I had a standard answer to the question with which adults love to torture adolescents: “what do you want to be/do when you grow up?” “I don’t know,” I would reply, “but I do know two things I won’t do: marry an academic or become one myself.” So what happened?

Early Imprints

Like most accounts of “how I came to be what I am”, mine begins with my parents and the environment, or environments—for they went their separate ways before I reached my third birthday—in which they immersed me. John von Neumann and his bride, Mariette, were both privileged, protected children of Budapest’s Jewish but fully assimilated haute bourgeoisie, born at a time when that city was a flourishing second capital of the Austro-Hungarian Empire. This golden age came to an abrupt end with the shock of the First World War and its aftermath: the breakup of the Empire, the 133 days of “Red Terror” brought on by a Communist coup and the declaration of the Soviet Hungarian Republic, and the creeping anti-Semitism that characterized the regime of Admiral Horthy, who led a successful counter-coup and was installed as head of state. He held that position until 1944, by which time my parents had recreated their lives in the new world across the Atlantic.

My father, already a precocious star in the world of mathematics, accepted a Rockefeller Foundation teaching fellowship at Princeton in 1930, dividing his time between there and Berlin, for the most pragmatic of reasons. He had calculated that the likelihood of moving up the academic ladder was greater in the United States than in Germany, where he had begun his academic career and where full professorships were few and far between. My mother, spoiled, glamorous, and always ready for adventure, leapt at the opportunity to create a life with her handsome, brilliant new husband beyond the reach of her overprotective parents. By the time von Neumann became one of the founding members of the Institute for
Advanced Study in 1933, though, Hitler was in power and the Nazis had passed laws mandating the firing of all civil servants, including university professors, of non-Aryan background.

Now there was no turning back. Throughout the 30s, my father watched Europe “relapsing into the dark ages,” and became convinced that only an Allied victory, led by the United States, could save civilization from destruction by the spread of totalitarianism from the right: Nazism and Fascism. Once that battle had been won, he became equally convinced that the threat now came from totalitarianism on the left, in the form of Soviet Communism. Haunted by memories of the “Red Terror” that had forced his family into brief exile during his adolescence; he became a super-Cold War hawk, even advocating preventive war on the Soviet Union before they, too, acquired the ultimate weapon.

My childhood played out against the background of my parents’ all-out commitment to the Allied war effort—a commitment shared by the new partners each had acquired very quickly after their divorce. My father shuttled between Princeton and Los Alamos, where he played a major role in the Manhattan Project, in addition to working on the development of the stored-program electronic computer under the auspices of the Army’s Aberdeen Proving Ground in Maryland, with a secret side trip to England to apply the insights of the mini-max theorem to the problem of sweeping German mines in the English Channel. My mother, meanwhile, having accompanied her American husband to Cambridge, where he had been recruited to work at MIT’s Radiation Laboratory, soon abandoned her life of leisure in favor of joining the army of Rosie the Riveters, assembling radar sets. She soon rose to foreman and, within the same year, joined her husband at Rad Lab, where she became the supervisor in charge of training women technicians.

My parents not only devoted their superhuman energies to the Allied war effort, they also tackled head-on some of the eruptions of man’s inhumanity to man that characterized the times. My father pulled every string he could find, both in Princeton and in Washington, to help other Jewish intellectuals escape from Europe and to find them jobs here—the promise of a job being a prerequisite to acquiring the means of salvation, a U.S. visa. My mother, told by her superiors to put to a vote of her supervisees the question of whether they would be willing to
have “negroes,” as co-workers, marshaled the force of her personality, combined with a judicious use of Hungarian profanity, to make sure that the winning vote was “yes.”

