U.S. MULTINATIONALS AND PREFERENTIAL MARKET ACCESS

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ABSTRACT. This paper examines the relationship between offshoring activity by U.S. multinational firms and the structure of U.S trade preferences. We combine firm level panel data on U.S. foreign affiliate activity from the U.S. Bureau of Economic Analysis (BEA) with detailed measures of U.S. trade preferences from the U.S. International Trade Commission (USITC) to create a three-way panel that spans 80 industries, 184 countries, and ten years (1997-2006). Consistent with existing theory, we find that offshoring and preferential market access are positively and consistently correlated, both in the pooled sample and within countries, industries, and years. Using both instrumental variables and simultaneous equations approaches to address the endogeneity of export-oriented foreign investment, we find that a 10% increase in U.S. foreign affiliate exports to the U.S. from a particular industry and country is associated with a 4 percentage point increase in the rate of preferential duty-free access. Restricting attention to the Generalized System of Preferences (GSP) among potentially eligible developing countries, we find that the influence of multinational affiliate sales on preferential market access more than triples.

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1. Overview

Recent theoretical work suggests that the pattern of international investment and multinational enterprise (MNE) activity may play an important role in shaping government preferences over trade policies: when a multinational firm owns export-oriented (i.e. 'offshoring') affiliates abroad, the MNE's 'home' country government has an incentive to improve market access for imports from its MNEs' foreign affiliates, for the simple reason that greater market access means higher rates of return to the government's MNE constituents. To the extent that governments tailor their commercial policies in response to the interests of constituent industries, differences in the pattern of firm operations across the globe may be reflected in trade policy.

These ideas are formalized in two theory papers, Blanchard (2007) and (2010), which explore the trade policy implications of international investment and cross-border ownership. Blanchard (2010) explores how international ownership – in any sector and by any country – may translate into an altered role for multilateral tariff negotiation under the auspices of the GATT and its successor institution, the World Trade Organization (GATT/WTO). Blanchard (2007) uses a more specialized model to demonstrate how a large country's overseas investments can influence its government's optimal tariff policy towards the FDI-host countries when international investment is endogenous and therefore dependent on trade policy. The paper extends the model to a multi-country setting to argue that the possibility of preferential tariff arrangements can prove an effective means for harnessing the trade liberalizing potential of foreign direct investment if (but only if) international investment is of the export-oriented, or offshoring, type.

Unfortunately, these clear empirical predictions are not easily taken to the data, which may explain why so little work has been done in this regard. Empirically testing the hypothesis that cross-border investment influences governments' most preferred trade policies proves problematic first and foremost because most advanced economies have set tariffs cooperatively since the inception of the General Agreement on Tariffs and Trade (GATT) in 1947. Since tariff concessions are negotiated multilaterally through coordinated rounds and are subject to the Most Favored Nation (MFN) non-discrimination clause, the econometrician is challenged to identify the influence of the pattern of foreign direct investment (FDI) apart from other, often unobservable yet likely predominant, multilateral pressures at the negotiating table. A government is unlikely to change its MFN tariff on a particular good (reducing the tariff for all of its MFN trading partners) if its underlying objective is to improve market access for its foreign affiliates in just a handful of countries. Similarly, many of the MFN tariff concessions negotiated within the GATT/WTO framework apply to broad classes of goods rather than narrowly defined HTS-8 categories, further diluting the potential for MNE offshoring activity in a particular industry and country to influence the MFN tariff.

Our empirical strategy sidesteps these difficulties by focusing on the potential influence of MNE activity not on MFN tariffs, but on the recent proliferation of various preferential trade agreements and the Generalized Systems of Preferences (GSP) by which industrial nations grant developing countries facilitated market access. Preferential treatment of trade flows is exempt from MFN (under Article XXIV and the Enabling Clause of the GATT, for FTAs and GSP respectively), and therefore may be considered a closer reflection of a government's unilateral trade preferences.[1]

A second potential complication for empirical testing lies in differentiating export-oriented (vertical) FDI apart from import-substituting (horizontal) FDI. While theory predicts that export-oriented FDI will exert downward pressure on tariffs in the investment-source country, the converse holds for market-seeking investment. To the extent that multinationals operate horizontal 'tariff jumping' operations abroad, those

[1]While in principle Article XXIV and the Enabling Clause limit the extent of unilateral discretion, the intended uniformity (across industries and/or countries) of MFN exemptions appears to be weak in practice. (See Blanchard and Hakobyan (2012) for a detailed accounting of the sources and extent of discretion under the GSP.) Indeed, were bilateral free trade deals truly non-discriminatory across industries – or were GSP preferences in practice determined only by country and industry (but without discretion at the level of the country-industry pair or year-to-year variation) – our results would not withstand country-, industry-, and year-fixed effects, as they do. See the discussion in Section 3.1 for more background on the institutional structure governing U.S. trade preferences, and Figure 1 to see the extent of variation in our data.

activities will have either a negligible or small *positive* effect on the investment-source country's optimal tariff.[2] Indeed, market-seeking FDI and trade protection are often positively correlated in practice, largely due to the reverse causality as firms circumvent protectionist barriers *abroad* by building tariff jumping factories in their target market.[3] Fortunately, the richness of the BEA data offers us an empirical solution. In our data, MNE sales are disaggregated by destination; we are thus able to distinguish export-oriented investment (measured as U.S. foreign subsidiaries' sales of goods back to the U.S.) apart from horizontal investment (subsidiaries' sales to the foreign local market).[4]

The last and biggest hurdle in identifying a potential effect of MNE activity on trade policy is the obvious potential endogeneity of export platform investment. A candidate FDI host country becomes a more attractive venue for offshoring operations if it has preferential market access to the anticipated market for its exports. Mexico's Maquiladora program, the well known North American Free Trade Agreement (NAFTA) predecessor, is an obvious example: duty-free access to the U.S. market was precisely the carrot offered to entice investors to set up export-oriented manufacturing bases south of the border. Indeed, as the theory laid out formally in Blanchard (2007) makes quite clear, export-oriented FDI will in general increase as tariffs to the export-destination market are lowered.

Our response is to use an instrumental variables (IV) approach to control for the endogeneity of export-oriented FDI. The theory makes it clear that horizontal investment should not itself influence or be influenced by U.S. tariffs. In practice, however, we expect that market-seeking foreign investment is likely to be positively correlated with export-oriented investment, since both rely on a favorable climate for investment (comparative

[2] In a general equilibrium model, an increase in the investment source country's tariff will cause the world relative price of the foreign import good (and thus the return to horizontal FDI) to rise.

[3] Recall the prominent example of 'tariff jumping' Japanese car manufacturing plant construction in the U.S. in response to the voluntary export restraints in the 1980s, studied by Bhagwati, Brecher, Dinopoulos, and Srinivasan (1987) among others.

[4] The BEA data include a third category: MNE sales to the rest of the world (ROW). Although these sales are clearly also export platform, they would not benefit directly from improved access to the U.S. market; we use the information in these 'other' sales to the ROW in falsification exercises.

advantage via natural resources, policy, or otherwise). Thus, we use (horizontal) MNE sales to the local market as an instrument for (export-oriented) MNE goods sales to the U.S. Crucially, we construct our instrument using local sales by only those multinational affiliates that do not also sell goods back to the U.S., which addresses the potential concern that internal returns of scale at the affiliate level could challenge the exclusion restriction.[5]

For this paper, we assemble a three-way panel data set including 80 industries, 184 countries, and ten years (1997-2006) to answer the question of whether export-oriented operations by U.S. MNEs cause higher rates of trade preferences for imports originating from countries where U.S. firms have set up shop. Our findings are consistent with the presence of such a causal relationship: conservatively, a 10% increase in U.S. foreign affiliate exports to the U.S. is associated with an increase in the duty-free access rate of about 4 percentage points (corresponding to about 20% of the mean value of preferential market access), controlling for the endogeneity of MNE foreign affiliates' exports to the U.S. Among potentially GSP-eligible developing countries, the estimated effect for GSP preferences is more than three times higher: a 10% increase in affiliate goods exports to the U.S. is associated with a 14 percentage point increase in the rate of duty-free access under the Generalized System of Preferences (more than seventy percent of the mean level of GSP access among potentially eligible countries). In a simultaneous equations version of the model, we find evidence consistent with the theoretical prediction that causality does indeed run both ways: expanded market access seems to increases MNE sales to the U.S. The baseline estimates prove to be remarkably robust in a variety of different empirical specifications.

Our empirical results offer compelling evidence that offshoring MNE activity spurs preferential trade liberalization to the MNE's home country, which in turn may deepen economic integration between the investment host and investment source countries even further. To the extent that more generous preferential tariff treatment fosters additional

[5]Intuitively, we would worry that U.S. trade policy could influence an affiliate's local sales if the decision to enter (or expand in) the market depends on both the level of exports back to the U.S. and local sales, as could arise under increasing returns to scale.

export-oriented investment, the cycle of improved market access and increased FDI may continue. At the same time, however, it stands to fear that the same mechanism can lead to substantial trade and investment diversion; a particular concern is that just as some trading partners experience ever-greater economic integration through this investment-trade nexus, other countries may be left out entirely.

The remainder of the paper proceeds in the usual sequence. In the next section, we briefly relate this paper to earlier work. Section 3 then reviews the theoretical motivation that guides our empirical approach and discusses the political process through which U.S. trade policy solicits and responds to multinational firms' concerns. Section 4 outlines our empirical strategy; Section 5 describes the data; and Section 6 presents the results. We describe a series of extensions and robustness tests in Sections 7 and 8 before concluding in Section 9.

2. Related Literature

Our study complements and considerably extends the empirical literature on the determinants of preferential treatment and the influence of international investment. The literature on trading blocs is sufficiently broad that discussion is restricted here to the small subset of work most closely related to this paper.[6] Most relevant to this paper's objective are Magee (2003) and Baier and Bergstrand (2004), which test empirically the determinants of preferential trade agreements. Both papers find qualitatively similar results: trade agreements tend to form between countries that are similar in size, geographically proximate, and politically liberal. The close concordance of their results is particularly striking given that these papers were written concurrently but independently from each other (each uses a distinct empirical strategy and data set). Both Magee (2003) and Baier and Bergstrand (2004) also have in common that they view preferential treatment as a binary all-or-nothing decision between two countries and do not exploit, as we do, the considerable variation in preferential treatment at the industry level over time.

[6]See Bhagwati, Krishna, and Panagariya (1999) for a broader review.

Also related is DeVault (1996), which examines whether U.S. trade preferences during 1988-1994 for country-product pairs within the GSP framework are explained by domestic import-competing industry and exporting country characteristics. Along a similar vein, Lederman and Özden (2007) and Özden and Reinhardt (2005) recognize and explore the geo-political determinants of U.S. trade preferences in the process of examining the effect of trade preferences on beneficiary countries' export patterns. Kee, Olarreaga, and Silva (2007) investigate whether foreign lobbying increases preferential market access to the U.S. in a Protection for Sale framework where the government values both foreign lobby contributions and domestic welfare, but with differing weights. Finally, in an important recent contribution, Ludema, Mayda, and Mishra (2011) examine the extent to which U.S. tariff suspensions (relatively small but highly discretionary exemptions to MFN tariffs) respond to both monetary and non-monetary lobbying by U.S. firms. While each of these earlier studies highlights important determinants of preferential tariff setting, none consider the potential influence of international investment.

A second subset of related work consists of a handful of studies that examine the role of (exogenous) PTAs in determining investment flows. The seminal theoretical work by Motta and Norman (1996) identifies the potential for PTA member countries to attract both export-oriented and import-competing investment from investors within and outside a PTA, depending on the external barriers to the trading bloc. Balasubramanyam, Sapsford, and Griffiths (2002) offer a first test for the potential influence of regional integration on investment flows, treating preferential trade agreements ("regional integration areas" or "RIAs," in their lexicon) as exogenous. Using cross-sectional data for 1995 on the aggregate bilateral investment flows for 381 country pairs, they find that once standard gravity variables and country characteristics are included in the estimation, the presence of RIAs has no predictive power for FDI flows. To our knowledge, this is the only study apart from our own to evaluate empirically the relationship between investment flows and trade agreements, though it examines the reverse causality and does not address the question of endogeneity.

Finally, this project provides an important complement to a companion paper, Blanchard and Matschke (2006). There, we use United Nations (UN) data on aggregate bilateral investment positions to predict the future formation of free trade agreements, in the spirit of Magee (2003) and Baier and Bergstrand (2004). Although this earlier study offers a wide geographic scope, spanning 37 countries and several decades, it suffers from systematic data deficiencies borne of the large coverage area and multiple reporting agencies.[7] Moreover, since only 12 of the 178 reporting UN members have *ever* provided the UN with investment data disaggregated by industry, only aggregate measures of bilateral investment could be used in the analysis. A key finding from that project is the importance of using industry-level investment data to estimate the effects of vertical (export-oriented) investment apart from the influence of horizontal (market-seeking) investment, which is precisely one of the strengths of the current study.

3. Theoretical Motivation and Political Mechanisms

In this section, we review the theory that motivates and guides our empirical analysis and offer a short overview of how multinational firms can influence trade preferences in practice. Because the focus of this paper is empirical, we review the existing theory briefly and in broad terms; the interested reader is referred directly to the cited papers for additional detail.

In a flexible general equilibrium framework, Blanchard (2007) and (2010) demonstrate that export-oriented foreign investment reduces the optimal (bilateral) tariff imposed by the investment source country. The mechanism is intuitive. When a country is large, its import tariffs shift part of the tax burden onto foreign exporters, generating the well known 'terms of trade cost shifting externality.'[8] When part of the foreign export sector is owned by domestic constituents in the importing country (e.g. through

[7]The bilateral investment position data from each country are self-reported to the UN and exhibit substantial internal discrepancies due to different accounting methodologies across countries.

