

Become an Economist – See the World



Jeffrey Frankel

CHILDHOOD: THE GOLDEN STATE

I grew up in California in the 1950s and 1960s. I considered myself then entirely a child of that time and place. I have always enjoyed being able to tell people that I was born in San Francisco.

To me, California in that golden age had nothing to do with hedonism. During an era known for psychedelic rock in San Francisco – the late 1960s – I was probably one of the few kids in my high school never to try drugs or even touch alcohol.

California to me seemed the culmination of a linear westward march of civilization throughout history. Here is how it went. The first great civilizations arose in Asia, followed by the Egypt of the pharaohs. Progress had flowed westward ever since: the Greece of classical culture, the Rome of the Senate, the Florence of the Renaissance, the England of the Industrial Revolution, the America of the thirteen founding states, and the legendary pushing westward of the frontier.

In my imagination my parents had participated in the final phase of this logical progression. My mother and father grew up in Detroit and Cleveland,

respectively, and met at the University of Chicago, at a time when it was dominated by the great books curriculum.

I puzzled a bit over what the logical next stage would be in this historical progression. The possibility that it would be a leap across the Pacific to East Asia did occur to me, even then. But it seemed more likely that the West Coast was the limit. There were no more frontiers left. After all, the millennium was coming. At age twelve, I made a list of things that could be counted on to occur in my lifetime: the bicentennial (1976), 1984 (as in Orwell's book), the return of Haley's Comet (1986), and the millennium. After that? Nothing. No more known dates.

Such words may sound apocalyptic. But that is not at all what I meant. I viewed my time and place as a pinnacle of human well-being – a limit in the sense of an “absorbing barrier,” not in the sense of the end. I knew that few decades of history, and few parts of the world contemporaneously, enjoyed the standard of living that my close nuclear family enjoyed, as my brother and I grew up in a California suburb with the sedate name of Kensington. There was a probabilistic paradox: What were the odds that I, thinking about this, would have been born in such a unique time and place? In retrospect, my thinking was excessively linear and extrapolative. Today, I think far more in terms of cycles. Nevertheless, I had a point at the time. First, I correctly perceived how lucky I was. Second, the statistical paradox is similar to the scientists' puzzle that is still unresolved: Is the origin of life on this particular planet an improbable miracle? Or is that a silly question, because if it hadn't happened, there would be nobody to ask it?

ETHNIC IDENTITY

Of my childhood friends, one requires mention. Barry Eichengreen was my classmate starting in preschool (age three). We were best friends and played chess together every weekend. He went to Hebrew school, so I did know he was Jewish, the lone exception to the secularity of my environment. (My family was Jewish, but I barely knew it.) What can one say about the fact that Barry and I eventually ended up in the same field, international economics, and even on the same Economics Department faculty (UC Berkeley, in the late 1980s and the 1990s), other than “there must have been something in that water in Kensington.”

Just to finish off the subject of religion: Every Saturday, my father would conduct my brother and me in what I now recognize as an excellently designed substitute for religious education. We read books like *Myths and Legends of All Lands*. I am not sure whether my father was trying to send the

message that the Bible stories stood on the same footing as the Greek myths and the rest of them, but that is the message I got. Personally, I preferred the Greek myths, a taste that my own son has now taken on with enthusiasm. I don't want to offend anyone, but Prometheus seems to me a worthier hero than Moses. At one time I briefly bought the line that monotheism had been a step forward historically in that it brought the end of human sacrifice. But then I discovered that the Greek gods detested human sacrifice (see Tantalus). The lesson of the Abraham and Isaac story, meanwhile, seems to me that any atrocity, including human sacrifice, is justified if you are following orders from a higher authority.

LIBERALISM AND THE VIETNAM WAR

I also felt myself intellectually a child of the Enlightenment. Perhaps I naively thought that everybody was a child of the Enlightenment. I am confused today by what most Americans mean when they say "liberal" and "conservative."¹ But to me, liberal meant the Enlightenment, the American Revolution, and freedom, and conservative meant oppressive hidebound institutions such as monarchies, dictatorships, and religious establishments.

The Vietnam War dragged on throughout my teen years. I, like others, thought it was a huge mistake from the beginning. How could the US military, fighting far from home, hope to prevail over a guerilla army that felt it was fighting for its country's freedom? Yes, the army could clear the Vietnamese out of any given geographic patch of ground. But how did that help transform the country in the way we wanted? Didn't we remember the lessons of the American Revolution? Didn't we understand that we were now the Redcoats? Yes, communism was a bad way to run a country. But the sooner we got out of the way, the sooner the Vietnamese would figure that out for themselves.

Further, Lyndon Johnson had originally misled the country about specifics in order to get us into the war (the Tonkin Gulf Resolution), was repeatedly wildly optimistic about what would be required, and was reluctant to raise taxes to pay the cost. Even after it was clear that the initial goals were not achievable, Richard Nixon came up with new reasons we had to stay in. One of the arguments, as is so often invoked in military interventions, was that to pull out would mean a loss of face and credibility for the United States. It never seems to occur to those who make this argument that we lose much more face and credibility if we stay in, double the stakes yet again, and end up eventually pulling out anyway. It seems to me that the United States repeated all these mistakes more recently in Iraq.

