Supplemental Appendix:
Barriers to Trade:
How Border Walls Affect Trade Relations

October 15, 2019

Abstract

This supplemental appendix contains additional results and discussions that are not included in the main manuscript for reasons of space. Specifically, we include the following additional discussion and results:

1. We report the results of Oster's test for sensitivity to selection on unobservables across three of our models and explain the test more fully than is possible in the main text;
2. we show that there are temporal changes in the strength of the negative effect of walls on trade flows, which are consistent with our argument that barriers are increasingly associated with general border security programs in the post-Cold War era;
3. we show that there are modest "second order" border wall effects, as states that erects a barrier also experience depressed trade with non-contiguous states;
4. we explore whether wall building dyads are systematically different from non-wall building dyads in a way that might make our results less substantively important; specifically we explore whether *contiguous states that have a constructed physical barrier between them*:
   (a) trade comparatively little relative to non-wall building dyads to begin with,
   (b) have systematically different bilateral trade policies relative to non-wall building neighbors,
   (c) or, they rely less on land-based trade relative to non-wall-building neighbors.
5. we estimate an interaction between *Barrier Case Contiguous Neighbors* and *Income Inequality* and also present density plots for both the logged version of *Income Inequality* that we employ in the paper and a raw unlogged version that we do not employ;

6. we do more in-depth comparison of the three main data sources on wall-building, Hassner and Wittenberg (2015), Avdan and Gelpi (2017), and Carter and Poast (2017).

7. we report results using the Hassner and Wittenberg (2015) data or the Avdan and Gelpi (2017) data rather than the Carter and Poast (2017), which is what we primarily use in our main analyses. The direction of the effect is quite similar, although the standard errors are larger.

The numbers above correspond to the section heading numbers, to facilitate quickly finding each set of additional results.

1 Sensitivity Analysis: Border Walls and Selection on Unobservables

All of our results rely upon the standard assumption that our key result is not sensitive to selection on unobservables, i.e., omitted variable bias. Like many assumptions that are required to estimate regression models, there is no definitive way to rule out the idea that the estimated relationship between border barriers and trade flows is biased by the exclusion of key unobservables from our specifications. Nonetheless, recent econometric work by Altonji, Elder and Taber (2005) and more specifically Oster (2017) provides a credible test by which we can put explicit bounds around how sensitive our key results are to selection on unobservables. Specifically, to place bounds on the bias of an estimate caused by omitted variables, the formal test proposed by Oster (2017) uses information from changes in both point estimates and R^2 values derived from comparison of the unconditional estimated impact of *Border Barrier* in a univariate regression to the estimated effect of *Border Barrier* after conditioning on other observable covariates, i.e., all the additional covariates we include in our model specifications. The procedure allows researchers to evaluate the degree to which unobservable factors are likely to bias their estimates of the quantity of interest under the fairly reasonable assumption that the degree of selection on unobservables is proportional to selection on observables in the "full" model with all controls. Additionally, the researcher must specify a "maximum" value for R^2 that can be realistically expected from a model with all unobservables included. Given these assumptions and two regressions, i.e., a bivariate regression and the "full" model, we can calculate two related quantities of interest: 1.) the $\hat{\beta}$ that would hold if we assume that the degree of selection on unobservables is proportional to selection on all of our observable control variables, i.e., $\delta = 1$, and 2.) the degree of selection, δ, that would be necessary to reduce our estimated $\hat{\beta}$ for *Border Barrier* to $\hat{\beta} = 0$. Both of these quantities depend on us specifying a value for how high R^2 could be with the "best possible" specification, or R^2_{max}.

Oster (2017) examines a sample of results based on economics articles with randomized data to suggest a "rule of thumb" threshold above which a result should be considered robust (see Oster (2017, 13–17) for details). She suggests considering estimated coefficients that survive 1.3 times the

R^2 associated with the regression containing only the full set of observed controls to be robust. We use this rule of thumb as well as higher thresholds here to explore how sensitive our key estimates are to omitted variable bias. Our focus is on the within-dyad R^2, as this is the variation left for our high-dimensional fixed effects gravity models to explain. We also focus on the OLS specifications that include only neighboring countries, as these are our baseline models and implementation and interpretation of the results of the tests are quite straightforward. We implement the test by first demeaning out the country-year fixed effects, then estimating OLS with directed dyadic fixed effects. This approach works well for us here as all we need is the correct coefficients and the correct within directed-dyad R^2 value, but do not use information over the standard errors, for example.

We report the results of tests for the sensitivity to selection on unobservables for our estimates of *Border Barrier* over the three different model specifications shown in table 1. The test is not easily implemented using the **reghdfe** command that we implement for OLS models in the main text, so we take an equivalent approach here that allows us to perform the test on within-directed dyad variation. The regressions in table 2 are performed by first conditioning out country-year fixed effects for both importer and exporter by demeaning all of the variables by each indicator. We then perform directed dyadic fixed effects regression with the twice demeaned data. This approach yields the same regression coefficients and also yields the correct within-directed dyad R^2, which are all that is required to correctly implement Oster's test.[1] The regression coefficients, shown in table 2 are substantively no different than those reported in the main text using **reghdfe**, although there are very slightly differences due to the different estimation the high-dimensional fixed effects. This is also immaterial as the test is implemented on the coefficients estimated from as just described, which are substantively not different from those reported in the main text.

