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EMPIRICAL RESEARCH ON FINANCIAL CAPITAL FLOWS

by

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This paper is concerned with the growing body of literature reporting on empirical research on international capital flows.^{1/} The term "financial" capital flows (or, alternatively, portfolio capital flows) is used in the paper to connote capital flows other than direct investments. Readers interested primarily in the research on direct investments and the multinational corporation should consult the Hufbauer paper and its references elsewhere in this volume.

The scope of this paper is restricted in another way as well. There have been a number of articles in recent years, primarily theoretical in nature, dealing with what might be termed macroeconomic models of international trade and capital flows. Some of this literature is discussed in the Helliwell paper in this volume. I have not attempted to cover this ground except insofar as it has a direct bearing on empirical research per se.

As a glance at the list of references at the end of the paper will show, most of this literature is of recent vintage. Little of it pre-dates the decade of the 1950's; the major part of it was published within the last five or six years. This fact underlines the relatively underdeveloped state of econometric research on the capital account in the balance of payments. Econometric studies of the impacts of real activity and prices on the current account in the balance of payments have a much longer and more sophisticated history -- not to

^{1/} It will appear as a chapter in Peter B. Kenen (ed.), International Trade and Finance: Frontiers for Research, (forthcoming, Cambridge University Press). References in the text to other papers in "this volume" are to other chapters in this book.

mention the great volume of empirical research carried out on domestic financial markets, domestic expenditures on real goods and services, and the linkages between them.

I should caution the reader with one further introductory comment. This paper is not, at least in some senses of the word, a survey of the empirical research on financial capital flows; I have not tried in most cases actually to summarize the literature. The paper is most accurately described, not as a survey or a summary, but as a selective assessment of empirical knowledge and of the state of the art in research in this field.

Before plunging into a discussion of the strengths and weaknesses of the work done in this area, it seems useful to begin by providing a brief road map of the literature. That is the purpose of the following section.

I. A Brief Map of the Literature

One method of mapping the literature is to classify it by the main country of interest and the extent of coverage of that country's capital account. The bulk of the research has been done on the United States and on Canada. Bilateral capital flows between the United States and Canada have also received considerable attention in their own right. Recently, more intensive empirical work has begun on capital flows in the balance of payments of European countries and Japan. Empirical research on the Eurocurrency markets, although showing promising signs of growth, is very much in its infancy.

The first systematic econometric work on the U.S. capital account was begun in the first part of the decade of the 1960's. The growth of interest in capital flows reflected in part a rising concern about weakness in the U.S. balance of payments and instability in the international monetary system. The growing interest also coincided with the development by academic economists of a literature on the so-called "assignment" problem; in much of this literature, it was suggested that monetary policy should be directed at the balance of payments, while fiscal policy should be aimed at domestic policy objectives.^{1/} Early publications by Bell (1962) and Kenen (1963), commissioned by the U.S. Government, drew attention to the

^{1/} See Mundell (1963, Chaps. 11, 16-18) for several of the important early contributions and Whitman (1970) for a survey.

area and stimulated further work -- see, for example, Cohen (1963) and Kenen's (1973) subsequent research (carried out in 1964-67, but not made available for general circulation until 1973). Stein's 1965 article in the American Economic Review had a similar effect in inducing critical comments (see Heckerman, 1967; Hendershott, 1967; Laffer, 1967; Stein, 1967) and focused more attention on the exchange-market aspects.

By the end of the decade, a large volume of work had been done, employing increasingly sophisticated methodology and statistical techniques. Branson's important Ph.D. dissertation (published in 1968) was followed with further articles refining his work (1970; Branson and Hill, 1971, pp. 11-26; Branson and Willett, 1972). The collaboration of Miller and Whitman resulted in three contributions (1970a, 1970b, 1972). Bryant and Hendershott (1970, 1972) focused on bilateral capital flows between the United States and Japan in order to illustrate the general problems confronting empirical research in this area.

Kwack (1971a; 1971b; Kwack and Schink, 1972) has estimated equations for the U.S. capital account as part of an ambitious effort to construct an econometric model for the U.S. balance of payments as a whole. Prachowny (1969) had a similar objective. Marston and Kwack (1973) have recently abridged Kwack's model as part of an effort to incorporate it into the Wharton School model of the U.S. economy. In a recent Princeton Ph.D. dissertation, Richard Herring (1973)

estimates an aggregate equation for net capital flows and compares the results with the disaggregated results of Branson and Hill (1971). The substantive contributions of these various studies are discussed in subsequent sections below.

Early contributions to the literature on the Canadian capital account included those of Rhomberg (1960, 1964) and Powrie (1964). Black (1968) studied bilateral U.S.-Canadian flows in the course of testing his extensions of Tsiang's (1959) model of exchange-market behavior. Arndt's study (1968) applied the partial-adjustment, distributed lag model to U.S.-Canadian data, focusing on speculative behavior. Lee (1969) looked at U.S. residents' holdings of Canadian long-term securities; he was the first explicitly to apply the portfolio-adjustment theory -- see below -- to U.S.-Canadian flows. Still another paper employing the bilateral U.S.-Canadian data was done by Hawkins (1968). Caves and Reuber (1971, 1972) have carried out extensive research on the Canadian capital account, with special emphasis on Canadian economic policy. Virtually all these studies, in one way or another are concerned with exchange-market behavior; this preoccupation can be traced, no doubt, to the interest in and concern about this subject on the part of Canadian policymakers.

For full-scale econometric models of the Canadian balance of payments, one can turn to Officer (1968), the TRACE model constructed at University of Toronto (Choudhry et al., 1972; Carr and Sawyer, 1973)

and the impressive efforts sponsored by the Bank of Canada -- see, for example, Helliwell et.al. (1971) and Stewart (1972). Helliwell (1969, 1972a) and Helliwell and Maxwell (1972b, 1972c) in particular have made original contributions to the capital-account part of this modelling effort (see Section II below). The chapter done by Helliwell et.al. in the volume on Project LINK (Ball, 1973) is also relevant. Recent empirical work on Canadian capital flows includes that of Charles Freedman; the empirical work itself is not yet available in published form, but see Freedman (1973).

Econometric analysis of capital flows in the U.K. balance of payments is currently going forward at the London Business School as part of their model project for the U.K. economy; see, for example, Boatwright and Renton (1972). Hodjera (1971), Branson and Hill (1971), and Herring (1973) have also reported equations for the United Kingdom.

The studies by Branson and Hill (1971) and Herring (1973) include research on the capital accounts of several other European countries. Additional work on German capital flows includes Willms (1971) and the interesting papers of Porter (1972), Kouri and Porter (1972a), Argy and Kouri (1972) and Kouri (1973). These latter papers are discussed in Section III below. The University of Bonn model of the German economy incorporated in Project LINK now includes some equations for the German capital account (see Sandermann, 1972; Martiensen and Sandermann, 1973).

Kouri and Porter (1972a) and Argy and Kouri (1972) also report equations for the Netherlands and Italy. Empirical research on the capital account in the Italian balance of payments is being carried out by economists at the Banca d'Italia, and by Basevi and others at the Istituto di Scienze Economiche at the University of Bologna.

Australia is the fourth country studied by Kouri and Porter (1972a), and also is the focus of Zecher's unpublished research (1973) done at the University of Chicago. The econometric model project of the Reserve Bank of Australia has also devoted attention to Australian capital flows.

Despite having worked myself on certain types of capital flow in the Japanese balance of payments (Bryant and Hendershott, 1970, 1972), I am not sure how extensive the research has been on the Japanese capital account. To some degree, this area has been investigated as part of the econometric model projects at the Institute of Economic Research, Kyoto University, at the Economic Planning Agency in Tokyo, and at the Bank of Japan. Amano's forthcoming book (1973b) is one important source of information on this research.

In general, as a selective scanning of the volume reporting on Project LINK (Ball, 1973) will show, most national econometric models have concentrated on the current account in the balance of payments. Given the relative lack of development of the monetary

sectors of these models and the security of data for the capital account that is both reliable and accessible, the omission of equations for the capital account is not surprising. Several participants in Project LINK hope to see the financial and capital-flows aspects of LINK given greater attention in the future (see, for example, Basevi and Waelbroeck, 1973).

Theoretical modelling and empirical study of the Eurocurrency markets has only recently begun to grow rapidly. This lag can be attributed, at least in part, to the fact that appropriate and reliable data on the assets and liabilities of Eurobanks have been difficult to come by. Moreover, time series of sufficient length to allow medium-term econometric work only became available towards the end of the 1950's. Hendershott (1967b), Kwack (1971c), Argy and Hodjera (1973), Mills (1973b), and Herring (1973, Chapter 6) have all considered the determination of Eurodollar interest rates. Papers by Makin (1972) and Marston (1974) attempt to estimate equations explaining deposits in and borrowing from banks in the Eurodollar market. I have myself some uncompleted research (Bryant, 1971) on the demand by nonresidents of the United States for liquid dollar assets. Several recent papers by two economists at the International Monetary Fund (Newson and Sakakibara, 1973a, 1973b, 1974) are important contributions, for their more careful theoretical approach as well as for their empirical efforts.

Black (1971) studied the weekly behavior of the liabilities of U.S. banks to their foreign branches, a very important element of

the behavior of the Eurodollar market during the period of his study; for two comments on Black's paper, see Valentini and Hunt (1972) and Massaro (1972). Mastrapasqua (1973) has also reported equations for the liabilities of U.S. banks to their foreign branches. Unpublished research bearing on this aspect of the Eurodollar market includes the University of Maryland Ph.D. dissertation of Bradshaw (1973) and a paper by Ciccolo and McKelvey (1971).