At high school graduation, I gave the valedictorian speech. My subject was the war, though I focused mainly on the importance of opposing it non-violently so as to avoid alienating the undecided middle-American opinion. Some parents walked out. Evidently they were alienated anyway.

I had a college deferment, and later a high draft number (211): I came along late enough that I never had to face being asked to participate in a war to which I was opposed.

EDUCATION

I attended excellent schools growing up in California: first, great public schools, during that shining era when California had the best public education in the world and before tax-cutting fanaticism became the sole guiding economic ideology of a substantial fraction of the electorate; and then a great private – but free – high school in San Francisco, with the Hogwarts-sounding name of Lick-Wilmerding.

When I first went east to begin college at Swarthmore, I would not have expected to major in economics. The reason is simply that I, like many students at that stage, had virtually no idea what economics was about. Today, if I had to define the discipline, I would say something about maximizing objective functions subject to constraints. At age seventeen, I would have said economics was all about money.

Sampling various academic fields, I soon developed a way of viewing them that, in retrospect, only a future economist could dream up. It seemed to me that one could array the disciplines along a continuum, with mathematics at one end, followed by physics, chemistry, and biology, and with philosophy at the other end, preceded by the humanities and then the social sciences. At one pole, mathematics held questions that could be answered with enormous precision but were in themselves of no direct import. The opposite pole, philosophy, consisted of questions that were of the largest possible consequence but could not be answered at all.

How, then, to choose a field in which to specialize after the completion of one's liberal arts education? The objective function that seemed the right one to me was the product of two factors: the importance of the questions in a field, multiplied by the ability to answer them. What use was a field where the questions were of cosmic importance but the ability to answer them, when all was said and done, was zero? At that end of the spectrum, the product of the two factors was zero. But what use was a field where precise answers were possible but of no direct use in my daily life, as either an individual or a public citizen? The product again was zero. What field

maximized the product? The one in the center, of course: economics. The questions were important, if not as important as the meaning of the universe. The answers to those questions were substantive, even if not as precise as mathematical theorems.

Within economics, my greatest interests were in international economics, macroeconomics, and econometrics. I originally learned international economics from the first edition of a textbook by Richard Caves and Ron Jones, little dreaming that one day I would be coauthor of the fifth through tenth editions.

I didn't discard other disciplines. Looking back on things I learned in high school and college, I am happiest perhaps recalling some of the unlikely-sounding intellectual connections across fields that we overspecialized modern academics usually do not get to make in our adult professions. Here are a select few that I happen to have made use of at some point or other in my economics career:

- From classics: The Greek myth of Odysseus tying himself to the mast is a versatile metaphor for solutions to the problem of “dynamic inconsistency” in monetary economics and elsewhere.
- From American intellectual history: *The Wizard of Oz* is an allegory for the nineteenth-century gold standard.
- From French literature: Albert Camus describes how a deadly plague (in Oran, Algeria) peaks one day and begins to ebb – after what seems like forever and without any clear evidence that the heroic efforts of the medical workers fighting the contagion in fact made the difference. The description fits well with a modern economic crisis.
- From biology: Although one can “feel the pain” when a gazelle on the savannah or a manufacturing firm in a competitive market meets a brutal end, as a scientist one needs to understand the general equilibrium of the system.
- From chemistry: A form of Le Chatelier's principle was generalized beyond the physical sciences by Paul Samuelson. If you exogenously change one variable in a system (heat or money supply), the reaction of one of the endogenous variables (the pressure or exchange rate) will be greater if a third endogenous variable (the volume or price level) is held fixed than if it too is allowed to respond.
- From mathematics: Even though most of us cannot name more than three irrational numbers, an easily understood proof reveals that there are in fact more of them than of the (much more familiar) rational numbers. This can be used to illustrate the limits to inductive reasoning

in philosophy, the dangers of sample selection bias in econometrics, the “availability heuristic” bias of psychology, the need for Bayes’s theorem in probability, and the problem of “black swans” in the housing market or in antiterrorism policy.

True, as PhD students soon discover, narrow specialization is the only way to complete a dissertation, to get a job teaching in a university economics department, and to get tenure. But I think of those stages as akin to basic training in the army or to thirty-hour shifts in medical residencies. After one has achieved the prize (tenure), one can work on whatever one wants to work on.

MIT

My mentor at Swarthmore had been Bernie Saffran, unparalleled Chiron of economics neophytes and a sterling human being. When he packed me off to MIT for grad school in 1974, it was like d’Artagnan’s father in the provinces sending him off to join the King’s Musketeers in Paris. He told me of his impression that students in the MIT economics program sorted themselves out by ability pretty quickly, implying that one did not have to be insecure about where one stood after that. Within a few weeks of the beginning of classes at MIT, we all knew that Paul Krugman was the smartest student in our year. I have never felt insecure about that; Bernie was right.

My fields included econometrics, where my professors were Bob Hall and Jerry Hausman, and macroeconomics, where my professors included Franco Modigliani and Robert Solow. The latter two were obvious candidates for Nobel Prizes; they got them ten years later. Paul Samuelson, who was one of my micro theory teachers, had already gotten his.

