

Do WTO Disputes Increase Trade?  
**Tables and Supporting Information**

July 2, 2015

Over the process of writing this paper, we subjected our original findings to a battery of robustness checks. This appendix describes and motivates each robustness check and then presents the results. Many of these suggestions came from commenters, and we owe them our appreciation for this advice.

## **A1 Dispute Conclusion**

Our original analysis focused on a binary variable that was coded zero for years before the the *initiation* of dispute and one for years after the initiation. It is plausible that any trade-increasing effects of a dispute would be seen, not after dispute initiation, but after dispute *conclusion*. To ensure that our original findings were not artifacts of our original coding of the dispute variable, we constructed a new variable, *Post Disp.* This variable is coded zero for years before the *conclusion* of a dispute and one for years after.

Table A1 and Table A2 show the results from replicating Tables 2 and 3 from the original manuscript, using the *Post Disp.* variable in place of the original *Dispute* variable.

Results are similar to our original results. The conclusion of a WTO dispute is not associated with an increase in imports, compared to years before the conclusion of a dispute. In general, the estimated effect of *Post Disp.* is closer to zero than in the original specifications. Even in the models without importer-year fixed effects, the estimated effect of *Post Disp.* is insignificant, and close to zero, in contrast to the original specifications where the coefficient for the dispute variable was occasionally positive and significant. The results which replicate Table 3 are also consistent with our original findings. The lagged *Post Disp.* variables often have small, negative, and insignificant coefficients. None were positive and significant.

Table 4 in our original manuscript also helped address the concern that the positive effect of WTO disputes was only felt after the conclusion of disputes. In that table, we coded indicator variables that described that “status” of a particular country-product trade flow. The country-product flow could have a status of “no WTO dispute has been initiated against this product,” or “a dispute has been initiated but not concluded,” or “a dispute has been initiated and concluded via a panel ruling, MAS, or withdrawal.” This table allowed us to compare import flows during each stage, and since the omitted category was the first one (pre-dispute), the coefficients on the other indicator variable compared import flows during those stages to imports before the dispute.

For thoroughness, we re-estimated these models. However, instead of having an indicator variable for

each type of dispute conclusion (panel, MAS, withdrawal), we simply grouped these together in one single post-dispute-conclusion indicator variable. This allows for an easier comparison of imports before, during and after a dispute, regardless of how that dispute ended.

The results are presented in Table A3. In this table, Dispute Status 1 indicates a dispute is ongoing, but not yet concluded. Dispute Status 2 indicates that a dispute has been initiated and concluded in some fashion. The results are consistent with our original findings. There is not a significant increase in imports either during or after disputes, compared with before.

## **A2 Later Disputes vs. Earlier Disputes**

Our original analysis focused on a full sample covering the years 1995-2010. We included all WTO disputes that were initiated during this time period. It is plausible that some WTO disputes have occurred “too recently,” and therefore haven’t yet had a chance to have import-increasing effects. While a dispute that was initiated in 1998, for example, might have had time to increase imports by 2010 (the end of our sample), perhaps the same could not be said of a dispute that was initiated in 2005, and therefore only had 5 years to increase imports before the end of our sample.

To address this, we re-estimated the models in Table 2 excluding importer-product observations for which a WTO dispute had not concluded by certain cutoff years. We chose three cutoff year: 2000, 2002, and 2005. In other words, the coefficient on the dispute variable is not influenced by observations where the dispute had not concluded by 2000, 2002, or 2005 respectively.

The results are very similar to our original findings, as shown in Table A4, Table A5, and Table A6. In the models using 2000 as the cutoff year, the dispute coefficient is insignificant in all the specifications. In the models using the 2002 and 2005 cutoffs, we see the same pattern as our original results. In some specifications without importer-year fixed effects, we find a positive and significant coefficient for disputes. However, none of these positive results obtain with the importer-year fixed effects.