The distinction between "short-term" and "long-term" financial capital flows is in some respects not analytically interesting; as the discussion in Section II below makes clear, in principle both need to be considered together as part of an integrated theory. See also Section III's discussion of the appropriate degree and type of disaggregation. Moreover, existing statistical data distinguish between them (if they are separated at all) on quite arbitrary grounds -- usually on the basis of whether the original maturity of the financial instrument is greater or less than one year. For these reasons, I have not emphasized this distinction in this paper. Those especially interested in empirical research on portfolio capital flows of a longer-term nature should consult, inter alia, Spittaller's survey article (1971), Miller and Whitman (1970), Lee (1969), Boatwright and Kenton (1972), and the relevant parts of Branson and Hill (1971), Kwack (1972), and Amano (1973b).

Another method of mapping this literature is to classify the contributions by the theoretical approach or type of theoretical specification employed by the researcher.

A majority of the early contributions relied on what came to be called a "flow theory". (See Section II below for a discussion.) This specification was used in part of Bell's work (1962; contrast the form of the equations in Appendices II and III), and is found for example in Kenen (1962), Rhomberg (1964), Powrie (1964), Stein (1965), Kenen (1973), Arndt (1968), Hawkins (1968), and Prachowny (1969).

More recently, the bulk of the research has been based on more or less sophisticated versions of a "stock-adjustment" or portfolio-balance type of specification (again, see Section II below). Branson (1968), Grubel (1968), and Hendershott (1967a) were among the first to criticize the theoretical shortcomings of the earlier research (discussed below). For a representative sampling of contributions based on the portfolio-adjustment approach, see Amano (1973a), Branson and Hill (1971), Bryant and Hendershott (1970), Kwack (1971a), Lee (1969), and Miller and Whitman (1970a).

Most of the studies cited here, with the important exceptions of those dealing with the Canadian capital account, have not shown an especially strong interest in exchange rates and the foreign-exchange markets as such. Of the research focused on this area, Black (1968, 1973) and Helliwell et al. (see the various references cited above) stand out in importance. Additional studies that concentrate attention on exchange-market aspects of capital flows include Stoll (1968), Kesselman (1971), and Dooley (1974).

Hardly any empirical research motivated by a "monetarist" approach to the balance of payments has yet been published. Theoretical papers such as Johnson's (1972), however, have prompted several researchers to try their hand. Examples are Zecher (1973) and Girton and Roper (1974). More work in this vein will probably be available in the near future. On the distinguishing characteristics of the monetarist approach to the balance of payments, see Salop (1973).

Some empirical work recently coming out of the Research Department at the International Monetary Fund -- see for example Kouri and Porter (1972a) -- is difficult to characterize as either "monetarist" or "portfolio-balance" in its approach. In the minds of its authors, it constitutes a synthesis of both approaches.

Finally, to conclude this road map, I should alert the reader to a recent survey by Hodjera (1973), which in a different manner is also a map of the literature on international capital movements (although Hodjera restricts his survey to short-term capital flows).

Although in what follows I discuss selected aspects or results of a number of the individual studies mentioned above, it is not practical to try to summarize in any detail the theoretical specifications used or the empirical results reported. Instead, my limited objectives are to call attention to some important general issues that arise in connection with the theoretical

specifications used in these studies, to point to some unresolved questions of strategy for future research, and to present my own assessment of how far the profession has gotten in our empirical work in this area. The following sections of the paper take up these topics in turn.

II. General Issues of Theoretical Specification

As already noted, it is the so-called portfolio approach to capital flows that underlies the bulk of recent literature in this area. The basic ideas underlying this theoretical approach go back at least to the works of Markowitz (1959) and Tobin (1965).

At their lowest common denominator, all those studies that base themselves on the portfolio approach imply an underlying specification of the following form:

$$F_1^* = f_1(S, R_1, \dots, R_n, \sigma_1, \dots, \sigma_n, X_1, X_2, \dots)$$

$$F_2^* = f_2(S, R_1, \dots, R_n, \sigma_1, \dots, \sigma_n, X_1, X_2, \dots)$$

$$F_n^* = f_n(S, R_1, \dots, R_n, \sigma_1, \dots, \sigma_n, X_1, X_2, \dots)$$

The variables F_1^*, \dots, F_n^* represent desired quantities of the financial instruments held in the decision-making unit's portfolio of assets and liabilities. These functional forms are, in the case of assets, stock demand functions; when F_i^* is a liability, the equation is a stock supply function. Ideally, the system of equations should refer either to an individual decision-making unit or to an aggregation of decision-making units that are reasonably homogeneous in character.^{1/}

^{1/} See Section III below for a discussion of the appropriate degree and type of disaggregation in empirical applications.

The symbol S represents the "scale" variable indexing the size of the decision-making unit's portfolio. In the case of an individual household, for example, the scale variable would typically be taken as wealth (net worth). If the economic unit has no liabilities-- that is, if expected returns from investing do not exceed the expected costs of borrowing by enough to overcome the risk aversion of the unit-- total assets and net worth will be identical. In the more general case where expected asset yields exceed expected liability costs by an amount sufficient to make borrowing desirable, the unit will have a determinate scale if it has aversion to risk and if its marginal utility of wealth is nonincreasing. In principle, with the entire portfolio correctly specified, a balance-sheet constraint will hold such that S will be equal to the sum of the P_i^* (where liabilities are treated as negative assets).

In a true general equilibrium approach to the theory, the scale variable S would be determined endogenously and simultaneously with all the other components of the portfolio. In practice, both theory and empirical applications have assumed that the scale variable can be taken as exogenously determined. For households, this assumption is rationalized by arguing that income can be taken as predetermined (that is, based on past decisions) and that the saving-consumption decision can be analyzed independently of decisions about the composition of the balance sheet.^{2/}

^{1/} See Bryant and Hendershott (1970, pp. 5-6) for a more detailed discussion.
^{2/} See, for example, the discussion in Tobin (1961).

For firms, an analogous rationalization is that profits can be taken as predetermined and that the dividend-retained earnings decision can be studied independently of balance-sheet decisions.

The vector of returns in the above set of equations, the R_i , and the vector of associated risks, the σ_i , are in principle the expected risks and returns associated with each of the assets in the portfolio of the decision-making unit. In the case of liabilities, these vectors are expected borrowing costs and the expected risks associated with these borrowing costs. Each composite return or borrowing cost may have several components: for example, the nominal interest rate, the expected capital gain or loss, and--when assets or liabilities denominated in several different currencies are part of the portfolio--the expected change in relevant exchange rates. In principle, each of the return and risk variables appears in the equation for every asset and liability in the portfolio.

The vector of variables X_1, X_2 , etc. represent all those other variables that are relevant to demand or supply. An example would be the volume of transactions of the decision-making unit. If a variable X_j influences the demand or supply for one particular asset or liability, it must (because of the balance-sheet constraint) influence demand or supply for at least one other asset or liability.

Suppose that one or more of the F_i^* represent a claim on or liability to economic units outside the country in which the holder of the portfolio is resident. There is then the potential for international

Table 1

Hypothetical Balance Sheet of a Japanese City Bank
(June 30, 1973; billions of yen)

| <u>Assets</u> | | <u>Liabilities and Net Worth</u> | |
|--|-----|---|------|
| Liquid assets denominated in yen | LAd | Deposit liabilities (in yen) to Japanese residents | Dj |
| Liquid assets held abroad denominated in \$ and other foreign currencies | LAF | Deposit liabilities (in yen) to foreign residents | Df |
| Loans and discounts (in yen) to domestic customers | Ld | Borrowing in yen from Bank of Japan and other Japanese commercial banks | Bj |
| Loans and discounts (in yen) to foreign residents | Lf | Borrowing denominated in \$ from U.S. Banks | Bus |
| Security holdings denominated in yen | Sd | Borrowing denominated in \$ from Eurodollar market | Be\$ |
| Other assets (in yen) | OA | Other liabilities (in yen) | OL |
| | | Capital Accounts | NW |

capital flows -- changes in assets or liabilities involving transactions between residents of two different countries.

As an example, consider the simplified balance sheet shown in Table 1. The assets and liabilities of a Japanese city bank are broken down into twelve main categories plus net worth. Five of these categories -- LAF, Lf, Df, Eus, and Ee\$, three of which are denominated in foreign currencies -- represent claims on or liabilities to nonresidents of Japan. Changes in any of these five components of the bank's balance sheet give rise to an international capital flow as recorded in Japan's balance of payments. An analysis of any one of these types of capital flow along the lines of the portfolio approach requires the specification of a consistent set of stock asset-demand and liability-supply equations for each of the separately identified components (in this illustration twelve) of the bank's portfolio.^{1/}

Typically, though not exclusively, it is assumed that the demand/supply functions for the decision-making unit are linear homogeneous in the scale variable:

$$F_1^* = \varepsilon_1(R_1, \dots, R_n, \sigma_1, \dots, \sigma_n, \frac{X_1}{S}, \frac{X_2}{S}, \dots) S$$

$$F_2^* = \varepsilon_2(R_1, \dots, R_n, \sigma_1, \dots, \sigma_n, \frac{X_1}{S}, \frac{X_2}{S}, \dots) S$$

$$F_n = \varepsilon_n(R_1, \dots, R_n, \sigma_1, \dots, \sigma_n, \frac{X_1}{S}, \frac{X_2}{S}, \dots) S$$

^{1/} The balance sheet illustrated here is similar to the consolidated balance sheet for all Japanese city banks studied by Bryant and Henderson (1970, 1972). For another illustration, see the balance sheets specified in Black's "two-country model" (1973, pp. 2-10).

