Interview with Robert A. Mundell

Howard R. Vane and Chris Mulhearn

Robert A. Mundell has been Professor of Economics at Columbia University in New York City, New York, since 1974 and University Professor since 2001. In 1999, he was awarded the Nobel Memorial Prize in Economic Science “for his analysis of monetary and fiscal policy under different exchange rate regimes and his analysis of optimum currency areas.” We interviewed Professor Mundell at his hotel in Boston, Massachusetts, on January 7, 2006, while attending the annual meeting of the Allied Social Science Associations.

Background

VANE and MULHEARN: As an undergraduate you studied Economics and Slavonic Studies at the University of British Columbia. What attracted you to study that particular subject combination?

MUNDELL: I went to UBC, into the sophomore year, in 1950. At that time the Korean War had broken out and was a big issue. I was in the COTC [Canadian Officers’ Training Corps], and many of us expected to be part of a second Canadian brigade in Korea. But it never materialized. From an early age I acquired a keen interest in foreign affairs. During World War II, my father, who was in the army, kept maps of the war theaters on our kitchen wall, and I kept up that interest in high school. I remember particularly being appalled by the Soviet occupation of Eastern Europe. It seemed to me as I entered college that the major problems in the world concerned international relations—especially with the Soviet Union—

Howard R. Vane is Professor of Economics and Chris Mulhearn is Senior Lecturer in Economics, both at Liverpool John Moores University, Liverpool, United Kingdom. Their e-mail addresses are (H.R.Vane@ljmu.ac.uk) and (C.J.Mulhearn@ljmu.ac.uk).
and living standards. So I studied Russian and Slavonic Studies at UBC, along with economics, which gradually evolved into a deeper passion.

VANE and MULHEARN: Having graduated from the University of British Columbia with a BA in 1953 you then undertook an MA at the University of Washington. Did any of your lecturers at either British Columbia or Washington stand out as being particularly influential or inspirational?

MUNDELL: At UBC the first professor of economics I had was Joseph Crumb, an American from the University of California who had settled in Vancouver. Instead of using the trendy Samuelson text, he used the “classical” textbook of John Ise (1946), an economist at the University of Kansas. Crumb had his own ideas and taught the \( P = E/O + (1 - S)/O \) model\(^1\) of Keynes’s *Treatise on Money* (1930) at a time when virtually everyone else in the profession was teaching the theory of the multiplier from Keynes’s later and more important book, *The General Theory of Employment, Interest and Money* (1936a). Crumb was an interesting lecturer, and I can definitely say that it was his course in “advanced theory” that hooked me on economics.

At the University of Washington there was a group of young professors in their 20s and 30s. I took a course in mathematical economics that introduced me to calculus from Donald F. Gordon. I should mention Douglass North, who shared the 1993 Nobel Memorial Prize for his work on economic history; James Cartwright, who had been a student at Stanford of Lorie Tarshis, who had studied under Keynes; Richard Huber, the chairman of the department, in International Economics; Dean Worcester, an exceptionally able microeconomist; and James Lampman, a representative of institutional economics. It was an excellent university but I wanted to go to a more famous place and so I went to MIT, then perhaps the top place in the world in economic theory.

VANE and MULHEARN: Your 1956 Ph.D. thesis for MIT was written while you were at the London School of Economics under the guidance of James Meade. What led you to choose the theory of international capital movements as your area of study?

MUNDELL: Most Canadians get involved automatically with the international side of things because it is an open economy trading heavily with its big neighbour to the south. I took a reading course with Charles Kindleberger, and, together with his policy-oriented ideas, I got fascinated by James Meade’s book *A Geometry of International Trade* (1952). At MIT, I only took three courses, but they included Samuelson’s course on mathematical analysis, which used his *Foundations*, and Samuelson’s seminar on business cycles, besides the reading course with Kindleberger. What brought me to the London School of Economics was a MacKenzie–King Travelling Scholarship, provided by the Canadian government, and the urge to see Europe. It paid only $1500. I left LSE partly because Meade had left for New Zealand in March 1956, but also because I ran out of money. I came back to Boston,

\(^1\) The model focuses on the price level and how it is changed, not on changes in real output and employment.
got a job teaching at Boston University in the spring and a research assignment from Robert Baldwin in the summer, while putting the finishing touches on my dissertation. I was one of the last students to get a Ph.D. from MIT in “Industrial Economics.”

VANE and MULHEARN: When did you first decide to pursue a career in academia? Was it before or after you were awarded your doctorate?

MUNDELL: I fell in love with economics as an undergraduate, and it became a kind of passion with me. I wanted to study with the best economists I could and keep in contact with the best minds after I graduated. That was a powerful incentive to excel and get an offer from one of the best universities.

Principal Contributions

VANE and MULHEARN: In discussing your principal contributions to economics, the focus of your early work in the late 1950s was on international trade theory (Mundell, 1957; 1960b). That work involved improving and developing the classical model of international trade and factor movements. What was the inspiration behind your 1960 *Quarterly Journal of Economics* paper on “The Monetary Dynamics of International Adjustment under Fixed and Flexible Exchange Rates”? This was your first major published paper in which you introduced macroeconomic considerations.
MUNDELL: As you noted, my early work was on “pure theory”—developing the classical model, abstracting from monetary considerations or employment issues. That background, however, provided some of the building blocks for a comprehensive theory of international macroeconomics. At that time, almost nothing had been written in the theoretical field to integrate interest rates, capital transfers, and monetary considerations into international economics. Yet it was impossible to discuss economic policy without reference to these macroeconomic considerations. Any subject that concerned international trade between Canada and the United States had to take into consideration both capital movements and exchange rates. Canada was, in the 1950s, the only one of what we now call the G-7 countries that had a flexible exchange rate. So I started to work in that sphere. My 1960 QJE paper was, I think, one of my most original and most interesting papers. It involved classical, not Keynesian, analysis of an economy dominated by a goods-and-services market and a foreign exchange market. I showed in that paper that even if prices and wages were flexible, flexible exchange rates systems were fundamentally different in their dynamics from fixed exchange rate systems.

