

LETTER TO A YOUNGER GENERATION

ARNOLD C. HARBERGER*

University of California, Los Angeles (UCLA)

I. Introduction

It is an honor for me to be asked to present the closing address at this conference, which celebrates 20 years of achievement by those involved in CEMA's research and teaching activities. Starting from a *tabula rasa*, CEMA has become a leading contributor in both these dimensions, and has solidified its position in the vanguard of the great campaign to bring good economics to Latin America

Many of you are too young to remember, but it was not too long ago that the policies pursued by many governments in Latin America, and the courses taught in most universities across the region, reflected more bad economics than good. I and others of my generation were very fortunate in being able to witness major transformations, both in the conduct of economic policy and in the understanding and teaching of the discipline of economics. A goodly number of those present here today, and an even greater number of their friends and colleagues from all over the region, played important personal roles in both of these great transformations.

Even as a graduate student, I already carried the strong conviction that we economists, the profession as a whole, had a great mission — to help harness the concepts and ideas of our discipline so as to improve the economic lives of our fellow citizens in our countries and our fellow men across the

* Paper prepared for the Conference on Applied Economics, celebrating the 20th Anniversary of CEMA.

world. The experiences just referred to only served to strengthen and solidify this conviction — they turned the acolyte of graduate school into a seriously dedicated missionary who has spent most of his professional efforts in the search for better economic policies and in trying to better prepare the profession itself for carrying on this search. How did the real-world experiences manage to produce this effect? Simply by convincing me, totally and beyond any possibility of doubt, that bad policies really *can* bring a nation's economy to ruin, where it is likely to stay until good economic policies enter as a vital part of any successful rescue plan.

But to say that bad economic policies are capable of bringing on disaster does not mean that all disasters stem from bad policies. Similarly, not all economic booms and bonanzas are the result of simply instituting good policies. Quite the contrary, forces and shocks that are largely beyond the control of policymakers sometimes far outweigh the force that they can exert. An apt analogy is piloting a small boat on an ocean that is sometimes placid, other times difficult, but quite navigable, and still other times tempestuous and stormy, beset by hurricane winds and monstrous waves. The tasks of piloting are simple in the first case, somewhat demanding in the second, and awesomely daunting in the third. Good pilots are welcome in all cases, but their expert contribution to a safe journey grows exponentially as nature's fury mounts. Another analogy that I have used in the past compares the economic system to the human body. Those who keep themselves in good shape may not escape an epidemic, but they are likely to suffer less if they are struck with it, and to rebound more quickly when it is over.

Economists have to deal not only with external forces but also with the complex interactions of a country's political processes. Economic technocrats may be able to structure a tax reform here, a new social security law there, or a modified exchange rate regime somewhere else, but they really never have the luxury of a clean slate on which to set up, in full flower as it were, their complete preferred economic policy framework. In short, even the best economic technocrats, even when they have the most power and authority,

are unable to dictate the entire structure of economic policy. They can influence but not determine the pace and shape of reform.

The analogy is very apt between the whole structure of a nation's economic policy at any point in time and a patchwork quilt that has been built up, patch by patch, over a great many years. Each new chief executive, and each new finance minister inherits the patchwork quilt put together by their predecessors. Typically it has many flaws, even internal inconsistencies. Like it or not, each new administration will end up adding its own new patches to this quilt. The question is, do they make it better or worse? Do they add new flaws and inconsistencies, or do they get rid of old ones? Do they add to or subtract from the strength, utility, and aesthetic integrity of the quilt? These are the questions that economic historians will ask as they try to assess the contributions of different presidents, ministers and other protagonists.

Yet even these leaders do not determine their own fate. Moments of crisis open up opportunities, both for good and for ill, because people are more willing to make sacrifices and take risks in the hope of surmounting the crisis. It is thus no accident that the waves of modernization and liberalization in Argentina, Bolivia, Brazil, Chile, Mexico, Peru and many other countries were concentrated in periods of crisis. Later, when the crisis was past, additional steps in the reform process came slower and with more difficulty, in the face of popular indifference, bureaucratic inertia and political resistance and maneuvering.

So what about the lower-level technocrats, the economists who form part of the standing bureaucracy that performs the day-to-day tasks of governments? These people rarely have the chance to perform glorious feats. There is a continuing struggle to keep the good parts of the economic policy framework from being eroded, to prevent the creeping growth of the bad parts, and to bring good economics to bear on any new decisions they have a chance to influence. A good example of their potential contribution is the field of economic project evaluation. A good system for evaluating public sector projects and programs requires expertise that goes down to the level where each individual project is evaluated. This requires hundreds, even

thousands of well-prepared people. It also requires a framework of evaluation that is based on sound economic principles, and a top leadership that is willing to stand up, at least some of the time, against the political pressures that build up, even behind bad projects (based on the interests of the particular groups that benefit from the project while others bear much, or most of its cost). Our experience has shown that even rather rudimentary systems of economic project evaluation, given half a chance, can save billions of dollars for the citizens and taxpayers of a country, mainly by helping to stop huge expenditures on enormously wasteful projects. Other “working economists” in the public sector can contribute in similar ways, serving as antibodies to combat anti-economic ideas and policies, and/or as carriers and supporters of sound economic concepts, contributing step by step to the progress of economic policy and its implementation.

In light of all this, what tasks do I see for CEMA and its sister institutions as they forge the future of economic research and teaching throughout Latin America? Among the many that could be listed, I have selected four:

- 1) Never fail to emphasize fundamentals in both teaching and research.
- 2) Try seriously to distill, out of the huge corpus of knowledge inherited from the past, those parts that are most relevant for understanding the world we see around us.
- 3) Be skeptical and wary when exploring the frontiers of economic theory and research.
- 4) Participate actively and eagerly in the struggle to bring diagnostics to center stage in both teaching and research.

These will be dealt with in turn in the sections that follow.

II. Emphasizing Fundamentals

I had the good fortune to study economics at a time (1946-49) when and in a place (the University of Chicago) where the fundamentals of the discipline held center stage. And even more good luck brought me back to the University of Chicago for a long period (1953-91) during most of which

that university was very likely the leading center of applied and policy economics. These experiences had a defining influence on my professional life and thinking. They helped create the vision and, if you like, the biases that I will be expressing here.

Perhaps a good starting point is Milton Friedman's course in price theory, whose content is enshrined for all to see in his book of the same name. The question that many will ask is why an economist as profound, as versatile and as brilliant as Friedman would devote virtually the whole of a two-quarter sequence in graduate-level price theory to an extended exegesis on the subjects of supply, demand and markets. The answer is, because he thought they were so important as to take priority over the more esoteric and frilly topics he left out. I learned from that sequence, more than 50 years ago, that there was a great deal to be gained from revisiting supply and demand many times, at progressively subtler, deeper and richer levels. So much so that I have repeated the experience on my own, all through my professional life. Even after 50 years of this, I feel I am still learning, still deepening and enriching my understanding of those simple topics — supply, demand and markets.

