

Appendix to “Democracy and Trade Discrimination”
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This appendix provides a detailed discussion of issues that could not be discussed in depth in the paper. It consists of the following parts:

- I. The Factor Content of Dyadic Trade
- II. Control Variables for the Openness Regressions
- III. Democracy, Property Rights, and Trade
- IV. Control Variables for the Trade-Barrier Regressions
- V. (Non)Effects of Centering Independent Variables
- VI. Fixed-Effects Regressions
- VII. US-China Trade
- VIII. Balance-of-Payments Issues
- IX. Alternative Explanations
 - A. Intra- versus Inter-Industry Trade
 - B. Consumers versus Producers
- X. More Robustness Checks

I. The Factor Content of Dyadic Trade

Because my argument about democracy’s dyadic effects requires an extension of Heckscher-Ohlin-Vanek (HOV) theory to a dyadic setting, a few words about this extension are in order. Like Dornbusch, Fischer, and Samuelson (DFS, 1980), I assume that there are two factors of production (capital and labor) and a continuum of goods, $z \in [0,1]$, where goods can be ranked in order of increasing capital intensity. DFS show that, if there is no factor price equalization (FPE)—so that the wage-to-rental ratio is higher in more capital-abundant countries—the pattern of trade is determinate: countries produce goods whose factor content mirrors national factor endowments. As Feenstra (2004: 93-94) notes, this result holds for any number of countries: “If factor prices are not equal, then... a country with a capital/labor endowment higher than its neighbor’s must have higher capital/labor intensities in *all of its* traded goods.” The intuition behind this result is illustrated in Figure A1.¹

¹ This figure is a modified version of Figure 3.10 from Feenstra (2004: Chapter 3).

Figure A1. Factor Endowments and Patterns of Specialization without Trade Barriers

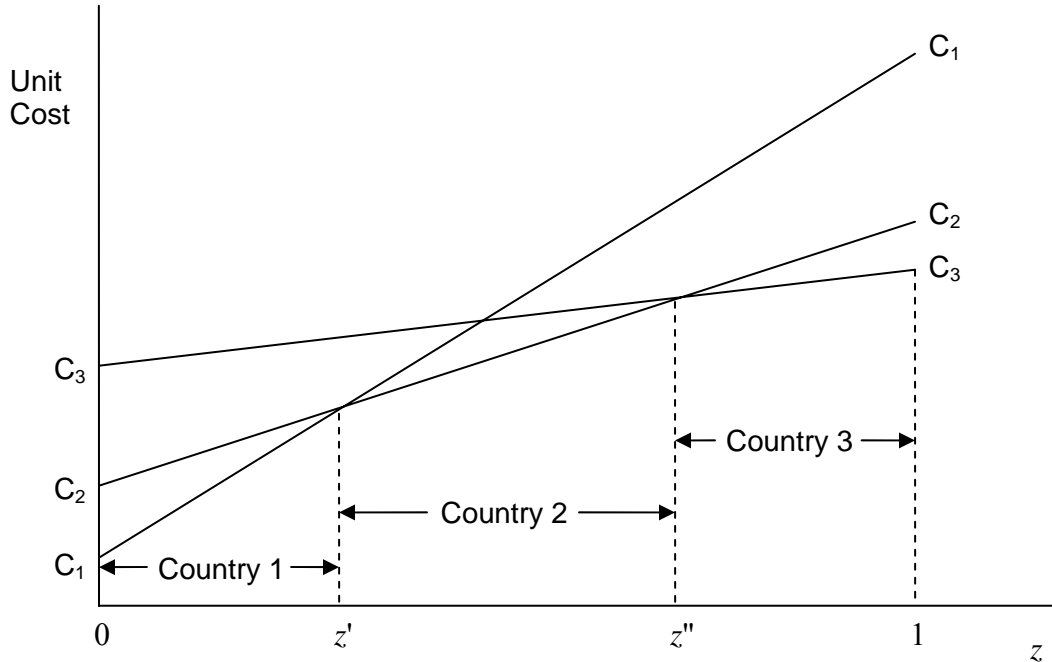


Figure A1 graphs the unit costs of producing goods of differing capital intensities for three countries: 1, 2, and 3. Unit costs are shown on the y-axis, while goods (z) are ordered by increasing capital intensity along the x-axis. The line C_1C_1 is the unit-cost curve for country 1, the most labor-abundant country. Because wages are relatively low in country 1 while rents are relatively high, country 1 produces labor-intensive goods cheaply but capital-intensive goods at high cost. Country 1's unit-cost curve thus rises sharply with the capital-intensity of goods. Country 2's capital-labor and wage-rental ratios are higher than country 1's; hence country 2's unit-cost curve (C_2C_2) rises less steeply than country 1's. Finally, country 3 is the most capital-abundant country and thus has the relatively flat unit-cost curve C_3C_3 .

Each country produces goods that it can make at lowest unit cost, i.e. it produces goods if its unit-cost curve for those goods lies below those of other countries. Country 1 thus produces goods 0 to z' ; country 2 produces goods z' to z'' ; and country 3 produces goods z'' to 1. Note that this pattern of specialization corresponds perfectly to national factor endowments: the capital-intensity of goods rises with national capital-labor ratios. It follows that any imports from wealthier partners will be more capital-intensive than domestically produced goods, while any imports from poorer partners will be more labor-intensive. This result of the DFS (1980) model without FPE is the starting point for my dyadic argument.

The continuum-of-goods model with no FPE is preferable to the standard HOV model, not only because it predicts the factor content of dyadic trade, but also because it enjoys strong empirical support. Although numerous studies have rejected the basic HOV model—leading Maskus (1985) to dub the Leontief paradox the “Leontief commonplace”—Davis and Weinstein (2001) show that an HOV model with minor amendments predicts the factor content of trade very well. Specifically, after replicating the standard null result for the basic HOV model, they show that relaxing the assumptions of common technology, factor price equalization, and zero trade costs dramatically improves model performance. Once one allows for cross-country differences in production technology, trade costs, and a breakdown in factor price equalization,

“countries export their abundant factors and they do so in approximately the right magnitudes. [These] results are extraordinarily consistent across specifications and are robust to changes in the sample.” (Davis and Weinstein 2001: 1444) Davis and Weinstein also show that (1) FPE does not hold even among the rich OECD countries, and (2) the continuum-of-goods model with no FPE predicts trade, not only between OECD countries and ROW, but among OECD countries themselves. If the breakdown of FPE leads to specialization even among the wealthy OECD countries—whose factor prices are similar—then the no-FPE model is presumably appropriate for a world containing countries at very different income levels.

For my purposes, one potential problem with the no-FPE model is evident from Figure A1: countries specialize in entirely different goods. If no country produces goods exported by others, then trade liberalization will not affect the price of domestically produced goods and Stolper-Samuelson wage effects will not occur. In other words, trade liberalization only has Stolper-Samuelson effects if different countries’ production structures overlap. Note, however, that the above discussion of DFS (1980) has assumed that trade barriers do not exist. Not only is this unrealistic, but the presence of trade barriers should cause national production structures to overlap, as shown in Figure A2.