VANE and MULHEARN: Having taught at the University of British Columbia (1957–8), Stanford University (1958–9), and the Johns Hopkins University School of Advanced International Studies in Bologna, Italy (1959–61), you joined the research department at the International Monetary Fund as a senior economist in 1961. During your time at the IMF you wrote some of your most influential and frequently cited papers. First, in a series of papers you explored how the effects of monetary and fiscal policy critically depend on the degree of capital mobility and whether the exchange rate is fixed or flexible (Mundell, 1961a; 1961b; 1962; 1963a). Second, you put forward “A Theory of Optimum Currency Areas” (Mundell, 1961c). We would like to pursue the inspiration behind these two strands of research.

In your 1962 IMF staff paper, you considered the appropriate mix of monetary and fiscal policy, demonstrating that under a fixed exchange rate regime with imperfect capital mobility, economic stability requires that monetary policy should be assigned to achieving external balance and fiscal policy to achieving internal balance. How was this paper received, given that it contradicted the conventional wisdom of the time?

MUNDELL: There was opposition to it both within the Fund and outside it. My very short paper became a “departmental memorandum” which was circulated to all the member countries of the IMF, including, of course the United States. Inside the Fund, the question was whether it would be published in the IMF Staff Papers. It caused a great controversy in the editorial board. Nevertheless, because the arguments against publishing it were all different, the editor decided to go ahead with publication! The paper was controversial because it went against the (then) policy mix of the United States, and IMF advice to the U.S. At that time, the U.S. was following a policy mix based on the Tobin–Samuelson “neo-classical synthesis” policy mix of low interest rates to spur investment and a budget surplus to siphon off excess liquidity and prevent inflation. My theory was that the policy mix should be reversed. What was needed under fixed exchange rates was a policy of tight
money to correct the balance of payments, coupled with a tax cut or increased government spending to stimulate the economy. A legitimate concern of the IMF was its influence on developing countries, which had chronic budget deficits and excessive inflation. My model fits best for developed countries that had well-developed capital markets and confidence in their currency parities. The paper caused a lot of discussion within the U.S., both before and after it was published in March 1962. By the end of that year, President Kennedy announced a change in the U.S. policy mix at speeches at Yale and in New York, moving to a tax cut and tighter money. After Kennedy’s speech, Jacques Polak, the head of the IMF’s research department, came to see me and said, “Well, I guess you must be happy.”

VANE and MULHEARN: Is it fair to say that by investigating the stability properties of a dynamic system, you were in effect examining the operational solution to Tinbergen’s (1952) rule?

MUNDELL: Yes, Tinbergen’s rule was that a policy framework had to contain an equal number of instruments and targets. In economic theory there are always two issues relevant to a consistent model. One is whether an equilibrium exists. The other issue is whether the equilibrium is stable. Tinbergen’s rule for policy was relevant to the first issue of existence. Mine was relevant to the second issue of stability. Now there has been a long history in economics, starting mainly in the late 1930s, of discussion about existence and stability. Samuelson was the pioneer in investigating the stability properties of dynamic systems. I think I was the first to apply that apparatus to policy systems. Tinbergen’s rule and policy is static. One instrument and one target, and you have to find the right solution. It was the right place to start, but it didn’t consider the dynamics of adjustment. My approach to economic policy can be interpreted as looking first at the nature of the disequilibria and then looking at the dynamics (or policy actions) that will restore that equilibrium. The issue here was how the monetary and fiscal policy instruments should be allocated between the balance-of-payments or employment targets. I called the general dynamic problem the “principle of effective market classification.”

VANE and MULHEARN: In your 1963 paper in the Canadian Journal of Economics and Political Science (1963a), you considered stabilization policy for a small open economy, with perfect capital mobility, under fixed and flexible exchange rates. Given that the early 1960s was a time of restricted capital movements and fixed exchange rates, the approach you took was prophetic. To what extent was this paper influenced by your Canadian (open economy) background?

MUNDELL: The Canadian background factor was always there. But it is easy to exaggerate it. What was significant about the paper was not that I recognized the importance of capital movements. The whole world recognized their importance. What was significant was that I found a way to put them conveniently into a simple model that had dramatic policy implications.

VANE and MULHEARN: Some of the most influential contributions to the monetary approach to the balance of payments and exchange rate determination were made in the 1970s by your former Chicago colleague Harry Johnson and Chicago Ph.D. student Jacob Frenkel. Did you supervise Frenkel’s Ph.D.?
MUNDELL: I played a significant role at the beginning, in the theses of Mike Mussa, Rudiger Dornbusch, Jacob Frenkel, Manuel Guittian, and some others, especially in setting the basic area of research and approach to the problem. But I received a Guggenheim Fellowship in 1971 and decided to spend some time in Canada. So I was not on their committees or at their defenses. Harry Johnson saw their theses through to completion. Johnson and Frenkel (1976) edited a book on The Monetary Approach to the Balance of Payments that contained several articles on the subject, including their own and some of my contributions.

VANE and MULHEARN: Was the monetary approach actively discussed by faculty staff and students in workshops during your time at Chicago (1966–71)?

MUNDELL: I wouldn’t put it that way. The workshops were always addressed to the topic of the day, given nearly always by visitors. You could say that I brought the idea of the “monetary approach” to the University of Chicago, and that got the theme accepted. My paper on the monetary approach was written in 1963, while I was at the IMF, and presented at a Fund seminar; and a later version of this was presented at a conference at the World Bank in 1965 organized by J. H. Alder, and published in his book of proceedings in 1967 and then reprinted in Johnson and Frenkel (1976).2 Oddly enough, it came in for substantial criticism at the IMF, even though Jacques Polak, Director of Research at the IMF, had a valid claim to be one of the modern originators of the monetary approach, with some articles he published on money and the balance of payments in IMF Staff Papers.3 But the monetary approach had its origins in the classical literature and would not have been news to David Hume or David Ricardo, although some important elements were missing in the Hume and Ricardo analyses.

