

My Work on International Monetary Problems, 1940-1964

This is the continuation of an essay published in 1980 in the pages of this *Review* under the title "My Early Work on International Monetary Problems". That essay surveyed those of my publications during the years 1923 to 1940 that dealt with international monetary economics. A list of its subheadings may offer a convenient synopsis of its contents: The Gold-Exchange Standard (1923, 1925), The New Currencies of Europe (1927), The Transfer Problem (1928 and 1930), Flight of Capital (1932), Official Memoranda and Articles in Newspapers (1931-1934), The Theory of Foreign Exchange (1939-1940), and finally, A Few Observations, in which I announced that there was more to come.

In this second installment I shall cover the years 1940 to 1964. The break between this second and the third installment, to be published later, is not quite clean: I am leaving for the third some of my activities that began in 1963, particularly the institution of the Bellagio Group and the consultants' meetings in the U.S. Treasury Department. My accounts of the proceedings and consultations in these groups would go on to 1977, and thus cannot be fitted into the space allotted to the present article.

Eight Questions on Gold (1940-1941)

At the annual meeting of the American Economic Association in December 1940 Hans Neisser and Charles O. Hardy presented papers on the past and current gold policy of the United States. I was invited to be one of the discussion speakers. My comments under the title "Eight Questions on Gold" were published in the volume of proceedings in

February 1941.¹ Since I agreed with what Neisser and Hardy had presented, I chose to formulate eight questions and to answer them in a deliberately provocative way. I shall restate the eight answers, hoping that it will be clear what the questions were:

1. Raising the price of gold in 1934, from \$ 20.67 to \$ 35.00 per ounce, was a mistake; the chief motives were fallacious, some other objectives could have been achieved by better methods, and some indirect effects were quite undesirable (p. 31).

2. Reducing the price of gold at this time (1940) would be a mistake; it would not be beneficial to the United States and would definitely harm the British, who were financing their purchases of airplanes and other urgently needed war materiel through sales of gold (p. 32).

3. The large purchases of gold by the United States during recent years (1934-1940) did not involve heavy real costs for the United States; considering the large unemployment and excess capacity, the opportunity costs of the exports sold in exchange for the gold were rather low (p. 33).

4. "The United States need not be much concerned that other nations after the war may repudiate gold as long as the United States itself does not repudiate it" (p. 34).

5. "Unless gold production becomes as exuberant as silver production once did, it is not likely that gold will lose value in terms of foreign currencies while we maintain its dollar value.... The commodity value of gold is much more a function of government spending and credit expansion than a function of the gold supply" (p. 35).

6. To readmit gold into domestic circulation by minting new gold coins would not be a good idea, though it would do no harm either; but there is "no advantage in our committing ourselves to a definitive gold price at this moment when everything is in flux and nobody knows how the world will look in a few months" (p. 36).

¹ "Eight Questions on Gold: A Review", *American Economic Review*, Proceedings, Volume 30 (February 1941), pp. 30-37. Reprinted in my book *International Payments, Debts and Gold* (New York: Scribners, 1964; second edition, New York: New York University Press, 1976), Chapter X, pp. 228-238; also in FRITZ MACHLUP, *International Monetary Economics* (London: Allen and Unwin, 1966); Japanese translation in 1973.

7. The idea to use the large gold stocks of the Treasury to reduce the government debt is quite silly, quite apart from the fact that gold certificates have been issued against the gold; the former owners of government securities would not want to hold the proceeds in gold but would immediately resell it to the government in exchange for dollars in order to purchase earning assets; to print paper money to redeem the government debt would be no more inflationary, and much simpler, than using gold for this purpose (p. 37).

8. Possession of the large gold reserve is not, and will hardly ever be, an advantage to the United States; the other countries will need loans after the war, but will hardly have enough exports to buy back the gold (p. 37).

I was wrong on the eighth question. The gold holdings of the United States increased still further after 1940, and on balance no gold was used (lost) until 1949. But from then on, gold was gradually redistributed among several (chiefly European) countries, and after 1960 the United States engaged in all sorts of policies designed to avoid losing more of its gold at a fast rate. I had qualified my answer in 1940 by saying that "in a serious inflation [in the United States] fast rising prices might create and permit an import surplus and in payment for ... [it], as well as in a flight of capital, we might lose a portion of our gold stock". But I added that these were "rather improbable possibilities" (p. 37). In this I was mistaken.

The Foreign-Trade Multiplier (1941-1943)

In 1939 I had started to work on multiplier analysis and made a polite bow to the "leakage" from the investment-generated income flow due to larger imports induced by the increased income.² I then turned to the analysis of export-generated incomes. In other words, from the analysis of the home-investment multiplier I proceeded to the foreign-trade multiplier; the resulting multiple reflected, besides the positive income-generating effects of autonomous increases in exports, the

² "Period Analysis and Multiplier Theory", *Quarterly Journal of Economics*, Volume 54 (November 1939), pp. 21-22; reprinted in Gottfried Haberler, ed., *Readings in Business Cycle Theory* (Philadelphia: Blakiston, 1944), Ch.10, pp. 203-234; translated into Spanish and German.

negative effects of income-induced increases in imports and the positive or negative effects of the changes in exports induced by income changes abroad. I lectured on these problems in 1941 and published a book, *International Trade and the National Income Multiplier*, in 1943 with the Blakiston Company.³

As I said in the preface of the book,

"I have tried to improve the multiplier technique by discarding the idea of the instantaneous or timeless multiplier and introducing time as an important variable. I believe that period analysis, showing the step-by-step adjustments of incomes, imports and exports as sequences in time is best adapted to the ... purposes. And ... [it] constitutes really 'dynamic economics'" (p. vii).

The book contained thirty tables of arithmetic illustrations of various model sequences, besides simple algebraic expositions of the models. (The algebra was indispensable, not an exercise in sophistication.) I considered the primitive arithmetic sequences necessary for students' full understanding of the models and, after all these years, I still do. Students who have gone only through the algebra rarely develop the intuitive comprehension of the significance of some of the relations involved, especially of the lapse of time in processes approaching "income equilibrium" at the level of the "full" multiplier, which could be reached only after an infinite length of time (p. 49). In order to reduce the full multiplier to a somewhat less impossible construct, I provided formulas for the computation of time-specific multipliers ("the multiplier as a function of time") and a tabulation showing "the time necessary for the income increase to reach 90% of the full multiple" (p. 51). [One purpose was, of course, to disabuse students from the erroneous belief that it could ever be possible that nothing else would change on the way approaching equilibrium and hence, that the "neighborhood of equilibrium" could actually be reached.]

The arithmetic sequences disclosed something that algebraic formulas of the full-multiplier values might have left concealed: the existence of waves in the series of income increases and of changes in trade balances as adjustment proceeds from period to period. Indeed, I found that, where such "wavelike movements in the changes in income occur, the respective figures may once pass through the ten-percent neighborhood of equilibrium and then leave this zone again in order to return there only on the way back" (p. 89). These waves had puzzled me

³ I had become economic editor of the Blakiston Company and developed for it a series of economic monographs, texts, and volumes of readings. After a take-over of the company most of its books were republished under other imprints.

at first sight, but then I found algebraic and verbal explanations that gave me the satisfaction of a discoverer, although the matter was relatively simple. [The interested reader can find the answer on pp. 83-84 of the book.]

Most of the problems analysed in the book related to the effects which certain changes, occurring at home or abroad, would over time have upon national income and the trade balance. The disequilibrating changes could be divided into four major groups: (1) autonomous changes in exports or imports (where "autonomous" stands for "not induced by changes in income"), (2) changes in exports induced by changes in incomes abroad, (3) opposite changes in investments (which stands for "primary disbursements") at home and abroad, and (4) parallel changes in investments at home and abroad. Included in the third and fourth of these groups are problems of international movements of capital and unilateral payments, which comprise the income effects (in contradistinction to the price effects) of the mechanism involved in the transfer problem.