Figure A2. Factor Endowments and Patterns of Specialization with Trade Barriers

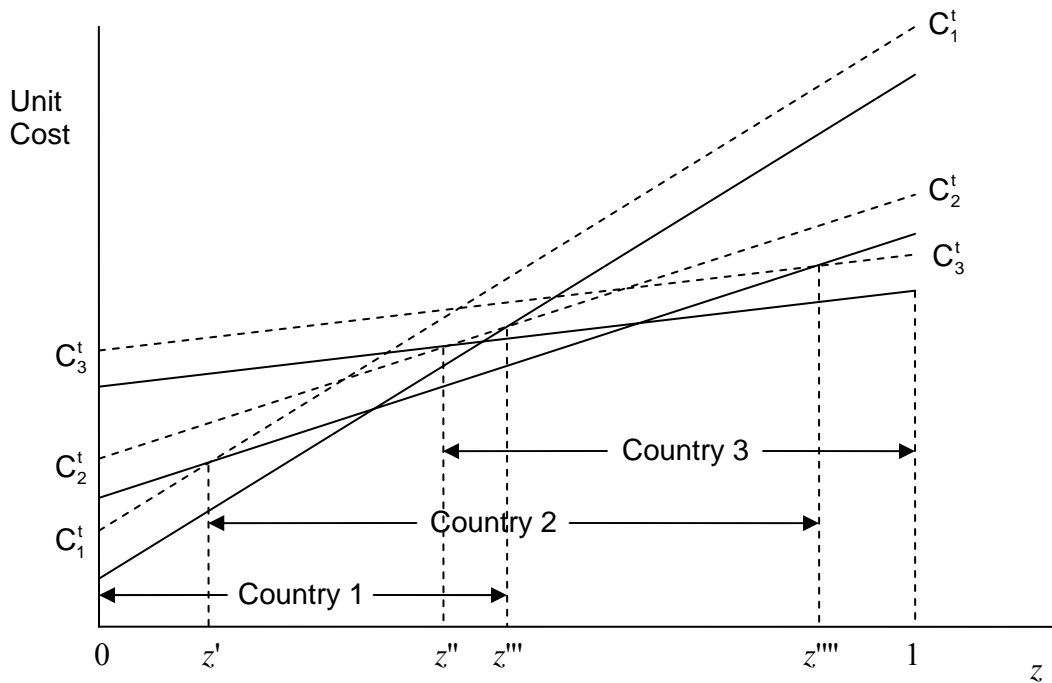


Figure A2 is similar to A1, except that all countries now have a uniform tariff on all goods. Why these tariffs exist is not important: they might reflect revenue needs rather than political-economy forces. What is important is that tariffs do exist empirically and affect the unit cost of imports. The dashed line $C_1^t C_1^t$ is country 1’s unit cost curve ($C_1 C_1$) plus the additional cost imposed by foreign tariffs on country 1’s exports. It is thus the unit-cost curve that matters

for foreign consumers comparing country 1's exports with their own domestic production. $C_2^t C_2^t$ is, similarly, country 2's unit-cost curve adjusted for foreign tariffs, while $C_3^t C_3^t$ is the analogous curve for country 3. The solid lines in 1(b) are the original unit-cost curves $C_1 C_1$, $C_2 C_2$, and $C_3 C_3$. These curves remain relevant because they determine the domestic costs of domestically produced goods, which are not subject to tariffs.

In this scenario, each country produces a broader range of goods than it would in a world of completely free trade because tariffs cause some previously imported foreign goods to be more expensive than domestically produced ones. For example, country 1 produces all goods for which its domestic unit-cost curve lies beneath foreign tariff-adjusted cost curves. Because country 1's domestic cost curve intersects country 2's tariff-adjusted cost curve at z''' , country 1 produces goods 0 to z''' . For the same reason, country 2 produces goods z' to z''' , while country 3 produces goods z'' to 1. Note that all three countries' production structures now overlap: both country 1 and country 2 produce goods z' to z''' , country 1 and country 3 produce goods z'' to z''' , and country 2 and country 3 produce goods z'' to z''' .

Under these conditions, dyadic trade liberalization will have Stolper-Samuelson effects. For example, if country 2 liberalizes trade, it will import both capital-intensive goods in the range z'' to z''' and labor-intensive goods in the range z' to z''' . The former imports will raise the returns to labor but lower the returns to capital, while the latter imports will have the opposite effect. An interesting corollary is that the income effects of across-the-board trade liberalization (i.e. across all partners) are theoretically ambiguous, which may explain why empirical support for Stolper-Samuelson is mixed. The income effects of dyadic liberalization, in contrast, are clear: liberalization with wealthier partners will help labor but hurt capital, while liberalization with poorer partners will have the opposite effects. Hence, if democratization shifts political power from capital to labor, it should encourage liberalization with wealthier partners but protection against poorer ones.

II. Control Variables for the Openness Regressions

I include a large number of controls that previous research has found to influence trade. Five are standard gravity-model controls. $\ln(GDP_{jt})$ is the log of country j 's real GDP at time t . Because wealthier countries should export more, GDP_{jt} should be positively signed. $\ln(Population_{jt})$ is the log of country j 's population at time t . Because—holding GDP constant—larger countries should be less open to trade, $population_{jt}$ should be negatively signed. $\ln(Distance_{ij})$, the log of the Great Circle distance between i and j , controls for the negative impact of distance on trade and should be negatively signed. $Contiguity_{ij}$, a dummy variable coded 1 if i and j share a land border, controls for the tendency of contiguous countries to trade more with one another and should be positively signed. Finally, $\ln(Land_i)$ is country i 's land area and is included because larger countries should, *ceteris paribus*, be less open.

I also include five political-institutional variables. $Democracy_{jt}$, country j 's democracy score at time t , controls for partner regime type. If democracy promotes openness, then countries should trade more with more democratic partners and $democracy_{jt}$ will be positively signed.²

² Although much research shows that democratic *dyads* trade more with one another, I cannot include dyadic democracy scores because I have to include i 's monadic score to test my hypothesis. Including both simultaneously is problematic because i 's monadic score is also incorporated into the dyadic one.

$Alliance_{ijt}$ is a dummy coded 1 if i and j belonged to the same military alliance at time t . Because allies typically trade more than non-allies (Gowa and Mansfield 1993), alliance should be positively signed. MID_{ijt} , a dummy coded 1 if i and j were involved in a militarized interstate dispute at time t , controls for the negative impact of MIDs on trade (Mansfield and Bronson 1997) and should be negatively signed. $GATT_{ijt}$ is a dummy coded 1 if i and j belonged to the General Agreement on Tariffs and Trade (GATT) at time t . Although GATT membership should boost trade openness, Rose (2004) finds no evidence of this and Milner and Kubota (2005) find that GATT members actually have higher tariffs. The expected sign of the GATT dummy is thus unclear. Finally, RTA_{ijt} is a dummy coded 1 if i and j belonged to a regional trade arrangement at time t . Because such arrangements tend to boost trade (Rose 2004), RTA should be positively signed.³