Some people think that the paper I wrote while at the IMF is my most important paper, as it threw away the Keynesian system and went back to the classical framework. But I think that paper can also be interpreted as integrating the two systems. I added money to the classical barter model, and that is why the title of Chapter 8 is “Barter Theory and the Monetary Mechanism of Adjustment” (Mundell, 1968b). But it showed explicitly how the quantity theory could be related directly to income and expenditure and the balance of payments. Between Polak’s work in the 1950s and my general equilibrium approach, there was also an important article published in the Staff Papers by S. J. Prais (1961) who made an important contribution introducing explicitly an expenditure function that had been missing from, or was at best implicit in, Hume’s analysis.

Getting back to Chicago, all the students at Chicago read my paper on “Barter Theory and the Monetary Mechanism of Adjustment.” I also gave a paper to a general student audience called “Gold and the Gulliver Problem,” emphasizing the importance for the monetary system of a giant economy like the United States. In that unpublished paper, I outlined the dynamics of growth and the balance of payments with the equations that were later widely used in expositions of the

2 Also Mundell (1968b).
3 For example, see Polak (1957) and Polak and Boissonneault (1960).
monetary approach. Harry Johnson read it and liked it immensely, scribbling in the margin of my draft with the growth of domestic assets and the money supply, the words, “You should publish this!”

Harry Johnson’s main contribution to the monetary approach was, as I see it, apart from exposition, not what he did after my work, but rather a short article he published in the *Pakistan Economic Journal* in 1958, in which he noted that if the government did nothing, the balance of payments would correct itself. This idea would not have been new to Hume, but it was new to most of the generation that learned their economics after World War II.

VANE and MULHEARN: Did you anticipate the impact that your work in this area would have?

MUNDELL: Yes, to some extent. I think my experience at the IMF led me to anticipate its relevance for every country on fixed exchange rates. But while I knew its importance, I did not think of it as spectacularly new. Today I think of it in the same way as I do about supply-side economics, which I had a hand in developing. It was not new to the field of economics, but it was new to the postwar Keynesian generation. No one who read the classical literature could presume that the idea of the monetary approach was new. It was rather a question of bringing back an old idea that was valid.

I first applied the monetary approach to an actual policy situation in 1962 or 1963 when the IMF asked me to help the Middle East Department determine how much Tunisia should devalue the dinar. Instead of calculating the elasticities of demand for dates, grapes, and other exports of Tunisia along the lines of the “elasticity approach to the balance of payments,” I gave an answer in terms of the monetary gap, which I thought of as the devaluation that would be required to eliminate the excess demand for money. I have to mention that Polak at that time thought that I should nevertheless do the elasticities calculation on the grounds that otherwise no one would believe the result.

The monetary approach wouldn’t have been considered novel by David Hume or David Ricardo, although the latter might have been surprised and appreciated some of the corrections and refinements. The idea of the monetary mechanism had been thrown out of the window by the Keynesian revolution. Keynes himself seemed to have the idea that the balance of trade was something almost exogenous to the system, and this defect, as I would claim it is, recurs in his writings and in his persistent and curious reluctance to accept income effects into the transfer problem analysis.4 Notwithstanding this “block” on the part of Keynes, it is easy enough to take a Keynesian model, open it up to the rest of the world and show how the monetary approach works within the framework of Keynesian assumptions. This is exactly what I did in my “The International Disequilibrium System” 1961 paper, published in *Kyklos*, and reprinted in my book *International Economics* (1968b).

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4 The transfer problem, which refers to the relationship between international payments and the real exchange rate, is a classic question in international trade. The opening exchange was a series of articles by John Maynard Keynes and Bertil Ohlin in the March, June, and September 1929 issues of the *Economic Journal*.
VANE and MULHEARN: Turning to your 1961 American Economic Review paper (1961c) on optimum currency areas, Andrew Rose (2000) has described the concept as one of your greatest triumphs. What inspired you to consider the then-radical and far-sighted question of when it would be advantageous for a number of regions to relinquish their monetary sovereignty in favour of a common currency? Did your Canadian background act as a catalyst to this chain of thought?

MUNDELL: I don’t know whether it’s useful to seek the origins of ideas in national backgrounds. It’s like saying that monotheism was born of the desert! Tennyson has Ulysses say: “All experience is an arch wherethrough gleams that untravell’d world, whose margin fades forever and forever when I move.” Plato advocated an overvalued currency with exchange controls, perhaps leading his pupils to unsettling inflationary policies as in Syracuse; while his pupil, Aristotle, reacted against the inflation of his times and became a hard-money gold-standard man. Is that a Greek experience? Famous Canadian economists like Jacob Viner, Kenneth Galbraith, and Harry Johnson did not think in terms of optimum currency areas, so what makes it a “national” idea?

Having said that, of course my Canadian background was part of my experience. Canada is, like the United States and Australia and Russia and Brazil and China, a “multi-regional” economy, and Canada did have a flexible exchange rate in the 1950s. The example I used in my 1961 article on “Optimum Currency Areas” was North America divided into East–West regions and North–South countries. I think it is interesting to search for the intellectual origins of ideas. Certainly one major stimulus was the political innovation of the Treaty of Rome establishing the common market in Europe in 1957. As I mentioned earlier, I was studying in England in 1955–6 at the London School of Economics and my wonderful adviser, James Meade, who was an advocate of flexible exchange rates, wrote a paper for the Three Banks Review in which he argued that the six common market countries each had to have balance-of-payments equilibrium and could achieve this best by having flexible exchange rates (Meade, 1955a). Although I greatly admired Meade, not just for his superlative economics but also for his modesty and kindness, I thought it very strange that countries that were bent on integrating their economies should move to disintegrate their monetary systems. This got me thinking about the problem. When I came to the University of Chicago the following year in 1956 as a postdoctoral fellow, I talked to Milton Friedman about this issue. If the flexible exchange rate theory is valid at all, it should apply to different regions in the United States. It should apply to Oklahoma as well as Illinois. I asked Milton what he thought about that. He said that he basically agreed that exchange rate adjustment might help those areas but that over time adjustment could be made without changes in exchange rates.