Just as the multiplier mechanism implies that changes in foreign trade may be regarded as either autonomous or induced, changes in capital movements may likewise be disequilibrating or equilibrating; the former pair of adjectives, however, would be inappropriate in connection with capital movements because it was defined as independent of or induced by changes in *income*. I proposed to distinguish "spontaneous" movements of capital from "accommodating" ones. For example, every system of fixed exchange rates "implies the preparedness to accommodate the exporters [of merchandise], who offer foreign claims or balances, by taking these assets off their hands and holding them for the time being ... Behind this foreign lending is *not a spontaneous demand for foreign claims or securities* but rather a latent preparedness to accommodate those who come to dispose of foreign balances". Not that the accommodation is "done out of sheer kindness or as a service to customers All except the monetary authorities engage in these transactions strictly for business reasons", in the expectation of gaining through slight differentials between foreign and domestic interest rates or between spot and forward exchange rates, or through small changes in spot rates etc. The accommodating buyers and sellers of foreign exchange from and to exporters and importers may be banks, dealers, speculators, and monetary authorities (pp. 134-135).

The newly coined term — accommodating capital flows — won wide acceptance, particularly after James Meade adopted it for his book *Balance of Payments*. It served me well in the analysis of the old controversy whether changes in the trade balance caused changes in the capital ba-

lance, or the other way around. I was able to show that the chain of causation could not be seen at all unless "net capital movements" were divided into spontaneous and accommodating ones (pp. 139-143).

The last chapter of my book was given to "Apologies and Confessions". I confessed that the theory of the multiplier was only in most exceptional circumstances applicable to real-world situations. And I apologized for having offered detailed models and exact formulas for inapplicable theories. To be sure, the "fundamental" assumptions of most theories are counterfactual and surely unrealistic, but as a rule the weakening of such assumptions and the recognition of their "as if" function does not make the inferences from the theories completely irrelevant. In the case of multiplier theory, however, the conclusions deduced from the assumptions prove inapplicable to most historical situations. The chief merit of its careful exposition and examination is that in the process one learns why the theory rarely applies.

I remember with a mixture of pride and embarrassment the praise which Jacob Viner (who preceded me as Walker Professor of Economics and International Finance at Princeton University) gave to that last chapter of my book. For he added sarcastically: "If you had placed this chapter at the beginning rather than at the end of the book, the readers would learn that they do not have to spend their time going through the rest of the book". It was precisely because I had realized this possibility that I deferred the denouement until the end of the book. For I firmly believe that even theories without practical significance should be fully understood; indeed, only such understanding helps us see that these theories cannot be accepted in support of policy recommendations (p. 218).

We may never "have the information enabling us to substitute real numbers in our formulas"; we may have no ground for assuming that any of the variables, even if we knew the value it had yesterday, would have the same value tomorrow; we may be convinced that none of the other necessary conditions holds in reality; still the formulas serve a purpose.

"Their purpose is to exhibit certain relationships between independent and dependent variables, to show whether they are positively or negatively correlated, to tell whether it is their magnitudes or their proportions which matter, to indicate which ones are more important and which less, and, last but not least, to warn us about the things which we are to find out before we try to make general statements, not to speak of predictions" (pp. 199-200).

I may, to keep this account of my work more personal, insert that I spent the years from 1942 to 1946 in Washington, the first year at the

Brookings Institution, the other three years in a Government position. In 1947 I left the University of Buffalo to accept a professorship at the Johns Hopkins University in Baltimore. The next 13 years at Hopkins were the years of my greatest productivity as a teacher. I had the most extraordinary group of graduate students working for the Ph. D. degree, attracted by high stipends and a unique program of instruction and research.

Dollar Shortage, So-Called (1949-1954)

From the end of the 1940's to the early 1960's the discussion of international monetary problems was plagued by a misunderstanding, widespread among official experts and journalists, and aided and abetted by some respected economists: the idea of dollar shortage in nondollar countries. What ailed these countries were two conditions: one, they were trying to maintain the foreign-exchange rates of their currencies at levels that overvalued them relative to domestic prices and incomes, partly in consequence of excessive expansions of credit; and second, with capital stocks depleted in the war and postwar period, they had not yet been able to build up adequate monetary reserves. The first problem was a problem of the "flow supply" of foreign exchange, inadequate because they were paying too little for it; the other was a problem of the "stock supply" of foreign exchange, inadequate because it takes time to accumulate reserves even after the inflow has responded to appropriate adjustment measures. The dollar-shortage theorists, however, like the balance-of-payments theorists after the first world war, did not understand that monetary policy and disaligned exchange rates were the strategic variables. They looked for some "structural" explanations of a supposedly "chronic" dollar shortage. With these two adjectives, structural and chronic, they meant to dismiss the relevance of monetary factors in the situation.

The situation was similar to that after the first world war, and the same theoretical fallacies reappeared. Thus, my attempts to clear up the misunderstandings were, in a way, repeat performances after 25 years, though perhaps on a somewhat higher level of theoretical craftsmanship, reflecting the advances of economic theory and the greater competence of the exponents of the erroneous view. I wrote three articles on the subject; two were published in 1950 and the third in 1954.

The first article examined the arguments of those who opposed the judgement that higher prices for foreign currencies would reduce or eliminate the excess demand for, or the shortage of, foreign exchange. Many of these arguments rested on the belief that a depreciation or devaluation of the currency would not lead to an improvement of the trade balance, because the price elasticities of supply and demand in international trade were too low. I coined the term "elasticity pessimism" to denote this position, and titled my article "Elasticity Pessimism in International Trade".⁴ The term has been widely adopted.

"Moderate elasticity pessimism pervades [the thinking of] those who warn of miserable terms of trade or an intolerable distribution of income. Hopeless elasticity pessimism pervades those who warn of perverse elasticities making the disequilibrium in the exchange market incurable". My article dealt with the severe cases of elasticity pessimism; I attempted to show that this view tended "to overestimate the magnitudes of the elasticities which are required if exchange depreciation is to have remedial effects and, second, ... to underestimate the actual magnitudes of the elasticities" (p. 52).

The overestimation of the *required* elasticities is most explicit in the Lerner formula,⁵ which concentrates on elasticities of demand for foreign goods, that is, on the sum of the price elasticity of domestic demand for imports and the price elasticity of foreign demand for exports of the country that lets its currency depreciate in the foreign-exchange market. The "critical values" of these elasticities are those at which a depreciation of the currency will neither reduce nor increase the excess demand for foreign exchange. According to that formula, the sum of the absolute values of the two elasticities of demand for commodities (absolute, that is, disregarding the minus signs for demand elasticities) has to be 1 (unity); if it is smaller than unity, the excess demand for foreign currency will increase when its price is increased. This theorem rests on two assumptions: (1) that the price elasticities of supply of commodities are zero, that is, that the domestic prices of exportable and importable commodities remain unchanged in the countries' own currencies, regardless of the quantities demanded;

⁴ "Elasticity Pessimism in International Trade", *Economia Internazionale*, Volume 3 (February 1950), pp. 118-137. Reprinted in my book, *International Payments, Debts, and Gold*, pp. 51-68. Page references in the text will be to that volume. Translated into Japanese (1973).

⁵ Several writers confuse the Lerner formula with another formula by Alfred Marshall, which relates to reciprocal-offer curves. Those who speak of a "Marshall-Lerner formula" have slipped up on their reading assignments.

and (2) that the initial position is one of balance, that is, that the values of exports and imports are equal (pp. 54-55). If, contrary to these assumptions, the elasticities of supply are greater than zero or the value of imports initially exceeds the value of exports, the sum of the demand elasticities may be well below unity without causing a perverse result (pp. 55-58).

The underestimation of *actual* elasticities was a matter of statistical and econometric research. I showed that the attempts at measuring the price elasticities of the demand for imports were beset by conceptual as well as statistical problems, which made the findings suspect of being biased in a downward direction. The major conceptual problem lies in the fact that the "statistical demand curve" in these cases relates not to a single commodity but to a "non-homogeneous aggregate of many commodities" with very different observations of past price-quantity changes. Investigators did not calculate weighted averages of the elasticities for all commodities in the bundle, but instead resorted to working with indices of total physical imports and of average price changes of import goods (p. 61). I offered illustrations to show that, depending on what changes in prices and quantities were observed, "the elasticity of demand for [a] bundle of two commodities seems to be far below the elasticity of demand for any one of the commodities" (p. 62), which of course is a ridiculous result. I also reported on research by Arnold Harberger and Guy Orcutt that proved biases inherent in index-number techniques, curve-fitting techniques, and arbitrary (though customary) choices of assumptions (pp. 62-68).