This assumption implies that an increase of z per cent in the scale variable S and in the non-return "distribution" variables X will, other things equal, raise the desired stock demand or supply F_i^* by z per cent. This particular specification of the equations has several practical advantages; among other things, it allows the researcher more readily to enforce the balance-sheet constraint in empirical estimation. It also makes it easy to see that the impact of increments in the scale variable on the desired quantities is dependent on the levels of the risk and return variables, and the impact of changes in the return and risk variables is dependent on the level of the scale variable. Flow demands or supplies -- changes in stock asset demands or liability supplies -- are the time rate of change, or first difference, of the above equations:

$$(2a) \quad \Delta F_i^* = \xi_i(\dots)\Delta S + S_{-1}\Delta \xi_i(\dots) \quad i = 1, \dots, n.$$

One can see from this last set of equations the now accepted conclusion that international capital flows, seen in a portfolio-balance perspective, have both a "continuing-flow" and a "stock-adjustment" component. Given a once-for-all change in returns or risks, the existing-stock effect produces capital flows that are also once-for-all in nature (a reallocation of existing portfolios), while a continuing-flow effect persists indefinitely as long as the change in the scale variable is not zero.

To digress for a moment, I should point out that much of the early empirical literature on international capital flows suffered from a lack of familiarity with contemporary trends in monetary theory, and hence prominently featured a confused debate on "stocks versus flows." For example, many studies regressed capital flows on levels of interest rates and other variables such as the level of trade flows. The specification of these equations took the basic form:

$$(3) \quad \Delta F_i = f(R_1, R_2, \dots, X_1, \dots)$$

where R_1 , R_2 , etc. were the levels of nominal interest rates and X might be the flow of imports or exports. This so-called "flow theory" had no theoretical justification when carefully examined. It implied that desired equilibrium stocks of assets or liabilities depend on the sum (integral) of the current and all past values of the relevant return variables, and thus that elasticities with respect to these returns are infinite.^{1/} Similar difficulties arose from the assumed dependence of capital flows (that is, changes in stocks) on the flow, rather than changes in the flow, of imports or exports.

^{1/} Examples of equations with this incorrect specification were reported by Kenen (1962; see also 1973), Powrie (1964), Rhomberg (1964), Stein (1967), Laffer (1967), Arndt (1968), Hawkins (1968), and Prachowny (1959). Bell (1962) was also unclear on this issue. A similar problem was prevalent in many of the theoretical contributions to the internal-external-imbalance literature, where capital flows were made a function of the level of interest rates. Comments on this stock vs. flows controversy include Hendershott (1967), Willett and Forte (1967), and Branson (1970).

An alternative "stock theory" employed in the earlier literature did not suffer from the serious theoretical defects of (3), but was still incomplete. In this latter case, capital flows were regressed on changes in the levels of interest rates and, for example, changes in trade flows:

$$(4) \quad \Delta F_i = g(\Delta R_1, \Delta R_2, \dots, \Delta X_1, \dots)$$

This formulation can be interpreted as approximating the existing-stock responses, but it disregards the continuing-flow effects.^{1/} On theoretical grounds, therefore, the specification in (2) above is clearly preferable to either (3) or (4).

There is one further important addition that one must make to the preceding general description of the portfolio approach. Responses to changes in the variables determining desired demand/supply are typically not immediate. Therefore a complete theoretical framework requires the specification of a relationship between short- and long-run desired holdings of the financial instruments, where demand/supply in the short run -- F_i^s -- is expressed as a function of current and lagged values of all the determinants of long-run desired holdings.^{2/}

^{1/} Bell (1952) estimated equations of this so-called "stock" form. Branson's early work (1968) also employed specifications of this type.

^{2/} Branson (1962) was among the first to incorporate an awareness of the importance of lagged responses into empirical research on capital flows. See also Hendershott (1967).

Such, in the briefest of terms, are the theoretical underpinnings of the portfolio approach to capital flows.^{1/} How faithfully is this framework implemented in actual empirical research?

There are several important respects, I am afraid, in which all of us who have been working in this area tend to be cheating when we claim to be applying a Markowitz-Tobin type of theory to international capital flows. For one thing, many studies are embarrassingly sloppy in defining which decision-making units are actually being studied and what their aggregate balance sheets look like. In principle, studies dealing with changes in assets and liabilities that are international capital flows are concentrating on a subset of the π_1^* for some particular aggregate of economic units.^{2/} In many cases, because authors have not even thought their way through the problems at a conceptual level, readers of the literature on capital flows encounter difficulty in determining who these transactors are, let alone what other assets or liabilities they hold in their portfolios. There are egregious

^{1/} For more detailed discussions of this theoretical framework and its applicability to empirical research, see for example Bryant and Hendershott (1970), Branson and Hill (1971), and Miller and Whittman (1970a).

^{2/} To continue the example given in Table 1 above, Bryant and Hendershott (1970, 1972) were concentrating on changes in the variable B_{us} , aggregated over all Japanese banks.

examples of this sloppiness in much of the early literature (written before the perspective of the portfolio approach became widespread).

And even in the better recent studies -- for example, Branson and Hill (1971) or Miller and Whitman (1972) -- there is little or no emphasis on careful delineation of the decision-making units and their balance sheets.^{1/}

For another thing, many studies simply omit a large number of the variables that the theory argues should be relevant. These omissions sometimes occur for valid reasons. To illustrate, there is typically a severe problem of including all the return variables in any given empirical regression because of collinearity among them. If the collinearity is serious enough, the researcher may argue that he is justified in omitting some of the variables.^{2/} In other instances, however, the researcher may not even attempt to collect all the return variables that are relevant and may not even bother to argue for their exclusion on grounds such as multicollinearity. (Of course, if no explicit attempt is made to specify

^{1/} See also Section III below, where there is a discussion of the decision a prospective researcher must make about the appropriate degree and type of disaggregation of aggregative data.

^{2/} Interest rates in Canada and the United States, for example, tend to be strongly correlated, and this difficulty plagues all the research on Canadian-U.S. capital flows. Eurodollar and U.S. interest rates move closely together. The problem of collinearity is still more severe among interest rates within a particular country; as is clear from (1) above, many of these play an important, separate role in the portfolio decisions of transactors resident within the country.

the appropriate aggregate balance sheet, then one does not even know in theory which return variables are relevant.) Even in cases where a careful researcher excludes certain variables because of practical considerations, one is bound to be left uneasy. We know from studies such as that of Brainard and Tobin (1968) that it is possible to get into serious trouble in financial model building if one forgets too easily about balance-sheet constraints and omitted variables.

The literature pays little more than lip service to the theory in another respect. Risk variables may be mentioned in introductory paragraphs, or in a section describing the theoretical framework which supposedly underpins the empirical research.^{1/} The actual empirical work, however, seldom includes variables purporting to represent these risks. The lip service varies from study to study, but the most that any have done is to attempt on an ad hoc basis to create a few proxy variables to represent these risk effects.

As an example of such an attempt, consider the Miller and Whitman papers (1970a, 1972). They argue that the riskiness of domestic assets in the United States is inversely correlated with fluctuations of aggregate economic activity about its long-term trend (1970a, pp. 162-183). They further posit a relationship

^{1/} See Grubel (1969) for one of the earlier discussions of risk aversion as an explanation for the international diversification of portfolios.

between the capital control programs of the U.S. Government and the riskiness to U.S. asset holders of foreign lending. Finally, they argue that:

. . . since risk is a manifestation of imperfect information, the risk-estimate associated with an asset should diminish as information concerning the probable return on the asset increases. For this reason, we hypothesize that there has been a secular downward trend in σ_y (the risk of investing abroad), stemming from the increase in knowledge and communications, which symbolizes the gradual movement toward integration of international short-term capital markets since World War II (1972, p. 264).

In actual practice, what are Miller and Whitman's empirical proxies? They are deviations of U.S. GNP (not seasonally adjusted) from its trend regression line over the estimation period; a dummy variable equal to zero until the first quarter of 1965 and unity thereafter; and a simple time trend. The reader can judge for himself whether these variables seem convincing proxies for the risks discussed in portfolio theory.^{1/}

The portfolio-choice theory worked out by Markowitz and Tobin also argues that covariances among returns are important. The existing empirical literature on capital flows does not even bother to pay lip service to these covariances. They are typically not mentioned at all in the paragraphs of a study describing its

^{1/} Miller and Whitman pay more attention to this problem than most researchers, and therefore deserve less criticism than the average study.

theoretical framework.^{1/} (The same criticism also applies to the exposition above.)

I noted earlier that the returns and risks that are relevant in the theoretical specifications are expected effective returns and risks. Most of the existing empirical research sidesteps the question of expectations and expectations-formation altogether. It simply uses observed values of the variables, making no effort to collect data on expectations or to formulate proxy variables thought to be correlated with the theoretically appropriate expected values. An outstanding exception is Black's application (1973, pp. 22-27, 39-52) of the rational expectations hypothesis to exchange markets.

How important are such divergences between the theory believed to be relevant and the equations that are actually estimated in practice? If we answer this question honestly, we have to admit that we do not know.

