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On the History of the Mundell-Fleming Model

Keynote Speech

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It is a great pleasure for me to speak at this opening of the first IMF Annual Conference on International Macroeconomics and a special honor to be here at the inauguration of the annual Mundell-Fleming Lecture. Quite apart from the flattering distinction it confers on Marcus Fleming and me, it serves to commemorate a very special period in the Research Department at the Fund when, under the able leadership of Jacques Polak, it made an enduring contribution to the development of the standard international macroeconomic model.

I think the best service I could provide tonight would be to give some of the background behind the development of the model, some of the influences that were important, and, yes, some of the defects of the model. If what I have to say seems too autobiographical, I can only respond that I wish Marcus Fleming could have been here to fill in the blanks from his point of view and redress the balance.

I

I have read interpretations of my work that have made stylistic points about the "early" and the "late" Mundell, the first being a Keynesian, the second a classicist. Such periods may be relevant to painters, but are they really applicable to economists? I am not myself aware of any basic shift of direction. I did write on different subjects and use different models at different points in time, but why not? I worked on what came to be called the Mundell-Fleming model mainly over the years 1960–64, but before, after, and during this period, I was also publishing my work on the pure theory of trade, monetary theory, optimum currency areas, the public debt, the monetary approach to the balance of payments, customs unions, and the theory of inflation. The agenda, models, and information changed, but the periodization doesn't ring true.

If there was an "early" Mundell, it was a classical one. Let me start as close to the beginning as seems necessary. After graduating from the University of British Columbia (UBC) in 1953, I went as a teaching fellow and graduate student to the University of Washington, where I had my first real brush with macroeconomics, mathematical economics, and international trade. My next stop was the Massachusetts Institute of Technology (MIT) in 1954–55, where I was of course especially influenced by Samuelson and Kindleberger. After completing my doctorate exams at MIT in the spring of 1955, I made use of a Mackenzie King Traveling Scholarship from Canada to study at the London School of Economics (LSE). I had a special interest in Lionel (later Lord) Robbins and (later Sir) James Meade.

I got a nice letter of acceptance directly from Meade, and he agreed to "supervise" my thesis for MIT up until March 1956, when he was to leave for New Zealand. I want to discuss my relations with Meade. I saw him in his office about once a week, and also participated in, besides the Robbins theory seminar, the Meade-Robson (Robson was a political scientist) seminar on international economics, as well as lectures by Harry Johnson, who came up from Cambridge once a week to give a course in which he read—yes, *read*—his latest papers. In those two terms I wrote two papers, "Transport Costs in International Trade Theory" (Mundell, 1957b), and "International Trade and Factor Mobility" (Mundell, 1957a), which were two of five chapters of my MIT Ph.D. thesis. The latter article I presented in the Meade-Robson seminar, and I got helpful comments on it from Tadeusz Rybczynski, Dick Lipsey, Max Corden, and Steve Ozga, as well as James Meade and Harry Johnson. Throughout that year and the following summer in Boston, my work was entirely on aspects of the classical or Heckscher-Ohlin theory of trade, and I had no discussions about macroeconomics with Meade or anyone else.

I had "read" Meade's (1951b) *Mathematical Supplement.* In June 1998 Max Corden stayed with me in Siena a few days, and reminded me of a conversation we had at the time. When asked whether I had read Meade's (1951a) *Balance of Payments,* I replied, "No, but I have read his *Mathematical Supplement.*" This gave me the reputation (along with the prestige of coming from MIT), quite unmerited, that I was a mathematician. I never told anyone that when I began graduate work, I had zero knowledge of even rudimentary calculus. But my reply to Corden was not quite accurate. One didn't read the *Mathematical Supplement.* It was almost as tedious as the main book. What was exasperating was the taxonomy, roundly criticized by Harry Johnson in his review.[1] Meade has a very amusing footnote on combinations at the bottom of page 33, where he contributes the information, confirmed by William Baumol, that there were precisely 28,781,143,379 possible solutions to his model.

[1]This negative, even harsh, review of Meade's book cost Harry Johnson a friendship with Lionel Robbins, who was tenaciously loyal to his friends, and who only agreed to speak to Harry again on the occasion of Al Harberger's wedding in London in 1958.