### **A3 Number of Products Under Dispute**

Different WTO disputes name different numbers of products. WTO disputes also differ in the level of product named, with some disputes naming very specific products and others naming groups of products. It is possible that disputes which name specific products have different effects from disputes which name larger numbers of products.

We addressed this possibility by re-estimating Table 2 using cutoffs for the number of products named in the dispute. More specifically, we coded indicator variables describing whether the dispute named more than 26, 100, and 750 products. We then re-estimated Table 2 excluding disputes which named more than 26, 100, or 750 products. We chose these cutoffs because they corresponded approximately to the sample 10th, 25th, and 80th percentiles.

The results are presented in Table A7, Table A8 and Table A9. The results are consistent with our original findings. The effect of disputes is generally weaker than our original findings. There are no positive and significant estimates of the effect of a dispute, and even the occasional significant results from the models without importer-year fixed effects are not present. There was also not a strong pattern across these three specifications. Disputes do not appear to have larger or smaller effects on imports depending on the number of products named in the dispute.

### **A4 Disputes About Exports**

Many WTO disputes are about trade policies which block imports from the complainant country into the respondent country. However, some WTO disputes are about respondent policies which are designed to promote exports from the respondent country into other markets. For example, some disputes pertain to unfair research and development subsidies from the respondent government to its domestic firms. If a WTO dispute successfully resulted in the removal of these subsidies, we would not expect to necessarily see an increase in imports into the respondent country.

Our original analysis did not delineate between disputes pertaining to import policies and disputes pertaining to these types of export-promoting policies. We wanted to ensure that our results were not driven by our inclusion of these export-oriented disputes.

The DSU has a categorization of disputes along these lines. We re-estimated Tables 2 and 3 from the original analysis, only we excluded disputes pertaining to export-oriented policies.

The results are presented in Table A10 and Table A11. The results are very similar to our original results.

## **A5 Pre-Dispute Periods**

In our original analysis, because of the use of importer-product fixed effects, we compared import flows from before a dispute to after a dispute. The length of the pre-dispute time period obviously varied across importer-products. So for some importer-products, our model compared imports in X number of pre-dispute years with imports in Y number of post-dispute years. It is possible that, prior to a dispute, imports increase sharply (perhaps as market conditions change and the complainant begins to export more to the respondent), then decrease (perhaps because of the import-restricting policy implemented by the respondent country, which triggers a WTO dispute). If this were the case, then including the high-import pre-dispute years, and then comparing them to post-dispute years, might bias our findings against a positive effect of WTO disputes.

For example, consider a dispute over an antidumping duty. In the canonical antidumping situation, we would expect to see an initial surge of imports (resulting from the dumping), then a decrease (resulting from the antidumping duty), and then a subsequent increase in imports after the WTO dispute potentially resulted in the removal of antidumping duties. Since our regression specification compares variation in pre- and post-dispute import levels within particular country-products, the initial surge in imports might artificially bias the coefficient on the dispute variable downward. While this is plausible, we do not see evidence of increases in imports before disputes. Looking again at Figure A1 (reproduced from the manuscript, below), there are not distinct increases in imports, above mean levels, before disputes. In the top pane, imports decrease and then increase a long time 5-10 years before a dispute. In the bottom pane, there is very little variation at all.

We explored this possibility directly by checking that including different ranges of pre-dispute years did not influence our results. To do this, we re-estimated Table 2 using only observations pertaining to 2, 3, or 5 years before the initiation of the dispute and after the conclusion of the disputes. This is similar to what we did in the Figures, where we looked at “windows” of time, before and after a dispute.

The results are presented in Table A12, Table A13, and Table A14. The pattern of results is similar to

that of the original paper. Disputes have a positive effect in some of the specifications without country-year fixed effects, but none of these results obtain when including country-year fixed effects.