I have elsewhere stated that I attribute to Friedman another purpose in making his course so totally fundamentals-oriented. In my opinion, he did not want us to learn price theory but to *imbibe* it — to make it part of our natural way of thinking, observing, and reacting to what we see.

That same spirit — of economics as a set of tools whose *raison d'être* was to be used to help us understand and interpret what we see in the world around us — came to Chicago students not only from Friedman but from many other sources as well — T.W. Schultz, D. Gale Johnson and George Tolley in agricultural economics; George Stigler in industrial organization; Gary Becker in the economics of the family and of human behavior in general; Harry Johnson and Robert Mundell in international economics; H. Gregg Lewis, Sherwin Rosen and James Heckman in labor economics; Larry Sjaastad and myself in public finance and in the economics of developing countries; Robert Fogel and David Galenson in economic history. From

whom in this entire group could you get any sense that economics was anything but a real-world science? Every one of these economists had a deep belief in and respect for economic theory — but in the sense that theory gained merit only by helping us to better understand and interpret (and maybe at times even predict) reality. Put another way — you can scan the economic stratosphere and heavens, and you'll find plenty of postulates and theorems and lemmas, but not from any of this group of economists.

I go to these lengths to describe the aura of Chicago in the era of the listed people because I truly believe it was an unusually rich source of leading real-world economists. Not only was it the cradle of the founders and of many leading professors at CEMA, it had similar concentrated influence at the Universities of Cuyo and Tucumán in Argentina, at the Catholic University and the University of Chile in Santiago, and at ITAM in Mexico City. Directly or indirectly its alumni played way more than their expected share of the key roles in the great economic reforms of these three countries, and of Panama and Uruguay as well. And so too in the main international organizations, where they have occupied far more than their expected share of leading posts.

I mention all these things because I believe that this influence and impact came about largely because of the nature and type of training that characterized Chicago during the years in question — most particularly, its fierce concentration on fundamentals and its unrelenting real-world orientation. Deeply, almost with a religious fervor, I believe this is the way CEMA and the other leading centers of economic research and training in Latin America ought to focus their activities. If anybody is to engage in stratospheric and maybe astronomical economics, let it be large institutions in wealthy countries, which can better afford such a luxury.

III. Decanting for Current Relevance

It is probably best for me to inform you, at the outset of this section, that I have been involved for some time with the work of the Instituto Superior de Economistas del Gobierno (ISEG), here in Buenos Aires. This institute deals

with various aspects of the training of government economists, but my work to date has been concentrated on master's level courses that the Institute sponsors in four universities (among them CEMA) for the purposes of “finishing” the training of entry-level economists and of upgrading the professional preparation of some who are already working in government jobs.

Precisely the spirit that I tried to express in the preceding section — my deep belief in fundamentals and in the power of economic policy — has led me to take the tasks of ISEG very seriously. In this context it is easy to see the importance of decanting for current relevance. The ISEG course originally covered four semesters, spread over a 2-year period. Recently, for budgetary and administrative reasons, it was changed to three semesters compressed into a single 12-month span. In either case the most important budget constraint, from an academic point of view, was that of *time*. Every topic, every issue, every problem that is treated in such a program gets there at the expense of something else. In a sense, picking the course titles is the easy part — Micro, Macro, Quantitative Methods, Money and Capital Markets, International Trade, Public Finance, Project Evaluation, Development Economics, Labor Economics, Agricultural Economics — all quite clearly have a valid claim (certainly in most Latin American countries). To these ISEG adds half-courses in the Economics of Education, Energy Economics, the Economics of Transport and Communications, and Natural Resources and the Environment.

The more challenging task is deciding what goes into a course, once its title appears on the list. Now there are only 48 classroom hours (including exam times) for a full course, 24 for a half course. Huge tradeoffs exist as one contemplates what should be the coverage within each course.

Let me try to take you down the road of thinking about these tradeoffs. I prefer to take a positive approach, focusing on what has the greatest claim to be in the course. That way, in a sense, what gets left out is what there is no room for.

In the field of the real side of international trade, I think the right frame is that of a small open economy facing many given prices in world markets.

Here is where one discusses issues like tariff and commercial policy. Here is where one sees the key role of the real exchange rate in determining a country's comparative advantage at any given moment. Here one analyzes effective protection, Dutch disease, and the real exchange rate effects of capital inflows (spent on nontradables) and capital outflows. Here is where one discusses the pros and cons of uniform tariffs. As a next step one relaxes the "price-taking" assumption for at least some exports and explores the pros and cons of using export taxes to try to exploit the downward slope of the demand curves a country might face in world markets. One notes in passing that no developing country has monopsony power over its import goods, so the optimum tariff concept has no real meaning for it. (Import tariffs in small countries may, however, produce indirect positive external effects through reducing exports characterized by some degree of as yet unexploited *monopoly* power.)

In the field of monetary economics I see center stage being occupied by: a) the demand function for real monetary balances; b) the consolidated balance sheet of the monetary-cum-banking system; c) the key monetary approach concept that people work down undesired real cash balances by spending more, and make up shortfalls of real cash balances by spending less than they otherwise would, and d) the idea that new net borrowing gives rise to spending in excess of what would otherwise occur.

The demand function for real cash balances is a more subtle concept than most economists appreciate. The holding of time and savings deposits, for example, should be regarded as a negative function of the rate of inflation and as a positive function of the nominal rate of interest *on these deposits*. That is, the own-price elasticity of demand for deposits (or better, of *supply of funds* in this form) is positive. It is the *cross-price elasticities* that are negative, as that of the demand for currency with respect to the interest rates on time and savings deposits and on bonds of different types. All forms of money holding are, of course, negatively related to the expected rate of inflation, but it is really a mistake to treat the demand for real monetary balances as a function simply of a rate of interest that supposedly incorporates

the expected rate of inflation together with a real interest rate. It is much better to treat the inflation rate and the various interest rates as separate variables in this key demand function.

The consolidated balance sheet that is relevant for monetary analysis has M_2 (or, more generally, “broad money”), as the principal liability of the monetary-cum-banking system. Its main assets are net foreign assets, credit to the public sector, and credit to the private (or productive) sector. In a simple but common case, net foreign assets are mainly the international reserves holdings of the central bank, and are determined by past history plus the current policy of that institution. Banking system holdings of government obligations are in this simple case determined by how much credit the government has insisted that they (most often the Central Bank) provide. Finally, what is left of available funds is what goes to the private sector, with the interest rate on bank loans (unless it is controlled) being determined by the scarcity value of these funds, perhaps supplemented by inflows from abroad, in the local *plaza*. Many other scenarios can, of course, be developed using the same general framework; including eliminating the distinction between the two types of domestic credit. Among the things that can be easily shown with this framework is how any effort by the government to capture the whole inflation tax $[(M/P)(\Delta P/P)]$ in a context of steady inflation will have as its natural consequence the steady erosion (toward zero) of the real volume of private sector credit.

