the case. K-M may conceivably (though we doubt it) be willing to assert that the *true* impact of an exogenous increase in the cyclical variable upon gross-of-tax profits can plausibly be negative, but we must insist that what they say in footnote 1 about the corresponding estimated impact simply is not relevant to our argument.

The second type of bias is estimation bias, stemming from the fact that the employment variable E_t is endogenous. In our equation (9.11′) we derive an expression for this estimation bias:

$$\hat{f}_1 - f_1 = \frac{f_2 M_{vE} M_{EZ^*}}{M_{LZ^*} M_{EE} - M_{EL} M_{EZ^*}} . \tag{9.11′}$$

Here f_1 is the true coefficient of L_t, equal to $(b_1 - b_2 c_1/c_2)$ in equation (9.9). It is already upward biased due to specification error. The quantity $\hat{f}_1$ is the instrumental-variables estimate of f, when Z_t^* is used as the instrument for L_t. K-M assert (p. 769) that "the pressure variable actually introduced by C-H-M is endogenous, so that the resulting estimating equation ceases to be in reduced form. Since the current-pressure variable may be affected by the very tax changes whose effect is to be measured, the degree of shifting may thus be understated." The expression for bias in equation (9.11′) contains one "true" coefficient (f_2, measuring b_2/c_2 in [9.9′] and presumed to be positive), one unobservable sample moment M_{vE} (measuring the presumably positive relation between disturbances in equation [9.3] and the level of employment), and four observable sample moments (M_{EZ^*}, M_{LZ^*}, M_{EE}, and M_{EL}). K-M's comment, cited above, appears to conjecture that a rise in the tax rate would reduce profits not only directly but also indirectly, through dampening economic activity. The true total effect, they seem to say, would be greater than the direct effect as measured by f_1. There can be no doubt that the causal chain postulated by them is plausible; however, it must be recognized that it is a statement about M_{EZ^*} in equation (9.11′). And M_{EZ^*} is an observable sample moment, which happens to have a positive sign, rather than the negative one that their hypothetical causal chain postulates. We will not deny that in some conceivable future samples of U.S. data, M_{EZ^*} will turn out to be negative, leading to a negative rather than positive estimation bias in the tax-shifting coefficient. But in their own sample M_{EZ^*} is positive — in our view because of the fortuitous historical accident that corporation income tax rates were low in the 1930s (when there was substantial slack in the economy) and high in the late 1940s and 1950s (when full employment prevailed save for relatively mild recessions). This is both the reason why we feel that a cyclical or, as K-M put it, a "pressure" variable must be included *and* the reason why, when it is included, the tax-shifting coefficient is subject to an upward estimation bias.

REFERENCE

Krzyzaniak, M., and R. A. Musgrave. "Corporation Tax Shifting: A Response," *J.P.E.* 78 (pt. 1, July/August 1970): 768–773. [This volume, Chapter 9, Appendix I.]

VIII

The next article was prepared as a comment on four different studies attempting to estimate the results of the tax stimuli (more liberal depreciation allowances and the investment tax credit) that were introduced by the Kennedy administration in the early 1960s. These four studies — by Robert E. Hall and Dale W. Jorgenson, Charles W. Bischoff, Robert M. Coen, and Lawrence R. Klein and Paul Taubman — and my comment are all published in *Tax Incentives and Capital Spending*, edited by Gary Fromm (Washington, D.C.: The Brookings Institution, 1971).

In contrast to Chapter 9, which concentrates on the specific study under review, Chapter 10 contemplates some fundamental issues in the theoretical and empirical analysis of changes in institutional arrangements (in this case new tax provisions) that are suggested by the papers being discussed. The key issue concerns the assumptions that one can make concerning the principles guiding day-to-day economic policy — in this particular case monetary policy. The analysis is carried out under three alternative assumptions regarding monetary policy: (i) that the monetary authorities operated in such a way as to generate a time path of income that would be the same as it would have been in the absence of the tax incentives; (ii) that they behaved so as to make the time path of the money supply the same in the presence of the incentives as it would have been in their absence; and (iii) that the target variable of the monetary authorities was neither income nor the money supply but rather the level of interest rates, and that this latter variable would have followed the same track in the presence of the special incentives as in their absence.