I knew from the start that my primary interests were international. Jagdish Bhagwati, another of my mentors, was my international trade professor. But at that time, the macro and finance side of international economics seemed more exciting than the trade side. Exchange rates had begun to float in 1973; four years later we had enough monthly data to run regressions; capital flows, inflation, and unemployment were all unusually high in the mid-1970s; and the rational expectations revolution was remaking macroeconomic theory from the ground up.

During my first year at MIT, I studied international finance with Charlie Kindleberger, a scholar and a gentleman. But in my second year, a young new professor arrived, named Rudiger Dornbusch. I have been pleased to be sometimes known as Rudi’s first student. (Paul Krugman could claim

the honor, since he finished his PhD in three years rather than my four. But I think Paul was in a hurry to establish his independent identity and was happy to leave the title to me.)

Rudi and Stan Fischer taught open-economy macro together. Ken Rogoff, Maury Obstfeld, and Ben Bernanke were among those in the year behind me. Some of our contemporaries two miles away at Harvard, including Jeff Sachs and Larry Summers, came down to audit the class. As Ken wrote not long ago, regarding Rudi's habit of cold-calling students with impossible questions, "I would venture that Dornbusch's international finance course at MIT is the answer to the trivia question 'When was the last time these guys were completely humiliated in public?'"²

I would give anything to have a videotape of one of those classes, especially one relevant to balance-of-payments crises in developing countries. Later, during the period of the emerging market crashes in the 1990s, Sachs strongly attacked the management of the crises by the US Treasury (where Summers was calling most of the shots, as undersecretary) and the International Monetary Fund (where Fischer was calling most of the shots, as deputy managing director). Newspaper readers must have wondered what the story was behind this conflict, in terms either of schools of thought or of personal conflict.³ It is interesting to recall, then, that meetings of the Dornbusch-Fischer course in the mid-1970s included, in one room, the following dramatis personae: two students who were to become two of the most important country policy makers presiding, for all their brilliance, over the run-ups to the first and last of the 1990s crises, respectively (Pedro Aspe, finance minister of Mexico in 1994, and Domingo Cavallo, economy minister of Argentina in 1991–1996 and 2001); perhaps the most important hands-on fashioners of the response in Washington (Summers at the Treasury and Fischer at the IMF); and three of the most important outside kibitzers (Dornbusch, an unwelcome augur of the Mexican peso crisis; Sachs, the most sweepingly critical of austerity programs; and Krugman, less critical). There were no big doctrinal disputes or personal animosities to speak of, either in the 1970s or in the 1990s – just different interpretations of what should be done in difficult situations.

Stan and Rudi, my main mentors, were the most popular duo for advising theses in those years at MIT. Neither one ever needed to spend any of the twenty-four hours in a day on sleep, so far as I am aware. Stan always seemed able to find time to read any paper that one of his students sent him and return it rapidly with perfect comments. Rudi would call students up at night to invite them to meet a visiting economist for cappuccino in the North End.

One day, in his office, Rudi tried out the idea of exchange rate overshooting on me and asked what I thought. I was appropriately flattered but told him that I would have to think it over first. The next day I came back and told him I thought it was a good idea.

MIT at this point was, I think, establishing the template that a thesis could be “three essays on X.” My X was exchange rates. My central essay was later described by somebody as the first empirical implementation of Dornbusch overshooting. I guess that is a fair description. Certainly I remember that Rudi gave my paper its title (“On the Mark”), without first consulting me, when he signed me up for a job market seminar at the University of Chicago.

THE RESEARCH OF A JUNIOR PROFESSOR

One could also say, in broad perspective, that much of my early research took off from the overshooting theory and then went off in varied directions. Some papers dealt with one or the other of the two key building blocks of the model: uncovered interest parity in the short run and purchasing power parity in the long run. (Overshooting is a consequence of the combination of slow adjustment of prices in goods markets and instantaneous adjustment of asset markets.) Other papers transplanted the insights from the foreign exchange market either to the determination of the interest rate term structure or to the determination of prices of agricultural and mineral commodities. The latter application was the more successful. Just as even a stopped clock is right twice a day, the prediction that an increase in real interest rates should cause a decrease in real prices of oil, gold, and other commodities struck some as right on target in the early 1980s, and the reverse prediction seemed right on target in 2008 and 2011.

A few of my early papers were theoretical. But I soon discovered that, for the most part, my empirical papers sold much better. In some cases, coming up with a new data set took almost as much work as writing the paper. That perhaps applies to my papers coauthored with Charles Engel or Gikas Hardouvelis that used weekly money supply announcements relative to market expectations for “event studies”; my work with Ken Froot that used survey data to study expectations in the foreign exchange market; and my research coauthored with Kathryn Dominguez that used previously confidential daily data to study the effectiveness of intervention in the foreign exchange market. As even this early list shows, I have always been blessed with excellent PhD students and other young colleagues with whom I have collaborated.