III. Democracy, Property Rights, and Trade

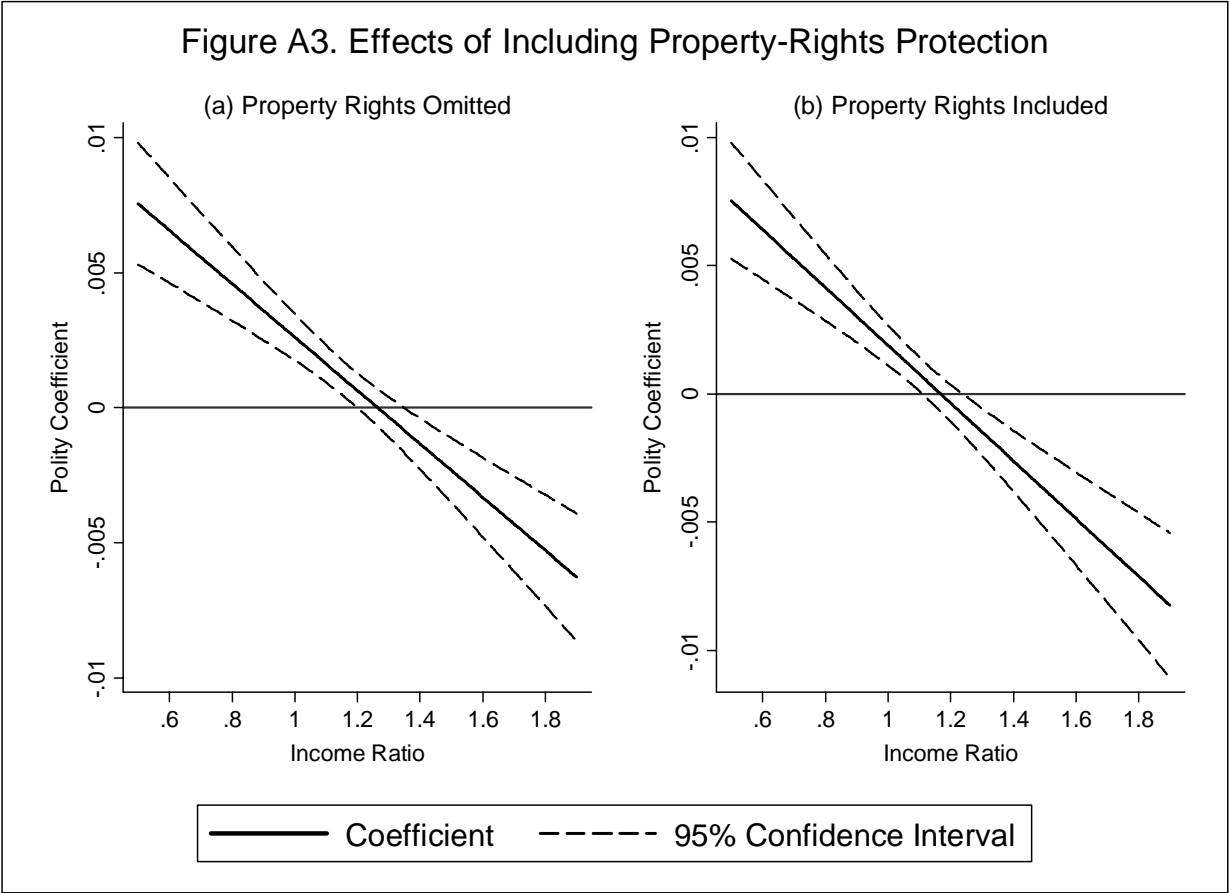
In theory, democratization should lead to trade liberalization (greater openness) when the income ratio is less than 1 and to protection (less openness) when this ratio is greater than 1. In practice, the cut-point at which the impact of democracy equals zero is 1.10, and hence slightly above the predicted tipping point. This probably occurs because democracy affects trade, not only via trade policies, but in other ways as well. For example, research shows that democracy promotes the security of domestic property rights (Li and Resnick 2003), and that secure domestic property rights promote imports (Ranjan and Lee 2005; Smith 1999). If so, then democracies should import more *from all partners* than we would expect on the basis of trade policies alone, which would explain the anomalous intercept in Figure 1 of the paper.

To test this hypothesis, I repeated my openness analysis with a commonly-used measure of property-rights protection on the right-hand side. This measure, developed by Knack and Keefer (1995), is based on five dimensions: expropriation risk, risk of repudiation of contracts by government, rule of law, quality of the bureaucracy, and corruption in government. Countries are scored from 0 to 10 on each dimension, with higher values indicating more secure property rights. These scores are summed to create a composite index ranging from 0 to 50, which, for presentational purposes, I have rescaled to range from 0 to 5. All scores are based on survey data from the *International Country Risk Guide*. If property-rights protection mediates some of democracy's positive impact on trade, then the inclusion of this variable should shift the intercepts in Figure 1 leftward.

Because the inclusion of property rights reduces the sample (from 780,754 observations to 233,322), and because this sample change could affect my results independently of the new control, I constrain the sample to be the same for both analyses. The conditional effects of democracy before and after controlling for property rights are shown in Table A1 and Figure A3.

³ Population data are from Penn World Tables 6.1. Distance data were assembled by Gleditsch and Ward and are available at <http://privatewww.essex.ac.uk/~ksg/capdist.html>. Contiguity data are from Stinnett, Tir, Schafer, Diehl, and Gochman (2002); alliance data are from Gibler and Sarkees (2004); and MID data are from Ghosn and Bennett (2003). GATT membership data are from Reinhardt (1999), and RTA data are from Rose (2004).

Table A1. Effects of Including Property-Rights Protection		
Explanatory Variable	Property Rights Omitted	Property Rights Included
Democracy _{it-1}	1.25 (.193)***	1.32 (.203)***
Democracy _{it-1} × Ratio _{ijt-1}	-.988 (.161)***	-1.13 (.179)***
Ratio _{ijt-1}	.145 (.018)***	.054 (.022)**
Property Rights _{it-1}		.002 (.000)***
Observations	233,322	
Dependent Variable: Δln(Openness _{ijt})		
*p<.10 **p<.05 ***p<.01 Robust-cluster standard errors in parentheses		



For presentational convenience, Table A1 shows only long-run multipliers, which incorporate both immediate and lagged effects, only results for variables of interest, and only results for the Polity measure, since the Freedom House results are very similar. I follow this convention throughout this appendix. As the table shows, the inclusion of the property-rights control has no qualitative impact on my central results.

Of more interest are the conditional coefficients shown in Figure A3. The model specification in Figure A3(a) is identical to that used for Figure 1(a) in the paper; hence, any change in the results is entirely due to the sample change. The intercept at which the impact of democratization equals zero is now 1.27: hence the sample change has shifted this intercept upward by .17. Note, however, that the conditional relationship between democracy, the income ratio, and openness is qualitatively very robust to this large and non-random sample change.

Figure A3(b) shows what happens when the property-rights control is included. Again, the conditional relationship remains qualitatively unchanged. The inclusion of the control has, however, shifted the x-intercept downward by .10, to 1.17. If the inclusion of this control had the same effect in the full sample, this would completely eliminate the anomaly in Figure 1 of the paper. I thus attribute this anomaly to the omission of property-rights protection.

Although democracy clearly affects imports via property rights, I omit this variable from the analysis in the paper for two reasons. First, inclusion of this variable leads to a huge loss of cases (the sample falls from 780,754 to 233,322 observations), and this sample change is non-random: the countries with missing data tend to be very poor. Including the property-rights control thus appears to cause more bias than its omission. Second, the inclusion of this variable induces only an intercept shift but leaves the conditional relationship between democracy, the income ratio, and openness qualitatively unchanged. Including this variable thus does not fundamentally alter my conclusions.

IV. Control Variables for the Trade-Barrier Regressions

Most of the independent variables for the trade-barrier analysis are the same as in the openness analysis, except that, like the dependent variable, they have been averaged across the years for which trade barrier data are available. I do, however, include several new variables that may influence trade policy.⁴ *Government Spending_i*, country *i*'s government spending as a percentage of GDP, is included because generous income transfers may reduce opposition to free trade and thus lead to lower trade barriers (Adsera and Boix 2002). *Growth_i*, country *i*'s GDP per capita growth rate, is included to control for business cycle effects (Cassing, McKeown, and Ochs 1986). *ln(Export Dependence_i)*, the log of country *i*'s exports to country *j* as a percentage of *i*'s GDP, is included because more export-dependent countries may be less willing to employ trade barriers that provoke retaliatory measures from trading partners (Gawande and Hansen 1999).

⁴ The trade-barrier analysis does not include all controls from the openness analysis because not all determinants of openness (e.g. land area) are determinants of trade policy. The openness analysis does not include all controls from the trade-barrier analysis for several reasons. I omitted government spending from the openness regressions because its inclusion would have led to a severe loss of cases. I omitted per capita GDP growth because it is already incorporated into the dependent variable. I omitted export dependence because it is simultaneously determined by most of the same variables that determine import openness and is thus almost perfectly correlated with the latter.

V. (Non)Effects of Centering Independent Variables

In the paper, I center the components of the interaction terms for presentational reasons. When the components are centered, the coefficient on each component gives the impact of that component when the other is at its mean, which is a meaningful result. In contrast, if the components are not centered, each component's coefficient gives the impact of that component when the other equals zero. This may be a meaningless result: for example, it is not useful to know the impact of democracy when the income ratio equals zero because, empirically, this value never occurs. Centering the components thus makes the regression results more informative in this case.