The effects of the tax incentives on important economic variables will differ substantially, depending on which of

these hypothetical rules is followed by the monetary authorities. If the income time path is held the same, the effect of the incentives is to raise interest rates and to improve the balance of payments. If, on the other hand, interest rates are constrained to follow a given track, the incentives lead to an increase in real income and a deterioration of the balance of payments. Finally, when the money supply is the variable whose time path is held constant, the results are an increase in income, and a rise in interest rates, with an uncertain net effect on the balance of payments.

Of course, one may be quite certain that actual Federal Reserve policy in the post–tax-incentive period did not follow any simple rule like (i), (ii), or (iii), but was much more complicated. An accurate delineation of the effects of the tax changes is therefore practically impossible in the absence of a detailed knowledge of the twists and turns of monetary policy. Despite the complexity of the problem, however, I believe that the crude evidence is sufficiently strong to support the conclusion that at least one principal effect of the tax incentives was to alter the composition of investment — drawing a significant amount of investible funds out of residential construction and into those uses (business plant and equipment) covered by the incentive provisions.

Chapter 10

Tax Stimuli and Investment Behavior

I cannot conceive that, even as little as five years ago, a book of this type would have come anywhere near this one [*Tax Incentives and Capital Spending*] in linking the discussion of concrete policy issues to the subtleties of modern economic theory, to the nuances of interpreting particular data series, and to the details of advanced econometric method. Economics is becoming more and more a science, and economists are more and more professional in its practice. These authors are among the leaders in this exceedingly healthy development.

Having said that, I cannot help reflecting on the disparity of the results emerging from the four treatments of the relation of tax incentives to investment behavior. It is naïve to expect there to be only one way in which economic science can be brought intelligently to bear upon a given set of data to answer a given question. Several models, each soundly based, may still have differing implications, because theory has yet to achieve — if, indeed, it ever will — a unique set of propositions on which all professional economists agree. And where divergent theories are tested on a given body of data, the data might not be sufficiently extensive or robust to show that one approach is superior to another. Alternative basic series may likewise exist, each with its own defects; economists may choose differently among them, and may elect alternative ways of adjusting and using the series to correct for the error components they conceive to be most important. Finally, the matter is still further complicated when distributed lags enter the postulated relationships in a significant way; here the choice of the precise type of lag distribution to be imposed can itself have strong bearing on the results.

The basic trouble is that, in much of their work, economists are destined to deal with a limited body of data. In statistical terms, they have limited degrees of freedom. For the data to tell them anything, they must make arbitrary judgments about specification of the model itself, forms of functional relationships, nature of lag distributions, and the like. Once the investigator makes these judgments, he is in a sense their prisoner; they become the "maintained hypotheses" under which his econometric exercises are carried out. Working within such a framework, the economist can find the results that yield him the best fit for a particular body of data; they may have high levels of significance

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and good explanatory power, and in this sense be better than any alternative results that can be drawn from the same data within the same framework. But they may, unfortunately, be closely tied up with the framework of maintained hypotheses, in the sense that a modest change in the latter may produce a quite different set of best results.

A long-standing commonplace of scientific method holds that there are in principle an infinite number of hypotheses capable of explaining a given finite body of data. Two points determine a straight line, but an infinite number of parabolas, circles, ellipses, and hyperbolas will also fit those points. In econometric work, complicated functional forms have traditionally been ruled out on essentially a priori grounds, but even so the specification of functions — their form, the determination of which explanatory variables will be introduced in them, and so forth — has been an extremely important issue. As analysts have moved forward to incorporate lag structures in their work, the number of possible hypotheses that might be entertained to explain a given body of data has multiplied as well, thus raising the probability that two — or even four or six — authors, approaching the same question with somewhat different basic models, will each come up with results indicating that his approach is consistent with the data.