Tables 2–4 all report the degree of selection on unobservables we would have to see relative to selection on observables, i.e., control variables, to make our estimated β for *Border Barrier* disappear. This quantity, δ, is quite large at the "rule of thumb" value of $R^2_{max} = R^2 \times 1.3$ suggested as a threshold for robustness by Oster, and is always greater than 1 even when we specify R^2_{max} at

[1] The standard errors are incorrect as is the overall R^2, as the we are accounting for neither the needed degrees of freedom correction nor all of the variation that is "fixed effected" out by the country-year indicators. Again, this does not matter for our implementation of the test.

Table 1: Regression Models for Oster Tests

	Model I (OLS)	Model II (OLS)	Model III (OLS)
Physical Barrier	-0.772**	-0.662**	-0.460*
	(0.30)	(0.28)	(0.26)
Democratic Dyad	-0.101	-0.139	-0.122
	(0.11)	(0.11)	(0.12)
Defensive Alliance	0.201**	0.200**	0.136**
	(0.07)	(0.07)	(0.06)
Strategic Rivalry	-0.247*	-0.239*	-0.182
	(0.14)	(0.13)	(0.13)
Territorial Dispute	-0.229	-0.174	-0.074
	(0.16)	(0.15)	(0.14)
Income Inequality		-0.104	-0.104
		(0.07)	(0.06)
Preferential Trade Agreement			0.225**
			(0.08)
Common Currency			0.142
			(0.19)
Both in GATT/WTO			0.089
			(0.11)
Country-Year Fixed Effects	Yes	Yes	Yes
Directed-Dyadic Fixed Effects	Yes	Yes	Yes
Years Included	Yes	Yes	Yes
N	47746	44140	40002

Standard errors clustered
by dyad in parentheses
** $p < .05$; * $p < .10$

Table 2: Oster Tests for Effect of *Border Barrier* on Trade in Model 1

$R^2_{max} =$	$R^2 \times 1.3$	$R^2 \times 3$	$R^2 \times 5$	$R^2 \times 10$
$\beta =$	-0.83	-0.70	-0.55	-0.13
$\delta =$	16.45	4.63	2.51	1.17
Controlled $\beta =$ -0.86				
Uncontrolled $\beta =$ -0.87				

Table 3: Oster Tests for Effect of *Border Barrier* on Trade in Model 2

$R^2_{max} =$	$R^2 \times 1.3$	$R^2 \times 3$	$R^2 \times 5$	$R^2 \times 10$
$\beta =$	-0.81	-0.73	-0.64	-0.39
$\delta =$	29.59	7.19	3.80	1.75
Controlled $\beta =$ -0.82				
Uncontrolled $\beta =$ -0.84				

Table 4: Oster Tests for Effect of *Border Barrier* on Trade in Model 3

$R^2_{max} =$	$R^2 \times 1.3$	$R^2 \times 3$	$R^2 \times 5$	$R^2 \times 10$
$\beta =$	-0.69	-0.61	-0.52	-0.27
$\delta =$	29.35	6.49	3.39	1.54
Controlled $\beta =$ -0.70				
Uncontrolled $\beta =$ -0.72				

a much higher threshold, i.e., $R^2_{max} = R^2 \times 10$. Note that the results in table 4 are those that we report in the main text as they are based on the most fully specified model that includes variables such as *Preferential Trade Agreement* that have large estimated effects. Comparison across the tables indicate that the results in table 4 are the most robust of all of the regressions, which makes sense as model III only includes post-1947 observations, which captures the period where the vast majority of border barriers were built.

2 Temporal Change in the Effect of Walls

We argue in the theory section that two related factors lead trade flows to decrease in the face of barrier construction. First, increasingly fortified (and usually also militarized) border areas lead to increased flows of illicit goods into ports of entry, i.e., a substitution effect. Second and relatedly, countries that built barriers to block illicit flows pair them with general border security and inspection programs to police the border region and more specifically to filter out illicit flows of people and goods at ports of entry. We built on recent work on border walls in making these arguments, as there is compelling evidence that the uptick in barrier construction in the post-Cold War era is primarily motivated by concerns about cross-border smuggling (Hassner and Wittenberg, 2015; Carter and Poast, 2017). However, border fortification is not a feature of world politics exclusive to the post-Cold War era; in addition to well-known historical instances such as the Great Wall of China, for example, the Soviet Union engaged in it on its frontiers with newly formed states such as Poland in the early 1920s, as did France when it completed the Maginot line on its border with Germany several years later. However, the overriding motivation for barrier construction being concerns about illicit cross-border flows, as opposed to more traditional military security concerns, is relatively new, as the Maginot line's purpose of blocking the German army suggests. Moreover, the coincidence of barrier construction and general border security programs meant to stringently filter individuals and goods at the border are very prominent features of the contemporary boom in border fortification, but are somewhat less prominent during the Cold War, and much less prominent prior to the Cold War. Along these lines, Brown (2010, 26–27) argues in her highly influential book "for the validity of conceiving the new [early 21st century] walls as a single historical phenomenon... Each of the new walls can be seen to issue from certain pressures on nations and states exerted by the process of globalization."