Much later, in 1970, during a walk in the foothills of Mount Fuji, Meade told me that he had a mind like Pigou's—a "meat-grinder's mind," he said. He told a story about Pigou on his way out after a lecture being asked by a student if he had not made an error in the sign of an elasticity, at which point Pigou marched back up to the podium to his notes (presumably left for his assistant to return), looked up the relevant section, and simply replied "no." Meade said that he wrote down the equations, differentiated them, and reported the results in his book. It wasn't very exciting, but his two volumes and their appendices were nevertheless landmarks in the development of international economic theory.[2]

I learned a lot from Meade, of course—not macroeconomics, but his brilliant contributions to the classical model. This influence can be seen all through my "Pure Theory of International Trade" article (Mundell, 1960b), which was an expansion (and contraction) of two of the five chapters of my thesis. When you asked a question like "How much will a tariff, or unilateral transfer, or productivity change alter the terms of trade (or some other variable)?," you would find that Meade had produced the first definitive answer to that question. I was able to develop his work in some new areas, develop some of the dynamics, and generalize the model, following up on Mosak, in a multicountry framework.

There are nevertheless in Meade's *Mathematical Supplement* (1951b) the equations of an international macroeconomic model. But when I was doing my work on this subject a few years later, I didn't make any connection with Meade's work. The reason, I think, is that my approach came through a Walrasian-like general equilibrium theory, which was at best only implicit in Meade's analysis. But there was one insight in Meade's work that I used extensively, and this came through in my "Pure Theory . . ." article. This was the role of "domestic expenditure," called "absorption" in Sydney Alexander's 1952 article in the IMF's *Staff Papers*. Of course Metzler and Machlup had used expenditure functions depending on income in their international multiplier work, Laursen and Metzler (1950) had made them dependent on income and the real exchange rate in their famous joint article in the *Review of Economics and Statistics*, and Chipman, Goodwin, and Metzler had used them in their treatments of the matrix multiplier. But Meade's equations in the *Mathematical Supplement* broke new ground by making domestic expenditure a function of income, interest rates, exchange rates, some prices, and all kinds of policy variables. He did not develop the implications of this emphasis.

In his introduction to the *Mathematical Supplement*, Meade says he hopes his "model may somewhat further . . . the marriage between the 'classical' and 'Keynesian' analysis of the mechanism of the balance of payments. . . . What we need for balance-of-payments theory is a marriage of the Keynesian and the Hicksian type of analysis; and our model constitutes such an attempt." I think that does explain what he attempted to accomplish, and I think he was partly successful in doing so. It was *not*, however, what I was trying to do in my international macroeconomic model.

[2]You have to understand Meade's remark in the context of his own innate, self-effacing modesty. You wouldn't want to take too literally John Stuart Mill's statement in his *Autobiography* that he wasn't smarter than his contemporaries, only that he started a generation ahead of them.

Meade had been, since 1950, an ardent advocate of flexible exchange rates, and it was a still a hot subject at LSE. He had suggested that the signers of the Treaty of Rome achieve balance of payments equilibrium for each country by letting exchange rates float. I didn't have a position on this at the time but could not see why countries that were in the process of integrating with a common market should saddle themselves with a new barrier to trade in the form of uncertainty about exchange rates, or how economic theory could prove that flexible rates were preferable to fixed rates or a single currency.

II

My interest in macroeconomics in that year, 1955–56, in London was very much beneath the surface, as I was writing a thesis that was entirely a development of the classical and Heckshér-Ohlin models. I spent the following year, 1956–57, as the post-doctoral fellow in political economy at the University of Chicago, and here I became especially interested in the work of Lloyd Metzler in theory and Milton Friedman in policy. Metzler's (1951) architectonic "Wealth, Savings, and the Rate of Interest" started me thinking about that model as a more suitable paradigm for macroeconomics than the Keynesian model. By 1955, Patinkin's work had appeared and the Metzler-Patinkin general equilibrium approach to the closed macroeconomy provided a more classical full-employment counterpart to the standard IS-LM framework.

It was around this time that I moved on from writing about the pure classical model and started to think about the way to write down the general equilibrium equations for an open economy taking into account monetary variables, exchange rates, and capital movements. The facts that Canada had a flexible exchange rate and capital flows between Canada and the United States were significant background influences, but there was absolutely no model that was capable of dealing with the subject. I had a few fruitful conversations with Metzler that year that were important. His powers were much reduced after his brain surgery, however, and I remain convinced that had he remained healthy, he would have pioneered the international macroeconomic model.