[8]See Bagwell and Staiger (1999) for the canonical application to GATT/WTO rules, and Johnson (1951-52) for the foundational exposition of the optimal tariff argument.

offshoring investments by multinational firms), the terms of trade externality is internalized, causing the optimal tariff to decline in the investment *source* country.[9] If trade protection is politically motivated and offshore investors (or multinational firms) actively lobby their local government, we would expect tariffs to fall even further. In a many country world, and in the presence of the GATT/WTO most favored nation clause, Blanchard (2007) shows that a large importing country has an incentive to use discretionary tariff preferences – exemptions from MFN – to the advantage of its overseas investors instead of MFN tariffs, particularly when the investment host countries are small.

Our first and most important empirical prediction thus follows directly from the theory: ceteris paribus, offshoring activity by firms from a given source country – here the United States – in a particular host country and industry will increase the incentives for U.S. policy makers to improve market access for imports from U.S.-affiliated producers overseas.[10] Because trade policy by law cannot discriminate at the firm level (with the notable exception of anti-dumping cases), we expect a greater rate of trade preferences to be afforded at the (HS-8) tariff line level for the country in question.

According to the theory, the degree to which trade preferences will respond to multinational firms' offshoring operations depends on four key factors: the sensitivity of MNE investors' profits to U.S. trade policy; the political influence of the multinational affiliates relative to potential import-competing domestic groups; the opportunity cost of lost tariff revenues; and the extent to which preferences offered to one country and industry dilute the multinational affiliate profits derived from imports from the rest of the world.[11] Translating to empirically testable predictions in the context of U.S. trade preferences and multinational affiliate activity, we draw the following predictions:

[9]While offshoring operations are only one form of export-oriented foreign investment, they are also the most easily measurable, which is why we use them as the focus of study in this paper.

[10]See Lemma 3.5 in Blanchard (2007) for formal treatment.

[11]The first three predictions can be found in both Blanchard (2007) or (2010), while the last derives from the multi-country framework in Blanchard (2007) for the case of a finite number of trading partners.

Empirical Prediction 1. *U.S. trade preferences for a given product j from country c will be:*

(*i*) *increasing in U.S. multinational affiliates' sales of product j from country c back to the U.S.;*

(*ii*) *decreasing in U.S. domestic sales of product j;*

(*iii*) *decreasing in total U.S. imports of product j from country c;*

(*iv*) *decreasing in total U.S. imports of product j from multinational affiliates located in the ROW.*

These predictions are largely self-evident, but a few points of explanation are worth noting. First, predictions (*i*) and (*iv*) use MNE affiliate sales of goods back to the U.S. as the relevant measure of the sensitivity of U.S. multinational investors' profits to U.S. trade policy; our choice of this measure rests on the assumption that an increase in preferential market access will translate to higher local per-unit revenues, and thus higher profits, earned by those multinational affiliates supplying the U.S.[12] In prediction (*ii*), we use the size of domestic U.S. sales as a proxy for potential protectionist pressure, with greater domestic sales associated with a more protectionist stance. In our empirical specifications, we also include a quadratic term for U.S. domestic sales, to capture potential concavity in the political response, loosely in the spirit of Bombardini and Trebbi (2011).[13] Prediction (*iii*) reflects the opportunity cost of tariff preferences, which is greater the larger the volume of imports that would no longer pay MFN duties. Prediction (*iv*) captures the possibility that enhanced market access for one country could, via trade diversion, cannibalize profits from other MNE affiliate suppliers to the U.S. located elsewhere in the world.

[12]By Hotelling's lemma, recall that the derivative of the profit function with respect to selling price p is supply ($\frac{\partial \pi(p,\vec{w})}{\partial p} = y(p,\vec{w})$). Thus, if foreign investors are the residual claimants of affiliate profits and view local wages as fixed, then our sales measure is *exactly* the derivative theory suggests; otherwise the relationship is positive, but not exact.

[13]The mechanism could be quite different, however, as their focus is the relationship between industry contributions (not the political outcome) and industry size.

In addition, we expect that both MFN tariffs and total U.S. industry imports could play an important role, but the sign of the predictions is ambiguous. A higher MFN tariff implies a greater opportunity cost of offering tariff preferences (which would reduce equilibrium preferences), but at the same time, greater MFN tariffs may induce more active MNE political lobbying for discretionary policies (which would work in the opposite direction).[14] The net influence of the level of total U.S. imports is similarly unclear: else equal, greater total U.S. imports may imply less domestic political pressure against preferences (countering domestic sales and thus increasing the equilibrium use of preferences). Conversely, the higher the level of total U.S. imports, the greater the potential loss in U.S. tariff revenue from the rest of the world as the result of trade diversion (lowering predicted equilibrium preferences).

Finally, the theory issues a clear warning about the endogeneity of export-oriented investment (or multinational affiliate operations), while also offering a potential identification strategy. First, it is immediately obvious that countries with preferential market access to the U.S. will be more attractive to potential MNE investors planning to sell goods back to the U.S., all else equal. Blanchard (2007) models this channel explicitly, demonstrating that trade liberalization and export-platform investment are complementary. The theory is equally clear, however, that market-seeking (horizontal) investment should have no such trade liberalizing effect; indeed, in a general equilibrium framework, market-seeking investment that competes for resources with the host-country's export sector could cause optimal tariffs in the investment source country to *rise*. To the extent that a many-sector economy more closely resembles a partial equilibrium framework, there would be no relationship between improved market access to the U.S. and horizontal investment. At the same time, however, we might expect vertical and horizontal investment to be positively correlated to the extent that both respond to the local investment climate. Absent better measures of the local investment conditions at

[14]Anecdotally, MNEs do seem more politically active in practice when there is more on the line. We offer some evidence below.

the host-country-product- year level, we argue that horizontal MNE activity – by *different* firms than those serving the U.S. market – thus may serve as a reasonable instrument for offshoring by U.S. MNEs. (We discuss potential caveats and solutions in the data section below.)

3.1. **Political Mechanisms.** We now offer a quick overview of the political process that determines U.S. trade preferences in an effort to convey three important points: first, it is reasonable to take MFN tariffs as exogenous in the context of our analysis; second, there is substantial discretion available to policy makers in the determination of trade preferences each year; third and most importantly, multinational firms are actively engaged in determining U.S. trade preferences.

By definition, MFN tariffs cannot discriminate on the basis of exporting country. Moreover, U.S. MFN tariff bindings were last set in 1994 under the Uruguay Round negotiations – three years before the start of our sample period in 1997. Applied tariffs have moved little since, and the variation that has occurred has been largely the result of extended phase-in schedules negotiated before the beginning of our sample period (and thus independent of conditions after 1997).

Our second point is simply that there is considerable scope for discretionary application of trade preferences under U.S. trade rules.[15] While the most favored nation clause of the GATT limits the extent to which countries (including the United States) can offer trading partners preferential market access, there are important exceptions. Free trade agreements, which are exempt from MFN under Article XXIV, offer preferential market access at the bilateral level. Within these free trade agreements, sharp carve-outs and differential phase-in periods at the product level introduce the potential for discretionary application across goods in practice. Likewise, the Generalized System of Preferences, which is designed to allow countries the ability to offer preferential market

[15]Article XXIV of the GATT defines the rules for PTAs; for the U.S. GSP, duty-free treatment is specified by Title V of the U.S. Trade Act of 1974 (19 U.S.C. 2461).

access to developing countries, introduces the potential for discretion at the country-product level.[16] Under the GSP, so-called 'competitive needs limitations' waivers and exemptions, differentially binding rules of origin, and product-level application of country penalties lead to substantial variation in applied preferences at the country-industry level and year to year. (For example, following a 1997 violation of intellectual property rights in pharmaceuticals, Argentina lost GSP access in a range of seemingly unrelated products ranging from anchovies to raw cane sugar.[17])

The bottom line is that the institutional structure governing U.S. trade policy leaves scope for discretionary application of trade preferences on a year-to-year basis, even within countries and industries. We document the extent to which we observe such variation empirically when we turn to the data in Section 5.

Our last point about the existing institutional structure is that we do in fact observe multinational firms publicly and actively engaged in the political process that determines the structure of U.S. trade preferences. Below, we offer a handful of examples publicly available from government documents via the U.S. Trade Representative (USTR). All of the information below was collected independently from public petitions to the USTR and bears no relationship to the BEA data.

In one example, during a 2005-2006 USTR review of country-level GSP eligibility, Dana Corporation, a large multinational vehicle parts manufacturer headquartered in Toledo, Ohio, requested extension of GSP preferences for its offshore operations in Argentina, Brazil, India, and Venezuela, "recognizing the immense investment Dana has already made in these countries..."[18] Dana wrote, "The elimination of GSP benefits...will result in significant harm to Dana's foreign investments and will also cause further economic harm to the U.S. auto parts industry, to Dana in particular – and to the auto

[16]See Blanchard and Hakobyan (2012) for a detailed analysis of the extent and sources of discretion exercised under the U.S. GSP.

[17]Ibid., footnote 2.

[18]2006 GSP Annual Review, written comment of Dana Corporation, September 5, 2006 (USTR docket FR-0052)(14).

industry as a whole."[19] In 2011, Dana derived 16% of its overall revenue from its South American operations,[20] from which it imported axles, drive shafts, brake parts, and other auto parts for use in multiple U.S. manufacturing facilities in Virginia, Kentucky, and Indiana.[21]

In another example from the 2005-2006 GSP Review process, Alcoa, a leading producer of aluminum headquartered in Pittsburgh, Pennsylvania, with operations in 44 countries, petitioned for continued GSP eligibility for Brazil, Russia, and Venezuela. In its written comments to the USTR, Alcoa argued that loss of GSP access would "cause significant disruption to [its] supply chain" and could cause "the imposition of over $3 million in additional costs."[22] It went on to add, "If Brazil, Russia, and Venezuela have their eligibility restricted in some way, we request that the following products not be removed," before listing the relevant HTS codes for its imports from those countries.[23]

There are many such examples, and they occur every year. Two examples from the recent 2011 Annual GSP Review include SC Johnson and Co. (headquartered in Racine, WI), which petitioned for continued GSP benefits on behalf of its Thai subsidiary (SC Johnson and Son, Ltd. (Thailand)) from which it imports pinch-seal reclosable bags (HTS 3923.21.0030); and MAT Holdings (Long Grove, IL), which petitioned on behalf of its production facilities in India, from which it imports brake pads and friction pads (HTS 8708.30.50). MAT Holdings spells out the mechanism plainly, writing in its petition that "entering the United States duty-free under GSP also contributes to the success of U.S. investments abroad..."[24]

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[19]Ibid., p. 13.

[20]Dana Holding Corporation Investor News Release, October 2, 2012 "Dana Highlights Full Driveline Capabilities at 2012 SAE Congress in Brazil, Oct. 2-4."

[21]Dana Corporation, 2006 Eligibility and CNL Waiver Review, September 5, 2006, p. 18.

[22]2006 GSP Annual Review, written comment of Alcoa, September 5, 2006 (USTR docket FR-0052)(2).

[23]Ibid.

[24]Dockets USTR 2011-0015-0125 and -0083 as well as USTR-2011-0015-0012, respectively.

With a careful understanding of both the theoretical motivation and the institutional mechanisms that drive trade preferences in practice, we now turn to the empirical analysis.

4. Empirical Strategy

As in both Baier and Bergstrand (2004) and Magee (2003), we adopt a standard qualitative choice approach for the empirical estimation. We define the latent variable θ^*_{cjt} to be the underlying U.S. preference for offering duty-free market access to imports of a given product j from country c in year t.[25] Under U.S. trade law, preferences must be binary – either eligible for duty-free access or subject to the MFN rate – so that in principle the observed tariff exemption θ_{cjt} is either 0 or 1.

Although we cannot observe θ^*_{cjt} directly, we can observe many of its determinants. We define the econometric model:

$$\theta^*_{cjt} = \tilde{\alpha}_0 + \tilde{\alpha}_1 FDI_{cjt} + \tilde{\beta} \cdot X_{cjt} + \tilde{\gamma}_c + \tilde{\gamma}_j + \tilde{\gamma}_t + \tilde{\epsilon}_{cjt}, \tag{4.1}$$

where FDI_{cjt} is a measure of offshoring activity by U.S. multinationals, X_{cjt} is a $k \times 1$ vector of other explanatory country-year, product-year, and country-product-year varying characteristics, $\tilde{\alpha}_0$ and $\tilde{\alpha}_1$ are scalar parameters, and $\tilde{\beta}$ is a $1 \times k$ vector of parameters. The parameters $\tilde{\gamma}_c$, $\tilde{\gamma}_j$, and $\tilde{\gamma}_t$ stand for country-, product-, and year-fixed effects, respectively. The remaining error term, $\tilde{\epsilon}_{cjt}$, represents unobserved heterogeneity in each country-product-year observation and is assumed to be independent of both X_{cjt} and FDI_{cjt}.[26] To the extent that the errors are correlated within countries or industries,

[25]Clearly, the latent variable need not be in the set {0,1}. It is quite reasonable to assume that θ^*_{cjt} lies inside the closed interval $[0, 1]$, but moreover, it might be negative (meaning that in principle, the U.S. would like to impose a tariff above MFN level) or above 1 (meaning that the U.S. would like to offer an import subsidy).