## A6 Power Analysis

Given our emphasis on null findings, we wanted to check that we had sufficient power to detect “true” positive effects of a dispute, if they existed. To do this, we conducted power analysis based on comparisons of  $R^2$  values. We first estimate a “full” regression, regressing imports on the dispute variable and the country-year level covariates. We then estimate a “reduced” regression, regressing imports only on the country-year level covariates. We collect the  $R^2$  values from both and calculate power based on the difference.

For the first test, the full and reduced  $R^2$  values are 0.1696 and 0.1688, respectively. We set our power level to 0.99, meaning that we have only a 1% chance of failing to detect a significant effect, if it exists. Note that the 0.99 threshold is very, very conservative. It is much more conservative than the thresholds usually used in power analysis.

With five variables and one variable being tested, the sample size required for detecting an effect with 0.99 power, given those R-squared values, is 17,920. With five variables and all five being tested, the sample size required is 26,880. These calculations were made using the *powerreg* command in Stata 14.

Our sample sizes are much larger than this. For perspective, our full sample regressions have an N of 1.6 million. Even if we only count products with a dispute, the N is 83,000, approximately three times what is required to confidently draw inferences from the estimates.

To further demonstrate that this power analysis is conservative, we can consider how the required sample size changes as we shrink the difference in R-squared values. Bigger differences in the R-squared values means that the researcher has more power and thus needs a smaller sample size for a particular test. In our analysis, even if we reduced the difference in R-squared values for the full and reduced models by a factor of 10 (R-squared full = 0.01696 and R-squared reduced = 0.01688) we still have sufficient sample size to achieve 0.99 power. With those R-squared values and on variable being tested, the required sample size is 215,000.

The R-squared values above were from regressions that did not include fixed effects. To make sure that the omission of fixed effects wasn't creating drastic differences in our power tests, we repeated those

same two regressions, only we limited ourselves to country-products which experienced a dispute. Again, this is the subsample of observations that contribute information to the dispute coefficient.

Our statistical power actually increases in this sub-sample, because the difference in the R-squared values for the full and reduced regressions is even larger: 0.1679 for the full sample and 0.1438 in the reduced sample. With this larger difference, we would need an N of only 672 to achieve the conservative 0.99 power.

Table A1: Replication of Table 2 using years after the conclusion of a dispute

	All Controls	All FEs	No US-EU Controls	No US-EU FEs	OECD Controls	OECD FEs	Non-OECD Controls	Non-OECD FEs	Non-Steel Controls	Non-Steel FEs
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Post Disp.	.022 (.088)	-.003 (.089)	.003 (.104)	.035 (.114)	-.048 (.057)	-.061 (.043)	.067 (.224)	.010 (.167)	.019 (.092)	.011 (.095)
Polity	-.012 (.009)		-.012 (.009)		-.016 (.084)		-.012 (.008)		-.013 (.009)	
ln(PC GDP)	2.846 (.422)***		2.859 (.412)***		2.571 (.495)***		2.957 (.466)***		2.792 (.407)***	
Respondent	-.182 (.037)***		-.198 (.037)***		-.055 (.068)		-.233 (.030)***		-.179 (.038)***	
Crisis	-.039 (.019)**		-.042 (.019)**		-.031 (.017)*		-.039 (.023)*		-.039 (.019)**	
N	1,606,257	2,228,653	1,536,050	2,087,814	467,135	519,541	1,139,122	1,709,112	1,508,124	2,093,198

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This table replicates the models found in Table 2 of the original manuscript. For these models, we used a binary variable that indicated years after the conclusion of a dispute, as opposed to the original coding, which indicated years after the initiation of a dispute.

