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MD Interview

TOWARD AN ECONOMIC THEORY OF REALITY: AN INTERVIEW WITH GUILLERMO A. CALVO

Interviewed by Enrique G. Mendoza
University of Maryland and NBER

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Guillermo Calvo is one of the most influential economists in the field of international macroeconomics of the past 30 years. He has produced seminal articles in every area of macroeconomics and international economics that he has worked on, including his early classic articles on capacity utilization and time inconsistency, his 1980's works on efficiency wages, price stickiness, and policy credibility, and his recent studies on sudden stops and emerging market crises. Yet, the defining feature of Guillermo Calvo's contribution to our profession is not the depth and wide scope of the economic theories he has developed, but the central emphasis he puts in all his work on the role of economics as a tool for understanding reality and improving the quality of human life.

Guillermo Calvo's passion for the policy implications of economic theory is obvious to anyone who has met him since his days as Senior Advisor of the Research Department of the IMF in the mid-1980's. This feature of his professional interests was much less obvious to those who interacted with him during his early years as an important figure of the rational expectations revolution. It was probably hard to see that behind the highly technical treatment presented in his articles at that time was an author who had his feet soundly set on the ground and focused on understanding how society could benefit from the renaissance of macroeconomic theory that was taking place. Interestingly, Michael Rothschild did figure out the true nature of Guillermo Calvo in those early years. When the University of California at San Diego tried to hire Calvo in the mid-1980's, Rothschild explained to Calvo that he was an excellent fit for San Diego because he was a particular type of theoretician: "Most theoreticians make theory out of theory," Rothschild noted, but Calvo was different because he made "theory out of reality, taking what is really out there and presenting it in a much wider and complex form than a well-developed but narrow theory."

The following pages are excerpts from three interview sessions that Guillermo Calvo and I had at his office in the Inter-American Development Bank in the

Address correspondence to: Enrique G. Mendoza, Professor of International Economics & Finance, Department of Economics, University of Maryland and NBER, College Park, MD 20742; e-mail: mendozae@econ.umd.edu.

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FIGURE 1. Guillermo A. Calvo.

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spring of 2003. These interviews provide a clear summary picture of Calvo as the theoretician of reality that Michael Rothschild described. The recollection of the conversation with Rothschild is one of many fascinating memories of Calvo's personal life and professional career that emerged during our meetings. We taped and transcribed the three meetings in Spanish and then Calvo and I together edited that material to produce this much shorter interview in English for *Macroeconomic Dynamics*.

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Mendoza: Guillermo, you were born and raised in Argentina. I imagine that the experiences you went through growing up in turbulent Argentina played a central role in developing your interest in economics, and in a particular class of economic problems. Would you like to say a few words about how growing up in Argentina shaped your interest in the economics profession?

Calvo: When I was finishing High School in Buenos Aires, there was no school of economics to speak of. However, I was very lucky because my father, who worked at the central bank, brought me economics books from the library there, under the advice of some of his friends who had worked with Raúl Prebisch. I found the material strange and fascinating at the same time. One day, he brought me the *General Theory* and I almost decided that economics was not for me! Fortunately, in school there was a course called Economics. As programmed, it consisted of some kind of history of economic thought, but, once again, I got lucky. The course was taught by Julio Olivera who had a passion for Walras. As a result, we spent the whole semester discussing the Walrasian system. I could hardly believe that a subject that appeared impossible to tackle reading Keynes, suddenly became so clear and elegant. One year later, I joined the central bank and worked under his son, Julio H.G. Olivera, whose orders to me were, essentially: Go to the library, get a copy of Allen's *Mathematics for Economists* and Hicks' *Value and Capital* and don't leave your room until you are done with them. I felt like I had reached Nirvana! In addition, there was a seminar series in which we discussed some key papers coming out in journals like the *JPE*. One of the first such papers was Samuelson's overlapping generations model. Quite frankly, I must say that I became a little dizzy trying to read papers on the frontier of economics, but I found it very encouraging that everything we read had a solid mathematical basis. Thus, even though the deeper economics often escaped me, I felt I had a firm grasp of the reasoning in each one of the papers, and that, I felt, was good enough. Did growing up in such a fascinating economic laboratory, Argentina, influence my decision to go into economics? Maybe, because before I learned economics it was very hard for me to follow the public debate, which was heavily peppered with economic terms. But I believe that what really hooked me at the beginning was the beautiful theory, and finding out that "emotion" could be subject to mathematical analysis.

Mendoza: Let's talk about your decision to go to graduate school. Can you describe how early in your college years you decided that you would go

for an economics Ph.D. abroad, and how you ended up choosing to attend Yale?