In all empirical work, it is true, there are serious problems in obtaining empirical approximations for the theoretical constructs that the theory says is relevant. These problems of obtaining suitable empirical approximations may not be any more difficult in the area of international capital flows than in other areas of empirical research. Moreover, one often hears an argument -- intended to be comforting -- that empirical equations actually estimated

^{1/} Miller and Whiteman show the covariance terms in their theoretical discussion, but do not try to incorporate them in the empirical analysis.

would not fit very well if omitted considerations were really important. In other words, it is argued, one can relax about the divergences between theory and empirical practice so long as one's estimated equations fit the data reasonably well.

This line of reasoning, while I believe it has some validity, still does not comfort me very much. It reminds me of the story of a woman who walks up to a street corner to find a man standing there snapping his fingers, and looking all around in different directions. The woman asks the man: "Why are you standing on this street corner snapping your fingers?". The man replies, "I'm snapping my fingers to keep the tigers away." The woman with a puzzled look on her face asks the man: "You don't expect me to believe that story, do you?" And the man, continuing to snap his fingers while he looks over his shoulder, says: "You don't see any tigers hereabouts, do you?"

The argument that our estimated equations would not fit very well if we had failed to include those explanatory variables which are quantitatively most important is somewhat analogous to the man's contention that the tigers are staying away because he keeps snapping his fingers. For a high value of the coefficient of determination in a reported regression is not sufficient evidence that the regression actually represents a good approximation to economic behavior. Few researchers are rigorous in specifying a hypothesis and then rigorously testing that one hypothesis alone. It is tempting to rerun

equations to see what would happen if this variable were included or that variable omitted. By the end of such a process -- which can easily, often inadvertently, turn into a fishing expedition -- one ought to have greatly reduced confidence in the final results. Strictly speaking, the traditional tests of statistical significance are not valid. Certainly, one cannot convincingly claim that any omitted considerations that are theoretically relevant but not embodied in the empirical estimates really are unimportant.^{1/}

In Bryant and Hendershott (1972, pp. 227-236), a number of deliberately mis-specified equations^{1/} were reported for purposes of comparison. Several of the mis-specified equations had standard errors of estimate that compared favorably with the SEE's from more correctly specified equations. Yet the mis-specified equations implied implausible or even ludicrous economic behavior. These results demonstrate all too clearly that a researcher may frequently be unable to discriminate, in purely statistical terms, between alternative imperfect specifications, and that fishing expeditions are virtually bound to be "successful" if one is satisfied with merely finding some specification that will give a good statistical fit for the sample period.

^{1/} For another view in a similar vein, see Røgnberg (1972, pp. 314-318).

By way of another illustration of the gap between theory and empirical practice, consider a particular set of problems that arise in connection with the scale variable. There are, of course, enormous headaches involved in obtaining adequate data either for the international capital flows themselves or for an appropriate scale variable. Lack of adequate data forces rather drastic compromises. One possible compromise is to omit the scale variable altogether from one's estimated equations. This, for example, is the route chosen by Branson and Hill in their study of capital movements among OECD countries (1971, p. 27ff). Whatever the empirical consequences of this assumption, omission of the scale variable clearly guts the theory. Another compromise is to make some rather strong, arbitrary assumptions about the scale variable. An example here would be the studies by Miller and Whitman of short-term capital flows in the U.S. balance of payments (1972, 1970b). Their theory calls not only for a scale variable to represent the net worth of economic units resident in the United States, but also a scale variable representing the aggregate net worth of foreigners. Data for the latter scale variable is of course impossible to obtain in practice. It is difficult even to construct a weighted average of foreign GNP's to serve as a proxy. Miller and Whitman therefore resort to an arbitrary assumption that the ratio of the foreign scale variable to the domestic scale variable is constant over the period of their study. All things considered, this assumption may have been pragmatically reasonable. It is nevertheless impossible to

justify on the basis of the theory and even stretches the imagination a bit to assume it was approximately operative in practice.

Even if data are available for the scale variable, the assumption of linear homogeneity -- see equation (2) above -- cannot be justified on the basis of the portfolio-balance theory ^{1/} per se. Tests of this homogeneity assumption, moreover, are seldom attempted. Branson and Hill (1971) did make an effort to allow for the possibility of non-homogeneity in their empirical specification for certain capital flows in the U.S. balance of payments. I suspect, however, that in those few equations in their study where there is an apparent significant departure from homogeneity -- see, for example, the equation on page 18 for U.S. short-term liabilities to foreigners -- other factors are at work in the equations and the empirical estimates do not represent a good test of the homogeneity assumption. Bryant and Hendershott (1972, pp. 224-25) made an explicit attempt to test this assumption within the context of a non-linear regression program. In effect (using the notation above), they estimated an equation of the following form:

$$(5) \quad F_i^s = \beta_i(\dots) S^\phi$$

Instead of assuming that the parameter ϕ was equal to unity, in other

^{1/} For a more extended discussion of this point, see Bryant and Hendershott (1970, p.7). See also Norman Hill's discussion of this point in his comments in this volume (p. . . . below).

words, they allowed the parameter to take on any positive value. In fact, the estimate of ϕ was close to unity, lending some apparent support to the use of the homogeneity assumption. But this isolated test is hardly conclusive evidence about the general applicability of the assumption.

Another respect in which the specifications used in the empirical literature fall short of what would be desirable is their failure adequately to incorporate the effects of exchange-rate changes. Here again, most studies pay lip service to the importance of exchange rates, while doing little, if anything, to try to take these effects into account. The most careful and interesting work of which I am aware has been done by Black (1973) and by Helliwell and his colleagues (1971, 1972b, 1972c).^{1/} A distinguishing feature of these studies is the endogenous determination of spot and forward exchange rates within a model that specifies all the demands and supplies for foreign exchange. This is a big improvement over specifications that either ignore exchange rates altogether, or include them simply as an exogenous element in the return variables entered on the right-hand side of the equation.

In view of the international monetary upheavals of the 1971-73 period, which produced such dramatic alterations in exchange regimes,

^{1/} For earlier studies that recognized the importance of the problem, see Stein (1965) and Black (1968). Readers especially interested in the relationships between capital flows and exchange rates should also consult Stoll (1968) and Kessleman (1971). Dooley (1974) has recently completed a paper which I have not yet had time to study carefully.

an explicit consideration of exchange rates in research on international capital flows has become even more important. Indeed, now that a majority of major exchange rates are floating subject to management via official intervention, researchers studying capital flows have no reasonable alternative: they must, by a quantum jump, increase the sophistication with which they handle this set of problems.

To make matters still more difficult, basic questions of research strategy are involved for those who try to tackle the capital account in the balance of payments as a whole in their empirical work. The traditional approach -- see, for example, Kwack and Schink (1972) or Branson and Hill (1971) -- has been to estimate separate equations for each component of the capital account other than the change in official reserves, with the exchange rate assumed to be determined exogenously. The change in reserves (the official settlements balance) is then derived from the balance-of-payments identity. Even with an exchange regime of the Bretton-Woods type, this approach had unsatisfactory features.^{1/} With widespread floating of exchange rates, the rationale for this traditional approach has been undermined quite seriously.

^{1/} Within the Bretton-Woods exchange margins, spot exchange rates were endogenous variables: forward exchange rates were not even constrained by margins. Moreover, movements of both spot and forward rates had important effects on exchange-rate expectations (see, for example, White, 1968), which in turn were influential determinants of capital flows (especially at times when the ability or willingness of governments to maintain the spot margins were suspect). To complete the circle, capital flows often triggered changes in the spot margins. Hence, especially over some longer time period, there were many elements of artificiality in treating exchange rates as exogenously determined outside one's model of the capital account.

The line of attack followed in the papers of Helliwell and his various collaborators may turn out to be more promising. This alternative approach involves direct specification of one or more equations describing the intervention behavior of monetary authorities in the exchange market. (Looked at from another perspective, these equations can be construed as modeling the official demand for reserves.) Such equations, taken together with current-account and certain capital-account net demands for foreign exchange, simultaneously determine the exchange rate and the change in official reserves. The residual item derived from the balance-of-payments identity is then that part of net capital flows not already explicitly accounted for by behavioral equations. The strength of this approach lies in its possibilities for the modeling of many alternative exchange regimes (including, of course, managed floating) and its possibilities for the specification of official exchange-market intervention as an important endogenous component of the model.

Note, however, that even this latter approach runs into great difficulty in circumstances of widespread floating. The Canadian models of Helliwell and his colleagues could reasonably use the Canadian dollar/U.S. dollar cross rate as "the exchange rate" so long as any change in the Canadian dollar vis-a-vis the United States also involved a similar change against third currencies. With widespread floating, no single cross rate -- even for a country such as Canada --

can adequately proxy for "the exchange rate." One has no choice but to resort to concepts such as the "effective exchange rate" and attempt to approximate them empirically with various weighted averages. Yet this path runs right through the middle of the familiar swamp of index-number problems. Difficulties of this sort are particularly severe when modeling the exchange regime for a model of the U.S. balance of payments.

My main purpose here has been to point out some respects in which the empirical literature relies on equation specifications that tend to be theoretically invalid or inappropriate. Still another important illustration is the failure of the literature to incorporate the effects of governmental restrictions on international capital flows. None of the studies with which I am familiar, with the exception of Bryant and Hendershott (1970, 1972), has even tried to cope with this problem at the level of the theoretical framework. Instead, researchers have relied on doing something ad hoc when it comes time actually to run a regression. Typically, simple off-on dummy variables are inserted into the empirical regressions in an ad hoc way with no attempt to blend the underlying theory of the capital flows with the governmental restrictions which stand in the way of private individuals carrying out their portfolio decisions.^{1/} Needless to say, the form

^{1/} For a useful summary of the limited work that has been done in this area, see Cheng (1973). See also Kwack (1973).

of such dummy variables, which is usually highly arbitrary, forces a particular (arbitrary) interpretation of the effectiveness of the capital controls.