Table A2: Replication of Table 3 using years after the conclusion of a dispute

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post Disp. (Lag 1)	.014 (.095)	-.022 (.082)										
Post Disp. (Lag 2)			-.007 (.108)	-.034 (.073)								
Post Disp. (Lag 3)					-.048 (.126)	-.055 (.065)						
Post Disp. (Lag 4)							-.017 (.102)	-.048 (.054)				
Post Disp. (Lag 5)									-.003 (.096)	-.059 (.049)		
Post Disp. (Lag 6)											-.018 (.092)	-.075 (.050)
Polity	-.012 (.009)		-.012 (.009)		-.012 (.009)		-.011 (.008)		-.010 (.008)		-.008 (.008)	
ln(PC GDP)	2.845 (.422)***		2.845 (.423)***		2.846 (.425)***		2.930 (.416)***		3.139 (.337)***		3.300 (.282)***	
Respondent	-.181 (.037)***		-.181 (.037)***		-.180 (.037)***		-.211 (.039)***		-.263 (.055)***		-.285 (.071)***	
Crisis	-.039 (.019)**		-.038 (.019)**		-.038 (.019)**		-.033 (.020)*		-.021 (.020)		-.022 (.017)	
N	1,606,257	2,155,821	1,606,257	2,074,688	1,606,257	1,978,385	1,555,644	1,877,994	1,474,646	1,770,796	1,368,881	1,660,351

This table replicates the models found in Table 3 of the original manuscript. For these models, we used a binary variable that indicated years after the conclusion of a dispute, as opposed to the original coding, which indicated years after the initiation of a dispute.

Table A3: Replication of Table 4 using 3 categories of dispute “status”

	All	No US-EU	OECD	Non-OECD	Non-Steel
	(1)	(2)	(3)	(4)	(5)
Dispute Status 1	.097 (.086)	.129 (.104)	.085 (.053)	-.001 (.109)	.097 (.088)
Dispute Status 2	.085 (.104)	.122 (.127)	.039 (.062)	.069 (.156)	.090 (.110)
N	2,228,653	2,087,814	519,541	1,709,112	2,093,198

This table reports estimates from rerunning the models found in Table 4 of the original manuscript. Here, we recode dispute statuses to include 3 categories. Doing so ensures that our categorization of dispute outcomes is not shaping our inferences.

Table A4: Replication of Table 2 with sample restricted to disputes ending before 2000

	All Controls	All FEs	No US-EU Controls	No US-EU FEs	OECD Controls	OECD FEs	Non-OECD Controls	Non-OECD FEs	Non-Steel Controls	Non-Steel FEs
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dispute	.109 (.086)	-0.90 (.171)	.056 (.114)	-.089 (.265)	-.016 (.076)	-.052 (.033)	.122 (.107)	-.249 (.210)	.112 (.090)	-.080 (.178)
Polity	-.039 (.037)		-.015 (.019)		-.080 (.058)		-.008 (.006)		-.039 (.037)	
ln(PC GDP)	2.841 (.153)***		2.759 (.156)***		2.599 (.374)***		2.690 (.156)***		2.827 (.153)***	
Respondent	-.238 (.062)***		-.320 (.030)***		-.120 (.035)***		-.305 (.029)***		-.235 (.065)***	
Crisis	-.060 (.039)		-.070 (.043)*		.005 (.027)		-.111 (.023)***		-.062 (.039)	
N	39740	49544	25366	31397	25700	25722	14040	23822	38667	48199

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This table reports estimates from rerunning the models found in Table 2 of the original manuscript. In these models, we restrict our sample to only those disputes concluding before the year 2000.

Table A5: Replication of Table 2 with sample restricted to disputes ending before 2002

	All Controls	All FEs	No US-EU Controls	No US-EU FEs	OECD Controls	OECD FEs	Non-OECD Controls	Non-OECD FEs	Non-Steel Controls	Non-Steel FEs
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dispute	.145 (.084)*	.146 (.125)	.172 (.088)*	.178 (.158)	.011 (.050)	.108 (.084)	.217 (.119)*	.096 (.181)	.157 (.082)*	.141 (.136)
Polity	-.044 (.024)*		-.033 (.021)		-.116 (.039)***		-.032 (.022)		-.040 (.024)*	
ln(PC GDP)	3.408 (.253)***		3.392 (.269)***		3.093 (.311)***		3.553 (.475)***		3.385 (.251)***	
Respondent	-.254 (.055)***		-.313 (.040)***		-.144 (.036)***		-.309 (.036)***		-.266 (.052)***	
Crisis	-.032 (.026)		-.037 (.027)		-.016 (.023)		-.065 (.034)*		-.033 (.026)	
N	70059	92027	52136	66238	47530	50738	22529	41289	65673	83434