Calvo: I had practically no college years because, as I told you, when I started there was no school of economics. Thus, I enrolled in accounting (which I found quite uninteresting). However, at the University of Buenos Aires, where I attended, Professor Julio H.G. Olivera ran a series of seminars that his best students attended, and I was accepted because I worked with him at the central bank. (By the way, Rolf Mantel and Miguel Sidrausky also attended those seminars.) In those seminars, we read Hicks' *Value and Capital*, Samuelson's *Foundations*, and Koopmans' *Three Essays on the State of Economic Science*. With a group of adventuresome classmates, I also independently read the recently published Debreu's *Theory of Value* (which required learning some basic topology). The Koopmans-Debreu duo (both of whom were at Yale at the time) put Yale among my favorite places to go, and made me think of graduate school, not as the next step in a professional career, but as a door to heavenly intellectual delights! However, scarcely having one-third of college under my belt made my chances of acceptance in graduate school extremely slim. Moreover, my willingness to learn the intricacies of accounting was declining at an alarming speed. Thus, there came a time when I felt that I had fallen into a cruel trap, no exit in sight. However, once again my good fortune gave me a hand this time in the form of the USAID (United States Agency for International Development), which offered scholarships in a program at Yale! This was an M.A. program but the best students had a chance of being considered for their Ph.D. program. I was accepted on the basis of recommendation letters, and nobody seemed to care that I was far from graduation. As I was later told, this had been simply an oversight of the Admissions Committee!

Mendoza: What about your years at Yale? Can you give us a quick review of the basic facts (what years where you there, who were some of your classmates, who were the leading members of the faculty), and more importantly, can you tell us about your mentors and your dissertation, and reflect back on how the experience at Yale matched your pre-graduate-school aspirations?

Calvo: When I got to Yale in 1964, Debreu had already left for Berkeley, but Koopmans and Herb Scarf were there. Moreover, Ned Phelps taught growth theory, and David Cass was a young assistant professor who paced around the department like a caged animal, full of ideas and projects. In my second year, Joe Stiglitz showed up and the place "caught fire!" At the same time, we had the strong presence of Tobin and several other famous names. In my particular case, the presence of Carlos Díaz-Alejandro was very important because he was in the process of writing his economic history of Argentina, a masterpiece. It was very good to have someone with whom to talk plain, but deep, economics. Besides, Carlos was a great guy who knew how difficult it is to plunge into a new culture and language, and did his best to lighten the burden. Thus, the straight answer to your question is that I was fully satisfied with my experience, but I had not realized how lonely one could become as a foreigner. Classmates? Edmar Bacha was my buddy. We struggled together to disentangle the mysteries of a language that none

of us had a good command of, a problem that became especially acute when we had to learn about the inventions of the Industrial Revolution in an economic history course (the Flying Shuttle? . . . we still don't know what it is!). Ted Truman was another classmate, but I must admit that I socialized very little (I was already married), and there was not a lot of scientific interaction among students.

Mendoza: So the year is now 1967 (you were 26 years old), but you finished your Ph.D. at Yale at the age of 33. Why did it take you so long? What were the placement options that you contemplated for the beginning of your career and which one did you take?

Calvo: In 1967, I went to a summer program in mathematical economics run by Hirofumi Uzawa, thanks to Koopmans' recommendation. That was a wonderful three months, when I started to work on my dissertation on optimal growth in a vintage capital model. Uzawa had a lighter touch than Koopmans and thus encouraged me to attack problems without, at first, being too careful about the mathematical details. That was all very positive. However, the Vietnam war was still raging and campuses had become ground zero for antiwar demonstrators. That plus my own misgivings about the Vietnam war made me feel more and more at a disconnect with the world around me. As a result, I started to look for options outside the United States. That is why in 1968 I left without finishing my thesis and headed for Lima, Peru, where the Ford Foundation sponsored a postgraduate program at the central bank. Soon after I arrived, there was a military coup, and in 1969, I headed off to Bogotá, Colombia, another Ford Foundation teaching post. I stayed there until December 1972. I wrote several papers inspired by many interesting issues in those countries . . . but no thesis. Remember, this is way before Internet, and way before mathematics became so easy to type! Thus, sending a draft to your advisor via snail mail was a painful and not very promising endeavor. In a way, I had fallen into another trap. But my good fortune prevailed. Carlos Díaz-Alejandro heard that Columbia was looking for assistant professors without success, and mentioned my name to his friend (and now mine) Ron Findlay. In short, I visited Columbia and my good rapport with wonderful people like Ron Findlay and Kel Lancaster was immediate. Two or three days later, I got a telegram (no faxes, no e-mails, mind you) in Bogotá with an offer as a lecturer, which would be transformed into assistant professor upon writing my dissertation. That I did in 1974, at the ripe age of 33! With the benefit of hindsight, however, this long gestation period was very beneficial because I learned a lot of math to tackle an existence proof in my dissertation. (Such proof, by the way, never saw the light of day because it had become so technical that Koopmans thought he would have to ask the math department for an advisor. To make sure that there were no serious existence problems, he did some informal checking with some mathematicians there who felt my proof was OK, which led Koopmans to give me the green light, allowing me to *assume* existence). Equally important, in the years prior to actually writing my dissertation, I was exposed to a set of very rich economic issues first hand, which served me as a source of inspiration for many years to come. Thanks to that detour in the "real world," I realized how relevant economics was for