The monetary approach idea that excess cash balances are sooner or later going to be spent is such a vital concept that I find it hard to understand why it is not a living part of all economists’ vision of how the world works. It lets one see easily how putting newly printed money in the hands of the public tends to stimulate demand, and how arbitrarily curtailing the money supply puts a damper on the economy as people attempt to rebuild their real monetary balances. Also, it shows one a less familiar scenario of how a drop in (M^d/P) stimulates the economy, and how a drop in (M^d/P) accommodated by a corresponding fall in M^s is simply neutral in its influence on effective demand.

The idea of net borrowing being an independent argument in demand functions is not new, but neither is it “common practice”. Some international trade models have used total real expenditure in place of real income as a determinant of demand, but this assumes that borrowed funds are spent in a pattern similar to increments of income. This being quite easy to disprove, one is left with treating net increments to credit as separate variables from income in determining demand. I believe that not only do we want the increment of credit to be a separate variable, with a coefficient different from that applying to income, but we want to always keep in our minds the idea that this year’s increment to credit may be spent in ways that are totally different from last year’s. One year may witness a boom in industrial investment; another year a surge in consumer durables demand; yet another year a sharp rise in residential construction — each of these, say, being financed by its respective increment of borrowing from abroad. Particularly critical, in the analysis of real-world scenarios, is the issue of whether the credit increment is being spent on tradables or (predominantly) on nontradables.

Several of the ideas put forth in this section are conveniently summarized in the following demand equation for tradable goods:

$$T^d = a_0 + a_1y + a_2(p_T/\pi_d) + a_3[(M^s - M^d)/\pi_d] + a_4\Delta B_f \quad (1)$$

where

T^d = demand for tradable goods and services

y = real income

p_T = price of tradable goods

π_d = general price level

(M^d/π_d) = real money balances demanded

(M^s/π_d) = actual supply of real money balances

ΔB_f = increment of credit from abroad.

I like to present this as a demand for tradable goods because it fits so neatly into the history of thought in the field of international trade. All the different processes of international adjustment are reflected here. The coefficient a_2 embodies the elasticities approach, a_3 the monetary approach and a_4 the income-expenditure approach. In point of fact, however, this is not just an international trade story – the same type of demand equation can (and in my opinion should) be used quite generally. One important provision is that we should expect the coefficients a_3 and a_4 to be potentially quite unstable from episode to episode, while a_1 and a_2 should have whatever degree of stability one associates with income and price coefficients generally.

IV. Maintaining a Healthy Skepticism

While in the previous section I chose to focus on the positive side — how we *should* proceed, what we *should* emphasize – this section’s topic of skepticism practically forces me to the negative side, i.e., to illustrate how we can go wrong by accepting too naively certain concepts or approaches.

Let me state at the outset that I am a firm, even passionate believer in simple economic models. I draw a sharp line between models whose mechanics we can actually comprehend, and those that are so complicated that, if we act on what they tell us, it is because we trust rather than understand their results. I cannot imagine going to a ministry or a central bank citing an urgent need for policy action or policy change, and having to respond, when asked why I am pressing this case, “you must do this, — because that is what my model says should be done.” Among other things, it is almost certain that somebody else’s model will, at the same time and place, be indicating some different action.

So simple models are needed, just so we can understand what they are really saying, i.e., so we can explain to a minister or Central Bank president what is the precise causal chain through which our proposed policy will work. But, since the complexities of the world are infinitely greater than any man-made model can capture, we have no alternative not just to

simplifying but to greatly oversimplifying, any time we seek to reduce those complexities to something we really can understand. But herein lies the problem: most oversimplifications will end up missing the point, just getting things wrong. In our work in economics, we look for artful and meaningful oversimplifications, that somehow capture the essence of the situation or problem or mechanism that we are studying. This is the wonder of the time-tested tools that represent the fundamentals of our discipline.

But not all of our oversimplifications are as robust as the concepts of supply and demand, or as the demand function for real monetary balances. And in fact, some quite fragile ones have somehow found their way to center stage in the economic literature of recent decades. The rest of this section is devoted to a brief review of a small selection of these.

The Representative Consumer

Economic models embodying a representative consumer are very convenient ways to illustrate certain basic economic forces. What these models do, in effect, is display the analogies between almost any economy and Robinson Crusoe's island autarchy. When Crusoe saved, he also invested, and that is true of modern economies too in a certain very real sense. Faced with variable harvests of storable foodstuffs, Crusoe would practice "consumption smoothing", building up stocks in boom years and running them down when harvests were bad. This, too, is what much more complex modern economics tend to do. So the analogy is apt, up to a point.

But the analogy fails badly when applied to big macroeconomic events. The Latin American debt crisis experiences of the 1980s are a fine example. Here borrowing reached its peak at the same time as output and income were peaking, and loans were paid back when output and income were hitting their lows. What we see, in this period is, if anything, consumption *de-smoothing*. At the heart of it were changes in expectations, both those of foreign lenders and those of domestic investors and other economic agents. I believe that for studying the debt crisis experiences of Latin America, as well as the current crisis experiences in Thailand, Malaysia, Indonesia and

other countries of Southeast Asia, we have to separate the saving decision from that of investment, and recognize that the big shocks come from the side of investments and of foreign financing.

When you stop to consider it, it is hard to think of any way of dealing with the macroeconomic fluctuations that we observe in the world without a sharp separation between the decision to save and the decision to invest. This is even true for a closed economy, where saving and investment must always be equal, *ex post*, though not *ex ante*. In short, it is the volatility of investment that drives economic cycles, and the decisions to invest are made by different people, with quite different considerations in mind, than the decisions to save. Hence we should in all such cases model investment and saving decisions separately.

Until recently, I was quite happy with the use of the representative consumer assumption when studying and modeling economic growth. But now I no longer am. As I have revisited the topic of economic growth in recent years, I want to play up the role of investment and keep it distinct from the role of saving. In the first place, the standard breakdown of growth sets up separate categories for the contributions of increased labor force, improved quality of labor, increased capital stock and total productivity improvements. Right at this point we should recognize that increased capital stock is the variable in question, not increased saving by the entity whose growth we are considering. The need for distinguishing investment from saving is obvious when we are considering the growth of firms or industries or cities, for all of which the link between saving and investment is quite weak. The need for maintaining the distinction as we aggregate into provinces or states or regions or nations is best defended by noting that economic growth really takes place at the level of the firm, and must ultimately be understood in that context. A subsidiary reason for maintaining the distinction is that even provinces, states, regions and nations are open economies, where at times more than half of investment is financed from outside, just as at times more than half of saving is invested someplace other than within the entity.