COMPARISON OF APPROACHES

These remarks are relevant to comparing the work under discussion. Of the four, the Klein and Taubman paper has the least restrictive investment function, in the sense that fewer restrictions derived from economic theory are imposed in the derivation of that function. Where distributed lags are used by Klein and Taubman, they are in the main arbitrarily drawn from Shirley Almon's study,[1] and applied to three distinct classes of industries (manufacturing, regulated, and other). But a few unexplained variations occur in the treatment of different sectors. For manufacturing, an Almon-lagged capital stock variable is used, along with others, to explain investment; for the regulated industries an unweighted two-quarter moving average of lagged capital stock is used; for the remaining sector, the variable is simply last quarter's capital stock. A lagged capital utilization variable appears in the equation explaining manufacturing investment, but not in those for the other two sectors. A cash flow variable likewise appears in the manufacturing equation but not in the others. While these discrepancies of approach may have sound justifications, the failure of the authors to make them explicit leaves the reader wondering to what degree the equations presented were the result of an extensive process of experimentation, within a rather loose theoretical framework, in search of the best fit.

On the other hand, Klein and Taubman appear to have taken greater pains to justify the particular data series they have used, and to accommodate their analysis to the nuances of the investment-stimulating legislation, than have the other authors.

By way of contrast, the paper by Hall and Jorgenson adopts the explicit

[1] Shirley Almon, "The Distributed Lag between Capital Appropriations and Expenditures," *Econometrica*, Vol. 33 (January 1965), pp. 178–96.

framework of neoclassical capital theory, and imposes on the model derived from that framework the restrictive assumptions of (1) competition, (2) Cobb-Douglas production functions, (3) exponential depreciation of capital goods, and (4) a constant before-tax discount rate. The solid theoretical foundation for Hall and Jorgenson's work strikes me as a distinct advantage. Except for the assumption of a constant before-tax discount rate, the other restrictions appear also to have some independent justification. The assumption of competition is clearly violated in the real world, but the results would be much the same if it were replaced by an assumed constant degree of monopoly; only if the degree of monopoly were itself a function of the tax changes would a serious problem be involved here. The Cobb-Douglas production functions are defended on the ground that a significant body of empirical evidence appears to be consistent with the Cobb-Douglas hypothesis, although the authors recognize that some studies suggest an elasticity of substitution between capital and labor of less than one. The assumption of the exponential depreciation of capital goods has great advantages of mathematical convenience in a model of the type Hall and Jorgenson employ, and appears, like the assumption of Cobb-Douglas production functions, at least not to have been controverted by the weight of existing evidence.

A constant before-tax discount rate is, however, another matter. In accepting it, Hall and Jorgenson implicitly make an extreme and implausible assumption about the shifting of the corporation tax, namely, that with the imposition of an increase in the tax, the after-tax rate of return will fall by the full amount of such increase, and will rise by the full amount with a decrease in the tax rate. This result would be plausible if the corporation income tax struck all income from capital equally, but it is not plausible for the U.S. case, in which the corporation income tax applies to the income from only about half the capital in the economy. All income from capital in unincorporated enterprise (predominantly housing and agricultural capital) is exempt from the corporation income tax, and that part of corporate capital that is financed by debt also is exempt. As a consequence, so long as equilibrium prevails in the capital market before and after a change in the corporation income tax, one would expect an increase in the corporation tax rates to depress the after-tax rate of return on all capital, and a decrease in the tax rate to enhance it. But it is indeed extreme to assume that the after-tax rate of return to corporate equity capital falls by the full amount of the tax, for then equilibrium in the capital market would require a corresponding fall in the rate of interest on debt capital and in the rate of return obtained in the noncorporate sector. Capital as a factor of production would under these circumstances bear substantially more than the full burden of the tax.[2] It is far more plausible to assume that capital bears approximately the full burden of the corporation income tax, which

[2] In the United States, the capital impact would be approximately twice the full tax burden. For an elaboration of this point see Arnold C. Harberger, "The Corporation Income Taxes," in *International Encyclopedia of the Social Sciences*, Vol. 15 (Macmillan, 1968); Arnold C. Harberger, "The Incidence of the Corporation Income Tax," *Journal of Political Economy*, Vol. 70 (June 1962), pp. 215–240; and John G. Cragg, Arnold C. Harberger, and Peter Mieszkowski, "Empirical Evidence on the Incidence of the Corporation Income Tax," *Journal of Political Economy*, Vol. 75 (December 1967), pp. 811–821. [This volume, Chapters 6, 7, and 9.]

means that the after-tax rate of return to corporate equity capital would rise by approximately half of any decrease in the tax rate, or fall by approximately half of any increase in that rate, with the rates of return to noncorporate and corporate nonequity capital falling or rising equally with the after-tax yield on corporate capital in the case, respectively, of a rate increase or a rate decrease.