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5 An “optimal currency area” refers to the geographical area over which it is optimal in terms of efficiency and output to have a single or common currency, rather than separate or—for example, in the case of the European Union—national currencies.
After my postdoctoral fellowship at Chicago, I went back to Canada as an instructor to the University of British Columbia for the year 1956–7. At that time Canada had a flexible exchange rate with the United States, but I quickly noticed that it was no use for British Columbia! If the argument for flexible exchange rates were correct, British Columbia should have had a separate currency. I made a model of the idea and gave a faculty seminar on the subject to my colleagues at British Columbia, identifying the optimum currency area (from the standpoint of separating zones of fixed and flexible exchange rates) as the region. My Canadian background came through! After my year at UBC, I went to Stanford University and presented a much more comprehensive paper, of which the section on optimum currency areas was only a part.

VANE and MULHEARN: Was your paper on optimum currency areas accepted for publication when first submitted?

MUNDELL: There was at first no single paper on optimum currency areas. Most of the ideas and analysis of my QJE (1960a), Kyklos (1961b), the Canadian Journal of Economics and Political Science (1961a), and AER (1961c) articles were put all together in the paper I gave at Stanford in the academic year 1959–60. The ideas came in a rush, and I put them all in a single paper. In retrospect, how foolish! I submitted the paper to the Economic Journal for publication, where (later Sir) Roy Harrod was editor. He rejected it, mentioning in passing that I could find some of the ideas in his International Economics (1958). Naturally, I was disappointed, but in retrospect he did me a great favor. Much better to make use of Tinbergen’s idea in economic policy that you should have the same number of instruments of policy as targets for policy. Applied to publication, it is: one idea, one paper. There were too many ideas in the paper. I eventually published four papers from that article, one of which was “The Theory of Optimum Currency Areas.” It was rejected first by Economica and then published by the AER (1961c).

VANE and MULHEARN: Your analysis emphasises the importance of a high degree of labor mobility to ensure full employment when one particular region experiences a disturbance. Surprisingly, given Europe’s relatively inflexible labor market, you have been a strong supporter of European monetary union. Is your support for a common currency largely based on the advantages of a shared monetary discipline that the euro would bring?

MUNDELL: That is part of it, but not all. Half of my criterion for the optimum currency area, based on the argument for flexible exchange rates, was the zone of factor mobility. I was connecting it with the classical literature. Ricardo’s reason for a separate theory of international trade was that factors of production are mobile inside a country but immobile between countries. If the world fitted Ricardo’s assumptions, and a flexible exchange rate was needed to achieve real wage flexibility, each country needed to have a separate currency in order to achieve the goals of stabilization policy.

In practice, factors of production were often mobile between countries and immobile within (especially multiregional) economies. Capital was becoming highly mobile and labor was mobile along certain circuits. John H. Williams of
Harvard University had back in the 1920s challenged the assumptions of international trade theory from the standpoint of the real economy (Williams, 1929), and I was doing the same from the standpoint of the monetary economy. The nation-state is not the optimum currency area. Is it the region? Assuming that flexible exchange rates could restore the real wage flexibility that rigid nominal wages inhibited, the optimum currency area is the region—provided that full employment is the only goal of economic policy and provided that the functions of money are being taken care of. The qualifications are important, both with respect to the definition of region and with respect to the ability of flexible exchange rates to achieve real wage flexibility. The region itself is not always black and white. And the theory that flexible exchange rates can restore real wage flexibility puts too much faith in money illusion and ignores the fact that currency devaluation cannot change relative real wage rates within a country.

The first half of my paper made the argument for currency areas being smaller than nations; the second half of my paper outlined the case for making them larger. The smaller the currency area and the larger the number of currencies in the world, the more ineffective currencies are in overcoming the disadvantages of barter. If the number of currencies in the world equals the number of commodities, the gains from money completely disappear. The only reason today that flexible exchange rates are not an unmitigated disaster for the world community is that most of the world gets to use a quasi-global currency like the dollar or its new rival the euro as a de facto international money. No economist of any repute would ever advocate flexible exchange rates in a world filled with small countries and no alternative global money—whether gold, dollars, or euros—to fill the vacuum.

The optimum currency area argument has been used both for and against the creation of the euro. People who object to the euro point to labor immobility in Europe. But in fact Europe has just as much labor mobility as it wants. The European Commission sends money to depressed regions so labor won’t have to emigrate. The fact is there are strong arguments for making currency areas large and these dominate the case for making them small.

In the last half of my paper I argued that money illusion—the favorite assumption of flexible-rate advocates—was itself a function of the size of the currency area. We can grant that after long periods of stability, countries can devalue and become more competitive because labor swallows the reduction in real wages caused by higher prices. This was very much the case in the 1930s when Keynes popularized the money-illusion argument and there was mass unemployment. But since the late 1930s, prices have gone ever upward. The money-illusion argument for flexible exchange rates has faded away. Policymakers have learned that “surprise devaluation cum inflation” only works for a short period, and is often reversed. Once the money-illusion argument is taken away, the case for devaluation disappears. The only valid argument for flexible exchange rates is that with it, a country can have monetary independence and can choose its own rate of inflation. Any sustainable zone of fixed exchange rates implies a common rate of inflation. If there were
consensus on that common rate of inflation and who should manage the global money, the optimum currency area would be the world.

VANE and MULHEARN: Rudiger Dornbusch (2000) has suggested that, to quote, your “view of the case for a European money was in the end predominantly political and has nothing to do with the celebrated optimum currency argument.” Is this a fair comment?

MUNDELL: Rudiger Dornbusch was one of my favourite students and his untimely death was a tragedy for international economics. He was always a breath of fresh air in economic discussions. He was also sometimes an intellectual “bomb-thrower,” by which I mean that he would take sides on a politically sensitive position and make a mountain out of a partial truth. So let’s start with the partial truth.

