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A Comment on "Tax Policy and the Activities of Multinational Corporations"

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International taxation is a field that international economists have for the most part defaulted on, leaving it to public finance economists, who more patience for the details of tax law. Jim Hines' survey (1996) of the research, to which he has been a major contributor, is extremely clear. It will be my bible on this subject in the future.

The topics surveyed

To review the topics that are surveyed, they fall into three categories: (1) the impact of taxation on Foreign Direct Investment (FDI), (2) the impact on financing of multinational corporations (MNCs), and (3) the impact on other specific activities of MNCs.

The first category includes both studies of US direct investment in various foreign countries, and studies of FDI coming into the US from various source countries. He notes that the former set of studies are naturally suited to studying differences in host country tax systems and the second to differences in the source country. My first question is: Are there no tax studies of bilateral FDI among a set of countries? I am well aware that there are data problems, including many missing pairs, that are even more severe than those in the studies surveyed here. But it can be done. I know there are a few studies of bilateral FDI motivated by a desire

to test for regional blocs; Shang-Jin Wei and I have been doing one on a data set that we cobbled together, with 12 source countries and 54 host countries. The closest we get to public finance considerations is a dummy variable for whether there exists a bilateral tax treaty between the pair in question. (We find a statistically significant effect of 30 per cent.) There are simultaneity problems, to be sure. Furthermore, as I say, our data are cruder than those used here, and perhaps the audience for this conference and book are only interested in behavior that involves the United States, either as source country or host. But it seems to me there might be something to be learned from studying a larger cross-section of experience.

The second category includes implications of tax policy for leveraging, for transfer pricing, and for dividend payments. The third includes implications for R & D spending, exports, bribery of foreign officials, and choice of home country.

In almost all cases, firm behavior is found to respond significantly to tax incentives in the direction hypothesized. I find this quite impressive, even amazing. Apparently the typical procedure runs as follows. The researcher actually figures out what the incentive influencing some decision is, a priori, from reading the tax code (itself quite an impressive feat, given the complexity of the code), then finds the right data set for the question (or one that is close enough), tests for the hypothesized response, and finds effects that are significantly greater than zero statistically. Hines even deems it worth noting that the estimated elasticities are not infinite, that corporate behavior is governed by a trade-off in which non-tax factors enter along with tax factors.

Can things really be this clear-cut?

I said at the outset that the paper was very clear. But I am now going to suggest (mischievously) that it seems "almost too clear."

My question is: Is there any selection going on here (whether by the authors in reporting their results, the journals in publishing them, or Jim in choosing to include them in his survey)?

Or, at a minimum, do the authors consult with tax lawyers and their clients to find out directly

the aspects of the tax code that are operationally governing behavior, before formulating their hypotheses?

Why does it occur to me to be suspicious? It is because things never work out this neatly in the research I am accustomed to. Now part of this may be because in my fields of specialization -- international trade and macroeconomics -- we have such a hard time getting significant results. One would expect behavioral response to direct tax incentives to be relatively easier to find.

Consider the field of finance, however. Incentives in financial markets should be clear and arbitrage should be powerful. But the norm in finance is to *fail* to reject market efficiency, which is generally the null hypothesis; and here I am talking about the *successful* papers. To repeat, a successful paper must *fail* to get significant results. Sometimes the finance people fail to fail to reject the Efficient Markets Hypothesis. Then they get very upset, and call it an "anomaly." Familiar examples include the January effect, the small firm effect, the Initial Public Offering effect, the term structure bias, the forward discount bias, and the home country bias. The finance people don't calm down until someone comes up with an explanation for the anomaly -- usually a tortured one, in terms either of a tax effect that nobody had thought of before, or a risk premium. Then that explanation becomes the new Efficient Markets Hypothesis, which people can then fail to reject. Of course, it is generally much easier to fail to get significant results than to succeed in getting significant results, which is why we macroeconomists over the last decade or so have gone over to the system used by the finance people.

To put my reaction to this paper in a positive manner, it is refreshing to see so many researchers looking for and finding statistically significant evidence *in support of* their hypotheses, instead of either rejecting or failing to reject their hypotheses.

Transfer pricing

I have a few more specific comments on substance.

One place where Hines deviates a bit from the pattern of explaining the incentive first, and then the tests, is the issue of transfer pricing. As he notes, it has been alleged, particularly during the 1992 election campaign, that Japanese and other foreign-owned corporations set transfer prices so as to reduce their US tax obligations. This section of the paper deviates from most of the rest, first in the respect that the evidence is not especially strong, and second in that he doesn't explain the incentive up front. I think the right answer is that the corporate tax rate is higher in Japan and some European countries than in the United States; thus the incentive is to *overstate* artificially US earnings and thus US tax payments, and to reduce them elsewhere, just the opposite of what one would think from listening to the campaign rhetoric.

