

# Remembering Alan Stockman

*Maurice Obstfeld*

July 14, 2010

I would like to share some memories of Alan from the perspective of someone who received an economics PhD and embarked on an academic career at about the same time, in the late 1970s.

Alan received his degree from Chicago a couple of years before I got mine from MIT. Then he did a postdoc at UCLA before taking up an assistant professorship at Rochester, where he gained tenure and remained for the rest of his career. At Rochester Alan produced a remarkable string of his own PhD students, some of whom are here in this room today. I've had the privilege of learning from many of them and collaborating on papers with two, Hal Cole and Margarida Duarte.

Inevitably my memories mix the personal with the scholarly. As struggling young academics, Alan and I – as well as others in our talented cohort – shared the fellowship of those striving to establish careers, were competitors in advancing our papers and ideas, but also learned much from each other through personal conversation and outright collaboration.

In that connection I must take this opportunity to thank our host, the National Bureau of Economic Research, whose Summer Institute and conference series have created unprecedented and irreplaceable venues for intellectual network effects for more than three decades now.

Alan is certainly in the first rank of those I have learned from throughout my career. By way of describing some of my personal gains from intellectual trade with Alan – and time will allow description of only a proper subset – I can start with our very first meeting.

As a student of Rudi Dornbusch, I inevitably met Rudi's former grad-school classmate Professor Jacob Frenkel of Chicago, prior to his service as research director at the IMF and Governor of the Bank of Israel. Jacob was on the program committee for the June 1979 North American summer meetings of the Econometric Society in Montreal, and he generously invited me to discuss Alan's paper "A Theory of Exchange Rate Determination," which appeared a year later in the *JPE*. This paper did not focus on current-account dynamics, which interested me at the time, but it built on intertemporal micro-foundations that clearly had the potential to elucidate the drivers of the current account.

So this paper made a big impression on me. Alan's focus was on incorporating a cash-in-advance framework for money demand, which Bob Lucas (following Bob Clower) had been pioneering, and investigating the implications for exchange-rate determination. Alan's 1980 *JPE* paper was later extended in important ways by Lars Svensson, both in individual work and in collaborative work with Alan.

Unfortunately, the implied tight link between consumptions (or outputs) and exchange rates was a dead end empirically, as underlined by the later literatures on the exchange-rate disconnect and the consumption-real exchange rate anomaly. Alan's 1989 *JME* paper with Marianne Baxter, "Business Cycles and the Exchange Rate Regime," is one of the fundamental contributions to the former literature. Needless to say, this was not the point of my 1979 discussion – I had never discussed a paper at a conference before, I was terrified, and I have no idea what I mumbled on that occasion.

Our session included another future *JPE* paper by two other Chicago doctors, Leonardo Leiderman and Mario Blejer. After the session Alan, Leo, Mike Mussa, and I repaired to a nearby restaurant for lunch. This was very exciting for me – as a newly-minted MIT PhD, my job-market tour had been my only opportunity to interact with economists outside of the narrow focus of Cambridge, Massachusetts. So I didn't notice that a full hour had passed since ordering and food had still not arrived. Mike Mussa did notice, however. He angrily made a no-doubt accurate allegation of Gallic mistreatment of English speakers, and stormed out of the restaurant. Alan, Leo, and I sat there, rather stunned, but still content to stay and talk about macroeconomics until the food arrived (after about another 45 minutes).

At the meeting Alan handed me a first draft of a paper that eventually became "Real Exchange Rates under Alternative Nominal Exchange Rate Systems," published in 1983 in the *JIMF*. Busy with moving to Columbia to take up my first faculty appointment, as well as with initial course preps, I confess I barely glanced at the manuscript. But it contained an important idea that I had independently stumbled on during my final year in grad school, that the main transmission mechanism for sterilized foreign exchange intervention is undermined by Ricardian equivalence. Alan's treatment was much more elegant than mine.

Later on, Alan and I coauthored a chapter on "Exchange Rate Dynamics" for the *Handbook of International Economics*, volume 2, published in 1985. Originally the chapter was to have been written by Pentti Kouri, but Pentti showed up at the *Handbook* pre-conference without a first draft. The editors of that *Handbook* were Ron Jones, Alan's senior colleague at Rochester, and Peter Kenen, who had served as provost at Columbia and therefore took an avuncular interest in my own career. Nervous about the chapter, they approached Alan and me in 1982 or 1983 with the proposal that we take it over, and we accepted.