After Chicago, I returned to UBC for the year 1957–58. It was here that I presented the first discussion of "Optimum Currency Areas" (Mundell, 1961c) at a faculty seminar. That explains the North American flavor of the article. At the same time I wrote an expository piece for a government publication on macroeconomic developments in Canada, and this exercise led me into putting together the basic equilibrium equations for the open-economy macroeconomic model with capital mobility. I was still thinking along these lines when I left UBC for Stanford for the year 1958–59.

It was at Stanford that the model really came together. I was teaching the graduate course in international economics and taught my new equations in it; Jeffrey Williamson probably remembers that class. Equally important was a faculty seminar, attended by Bernard Haley (editor of the *American Economic Review*), Ken Arrow, Lorie Tarshis, Ed Shaw, Melvyn Reder, and also Tibor Scitovsky and Abba Lerner who had come up from Berkeley. I had titled the talk "A Theory of Optimum Currency Areas," but most of it was the Mundell-Fleming model, and it made a big hit. Afterward, Lerner chided me for not talking enough about optimum currency areas, but I was able to give him the gist of the basic argument in a few minutes after the seminar.

All these ideas were put in a single paper, tied together by the general equilibrium link. It included not only optimum currency areas but also much of the comparative statics of the *Kyklos* (Mundell, 1961b) and *Canadian Journal* (Mundell, 1961a) papers, and some of the macrodynamics that became my *Quarterly Journal of Economics* article (Mundell, 1960a). I sent it to the *Economic Journal* and was disappointed when (later Sir Roy) Harrod rejected it—partly on the grounds that I hadn't referred to his treatment of some of the subjects. But the rejection turned out to be a blessing in disguise. It led to a much more sensible separation of the article into different parts, to become "Monetary Dynamics of International Adjustment Under Fixed and Flexible Exchange Rates" (Mundell, 1960a), "Optimum Currency Areas" paper (Mundell, 1961c), article on "Flexible Exchange Rates and Employment Policy" (Mundell, 1961a), and the *Kyklos* paper, "The International Disequilibrium System" (Mundell, 1961b). Ever since, I have suggested to students and colleagues the merits of a variant of Tinbergen's Rule: one idea, one paper.

Later, when I became friends with Harrod, I teased him about his rejection of my paper, and he explained that he had been going through a very stressful situation at the *Journal*, sorting out a controversy between Harry Johnson and Don Patinkin over the definition of real marginal cost. He gave up the editorship soon after.

It is necessary now to distinguish between two strains of my models. What is called the "Mundell-Fleming model" is usually taken to refer to that group of articles that includes Mundell, 1961b, 1961a, 1962, and 1963a—that is, Chapters 15, 17, 16, and 8, of my *International Economics* (Mundell, 1968), including the appendix to Chapter 8, which was published in the *Canadian Journal* (Mundell, 1964). Also relevant is my article in the *Banca Nazionale del Lavoro Quarterly Review*, "The Nature of Policy Choice" (Mundell, 1963c). This article was, I think, the first fully developed global empirical model of the world economy in a Keynesian framework, a precursor of the forecasting models used by professional forecasting companies like Otto Eckstein's Data Resources and Laurence Klein's WEFA.[3] One of the few references I've seen to this article is by Egon Sohmen in his paper "The Assignment Problem" in the Mundell-Swoboda book (Sohmen, 1969, pp. 183 and 186). These articles, usually thought about as the Mundell "half" of the Mundell-Fleming model, are more or less in the tradition of the internationalized IS-LM model. It could also be thought of as an international multiplier model generalized to incorporate the securities and money markets.

III

When I first heard the expression "Mundell-Fleming model," I supposed it included all my papers on international macroeconomics, including the first one. It took me some time before I realized that some economists did not count the model in "The Monetary Dynamics of International Adjustment Under Fixed and

[3]Originally, WEFA was an acronym for Wharton Econometrics Forecasting Associates.

Flexible Exchange Rates" (Mundell, 1960a, ch. 11), as part of the Mundell-Fleming model. Yet in some respects this first in the series was the most important and set the methodology for the others.