[26]One interpretation of the error term is that it captures unobserved noise in the legislative process by which preferential trade policies are determined. For instance, in the case of GSP preferences, the annual review by which policies are adjusted each year includes a series of petitions, public hearings, and a formal comment period, after which the executive branch (under the USTR's interagency Trade Practices Staff Committee) makes its decisions for the subsequent year. Other trade preference programs

country- and industry-fixed effects will mitigate (if not necessarily eliminate) the Moulton problem; to be conservative, we also cluster the standard errors by country in our benchmark specification. To test robustness to alternative assumptions over the error structure, we also report results under heteroskedasticity-robust (unclustered) standard errors, and with clustering at the country-industry pair and country-by-industry (two-way) level following the technique in Cameron, Gelbach, and Miller (2006).

Assuming that $\tilde{\epsilon}_{cjt}$ is normally distributed, the model in (4.1) should be estimated using Probit. The theory predicts that among otherwise identical country-product-year pairings, U.S. trading partners with more offshoring activity by U.S. multinationals should have an increased likelihood of receiving preferential market access in those sectors. Thus, the key theoretical prediction is that $\tilde{\alpha}_1 > 0$.

There are, however, several practical complications that call for modifications to our estimation strategy. First, several of our variables are not available at the product level j, but only at the industry level i, where each industry produces several products. In particular, while preferential market access is determined at the 8-digit HTS (tariff-line) level, our information on U.S. multinational affiliates is coded only to the more aggregated 4-digit NAICS level. For this reason, the dependent latent variable is recast as the average preference share for all imports within a given 4-digit NAICS category:

$$\theta^*_{cit} = \frac{\sum_{j \in i} M_{cjt} \theta^*_{cjt}}{\sum_{j \in i} M_{cjt}}, \tag{4.2}$$

which clearly may take intermediate values between zero and one. Moreover, because t in our data is a calendar year rather than just a point in time, even θ_{cjt} may take on intermediate values if the exemption status of a product switches within a calendar year.

For both of these reasons – aggregation and part-year program eligibility – the dependent variable in our data must be treated as continuous. Our econometric model thus becomes:

$$\theta_{cit} = \alpha_0 + \alpha_1 FDI_{cit} + \beta \cdot X_{cit} + \gamma_c + \gamma_i + \gamma_t + \epsilon_{cit}, \tag{4.3}$$

go through the U.S. House Ways and Means Committee or Congress as a whole, introducing further scope for unobserved political influence.

where i stands for a 4-digit NAICS industry and t stands for calendar year.

We include in the vector X_{cit} both the variables directly suggested by the theory, as well as a small set of standard political economy and gravity control variables. As discussed earlier, the key explanatory variables in X_{cit} include total U.S. domestic sales in industry i and year t and its square;[27] total U.S. imports from the world in industry i and year t; total industry i exports to the U.S. from country c in year t; U.S. MNE sales from the ROW (other than country c) to the U.S. industry i and year t; and the U.S. MFN ad-valorem tariff rate in industry i and year t. Additionally, we include a set of industry-year controls for U.S. domestic political pressure: U.S. domestic payroll, number of establishments, import penetration, number of employees, and the year-on-year log change in U.S. employment and import penetration. Finally, we add two gravity variables: GDP per capita and population, which vary at the country-year level.[28] (Our results are robust whether or not we include these additional political economy or gravity variables; we find them to be sensible additions as they are both intuitive and prove to be statistically significant.)

With a normally distributed error, the correct specification is a double-censored Tobit model. In practice, however, the three dimensions of fixed effects specified in (4.3) introduce a tradeoff between computational feasibility and adherence to this ex-ante preferred non-linear (Tobit) model specification. Thus, we pursue two different strategies for implementing our empirical test. In the first, we estimate a linear probability model in which we can remove industry- and country-fixed effects γ_i and γ_c by demeaning the data – but of course we must then ignore empirically the censoring process that generates the mass points for θ_{cit} at 0 and 1.[29] In a second version of the empirical

[27]Again, including the quadratic term to capture concavity.

[28]In a pooled sample that we use in a robustness check, we also include country-specific, time-invariant characteristics for distance from the U.S. and indicator variables for whether the country was a communist or terrorist state during our ten-year sample period, as well as time invariant industry-level dummies for agriculture and textile sectors.

[29]Year-fixed effects are still removed by dummy variables. Because our data set is not balanced, we take care to demean the year dummies. We also correct the standard errors to account for the

strategy, we adopt the non-linear double censored Tobit model with country-, industry-, and year-fixed effects included as dummy variables, but the need to achieve convergence limits both our choice of estimator and the set of control variables we can include.[30]

We address the potential simultaneity between offshoring activity and preferential access by instrumenting for our measure of multinational affiliates sales of goods back to the United States. Finding suitable excluded instruments that predict U.S. imports from offshore MNE affiliates, but are at the same time uncorrelated with the error term, is in general quite challenging. Fortunately, the BEA data separate MNE sales data by both type (goods vs. services) and destination (local, to the U.S., or to the rest of the world (ROW) ('other' in BEA parlance)).[31] Guided by theory, we argue that market-seeking (horizontal) MNE sales to the local market should be independent of U.S. trade preferences. At the same time, it seems likely – and we confirm in the data – that local and offshoring MNE sales are positively correlated, presumably because both capture the attractiveness of the local market for foreign investors.[32] Thus, we instrument for (export-oriented) sales of goods to the U.S. with (market-seeking) MNE affiliate sales (of goods and services) to the local market. We are careful to construct our instrument using *only those affiliates that do not also sell goods to the U.S.*, to address the potential concern that increasing returns to scale at the affiliate level could cause a firm's joint decision to enter and sell to the U.S. and local markets to depend directly

demeaning (to account for the difference in degrees of freedom); given the large number of observations, the standard error correction is negligible, however.

[30]To achieve convergence in the instrumented Tobit specification, we adopt the Newey (1987) efficient two-step estimation and reduce the set of controls to only those variables explicitly indicated by the theory.

[31]The BEA sales data are additionally categorized into sales to affiliated vs. unaffiliated buyers, a distinction we use in robustness tests reported later in the paper.

[32]One might reasonably raise the concern that MNE sales to the local market could be a substitute for MNE sales to the U.S., and thus be (negatively) correlated with the second-stage error term. The first-stage results suggest a strong positive correlation between sales to each destination, however, which argues against the substitutes story.

on U.S. preferences.[33] Based on the same rationale, we construct a second instrument, total (worldwide) MNE sales of *services*, by all MNE affiliates that do not also sell goods to the U.S.

Finally, we estimate all specifications of the model taking logs of the explanatory variables (including instruments) that take large integer values. We do this both to dampen the potential influence of outliers and for ease in interpretation across coefficient estimates.[34] In baseline specifications of the model, we lag the explanatory right-hand side variables by one year to account for inherent time lag in policy changes.[35] The simultaneous equations version of the model eliminates the lags when considering joint causality, and finds the estimated results virtually unchanged.

5. Data

Preferential Market Access. The dependent variable of interest is preferential market access by industry, country, and year to the U.S. market. There are two ways to construct the preferential access variable using slightly different data sources; we consider both definitions to evaluate the robustness of our findings, and thus to ensure that our results rest on meaningful economic factors rather than specific variable definitions.

Our first data source for preferential market access comes from the U.S. Trade Representative harmonized tariff schedules (HTS-US). Imposed tariff rates and the relevant indicators for preferential program eligibility are reported at the 8-digit HTS level by

[33]Each 4-digit NAICS industry category allows a host of firms selling a range of products within each industry. In general, the firms and products selling to the local market are distinct from those selling to the U.S. On average, only about a third of firms in our sample sell both locally and to the U.S.; accordingly, 62% of all local sales are made by affiliates that do not also sell goods to the U.S.

[34]To preserve zeroes, we add one to (all) variables' values before taking the natural log (thus we replace x with $ln(1+x)$); the usual caveats apply. See, e.g., Wooldridge (2009), page 192.

[35]Indeed, an eight-month lag is institutionalized in the GSP annual review process – decisions made in (typically) autumn of one year are not implemented until July 1st of the subsequent calendar year.

country, industry, and year.[36] Thus, one way to define the preferential treatment variable is so that $\theta_{cjt} = 1$ if the country-product pair is eligible for a special rate code in year t, and 0 otherwise.[37] When we aggregate to the 4-digit NAICS level (necessary to concord the preferential tariff data to the investment data), we construct both historic trade weighted[38] and straight (unweighted) averages across the relevant subcategories. We view the trade-weighted version as the more appropriate measure, as it captures the empty promise of preferences for goods that are not produced by a beneficiary country (mangoes from Iceland, semiconductors from Afghanistan, etc.). In the tables, we label the trade weighted eligibility measure of duty-free access under any preference program *El Any*. We report robustness results for the unweighted eligibility measures in the footnotes of Section 8; the results are qualitatively unchanged by removing the trade weights.

While this simple eligibility-based definition of the preferential treatment variable is appealing in its parsimony, one can easily challenge the definition on the grounds that even when preferential eligibility is indicated by the HTS-US, preferential treatment is often afforded to only a subset of the imports in question. Partial-year program eligibility is a key concern, as many program changes (including virtually all changes under GSP) are effective July 1st (or idiosyncratically), rather than January 1st of a calendar year. Moreover, GSP preferences can be (and often are) limited by additional "competitive need limitations" (CNLs), which offer duty-free treatment only until a certain level of exports is reached. Finally, restrictive rules of origin restrictions or other bureaucratic costs under some programs may make *de jure* preference eligibility useless in practice. A preference measure based on countries' actual usage of the programs can capture such otherwise unobserved limitations to program use in practice.

[36]As of January 1, 2006 (the last year in our sample), the HTS-US schedule included 19 preferential treatment codes for GSP (and its subcategories), regional agreements, etc.

[37]We code a country-product-year observation as preference eligible if it is eligible for more than one quarter of the given calendar year.

[38]Time invariant trade weights are constructed using 1997 trade flows, the year immediately preceding the first year in our sample.

With these caveats in mind, we define a second form of the dependent variable using more detailed data from the U.S. International Trade Commission (USITC). Each year, the USITC reports the proportion of bilateral trade that clears U.S. Customs under each preferential program code, by industry and country of origin.[39] We use this information to construct our baseline measure of the dependent variable, *Any Pref Share*, so that θ_{cjt} is the (exact) share of country c exports of product j in year t that entered U.S. Customs claiming duty-free access under any preferential program code. Note that this version of the dependent variable based on U.S. Customs data offers the additional advantage that it does not require an ad-hoc weighting scheme to aggregate to 4-digit NAICS.[40]

In addition to these two measures of U.S. preferential market access under any and all preference programs, we create three alternative dependent variables: two measures of duty-free market access under GSP only – one eligibility based, *El GSP*, another based on Customs data and actual GSP use, *GSP Share*, and a third alternative for a robustness check, *Non-GSP Share*, the rate at which a country's exports claim any preferential market access under a program other than GSP. There are several reasons that we find

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[39]For instance, when a product enters the U.S. under a GSP eligibility clause, it receives special tariff code A (or A*, A+ depending on the particular sub-classification of GSP eligibility). When a product enters the U.S. duty-free under a free trade agreement, then an agreement-specific code is entered (for instance "MX" for Mexican products entering under the NAFTA or "R" for products entering under the Caribbean Basin Trade Partnership). These codes match those used by the USTR.

[40]It should be noted that some of the deviation between the Customs-use and eligibility-based measures is caused by foreign exporters failing to (or choosing not to) claim preferential access when eligible. (See recent work by Hakobyan (2010) on underutilization of GSP preferences.) The relevant concern for our study would be if MNE foreign affiliates are more able to *use* preferential trade programs than are commensurate locally owned firms (which, to be fair, is not obvious – while U.S. MNE affiliates may find it easier to file the paperwork necessary to get preferential market access, their disproportionate use of sourced inputs may make rules of origin harder to satisfy). The robustness of our results to either preference variable definition – program use, which could potentially be contaminated by differential uptake of preference program usage by MNE affiliates (but is otherwise a more precise measure), and eligibility, which is clearly free of any such concern (but is also a less perfect measure given aggregation and related issues outlined earlier) – suggests that this discrepancy is of minimal consequence in the context of our study.

GSP preferences of particular interest. The first is simply that the GSP program comprises roughly two-thirds of country-industry-year observations with preferential market access in the U.S.[41] At the same time, the mechanics of the GSP program feature important institutional differences compared to regional or other preferential programs. As mentioned earlier, there is a formal process of annual reviews of GSP eligibility in which domestic and foreign firms, foreign governments, labor unions, and other interest groups can, and regularly do, take part. Year to year, GSP eligibility can be revoked on a discretionary product level basis due to human rights, labor, or intellectual property violations, and is regularly limited by binding competitive need limitations that cap imports from the most productive developing countries. Finally, from an econometric perspective, the GSP-based preference measure is largely immune from concerns over reciprocal trade policy implications or bilateral investment protections that may arise under Article XXIV free trade agreements.

It is worth spending a moment to examine the extent of variation in our dependent variable, whichever way it is defined. Per the earlier discussion, a literal reading of Article XXIV rules would lead one to expect that country-fixed effects would explain most of the observed variation in trade preferences under regional or bilateral free trade deals. The Enabling Clause, which authorizes GSP programs, allows more discretion at the industry level – certain industries may be excluded from GSP eligibility entirely – but these industry exclusions must be uniform across GSP beneficiary countries (hence the moniker, *Generalized* System of Preferences).[42] In principle, we then would expect that together, country- and industry-fixed effects would account for virtually all of the observed variation in the GSP variables.

Of course, it is well understood that there is some leeway in incorporating exemptions and exclusions both during the initial negotiation of preferential agreements and through

[41]Weighting by trade volume, however, GSP is predictably a smaller share; total U.S. imports under GSP comprise 7% of all preferential imports.