The second article of this triad was a conceptual discourse designed to disentangle a hodge-podge of concepts that had been given the same name: "balance of payments". Since dollars shortage had been described as a chronic deficit in the balance of payments, I thought it would help if the equivocation inherent in this term could be made transparent. My article, "Three Concepts of the Balance of Payments and the So-Called Dollar Shortage",⁶ was widely quoted, but I doubt that it persuaded many economists to be more careful in stating what they meant when they spoke of a deficit in the balance of payments.

⁶ "Three Concepts of the Balance of Payments and the So-Called Dollar Shortage", *Economic Journal*, Vol. 60 (March 1950), pp. 46-68. Reproduced in William R. Allen and Clark Lee Allen, eds., *Foreign Trade and Finance* (New York: Macmillan, 1959), Ch. 5, pp. 97-123, and in FRITZ MACHLUP, *International Payments, Debts, and Gold* (1964, 1966, 1976), pp. 69-92. Page references in the text will be to the last-named volume. The article was translated into Japanese in two versions (1953 and 1973).

The three basic concepts are the market balance of payments, a balance of supply and demand; the program balance of payments, a balance of hopes and desires; and the accounting balance of payments, a balance of credits and debits (p. 69). Each of the three types of balance allows for an indefinite number of sub-types. The *market* balance is purely hypothetical and essentially *ex ante*; for example, it is addressed to the following kind of question: "how would the quantity of foreign currency offered for sale increase or decrease if the price of foreign currency were 5 per cent higher, with no expectations being entertained regarding a further rise or subsequent reduction in that price, and with no change occurring in the domestic money supply and in national income". The *program* balance may be part of a formal national plan, a forecast consistent with national targets, a projection of past experiences adjusted for expected or desired changes, an estimate of hoped-for sources of foreign funds and their preferred uses — all affected by judgements of political "realities" or perhaps wishful thinking; needless to say, it is *ex ante*, but the hypotheses are less in the nature of micro- and macro-economic theorizing than in the nature of national planning. The *accounting* balance is an arrangement of statistical data derived *ex post* from records, reports, and estimates, organized by conventional categories and with all items operationally defined. [The statistical balances of goods and services, balances on current account, balances on current and long-term-capital account, balances on liquidity basis, and balances on official-settlements basis are subtypes of the accounting balance.]

The three major types of balance serve altogether different purposes. The market balance is an analytical tool of the economic theorist who reasons about shifts of supply and demand curves, and about movements along these curves, consequent upon a variety of hypothetical changes. The program balance is a kind of budget made by an appointed or self-appointed national planner who thinks about the next year or a longer planning period; it may also be a program made by a forecaster who takes political "necessities and possibilities" as given. The accounting balance is the compilation of empirical data for a past period by a statistician who uses conventional or otherwise defined procedures of organizing the accounts.

In each of these types of balance the notion of a deficit varies with the purpose; "there is no necessary relationship between these deficits; indeed a deficit in one of the three senses may be compatible with a simultaneous surplus in one of the other senses" (p. 89). "In discussions of the equivocal 'dollar shortage' we should insist on full identification

of the concept employed" (p. 92). I am afraid, few economists have insisted and still fewer have complied.

The third article was on "Dollar Shortage and Disparities in the Growth of Productivity".⁷ It differed from the earlier two articles in that it was directed at one specific structural explanation of dollar shortage — growth disparities — and at three proponents of that explanation: Thomas Balogh, John H. Williams, and John R. Hicks. The expositions by Balogh and Williams in 1946 and 1952, respectively, had not been sufficiently clear to allow a well-focused critical analysis. The argument presented by Hicks is straightforward, pointed, and subject to clear-cut dissection. Hicks distinguished:

"between barter effects and monetary effects. Barter effects are those which persist whatever the course of money incomes. Monetary effects are those which arise out of difficulties in the adjustment of money incomes. If a difficulty is purely monetary, people ought to find a means of doing something about it; but nothing can be done about a 'real' difficulty, one inherent in a barter effect" (p. 100).

What effects can be expected from an increase in productivity in country *A*, with no change in productivity in *B*? Hicks proposed three different answers, depending on the sectors in which productivity improves. If productivity in *A* increases uniformly in all industries, there may be pleasant barter effects but unpleasant monetary effects for *B*. Difficulties can be avoided by an appropriate monetary policy of *A*, allowing incomes in *A* to rise. In the second case, "if improvements are concentrated on *A*'s export industries, and *B* can thus get more imports for the same money, no monetary troubles and only a real boon for *B* may result from this kind of development" (p. 101). In the case of "import-biased improvements", however, Hicks sees trouble for *B*. The demand for *B*'s exports will be reduced when *A*'s efficiency in making import-substitutes increases; and no monetary arrangements can be devised to avoid a loss for *B*. Hicks believed that most of the progress of productivity in the 20th century had been in North America and was of the import-biased variety, causing continuing dollar shortage in Britain (p. 102).

My criticism of Hicks' explanation fastened on several points of alleged but not substantiated facts. They related to Britain's terms of trade (which for some periods were rather favorable), America's monetary expansion (which often exceeded that of Britain), and British per

⁷ "Dollar Shortage and Disparities in the Growth of Productivity", *Scottish Journal of Political Economy*, Vol. 1 (October 1954), pp. 250-267. Reprinted in *International Payments, Debts, and Gold*, pp. 93-109. Page references in the text are to that volume. The article was translated into Japanese (1955 and 1973) and Portuguese (1955).

capita real income (which increased during most of the time, whereas it should have declined if the theory were both valid and applicable to the British case). The main point, however, was a point of theory. Hicks' explanation of dollar shortage in Britain was based on an absolute loss of real income due to steadily deteriorating barter terms of trade. If in fact, however, real income in Britain increased, a worsening of the terms of trade could have, at worst, slowed down the increase. To be sure, if British labor costs were raised at the gross rate of productivity increase without taking any account of the deduction due to adverse changes in the terms of trade, one would have to expect unemployment or price inflation and dollar shortage. But the same result would have arisen if wage rates had increased faster than productivity — without any deterioration of Britain's terms of trade, without any differences in the growth of productivity, and without any import-bias in the productivity growth of Britain's trading partners. Hence, growth disparities and adverse terms of trade are neither necessary nor sufficient conditions of dollar shortage (p.106):

“If a nation has a dollar shortage while its plane of living is rising, the shortage should not be explained as the consequence of structural obstacles to a faster increase in national income; it is satisfactorily explained by the economic policies adopted” (p. 109).

Relative Prices and Aggregate Spending (1955-1956)

Whether and under what conditions the trade balance of a country would be improved by a depreciation or devaluation of its currency in the foreign-exchange market had been debated with reference to elasticities and/or to relative monetary expansion. As I have said before, elasticities are relevant if one discusses given conditions of supply and demand, particularly given and unchanged money stocks and incomes; otherwise monetary variables have the leading roles. When the elasticity pessimists contended that “actual” elasticities were too low to promise any improvements in the trade balance, I attempted to show why their pessimism was not justified. When balance-of-payments theorists disregarded or downgraded the role of monetary policy, I attempted to show that the role of money in the play of market forces was strategic. In 1952 an article was published proposing an altogether new approach, doing

away with the elasticities approach and glossing over the monetary approach. The proponent, Sidney Alexander, called it the “income-absorption approach”.⁸

I examined the new approach in two articles published in 1955 and 1956: “Relative Prices and Aggregate Spending in the Analysis of Devaluation”⁹ and “The Terms-of-Trade Effects of Devaluation upon Real Income and the Balance of Payments”.¹⁰ In the first of these articles I gave a streamlined exposition of Alexander's argument. At its core is the truism that a country can have an export surplus only if it absorbs (for consumption and domestic investment) less output than it produces, and an import surplus if it absorbs more than it produces. It follows that a country, by letting its currency depreciate, will improve its trade balance only if its income [output] “increases while absorption increases less or stays unchanged or falls; or if absorption decreases while income decreases less or stays unchanged” (p. 175). There is nothing wrong with this argument; it is important and unexceptional. What is needed, however, is a theory which satisfactorily explains the relationships between changes in foreign-exchange rates and changes in domestic production and absorption. [I usually make a distinction between devaluation and depreciation, but in this discussion I did not want to complicate matters and used the two terms as equivalents.]