After the war, cosmopolitanism suffused family life in the households of both my parents. Gathered around my father’s dinner table were not only such eminent mathematicians and physicists as Edward Teller and Eugene Wigner, but also Europeans from the world of arts and letters like Arthur Koestler and Emery Reves, friend and publisher of Winston Churchill. Oskar Morgenstern, a non-Jewish refugee on principle from Hitler’s Austria and my father’s *Theory of Games* co-author, courted his bride in our living room. During those same early post-war years, when my mother had become the first employee of Brookhaven National Laboratory and its ambassador and hostess-in-chief, her Long Island home became the gathering place for visiting scientists from every corner of the earth.

**Choices and More Choices**

So what has this background to do with the career path I ultimately took? At the most basic level, it enabled me to escape the “1950s trap” that ensnared so many of my female contemporaries, indoctrinating them from childhood in the belief that their only choice was between that of full-time housewife or childless—and most likely unmarried--professional woman. On the contrary, my father impressed on me, virtually from my earliest conscious moment, the moral imperative of making full use of whatever intellectual capacities we were endowed with, whether man or woman, paid or unpaid. My mother, on the other hand, taught by example, combining brainpower and feminine charm to create a lifelong career that she made up as she went along.

My father’s focus on intellectual achievement made him a strong opponent of my early marriage, fearing that it would condemn me to the housewife trap, an opposition that gained moral authority from the fact that he was dying at the time. Much of the competitive drive that has infused every stage of my career was born of the desire to prove him wrong, to show his reproachful ghost that I had managed to combine the marriage and family I craved with the professional achievements he had feared I was denying myself. At the same time, I was
determined to escape from under the shadow of a world-famous genius parent, to establish an independent identity.

A year of working as a glorified clerk at the Educational Testing Service in Princeton persuaded me that my summa from Radcliffe in political theory was an inadequate base for a meaningful career, and that further training in a more “practical” area was essential. But, given my determination to escape my father’s shadow, what on earth made me choose economics, a field in which he had made more significant contributions than I could ever hope to? The answer is twofold. First, I didn’t actually intend to become an academic economist. My initial goal was to get two master’s degrees, one in economics and one in journalism, with a vision of writing articles for the business pages of the New York Times, the Washington Post, or the Economist. It was pretty much a toss-up that I chose to pursue the degree in economics first.

It wasn’t until late in my first graduate year at Columbia that Gary Becker, in whose microeconomics course I had done surprisingly well, asked me if I had considered pursuing a PhD in Economics. If so, he said, he was in a position to offer me an excellent award, the Earhart Fellowship. The only requirement imposed by its conservative donor was that I evince support for a free-market system. By that time, I had begun to rethink my dual-degree career plan, partly because it might be more interesting to participate in the shaping of economic ideas and policies than simply to write about them, and also because the peripatetic life of a journalist didn’t seem to combine well with family life. And a predilection for a non-statist form of economic organization came naturally to me, grounded in my parents’ conservative views and nurtured by my own study of political philosophy. So, with no more reflection than a “sure; why not?” the die was cast.

The other part of the answer lies in my incredible ignorance and naiveté at that stage of my life. Having somehow talked my way into Columbia’s graduate program with nothing more than Harvard’s Ec 1 under my belt, I had simply never heard of the Turnpike Theorem and knew nothing about the role of game theory in economics. I knew about my father’s contributions to physics and to what has become computer science, but the fact that in choosing the economics route I was treading on dangerous ground never occurred to me.
Although my parents’ impact on my career was primarily positive, it had its negative aspects. I hold the fact that I am von Neumann’s daughter directly responsible for a degree of mathematical illiteracy that no first-year graduate student could get away with today. My experience in the first term of calculus (Calculus 1a) at Harvard, was uneventful. But, just after the term had ended, I had a hallway encounter with Garrett Birkhoff, chairman of the Harvard math department and a renowned mathematician who had co-authored articles with my father. Thinking that he was making small talk, he commented, “Well, Marina, I’m glad to see that you’ve upheld the family honor by getting an A in Calculus.” “Good God,” I thought to myself, “what would happen to the family honor if I were ever to get an A-?”