Since their origins, currencies have had a political component. A single currency in Europe could not survive without considerable political consensus and integration. The Treaty of Maastricht (1991) established the goals of both monetary union and political union for Europe. Monetary union came first because it was easier, but greater political integration—notwithstanding the setbacks in 2005—is needed. There were strong economic reasons for the euro before German unification, but the latter created a political urgency that made the euro a necessity. The rest of Europe was afraid that Germany’s tendency to dominate Europe would be restored with German unification. In return for consent to German unification, Chancellor Kohl committed unified Germany to monetary and political union in Europe—moves that would tie Germany peacefully into Europe’s future. The countries that entered into the euro area, as well as making themselves better off, made a contribution to future European political stability. It would, of course, have been better if Britain also had joined. The same political urgency did not exist back in 1969 when I made the first plan for a European currency (published as Mundell, 1973). The arguments then were primarily economic, not political, although the political advantages were present then too. So let’s just say that Rudiger Dornbusch was about one-third right. After all, if the Europe of the 1930s had a democratically-run common currency in the 1930s, there would have been no World War II.

In passing, I made a bet with Rudiger on the issue of a European currency. Our bet was made in 1992 at the Bank of Italy Conference in Perugia, in memory of Rinaldo Ossola, a much-admired monetary economist and official. I bet a bottle of fine wine that Europe would have its single currency, and he bet it would not. Like many of his MIT colleagues, he believed it would not happen. My own view of course is that European unification is not just better for Europe but also for the United States and the rest of the world as a whole.

VANE and MULHEARN: Many regard you as the father of the euro. How do you react to this label?

MUNDELL: Yes, it is true that some people have called me that. Initially when many people didn’t like the idea of the euro and thought that it would fail, they were glad to call me its father. Then it became a success, and that brings up the saying, “Failure is an orphan, success has many fathers.” Two years ago, at a Committee of Thirty meeting at the IMF annual meeting, [Jean-Claude] Trichet,
the President of the European Central Bank, said quite explicitly that I was the father of the euro.6

I am willing to wear the label if we agree that the euro has many fathers. I made what I believe was the first plan for a European currency in 1969, which was widely circulated in the European Community and I advised the Commission of alternative approaches to it the following year and in 1973.

VANE and MULHEARN: What do you think are the medium-term prospects for the euro?

MUNDELL: I think they are too good for Europe’s sake. What I mean is that the euro is likely to be stronger, on average, than the Europeans should want it to be, mainly because of the potential of the dollar for weakness. I deplored the descent of the euro below $0.85 and I recommended in September 2000 that they put a floor to the euro around that level so that they would have a precedent for putting a ceiling on it when it went above $1.15.

Barring some completely unexpected political calamity, a war, or a serious global disturbance—I think the euro is in fine shape. I wrote in 1998, before the euro came into being, that a strong currency needs the backing of a strong central state (Mundell, 1998). Europe isn’t that yet. But I also pointed out that the weakness of Europe as a strong central state is partly mitigated by the fact that Europe is part of the NATO alliance, and as long as that endures the euro can flourish if Europe continues to make progress toward political integration.

The Development of International Economics

VANE and MULHEARN: Aside from your own contributions, which books or papers do you feel have had the biggest impact on the development of international economics in the postwar period?

MUNDELL: James Meade’s (1951, 1955b) two books on The Theory of International Economic Policy, one on “The Balance of Payments” and the other on “Trade and Welfare,” both with important mathematical supplements, are classics. Also Samuelson’s (1948, 1949) two papers on factor price equalization and his two papers on the transfer problem (Samuelson, 1939; Stolper and Samuelson, 1941). On the monetary side, I would count Dornbusch’s (1976) overshooting model as an important development. That model showed that flexible exchange rates lead to systematic overshooting, and it should have been the funeral of flexible exchange rates as a system. But people never picked that up as a criticism of flexible exchange rates.

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6 Background on the Committee of Thirty, also called the Group of Thirty, is available at the website (http://www.group30.org/home.php). Here’s a short overview: “The Group of Thirty, established in 1978, is a private, nonprofit, international body composed of very senior representatives of the private and public sectors and academia. It aims to deepen understanding of international economic and financial issues, to explore the international repercussions of decisions taken in the public and private sectors, and to examine the choices available to market practitioners and policymakers.”
VANE and MULHEARN: For your Nobel Memorial lecture, you choose as your subject area “A Reconsideration of the 20th Century” (Mundell, 2000). How important do you think knowledge and interpretation of historical events are for understanding the problems presently associated with the international monetary system?

MUNDELL: Extraordinarily important. The great economists of the past had a superlative knowledge of history: Hume, Smith, Ricardo, Cournot, the Mills, Marshall, Pareto, Keynes, Schumpeter, Mises, Samuelson, and so on. One of the reasons why Keynes was such a successful economist in policy analysis was because he was both a mathematician and an avid student of history—political, philosophical, intellectual, and monetary history. We apply models today in forms that can be applied back to past economic situations, adjusting for the differences in institutions. Currency theory fits issues today as well as 3,000 years ago. Valid models have no half-life. Keynes talked about what makes a good economist and wrote long essays on Jevons and Marshall (Keynes, 1936b, 1924). He had the mind of an accountant, was a good theorist, and was a speculator in ideas. In the 1920s he spent a lot of time thinking about and writing on ancient monetary history, as I have. But his work was never published at the time of writing and has only appeared in his Collected Writings.

VANE and MULHEARN: Where do you think the direction of research in international economics is most likely to go in the next five to ten years?

MUNDELL: I think too much attention has been put on tricky little models in microeconomics, and not enough along the main course of general equilibrium theory. We need bolder theories that try to capture the new reality of international competition in a globalized world and particularly theories to improve the current state of the literature on monetary and exchange rate analysis. I don’t think economists have made much progress on predicting exchange rates over the past three decades.

Supply-Side Economics and the International Monetary System

VANE and MULHEARN: In the 1970s you switched the focus of your work to policy issues, in particular supply-side economics and the international monetary system. Your 1971 paper on “The Dollar and the Policy Mix” (1971a) in which you advocated tax cuts to stimulate the supply side and tight money to prevent inflation, was at the time highly controversial. How well received were your views in academic circles?