It should be emphasized that the centering procedure is completely innocuous and does not affect any results of interest. It does not affect the sign, magnitude, or significance of the interaction term itself, nor does it alter any of the conditional coefficients and standard errors (Kam and Franzese 2007). The effects of centering are thus purely presentational. Tables A2 and A3, which replicate Tables 1 and 2 from the paper but with uncentered variables, and Figures A4 and A5, which show conditional coefficients based on uncentered variables, demonstrate this point. The only coefficients that change are those on the interaction components, but this is because these coefficients now show the impact of each component when the other equals zero: a value that, empirically, does not exist. Figures A4 and A5 show that this presentational change is irrelevant, in that the conditional effects of democracy remain the same across all sample values of the income ratio.

Table A2. Democracy and Trade Openness, Uncentered	
Democracy _{it-1}	.538 (.099)***
Democracy _{it-1} × Ratio _{ijt-1}	-.484 (.085)***
Ratio _{ijt-1}	.155 (.014)***
Observations	780,754
F (P > F)	43.90 (0.0000)
Dependent Variable: Δln(Openness _{ijt})	
*p<.10 **p<.05 ***p<.01 Robust-cluster standard errors in parentheses	
All democracy coefficients multiplied by 100	

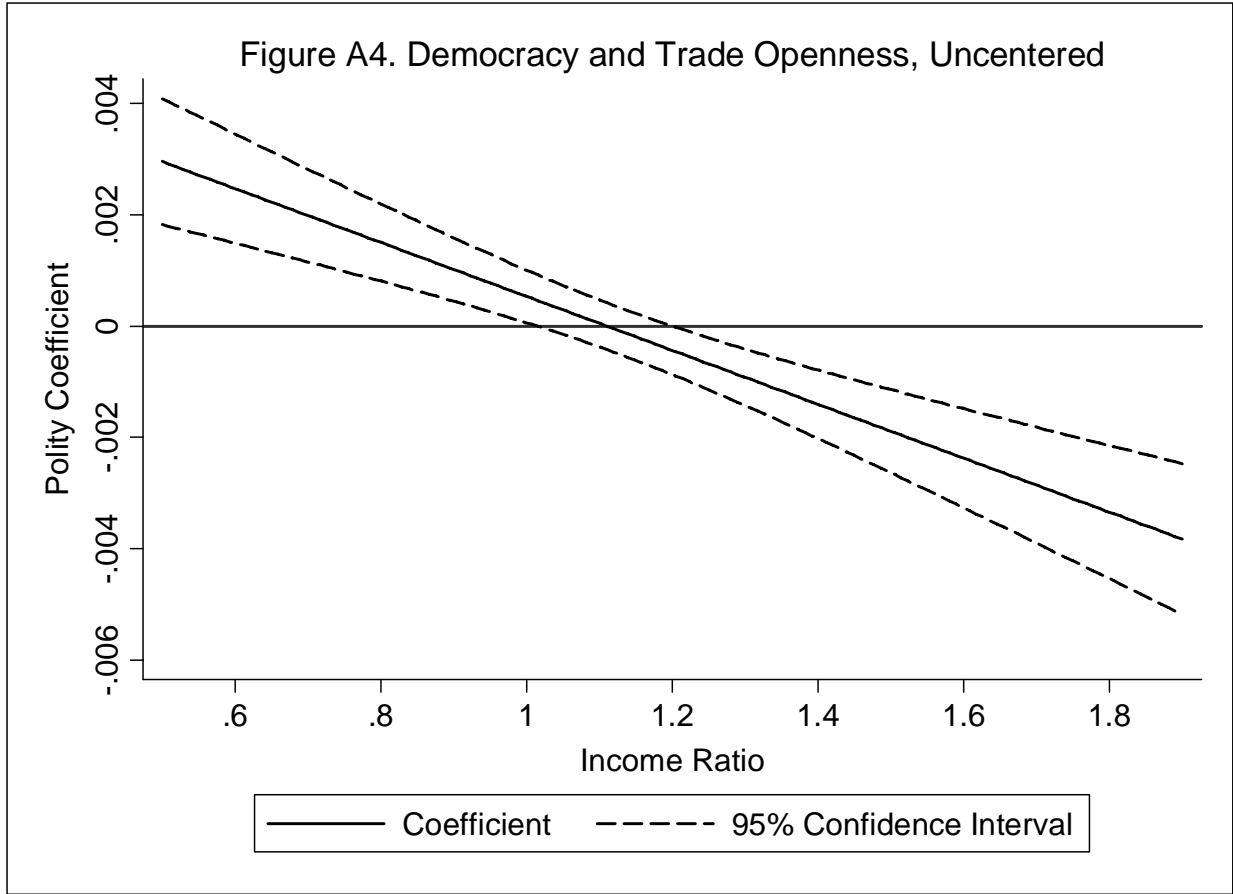
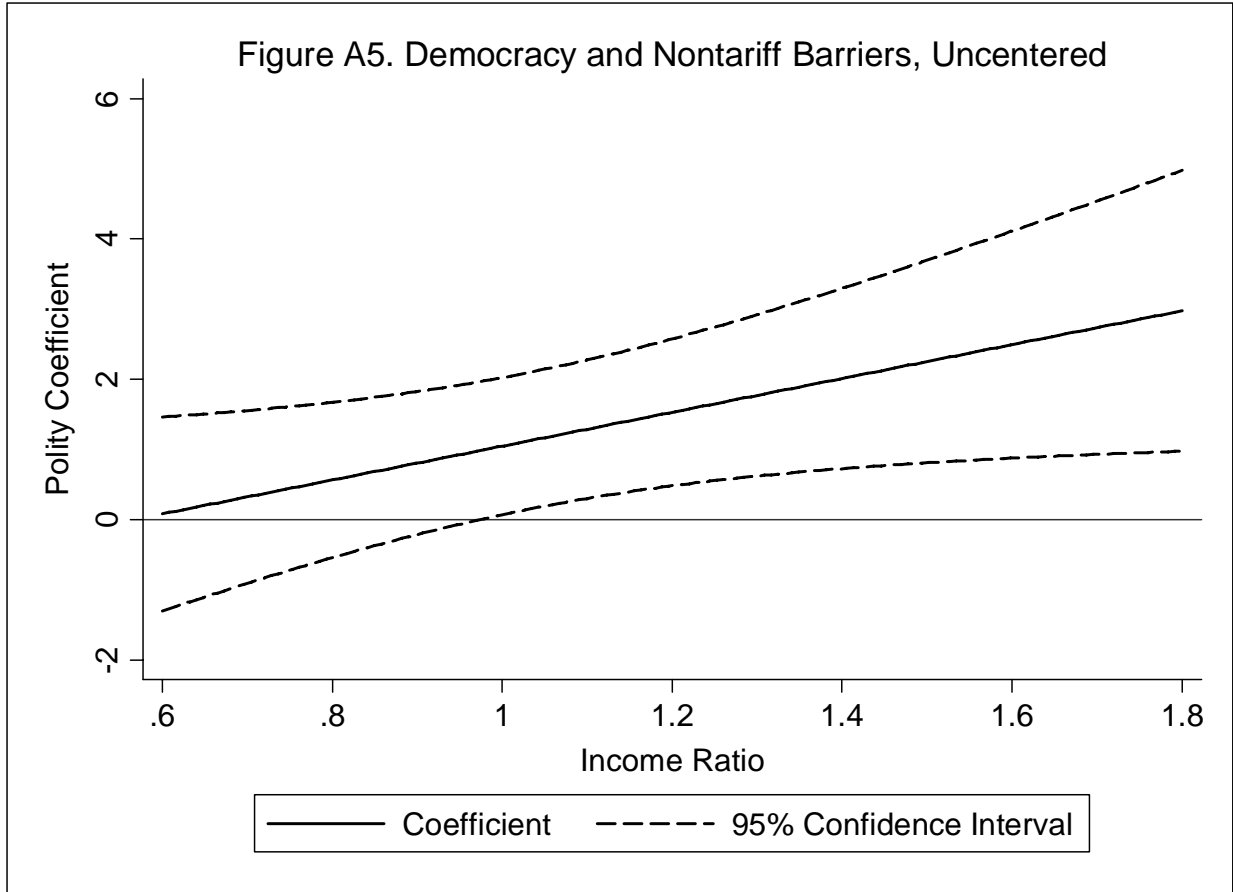