I believe that the returns in knowledge from adding to the data set and performing some simple statistical test are greater than the marginal benefit of running the same old overstudied data – such as the standard macro variables for the G-5 countries – through some pointlessly more sophisticated theory or econometric technique. In the 1980s, it became fashionable to claim that the real exchange rate followed a random walk, because statistical tests were unable to reject that null hypothesis at conventional significance levels. (Analogous claims were made about all sorts of variables in macroeconomics and finance.) But these tests were typically run on a few decades of data. I argued that one would not expect such limited data sets to offer enough power to reject the random walk even if mean reversion were the right answer. Economists had forgotten the lesson from introductory econometrics: failure to reject the null hypothesis does not entitle you to assert that the null hypothesis is necessarily true. More provocatively (in “Zen and the Art of Modern Macroeconometrics”), I alleged that economists had subtly redefined the rules for a specific reason: it was too hard in macroeconomics to find statistically significant relationships. It is much easier to fail to find significant relationships. It hardly takes any work at all. But the affirmation “my research supports the hypothesis that the exchange rate follows a random walk” sounds much more respectable and publishable than “I have been studying exchange rates statistically for a year and have absolutely nothing to say about what makes them move.”

If one is in pursuit of the right answer, one needs to cast the net wider, to encompass a century-long time series or a panel of countries. On a priori grounds, that is how much data it should take before the test will have the requisite power. Sure enough, when one did that, one could reject a random walk in the real exchange rate and find mean reversion.

Many have taken to using the “black swan problem” to mean a highly unlikely event, as the subprime mortgage crisis of 2007–2008 is interpreted to have been. The way I would prefer to define it is as an event that is considered to be virtually impossible by those whose frame of reference is limited in time span and geographical area, but that is well within the probability distribution for those whose data set includes other countries and other centuries (or those who make appropriate use of a priori theory, as with those irrational numbers). Analysts don’t cast the net widely enough. They can’t imagine that terrorists might inflict mass casualties by bringing down some buildings (New York, 2001) or that housing prices might fall in dollar terms (United States, 2007) or that an advanced economy might suffer a loss of confidence in its debt (Greece, 2010). “I haven’t observed such a

thing in the past, so it won't happen in the future." These things had happened before, but mostly in times and places far away.

What do "black swans" have to do with it? An Englishman in the nineteenth century who encountered a black swan for the first time might have considered it a "7-standard deviation event," even though one could have learned of the existence of black swans from ornithology books.⁴

A VOYEUR IN POLITICS

I spent many hot summers in the nation's capital, usually at the Federal Reserve Board, the IMF, or the Institute for International Economics. Those visits were highly rewarding, but strictly research-oriented. Then, in 1983, Martin Feldstein asked me to work for him at the Council of Economic Advisers (CEA). It was the Reagan administration, of which I was not especially fond. But one reason I happily took the job was the opportunity to work with Feldstein. There was extra prestige, at least in retrospect, from the fact that the position I was filling had been held during the preceding year by both Paul Krugman and Larry Summers. (I was single, worked very long hours in those days, and was happy to fill in for two.)

Surprising as this often is to outsiders, the CEA is a rather technocratic, nonpolitical outfit. Making one's best forecast of the trade deficit and the growth rate is the same in either a Republican or a Democratic administration. Trying to explain the virtues of free trade in an interagency meeting is the same in either case. Putting into a presidential speech an explanation as to why a skeptical Congress must approve a quota increase for the IMF is the same.

My best Zelig story dates from November 4, 1983. At that point I did not yet have clearance to enter the White House proper, as opposed to the Old Executive Office Building next door, which housed the CEA office. Nevertheless, through a chain of coincidences, I found myself in the Oval Office for half an hour with President Reagan and his top cabinet members.⁵ Chitchat focused on the casualties of the recent Grenada invasion and the bombing of the Marine barracks in Beirut. Nobody asked me who I was, because they assumed that, if I was there, they should already know who I was. Eventually, I figured out that I was at the wrong meeting and left.

During this period, Feldstein popularized the notion of the twin deficits: that the then-new large US trade deficit was the result of a large budget deficit. The analysis was an implicit rebuke to those who had foolishly predicted that the tax cuts enacted in 1981 would lead to smaller budget deficits and higher national saving, rather than the reverse. Others in the White House

and the Treasury rejected our forecast in the 1984 Economic Report of the President that the trade deficit would continue to rise, let alone our diagnosis as to why. It made front page headlines when Secretary Regan responded to a question in congressional testimony by confirming that, so far as he was concerned, the ERP could be thrown in the trash. I couldn't have been more pleased. (Our forecast proved right on target the next year.)⁶

THE BUREAU

With Feldstein, my array of mentors was pretty much complete. After his term on the CEA, Feldstein returned to Harvard and the presidency of the National Bureau of Economic Research, with which I became increasingly involved. Later, he decided to divide the NBER's International Studies Program into a trade half and an international finance and macroeconomics (IFM) half. Our forged-in-fire relationship was perhaps one reason he asked me to be the director of the IFM program. This position has helped me ever since to keep my fingers on the pulse of what is the hottest new research in the field and who are the young researchers doing it.