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This table reports estimates from rerunning the models found in Table 2 of the original manuscript. In these models, we restrict our sample to only those disputes concluding before the year 2002.

Table A6: Replication of Table 2 with sample restricted to disputes ending before 2005

	All Controls	All FEs	No US-EU Controls	No US-EU FEs	OECD Controls	OECD FEs	Non-OECD Controls	Non-OECD FEs	Non-Steel Controls	Non-Steel FEs
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dispute	.134 (.077)*	.171 (.114)	.152 (.086)*	.203 (.140)	.004 (.034)	.106 (.065)	.234 (.125)*	.162 (.180)	.139 (.079)*	.167 (.123)
Polity	-.018 (.036)		-.009 (.036)		-.111 (.045)**		-.001 (.041)		-.013 (.038)	
ln(PC GDP)	3.405 (.242)***		3.386 (.251)***		3.065 (.297)***		3.563 (.420)***		3.362 (.234)***	
Respondent	-.248 (.055)***		-.296 (.057)***		-.134 (.030)***		-.324 (.049)***		-.255 (.055)***	
Crisis	-.033 (.026)		-.038 (.027)		-.016 (.022)		-.067 (.032)**		-.035 (.026)	
N	77617	101844	59230	75159	52588	56115	25029	45729	73015	92973

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This table reports estimates from rerunning the models found in Table 2 of the original manuscript. In these models, we restrict our sample to only those disputes concluding before the year 2005.

Table A7: Replicaton of Table 2 excluding disputes with average products > 26

	All Controls	All FEs	No US-EU Controls	No US-EU FEs	OECD Controls	OECD FEs	Non-OECD Controls	Non-OECD FEs	Non-Steel Controls	Non-Steel FEs
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dispute	.040 (.087)	-.003 (.095)	.017 (.099)	.040 (.116)	-.044 (.060)	-.063 (.049)	.101 (.239)	.003 (.167)	.034 (.089)	.011 (.101)
Polity	-.012 (.009)		-.012 (.009)		-.015 (.084)		-.012 (.008)		-.013 (.009)	
ln(PC GDP)	2.841 (.423)***		2.855 (.413)***		2.576 (.497)***		2.952 (.466)***		2.787 (.408)***	
Respondent	-.180 (.037)***		-.196 (.037)***		-.055 (.069)		-.231 (.030)***		-.177 (.038)***	
Crisis	-.038 (.019)**		-.042 (.019)**		-.031 (.017)*		-.039 (.023)*		-.039 (.019)**	
N	1597122	2219168	1530457	2082221	461451	513507	1135671	1705661	1500102	2085106

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This table reports estimates from rerunning the models found in Table 2 of the original manuscript. The models reported here exclude disputes with more than 26 (6-digit) targeted products. This sampling restriction confirms that the number of products targeted in a dispute is not driving our core result.

Table A8: Replication of Table 2 excluding disputes with average products > 100

	All Controls	All FEs	No US-EU Controls	No US-EU FEs	OECD Controls	OECD FEs	Non-OECD Controls	Non-OECD FEs	Non-Steel Controls	Non-Steel FEs
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dispute	.048 (.092)	.004 (.097)	.026 (.106)	.048 (.119)	-.047 (.061)	-.064 (.050)	.137 (.261)	.020 (.175)	.043 (.095)	.019 (.103)
Polity	-.012 (.009)		-.012 (.009)		-.015 (.084)		-.012 (.008)		-.013 (.009)	
ln(PC GDP)	2.842 (.422)***		2.855 (.412)***		2.574 (.496)***		2.953 (.466)***		2.788 (.407)***	
Respondent	-.180 (.037)***		-.197 (.037)***		-.055 (.069)		-.231 (.031)***		-.177 (.038)***	
Crisis	-.038 (.019)**		-.042 (.019)**		-.030 (.017)*		-.039 (.023)*		-.039 (.019)**	
N	1598676	2220791	1531724	2083488	462038	514163	1136638	1706628	1501509	2086582