The effect of assuming a constant before-tax discount rate is therefore to exaggerate considerably the effect of tax stimuli upon investment, because it ignores their indirect influence on investment through rises in the rate of interest. I shall return to this issue.

Bischoff's paper is in much the same spirit as Hall and Jorgenson's, although somewhat less restrictive in its assumptions. The basic differences are that (1) Bischoff does not impose a Cobb-Douglas production function but instead fits the more flexible constant-elasticity-of-substitution function; (2) Bischoff permits the lag pattern of investment response to changes in relative prices to be different from the lag pattern of response to output changes, whereas the Hall and Jorgenson procedure effectively imposes the same lag pattern in the two cases; and (3) Bischoff permits the discount rate governing investment decisions to respond to changes in market interest rates, whereas Hall and Jorgenson do not. Bischoff's estimates of the elasticity of substitution that yield the best fits are sufficiently close to unity that one cannot attribute significant differences between his results and those of Hall and Jorgenson to his relaxation of the Cobb-Douglas assumption.

Far more important is Bischoff's allowance for different lag patterns of response to changes in relative prices and in output. Bischoff's Table 3.11 reveals a rapid response of investment to output changes, initially more than double the steady-state response and remaining greater than the steady-state response for eleven quarters. On the other hand, Table 3.11 also indicates that the response of investment to price changes (which include changes in equipment cost, interest rate, and taxes) is slow, starting with a small negative effect relative to the steady-state response and taking nine quarters to build up to 90 percent and eleven quarters to reach 100 percent. These results suggest that Hall and Jorgenson have probably overestimated the effectiveness of the tax incentives in stimulating investment; their common pattern of lagged response of investment to changes in price and output probably approximates a weighted average of Bischoff's slow response to price changes and his rapid response to output changes, and hence overstates the speed of reaction to price changes and understates that to output movements.

Bischoff's approach also has an advantage over Hall and Jorgenson's in that it permits the cost-of-capital variable to be influenced by movements in the market interest rate and in the dividend yield on stocks, as well as by tax changes. It is quite clear that the estimates in both papers of investment responsiveness are dominated by output changes, Bischoff's probably substantially less so than Hall and Jorgenson's, as his price variable captures additional (and thoroughly valid) sources of movement in the cost of capital.

But let it be noted that, like Hall and Jorgenson's, Bischoff's estimates of the actual effect of tax concessions on investment fail to allow for the effects of the

concessions on interest rates. Whereas Hall and Jorgenson assume the before-tax rate of return in corporate capital to be constant, and measure the influence of tax incentives on this basis, Bischoff assumes in effect that in the absence of tax concessions the interest rate and dividend yield would have followed the same time path as they actually did in the presence of the concessions. Bischoff's treatment of interest rates and stock yields as exogenous is thus subject to the same criticism as Hall and Jorgenson's treatment of the before-tax rate of return as given.

Coen's approach is more general than both Bischoff's and Hall and Jorgenson's in that (1) Coen does not postulate an explicit form for the production function; (2) Coen directly estimates separate coefficients for his new orders (corresponding to output) and price variables, whereas the user cost (price) variable and the output variable enter Bischoff's and Hall and Jorgenson's estimated equations multiplicatively, so that their separate influences are either equal (Hall and Jorgenson) or constrained by a single estimated coefficient (Bischoff); and (3) Coen introduces cash flow as a determinant of investment, while Bischoff and Hall and Jorgenson do not. Coen's model is less general than Bischoff's in that Coen imposes a single exponential lag structure according to which investment responds to price and output stimuli.

Unlike Klein and Taubman and like Hall and Jorgenson and Bischoff, Coen derives his estimated equation directly from a theoretical framework that incorporates profit maximization. He handles the cash flow variable (which is indeed difficult to incorporate in a maximizing model) most ingeniously, by postulating that variations in cash flow do not influence the target capital stock determined by price and output, but rather affect the speed with which that target level is approached. He differs from Bischoff and Hall and Jorgenson in imposing an exponential adjustment lag, whereas they obtain their lag structure from the data; but he adds some flexibility by incorporating a fitted expectations lag (the same for the price and new orders variables). I doubt, however, that these differences are of great importance in accounting for the differences in results.