Its purpose was to find a way to analyze the difference between an economy with fixed exchange rates and flexible prices, and an economy with flexible exchange rates with fixed prices. I needed a coherent and plausible international macroeconomic model that was consistent with a full-employment economy. There was no such model in the literature. The paper introduced an internal balance schedule for an open economy and a foreign balance schedule (for the first time in the literature). The variables were the interest rate (representing monetary policy) and the real exchange rate (or the relative prices of home and foreign goods). The comparative statics of the model could show the effects of expenditure changes on interest rates and the relative prices. The two schedules demarcated four zones of disequilibrium, and this made possible an examination of the dynamics relevant to two different policy situations: an economy in which monetary policy was directed at fixing the exchange rate, compared with an economy in which monetary policy was directed at price-level stabilization—in modern language, the choice between exchange rate and inflation targeting.

To me this formulation—the diagram with the *FF* and *XX* curves in a plane depicting the rate of interest on one axis and the real exchange rate (or some other relative price) on the other—fits the world of today better than the variable output versions. Of course it has to be updated to make a distinction between nominal and real interest rates, growth curves along the lines depicted in my *Monetary Theory* (Mundell, 1971) and a more explicit treatment of the relation between capital movements and domestic expenditure to produce Ohlin-type transfer effects.

The model found a new application for economic dynamics. Meade, who was at heart a Marshallian, had not been concerned at all with dynamics. There were, of course, precedents in the dynamics. Samuelson had formulated the dynamics of the Walrasian system, and Lange, Metzler, Arrow, and others had added more theorems on its dynamic stability. Laursen and Metzler (1950) had analyzed flexible exchange rates, including a dynamic appendix, in the context of a multiplier model; Hicks had developed dynamics of trade theory. Metzler (1951) had an appendix on dynamics in his "Wealth, Savings and the Rate of Interest." Patinkin had followed in Metzler's footsteps. And Polak had analyzed some dynamics of an international general equilibrium model. But theorems about dynamic stability had not before been used to settle the choice between economic policy alternatives, and that was one of the novelties of my paper.

When I started writing it, I had no idea what conclusions would emerge. I didn't make the model to elucidate or make appealing to the reader conclusions I had already reached by other means. I used the model as an engine of discovery. I wanted to find out what the mathematical dynamics of the model could teach me. To differentiate the dynamics of fixed and flexible rates, I used the same static model for both. The comparative statics of fixed and flexible exchange systems were essentially the same. But what about the dynamics? At first I thought that the different dynamics of the two systems (fixed and flexible rates) didn't really matter much.

From the diagrammatic analysis, it was apparent that the business cycle sequences were inverted. But why should that matter?

Nevertheless, as a good student of Samuelson, I routinely derived the stability conditions for the two systems. It turned out that, under normal assumptions, both systems were stable. But that was not the end of it. It was with great excitement—and I remember the very moment on that Sunday afternoon in November 1958 in my Menlo Park apartment, just a month before the birth of my first son—that I noticed that while the stability conditions for fixed and flexible exchange rates were both satisfied, they were different. In particular, the terms under the discriminant determining whether the roots were real or imaginary were *different*. They could be positive or negative, giving rise to either asymptoticity or cyclicity in the path to equilibrium, depending on the sizes of some parameters or slopes. There suddenly spread before me now a whole new world of implications including the "principle of effective market classification." I was so taken with the idea—elated might be a better word—that I put pencil and paper down, to prolong the enjoyment of the suspense about what would, with a little more work, unfold.

One implication of the model was that a domestic boom (shift up and right of the *XX* curve) would raise interest rates, attract capital inflows, appreciate the real exchange rate, and worsen the balance of trade, a conclusion that would hold under either fixed or flexible exchange rates. This was very relevant to an understanding of the economy of Canada, which was the only major country with a flexible exchange rate in the 1950s, and of course later very relevant for understanding the Reagan boom in the early 1980s and the German unification boom in the context of the exchange rate mechanism crisis in the early 1990s. Under the old Keynesian model, which typically assumed capital immobility, it was generally assumed that domestic expansion would weaken the currency.

After the article appeared, I had a nice letter from Harry Johnson, saying something to the effect that it carried the subject to a different level.