[42]The U.S. GSP includes a two-tiered system to allow enhanced market access for "least developed beneficiary countries," but as will be clear from both the following figures and our empirical results, the two-branch system is not responsible for our results.

subsequent formal and informal review processes thereafter. To convince the reader that there is, in fact, sufficient variation left to explain after including country-, industry-, and year-fixed effects, we offer the following simple plots. On the vertical axis, we plot the residual of our dependent variable, preferential market access after controlling for country-, industry-, and year-fixed effects. On the horizontal axis, we plot country per capita GDP. Each plot is for a different definition of the dependent variable: *Any Pref Share* and *GSP Share* in the top row, and *El Any* and *El GSP* in the bottom row. For the two GSP-specific measures, we include data only for the set of countries that is potentially GSP eligible. In all four plots, we see quite clearly that there is considerable variation in every measure of preferential market access and that the degree of variation is higher among the U.S.' less developed (low GDP per capita) trading partners.

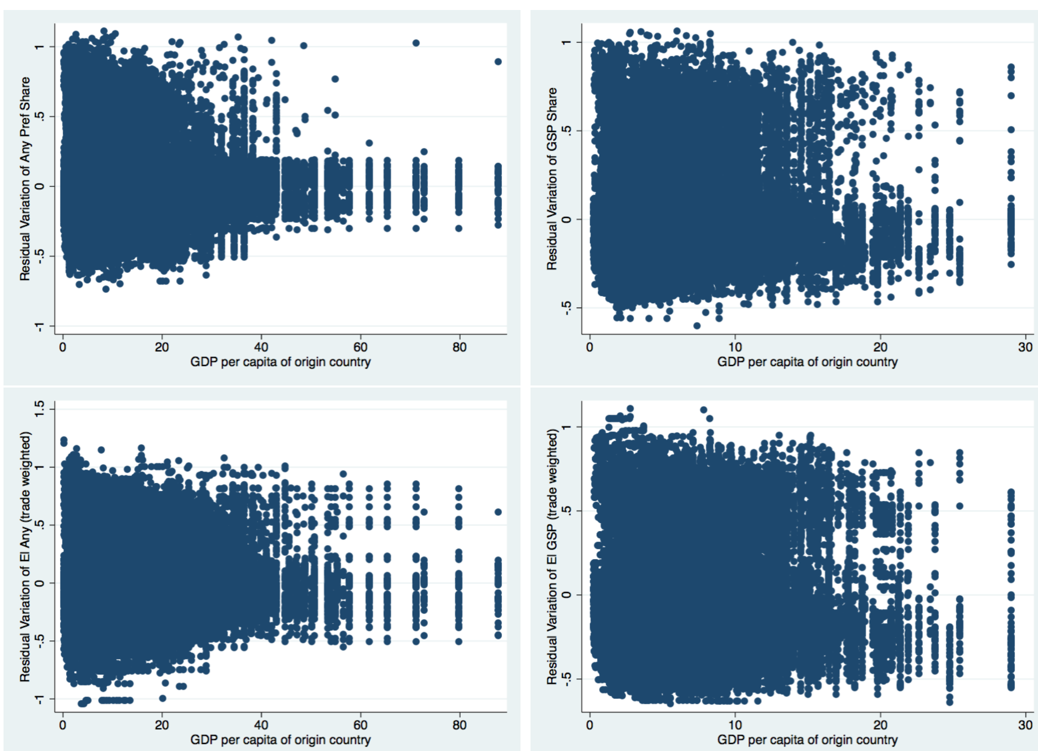

FIGURE 1. Residual Variation in Trade Preferences afer removing Country-, Industry-, and Year-Fixed Effects

Multinational Affiliate Activity. Our data on U.S. multinational affiliate sales are from the U.S. Bureau of Economic Analysis survey of multinational affiliates operating abroad. The BEA data consist of detailed firm level financial and operating data for all foreign affiliates of U.S. multinational firms in which a U.S. entity holds an ownership interest of 10% or more. We necessarily restrict our sample to majority owned foreign affiliates (MOFAs), however, as only MOFAs report sales disaggregated by destination.[43] The data are collected in the BEA's benchmark and annual surveys of U.S. direct investment abroad for the purpose of producing aggregate statistics on U.S. multinational company operations for release to the general public. (The confidential microdata that BEA maintains in its databases for research purposes are a by-product of its legal mandate to produce for the public aggregate statistics on multinational corporation (MNC) operations.) Industries are coded at the 4-digit NAICS level, so that each 4-digit NAICS industry includes multiple 8-digit HTS product lines.[44, 45] For the purpose of testing equation (4.3), we define FDI_{cit} as multinational affiliate sales of goods to the U.S., prorated by the percentage of U.S. ownership.[46]

For our instruments, we use the two additional measures from the BEA data discussed earlier: total MNE affiliate sales of goods and services to the local market by only those firms that do not also sell goods to the U.S., and MNE affiliate sales of services (worldwide), again by only those firms that do not simultaneously sell goods to the U.S.

[43] MOFAs constitute the majority (70% in 2007) of all foreign affiliate sales of U.S. MNEs, and virtually all sales when pro-rated by percentage of U.S. ownership.

[44] In a few instances, a given 8-digit HTS code concords to multiple 4-digit NAICS, in which case we divide the HTS8 import data evenly among the relevant NAICS codes.

[45] The BEA uses modified 'BEA NAICS' codes for industry categorization. Most codes are identical to the standard NAICS, but several are aggregations of standard NAICS (in which case we aggregate to concord the standard NAICS to the coarser BEA NAICS), and a few others are a disaggregation of the standard NAICS, in which case we concord the finer BEA NAICS to the 4-digit NAICS.

[46] Most foreign affiliates in our sample are wholly U.S. owned, and the prorating does not influence our results.

Control Variables. Finally, we include the control variables at the country, industry, country-year, industry-year, and country-industry-year levels as discussed in the previous section. When we include country- and industry-fixed effects, of course, the time-invariant country and industry level controls (such as distance to the U.S. and indicator variables for textile or agricultural industries) are dropped. Table 1 summarizes the variables that we include in our data set and their sources.

In principle, our data set would have 204,160 observations: 232 countries by 88 industries for 10 years (1997-2006). In practice, however, our sample is smaller. We have data for a subset of 184 countries and 80 industries over the ten-year sample. When we lag the independent variables for one year (as seems most appropriate given the time needed for policy to change) and include variables for the log change in U.S. employment or import penetration, our data are further reduced to a 9-year panel beginning in 1998.[47] Of these remaining data, another limitation arises. When we report the preference variable as the share of imports entering the U.S. with preferential treatment, we lose all observations for which U.S. imports (the denominator of the ratio) are zero, leaving 68,130 observations. To make our baseline results consistent across specifications, we use this smallest data set for all specifications of the model, even in cases where we have access to more observations (i.e. for the eligibility-based definitions of the preference variables, which need not preclude observations for which the U.S. import volume was zero). Table 2 reports descriptive statistics for the baseline data set with 68,130 observations. Table 3 provides the same statistics for the data set with only de jure GSP eligible countries included, which contains 42,849 observations. A list of the industries and the countries included in our sample is given in Table 4.

[47]While some preference programs are reviewed only on an ad hoc basis, others, like the GSP, have a formal annual review process for petition, study (by the USITC), and ultimate implementation by USTR on a regular yearly schedule.

6. Results

Table 5 reports the estimation results when the dependent variable is the share of imports by industry i from country c in year t for which duty-free treatment was claimed under any trade preference program. Country- and industry-specific fixed effects are removed by demeaning, and time-fixed effects are incorporated as year dummies.[48] In the interest of space, the table reports coefficient estimates for only the variables of primary interest.[49] The first column reports the results of the just-identified IV model when total affiliate sales to the local market (by firms that do not sell to the U.S.) is used as the only instrument. Jumping immediately to the key result in the first row, we find that a 10% increase in multinational sales to the U.S. implies a 4 percentage point increase in the share of imports receiving duty-free market access, which translates to a 20% increase in the rate of preferential access relative to the sample mean.[50] The result is statistically significant at the 1% level with a p-value below .001.

Reading down the table, the estimated coefficients for the remaining empirical predictions take expected sign and are all statistically significant at the 5% level or better. Consistent with standard political economy motives, we find that larger domestic industries (as measured by domestic sales) are more protected (offering less generous trade preferences), and that the relationship is both economically large and concave.[51] We also find evidence that more successful exporting countries and industries receive less

[48]Standard errors are corrected accordingly.

[49]Explanatory variables included but not reported (noted as 'additional controls' in the table) are U.S. import penetration, the annual log change of U.S. industry import penetration, number of U.S. establishments, and country population. Of these, only the number of U.S. establishments is significant at the 10% level in any of the specifications, with an economically modest coefficient estimate (.017**(.009) in specification (1)).

[50]From Table 2, the average rate of preferential market access is 19.8% in the full sample.

[51]Our coefficient estimates imply that the derivative of *Any Pref Share* with respect to U.S. domestic sales is negative at the sample minimum of domestic sales, but positive at the sample maximum, suggesting a U-shaped relationship.

preferential market access, which may reflect a higher opportunity cost of offering duty-free access to these sectors and countries. We find that preferences are also less generous for industries with higher MNE affiliate sales from the rest of the world (which may compete with the exports in question), which is once again consistent with our initial empirical predictions.

The theory did not offer clear priors for the sign of the remaining coefficients, but we venture that by at least a rough approximation, all of the estimates make sense. Our estimated coefficients are positive and negative for MFN tariffs and total U.S. imports, respectively. Tying back to the discussion in Section 3, the first result suggests that higher MFN tariffs may indeed shift political attention towards the use of preferences, while the second is consistent with the concern that increasing preferences for one country may reduce overall tariff revenue from the rest of the world via trade diversion. Continuing down the table, we find that countries with lower GDP per capita receive ceteris paribus a greater rate of preferential market access, presumably due to the large role of GSP preferences in the full sample. Finally, as one might expect, healthier U.S. industries with larger payrolls and growing employment tend to offer more generous preferential access (or put another way, senescent industries receive more protection); at the same time, bigger industries as measured solely by number of employees (holding payroll fixed) receive more protection in the form of a lower trade preference share.

Finally, notice that the excluded instrument performs well in terms of explaining the potentially endogenous variable "MNE sales of goods to U.S.", as is apparent from the first-stage F-statistic for the excluded instrument of 50.21. Moreover, the Anderson-Rubin Wald test statistic for statistical significance of the endogenous regressor has a p-value less of .0001. Note, too, that the Anderson-Rubin test statistic is robust to weak instruments (for which we do not have any indication in our case). Because our model is just identified, however, we cannot yet test for instrument validity. We note, however, that the first stage results are as expected: the coefficient estimate for the effect of MNE local sales on MNE sales of goods to the U.S. is positive and significant, with a p-value less than .001.[52]

In column 2, we reestimate the model to test for instrument validity and specification robustness. For the estimation, we include a second excluded instrument, MNE sales of services, again excluding firms that also sell goods to the U.S. The coefficient estimate for MNE sales to the U.S. edges up only slightly to 0.409***, and remains statistically significant at the 1% level. The remaining coefficient estimates are also virtually unchanged from the just-identified version of the model, offering comfort that the over-identified model has not introduced systematic bias.[53] Most importantly, the J-statistic p-value of 0.833 exceeds 0.1 by a substantial margin, passing at least this simple test for instrument validity.

Column 3 presents our baseline specification, in which we reestimate the over-identified version of the model under the more conservative assumption allowing for correlation of the error term within countries. Clustering by country, the standard error of the coefficient on MNE goods sales to the U.S. increases slightly relative to the unclustered version, but the coefficient remains statistically significant at the 5% level with a p-value of .045.

The remaining columns offer three robustness checks. Column 4 tests the robustness of our findings to changing the set of control variables. We alter the estimation by reducing the set of controls to include only those variables that are directly indicated by the theory. The estimate of the effect of a 10% increase in multinational sales to the U.S. drops only slightly to 3.6 percentage points and is significant at the 10% level, even when clustering by country. The remaining variables still included in the regression remain virtually unchanged from the baseline version in column 3, while the first-stage Kleibergen-Paap F statistic falls slightly and the Anderson-Rubin Wald test p-value rises, suggesting a marginally weaker fit in the first stage.

[52]The first stage coefficient estimate on MNE sales to the local market (by only those firms that do not also sell goods to the U.S.) is .083*** with a heteroskedasticity robust standard error of .011.

[53]An additional test for bias in over-identified models, comparing 2SLS results against LIML, also revealed no meaningful changes in the results. See Angrist and Pischke (2009), p. 209.

Finally, columns 5 and 6 push the data to the limit, adopting increasingly strict assumptions about the structure of the error term. Clustering by country-industry pair in column 6 reduces the statistical significance to the 10% level, but only clustering by country and industry (column 5) finally pushes the p-value above 10% to .19. It is worth noting that the first stage estimation results remain sharp under even the most restrictive assumptions about the error term: the p-values for significance of the instruments in the first stage remain below .001 and .03 (for local and service sales respectively), even with two-way clustering in the presence of three-way fixed effects.

We now turn in Table 6 to consider the influence of multinational affiliates' sales on preferences under the Generalized System of Preferences. Because many countries included in our sample are automatically excluded from the GSP program according to U.S. trade law, we restrict the sample to exclude countries that are *de jure* GSP ineligible under the 1974 Trade Act.[54] In the reduced sample, 91% of observations are for *currently* GSP eligible countries, opposed to 57% in the full sample. The first column presents results for the *Any Pref Share* variable, for comparison with the benchmark specification in the full sample (Table 5, column 3), while the remaining columns report results for the preferential market access under the GSP program specifically.