The result was that I never again took a math course for credit, though I did work my way through R.G.D. Allen’s *Mathematical Analysis for Economists* while giving my firstborn his midnight bottle, and audited a course in matrix algebra at Carnegie Mellon when I was teaching at Pitt. To my delight, I discovered recently that Marty Feldstein’s mathematics training at Harvard was identical to mine. Peter Kenen was way ahead of us; he actually finished Calc 1b as well. So now the secret is out, and today’s graduate students can roll their eyes in astonishment.

My training in the other major tool set economists are expected to have under their belts, statistics and econometrics, was every bit as inadequate. The single graduate course in statistics available in Columbia’s Economics department while I was there was taught by F.C. Mills, a great man in his day, but then in his final year of teaching before heading into overdue retirement. The very next year, Jacob Mincer arrived and completely revamped the statistics course, bringing it up to the cutting edge, but by then I had passed my comprehensive exam and moved on. I eventually audited an econometrics course taught by one of my colleagues at Pitt, but the gap was never adequately filled.

My decision to concentrate in international economics, again, arose from considerations both intellectual and practical. As the child of émigré parents with cosmopolitan perspectives and networks, who spent my childhood years focusing their supercharged energies on ensuring victory for the forces of light in a global conflict, I had always been
fascinated by relationships among nations and the factors that rendered them either peaceful or hostile. Although I had long since abandoned the naïve vision of world government that had led me to join the World Federalists during my high school days, I had already developed an instinctive preference for open economies and freedom of transactions across international boundaries.

My choice of specialty was bolstered by the fact that all the teachers of international economics I encountered at Columbia—Ragnar Nurkse, my original dissertation supervisor; Albert Hirschman, who took over after Nurkse’s sudden and untimely death; and the newly-minted Peter Kenen, who became a valued informal adviser while I was in the writing stage—took me seriously as a potential teacher and scholar in the field. This was in contrast to several distinguished faculty members in other areas of economics, who made clear to me that they felt it was a waste of time to invest in a female student who would almost certainly abandon a serious career commitment in favor of husband and children. The dissertation Hirschman and Kenen mentored looked at the effectiveness of public funds—from both the United States and international institutions—in stimulating private investment in developing countries. In it, I combined my focus on international issues with my emerging interest in examining interactions between the public and private sectors. With Kenen’s advice guiding my revisions, I turned this thesis into a book published by the Princeton University Press.

Teaching, Research, and Practice Inside the Beltway

When my husband accepted an appointment in the English Department at the University of Pittsburgh, whose ambitious Chancellor had gotten financial backing from the Mellon family to raise that streetcar college to the level of a nationally recognized research university, I went along as the trailing spouse. My dissertation completed, I was looking around for something to do when I was lucky enough to be taken on as a clean-up researcher by two leading regional economists, Ben Chinitz and Edgar Hoover, who were trying to complete a multi-volume study of the Pittsburgh Region. I assembled the final volume from the bits and pieces of research left behind by the study’s participants when the money ran out and they
returned to their home universities, prompting me to quip that I had written one more book in the field of regional economics than I had read.\textsuperscript{iv}

When Hoover and Chinitz returned full-time to their positions in Pitt’s Economics department they took me along with them as an instructor, blithely ignoring the University’s strict anti-nepotism rule; by the time the HR department discovered this violation, I was too entrenched to be uprooted. The following year, when the senior professor in international economics dropped dead of a heart attack in the departmental office on the first day of the fall term, I found myself responsible for teaching the foundation courses in the field.

