The Effects of Bilateral Tax Treaties on U.S. FDI Activity

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Abstract

The effects of bilateral tax treaties on FDI activity have been unexplored, despite significant ongoing activities by countries to negotiate and ratify these treaties. This paper estimates the impact of bilateral tax treaties using both U.S. inbound and outbound FDI over the period 1980–1999. Robust to a wide variety of alternative specifications, we find little evidence that bilateral tax treaties increase FDI activity, contrary to OECD-stated goals for such treaties.

Keywords: foreign direct investment, tax treaties, multinational corporations

JEL Code: F21, F23, H25

1. Introduction

Empirical studies on the effects of taxation on foreign direct investment (FDI) are as numerous and varied as the tax policies they study. Until this point, however, the impacts of bilateral tax treaties governing the taxation of FDI activity have been unexplored.[1] Worldwide, there exists a network of over 2,000 treaties which affect the taxation of the large majority of FDI (Radaelli, 1997). According to the Bureau of Economic Analysis (BEA, 1998), by 1998 these treaties covered approximately $774 billion of U.S. investment abroad (outbound FDI) and $586 billion of foreign investment within the U.S. (inbound FDI). These amounts represented 78% of total U.S. outbound FDI and 96% of the total U.S. inbound FDI.

The introduction to the OECD's model tax treaty states that a primary goal of a tax treaty is "removing the obstacles that double taxation presents", thus reducing its "harmful effects on the exchange of goods and services and movements of capital, technology, and persons" (OECD, 1997, p. I-1). Janeba (1995), Davies (2003), and Chisik and Davies (forthcoming) model these goals and demonstrate that by lowering withholding taxes on reparations to treaty partners and harmonizing tax laws, tax treaties can increase FDI. Empirical evidence by Altshuler and Newlon (1991), Hines (1992), Altshuler, Newlon and Randolph (1995), and Mutti and Grubert (1996) finds that changes in withholding taxes affect both the timing

of repatriation decisions and the mode of repatriation by firms. While this suggests that these treaties can affect the profitability of existing foreign investors, it does not necessarily say anything about the effects of these changes on overall FDI activity.

Furthermore, there are several legal and economic arguments contrary to the FDI-promotion effect of treaties. First, it is not clear that reducing firms' withholding tax burdens necessarily impacts the amount of investment. Hartman (1985) and Sinn (1993) point out that, since withholding taxes are only paid upon repatriation, it is less expensive to expand an overseas affiliate through retained earnings than through repatriated and re-exported funds. Because of this, they contend that only mature, non-expanding foreign affiliates will repatriate earnings. As Sinn notes, this implies that treaty-specified withholding tax reductions may have no effect on the size of FDI activity. In addition, there is the possibility that treaties stem from the rent-seeking lobbying efforts of current foreign investors rather than a desire by governments to reduce distortions and enhance efficiency.[2] Second, there are reasons to think that treaties may reduce the incentive to engage in FDI. Dagan (2000) notes that double taxation can be alleviated unilaterally just as well as through a bilateral agreement, and suggests that the actual intent of treaties is to reduce administration costs, reduce tax evasion, and to extract tax concessions from host countries. In fact, she goes so far as to condemn the double taxation objective as "a myth" (p. 939). Radaelli (1997) and Gravelle (1988) also assert that U.S. tax treaties are primarily geared towards reducing tax evasion rather than promoting FDI. As discussed by the OECD (1994), treaties combat transfer pricing and treaty shopping (a practice in which investments are funneled through a treaty country by a third nation for the purpose of avoiding or reducing taxes) by promoting the exchange of information between tax authorities.[3] Thus, if investment is taking place on the margin to avoid taxes, treaty formation may reduce FDI.

These varied arguments indicate that the effect of bilateral tax treaties on the amount of FDI between the U.S. and partner countries is an open question. The answer is of particular importance in light of the efforts expended by the U.S. Treasury to negotiate the treaties, as well as the necessary ratification by the U.S. Senate. This paper makes a first attempt at answering this question by estimating the impact of bilateral tax treaties on both U.S. inbound and U.S. outbound FDI over the period from 1980 to 1999. The sample we construct represents easily the most comprehensive set of consistent data on FDI used in the literature to date and allows us to examine the impact of new bilateral tax treaties for a wide variety of U.S. partner countries that occurred during the 1980s and 1990s. Our initial empirical framework for testing the determinants of FDI activity is one recently developed by Carr, Markusen and Maskus (2001), as modified by Blonigen, Davies and Head (2003). As opposed to the traditional gravity model, this framework is more strongly based on established theory of multinational enterprises (MNEs). However, we also consider modifications to the CMM framework that allow us to better fit the data.

Robust to a wide variety of alternative empirical specifications, we find little evidence that bilateral tax treaties increase FDI activity, contrary to OECD-stated goals for such treaties. While there is some heterogeneity in individual country experiences, we generally find that the average new treaty effect is not statistically different from zero with a very small point estimate. This is true for both FDI into the U.S. as well as U.S. outbound FDI. Thus, our results find no support for the idea the tax treaties increase FDI and may instead

lend credence to the concerns expressed by Radaelli (1997) and Dagan (2000). The paper proceeds as follows. In Section 2, we discuss U.S. tax treaties including their history, how they are formed, and their key functions. In Section 3, we present our empirical methodology and data. Section 4 presents our results and Section 5 concludes.

2. Tax Treaties

In this section, we briefly discuss the history and functions of bilateral tax treaties. Given our paper's focus, this section concentrates on these issues from a U.S. perspective; however, most of the discussion applies to other countries as well. The process of creating an international tax treaty with the U.S. is the same as for other international treaties. The Assistant Secretary for Tax Policy and the International Tax Counsel, acting on behalf of the Department of the Treasury, undertake the actual treaty negotiation. After the treaty is signed by the President or his delegate, it proceeds to the Senate. There, the Senate Foreign Relations Committee holds hearings before the treaty proceeds to the full Senate for ratification.[4] Not all signed treaties receive Senate approval. For example the Bangladeshi treaty was signed in 1981 but failed in the Senate. Some treaties fail multiple times before they are finally ratified.[5] Note that because of this delay, there can be a lag between when a treaty is signed and when it enters into force. Additionally, the enforcement date can differ from the effectiveness date which is the year in which the treaty provisions initially applied. The effective date can come before or after the enforcement date and can even precede the signing date. Once entered into force, treaties usually remain in effect, although most contain provisions for termination after a six-month notification by either nation (Doernberg, 1997). Of those treaties allowed to lapse, the majority (Aruba, Malta, and the Netherlands-Antilles) were the result of insufficient attempts by partner nations to prevent treaty shopping.[6] As with many types of international agreements, the number of tax treaties the U.S. has in force is on the rise. Table 1, which lists U.S. treaty partners and the effective dates, shows that the U.S. has gone through three major waves of treaty negotiations. The first followed WWII and saw the U.S. complete treaties with its major economic partners: the western European countries, Canada, Japan, Australia, and New Zealand. The second wave of the late '70s and early '80s focused on a wider variety of countries, including China, Egypt, Korea, India, Indonesia, the Philippines, and Spain. More recently, beginning in the early 1990s, the fall of the former Soviet Union has led to additional U.S. treaty activity with respect to many of the former Soviet Bloc countries. Overall, the pace of new treaty implementation has increased. In fact, half of the treaties currently in force were ratified only in the past twenty years with nearly one-third of the current treaties implemented since 1990.