Just to close this particular section, let me outline the vision of the growth

process to which my recent work has led me. In this vision, real cost reductions are at the heart of the growth process. Where opportunities for real cost reduction are great, they typically give rise to increased investments, which promise (and usually deliver, at least for some time) unusually high rates of return. A “good” growth experience thus displays higher-than-normal rates of investment, plus higher-than-normal real rates of return, plus higher-than-normal rates of real cost reduction. “Bad” growth experiences show the opposite – real cost increases, low rates of return and low (sometimes even negative) rates of investment. This syndrome of bad growth experiences often sits side by side with good experiences in the same industry, as successfully innovating firms in that industry thrive and expand, while their less fortunate competitors are driven to the wall. Real cost increases in such cases often stem from output being driven down well below the levels appropriate for existing installed capacity. It is easy to reflect the syndrome that combines the contributions of investment, rate of return, and real cost reduction if one pinpoints investment as a key variable to target. In contrast, one gets virtually nowhere if one targets saving as the key concept.

Ricardian Equivalence

To understand what I mean in urging you to be skeptical about ideas, even about fashionable ones, consider Ricardian equivalence. Just try to analyze any major Latin American inflation with a model that implies that there is no difference between financing additional public expenditure with incremental debt or with incremental taxes. Very obviously, fiscal deficits lay at the heart of nearly all Latin American inflations, and although we do leave room in our scenario for the financing of such deficits in non-inflationary ways, the fact is that such sources were insufficient to deal with the problem.

Just as in the case of the representative consumer, one can build a scenario in which Ricardian equivalence more or less rigorously holds. Today’s increment to debt must be paid someday, or equivalently its real interest costs must be carried indefinitely. Ultimately, this scenario says, taxes must come

into play to make these payments. With the most convenient assumptions, the present value of these future payments equals today's increment to debt, hence people will be reacting to today's actual taxes in one case, and to an equivalent sum representing the present value of future taxes in the other case. Why should their responses to these two "equivalent" stimuli not be the same? This is the line of argument supporting Ricardian equivalence.

Many papers have been written suggesting different reasons, some of them quite subtle, why Ricardian equivalence might not hold in the real world. But I prefer to focus here on a single main idea – not *whether* it holds, but *when* it gets to hold. The first step has to do with the perception of future tax liabilities. If people always and accurately perceive all future liabilities, they are likely to act on this perception, and if their reaction to future liabilities is similar to the way they react to current liabilities, we have Ricardian equivalence. The second step says that for the tax that's due next quarter, or next month, or tomorrow, these conditions are quite likely to be met. But the third step says that we do not have a very clear view, even of what our income will be five or ten years from now, nor of what the tax code will then look like, nor of how we will fit into it to generate our future tax liability. Thus we would have a hard time taking that liability into account, even if our government never engaged in deficit financing. Finally, if all we know at this moment is that the government has added a certain amount to its debt, it is *rational* for us to adopt a wait-and-see attitude, waiting until our available information set gives us a reasonable clue as to what we will owe, and then incorporating that liability in our financial and economic planning. Of course, we can always spread over many future years our response to today's (or tomorrow's) tax increase.

Let me take you down a particular path with respect to Ricardian equivalence, which to me leaves useful messages about economic thinking and observation in general. At the center of this path is the idea that the Austrian economists were *right* in considering that introspection is an important part of economic thinking. The short version of my story is simply this. Ask any true believers in Ricardian equivalence how they change their consumption decisions upon learning that the government has just issued an

extra \$10 billion in debt. Then compare that with the changes they would make if they received (or quite specifically expected at a known future date) an extra tax assessment of any given amount.

The longer version of the story harks to the modern analysis of financial markets and portfolio choice. On the whole, there are no “money trees” out there, whereby anyone can get rich quick. But there is a legitimate profession of financial advising, in which serious practitioners suggest quite different portfolios and investment strategies to people depending on their situation and objectives. These same advisers will tell me how to behave if my children mature and “leave the nest”, or if I am struck with an incapacitating disability, or if I win the lottery. Their books are not at all explicit, however, in how I should modify my behavior in response to a \$10 billion increment in government debt. The challenge to supporters of Ricardian equivalence is for them to fill in this gap.

Modeling Monetary Policy and the Banking System

The message on this point is that it is a terrible violation of reality to consider monetary policy as being fully summarized in the Central Bank’s issuance of monetary base or high-powered money, H . Just as one should not treat savers and investors as if they were a single Robinson Crusoe, one should not blur the vital distinction between borrowers from the financial sector on the one hand, and those who make resources available to it on the other. Working with the consolidated balance sheet of the monetary-cum-banking system is, to my mind, essential for any macro modeling in which monetary policy decisions play any significant role.

I do not even remember when I began to use the consolidated balance sheet as the focal point of my own thinking about monetary matters. It may have been as early as 1950, when I spent a summer working at the International Monetary Fund, and it may have been as late as 1956, when I first started working on the Chilean economy. In any case, I can honestly say that over all the intervening years, this way of thinking has never let me down. I immediately give great credit to the designers of the tables in

International Financial Statistics, whose “Monetary Survey”, built precisely around this concept, dates from the late 1940s.

The distinction between focusing just on H and focusing on the entire consolidated balance sheet would not matter if M_2 were always equal to μH , with μ being a constant money multiplier, and if credit to the government and credit to the private sector were likewise firmly tied to H , say with $C_g = \gamma_1 H$ and $C_p = \gamma_2 H$. But what drove me away from focusing on H in the first place, and what has continuously reinforced the wisdom of that decision, was experience and observation. There are just too many ways one can be led astray by focusing just on H . Central Banks have regularly modified the money multiplier by changing reserve requirements, which I have seen as high as 80 and 90 percent and as low as 2%. Governments can create special bonds that are then allowed to function as bank reserves, thus changing the amount of effective high-powered money without the Central Bank’s taking any action at all, and without the “statistical H ” changing. And people themselves can significantly modify the money multiplier by changing their tastes for holding currency, demand deposits, time and savings deposits, CDs, and the like. Some of these changes can come as a consequence of technical innovations like credit cards; some can derive from changes in the rules governing bank behavior (e.g., permitting the payment of competitive interest rates). Some can even come as a result of general macroeconomic policy such as changes in the rate of actual and expected inflation. Reducing the rate of inflation can even work to give a special push to M_2^d by turning a maximum nominal interest rate (say, 20% per year) that with high inflation was strongly negative in real terms, into one that ends up not constraining market equilibrium in any way, as when supply meets demand at a nominal rate of 10% or 15% after the price level has stabilized.