One interesting implication of this idea that the recent uptick in wall-building has distinct roots relative to earlier historical periods is that border walls should exert greater negative effects on trade in the post-Cold War than they did in earlier periods.[2] Specifically, the argument that wall-building in recent decades is unique in the overriding concern about illicit cross-border flows and general border security implies that we should find differential effects of walls on trade during this time period relative to earlier periods. Our argument is based on the idea that walls are associated with heightened general security programs that aim to filter the individuals and goods that cross a border, which suggests that the effects of walls on trade should differ in earlier periods of time when the motivations for border fortification were, at minimum, more varied.

Table 5 provides two different modeling approaches to assess the idea that the effect of border barriers on trade has changed across time. Models I and II both include an interaction between *Physical Barrier* and *Time*, which is a year counter that starts at zero in 1900, the first year for which we have trade data.[3] Models III and IV provide another approach, where we interact *Post-Cold War*, a binary variable that takes a value of one after 1989, with *Physical Barrier*.[4] We choose to compare the post-Cold War period with the entire period 1900–1989, as this provides a baseline time period where there are a fair number of walls to compare to the post-Cold War period.[5]

The results from table 5 suggest that the estimated effect of wall-building on trade flows has become more negative across time. Specifically, the interaction *Physical Barrier X Time* is negative and significant in both models I and II. The positive and significant estimate for *Physical Barrier* is not meaningful to interpret as it is an estimate of the effect of *Physical Barrier* in 1900, a year in which there were no barriers in place according to our data (Norway built one on its border with Sweden in 1901). Models III and IV also report large, negative and significant coefficients for

[2]We thank an anonymous reviewer for suggesting this idea to us.

[3]The only difference between models I and II is that we include three additional variables in model II, all of which are only available post-World War II. The same is true of models III and IV.

[4]We do not include the constituent terms for either *Time* or *Post-Cold War* as they are subsumed (and collinear) with the country-year fixed effects.

[5]That being said, the results are substantively similar if we analyze three time periods, 1900–1945, 1946–1989, and 1989–present.

Table 5: Do Walls have Differential Effect Across Time?

	Model I (OLS)	Model II (OLS)	Model III (OLS)	Model IV (OLS)
Physical Barrier X Time	-0.032**	-0.032**		
	(0.01)	(0.01)		
Physical Barrier X Post-Cold War			-0.826	-1.026**
			(0.52)	(0.52)
Physical Barrier	1.908**	2.127**	-0.267	0.109
	(0.59)	(0.71)	(0.38)	(0.39)
Democratic Dyad	-0.137	-0.118	-0.115	-0.095
	(0.11)	(0.12)	(0.11)	(0.12)
Defensive Alliance	0.212**	0.147**	0.202**	0.136**
	(0.07)	(0.06)	(0.06)	(0.06)
Strategic Rivalry	-0.248*	-0.196	-0.246*	-0.189
	(0.13)	(0.13)	(0.13)	(0.13)
Territorial Dispute	-0.184	-0.087	-0.184	-0.088
	(0.14)	(0.13)	(0.15)	(0.14)
Income Inequality	-0.089	-0.092	-0.095	-0.091
	(0.07)	(0.06)	(0.07)	(0.06)
Preferential Trade Agreement		0.209**		0.223**
		(0.07)		(0.07)
Common Currency		0.129		0.125
		(0.19)		(0.19)
Both in GATT/WTO		0.076		0.091
		(0.11)		(0.11)
Country-Year Fixed Effects	Yes	Yes	Yes	Yes
Directed Dyadic Fixed Effects	Yes	Yes	Yes	Yes
N	44140	40002	44140	40002
Years Included	1919–2011	1948–2011	1919–2011	1948–2011

Standard errors clustered
by dyad in parentheses
** $p < .05$; * $p < .10$

Physical Barrier X Post-Cold War, which indicate that the negative effect of border walls on trade is significantly heightened in the post-Cold War era when compared to the period before 1990. While the negative coefficient on *Physical Barrier* is negative and not of negligible magnitude, the standard errors for the estimates are so large that it is difficult to draw a conclusion about the pre-1990 effects of walls.[6] While these results are not definitive evidence in favor of our arguments, they are consistent with it.

3 Are There "Second Order" Border Barrier Effects?

One reason that a state implements policies aimed at stopping trade between itself and a neighbor is worsening political relations. In other words, some unmeasured aspect of dyadic hostility or policy drift might drive the relationship between wall building and trade flows. One take on this argument is that the reduced trade flows should be rather specific to the neighbor targeted with the wall. If it is not the wall or its associated security programs that reduce trade, then there is little reason to think that we will see effects on trade with a large number of third parties. We leverage the idea here that the substitution argument suggests that border fortification and the associated security program should in some way affect all trade flows. In many cases imports from other states will also cross checkpoints or ports of entry along the fortified border, which we would expect to be highly securitized, even if in lower volume. For example, the border fortifications between Turkey and Bulgaria in started in 2014 should not only affect their bilateral trade flows but plausibly also impact Turkish trade relations with countries such as Serbia and Hungary, who do not border Turkey but might find routes over its European land border with Bulgaria a natural route for commercial goods. Moreover, the arguments that more illicit flows divert to ports of entry when border fortifications increase the difficulty of crossing elsewhere and that border barriers (particularly in the post-Cold War era) are generally associated with increased security programs suggest that trade costs should be heightened to some degree at all ports of entry. We do not expect these "second-order" wall effects to be as large as those for contiguous dyads that share a fortified border, but our explanation

[6] Note that the coefficient for the constituent term *Physical Barrier* identifies the pre-1990 effect when the *Post Cold War* variable is equal to zero.

of the findings in the main text does imply they should be present in many cases.