The position also made me a member of the NBER Business Cycle Dating Committee (BCDC), which officially declares the starting dates and ending dates of US recessions. I came on board at the beginning of what turned out to be the longest period of economic expansion in American history (1992–2000). So for nine years I could joke that I was on the best sort of scholarly committee: one that never had any reason to meet. But then came the recession of 2001. We dated the peak of the preceding expansion – that is, the beginning of the 2001 recession – as coming in March of that year, and the trough – the end of the recession – in November.

Part of the job of being on the BCDC is being good-natured when observers react to our announcement of a business cycle dating point by questioning the need for the Committee, housed at a nongovernment research organization, the NBER. Most of the teasing takes one of two (mutually inconsistent) lines of argument. One is that everybody knows that a recession is defined as two consecutive quarters of negative GDP growth; so who needs the more complicated and less easily quantified procedures of the BCDC? The other line of argument is that “everybody has known for a long time” that the country has been in a recession, so it is ridiculous for the Committee to announce it so much after the fact.⁷ One rebuttal to both of these criticisms is that the relevant economic statistics come out with lags, are subject to major revisions, and often give signals that conflict with each other. Official GDP fell in the first and third quarters of 2001 but *rose*

in the intervening second quarter. So if we had followed the simple two-consecutive-quarters rule of thumb, then we would not have found a recession at all. (We factored in other indicators, including job loss, to reach our judgment.)

At the time when we announced that the 2008 recession had begun with a peak toward the end of 2007, the government estimates still reported that the official GDP measure of output was actually higher in both the first and second quarters of 2008 than the last quarter of 2007. (We again based our call on other indicators, such as job loss and the national income measure of output.) Much later, the Commerce Department revised its statistics, as it always does. The current estimates reassuringly show that GDP was in fact lower in both of the first two quarters of 2008 than in the last quarter of 2007. Even though our announcement of the beginning of the recession was greeted as long overdue when we made it, we would have had to wait another year and a half to get that crucial revision from the Commerce Department. Dating the ends of recessions is even tougher.

Incidentally, some Americans vaguely think that the terrorist attacks of September 11 caused the 2001 recession or the disappearance of the budget surplus that President Bush had inherited in January of that year. Of course, both were in fact well under way before.⁸ But I don't blame Bush for the 2001 recession.⁹ It usually takes awhile before a new president's actions have an effect on current conditions, whether for good or ill.

UC BERKELEY

I joined the faculty of the Economics Department of the University of California at Berkeley – just a few miles from where I grew up – in 1979. I spent most of the 1980s and 1990s there. I loved walking to work, down the hill, along rose-lined paths and past redwood trees. I grew to enjoy teaching classes of 200 or 300 students.

When I first arrived, the Economics Department happened to be unusually short of faculty members who were close to me in either age or field. But eventually I was joined by Barry Eichengreen, Maury Obstfeld, Ken Rogoff, David Romer, and Christy Romer, who were close colleagues in both the personal and professional senses. And Andy Rose at the Business School. One of my few regrets in life arises from the circumstance that after they all came, and just as the Economics Department had been restored to its status as one of the top half dozen in the national rankings, I left to move east. Since the year I left, others in the Berkeley department have reaped an avalanche of Nobel Prizes and Clark Medals. My mixed feelings about

having left derive not from that, but from having left behind good friends and colleagues – and the landscape of my native state. I miss the redwood, live oak, and bay trees; the mountains; and the view of San Francisco Bay. No matter how lost you get in the Berkeley hills, you always know which way is west.

MEMBER OF THE COUNCIL

In 1996 Joe Stiglitz, who was chairman of President Clinton's Council of Economic Advisers, asked if I was interested in being a member of the Council. This is a political appointment – not a staff job like I had had at the CEA thirteen years earlier. Thus, it requires nomination by the president and confirmation by the Senate. The procedures for clearance and confirmation are among the many processes in Washington that are thoroughly broken. I don't think the public understands how many top positions in policy making are empty at any given moment, usually for the silliest of reasons. The Senate did not give me a hard time, in large part because we were in the midst of the strongest expansion in US history. I was sworn in by Vice President Al Gore a mere eight months after taking up residence.

There are three members of the Council. The chair is overall in charge. The other two members divide up responsibility for issue areas. I had international economics, macroeconomics, and a few areas of microeconomic policy.

The main role of the CEA is presenting to the president and to others in the government, through the "interagency process," what, in its view, the field of economics has to say about the policy issues that need to be decided at the time. A hundred policy issues arise every month. The Council does not have any built-in constituencies, in the way that the Agriculture Department has farmers, the Commerce Department business, and the Labor Department workers. Thus, its influence is only as big or as small as the president or others choose to value its advice. Where most agencies have many "line responsibilities" – things that won't get done if the agency doesn't do them – the CEA has only a few. On an annual basis, the CEA writes the Economic Report of the President. On a daily basis, it writes confidential evening memos to the president explaining the official economic statistics that are to be released early the following morning. President Clinton got our memos on an almost daily basis, so great was his thirst for facts and figures. I know that some other presidents have been much less interested in such details.