I examined all the links between an exchange-rate change and changes in income and absorption which Alexander had suggested, and found that they actually presupposed elasticities or monetary changes or both. The “idle-resources effect” presupposes a response of exports to the higher price of foreign currencies (elastic foreign demand for exports and elastic domestic supply of exports), an increase in domestic money incomes (expansion of the money stock or its velocity when the foreign-exchange proceeds of exports are acquired by domestic banks), and increased spending out of increased incomes (marginal propensity

⁸ SIDNEY S. ALEXANDER, “Effects of a Devaluation on a Trade Balance”, *International Monetary Fund Staff Papers*, Volume 2 (April 1952), pp. 263-278.

⁹ “Relative Prices and Aggregate Spending in the Analysis of Devaluation”, *American Economic Review*, Vol. 45 (June 1955), pp. 255-278. Reprinted in *International Payments, Debts, and Gold*, Chapter 8, pp. 171-194. Page references in the text are to this collection. The article was reprinted also in *Selected Economic Writings of Fritz Machlup*, edited by George Bitros (New York: New York University Press, 1976) Chapter 14, pp. 255-278. Japanese translations of the article appeared in 1955 and 1973, an Italian translation in 1971.

¹⁰ “The Terms-of-Trade Effects of Devaluation upon Real Income and the Balance of Payments”, *Kyklos*, Volume 9 (No. 4, 1956), pp. 417-452. Reprinted in *International Payments, Debts, and Gold*, Chapter 9, pp. 195-222. Page references in the text are to this collection. Japanese translation in 1973.

to spend, or income elasticity of demand). The "terms-of-trade effect" presupposes changes in the ratio of export prices to import prices (which depend on a complex set of elasticities of supply and demand, abroad and at home). The "cash-balance effect" — the effect on domestic absorption due to the decline in the purchasing power of nominal cash balances that is associated with a rise in the price level consequent upon higher prices of traded goods — presupposes both given elasticities (for example, of the demand for cash balances) and specified monetary-policy reactions (p. 179). The "income-redistribution effect", that is, the differential impact of higher prices on groups with different propensities to spend (implying shifts of real incomes from fixed-income recipients to others; from wage earners to profit earners, and from tax payers to governments) presupposes given marginal propensities (or income elasticities of demand). The "money-illusion effect", that is, the effect on real absorption (and on saving) which one may expect when income recipients disregard a decline in the real value of their money income, presupposes ongoing price increases, for which monetary expansion is evidently a necessary condition (p. 180).

I mentioned three other direct effects on absorption which Alexander had included but not named. I called them the "price-expectation effect", the "high-cost-of-investment effect", and the "high-cost-of-imports effect". Then I added, what Alexander had failed to include, the "resource-reallocation effect", that is, the possible increase in output due to a more efficient allocation of resources when disaligned exchange rates are corrected (pp. 181-182). That such reallocation again presupposes a set of price elasticities of supply and demand needs no elaboration.

I emphasized the common fallacy implied in reasoning from definitional identities. Just as the trade balance can be seen as the difference between income and absorption, one may see absorption as the difference between income and the trade balance, or income as the sum of absorption and trade balance. To regard the trade balance as the residual, determined by the two other aggregates, may be seriously misleading (pp. 186-188). The possibility of a logical snare becomes most apparent when one remembers that one country's trade deficit is another country's (or all other countries') trade surplus. If one believes that the effects of devaluation on a country's trade balance can be deduced from the effects on its own national income and its own absorption, how can this be reconciled with the same claim of causation in the other country or countries? Assuming only two countries trading with each other, should one assume that the improvement in one country's trade balance imposes a deterioration on the other country's

balance and that this "dictates" or "enforces" changes in that country's income and absorption? Or, if the same theory is to hold for both countries, so that in each country the effects on income and the direct and indirect effects on absorption determine the net effect on the trade balance, is the balance not clearly overdetermined? What forces will reshape the two balances to become equal in magnitude and opposite in sign? These questions showed clearly that Alexander's first version of the income-absorption approach was untenable.

In the second article I concentrated on the terms-of-trade effects of the devaluation. Although most writers on the subject, including Alexander, have used the "commodity terms of trade" in their arguments, and although Dennis Robertson had found that the "double-factor terms of trade" were the "most relevant to discussion of relative national standards of living, or of equilibrium rates of exchange", I came out in favor of using the "single-factor terms of trade". My reasons were that changes of the relative national incomes are irrelevant for positive economics — relevant are only the effects on the income of the devaluating country — and that the commodity terms of trade would not tell us anything about these effects if productivity in that country were increased, as it well might when devaluation results in a reallocation of resources (pp. 198-201).

Alexander had reasoned that devaluation would most likely worsen the commodity terms of trade, and that this would imply a reduction of national income and also an "initial" deterioration of the trade balance by the same amount (partially abated later by the indirect effects of the income reduction upon domestic absorption). I found this reasoning faulty on several grounds: (1) the commodity terms of trade need not always deteriorate as a result of devaluation; (2) even when they do deteriorate, the single-factor terms of trade may improve and, hence, national income may rise instead of falling; and (3) even if national income falls, the trade balance need not deteriorate, either by the same amount or at all. Alexander's contention was that the fall in national income would cause an equal and simultaneous deterioration in the trade balance, evidently because people would "initially" import unchanged quantities of goods at increased prices. One might with equal justification argue that people would "initially" refuse to spend more of their income on imported goods and would instead reduce their real absorption accordingly. Both these suppositions are extreme, and some middle position looks more likely (p. 204). However, if the country has no exchange reserves left, or does not want to deplete them any further,

and if it cannot increase its foreign borrowing, the second extreme will be the only possible outcome. This indeed constitutes the “immediate adjustment” under freely flexible exchange rates when neither domestic nor foreign financing is forthcoming (p. 204).

Having distinguished three possible effects of currency devaluation upon national income, I found that they may be interrelated. Thus, a positive idle-resources effect or a positive resource-reallocation effect may be the consequence of a negative terms-of-trade effect in the sense that the positive effects can be had only if export prices were lowered enough to induce the increase in exports. In such instances, attempts to isolate the terms-of-trade effect may obstruct the understanding of the relationship (p. 205).

The meanings of “changes in national income” and “changes in absorption” in this and similar analyses have usually been assumed to be clear and not subject to challenge. That there are substantial differences between various operational definitions would not vitiate theoretical arguments, but differences between fundamental constructs may be stultifying. For our immediate purposes we should distinguish three theoretically relevant constructs:

“(1) changes in the total domestic production of finished [read: final] goods and services valued at constant prices (‘real domestic output’), (2) changes in the total domestic use, for consumption and investment, of goods and services valued at constant prices (‘real intake’), and (3) changes in the total amount of income received or earned, with goods and services valued at constant prices, and with [international] income transfers, capital earnings, and all changes in foreign assets and liabilities accounted for insofar as they arise from current transactions (‘real income’)”.

The names used for the triad, “output, intake, and income” combine simple prepositions — out, in — with short verbs — put, take, come. The concept of real intake is an equivalent of what Alexander called absorption, and what other writers have designated with half a dozen other names. My hope that the term “intake” would be adopted by the professional literature has not been fulfilled.

The difference between output and income is important for several arguments. A frequent example is the case of international unilateral transfers, such as donations or reparations payments; but for the problem before us, it is the effect of price changes on foreign claims and liabilities that is of major significance. “We can imagine a situation in which there is absolutely no change in physical output, physical intake, physical exports, and physical imports, but where the foreign prices of

exports are reduced and/or the foreign prices of imports increased. A loss in foreign assets or increase in foreign debt would occur as a result. Hence, real income would be reduced although domestic output at constant prices was not changed” (p. 216). The proposed definition of real income takes account only of changes in assets and liabilities “as they arise from current transactions”. It disregards therefore changes in the value of old assets and liabilities. Take, for example, the changes in the commodity values of existing foreign claims or debts. Does a reduction of the foreign purchasing power of foreign-exchange reserves held by the monetary authorities reduce the national income, either in the year when the change occurs or in the years when the reserves are used? What does an increase in the price of gold do to the real income of a nation that holds large stocks of gold? Most theorists have decided to close their eyes to such problems; they are satisfied to overlook differences between output and the numerous variants of income by denoting all of these by the letter *Y* and solving the simultaneous equation as if *Y* were identical with *Y*.