understanding, and occasionally solving, important problems. At that point, my "marriage" to economics had become complete: beauty and relevance converged!

Mendoza: I have the impression that your Columbia days were very important for your personal and professional life. Can you describe for us the environment that you were exposed to there and how it affected your developing career?

Calvo: Columbia was a crucial step in my career. I had a "dream team" of colleagues: Ned Phelps, Phil Cagan, Ron Findlay, Bob Mundell, Jagdish Bhagwati, Carlos Rodriguez, John Taylor, Stan Wellisz, Maury Obstfeld, Carlos Díaz-Alejandro, and more. This outstanding group of scholars provided the right atmosphere to develop abstract ideas. You see, the "real world" is very good for inspiration, but real-world people, no matter how intelligent they are, have very little patience for theory. I am sure that if I had stayed in the "real world," my career would have been very different.

Mendoza: Together with the classic article by Kydland and Prescott, your article on time inconsistency is credited with making one the most substantial contributions of the rational expectations revolution to economics, both theory and policy. Can you tell us about the gestation process of the idea itself and also about the process leading to the publication of the paper?

Calvo: My friend Assaf Razin claims that time inconsistency was an obsession with me as far back as 1967 (during a seminar organized by Uzawa in Chicago that Assaf also attended). In fact, I used to tease my younger siblings when we were children with time-inconsistency tricks, so Assaf is likely to be right (although I, quite honestly, do not recall. Was I so sickly obsessive that I did not notice?). In any case, soon after I arrived at Columbia, and prompted by Ned Phelps, I became interested in the theory of justice (Utilitarians, Rawls, and all that). I wrote a paper on the time inconsistency of Rawlsian maxi-min principle, for example. Simultaneously, I spent many hours with Carlos Rodriguez trying to disentangle the implications of Auernheimer's recently published *JPE* paper on an "Honest Government Rule for Money Creation" where, it turns out, one can find the seeds of macro time inconsistency. First I wrote an open-economy time-inconsistency example which was published in the *Journal of Money, Credit and Banking*, and later the better known closed-economy example published in *Econometrica*. Motivation for writing the latter I owe to Alvin Marty who, learning about the open-economy paper, told me that if I wanted to reach mainstream macroeconomists, I should make the case in terms of a closed-economy model. He was certainly right. It shows, incidentally, how U.S.-oriented macro was at the time (and still is in some places). In any case, when I was working on macro time inconsistency, Ed Prescott showed up at Columbia to give one of his seminal papers on the subject, but I was out of town. When I came back, I asked a senior colleague about Ed's paper, and he said that he could not quite figure out. Thus, I placed the paper on my To Read list, and did not read it until, during the publication process, the referee (who turned out to be Ed, on his own recognition) pointed an accusing finger at my oversight. This I quickly fixed, the paper was published, and the rest is history!

Mendoza: It is interesting that at times a seminal idea in economics leads to interpretations that are in conflict with those of the originator of the idea, particularly when it comes to economic policy. For example, Mundell's arguments on currency areas and exchange rates have been and are still used by a large number of economists to favor flexible exchange rates whereas Mundell himself sees the same arguments as a strong argument in favor of fixed exchange rates. Do you feel that something similar happened with the notion of time inconsistency? To be more precise, what is your own view on the key policy lessons one should take from time inconsistency?

Calvo: It shows that a policymaker could be a saint and still be a damned liar during the election campaign. In other words, time inconsistency cannot be simply dismissed as a character failure, or a political party failure, which the electorate should be able to weed out. Moreover, the examples show that the negative effects of time inconsistency could be reduced, and even eliminated, by devising new policy instruments or institutions. Hence, there are potentially important lessons for policy and institutions. I believe that the "independence of central bank" issue got a big boost from this literature. I would say the same thing about the political economy literature.

Mendoza: On the theoretical side, I find it very interesting that while some researchers went on to study time-consistent policy in dynamic games, a large chunk of the research you did in the second half of the 1980's focused more on the macroeconomic consequences of having to live with time-inconsistent policies (or with the "lack of credibility" of policies as you referred to it). This use of the word "credibility" is sometimes questioned because the credibility literature that your work started is not based on time-consistent dynamic games. Can you tell us why your interests flowed in a different direction, and what your opinion is of the "proper" use of the word "credibility?"