As an example, consider the equation reported in Branson and Hill (1971, pp. 13-15) for changes in short-term claims of U.S. residents on foreigners:

$$\Delta C_t^S = f(\text{U.S. wealth variable, U.S. interest rate, U.K. interest rate, U.S. merchandise exports}) + \text{seasonal dummies} - 392 \text{ IET1} - 607 \text{ DF1} - 213 \text{ DF2}.$$

In this equation, IET1, DF1, and DF2 are all dummy variables. Branson and Hill explain as follows:

"Since the target of the [U.S. capital control] programs is the stock of outstanding claims, we would expect the annual tightening of the programs to be associated with continuing inflows, or reduced outflows, of capital. Thus we have added two dummy variables to the analysis to reflect the programs. The first, DF1, is set to unity in 1965I-1965III and zero elsewhere, reflecting the fact that the largest change in the program occurred with its initial imposition. A second dummy, DF2, is set to zero through 1965III, and unity from 1965IV on to reflect the continued tightening of the programs. These variables may be crude, but exact quantification of increases in pressure through "moral suasion" and a change from voluntary to mandatory is impossible."

"The IET dummy is 1.0 in 1963III. This dummy was added because of a large unexplained (the IET applied only to long-term capital) residual in that quarter, and effectively drops that observation from the regression data."

As Branson and Hill acknowledge, since the Interest Equalization Tax did not even apply to the capital flows in their equation, their IET dummy variable serves only the purpose of throwing out an awkward observation; it certainly does not indicate that the Interest Equalization Tax reduced U.S. short-term claims on foreigners by \$392 million in the third quarter of 1963. The other two dummy variables, if taken at face value, suggest that the U.S. Voluntary Foreign Credit Restraint (VFCR) program reduced the flow of U.S. short-term lending to foreigners by \$607 million (compared with what it otherwise would have been) in each of the first three quarters of 1965, and that in each quarter thereafter reduced the flow by \$213 million.

The arbitrary specification of the impacts of the VFCR program by Branson and Hill may be contrasted with an equally arbitrary and incompatible -- specification in the Miller and Whitman studies of the same capital flow. As noted above, Miller and Whitman employ a dummy variable equal to zero through the first quarter of 1965 and equal to unity in each quarter thereafter. They also employ a "partial-adjustment" specification in an effort to capture lagged responses, which inter alia forces the impact of their VFCR dummy variable into the same geometrically-decaying lag pattern forced onto all the other explanatory variables in their equation. Since Miller and Whitman estimate a low value (.25) of the "speed-of-adjustment" coefficient in

their preferred equation (1972, pp. 272-278), their results -- if taken at face value -- suggest that the effects of the VFCR in restraining U.S. short-term lending to foreigners were still gradually building up by the first quarter of 1966 and were still having only two-thirds of their eventual impact.

To put the point mildly, neither the estimates -- or, to speak more precisely, the assumptions -- of Branson and Hill nor those of Miller and Whitman inspire confidence as reliable indications of the impacts of the VFCR program.^{1/}

If government controls affect the returns or other variables in a way that can be specified more or less exactly, the preferable thing to do is to attempt to adjust the return variables directly to take these effects into account. For example, in studying purchases of foreign securities by U.S. residents or long-term lending by U.S. banks to foreigners in the 1963-73 period, one can add the rate of the Interest Equalization Tax directly on to the appropriate return variable used in the regression. Similar quantitative adjustments can be made if, for example, negative interest charges or reserve requirements are imposed on bank liabilities.

But usually the problem is more complicated. It is worst of all when the governmental restraint or stimulus is carried out just by administrative guidance (commonly known by such euphemisms as jaw-boning, car-stroking, threatening to use the big club in the closet,

^{1/} Yet note that these dummy variables, in both the Branson-Hill and Miller-Whitman studies, are highly significant by the customary statistical tests!

etc.) One suggestion made by Bryant and Hendershott (1970) is that, except in cases where controls are known to be strictly binding on all economic units, the researcher can assume that the tightening or relaxation of controls will reduce or increase, but not dampen altogether, the response of desired quantities to changes in their economic determinants. For some particular capital flow being controlled, this view can be represented formally in the following way:

$$(6) \quad F_i = \alpha F_i^s$$

$$(7) \quad \alpha = h(C_1, C_2, C_3, \dots)$$

where F_i is the observed quantity of asset demand or liability supply, and as before F_i^s is short-run desired demand. In this formulation alpha would equal unity when the controls are absent or not binding at all, and would be less than unity when the controls keep the observed quantity below the desired quantity. In the case of capital controls which government authorities use to stimulate capital flows, alpha could take on values greater than unity. This treatment of the effects of capital controls requires the researcher to construct variables -- C_1, C_2 , etc. -- which reflect changes in the intensity of the different controls.^{1/} This is, of course, an extremely difficult task. But it

^{1/} For example, Bryant and Hendershott attempted to construct a variable to measure the effects of the U.S. VIGR program (1970, pp. 60-62). See Bryant and Hendershott (1972, pp. 223-230) for comparisons of alternative specifications (including the use of an off-on dummy variable).

has the merit of forcing the researcher to try to specify explicitly the manner in which the controls have their impacts and how the intensity of the controls varies over the period of the study -- something that should in any case be attempted.^{1/} This type of specification also tends to force the researcher to use nonlinear estimation techniques. While these techniques involve added expense and inconvenience, these costs may well be worth it if the alternative is nothing better than the use of ad hoc dummy variables that do nothing more than alter the constant term in empirical regressions.

The use of various types of capital controls has been much more the rule than the exception throughout the postwar period. For a survey of countries' practices, see for example Hills (1972, 1973a) or the annual reports of the International Monetary Fund on exchange restrictions; see also Johnson (1973). Moreover, if anything, governments have been increasingly prone to utilize these controls in recent years. Thus, despite the practice in the literature so far, it is simply not possible to downplay the importance of these controls and still do valid empirical research on the capital accounts of most countries' balances of payments.

I have said little up to this point about estimation problems per se, and for a good reason: no estimation technique, however powerful or sophisticated, can produce satisfactory empirical results if the theoretical specification itself is inappropriate. Moreover,

^{1/} Perhaps this point should be put even more strongly: a researcher has no business applying econometric techniques to the data until such an attempt has been made!

inadequate theory or faulty empirical approximations of theoretical constructs are the most frequent causes of estimation difficulties (e.g., serial correlation of the residuals).

Nonetheless, it is probably useful here to remind the reader that the simultaneous-equations problem -- already referred to implicitly in the discussion above about exchange rates -- is an important complication in empirical research on capital flows. Turn back for a moment to the set of equations (1), in which the various F_i depend on the vector of return variables, the R_i . In principle, this causation is not just one-way: the return variable R_j on some financial instrument j will be determined by all the demands and supplies for j , including the particular demand or supply F_j of the transactor whose portfolio is specified in (1). The more important is the particular economic unit being studied in relation to the market for j -- and a fortiori if one is studying a large aggregation of economic units -- the less valid will it be to concentrate on the influence of R_j on F_j and to ignore the simultaneous influence of F_j in determining R_j .

For many purposes, therefore, and especially in macroeconomic studies, it may be necessary to specify a theoretical model in which both F_j and R_j appear as endogenous variables. To be useful in quantitative research, of course, such a model must be well enough articulated to yield explicit equation specifications that can be empirically tested.

Stein (1965) was one of the first to stress the need for a simultaneous-equations approach in the context of international capital flows. For additional references, see Black (1968), Miller and Whitman (1970b), Bryant and Hendershott (1970, Appendix A), Kouri and Porter (1972a), Herring (1973), and Norman Miller's comments in this volume (pp. below). For two examples of papers that derive reduced-form equations from a more complete model, thus solving out an endogenous variable and removing one source of simultaneous-equations bias, see Kouri and Porter (1972a) and Black (1973).

III. Some Unresolved Questions of Strategy for Future Research

As the preceding discussion indicates, there is no shortage of inadequacies in the existing literature to which current and future researchers, aspiring to remedy the deficiencies in the work of their precursors, can address themselves. Quite the contrary: there is a surfeit of opportunities, and only a shaky foundation of existing knowledge on which to build.

To make matters still more difficult for the prospective researcher, there are several major questions of strategy to be grappled with at the outset of one's work. At the present time, we simply do not know enough even to give sound advice on the most appropriate resolution of these questions of research strategy. No wonder, then, that one cannot yet narrow down the choice of research topics and methods of attack to a small range of possibilities and confidently claim it is within that particular range that future research will have the highest pay-off.

One of the most important strategic questions facing the prospective researcher is the degree to which he or she should pursue a "structural," as contrasted with a "reduced-form," approach. The ramifications of this initial choice may be particularly important if one aspires to carry out research relevant to government policy decisions.