We also had something called the Weekly Economic Briefing of the President. As soon as I arrived, I was struck by how the WEB went into detail, such as explaining conflicting scholarly studies regarding the success of a school voucher program in Milwaukee. I was sure that this was more than the president needed to know. But I had not yet learned how different this president was from the one I had worked for in the 1980s. The next week, Clinton cited the conflicting evidence over the Milwaukee experiment in a campaign debate on national TV. After that, we kept the facts, figures, charts, and analysis flowing.

One “line responsibility” of the CEA is to lead the process, twice a year, of forecasting the rate of economic growth and the other key macroeconomic variables that feed into the making of the federal budget. The Treasury and the Office of Management and Budget are the other two agencies that participate in the “troika.” I was fortunate to be there during a period when the economy repeatedly surprised all observers with good news on all fronts. It was a little embarrassing that the economists in the administration kept underforecasting economic growth. And unemployment, which macroeconomists had long said probably could not go below 5 percent without pushing up inflation, did so in 1997 and eventually went even below 4 percent. Every time we sat down to prepare a new forecast, some of the participants wanted to rely on the historical statistical relationships, while others argued that there had been a fundamental shift in the parameters due, in particular, to information technology. At the time, the latter sort of thinking was called the “new economy.” Now it is called the “Internet boom” or even the “Internet bubble” – though it is important to realize that the economic performance was genuine and originally based on fundamentals,¹⁰ even if the stock market got carried away by dot-com-mania, as it clearly did.

My approach was “Bayesian”: every six months, if the growth rate had again remained above traditional estimates of “potential” and the unemployment rate had again remained below traditional estimates of the “non-accelerating inflation rate of unemployment,” with no signs of inflation, we would again adjust our estimates of those parameters just a little. But we would not throw in the towel and jump the estimates discretely. I told the staff that the year in which the government adjusted its estimates sharply in the optimistic direction would be the year that the stock market crashed and the economy entered recession. In the event, that is precisely what happened after we left. I am convinced that the grossly overoptimistic forecasts made by the government in January 2001, not just for the short term but for the long term as well, were a major reason President Bush was able to convince the public that the budget surpluses he had inherited not only

would continue in the future, but would be so large that they warranted huge long-term tax cuts.

Two issues took up more of my time while I was on the Council than did any other. One was the emerging market crises that hit East Asia in 1997 and Russia in 1998. The other was the Kyoto Protocol on Global Climate Change, which was negotiated in November 1997. The first involved issues that were familiar to me. The parallels to the international debt crisis that began in 1982 were greater than most observers realized. The second issue was unfamiliar to me and required a lot of hurried studying up, followed by a hundred interagency meetings.

It is one of the ironies of working in government that one can sometimes find far more room to influence policy in an area where one knows nothing than in areas where one is putatively a world expert. While I, like most economists, was leery of the high economic costs if greenhouse gas emissions were to be cut very suddenly, I eventually became committed to the Kyoto Protocol.¹¹ I thought that its design – particularly the provisions for international trade in emissions permits and for trading off among the six greenhouse gases – offered the best hope for addressing the environmental goal in an economically efficient way. The Protocol left a lot out, to be sure. The three biggest gaps that remain to be filled are full participation by all countries, a mechanism for setting emission targets well into the future, and some reason to expect countries to comply with their commitments. These are issues that I have done research on over the years subsequently.

HARVARD

I left the CEA in 1999. Rather than returning to Berkeley, I accepted a job offer from Harvard University's Kennedy School of Government. Ten years earlier I had been leery of moving from an economics department to a school of public policy. One obvious reason for the move in 1999 was that, by that stage in my career, I had developed some interest in participating in the public policy debate, which would be easier to do from Harvard Kennedy School than from the West Coast. Another reason is that I no longer thought I would be giving up much to get these benefits: I could continue to collaborate on research with Andy Rose via email and could have lunch with all the excellent economists in the vicinity just as easily at the Kennedy School as at Berkeley. To name only four of those who are located intellectually in international economics and physically in my building: Ricardo Hausmann, Robert Lawrence, Dani Rodrik, and Carmen Reinhart. But an advantage of the Kennedy School is that it is in fact easy to partake

from the elusive grail of interdisciplinary communication, for example at the faculty lunch seminar. Further, being a senior professor at Harvard is a charmed status.

At Berkeley, I had taught undergraduates and PhD students, as one would do in any economics department. I still have both kinds of students at Harvard, but most of my students at the Kennedy School are master's students, who are in between. I like teaching these classes. In an economics department, there is a wide artificial gap between teaching undergraduates and teaching PhD students. On the one hand, undergraduates like to hear about the real world, but there is a limit on how far you can go in terms of theory (though Harvard undergraduates, whom I teach in a course cross-listed in the Economics Department, are very smart). On the other hand, PhD students can do the math, but you are doing them a disservice if you talk about the real world and thereby give them the impression that if they do the same they will be able to write a thesis or get an academic job. The classes I now teach at Harvard Kennedy School are the best of both worlds, for me. I can mix theory and the real world.