Tax treaties perform four primary functions. The first is to standardize tax definitions and solidify the tax jurisdictions of treaty partners. It is common practice that only income generated by foreigners through a permanent establishment in the host country is subject to host taxation. If the countries differ in their definitions of permanent establishments, this can lead to double taxation and inefficient capital flows (Hamada, 1966). The impact of differing tax definitions on FDI is explored theoretically by Janeba (1996). Hines (1988) finds that the 1986 Tax Reform Act, which revised U.S. definitions, had a significant effect

Table 1.   List of U.S. tax treaties (years effective).

Australia (1953–)	Austria (1957–)	Barbados (1986–)
Belgium (1953–)	Canada (1937–)	China (1986–)
Cyprus (1985–)	Czech Republic (1993–)	Denmark (1948–)
Egypt (1981–)	Estonia (1999–)	Finland (1952–)
France (1936–)	Germany (1954–)[a]	Greece (1953–)
Honduras (1957–1966)	Hungary (1979–)	Iceland (1975–)
India (1990–)	Indonesia (1990–)	Ireland (1951–)
Israel (1994–)	Italy (1956–)	Jamaica (1981–)
Japan (1955–)	Kazakhstan (1996–)	Korea (1979–)
Latvia (1999–)	Lithuania (1999–)	Luxembourg (1964–)
Malta (1982–1995)	Mexico (1993–)	Morocco (1981–)
Netherlands (1948–)	New Zealand (1951–)	Norway (1951–)
Pakistan (1959–)	Philippines (1982–)	Poland (1976–)
Portugal (1995–)	Romania (1976–)	Russia (1993–)
Slovakia (1993–)	Slovenia (2001–)	South Africa (1952–1987, 1997–)
Spain (1990–)	Sweden (1939–)	Switzerland (1951–)
Thailand (1997–)	Trinidad and Tobago (1967–)	Tunisia (1990–)
Turkey (1997–)	Ukraine (2000–)	United Kingdom (1946–)
U.S.S.R. (1976–)[b]	Venezuela (1999–)	

Notes. [a]Extended to cover reunified Germany 1990. [b]Treaty provisions extended to former members until individual treaties can be negotiated.
Source: Worldwide Tax Treaties Database at Tax.com.

on U.S. MNEs. Since a common goal for tax treaties is to reduce double taxation and the inefficiencies it causes, standardization of tax definitions and jurisdictions is a powerful tool. Treaty tax definitions generally match those provided by the OECD's model tax treaty (OECD, 1997).

A second role for a tax treaty is to reduce transfer pricing and other forms of tax avoidance. They do so in three key ways. First, they influence the method used by tax authorities to calculate the internal price used by firms.[7] Second, they establish rules for resolving conflicts between tax authorities or between governments and firms. Third, they promote the exchange of tax information. Information exchange can be narrow or broad. The narrow view limits exchanges to those necessary to implement the treaty's provisions of the treaty. The broad view uses exchanges to help in overall tax policy implementation. The OECD's model treaty takes the broad view in its commentary where it states that information exchange is intended to be used to the widest possible extent (OECD, 1997). The U.S. has taken this to heart and is one of the most vigorous users of information exchange (Easson, 1999).[8] Furthermore, the U.S. has made information exchange a requirement for continuing existing treaties and cancelled some treaties for their failure to do so (Eden, 1998). Information exchange is also linked to the third goal of tax treaties, which is to prevent treaty shopping. According

to Ault and Bradford (1990) and Radaelli (1997), this is a primary focus of recent treaties and has prompted the U.S. to renegotiate many of its older treaties as well. Although there are many variations in the regulations regarding treaty shopping, the most common rules restrict treaty benefits if more than 50% of a corporation's stock is held by a third, non-treaty country's residents (Doernberg, 1997).

Finally, tax treaties affect the actual taxation of MNEs. They do so through the rules affecting double taxation relief and the withholding taxes levied on repatriations by FDI. Following the OECD model treaty guidelines, tax treaties with the U.S. specify that both countries must offer either foreign tax credits when calculating the domestic tax bill or exempt foreign-earned profits from domestic taxation.[9] This does not usually affect U.S. tax policy as it already provides foreign tax credits to its investors whether their income is earned in a treaty or a non-treaty country. It does, however, affect the taxation of some treaty partners.[10] In addition to the provisions for double taxation relief, the treaties also often reduce maximum allowable withholding taxes on three types of remitted income: dividend payments, interest payments, and royalty payments. There are no tax treaties which raise tax rates.

From the above discussion, the primary costs and benefits of entering into a tax treaty can be summarized as follows. Since a treaty can lower the overseas taxes, more income is repatriated to the home nation. At the same time, lower overseas tax rates can promote a more efficient global allocation of investment.[11] This gain is accompanied by potentially reduced tax receipts since treaties also lower the tax rates levied on inbound investment. This cost can be mitigated in two ways. First, improved international cooperation may reduce tax evasion. It can also reduce the administrative costs associated with enforcement and tax collection. Second, since withholding taxes can be tailored to the specific investment from a treaty partner, it may be possible to set tax rates which encourage tax-sensitive inbound investment and actually raise total tax receipts.[12] Finally, a tax treaty, regardless of its specific terms, can promote investment by reducing uncertainty about the overseas tax environment. Since a treaty can signal the willingness of governments to work out differences, this may boost investor confidence which leads to increased willingness to export capital.[13]

These treaty provisions, however, can cut both ways and may instead reduce the incentive to engage in FDI. If firms maximize after-tax profits, some of them may choose to operate in multiple countries in an effort to reduce their taxes. For example, it is well known that MNEs can minimize their tax burden through transfer pricing. By manipulating the price of goods traded between their various subsidiaries, firms can shift profits to low tax locations. As put forth by Casson (1979), this provides an incentive to invest in those low tax locations in order to shield profits from taxes. A treaty which reduces the ability to transfer price also reduces the incentive to invest for tax minimization reasons. Additionally, because treaties streamline the tax environment and encourage the exchange of tax information, a treaty may make it easier for governments to reduce other types of tax evasion, such as misrepresentation. Therefore, to the extent that some firms engage in FDI simply to minimize taxes, tax treaties may reduce FDI activity, not increase it. Thus, the effect of tax treaties on FDI activity is an open question, and we next turn to the data to estimate these effects for U.S. FDI activity.