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This table reports estimates from rerunning the models found in Table 2 of the original manuscript. The models reported here exclude disputes with more than 100 (6-digit) targeted products. This sampling restriction confirms that the number of products targeted in a dispute is not driving our core result.

Table A9: Replication of Table 2 excluding disputes with average products > 750

	All Controls	All FEs	No US-EU Controls	No US-EU FEs	OECD Controls	OECD FEs	Non-OECD Controls	Non-OECD FEs	Non-Steel Controls	Non-Steel FEs
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dispute	.021 (.089)	-.002 (.092)	.001 (.106)	.040 (.117)	-.054 (.060)	-.064 (.045)	.067 (.224)	.010 (.167)	.017 (.094)	.013 (.099)
Polity	-.012 (.009)		-.012 (.009)		-.016 (.085)		-.012 (.008)		-.013 (.009)	
ln(PC GDP)	2.845 (.422)***		2.859 (.412)***		2.570 (.495)***		2.957 (.466)***		2.792 (.407)***	
Respondent	-.182 (.037)***		-.198 (.037)***		-.055 (.068)		-.233 (.030)***		-.179 (.038)***	
Crisis	-.039 (.019)**		-.042 (.019)**		-.031 (.017)*		-.039 (.023)*		-.039 (.019)**	
N	1604440	2226836	1535299	2087063	465318	517724	1139122	1709112	1506307	2091381

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This table reports estimates from rerunning the models found in Table 2 of the original manuscript. The models reported here exclude disputes with more than 750 (6-digit) targeted products. This sampling restriction confirms that the number of products targeted in a dispute is not driving our core result.

Table A10: Repliation of Table 2 excluding disputes about exports

	All Controls	All FEs	No US-EU Controls	No US-EU FEs	OECD Controls	OECD FEs	Non-OECD Controls	Non-OECD FEs	Non-Steel Controls	Non-Steel FEs
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dispute	.137 (.075)*	.092 (.097)	.131 (.089)	.131 (.120)	-.021 (.057)	.044 (.056)	.233 (.115)**	.067 (.141)	.134 (.079)*	.094 (.105)
Polity	-.012 (.009)		-.012 (.009)		-.015 (.084)		-.012 (.008)		-.013 (.009)	
ln(PC GDP)	2.847 (.421)***		2.859 (.411)***		2.567 (.494)***		2.957 (.465)***		2.793 (.406)***	
Respondent	-.185 (.038)***		-.200 (.037)***		-.057 (.069)		-.236 (.031)***		-.181 (.038)***	
Crisis	-.039 (.019)**		-.042 (.019)**		-.031 (.017)*		-.039 (.023)*		-.039 (.019)**	
N	1606257	2228653	1536050	2087814	467135	519541	1139122	1709112	1508124	2093198

