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The Washington Consensus in History: An Interview with John Williamson

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By Kurt Schuler, Theresa Williamson, and Robert Yee*

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The Washington Consensus in History: An Interview with John Williamson

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John Williamson is best known for having coined the term “Washington Consensus” to describe conditions that Washington-based institutions (the International Monetary Fund, World Bank, and U.S. Treasury) have considered ideal for developing countries. He has also done influential work on exchange rates, adjustment to external imbalances, and other macroeconomic topics. Until 2012 he was Senior Fellow at the Peterson Institute for International Economics. His other positions include Chief Economist for South Asia at the World Bank and professor at several universities. In this 2018 interview, he gives an overview of his life and work and goes into some detail about key ideas he has developed.

JEL codes: B31, F30.

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Chronology and Select Writings

| | |
|-----------|---|
| 1937 | Born in Hereford, England |
| 1955 | Graduated from Hereford High School for Boys |
| 1958 | Graduated from the London School of Economics (B.Sc., Economics) |
| 1962–1963 | Teaching Assistant, Princeton University |
| 1963 | Graduated from Princeton University (Ph.D., Economics); dissertation: “Patent Licensing and Royalty Terms” |
| 1963–1968 | Lecturer, University of York (subsequently Reader) |
| 1965 | <i>The Crawling Pea</i> , first monograph |
| 1968–1970 | Economic Consultant to the UK Treasury |
| 1967 | Visiting Assistant Professor, Massachusetts Institute of Technology |
| 1970–1977 | Professor, University of Warwick (subsequently Honorary Professor) |
| 1972–1974 | Adviser to the International Monetary Fund |
| 1972 | “Surveys in Applied Economics: International Liquidity,” <i>Economic Journal</i> |
| 1977 | <i>The Failure of World Monetary Reform, 1971–74</i> |
| 1978–1981 | Professor of Economics, Pontifícia Universidade Católica do Rio de Janeiro |
| 1980 | Visiting Professor, Massachusetts Institute of Technology |
| 1981–2012 | Senior Fellow, Institute for International Economics (later Peterson Institute for International Economics) |
| 1983 | <i>The Exchange-Rate System; The Open Economy and the World Economy</i> (textbook) |
| 1987 | <i>Political Economy and International Money</i> (editor) |
| 1990 | <i>Latin American Adjustment: How Much Has Happened?</i> (editor) |
| 1992 | Visited London School of Economics (Professor) |
| 1994 | <i>Estimating Equilibrium Exchange Rates; The Political Economy of Policy Reform</i> (editor) |
| 1995 | <i>What Role for Currency Boards?</i> |
| 1996–1999 | Chief Economist for South Asia, World Bank |
| 1996 | <i>The Crawling Band as an Exchange-Rate Regime</i> |
| 2001 | Project Director, United Nations High-Level Panel on Financing for Development |
| 2003 | <i>After the Washington Consensus</i> (edited volume) |
| 2004 | <i>Dollar Adjustment: How Far? Against What?</i> (with C. Fred Bergsten) |
| 2006 | Visited London School of Economics (Researcher) |
| 2007-2008 | Houblon-Norman Fellowship with Bank of England |
| 2012 | Retired from Peterson Institute; honored with Festschrift, <i>Global Economics in Extraordinary Times: Essays in Honor of John Williamson</i> |
| 2015 | <i>International Monetary Reform: A Specific Set of Proposals</i> |
| 2017 | <i>Growth-Linked Securities</i> |

John Williamson retired in 2012 as Senior Fellow at the Peterson Institute for International Economics. Self-described as a “saltwater” economist,¹ he has been influential in his work as the Project Director for the United Nations High-Level Panel on Financing for Development (2001) and the Chief Economist for South Asia at the World Bank (1996–1999). He has been a professor at the London School of Economics, Pontifícia Universidade Católica do Rio de Janeiro, University of Warwick, Massachusetts Institute of Technology, University of York, and Princeton University.

Williamson is best known for having coined the term “Washington Consensus” to describe a set of conditions which Washington-based institutions (the International Monetary Fund, World Bank, and U.S. Treasury) have often considered ideal requirements for developing countries. He has also authored, co-authored, and edited a number of books and papers on international monetary policy and economic development, which include *Growth-Linked Securities* (2017), *International Monetary Reform: A Specific Set of Proposals* (2015), *Who Needs to Open the Capital Account?* (2012), *Curbing the Boom-Bust Cycle: Stabilizing Capital Flows to Emerging Markets* (2005), *After the Washington Consensus: Restarting Growth and Reform in Latin America* (2003), and *Latin American Adjustment: How Much Has Happened?* (1990).

The transcript has been edited for clarity, content, and style.

My name is Kurt Schuler and I’m with Robert Yee. We are interviewing John Williamson at his home in Chevy Chase, Maryland on September 29, 2018. John’s daughter, Theresa, is also present. Over the previous year she conducted an in-depth interview with John. Certain parts of that interview dealing with John’s professional life have been incorporated here. John has given other interviews over the years, such as one that was part of the World Bank’s Oral History project, about his time as the Chief Economist for its South Asia division.² This interview is meant not to overlap too much with those, but we would like to make it so that someone who is not familiar with John’s work can obtain a brief overview of his research. We also want to go into depth on his ideas so that other economists can benefit from his deep insights over his career.

EARLY LIFE AND EDUCATION

Could you tell us a little about your childhood and life at home?

I was born at home. My father was a rosegrower/nurseryman who had started his business when a big nursery went bust in the Great Depression and he bought a corner of it. My mother and elder sister³ (who grew up to be a noted linguist) were my main role models.

We lived in the same location throughout my childhood, which was dominated by the war [World War II], which involved frequent absences by my father. Rationing occurred not just during the war, but for many years after. The only time I went abroad was through a school-organized trip to Paris for a week in the spring of 1954.

¹ A term coined in the 1970s distinguishing economists who continued to be strongly influenced by John Maynard Keynes, concentrated in the United States at universities near the east and west coasts, from “freshwater” economists who wished to replace key elements of Keynesianism and who were concentrated at universities in the interior of the United States, especially near the Great Lakes.

² See section C of the bibliography at the end of the paper for details.

³ Ruth (later Kay) Williamson (1935-2005), an authority on Nigerian languages.

When did you first learn about the field of economics?

From my headmaster at Hereford High School for Boys. About a year before I went to LSE [the London School of Economics], I was planning to go into civil engineering. But then my mathematics teacher decided my math wasn't good enough for that. The headmaster, who also taught math, wrote to an alumnus who was an economics lecturer at [University College of] Swansea (I think), and he encouraged me to go to LSE. I thought of doing economic geography, mainly because geography was the one subject in which I excelled (but also because it was taught relatively well at our school). He told me a much better course would be straight economics. And I think he was right.

I think the idea of going to LSE had occurred to me beforehand. I'm not sure how that originated—probably from the headmaster of my previous school, who went to LSE in the interwar years. I didn't know what subject I wanted to study, but I knew I wanted to study in the middle of London, because that's where the action was. (The disadvantage of a sleepy little town like Hereford to a teenager!) Of course, LSE isn't the only college in the middle of London.

At LSE, who introduced you to the idea of economics as a science?

Although I never took a course with Bill Phillips,⁴ he influenced me greatly. He gave me the paper on the Phillips curve in advance of publication. And then I used that as the basis for the argument that the Keynesian model was true in the short run but that it had classical features in the long run, to which I attribute my success in Finals.

The other economist who struck me as outstanding was Dick Lipsey,⁵ then near the start of his career (less than ten years older than me). He gave an outstanding lecture course when drafted in to replace Helen Makower,⁶ who was injured in a traffic accident.

This was the period dominated by Paul Samuelson,⁷ who was the hero at that time: he'd made economics systematically mathematical. Nowadays, I think that idea may be overblown. Economics is about much more than just mathematics; that's an important part of it, but there's much more—one needs to have an understanding of the world as well.

Do you think economics nowadays focuses too much on mathematics?

Yes, I regret the fact that mathematics has displaced economic history. I think one needs to develop a sense of what things are important.

Are economists rethinking their claims based on new behavioral research?

I hope so. Some economists believe that humans are inherently greedy. I think it's necessary to recognize that greed is an important part of what drives human beings, but there's much more to us than that.

⁴ Alban William Housego "Bill" Phillips (1914–1975), inventor of the Phillips curve.

⁵ Richard Lipsey (born 1928), best known for his work on the economics of the second best.

⁶ Helen Makower (1910–1998), known for work in mathematical economics.

⁷ Who surely needs no footnote for the intended audience of this paper.

At LSE, I don't think we had a feeling that we were all being trained to become selfish.

What did you do after you graduated from LSE?

Two years of military service, that were compulsory in those days, in the Air Force, and then three years of grad school at Princeton.