Having tested my own understanding of the basics by mentally correcting the proofing errors in the first edition of Charles Kindleberger’s \textit{International Economics} textbook during my first year as a graduate student, I turned to the next edition as the underpinning for my own undergraduate course.\textsuperscript{v} This approach, standard at the time, focused primarily on the current account in the balance of payments, with various types of capital movements treated almost as an afterthought, rather being integrated into a full equilibrium model. At the graduate level, I teamed up with my colleague Jerry Wells to teach a course on International Trade and Economic Development. Jerry, a development economist who had written his dissertation at Michigan under Wolfgang Stolper, of Stolper- Samuelson fame, had done his field research studying agriculture in Nigeria. Our course stressed the positive contributions of trade to economic development and the resulting advantages of policies that increased a developing country’s openness to such transactions. At a time when the “dependencia” theory of Raul Prebisch and others, urging developing countries to develop self-sufficiency in manufactured goods and reduce dependence on the outside world, held sway, this was a distinctly contrarian view for a development economist, but one that Jerry embraced courageously and enthusiastically.

Two things happened in the early 1970s to change my approach to understanding, teaching, and writing about international economics. Most crucially, I was plunged into the real world of economic policymaking three times in quick succession: first as a senior staff economist at the Council of Economic Advisers, covering international economic issues; then as
a member of the ill-fated Price Commission that grew out of President Nixon’s New Economic Policy announced on August 15, 1971, a date best remembered for the closing of the “gold window” that marked the end of the Bretton Woods system; and finally as one of the three members of the CEA, where my remit included both the international economy and the wage-price controls program.

The most intellectually stimulating project I was involved in during this last assignment was the effort, led by Paul Volcker, to design an international monetary system to replace the now-defunct Bretton Woods arrangements. Our suggestions foundered on the clash of divergent national interests, and the world defaulted to a disorderly non-system of managed floats. But the problems our proposal was designed to relieve—by instituting symmetrical pressures for adjustment on surplus as well as deficit countries and reducing the instabilities inherent in the use of a national currency, the dollar, as the primary source of international reserves—continue to plague international currency relationships today.

These Washington assignments gave me a crash course in the intricate web of feedbacks between policies aimed at dealing with problems of the domestic economy and those directed at supporting our goals in the international trade and payments arena. I also learned more than I could have imagined about the political constraints on economic policy-making, about the law of unintended consequences, and about the need to balance not only the interests of the United States against those of other nations but also of the myriad departments and agencies of our own government, each fiercely defending its own turf and parochial interests.

How to organize this hard-won wisdom coherently enough to communicate at least some of its essence to students posed a real conundrum for me as I returned to academic life. By a stroke of luck, I found a useful organizing framework in Bob Stern’s just-published (in 1973) book, *The Balance of Payments.* To me, at least, his approach was revolutionary, and far more realistic than the one I had absorbed in graduate school and had incorporated into my own teaching ever since. Rather than analyzing international economic interactions using what was essentially a closed economy model with the current account tacked on, adding the term (X-M)
to the definition of national income, Stern outlined a full-fledged model of an open economy, incorporating as endogenous shifts in both the current and capital accounts. Rather than defining balance-of-payments equilibrium in terms of a zero balance on current account, as I had absorbed from Kindleberger, he defined it as zero net flows of “accommodating transactions” or balancing items, that is, short-term flows of official capital and movements of international reserves. Finally, he incorporated into what was effectively a graduate-level textbook Mundell’s two-instrument solution to achieving both internal and external balance under different exchange-rate systems. Eureka!

My style of research was similarly formed by a combination of my natural way of thinking and the glaring gaps in my training in the standard tools of economic research. During my first few years of teaching, before my forays into government, I paid my dues to quantitative empirical analysis. But in the first such effort, an article in the *Journal of Money, Credit, and Banking* titled "Economic Openness and International Financial Flows," vii Peter Kenen kindly ran the rank correlations my analysis required because I was making such heavy weather of the task. In the early 1970s a junior colleague, Norman Miller, and I published a series of articles that attempted to separate outflows of portfolio capital from the United States into their stock and flow components. Again, it was someone else (in this case, Miller), who bore the brunt of the statistical work. The statistical techniques we used never went beyond simple OLS regressions; our work was innocent of any attention to issues of robustness, colinearity, endogeneity, or any of the other problems that empirical work today must wrestle with. Yet our articles were accepted by the *Quarterly Journal of Economics* viii and the *Journal of Finance* ix without even a requirement to “revise and resubmit.” How times have changed!