MUNDELL: The paper came out as a paper in the Princeton International Finance Series, which is a prestigious publication but definitely limited in circulation. It created discussion in policy circles, but it was a challenge to the conventional wisdom and therefore—because it was not looked upon as a threat—ignored. I never saw any refutations of my basic idea, although it received considerable discussion at conferences. The 1971 paper generalized somewhat my
1962 *IMF Staff Paper*. In the latter paper, I argued for a policy mix, under fixed exchange rates, of tight money and tax cuts. The 1971 paper applied that policy mix to flexible exchange rates. It had a big influence later on in supply-side circles, which really began in the United States in 1974 when I came to Columbia University.

VANE and MULHEARN: Supply-side economics is closely associated with Arthur Laffer, who acted as an economic adviser to President Reagan during his first administration (1981–4). Was Laffer one of your students?

MUNDELL: Arthur Laffer was never formally a student of mine in the sense that he took classes from me. He was a postgraduate student completing his Ph.D. from Stanford University when he came to the Business School at Chicago as an assistant professor. He regularly attended the Johnson–Mundell workshop in international economics at Chicago, and we became good friends. Unlike most graduate students in economics, he was a conservative and even at that early time an admirer of Ronald Reagan, then Governor of California. One of the big policy issues in the late 1960s, and for the incoming Nixon administration, was how to stop the inflation without creating a recession. In the summer of 1968, the Johnson administration had imposed a 10 percent surtax and money was kept easy. With Nixon’s election, I argued that the new Nixon administration should shift the policy mix to tight money with a tax cut, partly to reverse the recessionary effects of the 1968 tax surcharge. Arnold Harberger, Chairman of the Department, headed the Nixon task force on economic policy after the election, and both that report and Milton Friedman came out against any tax cut. The *Chicago Tribune* made a big deal of this split within “Chicago.” Laffer was on my side of the issue.

Supply-side economics had its first victory in Canada. I made a speech at the Ottawa Economics Club making the case for indexing the tax rates for inflation. I believed this to be important in an inflationary environment because otherwise inflation would increase the progressivity of the tax system by pushing taxpayers into higher income-tax brackets, even if real income stayed the same. Canada did so the following year and was the first of the industrialized countries to do so. This idea represented a new principle in tax theory. The basic purpose of the tax change was not to reduce the budget surplus but to reduce disincentives to production and employment. Then in June 1974, when I was moving to my new position at Columbia University, I was invited by David Meiselman and Arthur Laffer to present a paper at a conference on inflation organized by the American Enterprise Institute in Washington, D.C. (Mundell, 1975).

This date—still the spring of 1974—was just after the oil embargo in the Middle East and the four-fold increase in oil prices. Now an oil price increase in an oil-importing country is something like an increase in tax rates, where the proceeds of the tax are sent to oil producers abroad. This shock was going to cause a recession. Although my formal paper was directed at inflation, in the discussion I proposed a $15 billion tax cut. That proposal really was the first wedge in what became the supply-side program for tax reductions. After the seminar, Jude Wanniski, at the recommendation of Arthur Laffer, came to see me and we talked for
hours about economic policy. He was then a journalist for the Wall Street Journal and had learned a lot about economics from Arthur Laffer. In the next few years in New York, Jude would take ideas he learned from me and Laffer and promote them energetically and skilfully in the columns and editorials of the Wall Street Journal and in other outlets.7 Robert Bartley, the editor of the editorial page and later editor of the Wall Street Journal, would take part in our meetings at Michael One restaurant in the Wall Street area and play devil’s advocate. Bartley soon became a convert to the new ideas and brought in Jack Kemp, Congressman from Buffalo, who parlayed the idea in the political domain.

Getting back to 1974, the recession that was unfolding in the spring of 1974 was getting worse, and in September I argued for a $30 billion tax cut, a number that would have to increase if the relief measure were delayed. To show how far removed this was from both conservative and Keynesian liberal thinking at the time, look at what leaders of the economics profession were saying. President Gerald Ford, who had assumed the presidency after Nixon’s abdication, saw inflation as the main danger and had spread his WIN (for “Whip Inflation Now”) buttons around the administration. In September, he called a bipartisan “summit” meeting of top economists to get their advice (or gain their agreement) on a tax increase. They made fools of themselves by calling for a 5 percent tax increase! By October, the decision proved to be an embarrassment. The greatest recession since the 1930s was unfolding. In October, President Ford backed away from his tax increase, but said he was not going to reverse himself 180 degrees and have a tax cut. From completely wrong, the decision had become half right. A 90-degree reversal from a disastrous policy was better than nothing!

Ford did accept the idea of a tax cut in the following spring, but it didn’t have any supply-side components. It gave people money they could spend to buy up unwanted inventories, but it provided no incentive for further production or investment. This episode crystallized the nature of the supply-side revolution. Prior to this period, Keynesian economists proposed tax cuts or increases in government spending to increase aggregate demand and “prime-pump” the economy. It was myself and Arthur Laffer, with assists from Jude Wanniski and Robert Bartley, who led the big shift in thinking away from this paradigm, from Keynesian deficits to supply-side incentives for increased production and investment. Producers would benefit from the tax cut in proportion to production and employment.

The relation of tax cuts to the budget deficit became a prime issue. According to conventional theory, cuts in tax rates reduced government revenue. But supply-side economics raised the point—already known to some of the classical economists but long since forgotten—that there are normally two tax rates that give the same revenue. For example, a zero tax and a prohibitive tax both generate zero revenue. A small tax and a slightly less-than-prohibitive tax can also produce the same revenue. Only at the tax—that-maximizes-revenue point is there a single tax. Wan-

7 See, for example, Wanniski (1978a, 1978b).
niski coined the phrase “Laffer curve” for the dual-valued relationship between tax rates and government revenues, and beat that phrase into national consciousness in several articles. It provided a strong rallying cry and selling point and gave Arthur Laffer the publicity that identified his name with supply-side economics. However, no one who knows the history of supply-side economics would say that I played a lesser role than he in the development of the new theory.

VANE and MULHEARN: It is clear that your enthusiasm for tax cuts helped found supply-side economics. What do you feel remains of the supply-side revolution today?