To see why this may be so, the reader should ask himself how far the existing empirical research in this area carries us

towards the objective of improving the formulation of economic policy. In an open economy, the effects of monetary policy on capital flows may be of such importance that they alone may determine whether or not monetary policy can be successful in facilitating the attainment of desired levels of real activity, employment, and prices. Yet the bulk of the empirical research surveyed in this paper does not allow one to trace the impacts of a change in a policy instrument (e.g., an open-market sale of securities by the central bank) all the way through the financial and real sectors of the economy to the ultimate effects on the balance of payments, domestic activity, and the price level. Typically, the estimated equations for capital flows contain explanatory variables -- for example, interest rates or activity variables -- that are rather far removed in the chain of causation from the policy instruments themselves. Before one can predict the impact of changes in policy instruments on capital flows, therefore, it is necessary to predict the impact of changes in the policy instruments on the explanatory variables in the estimated equations. Since this latter prediction problem is very difficult (the relevant empirical research being either non-existent or also at a relatively primitive stage), the estimated equations for capital flows by themselves are of limited help to policymakers.

An appreciation of this dilemma has led some researchers to attempt to estimate "reduced-form" equations, with policy instruments appearing explicitly in the estimated equations as

regressors. If successful, such an approach would obviate the need for an elaborate system of structural equations or the full-scale econometric models that would otherwise be required.

For example, interesting work along these lines has been done recently at the International Monetary Fund by Pentti Kouri, Michael Porter, and Victor Argy.^{1/} They begin their analysis with a simplified portfolio-adjustment model focused on aggregative economic behavior in a small open economy. By solving out the domestic interest rate, they are left with an estimating equation which relates capital flows to various monetary policy variables, the current account in the country's balance of payments, the change in domestic income, and the change in a foreign interest rate. Foreign variables, in particular foreign interest rates, are assumed to be exogenous in the analysis; that is, the country is assumed to be small enough in relation to the rest of the world so that monetary conditions and real activity in the rest of the world can be taken as independent of activity and monetary conditions in the country being studied. As noted above, this research at the IMF has produced some interesting empirical regressions for capital flows in the balances of payments of Germany, Italy, the Netherlands, and Australia.

^{1/} See Kouri and Porter (1972a, 1972b), Porter (1972), Argy and Kouri (1972), and Kouri (1973).

From the point of view of policy, however, it can be argued that equations of the type estimated by Kouri and Porter -- even if they turn out to be reliable relationships -- may not get us much further than the traditional equations in the literature that purport to be closer to "structural" relationships. In order to use the empirical results of, say, Branson and Hill, Kwack, or Miller and Whitman to forecast the impacts of monetary policy on capital-flow aggregates, one requires separate predictions of domestic activity and domestic interest rates, as well predictions of foreign explanatory variables. The equations of Kouri and Porter require separate predictions of foreign variables, of domestic activity, and of the current account in the balance of payments. The practical difference between the two approaches in their present incarnations thus boils down to the question of whether or not one is in a better position to predict the current account in the balance of payments than domestic return variables. The record of explaining and forecasting changes in the current account balance is not especially good, as we know from some of the research that is surveyed elsewhere in this volume by Magee, Stern, and Klein. Another drawback of the Kouri-Porter approach that mitigates its practical usefulness in many circumstances is its heavy reliance on the small-country assumption. Without an explicit relaxation of this assumption, for example, the approach could not be applied to the United States.

Further work needs to be done along the lines pursued by Kouri and Porter before we can more confidently judge the relative merits of this approach and its practical usefulness to policymakers. But this work, as well as recent empirical studies that take the "monetarist" approach to the balance of payments as their starting point (see Section I above), do serve to emphasize the need for prospective researchers to be more self-conscious about their research objectives -- and in particular the balance they want to strike between on the one hand analyzing behavioral, structural relationships, and on the other hand finding a reduced-form short cut to prediction of the effects of policy instruments on ultimate target variables.

The choice of emphasis between a structural or a reduced-form approach would pose no problem, of course, if one could be confident of a reduced-form approach being successful and reliable. With such a happy state of affairs, those who had an intrinsic interest in the structural relationships underlying economic behavior would focus their efforts on these relationships, while everyone else would clearly choose the short cut of going directly to the reduced forms. But in practice, there is substantial uncertainty as to whether reliable short cuts actually exist. Unfortunately, in other words, it is still essentially a matter of personal hunch and judgment -- not of verifiable fact -- whether reliable predictions of the effects

of policy instruments on target variables can be made without first spelling out the underlying structural models.

My own personal bias is to doubt the existence of both free lunches and reliable short cuts. Yet I feel sure that a full-blown "structuralist" approach to the empirical study of international capital flows will take years, if not decades, to come to real fruition. Important policy decisions will continue to be made in the meantime -- for the most part, in a glass darkly. Under the circumstances, it seems fully justifiable -- at least for a further exploratory period of several years -- to have the available resources dispersed all along the spectrum from the structural microeconomic extreme to the other extreme of highly-aggregative reduced-form equations.

Regardless of the balance one strikes between a structural and reduced-form approach, but especially if one is inclined to emphasize structural relationships, a researcher in this area must resolve another major question of strategy at the outset of his work: what is the appropriate degree and type of disaggregation?^{1/} As in the preceding case, one's answer to this question will partly depend on the specific objectives of the research; if one has an intrinsic

^{1/} Leamer and Stern (1972) discuss this question in their survey of the problems in the theory and empirical estimation of international capital movements.

interest in the behavior of capital flows generated by commercial banks, for example, it is obviously essential to disaggregate the capital-flow data sufficiently to eliminate changes in the assets and liabilities of nonbanks. But suppose one's primary objective is to obtain, for some given country, predictions of the capital account as a whole. At what point do the additional costs associated with further disaggregation come to outweigh the incremental benefits (if such benefits actually materialize) of more precise and reliable predictions? ^{1/}

The question of the appropriate degree of disaggregation of aggregative data turns out to be, in practice, inseparably related to decisions about the appropriate type of disaggregation. There are three main conceptual possibilities: disaggregation by transactor (type of decision-making unit), by geographical region (point of origin or destination of the capital flows), or by instrument (characteristics of the assets or liabilities being exchanged).

A moment's recollection will make it evident that the theoretical framework spelled out at the beginning of Section II above presupposes all three types of disaggregation, since it assumes the existence of detailed data on the balance sheets and income accounts of individual economic units. In practice, researchers in this area seldom can acquire such data for individual economic units. And, to repeat, even if these data were available, one's objective will often be to analyze or

^{1/} Contrast, for example, the approaches of Kwack (1971a, 1971b, 1972) and Herring (1973), both of whom wish to produce estimates for the U.S. capital account as a whole. Herring employs a single equation for the entire capital account (including U.S. Government capital and direct investment). Kwack's model is the most disaggregated of any that deal with the entire capital account.

predict capital flows at a more aggregative level. To make one's research germane to policy decisions, it is certainly the case that conclusions must have a bearing on aggregative data. Hence the familiar problems of all empirical research: the microtheory of individual behavior has to be supplemented with numerous additional assumptions and questionable approximations before it is possible to carry out any empirical testing at all.

When data are aggregated over a set of relatively homogeneous economic units, the hope is that an aggregate function which takes the same general form as the individual functions will not introduce an unacceptably high degree of aggregation bias. The more heterogeneous the individual economic units in the group and the more heterogeneous the economic environments in which they operate, however, the less confidently one can hold to such a hope.

The rationale for some significant amount of disaggregation of aggregative data by type of decision-making unit is strong. Commercial banks, nonbank financial institutions, nonfinancial corporations, government agencies, and households, for example, all face different constraints and regulatory environments. Differences in their asset preferences or other aspects of their economic behavior may be quite important. Moreover, some disaggregation by type of transactor may be essential if one is to match data for capital flows with appropriate data for the relevant economic determinants (e.g., the scale variable

S).

Some of the greatest disparities among economic units and economic environments are those created by or embodied in national political boundaries. It is certainly the case that the nature and timing of economic policies of national governments -- to take the most pertinent example, policies intended directly to influence international capital flows -- differ markedly across countries. Even in instances where it is less difficult to swallow one's doubts about heterogeneity of environments across countries, there are no easy, clearly appropriate ways to consolidate economic conditions across a group of countries into a few summary variables.^{1/} These considerations argue strongly for some significant degree of disaggregation by geographical region.

Some data on international capital flows are collected by type of financial instrument, with little or no cross-classification by type of transactor or geographical region. Moreover, published data for most countries tend to be in this format. Thus a researcher's

^{1/} For example, in the study of any given capital flow, what weights should be used to construct weighted-average variables for GNP, or monetary aggregates, or short-term interest rates in the countries of the European Economic Community? We are of course back again in the swamp of index-number problems as soon as we ask such questions.

need, based on his theoretical framework, for some disaggregation along this third line may well be satisfied.