I have, of course, used mathematical models to illustrate and develop ideas in many of my papers. But these were always simple systems of linear equations analyzing equilibrium states, and they did not require anything more than algebra and the techniques of simple differentiation I had learned in Calculus 1a. When dynamics were involved, I invariably reverted to verbal and/or graphical descriptions of the analysis. For me, mathematical modeling has
always remained a “second language”, into which I have had to painstakingly translate my economic insights.

I have made my primary contributions to the economic literature in the form of synthesis: absorbing bits and pieces from the theoretical and empirical work of others and pulling them together to discern and describe patterns that add up to a coherent whole. Sometimes the result has been to show how different models relate to one another; at others, it has been to develop an integrated explanation of apparently unrelated developments in the real world. One early synthesis was of various contributions to the targets-and-instruments literature on the assignment of macroeconomic policies in Policies for Internal and External Balance (1970)x. In “Global Monetarism and the Monetary Approach to the Balance of Payments” (1975)xii, I provided a formal reconciliation of the then-conventional Keynesian approach to balance-of-payments analysis with the extreme assumptions of “global monetarism,” laying bare their radically different implications for policy. Many years later, after I had circled back to the academic world at the University of Michigan, my book New World, New Rules (1999)xii offered an underlying explanation for the transformation of the successful large American corporation from the secure, paternalistic and globally-dominant organization of the 1950s and 60s into the lean, mean global competitor it had become by the century’s end. If I deserve designation as an “eminent economist,” it is works like these that provide the core of my intellectual contribution to the field.

Lessons Learned

My reputation rests partly, of course, on the fact that I have plied the economist’s trade in a variety of venues, government and business as well as academe. I have always regarded the discipline of economics as a sort of mental filing cabinet that enables its practitioners to break a complex question into its component parts, discard inessential details, and then reassemble the pieces in an orderly way to arrive at an answer or solution. I first tested the usefulness of this filing cabinet, as well as my own powers of communication and persuasion, when I ventured out of the classroom and the professional journals into the world of economic
policymaking inside the Beltway. This was a path that numerous economists had trod before me, with varying degrees of success and satisfaction.

I took away from my own government experience a hard-won realism about real-world economic policies and those who formulate them, lessons that can be summarized in three observations. The first is that a technocrat can’t expect to bring about dramatic changes in policy; if one moves the discussion a degree or two in one direction or another, that’s a substantial accomplishment. Second, any success is bound to be covered with so many fingerprints as to make finding the partial derivative of one individual contribution impossible.

Finally, I discovered that many of my achievements were negative; more than once I went home at night satisfied that I had prevented a move in the wrong direction. It was in this spirit of minimizing harm that I accepted membership on President Nixon’s Price Commission, despite the fact that every economist’s nerve in my body screamed that price controls were a terrible idea. Someone was going to fill the role, I told myself, and maybe I could help hold down the economic distortions such a program was bound to create.

Despite recognizing these inherent limitations on my effectiveness, I enjoyed every minute of my time as a member of the CEA, and it was with real regret that I resigned once the mounting evidence no longer allowed me relegate to the back of my mind the growing evidence that the President himself was implicated in the Watergate scandal. But the satisfactions of the job did not come without costs. The contradiction inherent in the very existence of the CEA was spelled out by Carl Christ in his highly critical review of the 1973 Economic Report of the President: “…the report is inherently a somewhat schizoid document. It is intended to serve two purposes that are not entirely compatible—first to function as an apology for or celebration of the President’s economic program, and second, to constitute a professional job of economic analysis and policy recommendation.”

Every CEA, both before and since the one I served on, has been confronted by this dilemma, and none has fully resolved it.