But the main reason for focusing on the consolidated banking system is because the main liability of that system is something that is ultimately determined in *real terms* by the balances that the public wants to hold. As I see it, the monetary authorities have to watch with care the behavior of people in this regard, and then take action that makes sense. If people want more real balances as an inflationary economy stabilizes, the authorities should

not hold H constant (forcing an absolute deflation) but should move to accommodate people's changing tastes. If the government resorts to inflationary financing of its deficits, the monetary authorities should prevent this from totally eroding private sector credit, even if permitting such erosion causes an increase in the resulting "equilibrium" rate of inflation. The deficit causes inflation; inflation reduces (M_2/P) and with it the total real volume of domestic credit outstanding. The monetary authorities should seek a division of this real domestic credit into an acceptable portion going to the private sector and its complement going to the government. Likewise, a monetary authority pursuing a fixed exchange rate (or *tablita*) policy should realize that a monetary target is in the final analysis incompatible with such a policy. Just as credit to the government should not be allowed to squeeze real credit to the private sector indefinitely, so, under a fixed rate or *tablita* system, one should not allow net foreign assets to expand indefinitely, similarly squeezing real private sector credit down to (or close to) zero. At some point the lesson should be learned (and applied) that with a fixed exchange rate system the only way for the real exchange rate to adjust is via price levels and that an equilibrium fall in the real exchange rate that is needed to accommodate an export price boom or a large capital inflow should simply be allowed to occur. The resulting rise in the internal price level should properly be thought of not as "inflation" but rather as an equilibrium adjustment of relative prices.

The virtue of focusing on (M_2/P) should be very well appreciated in Argentina, where during the first quarter of the *tequila* crisis total foreign assets of the monetary authorities fell by more than 35%, and net foreign assets by more than 50%. The strict rules of a currency board would have required a contraction of similar order of magnitude in M_2 , with devastating effects on credit to the private sector and on real output. But, in a magnificent display of economic statesmanship and ingenuity, the Argentine authorities managed to hold M_2 to a fall of only about 13,5%, mostly reflecting the reduction of international reserves. Credit to the private sector fell by only a little more than 2%. This miracle was accomplished, as most of you know, by a sharp reduction of the reserve (*encaje*) requirements applied to

commercial banks, and by taking advantage of a sly provision of the convertibility law, which allowed certain Argentine government bonds denominated in dollars to be counted as backing for high-powered money. Thus while Central Bank foreign assets (both gross and net) fell by some \$6 billion, high-powered money only fell by about \$3 billion and banking system credit to the private sector by about \$1 billion. I do not believe that this miracle would have occurred had not the Argentine authorities been very watchful both over M_2 and over credit to the private sector. This alertness, in my view, lies behind one of the greatest monetary policy achievements I have witnessed in some 50 years of professional life.

Modeling International Capital Flows

This is the area where I have been most troubled by the treatments I keep encountering in workshop and journal presentations. The most common treatment departs from an international nominal interest rate i^* , and moves from there to a domestic interest rate i_d by adding a country risk premium δ plus the expected rate of devaluation φ . These two adjustments meet the conditions for covered (or uncovered) interest arbitrage, taking the risk premium into account.

I have problems with this treatment in several related dimensions. The source of all of them is the assumption that the country in question faces an infinitely elastic supply curve of credit and of capital. I see this assumption as implying a total de-linking of domestic saving and domestic investment. Under this assumption, *all* increments to saving in effect *go* abroad. There is no crowding out of alternative investments, no matter how big an investment boom gets. Likewise, there is no tightening of local interest rates, when domestic saving is cut in half, nor is there any easing of these rates when domestic savings double. Further, the economic opportunity cost of capital is totally uninfluenced by the internal marginal productivity of capital or by the tax distortions that work on both the saving and investment sides of the capital market.

The above are sort of “structural” implications of the modeling assumption. They apply to the middle and long run as well as the short. They apply in perfectly normal periods as well as in crises. But if one focuses directly on crisis situations, one sees immediately the debility of the infinite-elasticity assumption. Imagine telling the finance minister of Mexico or Argentina in early 1995, or the president of the Central Bank of Chile in early 1983, or of Indonesia or Thailand today, “Sir, don’t worry, all you have to do is borrow abroad to cover your needs for foreign currency. To be sure, you have to pay a high premium, but at the price $(i^* + \delta)$ you can get all the money you require.” They would all consider the assertion laughable, and rightly so.

The fact is that nearly all developing countries, nearly all the time, face upward rising supply curves of foreign funds. As clients of lenders, they do not fit into the mold of the capital asset pricing model, where the price of a class of assets depends on just a few determining variables, so that for given values of these variables those assets face an infinitely elastic demand (= supply of funds). No, a developing country’s capital market is more like the different lots in a resort development. Some buyers are willing to pay very high prices; they usually buy the first lots. Others are more reluctant, and developers often have to cut their initial prices to attract their demand. To be sure, specific attributes of each lot may cloud the real-world application of this story, but I believe it would be true even in cases of fully homogeneous lots. We are simply dealing with a downward sloping demand curve, representing buyers placing different valuations on a lot.

I believe this analogy applies to developing countries as they face the world capital market. Not only does the supply curve of funds facing a country slope upward, but it shifts around through time. No commercial lenders wanted to lend to Chile in 1973 (or even 1975) or to Peru in 1987. Yet with time these same countries found it easier to get loans, until in the late 1980s and early 1990s Chile effectively placed a tax on inflows of funds, so as to permit an internal equilibrium interest rate that was higher than $(i^* + \delta + \phi)$.

Trying to capture the essence of what is happening on these cases, I like

to think of, say, four risk categories in which the international capital market can place a country. In each case the foreign borrowing rate rises as B_f/y increases. But both the intercept and the slope of the curve change as a country shifts from one risk category to another. In category D, the worst one, lending might begin at LIBOR plus 4 or 5 percent, and this rate move up rapidly as credit increased to a limit that was but a low fraction of a year's GDP. By the time a country reaches category B, however, lending might begin at, say, LIBOR plus 1 or 2 percent, with this rate moving up quite slowly until a credit limit of, say, a full year's GDP is reached. Finally one gets to countries like Luxembourg (category A), which are integral parts of the world capital market, and where, for most practical purposes, the CAPM model can be used, implying a (nearly) infinitely elastic supply of funds at a zero or negligible premium over LIBOR.

Working with actual data for Latin American countries, I have found it far preferable to treat the annual capital flow (or net resource transfer) into the country as an exogenous variable, rather than taking the supply of funds as infinitely elastic. But I am now of the opinion that it is even better to work directly with a shifting supply curve of funds, passing through the points $(0, i_t^*)$ and (B_{ft}, i_{ft}) , where i_{ft} is the actual rate specified on new loans in dollars (or other major currency). Such a curve could track a country like Chile as it moved, say, from category D in 1973 to category B+ in 1980, then back to, say, C- in 1983, drifting finally up to A- by around 1990. All three assumptions, this one of a rising curve with intercept at i_t^* , and the alternatives of a flat curve at i_{ft} and of a vertical curve at B_{ft} , are built to pass through each observed point. The important differences come when we simulate responses to assumed disturbances, where, at least in my opinion, the upward rising curve has strong advantages over the other two, while at the same time its shifts capture the changing risk status of a country.