Even if adequate data were available to allow a significant degree of disaggregation along all three lines simultaneously (typically they are not), researchers in this area would still be faced with an enormously difficult judgment. To put the matter bluntly, the requirements of theory and the dictates of practicality conflict violently. And I know of no easy generalizations with which to help, or comfort, those who must make the awkward compromises.^{1/}

The problem of the availability and reliability of data deserves a comment in its own right. For here, too, prospective researchers must resolve for themselves a strategic question: how much effort to devote to data collection per se. There is of course the inevitable difficulty that "whatever we measure is never quite the thing that represents, or closely corresponds to, the theoretical concept of our theories" (Machlup, 1972). Nonetheless, many researchers may lean too heavily on this fact of life as a rationalization for avoiding the effort to acquire institutional knowledge and for devoting

^{1/} As suggested earlier, it is the "structural" purist who most agonizes over these inevitable compromises. The pure monetarist who aspires to explain the entire balance of payments with a single reduced-form equation may feel he has hurdled the dilemma entirely. Everyone else must suffer to a greater or lesser degree somewhere in between.

as little time as possible to the tedious job of careful examination of all potential sources of data. Yet innovative, diligent searching can often turn up information that will either greatly improve a data set or, alternatively, prevent an investigator from drawing unwarranted inferences from statistics that purport to measure one thing and actually measure something else.^{1/}

Careful scrutiny and compilation of the data are especially important in the field of international capital flows because far too few data are available, and much of what is available is of poor quality. Any researcher who has used balance-of-payments data for the United States in an empirical study but has not read the report of the Bernstein Committee (Review Committee, 1965) should be put in a straitjacket until he has done so -- perhaps twice.^{2/} For a study that discusses data problems in some detail and attempts to construct an integrated matrix of capital flows across individual countries for the 1950-54 period, see Michael (1971). Smith (1967) is another paper that highlights problems with the consistency and reliability of the data. For illustrative examples of serious data problems in the context of an empirical study, see the discussion in Bryant and Hendershott (1970, Appendices B and C).

^{1/} Compare Machlup's "grumble" (1972, p. 4): "too often researchers do not question the meanings of the terms with which they work; they are diving into piles of third-rate statistical data which they believe, or assume, to be suitable proxies for the vague or ambiguous concepts with which their supposedly first-rate models are furnished."

^{2/} For another useful document, that contains some of the details (including reporting forms) of how U.S. data on international capital flows are collected and processed, see the guide to international financial statistics compiled by the Federal Reserve Bank of Atlanta (1972).

There is more than a little doubt in my mind whether empirical research on international capital flows can make great progress in the future in the absence of substantial improvement in the availability and quality of the basic data. In fact, it would be surprising if this were not so. No one has yet discovered a way to squeeze blood out of a turnip.

IV. Concluding Comment: How Far Does Existing Empirical Knowledge Take Us?

One way to assess the empirical research in the area surveyed in this paper is to ask: How much do we know now that we did not know, say, fifteen years ago? My summary answer to this question is that we do have some useful knowledge we did not have then; but we still know much less than we need or would like to know.

One thing about which we have definitely learned something is the responsiveness of international capital flows to changes in monetary conditions. There is ample evidence from a number of recent studies that capital flows are quite interest elastic. Representative results may be found in Branson and Hill (1971), Kwack's research (1972), the studies of Miller and Whitman (1972) (1970b), those by Bryant and Hendershott (1972), the paper by Helliwell and Maxwell on Canadian capital flows (1972b), Black's recent work (1973), and Herring's Ph.D. dissertation (1973).

The evidence from these recent studies contrasts sharply with the conclusions drawn from some of the earliest research in this area. Neither Bell (1962) nor Kenen (1963), for example, thought that changes in interest rates would produce sizable impacts on the capital account. Today, as Kenen notes in the introduction to his 1973 monograph, there tends to be little dispute: "scholars and practitioners alike are thoroughly persuaded that monetary policy has an immediate, pervasive effect on private capital movements" (1973, p. 3).

Branson and Hill, and Kwack, have carried their work far enough to be able to put forward estimates of the total net effect on the overall U.S. balance of payments of a change in relative monetary conditions in the United States and abroad.^{1/} Herring, following the lead of Branson and Hill, has attempted the ambitious objective of estimating equations for both interest rates and the net capital account for six major industrial countries. In his

^{1/} For an overview of Kwack's quantitative results, see Kwack and Schink (1972), especially pp. 18-27. For an overview of the quantitative results for the United States in Branson and Hill (1971), see the table of stock-shift multipliers on page 25 and the associated discussion in the text. The following quotation indicates the order of magnitude of the Branson-Hill estimated effects on the net financial private capital account (KA) as a whole: "For example, a one-point increase in the U.S. short-term rate, by itself, will temporarily reduce the KA deficit (or increase the surplus) by a one-time stock-shift effect of \$2.6 billion over three quarters. With assets growing at about 7 percent per year, this would give a continuing improvement in the KA balance equal initially to \$180 million at an annual rate. But if the Eurodollar rate adjusts point-for-point to movements in the U.S. rate, the total coefficient of a one-point change in the U.S. rate on KA is \$1.5 (= 2.6-1.1) billion, while the effect of a one-point change in the Eurodollar rate relative to the U.S. rate is \$1.1 billion. Thus in this case the one-point increase in the U.S. bill rate would give a \$1.5 billion net stock-shift effect over three quarters, with an initial continuing-flow effect of about \$77 million at an annual rate. If the U.S. bill rate increase was accompanied by an increase in velocity of about 0.1, not unreasonable in U.S. experience, another \$602 million would be added to the stock-shift effect, and \$42 million to the initial level of the continuing-flow." For an illustrative discussion of how such results might be used for projections of the capital account, see Branson (1970, pp. 253-257); note, however, that the empirical results used in this 1970 article were superseded by the later estimates in Branson and Hill (1971).

concluding chapter, Herring has pulled these estimates together so as to describe, albeit very roughly, the linkages that (partially) integrate national financial markets into the beginnings of a world market for money and capital.^{1/}

Although all these estimates are subject to the problems noted in Section II, and hence probably subject to very large margins of error, they nonetheless represent the best quantitative evidence we so far have on the interrelationships between international capital flows and national monetary conditions.

The evidence is also fairly strong that many individual capital flows are associated with trade flows of one sort or another. Lending by banks or inter-company credits often have an important element of trade financing. For examples, note the relationship between Japanese imports and borrowing by Japanese banks from the United States studied in Bryant and Hendershott (1970, 1972), or the dependence of several components of the U.S. capital account on trade flows or the trade balance in Kwack's model (1972). On the subject of the relationship between capital movements and trade flows, see also Bent Hansen (1961).

We know rather less about the relationships between capital flows and fluctuations in growth and business activity: It is true that variables representing the level of or fluctuations in business

^{1/} Herring estimated a single, aggregative equation for the capital account as a whole (for the United States as well as for each other country). An overview of his results can be found in Chapter 7, pp. 287-315; see especially Tables 7.4 and 7.5.

activity appear in many of the empirical equations that have been estimated. But the interpretation of these results is often strained. For example, as noted above, Miller and Whitman regard deviations from trend in business activity as a proxy for risk. Others interpret real activity variables as saying something about the demand for money (Kouri and Porter, 1972a). In my view, it may be some time before we will know how to interpret the correlations that have been observed in the actual empirical work already done.

I have already stressed in Section II above the failure of equation specifications adequately to incorporate the effects of exchange-rate changes. It is hardly surprising, therefore, that we have a very imperfect understanding of the relationships between capital flows, spot and forward exchange rates, and exchange-rate expectations. What knowledge we do have is mostly qualitative and tends to come, not from empirical research, but from the exploration of theoretical models. The main exceptions to this generalization, as already noted, are Black (1973) and Kelliwell and his collaborators (1971, 1972b)^{1/}.

I am still less confident that we have useful knowledge about the time pattern of all the various responses of capital

^{1/} Black's analysis of and inferences about private speculative behavior (1973, pp. 39-52) are especially interesting. He concludes that the flexible exchange markets of 1936 to 1938 "were not destabilizing. In most cases, they facilitated the response of participants in the markets to the destabilizing political events of the period" (p. 53).

flows to their economic determinants. The estimation of lagged responses is still very difficult and problematical in all empirical research. But it is a fair judgment on the vast majority of the work done so far in the area of international capital flows to call it unsophisticated relative to advanced empirical work in other areas. The existing studies often resort to crude techniques, such as the inclusion of the lagged dependent variable in the equation.^{1/} The potential pitfalls associated with this formulation are notorious.^{2/} Quite apart from problems of statistical bias, inclusion of the lagged dependent variable in a regression equation can generate superficially plausible results even though the basic economic relationship is seriously mis-specified. (In an extreme case, lagged values of a dependent variable -- if it is a rather smooth economic time series -- can alone "explain" the dependent variable quite well, suggesting a "significant" slow speed of adjustment even when there are no theoretically appropriate variables in the relationship to which the dependent variable is allegedly adjusting.^{3/}) In other cases, researchers have made use of polynomial interpolation techniques, but again in ways that are rather ad hoc and that do not give one much feeling of confidence that the actual results can be relied on.^{4/}

^{1/} See, for example, Arndt (1968) and the Miller and Whitman studies.
^{2/} For a discussion of the state of the art as of 1966, see Griliches' useful survey (1967).

^{3/} Bryant and Henderson (1972) give some illustrations of the consequences of mis-specification of equations including a lagged dependent variable.

^{4/} Branson's early work (1963), for example, made extensive use of the polynomial interpolation technique developed by Shirley Almon.