Missing topics

One kind of study that seems to be missing is studies of the *simultaneous* determination of the various decisions made by the MNC: whether and where to undertake FDI, how and where to finance it, how much profits to remit, and in what form. He mentions that these decisions should in principle be simultaneous, but I wonder, if indeed nobody has done it, then why not. Is it data limitations? The data apparently exist to address each of these questions independently, so why not together? Or is the problem that if too much is made endogenous, then there are not enough exogenous variables to identify the equations?

Trying to think of topics that might be missing from the paper, I have come up with another two. A relatively minor one (perhaps) is taxation of multinational corporations at the state level, particularly the unitary tax that has been such a big issue here in California.

Differences in the cost of capital

A bigger topic is the effect of international tax differences on the corporate cost of capital. This is one area of the literature that I happen to be familiar with, particularly the hypothesis that the cost of capital facing Japanese corporations is -- or, more accurately, was (in the 1980s) -- lower than that facing their American competitors. I must admit that this issue is the origin of some of my surprise that the lessons from the research described in this survey seem relatively clear.

Consider a fact that I already mentioned: the Japanese corporate tax rate during most of the 1980s, at 42 per cent, was higher than the US rate, at 34 per cent. You might think that the implications for the relative after-tax cost of capital in the two countries would be clear. Or that the implications of Japan's reduction of its corporate tax rate at the end of the decade, from 42 per cent to 37 1/2 per cent, would be clear. But the leading public finance authorities are divided on how to think about this.

Some authors, including Ando and Auerbach (1988), reach the intuitive conclusion, that "it is Japanese, not American, firms that are taxed more heavily..." who have a higher after-tax cost of capital. Others, however, notably Bernheim and Shoven (1986) and Shoven (1989), reach the conclusion that the high *average* corporate tax rate in Japan worked to reduce the effective *marginal* rate on new investment. The reason is that the high corporate tax rate increased the value to the corporation of borrowing to finance investment and deducting the interest payments from taxable income (an effect that is especially powerful for Japanese firms, because they are more highly indebted than American firms). Shoven (1989) thus finds that Japan's decision to reduce the average corporate tax rate in 1988 raised the effective tax rate on investment (by 9 percentage points).

Now this is the kind of directly conflicting conclusions to which I am accustomed. Ando and Auerbach were well aware of the tax advantages of borrowing in Japan, but put an upper bound on its size, and so claimed to be able to "rule out" the claim that the corporate tax

system gave firms there a cost-of-capital advantage.

Incidentally, the economists writing on this subject tended to agree that tax differences between the U.S. and Japan, however, they are interpreted, are much less important than differences in the *before-tax* real interest rate, in determining the relative cost of capital. (Low real interest rates did indeed produce a low cost of capital for Japanese firms in the 1980s, but this changed abruptly in 1990, when real interest rates were raised sharply and stock market values collapsed.)¹

Returning to the Hines paper, a good literature review of this sort can be put to several uses. I can think of four. First, it can be used to confirm our belief in the maximizing behavior of firms. I consider this the least interesting of the possible uses. Second, and most successfully in my view, the paper can be used to verify what are the important international attributes of the tax law, distinguishing those that don't seem to affect behavior, either because they are minor in magnitude or because they are not binding, from those that do. Third, one can take away various specific parameter estimates, such as Hines' interesting estimate that \$1 of foreign profit has the same effect on dividends paid to shareholders as \$3 of domestic profit.

Finally, one might use the what one has learned, in light of internationalization of the economy, to pass judgment on policy reforms. The paper does very little of this.

I will put one possible policy recommendation on the table. In 1986 the law was changed so that a portion of firms' R & D spending must be allocated to foreign income, reducing the tax advantage for some firms, particularly those with foreign sales but without excess foreign tax credits. Hines (1993) finds that R & D subsequently grew more slowly for those firms than for others. My final question is: Was this an undesirable policy change. Many economists, whether working in the fields of growth theory, international trade, labor, or

income distribution, have concluded that R & D has positive spillovers within the country where it is undertaken. If this is right, shouldn't we be encouraging MNCs to undertake R & D in the United States, regardless whether it supports their sales abroad or domestically?

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Note

. I survey this literature in Frankel (1993).