The effect of removing the *de jure* GSP ineligible countries is striking: the coefficient estimates for the impact of MNE sales on all U.S. trade preferences (Table 6, column 1) increases more than three-fold relative to the benchmark estimate in the full sample in column 3 of Table 5. Among developing (potentially GSP-eligible) countries, a 10% increase in MNE sales to the U.S. leads to a 14.7 percentage point increase in the share of U.S. imports claiming any trade preferences. The remaining variable coefficient

[54]Countries are immediately deemed ineligible if they are members of the European Union, have high income according to the World Bank, are communist, have terrorist ties, or are members of an arrangement aiming at withholding supplies of vital commodities. The last two criteria appear to be implemented with considerable discretion, however, as some otherwise *de jure* GSP eligible OPEC members are consistently excluded (such as Iran and Libya), while others are not (such as Algeria, Angola, Ecuador, Iraq, Nigeria, and Venezuela for 2009). The 'terrorism' criterion is even less transparent. We therefore restrict the sample to exclude countries under only the first three criteria.

estimates are consistent with our results in the full sample with two notable exceptions: perhaps unsurprisingly, per capita GDP is no longer a statistically significant predictor of trade preferences when we restrict attention to developing countries. Likewise, the effect of MNE affiliate sales from the ROW is muted among developing countries, as one might expect to the extent that the trade diversion induced by a developing country is less likely to impose a competitive disadvantage on affiliate imports from the ROW.

Columns 2-5 report the results when the dependent variable is redefined as the share of U.S. imports that enter duty-free under only the GSP program. We again report the just-identified results in the first specification (column 2). The benchmark specification (over-identified, clustered by country) for GSP preferences is in column 3, and the clustering variants (with two-way clustering and by country-industry pair) are in columns 4 and 5, respectively.

All specifications of the model yield a coefficient estimate for MNE goods sales to the U.S. that is statistically different from zero at at least the 5% level. A priori, one might have anticipated that trade preferences for poorer countries would be less subject to domestic U.S. influences such as the change in the number of employees or U.S. domestic sales, but we find no support for such a view. Indeed, in the benchmark specification of the GSP model in column 3, the estimated impact of a 10% increase in the level of MNE affiliate exports back to the U.S. is nearly a 15 percentage point increase in the rate of GSP market access among potentially GSP eligible developing countries – roughly a 75% increase over the mean and just slightly *higher* than the estimated effect of multinational affiliate sales on preferences overall. The remaining coefficient estimates are consistent with the results in column 1 and expectations, so we see no need to discuss them further.

The instruments are somewhat weaker with this restricted sample, presumably because there are fewer industries among the developing countries with both market-seeking (local sales) affiliates and offshoring MNE affiliates selling goods back to the

U.S.[55] At the same time, instrument validity appears of little concern given J-statistic p-values of approximately 100% in these GSP-program specific specifications of the model.

We now turn in Table 7 to an IV Tobit specification for both the full and reduced samples, with double-censoring to restrict $\theta_{cit} \in [0, 1]$. As we noted earlier, demeaning the data to remove country- and industry-fixed effects is a legitimate exercise only in a linear framework, so we include country-, industry-, and year-fixed effects through dummy variables. Due to computational limitations in the estimation of the IV Tobit model with these three dimensions of fixed effects, we both reduce the set of controls to include only those explicitly specified by the theory and resort to the Newey (1987) two-step estimation procedure to achieve convergence.[56]

The first two columns of Table 7 report results for the full sample for each variation of the dependent variable (share of imports entering the U.S. duty-free under (*i*) any preference program or (*ii*) GSP specifically) while the second two columns repeat the same procedure for the reduced data set, eliminating *de jure* GSP ineligible countries. The results are qualitatively similar to the results of the IV panel estimation, in that we find a strong positive effect of MNE sales to the U.S. on preferences. For the *Any Pref Share* variable in column 1, the effect of MNE sales to the U.S. is more than triple in size compared to the IV panel specification,[57] which suggests that in the full sample, our baseline linear specification may understate the effect of offshoring by multinationals on U.S. trade preferences. The coefficient estimate is even higher if the dependent variable is the GSP share, which indicates that a 1% increase in MNE sales back to the United States is associated with a 2 percentage point increase in the rate of preferential access

[55] Interestingly, among potentially GSP-eligible developing countries, a greater share of total local sales are by firms that do not also sell goods back to the U.S. – 70%, compared to 62% in the full sample. It seems the line between vertical, offshoring MNEs and market-seeking, horizontal MNEs is even sharper in developing countries.

[56] Unfortunately, Newey two-step estimation precludes clustering and limits the set of available post estimation statistics.

[57] The coefficient on *MNE Sales* is 1.54^{***} in Table 7, column 1, versus 0.40^{***} in Table 5, column 1.

for countries and industries that already have at least some preferential access at the 4-digit NAICS level.

Reducing the sample to include only the *de jure* GSP-eligible countries in columns 3 and 4 leads to roughly the same estimated effect of FDI on preferences as in the full sample, and the results are markedly closer to our linear model findings in Table 6, suggesting that the extent of downward bias in the linear model is much less acute in the reduced sample. Finally, note that most of the censoring is at zero (55% of the observations for the *Any Pref Share* variable in the full sample) rather than 1 (just under 5% of the sample for the same dependent variable), which suggests that the lower bound at zero is likely responsible for the bias.

Summarizing our results thus far, we draw three broad conclusions. First, the empirical results are qualitatively consistent with the model, and our instruments MNE goods sales to the local market and MNE service sales (both by only those affiliates not exporting goods to the U.S.) perform strongly in the full sample and only somewhat less so in the reduced sample. The key finding, of course, is that U.S. multinationals' U.S.-destined goods sales seem to increase the rate at which exports from the MNE host country and industry will be afforded preferential market access. Our empirical specifications control for the likely endogeneity of export-oriented sales and incorporate country-, industry-, and year-fixed effects. Under the benchmark linear panel IV specification, a 10% increase in MNE sales to the U.S. is associated with an increase in preferential tariff exemption of about 20% of the mean for the sample of all countries.

When we restrict the sample to exclude *de jure* GSP ineligible countries, we find the estimates for both all preferences and GSP preferences to rise more than threefold relative to the full sample findings. Moreover, in the reduced sample, the estimate for the effect of MNE sales on GSP preferences is slightly *higher* than that for all preference programs in general. Lastly, the IV Tobit specification suggests that the linear IV panel estimates could represent a lower bound on the effect of MNE activity on preferential market access. In the IV Tobit specification, the effect of a 10% increase in MNE sales back to the U.S. entails a 15.4 percentage point increase in the rate of preferential market access under any program in the full sample, and a 21 percentage point increase in the rate of preferential market access under GSP in the reduced sample.

7. Extensions

We now consider two extensions to the benchmark specification, both designed to further inform our understanding of the underlying mechanism. The first extension measures reverse causality by introducing a simultaneous equation GMM version of the baseline model. (Recall the theoretical (and commonsense) prediction that increasing market access in the U.S. should make offshoring more profitable for U.S. multinational firms.) This amended model delivers estimates for *both* directions of causality: the influence of MNE sales on trade preferences (which mirror our earlier findings) and also the effect of U.S. trade preferences on MNE activity (which we find to be positive and statistically significant, as theory suggests). A second exercise digs deeper into the potential political economy mechanism by incorporating measures of political organization per the Grossman and Helpman (1994) 'Protection for Sale' literature.

Simultaneous Equations. A simultaneous equations variant of the baseline model allows us to estimate the reverse causal effect of trade preferences on MNE activity in addition to the (already examined) effect of MNE sales on U.S. trade preferences. We use three-stage least squares to estimate and test the system of two equations in which both trade preferences and MNE sales to the U.S. are endogenous and jointly determined. The preference equation simply mirrors the baseline panel IV specification (column 3 of Table 5). The FDI equation (which examines the determinants of U.S. multinational affiliate sales of goods back to the U.S.) includes U.S. trade preferences, the instruments from the baseline model (local MNE sales and worldwide MNE sales of services (again, only by firms not also selling goods to the U.S.)), and three additional explanatory variables: total country-industry-year exports to the U.S., country population, and country GDP per capita. Both equations are overidentified.

The top half of Table 8 reports results for the FDI equation. The coefficient estimates show a clear statistical relationship between the chosen explanatory variables and our

FDI measure. The first row provides strong evidence that causality runs both ways in the full sample: a one percentage point increase in the rate of preferential market access (captured in the first column, *Any Pref Share*) is associated with a 2.2% increase in MNE sales to the U.S., while a one percentage point increase in the rate of GSP access (*GSP Share*) appears to increase MNE sales to the U.S. by 5.6%. Interestingly, when we compare the results for the reduced samples in the last two columns of Table 8, we see that the estimated reverse causal effect of trade preferences on MNE sales switches sign (an estimated 1.1% sales decrease for a one percentage point increase in the any preference variable and a 1.4% decrease for a one percentage point increase in the GSP preference variable) just as the reverse effect of U.S. MNE sales on trade preferences increases in magnitude. In other words, while causality clearly runs both ways in the full sample, among the set of developing (*de jure* GSP-eligible) countries, the causality running from FDI to preferences becomes stronger whereas the reverse causality nexus appears to change qualitatively.

Scanning further down the list of explanatory variables in the FDI equation, we find unsurprisingly that total country-industry exports to the U.S. go hand in hand with more MNE sales to the U.S. Finally, we notice in passing that U.S. firms seem ceteris paribus to invest more in less populous countries, both in the overall sample and (perhaps more surprising) among *de jure* GSP eligible countries in the reduced sample.

The bottom half of Table 8 reports results for the preference equation. There, we see that the 3SLS estimates for the influence of MNE sales to the U.S. on preferential market access are quite similar to the IV panel results. For the full sample, the coefficient on MNE sales is very similar with values of .412 for all preferences and .451 for GSP preferences. For the *de jure* GSP eligible country sample, just as for the panel IV specification, the influence of FDI on trade preferences increases. The coefficient estimates equal 1.48 for all preferences and 1.50 for GSP preferences and are statistically significant at the 1% level. These results mirror the results of the linear panel IV specification. As in the first stage of our earlier IV specifications, our instruments for MNE sales to the U.S. are strong – the volume of MNE sales to the U.S. is closely correlated with both MNE goods sales in the (foreign) local market and worldwide MNE service sales (again, by firms not also selling goods to the U.S.), both in the full and the reduced samples.

Political Economy Mechanisms. Given the potential for political lobbying to influence trade preferences, we now consider in Table 9 an explicit political economy extension of the baseline specification. In the interest of brevity, we focus on the baseline version of the model (full sample, over-identified linear panel IV, with *Any Pref Share* as the dependent variable).

We develop an analog to the Goldberg and Maggi (1999) and Gawande and Bandyopadhyay (2000) tests of the 'Protection for Sale' model (Grossman and Helpman, 1994) by interacting our measure of MNE offshoring with an indicator variable for whether or not the industry is politically organized. We also interact U.S. domestic sales with the political organization indicator variable to capture the potential protectionist counterweight to MNE lobbying. For good measure, we use both the Goldberg and Maggi ('GM') and Gawande and Bandyopadhyay ('GB') designations for industry organization.[58]

Table 9 reports the results. The first two columns of the table use the Goldberg and Maggi classification of politically organized industries, while the second two use the designations from Gawande and Bandyopadhyay. We report results with both unclustered heteroskedasticity robust standard errors, and with country-level clustering. Notice that the first stage of the IV regression is weaker than in the baseline case under either definition of political organization, and that in general the precision of the estimates falls relative to the benchmark case.

Nonetheless, we find evidence that under either definition of political organization, U.S trade preferences appear to be differentially more responsive to MNE sales back to the U.S. in industries that are more politically active: comparing the point estimates between the first and second rows of Table 9, we see that across the board, it is in the

[58]The two measures do not coincide perfectly, as the first (GM) uses a simple cut-off value to separate organized vs. unorganized industries, whereas the second (GB) employs an auxiliary regression to define which industries are 'politically organized'.

politically organized industries that MNE sales back to the U.S. seem to attract greater preferential market access. The prediction is sharpest with the GM designations, but the results are qualitatively similar for the GB categorizations. Likewise, we find that trade preferences are somewhat more responsive to protectionist pressure by organized *domestic* industries (as measured by U.S. domestic sales). Comparing the third and fourth rows of results from Table 9, we see that greater sales by import-competing domestic firms seem to decrease the rate of preferential access afforded to imports, but that this (negative) effect is larger for politically organized industries.

While we remain cautious not to overstate the power of these results, given that the GM and GB measures are based on Political Action Committee campaign contributions data from the beginning of the 1980s, they certainly suggest that politics – and in particular political lobbying – may play an important role in amplifying the mechanism. Moreover, the simultaneous equations specification of the model additionally suggests that the relationship between preferential market access and offshoring activity by U.S. MNEs may be a reinforcing cycle.

8. Robustness Checks

We conduct a battery of robustness checks. We begin with a simple falsification exercise to confirm that we do not find results where we should not. In a second step, we examine the robustness of our model, reporting results for several alternative econometric model specifications and alternative definitions of the dependent variable based on program eligibility. A third set of tests looks even further into within-group variation, running the baseline (linear panel IV) specification once with industry-year pairwise fixed effects and country-fixed effects and once with country-year pairwise fixed effects and industry-fixed effects.

Falsification Tests. For ease of comparison, we re-report the results from our benchmark specification with the full sample in the first column of Table 10. The remaining columns report the results from two falsification exercises. Both tests are based on the idea that MNE sales to anywhere *but* the United States should in principle have no effect on U.S. trade preferences or (via general equilibrium effects) even a small negative effect.