When I graduated with first-class honors in 1958, Lionel Robbins⁸ said to me, "The world is at your feet, young man!" And I said, "Unfortunately, I've committed myself to do military service next, because I didn't expect to get this opportunity [to go to graduate school]." (Also, I didn't expect that [compulsory] military service would end the following year. It seemed it would mean waiting two years anyway, so I thought I might as well spend two years in the Air Force.)

How did you spend your military service?

After training, I was posted to the Department of the Scientific Adviser to the Air Ministry in Whitehall.⁹ I wore uniform only on special occasions, such as Mess Nights.

I did operations research. We developed a war game against an attack by 200 obsolescent Russian bombers aimed at our deterrent. Our deterrent was then provided by the RAF [Royal Air Force]. Even on the assumption that the Russians sportingly refrained from using a couple of missiles to knock out our radars first, I concluded that it was unlikely to deter. The unstated conclusion is obvious: leave deterrence to others (e.g. the Americans, or if you insist on a specifically British deterrent, the Royal Navy).

Why did you go to graduate school at Princeton?

I asked my tutor. I still remember the reply "There are three universities in the United States: Harvard, Yale and Princeton. If you apply to Harvard and Yale, you'll probably get in; if you apply to Princeton, you'll certainly get in, because I will tell Baumol¹⁰ to let you in." I discovered subsequently that there are slightly more than three universities in the U.S. Baumol was a leading light at Princeton.

Because I was too lazy to apply to more than one, I applied to Princeton. On recollection I think Princeton was not the best choice. The best choice for me would have been MIT; it was just at the high point of MIT's career. It was very famous in those days.

Could you describe more about your years at Princeton?

I was there three years in total and I taught in the final year because I got the Ph.D. halfway through the final year. And I hadn't really been planning to get a Ph.D. when I went, but everybody talked me into it. You know, it was regarded as such a serious qualification for gainful employment in the academic profession that one really had no alternative.

⁸ Lionel Robbins, Baron Robbins of Clare Market (1898–1984), author of *An Essay on the Nature and Significance of Economic Science* (1932) and a leader in the rise of the LSE to international eminence.

⁹ The area of London that houses the major British government offices.

¹⁰ William Baumol (1922–2017) had received his Ph.D. from the LSE. One of the most eminent economists not to receive the Nobel memorial prize in economics, he made contributions to labor economics, environmental economics, monetary economics, finance, and other fields.

I remember some courses. In the first year there were four courses: Game Theory with Oskar Morgenstern, a class in microeconomic theory with William Baumol, and a class in macroeconomics by Dick Quandt.¹¹ I don't remember the fourth course.

Which classes did you find most interesting?

I think probably the game theory course, although I didn't specialize in game theory. But Morgenstern was just so interesting in the way he'd wander off on these little intellectual excursions. He'd discuss all sorts of interesting things. One day he decided I was bright when I got the answer right to his question about U.S. steel capacity. (I had read a snippet in *The Economist* a few weeks prior saying it was 120 million tons a year). He went all around the class and took all sorts of answers and none of them got it right, and then there was one person who finally got it right, so he decided that he was bright.

And at Princeton, you said that Baumol, Kuhn,¹² and Morgenstern were the three most influential economists on your thinking. Was there one that most influenced you and your interests on exchange rates?

Fritz Machlup¹³ sustained my interest, but this was formed at LSE, as a result of reading rather than the influence of a professor.

What classes did you teach at Princeton?

I taught a section of [Economics] 101 and Economic Development. I remember the first lecture I gave in economic development. It was a real fiasco—the lecture—because, first of all, I gave a summary of what I was going to say, and then I said what I was going to say, and then I planned to summarize. And the students all stopped taking notes after the first section, and I thought that was terrible, so I sped up, and got through with about twenty minutes to spare. I had nothing to say for that last twenty minutes.

Your dissertation was in fact not about exchange rates. The title was “Patent Licensing and Royalty Terms.” How did you decide on your thesis topic?

Fritz Machlup had a series of potential topics which he then asked me to select one from, and so I selected one. He supervised my thesis. I wished subsequently that I had struck out on my own, rather than follow his lead. He made it feel like I had to choose one of these topics, but when I talked to Baumol, he assured me that if I wanted to do something else, I could, and the university would provide some way of financing it. I suppose I was sufficiently reassured that I put the nose to the grindstone.

¹¹ Oskar Morgenstern (1902–1977) was an Austrian-American who founded the field of game theory and its applications to economics along with Princeton University mathematician John von Neumann. Richard Quandt (born 1930) contributed to mathematical economics and other fields.

¹² Harold Kuhn (1925–2014), mathematician noted for his contributions to game theory and for the Kuhn–Tucker conditions for nonlinear programming, widely used by economists.

¹³ Fritz Machlup (1902–1983), Austrian-American from the same Vienna academic circle as Oskar Morgenstern; noted for contributions to exchange rate economics, economic semantics, and the knowledge-based economy.

Exchange rates did not dominate my life at Princeton, although when James Tobin,¹⁴ then a member of the Council of Economic Advisers, gave an evening talk in Princeton, I did ask a question which foreshadowed what I was to do later. Following an article I had read in *The Economist* that you might be able to change exchange rates bit by bit, I invited him to react to the idea. He responded that he read *The Economist* too, which was an effective put-down but not intellectually satisfying.

EARLY ACADEMIC CAREER; THE CRAWLING PEG

After Princeton, you began teaching at York. Could you describe what brought you there?

York was then a new university which planned to take economics seriously, as exemplified by the appointment of Alan Peacock¹⁵ to its founding chair. Hence, I was delighted when I was eventually approached. I went back to England at the end of August (1963) and university started in October, so I had a month to settle in and search for a place to live.

I wanted to live at the university, and hoped they'd have something. I looked at several new places and decided that I didn't find anything that particularly appealed to me. So then eventually the place I got was a flat below that of someone who was associated with the university. He was on the first floor and I was on the ground floor. And it was in a part of York close to the city center, but not on the main street. A narrow street (Skeldergate) that had a wider part where I could park my car. There was room for one other person to park there, but in fact there was nobody there most nights. One didn't have the phenomenon of universal car ownership in those days. And therefore there was much more parking space relative to the demand for parking.

What did it feel like to be all of a sudden in the role of a professor rather than a student?

I was very conscious of the change at the time. We used to joke that the students got younger every year. I mostly taught microeconomics to undergraduates. The joke was that the Department of Economics at York taught Public Finance, and John Williamson taught everything else.

This was because there were four people in the economics department that first year apart from me and the statisticians—Peacock, Wiseman, Dosser, and Hutton¹⁶—and they all specialized in Public Finance. Peacock was head of the department. Wiseman was in the Institute of Social Studies; he was not a full-time teaching professor. And Dosser was appointed to full professor one or two years later. Alan Williams¹⁷ came the following year. He was mainly famous for two things at York—he lectured for a full hour without notes, which enormously impressed students; but then he also became very well known for initiating the study of health economics. Now it's a big field. But in those days, it was unknown.

¹⁴ James Tobin (1918–2002), longtime professor at Yale University, recipient of the Nobel Memorial Prize in Economic Sciences in 1981, especially known for contributions to monetary economics.

¹⁵ Sir Alan T. Peacock (1922–2014); made contributions to the economics of national insurance, public finance, the economics of the arts, public choice, and studies of economic development in particular countries; from the 1970s onward, he was often an advisor to the British government on various subjects.

¹⁶ Jack Wiseman (1919–1991), professor of health economics; John Hutton (born 1948), emeritus professor of economics with specialties in health economics, tax policy, and econometrics; Douglas Dosser (born 1927), professor of economic theory.

¹⁷ Alan Williams (1927–2005), professor of economics.

By the final year at York, I was receiving invitations to apply for chairs (professorships). I ended up receiving offers of chairs at Manchester, Nottingham, and Warwick.

How did you feel about teaching microeconomics?

I was very enthusiastic to be teaching economics, and it didn't much matter what part of economics. When one first starts teaching, it's a good thing to be enthusiastic. I think I was teaching international [economics] by the second year.

With the graduate students, there wasn't as much formal teaching. It was a bit of a compromise between the American system and the English system which had existed up to then, in which there were no formal courses and one reckons to give graduate students a fair amount of personal attention instead.

The Robbins Report¹⁸ was published in my first term at York. It called for a big expansion of universities, which went from roughly 5 percent of the age group to 40 percent of the age group. It was a national policy. It was very good for academics in the short run. It indeed expanded economics enormously.

In one interview, you stated that your research on exchange rates has been your “primary contribution.”¹⁹ Could you describe some of your main findings on exchange-rate regimes?