Equilibrium and Disequilibrium (1958)

In the discussions of dollar shortage, payments balance, trade balance, exchange rates, and so forth, the terms equilibrium and disequilibrium were being banded about as if these were simple household words. Most noneconomists innocently believed that disequilibrium was “a Bad thing”, and equilibrium “a Good thing”; many thought that there could be “chronic” disequilibrium; and virtually all were convinced that one could “see” or “observe” an equilibrium if one looked at some data. To disabuse students of such views, I felt it necessary to write an article on “Equilibrium and Disequilibrium: Misplaced Concreteness and Disguised Politics”. I began writing it in 1955; it was published in 1958.¹¹

¹¹ “Equilibrium and Disequilibrium: Misplaced Concreteness and Disguised Politics”, *Economic Journal*, Vol. 68 (March 1958), pp. 1-24. Reproduced in *Essays in Economic Semantics* (Englewood Cliffs, New Jersey: 1963; New York: Norton, 1967; New York: New York University Press, 1975), pp. 43-72; and in *International Payments, Debts, and Gold* (1964, 1966, 1975), pp. 110-135. Page references in the text are to the last mentioned volume. The article was translated into Spanish, (1962 and 1974), French (1971), and Japanese (1973).

“The chief purpose of the essay [was] to show the dangers to clear analysis that may arise from the failure to notice the differences between analytical, descriptive, and evaluative equilibrium concepts” (p. 112). But it went beyond making such distinctions and attempted to show that in descriptions of factual situations and in judgements of positive and negative values the pair of terms was redundant, confusing, or both, whereas in abstract economic analysis it was an important methodological device, a mental tool helpful in suggesting “a causal nexus between different events or changes” (p. 113). The nexus can be shown in form of four mental steps: 1. the initial position, or “equilibrium”; 2. the disequilibrating change, or “new datum”; 3. the adjusting (equilibrating) changes, or “reactions”; and 4. the final position, or “new equilibrium”. This scheme serves as:

“a mental experiment in which the first and last steps, the assumption of initial and final equilibria, are methodological devices to secure that Step 2 is the sole cause and Step 3 contains the complete sequence of effects. The function of the initial equilibrium is to assure us that ‘nothing but 2’ causes the changes under Step 3; the function of the final equilibrium is to assure us that ‘nothing but 3’ is to be expected as an effect of the change under Step 2 (although the ‘completeness’ of the list of effects will always be merely relative to the set of variables included in the equilibrium)” (p. 115).

It is essential to understand that all equilibria are relative, because there is a wide choice of variables, and of relations among variables, to be included. In “equilibrium in international trade” we may choose either to include or to disregard such variables as expectations of future changes, the size of monetary reserves, the liquidity of the banking system, and so forth. For the sake of quick comprehension, especially for teaching purposes, one will prefer the simplest possible model with only a few variables, but of course one will avoid using such a model as the basis for policy recommendations. The heuristic value of a theoretical model (algebraic, geometric, or verbal):

“is not impaired if an important factor is left out, provided the omission is not inadvertent. Indeed, the importance of any factor can be demonstrated only by leaving it out of account and then showing the difference it makes when it is reinstated as one of the variables in the equilibrium system” (p. 117).

I defined equilibrium in economic analysis:

“as a constellation of selected interrelated variables so adjusted to one another that no inherent tendency to change prevails in the model which they constitute. The model as well as the equilibria are, of course, mental constructions (based on abstraction and invention)” (p. 119).

As an alternative but equivalent definition of equilibrium I proposed “mutual compatibility of a selected set of interrelated variables of particular magnitudes”; the assumed interrelationships may be behavioral, technological, psychological, institutional, or merely definitional. But

“the crux of the matter is that the addition of another variable, somehow related to one or more of the others, would change the picture ... One cannot overemphasize this *relativity* of compatibility and incompatibility regarding extra variables included in, or excluded from, the selected set” (p. 120).

“To characterize a concrete [historical] situation ‘observed’ in reality as one of ‘equilibrium’ is to commit the fallacy of misplaced concreteness ..., first, because of the general fallacy involved in jumping the distance between a useful fiction and particular data of observation and, second, because of the fallacy involved in forgetting the relativity of equilibrium with respect to variables and relations selected” (p. 122).

Apart from misplaced concreteness, there was also the matter of value judgements, and several respected economists have indulged not only in mixing evaluation with analysis — which is all right as long as it is explicit — but also in hiding value judgements in the concept of equilibrium:

“By imputing a value judgement, a political philosophy or programme, or a rejection of a programme or policy, into the concept of equilibrium designed for economic analysis, the analyst commits the fallacy of implicit evaluation or disguised politics” (p. 124).

I proceeded to examine the writings of several distinguished economists on the theory of international trade and finance. I found full awareness of the relativity of equilibrium (Joan Robinson), built-in politics and simulated stability (Ragnar Nurkse), openly embraced political criteria (Paul Ellsworth and Charles Kindleberger), explicit criticism of persuasive definitions of equilibrium (Paul Streeten), and vacillation between explicit relativity with value neutrality in some parts and implied value judgements in others (James Meade). Positive analysis was sometimes “sabotaged” by the use of value-laden equilibrium concepts; this occurred, for example, when the adjustment or “equilibration” was leading to a final position that did not meet the political criteria stipulated for “equilibrium” — say, full employment (pp. 125-135).

In view of the layman's idea that a trade deficit is always a "disequilibrium" of the balance of payments, it is important to comprehend that explaining the deficit means to show it as an adjustment, or equilibrating change, following an antecedent disequilibrating change, for example, increased government spending, expansionary monetary policy, devaluation of a trading partner's currency, receipt of foreign investment, etc. This, incidentally, was one of the main theses which I had proclaimed, as early as 1923, and hoped to impress on the reader (*Goldkernwährung*, pp. 137-139).

Proposal to Reduce the Price of Gold (1960-1961)

Two things happened in the second half of 1960, one to me, the other to the U.S. dollar; but both had much to do with my continuing concern with international finance. I left the Johns Hopkins University, where I had been for 13 years, and moved to Princeton University to become Walker Professor of Economics and International Finance and also Director of the International Finance Section. This meant that I devoted more time than previously to international monetary problems, in teaching, research, and editing various series of publications. The Princeton Essays in International Finance, together with the Studies and Special Papers, became my responsibility, and I took it very seriously; I solicited, suggested, and commissioned manuscripts on timely topics and I did much editing to make the essays more readable.

The other thing that happened was the first flight from the dollar into gold. In 1958 the dollar shortage had been transformed into a dollar glut, not by any drastic change in the balance of payments of the United States, but by the simple realization on the part of the central banks that they had accumulated all the dollars they wanted. There had been several proposals that the dollar price of gold be raised — doubled, according to some — and many Europeans acted on the expectation that these proposals would be adopted. This caused a large demand for gold in the London market and elsewhere; the price of gold in London climbed temporarily to \$ 40 an ounce, or more than 13 per cent over the official parity.

This episode caused me to make a "Proposal to Reduce the Price of Gold".¹² At the annual meeting of the economic associations, I was one of the discussion speakers in a session of the American Finance Association, where Frank Southard and Ralph Wood read papers. I used this occasion to make my strange proposal — which shocked a good many people.

My proposal was for reducing the price of gold in several small steps. "This would reverse historical experience, and those who persist in holding gold would lose money" (p. 240). To inflict such losses on gold hoarders was, however, not my purpose. Indeed, since the reduction would be announced several months in advance, speculators and hoarders could gain by selling gold before the reduction and, if they distrusted the authorities, buying back afterwards. After a few reductions, many people might decide to stay out of the market. The real purpose of my plan was to discourage further gold purchases by private investors or speculators and thus to avoid further losses of official gold reserves. The one-way expectations — due to rumors, recommendations, and pressures for a large raise in the official price of gold, but never a hint of a reduction — had made "long" positions, speculation for a rise, almost riskless. A series of small reductions, perhaps every three months, with the monetary authorities prepared to sell and buy unlimited quantities of gold, could redress this situation. It might lead to a net dishoarding of gold, an increase in official gold reserves, and a considerable easing of the money markets with lower interest rates.