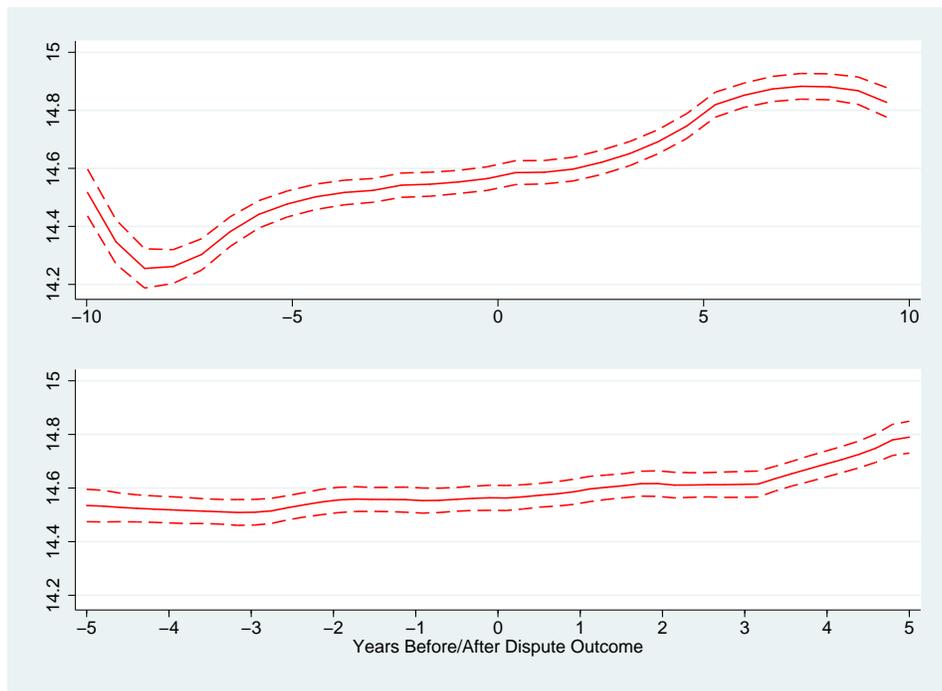
This table reports estimates from rerunning the models found in Table 2 of the original manuscript. Here, we exclude disputes about export-oriented policies. These disputes may be unlikely to shape imports, which are our core outcome of interest. Excluding these disputes ensures that our null finding is not dependent on cases that do not affect import levels.

Table A11: Replication of Table 3 excluding disputes about exports

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dispute (Lag 1)	.107 (.081)	.089 (.092)										
Dispute (Lag 2)			.075 (.080)	.092 (.079)								
Dispute (Lag 3)					.036 (.081)	.080 (.072)						
Dispute (Lag 4)							.035 (.087)	.056 (.066)				
Dispute (Lag 5)									.011 (.102)	.026 (.063)		
Dispute (Lag 6)											-.040 (.127)	-.007 (.059)
Polity	-.012 (.009)		-.012 (.009)		-.012 (.009)		-.011 (.008)		-.010 (.008)		-.008 (.008)	
ln(PC GDP)	2.846 (.421)***		2.845 (.421)***		2.845 (.422)***		2.929 (.414)***		3.138 (.336)***		3.300 (.281)***	
Respondent	-.184 (.037)***		-.184 (.037)***		-.182 (.037)***		-.213 (.040)***		-.264 (.056)***		-.284 (.071)***	
Crisis	-.039 (.019)**		-.039 (.019)**		-.039 (.019)**		-.033 (.020)*		-.021 (.020)		-.023 (.017)	
N	1606257	2155821	1606257	2074688	1606257	1978385	1555644	1877994	1474646	1770796	1368881	1660351

This table reports estimates from rerunning the models found in Table 3 of the original manuscript. Here, we exclude disputes about export-oriented policies. These disputes may be unlikely to shape imports, which are our core outcome of interest. Excluding these disputes ensures that our null finding is not dependent on cases that do not affect import levels.

Figure A1: Import Values by Year Before and After Dispute Conclusion



Smoothed value of  $\log(\text{imports})$  for country-products. Top pane is for 10 years prior to and after a dispute's outcome. The bottom pane is for plus or minus five years. Dashed lines are 95% confidence intervals.