The ambiguity of the CEA’s role is but one example of a larger point. An economist, or any expert in a particular field, can be an adviser or shaper of policy inside an organization, be it
an agency of government or a private company, or an outside critic, as academics generally are. Both are honorable roles, and I have played both at different times in my career. But one cannot fulfill both functions at the same time. You can argue as hard as you can for your position inside an organization. But once a decision has been taken, there is no alternative except either to support the organization’s view on an issue or, if the disagreement is fundamental enough, resign. Academic freedom exists only for academics; under any other circumstances, public criticism of or disagreement with the official position is bound to destroy one’s credibility and effectiveness inside an organization. More than one distinguished economist has come a cropper by failing to recognize this home truth.

**Life in the Corporate World**

I was enjoying a sabbatical blissfully free of responsibilities at the Center for Advanced Study in the Behavioral Sciences on the Stanford campus—the fellows seemed to devote a lot of energy to volleyball and spouse-swapping, both of which activities Bob and I watched from the sidelines—when Paul McCracken, my old mentor from the CEA, called to say that a man named Roger Smith from General Motors wanted to talk to me. I couldn’t imagine why Mr. Smith wanted this meeting, but I said fine; I’ll invite him up for one of the Center’s excellent lunches. After some small talk about his daughter’s experience at the Stanford Business School, Smith—who, unbeknownst to me, had been anointed as GM’s next CEO—suddenly asked if I would be interested in joining the company as a vice president and its chief economist. I really thought that this man with the fair hair and mottled complexion had been addled by too much California sunshine. But it turned out that he was serious.

When I recovered from my open-mouthed astonishment, I told Mr. Smith I’d think about it. For someone who had always regarded cars simply as a means of getting from here to there quickly and without getting wet, and who couldn’t tell a Chevrolet from a Ford without a scorecard, his proposition looked outlandish. But to an international macroeconomist, the idea of working in the nation’s biggest company—large enough to have a noticeable impact on our country’s GDP—and with operations on several continents, was enticing. And finding out if my mental filing cabinet could be useful and persuasive in a world where no one shared the
assumptions or the vocabulary of my profession seemed like an exciting new challenge. Finally, the offer would give me an opportunity to test in the private sector a conviction I had already carried into government, that the fundamental criterion for evaluating an economic theory should be the insights it yields regarding the economic environment in which people live and make decisions. After several months of sorting out the implications of such a move for our family life, I took a leap of faith and said yes.

Almost as soon as I had arrived at GM, I decided to focus on changing the role of the Chief Economist from serving as a personal assistant to the Chairman and the Financial Staff to producing output that could be useful to the operating units, the profit-making side of the house. My focus and that of my staff would be, in broadest terms, to explain the economic and competitive realities of the world to GM, and those of GM to the outside world. This pithy mission statement turned out to be far easier said than done, stymied as it was by GM’s deeply embedded and profoundly dysfunctional culture.

This culture, supported by the protective bubble in which the company’s senior executives lived their daily lives, provided an effective bulwark against reality, enabling them to cling stubbornly to their belief in a stable, reasonably predictable world. The incursion of new Japanese competitors who happened to be in the right place with the right kind of cars when the oil shocks hit was seen as a temporary or at least reversible aberration. For most of them, furthermore, the only vehicle market that mattered was the one in the United States, which then accounted for some 70 percent of GM’s production and sales. The idea that the markets and the competition relevant to the company’s fortunes were rapidly becoming global was foreign to their thinking.

The picture of the world I tried to persuade GM’s management and directors to accept was very different. Consistent with the change in my own views that had occurred as I read and absorbed Bob Stern’s *Balance of Payments*, I shifted the forecasts and analyses of the economics staff from treating the United States as a closed economy to regarding it as an open one, that is, as interdependent with the rest of the world, rather than as an economically self-
sufficient entity onto which international trade and investment were tacked almost as an afterthought.