Calvo: Good question! I believe there is such a thing as premature formalization, and also believe something like that happened with time inconsistency. Once the profession grabbed hold of time inconsistency, researchers started to play games with it, games not necessarily inspired by the real world or policy relevance, but games mostly inspired by the need to become noticed and be published. Don't take this as a criticism of the literature. I am a firm believer in pure research, but volume does not make relevance, or even indicate the priorities set by the profession, especially the seriously committed side of the profession. Speaking of myself, after finishing my papers on time inconsistency, I felt that I wanted to go back to simpler grounds where I could better understand the dynamics implied by time inconsistency. Thus, knowing that in a full-fledged model I could generate time inconsistency, I decided to simplify that part by inserting phenomena that resembled time inconsistency in an exogenous manner and focus more sharply on their dynamic implications. That is partly why my policy temporariness papers came to life. But, another important reason was that I was trying to explain why orthodox stabilization programs in Latin America in the late 1970s and early 1980s did not work out. I was afraid that public opinion would swing to a crazy

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heterodox extreme and all fiscal discipline would be thrown out the window. The credibility papers helped to show that when credibility is imperfect, even good orthodox policies could lead to unsatisfactory results.

Let me add that my sense is that time inconsistency is not an *everyday* problem but something that policymakers are drawn to in extreme circumstances. Leo Leiderman and I, for instance, showed in an AEA paper that one could rule out time inconsistency from the monetary policy of several developing countries. We did that by showing that the first-order restrictions of time-consistent optimal policy could not be rejected. This suggests that time inconsistency as a daily phenomenon may not be relevant, but does not rule out that one can find it once in a while—and in a big way! I don't think the formal literature has explored this avenue, with the exception of a few papers, such as one by Bob Flood and Peter Isard (entitled "Monetary Policy Strategies," *IMF Staff Papers*, 1989, pp. 612–632), which assumes the existence of a fixed cost in exploiting time inconsistency.

Mendoza: There were other important areas you worked on during the early part of your professional career. From my own work, I remember in particular your classic *AER* paper on capacity utilization. Can you tell us more about this paper and some of the other work that you were involved with in the 1970's?

Calvo: This paper was inspired by the debate in Colombia about capacity utilization. Several empirical studies suggested that capacity utilization was very low there, and I wanted to check what basic theory would say about it. However, if anything, my paper thickened the plot because the model implies that capacity utilization should increase with the real interest rate. Thus, if underdevelopment goes hand in hand with high interest rates (which was the conventional wisdom in that world of low capital mobility), then Colombia should rather display high capacity utilization.

On the other work, I already mentioned the theory of justice, but what really absorbed my attention in the early 1970's was the theory of supervision. I wrote several of those papers on the subject with Stan Wellisz. Stan, by the way, is one of my admired figures at Columbia University, and at the time greatly helped me to put relevance into my math scribbling. The focus in these papers was to explain hierarchical ladders in an organization, and structural unemployment. Based on the Imperfect Supervision paradigm, I wrote one of the first papers on what was later called Efficiency Wage hypothesis to explain unemployment. However, I eventually stopped working on this field because I felt that the next necessary step was empirical analysis at the micro level. At the time, I could not find a partner to help me plunge into that uncharted territory, and I felt that doing it by myself would have distracted me too much from the macro issues that still captured my imagination.

Mendoza: To continue on the track of your professional career, you moved from Columbia to Penn in 1986. What were the motivations for your move? Can you summarize for us your experience at Penn?

Calvo: Maury Obstfeld and I left for Penn at the same time. The Department of Economics at Columbia was in a shambles; young assistant professors like Maury were overburdened with thesis projects and got little administrative support.

There were fights among the faculty, some of which became public and made the department a butt of jokes in the profession. Leaving was a very hard decision for my wife and me because we both loved New York and had many good colleagues at Columbia. My stay at Penn was pleasant but short, however. Along came an attractive offer to visit the IMF Research Department, and later a permanent offer, right after the collapse of the Berlin Wall. Being at the Fund meant having a front seat to observe this colossal drama.

Mendoza: Your 1983 *JME* article on staggered prices is one of the most widely cited articles in the recent literature on general equilibrium models with nominal rigidities. Readers will be very interested in learning what motivated you to develop the ideas you proposed in this paper.

