the efficiency costs of the existing pattern of taxation lie somewhere between \$1 billion and \$3 billion, more probably between \$1.5 billion to \$2.5 billion per year.[1]

These are probably underestimates of the true efficiency costs involved because, in the process of aggregating industries into broad sectors, distortions induced by the tax system among the industries within each broad sector were ignored. Taking these distortions into account would surely add to the estimated efficiency costs. Moreover, the calculations assume that the taxes on income from capital do not introduce any substitution effect away from saving and toward consumption — that is, so far as the substitution effect alone is concerned, it is assumed that the elasticity of response of savings with respect to the net rate of return on capital is zero. If this substitution effect were not zero, there would be an additional efficiency cost stemming from the distortion of choices between consumption and saving. I do not wish to press this latter point too far, however, because in any taxation of the income from capital — even if such income in all industries and sectors is treated equally — some distortion of the consumption-savings choice is implicit. Only a consumption tax of the Kaldor type would avoid this distortion altogether. My main point in presenting the cost calculations of Table 8.3 is to show that we could substantially improve efficiency by simply rationalizing our existing pattern of taxes on income from capital so as to approach more equal tax treatment of the income generated by capital in the various industries and sectors of our economy.

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[1] The results in Table 8.3 for the cases where $S_x = S_y = V$ are not the same as those emerging from the earlier calculations which were based on the algebraic solution of the system for this special case. The reason for the discrepancy is that the values chosen for (K_x/K_y), (L_x/L_y), f_k, etc., are derived from the tax situation rather than the pretax equilibrium. This is similar to the discrepancy one obtains in measuring demand elasticities over a very broad range, depending on whether one takes initial or terminal points as the basis on which percentage changes are computed.

VII

Chapter 9, which originally appeared in the *Journal of Political Economy* in 1967, is basically a critique of the empirical study of corporation tax incidence by Marian Krzyzaniak and Richard A. Musgrave (*The Shifting of the Corporation Income Tax* [Baltimore: Johns Hopkins Press, 1963]). This study attracted substantial attention among public finance experts and others upon its publication and subsequently. This attention has been sufficient to warrant including the critical analysis which I wrote jointly with John G. Cragg and Peter Mieszkowski. Further belaboring of my position on the Krzyzaniak-Musgrave study would be redundant in this introduction. Rather, with their permission, I have included as appendixes to Chapter 9 their published reply and our published rejoinder, which appeared in the *Journal of Political Economy* in 1970, so that readers will have access to the full debate.

Chapter 9

Empirical Evidence on the Incidence of the Corporation Income Tax

John G. Cragg, Arnold C. Harberger, and Peter Mieszkowski

I

The incidence of the corporation income tax cannot easily be determined from empirical evidence for a variety of reasons. First, changes in the corporation tax rate are only one of many forces that operate secularly on the distribution of income; also operative are secular changes in the quality of labor and of capital equipment, secular changes in demand which may shift the pattern of production from relatively capital-intensive to relatively labor-intensive activities (or vice versa), secular changes in production functions themselves (reflecting, among other things, technical innovations which may be biased either in a labor-saving or capital-saving direction), plus, of course, non-tax-induced movements in labor supply and in the stock of capital in the economy. Second, and similarly, the cyclical movements of income distribution in the economy are the products of many forces other than corporation income tax changes — the complicated workings of the housing cycle, of inventory cycles, of investment and savings incentives for the economy as a whole, together with their interactions; plus the influences of monetary and fiscal policies (other than corporate tax rate changes themselves); plus the pressures and constraints imposed on the economy through its dealings with the rest of the world on both current and capital account — all these exert powerful influences on the short-term movements of both the level and distribution of income in the economy, above and beyond any impact that corporation income tax changes may themselves have. Third, and of serious importance in the case of the U.S. evidence, is the fact that movements in the corporation income tax rate

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itself have come in stages, with the rate drifting upward in the 1930s, rising to a peak in the war years, falling off in the late forties, rising again in the Korean war years, and stabilizing finally during the late fifties. Given this type of movement, the different phases being associated with radically different types of economic environment, one runs the risk of mistaking association for causation in any empirical attempt to assess the impact of changes in the tax rate.

Undaunted by these difficulties, Krzyzaniak and Musgrave [1963] have made a bold attempt to extract empirical evidence on the incidence of the U.S. corporation income tax, using data from the years 1935–1942 and 1948–1959. Using widely accepted techniques of statistical analysis, they came to the striking conclusion that the corporation income tax is probably more than fully shifted — that is, that the owners of capital actually gain, in after-tax income, as a consequence of a rise in the corporation tax rate! Not only does this result run counter to most economists' judgments of plausibility, it also opens questions concerning the pricing behavior of corporations which have wide ramifications beyond the specific issue of corporation tax incidence. Indeed, it is certainly not far from the truth to say that if we accept the Krzyzaniak-Musgrave results at face value, we must also accept the task of rebuilding the foundations of the theory of the behavior of the firm.

The striking nature of the Krzyzaniak-Musgrave (hereafter K-M) results, and the profound implications that they would have if they were indeed true, have motivated us to devote this paper to a detailed examination of the procedures by which these results were reached, to an exploration of alternative statistical approaches to the same problem, and to a more careful study of the problem of interpreting the results of these exercises. It should be stated at the outset that we remain profoundly impressed by the difficulties outlined in the opening paragraph of this essay, which confront time-series analyses of the tax-incidence question. In particular, we do not in any way contend that our modifications of the K-M procedures and experiments adequately cope with the difficulties listed above. Our purpose, in fact, is the much more modest one of showing that making highly plausible and theoretically justified modifications in experiments of the K-M type leads to a reversal of the main conclusions apparently implied and that, therefore, at the very least, serious doubt is cast on the validity of K-M's conclusions.

II

Our principal objections to the K-M procedures stem from our conviction that there exists, in the time series for 1935–1942 and 1948–1959, a spurious correlation between the corporation tax rate and the gross-of-tax rate of return on corporate capital in manufacturing. Krzyzaniak and Musgrave take the latter variable, which they denominate Y_{gt}, as the dependent variable in their principal regression equations. Alongside other independent variables, they introduce two alternative tax variables to explain the variations in Y_{gt}. The first of these tax variables, which they call L_t, is the ratio of corporation income tax liabilities in year t to the total capital stock of the relevant group of corporations in the same year. The second tax variable, Z_t^*, is the effective tax rate in year t and is calculated by dividing total tax liabilities by the total profits of the relevant group of corporations in year t. On a few occasions, they introduce the nominal statutory tax rate, Z_t, as the tax variable, but they find no significant difference between the results obtained using Z_t and those emerging when Z_t^* is used. They group their experiments using L_t under what they call Model A, and those using Z_t^* or Z_t under Model B.