This shift in perspective made an important difference in how we anticipated the impact of developments in the national economy on GM’s business. When exchange rates were fixed, foreign competition non-existent, and cross-border flows of capital relatively insignificant, as they were until the 1970’s, an increase in interest rates caused by tightened monetary policy had affected GM mainly through a drop in total vehicle sales, brought about by a decline in economic activity and the reduced availability of financing to both dealers and customers. In the more financially-integrated world of the 1980’s, in contrast, and with exchange-rates now flexible, the capital inflow created by Paul Volcker’s drastic tightening of monetary policy forced a rise in the value of the dollar relative to other major currencies. The result was to make imports cheaper and exports more expensive, putting the company’s products at a competitive disadvantage relative to imported vehicles and contributing to the ongoing decline in GM’s market share. Both the business risks and the appropriate responses were different in an economy that was now far more open to the outside world.

This integrated world-view didn’t directly challenge GM management’s complacency until I spelled out its implications for the company in more detail in a couple of op-ed pieces in the New York Times. “The trend toward more fuel-efficient cars is worldwide,” I wrote, and “This growing product overlap will create increased opportunities to achieve specialization and economies of scale wherever components are manufactured, and thus the development of ‘world cars’...The bottom line...will almost certainly be a stepped-up pace of innovation and competition in an increasingly global—rather than national—automobile industry.” I stressed too, some of the worldwide developments accelerated by the two oil shocks of the 1970s, including an uncomfortable transition from cheap to expensive energy and the diffusion of economic power toward the newly-industrializing countries (NICs).\textsuperscript{xiv}

With these forecasts as background, the Economics Staff I headed produced competitive analysis research demonstrating that the cost-disadvantage suffered by American car companies vis a vis their Japanese competitors was not, as their top managements insisted, due
primarily to factors outside their control, like an undervalued yen or differences in national systems of taxation, but mainly to the more efficient and effective design and production processes developed by the Japanese manufacturers.

With these pronouncements, my staff and I were striking at the heart of GM management’s deeply-held beliefs. The result was that my entire career at GM was marked by growing frustration as my economist colleagues and I were unable to persuade our top decision-makers that competition from foreign producers was here to stay, would only intensify, and that protection against imports offered no long-term solution. As we, along with a few other forward-looking managers, repeatedly tried to bring the fast-changing competitive dynamic to bear on senior management’s thinking, I began to feel like the Trojan princess Cassandra, whose dire warnings about the true nature of the Trojan horse were fated to be ignored by her countrymen, with fatal results.

If describing the outside world and its potential impact on the company to GM’s top management was frequently an exercise in frustration, explaining to the outside world GMs situation, its needs, and the difficulties it faced posed its own challenges. I gave these explanations in press interviews, in op-eds and, above all, in testimony before various Congressional committees and subcommittees. These encounters not only tried my patience and toughened my hide, but forced me to hone my skills as a communicator and persuader, unable to take refuge in my academic perspective and vocabulary when facing a row of politically-motivated and often hostile interlocutors.

Sometimes, my two personas as a professional economist and a defender of GM’s bottom line merged seamlessly. One didn’t have to work for an automobile company to believe firmly, and insist unyieldingly, that corporate average fuel economy (CAFÉ) standards are among the most costly and least effective policies to curb fuel consumption; that increasing gasoline costs by raising the gas tax would be both more efficient, in terms of cost, and more effective in attaining the desired goal.

At other times, though, I had to make compromises. I was successful in persuading GM’s Chairman to avoid succumbing entirely to the protectionist stance adopted by the other
two members of the “Big Three” and the United Auto Workers (although the company’s President and the managers of the business units that reported to him never forgave me). But I did squirm a bit as I laid out a rationale for the so-called Voluntary Restraint Agreement that for several years restricted the number of automobiles that could be imported from Japan. This discomfort arose not only from my economist’s conscience, but also from a conviction that the cartel-like conditions created by the restraints would ultimately redound to the benefit of the very competitors we were trying to neutralize—as they did.