I suspect that the important difference between Coen's approach on the one hand and Bischoff's and Hall and Jorgenson's on the other is the manner in which the user cost and output variables enter the equation; and it is difficult to say in this case that one approach is clearly better than the other. If the data were perfect, I think there would be little doubt that Coen's formulation would be preferable. For both Hall and Jorgenson and Bischoff, the lag-adjusted price and output series enter multiplicatively in the equation explaining investment; a single coefficient applies to their product. While admittedly something is lost in linearizing a product like, say, $(p/c)_t Q_t$ into $(\overline{p/c})Q_t + (p/c)_t \bar{Q}$, I do not feel that the loss is likely to be great. And if it is not great, performing the linearization and introducing $(p/c)_t$ and Q_t separately can be regarded as a test of the validity of the fundamental assumption that $(\overline{p/c})Q_t$ and $(p/c)_t \bar{Q}$ have the same coefficient. A formulation similar to Coen's can in this sense be regarded as providing a test of one of the basic assumptions of both Bischoff and Hall and Jorgenson, so long as the data are strong enough for this task.

Unfortunately, that is unlikely to be the case. Zvi Griliches, who first called this point to my attention, has expressed the judgment that the average measurement error in the user cost variables employed by these authors can easily be as high as 20 to 25 percent of the mean of such variables. In common, I believe, with most other observers, I share his qualms. If he is correct, when output and user cost are introduced as separate variables in a regression, the coefficient of user cost will be biased strongly toward zero because of its high error component. Coen's results, which indicate a much smaller effect of the tax stimuli than Bischoff's or Hall and Jorgenson's, can be explained in this way, provided one is willing to postulate a sufficiently high error component in his user cost variable.

The Hall and Jorgenson and Bischoff results are less influenced by this error, because by hypothesis they get information on the coefficient of user cost from variations in output as well as in user cost (price). Error variability is a significantly smaller fraction of the total variability of $(p/c)_t Q_t$ than it is of the total variability of $(p/c)_t$ alone; hence the coefficient of the $(p/c)_t Q_t$ variable is less biased than that of $(p/c)_t$. Similarly, one can say that the substantial variations in output that took place over the estimating period bring a lot more information to bear on the estimate of the user cost coefficient than Coen's approach will allow. Thus if one is willing to accept the theoretical framework of Bischoff or Hall and Jorgenson, with its implication that the output and price variables *should* be joined together, one can easily be led by econometric considerations to prefer their results to Coen's. But if one rejects the framework implying that $(p/c)_t Q_t$ is the right variable, or accepts that framework tentatively but judges the issue of measurement error to be of minor importance, then one would be inclined to regard Coen's results as superior.

My final point in connection with Coen's work is that he, like Hall and Jorgenson and Bischoff, measures partial rather than total effects of the tax incentives, in the sense that he does not allow for the effects of these incentives themselves on interest rates.

CONSEQUENCES OF THE TAX MEASURES

This section explores the likely macroeconomic consequences of the series of tax measures under consideration, and inquires what the four papers have to say about them. It is not unfair to characterize most proponents of the legislation at the time of enactment as thinking that the measures would increase investment in the U.S. economy in relation to some measure of its overall scale, such as gross national product (GNP). The facts, however, quite clearly belie this key expectation. Whereas in the years 1955 through 1961, gross private domestic investment bore an average relationship to GNP of 15.3 percent, the corresponding average from 1962 onward (including the first half of 1967) is 15.0 percent.

How can this apparent failure of the tax stimuli actually to stimulate be explained? The answer lies largely in the truism that investment must equal savings for the economy as a whole. This means, in national accounting terms,

that gross private domestic investment plus net foreign investment must equal gross private savings plus the government surplus. The tax measures under consideration, including the general tax reduction of 1964, produced no clear incentive to savings. Each entailed a reduction in the revenues the government might expect from a given level of GNP, and each entailed a corresponding rise in private disposable income out of a given GNP. But since increments to disposable income are not all saved, the net effect of the tax stimuli has very likely been a reduction in total saving (private plus government) as a fraction of GNP. This, indeed, is what the crude data show, with national saving averaging 16.0 percent of GNP in the 1955–1961 period and 15.9 percent in the 1962–1967 period. (The difference between national saving and gross private domestic investment is net foreign investment, which increased from an average 0.7 percent of GNP in 1955–1961 to an average .9 percent in 1962–1967.)