3.   Empirical Methodology and Data

3.1.   *Empirical Methodology*

To estimate the effect of U.S. bilateral tax treaties, one requires an empirical model of MNE/FDI activity that reasonably captures the effect of other factors. While variations of a gravity model have perhaps been the most popular empirical framework for examining FDI activity across countries, they lack connection to a recognized formal theory of MNE activity. In contrast, Carr, Markusen and Maskus (CMM, 2001) and Markusen and Maskus (2001, 2002) establish and test an empirical specification of FDI activity that is arguably more grounded in the formal theories of MNE activity. The CMM specification is based on the Markusen's (2002) knowledge-capital MNE model which allows for both the horizontal and vertical motivations for FDI. This specification for the FDI from country i to country j is:

$$FDI_{ij} = f(SUMGDP_{ij}, GDPDIFSQ_{ij}, SKDIFF_{ij}, SKDGDPD_{ij},$$
$$T\_COST_i, T\_COST_j, F\_COST_j, HTSKD_{ij}, DIST_{ij}) \qquad (1)$$

The first two terms control for partner countries sizes where SUMGDP is defined as the sum of the two countries' real GDPs and GDPDIFSQ is defined as the squared difference between the two countries' real GDP. Since horizontal MNEs are most common between large countries of similar size, there is an expected positive correlation between SUMGDP and FDI activity and an expected negative correlation between GDPDIFSQ and FDI activity. The intuition is that with some positive level of trade frictions, larger and more similar sized markets better support the higher fixed costs associated with setting up production across countries (versus exporting) and lead to greater MNE activity.

The next two terms capture relative factor endowment effects. SKDIFF is the skill difference between the home and host country and is intended to capture relative factor abundance motivations for FDI. According to the CMM interpretation of the knowledge capital model, this variable should have a positive coefficient. Greater skill differences should proxy for greater wage differences which encourages vertical FDI by firms looking to outsource some of their activities to low-wage areas. As demonstrated by Blonigen, Davies and Head (2003) (BDH), however, interpretation of estimated coefficients on difference variables is sensitive to whether the variable takes negative or positive values. With bilateral U.S. FDI data, observations of inbound FDI find that the skill difference between the home and host country (here, the U.S.) almost always has a negative value. In these cases, the coefficient on SKDIFF will tell us the effect on FDI as skill differences *decline*; i.e., as SKDIFF becomes less negative. Therefore, we allow the SKDIFF variable to have different coefficients depending on whether our SKDIFF variable is positive or negative in value. To more easily interpret coefficient estimates, we multiply our SKDIFF term by -1 for the U.S. inbound sample where SKDIFF is always negative. Thus, for both the inbound and outbound sample, a positive coefficient on SKDIFF will indicate that MNE activity increases as skill differences increase as hypothesized by CMM. However, BDH estimated a statistically significant negative coefficient on SKDIFF in their analysis, suggesting that skill similarities increase FDI. One explanation for this puzzling result is that skill similarities are proxying for other

(omitted) factors that increase FDI between developed countries beyond those captured by the GDP variables. Given that we use similar data to BDH, our empirical (not theoretical) expectation is a negative coefficient on the SKDIFF variable. The fourth term, SKDGDPD, interacts the skill difference with the difference in GDPs, which CMM hypothesizes will have a negative coefficient.

Finally, the last five terms capture trade and investment frictions. T_COST is the trade cost of either the home or host country. Lower trade costs in the host country should lower FDI activity because it makes exporting more attractive relative to FDI. Lower trade costs in the home country should increase FDI, since it makes it easier to ship goods back to the home country from foreign affiliates. F_COST is the cost of investing in the host. As this rises, FDI should fall. HTSKD interacts host trade costs with the squared skill difference. Since host trade costs should matter less when FDI is vertical (i.e. skill differences are large), this carries an expected negative coefficient. Finally DIST is the distance between countries. Since higher distances make both trade and control of overseas investment more difficult, the net effect is ambiguous. Table A1 in the data appendix provides descriptive statistics of our variables for various specifications we use in our analysis.

As discussed earlier, tax treaties may alter investment incentives in a variety of ways. Unfortunately, most of these are difficult to quantify, and it is unclear how important the few quantifiable aspects are for the overall tax treaty effect. Thus, we begin with inclusion of a simple binary variable in the CMM empirical framework that takes the value of "1" when an effective U.S. treaty is in place for a given partner country and year to examine bilateral tax treaty effects. From this initial examination, however, we employ a variety of alternative specifications of this bilateral tax treaty variable that explores the hetereogeneity of tax treaty effects across countries and over time.

One additional concern is endogeneity of our dependent variable and the tax treaty variable. The U.S. may be negotiating treaties with only the countries for which there are large and/or increasing amounts of FDI activity. Such an endogeneity problem would bias our coefficient on tax treaties upwards and possibly cause a spurious positive correlation. In our sample, there are two types of US treaty partners: those with which the U.S. has had a bilateral tax treaty before our sample begins in 1980, which we call old treaty partners, and those with which treaties are negotiated during our sample spanning the 1980s and 1990s, which we call the new treaty partners. Endogeneity problems are likely largest with the old treaty partners, which are primarily composed of Canada, Japan, Australia, New Zealand and the western European countries. However, since these treaties were in place well before our sample begins, we are already skeptical about our ability to separately identify tax treaty effects on FDI activity from other uncontrolled factors. Thus, our focus will be on the effects for new treaty countries whose treaties with the U.S. came into place after the beginning of our sample, and for these partners there is little evidence of an endogeneity problem, as we next show.

Table 2 lists the new treaty countries for which we have data, where they rank in terms of total FDI activity with the U.S. relative to other countries at the time of the treaty, and how many countries that were ranked ahead of the country did not yet have a bilateral tax treaty with the U.S.[14] Table 2 clearly shows that the U.S. was *not* pursuing a strategy of

Table 2.   Relative rank of new U.S. bilateral tax treaty countries in U.S. outward FDI position in the year the treaty is effective.

Year	Country	Rank of country in terms of U.S. outward FDI stock	Number of countries ahead in rankings with no treaty
1981	Egypt	30th	14
	Jamaica	44th	22
1982	Philippines	29th	15
1985	Cyprus	87th	51
1986	China	58th	30
1990	India	49th	23
	Indonesia	22nd	8
	Spain	15th	4
	Tunisia	76th	42
1993	Mexico	11th	2
1994	Israel	40th	16
1995	Portugal	43rd	18
1997	Thailand	32nd	11
	Turkey	56th	20
1999	Venezuela	27th	8

Notes. This table excludes those countries (both with and without treaties) for which FDI data were not available. Data may not be available for a number of reasons, but is often because data were publicly suppressed due to such small volume that it would reveal proprietary information on individual investing firms. The table also excludes new treaty activity with former Soviet bloc countries, since there is virtually no inbound or outbound U.S. FDI data for these countries before the treaty comes into place. Country rankings are relative to all countries listed by the U.S. Bureau of Economic Activity table of outbound FDI activity for the particular year.
Source: U.S. Bureau of Economic Activity and Worldwide Tax Treaties Database at Tax.com.

signing bilateral tax treaties with countries that had the largest FDI activity with the U.S. during the 1980–1999 sample period. To the extent that one may believe endogeneity exists to bias our coefficient toward showing a positive relationship between new tax treaties and FDI, the insignificant effects we report below are even more surprising.[15]

On a final note, we run empirical specifications on separate samples of U.S. outbound and U.S. inbound FDI activity. This allows us to examine whether bilateral tax treaties asymmetrically impact outbound FDI of U.S. firms versus FDI into the U.S. by foreign firms. Splitting into inbound and outbound samples also gets around the BDH critique of CMM.