To assess these "second-order" effects we again use directed dyadic data that includes every pair of potentially trading countries.[7] Furthermore, rather than measuring border walls when they are placed on a shared border, we construct a variable that indicates whether either state in a given non-contiguous dyad has a border wall on at least one of its borders, i.e., the *Non-Contiguous Barrier Case* variable. We focus first on non-contiguous dyads as we think it is much less plausible that our estimates in these cases could be a function of worsening bilateral relations, of which a wall is just a symptom. In other words, we are skeptical that a state building a wall on one of its borders is doing so in response to worsening bilateral relations or trade concerns with all of its non-contiguous trade partners. Thus, we view analysis of the *Non-Contiguous Barrier Case* variable as an important additional test of whether our argument that barrier construction and the associated security programs reduce trade. Finally, we note that these estimates are are likely attenuated somewhat, as we are not accounting for how stringent a security program is implemented across relevant ports of entry. Thus, this measurement strategy probably includes a number of cases that should work against finding an effect.

Table 6 contains the results of two Poisson gravity models of trade flows, which again include importer-year, exporter-year and directed dyadic fixed effects. The key variable of interest is *Non-Contiguous Barrier*. The other variables in the specification are very similar to those we included in prior specifications. The results in table 6 suggest that border barriers have modest negative effects on trade flows among non-contiguous dyads. Specifically, the coefficient on *Non-Contiguous Barrier* is negative and statistically significant in both specifications. This result suggests that physical border walls are associated with second order trade effects, as they do have significant negative effect on trade with third countries.

It is also important to note that our estimate of the *Physical Barrier* variable, the same measure of the presence of a physical barrier on a shared dyadic border analyzed in table 1 in the main text, remains negative and statistically significant. Thus, we again recover the same estimated effect we identified in the main text for contiguous states when we include all directed dyads.

[7] Given the potential for zero trade values to be problematic in this "full" sample of directed dyads we again estimate Poisson models with country-year fixed effects.

Table 6: Exploring Non-Contiguous States and Contiguous States without Barriers

	Model I (Poisson)	Model II (Poisson)	Model III (Poisson)	Model IV (Poisson)
Non-Contiguous Barrier	-0.108**	-0.078**		
	(0.04)	(0.05)		
Barrier Case Contiguous Neighbors			-0.023	-0.051
			(0.07)	(0.07)
Contiguous Barrier	-0.166**	-0.148**	-0.135**	-0.137**
	(0.06)	(0.06)	(0.06)	(0.07)
Democratic Dyad	-0.108**	-0.111**	-0.110**	-0.113**
	(0.04)	(0.04)	(0.04)	(0.04)
Defensive Alliance	0.422**	0.361**	0.423**	0.362**
	(0.07)	(0.06)	(0.07)	(0.06)
Strategic Rivalry	-0.155**	-0.130**	-0.155**	-0.129**
	(0.05)	(0.04)	(0.05)	(0.04)
Preferential Trade Agreement		0.220**		0.220**
		(0.03)		(0.03)
Common Currency		0.063		0.064
		(0.04)		(0.04)
Both in GATT/WTO		-0.075		-0.076
		(0.06)		(0.06)
Country-Year Fixed Effects	Yes	Yes	Yes	Yes
Directed Dyadic Fixed Effects	Yes	Yes	Yes	Yes
N	1096606	916556	1096606	916556
Years Included	1919–2011	1948–2011	1919–2011	1948–2011

Standard errors clustered
by dyad in parentheses
** $p < .05$; * $p < .10$

An even more difficult assessment of the general effects of border barriers and the security policies associated with them is to examine trade with a wall-building state's other contiguous neighbors, e.g., the U.S. and Canada. Contiguous states are typically among each others' most important trading partners, and the walled border is much less plausibly relevant for a state that shares a different (unwalled) border than it is for many non-contiguous states. Accordingly, we assess the effects of walls on trade with non-walled contiguous neighbors in models III and IV in table 6. *Barrier Case Contiguous Neighbors* is a binary variable that takes a value of one for contiguous dyads where one of the two states has a physical barrier at one (or more) of it's borders, but not on the border with the neighbor in question, e.g., the U.S.-Canada dyad.[8] The estimated effect of being neighbors with a barrier building state remains negative across both specifications, although the magnitude of the coefficients is small and the estimates are not statistically significant. Thus, it does not seem to be the case that neighbors without a wall generally experience substantial decreases in trade similar to that of neighbors with a wall.[9]

4 Substantive Relevance: Are Wall Builders Different?

The analyses in the main text show that wall construction does appear to reduce trade between countries. However, one might contend that this is of little substantive relevance because two states that have walls between one another are systematically different from contiguous dyads without walls. Specifically, contiguous states that have a constructed physical barrier between one another could differ from other contiguous states for three reasons: they trade little with one another to begin with; they have systematically different bilateral trade policies; or they have less reliance on land-based trade. While evidence supporting either of these possibilities do not undermine the above findings, they could minimize the scope and policy relevance of our findings. We demonstrate that there is little empirical support for either of these three ideas here.

[8] Thus, this variable differs from *Non-Contiguous Barrier* as that variable only measures non-contiguous dyads where at least one state in the pair has a barrier, while *Barrier Case Contiguous Neighbors* only measures contiguous dyads where at least one state has a barrier but the shared border has no barrier.