Once the issue of the savings rate is brought into the picture, it becomes quite obvious that those who expected the tax stimuli substantially to increase overall investment (and saving) in relation to GNP were doomed from the beginning to disappointment. This does not mean, however, that the measures must be written off as failures. Once the overall savings constraint is accepted, it is clear that the probable effect of the tax stimuli was a shift in the composition of total investment rather than a significant change in its relation to GNP. Such a compositional shift did in fact occur. From an average of 4.8 percent of GNP in the 1955–1961 period, residential construction fell to an average 3.9 percent in the 1962–1967 period. Counterbalancing this, other private domestic nonresidential investment (principally plant and equipment spending) rose from 9.8 percent of GNP in the earlier period to 10.2 percent in the later period, and was the principal beneficiary of all of the tax measures under consideration.

To the extent that the observed compositional shift of investment was the consequence of the tax stimuli, they should be given good marks, for it is obvious that the marginal productivity of capital in the nonresidential sector of the economy is substantially higher than it is in the housing sector. Tax provisions alone guarantee this; the relevant marginal productivity in the corporate sector is gross of corporate taxes, property taxes, and personal taxes, while in the bulk of the housing sector it does not even include personal taxes. To put it another way, the yield to an individual of a corporate investment is the marginal productivity of that investment less all the taxes that have to be paid out of that marginal product. In owner-occupied housing, the relevant yield is something like the mortgage rate, say 6 percent, reduced by the individual's marginal tax rate. Thus one can applaud the effects of the tax incentives to investment while recognizing that they may not have accomplished the global objective many believe they were intended to produce.[3]

[3] One should not make the mistake here of assuming that a stimulus to residential investment helps the poor at the expense of the rich. The available evidence on housing demand indicates an income elasticity in excess of unity. See Margaret G. Reid, *Housing and Income* (University of Chicago Press, 1962), and David Laidler, "Income Tax Incentives for Owner-Occupied Housing," in Arnold C. Harberger and Martin J. Bailey (eds.), *The Taxation of Income from Capital* (Brookings Institution, 1969).

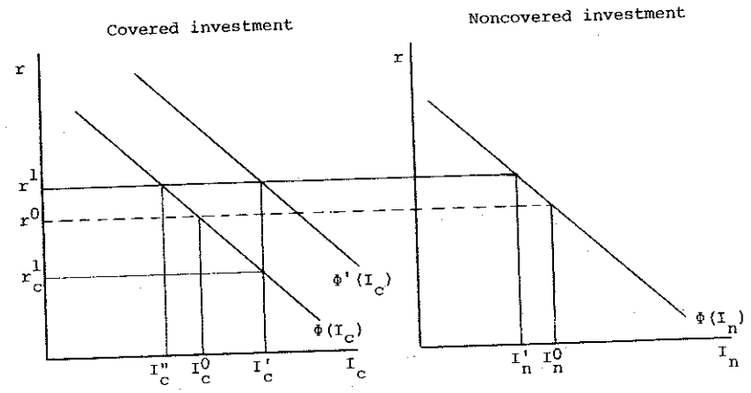

FIGURE 10.1

Effects of Tax Incentives on Covered and Noncovered Investment, Assuming Actual Time Path of Income.

What follows is an attempt, with the aid of some simple models, to isolate the effects of the tax stimuli on what may be called "covered investment," that is, those items of investment that benefited directly from the measures under consideration. In the first example, it is assumed that, in the absence of the tax stimuli, the monetary authorities would have been able to manage their policies so as to achieve the same level of income in each year as that actually attained. There is in this case no "income effect" of the tax measures, only a redistribution of investment from the noncovered to the covered category.