The observed simple correlations between Y_{gt} and $L_t (r^2 = .90)$ and between Y_{gt} and $Z_t^* (r^2 = .62)$ provide us with a convenient starting point for our discussion. We felt that it would be absurd to assert that these correlations represented causal relations proceeding from the tax variables to the profit rate. The five years of highest profit rates were 1941, 1942, 1948, 1950, and 1951. During these years, the average profit rate was 22.2 percent, while the average effective tax rate (Z^*) was 56.7 percent, and the average value of L_t was 11.7 percent. On the other side, the five years of lowest profits were 1935–1939, during which period profits averaged 7 percent of capital, the effective tax rate averaged 19.6 percent, and L_t averaged 1.1 percent. We simply cannot believe that the changes in either L_t or Z_t between these sets of years were the dominant causes of the variations in Y_{gt} between an average of 7 percent in the "low" years and an average of 22 percent in the "high" years. Other important forces were clearly at work influencing profit rates, these forces may, as in the cases of the World War II and Korean war years, have led to statutory tax rates being high under circumstances when profits were also high. There may indeed be a certain degree of causal connection running *from* profit rates *to* tax rates, in the sense that Congress would not be prone to raise corporation tax rates at times when profit rates themselves were low, or to make substantial cuts in tax rates at times when profit rates were very high.

We therefore approach the data with a strong suspicion that the observed simple correlations between tax variables and the profit rate do not reflect a strong causal connection in the tax-profits direction. We may now ask whether the additional explanatory variables which K-M introduced in their regressions may plausibly have represented the independent forces that caused profit rates to be low in the thirties, when tax rates were also low, and to be high in the war years, when tax rates were also high. The answer, we believe, is negative. These additional variables are:

ΔC_{t-1} = change from year $t-2$ to year $t-1$ in the ratio of consumption to GNP.

V_{t-1} = ratio of inventories to sales in manufacturing in year $t-1$.

J_t = ratio of tax accruals (other than the corporation income tax) minus government transfers to GNP in year t.

G_t = ratio of government purchases of goods and services to GNP in year t.

The failure of the additional variables introduced by K-M to correct adequately for the other forces indicated above is decisively shown in the following exercise. We take for granted, for the reasons indicated earlier, that the observed simple regression coefficient of Y_{gt} on L_t grossly exaggerates the effect of tax changes upon profit rates. This simple regression coefficient is 1.430; yet

when all the additional variables are introduced, the partial regression coefficient of Y_{gt} on L_t remains at approximately the same level.[1] Similarly, the simple regression coefficient of Y_{gt} on Z_t^* is 0.313, while after the addition of ΔC_{t-1}, V_{t-1}, J_t, and G_t as explanatory factors, the partial regression coefficient of Y_{gt} on Z^* is 0.481 (see below, regression [13.1]). Here the addition of the four extra variables has worked dramatically to exaggerate further what was already an exaggerated estimate of the power of the tax rate to influence profits. Obviously, K-M's additional variables have not corrected for the essentially spurious, noncausal connections between tax rates and profit rates which we pointed out in part I. Indeed, the additional variables they introduce make matters still worse!

We propose an attempt to correct for the deficiencies of the K-M model by introducing two additional variables: the employment rate (= [1 − the unemployment rate] × 100) and a dummy variable, W_t, for the mobilization and war years 1941, 1942, 1950, 1951, and 1952. The dummy variable is simply an attempt to correct for the fact that no one attributes the association between high profits and high taxes in these years to be principally due to the fact that tax rates were high; both profits and taxes were high due to the pressures of the mobilization and/or war situations. The employment rate, E_t, is introduced to provide a plausible explanation for the low profit rates of the thirties and of the dips in the profit rate during the recession years 1949, 1954, and 1958. Again, the low levels of profit rates achieved in these years cannot plausibly have been caused by changes in the tax rate or in the other explanatory variables introduced by K-M.

III

The use of a cyclical variable with the aim of correcting for some of the bias implicit in K-M's procedures was attempted by Richard Slitor [1966] and Richard Goode [1966] in papers criticizing K-M's work. In their reply to this criticism, K-M [1966] argue (a) that the cyclical variable (in Slitor's and Goode's case, the ratio of actual to potential GNP, and, in our case, the employment rate) "is itself a function of the corporate income tax. Hence if [such a] variable is included, the coefficient of the tax variable does not represent the isolated tax effect upon the rate of return, part of this effect being hidden in the coefficient of the [cyclical] variable" (p. 248). They also argue (b) that, when such a variable is included in the estimating equation for Y_{gt}, estimation by least squares yields biased and inconsistent coefficients (p. 249).

In this section, we propose to cope explicitly with both of these objections, in the context of a simplified model which we believe captures all the relevant

[1] K-M do not use direct least squares in obtaining the partial regression coefficient of Y_{gt} on L_t; in an attempt to correct for the positive bias implicit in the fact that $L_t = Y_{gt} \cdot Z_t^*$, they use Z_t^* as an instrumental variable for L_t; that is, where a direct application of least-squares regression would require taking moments with L_t, they instead take moments with Z_t^*. The simple regression coefficient of Y_{gt} on L_t, using Z^* as an instrumental variable for L_t, is $(M_{Z^*Y}/M_{Z^*L}) = 1.304$. When the additional variables are introduced, and Z_t^* is again used as an instrument for L_t, the resulting partial regression coefficient is 1.511 (see K-M, 1963, pp. 44–45, Table 6-1, regression 2). Thus, in this case, by including the additional variables, K-M further exaggerate what was already an implausibly high causal connection between tax rates and profit rates.

aspects of the problem. For simplicity, we eliminate ΔC_{t-1}, V_{t-1}, J_t, and G_t from consideration, and thus reduce the basic K-M regression to

$$Y_{gt} = a_0 + a_1 L_t + U_t. \tag{9.1}$$

K-M object to expanding this equation to

$$Y_{gt} = f_0 + f_1 L_t + f_2 E_t + V_t, \tag{9.2}$$

for the reasons given above. We do not assert that E_t is a truly exogenous variable, nor do we deny that f_2 may capture some of the effect upon Y_{gt} of movements in L_t. We thus accept from the outset the general framework adopted by K-M in their reply to Slitor and Goode.

We justify the use of E_t in (9.2) as a proxy for exogenous, cyclical forces that K-M have failed to capture in their regression. We thus postulate a true reduced-form equation for Y_{gt}:

$$Y_{gt} = b_0 + b_1 L_t + b_2 R_t + \omega_t. \tag{9.3}$$

Here R_t is defined as a truly exogenous (though possibly unobservable) variable, summarizing the influences of the exogenous forces of a cyclical nature which are not accounted for in the K-M regressions. As a truly exogenous variable, it is assumed to be uncorrelated with the residuals of any of the true reduced-form equations.