IV

In 1959–61, I taught at the Johns Hopkins School of Advanced International Studies Bologna Center, where I finalized several articles for publication: "The Pure Theory of International Trade" (Mundell, 1960b), "Optimum Currency Areas" (Mundell, 1961c), the *Kyklos* article (Mundell, 1961b), and the *Canadian Journal* article (Mundell, 1961a). I spent two years in Bologna and thought it was time to get back into the mainstream. The offer from the International Monetary Fund was particularly appealing. When I came to the Fund in September 1961, Marcus Fleming, chief of the Special Studies Division in the Research Department, was away, and Jacques Polak, head of the department, suggested that I work on a problem that had come up in economic policy circles in the United States. There was a great debate going in the U.S. government about the use of monetary and fiscal policy, with different approaches suggested by the Chamber of Commerce, the Council of Economic Advisers, and the Keynesians. The Keynesians wanted expansionary monetary and fiscal policies; the Chamber of Commerce wanted tight monetary and fiscal policies; and the Council of Economic Advisers (CEA), strongly influenced by Paul Samuelson (President Kennedy's first choice as chairman of the CEA) and James Tobin, a member of the CEA, wanted to use monetary and fiscal policy in different directions, with low interest rates to spur growth and a budget surplus to siphon off the excess liquidity. The theory behind the policy mix was called the Samuelson-Tobin "neoclassical synthesis."

When Polak asked me to work on this problem, I replied, "But I already solved that problem in my *Kyklos* article." Polak replied that "not enough people have got the message" and that I should try again.

So I took up what was essentially a selling job. The problem was to make the case succinctly, and I hit on the idea of using the two equations representing policy goals—internal and external balance—in target space, with monetary policy on one axis and fiscal policy on the other. Thus was born "The Appropriate Use of Monetary and Fiscal Policy for Internal and External Stability" (Mundell, 1962). I wrote it in a week, and it was on Marcus Fleming's desk when he returned from his vacation.

David Meiselman, then working in the Office of the Comptroller of the Currency, came over to the Fund to introduce himself and asked what I had been working on. I told him and he asked me what I thought of what I had written. I said that I felt like Bizet after he had written the Toreador Song to Carmen: "If it's trash they want, I'll give it to them!"

Fleming approved the paper, and it circulated as a departmental memorandum, which meant that it went to the governments of all the member countries, but most important, of course, to the U.S. government. It was an immediate candidate for publication in the IMF's *Staff Papers*, but it created quite a fuss. All kinds of objections to it were made: It was "contrary to U.S. policy," it would have a "bad influence on developing countries," there was "no difference between monetary and fiscal policy," the "use of monetary and fiscal policy in opposite directions would cancel out," and so on. Graeme Dorrance, on the editorial board, told me he was initially against it for *Staff Papers*, but when he heard the other objections, he changed his mind. What saved it for *Staff Papers* was that the editorial board couldn't reach agreement on reasons for rejecting it.

The article provided a new way of thinking about macroeconomic policy. At first it wasn't popular. This was to be expected because it recommended a complete reversal in the prevailing policy mix. The Samuelson-Tobin neoclassical synthesis might have had some merits in a closed economy, but it was completely indefensible in an open economy on fixed exchange rates.

Fortunately for the United States (and me), President Kennedy reversed the policy mix to that of tax cuts to spur growth in combination with tight money to protect the balance of payments. The result was the longest expansion ever (up to that time) in the history of the U.S. economy, unmatched until the Reagan expansion of the 1980s.

Meanwhile, however, the Federal Reserve Board of Governors had mounted an attack on my paper. Herbert Furth (Gottfried Haberler's brother-in-law) and Robert Solomon wrote a sharp critique. Instead of answering it point by point, I wrote the *Canadian Journal* paper (Mundell, 1963b) that is usually cited as the locus classicus of my half of the Mundell-Fleming model.

In my IMF paper, monetary policy had a comparative advantage in correcting the balance of payments. The critical assumption was that capital flows were responsive to interest rates. I decided to reply to the Federal Reserve critique by upping the ante, assuming complete capital mobility. This made the opposite policy mix even more absurd, because it showed that under fixed rates and perfect capital mobility, monetary policy was completely impotent. Open market operations to buy Treasuries would result in equivalent gold losses or buildup of dollar balances. The paper was presented at the spring meetings of the Canadian Economic and Political Science Association in Quebec, and published in the November 1963 issue of the *Canadian Journal* (Mundell, 1963b). This is the article that, as I said, has been so frequently reproduced and is usually cited in the Mundell-Fleming literature. A critical comment on it published the following year provoked me into extending the model to the two-country global context.