3.2.   *Data*

Empirical FDI studies have always been hampered by data difficulties. Missing or incomplete data for one or more variables often leads researchers to examine cross-sectional data,

with little or no time series dimension.[16] Data issues are also why researchers generally focus on U.S. data, as it is more complete and detailed than virtually any other countries' FDI statistics. Our approach is to collect data on inbound and outbound U.S. FDI activity for as many years as possible across as many countries as available. As a result, we have an unbalanced panel of 88 countries spanning up to 20 years from 1980 through 1999. As is typical in virtually all FDI studies, the data are for activity in non-financial sectors only.

Previous studies have typically measured FDI activity through affiliate sales or FDI stock in the host country. We use FDI stock as our dependent variable, as this provides a greater number of observations than affiliate sales data. We note, however, that our estimates are qualitatively identical when using affiliate sales as the dependent variable.[17]Data for FDI activity variables were collected from official BEA data available at the BEA's Internet site and converted into millions of real 1996 U.S. dollars using the U.S. chain-type price index for gross domestic investment as reported in the *Economic Report of the President*. A data appendix provides more details on our dependent variable, as well as the other variables we employ in our study.

With respect to our independent variables, data on real GDP come from the Penn-World Tables and are scaled in trillions of real U.S. dollars. Data on skill differences come from Barro and Lee International Data on Educational Attainment which reports the average years of schooling for those aged 25 or more. These data are measured every five years, so we used linear interpolation for in-between years. This measure differs from that of CMM who use occupation information as a proxy for skill. Our measure yields similar predictions regarding skill differences and FDI, but covers a longer time period and more countries.

Distance data come from the Bali Online Corporation (1999) and are measured in miles between capital cities. Following previous studies, trade cost measures come from the Penn-World Tables and are defined as 100 minus the ratio of the sum of imports and exports to GDP. In an ideal setting, we would decompose trade costs into institutional, political, and geographic components. However, such variables are difficult to come by and would limit our sample by excluding many of our developing countries. In any case, since an increase in any of these barriers would reduce the share of a country's economic activity devoted to international trade, our chosen measure acts as a reasonable proxy for general trade barriers and allows us to focus on our variable of interest. To construct our measure of investment barriers, we use the composite score compiled by Business Environment Risk Intelligence, S.A. (BERI). This composite includes measures of political risk, financial risk, and other economic indicators and ranges between zero and 100, with higher numbers meaning more openness. To construct a cost measure, we use 100 minus the BERI's composite score. The BERI measure allows us to consider more countries over a longer time period than CMM. There is a strong relation between the two, with a correlation of 0.81 for the observations in the CMM database. Finally, information on U.S. bilateral tax treaties was collected from the Worldwide Tax Treaties database at Tax.com (2002). As discussed above, there are three dates that we could use for each treaty: the date signed, the date in force, and the date effective. The date in force is no earlier that the date signed, however the effective date can pre-date the signing of the treaty or even follow the enforcement date. While we report the results using only the effective date, we get similar results regardless of which measure we use. These additional results are available upon request.

4. Results

4.1. Initial Results

We begin our exploration of the effect of bilateral tax treaties by using initial specifications that follow the CMM specification adding only treaty variables. Following BDH, we examine inbound and outbound samples separately and note that when we use U.S. inbound FDI data, subscript (i) refers to the foreign parent country and subscript (j) refers to the United States (the host country), while when we use U.S. outbound FDI data, subscript (i) refers to the United States (the home country) and subscript (j) refers to the foreign host country. As discussed above, we use FDI stock data for our dependent variable. After discussing these initial CMM specification issues in this subsection, statistical problems with our estimates will lead us to explore alternative specifications in following subsections.

Table 3 presents estimates for determinants of both inbound and outbound U.S. FDI stock using the CMM specification with various tax treaty variables. We begin with a CMM specification that includes only a simple binary variable for whether the observation involves an effective treaty country or not, with column (1) displaying these results for inbound FDI stock and column (3) displaying these results for outbound FDI stock. For both the inbound and outbound samples, the data appear to fit the overall model reasonably well with R^2s around 0.35 and an F-test statistic that rejects the null of zero slopes at the one percent significance level. The primary variables of the CMM model (SUMGDP, GDPDIFSQ, SKDIFF) are generally statistically significant and have estimated signs that are consistent with a world that is dominated by horizontal MNEs. In particular, the negative coefficient on the SKDIFF variable that suggests that FDI activity is greater for country pairs with lower skill differences, not high skill differences as predicted by a vertical model of MNE activity. The trade cost terms and interaction terms (SKDGDPD and HTSKD) are generally of incorrect sign, while the FDI cost variable and distance both display statistically significant negative impacts on FDI activity.

Our variable of interest, the indicator variable for the presence of an effective tax treaty between the U.S. and a given country in a given year, is estimated with a negative coefficient for both the inbound and outbound samples, with the inbound estimate statistically significant at the 1% significance level. In columns (2) and (4) we relax the assumption that bilateral treaty effects are similar across old treaty countries and new treaty countries. The estimates find that old treaties are significantly and positively related to both inbound and outbound FDI, whereas new treaties are significantly and negatively related to FDI in both directions.[18] Such results suggest that, at least for recent treaties, the role of treaties in enforcing anti-tax avoidance measures may well outweigh any FDI-promotion effects.

The magnitudes of the treaty variable coefficients in Table 3 are quite large. For example, a new treaty is estimated to lower inbound FDI stock by $9.9 billion annually even though the average inbound stock for these countries is just $329 million. Likewise, outbound FDI stock is estimated to be $7.8 billion lower as compared to an average of $3.05 billion of outbound FDI stock in new treaty partner countries. The estimated new treaty effects from these specifications are therefore implausibly large and hence we next explore alternative specifications to obtain estimates with more reasonable magnitudes.

Table 3. Estimates of treaty effects on U.S. inbound and outbound FDI stock: Initial results.