We first will demonstrate the source of bias in K-M's results. In our view, (9.1) is the wrong equation to estimate; in principle we should estimate (9.3), even though in practice, lacking a time series for R_t, we may not be able to do this. When K-M estimate (9.1) instead of (9.3), however, they introduce a serious bias into their results. We can represent this bias algebraically by taking moments of equation (9.1) with L_t:

$$M_{YL} = a_1 M_{LL} + M_{UL}. \tag{9.4}$$

Krzyzaniak and Musgrave recognize that an estimate derived from (9.4) will contain a bias because a rise in U_t will cause profits to be higher and therefore raise L_t, the tax yield divided by the capital stock. They therefore adopt the procedure of using Z_t^*, the effective tax rate, as an instrumental variable for L_t in the estimation process. This leads to the following equation:

$$M_{YZ^*} = a_1 M_{LZ^*} + M_{UZ^*}, \tag{9.5}$$

where the estimate is based on the assumption that M_{UZ^*} is zero.

But if (9.3) is the true reduced-form equation for Y_{gt}, then the U of equation (9.1) is equal to $b_2 R_t + \omega_t$, and $M_{UZ^*} = b_2 M_{RZ^*}$, which cannot be presumed to be zero, given the high correlation of Z_t^* with cyclically sensitive variables. The expected bias in K-M's estimate a_1 of the true reduced-form coefficient of L_t, assuming $M_{Z^*\omega} = M_{R\omega} = 0$, is equal to

$$(a_1 - b_1) = \frac{M_{YZ^*}}{M_{LZ^*}} - b_1 = \frac{b_2 M_{RZ^*}}{M_{LZ^*}}. \tag{9.6}$$

This bias is positive and likely to be significant in magnitude, since the true influence of R_t on profit rates is surely strong and since Z_t^* is so highly positively correlated with cyclical variables in the period of their sample.

We now proceed to explore, in an analogous fashion, the biases involved in the use of (9.2) as an equation for estimating b_1. To do this we must postulate a true reduced-form equation for E_t, which we have recognized to be an endogenous variable of the system. Let this equation be:[2]

$$E_t = c_0 + c_1 L_t + c_2 R_t + \nu_t. \tag{9.7}$$

We solve this equation for the unobservable R_t, expressing it in terms of the observable variables E_t and L_t:

$$R_t = \frac{-c_0}{c_2} - \frac{c_1}{c_2} L_t + \frac{E_t}{c_2} - \frac{\nu_t}{c_2}. \tag{9.8}$$

Substituting (9.8) into (9.3) we find

$$Y_{gt} = \left(b_0 - \frac{b_2 c_0}{c_2}\right) + \left(b_1 - \frac{b_2 c_1}{c_2}\right) L_t + \frac{b_2}{c_2} E_t + \left(\omega_t - \frac{b_2}{c_2} \nu_t\right). \tag{9.9}$$

This equation is exactly of the form of (9.2), with the cyclical variable E_t as an added explanatory factor serving as a proxy for the unobservable R. Equation (9.2) can be rewritten, keeping ω_t and ν_t as distinct components of V_t, as:

$$Y_{gt} = f_0 + f_1 L_t + f_2 E_t + \omega_t - f_2 \nu_t. \tag{9.2'}$$

Augmented to include the other exogeneous variables used by K-M, plus our additional exogenous variable W_t, this is the regression equation we propose to estimate.

Under the assumptions of this section, the instrumental-variables estimate of f_1 will be a biased estimate of b_1 — the coefficient whose value we really are seeking — for two reasons which correspond precisely to K-M's two objections to Slitor and Goode. In the first place, there is a specification bias, in that f_1 is equal not to b_1 but to $b_1 - (b_2 c_1/c_2)$. The term $-b_2 c_1/c_2$ measures the indirect effect of changes in the tax variable L_t upon Y_{gt}, which is "captured" by E_t when it is introduced, side by side with L_t, as an explanatory variable in the regression. But let us examine the presumptive sign of the adjustment: b_2, measuring the influence of R_t on Y_{gt}, is presumably positive; so also is c_2, measuring the influence of R_t on E_t. But c_1, reflecting the causal impact of L_t on E_t, is surely to be presumed negative. Hence the coefficient for f_1, which

[2] If R is a true scalar variable entering both (9.3) and (9.7), (9.7) can be interpreted as the true reduced-form equation for E, and we will so interpret it in the text. If R is considered as reflecting the influences of a series of truly exogenous variables X_i upon Y_{gt}, it will be of the general form $R = \Sigma\beta_i X_i$, where the β_i are weights reflecting the relative influence of the individual X_i upon Y_{gt}. The true reduced-form equation for E_t would presumably give different weights to the individual X_i, which might be summarized in a scalar variable Q $(= \Sigma\gamma_i X_i)$. Equation (9.7) in this case is obtained by substituting the regression of Q on R $(Q = g + hR + \varepsilon)$ for Q in the true reduced form for E. As long as the two cyclical variables Q and R are positively correlated $(h > 0)$, as is overwhelmingly likely, all relevant properties of (9.7) remain; the presumed signs of c_1 and c_2 are still negative and positive, respectively, and the correlation between R_t and ν_t still has an expected value of zero.

our expression attempts to estimate, is larger in magnitude than b_1, on which our interest centers.

This result can be seen intuitively. If taxes are raised while other exogenous variables, including R, are held constant, Y_{gt} will tend to rise because of the direct effect of taxes on gross rates of return, but will tend to fall because of the dampening effect of higher taxes on total effective demand. The reduced-form coefficient b_1 reflects the net outcome of these two effects. If, on the other hand, we raise taxes while holding employment constant, we must presume that other exogenous forces have operated to compensate the reduction in effective demand which the tax rise would otherwise entail, so f_1 reflects just the first of the two effects contained in b_1.

The second source of error is that the K-M regression techniques applied to (9.2′) will produce a biased estimate of f_1. This results from the fact that E_t is, as shown by the reduced-form equation (9.7), a direct function of ν_t, hence necessarily positively correlated with it as long as $M_{L\nu}$ and $M_{R\nu}$ are zero. Since ω_t does not appear in the reduced form for E_t, it will not be correlated with E_t except through such correlation as may exist between ω_t and ν_t. We shall assume for the moment that $r_{\omega\nu}$ is zero. We shall also retain the standard assumption that ν_t and ω_t are uncorrelated with the exogenous variables Z_t^* and R_t. Employing these assumptions, and taking moments of (9.2′) with Z_t^* and E_t, we obtain:

$$\text{(9.10)}\qquad \begin{aligned} M_{YZ^*} &= f_1 M_{LZ^*} + f_2 M_{EZ^*};\\ M_{YE} &= f_1 M_{EL} + f_2 M_{EE} - f_2 M_{\nu E}. \end{aligned}$$

Solution of these equations for f_1 yields:

$$\text{(9.11)}\qquad f_1 = \frac{\begin{vmatrix} M_{YZ^*} & M_{EZ^*} \\ M_{YE} & M_{EE} \end{vmatrix}}{\begin{vmatrix} M_{LZ^*} & M_{EZ^*} \\ M_{EL} & M_{EE} \end{vmatrix}} + \frac{\begin{vmatrix} 0 & M_{EZ^*} \\ f_2 M_{\nu E} & M_{EE} \end{vmatrix}}{\begin{vmatrix} M_{LZ^*} & M_{EZ^*} \\ M_{EL} & M_{EE} \end{vmatrix}},$$

or

$$\text{(9.11′)}\qquad f_1 = \hat{f}_1 - \frac{f_2 M_{\nu E} M_{EZ^*}}{M_{LZ^*} M_{EE} - M_{EL} M_{EZ^*}}.$$

Here $\hat{f}_1$ is the standard instrumental-variables estimate of f_1; its expected value exceeds the true value of f_1 by $f_2 M_{\nu E} M_{EZ^*} / (M_{LZ^*} M_{EE} - M_{EL} M_{EZ^*})$.