In column 2, we replace the key explanatory variable, MNE goods sales to the U.S., with MNE goods sales to the rest of the world (ROW) by all MNEs that are not also simultaneously selling goods to the U.S. For good measure, we additionally restrict attention to MNE sales only to unaffiliated (arms-length) buyers.[59] Since MNE affiliate sales are positively correlated across destinations, we are not surprised to find a positive (albeit smaller and noisier) estimated effect of MNE sales of goods to unaffiliated buyers in the ROW on U.S. trade preferences (second row of column 2). That said, the theory would suggest that this result might simply reflect omitted variable bias, in which case we would expect the positive coefficient on MNE sales to the ROW to disappear once we include the (highly correlated) explanatory variable, MNE sales to the U.S. And indeed, when we reestimate the model with both types of MNE goods sales in column 3, we find the theoretical predictions validated: the coefficient on MNE goods sales to the U.S. is positive, increasing by roughly half compared to the baseline specification, and is statistically significant at the 3% level with country level clustering, whereas the effect of MNE goods sales to unaffiliated buyers in the ROW on trade preferences is smaller in size and negative (and turns out to be statistically significant as well).[60]

In columns 4 and 5, we repeat the same exercise, but this time change the alternative explanatory variable to include *all* MNE sales of goods and services to unaffiliated buyers in the ROW. However, we find that the estimation results in columns 4 and 5 closely mirror the results in columns 2 and 3, with a slight reduction in the estimated negative

[59]Recall that the BEA sales data are divided in three dimensions: goods vs. services; sales to the local vs. U.S. vs. "other" countries (ROW); and sales to affiliated vs. unaffiliated buyers to avoid potential contamination by supply chain relationships within an MNE's multi-country umbrella. Our falsification test variable is therefore the sum of all MNE sales of goods to unaffiliated buyers in "other" countries for all MNE affiliates within each industry-country-year that are not also selling goods to the U.S.

[60]In columns 3 and 5, we instrument for MNE goods sales to the U.S., but not MNE sales to the rest of the world, as the latter satisfies the exclusion restriction. Estimates are little changed (qualitatively or quantitatively) if we instrument for both MNE sales variables.

effect of MNE sales to the ROW in column 5 (all sales) compared to column 3 (goods sales). We interpret these falsification exercises as additional evidence that it is indeed offshoring MNE sales destined for the U.S. – not something simply about FDI writ large – that leads to increased preferential market access.

Alternative Specifications and Dependent Variables. Although we find the most sensible modeling specification to be the instrumented panel versions presented in the main Results section, we now include two reduced form (no IV) linear versions of the model – one panel and the other pooled. The pooled specification includes year-fixed effects and is corrected for two-way clustering at the country and industry levels.[61]

Moreover, as a robustness check on our baseline definition of the dependent variable, we consider an alternative definition of trade preferences based on official program eligibility (rather than actual trade flows by program claimed, which is our baseline preference *'Share'* variable definition), which we aggregate to 4-digit NAICS using historical (1997) trade volumes.[62] For completeness, we also include a set of regressions for the influence of MNE activity on non-GSP preferences (the share of U.S. imports that claim duty-free access under any program *other* than GSP).

We report the results for each of these alternatives in Table 11 for the full sample and in Table 12 for the reduced sample. For brevity, the tables list only the coefficient estimate of interest – MNE sales to the U.S. – for each specification. Each row represents a given model specification, while each column features a different definition of the dependent variable. Thus, each cell in the table reports the key coefficient of interest for the model defined by the row label and the dependent variable definition designated by the column heading. For ease of reference, we also include the already reported baseline results from the linear panel IV and IV Tobit specifications (columns 1 and 2 in rows 1

[61]We report significance for both unclustered heteroskedasticity-robust and country-cluster robust standard errors in the panel OLS version and unclustered heteroskedasticity-robust and two-way clustered standard errors in the pooled OLS version to at least partly account for the likely existence of group peculiarities.

[62]We also test unweighted aggregations of the preference eligibility variables and find qualitatively consistent results; see next footnote.

and 2). Comparing results across the two tables, we suggest focusing on the full sample results for the estimates of all preference programs in general or non-GSP preferences, and on the reduced sample results for GSP programs specifically.

Reviewing the pair of 'Alternative Specifications' Tables 11 and 12, the estimated relationship between MNE sales to the U.S. and trade preferences is broadly consistent across specifications and alternative definitions of the dependent variable.

To evaluate the potential impact of alternative dependent variable definitions, we compare results across columns, focusing on the baseline panel IV specification in the first row. Juxtaposing columns 1 and 3 for all preference programs and columns 2 and 4 for GSP, we see that the estimates are qualitatively similar across the two dependent variable definitions – actual preference program usage from U.S. Customs data (the *Share* variables in columns 1 and 2) versus program eligibility from the USTR (the historical trade-weighted eligibility *El-* measures in columns 3 and 4).[63] The point estimates for the eligibility based measures of trade preferences are smaller and noisier than those for the share based variable definitions in the full sample, while the eligibility and share based variable definitions line up much more closely in the reduced sample. Finally, note that the *GSP Share* point estimate is always higher than the *Non-GSP Share* equivalent which is often statistically insignificant, suggesting that GSP preferences are generally more responsive to offshoring MNE activity than are non-GSP preferences.

[63]As an additional robustness check, we confirm that the trade weights used to aggregate program eligibility to 4-digit NAICS are not responsible for our findings in columns 3 and 4. In the baseline panel IV specification (top row), we find that the key coefficient estimates for *unweighted* versions of the dependent variables *El Any* and *El GSP* are higher than for the trade-weighted variables in the full sample and lower in the reduced sample, but neither sign nor significance are affected by dropping the trade weights: for *El Any*, the key coefficient estimate for the trade-weighted version is .29**, while that for the unweighted version is .70***; for *El GSP*, the key estimate for the trade weighted version of the dependent variable is .28***, compared to .52*** for the unweighted version. Removing the trade weights also does not qualitatively change the estimated effect in the reduced sample: for the panel IV specification in Table 12, the key coefficient estimates in columns 3 and (4) decline from 1.41*** to 1.00*** and from 1.61*** to 1.13***, respectively.

Comparing instead across the different model specifications in each row, we note the panel instrumental variables results to be quantitatively consistent across specifications in both the full sample (Table 11) and even more so in the reduced sample (Table 12). The OLS specifications yield smaller estimates, but also here the results are typically statistically significant.

To summarize, while the coefficient estimates vary, they are consistently positive and significant for all but a handful of the numerous variants of the model reported in Tables 11 and 12.

Pairwise Fixed Effects. Pushing the data in a different direction, we explore within-group variation more deeply by including pairwise fixed effects in Table 13. We rerun the baseline panel IV model for the full sample two more times: first with industry-year pair-fixed effects, country dummies, and the (instrumented) explanatory variable, MNE goods sales to the U.S., and then again with country-year pair-fixed effects, industry dummies, and (instrumented) MNE goods sales to the U.S. In each instance, we still find the coefficient of our key explanatory variable of interest, MNE goods sales to the U.S., is positive and highly statistically significant, while the instruments continue to perform exceptionally well (with a strong first stage and high J statistic p-value).

9. Closing Remarks

In this paper, we examine the relationship between U.S. multinational affiliates and the structure of preferential tariff access to the United States. Combining firm level panel data on U.S. foreign affiliate activity from the U.S. Bureau of Economic Analysis (BEA) with detailed measures of implemented U.S. trade preferences from the U.S. International Trade Commission (USITC), we obtain a uniquely rich panel data set spanning 80 industries, 184 countries, and ten years (1997-2006).

Using instrumental variables to account for the endogeneity of export-oriented foreign investment, we find that within a given (4-digit NAICS) industry in a given country and year, a 10% increase in U.S. foreign affiliate exports to the U.S. is associated with roughly a 4 percentage point increase in the rate of preferential duty-free access from all preferential programs combined. Restricting attention to countries eligible for the Generalized System of Preferences (GSP), the per-dollar influence of multinational affiliate sales on preferential market access increases by an order of magnitude. A 10% increase in MNE sales to the U.S. is associated with a 14.9 percentage point increase in the share of goods claiming GSP preferences. We find that this positive and significant relationship between U.S.-bound MNE sales and preferential treatment is remarkably robust across a variety of model specifications and robustness checks. The results are strong; the implication is clear: investment matters.

As with any empirical study, there are important caveats. The first is that our key explanatory variable, U.S.-bound MNE goods sales, almost certainly understates the extent of U.S. interests in the foreign export sector. Our data do not capture foreign arms-length suppliers to U.S. firms (multinational or otherwise) that may have as much sway in the preference setting process as the formal affiliates we measure in the BEA data. To the extent that we undercount the extent of U.S. MNE interests relative to the remaining variables, our quantitative estimates for the effect of MNE sales on U.S. trade preferences will be biased upward.

By the same token, however, our results will underestimate the political economy motive (if not the implementation) if WTO non-discrimination guidelines under Article XXIV and the Enabling Clause are binding. If so, then we would expect that absent these GATT limitations, discretionary application of trade preferences to favor a country's MNE affiliates would be even greater. Finally, we should emphasize that without detailed data on political lobbying (specifically) for trade preferences, we cannot say whether the inferred causal relationship between U.S.-bound MNE sales and trade preferences is the result of 'preferences-for-sale' or simply politicians' (apolitical) recognition of shifting national (aggregate) economic best interests. While it would be fascinating to decompose our findings into politically induced rent shifting versus maximization of utilitarian social welfare, such a separation is not necessary for the overall results or interpretation of our paper.

Even amidst the caveats, we conclude that discretionary trade preferences are influenced in part by the importing country's multinational firms' foreign direct investment decisions. Using a unique data set merging U.S. FDI and trade preference information, we show that more export platform FDI causes more generous trade preferences for goods originating from the country and industry towards which FDI is directed. GSP preferences, which are in principle designed to offer developing countries 'aid through trade' market access, seem to be particularly sensitive to MNE activities. Whether such trade policy sensitivity is good or bad depends on how the trade-investment nexus manifests itself in practice. For some trading partners, generous preferential tariff treatment may foster additional export-oriented investment, reinforcing a virtuous cycle of improved market access and increased FDI. To the extent that the same mechanism induces significant investment and trade diversion, however, other countries may be left out entirely.

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Appendix A. Tables

Table 1. Variables in Data Set

Variation	Variable	Source
	Dependent Variable:	
Country-Industry-Year	Preferential Access Indicator:	
	by Eligibility	4
	Effective	4
	Explanatory Variables:	
Country	Distance from U.S.	7
Country	Communist Country (yes=1)	2
Country	Terrorist Country (yes=1)	2
Country-Year	GDP	1
Country-Year	Population	1
Industry-Year	U.S. Sales	5
Industry-Year	U.S. Employment	5
Industry-Year	U.S. Import Penetration Ratio	4, 5
Industry-Year	U.S. Payroll	5
Industry-Year	Total U.S. Imports	4
Industry-Year	U.S. MFN Tariffs	4
Country-Industry-Year	MNE Data	6
Country-Industry-Year	Exports to the U.S.	3

Sources: 1. Penn World Tables; 2. CIA World Fact Book; 3. United Nations; 4. U.S. International Trade Commission, U.S. Trade Representative; 5. U.S. Census Bureau; 6. U.S. Bureau of Economic Analysis; 7. distance calculator: www.timeanddate.com/worldclock/distance.html.

Table 2. Data Summary Statistics: Full Sample

Variable	Mean	SD	Min	Max
distance to U.S. (km)	8,305	3,681	734	16,357
terrorist	.013	.115	0	1
communist	.021	.143	0	1
population (b.)	.051	.168	.00002	1.314
per capita GDP (USD)	13,547	11,776	288.4	87,825
textile	.058	.234	0	1
agriculture	.035	.184	0	1
U.S. employees (m.)	.318	.550	.015	3.47
U.S. payroll (mUSD)	8.69	7.06	.678	35.3
U.S. sales (mUSD)	62.3	54.1	5.03	546.8
U.S. total imports (bUSD)	17.2	23.2	.090	214.7
MFN ad-valorem eqv. (wt)	.027	.039	0	1.36
MFN$>.01$	.585	.493	0	1
U.S. import penetration	.990	.017	.600	1.00
U.S. number establishments	41883	192109	92	1150793
log change U.S. employees	$-.031$	.061	$-.494$	.145
log change U.S. import pen.	.001	.005	$-.020$	.176
c- i- t- exports to U.S. (bUSD)	.161	1.18	2.5x10^{-7}	59.2
Any GSP (indicator)	.297	.457	0	1
Any Pref (indicator)	.448	.497	0	1
country curr. GSP el.	.573	.495	0	1
country *de jure* GSP inel.	.388	.487	0	1
industry curr. GSP el.	.996	.064	0	1
Any Pref Share	.198	.340	0	1
GSP Share	.122	.275	0	1
Non-GSP Share	.076	.227	0	1
RTA share	.070	.220	0	1
El GSP (hwt)	.206	.367	0	1
El Any Pref (hwt)	.288	.392	0	1
El GSP (unwt)	.274	.333	0	1
El Any Pref (unwt)	.359	.326	0	1
MNE goods sales to U.S. (bUSD)	.027	.515	0	*D*
MNE goods sales to U.S. prorated by U.S. ownership (bUSD)	.026	.509	0	*D*
MNE local sales (only firms not also selling goods to U.S.) (bUSD)	.041	.245	0	*D*
MNE services sales (only firms not also selling goods to U.S.) (mUSD)	.349	8.67	0	*D*
any MNE	.209	.407	0	1
rest of world MNE sales (bUSD)	1.80	5.36	0	*D*