[9] See the appendix for models with an interaction between *Barrier Case Contiguous Neighbors* and *Income Inequality.* The estimates of the interaction are very similar to the unconditional estimates for *Barrier Case Contiguous Neighbors* in models III and IV.

First, it could be the case that the set of wall-builders trade an unusually small amount with their contiguous neighbors. While it is generally true that states' most important trade partners tend to be their neighbors, it might be the case that the set of wall-builders are somewhat anomalous in this regard. The upshot for our findings is that the reduction in trade is real, but that it is simply not that important for the wall-builder because trade was already unusually low with the neighbor across the border for some other set of reasons.

To assess this idea we again analyze directed dyadic trade data with all directed dyads regardless of whether they are contiguous or not. We do this so that we can estimate the effect of contiguity in dyads that at some point have a constructed barrier, *Contiguous – Barrier*, as well as in contiguous dyads that never experienced a constructed barrier, *Contiguous – No Barrier*.[10] The first two columns in Table 7, i.e., Models I–II, contain the results where we compare contiguous dyads that do experience a barrier at some point to those that never do. Note that models I–II of Table 7 capture whether a dyad ever had a barrier in place rather than conditioning on only the years in which the barrier was in place (as in Table 1 in the main text). We do this to assess whether these dyads are generally different across the whole period. In these two models, the results across all models show that there are not great differences, with respect to trade flows, between contiguous dyads that do not build barriers and contiguous dyads that do build barriers. The coefficients on both *Contiguous – No Barrier* and *Contiguous – Barrier* are large, positive and statistically significant, which reflects the known tendency of neighbors to be important trade partners.

Models III–IV in Table 7 are identical to the first two columns except in that we shift our focus to the period of time before a barrier was erected in the contiguous dyads where the border was at some point fortified.[11] Specifically, we include *Contiguous – Pre-Barrier* rather than *Contiguous – Barrier*. We do this to explore the degree to which trade relations between contiguous dyads that later experienced border fortification look "normal" for such pairs. Notice that the estimated coefficients for *Contiguous – Pre-Barrier* are again large, positive and statistically significant at

[10] We do not interact a contiguity variable with a barrier variable although the results are essentially no different. The constituent term for barrier is not estimable in any event as there are not non-contiguous dyads that share a land border barrier.

[11] In order to estimate these contiguity variables, we have to exclude dyadic fixed effects as contiguity does not vary across time. Accordingly, we also include a set of static dyadic variables that are excluded in our prior tables, such as the log of distance between two states' capitals.

any conventional threshold. These results make clear that the set of states that build walls at their border do have significant trade relations with their neighbor.

Second, it could be the case that the set of wall-builders have systematically different trade policies. Tables 8 and 9 show similar assessments of whether directed dyads in which the importing state builds a wall tend to systematically differ in terms of trade policy from those that do not. Table 8 shows the mean percentage of directed dyads with a PTA where the importer builds a wall and compares it to those directed dyads in which the importer does not build a wall. The difference between the two means is not significantly different, and furthermore, the mean percent of dyads with a PTA is *higher* for cases in which a wall is built relative to those in which no wall is built. This certainly does not suggest that the states that build border walls have relatively closed economies or that they are not in important trading relationships with their neighbors across the walled border. The results of table 9 show a similar lack of difference in joint GATT/WTO membership among directed dyads in which the importer builds a wall and those in which no wall is built. In short, there is scant evidence in our analysis of the differences among dyads with walls and those without that border walls emerge when the states in question tend to have relatively closed economies or do not have much of a trade relationship with their neighbor. Again, this provides further evidence that when leaders build border walls, they pay real costs in terms of trade.

Third, it might be the case that the set of wall-building states are not very reliant on land borders for trade, but instead have ample coastline to facilitate maritime transport via ports. Having established that contiguous neighbors that experience border fortification at some point do not experience unusually low levels of trade relative to other contiguous dyads, we now probe the geographic characteristics of wall-building states and compare them to states that do not build walls. Our objective here is to better understand whether and how border fortifiers differ in terms of how much coastal territory they have with shipping ports and numerous non-land boundary points where individuals can enter the country. As we note above when we discuss alternative explanations, it is possible that the trade costs are not that high for wall-builders because they are states whose land borders are not that important for trade flows. Specifically, given that much international trade flows over sea and through sea ports, we assess the possibility that the set of states that have fortified their borders are unusually reliant on seaports for international trade. While this is a difficult proposition to definitively test with data, we can use information on the geographic characteristics of each state to assess how plausible it is.

Table 10 uses difference of means tests to explore whether the states that build walls have a typical or greater than average amount of coastal territory relative to states that do not build border walls. Specifically, the tables show the relationship between wall building and the percentage of a country's area that is coastal territory. We use the NASA National Aggregates of Geospatial Data Collection (NAGDC) to measure coastline territory in a 200km buffer around each country's coast with a sea or ocean. Table 10 shows the mean percentage of coastal territory for states that build border barriers versus those that do not. The table shows that the set of directed dyads in which the importing state builds a border barrier have significantly less coastline relative to the vast majority of states that do not build a border barrier. The difference is clear, as the 35 states that build a wall have a mean of 4.3% of their area covered by coastal territory, while the non-wall building dyads have a mean of about 10% of their area as coastal territory. We conduct t-tests assuming unequal variances, and show that these mean levels are significantly different at any conventional level of statistical significance. While the statistics in table 10 are clearly not definitive, they do suggest that the reason why border walls cut against trade severely in our estimates in table 1 in the main text is that the set of states that build them also tend to have land borders that likely matter for the transport of people and goods to neighboring states (excluding air travel of course).