	Expected sign	Inbound FDI stock		Outbound FDI stock	
		Pooled	Pooled	Pooled	Pooled
TREATY$_{ij}$	?	−2174**		−1220*	
		(−2.51)		(−1.68)	
OLD TREATY$_{ij}$	?		7172.4***		6518***
			(4.96)		(4.87)
NEW TREATY$_{ij}$	?		−9879.0***		−7798***
			(−8.79)		(−6.30)
CMM controls					
SUMGDP$_{ij}$	+	7.4***	6.6***	−0.04	0.5
		(4.74)	(4.48)	(0.02)	(0.30)
GDPDIFSQ$_{ij}$	−	−0.001***	−0.001***	−8.9e−4***	−9.0e−4***
		(−5.90)	(−5.47)	(−4.15)	(−4.26)
SKDIFF$_{ij}$	−	−12774***	−6873***	−2231*	−1023
		(−8.43)	(−4.52)	(−1.67)	(−0.75)
SKDGDPD$_{ij}$	−	0.6***	0.3*	0.02	−0.1
		(4.14)	(1.82)	(0.12)	(−0.26)
F_COST$_j$	−	−610.6***	−500.1**	−326.7***	−65.4
		(−2.82)	(−2.40)	(−4.90)	(−0.91)
T_COST$_i$	−	−2115**	−2359.7***	−72.2***	−65.3***
		(−2.44)	(−2.84)	(−3.11)	(−3.03)
HTSKD$_{ij}$	−	7.8***	4.5***	1.3***	0.4
		(9.41)	(5.52)	(2.90)	(0.80)
T_COST$_j$	+	−51.0***	−45.3***	−3534***	−3694***
		(−6.18)	(−5.86)	(−3.20)	(−3.41)
DIST$_{ij}$	?	−1.4***	−1.1***	−2.4***	−2.1***
		(−10.51)	(−9.23)	(−7.66)	(−7.59)
Constant		230840***	232328***	372367***	362877***
		(2.65)	(2.79)	(3.40)	(3.43)
Observations		1470	1470	871	871
R^2		0.35	0.41	0.30	0.34

Notes. Robust *t*-statistics are in parentheses, with ***,**, and * denoting statistical significance (two-tailed test) at the 1, 5, and 10 percent levels, respectively.

4.2. *Alternative Specifications*

One concern with our estimates in Table 3 is that they are driven by unobserved characteristics not controlled for by the CMM independent variables. In fact, the residuals from these specifications without treaty variables included show that rich partner countries have

large positive residuals, while nonrich partner countries have large negative residuals.[19] Since old treaty partners are all rich countries and most new treaty partners are not, this systematic bias is likely captured by our treaty variables, leading to spurious inferences. A first step to mitigate this problem is the inclusion of country fixed effects. Unfortunately, this means we can no longer estimate old treaty partner effects, since these treaties became effective before our sample begins. Thus, the old treaty effects are subsumed into the fixed effects coefficients and cannot be separately identified.

Columns (1) and (5) of Table 4 present estimates that include country-specific fixed effects to filter out time-invariant country-specific heterogeneity. As one might guess, these fixed effects are quite important and their inclusion leads to R^2s around 0.80 with F-tests strongly supporting their inclusion. Because these fixed effects are so important for fitting the data, we necessarily focus on estimating treaty effects for only the new treaty partners in the rest of the paper, and concede that data constraints do not allow us to credibly estimate treaty effects for old treaty partners.[20]

After controlling for country-specific fixed effects, we still find that the coefficients on the new treaty dummy variable in columns (1) and (5) of Table 4 are negative and statistically significant for both U.S. inbound and outbound FDI stock. Though the magnitude of the new treaty coefficient falls somewhat, these numbers remain implausibly large. While the inclusion of fixed effects means residuals for any group of countries (such as rich ones) are zero on average, differing trends between groups may still remain. Specifically, over the time dimension of our sample, the rich countries' average residuals become increasingly positive, while the poor countries' average residuals grow increasingly negative. For both, each additional year in time changes the magnitude of the residual approximately $400 million in absolute value. Thus, the new treaty variable which tends to kick in later in the sample potentially reflects this negative trend in the residual of poor countries. We tried a number of alternative specifications to address these concerns, including year dummies, trend terms, separate rich country interactions with all our independent variables, and first differencing of the data with inclusion of country-partner-specific fixed effects.[21] Nevertheless, these approaches continued to yield implausible coefficient estimates for the new treaty variable.

A different approach is to log the data. The linear model proposed by CMM is not a structural equation derived from theory, so there is nothing inherently inconsistent with specifying a log-linear model. This alternative is particularly useful because the data are highly skewed. For example, the standard deviation of inbound FDI stock in our sample is almost four times larger than the mean. In particular, rich country inbound FDI is almost 20 times larger than inbound FDI for other countries. Logging the data reduces this skewness considerably. Furthermore, doing so leads to new treaty estimates that are plausible in size, free of many of the above-noted statistical problems, and quite robust. One issue when logging the data is dealing with negative values of the dependent variable and trade cost measures for some observations. We truncate these observations to 0.1 before taking logs although we get qualitatively similar results if we simply drop these observations. Additionally, note that once logged, the interaction terms in the CMM model, SKDGDPD and HTSKD, become collinear with the main control variables and must therefore be dropped.[22] Despite these changes, logging the data improves the fit of our regressions.

Table 4.   Estimates of treaty effects on U.S. inbound and outbound FDI stock: Alternative specifications.

	Inbound FDI stock				Outbound FDI stock			
	Levels, fixed effects	Logs, fixed effects	Logs, fixed effects	Logs, fixed effects	Levels, fixed effects	Logs, fixed effects	Logs, fixed effects	Logs, fixed effects
NEW TREATY$_{ij}$	−8570***	0.17	0.39	0.30	−5206***	0.06	0.02	0.07
	(−6.22)	(0.63)	(1.44)	(1.12)	(−4.68)	(0.53)	(0.13)	(0.67)
CMM Controls								
SUMGDP$_{ij}$	10.1***	8.5***	8.0***	87.7***	3.5***	6.7***	7.3***	8.5
	(4.06)	(6.02)	(4.83)	(3.79)	(2.06)	(3.15)	(2.88)	(0.63)
GDPDIFSQ$_{ij}$	−8.3e-4***	−2.7***	−3.5***	15.4***	−3.8e-4***	−3.2***	−3.4***	−3.4
	(−4.31)	(−4.13)	(−4.80)	(2.75)	(−3.07)	(−3.63)	(−3.40)	(−1.33)
SKDIFF$_{ij}$	−3991	−1.3***	−4.9***	−4.3***	7192***	0.2*	0.8	−1.1***
	(−1.31)	(−5.94)	(−4.64)	(−4.39)	(5.47)	(1.86)	(1.16)	(−4.36)
SKDGDPD$_{ij}$	−0.1				−0.6***			
	(−0.69)				(−4.92)			
F_COST$_j$	−344.7*	−0.3	−0.1	63.5***	177.3**	−0.6*	−1.1	−1.6***
	(−1.84)	(−0.50)	(−0.07)	(3.44)	(2.56)	(−1.68)	(−1.45)	(3.98)
T_COST$_i$	−1951***	4.8*	4.8	−0.16***	−100.1***	−0.2***	−0.3***	−0.1
	(−4.15)	(1.73)	(1.27)	(−2.74)	(−4.67)	(−3.64)	(−3.46)	(−0.03)
HTSKD$_{ij}$	9.9***				2.0***			
	(4.62)				(4.79)			
T_COST$_j$	44.5***	−0.1***	−0.1**	−2.3	−2357***	−6.8***	−5.6	−0.03
	(3.87)	(−2.92)	(−2.47)	(−0.54)	(−4.14)	(−2.92)	(−1.34)	(−1.05)
FX$_{ij}$				0.05				0.01
				(0.93)				(0.74)
TAX$_j$								−0.14**
								(−2.21)
Rich-country interactions	No	No	Yes	Yes	No	No	Yes	Yes
Constant	133051***	−41.9**	−23.1	−1296.2***	167174***	37.7**	30.7*	−4.4
	(2.69)	(−2.14)	(−1.12)	(−3.47)	(2.89)	(2.22)	(1.68)	(−0.03)
Observations	1470	1470	1470	1423	871	871	871	753
R-squared	0.81	0.91	0.91	.91	0.80	0.83	0.83	0.94