I ended my paper with a warning:

"While I regard my proposal as more than a 'gadget' or 'gimmick,' I do not regard it as a substitute for sound monetary and financial policies in this or any other country" (p. 244).

International Liquidity (1961-1962)

In my hypersensitivity regarding linguistic and semantic sloppiness I was disturbed by the promiscuous use of the term liquidity, domestic as well as international. The term liquidity had been used in connection

¹² "Comments on 'The Balance of Payments' and a Proposal to Reduce the Price of Gold", *The Journal of Finance*, Vol. 16 (May 1961), pp. 189-193. Reprinted partly as Chapter VI, partly as Chapter XI in *International Payments, Debts and Gold* (1964, 1966, 1975). The part on the Proposal is on pp. 239-244 of that volume. Page references in the text are to that chapter. The two chapters were translated into Japanese (1973).

with *assets* and in connection with individual *debtors*, including individual *banks*. The use was gradually extended to *groups* of individuals, firms, and banks, and finally to whole *countries*, *groups of countries*, *the whole world*. The chief proponent of the extension to an entire country was the Radcliffe Commission in Britain, which in 1959, in its *Report on the Working of the Monetary System*, held that the old concepts “quantity of money” and “amount of bank reserves” were too narrow and had better be replaced by the wider concept “liquidity”. The chief proponent of the concept of international liquidity was the International Monetary Fund, in its staff study *International Reserves and Liquidity* (Washington, 1958). The main idea was that an expression was needed for the combination of official reserves and borrowing rights. In 1961-62 I wrote an article protesting the looseness with which the term could be used; the National Bank of Belgium published it under the title “Liquidité, internationale et nationale”, and I included it later in a collection of essays under the extended title “The Fuzzy Concepts of Liquidity, International and Domestic”.¹³

Among my misgivings to the extension of the concept to groups of individuals, firms, and banks was the troublesome question “whether debts among members of the group and payments for intergroup purchases are to be included or ‘netted out’” (p. 251). There can be no unequivocal answer, because the problems are different for intragroup and for external debt-paying capacities; at least two different concepts are involved here. “In some cases a clear answer will be almost impossible because one ‘person’s’ money or near-money is another ‘person’s’ debt and it may not make much sense to ask for their combined liquidity...” (p. 252). For heterogeneous groups, or the country as a whole, “aggregate liquidity” cannot be meaningful until the intent of the question is specified. “The term ‘liquidity’ is concise only at the expense of precision”, but “what we need is not a [better] term ... A term in search of a meaning is a rather useless thing; a meaning in search of a term can easily be accommodated by imaginative word-coiners, provided the meaning is clearly and unambiguously specified” (p. 253).

¹³ “Liquidité, internationale et nationale”, *Bulletin d'Information et de Documentation, Banque Nationale de Belgique*, Vol. 37 (February 1962). Slightly revised English version, “The Fuzzy Concepts of Liquidity, International and Domestic”, *International Payments, Debts, and Gold* (1964, 1966, 1975), Ch. XII, pp. 245-259. Footnote references in the text are to that volume. The chapter was translated into Japanese (1973).

“If analysts, in discussions of a domestic monetary situation, refuse to speak of the quantities of currencies and demand deposits, and insist on ‘looking at the whole liquidity position’ [as the Radcliffe Report puts it], they evidently mean to say that other things besides currency and deposits ought to be taken into account. The term ‘liquidity’ may help to warn the audience not to disregard ‘other things’, but it will not help very much if these other things are not specified or, if specified, are not measurable or, if measurable, are not additive” (p. 254).

Analogous difficulties trouble the analyst of so-called international liquidity. To designate the assets as well as the liabilities of the reserve-currency countries as

“liquidity of the international monetary system contributes nothing to the problem — except a bit of confusion. It would be better if we admitted that the liquidities of banker-countries and depositor-countries, like the liquidities of bankers and depositors are not additive” (p. 255).

My objections to the extension of the concept of liquidity to groups, countries, and the whole world have been overruled by the profession. The new meaning of the term became conventional — and I had to yield. I do not think that I was wrong in my resistance, but perhaps I was too fussy. I have later used the term in its new meaning without further protest.

The last two sections of the article became, somewhat expanded, the first two sections of a new essay “Further Reflections on the Demand for Foreign Reserves” in my 1964 volume of collected essays.¹⁴ In the section on “Reserves for Contingent Liabilities” I explained why

“no amount of foreign reserve may be adequate for a country that indefinitely continues a policy of maintaining effective demand in the face of an excess demand for foreign currency at fixed exchange rates” (p. 264).

I went on to discuss that

“the accumulation of monetary reserves ... represents a formation of capital that remains unproductive (or almost so) in its present form and becomes productive only through its eventual use ... For a nation to hold ‘unnecessarily’ large foreign reserves is to forego productive domestic investment in order to be prepared for unlikely accidents” (p. 266).

On the other hand, I realized that the decision to hold reserves beyond anticipated needs may be based on undesirable side-effects of the

¹⁴ “Further Reflections on the Demand for Foreign Reserves”, in *International Payments, Debts, and Gold* (1964, 1966, 1975), pp. 260-276. Japanese translation in 1973.

measures that could be instrumental in precipitating the use of reserves, namely, accelerated monetary expansion or appreciation (or upvaluation) of the currency (pp. 266-67).

I examined the question of the optimal size of the monetary reserve of the nation, first on the assumption that the reserve was held by official monetary authorities, then that reserves of foreign or international moneys were held only privately in a system of free floating. Whether it is "as a rule socially beneficial to make reserve-holding a government function" depends on "whether the social marginal cost of reserve-holding is lower than the private marginal cost, and whether the social marginal product ... exceeds the private marginal product" (pp. 267-68). Later analysts of these questions have overlooked my tentative conclusions, which included considerations regarding (1) economies of risk-pooling through partial centralization of foreign reserves (in banks sufficiently numerous to safeguard competition) and (2) external benefits of reduced fluctuations of exchange rates (especially greater job security for labor employed in the foreign-trade sector of the economy). If external economies are substantial, "it might be argued that... inventories [of foreign contingency reserves] held by private business in trade and industry are apt to fall short of what would be socially optimal" (p. 271).

On the other hand, the concept of a private demand for foreign reserves implies the reserve holders' awareness of their opportunity costs: they forego alternative uses of their resources. But

"if central bankers or governing boards make the decisions about reserve holding, they are not themselves sacrificing anything nor are they even aware of the fact that they are imposing a sacrifice upon the nation" (p. 276).

Several authors discussing the demand or "need" for monetary reserves have the notion of "real" demand in their minds — real in the sense that nominal amounts are corrected for changes in commodity prices. In discussions of "inadequate international liquidity" it is often overlooked that a more generous provision of international reserves [and borrowing rights] to national monetary authorities "may induce credit policies leading to rising prices with the result that the real reserves end up no higher than they were before" (p. 275). If this is so, inadequate liquidity can only temporarily be made adequate, until prices catch up with the increase in liquidity expressed in terms of money.

A Survey of Reform Plans (1962, 1964)

In 1961-62 I wrote for the International Finance Section "Special Paper in International Economics, No. 3" (August 1962), a survey of plans for international monetary reform.¹⁵ These special papers were intended to serve as teaching material for students. (It may be of interest to some that Special Papers No. 1 and No. 2 had been by Gottfried Haberler and Oskar Morgenstern, my friends and fellow students at Vienna in the early 1920s, both in the United States since the 1930s.) My survey of reform plans has probably become the most widely read of my publications in this area, chiefly in its revised edition, published by the International Finance Section in 1964 and also included in my collection of essays in the same year.¹⁶

I began the survey with a description of the system as it existed at the time, including an explanation of the operation of the International Monetary Fund. This was followed by a presentation of the charges against the existing system, in particular, "difficulties with the balance of payments", "inadequacy of international reserves", and "danger of collapse". Then came "A Selection of Plans", classified into the following five types:

1. Extension of the gold-exchange standard
 - a. with continuing increase of dollar and sterling reserves;
 - b. with adoption of additional key-currencies.
2. Mutual assistance among central banks
 - a. with safeguards against expansive credit and fiscal policy;
 - b. with expansion of domestic credit and expenditures.
3. Centralization of monetary reserves and reserve creation
 - a. with overdraft facilities available to deficit countries;

¹⁵ *Plans for Reform of the International Monetary System* (Special Paper in International Economics, No. 3, Princeton, N.J.: International Finance Section, Princeton University, August 1962). An abridged version was included in *Factors Affecting the United States Balance of Payments*, Subcommittee on International Exchange and Payments, Joint Economic Committee, Congress of the United States, 87th Congress, 2nd Session (Washington: 1962). Part 3, pp. 209-237. Translations appeared in German (1962), Italian (1962) and Japanese (1963).