The presumed sign of this bias is positive, f_2 and $M_{\nu E}$ having presumptively positive signs, and M_{EZ^*} and the denominator having positive signs for the sample data.

Thus we have shown that both of the biases referred to by K-M in their reply to Slitor and Goode are likely to be positive, an instrumental-variables estimate of f_1 tending to overestimate its true value, and f_1 itself exceeding b_1 because of the specification error involved in using E_t as a proxy for R_t. We therefore proceed to estimate equations similar to those estimated by K-M, but including E_t as a proxy for exogenous cyclical forces, confident that the expected biases in the coefficient of L_t are both positive and hence that the amount of shifting of the corporation income tax is likely to be overstated. Needless to say, the

whole analysis of this section applies equally well to K-M's Model B, where Z_t or Z_t^* is the tax variable, as it does to Model A, in which L_t is the tax variable.[3]

In a similar vein, one can easily adjust the analysis of the text to accommodate the presence of additional exogenous variables in the overall model to be estimated. Equation (9.11) states that $\hat{f}_1$ is equal to f_1, the partial regression coefficient of Y on L, given E, plus a bias term which is equal to f_2 times the partial regression of ν on L, given E. In both cases, the regressions in question use Z^* as the instrumental variable for L. When the basic equation (9.1) is expanded to include ΔC_{t-1}, V_{t-1}, J_t, and G_t, f_1 in (9.1) should be interpreted as the partial regression coefficient of Y on L, given E_t, ΔC_{t-1}, V_{t-1}, J_t, and G_t, and the bias term becomes f_2 times the partial regression coefficient of ν on L, given these same variables. This partial regression is, of course, more likely to have the presumptive sign than the one treated in the text because it measures the relationships between $f_2\nu$ and L, holding other potentially relevant exogenous variables constant.

IV

Krzyzaniak and Musgrave present the following estimates (1963, Table 6–1, regression 2) for the regression of Y_{gt} on the set of independent variables listed above, using Z_t^* as an instrumental variable.

$$\text{(9.12.1)}\qquad \begin{aligned} Y_{gt} = {} & .2577 + .3013\Delta C_{t-1} - .4228 V_{t-1} - .7721 J_t \\ & - .1083 G_t + 1.5110 L_t \quad (r^2 = .96). \end{aligned}$$

Following their procedures and using their data but adding E_t and W_t as

[3] If we relax the assumption of zero correlation between ω and ν, (9.11[1]) would read:

$$\hat{f}_1 = f_1 + M_{EZ^*}[(M_{\omega\nu} - f_2 M_{\nu E})/(M_{LZ^*} M_{EE} - M_{EL} M_{EZ^*})].$$

The sign of the bias in $\hat{f}_1$ will be positive in all cases where $M_{\omega\nu}$ is negative, and as long as $f_2 M_{\nu E} > M_{\omega\nu}$ when the latter is positive. This last condition can be written $f_2 s_\nu s_E r_{\nu E} > s_\omega s_\nu r_{\nu\omega}$, and this can in turn be transformed into a form easier to interpret:

$$\frac{\bar{E} f_2}{\bar{Y}} \frac{s_E}{\bar{E}} r_{\nu E} > \frac{s_\omega}{\bar{Y}} r_{\nu\omega}.$$

$\bar{E} f_2 / \bar{Y}$ is the elasticity of the profit rate with respect to the employment rate, $s_E / \bar{E}$ is the coefficient of variation of the employment rate, and $s_\omega / \bar{Y}$ is the standard error of the residual of the reduced form explaining variations in Y, expressed as a percentage of $\bar{Y}$. There is a strong presumption that this condition will be met. The elasticity of profits with respect to employment is well over unity; $s_E / \bar{E}$ is the coefficient of variation of the employment rate itself, while $s_\omega / \bar{Y}$ is the coefficient of variation of Y after movements explained by L and R have been removed; and $r_{E\nu}$ is likely to be larger than $r_{\nu\omega}$, since the relation between ν and E is a directly functional one, while we have no reason to presume even the sign of the correlation between ν and ω, and certainly none to suppose its magnitude to be large. For all these reasons, we expect $\hat{f}_1 > f_1$ even when $M_{\nu\omega}$ is positive. And even under the remote possibility that $\hat{f}_1 < f_1$, it is extremely unlikely that this would outweigh the specification bias that makes $\hat{f}_1 > b_1$. We thus can remain virtually certain that the expected value of $\hat{f}_1$ exceeds b_1, even when positive correlation between ω and ν is allowed for.

independent variables, we obtain:

$$(9.12.2)\quad \begin{aligned} Y_{gt} = &- \underset{(.1517)}{.3097} + \underset{(.1269)}{.0774\Delta C_{t-1}} + \underset{(.1828)}{.1938V_{t-1}} - \underset{(.1848)}{1.2038J_t} \\ &- \underset{(.0691)}{.2102G_t} + \underset{(.1895)}{1.024L_t} + \underset{(.1669)}{.6178E_t} \quad (r^2 = .99); \end{aligned}$$

$$(9.12.3)\quad \begin{aligned} Y_{gt} = &- \underset{(.2545)}{.4198} + \underset{(.2330)}{.2631\Delta C_{t-1}} + \underset{(.2398)}{.1301V_{t-1}} - \underset{(.2753)}{1.2814J_t} \\ &- \underset{(.1524)}{.1019G_t} + \underset{(.6410)}{.6002L_t} + \underset{(.3064)}{.7693E_t} + \underset{(.0245)}{.0258W_t} \quad (r^2 = .98). \end{aligned}$$

The coefficient of L_t falls progressively as we add E_t and W_t to the list of explanatory variables, becoming statistically insignificantly different from zero when both E_t and W_t are present. (The number in parentheses below each coefficient gives its standard error of estimate.) This is precisely what one would expect to happen if L_t in (9.12.1) were acting as a proxy for the forces represented by E_t and W_t. That its coefficient should decline when the additional variables are added, if it were originally acting as a proxy for them, is obvious. That the standard error of the coefficient of L_t should rise is also probable, once it is recognized that if E_t and W_t are indeed capturing some of the same forces that were imbedded in L_t, they will be to that extent collinear with L_t, a situation likely to raise the standard error of the coefficient of L_t as compared with the case in which E_t and W_t are not present in the regression.