Notes. Robust *t*-statistics are in parentheses, with ***,**, and * denoting statistical significance (two-tailed test) at the 1, 5, and 10 percent levels, respectively.

Columns (2) and (6) of Table 4 provide estimates when we log our data but otherwise use the same fixed-effect specification used in columns (1) and (5) with the above-noted exceptions. The fit of the log specification for both the inbound and outbound samples is high with R^2s of 0.91 and 0.83, respectively. The control regressors are generally of the expected sign and are statistically significant. However, there is substantial change in the treaty coefficient. In both the inbound and outbound samples the treaty coefficient is now slightly positive although we cannot reject the null hypothesis that the effect is zero. Examining residuals from this regression, we still find that there is a statistically significant increasing trend in the residuals for the rich countries and a significant decreasing trend for non-rich countries. However, the magnitude of these trends is quite small. Nevertheless, in columns (3) and (7) of Table 4, we include rich-country interactions with our regressor matrix for the logs specification, which eliminates any rich or non-rich trends in our residuals. While the rich-country interactions are jointly significant at the 1% significance level, suggesting that their inclusion is important, the coefficient estimates of our original regressors remain qualitatively similar. We no longer estimate a significantly negative effect of new treaties, but continue to find that there is no significant positive effect.

We make one final modification to achieve our preferred specification. The CMM framework stems from a long-run general equilibrium model. In our panel data, however, FDI patterns may be substantially influenced by short-run factors. In columns (4) and (8) of Table 4 we include the log of the exchange rate (FX_{ij}) and year dummies in our specifications. We also include the log of a corporate tax rate variable (TAX_j, which is described in the data appendix) in the outbound regressions. This reflects the varying tax rates faced by U.S. firms in host countries.[23] While the tax rate variable is negative and significant in the outbound regression, as expected, the inclusion of these short-run factors does little to the new treaty coefficient.

While our modifications to the CMM framework eliminates systematic trends in our residuals for rich and poor countries, a Shapiro and Wilk (1965) test for normality of the residuals continues to reject the null hypothesis of normality. Likewise, a misspecification test by Ramsey (1969) examines whether higher orders of the fitted dependent variable have additional explanatory power in the regression equation. We find that this is indeed true in all of our specifications, which rejects the null hypothesis of no omitted variables. In response, we have tried numerous alternative specifications, including first differences in logs and inclusion of squared terms of all the variables. None of these specifications allows us to pass the Shapiro and Wilk or Ramsey tests. However, none of these alternative specifications finds significant effects of new treaties either. This clearly illustrates the need for continued efforts to develop better, econometrically robust empirical models of FDI.

Another issue we address is the binary nature of our treaty variable. As mentioned above, we choose this representation of bilateral tax treaties because such treaties often stipulate a number of changes between countries that are difficult to quantify. However, the most quantifiable difference in various treaties is the stipulated maximum withholding tax rates governing taxation of repatriated foreign income. As an alternative we tried specifications in which new treaty effects were measured as the withholding (pre- and post-treaty) rates for (1) dividend income for the parent firm, (2) dividend income for non-parent entities,

(3) interest income, and (4) royalty income. To be clear, these variables were an interaction between whether the country partner was a new treaty country during our sample and the withholding rates. For all other countries in our sample, withholding rates would likely not change and their effects would be subsumed into the country fixed effects. A negative coefficient is expected as it would indicate that FDI activity goes up when withholding rates come down from a treaty. Our estimates for both inbound and outbound FDI found no significant pattern in these withholding treaty variables, which is consistent with our results when using a binary treaty variable. These additional results are available on request.

4.3. *Individual New Partner Country Effects*

All of our regressions to this point have assumed identical treaty effects across new treaty partner countries which may mask important heterogeneity across these new partner countries. Table 5 explores this by estimating individual treaty-year dummies for each partner country using our logs specification with rich-country interactions from Table 4. Columns (1) and (3) of Table 5 provide our estimated effects by country for inbound and outbound FDI stock, respectively, after translating our coefficient estimates into millions of real U.S. dollars.[24] To gauge the relative magnitude of the estimated treaty effects, columns (2) and

Table 5. Individual treaty effects on FDI stock based on coefficient estimates from fixed-effects logs specification with rich-country interactions.

New treaty country	Inbound FDI stock		Outbound FDI stock	
	Estimated treaty effect	Stock in year of treaty	Estimated treaty effect	Stock in year of treaty
China (1986)	6.98253	11.6	1311.797	211.5
Egypt (1981)	—	2.5	−135.944	1376.2
India (1990)	−10.2265	31.7	−208.233***	392.8
Indonesia (1990)	584.5292***	26.4	−976.09*	3386.4
Israel (1994)	8173.057***	1986.5	−245.726	1499.2
Jamaica (1981)	−0.04847*	0.0	—	546.1
Mexico (1993)	1624.693***	1276.4	3324.761	15617.7
Philippines (1982)	−57.4505***	98.0	−502.543***	1586.1
Portugal (1995)	−21.1166**	23.0	−256.944	1411.0
Spain (1990)	624.9369***	836.3	1829.36**	8308.3
Thailand (1997)	178.7193	224.2	−1529.14	4335.0
Turkey (1997)	839.8532***	59.0	−67.9615	1033.7
Venezuela (1999)	—	−62.7	1123.919	7422.2

Notes. All numbers are in millions of real U.S. dollars. ***,**, and * denoting statistical significance (two-tailed test) at the 1, 5, and 10 percent levels, respectively.

(4) provide the amount of FDI stock in the first effective year of the treaty in millions of real U.S. dollars.