V. Emphasizing Diagnostics in Teaching and Research

It never ceases to amaze me how lax we in the economics profession

have been in the subject of diagnostics. I cannot remember ever having seen or heard of a course exclusively devoted to this subject, and not much beyond the simplest supply-and-demand examples typically appear in any course. The simplest case here is the link between price controls on the one hand, and black or gray markets on the other. Next might come the estimation of the free-market equilibrium price on the basis of the observed black-market and official prices in such cases. An important and easy macroeconomic case is the identification of the phenomenon of transfer-induced Dutch disease in countries like El Salvador and the Dominican Republic. Both these countries “suffer” from a chronically appreciated real exchange rate. Producers of tradable goods are constantly complaining about this, and pressing for a nominal devaluation, thinking that will solve their problem. A proper diagnosis of the situation leads to an understanding that the low real exchange rate is not an aberration but an equilibrium phenomenon, due to the chronic abundance of dollars created by emigrant remittances. Given this diagnosis, one can predict in advance that attempts to create a more favorable environment for the production of tradable goods by devaluing the nominal exchange rate will have only a transitory effect on the real rate. In the end one can predict that, barring changes in other real determining variables, internal prices will rise to restore the equilibrium real exchange rate, in the process nullifying the stimulating effect of the nominal devaluation. This scenario has played itself out, as predicted, in these and other countries of similar characteristics, when large nominal devaluations were attempted.

The preceding examples are the easy cases. But there are many others that are more difficult and that represent major challenges to the entire profession for the years ahead. In what follows I shall set out just a few of these “areas of challenge”.

Tracking Shifts in the Demand for Real Monetary Balances

I had the good fortune, recently, to sit next to Alan Greenspan at an economics luncheon. I took advantage of the occasion to test out on him my

own interpretation of Federal Reserve policy and strategy during his time as chairman of the Board of Governors. I had for quite some time inferred that he and the other governors took very seriously the concept of the demand for real monetary balances, but that they felt it was not stable enough to support, say, a constant rate of monetary expansion. Even after they included in this function all the variables they thought belonged there, it still exhibited important shifts that they were unable to predict in advance. But it was easy to observe each shift, once it happened, simply as the residual of the actual observation from the one predicted by their best available money demand function (or functions). Then came the real job, of interpreting these residuals. They can be ignored if they are purely random, stemming from errors of measurement or from highly transitory shocks. But they should be accommodated by changes in M when they reflect genuine changes in people's tastes and behavior. The task, then, is to do the necessary detective work so as to have an idea of when the current and recent residuals fall into one category or another, and when they do appear to represent a genuine change of tastes or behavior, to try to guess at an interpretation.

The detective work in question entails trying to examine *all* the evidence including, among other things, data disaggregated by state and region, by category of money holding, by type of holder, etc. If household cash balances in 50 different states are all falling below the predicted levels, it is hard to maintain that one is facing a random shift of a function. The task then is to pursue other types of evidence and other trails, in an effort to really understand what happened.

At that luncheon, I posed to Greenspan my hypothesis that the Fed really thought in these terms and that the research arm included a whole corps of detectives, some constantly trying to interpret residuals, others engaged in the related task of improving the underlying demand functions for real balances. When I asked Greenspan whether I had described reasonably well what the Fed actually does, he replied "Yes, but you make that detective work sound a lot easier than it really is!"

To give you an idea of one line of detective work, I can report on an

experiment we performed a few years ago. We started with a standard specification in which the stock of real monetary balances was a function of real income, the rate of inflation, plus sometimes other variables. We were considering shifts in this basic “stock demand” function. At one extreme, if all shifts were permanent, they would follow a random walk, and the right way to estimate the function would be in first differences. At the other extreme, if all shifts were purely temporary, coming in one period and going away (being replaced by a new one) in the next, the right way to estimate the function would be in the levels of the variables. These two could be considered as extreme cases of an autoregressive process, so we did estimates imposing autoregression coefficients of 0.25, 0.50, and 0.75 between successive residual terms. Then, having fitted five different variants of each of several countries’ demand functions for real balances, we looked at the residuals implied by each. We tried to put the shifts on a comparable basis, reflecting in each case the shift from where the basic “stock demand function” stood last period and where it stood in each current period. Somewhat to our surprise, and much to our satisfaction, we found a very high correlation between all five series for the implied (comparable) shifts. This meant that the identification of the shifts that Greenspan’s detectives would try to study and understand was quite robust across the five different specifications. It made us much more confident that there was a genuine reality of behavior underlying the shifts taking place in any period. This robustness was good news, but the real task of studying the shifts in depth, and trying really to understand them, still lies ahead.

Studying the Phenomenon of Overshooting

One of my favorite examples of dynamic adjustment is a hypothetical case in which the rate of monetary expansion was constant at, say, 5% per year for some time, and then jumped to, say 25% per year and stayed there. The equilibrium response to this change is an increase in the inflation rate of 20% per year, and a reduction in the equilibrium level of real cash balances.

Assuming no underlying real growth in order to simplify the exposition, we have a shift from an equilibrium inflation rate of 5% to one of 25%. This change will cause the equilibrium level of real cash balances (M/p) to fall, a result which can only occur if, over some adjustment period, the price level has a cumulative percentage increase that is greater than that of the money supply.

A super-rational expectations adjustment [full information and accurate expectations at all times, except the moment (t^*) of the shock itself (which is new information at that instant)] would have the price level adjust upward immediately at t^* , reducing (M/p) instantaneously to its new equilibrium level. So the time path of (M/p) would be a simple step function, with one step downward at t^* . The time paths of \dot{m} ($= \Delta M/M$) and \dot{p} ($= \Delta P/P$) would be upward step functions, jumping from 5% per year to 25% per year, exactly at t^* . But the \dot{p} function also would have a spike at t^* , which would be precisely sufficient to instantaneously bring (M/p) down to its new equilibrium level.

This is obviously not the way price level adjustments occur in the real world. If something like the postulated change in monetary expansion were to occur unexpectedly, we can be certain that the inflation rate \dot{p} would rise somewhat gradually. Until it reached 25%, it would be less than \dot{m} , so (M/p) would be increasing. Hence \dot{p} has to overshoot, passing through 25% and for some time staying above it, driving (M/p) down not just to its initial equilibrium level, but *through* it to its new, lower equilibrium.

In the scenario just described, we have an overshoot of \dot{p} as well as an overshoot of (M/p) . It does not take very strong assumptions — just any lag in the adjustments of \dot{p} to that of \dot{m} , in order to make this overshooting an absolutely necessary part of the adjustment process in the case examined.