Calvo: Ned Phelps and John Taylor had made big strides on that kind of model for several years (as I recall they were already hard at work on these issues in 1973). However, I did not get very interested in it, because I was trying to understand high-inflation countries where, I thought, price stickiness should not be a big issue. All of that changed in 1981 when Argentina abandoned its "Tablita" stabilization plan and underwent a series of large devaluations. To my surprise, unemployment took a big jump, and the real exchange rate suffered a sizable increase. I could not understand this in terms of flexible-price models, which led me to pay more attention to the Phelps–Taylor approach (let me add that nowadays I would first turn my attention to imperfections in the credit market, rather than price stickiness, but that was the early 1980's and the conventional wisdom in theory circles, although, not necessarily among development economists, was that the capital market was not a source of problems in developing countries). Since, once again, my main interest was to understand the dynamics of imperfect credibility, I made simplifying assumptions in order to be able to formulate the sticky-prices model in continuous time (where one can use phase diagrams), and start from utility functions. The latter was important for my purpose, because credibility analysis becomes too ad hoc if you start from demand functions. Let me add a vignette. I started working on this approach when I visited Chicago during the spring of 1981. However, I never made a presentation on it in the Money Workshop where price-stickiness was not part of the conventional wisdom, to put it mildly. I guess I was afraid of being butchered on the spot! You can imagine my surprise when much later I saw mainstream macroeconomists using the framework as a matter of fact. Let me add that the paper you are referring to is a closed-economy macro paper that I wrote after writing the open-economy version motivated by Argentina's experience. I wonder what would have happened to my citation count if I had stopped at the open-economy paper!

Mendoza: What is your opinion of the recent literature on dynamic general equilibrium models with nominal rigidities?

Calvo: It is motivated by the failure of pure RBC models, and it is a natural development. I have contributed to it also with Oya Celasun and Michael Kumhof (trying to account for inflation inertia). Like cash-in-advance, price stickiness models fill a vacuum in general equilibrium theory without which one cannot even

begin to address some basic policy issues in monetary economics. Unfortunately, the microfoundations are still weak.

Mendoza: We arrive at the mid-1980's. This is the time when two of your most famous papers on the macrodynamics of the imperfect credibility of economic policy appeared, one in the *JPE* in 1986 and one in the *JMCB* in 1987. The *JPE* paper aimed to explain the consumption booms associated with failed disinflation programs based on exchange-rate management, and the *JMCB* paper integrated this idea together with a Krugman-style model of currency crisis in an intertemporal optimization framework. Together with the paper that Helpman and Razin published in the *AER* around the same time (approaching the issue from a different perspective), these papers were the originators of the large literature on the real effects of exchange-rate-based stabilizations. Can you describe what sparked your interest to begin writing about this particular issue?

Calvo: Once again, Argentina's devaluation showed to me very clearly that nominal devaluations could result in real devaluation, and all kinds of important real effects could follow. That was the motivation. In my work, real effects followed from lack of full credibility, and that is how, in a way, I was tying up this line of research with time inconsistency. I insisted on bringing in utility and production functions because, once again, it is very hard to analyze credibility problems starting from ad-hoc demand functions.

Mendoza: I would like to switch now for a moment to your transition from Penn to the Research Department of the IMF, which took place in 1987–1988. I wonder what motivated you to consider leaving academia for the IMF. Can you describe the duties you performed at the Fund and give us your impressions about how this experience influenced your research?

Calvo: I first went to the Fund as a visiting scholar, and later was offered the position left vacant by the departure of Max Corden. The head of research was Jacob Frankel, and he gave me full rein to concentrate on research, and visit the field as much as I deemed necessary. The unexpected bonus was guys like you, Carlos Vegh, Pablo, Guidotti, and Carmen Reinhart, with whom I did many research projects. It was hard to go back to academia under those circumstances, when it appeared that I had been granted an NSF for life, an airline ticket that would never expire to see the world after the collapse of the Berlin Wall, and an army of first-rate collaborators!

Mendoza: You and I met at the IMF when I arrived there in 1989. My recollection, in line with what you just said, is that at that time you were the head of an active group of researchers in international macroeconomics. I recall two important research programs in which you were involved. One of them was your joint work with Carlos Vegh, in which you were pursuing further exchange-rate-based stabilization models to analyze the real exchange rate and introduce your staggered pricing setup; the other was your solo work and your joint work with Pablo on a self-fulfilling expectations models of public debt. Can you give us a short overview of these two research programs?

Calvo: Carlos Vegh gave me the extra kick I needed to bring my open-economy price-stickiness models to full fruition. He brought a lot of enthusiasm and creativity to this endeavor, and I am glad he did because the final product could be called Mundell–Fleming Mark II. With Pablo, we worked on the optimal currency denomination and term structure of public debt. This stemmed partly from my AEA 1988 paper on public debt ("Servicing the Public Debt: The Role of Expectations," pp. 647–671) which, in turn, was inspired by the repeated failures of Brazil and Argentina to lower inflation to reasonable levels. The original conjecture was that peso-denominated debt was behind this kind of problem because lack of credibility kept nominal interest high, resulting in unsustainable fiscal deficit (if inflation had actually been lowered). This research led us naturally to explore dollarization and term structure. Interestingly, a solution to the original high-interest problem was dollarization of public debt, while the current evidence and research makes one seriously question such a dollarization policy. Missing from those papers, however, is the sudden-stop phenomenon that has been prominent in recent financial crises, and leads one to take dollarization with a great deal more caution.