Comparable results emerge when we apply similar adjustments to the K-M procedure for Model B. Equation (9.13.1) is a regression of the K-M type, using Z_t^* as the tax variable. Equations (9.13.2) and (9.13.3) show how the coefficients are altered by the addition of E_t and W_t as explanatory variables.

$$(9.13.1)\quad \begin{aligned} Y_{gt} = &\ \underset{(.098)}{.386} + \underset{(.347)}{.291\Delta C_{t-1}} - \underset{(.386)}{.494V_{t-1}} - \underset{(.400)}{1.971J_t} - \underset{(.212)}{.220G_t} \\ &+ \underset{(.104)}{.481Z_t^*} \quad (r^2 = .87); \end{aligned}$$

$$(9.13.2)\quad \begin{aligned} Y_{gt} = &- \underset{(.304)}{.498} + \underset{(.291)}{.015\Delta C_{t-1}} + \underset{(.416)}{.350V_{t-1}} - \underset{(.322)}{2.128J_t} \\ &- \underset{(.170)}{.259G_t} + \underset{(.108)}{.273Z_t^*} + \underset{(.302)}{.908E_t} \quad (r^2 = .92); \end{aligned}$$

$$(9.13.3)\quad \begin{aligned} Y_{gt} = &- \underset{(.246)}{.537} + \underset{(.265)}{.358\Delta C_{t-1}} + \underset{(.345)}{.134V_{t-1}} - \underset{(.326)}{1.577J_t} \\ &- \underset{(.156)}{.050G_t} + \underset{(.113)}{.073Z_t^*} + \underset{(.244)}{.934E_t} + \underset{(.015)}{.041W_t} \quad (r^2 = .95). \end{aligned}$$

Here the reduction in the coefficient of Z_t^* is even more dramatic, as one moves from (9.13.1) to (9.13.2) and (9.13.3), than was the case with that of L_t in equations (9.12.1)–(9.12.3).

We have performed numerous experiments similar to those presented above, using alternative forms of the basic equations and alternative definitions of some of the variables. The results uniformly confirm the conclusions derived above, that the tax rate has not had a significant influence on before-tax rates

of profit. This is perhaps most dramatically illustrated by regression (9.14), in which only E_t, W_t, and Z_t^* appear as explanatory variables.

$$(9.14)\quad Y_{gt} = - \underset{(.195)}{.584} + \underset{(.255)}{.819E_t} + \underset{(.017)}{.073W_t} - \underset{(.114)}{.101Z_t^*} \quad (r^2 = .84).$$

Not only is the tax rate statistically insignificant in explaining variations in gross-of-tax profit rates, once E_t and W_t are introduced, but the sign of its (insignificant) effect is negative!

It should be noted, moreover, as the analysis of part III shows, that our estimates of the coefficients of L_t and Z_t^* are, for two distinct reasons, likely to be biased in a positive direction.

We turn now to the interpretation of the coefficients of L_t and Z_t^* in regressions of the types presented above. We believe that the forces working toward equilibrium in the capital market will tend to produce responses in the rate of return to capital in noncorporate forms which are similar to those of the rate of return to corporate capital. Hence if, as a consequence of a change ΔZ in the corporation tax rate, the gross-of-tax yield Y_{gt} on corporate capital remains unchanged, the net-of-tax yield will fall by $Y_{gt}\,\Delta Z$. Although initially there may be no effect on the yields of noncorporate investments, there will now be a tendency for investors to prefer these alternative forms of capital assets. Once sufficient time has elapsed to permit equilibrium in the capital market to be restored, it is to be presumed that the net rate of return will have experienced similar changes in all uses of capital. If, then, we interpret the coefficients of regressions (9.12.3), (9.13.3), and (9.14) to reflect comparisons in which the capital market has approached equilibrium, we must consider what they mean in terms of the incidence of the corporation tax.

Obviously, if the net yield of capital falls by $Y_{gt}\,\Delta Z$, in both corporate and noncorporate uses, as a consequence of the tax change ΔZ, capital will have borne more than the full burden of the tax change. Capital in both corporate and noncorporate uses will have lost the percentage ΔZ of its initial income, while the government will have gained only ΔZ times the amount of income accruing to corporate capital. Since in the United States approximately half of the total income from capital is generated outside the corporate sector (principally in farming and residential housing), a coefficient of zero for either L_t or Z_t^* would imply, under the assumption of capital market equilibrium, that capital was bearing approximately twice the burden of the tax change. A coefficient of unity for L_t, on the other hand, would imply that the net-of-tax return on capital was unchanged, the tax presumably being reflected in a rise in the prices of the products of the corporate sector. This situation can be interpreted as a case of consumers bearing the burden of the tax or, what amounts to the same thing, as a case in which labor and capital bear the tax in proportion to their contributions to the national income. The case of capital just bearing the full burden of the tax occurs when the net-of-tax rate of return falls by approximately .5 ΔL with a reduction of this amount in the rate of return to all capital (corporate plus noncorporate) adding up to the total burden of the tax as long as the total net income from capital is twice the amount of such income generated in the corporate sector. In order for this result to occur, the gross-of-tax rate of return to corporate capital must rise by .5 ΔL; hence a coefficient of .5 for L_t implies that capital bears approximately the full burden of the tax. This is almost precisely the figure emerging from our regression (9.12.3).

To interpret the coefficient of Z_t^* in equation (9.13.3) above, let us first note that this coefficient implies that an increase of 10 percentage points in the rate of tax will lead to an increase of .0073 in Y_{gt}. If Y_{gt} were initially 15 percent (approximately the average figure for the K-M sample), it would rise to 15.73 percent, and the net rate of return would fall by .84 percentage points, reflecting the rise of 0.73 percentage points in Y_{gt} minus the extra tax collection of 1.573 percentage points. This fall in the net rate of return is larger than the rise in the gross rate of return, reflecting that capital in this case is apparently bearing somewhat more than the full burden of the tax. The full burden, as indicated above, would tend to be borne by capital if the fall in the net rate of return to capital were about equal to the rise in the gross rate of return to corporate capital.

We cannot be certain that the assumption of a well-functioning capital market is the appropriate one for interpreting our results. However, it is important to point out that a regression analysis of this type in effect compares the *levels* of the variables at each observation with those at every other observation in the sample; it does not compare year-to-year changes. Add to this the fact that the tax rate has moved generally upward through the period rather than, for example, oscillating around a flat trend line. We feel reasonably confident that by the 1950s the economy had adjusted to the fact of high corporation tax rates and that in the 1930s it had pretty well adjusted to the relatively lower rates then prevailing. If this is the case, the bulk of the variance being explained in our regressions was between positions reasonably close to long-run equilibrium, and the assumption that the capital market did function to reflect in the noncorporate rate of return the influence of tax changes is closer to being correct than the alternative assumption of no linkage between changes in the corporation tax rate and the rate of return on capital outside the corporate sector. We therefore would draw the conclusion from regressions (9.12.3) and (9.13.3) that capital probably bears close to the full burden of the corporation income tax, and possibly somewhat more than the full burden.