Yes, I've long had the view that currency misalignment is a serious social problem, whereas volatility—which corresponds to the normal way of measuring the degree of exchange rate variation—is at most a minor nuisance. By “misalignment,” I mean prolonged departures from equilibrium rates, such as those experienced by the U.S. and the UK, and by Germany, Sweden, Switzerland, and the Netherlands, in recent years. Notice that I recognize that Germany *et al.* have suffered as a result of their undervalued currencies (though I doubt if the consequences of undervaluation are as severe as those of overvaluation): they have lower living standards than they might have had.

Your first contribution to exchange rates that was notable may have been the crawling peg monograph in 1965. Could you explain how your thinking developed from crawling pegs to the idea of what is now called the BBC (band, basket and crawl) approach²⁰ and to fundamental equilibrium exchange rates as a way of measuring imbalances.

The crawling peg system involves devaluing the currency bit by bit, making it known one will devalue but having interest rates sufficiently high to compensate holders for holding onto the currency being devalued. This can be contrasted with the adjustable peg, in which the rate is pegged in the short run, but the government retains the right to change it; and when the government finally devalues the currency, it wants to make sure that it has devalued enough.

I was a bit slow in recognizing how a wide band complements the crawling peg, but it does so in terms of increasing the size of the crisis-free parity changes that are possible.

¹⁸ *Higher Education: Report of the Committee Appointed by the Prime Minister under the Chairmanship of Lord Robbins 1961–63.*

¹⁹ [Carlos Lozada], “A Conversation with John Williamson, Economist,” *Washington Post*, April 12, 2009.

²⁰ See for instance *Exchange Rate Regimes for Emerging Markets: Reviving the Intermediate Option* (2000).

Pegging to a currency basket has the virtue of insulating your trade against changes in exchange rates of currencies within the currency basket.

Pegging to the same currency basket by a group of countries which trade intensively among themselves enables them to avoid disruptive impacts on their intra-trade as a result of changes in third-country exchange rates.

By the fundamental equilibrium exchange rate, I meant the rate that the International Monetary Fund was getting at in its concept of “fundamental disequilibrium.” This may be defined as the rate which achieves a satisfactory current account balance over the years when the economy is in internal balance.

Can you describe some of the concrete impacts that those early projects yielded in the world?

I will answer in terms of the three projects that I had in mind at York. I don't think the inflation project²¹ had much impact (which is good because it was misguided, in the sense that it was a pre-[rational] expectations analysis). I also don't think the sales/growth maximization project²² had too much impact in terms of the real world. I think it had an impact in terms of academic aspects. But I don't think it had a big impact in terms of the real world.

Regarding exchange rates [i.e., the crawling peg], initially I thought it would take a couple of years, but before long people would realize the good sense of what I was saying and adopt these policies. But that didn't happen. It never happened. I was a lone voice crying in the wilderness.

I gave several seminars about the crawling peg, including at Oxford. That was when Sir Roy Harrod²³ invited me to succeed him at Christ Church [college], which was a very prestigious post in Oxford. I didn't because I thought Oxford was overrated relative to other universities and I still dreamed of helping the new universities succeed in becoming equally prestigious.

When did you arrive to the Massachusetts Institute of Technology (MIT)?

The fourth year at York (my second trip to America), or 1967.

Charlie Kindleberger²⁴ was the main international economist at MIT in those days. And it was in that course that a student asked a question at the end of class, and Charlie muttered to me, “He's very smart!” and it was Stan Fischer! Stan eventually became Deputy Governor of the Federal Reserve and Governor of the Bank of Israel, and also Deputy Managing Director of the IMF.²⁵

²¹ “Phillips Curves, Expectations of Inflation and Optimal Unemployment over Time: Comment,” *Economica* (1968).

²² “Profit, Growth and Sales Maximisation,” *Economica* (1966).

²³ Sir Henry Roy Forbes Harrod (1900–1978), economist at the University of Oxford, known as the developer of the Harrod-Domar growth model and as the author of *The Life of John Maynard Keynes* (1951).

²⁴ Charles Kindleberger (1910–2003), best known for his books *Manias, Panics and Crashes* and *A Financial History of Western Europe*, both published after Williamson was at MIT.

²⁵ Stanley Fischer (born 1943), Rhodesian-Israeli-American macroeconomist, also former MIT professor, chief economist of the World Bank, and vice chairman of the Federal Reserve System.

Joe Stiglitz and Tony Atkinson were there also.²⁶ And there was a very bright Argentinian economist, who died prematurely.²⁷ He was young, but he had already gone a long way.

And, of course, the great Paul Samuelson was there. He was widely regarded as the leading economist of the world, although there are now many who put Milton Friedman in that position. I think Samuelson would have won a contest as to the world's leading economist then, and for many years thereafter.

GOVERNMENT ADVISOR

What did you do after MIT and York?

I joined the [UK] Treasury in October 1968. I was appointed as an advisor. (That was the official title, because otherwise they couldn't pay me as much as they thought was needed to get me; they thought they had to meet my best academic offer to get me, but in fact I didn't demand that much.) I just wanted to go into the Treasury to see what it was like on the inside and whether one had access to superior information. One really had all sorts of insights that were denied to other people, but not systematically superior information.

The job had two distinct aspects to it. One was giving advice to anyone who wanted advice. And the other was run-of-the-mill forecasting. I was appointed in charge of the European Economic Community, as it was then called. My particular specialty was France. Rachel Lomax was appointed my research assistant in charge of Germany. She rose to be Deputy Governor of the Bank of England and for a period was in charge of the Welsh Office; she was the top civil servant there.²⁸ And then she went to the Bank of England. So she did very well. She was a bright girl. She came to me straight out of Cambridge University where she'd gone for graduate training in economics.

At the Treasury, I was part of the committee that was set up to decide British attitude to international monetary reform (exchange rates and liquidity). It was chaired by Sir Douglas Allen,²⁹ the head civil servant of the Treasury, and there were several other members who were very senior. (In addition, there was Mr. M.V. Posner, of Cambridge University,³⁰ and then there was me!)

When I started they looked at me with a jaundiced eye, assuming I would be irrelevant to them, but before long I was in my element there. I failed to persuade the committee to go along with my ideas; I was very keen on the crawling peg in those days. And I was told that the long-term consequence of my advocacy of more flexibility was to inoculate the British against ever again attempting to peg the pound. It wasn't in fact my objective at all to inoculate the civil service against pegging the pound. They didn't peg the pound again, but that wasn't really the issue. I didn't think of pegging as so bad as they finally concluded

²⁶ Joseph Stiglitz (born 1943), American, received the Nobel Memorial Prize in Economic Sciences in 2001; Sir Anthony Atkinson (1944–2017), British, did extensive work on poverty and income inequality.

²⁷ Miguel Sidrauski (1939–1968), made contributions to monetary and growth theory.

²⁸ Rachel Lomax (born 1945), also former permanent secretary (top civil servant) of three other British government departments and vice president of the World Bank.

²⁹ Douglas Allen, Baron Croham (1917–2011), British civil servant.

³⁰ Michael V. Posner (1931–2006), economist at the University of Cambridge.

it was. They let it float after that.³¹ But what I was in favor of was flexibility in the sense that you change the value of the peg if the circumstances demand it.

The exchange rate was the main issue on which the committee focused. But it was also the early days of the SDR,³² and the question was whether there would be an allocation in 1970 and, if so, how large it would be. In 1969 the Americans were running a very tight monetary policy, which sucked the money out of Europe, and so even the Germans could see the advantage of having a boost in liquidity that year. The committee favored a large allocation (I found myself in a minority of one in being able to conceive of a situation in which British national interests were favored by less liquidity). The SDR was introduced on the 1st of January of 1970 with a \$3.5 billion allocation.

I also ended up giving advice to sundry other departments of the Treasury, varying from [foreign] aid (which was the least technical) to exchange control (which still existed in those days, and was highly technical). In 1970 I was appointed to a subcommittee which was formed to examine the case for allowing British Petroleum (then owned 48 percent by the British Treasury) to finance its expansion into North America by driving a coach and horses through the exchange control regulations. Of course, we said yes. The first time it occurred to me that this might have been a disservice to the world was 42 years later, when BP polluted the Gulf of Mexico.

Another part of my experience at the Treasury was forecasting the European Economic Community's imports. They wanted to know how much British markets were likely to expand the following year. We concluded that on average British export markets would expand by 10 percent in 1969 (the long-term average was 8 percent). I remember the chairman of that committee, who said, "We can't have expansion of British export markets of more than 8 percent two years in a row; such things don't happen." In fact, British export markets expanded 16 percent that year, if memory serves. So at least we got the sign of the deviation from the long-term average correct, but we weren't allowed to use the figure that we'd laboriously calculated to build the export forecast.

After the Treasury, you went to Warwick. What brought you there?