Table A12: Replication of Table 2 using trade 2 years prior to dispute years as reference

	All Controls	All FEs	No US-EU Controls	No US-EU FEs	OECD Controls	OECD FEs	Non-OECD Controls	Non-OECD FEs	Non-Steel Controls	Non-Steel FEs
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dispute	.180 (.076)**	.133 (.098)	.216 (.076)***	.142 (.124)	.027 (.046)	.081 (.078)	.299 (.100)***	.109 (.157)	.191 (.076)**	.114 (.105)
Polity	.011 (.045)		.027 (.046)		-.123 (.032)***		.047 (.056)		.018 (.047)	
ln(PC GDP)	3.532 (.279)***		3.530 (.303)***		3.176 (.323)***		3.678 (.486)***		3.488 (.272)***	
Respondent	-.316 (.066)***		-.403 (.042)***		-.166 (.036)***		-.416 (.050)***		-.322 (.065)***	
Crisis	-.022 (.027)		-.025 (.029)		-.012 (.022)		-.047 (.039)		-.023 (.027)	
N	74165	88016	55937	64353	50277	54280	23888	33736	70049	80481

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This table reports estimates from rerunning the models found in Table 2 of the original manuscript. In these models, our sample includes observations 2 years prior to the initiate of a dispute. This allows us to confirm that our core finding is not an artifact of pre-dispute trade levels.

Table A13: Replication of Table 2 using trade 3 years prior to dispute years as reference

	All Controls	All FEs	No US-EU Controls	No US-EU FEs	OECD Controls	OECD FEs	Non-OECD Controls	Non-OECD FEs	Non-Steel Controls	Non-Steel FEs
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dispute	.168 (.071)**	.118 (.115)	.195 (.071)***	.128 (.146)	.034 (.040)	.085 (.068)	.260 (.101)***	.039 (.172)	.174 (.072)**	.104 (.124)
Polity	.0001 (.040)		.014 (.040)		-.122 (.033)***		.028 (.045)		.006 (.041)	
ln(PC GDP)	3.521 (.274)***		3.520 (.298)***		3.178 (.323)***		3.666 (.482)***		3.476 (.266)***	
Respondent	-.308 (.061)***		-.387 (.041)***		-.168 (.033)***		-.400 (.049)***		-.312 (.061)***	
Crisis	-.020 (.027)		-.024 (.028)		-.012 (.023)		-.044 (.041)		-.022 (.028)	
N	76183	93665	57554	68655	51236	55338	24947	38327	71746	85803

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This table reports estimates from rerunning the models found in Table 2 of the original manuscript. In these models, our sample includes observations 3 years prior to the initiate of a dispute. This allows us to confirm that our core finding is not an artifact of pre-dispute trade levels.

Table A14: Replication of Table 2 using trade 5 years prior to dispute years as reference

	All Controls	All FEs	No US-EU Controls	No US-EU FEs	OECD Controls	OECD FEs	Non-OECD Controls	Non-OECD FEs	Non-Steel Controls	Non-Steel FEs
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dispute	.159 (.069)**	.134 (.108)	.186 (.071)***	.161 (.136)	.028 (.033)	.094 (.057)*	.256 (.097)***	.084 (.160)	.164 (.070)**	.127 (.116)
Polity	-.015 (.039)		-.003 (.039)		-.116 (.039)***		.004 (.045)		-.009 (.041)	
ln(PC GDP)	3.450 (.248)***		3.454 (.263)***		3.130 (.303)***		3.569 (.416)***		3.415 (.241)***	
Respondent	-.279 (.058)***		-.351 (.037)***		-.150 (.031)***		-.364 (.038)***		-.288 (.055)***	
Crisis	-.025 (.026)		-.029 (.027)		-.013 (.022)		-.050 (.037)		-.026 (.027)	
N	79118	101279	60115	73762	52497	56783	26621	44496	74390	92791

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This table reports estimates from rerunning the models found in Table 2 of the original manuscript. In these models, our sample includes observations 5 years prior to the initiate of a dispute. This allows us to confirm that our core finding is not an artifact of pre-dispute trade levels.