68130 observations; 'D' denotes BEA data redacted for confidentiality; redacted data is used in analysis

Table 3. Data Summary Statistics: Reduced (De Jure GSP Eligible) Sample

Variable	Mean	SD	Min	Max
distance to U.S. (km)	8,297	3,698	2301	16,357
terrorist	.021	.144	0	1
communist	0	0	0	0
population (b.)	.046	.136	.00002	1.11
per capita GDP (USD)	6,527	4,661	288.4	29,015
textile	.066	.249	0	1
agriculture	.041	.197	0	1
U.S. employees (m.)	.343	.585	.015	3.47
U.S. payroll (mUSD)	8.99	7.15	.678	35.3
U.S. sales (mUSD)	63.5	52.8	5.03	546.8
U.S. total imports (bUSD)	17.8	22.8	.090	214.7
MFN ad-valorem eqv. (wt)	.029	.043	0	1.36
MFN$>$ .01	.570	.495	0	1
U.S. import penetration	.990	.015	.600	1.00
U.S. number establishments	47663	205273	92	1150793
log change U.S. employees	−.032	.061	−.494	.145
log change U.S. import pen.	.001	.004	−.020	.176
c- i- t- exports to U.S. (bUSD)	.047	.400	2.5x10^{-7}	26.8
Any GSP (indicator)	.470	.499	0	1
Any Pref (indicator)	.547	.498	0	1
country curr. GSP el.	.910	.287	0	1
industry curr. GSP el.	.997	.057	0	1
Any Pref Share	.276	.377	0	1
GSP Share	.193	.326	0	1
Non-GSP Share	.083	.238	0	1
RTA share	.078	.231	0	1
El GSP (hwt)	.327	.416	0	1
El Any Pref (hwt)	.348	.422	0	1
El GSP (unwt)	.434	.326	0	1
El Any Pref (unwt)	.461	.317	0	1
MNE goods sales to U.S. (bUSD)	.0063	.1271	0	*D*
MNE goods sales to U.S. prorated by U.S. ownership (bUSD)	.0062	.1268	0	*D*
MNE local sales (only firms not also selling goods to U.S.) (bUSD)	.014	.111	0	*D*
MNE services sales (only firms not also selling goods to U.S.) (mUSD)	.087	3.31	0	*D*
any MNE	.191	.393	0	1
rest of world MNE sales (bUSD)	2.41	5.72	0	*D*

42849 observations; 'D' denotes BEA data redacted for confidentiality; redacted data is used in analysis

Table 4. Countries and Industries in Sample

NAICS-4 industries	1110, 1120, 2111, 2121, 2122, 2123, 3111, 3112, 3113, 3114, 3115, 3116, 3117, 3118, 3119, 3121, 3122, 3130, 3140, 3150, 3160, 3210, 3221, 3222, 3231, 3241, 3251, 3252, 3253, 3254, 3255, 3256, 3259, 3261, 3262, 3271, 3272, 3273, 3274, 3279, 3311, 3312, 3313, 3314, 3315, 3321, 3322, 3323, 3324, 3325, 3326, 3327, 3329, 3331, 3332, 3333, 3334, 3335, 3336, 3339, 3341, 3342, 3343, 3344, 3345, 3346, 3351, 3352, 3353, 3359, 3361, 3362, 3363, 3364, 3365, 3366, 3369, 3370, 3391, 3399
de jure GSP eligible countries	Afghanistan, Albania, Algeria, Angola, Antigua Barbuda, Argentina, Armenia, Azerbaijan, Bahrain, Bangladesh, Barbados, Belarus, Belize, Benin, Bhutan, Bolivia, Bosnia-Hercegovina, Botswana, Brazil, Bulgaria, Burkina Faso, Burundi, Cambodia, Cameroon, Cape Verde, Central African Rep., Chad, Chile, Colombia, Comoros, Congo (DROC), Congo (ROC), Costa Rica, Cote d'Ivoire, Croatia, Djibouti, Dominica, Dominican Rep., Ecuador, Egypt, El Salvador, Equatorial Guinea, Eritrea, Ethiopia, Fed. St. Micronesia, Fiji, Gabon, Gambia, Georgia, Ghana, Grenada, Guatemala, Guinea, Guinea-Bissau, Guyana, Haiti, Honduras, India, Indonesia, Iran, Iraq, Jamaica, Jordan, Kazakhstan, Kenya, Kiribati, Kyrgystan, Lebanon, Lesotho, Liberia, Libya, Macedonia, Madagascar, Malawi, Malaysia, Maldives, Mali, Mauritania, Mauritius, Moldova, Mongolia, Morocco, Mozambique, Namibia, Nepal, Nicaragua, Niger, Nigeria, Oman, Pakistan, Palau, Panama, Papua New Guinea, Paraguay, Peru, Philippines, Romania, Russia, Rwanda, Samoa, Sao Tome & Principe, Senegal, Serbia/Montenegro, Seychelles, Sierra Leone, Solomon Islands, Somalia, South Africa, Sri Lanka, St Kitts-Nevis, St Lucia, St Vincent & Grenadines, Sudan, Suriname, Swaziland, Syria, Tajikistan, Tanzania, Thailand, Togo, Tonga, Trinidad & Tobago, Tunisia, Turkey, Turkmenistan, Uganda, Ukraine, Uruguay, Uzbekistan, Vanuatu, Venezuela, Yemen, Zambia, Zimbabwe
de jure non GSP eligible countries	Australia, Austria, Bahamas, Bahrain, Belgium, Bermuda, Brunei, Canada, China, Cuba, Cyprus, Denmark, Finland, France, Germany, Greece, Hong Kong, Iceland, Ireland, Israel, Italy, Japan, Kuwait, Laos, Luxembourg, Netherlands, New Zealand, Norway, Macao, Mexico, Portugal, Qatar, Saudi Arabia, Singapore, Spain, Sweden, Switzerland, Taiwan, United Arab Emirates, United Kingdom, Vietnam
countries in both categories (due to change in status)	Czech Rep., Estonia, Hungary, Latvia, Lithuania, Malta, Poland, Slovak Rep., Slovenia, South Korea

Table 5. Panel IV Results: All Preference Programs

Dependent Variable: Share of U.S. Imports Claiming Any Program Exemption

	(1)	(2)	(3)	(4)	(5)	(6)
MNE goods sales to U.S.	.400***	.409***	.409**	.358*	.409	.409*
[ln, billions USD]	(.114)	(.111)	(.204)	(.201)	(.313)	(.210)
U.S. domestic sales	-.896***	-.897***	-.897***	-.930***	-.897*	-.897**
[ln, billions USD]	(.297)	(.297)	(.238)	(.202)	(.521)	(.354)
U.S. domestic sales, sq.	2.94***	2.94***	2.94***	2.92***	2.94***	2.94***
[ln, billions USD]	(.627)	(.627)	(.479)	(.463)	(1.02)	(.657)
c- i- t- exports to U.S.	-.064**	-.066***	-.066	-.059	-.066	-.066
[ln, billions USD]	(.026)	(.025)	(.046)	(.046)	(.068)	(.049)
ROW MNE sales to U.S.	-.009**	-.009**	-.009**	-.008**	-.009	-.009*
[ln, billions USD]	(.004)	(.004)	(.004)	(.004)	(.008)	(.005)
U.S. total imp. (all countries)	-.036***	-.036***	-.036***	-.024***	-.036**	-.036***
[ln, billions USD]	(.008)	(.008)	(.006)	(.004)	(.016)	(.010)
MFN ad-valorem tariff	1.62***	1.62***	1.62***	1.61***	1.62***	1.62***
[ln]	(.077)	(.077)	(.181)	(.180)	(.396)	(.142)
country GDP per capita	-.051***	-.051***	-.051**		-.051**	-.051***
[ln, thousands of 1996 USD]	(.008)	(.008)	(.023)		(.021)	(.014)
U.S. employees	-.175***	-.175***	-.175***		-.175	-.175**
[ln, millions]	(.052)	(.052)	(.043)		(.249)	(.075)
U.S. payroll	.031*	.031*	.031**		.031	.031
[ln, millions USD]	(.017)	(.017)	(.014)		(.028)	(.028)
log change U.S. employment	.099***	.099***	.099***		.099**	.099***
[year-on-year]	(.023)	(.023)	(.019)		(.045)	(.019)
additional controls‡	yes	yes	yes	no	yes	yes
country, year, & industry FEs	yes	yes	yes	yes	yes	yes
instrument: MNE local sales [ln]†	yes	yes	yes	yes	yes	yes
instrument: MNE service sales [ln]†	no	yes	yes	yes	yes	yes
KP Wald F-stat	50.21	25.16	17.30	16.90	10.24	8.87
AR Wald test χ^2 p-value	.0001	$<$ .0001	.124	.192	.275	.070
Hansen's J stat p-value	-	.833	.869	.767	.902	.885
cluster variable	-	-	country	country	ctry & ind	ci-pair
number of clusters	-	-	184	184	264	6,410
observations	68,130	68,130	68,130	68,130	68,130	68,130

Heteroskedasticity-robust standard errors in parentheses. ***, **, * denote significance at 1%, 5%, and 10% levels, resp.

‡ Additional controls: (industry-year) U.S. import penetration, annual log change of U.S. industry import penetration, number of U.S. establishments, (country-year) population. (See text). † Instruments include MNE sales by *only* those MNE affiliates that are not also selling goods to the U.S.

Table 6. Panel IV Results: Excluding *de jure* GSP Ineligible Countries

Dependent Variable: Share of U.S. Imports Claiming Any (1) or GSP ((2)-(5)) Exemption

	Any Pref Share	GSP Share			
	(1)	(2)	(3)	(4)	(5)
MNE goods sales to U.S.	1.47**	1.49***	1.49***	1.49**	1.49**
[ln, billions USD]	(.620)	(.378)	(.575)	(.610)	(.733)
U.S. domestic sales	-1.34***	-.971***	-.971***	-.971*	-.971**
[ln, billions USD]	(.400)	(.353)	(.316)	(.547)	(.439)
U.S. domestic sales, sq.	4.74***	3.04***	3.04***	3.04***	3.04***
[ln, billions USD]	(.843)	(.739)	(.661)	(.980)	(.808)
c- i- t- exports to U.S.	-.346***	-.401***	-.401***	-.401***	-.401***
[ln, billions USD]	(.098)	(.075)	(.096)	(.107)	(.146)
ROW MNE sales to U.S.	-.007	-.003	-.003	-.003	-.003
[ln, billions USD]	(.006)	(.006)	(.005)	(.009)	(.006)
U.S. total imp. (all countries)	-.052***	-.059***	-.059***	-.059***	-.059***
[ln, billions USD]	(.010)	(.009)	(.008)	(.016)	(.012)
MFN ad-valorem tariff	2.03***	1.11***	1.11***	1.11***	1.11***
[ln]	(.204)	(.073)	(.162)	(.359)	(.142)
country GDP per capita	.021	-.014	-.014	-.014	-.014
[ln, thousands of 1996 USD]	(.029)	(.012)	(.029)	(.025)	(.019)
U.S. employees	-.267***	-.176***	-.176***	-.176	-.176**
[ln, millions]	(.059)	(.061)	(.047)	(.131)	(.081)
U.S. payroll	.041**	.084***	.084***	.084***	.084***
[ln, millions USD]	(.020)	(.021)	(.015)	(.029)	(.029)
log change U.S. employment	.118***	.063**	.063***	.063*	.063***
[year-on-year]	(.028)	(.029)	(.024)	(.035)	(.024)
additional controls‡	yes	yes	yes	no	yes
country, year, & industry FEs	yes	yes	yes	yes	yes
instrument: MNE local sales [ln]†	yes	yes	yes	yes	yes
instrument: MNE service sales [ln]†	yes	yes	yes	yes	yes
KP Wald F-stat	3.91	10.59	3.91	4.07	3.60
AR Wald test χ^2 p-value	.004	$< .0001$	.0004	.011	.013
Hansen's J stat p-value	.875	~ 1.0	~ 1.0	~ 1.0	~ 1.0
cluster variable	country	-	country	ctry & ind	ci-pair
number of clusters	144	-	144	224	5,183
observations	42,849	42,849	42,849	42,849	42,849

Heteroskedasticity-robust standard errors in parentheses. ***, **, * denote significance at 1%, 5%, and 10% levels, resp.

‡ Additional controls: (industry-year) U.S. import penetration, annual log change of U.S. industry import penetration, number of U.S. establishments, (country-year) population. (See text). † Instruments include MNE sales by *only* those MNE affiliates that are not also selling goods to the U.S.

Table 7. IV Tobit Results

	Full Sample		Reduced Sample	
	Any Pref Share (1)	GSP Share (2)	Any Pref Share (3)	GSP Share (4)
MNE goods sales to U.S.	1.54***	2.10***	1.57***	2.01***
[ln, billions USD]	(.285)	(.652)	(.547)	(.541)
U.S. domestic sales	−.297	1.99***	−.774	2.04***
[ln, billions USD]	(.557)	(.740)	(.705)	(.723)
U.S. domestic sales, sq.	4.04***	−1.12	6.56***	−.953
[ln, billions USD]	(1.23)	(1.73)	(1.55)	(1.72)
total c- i- t- exports to U.S.	−.163**	−.433***	−.215*	−.405***
[ln, billions USD]	(.064)	(.142)	(.115)	(.113)
ROW MNE sales to U.S.	.004	−.005	−.004	−.006
[ln, billions USD]	(.009)	(.012)	(.012)	(.012)
U.S. total imports (all countries)	−.124***	−.185***	−.157***	−.189***
[ln, billions USD]	(.023)	(.030)	(.030)	(.029)
MFN ad valorem tariff	2.92***	2.19***	3.33***	2.24***
[ln]	(.097)	(.115)	(.115)	(.110)
country-fixed effects	yes	yes	yes	yes
industry-fixed effects	yes	yes	yes	yes
year-fixed effects	yes	yes	yes	yes
instrument: MNE local sales [ln]†	yes	yes	yes	yes
observations	68,130	68,130	42,849	42,849
left censored obs. (dep. var.= 0)	37,612	47,924	19,410	22,724
right censored obs. (dep. var.= 1)	3,122	1,959	2,997	1,953

Standard errors in parentheses.***, **, * denote significance at 1%, 5%, and 10% levels of significance, respectively.