It was one of the three places—Manchester, Nottingham, and Warwick—that offered me chairs two years prior, which I'd not accepted at the time, but which I realized I should have accepted and then postponed entry. Manchester was no longer available. The attraction of Nottingham was that Bob Coats³³ was in charge of economic history, so I'd have had a very compatible companionship there. There had previously also been a possibility of going to Cambridge or Oxford, but I had turned them both down. (I was also offered a fellowship at Christ's College, where Charles Darwin had gone, at Cambridge. It was the equivalent of a professorship anywhere else.)

But I eventually decided to take Warwick because it was another new university, like York, which appeared not to have any of the problems that were then happening on university campuses, like students revolting against professors. It was the last week in which I could have thought that Warwick was unproblematic in

³¹ Britain was, however, a member of the Exchange Rate Mechanism of the European Monetary System from 1990 to 1992.

³² Special Drawing Right (SDR), the currency unit used and maintained by the IMF. It has been used as an asset in foreign-exchange transactions and is calculated based on a weighted basket of currencies.

³³ Alfred William "Bob" Coats (1924–2007), also noted for contributions to the history of economic thought.

this respect, because a week after I accepted the offer, the students proceeded to revolt. So I found that I was indeed in a university where there was a major problem of staff-student relations.

Before I went to Warwick in fact, the last thing I had done was attend a university conference at Sheffield. And on the train back from Sheffield I shared the journey with Peter Oppenheimer,³⁴ who had taken the position at Christ Church college in Oxford that I'd turned down two years prior. We agreed to form a joint seminar each term between Warwick, Oxford and Reading. We called it the South Midlands Staff Seminar. I had some opposition to encounter at Warwick with Keith Cowling.³⁵ The fear was that teaming up with Oxford was really aping Oxford. He was against it, but I prevailed on that occasion.

What else characterized the Warwick school of economic thinking?

I arrived there in September 1970. They expanded the idea of having many professors in the department and used it early on in an attempt to attract the brightest un-chaired economists in the country to Warwick. I don't know that there's anything particularly unique about the economics they sought to "sell."

I was teaching International Economics, the main subject on which I specialized. But there was also macro. I think I gave the first-year macro course which students generally had to study. Presumably there was something else too.

I spent two years at Warwick, and then took a leave of absence to go to the IMF, which I'd been invited to do about a year into my stay at Warwick. Fred Hirsch³⁶ was leaving the Fund; he in fact came to Warwick (as Professor of International Affairs) in the end. When I went back to Warwick, I would talk to him often. I didn't really know him much at that time. He'd impressed me. He was interested in the crawling peg as well.

INTERNATIONAL MONETARY FUND; BRAZIL

And from there you went to the IMF?

Yes, I got a letter from Jacques Polak³⁷ in I think about January 1972 inviting me to replace Fred Hirsch who was going to leave that spring, and I said I couldn't manage the spring because I'd committed that summer to writing a research paper on international liquidity. Polak, who was the head of the research department and one of the most powerful men at the IMF at that time, said he would hold the position for me for a few months. So I agreed to go on October 1st. Warwick agreed to hold my spot for two years while I was away, which was normal.

Jacques Polak, along with Marcus Fleming,³⁸ made a very congenial working pair. Marcus resisted some of my ideas with great fervor, but he was nevertheless an interesting person.

³⁴ Peter Oppenheimer (born 1938), later chief economist at Royal Dutch Shell.

³⁵ Keith Cowling (1936–2016), noted for work on industrial structure.

³⁶ Fred Hirsch (1931–1978), British financial journalist and economist, best known for his book *The Social Limits to Growth* (1977).

³⁷ Jacques Polak (1914–2010), Dutch economist, originator of a model that guided the IMF's approach to assistance to member countries for balance of payments problems.

³⁸ John Marcus Fleming (1911–1976), British economist and deputy director of the IMF's research department.

The Committee of Twenty³⁹ was being formed at that time to investigate the redesign of the monetary system. The first meeting of the committee was scheduled for well after I arrived, in November just after Thanksgiving. I spent the first few weeks generally writing about the area and exploring topics which interested me, including the SDR/aid link, which was the first occasion on which I clashed with Marcus.

The second area in which we clashed was after I produced my first paper for the Committee of Twenty. It was very clear from the first meeting that there would be a big clash on indicators, so I wrote a paper on indicators, in which I was sympathetic to the idea that a country should adjust when it accumulated too many reserves. And because it was called indicators it was completely unacceptable to Marcus Fleming, who argued that it was indispensable to have policies determined by people, rather than blindly following indicators. We had a clash on that issue which continued for a long time.

In D.C., I found a place in Georgetown, a very agreeable apartment, though I did not stay the full two years. It had a good-sized front room, and it had a master bedroom and a kitchen. No office. It was enormously expensive at the time, \$400 a month! Georgetown seemed the nicest place. I liked the general atmosphere in Georgetown: the fact that there were lots of young people (I still felt young in those days).

I finally got married while in the IMF. If one marries an IMF staffer, as I did, then the chances are high that she will not have the same nationality. In my case, she is Brazilian.

How was the work at the IMF?

I was brought in to work on reform of the international monetary system. I was the most junior person responsible for writing papers.

The original monetary system, as designed by Keynes at Bretton Woods, collapsed because the speculative pressures proved overwhelming. And this is what we'd been afraid was going to happen.

In the [UK] Treasury we'd had a committee on reforming the international monetary system and the big fear was it would collapse, but nobody knew what would happen when the system collapsed. We knew it was going to collapse—this much was conventional wisdom, because the amount of speculative money which was released to slosh around the world just proved overwhelming. And one couldn't hold the exchange rates against that weight of money. Ted Heath, who was Prime Minister of the United Kingdom at the time, gave instructions that the Bank of England wasn't to allow any appreciation of the pound, and they weren't to buy more dollars either. (The way you prevent the pound rising is by buying dollars, so he gave instructions that were mutually contradictory.)

How would you summarize your experience at the IMF?

I loved the work with which I was engaged, but I despaired of accomplishing anything very early on. I didn't think that the attitude of the people in charge was right to accomplish a great deal and in fact they more or less admitted it was a fiasco in the end.

After the IMF, you returned to Warwick. What would characterize your return?

³⁹ Formally known as the Committee of the Board of Governors on Reform of the International Monetary System and Related Issues. It was established by the IMF in 1972 to prepare a draft for a reformed international monetary system after the Bretton Woods system in effect since 1945 broke down in 1971.

Warwick had sent me off as a bachelor and regained a family man, for I had acquired in addition to my wife two young fellows who would shortly become my sons, a teenager whom my wife had adopted as a child, a niece, and a dog. And during the first year back we added a daughter.

I remember the money study group where I used to meet and argue with David Laidler (ex-LSE a year behind me, [University of] Chicago, took the chair in Manchester) and Michael Parkin (who was a bright young man who had never been to Chicago but nevertheless had absorbed its ethos).⁴⁰ I had taken a bet with Michael Parkin in about 1970 that his wage equation which, as was then standard, lacked an expectations term, would underestimate wage inflation in the UK by at least two standard deviations in both 1971 and 1972, if memory serves me correctly. We would soon know who had won. The bet was undertaken in the expectation that it would provide definitive proof of cost inflation; rather, we ultimately agreed on the need for adding an expectations term. The meetings were chaired by Harry Johnson,⁴¹ a great unbeliever in cost inflation.

Something else I worked on was a conference at Brookings in the spring of 1976, about the effect of oil price increases on the world economy. One didn't worry too much about developing countries in general at the time, but the oil producers were very important. The oil price increase of 1973–74 was the first time people realized the power the oil producers had over the world economy. In due course the commercial banks began lending to developing countries, thus solving the problem of who would have a current account deficit corresponding to the oil surplus.

And there was also a study led by Sir Alec Cairncross⁴² by a group of economists in Britain about the effect of oil price increases on the world economy. In my paper for the UK group, I tried to establish whether there were distinctive effects on the pattern of non-oil international trade. On the whole I didn't think the oil price increase was the end of the world, which set me apart from most economists at the time. They tended to be of the view that oil price increases threatened to make oil producers the only capitalists.

One other incident from this period is worth relating. This was the time when Professor Wynne Godley, of Cambridge University,⁴³ began propagating the view that there was a way out of Britain's dilemma—through protection! I was responsible for organizing several academics to join me in denouncing this nonsense in a letter to *The Times* [of London].⁴⁴

After Warwick, you became a Professor of Economics at the Pontifícia Universidade Católica do Rio de Janeiro. How did you enjoy Brazil?

⁴⁰ David Laidler (born 1938), known for writings on the demand for money and the history of monetary theory; Michael Parkin (born 1939), known for best-selling introductory economics textbooks and wide-ranging research; both were English-born, and both began professorships at the University of Western Ontario in 1975. The originally released version of this paper contained an error in this sentence that has been rectified here.

⁴¹ Harry Johnson (1923–1977), Canadian, professor at the University of Chicago and possibly the most prolific writer ever of articles in economic journals.