¹⁶ Revised edition of the Princeton Special Paper No. 3 (March 1964). Reprinted in the *International Payments, Debts, and Gold* (1964, 1966, 1975), Chapter XIV, pp. 282-366. Russian translation (1966), Japanese (1973).

- b. with autonomous reserve creation by a world central bank;
 - c. with finance of aid to underdeveloped countries.
4. Changes, especially increase, in the price of gold
 - a. with the gold-exchange standard continued;
 - b. with the gold-exchange standard abolished.
 5. Freely flexible exchange rates
 - a. in order to make internal monetary policies more independent;
 - b. because internal monetary policies are too independent.

A main feature of the exposition was the use of T-accounts showing the sequence of changes in monetary reserves and demand deposits, domestic and foreign, private and official, that would be generated by the operation of the different plans. Documentation of sources — proposals as well as proponents — was exhaustive, accounting for 105 footnotes in the revised edition — an increase of 52 per cent over the first edition. The “growth” of the literature between the two editions was extraordinary: after only one year, 37 new publications with new proposals or comments on proposals were calling for my attention.

In the “Concluding Remarks” I explained my reticence regarding my support of any one of the plans examined. No plan can be judged to be the best irrespective of government policies in all areas. What can and should be done is to recognize inconsistencies and incompatibilities. Some plans are simply ruled out as long as people in charge or people of influence entertain strong views about certain economic and monetary objectives. I pointed this out in connection with monetary policies intended to maintain high rates of employment and growth and also fixed rates for foreign exchange (*International Payments, Debts, and Gold*, pp. 359-363). This theme had been present in a more primitive form in my first book, was most clearly stated in this survey of monetary plans, and was to be sounded in several variations in my later publications.

The Transfer Problem Revisited (1963-1964)

One of my earliest concerns as international monetary theorist had been the transfer problem. The two articles published in 1928 and 1930 addressed chiefly the problems of large payments of foreign debts and reparations. In the early 1960s, when Europeans and Americans began

to be worried about the dollar glut and the continuing deficits in the balance of payments of the United States, I became convinced that this country was experiencing transfer difficulties. Commitments for foreign aid, military expenditures abroad, and private investments abroad added up to large financial transfers not matched by real transfers in the form of export surpluses. This interpretation of the balance-of-payments problem of the United States — continuing outflows of gold and increases in official short-term liabilities to foreign countries — occupied my thoughts and induced me to write two new papers for inclusion in the volume of collected essays which I prepared for publication in 1964.

The first of these new papers appeared also in a German translation.¹⁷ “The Transfer Problem: Theme and Four Variations” stated the theme in the briefest form and then presented the four variations: I. Britain, 1793-1816, during the Napoleonic wars, maintaining armies on the continent of Europe, giving loans and subsidies to allies, paying foreign diplomatic agents; II. France, 1871-1875, after the Franco-Prussian war, paying indemnities to Prussia; III. Germany, 1924-1932, after the first world war, paying reparations to the victorious nations; and IV. United States, 1950-1963 [though continued for many more years], after the second world war, giving foreign loans and grants, paying for military expenditures abroad, and making private foreign investments.

Comparisons of the absolute amounts (of current money) involved were meaningless, but “the relative burden on the paying country” could be judged by ratios to relevant aggregates. I compared the foreign remittances with national-income estimates and with foreign-trade statistics. The former comparison may throw light on the budgetary problem of the paying country, not just in the sense of a fiscal or tax problem but in the wider sense of “the problem of reducing the domestic intake out of a given national income”. The second comparison “may help in sizing up the real transfer problem; the problem of increasing exports and/or reducing imports in order to have the trade balance adjusted to the remittances abroad” (pp. 374-375).

I found it preferable, for relevant comparisons of the four cases, to break the various periods of extraordinary remittances into subperiods,

¹⁷ “Das Transferproblem: Thema und vier Variationen”, *Ordo*, Vol. 14 (1963), pp. 139-167. The English version, “The Transfer Problem: Theme and Four Variations”, became Ch. XV in *International Payments, Debts, and Gold* (1964, 1966, 1975), pp. 374-395. Page references in the text are to that book.

though I compared also the two selected ratios for the entire periods. One astonishing finding was

“that the burden on the economy of the paying country was in several respects least heavy in the case of Germany. The reparations payments from 1924 to 1932 were only 2.5 per cent of national income. This is less than the British wartime remittances from 1806 to 1816, which were 2.7 per cent of national income; it is less than the United States postwar payments, which were 3.0 per cent of national income, no matter whether we look at 1949 to 1961, 1950 to 1955, or 1956 to 1961; and it is much less than the French indemnity payments, which were between 5.6 and 11.2 per cent, depending on what payments period is assumed to be relevant. The German reparations payments were, depending on the period, 5.1, 6.4, and 7.9 per cent of the trade volume. This is less than the British war payments from 1806 to 1816, which were 9.3 per cent of the trade volume; and it is much less than were either the French indemnity payments or the United States postwar payments in relation to foreign-trade volume” (p. 392).

“Another highly astonishing finding is that the burden of foreign payments is so very heavy in the case of the United States. In relation to national income, the French paid more, but this was a one-time effort, made and accomplished in a short time; the United States has shouldered its burden for about sixteen years so far [1963] and seems prepared to carry much of it, if not all, for many more years. In relation to foreign-trade volume the payments by the United States — some 33 or 34 per cent — are heavier than anything undertaken by any of the other countries. This should give us a good deal to think about the size of the task required if the payments are to continue in the present magnitude and ultimately must be matched by export surpluses achieved through an adjustment in the trade balance” (p. 393).

I deemed it necessary to qualify my findings in several respects. “First, a given percentage burden may weigh more heavily on small incomes than on larger ones”, which weakens the conclusions regarding relatively light burdens in the British case, almost two centuries ago. Secondly,

“to the extent that parts of the foreign payments are effected, not out of current income, but through borrowing from abroad or through the use of gold stocks and liquidation of foreign assets, the reduction of ‘domestic intake’ is deferred. In this case the ratio of payments to national income does not mean the same thing; it gives an exaggerated impression of the immediate ‘pinch’ on domestic consumption and investment” (p. 394).

Likewise, the ratios of the required transfers to the volume of foreign trade may not give the right indications of the difficulties of adjustment; the elasticities of foreign and domestic supply and demand for traded goods may be such as to facilitate adjustment of large trade

volumes, or make adjustment of even relatively small trade volumes more difficult. Still, while the compared ratios do not tell the whole story, they “have the merit of being measurable as well as relevant” (p. 395).

The second article, “The Transfer Problem Revisited” first attempts a simple restatement of the problem but immediately takes issue with it by asking the following questions:

“What assumptions are appropriate concerning primary disbursements¹⁸ in the receiving country? Can an expansion of disbursements in B take the place of a contraction in A? May we legitimately speak of a transfer problem if disbursements in A are not reduced by x dollars, [the amount of the financial transfer commitments], but only by less, or not at all, or are even increased? Can one properly say that the budgetary problem in A has been solved if tax payments have been facilitated by credit expansion, or if a contraction of the money supply has been offset by bank credit? If government lacks the courage or the callousness to insist on the reduction of effective demand in the paying country, is this a transfer problem or a budgetary problem? Is it an economic or a political problem?” (p. 434).

In an attempt to answer some of these questions I said among other things that,

“in a way, one may find it a little disingenuous to speak of a transfer problem when A, the paying country, has no intention to cut its domestic spending by the amount it is obligated to pay to B. Under certain conditions, particularly in the context of economic growth, one may relent and grant that a ‘smaller than otherwise warranted’ expansion of domestic disbursements may be equivalent to an absolute curtailment of domestic disbursements in the absence of growth.”