Table A3. Democracy and Trade Barriers, Uncentered		
Explanatory Variable	Dependent Variable	
	MFN Tariff _{<i>i</i>}	NTB Coverage _{<i>i</i>}
Democracy _{<i>i</i>}	-.102 (.727)	-1.37 (1.28)
Democracy _{<i>i</i>} × Ratio _{<i>ij</i>}	-.457 (.693)	2.41** (1.14)
Ratio _{<i>ij</i>}	-9.33* (5.37)	-1.51 (10.6)
Uncensored Observations	13,005	3,181
Left-Censored Observations	271	6,532
Countries	87	74
χ^2 ($p > \chi^2$)	79.81 (0.0000)	106.14 (0.0000)
*p<.10 **p<.05 ***p<.01 Robust-cluster standard errors in parentheses		



VI. Fixed-Effects Regressions

Readers may wonder whether my results are robust to the use of country fixed effects. The short answer is “yes,” although I argue below that fixed effects are inappropriate in this case and are therefore not used in the paper.

Table A4 replicates Table A2 above (i.e. the uncentered version of Table 1 from the paper) but with country fixed effects. It thus shows how controlling for fixed effects affects the openness results. Figure A6 presents conditional coefficients from this analysis. The main point to take away from these results is that controlling for fixed effects does not alter my conclusions. It induces a small intercept shift but otherwise leaves the conditional relationship between democracy and trade openness unchanged.

Table A5 replicates Table A3 above (i.e. the uncentered version of Table 2 from the paper) but with country fixed effects. It thus shows how controlling for fixed effects affects the trade-barrier results. Figure A7 presents conditional coefficients from this analysis. The use of country fixed effects has a large impact on the tariff results: although it is not obvious from Table A5, an examination of conditional democracy coefficients indicates that democracy now has insignificant positive effects on tariffs at all empirically observed income ratios. The use of country fixed effects thus eliminates the relationship between democracy and tariffs. This is unsurprising, given that the trade-barrier analysis is purely cross-sectional. MFN tariffs vary little across partners within countries; hence most of the variation is cross-national. Since country fixed effects eliminate this variation, there is little left to explain.

The use of country fixed effects also weakens the NTB results, in that the $Democracy_i \times Ratio_{ij}$ interaction term becomes insignificant. However, as Brambor, Clark and Golder (2006) and Kam and Franzese (2007) point out, we are less interested in the interaction term itself than in the conditional democracy coefficients. As Figure A7 shows, the use of country fixed effects does not alter the conditional effects of democracy on NTBs: democracy continues to have insignificant effects at low income ratios but significant positive effects at high income ratios. The conditional NTB results are thus robust to the use of country fixed effects, although the conditional relationship is weakened.

Table A4. Democracy and Trade Openness, Country Fixed Effects	
Democracy _{it-1}	.472 (.139)***
Democracy _{it-1} × Ratio _{ijt-1}	-.472 (.137)***
Ratio _{ijt-1}	-.122 (.027)***
Observations	780,754
F (P > F)	7.55 (0.0000)
Dependent Variable: Δln(Openness _{ijt})	
*p<.10 **p<.05 ***p<.01 Robust-cluster standard errors in parentheses	
All democracy coefficients multiplied by 100	

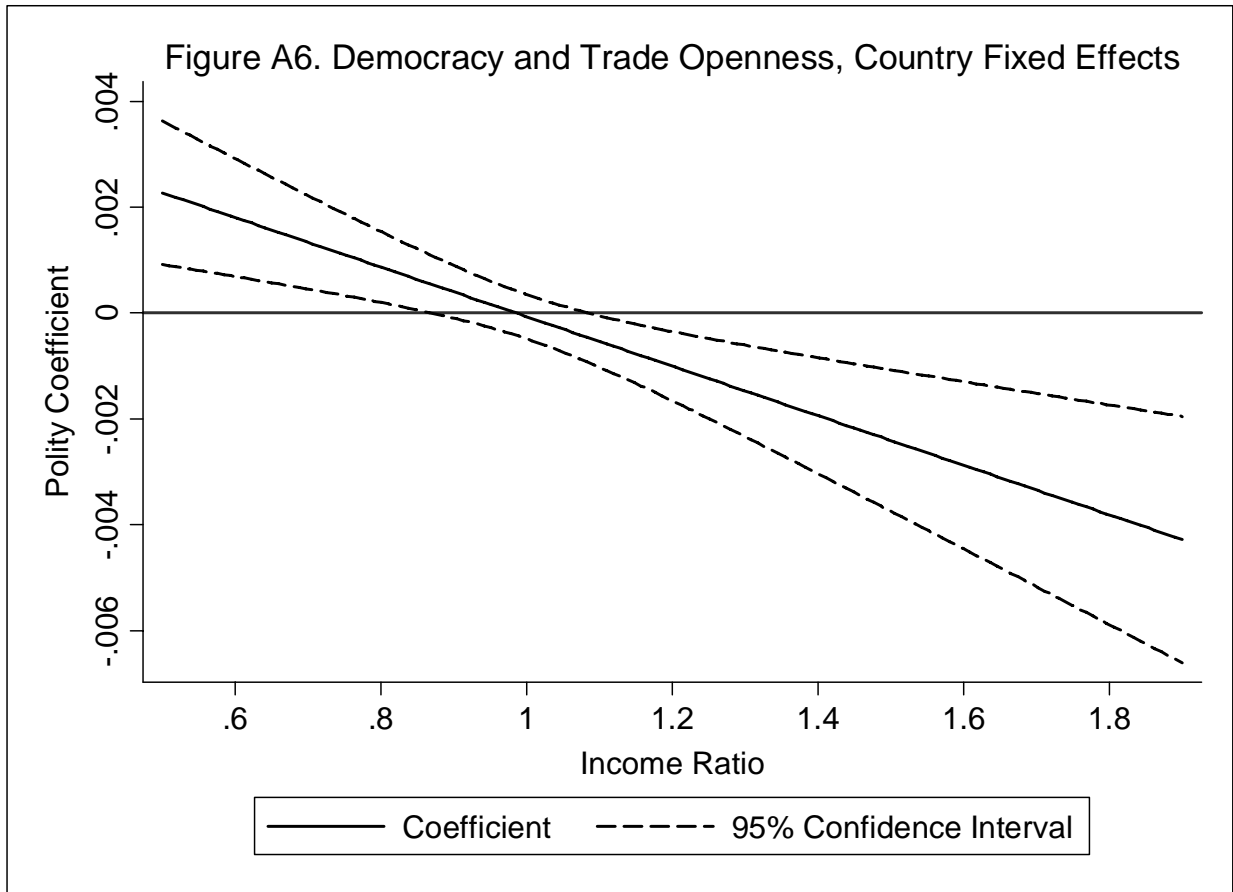
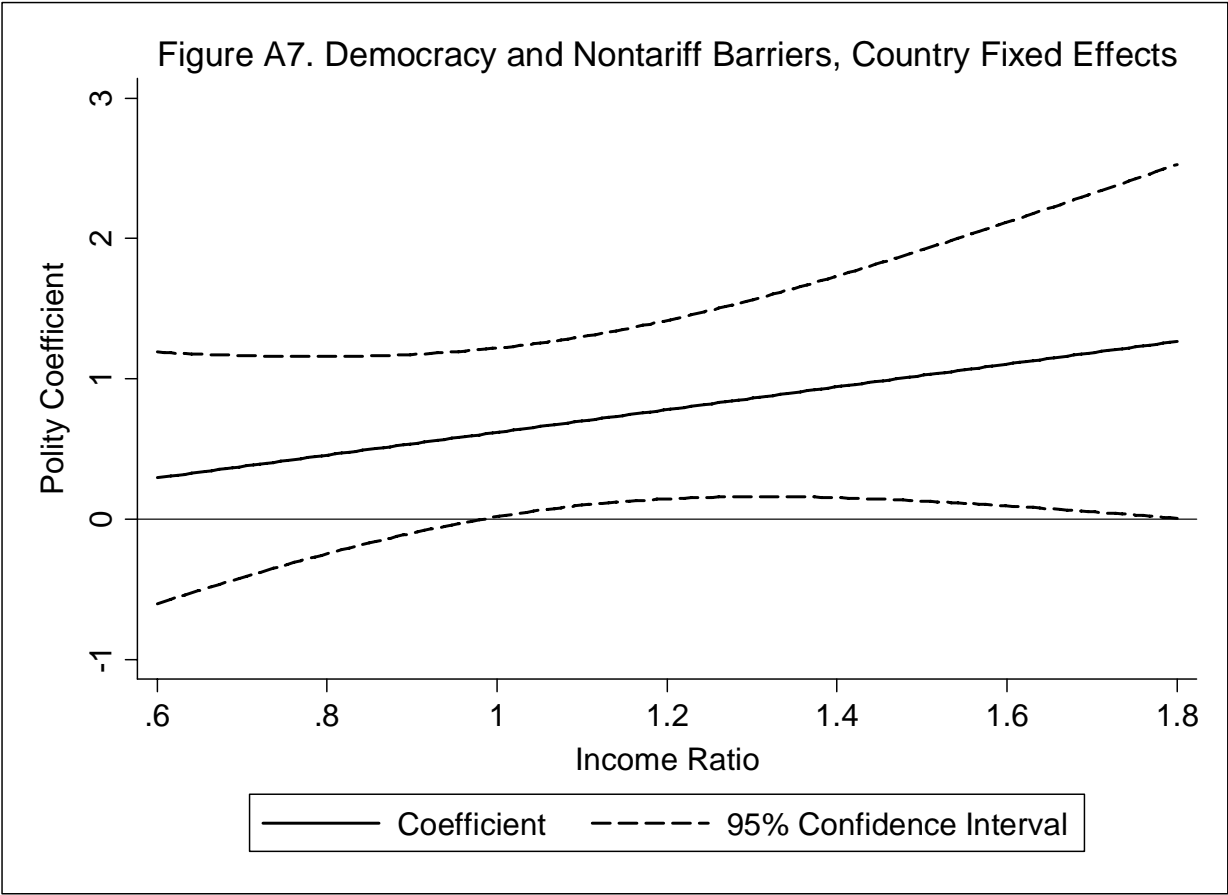