⁴² Sir Alexander "Alec" Cairncross (1911–1998), Scottish, frequent adviser to the British government and writer on British economic policy and recent British economic history.

⁴³ Wynne Godley (1926–2010).

⁴⁴ "Protection," *The Times* (October 11, 1976)

I didn't in fact go straight to PUC. I was hired initially by IBGE [the Instituto Brasileiro de Geografia e Estatística], the same outfit that hired my wife, whom it really wanted to lay hands on. Fortunately, the director recognized that but expressed doubts about the wisdom of paying someone 28 minimum wages to do nothing was not a clever way of appealing to someone with a nonconformist conscience, and suggested that I transfer to PUC, where they were aiming to start a graduate program.

I gave a paper on debt to the twenty-fifth anniversary conference of BNDES [the Brazilian Development Bank].⁴⁵ I argued that the world should not lose the ability to pass on the oil deficit to developing countries, but argued that it was risky to do this through the commercial banks. Of course, the Brazilian government vigorously contested doubts about the wisdom of doing this through the banks.

I went to a conference about inflation in Brasília. Brazil was then the land of high inflation: at the time of the conference (with several bouts of hyperinflation in future) it was already in its 97th month of inflation of mid single digits per year. That was my first acquaintance with the distinctively Brazilian analysis that would ultimately lead to successful stabilization (in 1992!).

I remember writing a letter to my mother in which I said that it was amazing that all the streets were one-way. That was a major part of my reaction to Brazil. I was also quite impressed by how many things were made in Brazil, the extent to which they'd developed. It wasn't regarded as a serious country in the 'north by and large; it was "just a developing country," one of the poor developing countries. And I was surprised by the extent to which it had developed an industrial base which was capable of making most things. Not everything, but most things. In fact, there were some things which they didn't make terribly well. It was when they opened up trade that they stopped making those things and instead bought them from abroad. I thought that was a good thing. But they had actually developed; for me it was pretty consequential.

During the third year in Brazil, Rudi Dornbusch was also there. (He was at that time also married to a Brazilian.) In fact, we arranged a swap: I taught his course at MIT that year.

During my last term in Brazil I decided I was going to write up my course into a textbook.⁴⁶ And I more or less kept up with the idea of doing a chapter a week. The idea was that I'd write a chapter each week that covered the material that I was simultaneously teaching. And I didn't quite keep up with that. I spent about a month when I got to Washington in the summer completing what I would have had to do to keep up with that objective. So I eventually finished it, it was a relatively successful book, made me \$8,000 or so, which I used to take the family to England for the first time (from the U.S.).

THINK TANK SCHOLAR; TARGET ZONES

In 1981, you became a Senior Fellow at the Institute for International Economics (IIE, later the Peterson Institute for International Economics). What was it like returning to Washington?

I was working on my book, my textbook. I had planned a very ambitious schedule, but I didn't quite fulfill it. I arrived in early July, when term finished at PUC.

I started at the Institute at the beginning of September. It was created at the beginning of October, and so I started working for the Institute a month early, in September, before the Institute was created. They

⁴⁵ "Transferencia de recursos o sistema monetário internacional" (1977).

⁴⁶ *The Open Economy and the World Economy* (1983).

got permission to pay me by the Carnegie Endowment, which fathered the Institute and was quite helpful in many ways, administering the Institute before it had any infrastructure.

To begin with, the big issue in the Institute was how we divided the forthcoming work program. Basically, I took macro and the IMF, Bill Cline took debt (though he retained an interest in trade), and Gary Hufbauer took trade policy.⁴⁷ And I remember Fred [Bergsten] being very concerned that we got more publicity for macro than trade policy in those days. But I think that was largely a consequence of what was on people's minds. Debt really exploded in the summer of 1982 with the Mexican debt crisis. It wasn't obvious that Bill Cline was taking a very potent subject in the first ten months of the Institute's existence, up until the Mexican crisis in the summer of 1982 when it broke. And then it was obvious.

Marjorie Deane⁴⁸ was an employee of *The Economist*; she was quite a character, but she wasn't a regular writer. She remarked to me sometime that the crawling peg was coming into its own again with the growing antipathy to floating rates. And so, encouraged by that remark, I was only too happy to return to thinking about crawling pegs and exchange rates. And I set to work on that.⁴⁹

The first publication I had within the Institute was actually a book based on a conference in Virginia that we held about the IMF.⁵⁰ The big new idea at the time was the structural adjustment programs which the World Bank had launched. And they were highly controversial.

What were your opinions of IMF structural adjustment initially?

I thought it was a sensible idea, but there was a danger the zealots would push it too far, seek to eviscerate some programs that were essential to the long-term future of the countries. But I didn't see that as negating the idea of having them. With time I became more convinced that in some places it had been taken in a perverted way, in particular by economizing on education. That was not what structural adjustment was supposed to be about, as I saw it.

The first draft of the [IIE] Policy Analysis (PA) about the IMF was just before I went on leave to England taking the family with me. That's when we used the money from the textbook [I had written]. The general line was that the IMF and the World Bank were drawing closer together and that the high-income countries had ceased to be a concern of both the IMF and the World Bank. This was before Greece, obviously.⁵¹ And so the general line that we more or less agreed to was that the IMF like the World Bank had become exclusively a middle- and low-income institution.

And when did you start thinking about target zones?

In the 1980s, after the IIE was founded, with Fred Bergsten⁵² as Director. Fred turned out to be very keen on target zones—a real ally in this cause.

⁴⁷ William Cline (born 1941) and Gary Hufbauer (born 1939); both are former U.S. Treasury Department officials.

⁴⁸ Marjorie Deane (1914–2008), employed by *The Economist* from 1947 to 1989, co-author of *The Central Banks* (1994).

⁴⁹ *Exchange Rate Rules: The Theory, Performance, and Prospects of the Crawling Peg* (1981).

⁵⁰ *Lending Policies of the International Monetary Fund* (1982).

⁵¹ A reference to Greece's troubles during the global financial crisis of 2008-09 and afterwards.

⁵² C. Fred Bergsten (born 1941), former Assistant Secretary for International Affairs at the Treasury.

By the 1980s or 1990s, you widened that further to the BBC (band, basket and crawl) theory.

This was the 1990s, or even the subsequent decade—after Rudi Dornbusch⁵³ christened my beliefs the BBC theory.

Regarding the crawling peg and other intermediate exchange rates, what is your evaluation of the empirical record of how sustainable they have been?

Some countries have done very well. Chile is the most frequently cited case. But even Chile felt that they had not reached steady state; since no developed country has ever shown the slightest interest in crawling, they regarded it as natural that they also would float as they became developed. (Floating does not seem a good solution either, but at least it avoids frequent crises.)

And are the reasons for why intermediate regimes have not been more durable mainly economic or political?

Maintaining competitiveness has often been a dominant economic motive. I don't think we can view those currencies as too weak or too strong.

It follows from what I argued in answer to your last question that the answer is "political."

When did you become known as the "exchange rate guru"?

With the publication of *The Exchange Rate System* [Policy Analysis 5, 1983]. The first mention I had of exchange rate targets and trying to keep them within certain pre-defined limits was a joint article with Fred in which we put forward the idea of target zones for exchange rates in Bill's book about the trade system *Trade Policy in the 1980s*.⁵⁴

So then my second publication for the Institute, PA5, was a development of that thesis asking what one could mean by a target zone. Could one contemplate the exchange rate hitting the edge of a band and then being allowed to go outside the band while still maintaining that one had the target zone, one was committed to getting it back to the band as soon as possible? Fred tended to think that if you're at the edge of a band you should defend it come what may, and I tended to think that one should allow it on occasion to go outside the band. That seems to me the essence of the distinction between target zones and the Bretton Woods system.

PA5 was first published in 1983, and then it was republished in 1985. So I think in the first year or so I worked largely on that. But then later on I'm quite sure I was working on other things. Like a book I did jointly with Don Lessard (of MIT).⁵⁵ We had a conference on capital flight, and wrote up the results in *Capital Flight and Third World Debt* [published in 1987]. We involved Charlie Kindleberger in the conference. The big point that we got right was the potential for flows of equity capital, to be invested in the stock markets of developing countries. Nowadays this is big business, but it was cause for derision in 1987.

⁵³ Rüdiger Dornbusch (1942–2002), German-born, professor of economics at MIT from 1975, known for contributions to macroeconomics.

⁵⁴ "Exchange Rates and Trade Policy" (with C. Fred Bergsten, 1983).

⁵⁵ Donald Lessard (born 1943), professor of international management at MIT.