I have always made sure that I lived within walking distance of my place of employment. I live in Cambridge and either walk or bicycle to work, often noting when I pass over the spot near Harvard Square where George Washington took command of the Continental Army in 1775. My son's elementary school is four blocks from our home. I enjoy walking him to school.

MORE RESEARCH

One of several big benefits of achieving tenure, and then full professorship, is that one can choose to work on whatever seems most interesting rather than whatever is most likely to demonstrate technical prowess and be published in the top journals. For me, this freedom included branching out in terms of subject matter, beyond the study of exchange rates and international financial markets. First I ventured into other parts of macroeconomics, including, for example, the coordination of monetary and fiscal policy when different policy makers believe in different models. Then I ventured into other parts of international economics, including, for example, the circumstances under which the "trade-creating" advantages of regional free-trade areas outweigh the "trade-diverting" disadvantages.

During the second half of my research career (so far), I have ventured further afield still, into questions such as why some countries are able

to achieve higher incomes than others and whether trade is bad for the environment.

Pontificating about big-think issues such as globalization has its role to play, if one wants to communicate with nonspecialists or even to influence the public debate. But, as any academic knows, one doesn't get articles published in refereed journals by writing judicious surveys of the literature or offering policy recommendations. One must, rather, contribute some incremental new methodological innovation, whether theoretical or econometric. (Preferably the outcome is to show why some other author is wrong, but one should at a minimum fill a supposed glaring gap in the literature.)

A single econometric idea underlies a fair number of my journal articles over the past decade, even though they appear in different subfields of economics. It has to do with geography. I have been fascinated by geography my whole life, since before I got interested in economics in college.¹² Although this must be a reason for my decision to specialize in international economics thirty years ago, at that time international economics had virtually nothing to do with geography. I am talking about all the standard theories of international economics that sought to predict, say, the trade patterns or growth rates of countries, and that dealt with the set of actual real countries when it came to empirical analysis, and yet featured no role for such fundamental geographic variables as distance, landlockedness, language, or historical relationships. Rather, countries were disembodied points that lacked any spatial coordinates and possessed only capital stocks, labor forces, productivity levels, money supplies, and a few other variables.

This has all changed over the past thirty years. Geography has entered international economics. I can't take any credit for the change. Krugman is the one who can.

Okay, so what was my idea? Perhaps the most ubiquitous and intractable obstacle plaguing all of empirical economics, especially macroeconomics, is the problem of causality. We observe that countries that engage in more international trade tend to benefit from higher incomes. But does trade cause growth or does growth cause trade? "Correlation need not imply causality."

In a 1999 article, David Romer and I used the gravity model of bilateral trade to try to solve the causality question. Newton's theory of gravity says that the attraction between two bodies is proportionate to the product of their sizes and inversely related to the physical distance between them. The gravity theory of trade says that trade between two countries (or provinces) is proportionate to their sizes and inversely related to the economic distance between them. Size can be measured by population.

Economic distance can be measured by geographic distance and other variables to capture transport costs, linguistic and political barriers, and so forth. The gravity model predicts bilateral trade quite well.¹³ We used the gravity model, first, to come up with an exogenous predictor of each country's overall level of trade and then to test whether economic growth, other things equal, had blessed those countries that were geographically well situated for trade versus those that were remote, landlocked, or otherwise encumbered. The answer was yes. We now felt better able to claim that, in the case of trade and growth, the relationship was indeed causal. A "point estimate" is that the difference between a hypothetical country with no trade (say, Burma) and one where exports plus imports totals 200 percent of GDP (say, Singapore) is by itself worth an 80 percent increase in income over twenty years.

I have used the geographic determinants of trade to address the causality problem in many other areas as well. "The Endogeneity of the Optimum Currency Area Criteria," with Andy Rose, demonstrated that higher trade between a pair of countries leads to more synchronized business cycles. Another paper with Andy showed that international trade is good for some measures of environmental quality, such as local air pollution, but not others, such as greenhouse gas emissions. A paper with Eduardo Cavallo established that countries that are more open to international trade were less likely to suffer severe financial crises.

BECOME AN ECONOMIST AND SEE THE WORLD

My career has afforded me the luxury of indulging my geographic interests in a more tangible way as well. International economics does not, like the field of development economics, oblige one to spend time in countries without reliable running water and electricity, at least not for more than a few weeks at a time. But I have been able to travel widely, always on somebody else's nickel. I have visited seventy countries so far. Some institution in the host country pays. Most trips are simply for conferences, but sometimes they are for teaching, sometimes research, sometimes consulting.

It all started at the midpoint of my graduate studies at MIT. In 1976 Dick Eckaus and our other professors packed five of us – Krugman, three other classmates, and me – off to Portugal for a summer. I remember thinking on the plane going over, "What do we know about advising a government?" The man we were to work for, José da Silva Lopes, governor of the central bank, apparently thought the same thing when we arrived in Lisbon and he

saw how young we were. Eventually we proved, both to ourselves and to our host country, that we had something to offer after all.