Table A5. Democracy and Trade Barriers, Country Fixed Effects		
Explanatory Variable	Dependent Variable	
	MFN Tariff _i Polity	NTB Coverage _i Polity
Democracy _i	.054 (.052)	-.192 (.838)
Democracy _i × Ratio _{ij}	-.043 (.053)	.810 (.745)
Ratio _{ij}	-.560* (.292)	-4.36 (7.39)
Uncensored Observations	13,005	3,181
Left-Censored Observations	271	6,532
Countries	87	74
χ^2 ($p > \chi^2$)	42.38 (0.0000)	99.52 (0.0000)

*p<.10 **p<.05 ***p<.01 Robust-cluster standard errors in parentheses



The fixed-effects regressions are useful, in that they tell us the degree to which my results reflect within-country dyadic variation as opposed to cross-national variation. That said, the use of fixed effects is inappropriate for testing my hypotheses because my theory predicts systematic variation across countries as well as across dyads. Dyadic income ratios are not randomly distributed across countries: most dyads in wealthy countries will have income ratios greater than one, while most dyads in poor countries will have income ratios less than one. Democracy should thus lead to greater protection in most dyads in wealthy countries but to liberalization in most dyads in poor countries. If we eliminate this cross-national variation in average levels of protection, we are eliminating much of what my theory explains. This will lead to an artificially weak conditional relationship between democracy, income ratios, and trade policy. For this reason, the analyses presented in the paper do not employ country fixed effects.

VII. US-China Trade

U.S.-China trade has grown steadily in recent years and has assumed great political importance. This seems puzzling from the standpoint of my theory, since neither country's regime type has changed: the U.S. is a stable democracy and China a stable autocracy. Both countries thus have stable incentives to protect their markets against each other. Why has bilateral trade risen?

Note first that, although China's imports from the U.S. have risen, this may not reflect trade liberalization in China. According to TRAINS, China's tariff toward the U.S. fell during the 1990s, but its NTB coverage rose. It is thus not clear that Chinese protection toward the U.S. has fallen greatly in recent years. This point is reinforced by openness data: although China's real imports from the U.S. grew by 416 percent between 1980 and 2000 in absolute terms, they grew by only 20 percent as a percent of GDP. Most of the rise in China's imports thus seems to reflect economic growth in China rather than more liberal trade policies *per se*. Hence, if U.S.-China trends are puzzling, the puzzle concerns U.S. rather than Chinese policy.

The U.S. has opened up to China: both its tariffs and NTBs have fallen, and U.S. imports from China rose by nearly 3,000 percent as a percent of GDP between 1980 and 2000. I propose three explanations for this trend. First, U.S. foreign direct investment in China has grown along with the Chinese market: between 1994 and 2001, it increased from \$2.56 billion to \$10.53 billion.⁵ This rise in investment exposure has increased the need for international rules that will safeguard the returns on these investments. U.S. investors have thus pushed hard for China's entry into the WTO and its associated rules on intellectual property rights and investment. Liberalizing agreements such as Permanent Normal Trade Relations (PNTR) with China have been stepping-stones toward this goal.

As my theory predicts, such liberalization has been opposed by a majority of Americans: a 2000 Pew poll indicated that 56 percent of Americans opposed PNTR with China, while only 28 percent supported it. Perhaps more importantly, a 2000 Hart poll found that 49 percent of respondents would be less likely to vote for candidates that supported PNTR, while only 28 percent would be more likely to vote for such candidates (Program on International Policy Attitudes 2000). US legislators thus appear to have voted for PNTR even at the risk of alienating some voters. This suggests that liberalization with China has reflected growing pressures from capital owners rather than a shift in public attitudes.

⁵ U.S. Bureau of Economic Analysis (www.bea.gov/bea/di/di1usdop.htm).

VIII. Balance-of-Payments Issues

In the paper, I note that a shift to full global autocracy would cause rich countries to import more but export less, while it would cause poor countries to import less but export more. Conversely, a shift to full global democracy would cause rich countries to export more (but would have little impact on rich-country imports), while the same shift would cause poor countries to import more (but would have little impact on poor-country exports).

These kinds of statements raise balance-of-payments questions because a rise in imports must be financed either by greater exports or by foreign borrowing. If the effects cited above would cause huge macroeconomic imbalances, we must question whether they could realistically occur.

Although the effects cited in the paper are substantial, they do not imply unsustainable changes in trade balances, except perhaps in the very long run. Consider, for example, the effects of moving from the status quo in 2000 to full global democracy. My figures imply that this shift would improve the aggregate trade balance of rich countries by only 0.4 percent of GDP. The effects on poor countries are larger, but even here, the shift to full global democracy would worsen poor countries' collective trade balance by only 1.1 percent of GDP. Empirically, many developing as well as developed countries have sustained trade deficits larger than this for long periods of time.