We are in the poorest position of all to appraise the effects of governmental restrictions on capital flows, for two reasons. First, as discussed in Section II above, the techniques used to study capital flows directly subject to governmental restraints are inadequate. We therefore know little about the quantitative impacts of the restraints even on those specific capital flows at which the restraints are aimed. Second -- and this is just as much of a difficulty when we are interested in the net effects of the restrictions on a country's capital account or balance of payments as a whole -- we know virtually nothing about the quantitative impacts of the controls on capital flows other than those specific flows at which the controls are aimed. Substitutions or "leakages" of funds through non-controlled channels are typically an important phenomenon -- too important to be safely ignored. Yet by and large they have been ignored in the empirical literature so far.^{1/}

^{1/} Consider the following examples pertaining to the effects of capital restrictions imposed by the U.S. Government. Because of the Interest Equalization Tax (IET), foreigners purchased foreign securities issued outside the United States that otherwise would have been issued in New York and bought by U.S. residents. Similarly, because of the program administered for direct investors by the Commerce Department Office of Foreign Direct Investments (OFDI), foreigners purchased a greatly increased volume of securities issued outside the United States by U.S. direct investors; absent the OFDI program direct investors would probably have borrowed much of their needed funds from U.S. residents. But to what extent did foreign investors therefore buy fewer U.S. securities than they otherwise would have purchased? (There were of course no controls requiring foreigners not to reduce the rate at which they otherwise would have accumulated such securities.) Branson and Hill (1971, pp. 19-20) do not even bother to test for this possibility in their equation for U.S. long-term liabilities to foreigners. Analogous problems arise in connection with the

(continued on next page)

These gaps in what we know about the effects of capital controls would not be troublesome if the use of controls were more the exception than the rule, or even if the controls were seldom varied in intensity. As noted earlier, however, such is not the case, and the deficiencies in our knowledge are serious.

Although the empirical research in this area has produced limited but useful additions to our knowledge, it unfortunately does not carry us very far towards the objective of improving the formulation of economic policy, and monetary policy in particular.^{1/} It is all very well, for example, to note that capital flows are sensitive to changes in interest rates, and hence that monetary policy can have a powerful influence on private capital movements. But can this influence and the other channels through which economic policies help to determine capital flows be spelled out in a way that is helpful to policymakers? By and large, they can not: there is a huge discrepancy between what economists know and what policymakers need to know in order to formulate policy intelligently.

(Continued from page IV-6)

Voluntary Foreign Credit Restraint (VFCR) Program restraining U.S. banks. Branson and Hill, and Kwack, both include a dummy variable in their equations explaining U.S. bank loans to foreign residents, but do not discuss the possibility that foreigners reduced their deposits in U.S. banks (compared with what they otherwise would have been) as an indirect result of the VFCR program.

^{1/} On this point, see also the discussion above on pp. 2-6/
Section III, pp. 2-6/.

To be sure, the existing empirical evidence puts a modest amount of flesh on the bones of the so-called "portfolio adjustment" view of capital movements. This view, it will be remembered,^{1/} argues that, in response to a change in monetary conditions or some other determinant of a given capital flow, there is a stock-shift effect which, though spaced out over as much as several quarters or maybe a year or longer, represents a once-for-all adjustment in portfolios -- and therefore gives rise to a once-for-all capital flow. (Under a regime of flexible exchange rates, the stock-shift effect gives rise to a once-for-all change in the level of the exchange rate.) Simultaneously, the change in monetary conditions induces a continuing-flow adjustment (an altered pattern of investing increments to wealth) that persists more or less indefinitely. The better studies in the field recently have all been based on formulations which allow both types of effects, and the resulting empirical estimates support the view that the size of the stock-shift effect is a good deal larger (during the period when it is taking place) than the continuing-flow effect.^{2/}

This latter point, taken together with the evidence of the relatively high interest elasticity of capital flows, has important implications for the use of monetary policy to influence the capital

^{1/} See also the discussion above on pp. Section II, pp. 6-87.
^{2/} Strictly speaking, the existing empirical estimates do not constitute a test of the portfolio-adjustment view of capital movements; the specifications employed force this theory on the data.

account in the balance of payments. It suggests that monetary policy can have sizable, but mainly transitory, impacts on the capital account and/or the exchange rate. Thus, with exchange rates pegged at a certain level, monetary policy -- if used for external objectives -- should be thought of mainly as an instrument for bringing about a one-time change in reserves (compared with what they otherwise would have been). It can be much less effective in bringing about an enduring change in the capital account. The counterpart of this argument for the case when exchange rates are floating is that monetary policy should be regarded primarily as an instrument for inducing a one-time shift in the level of the exchange rate (compared with what it otherwise would have been).

Although important, these conclusions about the impacts of monetary policy on capital flows and exchange rates tell policymakers very little of what they need to know. Consider the following representative illustration of the choices and uncertainties that confront policymakers in the United States. Suppose the Federal Reserve is contemplating the possibility of a tightening of monetary policy via open-market sales of Treasury bills. Suppose further that a proposal is being considered to lower the Regulation M and D reserve requirements on Eurodollar borrowings by U.S. banks. In addition to the need to project the effects of these proposed actions

on real activity, employment, prices, and financial conditions, the Federal Reserve needs to be able to project their net effects on the balance of payments and the value of the dollar in exchange markets.^{1/}

What analytical steps are required? In principle, one must project the effects of the proposed actions on the portfolio behavior of all categories of economic units and thus on the whole structure of interest rates and credit availabilities in U.S.

^{1/} For the purpose of the discussion in the text, the motives underlying these contemplated actions are not relevant, since -- whatever the motives -- judgments have to be made about the effects of the actions on all the main macroeconomic variables. Some of the situations that might conceivably give rise to such proposed actions include the following: (a) Prices have been rising too rapidly, domestic demand is thought to be expanding at too high a rate, while the balance of payments has been weak (or, alternatively, the dollar has been depreciating on exchange markets). (b) Domestic demand is judged not to have been growing at an excessive rate, but the balance of payments or the exchange value of the dollar have been very weak. (c) Domestic demand has been expanding excessively, but the balance of payments or the exchange value of the dollar have not been weak; in this case, the Regulation M and D actions might be under consideration primarily for regulatory reasons (e.g., to alter competitive inequities between U.S. banks and foreign banks), and there would be a concern about both actions strengthening the external position excessively.

financial markets. These changes in financial conditions will have an influence on domestic activity and prices, which then in turn will feed back and induce further changes in financial conditions. With the United States such an important part of the world economy, activity, prices, and financial conditions in the United States will have significant impacts on, for example, interest rates and activity levels in other countries. If exchange rates are being held relatively fixed by official intervention, these impacts will be transmitted in part through changes in reserves. In the absence of significant official intervention in exchange markets, exchange rates themselves will change and help transmit the impacts. In any case, it is clear that a large number of international channels of causation and feedback loops need to be taken into account. The proposed change in reserve requirements on Eurodollar borrowings, for example, might have particularly important impacts on the international assets and liabilities of U.S. banks, and hence on the Eurocurrency markets and then on conditions in financial centers in other major countries. The analysis has to be able to cope on the one hand with the many influences of economic activity and financial conditions, both in the United States and abroad, on capital flows and on trade flows, and on the other hand with the many feedback influences of capital and trade flows on economic activity and financial conditions.

Seen against this background of complex, interdependent transactions, how artificial an intellectual experiment it is for the Federal Reserve to ask how much the capital account of the U.S. balance of payments would improve as a result of an increase of, say, 100 basis points in U.S. interest rates with interest rates and other financial conditions in the rest of the world remaining unchanged. For any large, open economy -- but a fortiori for the United States -- it can not be plausibly assumed that the rest of the world will stand still. But in what manner, and by how much, will the rest of the world move? Economists' lack of quantitative knowledge of the determinants of international capital flows is even exceeded by our quantitative ignorance about the manner in which capital flows link national financial markets together.^{1/}

^{1/} Herring's Ph.D. dissertation (1973), which I have already mentioned, is a commendable first effort to tackle this area of ignorance. On the same subject, see also Argy and Modjera (1973). In an important recent paper, Girton and Henderson (1973) investigate these problems in terms of a two-country theoretical model.

If Herring's empirical results are taken at face value, they suggest that a tightening of monetary policy in the United States so as to bring about a 100 basis point increase in U.S. interest rates would exert so much upward pressure on other national interest rates that a weighted-average interest differential between the United States and the rest of the world would be increased by only 36 basis points (Chapter 7, Table 7.6). Herring's aggregative capital-flow equations yield the result that a 100 basis point increase in U.S. interest rates, in the absence of any adjustment in foreign interest rates, would produce a \$3½ billion capital inflow into the United States; the resulting inflow to the United States would be only \$1½ billion when it is assumed that foreign interest rates adjust to their new equilibrium levels (as calculated in Herring's interest-rate equations). I have considerable misgivings, as does Herring himself, about his specific estimates. But they do constitute a step forward on a path, as yet unmarked and uncleared, along which many more researchers will have to tread.

Just to complete the picture, there is the small matter of having something less than a perfect understanding of the linkages and interdependence between financial conditions, real activity, and the price level -- for the United States, or for any other economy. The unfortunate truth, therefore, is that the economics profession does not have sufficient knowledge -- about almost all of the analytical steps that must be carried out -- to enable the Federal Reserve or any other monetary authority to make reasonably accurate projections of the first-round and ultimate effects of any of their proposed actions.

I would not want the preceding remarks to be misinterpreted. Even the meager theoretical and empirical knowledge we do have is often sufficient to prevent policymakers from falling into gross errors -- such as, for example, ignoring the key interrelationships between domestic monetary conditions and monetary conditions abroad, or expecting an alteration in the relative levels of domestic and foreign interest rates to have as large an enduring impact on capital flows or changes in exchange rates as the initial, stock-adjustment impact. And I certainly do not share the views of those who are so agnostic as to doubt the value of a major expenditure of research resources in this area to advance our knowledge further.

What I am arguing, however, does amount to an overall judgment of the empirical literature on international capital flows that is quite critical. Viewed another fifteen years from now, we

will almost surely look back and see that by 1973 this area of research, if not in its infancy, was at best beginning to struggle with adolescence. I leave it to the reader to judge, since that depends on his or her own perspective and rate of time preference, whether this is basically an optimistic or a pessimistic assessment.

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