One story from that first experience at advising long ago stands me in good stead every year when I need to explain to my students the concept of “seignorage.” We were living in hotels. At the end of the first month, we had to pay the bill. But for bureaucratic reasons, the wire transfers we were expecting had not yet come through. We apologetically explained our problem to the governor. Responding “no problem,” he summoned an aide, who took us to the basement where the printing presses were turning out the national currency. They counted out enough escudos to tide each of us over. I don’t know if the Bank of Portugal ran the printing presses for an extra few seconds that day; if so, it was truly seignorage.

More of the important conferences take place in the United States and Western Europe than in the rest of the world. But the rest of the world is in some sense more interesting. The other places where I have become most involved (in the superficial way that we jet-setting international economists are accustomed to) include Japan, Korea, China, central Europe, Latin America, South Africa, and Mauritius. One benefit of having had what is by now a long line of students – first at Berkeley and now at Harvard – is that one finds them years later all over the world, often in important positions of responsibility. It can make the trips especially interesting.

FAMILY

Just as this manuscript was finalized, I got married to Kathy Moon, a smart and beautiful professor, who teaches political science at Wellesley College, not far away. By coincidence she, like I, was born in San Francisco. She is of Korean descent and an expert on Asian-American relations. Perhaps, as occurred to me half a century ago in California, the next big leap is indeed to Asia.

Notes

- 1 “Republican and Democratic Presidents Have Switched Economic Policies,” *Milken Institute Review* 5, no. 1 (2003): 18–25.
- 2 “Dornbusch’s Overshooting Model after Twenty-Five Years,” International Monetary Fund’s Second Annual Research Conference Mundell-Fleming Lecture, *IMF Staff Papers*, vol. 49 (2002).
- 3 At the same time, at least one critic had wild conspiracy theories along the lines that Sachs had once been seen at a meeting with Summers and Fischer and that this elite

- cabal must have plotted the deliberate downfall of Russia and Asia. Needless to say, the conspiracy theory at one extreme is even more misguided than the inference of personal animosity at the other extreme.
- 4 Black swans had been discovered in Australia in 1697.
 - 5 I followed Don Regan, then the secretary of the treasury, into the White House because I thought he was going to the same meeting I was. The Secret Service assumed that I was his aide, even though I was not trying to make it look that way. Intimidated, they neglected to ask for identification.
 - 6 I reviewed the history of what CEA chairs have done over the years when they find themselves at odds with the White House, in “What an Economic Adviser Can Do When He Disagrees with the President,” *Challenge* 46, no.3 (May/June 2003): 29–52.
 - 7 We did not announce the March 2001 peak until eight months later. We did not announce the December 2007 peak until twelve months later. We are even slower at announcing the ends of recessions. nber.nber.org/cycles/main.html.
 - 8 In 2004 there was some White House pressure to move the starting date of the recession from the first quarter of the Bush administration to the last quarter of the Clinton administration. The NBER Business Cycle Dating Committee decided not to make such a revision, based on an objective consideration of the data. (I have never heard any member of the Committee raise any political consideration, at that time, before, or since.) The episode is one illustration of the benefits of having institutions such as the NBER BCDC independent and thereby protected from political influence. The federal statistics-collecting agencies – in particular, the Bureau of Economic Analysis in the Commerce Department and the Bureau of Labor Statistics in the Labor Department – are also thoroughly insulated against political interference, contrary to casual and irresponsible inferences made by many commentators over the years.
 - 9 I *do* blame Bush for the severity of the 2007–2009 recession, incidentally, and did before it arrived: “Rather it’s the next recession that is going to be his fault. I don’t know when that will be. But when it comes, we are not going to have the ability to use fiscal policy, to cut taxes, the way they did in 2001 [because the inherited deficit will already be too high]”; “A Debate on the Deficit,” *Challenge* 47. no. 6 (November 2004): 22–23.
 - 10 The expansion of the 1990s was led by growth in private-sector demand and employment, whereas the expansions of the 1960s, 1970s, and 1980s had been led by fiscal expansion on the part of the federal government, as had the decade of the 2000s.
 - 11 At least I became committed to the Clinton-Gore version: although President Clinton signed the treaty, he said that he would not submit it to the Senate for ratification unless and until developing countries took on commitments that were qualitatively similar in nature to those agreed upon by industrialized countries. “You’re Getting Warmer: The Most Feasible Path for Addressing Global Climate Change Does Run Through Kyoto,” in J. Maxwell and R. Reuveny (eds.), *Trade and Environment: Theory and Policy in the Context of EU Enlargement and Transition Economies* (Cheltenham: Edward Elgar, 2005).
 - 12 It all started with a fascination with maps. My lifelong mode of doodling has been to draw maps freehand. I have never gotten around to patenting my special “Styrofoam cup” projection: I draw a map of the world around the sides of a coffee cup. (It is

- superior to the Mercator projection in that the greater land masses in the Northern Hemisphere are neatly accommodated by the tapered shape of the Styrofoam cup.)
- 13 In the mid-1990s I had already used it to estimate what were usual geographic patterns of trade in order to evaluate questions such as whether trade was unusually concentrated inside the East Asia region or in regional trade blocs generally (E.g., *Regional Trading Blocs* (Washington, D.C.: Institute for International Economics, 1997), coauthored with Shang-Jin Wei and Ernesto Stein).

PROOF