In other cases, overshooting also seems to be a virtually necessary part of the adjustment process. If some product experiences a rapid rise in demand, its new price will be determined by a movement along a short-run supply curve, based on a set of existing fixed factors of production. As these fixed factors turn, one after another, into variable factors, the relevant supply curve becomes

more and more elastic. The intersections of these shifting supply curves with the new demand curve trace out an overshoot in which price may have started at 100, then quickly jumped to a short-run equilibrium at 150, and then gradually drifted down to a new long-run equilibrium at, say, 120.

Speculative elements played no role in the two scenarios just presented, but they do enter importantly in real-world stock markets, and sometimes also in organized commodity and currency markets. There are different ways to model a speculative overshoot or bubble. The one I like best builds on the familiar Wall Street dichotomy between “technicians” and “fundamentalists”. A crude definition would say that the technicians bet on the prolongation of existing trends (betting on inertia, as it were), while the fundamentalists believe that the market obeys the principle of gravity, tending to be pulled to a central equilibrium point that is determined by the fundamentals of each case. Thinking in these terms, one describes an “overshoot” as a movement initially dominated by technicians extrapolating, say, an upward or downward trend. As the market gets farther and farther away from its medium- or long-run center of gravity, however, more and more erstwhile technicians turn into fundamentalists, and turn from betting on a continued trend to instead betting on a return to the fundamentals. In this story, everybody is right at least some of the time, so we can see why both technicians and fundamentalists have passed the test of survival. We see, too, how a market overshoot described and thought of by participants in these terms could easily track an overshoot that traced the reaction of, say, copper prices to an unexpected upward surge in demand.

The individual market participants, however, never really “know” if they are in an overshoot, or, if they are, whether the price trend will continue for some time before the fundamentals reassert themselves. So everybody is plagued by the conflicting emotions of greed and fear, which many veterans say are the forces that really drive speculative markets. Among other things, most participants in such markets end up unwilling to place large fractions of their wealth on any one or two bets. This is one reason why overshoots often end up being quite prolonged — they don’t attract enough “converts”

to fundamentalism early in the process, as those who bet too early on the reversal of an overshoot often end up with huge losses.

To get a feel for overshooting in a current context, think of the recent history of the Indonesian rupiah. Starting, pre-crisis, at about Rp 2400 to the dollar, the rate moved quickly to Rp 5000, then to Rp 10,000, and finally to over Rp 15,000. At the most recent sighting, it was oscillating between Rp 10,000 and Rp 11,000 to the dollar. I believe it is an absolute certainty that at this level (combined with the current general price level) the real exchange rate of Indonesia is “too high”, and that it will come down in the foreseeable future. But market participants from abroad are not ready to move back into the market and massively buy rupiah. Some fear renewed political unrest, others just want to move, but only with caution. Others, still more cautious, want to avoid commitment while they wait and see. As a result the end of the overshoot is not in sight, yet it is hard to blame participants for acting as they do.

These stories are not meant to give you “the real truth” about overshooting. They are rather meant to make you more aware of the phenomenon, and to make you realize how widespread and important it is. Above all, they are meant to help you see what a huge research challenge the study of overshooting can represent, and to stimulate some of you to take up this challenge.

Appreciating the Importance of Bank Credit

An easy way to introduce this section is to recall a point made earlier, that a reduction in the demand for money balances, say, in a fixed-exchange-rate setting, exerts an expansionary force in the economy while the downward adjustment is being made, and a neutral force once M^s and M^d are again equal. Why, then, are large reductions in the demand for money so troubling to economists and to economic agents generally? The answer, I think is that major reductions in the demand for real cash balances usually carry with

them major reductions in bank credit, particularly credit to the private sector. Argentina, 1995, was the exception, not the rule.

A reduction in the volume of outstanding credit exerts a depressing force on effective demand while it is in process. Once the volume of such credit has been reduced, its lower level appears to have a negative effect on output. At least, major real credit crunches are typically associated with declines in output, and revivals of real credit to the private sector are typically accompanied by general economic recovery. The problem is, we really do not understand the causal mechanism at work in these cases. Learning more about how credit really works is an important challenge for future research. Meanwhile, those who diagnose and prescribe can at least take the precaution of trying to find ways to forestall a major credit crunch, as the Argentine authorities did in 1995.

My own thinking about the role of credit has been propped up by the idea that credit is to the business sector what oil is to a machine — something that promotes the smooth functioning of the operation. I am, of course, very aware that this idea functions as a crutch, enabling me to assign an important role to private sector credit, even though I do not have a deep understanding of how it works. But I am confident that it is better, in this case, to walk with a crutch than not to walk at all — i.e., it is better to assign an important (though poorly understood) role to credit than simply to ignore it.

Different paths can and should be followed as we try to learn more about the role of credit. Studying the experience of individual firms is a fine place to start. We know that some firms operate successfully without credit, so one line of attack is to try to measure what “price”, if any, they pay for doing so. This price would presumably have to do with the fact that equity capital, at least in normal times, has an economic opportunity cost that is greater than debt capital. At the other extreme we can look at the experiences of firms that have been battered by a credit crunch. Here one should try to distinguish between firms that would be non-viable anyway (due to other causes) and firms that are driven to the wall simply because of very high interest rates on (or the denial of continued access to) bank credit.

Another line of investigation is the study of bad and nonperforming loans. I have the distinct sense that major credit crunches are characterized by a significant fraction of loans being bad at the beginning of the crunch, with that fraction growing as the scenario unfolds. The result, with total credit to the private sector being progressively squeezed, and bad credit growing at the same time, is a greatly exaggerated contraction of the amount of funds available for “good credit”, i.e., credit to healthy, viable firms. Can we find ways of identifying bad credits (even *ex post*), when the banks themselves do not classify them as bad?

At the macroeconomic level, it does not do much to save the situation when the Central Bank rescues weak commercial banks by simply exchanging its good obligations for their bad loans. This will probably stop a run on those banks by their depositors, but it does nothing at all to change the consolidated balance sheet of the banking system. The total amount of bank credit to the private sector remains the same after such an operation, as does the fraction of this credit occupied by bad loans. The scarcity of credit for healthy firms is not alleviated in any way. When “governments”, as distinct from Central Banks enter to bail out a weak banking system, they have the opportunity to “free up” the bank lending capacity that was previously locked up in bad loans. But to do this the government must buy that bad paper with cash. If all it does is give the Central Bank government bonds in exchange for the bad paper, then it is *these bonds* that will take up the lending capacity freed up by the operation. So, in order to really do some good, the government has to use fiscal surpluses (or perhaps new foreign borrowings) for its bailout operations. (Financing the bailout by selling government bonds in the domestic market will probably do no good, as it thus sops up an amount of funds that just matches the expansion of good credit that the banking system can undertake on the basis of the bailout. I believe some very interesting and useful results would flow from an attempt to examine in these terms some of the considerable (and growing) number of recent and current government bailout efforts.