I did a reasonably good job, I thought, of integrating my two personas in the Graham Memorial Lecture I gave at Princeton after I had been at GM for about a year. xv But some qualms remained. As my father once put it, when one leaves the world of academic abstraction (mathematics in his case, economics in mine) to join the battles in the trenches of real-world policymaking, one inevitably loses one’s purity and falls into sin. So be it.

Coming Full Circle

Ultimately, it was not a desire to retrieve my virginity, but rather my frustration at the inability to make a perceptible dent in GM’s culture, along with a growing conviction that my job as vice-president and group executive was redundant in a company beset with too many layers of management, that led to my resignation/retirement. I regarded my return to the academic world at the University of Michigan as a pleasant way-station, while I decided what I wanted to do with the rest of my working life. Some two decades later, I have to admit that the way station has become a permanent home; I have completed the circle of (professional) life, ending up where I started half a century ago.

There are some significant differences, though. During the years I spent away from academe, teaching became an interactive sport, and students metamorphosed from a captive audience into customers to be satisfied. The lecture method has diversified into a variety of techniques for holding and capturing students’ attention and even encouraging them to push back until the underpinnings of an assertion have been laid bare. My own choices have changed as well. During the years I spent working my way up the academic ladder at the University of Pittsburgh, I taught standard courses in a department of economics. Today, I have
neither the competence—there is no way that I could pass PhD comprehensive exams in any first-rate graduate program, now that economics has become a field of applied mathematics or statistics—nor the interest in teaching those courses. Rather, I have appointments in two professional schools—the Business School and the School of Public Policy—where my teaching has an interdisciplinary flavor, and where my experience in the worlds of government and business can be incorporated to good advantage.

My research interests have also evolved. I’ve always been attracted to studying the interactions between the public and the private sectors; today the questions I examine fall squarely into the realm of political economy. In my book *New World, New Rules*, I not only set forth an integrated explanation of the changing characteristics of American multinationals over the second half of the twentieth century, but also explored the implications of these changes for government policies. With the Cold War over and capitalism triumphant, I became interested in looking at different styles of capitalism, as practiced in the Anglo-Saxon countries, continental Europe, and Japan, respectively, and the extent to which they were or were not converging toward the American, or Anglo Saxon, laissez faire model. Most recently, I have been studying the emergence of the concept of global Corporate Social Responsibility and how it has expanded, not only geographically, from a company’s home community to all the countries in which it operates, but also to include issues of environmental sustainability and labor and human rights, as well as corporate governance. Furthermore, the definition of a company’s responsibilities in these arenas has extended beyond the legal boundaries of the firm to encompass such “outside” actors as suppliers and host governments.

Finally, my political convictions have been refined by age and experience, although the beliefs underlying them remain the same. My politics may have moved a bit to the left as income inequality has sharpened, but mainly it’s a case of “I didn’t leave the Republican party, it left me.” The innovations in social policy introduced during the Presidency of Richard Nixon, on whose Council of Economic Advisers I served, would never be countenanced by the Republican party of today; quoting Bob Dole, “they were far too progressive”. I continue to believe in the importance of the flow of goods, services, capital, and ideas across national
boundaries—it was not for nothing that president of the UAW once referred to me as “that free-trade bitch at GM”-- but I have taken account, in both my teaching and my writing, of the darker aspects of globalization that accompany its many advantages.

Finally, I continue to believe firmly that, to paraphrase Winston Churchill’s description of democracy, free-enterprise capitalism is the worst form of economic organization except all the others that have been tried, and that market-based incentives generally offer a less costly and more effective means to achieve social goals than command-and-control directives. At the same time, such a system must be firmly embedded in a framework of laws and regulations that is widely perceived as both effective and equitable. Harry Truman will have to keep searching for his one-handed economist!