V

To summarize, we have shown that the K-M estimates of the effect of changes in the corporation income tax rate upon the gross rate of return to capital in the corporate sector are subject to a strong positive bias. The gross rate of return has been high, during the period they examine, in times of prosperity and of mobilization or war; it has been low in times of depression or recession. Coincidentally the corporation tax rate was at its lowest levels during the depression years, reached its peaks during World War II and the Korean war, and remained at relatively high levels during the prosperous years of the late fifties. By failing to introduce variables which adequately capture the influence of cyclical and wartime phenomena, K-M have produced

estimates in which a good part of the effects of these phenomena upon profit rates is attributed to changes in the corporation income tax rate.

When we introduce a cyclical variable — the employment rate — and a wartime dummy variable as added explanatory variables in the K-M regressions, the estimated effect of changes in the tax rate upon the profit rate falls dramatically. Instead of implying the more than 100 percent shifting of the corporation tax which K-M found, our estimates imply that capital bears approximately 100 percent of the burden of the tax.

In part III we examine in detail K-M's objections to the use of the employment rate (which we recognize to be an endogenous variable of a broader system) as a proxy for the exogenous forces producing cyclical movements. We show there that our estimates are subject to two types of bias, both of which lead to an overstatement of the effect of tax rate changes on the gross profit rate. The fact that these biases are positive only strengthens our argument that the K-M estimates grossly overstate the true effects of tax changes on profits; and if the biases in our estimates are important in magnitude, it suggests that capital may indeed be bearing more than the full burden of the corporation income tax.

We remain impressed by the difficulties, brought out in the introduction to this essay, of making inferences concerning tax incidence from time-series data. In particular, we note that we have not undertaken the task of exploring alternative formal models which could conceivably improve upon the basic K-M framework. We have instead operated within that framework, modifying it only to the extent of incorporating the cyclical and wartime variables. Within this focus, we feel that we have shown unequivocally that the K-M procedure, corrected so as to reduce the bias stemming from the proxy role of their tax variables, does not lead to their conclusion of more than 100 percent shifting of the corporation income tax but, rather, to the conclusion that capital bears approximately the full burden of that tax.

It is noteworthy, however, that our results suggest that the incidence of the corporation income tax lies within the range obtained by Harberger [1962], using an entirely different approach. He developed a general equilibrium model of two sectors (corporate and noncorporate), in which the corporation income tax was viewed as a tax on the use of capital in the corporate sector. In that model, the incidence of the tax was shown to depend critically on three key parameters: the elasticities of substitution (a) between the products of the two sectors, (b) between labor and capital in the production of the corporate sector's product, and (c) between these same two factors in the production of the noncorporate sector's product. Then, applying a range of plausible values for these three elasticities, Harberger estimated that the plausible range for capital's share of the total burden of the corporation income tax lies between 90 and 120 percent.

Thus, to the extent that one is willing to accept the general framework set out by K-M and to run the risks of time-series analyses, one might construe the present paper as providing an independent confirmation of Harberger's earlier results. We are too skeptical to make such a claim, however, and rest our case with the assertion that K-M's conclusions are not valid.

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Appendix I Corporation Tax Shifting: A Response

Marian Krzyzaniak and Richard A. Musgrave

We welcome the growing interest in empirical approaches to corporation tax incidence, of which the recent paper by Cragg, Harberger, and Mieszkowski ([1967]; referred to below as C-H-M) is the latest evidence. In evaluating our earlier results (Krzyzaniak and Musgrave [1963]), the authors claim to have shown "unequivocally that the Krzyzaniak-Musgrave procedure, corrected so as to reduce the bias stemming from the proxy role of their tax variables, does not lead to their conclusion of more than 100 percent shifting of the corporation income tax, but rather to the conclusion [previously arrived at by Harberger] that capital bears approximately the full burden of the tax" (see Cragg, Harberger, and Mieszkowski [1967], p. 821). We do not think that their case sustains this claim.

INTRODUCTION OF EMPLOYMENT VARIABLE

C-H-M hold that our lagged nontax variables do not sufficiently reflect the current state of economic pressure, and that our current tax-rate variable acts as a proxy for such pressure. The degree of shifting is thus believed to be overstated, and to remedy this, C-H-M introduce a current-pressure variable, defined as the employment rate E_t. Following previous comments by Goode [1966] and Slitor [1966], introduction of such a variable reduces the shifting coefficient for our L_t formulation, but (see C-H-M, equation [9.12.2]) the coefficient remains significant and still sustains the hypothesis of full shifting.

However, C-H-M hold that this result still leaves a significant upward bias, and offer a proof for this. We do not find their proof of upward bias conclusive,[1]

[1] The first step in C-H-M's argument is to show that a_1 in their equation (9.1) is biased upward. According to their equation (9.6), the bias equals $(a_1 - b_1) = b_2 M_{RZ^*}/M_{LZ^*}$. Granting this formulation, we do not follow the conclusion that b_2 must be positive. It would have been helpful to show that b_2 is estimated from

$$b_2 = \frac{M_{LZ^*}M_{YR} - M_{YZ^*}M_{LR}}{M_{LZ^*}M_{RR} - M_{RZ^*}M_{LR}} = \frac{\begin{vmatrix} M_{LZ^*} & M_{LR} \\ M_{YZ^*} & M_{YR} \end{vmatrix}}{\begin{vmatrix} M_{LZ^*} & M_{LR} \\ M_{RZ^*} & M_{RR} \end{vmatrix}}.$$

Granting C-H-M's assumption that all the moments are positive, it does not follow that b_2 must be positive as well. It may be negative, depending on the actual values of the moments. Since

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and even if an upward bias existed no evidence is offered that it is of significant magnitude.

Omitting minor points of disagreement,[2] our position on the main issue may be restated briefly. As we suggested earlier, our nontax variables may fail to allow fully for current demand pressure, and the tax-variable coefficient acting as proxy for pressure may have an upward bias.[3] We now add that this bias need not be upward but may be in either direction.[4] But this is only part of the picture. 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.

C-H-M feel sure that the economy cannot generate a high degree of shifting and that, on balance, their corrections improve the result. We do not share their intuition but consider this the matter under investigation.[5] In time, the issue may be settled by resort to a superior structural system, which leaves no doubt that pressure is accounted for fully through predetermined variables and in which the reduced form is estimated from a fully identified structure. But this C-H-M do not offer. Until such superior model is available, the issue remains open.

INTRODUCTION OF DUMMY VARIABLE

C-H-M believe that during certain war years (1941–1942 and 1950–1952) in particular, high taxes were caused by high profits, and not vice versa. They feel that the combination of war and peace years results in a nonhomogeneous sample, and to deal with this they introduce a dummy variable for the indicated war years. The coefficient of the tax-rate variable is greatly reduced thereby

the R variable is unknown, the values of the moments are also unknown. The possibility of negative bias in a similar situation is illustrated by comparison of equations (2) and (3) in Table 61, p. 44, of our original study, where introduction of the G variable *raises* the L coefficient while giving a *negative* coefficient for G.