5 Neighbors without Walls: Do Differences in Wealth Matter?

Models I and II are specifications that include *Barrier Case Contiguous Neighbors*, a binary variable that takes a value of one for contiguous dyads where one of the two states has a physical barrier at one (or more) of it's borders, but not on the border with the neighbor in question, e.g., the U.S.-Canada dyad. These are the same estimates as are reported in models III and IV of table 4 in the main text and we include them here for the sake of comparison to the last two columns of table 11. Models III and IV are similarly specified except that they include an interaction between *Barrier Case Contiguous Neighbors* and *Income Inequality*, where higher values of the latter variable indicate that the disparity between the two neighbors' GDP per capita is increasing. The interaction *Barrier Case Contiguous Neighbors* X *Income Inequality* in models III and IV results in coefficients that are essentially no different from the unconditional estimate for *Barrier Case Contiguous Neighbors* in models I and II. Thus, we don't find any systematic differences in the effect of wall-building on contiguous neighbors that do not share the barrier, e.g., the U.S. and Canada.

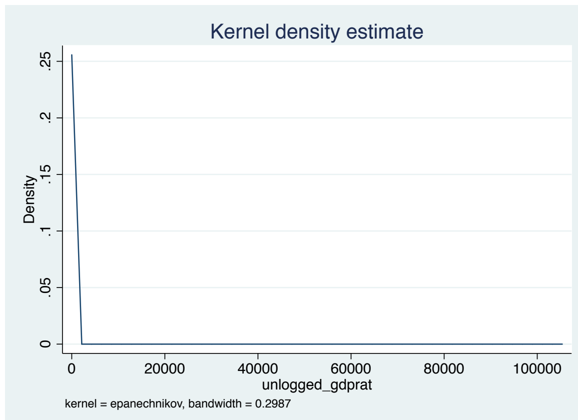
Figure 1 contains two density plots, where 1(a) is an unlogged (raw) directed ratio of two contiguous states' incomes, while 1(b) is the logged directed ratio. The point of showing these two density plots is to demonstrate our reasoning for preferring the logged ratio. While both exhibit some right-skew, the right-skew of the raw version (figure 1(a)) is quite severe, which leads us to prefer the logged version.

6 A Comparison of Walls Data Sources

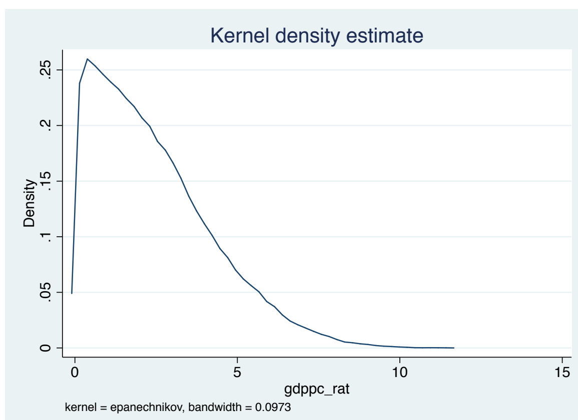
CP code walls constructed between 1800 and 2015. They began with two sources, Sterling's (2009) list of walls and the Global Security Organization's list of "walls, lines, and frontiers."[12] These sources use Keegan's definition of a strategic defense to identify walls, lines, and frontiers. Specifically, Keegan, focusing on the military purposes of fortifications, writes, "[s]trategic defenses may be continuous, as Hadrian's Wall was when kept in repair, or more commonly may comprise individual strongpoints so positioned as to be mutually supporting and to deny avenues of attack to an enemy across a wide front" (Keegan, 1993, 142). CP modify the final clause of the definition: while strategic defense lines can indeed be directed towards denying "avenues of attack to an enemy", they are more broadly intended to prevent entrance by any unwanted entities, be it persons or things (including, of course, a foreign army). Hence, CP refer to all strategic defenses as "walls", setting aside the nuances between 'walls' and 'lines'. Once CP use Sterling and GSO to create their "core list", they turned to Lexis-Nexus, news papers, and the online encyclopedia Wikipedia's list of "Separation Barriers".[13]

[12]Available at `https://www.globalsecurity.org/military/world/walls.htm`. Accessed on November 14, 2011.
[13]Available at `https://en.wikipedia.org/wiki/Separation_barrier` Accessed on December 23, 2011.

Figure 1: Unlogged and Logged Income Inequality Measures



(a) Unlogged Income Inequality Measure



(b) Logged Income Inequality Measure

AG coded walls constructed between 1901 and 2015. While CP use Sterling (2009) and the GSO to develop a core list, AG begin with the International Boundaries Research Unit's (IBRU's) online archives on international borders at the University of Durham. AG performed a keyword search for "walls," "fences," "barriers," and "fortifications" using IBRU's Boundary & Security Bulletin in order to compile their list.[14] While IBRU's published bulletin runs up to 2001, the IBRU's online search also captures current news on borders. Like CP, AG supplemented their "core list" with other sources. AG used boundary-specific news from Borderbase, Wikipedia's list of separation barriers, and cross-checked cases using general internet searches.[15]

The HW dataset is focused on post-1945 wall construction. While they do not provide detailed information in their article on the underlying source material used to construct their wall data, they do provide an indicator of the confidence they have in the reported date of wall construction. However, without the source material it is difficult to know the basis for these confidence estimates so we do not use them here. We do employ the HW data when we combine information from all three sources though, as it helps us to provide a nice check on whether our results are overly dependent on our preferred data source.

