longer view.

Q: What’s your advice to a young graduate student just starting out today?

A: This turns out to be a very hard question for me to answer, for reasons that may have become apparent from what I lamented earlier. I think that our system of incentives, and the equilibrium

culture that emerges from our institutional set up, promotes narrowly conceived thinking, entailing little risk, and saying nothing that may be politically controversial. Rewards lie with “professionalism.” And a lot of students enter the graduate study of political science because, while they have some superficial interest in politics, they think that academic jobs provide safe incomes and good life. I would love to say, “Think big,” “Take risks.” But this would be cheap advice: I already have a safe job at a good university. So I do not give advice: I explain what I think the choices are and leave it to each to decide.