The second step in C-H-M's argument is to prove upward bias of f_1 in their equation (9.2). This again involves the claim that b_2 is positive, which we believe to be questionable. Moreover, the second step requires knowledge of the c-coefficients in their equation (9.7) which, being similar to (9.3), raises identification problems for all c.

[2] Several of these points may be noted: (1) The C-H-M choice of the unemployment rate seems inferior to Slitor's use of the GNP gap. (2) It would have been appropriate to use our standard equation (Krzyzaniak and Musgrave [1963], p. 44, equation [3]) rather than that including the G variable, since this is our preferred formulation. (3) If a current-pressure variable is used, use of the lagged variables becomes unnecessary, and they should be dropped. (4) As the C-H-M equation containing E_t is not in reduced but in structural form, estimation by least squares becomes questionable. (5) It should be added finally, that our interpretation of our earlier finding has been to emphasize not the point estimate of a, say, 1.339 shifting coefficient, but rather the strong support for the hypothesis of high shifting. See Krzyzaniak and Musgrave ([1963], pp. 46–49).

[3] See our response to Goode and Slitor, in Krzyzaniak ([1966], p. 248).

[4] See n. 1.

[5] C-H-M hold that our results, if valid, would call for a complete reformulation of price theory. We think this is an exaggeration. Restrained monopoly pricing, satisficing, sales maximization under profit constraint, signaling among oligopolists, profit-oriented wage bargaining, price umbrellas, all may explain substantial shifting in the context of a more or less traditional price theory (see also Krzyzaniak and Musgrave [1963, chap. 1, and p. 46, n. 4]).

(see C-H-M, equations [9.12.2], [9.12.3]), its standard error is more than tripled, and the t-ratio becomes less than one. While this does not demonstrate zero shifting as C-H-M conclude, it does suggest that the model does not permit us to choose between the hypotheses of zero and full shifting. However, we cannot accept C-H-M's procedure in dealing with the war years.

The C-H-M result is obtained by adding the dummy variable to the prior introduction of the E_t variable. We find that the result differs if the dummy variable is introduced without first adding E_t. This procedure gives a very high and significant shifting coefficient, with an insignificant coefficient for the dummy variable.[6] C-H-M's results thus hinge wholly on the compounded effect of introducing both the E_t and dummy variables. As these variables are highly collinear with the other variables in the system,[7] it is not surprising that the value of the coefficients should change sharply and become insignificant.

While we do not know whether or how the shifting process differed between war and peace years, we are prepared to test this possibility.[8] To us, a more constructive approach is to omit the war years and to re-estimate our earlier equation for the nonwar years only. We find that the shifting coefficient remains high and significant and does not differ greatly from that for the total period.[9] If the E_t variable is added, the shifting coefficient remains above one, but its significance is reduced.[10] However, the loss of significance is not nearly so great as results in the C-H-M formulation (equation [9.12.3]).

A further and better way of testing C-H-M's hypothesis of nonhomogeneity is to retain all years in the equation but to estimate separate shifting coefficients

[6] The resulting equation is

$$Y_t = .2028 + \underset{[.0638]}{.0158\Delta C_{t-1}} - \underset{[-.1995]}{.0553V_{t-1}} - \underset{[-4.550]}{.6837J_t} - \underset{[-2.1171]}{.3001G_t} - \underset{[-.8517]}{.014W_t} + \underset{[5.4430]}{1.8619L_t}, \qquad R_a = .984.$$

The t-scores are reduced and overshifting becomes very high, reflecting collinearity of W_t with other variables in the equation.

[7] See Farrar and Glauber [1967], who show that dummies turn collinear with other variables already at low correlation levels.

[8] C-H-M at one point suggest that during the war period the correlation may have run from high profits to high tax rates rather than vice-versa. This is indeed a nightmarish possibility for anyone concerned with incidence analysis. If tax rates are endogenous, the incidence concept dissolves. In this case, nothing is gained by introducing W. The only way out is to omit the period. If C-H-M do not claim reversal of causation but only a change in the nature of the reaction to taxes, there are several ways to account for this, among which introduction of W seems to us the weakest.

[9] Using the peace years only, we obtain

$$Y_t = .3315 + \underset{[2.1145]}{.6523\Delta C_{t-1}} - \underset{[-2.0796]}{.6101V_{t-1}} - \underset{[-3.0642]}{1.0199J_t} + \underset{[3.0772]}{1.2853L_t}, \qquad R_a = .957,$$

for our preferred equation, and

$$Y_t = .2615 + \underset{[1.4682]}{.2754\Delta C_{t-1}} - \underset{[-.8720]}{.1478V_{t-1}} - \underset{[-3.0778]}{.7358J_t} - \underset{[-3.4863]}{.5125G_t} + \underset{[3.9061]}{1.9838L_t}, \qquad R_a = .985,$$

if G is included.

[10] Adding E, this becomes

$$Y_t = -.1802 + \underset{[1.2000]}{.2245\Delta C_{t-1}} + \underset{[.2863]}{.0590V_{t-1}} - \underset{[-3.4829]}{.8755J_t} - \underset{[-2.5280]}{.4086G_t} + \underset{[1.5661]}{.4897E_t} + \underset{[1.9432]}{1.1524L_t}, \qquad R_a = .975.$$

for the two sets of years. The results show that the shifting coefficients do not differ greatly between the two periods, and remain significant.[11] All this renders one skeptical about whether the presumed nonhomogeneity indeed exists. Application of Chow's test supports this skepticism and shows that the years in our sample are fairly homogeneous.[12] In all, these results suggest that inclusion of the war years in the total sample is justified and that introduction of a dummy variable is not called for.

RELATION TO HARBERGER MODEL

C-H-M then proceed to use their tax coefficient of about .5 to verify Harberger's [1962] earlier model. This is not the place to consider the realism of that model, nor do we want to query why C-H-M bother to verify it with a coefficient which they show to be totally insignificant. Our concern is only with whether this conclusion (assuming their coefficient of .5 to be significant) is in fact relevant to Harberger's earlier reasoning.

The essence of the earlier Harberger model is that introduction of the corporation tax (or of a rate increase) leads to capital flow from the corporate to the unincorporated sector, thereby raising corporate rates of return before tax in order to equate net rates of return in both sectors. This means that the adjustment is made over a period sufficiently long to permit the required capital flow. Our model, and with it the C-H-M estimate, derive the tax-rate coefficient from a system in which the tax rate is unlagged. While use of an unlagged tax variable does not mean that the entire effect must occur in the same year (see Krzyzaniak and Musgrave [1963], p. 64), it does suggest a process which is quite speedy and which hardly permits occurrence of the capital flow needed to implement Harberger's adjustment mechanism. Yet, the C-H-M reasoning implies that the adjustment was of this sort.