Third World debt became a headline in 1985.⁵⁶ This was one way in which I got involved in the debt debate. I couldn't resist taking an interest in Bill Cline's field, though he was clearly the voice at the Institute on this question. In fact, Bill was not at all possessive: he invited Fred and me to join him in editing a publication on banks and Third World debt, *Bank Lending to Developing Countries* (PA10), in 1985. It was the one publication of which I have reservations in retrospect, because we made it sound like we believed that debt relief was unacceptable. It was unacceptable to Bill, so it was unacceptable to the collectivity.

What were your views of Ronald Reagan during his terms?

I was very critical of Reagan. He had a way of appealing to middle-class people through his anecdotes. He would just talk as though departing from the rules for one individual solved the problem for others. And he neglected the fact there were X million people out there suffering from the same problem without the same resources (or luck).

I disagreed strongly with his tax policy. Basically because it rewarded the rich and penalized the poor. To give any chance of moving toward equalization you need to have high taxes on the wealthy.

What were your views on economics in the late 1980s?

There was a big change starting in 1985, when James Baker became Reagan's Secretary of the Treasury. The Baker Plan was announced at the Annual Meetings of the IMF and World Bank in 1985. It basically confessed that the debt strategy hadn't worked too well so far and that the problem was that countries were not reforming sufficiently enthusiastically. It pressed for structural change. And by that it meant things like Britain had done under Margaret Thatcher and America under Ronald Reagan, who had made lots of structural changes within their own countries. I agreed with part of it, part of it which later on I put in the Washington Consensus. But I didn't agree with all of it. There were things like having monetary policy dictated by a constant rate of growth of the money supply that I thought were crazy. And they were dropped subsequently.

Could you describe the main components of the Baker Plan?

That countries should engage in structural adjustment—it hadn't previously been regarded as the essence of what countries should be doing, but now it was: the previous message was simply that countries should tighten their belts.

Most developing nations weren't doing much in the way of structural adjustment at the time. They were simply doing austerity, as I said before. They were being encouraged to regard that as their obligation, rather than structural adjustment. Prior to the Baker Plan. So the Baker Plan brought the need for structural adjustment.

But there's no point having structural adjustment if there's no room for everything to be done at the same time (i.e., export expansion). There's no point in new export industries, for example, unless they are going to have resources, to in fact produce the exports. And so one has to make sure that the resources are

⁵⁶ Mexico had defaulted on its foreign debt in August 1982 and many other developing countries had followed. See below on developments in 1985.

available for these extra activities to take place. That may require deflation; but necessarily less deflation than by adjustment through austerity alone.

Because the Baker Plan marked a change in attitude of the powers that be towards the debt crisis, there was no longer an attempt to pretend that countries were going to pay up in full if only they were given a bit more time. Instead it was admitted that they weren't likely to be able to restore solvency on the basis of the present policies. So they had to change the policies. Things like privatization, which was very conspicuous on the list. The Baker Plan wasn't supplemented by saying what should qualify for debt relief. It seemed to be implicitly accepted that debt relief was necessary, but there was no concept of how it should be affected.

After the Baker Plan, you wrote another Policy Analysis. Who was this directed toward?

Primarily directed to banks. It was a discussion of providing a systematic but varied approach to debt relief. The PA was called *Voluntary Approaches to Debt Relief* [1988], which was a little bit disingenuous in the sense that banks cannot be expected to volunteer debt relief. A better (more accurate) title would have been *Varied Approaches to Debt Relief*: there has to be a sanction for not giving debt relief, for not adopting any of the approaches. There was a strong argument for giving a choice, rather than compelling all banks to adopt the same policy. I mean some wanted to just give an interest concession. And some wanted to write off principal. And others wanted to maintain the value of the debts on their books but to engage in new lending. And I thought that was perfectly all right so long as one required all banks to give the same present value. The point is one had to compel the banks to do something.

So then in 1989, the Baker Plan was complemented by an official endorsement of debt relief in the form of the IMF giving loans to countries in return for countries agreeing they would maintain debt service payments on the restructured debt. They would also guarantee payment of the principal and part of the interest by purchasing U.S. zero coupon bonds, so they would have the wherewithal in twenty years' time to repay the principal. Since they don't pay interest, they are cheap to buy, much below the value of the debts. This way there was a guarantee that the principal of the debt was repaid. Countries had been paying the interest anyway; the interest burden of the restructured debt was lower.

THE WASHINGTON CONSENSUS

And then that brings us to the "Washington Consensus," because that arose out of the occasion when you were testifying for a Congressional committee.

I encountered the belief among members of the U.S. Congress that Latin American countries had done nothing to structurally adjust.⁵⁷ They had wasted their time. And I thought that was a misrepresentation of what had been happening in Latin America. So then I said, "Let's call a conference, and let's discuss, get every country to lay out what it has done." And as background to that I prepared a document, "What Washington Means by Policy Reform" [1990], in which I described the "Washington Consensus."⁵⁸ It was basically laying out what countries should do according to the canons of Washington at the time.

⁵⁷ U.S. Senate, "Implementation of the Brady Plan: Hearing Before the Subcommittee on International Debt of the Committee on Finance," (1990), p. 36.

⁵⁸ "What Washington Means by Policy Reform," in *Latin American Readjustment: How Much has Happened?*, ed. John Williamson (1989).

What was your initial definition of the Washington Consensus?

At IIE, we had a sense that countries were not getting credit for the major changes in economic policy that they had already made. So we planned a conference at the IIE designed to change that with economists and specialists, from these different countries. They got my paper ahead of time, outlining the types of changes in economic policy (“adjustment”) that Washington institutions agreed they were looking for, and then they came to discuss and present their own papers.⁵⁹

I invited two people from Washington who I expected to be critical of it. One was Allan Meltzer and the other Richard Feinberg.⁶⁰ But, in fact, they didn’t take a strong line, which I took as more or less an endorsement. And Stanley Fischer—the third invitee—was very complementary in general.

I don’t think the Washington Consensus was a big deal immediately. It was only when I started using the results to urge countries to continue, or in some cases to initiate, adjustment. Particularly in Brazil, it was the case that they hadn’t done much structural adjustment up to then.

That was when Rudi Dornbusch attacked me. Rudi seemed quite sympathetic when he was at the conference. But then later he became very critical, saying “Williamson surrendered to Washington.”

When the Washington Consensus was first coined, did it become immediately popular or did it take some time?

Most people thought there was not a consensus in Washington about anything—that Washington was divisive rather than a source of consensus. But, in terms of economic policy, I would argue there was more of a consensus then than either before or since—although I have to say there is broader agreement on the relevance of these conditions for other countries than for the United States. Since then, the phrase became regularly used much too much for my taste.

The Consensus has generally been attacked for assigning a large role to markets. But the fact that there should be a large role of markets in the system, I think, is correct, or at least a defensible judgement.

When you first coined the phrase, who did you think your audience was?

The same audience as typically buys IIE products, which we like to think of as those interested in U.S. foreign economic policy. That is, members of the administration and Federal Reserve, both political appointees and civil servants; others interested in politics; members of the press; academics; etc.; both domestic and foreign. In this instance, members of Congress.

Did people interpret you as essentially taking on the role of the U.S. government or the World Bank/IMF in trying to influence other nations’ policies?

I’ve no idea what other people thought, but I knew that I personally don’t take the notion of noninterference seriously. (The sooner we drop this relic of sixteenth-century thought, the better). That

⁵⁹ Appendix A summarizes the main headings of the Washington Consensus.

⁶⁰ Allan Meltzer (1928-2017), professor at Carnegie Mellon University and a leader of the monetarist school; Richard Feinberg (born 1947), U.S. government official, think tank scholar, and professor whose work has focused on Latin America.

goes for the U.S. government, and for any foreign governments too. So far as the IMF and World Bank are concerned, they have a duty of trying to influence the policies of their clients.

The charge was made that the IMF and World Bank were dictating countries' policies. It [supposedly] wasn't a case of influencing; it was a case of dictating. And that's where I think they don't have the power. They don't have the power to dictate even in the smallest countries.

When did the Washington Consensus begin to be seen differently from your intention?

I first got wind that the Washington Consensus was being used in a totally different way to the way I'd intended it be used, to mean all the illiberal things which Reagan and Thatcher had promoted, as well as the arguably liberal, by two incidents.

One was when I went to this meeting of the Latin American branch of the Smithsonian, now called the Wilson Center [Woodrow Wilson International Center for Scholars]. They used to meet in the old building of the Smithsonian, before they built the Reagan Building. I was expected to talk vigorously; instead I couldn't believe my ears when I heard the sense in which people were using the Washington Consensus.

And then the other occasion was with a Brazilian, the former finance minister Luiz Carlos Bresser-Pereira, generally known as Bresser.⁶¹ He explained to me that the Washington Consensus had escaped my control and it had become whatever people meant by it. And so he was going to use it in the sense it was being used in Latin America in general.

How was the term being used in Latin America in general?