Quantifying the Social Costs of Unemployment

A good way to start this section is for me to cite a famous remark by James Tobin – “It takes a heap of Harberger triangles to fill an Okun gap.” In that remark, Tobin was implicitly attributing as a social cost the entire gap between actual and potential GDP, and by innuendo criticizing the approach of measuring costs solely in terms of the types of distortions usually considered in applied welfare economics. In effect, he was saying that the distortions approach misses the point, that it fails to capture the lion’s share of the real costs of a shortfall of output.

Now when I first heard that remark, I issued a verbal challenge to Tobin, for him to try to express the Okun gap in the language of applied welfare economics. I have not seen any such effort, and I think the reason is not hard to find. The easy way to at least start down the road to a reply is to assume the social opportunity cost of labor to be zero, at least when that labor is being absorbed from the ranks of the unemployed. Now the issue of zero opportunity cost has a long history in the literature, and the weight of the evidence and analysis is very strongly against such an assumption. My own one-line comment on this subject is “I’m happy to assign a zero economic opportunity cost to any workers who are ready to work for a wage of zero; otherwise I’ll take the voluntary supply price of their labor as my starting point.”

This answer is certainly the answer of applied welfare economics, in which demand and supply prices play such a key role. It is interesting to note that modern representative-consumer models take a similar tack. In most of those models, when there is a change in employment it usually represents a voluntary movement along the supply curve of labor, with no efficiency cost being involved. In such cases, however, we are not dealing with involuntary unemployment. One way to simulate that in such models would be to restrict the representative consumers, giving them more leisure than they really wanted. This would give rise to a triangle of efficiency cost – maybe one of the Harberger triangles Tobin was referring to.

This leads quite naturally to a next step, in which one would assign to unemployed labor an economic opportunity cost equal to its estimated voluntary supply price. This is what I and many others have done, over a number of decades. It causes little problem to assign an opportunity cost of, say, two-thirds or three-fourths of the going market wage, to unemployed workers who are given employment as a consequence of some project or policy we are studying. In addition, of course, one should count any reduction in unemployment compensation payments that results from the project or policy in question. Such payments represent a genuine externality. Workers who assume active employment, just at their respective supply prices, are reflecting a state of indifference between “working at that wage” on the one hand and “enjoying leisure plus unemployment compensation” on the other. Thus the unemployment compensation given up by the workers is part of the opportunity cost that is being just barely compensated by the wage. The saving to taxpayers, however, stemming from not having to pay that compensation, has not yet been taken into account. Hence it is a genuine positive externality, to be assigned to the action of re-employing the unemployed.

The above as if we have the problem sewn up tightly, with no loose ends. The difficulty comes when we seek evidence on voluntary supply price. The great bulk of unemployed workers, when asked what is their supply price, will state a sum very close to the going wage for their type of labor. If we follow this evidence to where it leads, we will end up saying that unemployment creates no problem (other than the unemployment compensation we pay), i.e., that the workers are perfectly happy with the tradeoff between work and leisure, as they see it. This would be fine, if it were true, but in most cases it is quite clear that they are not happy with the tradeoff. An unemployed worker, Mr. U, has a stated supply price equal to the market wage, of, say \$50 per day. He regularly goes searching for work, and comes home downcast to a disappointed family as for many weeks running he has had no success. He does not, however, lower his stated supply price below \$50, as that is indeed the going market wage for this type of work. Then, one day, he comes home reporting that he has indeed found a good job at a wage of \$50.

The whole family is full of joy, and mounts a big celebration.

If this scenario has wide validity, as I think it really does, that means we are making a mistake in assuming indifference on the part of the workers between failure and success in their search activities. How should we really represent the gain that comes from absorbing unemployed workers. Right now, that is a conundrum, but with powerful social and political implications. It is something that we ought seriously to think about, and hopefully find a satisfactory answer.

I certainly do not have the answer, but I think I can help you see that the problem is genuine. The key is to recognize that the supply price of labor is not as simple a concept as it appears. Consider most professors of economics. If they are located in the United States, they expect to be offered a salary corresponding to the U.S. market for economics professors. This is substantially higher than the U.K. market, but if some of those U.S. professors end up moving to the U.K. for reasons of family or health, many, probably most, would be willing to offer their services to the U.K. universities, at a U.K. wage. Chilean professors earn still less than those in the U.K., but again, many U.S., professors, if fate brought them to Chile for reasons other than a job, would probably willingly offer their services to the Chilean universities, for work at the going Chilean wage.

So we have at least two different supply curves. In one of them the suppliers of labor are inserting themselves in a market where they and everybody else get the same market wage. Every point on a market supply curve is a potential point of market equilibrium. The individual supply curves that one sums to get the market supply curve should be built on the same assumption. Yet the supply curve along which the unemployed are operating as they make their choices is one where “everybody else” who is employed is getting the market wage of that time and place, while they (the unemployed) offer different supplies of labor at different wage rates. I prefer not to think of this type of function as a real supply curve. Instead, I prefer to think of Mr. U guessing at the excess demand curve for his type of services, in that market. So very likely his stated supply price isn’t even a true supply price.

Maybe he is better thought of as playing a game of search, in which the reward is a permanent or semi-permanent job at the market wage – a reward which he gets on that one lucky day when he finds a job — and the return on the losing days consists of the value of leisure (probably related to some relevant ordinate on his “true” supply curve) minus the disutility of whatever search activity he undertakes.

The above simply takes a stab at what a final answer to the conundrum of the social opportunity cost of unemployed labor might look like. I hope this gives you enough to see that it is a genuine intellectual challenge. In addition, of course, it is a matter of serious importance in the real world. People laugh at us when we impute to the involuntarily unemployed a benefit from leisure equal to the market wage (in cases where they are receiving no unemployment compensation). Yet that is where certain traditional paths have led us. How can we come up with an appropriate diagnosis of the severity of the problem if we have no real clue as to its cost? And how can we give sensible prescriptions for relieving the problem, if we operate out of such a state of ignorance? So, I submit, it is a real challenge to us, both as economic analysts and as “country doctors”.

VI. Concluding Observations

I realize that the format of “a letter to a younger generation” is far from the typical conference contribution. But I hope that listeners and readers will understand that it is motivated by the special ties that bind me to CEMA, to its founders, to its professors and its students. And it is also motivated by my love for the science, the discipline, the profession and the practice of economics, and by my great desire to see all of these thriving and flourishing together in the coming decades. I hope, then, that you will find in these rambling comments about many subjects some sense of where we as a profession have come from, of where we are and of where we might fruitfully go, that will help you make your own contributions to the fulfillment of my great wish.