For inbound FDI, about two-thirds of the treaty effects have a positive coefficient. Of these, five—Indonesia, Israel, Mexico, Spain, and Turkey— are significant. On the other hand, the estimated treaty effects for the Jamaica, the Philippines, and Portugal are negative and significant, whereas the remaining four estimated treaty effects are statistically insignificant. Thus, for inbound FDI, we find significantly positive effects of treaties less than half of the time. For outbound FDI, the individual country treaty effects are even more mixed. Here, eight of the thirteen treaty estimates are negative, with three of those statistically significant. Only one treaty effect (Spain) shows any significant positive effect. Thus, our individual treaty estimates continue to find no overwhelming evidence for treaties increasing FDI. An important qualification of our individual country effects is that broader regime changes for new treaty partners may coincide with the date of a newly effective treaty. For example, the bilateral tax treaty with Mexico roughly coincides with the NAFTA agreement. This is something that we cannot address with our estimates.

5.   Conclusion

This paper presents the first estimates of the effects of bilateral treaties governing the taxation of FDI on bilateral FDI activity. Though our sample covers the most comprehensive set of countries and years to date, the data constraints allow us to only credibly estimate the effects of U.S. bilateral tax treaties that were implemented in the 1980s and 1990s, which we call "new" treaties. Our initial estimates based on the an empirical model of MNE activity provided by Carr, Markusen, and Maskus (2001) suggest very large and statistically negative effects of new treaties on FDI, which would be consistent with the hypothesis that such treaties allow tax authorities to substantially reduce tax evasion practices which were a significant motivation for FDI. However, we find a number of undesirable statistical properties of these initial CMM estimates. We eliminate these statistical problems by modifying the CMM framework to include country-partner fixed effects and a log-linear specification of the variables. Our estimates from this specification provide robust estimates of new treaty effects that are reasonable in magnitude. In general, we find that the average new treaty effect is not statistically different from zero with a very small point estimate. This is true for both FDI into the U.S. as well as U.S. outbound FDI. We find substantial hetereogeneity in new treaty effects when we identify them on a country-by-country basis. Specifically, a number of treaties appear to have had significant positive effects on FDI activity (especially inbound FDI) while a handful of treaties are estimated to have had significant negative effects. It must be noted, however, that a number of the cases where we find significant positive effects are arguably situations where much larger regime changes coincided with the bilateral tax treaty. Thus, we find no systematic evidence that bilateral tax treaties affect FDI activity, despite statements by the OECD and other sources that such agreements are meant to increase efficiency of world capital flows. Instead, our results suggest either that the provisions of a treaty have no effect or that the positive and negative aspects of treaty formation largely cancel one another.

Data Appendix

BEA Data on FDI Activity

The FDI activity data reported by the BEA is subject to two types of censorship. First, if revealing the bilateral FDI activity would reveal the information of a single firm, only this information is reported due to confidentiality requirements. These observations were dropped. Second, if the FDI activity lies between −$500,000 and $500,000, only this information is given. These observations were set to zero. This censorship then left us with an unbalanced panel which covered different time periods for different activity measures. Outbound FDI stocks were available as far back as 1966 for some partner countries, while inbound FDI stock data were available back only to 1980. All measures are for non-financial institutions and can be found at http://www.bea.doc.gov/bea/dil.htm.

Tax Treaties

Information on tax data all came from the Worldwide Tax Treaties database at Tax.com (2002). This database reports the calendar year a treaty was signed, the calendar year it became effective, and the calendar year that the treaty entered into force.

Other Data

Our GDP (both total and per capita), trade openness, and exchange rate measures are those from version 6.1 of the Penn-World Tables, which are available online at http://pwt.econ.upenn.edu. For a detail discussion of these measures, see Summers and Heston (1991). Total GDP is measured in millions of real base 1996 dollars. Our education variable is the mean years of education for adults over age 25. This data comes from the Barro-Lee dataset which is available at http://www.worldbank.org/research/growth/ddbarle2.htm. Details on these data are given by Barro and Lee (1996). The exchange rates are measured as the home country currency price of one unit of the host country currency. Distance was measure as the distance between capital cities as reported by the Bali Online Corporation. This distance calculator can be found at http://www.indo.com. For our measure of investment costs, we use the composite score compiled by Business Environment Risk Intelligence, S.A. (BERI). This composite includes measures of political risk, financial risk, and other economic indicators and ranges between zero and 100, with higher numbers meaning more openness. To compare these estimates to previously used measures of investment barriers, we define Investment Barriers as 100 minus the BERI's composite score. Corporate tax rate data are average rates by country as reported in appendix Table 1a of Altshuler, Grubert and Newlon (1998) for year 1980–1992 and the IRS website for years after 1992: http://www.irs.gov/taxstats/article/0,,id=96282,00.html. The IRS website's data are only available for even-numbered years, so following their lead we take the average of the two adjoining even-numbered years to estimate these corporate tax rate data for odd-numbered years. For 1999, the value in 1998 was used, provided it was available. The rates were

calculated by dividing the income tax paid by the current earnings and profits before income taxes, using only firms with positive current earnings and profits, similar to Altshuler, Grubert, and Newlon (1998).

Descriptive Statistics of Data

Summary statistics for the data set are reported in Table A1.

Table A1. Descriptive statistics.

	Inbound			Outbound		
Variable	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.
		Levels specification				
$Stock_{ij}$	1470	5897.798	20662.22	871	11100.92	22311.85
$SUMGDP_{ij}$	1470	6958.45	1458.937	871	7146.119	1523.354
$GDPDIFSQ_{ij}$	1470	4.22e+07	1.76e+07	871	4.00e+07	1.70e+07
$SKDIFF_{ij}$	1470	5.829699	2.63687	871	4.973219	2.401347
$SKDGDPD_{ij}$	1470	36746.31	18079.52	871	30162.27	15258.98
F_COST_j	1470	29.20743	2.578377	871	47.16219	12.4759
T_COST_i	1470	80.7157	4.947221	871	42.38257	43.25036
$HTSKD_{ij}$	1470	3329.2	2526.155	871	1492.986	2208.56
T_COST_j	1470	34.94435	44.61486	871	80.6973	4.909313
$DIST_{ij}$	1470	5033.416	2419.184	871	5123.3	2256.359
$NEW\ TREATY_{ij}$	1470	.1659864	.372195	871	.2089552	.4067959
		Logs specification				
$Stock_{ij}$	1470	3.201628	4.436201	871	7.915625	2.111452
$SUMGDP_{ij}$	1470	8.826261	.206615	871	8.852257	.2094638
$GDPDIFSQ_{ij}$	1470	17.47075	.4276306	871	17.41155	.4407623
$SKDIFF_{ij}$	1470	1.616645	.6138984	871	1.458387	.5884139
F_COST_j	1470	3.370186	.0940595	871	3.811664	.3053918
T_COST_i	1470	4.389011	.0623969	871	3.292179	1.848956
T_COST_j	1470	2.900666	2.124915	871	4.388814	.0619008
$NEW\ TREATY_{ij}$	1470	.1659864	.372195	871	.2089552	.4067959

Notes

1. Hines and Willard (1992) empirically examined the number of treaties a country signs as well as the tax concessions dictated by a particular treaty. However, they do not include the amount of FDI activity as an explanatory variable nor do they consider the effect of taxation on FDI. A handful of recent studies have examined whether bilateral investment treaties (BITs) increase FDI activity. BITs more clearly address promotion and protection of FDI than bilateral tax treaties and do not address tax concerns. Empirical evidence by United Nations (1998) and Hallward-Dreimeier (2003) find only limited evidence for positive effects of such treaties.