Mendoza: At the beginning of the 1990's, the nature of the problems affecting international capital markets was changing dramatically. We went from the debt crisis and the problems of disinflation, to the bonanza of the first half of the 1990's and the surge of inflows of foreign capital into what would be called later the "emerging markets." Amid the chorus of optimistic voices praising this phenomenon as an outcome of the painful years of stabilization and reform in developing countries, a paper that you co-authored with Leo Leiderman and Carmen Reinhart argued that this was probably not the case. Would you like to elaborate on the details of your argument?

Calvo: This line of research surged almost fortuitously from several trips I took around Latin America for the Fund at the beginning of the 1990's. In country after country that I visited, people talked about capital inflows and explained them by some change in *domestic* policy. The conventional wisdom at the Fund also attached primary responsibility to domestic policy, particularly the fact that several countries were beginning to undertake structural reform, and the implementation of the Brady Plan. I became suspicious about this line of explanation as I realized that capital inflows were taking place in countries that were pursuing very different policies and, besides, that some beneficiaries of those inflows were still dealing with serious domestic security issues (e.g., Shining Path in Peru). Thus, I conjectured that there must be a common factor in all of these episodes, and that it was likely to lie outside the region; for example, the U.S.'s Carmen Reinhart (who was working at the Fund's Research Department at the time), Leo Leiderman (who was a visiting scholar there), and I joined forces to explore this conjecture and, sure enough, we found strong evidence of the relevance of "external" factors. Nowadays, several other papers have replicated our results but, at the time, we got a lot of flak, particularly at the Fund where the prevailing view was that if a country did its homework, the capital market would reward it with stable capital

inflows. Our concern was that the flows might be reflecting low interest rates in the United States, which could be reversed very rapidly (like in the early 1980's) and cause financial chaos. Unfortunately, we were right. The Tequila crisis in Mexico 1994–1995 took place after U.S. interest rates started to rise.

Mendoza: While at the IMF, you also worked on the issues related to the transition economies moving out of socialism. In your opinion, what were the main challenges that these economies were facing? If you had to "grade" the contribution that the "West" and the international financial organizations made to the process of "transition," what grade would you give it? Did you have the impression that the tools of economic theory that we had at our disposal were useful in this context or did you find yourself looking for a new toolbox?

Calvo: Transition economies provided us a colossal experiment where everything, all aspects of the economy, had to be thought out at the same time. The policy issues were at the polar opposite of fine-tuning. Even new institutions had to be established. My impression, however, is that the Fund treated some of these cases as if they were economies with well-running capitalist institutions. Thus, for example, bank credit was drastically curtailed without paying much attention to the resulting credit crunch, given that banks in those countries were, by far, one of the main credit suppliers (coupled with inter-enterprise credit). This led Fabrizio Coricelli and I to write a paper on Poland, and a couple of other papers on inter-enterprise credit (the main alternative to bank credit) in transition economies. But these pieces had little impact. Fortunately, several transition economies have grown new institutions, and whether or not we were right is now a largely irrelevant issue.

Mendoza: Now let's talk about your own transition from the IMF to the University of Maryland in 1994. What motivated your departure from the Fund and what factors made you decide to move to the University of Maryland?

Calvo: The atmosphere at the Fund had deteriorated since Jacob Frenkel's departure. This was partly due to losing his protective umbrella, but also to a phenomenon that is quite common in bureaucratic institutions whose main focus is not research: an occasional tendency to devote more resources to the main tasks of the organization, and away from research. Besides, the offer at Maryland was very tempting.

Mendoza: Early in 1994, you prepared some comments for a *Brookings Papers* article that the late Rudi Dornbusch co-authored with Alex Werner on Mexico's slow growth performance despite its impressive reform record. Your comments went on a different track and argued that there were causes of grave concern over the possibility of a balance-of-payments crisis in Mexico because of large and growing financial imbalances in the Mexican economy. These comments became the seeds of your well-known paper "Varieties of Capital Market Crises" and of our two 1996 joint papers on the Mexican crisis (one published in the *AER Papers & Proceedings* and the other in the *JIE*). The comments also led to the *New York Times* article on your work, under the heading "The Prophet of Doom That Was Right." Can you describe the key points of your argument and the thought process that led you in this direction?