MUNDELL: Tax cuts were the most important part of supply-side economics but also important were issues of deregulation. I do not expect supply-side economics to disappear but rather to be around forever, receding when its job has been done and coming forth again when it becomes necessary. Much of the supply-side program remains in the tax structure that we have today. Many economists oppose tax cuts for political reasons because they see the tax system as an engine for distributing income from the rich to the poor. The redistribution can be achieved either through spending programs or direct subsidies. Most people accept the need for concern about the distribution of income and redistributive taxes. After all, even proportional or “flat” taxes mean that the rich pay more than the poor. But the main issue for economics was how far redistributive tax rates can be used without seriously impeding production, investment, and growth. Economists have to be concerned not just with how the economic pie is distributed but also how large the pie is. Supply-side economics made the argument that steeply progressive tax rates reduced the size of the pie to be distributed. The poor might be better off with a smaller share of a larger pie than with a larger share of a small pie.

Supply-siders captured the White House when, during the competition for the Republican nomination in 1980, Jack Kemp withdrew from the contest in exchange for Reagan’s adoption of the supply-side program, epitomized by the Kemp–Roth Bill which called for a 30 percent cut in all tax rates. The steepness of the progressive income tax structure is hinted at by the level of the tax rate in the highest income-tax brackets. At the time of the Reagan tax cuts, personal income tax rates were 70 percent at the federal level alone. The 1981 Economic Recovery Act brought the top tax rate down to 50 percent and the second round of the tax cuts in 1986 slashed it to a stunning 28 percent. But this last rate did not last long. Under President George H. W. Bush, it was raised to 33 percent and then further raised in the Clinton–Gore administration to 39.6 percent. More recently, in the administrations of President George W. Bush, who is the second supply-sider to occupy the White House, the top tax rate was lowered to its present level of about 35 percent. The top tax rate is half what it was under Reagan’s predecessors, so much of the supply-side revolution remains. Moreover, it has travelled abroad. Most of the rest of the world has imitated those tax cuts, and the movement that began in Canada and the United States has spread worldwide.

VANE and MULHEARN: In the 1970s, along with Robert Triffin, you were at the forefront of debate about the best way to restructure the international monetary system in order to address such key questions at the time as confidence,
liquidity, and adjustment (Mundell, 1972). In retrospect what lessons can we learn from the debate that took place?

MUNDELL: In 1944 the whole idea was to go back to a fixed exchange rate system, and that was accomplished. The postwar system was a continuation of the pre-war structure, with major currencies tied to the dollar and the U.S. fixing the price of gold. There was general consensus that this was a workable and good system—except that it had a flaw that doomed it. There was no mechanism for keeping the U.S. price level from getting out of line with the fixed price of gold. With the inflations of three wars, prices rose and gold became undervalued, leading to a gold shortage and a crisis that eventually brought down the system in 1971.

By the 1960s, with the system in trouble, some economists had begun to advocate flexible exchange rates, including such distinguished figures as Milton Friedman, James Meade, Harry Johnson, and Gottfried Haberler. The prime motive was to give independence to monetary policy in the U.S. and Britain. After the system had broken down, we moved by default to flexible exchange rates.

I'm not sure that we've learned the right lessons from that period. It is probably fair to say that the majority of people have bought the arguments that flexible exchange rates are a free market solution and better than any alternatives. I disagree with that. If flexible exchange rates have proved to be acceptable, it is only because countries have been able to rely upon a fairly stable dollar (and now the euro) as measuring rods for their own units of account. If there were no dominant economy in the world, flexible exchange rates would be an unmitigated disaster.

I regard flexible exchange rates as an expression of monetary nationalism. It is true that with a flexible exchange rate a country gets monetary independence. It is free to set its own inflation rate where it wants to. Thus, after the former Soviet Union countries shifted from communism to capitalism, they created their own currencies and, following the advice of the international institutions, adopted flexible exchange rates. This let them have whatever inflation their money-financed fiscal deficits produced, and without exception they had hyperinflation. Ten years after the transition, measured GDP was lower than it was under communism.

My own preference is for monetary internationalism, where we use an international monetary system for the benefit of the world economy. This equalizes the playing field between big and small, rich and poor countries. Even a small country can gain monetary stability by fixing its currency to a large stable neighbouring country. The world as we have it is biased against the small and poor countries. The United States does not feel any pressure to restore the fixed-exchange rate international monetary system because all of its massive internal trade and most of its external trade is conducted in terms of the dollar.

At the very time when the U.S. was pushing the world into flexible exchange rates, the civilized countries of Europe were moving in the opposite direction, by creating the European Monetary System (to replace the IMF) and moving toward fixed exchange rates, culminating in the apotheosis of fixed exchange rates, the
single currency called the euro. Other nations have seen what a great success this is for Europe and are imitating it in other regions of the world. The regions are trying to make up for the disaster created by the center.

VANE and MULHEARN: In your Nobel Memorial lecture, you concluded that “the international monetary system depends on the power configuration of the countries that make it up” (Mundell, 2000). Today we can observe three major currency blocks in the world: the U.S. dollar area, the euro area, and the Japanese yen area. Medium-term misalignments between these three major currencies seem to be a serious problem. If you had carte blanche, what would you think would be the appropriate thing to do about such potential misalignments?

MUNDELL: As an interim solution, I would create an international unit, which I call the DEY, for dollar, euro and yen, and use a basket of these three currencies as the building blocks for a reformed international monetary system. I would also not oppose adding to this trio the Chinese yuan and the British pound, making it a five-currency basket. Using that as a base, I would then incorporate into the administrative structure some other major countries and create a global unit of account in which all countries have a share. I call this unit, designed to replace the Special Drawing Rights (SDRs) of the IMF, the INTOR.

VANE and MULHEARN: Do you think that this will ever happen?

MUNDELL: I believe that the basic idea (as opposed to the specific plan) will come to pass. Just as the world needs a common language as a medium of communication, so it needs an international unit of account as a medium of exchange between countries. You should realize that throughout most of history, the world has had a universal unit of account. Two thousand years ago, in the days of Caesar Augustus there was the *aureus*. A thousand years ago there was the gold *bezant*. A hundred years ago there was a unit of gold. Fifty years ago there was the 1944 gold dollar. Today there is nothing. The absence of an official global unit of account in the world today and since 1971 amounts to what I consider an international monetary crime. As Paul Volcker has frequently said, the global economy needs a global currency (for example, Volcker, 2000). Why have globalization in everything except money?