2. Radaelli (1997) argues that, at least for the U.S., treaty formation is well-insulated from the interests of business groups.

3. For an explanation of transfer pricing, see Caves (1993). Graham and Krugman (1995) provide examples of firms recently prosecuted for transfer pricing by the U.S.

4. Since tax treaties are federal instruments, they supercede state or local laws. Generally, the only limitation U.S. tax treaties place on state taxation is that a foreign corporation must be treated the same as a firm incorporated in another state (White, 1991).

5. The original draft of the U.S./Cyprus treaty was signed in 1981 but did not pass the Senate. An amended version also failed in 1985. In 1987, a final version of the treaty was signed but was not ratified until 1988.

6. The treaty with South Africa was terminated as part of the Comprehensive Anti-Apartheid Act of 1987. It was reinstated in 1997. The U.S./U.S.S.R. treaty, which was officially terminated in 1992, has been extended to cover former Soviet block countries until individual treaties can be negotiated. The only terminated treaty which was not canceled by the U.S. was that with Honduras. This treaty eliminated withholding taxes on FDI by both countries. Due to the almost entirely one-way nature of FDI flows, Honduras felt that all gains from the treaty were accruing to the U.S. and terminated it in 1967 (Diamond and Diamond, 1998).

7. The three most common provisions are the comparable uncontrolled pricing, cost plus pricing, or resale pricing methods.

8. The U.S. has also entered into a small number of tax information exchange agreements with countries which are not tax treaty partners. Additionally, the U.S. ratified the multilateral OECD Convention on Mutual Assistance in Tax Matters which provides for information exchange (OECD, 1989). However, many OECD members have not ratified this agreement so its usefulness is limited.

9. Under a tax credit, the domestic tax bill is calculated by applying the standard domestic tax rate to the pre-foreign tax level of overseas profits. A credit against this amount is then applied up to the amount of the foreign taxes paid. If this credit exceeds the domestic tax liability, the firm is in an "excess credit" position and pays no additional taxes on these overseas profits. If the home tax bill is greater than the amount of the credit, the firm is in an "excess limit" position and pays the remaining amount to the home government.

10. For instance, Belgium applies a reduced domestic tax rate to the foreign-earned profits of its residents. Under the U.S./Belgium treaty, however, income earned by Belgian firms in the U.S. is exempt from Belgian taxation.

11. Note that a lower foreign tax rate does not guarantee a reduction in the firm's total tax bill. As noted by Altshuler and Newlon (1991), only firms in excess credit will benefit from this change. Thus, a decrease in the overseas tax rate may not improve capital flows. Also, as discussed by Ramaswami (1968) and Bond and Samuelson (1989), this effect may be to the detriment of the home country due to foreign factor market effects.

12. As derived in Bond and Samuelson (1989) among others, the tax revenue maximizing tax rate on inbound FDI is $1/(1 + \varepsilon)$, where ε is the elasticity of inbound FDI supply. If ε varies across countries, the revenue

maximizing tax rate will differ across countries. Since a treaty allows a country to lower its tax rate, this can actually raise tax revenues if the current tax rate is greater than the optimal tax on FDI from that particular country.

13. See Jones (1996) or Sasseville (1996) for additional discussion of the effects of treaties on tax certainty.

14. Table 2 also reports that for a number of countries, primarily former Soviet Bloc countries, data on FDI activity with the U.S. is unavailable before the treaty was signed, making it difficult to estimate the treaty effect just as in the case of old treaty partner effects. These treaty countries are the Czech Republic, Estonia, Kazakhstan, Latvia, Lithuania, Russia, and Slovakia. We designate these countries as new treaty partners in our sample, but note that they have no impact on our new treaty coefficient once we employ country fixed effects.

15. One referee of the paper made the point that Latin American countries have historically refused to sign tax treaties with the U.S. because they did not want to lose tax revenue on a significant stock of existing FDI. In fact, Chisik and Davies (2004) find that this concern exerts influence over both the existence of a treaty and the withholding taxes setout therein. In any case, this idea would also argue against any systematic trend for the U.S. to sign up large FDI partners to treaties, especially with developing economies for which such taxes are an important source of revenue.

16. Grubert and Mutti (1991) examine a cross-section of 33 countries for 1982, while Brainard (1997) samples U.S.-country-industry combinations across 63 tradable industries and 27 countries for 1989. Markusen and Maskus (2001, 2002) examine an unbalanced panel data set of 36 U.S.-country pairs from 1986–1994. In contrast, this study examines an unbalanced panel of up to 88 countries for up to 20 years.

17. Affiliate sales data are only available back to 1983 for outbound affiliate sales and 1984 for inbound affiliate sales. As one would guess, there are strong, statistically-significant correlations between affiliate sales and FDI stock. In our sample, the pairwise correlation is 0.92 for inbound FDI activity and 0.90 for outbound FDI. The results from the affiliate sales data are available on request.

18. An earlier draft of this paper (Blonigen and Davies, 2000) found no significant effect for the simple treaty dummy variable. In that version, we then moved towards a "treaty age" variable to measure vintage effects. There, we found that older treaties were significantly and positively correlated with FDI activity. Initially, we attributed this to significant lag effects from the treaties. Given the current results, however, the evidence suggests that this effect is driven by the particular experience of the old treaty countries. Unfortunately, given the limitations of the data set, we cannot say more on the effects of older treaties.

19. We define rich partner countries as the EU countries, as well as Austria, Australia, Canada, Finland, Hong Kong, Japan, New Zealand, Norway, Sweden, and Switzerland.

20. Credible estimation of the old treaty effects would first require consistent data on FDI activity back to at least World War II. In addition, since these old-treaty countries were clearly the U.S.'s main economic partners, endogeneity is likely a greater concern. Finally, one would have to control for the many economic factors, such as post-war reconstruction, that were occurring at the time.

21. Inclusion of country-partner-specific fixed effects with first-differenced data will control for country-partner-specific *trends* in FDI activity. These additional results are available on request.

22. The treaty variable is still in levels and takes either the value of zero or one.

23. A similar variable for inbound U.S. FDI would capture only changes over time in the U.S. corporate tax rate. This is already captured in the year dummies.

24. The coefficient on our new treaty variable indicates how much the log of FDI stock changes with a new treaty in place. How much this log change means for the underlying change in the level of FDI stock is dependent on the starting value one chooses. We calculate the marginal effect by starting with the log of FDI stock in the year before the treaty becomes effective.

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