Calvo: Rudi and Alex seemed to believe that Mexico could be put on the right track by devaluing 15%, and I thought they were leaving out of the picture a key ingredient: financial stocks. In particular, I feared that devaluation would breed distrust in the minds of investors and lead to a run, which is what happened. When it happened, I was so excited that I worked around the clock on the Varieties paper, and a first version was ready in about three days. The *NYT* note brought me a level of notoriety that I had never known, expected, or looked for, but I cannot deny that it was fun—and profitable! In Argentina when I get an interview, they still refer to me as the guy who anticipated the Tequila. This is funny, of course, but even funnier is the fact that although I may have anticipated the Mexican crisis, I did not anticipate the Tequila—because what distinguishes the Tequila from a garden-variety balance-of-payments crisis is the contagion that the Mexican crisis provoked around the globe.

Mendoza: In our 1996 papers, we argued strongly that Mexico's crisis was not an isolated phenomenon and instead ought to be seen as the first case of a new breed of global capital markets crises. We wrote that these crises could occur even if the "observable" fiscal accounts and other so-called fundamentals were in good shape and that, instead, phenomena such as the anticipation of banking crises and financial contagion in international capital markets could play a central role. My recollection is that authors such as Tim Kehoe and Hal Cole or Andres Velasco and Roberto Chang shared our views but, for the majority of the profession, it took the Asian crisis of 1997 and the Russian debacle of 1998 to accept this notion. With the large number of crises that have now occurred, do you think that the facts validated the views we expressed in 1996?

Calvo: Yes. Capital markets are incomplete, especially so in emerging markets. In that context, capital inflow episodes do not necessarily generate efficient solutions, but at least do not generate catastrophes, while inflows last. As credit increases on average, borrowing from a new source can easily offset a cut in a line of credit from another. Thus, under those conditions it is unlikely that serious systemic problems would develop. On the other hand, as capital flows dry up or are reversed, there are few efficient ways to deal with the situation. For the private sector, we have bankruptcy regulations, which may be efficient for big firms, but create havoc everywhere else. Moreover, there is no equivalent to Chapter 11 for sovereign countries. Thus, there is a strong element of self-fulfilling expectations when capital goes out—which need not be correlated with traditional fundamentals such as fiscal deficits. For the capital outflow to cause damage, the country must display significant debt levels. This explains why all of these crises have followed periods of significant capital inflows.

Mendoza: It appears that, compared to the 1990's, we have now entered a very different period in global capital markets in which non-FDI inflows into emerging markets have all but dried up. What lessons should we draw from the crises of emerging markets? For example, do you think developing countries should try to manage or control capital inflows? What role should international financial institutions play in the new era of globalized capital markets?

Calvo: Controls on capital inflows should be a policy of last resort. I believe there is a role for the G7 here, a "traffic control" role, in which a new institution or facility is created to ensure that the price of emerging market debt is not subject to large swings, one way or the other. In a recent paper in *Economia*, the journal of the Latin American and Caribbean Economic Association, LACEA, I called the new institution EMF, Emerging Market Fund. In this fashion, there will be some control at a global level, helping to prevent global systemic problems, but capital would still flow freely across emerging markets. The alternative of each country establishing its own regulation could give rise to problems akin to competitive devaluations. The end result could be a worldwide investment allocation that could hardly be called efficient. Besides, without international cooperation, it is very hard for an individual country to establish effective controls on capital flows.

Mendoza: From the standpoint of economic theory, explaining the phenomenon that you labeled the sudden stop (a sudden, large reversal of the current account and capital inflows coupled with Great-Depression-size collapses in domestic production, absorption, and relative prices) remains a major challenge. We published a short article in the 2000 *AER Papers & Proceedings* suggesting some lines of research based on various forms of informational and financial-market imperfections that seemed promising venues to account for sudden stops. In your opinion, how far are we in developing a sound theory of sudden stops, and are you confident at this point that we can use this theory to propose policy measures that can be useful to prevent sudden stops?

Calvo: Developing a sound theory of sudden stops requires making assumptions on the nature of market imperfections. Thus, it will be hard, if not impossible, to develop a general theory of sudden stops. Therefore, the goal I have set for myself is to develop a set of examples (a bit like I did in my Varieties paper), continue to look closely at emerging markets, and conduct some econometric testing. Unfortunately, the typical theorist in the profession, who feeds himself on theory, will likely have a tendency to try to find explanations in complete-markets theory, because developing that theory can be done without crossing the Rio Grande. Thus, if crises cease to be as frequent as they have been in the past decade or so, I am afraid that the profession will tend to commit sudden stop theory based on market imperfections to the trash can. I said "I am afraid" because, as a result, future policymakers (including the IMF) are thus likely to make the same mistakes they made in the 1990's.