Given differences in source material and temporal coverage, one should expect differences in the list of walls identified by the three datasets. While this is the case, there is a great deal of overlap in the walls identified by CP, AP, and HW. Consider the data on post-1945 wall construction (since HW only coded walls constructed after 1945) between Correlates of War identified sovereign states (hence we are not counting, for example, the wall between Morocco and Western Sahara).[16] CP find 46 walls, AG find 55 walls, and HW find 41 walls.

Twenty eight walls were identified by all three sources. These included the Berlin Wall between East and West Germany, the wall between the United States and Mexico, the fortification between Israel and Lebanon, and the barrier between India and Burma. Moreover, all three datasets identify that wall construction has become a more frequent occurrence after the Cold War. This shows that as globalization has increased the interconnectedness between states, the governments of those

[14] Avaialble at `https://www.dur.ac.uk/ibru/publications/bulletin/`.

[15] They also referred to the cases identified by Jones (2012a) and Jellissen and Gottheil (2013) to ensure the keyword search had not missed cases.

[16] CP lists the most walls built between a state and a non-state (8), with HW identifying 5, and AG identifying 1.

states have adopted aggressive strategies to assert control of their border.

An additional twenty four walls were identified by at least two of the sources. CP and AG agree on 13 additional walls that are not found in HW, such as the barrier between Russia and Finland (built in 1947) and the DMZ between North Korea and South Korea. HW likely omit the latter barrier because they consider it a fortification built for explicitly military deterrence purposes, while CP and AG do not omit such barriers from their lists. CP and HW agree on an additional 4 walls that are not found in AG, such as the barrier between Botzwana and Zimbabwe or between Iran and Pakistan. AG and HW agree on 5 more walls that are not found in CP, such as the barrier between Kazakhstan and Krygzstan.

Finally, several walls are identified by only one source. CP have four (4) walls unique to their dataset, such as between the United States territory in Cuba and the the remainder of Cuba.[17] AG have thirteen (12) walls unique to their dataset, such as between Russia and Mongolia and Botswana and Namibia. HW have eight (7) walls unique to their dataset, such as between Bulgaria and Turkey.[18]

Overall, there are differences in the wall coding between the datasets, but none of the datasets appear to be exceptionally different from the others. We use the Carter and Poast data for most our analysis, as their list is comprehensive temporally and uses a definition of border barriers that does not make the coding conditional on what the state seemed to intend, e.g., the North Korea-South Korea wall is included. However, we do check whether the results of our analysis are similar if we also leverage information from the other two data sources. Specifically, we combine the measures using item response theory (IRT), where we implement a latent trait model for dichotomous data. We also use principal component analysis (PCA) to combine the three measures, which produces a very similar measure to the IRT method (their correlation is close to 1). Finally, we also calculate a simpler measure that calculates the percentage of all sources that code a border as having a wall in a given year. Thus, if all three sources agree, this measure takes a value of 1, while it takes values of 0.333 if only one of the three sources codes a wall, 0.666 if two out of three code a wall,

[17] Which, of course, is the wall made famous by the play/movie "A Few Good Men".

[18] They also identified a few cases where it is not clear that there is a border between the two states (e.g. Russia and Austria) or the case might be redundant with a case already identified (Uzbekistan and Kazakhstan is identified as separate from Kazakhstan and Uzbekistan).

and so on. We use the simple proportion measure in the paper as it is correlated with the PCA and IRT at over 0.99 and has a much more intuitive interpretation.

7 Results with Alternative Walls Data Sources

Here, we demonstrate that using the alternative measures of walls from either Hassner and Wittenberg (2015) or Avdan and Gelpi (2017) leads to qualitatively similar results. Specifically, in table 12 we estimate Poisson gravity models with country-year and dyad fixed effects similar to models VI–IX of Table 1 in the main text. We recover a negative coefficient of similar magnitude to that estimated in the main text, although the standard errors are larger for these two measures than for the data from Carter and Poast (2017).

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Table 7: Do Contiguous States with Border Walls Trade Less?

	Model I (Poisson)	Model II (Poisson)	Model III (Poisson)	Model IV (Poisson)
Contiguous – No Barrier	0.630**	0.544**	0.597**	0.512**
	(0.08)	(0.08)	(0.08)	(0.09)
Contiguous – Barrier	0.709**	0.708**		
	(0.16)	(0.16)		
Contiguous – Pre-Barrier			0.654**	0.600**
			(0.15)	(0.16)
Log Distance	-0.726**	-0.635**	-0.744**	-0.650**
	(0.03)	(0.04)	(0.03)	(0.04)
Democratic Dyad	-0.374**	-0.452**	-0.380**	-0.458**
	(0.09)	(0.09)	(0.09)	(0.09)
Defensive Alliance	0.157**	0.064	0.164*	0.077
	(0.09)	(0.08)	(0.09)	(0.08)
Strategic Rivalry	-0.195	-0.181	-0.199	-0.184
	(0.14)	(0.14)	(0.14)	(0.14)
Log Product GDP	0.047**	0.041**	0.048**	0.042**
	(0.01)	(0.01)	(0.01)	(0.01)
Preferential Trade Agreement		0.425**		0.435**
		(0.07)		(0.07)
Common Currency		0.166*		0.159*
		(0.09)		(0.09)
Country-Year Fixed Effects	Yes	Yes	Yes	Yes
Directed Dyad Fixed Effects	No	No	No	No
N	1054175	935459	1054175	935459