**Personal Reflections**

VANE and MULHEARN: Your papers have had a profound impact on the direction of research in international economics. What do you consider to be your most important contributions to economics?

MUNDELL: The Nobel Prize Committee focused on two of my contributions—the creation of the mainstream international macroeconomics and the theory of optimum currency areas. Dominick Salvatore (2000) wrote an article after I was awarded the Nobel Prize, in which he said “three brilliant ideas, one Nobel Prize.” The Nobel Prize Committee didn’t mention the third idea, supply-side economics, which I agree was too hot to handle. I could also mention a fourth area
with my work in monetary theory and the theory of inflation (Mundell, 1963b; 1965; 1971b). I think I should leave the answer to your question to posterity.

VANE and MULHEARN: Your faculty colleagues during your period at the University of Chicago (1966–71) included some very prominent and influential economists. Names like Milton Friedman, George Stigler, Ted Schultz, Harry Johnson, and Al Harberger immediately spring to mind. That must have been a very exciting and creative environment in which to work.

MUNDELL: Yes, it was a very stimulating intellectual environment. You should mention several others, among whom I would include Hirofumi Uzawa, the gifted mathematical economist; Robert Fogel, the economic historian; Frank Knight, a revered figure from the past generation who I learned a lot from; and also Lloyd Metzler, one of the most creative economic theorists in the world. Some people think of the last half of the 1960s as the Golden Age of University of Chicago economics, comparable to Vienna a hundred years ago or Cambridge, England, in the first half of the twentieth century.

The workshop system was a great way of getting groups together. What was exciting in that period was that economics was a passion with almost everyone. We talked shop all the time, not gossip. Milton [Friedman], of course, was the best talker! Every Wednesday we had faculty lunch, attended by most everybody. Nobody wanted to be left out. Milton deserves credit here as, not only a good economist, but as the provocateur! I had a close relationship with Harry Johnson in the workshops and we read each other’s papers. Harry had a theory about Milton, that he would read something in the newspapers, take a position on it, try it out on [his wife] Rose at breakfast, and then launch it full-flown at our faculty lunches. I think that at this time the other department which was most like Chicago was MIT. At MIT, people looked up to Paul Samuelson. I had an offer from MIT around 1966, and I was tempted; but my family had settled in, and the Chicago bug was already biting.

VANE and MULHEARN: Over the course of your career you have acted as an advisor to numerous international organizations and governments including the United Nations, the World Bank, the European Commission, the Federal Reserve Board, the U.S. Treasury, and governments in Europe and Latin America. To what extent has this work been motivated by a desire on your part to influence the policy agenda?

MUNDELL: Certainly it played a part. But my choice of things to do has also been influenced by my curiosity at meeting interesting new people, looking at and trying to solve new problems, and improving my own economics. Most recently I have had a very strong interest in China—ever since I spent four months teaching in the Ford Foundation graduate program at People’s University of China in Beijing in 1995; unfortunately that excellent program ended at that time. After I was awarded the Nobel Memorial Prize, I came to China more and more and played a role in discussions about economic policy, particularly in connection with the yuan/dollar exchange rate policy.

VANE and MULHEARN: This consultancy work has been paralleled by academic posts held at a number of universities. Since 1974 you have been Professor of Economics at Columbia University. Do you still teach at Columbia?
MUNDELL: Yes, I have a full teaching load and I still enjoy teaching.

VANE and MULHEARN: We went to a session yesterday where Bob Solow said that whenever he taught a course he would destroy his notes at the end of the year and start again afresh.

MUNDELL: What a loss for economics! His descendants would love to see Bob Solow’s notes, to see how his ideas evolved and changed. I’m against note-burning—at least other people’s notes. We know very little about Adam Smith’s life because he himself, and after his death his executors, burnt most of his notes and letters. The same happened to Alfred Marshall, an act of arson in which Keynes participated.

VANE and MULHEARN: What importance do you attach to having been awarded the Nobel Memorial Prize in Economics?

MUNDELL: It’s recognition of the importance of my work by the profession. Although the decisions about Nobel Prizes are made by a small committee of Swedes, they rely heavily on nominations and the opinions of the profession at large. This recognition was particularly pleasing to me as my work has been quite controversial and no doubt stepped on a lot of intellectual toes.

It’s also been very important from a practical standpoint because it created access to people I would not have otherwise met and gave me new and more important platforms to express my ideas. Instead of ministers of finance, I now as often as not meet heads of state. Even more than that, when I say something, people listen. Maybe they shouldn’t, but they do.

VANE and MULHEARN: Two final questions. We have read that you are an accomplished and enthusiastic painter. Portraits or landscapes?

MUNDELL: I started with landscapes and portraits, but once I did my first abstract, I never looked back. I have maybe 200–300 paintings that I have put in storage. As I look back, they are not nearly as good as I would like them to be.

VANE and MULHEARN: Finally, we have read that you bought a twelfth-century castle in Santa Colomba, Italy, in the late 1960s as a hedge against inflation.

MUNDELL: I bought it because I wanted to live in it; but it is true that I realized that it would be a good investment against inflation. I bought it in 1969 during the last phase of the dollar standard, and the end of system and probable inflation was already being signalled by the free price of gold.

The castle was built in the thirteenth century, but it got razed to the ground by the English condottiere (land-pirate) Sir John Hawkwood, a veteran of the Battles of Crecy and Poitiers, in 1364. A renaissance villa was then built on top of the ruined castle, and two big wings were added at the back toward the end of the eighteenth century. I bought the “castle” in 1969, the same year I made the plan for a euro, and I’ve been restoring its 65 rooms ever since, little by little. I had some conferences there in the 1970s and 1980s, and in the past few years I’ve been holding an annual conference on the main issue of international monetary reform: restoring the international monetary system and creating a world currency.

VANE and MULHEARN: Thank you for a very interesting and enjoyable interview.
References