V

Meanwhile, Marcus Fleming had been writing his paper, “Domestic Financial Policies Under Fixed and Flexible Exchange Rates,” published in *Staff Papers* (Fleming, 1962). This article was later published again in his collected papers on international economics, just following a paper written in 1958 on “Exchange Depreciation, Financial Policy and the Domestic Price Level” (Fleming, 1958). The 1958 paper is entirely in the Bickerdike-Robinson-Metzler-Meade tradition and shows no traces of what came to be called international macroeconomics. But his 1962 paper is an almost fully mature international macroeconomic model, and this constitutes Fleming’s contribution to the Mundell-Fleming model.

The question arises as to the relation between the two models. He had probably been working on his model before I arrived at the Fund, and of course my papers owed nothing to his. He had certainly read my *Quarterly Journal of Economics* (Mundell, 1960a), *Kyklos* (Mundell, 1961b), and *Canadian Journal* (Mundell, 1961a) papers, as well as the paper on the policy mix (Mundell, 1962) that I wrote at the Fund and that he approved. When he was putting the finishing touches on his own paper in the spring of 1962, he asked me which of my articles I thought he should refer to. I said, why not them all? But he said, “No, I am only going to refer to one of them.” That’s exactly what he did. Curiously, he chose the least relevant article to his or my topic—my 1961 “Employment Policy and Flexible Exchange Rates” (Mundell, 1961a). Even more curiously, he repeated the reference to this paper alone years later when he published his article on “Wider Exchange Margins” as Chapter 13 in his collection, *Essays in International Economics* (Fleming, 1971). What must have been going through his mind to single out that paper (which showed that commercial policy was ineffective or counterproductive under flexible exchange rates but no capital mobility) as the most relevant of my papers on monetary and fiscal policy?

There is a difference between our articles that gets Marcus into trouble. On the second page of his article, he examines the effect of an expansionary shift in fiscal policy in the form of an increase in public expenditure under (1) fixed and (2) flexible exchange rates. The increase in expenditure leads, he says, to a deterioration in the current account. Then he writes: "In order to isolate the effect of a change in budgetary policy, it is necessary to assume that monetary policy remains, in some sense, unchanged. In this essay, that is taken to mean that the stock of money is held constant. . . ." But this assumption is not consistent with fixed exchange rates. As I showed in my *Kyklos* paper (Mundell, 1961b), sterilization policy is incompatible with fixed exchange rates and leads to a "disequilibrium system."

Here is the problem. With a stock of money constant, the increase in government expenditure will increase interest rates, which will check expenditure and lead to an increased net capital inflow. While the trade balance worsens, the capital account improves, and this means that the balance of payments may improve or worsen depending on certain coefficients (in my framework, it will worsen or improve depending on whether the *LL* curve has a flatter or steeper slope than the *FF* curve). Fleming now has to conclude with ". . . if the policy of budgetary expansion results in a deterioration of the balance of payments, shortage of reserves may ultimately lead the authorities to abandon the policy and to renounce the associated expansion in income and employment." His system has no mechanism of adjustment for the balance of payments.

In my earliest works on the model I identified monetary policy with interest rate policy. That was certainly true in my *Canadian Journal* paper (Mundell, 1961a) and probably explains why Marcus chose that paper to refer to. It makes a starker contrast between our models. Later, however, when I made the assumption of perfect capital mobility, monetary policy had to be redefined and was correctly treated as an open market operation, or a change in domestic credit. The money supply is an endogenous variable under fixed exchange rates.

In my *Kyklos* paper (Mundell, 1961b), I showed that the balance of payments can be kept in disequilibrium under fixed exchange rates only if automatic effects of reserve changes on the money supply are sterilized, a temporary solution. Had Fleming used constant domestic assets (no open market operations) as the criterion of a constant monetary policy, he would have been able to complete his analysis of the effects of an increase in government expenditure.

VI

I am not quite sure when the term "Mundell-Fleming model" first appeared in the literature. At a conference in March 1997 in Claremont, California, I was objecting to the use of the misleading term "Marshall-Lerner condition," a term that originated with Charles Kindleberger. The relevant Marshall here is the writer of the *Pure Theory of Foreign Trade* (Marshall, 1879), and Lerner refers to the *Economics of Control* (Lerner, 1944). Marshall had, of course, died (1924) several years before Lerner became an economist (early 1930s), and their themes were quite different. Marshall was talking about changes in relative prices (the terms of trade), while Lerner was talking about the exchange rate. Marshall would have been absolutely horrified at the connection, when he took such careful pains to distinguish between the terms of trade and the exchange rate and to reject any hint of a connection between the stability of his barter model (based on Mill) and the stability of exchange rates. He explicitly made clear that the reader should not confuse the exchange rate with the terms of trade.