The effects of shifting to full global autocracy are larger, but again appear to be macroeconomically sustainable. This shift would worsen the rich-country group's trade balance by 1.2 percent of GDP, which, as noted above, does not seem to be an unsustainable deficit. It would improve poor countries' trade balance by 6.3 percent of GDP. This is a large effect, but could also be sustained for long periods if governments sterilized capital inflows to moderate their effect on the exchange rate.

In the very long run, trade imbalances should influence exchange rates in ways that make the imbalances disappear. Empirically, however, trade imbalances do persist for very long periods of time without being corrected by exchange-rate movements. For example, Mexico ran a trade deficit continuously—averaging 1 percent of GDP—from 1950 to 1980. South Korea maintained a trade deficit from 1950 to 1985 averaging 4 percent of GDP. Turkey's trade balance was continuously negative from 1950 to 2000, with an average deficit of 2 percent of GDP. Given these figures, democratization could plausibly have the effects discussed in the paper, if not forever then for very long periods.

IX. Alternative Explanations

A. Intra- versus Inter-Industry Trade

A possible alternative explanation for my results might invoke intra- versus inter-industry trade. As many scholars have noted, much North-North trade is intra-industry, while much North-South trade is inter-industry (Greenaway, Hine, and Milner 1994). Some scholars have argued that, because intra-industry trade (IIT) involves lower adjustment costs than inter-industry trade, it also generates less political resistance to trade liberalization (Krugman 1981). One might thus argue that my results—liberalization among Northern countries, protectionism

between North and South—reflect an intra- versus inter-industry distinction rather than factoral income effects.

Although the intra- versus inter-industry distinction may be important, it provides a less compelling explanation for my results than the theory presented here. First, some of my results are consistent with my theory but inconsistent with one based on IIT. Both theories predict that North-North trade should be liberalized and that Northern countries should be protectionist toward the South. However, by symmetry, an IIT-based theory would also predict that Southern countries should be protectionist toward the North because North-South trade is inter-industry. The IIT-based theory thus does not predict Southern liberalization toward the North, which my theory predicts and which we observe empirically.

Second, it is not clear why the effects of democracy would depend on the degree of intra-industry trade: to motivate the effects of democracy in wealthier countries, one would have to argue that democratization empowers consumers when IIT is high (which is plausible) but empowers protectionist producers when IIT is low (which seems less plausible). Perhaps such an argument can be made, but I do not know what it is.

Third, it seems unlikely that dyadic income ratios are a proxy for intra-industry trade. Theory and empirical research on IIT indicates that IIT declines with differences in factor endowments. IIT should thus be high when the home-foreign income ratio is around 1, but should be low *both* when the income ratio is much higher *and* when the income ratio is much lower. Dyadic intra-industry trade levels should thus be uncorrelated with dyadic income ratios, and empirically this seems to be true: the bivariate correlation between dyadic intra-industry trade (measured with the Grubel-Lloyd index and discussed further below) and dyadic income ratios is -0.02. Since IIT should exhibit a nonlinear relationship with income ratios, and since it is uncorrelated in practice, it is hard to explain why the effects of democracy change monotonically with the income ratio on the basis of IIT.

Finally, as Gilligan (1997) observes, the political implications of IIT are neither theoretically clear-cut nor empirically well established. If Gilligan is correct, intra-industry trade may actually generate greater protectionist pressures than inter-industry trade because trade barriers on differentiated products approximate private goods (they cover few producers), whereas barriers on homogeneous products are public goods (they cover many producers). Although Gilligan’s empirical evidence is limited to one country (the US), it should be noted that there is little systematic evidence supporting the conventional wisdom that IIT is politically easier to liberalize. For these reasons also, the theory presented here provides a more satisfactory explanation for my results.

In addition to these theoretical arguments, it is possible to verify empirically that controlling for IIT has little impact on my results. To do this, I measure each dyad’s intra-industry trade as a proportion of total dyadic trade using the Grubel-Lloyd (1975) index of intra-industry trade. For a given sector, IIT is measured as

$$IIT_{ijk} = \left[1 - \frac{|\text{Exports}_{ijk} - \text{Imports}_{ijk}|}{\text{Exports}_{ijk} + \text{Imports}_{ijk}} \right] \times 100 ,$$

where IIT_{ijk} is country i ’s intra-industry trade with country j in sector k as a proportion of its total bilateral trade in that sector, Exports_{ijk} is country i ’s exports to country j in sector k , and Imports_{ijk} is country i ’s imports from country j in sector k . I calculate dyadic sectoral IIT scores

for all 3-digit SITC sectors for all available countries and years (1970-1997) using data from Feenstra, Lipsey, and Bowen (1997) and Feenstra (2000).⁶

To obtain an aggregate measure of dyadic IIT, I weight each sector's IIT score using the following formula:

$$w_{ijk} = \frac{\text{Exports}_{ijk} + \text{Imports}_{ijk}}{\sum_k \text{Exports}_{ijk} + \sum_k \text{Imports}_{ijk}} .$$

That is, each sector's IIT score is weighted by that sector's share in total dyadic trade. The dyadic IIT index is simply

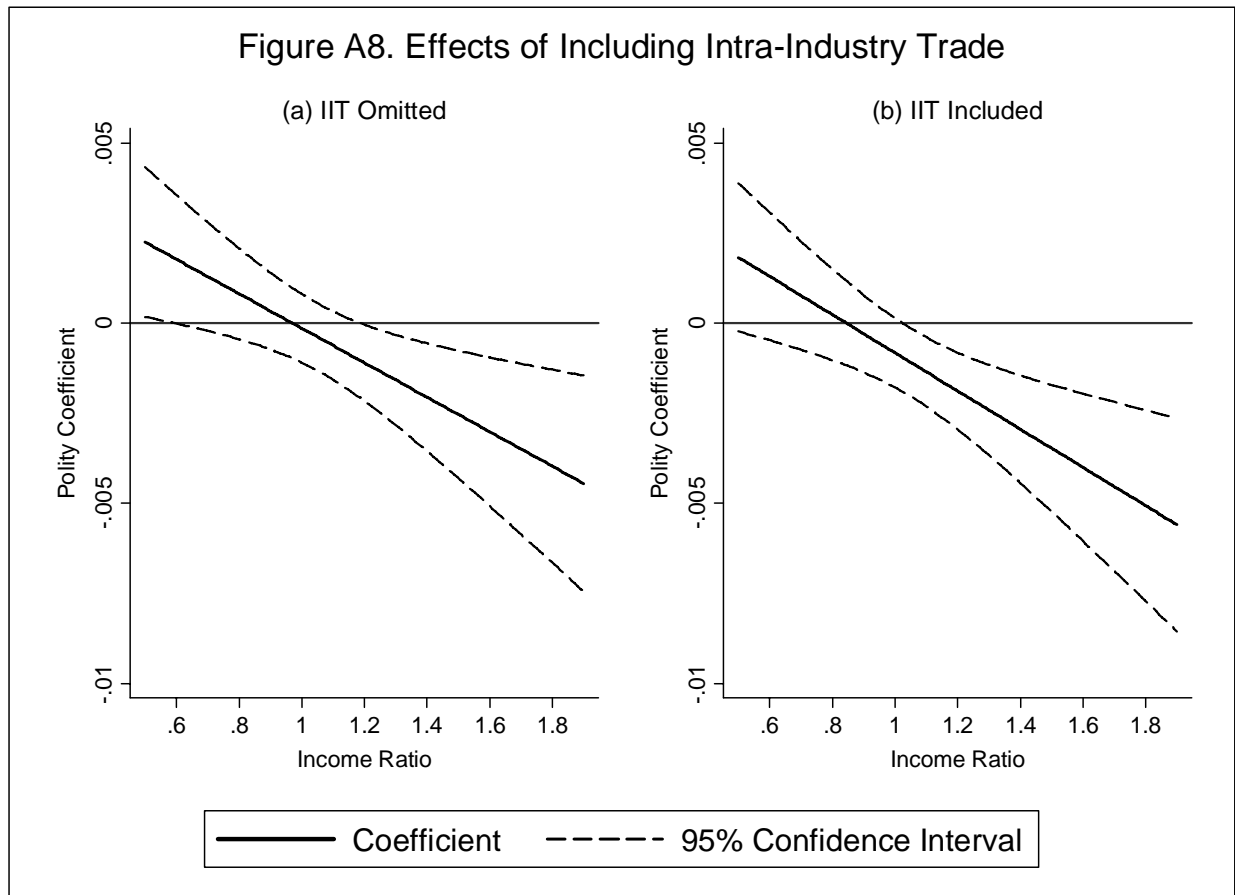
$$\text{IIT}_{ij} = \sum_k w_{ijk} \text{IIT}_{ijk} .$$