Mostly as a neoliberal tract. My intent was to pick out the bits of Reagan-Thatcher economics that had survived rather than give my blessing to the whole kaboodle. There were certainly elements that weren't Reagan-Thatcher, such as the insistence not to cut education and health spending. One could see a constant rate of growth of the money supply as a part of the Reagan-Thatcher doctrine when they took office that didn't survive.

I don't think there's anything particularly neoliberal about economic stabilization. The Consensus was very firm in favor of stabilization.

Did you talk to many members of Congress or Congressional staff to help write this assessment?

I did no survey of Congress to assess their opinions. But my idea was informed by casual conversations with members of Congress and with Congressional staff.

What did you do when you realized that the Washington Consensus was taking on this distorted form?

I responded as an academic by writing papers about it. I began writing articles in which I explained my position, and which said there were different interpretations of the Washington Consensus.

⁶¹ Luiz Carlos Bresser-Pereira (born 1934), Brazilian economist and former Minister of Finance.

I also ran a conference on the political economy of policy reform. [This was later published as *The Political Economy of Policy Reform*, 1994]. It essentially examined the politics of economic stabilization rather than concentrating on the economics. It looked at the politics much more.

What was the most unpleasant run-in you've had in relation to the Washington Consensus?

I suppose it was with Frances Stewart's⁶² criticism. She was very much against the Consensus. According to her, I let the cat out of the bag that Washington was imposing this on the rest of the world. And I didn't have that view, partly because I thought the Washington Consensus was a good thing, but mainly because I didn't think the Washington institutions were in a position to impose their views on the rest of the world. Even small countries have the opportunity of imposing their own views.

Has the definition of "Washington"—that is Congress, senior members of the administration, and technocrats in international institutions, economic agencies, Federal Reserve, and think tanks—changed?

Every time the administration changes, Washington changes. And there is not much of a consensus about anything now. The institutions that determine the attitude of "Washington" to the outside world are accurately depicted in the question.

At one point, you actually said that you've come to regret the term Washington Consensus.

This was said during the [presidency] of George W. Bush: I felt that to tie economic reform to Washington was a disservice to the cause of reform. In the age of Barack Obama, it wouldn't have been so. And in the age of Trump it is. Anyway, he doesn't believe in the Washington Consensus, nor believe in anything much, except the greater glory of Trump.

In 2000, you noted that you wanted to update several of those policies.⁶³ Would you want to update any of your revised policies today?

I wished to replace item (4) on the list in Appendix A by the more general term "financial liberalization" (in place of "interest-rate liberalization") and that in item (5) I was probably guilty of confusing thought and desire. That is, I assumed (wrongly) that Washington shares my belief in the virtues of a competitive exchange rate (meaning a rate that is determined by the achievement of a desirable current account, rather than by the nominal anchor stuff).

With those two revisions, I am happy to recommend the policies. But there is a problem. Even if Washington can be assumed to go along with the first change, one certainly cannot say the same for the second. I doubt if I can be criticized for immodesty in saying that I doubt whether Williamson's personal reform recommendations will carry the same clout as the Washington Consensus.

And what about the popularity of the phrase? Do you ever feel that when other people write about the Washington Consensus, the meaning has perhaps changed over time?

⁶² Frances Stewart (born 1940), development economist at the University of Oxford.

⁶³ "What Should the World Bank Think about the Washington Consensus?" (2000).

Certainly. Many people think it simply means liberalization of an economy. They only focus on a couple of points, either 4 or 6-9; or, worse still, they misinterpret 7 as a call for complete capital mobility.

The phrase has also captured something beyond policies. There does seem to be a largely shared way of thinking partly based on geographical proximity. For instance, the Treasury is around three blocks away from the World Bank and the IMF, while the Inter-American Development Bank is one block in the other direction. The economists at these places seem to come from a limited variety of universities as well. So, aside from agreeing on particular policies, there is also a certain way of thinking just because economists talk to other economists and develop a group interpretation as a common way of thinking.

Yes, I agree that they develop a common way of thinking about many issues. Fundamentally it's due to the fact that they went to the same universities, and there's a limited variety of universities. It's natural that one gets an economists' way of thinking during that process. But the range of universities from which they've tended to come is enlarging over time, even though the rate of progress is slow.

Your personal involvement in Latin America has been Brazil, which has perhaps been an outlier of the Washington Consensus.

Certainly Brazil lagged in the initiation of reform, and certainly Brazil has shown less enthusiasm about enrolling in someone else's plan than some smaller countries. But in terms of actually changing policies that have outlived their sell-by date, it is not clear that Brazil is such a laggard.

WORLD BANK; UNITED NATIONS

After the Washington Consensus became more well known, you began work at the World Bank. What was your experience like?

I got pretty interested in World Bank policy problems when I was at the IIE, testifying several times about those policies, and they invited me to go to several conferences. So in terms of knowing their research program I felt well-qualified for the job.

Sebastián Edwards⁶⁴ was chief economist of Latin America and was a very good friend. Then he began thinking about retiring from his job, and it became vacant. That's when I got interested in a chief economist job. What I was interested in was Chief Economist for Latin America. But that wasn't available because they wanted a Latin American.

Meanwhile, Joe Wood⁶⁵ was then Regional Vice President for South Asia at the Bank. When he learned I was making inquiries, he had the idea of getting me to go to another chief economist job, one that he had under his control.

So you initially entered as head of the South Asia division?

⁶⁴ Sebastián Edwards (born 1953), specialist in Latin American economic development, later professor at the University of California, Los Angeles.

⁶⁵ Donald Joseph Wood (born 1941), worked at the World Bank from 1968 to 1998, holding a number of senior positions.

No, I was Joe's Chief Economist, which involved overseeing the economists working in South Asia which covered India, Pakistan, Bangladesh, Sri Lanka, Nepal, Bhutan, and Maldives. Afghanistan was included in principle, but was inactive because it had a Taliban government.

There wasn't an enormous highlight for me. In that sense, it was a bit disappointing. The two conferences I organized perhaps were envisaged as being highlights when I organized them, but they turned out not to be. One was a conference in Sri Lanka with Asian economists, mostly national academics and government economists in the region. There were some World Bank economists as well. But the second was a conference which was essentially World Bank economists. There were outside speakers like Stan Fischer, who came over from the IMF.

What else would you like to talk about that happened in the late '90s, before 2000?

The other experience at the World Bank which I think I should relay to you, was my impression of the Bank's bureaucracy. I was not impressed. The Bank is much less efficient than the IMF. So I left the World Bank and returned to the Institute at the end of 1999. I had been there [the World Bank] for three years. They offered to extend to six years, but I said no.

Why did you decline?

Because I thought, rightly or wrongly, that I was of more use at the Institute than the Bank. At the Institute, I wrote one PA with Fred Bergsten about the overvaluation of the dollar,⁶⁶ but the prescriptive side was weak because he was still relying on intervention to cure the overvaluation.

I also ended up writing pieces with Bill Cline. He volunteered to use his world trade model to update the estimates of equilibrium exchange rates.

What project or projects were you most proud of in those years from 2000 to 2005?

There were several at the Institute, including the PA with Fred on the overvaluation and one replying to an idea that suddenly became fashionable, and one outside the Institute.

The idea that suddenly became fashionable is that the only permanently viable exchange-rate regimes are fixed or free floating.⁶⁷ What this excludes are managed floating and crawling. The empirical evidence used to support this viewpoint relates solely to the G7. Among the wider international community there has been essentially no sign of the desired hollowing-out.

Another one was about Brazil. When Lula first emerged as a front-runner in the presidential election, there was an immediate loss of confidence in Brazil, because of a widespread assumption that Lula would and should default on Brazil's (very considerable) external debt. I wrote a policy note called *Is Brazil Next?* [2002] in which I pointed out that the outlook for Brazil was favorable if only they could overcome the confidence problem. Fortunately Lula agreed with me (rather than the pundits), as I had been assured he would during a pre-election trip to Brazil, and the way was clear for Brazil to take the then-largest IMF loan in history to deal with the confidence problem and proceed to enjoy a boom that lasted just as long

⁶⁶ *Dollar Overvaluation and the World Economy* (with C. Fred Bergsten, 2003).

⁶⁷ For this so-called bipolar view, see Fischer (2001); for Williamson's views, see *Exchange Rate Regimes for Emerging Markets: Reviving the Intermediate Option* (2000).

as Lula was in power. Lula was lucky in his terms of trade, which moved decisively in favor of Brazil when he first became president and stayed strong during his presidency, only collapsing after. Unfortunately, Lula did not save enough during the good years to save his successor.

Another PA was about exchange rate regimes and capital mobility.⁶⁸ Developing countries repeatedly experienced too much capital coming in and then it all leaving. The logic of capital mobility is to divorce the level of domestic investment from domestic saving, but this logic does not work if you have to maintain 100 percent marginal reserve requirements for fear of the capital leaving. Only capital which is prepared to stay through a devaluation is worth having.