Standard errors clustered by dyad in parentheses

** $p < .05$; * $p < .10$

Table 8: Preferential Trade Agreements and Wall Building

	Mean Percent of Dyads with a PTA	95% Conf. Interval
Wall Built = 38 Cases	0.65	[0.62, 0.67]
No Wall Built = 1,050 Cases	0.56	[0.41, 0.70]
All Cases= 1,088 Cases	0.65	[0.62, 0.67]
Ha: diff < 0	Ha: diff $! = 0$	Ha: diff > 0
$Pr(T < t) = 0.8888$	$Pr(\|T\| > \|t\|) = 0.2224$	$Pr(T > t) = 0.1112$

Table 9: GATT/WTO Membership and Wall-Building

	Mean Percent of Dyads with Both in GATT/WTO	95% Conf. Interval
Wall Built = 39 Cases	0.037	[0.041, 0.050]
No Wall Built = 1,207 Cases	0.045	[0.016, 0.057]
All Cases= 1,246 Cases	0.045	[0.040, 0.049]
Ha: diff < 0	Ha: diff $! = 0$	Ha: diff > 0
$Pr(T < t) = 0.7892$	$Pr(\|T\| > \|t\|) = 0.4216$	$Pr(T > t) = 0.2108$

Table 10: Coastal Area and Wall Building

	Mean Percent of Country Area Covered by 200km Coastline Buffer	95% Conf. Interval
Wall Built = 35 Cases	4.3	[2.3, 6.3]
No Wall Built = 1,031 Cases	10.1	[9.1, 11]
All Cases= 1,066 Cases	9.9	[9, 10.8]
Ha: diff $<$ 0 $Pr(T < t) = 1.0000$	Ha: diff ! = 0 $Pr(\|T\| > \|t\|) = 0.0000$	Ha: diff $>$ 0 $Pr(T > t) = 0.0000$

Table 11: Contiguous States with a Barrier on Another Border

	Model I (Poisson)	Model II (Poisson)	Model III (Poisson)	Model IV (Poisson)
Physical Barrier – Contiguous	-0.023	-0.051	-0.004	0.008
	(0.07)	(0.07)	(0.12)	(0.11)
Barrier Case Contiguous Neighbors	-0.135**	-0.137**	-0.131**	-0.152**
	(0.06)	(0.07)	(0.06)	(0.07)
Barrier Case Contiguous Neighbors X Income Inequality			-0.024	-0.045
			(0.05)	(0.05)
Income Inequality			0.014	0.016
			(0.03)	(0.03)
Democratic Dyad	-0.110**	-0.113**	-0.105**	-0.107**
	(0.04)	(0.04)	(0.04)	(0.04)
Defensive Alliance	0.423**	0.362**	0.425**	0.362**
	(0.07)	(0.06)	(0.07)	(0.06)
Strategic Rivalry	-0.155**	-0.129**	-0.145**	-0.131**
	(0.05)	(0.04)	(0.05)	(0.05)
Preferential Trade Agreement		0.220**		0.223**
		(0.03)		(0.03)
Common Currency		0.064		0.082
		(0.04)		(0.05)
Both in GATT/WTO		-0.076		-0.127
		(0.06)		(0.08)
Country-Year Fixed Effects	Yes	Yes	Yes	Yes
Directed Dyadic Fixed Effects	Yes	Yes	Yes	Yes
N	1096606	916556	1007128	897502

Standard errors clustered by dyad in parentheses
** $p < .05$; * $p < .10$

Table 12: Border Walls and Dyadic Trade Flows: Alternative Wall Measures

	Model I (Poisson)	Model II (Poisson)	Model III (Poisson)	Model IV (Poisson)
AG Wall Measure	-0.077	-0.094		
	(0.06)	(0.06)		
HW Wall Measure			-0.089	-0.097
			(0.06)	(0.06)
Democratic Dyad	-0.106**	-0.108**	-0.108**	-0.111**
	(0.04)	(0.04)	(0.04)	(0.04)
Defensive Alliance	0.424**	0.362**	0.420**	0.357**
	(0.07)	(0.06)	(0.07)	(0.06)
Strategic Rivalry	-0.146**	-0.131**	-0.146**	-0.131**
	(0.05)	(0.04)	(0.05)	(0.04)
Income Inequality	0.013	0.016	0.013	0.015
	(0.03)	(0.03)	(0.03)	(0.03)
Preferential Trade Agreement		0.223**		0.222**
		(0.03)		(0.03)
Common Currency		0.082		0.081
		(0.05)		(0.05)
Both in GATT/WTO		-0.120		-0.126
		(0.08)		(0.08)
Country-Year Fixed Effects	Yes	Yes	Yes	Yes
Directed Dyad Fixed Effects	Yes	Yes	Yes	Yes
N	1007128	897502	1007128	897502

Standard errors clustered
by dyad in parentheses
** $p < .05$; * $p < .10$