C-H-M do not raise this point explicitly, but the issue seems to be touched on in a brief passage, where it is argued "that a regression analysis of this type in effect compares the *levels* of the variables at each observation with those at every other observation in the sample; it does not compare year to year changes" (Cragg, Harberger, and Mieszkowski [1967], p. 820). We are unable to follow the reasoning involved in this statement, or to discover the statistical theory on which it is based. If the time-series regression yields significant coefficients for an unlagged variable, while experiments with lagged variables fail to do so, one is justified in concluding that the effects come about in a relatively short period. It is highly unlikely, therefore, that Harberger's capital-flow process is involved.

[11] The resulting estimate for our preferred form is

$$\begin{aligned} Y_t = .2856 + \underset{[2.7576]}{.4940\Delta C_{t-1}} - \underset{[-2.7772]}{.5557V_{t-1}} - \underset{[-3.6060]}{.7531J_t} + \underset{[5.250]}{1.1951L_t^{(1)}} + \underset{[11.0145]}{1.3416L]_t^{(2)}}, \\ R_a = .973, \end{aligned}$$

where $L_t^{(1)}$ applies for peace years, being set equal to zero in war years (1941, 1942, 1950, 1951, 1952), and vice versa for $L_t^{(2)}$.

[12] Estimating Chow's equality-of-coefficients test for equation line 4, table 6-1, p. 44, in Krzyzaniak and Musgrave [1963], we obtain $F_o = 1.135$, while the critical value at 95 percent (with 5 and 15 degrees of freedom) is 2.9 (see Johnston [1963], p. 138).

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Appendix II Corporation Tax Shifting: Rejoinder

John G. Cragg, Arnold C. Harberger, and Peter Mieszkowski

If there is anything in our paper which the response of Krzyzaniak and Musgrave (hereafter referred to as K-M) has caused us to regret, it is the sentence from which they quote in their initial paragraph. It is, and has from the beginning been, our judgment that the pitfalls associated with estimating corporation-tax incidence from time-series data, in K-M fashion, are too numerous and serious for the results to be trustworthy. Section I of our paper concludes: "we do not in any way contend that our modifications of the K-M procedures and experiments adequately cope with the difficulties listed above. Our purpose, in fact, is the much more modest one of showing that making highly plausible and theoretically justified modifications in experiments of the K-M type leads to a reversal of the main conclusions and that, therefore, at the very least, serious doubt is cast on the validity of K-M's conclusions." And our concluding paragraph reads: "Thus, to the extent that one is willing to accept the general framework set out by K-M and to run the risks of time-series analyses, one might construe the present paper as providing an independent confirmation of Harberger's earlier results. We are too skeptical to make such a claim, however, and rest our case with the assertion that K-M's conclusions are not valid." Even the paragraph containing the offending phrase begins: "We remain impressed by the difficulties, brought out in the introduction to this essay, of making inferences concerning the incidence from time-series data." Yet the phrase is there, its spirit is out of tune with our basic position, and we (particularly the middle author, whose temptation is probably most responsible for this lapse) are sorry. Our purpose was to demonstrate, without accepting K-M's framework, how sensitive were their results to plausible and indicated changes. Given our skepticism about the framework itself, it was inappropriate for us even to hint that our experiment might conceivably be taken to confirm Harberger's earlier results.

With the above expiation, we are in a better position to confront K-M's result that an alternative method of dealing with the years of war and mobilization (1941–1942, 1950–1952) — simply leaving them out — produces a shifting coefficient substantially higher than that obtained by us. This simply confirms what we were trying to say in our paper. Making modest and apparently quite plausible changes in specification, grossly different results are obtained. This suggests, as we claim, that one should be skeptical and cautious about the framework generating such fragile and volatile outcomes.

While we find that when K-M eliminate the observations for 1941–1942 and 1950–1952, their results simply underline one of our main points, the same can not be said for their calculation of separate coefficients for $L_t^{(1)}$ and $L_t^{(2)}$ in their equation (5), footnote 11. Here we must demur outright. Our contention is that the years in question exhibited (for reasons extraneous to corporation taxes themselves) high profits and high taxes as compared with normal years. Our argument thus refers to the *intercept* of the profit function; we do not say anything about whether, within this small subset of years, the profit-tax function had a significantly different *slope* from that of other years. We are therefore both unperturbed and unconvinced by the K-M finding that the coefficients of $L_t^{(1)}$ (for the peace years) and $L_t^{(2)}$ (for 1941–1942, 1950–1952) are not significantly different from each other. We find it more appropriate to use a single tax variable L_t, and either to introduce a dummy variable for the years in question (as we do) or to throw out these observations entirely (as K-M do in their equation [2]).

Our final point relates to the fact that there is an upward bias in estimates of the tax-shifting coefficient derived from equations of the K-M type, even when they are "corrected," as we suggest, to include a variable reflecting the degree of employment (or some other indicator of the cyclical stage of the economy). K-M fault us for running regressions in which the employment variable (endogenous to the system) appears on the right-hand side. But in point of fact all of section III of our paper is devoted to a demonstration that, precisely for this case, upward biases of two types exist.

The first type is a specification bias. To demonstrate that it exists, we postulate a true (but possibly unobserved) exogenous cyclical variable, R, which appears along with the tax rate (and possibly other exogenous variables) in the reduced-form equation (9.7) determining the employment rate, E:

$$E_t = c_0 + c_1 L_t + c_2 R_t + \nu_t. \tag{9.7}$$

This equation is solved for R_t, and the result is substituted into the true reduced-form equation (9.3) determining Y_{gt}, the gross-of-tax rate of return in capital:

$$Y_{gt} = b_0 + b_1 L_t + b_2 R_t + \omega_t. \tag{9.3}$$

The resulting equation is:

$$Y_{gt} = \left(b_0 - \frac{b_2 c_0}{c_2}\right) + \left(b_1 - \frac{b_2 c_1}{c_2}\right) L_t + \frac{b_2}{c_2} E_t + \left(\omega_t - \frac{b_2}{c_2} \nu_t\right). \tag{9.9}$$

We are interested in the coefficient of L_t. The true causal influence of L_t on Y_{gt} is measured by b_1 in the true reduced-form equation. The coefficient of Y_t estimated from (9.9) will be subject to a specification bias of $-b_2c_1/c_2$. Since c_2 and b_2 (measuring the impact of our cyclical variable on employment and profit rates, respectively) are positive, and since c_1 (measuring the true impact of increased tax rates on employment) is presumably negative, the net effect is for the ill-specified coefficient of L_t in equation (9.9) to be greater than the correctly specified coefficient of L_t in equation (9.3). On this point it appears that K-M did not understand fully what we were doing. In our discussion it is clear that the coefficients of equations (9.3) and (9.7) are the true (not estimated) reduced-form coefficients; yet, when K-M argue that the sign of the bias might be negative as well as positive (their n. 1), they act as if b_2 referred to an estimated coefficient. To this we can only respond that it is not 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 $(b_1 - b_2c_1/c_2)$ 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.]