Yet another PA, about Latin America, was co-authored with Pedro Pablo Kuczynski. That was called *After the Washington Consensus*.⁶⁹ The objective was to rekindle enthusiasm for reform in Latin America. They had developed a great deal of enthusiasm about reform in the early 1990s, but this had evaporated subsequently, without, to our minds, their having reaped the full benefits of the earlier reforms.

I know for sure that I met Pedro Pablo when we were both in graduate school at Princeton. We sat a course together. I think it was Baumol's course. But he'd been to school in England as well, and I may have met him then. We naturally had some things in common. And he went on and did all sorts of things after that, culminating in the presidency.

Interestingly, the outside project during this period also involved someone I'd met prior to them becoming a Latin American president: Ernesto Zedillo. I had met him in the late 1980s, in London I believe. I carried around his phone number in my diary for many years, before he became president. It never occurred to me he was likely to be president [of Mexico]. He wouldn't have become president except for an accident of history. The candidate was assassinated during the election campaign and Zedillo (the vice-presidential candidate) became the candidate in his place.

He was elected in 1994, and upon completing his term, in 2000, the UN Secretary General initiated the Zedillo Commission, charged with assessing options for economic reform. Zedillo then invited me to direct the Commission's High-Level Panel, which included Robert E. Rubin, Jacques Delors, Manmohan Singh and Masayoshi Son.⁷⁰ It was meant to influence the Financing for Development conference of 2002.⁷¹ I was very enthusiastic about the opportunity.

After a survey of the various ways in which development may be financed, the Zedillo Report proceeded to an analysis of the existing set of international institutions. It argued that the World Trade Organization and the International Labour Organization are underfunded; that (in a world where a substantial portion of income is earned abroad) the world urgently needs an International Tax Organization; and that responsibility for the environment should be centralized in a Global Environmental Organization.

Since this age has not been noted for its proclivity for forming (or reforming) international institutions, it was doubtless predictable that these recommendations would fall on deaf ears. Nevertheless, necessity

⁶⁸ "Choosing Monetary Arrangements for the 21st Century: Problems of a Small Economy" (2006).

⁶⁹ *After the Washington Consensus: Restarting Growth and Reform in Latin America* (2003). Kuczynski was later president of Peru.

⁷⁰ United Nations, *Report of the High-level Panel on Financing for Development* (2001).

⁷¹ International Conference on Financing and Development, sponsored by the United Nations and held in Monterrey, Mexico, March 19-22, 2002.

is the mother of invention, so when the need became sufficiently acute the international system responded. Thus, when the West realized that it too stood to lose significant revenue to tax cheats, it told the OECD [Organisation for Economic Co-Operation and Development] (a sort of mini United Nations restricted to the economic and social spheres and limited in its membership to rich countries) to arrange for a series of bilateral tax treaties. Each country would reserve against (at a prohibitive rate) the accounts of foreign nationals suspected of receiving taxable income, the reserve being repaid with interest on receipt of information that the income has been reported to the relevant authority. Crucially, the bilateral tax treaties are not limited to OECD members.

Second, the global climate crisis persuaded the United Nations to convene a series of climate conferences. While these have still not reversed the rise in temperature, it appears that world public opinion is now sufficiently aroused to support decisive action.

Finally, the panel called for a summit conference on globalization to follow the Financing for Development conference. This would have given the leaders the powers to remake the international economy if they had been so moved.

RECENT WORK; REFLECTIONS ON THE DEVELOPMENT OF ECONOMICS

In your later years at the Institute, you did more research on capital controls. Could you describe some of your findings on that?

The justification that we had for capital controls was clear enough. Capital mobility without controls tends to be pro-cyclical. So when there's a downturn in the business cycle, then the capital goes out. And the worst is like in the Asian Crisis when countries found they'd embraced capital mobility and then got clobbered as a result.

How would you describe the processes of capital-account opening? What are some of the lessons economists have gained from capital-account liberalization following the 2008 financial crisis?

While I don't think the 2008 crisis was caused by capital account imbalances, I think it *was* true for the previous crisis, the 1997 Asian financial crisis. That crisis was caused by too much capital flowing into the East Asian countries prior to the crisis—and then all leaving at the same time.

Since the collapse of the Bretton Woods system, what should we learn about opening capital accounts?

Since the 1997 Asian financial crisis, economists have developed a view that it's alright to block capital movements which are aggravating the cyclical situation. I wonder whether we should not go further and block all capital movements which are not prepared to accept the risk that their currency will devalue in the future. For example, I wonder whether capital movements which are not prepared to stay a certain number of years (at a minimum, five years; preferably ten years) in a country should be automatically eliminated.

Have countries liberalized their capital accounts too fast?

Yes.

What about since retirement from the Institute? Have you continued writing about economics?

The latest book I wrote⁷² is urging the world towards a system of growth-linked securities. Instead of having a fixed interest rate, one version of the security would bear an interest rate which was positively related to growth in the country. So that a country which grew at 5 percent would pay a higher interest rate than a country which grew at 3 percent. Now in order to sell such securities, one would have to make the security of the slower-growing country attractive to investors by having a higher interest rate than the country which was expected to grow at 5 percent when they launch. Such securities have attractions to borrowers, in providing a lower interest cost following an unexpected decline in growth, and to investors, who can profit from their knowledge of relative growth rates.

In the areas of economics in which you have worked, what do you see as the major advancements that have occurred during your career, either in theory or in policy? Are there any major matters on which you think there has been a retrogression, where economists today have forgotten or neglect something that the economists or policy makers of your youth knew?

The big developments in economics in my fields occurred in the 1930s rather than my lifetime. Where I do believe we have advanced is in terms of the empirics, or at least the capacity to bring correct empirical information to bear.

The big retrogression has been in macroeconomics, where it is now common to denigrate Keynes, because of a theory that has been rigorously demonstrated to be wrong. (For proof, see Mervyn King, *The End of Alchemy*.)

The global financial crisis of 2008-09 has not resulted in any big new theory sweeping economics in the way that Keynes's theory did after the Great Depression. In your judgment, does the financial crisis call for a fundamental reassessment of how we should think about domestic macroeconomic policy or about international policy coordination?

I agree that there has been no big new theory since 2008. In my view this is because there are no large gaps in economic theory waiting to be filled in. I see no need for a fundamental reassessment.

What do you consider your legacy, and how would you hope others might?

Exchange rate policy, clearly. I disagree with the whole notion [shaking head] of freely floating exchange rates. I don't think the arguments against have been presented adequately. Freely floating exchange rates mean government is indifferent to the level of the exchange rate, and I don't think that's a sensible attitude to have. I think one can target an exchange rate without getting locked into permanently fixed rates. And that's what I'm in favor of, for most countries. Now, I do draw an exception for countries that want to integrate and want to create an internal market between themselves.

In a book published by the Institute called *Reference Rates and the International Monetary System* [2007], I advanced the idea that countries would agree on a series of exchange rates and their intervention would be limited to what was going to push their rates towards the reference rates. In other words, if they already had a weak currency they couldn't intervene to further weaken their currency. And if they had a strong currency, they couldn't intervene to further strengthen their currency.

⁷² *Growth-Linked Securities* (2017).

The problems are, first, to get the G7⁷³ to back the idea, and, second, to draw up a grid of exchange rates that all countries would agree to as the reference rates. I do not regard this as a silly quest, but this reflects my belief that countries' interests in exchange rate policy are fundamentally parallel rather than conflictual.

What do you think is the biggest issue facing humanity going into the future?

Climate change, no question about that. I think we ought to have moved quicker to combat it and the United States should have been in the lead instead of being a laggard. I think it's worth a big effort to avoid it now, with determined government action to implement a carbon tax (something that I've favored since at least 1990).

⁷³ Group of Seven large advanced economies: Canada, France, Germany, Italy, Japan, United Kingdom, United States.

Appendix: Williamson's Original and Revised Washington Consensus⁷⁴

1. Fiscal discipline
2. A redirection of public expenditure priorities toward fields offering both high economic returns and the potential to improve income distribution, such as primary health care, primary education, and infrastructure
3. Tax reform (to lower marginal rates and broaden the tax base)
4. Interest rate liberalization (revised version: financial liberalization)
5. A competitive exchange rate (revised version: note that this relates to a desirable current account)
6. Trade liberalization
7. Liberalization of inflows of foreign direct investment (revised version: be cautioned that this is not necessarily a recommendation for allowing complete capital mobility)
8. Privatization of state enterprises
9. Deregulation (to abolish barriers to entry and exit)
10. Secure property rights

⁷⁴ "What Washington Means by Policy Reform" (1990); "What Should the World Bank Think about the Washington Consensus?" (2000).

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