Mendoza: In 2001, you took a two-year leave of absence from your Maryland position to take on the job of Chief Economist and head of the newly created Research Department of the Inter-American Development Bank. Can you tell us what lessons you have learned from your experience as chief economist and head of research of an international financial organization?

Calvo: What is exciting about RES at IDB is that it represents the only policy-oriented research unit in all of Latin America that has a serious regional and global view. Moreover, through a series of networks, RES is in close contact with key policymakers in the region. I believe this kind of activity is very productive

FIGURE 2. Group picture taken at the Inter-American Development Bank in November 2003. Back row, left to right: Eduardo Fernández-Arias, Carlos Vegh, Ernesto Talvi, Alejandro Izquierdo, and Eduardo Borensztein. Front row, left to right: Sara Calvo, Enrique Mendoza, Graciela Kaminsky, Guillermo A. Calvo, Carmen Reinhart, and Pablo Guidotti.

because, to the extent that countries choose the globalization route, they have to engage in serious, professional dialogue with other countries pursuing the same goals at a regional and global level. Moreover, providing countries with a global view is very useful because, in a globalized environment, countries are highly vulnerable to external shocks. In the absence of a global view, typically the domestic policy debate tends to ignore global factors and focus mostly on domestic issues. Politicians love grabbing each other's throats!

Mendoza: In this last section of the interview, I want to ask you some "frequently asked" questions about the economics profession and its future. In your opinion, which are the most influential contributions to macroeconomics and international economics over the past 30 years that you have been an active researcher in these fields?

Calvo: I, for one, benefited greatly from the so-called rational expectations revolution. The reason is that it gave me a framework in which I could tackle credibility-type issues. Prior to that, one has to recall that macroeconomists thought that monetary theory could not be formulated under rational expectations. In his famous and very influential hyperinflation paper, for example, Cagan shows that as the economy converges to RE (perfect foresight in his paper because uncertainty was not explicitly modeled), the steady-state equilibrium became unstable. In particular, the model could display hyperinflation with a constant money supply!

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As a result, until the early 1970's, most papers assumed adaptive expectations with sufficiently long lags. It could be argued that AE reflects some kind of lack of credibility—people don't pay attention to policy announcements, and prefer to extrapolate from the past. But aside from the fact that AE would be just one particular form of lack of credibility, there is the fundamental issue that one simply would not know the implications of full credibility because the latter would imply rational expectations! On the other hand, in the field of international finance, I believe the work of Fleming and Mundell was very important because it showed the macroeconomic relevance of the capital account of the balance of payments for small economies. In addition, it helped to extricate the field from its obsession with not looking outside the womb, and only focusing on closed-economy models.

Mendoza: If I ask you to name one or two economists that you consider your "favorites" or the ones who have influenced your own work the most, who would you name and why?

Calvo: Here is my short list: Mundell, Phelps, Lucas-Sargent-Wallace. I already explained why Mundell is on the list. Phelps was my teacher at Yale and I found him fascinating. He really helped me grow when we were colleagues at Columbia. The last trio I have also implicitly alluded to before. The three of them, however, have not only shown that rational expectations is an interesting hypothesis, but have done fundamental research that helped to establish RE as a valid scientific discipline. By doing that, incidentally, they also opened the door for eventually *rejecting* the RE hypothesis.

Mendoza: In terms of the social value of our profession, what do you think have been the most important contributions of the fields you work on to improving social welfare?

Calvo: It is very hard to tell. I sometimes think that countries learn from their own mistakes (and sometimes not even that!), and there is little an economist can do to help. Having said that, however, I believe the main contribution of macro has been to teach basic issues such as the price level has a lot to do with monetary variables; expectations are key, and thus for policy effectiveness, credibility is central; external factors are important; the financial sector plays a critical role for macro stability; and financial vulnerabilities are behind major catastrophes in recent emerging market crises, etc.

Mendoza: If you had a genie that offered to give you the answer to two unresolved economic problems, which ones would you choose? Or to put it differently, on which areas do you think we should direct our efforts as researchers in the next few years?

Calvo: The two areas where we have made some progress but I feel there is a lot ahead of us are growth and poverty alleviation. Maybe triggering growth will take care of the latter, but poverty alleviation is so important that we should not wait any longer and put lots of research effort in that direction.

Mendoza: Finally, and since you are one of the masters of time inconsistency, if you were given the chance to start over from any of the different periods of

your career that we covered here, would you reoptimize and move in a different direction than you did?

Calvo: Not really. I am glad that I have straddled academia and policy circles. I have learned a lot and, above all, I've had a lot of fun!

Mendoza: Thanks a lot for this fascinating review of your career.

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