I can't let Pentti Kouri's name pass without a few words, especially as he passed away in January 2009, also of cancer, and also just shy of his 60<sup>th</sup> birthday. Most of you probably never met him. He was an imposing personality, as well as one of the most brilliant minds ever to grace international finance. The list of his contributions is relatively brief, as he quickly left academe to become a venture capitalist (eventually collaborating with George Soros in a leveraged buyout of the Finnish economy). However, Pentti's writings make up in quality what they lack in quantity. One chapter of his MIT doctoral dissertation stands with Alan's chapter in the 1978 Frenkel-Johnson book on *The*

*Economics of Exchange Rates* as one of the best early papers on the foreign exchange risk premium.

In person, as I hinted earlier, Pentti was quite intimidating – pretty much the opposite personality type as Alan, who was approachable, warm, and modest. Even though Pentti was not much older than Alan and I, we somehow felt that we were supplanting a much more senior figure. We were not alone in our awe of Pentti. I remember the story of young Paul Krugman, who was traveling on an airplane in economy class, timidly soliciting comments on his new paper from Pentti, who was traveling on the same plane in first class. The paper built on one of Pentti's models; it was called "A Model of Balance of Payments Crises!" In the end, Alan and I got over our trepidation and divided the *Handbook* chapter up according to our comfort zones, with me taking the sticky-price and Kouri-style portfolio models, and Alan pursuing models based on the Lucas-Phelps asymmetric information islands approach. We mapped all this out one evening at the Summer Institute (of course), over a pizza and a bottle of Conn Creek cabernet sauvignon.

As a product of the Chicago school dominated by Friedman and Stigler, Alan had an intense respect for the empirical basis of economic theories and a thoroughly scientific openness to the implications of data and evidence. He was never doctrinaire and definitely willing to change his mind in the face of new facts. Attracted by the real business cycle approach, he nevertheless found, in his classic 1995 *AER* piece written with his student Linda Tesar, that something besides technology shocks was needed to rationalize the international comovements in the data. An important 1988 *JME* paper documented country-specific as well as industry-specific (i.e., technology) shocks.

A 1988 Carnegie-Rochester paper on "Real Exchange Rate Variability under Pegged and Floating Nominal Exchange Rate Systems" tried to contest Mussa's interpretation of the empirical record as supporting sticky-prices; in that paper Alan posed what was to become known as the "purchasing power parity puzzle." But eventually, as Alan confessed to me at a Gerzensee conference in the early 1990s, he concluded that some price stickiness was needed to explain the data, and his subsequent theoretical work with his student Lee Ohanian reflects that conclusion.

One other facet of Alan's work that may be little known is an embryonic graduate text in international finance that he began writing with Torsten Persson during the 1980s while Torsten was a visitor at Rochester. The book was never completed, but I was fortunate to receive some chapters in the mail. (I believe Torsten sent them to me; Alan was not too organized about distributing his own work.) The idea that such a text was needed struck me at the time, and helped inspire my later efforts in collaboration with Ken Rogoff on our *Foundations* book.

I could go on enumerating other key insights contained in Alan's work, but the organizers have asked for a focus on personal recollection and I would like to respect their wishes.

In 2001 I attended the Carnegie-Rochester meeting in Pittsburgh. Alan and Margarida Duarte were to discuss a paper by Mick Devereux and Charles Engel, but Ben McCallum made an unexpected announcement to the conference: Alan would not be able to attend. He was having intestinal surgery, cancer having been discovered a short time before. Margarida had to do the discussion on her own. Alan (who was born in 1951) had gone for the routine colonoscopy that physicians recommend at age 50. And the cancer was discovered.

Some months later, in October 2002, I sat next to Alan at the memorial service MIT organized for Rudi Dornbusch. Rudi had passed away the summer before after a long struggle with cancer. Rudi's many close friends and students turned out in droves, of course. I was happy but surprised to see Alan, who was not personally close to Rudi. I think Alan was drawn to make the trip from Rochester to Cambridge by more than his respect for Rudi's immense intellectual contributions to the field they both loved. There was also a feeling of special kinship, because Rudi had been felled by the same disease Alan was battling. At that time Alan seemed to be doing well, though, and we all were hoping the surgery had been a success.

We know now that it was not, and that Alan was to be taken from us early, too early. But not before he had fought his disease for nearly eight more years, all the time continuing to contribute to our profession and to his department, where, remarkably, he served as department chair. He did this in the face of turmoil and difficulties in his personal life that seemed unfair and undeserved to the many in this profession who saw Alan as a role model and an inspiration, as a *mensch* in every sense of that old-world expression.

I have gone on way too long. I have taken undue advantage of the fact that because I am not actually standing in front of you, you cannot tell me to shut up. My excuse is simple: once my memories put me in Alan's company, it is so very hard to leave. I know the leave-taking has been equally hard for all of you who had the privilege of knowing Alan Stockman. He behind leaves a remarkable legacy, in our minds and in our hearts.