To ensure that my results are not driven by intra-industry trade, I include the dyadic IIT measure in the openness regressions. Before presenting the results, it is important to note that including IIT biases the sample severely. This is not simply because the sectoral trade data are limited to the years 1970-1997 and are missing for some (typically poorer) countries. Rather, *by its construction*, IIT cannot be calculated for many dyads, which consequently have to be dropped. Because IIT_{ij} measures intra-industry trade *as a share of total trade*, it cannot be calculated when total trade is zero. Including IIT thus forces me to drop all dyads in which trade equals zero, which constitute 45 percent of all dyads and are typically poor-poor dyads. Because this selection bias will presumably affect my results, it is important to ensure that any changes to my results stem from the inclusion of IIT per se rather than this sample change. I do this by presenting results based on the reduced sample with IIT both omitted and included. These results are shown in Table A6.

Table A6. Effects of Including Intra-Industry Trade		
	IIT Omitted	IIT Included
Democracy _{it-1}	.465 (.187)**	.448 (.184)**
Democracy _{it-1} × Ratio _{ijt-1}	-.479 (.171)***	-.530 (.169)***
Ratio _{ijt-1}	.366 (.034)***	.279 (.035)***
ln(IIT _{it-1})		.005 (.001)***
Observations	239,500	
Dependent Variable: Δln(Openness _{it})		
*p<.10 **p<.05 ***p<.01 Robust-cluster standard errors in parentheses		

⁶ The data are available at <http://cid.econ.ucdavis.edu/>. This website also provides a larger dataset with sectoral trade data from 1962-2000. However, the larger dataset omits dyadic trade flows with annual values under \$100,000, which constitute a substantial proportion of the 1970-1997 data. I thus employ the 1970-1997 data to avoid the bias resulting from the omission of low trade values.

The most striking thing about Table A6 is how similar the results are to the long-run multipliers from Table A2. The large sample change (from 780,754 to 239,500 observations) has very little impact on the strength of the conditional relationship. The inclusion of IIT *per se* has almost no impact on the results, which remain much the same whether or not IIT is included. Hence, to the extent that the results have changed, this is due mainly to the sample change rather than to the inclusion of IIT. This point is reinforced by Figure A8, which presents conditional coefficients. Compared with my original results (see Figure A4), there has been a downward shift in the effects of democracy on openness, so that democracy rarely has positive effects at observed income ratios. This shift, however, occurs both when IIT is omitted (Figure A8(a)) and when IIT is included (Figure A8(b)). It is thus mainly attributable to the sample change rather than to the inclusion of IIT *per se*. The close similarity between the “IIT Omitted” and “IIT Included” figures underscores this point.



It is worth noting that the existence of large volumes of intra-industry trade poses no fundamental challenge to my argument. Davis (1995) has shown that IIT can be explained by a Heckscher-Ohlin-Ricardo model with constant returns to scale and perfect competition if production technologies vary across countries. The prevalence of IIT in North-North trade thus does not imply the irrelevance of Heckscher-Ohlin. Even if IIT is not fully explained by the Heckscher-Ohlin-Ricardo approach, it can nonetheless exist alongside Heckscher-Ohlin trade: for example, Romalis (2004) combines an HOV continuum-of-goods model with a model of

monopolistic competition, while Levy (1997) combines IIT variety gains and Stolper-Samuelson effects in a single political-economy model. My Heckscher-Ohlin-based theory may thus have important implications for trade politics even in a world with substantial intra-industry trade.

B. Consumers versus Producers

Some scholars have argued that democratization shifts political power from import-competing producers to voters-as-consumers (e.g. Kono 2006). This might also have dyadic implications if trade with some partners offers greater consumer gains than trade with others. For example, in Levy's (1997) model, voters experience both factoral income effects and variety gains from trade. Since wealthier countries typically produce a greater variety of goods, trade with wealthier countries might offer greater variety gains than trade with poorer countries. If so, then democracy might have more positive effects on trade with wealthier partners than with poorer ones.

This argument is a reasonable complement to my own, in that such consumer effects could well exist alongside producer effects. It could also help explain why the effects of democracy are slightly more positive than we would expect on the basis of my theory alone. However, the consumer-gains argument is not a substitute for mine because it cannot explain why democratization would have protectionist effects toward poorer partners. Even if we accept that trade with poorer partners generates lower consumer benefits than trade with richer partners—and this is not obvious—these are benefits nonetheless. Democratization might thus lead to *less liberalization* with poorer partners than with richer ones, but a consumer-based argument alone cannot explain why democratization would lead to increased protectionism against poorer partners. To explain this, one must invoke producer interests, as my theory does.

X. More Robustness Checks

Readers may wonder whether the effects of democracy on trade openness vary over time. To address this question, I have broken the sample down by decade and performed the analysis separately for the 1960s, the 1970s, the 1980s, and the 1990s. Results are shown in Table A7.

	1960-1970	1970-1980	1980-1990	1990-2000
Democracy _{it-1}	.517 (.201)***	1.36 (.380)***	.736 (.129)***	.817 (.170)***
Democracy _{it-1} × Ratio _{ijt-1}	-.372 (.173)**	-1.12 (.313)***	-.598 (.108)***	-.720 (.154)***
Ratio _{ijt-1}	.069 (.020)***	.389 (.060)***	.133 (.015)***	.165 (.020)***
Observations	142,116	191,424	199,845	242,843
Dependent Variable: $\Delta \ln(\text{Openness}_{ijt})$				
*p<.10 **p<.05 ***p<.01 Robust-cluster standard errors in parentheses				

As Table A7 shows, the conditional effects of democracy on openness are very similar across all four decades. The democracy coefficient is always significant and positive, while the interaction term is always significant and negative. This analysis thus shows that my results are not particular to any time period but are rather quite consistent over time. The result does not hold up for the 1950s, but this probably reflects the paucity of data for poor trading partners, and consequent sample bias, during this period.

Readers may also wonder whether my results reflect the influence of natural resource dependence, since such dependence might affect trade policy (e.g. by reducing reliance on tariffs for revenue) and may also be correlated with economic development. To ensure that this is not the case, I repeat the openness analysis, including three commonly used measures of resource dependence: oil production per capita, diamond production per capita, and agricultural output as a percent of GDP. All three variables are from Humphreys (2005). Results are shown in Table A8. Note simply that the inclusion of these controls does not alter my results.

Table A8. Effects of Controlling for Natural Resources			
	1960-1970	1970-1980	1980-1990
Democracy _{it-1}	.503 (.112)***	.660 (.111)***	.469 (.120)***
Democracy _{it-1} × Ratio _{ijt-1}	-.413 (.096)***	-.616 (.096)***	-.474 (.107)***
Ratio _{ijt-1}	.144 (.012)***	.186 (.015)***	.177 (.021)***
Oil Production per capita _{it-1}	.042 (.012)***		
Diamond Production per capita _{it-1}		-.004 (.001)***	
Agriculture as Percent of GDP _{it-1}			-.000 (.000)
Observations	650,698	650,698	495,035
Dependent Variable: $\Delta \ln(\text{Openness}_{ijt})$			
*p<.10 **p<.05 ***p<.01 Robust-cluster standard errors in parentheses			

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