Figure 10.1 depicts investment I as a function of the interest rate r for both the covered c and noncovered n categories. It is assumed that, in the absence of the tax stimuli, the interest rate required to achieve the actual level of income would have been r^0. In the presence of the tax measures, the interest rate required to produce this same level of income is r^1. As a consequence of the tax measures, therefore, noncovered investment falls from I_n^0 to I_n'. Covered investment, however, is subject to two influences. As a consequence of the rise in the interest rate, it would normally tend to fall from I_c^0 to I_c'', but the tax measures shift the investment demand curve to the right, causing a net increase from I_c^0 to I_c'. Another way of interpreting the adjustment is to conceive of the tax stimuli as giving an implicit subsidy to covered investment. This subsidy can be converted into an equivalent reduction in the interest rate, equal to $(r^1 - r_c^1)$. Under this interpretation the effect of the rise in the interest rate itself would again be to reduce covered investment to I_c'', and the partial or direct effect of the tax stimuli would have been movement along $\Phi(I_c)$ increasing investment from I_c'' to I_c'.

All four analyses follow this second interpretation, in that they convert the tax measures into equivalent reductions in the interest rate or — what amounts to much the same thing — the user cost of capital. But three of the four take as the effect of the tax stimuli what I have called the partial or direct effect, and make no allowance for the rise in interest rates caused by these same tax measures. Thus, the Hall and Jorgenson, Bischoff, and Coen measures of the increase in investment caused by the stimuli correspond to $I_c' - I_c''$ in Figure 10.1, not to $I_c' - I_c^0$, which represents the total effect. Klein and Taubman, on the other hand, indicate that their results stem from inserting the tax stimulus $(r^1 - r_c^1)$ into the Wharton Econometric Forecasting Unit model, and solving for the resulting increase in gross fiscal nonresidential investment. Their answer thus corresponds, in a sense, to the general equilibrium answer of $I_c' - I_c^0$, but their underlying model was different from the one discussed here, as they obtain different time paths of GNP with and without the tax stimuli, while this model assumes the course of GNP to be unaffected by them.

In a second simple model, in which the tax measures can have an effect upon GNP, it is assumed that the time path of the quantity of money would have been the same in the presence or absence of the tax measures. This is analogous to saying that the *LM* curve of the traditional Keynesian or neo-Keynesian analysis would be unaffected by the policies in question. Here the tax stimuli result in a rightward shift of the *IS* curve, causing both income and the interest rate to rise. The effect on covered investment is depicted in Figure 10.2. The indirect effect of the measures through the induced change in the interest rate is given by a movement along $\Phi(I_c)$ from A to B; the indirect effect through the induced change in income is given by a shift of the investment demand curve from $\phi(I_c)$ to $\phi'(I_c)$ and is reflected in a movement from B to C; and the direct price effect, which Hall and Jorgenson, Bischoff, and Coen attempt to measure, is given by a movement along $\phi'(I_c)$ from C to E. (This last step can equivalently be considered a shift of the investment demand from C to F.) The total effect of the tax stimuli on covered investment is thus given by $I_c' - I_c^0$. The effect on noncovered investment is indeterminate, consisting of a reduction caused by the rise in the interest rate and an increase caused by the rise in GNP.

A third possible policy alternative is that in the absence of the tax measures the time path of interest rates would have been the same as that which actually developed, a not implausible assumption in the light of the key role of the

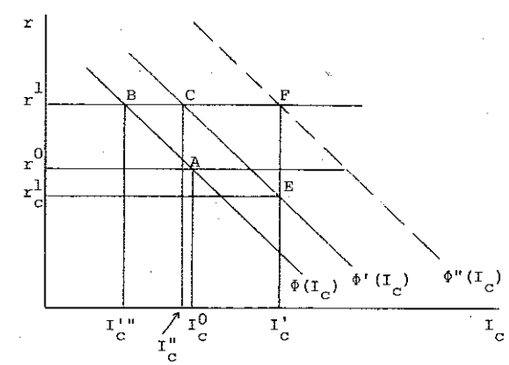

FIGURE 10.2

Effects of Tax Incentives on Covered Investment, Assuming Actual Time Path of Money Supply.