“As a matter of fact, all historical situations in which “transfer problems” arose were characterized by expansions, not contractions, of domestic demand — although the theoretical discussions always started from the assumption that the domestic funds set aside for transfer had been successfully ‘extracted out of the pockets of the people’ (as Keynes put it). Thus, from the very beginning, the theory of the transfer and the identification of historical transfer problems were at odds with each other, and many writers were lacking in

¹⁸ Positive primary disbursements are new additions to the income stream, in contrast to secondary disbursements by income recipients who respond some or all that they have received. Secondary disbursements are assumed to be “induced” by changes in income, according to the marginal propensity to consume. Primary disbursements are “autonomous” changes in investment or consumption, though usually they are thought of only as investments. Negative primary disbursements are autonomous reductions in investment or consumption.

candor when they were diagnosing "transfer difficulties", although the budgetary problem had not been solved and no solution had even been attempted. (The possible reconciliation through explicit references to economic growth did not occur to any of the earlier writers on the subject)" (p. 434).

Before I embarked on an analysis of the transfer problem under conditions of economic growth, I engaged in the semantic analysis which I had found wanting in my article on "Transfer and Price Effects" back in 1930. Now, in 1964, I

"distinguished, besides 'shortage of foreign exchange' and besides 'political difficulties', six types of economic loss: primary burden, terms-of-trade losses, losses through increases in the real value of the fixed obligations, transitional unemployment, transitional misallocations, and long-term unemployment. If these different types of economic loss are no longer scrambled together as 'transfer difficulties', analysts of the transfer problem will begin to understand one another" (p. 440).

In both cases, transfer under conditions of growth and transfer between stationary economies,

"the problem is to ascertain the "warranted" rate of primary disbursements in the paying country; that is, that rate which, given the rate of primary disbursements abroad and given the countries' propensities to save and to import, is apt to create the desired increase in its balance of trade" (p. 441).

Of course, this warranted rate of increase or decrease in domestic disbursements would depend on price effects as well as income effects. Realizing that the analysis of price effects was quite complex (unless one simplifies the argument by limiting the number of goods traded to two, or to two bundles of fixed composition), I had in 1943 analyzed the transfer problem exclusively on the basis of income effects. This meant, in accordance with the conventional assumptions of multiplier analysis, that the (full or partial) solution of the transfer problem, that is, the adjustment of the trade balance to the financial transfer commitment, has to operate through changes in employment: in favorable cases, there could be increased employment in the recipient country, but in any case there would be reduced employment in the paying country. This conclusion applied to transfers between stationary economies. Now I saw that under conditions of economic growth in both the paying and receiving countries, changes in income and employment would still be the only equilibrating forces as long as one sticks to the assumptions of

income-multiplier analysis; but it is now possible for employment to increase also in the paying country. If both countries have increasing amounts of real resources available, and if the receiving country expands its primary disbursement sufficiently to employ much or all of its increased capacity to produce, the paying country need not reduce its domestic disbursements, and therefore its employment, in order to achieve the required trade surplus.

The question of the "warranted" change of disbursements in the paying country can be attacked in the following way:

"Assume that [the receiving] country ..., realizing its full growth potential, increases its disbursements ... by a given rate every period. At what rate will then the paying country be permitted to increase its primary disbursements without running into trouble, that is, without reducing its export surplus below the amount ... which it is obligated to transfer every period?" (p. 444).

I developed a formula for the warranted rate of disbursements in the paying country; it showed clearly that it depended, apart from the marginal propensities to save and to import in the two countries, on the rate of expansion in the receiving country. No matter how large the growth potential is in the paying country,

"the paying country cannot expand as fast as its own resources would permit, without running into transfer troubles. The warranted expansion ... is tied to the actual expansion in [the receiving country]..., as long as the assumptions of fixed wage rates, prices, and exchange rates are maintained" (p. 445).

The warranted expansion in the paying country is, of course, also limited by the growth of its own resources. I also examined some special cases regarding saving and import propensities. There are constellations which make the transfer depend exclusively on the spending policies of the paying country, and others in which only the policies of the receiving country determine the outcome (p. 446).

I thought in 1964, and I still do, that this revisit to the transfer problem secured new insights. Unfortunately, the essay has failed to elicit any response from my professional colleagues. I suspect it was overlooked. In a volume containing some twenty pieces, most of them well known to the well-read members of the profession, a new essay tacked in between the old ones, can easily escape the readers' attention. Perhaps this notice will persuade some to look up the overlooked chapter.

The Mysterious Numbers Game (1964)

Another chapter in the same volume was also new but not overlooked, perhaps because it had an eye-catching title and was particularly suitable for teaching purposes. It was “The Mysterious Numbers Game of Balance-of-Payments Statistics”.¹⁹

Since it dealt with statistical data, the chapter concerned itself only with the accounting balance of payments, but showed that its components can be arranged in many different ways.

“Where accounts are kept on a double-entry [book-keeping] system, so that the sum of all credits must equal the sum of all debits, the selection of categories of entries to make up the [credit or debit] *balance* determines, obviously, the categories of entries which make up the *offsets* to the balance ... equal in amount ... with the signs reversed” (p. 141).

What I called “offsets” in my essay has later been generally called “financing items”, suggesting that the balance (surplus or deficit) of all items “above the line” is financed by the items “below the line”. Where the line is drawn, however, depends on the statistician’s or accountant’s understanding of the theories purported to interpret the data — and these theories as well as the understanding of the theories change over time.

I exhibited a set of 20 different reports of the balance of payments of the United States for the year 1951. These reports, published between 1952 and 1963, stated 17 different results for 1951. Exactly one half of them — dated between 1952 and 1957 — reported surpluses, the other ten — only one dated 1952, the other nine dated between 1959 and 1963 — reported deficits for 1951. The surpluses varied from \$ 5,029 million to \$ 99 million; the deficits from \$ 300 million to \$ 995 million. The same basic data were used by all these reports, only their arrangement was changed. Most of the earlier reports recorded surpluses because this was in line with the theories of dollar shortage widely held at the time; later reports for the same year recorded deficits because, if the statisticians had continued to use the old arrangements of

¹⁹ Chapter VII in *International Payments, Debts, and Gold* (1964, 1966, 1975), pp. 140-166. Japanese translation (1973).

categories, they would have to record surpluses for later years when the dollar glut had become manifest and the theories of dollars shortage had been abandoned.

One lesson to be drawn from this exercise is that one must not believe that statistical data can tell their own story. We must not be taken in by admonitions such as “let us look at the facts”. The so-called empirical facts, or records of observation, can neither suggest nor verify theories. Instead, they presuppose and contain large chunks of theory; and

“changes in the theoretical presuppositions or preconceptions may result in drastic changes of the observations, of the empirical data, of the supposedly stubborn facts [of history]” (p. 147).

Preview of the Next Installment

In attempting to outline the next installment of this review of my work on international monetary problems, I have found that its form may have to differ in some respects from that of the first two installments. It will be somewhat less chronological and less sharply focussed on single titles among my publications; and it will include accounts of activities not directly related to publications, but rather to conferences, consultations, and personal relations. Both the departure from a strictly chronological treatment and the wider focus on broad topics rather than single titles are justified by the fact that I have treated particular subjects in numerous publications spread over the years. To review writings on subjects such as special drawing rights, flexible exchange rates, the price of gold, xenocurrency markets, and schemes for regional monetary integration in a topical approach is surely more sensible than to hop around among the subjects in order to preserve a chronological order.

The conflict between topical and chronological arrangement becomes most apparent in the account I plan to give of activities extending over many years. For example, the work of the Second Bellagio Group — the “Joint Conference of Officials and Academics on International Monetary Reform” — extended over fourteen years, from 1964 to

1977; it would be extremely awkward to interrupt the story in order to preserve the chronology of my publications or other activities. The same is true regarding my experiences as witness in Congressional Hearings and as consultant of the U.S. Treasury Department between 1963 and 1977. It is in connection with these activities that my report will be permeated with references to personal relations with "officials and academics", American and foreign.

The first installment of this autobiographical undertaking covered 17 years, the second, 25 years. I do not want to make a prediction about the period to be covered by the third installment.

Princeton

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