† Instrument measures local sales by *only* those MNE affiliates that are not also selling goods to the U.S.

Table 8. 3SLS Results

	Full Sample		Reduced Sample	
	Any Pref Share	GSP Share	Any Pref Share	GSP Share
FDI Equation: *dep.var: MNE goods to US (ln)*				
Preference measure	**.022*****	**.056*****	**-.011*****	**-.014****
[share]	(.008)	(.016)	(.004)	(.006)
MNE local sales†	.076***	.074***	.095***	.096***
[ln, billions USD]	(.003)	(.003)	(.004)	(.004)
MNE service sales†	.427***	.428***	.274***	.273***
[ln, billions USD]	(.046)	(.046)	(.068)	(.068)
total c -i- t- exports to U.S.	.213***	.214***	.201***	.200***
[ln, billions USD]	(.002)	(.002)	(.002)	(.002)
country population	-.155***	-.165***	-.086***	-.081***
[ln, billions USD]	(.045)	(.046)	(.021)	(.022)
country GDP per capita	.0005	.003	.001	.0002
[ln, thousands of 1996 USD]	(.002)	(.003)	(.002)	(.002)
Preference Equation *dep.var: Pref Share*				
MNE goods sales to U.S.	**.412*****	**.451*****	**1.48*****	**1.50*****
[ln, billions USD]	(.123)	(.104)	(.293)	(.257)
U.S. domestic sales	-.780***	-.389	-1.33***	-.967***
[ln, billions USD]	(.300)	(.247)	(.427)	(.375)
U.S. domestic sales, sq.	2.78***	1.36***	4.66***	2.93***
[ln, billions USD]	(.608)	(.500)	(.888)	(.779)
total c- i- t- exports to U.S.	-.067**	-.100***	-.347***	-.403***
[ln, billions USD]	(.028)	(.024)	(.062)	(.054)
ROW MNE sales	-.011***	-.005	-.009	-.005
[ln, billions USD]	(.004)	(.003)	(.006)	(.005)
U.S. total imports (all countries)	-.039***	-.041***	-.052***	-.059***
[ln, billions USD]	(.008)	(.006)	(.011)	(.009)
MFN ad valorem tariff	1.61***	.712***	2.02***	1.10***
[ln]	(.045)	(.037)	(.060)	(.053)
country GDP per capita	-.051***	-.063***	.021	-.015
[ln, thousands of 1996 USD]	(.008)	(.007)	(.013)	(.012)
U.S. employees	-.169***	-.106**	-.263***	-.171**
[ln, millions]	(.055)	(.045)	(.077)	(.068)
U.S. payroll	.032*	.051***	.041*	.085***
[ln, millions USD]	(.018)	(.014)	(.025)	(.022)
log change U.S. employment	.102***	.053***	.121***	.067***
[year-on-year]	(.024)	(.020)	(.035)	(.031)
additional controls‡	yes	yes	yes	yes
country, year, and industry FEs	yes	yes	yes	yes
observations	68,130	68,130	42,849	42,849
$\mathcal{X}_1^2$	23050	22670	16598	16554
$\mathcal{X}_2^2$	2181	1301	35490	22499

Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. ‡ Additional controls: (industry-year) U.S. import penetration, annual log change of U.S. industry imp. pen., number of U.S. establishments, (country-year) population. (See text). † Instrument measures local/service sales by *only* those MNE affiliates not also selling goods to the U.S.

Table 9. Political Economy Interactions

Dependent variable: Any Pref Share	GM Political Indicator		GB Political Indicator	
MNE goods sales to U.S., organized industry	1.16***	1.16*	1.25**	1.25
[ln, billions USD]	(.304)	(.607)	(.561)	(.843)
MNE goods sales to U.S.	.140	.140	-.492	-.492
[ln, billions USD]	(.170)	(.246)	(.589)	(.927)
U.S. domestic sales, organized industry	-.331***	- .331**	-.337**	-.337
[ln, billions USD]	(.076)	(.143)	(.143)	(.223)
U.S. domestic sales	-.903**	-.903**	-.638	-.638
[ln, billions USD]	(.368)	(.352)	(.396)	(.453)
U.S. domestic sales, sq.	3.03***	3.03***	2.97***	2.97***
[ln, billions USD]	(.721)	(.648)	(.717)	(.646)
c- i- t- exports to U.S.	-.120***	-.120**	-.101***	-.101*
[ln, billions USD]	(.043)	(.053)	(.032)	(.054)
ROW MNE sales to U.S.	.0008	.0008	-.006	-.006
[ln, billions USD]	(.006)	(.007)	(.006)	(.006)
U.S. total imp. (all countries)	-.029***	-.029***	-.028***	-.028***
[ln, billions USD]	(.010)	(.009)	(.009)	(.009)
MFN ad-valorem tariff	1.66***	1.66***	1.67***	1.67***
[ln]	(.081)	(.190)	(.082)	(.193)
country GDP per capita	-.059***	-.059*	-.055***	-.055*
[ln, thousands of 1996 USD]	(.011)	(.031)	(.010)	(.030)
U.S. employees	-.394***	-.394***	-.389***	-.389***
[ln, millions]	(.070)	(.072)	(.069)	(.072)
U.S. payroll	.020	.020	.015	.015
[ln, millions USD]	(.021)	(.020)	(021)	(.020)
log change U.S. employment	.061**	.061***	.060**	.060***
[year-on-year]	(.030)	(.022)	(.028)	(.022)
additional controls‡	yes	yes	yes	yes
country, year, & industry FEs	yes	yes	yes	yes
Instruments†: MNE local sales [ln]; MNE service sales [ln]; MNE local sales [ln] x Pol. Org.				
KP Wald F-stat	10.44	5.03	4.32	1.25
AR Wald test χ^2 p-value	$< .0001$	.0001	$< .0001$	.019
Hansen's J stat p-value	.328	.296	.809	.830
cluster variable	-	country	-	country
number of clusters	-	184	-	184
observations	60,704	60,704	60,704	60,704

***, **, * denote significance at 1%, 5%, and 10% levels, resp. ‡ Additional controls: (industry-year) U.S. import penetration, annual log change of U.S. industry import penetration, number of U.S. establishments, (country-year) population. † Instruments include MNE sales by *only* those MNE affiliates that are not also selling goods to the U.S.

Table 10. Falsification Exercises

Dependent Variable: *Any Pref Share*	(1)	(2)	(3)	(4)	(5)
MNE goods sales to U.S.	.409**		.635**		.636**
[ln, billions USD]	(.204)		(.293)		(.293)
MNE goods sales to unaffiliated buyers in the ROW†		.318*	-.184**		
[ln, billions USD]		(.177)	(.081)		
All MNE sales to unaffiliated buyers in the ROW†				.318*	-.181**
[ln, billions USD]				(.176)	(.081)
U.S. domestic sales	-.897***	-.875***	-.908***	-.874***	-.910***
[ln, billions USD]	(.238)	(.234)	(.244)	(.234)	(.244)
U.S. domestic sales, sq.	2.94***	2.96***	2.93***	2.95***	2.93***
[ln, billions USD]	(.479)	(.470)	(.492)	(.470)	(.492)
c- i- t- exports to U.S	-.066	.019	-.114*	.019	-.114*
[ln, billions USD]	(.046)	(.015)	(.059)	(.015)	(.059)
U.S. total imp. (all countries)	-.036***	-.039***	-.035***	-.039***	-.035***
[ln, billions USD]	(.006)	(.007)	(.007)	(.007)	(.007)
ROW MNE sales to U.S.	-.009**	-.010***	-.009*	-.010***	-.009*
[ln, billions USD]	(.004)	(.004)	(.005)	(.004)	(.005)
MFN ad-valorem tariff	1.62***	1.63***	1.61***	1.63***	1.61***
[ln]	(.181)	(.185)	(.180)	(.185)	(.180)
country GDP per capita	-.051**	-.052**	-.050**	-.052**	-.050**
[ln, thousands of 1996 USD]	(.023)	(.023)	(.023)	(.023)	(023)
U.S. employees	-.175***	-.171***	-.177***	-.171***	-.177***
[ln, millions]	(.043)	(.043)	(.044)	(.043)	(.044)
U.S. payroll	.031**	.033**	.030**	.030**	.030**
[ln, millions USD]	(.014)	(.014)	(.014)	(.014)	(.014)
log change U.S. employment	.099***	.100***	.099***	.100***	.100***
[year-on-year]	(.019)	(.019)	(.019)	(.019)	(.019)
additional controls‡	yes	yes	yes	yes	yes
country, year, & industry FEs	yes	yes	yes	yes	yes
instrument: MNE local sales†	yes	yes	yes	yes	yes
instrument: MNE service sales†	yes	yes	yes	yes	yes
KP Wald F-stat	17.30	21.34	10.65	20.73	10.68
AR Wald test χ^2 p-value	.124	.124	.040	.124	.040
Hansen's J stat p-value	.869	.588	.933	.643	.976
cluster variable	country	country	country	country	country
number of clusters	184	184	184	184	184
observations	68,130	68,130	68,130	68,130	68,130

***, **, * denote significance at 1%, 5%, and 10% levels, resp. ‡ Additional controls: U.S. imp. pen., annual log change of U.S. industry imp. pen., # U.S. establishments, ctry. population.

† Measure includes MNE sales by *only* those MNE affiliates that are not also selling goods to the U.S.

Table 11. Alternative Specifications: Full Sample

Reported: Coefficient Estimates for MNE Goods Sales to U.S. (prorated by U.S. ownership)

Model	Any Pref Share (1)	GSP Share (2)	El Any (3)	El GSP (4)	Non-GSP Share (5)
Panel IV [unclustered/country clustering]	.41$^{***/**}$	.45$^{***/***}$	.29$^{**/-}$	.28$^{***/-}$	*ns*
IV Tobit [with c-, i-, t- fixed effects]	1.54***	2.10***	.31	1.06	.33
Panel OLS [unclustered/country clustering]	.08$^{***/***}$	.07$^{***/***}$	.02$^{*/-}$	.06$^{***/**}$	.02$^{-/-}$
Pooled OLS [unclustered/2-way clustering]	.14$^{***/***}$	.09$^{***/***}$	.09$^{***/**}$	.04$^{***/-}$	.05$^{***/*}$

'ns' denotes results that are negative, but not statistically significant from zero at the 10% level. ***, **, * denote significance at 1%, 5%, and 10% levels, respectively. Where noted, significance is reported for both robust and cluster-robust standard errors, respectively.

Table 12. Alternative Specifications: Reduced Sample

Reported: Coefficient Estimates for MNE Goods Sales to U.S. (prorated by U.S. ownership)

Model	Any Pref Share (1)	GSP Share (2)	El Any (3)	El GSP (4)	Non-GSP Share (5)
Panel IV [unclustered/country clustering]	1.47$^{***/**}$	1.49$^{***/***}$	1.41$^{***/-}$	1.61$^{***/-}$	*ns*
IV Tobit [with c-, i-, t- fixed effects]	1.57***	2.01***	.77	1.14*	1.72**
Panel OLS [unclustered/country clustering]	.34$^{***/***}$	.30$^{***/***}$	.31$^{***/***}$	.34$^{***/***}$	.04$^{-/-}$
Pooled OLS [unclustered/2-way clustering]	.28$^{***/*}$	.22$^{***/**}$	.29$^{***/**}$	.29$^{***/**}$	.06$^{**/-}$

'ns' denotes results that are negative, but not statistically significant from zero at the 10% level. ***, **, * denote significance at 1%, 5%, and 10% levels, respectively. Where noted, significance is reported for unclustered robust and cluster-robust standard errors, respectively.

Table 13. Pairwise Fixed Effects

Dependent Variable: Share of U.S. Imports Claiming Any Program Exemption

	(1)	(2)
First Stage		
dependent variable: MNE goods sales to U.S. [ln]		
MNE local sales (only firms not also selling goods to U.S.)	.148***	.148***
[ln, billions USD]	(.013)	(.013)
MNE sales of services (only firms not also selling goods to U.S.)	.524	.510
[ln, billions USD]	(.349)	(.347)
Second Stage		
dependent variable: Any Pref Share		
MNE goods sales to U.S.	**.252***	**.237***
[ln, billions USD]	(.052)	(.049)
observations	68,130	68,130
industry-year (pairwise) fixed effects	Y	N
country fixed effects	Y	N
country-year (pairwise) fixed effects	N	Y
industry fixed effects	N	Y
First-stage KP F-stat	69.66	68.80
First-stage AR Wald χ^2 p-value	$< .0001$	$< .0001$
Hansen's J stat p-value	.543	.755

Heteroskedasticity-robust standard errors in parentheses. ***, **, * denote significance at 1%, 5%, and 10% levels of significance, respectively. In each specification, pairwise fixed effects are removed by demeaning, while the second dimension of fixed effects is included as dummy variables.