interest rate in the defense of the U.S. balance of payments. Under this alternative, no part of the upward drift of interest rates that has taken place since 1962 is attributed to the tax measures since, by assumption, it would have happened even in their absence. The role of the tax measures is then to prevent this tightening of credit markets from having the negative effect on economic activity that it would normally entail. Analytically, in this case, starting from the position (probably of less than full employment) that the economy would have reached with the same interest rate but in the absence of the tax measures, the effect of the measures is to produce a rightward shift of the *IS* curve, and a concomitant rightward shift of the *LM* curve, so as to permit equilibrium at a higher level of income with the same interest rate. The effect on covered investment is shown in Figure 10.3. Here the change in this category of investment occasioned by the tax stimuli consists of two steps: a move from *A* to *B* occasioned by the induced increase in income, and a move from *B* to *C* representing the direct price effect of the tax incentives.

This discussion serves only to open up a series of issues that lamentably were not treated by any of the authors. Hall and Jorgenson, Bischoff, and Coen simply bypass all general equilibrium considerations and concentrate on attempting to measure the direct price effect of the incentives. They therefore cannot purport to say anything about the total effect. The total effect must be less than the direct effect where alternative monetary policy would have kept the time path of income the same; it must be greater than the direct effect where alternative monetary policy would have kept the time path of interest rates the same; and it can be either greater or less than the direct effect in the case where the alternative policy would have maintained the same time path of the money supply — all this in the context of the simple models outlined above, and referring only to covered investment.

Klein and Taubman, on the other hand, though unique in working within a general equilibrium framework, fail to spell out the key assumptions about policy and behavior underlying their solution, and do nothing at all to explore the implications of the tax incentives under different assumptions about

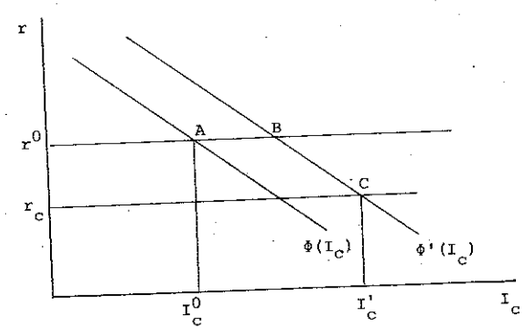

FIGURE 10.3

Effects of Tax Incentives on Covered Investment, Assuming Actual Time Path of Interest Rates.

monetary policy. On the first point, they are clearly in a better position than any reader to distinguish the key behavioral and policy forces that, in the Wharton model, contribute important elements to their solution. Had they done this, they could have given at least a rough idea of the nature, in their model, of the principal indirect effects of the tax stimuli on both covered and noncovered investment. They could also, of course, have stated explicitly the main policy assumptions on which their solution is based, so that readers could readily judge their plausibility.

On the second point, I would like to have seen alternative solutions based on different policy assumptions, analogous but not necessarily equivalent to the three cases discussed above. I have no doubt that with a sufficiently easy monetary policy the actual time path of GNP could have been approximately replicated even in the absence of the tax incentives. It is therefore of some interest to explore this case. On the other hand, the preoccupation of the monetary authorities with the balance of payments makes it unlikely that they would have pursued such a policy; on this ground the equal-interest-rate case is worth exploring. Even more interesting would be a model in which the actual time path of the balance of payments was replicated, and its consequences for GNP, interest rates, and covered and noncovered investment derived, on the assumption the tax incentives did not exist. But this is probably asking too much, as I know of no way reliably to capture, in an econometric equation, the volatile function explaining the capital account of the balance of payments. All the difficulties encountered in explaining private domestic investment — and many more — are present when the capital account is the dependent variable.

In sum, therefore, the quality of the work presented here is very high, but the analyses leave unfinished and unclear the picture of the quantitative effects of the tax incentives that have been examined. I remain prone to the same rough judgment about these incentives that I have held for several years: that they played an important role in permitting us to come reasonably close to full employment in the presence of a strong balance-of-payments constraint; that they accomplished this by creating a situation in which the interest rate level consistent with full employment was significantly higher than it otherwise would be; that, viewed against alternative ways of achieving full employment, these tax stimuli produced a massive shift of investment from the noncovered sector (principally residential housing) to the covered sector, without much change in total investment; and that, viewed against the alternative of maintaining the same interest-rate or balance-of-payments posture that obtained, the incentives probably accounted for a significant increase in total investment and in income.