Max Corden then asked me why, if I objected to that connection, did I object to the name "Fleming-Mundell" model rather than "Mundell-Fleming model." I pointed out what I have said above—that his work, if not dependent on mine, at least followed mine, whereas mine was completely independent of his. He had read my earlier papers. That was one of the reasons he wanted me to come to his division in the Fund.

I am not suggesting Fleming's work wasn't in an important sense independent of mine. It was certainly to a large extent subjectively (to use Schumpeter's phrase) original. You can see a connection in his model to a paper he wrote on macroeconomics in the late 1930s, analyzing a closed economy in a quasi-general equilibrium framework. The problem was already "in the air" at the Fund, and it was natural that he would have tried his hand at solving it when it had become such a bone of contention in the United States. The assumptions, style, and notation are characteristic of Fleming. The notation is completely anti-mnemonic.

Marcus Fleming was a gifted and original economist. He was a "purist" in many senses. Sometimes this trait, combined with his integrity, would get in the way. When he was working at the U.K. Treasury in the 1940s, he was aghast, Lionel Robbins told me, to find that the government was accepting the Treasury's recommendations for the wrong reasons. He would rather be right than president.

He could be exasperating to people in his division. A couple of stories, called up from the far recesses of the mind, can be mentioned. I used to go into the office quite early and stay late, partly to avoid the rush hour. But for an hour or two after lunch I was not to be seen. I was at the nearby Washington Athletic Club. Long after I left the Fund, Ann Romanis told me that Marcus would frequently come to see me after lunch and get in a frightful stew when I was not to be found. At the same time, Ann would come into my office tearing her hair after an intensive discussion with Marcus, usually about "incomes policy."

Despite his predisposition for precision, Marcus was a Keynesian. In the spring of 1963, I presented my "return to the classics" paper, "Barter Theory and the Monetary Mechanism of Adjustment" (Mundell, 1963a) at a Fund seminar. This paper would later start a kind of Mundell-Dornbusch literature. It was then that Fleming made his humorous comment that the only two Keynesians left at the Fund were himself and the managing director (Per Jacobbsen). Marcus was best at developing and refining fine points and details in abstract theory rather than in the rough-and-tumble and necessarily inexact world of forging new systems.

He really disliked that paper, and in his written comments on it, he penciled in "lament for economics." It never saw the light of day as a Fund paper, and Marcus had the chance to critique it in detail (but unsuccessfully) when he was its discussant at the 1965 World Bank Conference where I first presented it outside the Fund. It is interesting to note that the literature that came from that paper thus also originated at the Fund, as did my earliest *Journal of Political Economy* papers on inflation theory.

There was no Mundell-Fleming paper. We never collaborated on macroeconomics. But there is a Fleming-Mundell paper, "Official Intervention on the

Forward Exchange Market," published in *Staff Papers* (Fleming and Mundell, 1964). Marcus wrote the first draft of this paper and it was his idea to treat the forward market as a stock, rather than a flow, market. It's a great idea, and it's a pity the article has been somewhat neglected. I developed the diagrams and the explanations. In the exchanges between us, relating to our two-country framework, I replaced his "*A*" and "non-*A*" with "*A*" and "*B*." We went through this exchange a couple of rounds, but he had the last word. That was my first and (almost) last experience with collaboration.

I am proud of the fact that our names will be linked together in the Mundell-Fleming Lecture to commemorate a very exciting and fruitful period of interaction and collaboration in the Fund.

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In statistical matter throughout this issue,

dots (. . .) indicate that the data are not available;

a dash (—) indicates that the figure is zero or less than half the final digit shown, or that the item does not exist;

a single dot (.) indicates decimals;

a comma (,) separates thousands and millions;

“billion” means a thousand million; and “trillion” means a thousand billion;

a short dash (–) is used between years or months (for example, 1998–99 or January–June) to indicate a total of the years or months inclusive of the beginning and ending years or months;

a slash (/) is used between years (for example, 1998/99) to indicate a fiscal year or a crop year; and

components of tables may not add to totals shown because of rounding.

The term “country,” as used in this publication, may not refer to a territorial entity that is a state as understood by international law and practice; the term may also cover some territorial entities that are not states but for which statistical data are maintained and provided internationally on a separate and independent basis.

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