Border Walls, Cooperation, Illicit Trade

David Carter[*], Bailee Donahue[†], and Rob Williams[‡]

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Abstract

In the last twenty years, the number of fortified borders around the world has risen precipitously. A growing body of research shows that cross-border economic inequality drives wealthier states to construct border walls. This surge in walls is further argued to be a reaction to the unwanted "externalities" of economic openness and globalization, namely, illicit trade and smuggling. While recent studies analyze the effect of walls on legal trade, no studies of which we are aware explore how walls might affect illicit trade. This is a notable omission for two key reasons. First, the most common explanation for wall construction puts combating illicit trade front and center. Second, recent work that finds walls significantly reduce legal trade suggest that this finding derives from border fortifications diverting illegal trade to ports of entry, which leads to more inspection, security, and transaction costs. We begin to fill this gap here by developing new measures of illicit trade flows and assessing their connections to border wall construction and legal trade flows.

[*]Department of Political Science, Washington University in St. Louis

[†]Department of Political Science, University of North Carolina at Chapel Hill

[‡]Weidenbaum Center on the Economy, Government, and Public Policy, Washington University in St. Louis

Introduction

Border wall construction is on the rise globally. While there were fewer than ten walls or fences in place at the end of the Cold War, the number of walled borders rose slowly during the 1990s and skyrocketed after 2000, with over 30 in place by the early 2010s and well over 50 by the end of the decade. Recent research points to wealth disparities between neighbors as being a prime driver of wall building. Cross-border wealth disparities are argued to drive wall construction because they incentivize illicit trade flows from the poorer to the wealthier neighbor (Hassner & Wittenberg 2015, Carter & Poast 2017). Leaders view border walls as a way to raise the costs of illicit cross-border operations (e.g., (Carter & Poast 2017)), or at least to create the perception that the state is in control of its borders (e.g., (Brown 2017)). This problem with illicit cross-border flows is consistent with the claims made by prominent observers such as Naim (2006), who highlights how illicit cross-border flows have proliferated in the wake of the liberalization of the post-Cold War world economy. Despite the fact that reducing the cross-border movement of illicit trade is central to ideas about why border walls are constructed and the reported increases in global illicit trade prior to the sharp increase in border wall construction, the existing literature provides no systematic evidence of which we are aware over what, if any, effect walls have on illicit flows.

Recent work on the economic consequences of border fortification shows that walls significantly reduce *legal* trade flows between neighbors (Carter & Poast 2020, Kamwela, van Bergeijk et al. 2020). This is an interesting finding as it is not immediately obvious why walls along a land border would affect legal flows through ports of entry. Carter & Poast (2020) argue that walls divert illicit flows to ports of entry, and are accordingly also accompanied with general security programs that increase transaction costs for legal trade flows. In short, walls push smugglers to move more illicit trade through legal ports of entry as fortifications and an increased security footprint along the fortified land border complicate former land routes. Moreover, walls are almost always accompanied by heightened general security measures at ports of entry, which are in part a reaction to the diversion of more illicit goods to the ports. While these dynamics help explain why walls would adversely affect legal trade flows, the lack of systematic evidence over how walls affect illicit flows remains a major blind-spot in the burgeoning literature on the consequences of border walls.

We argue that border wall construction *increases* the volume of illicit trade that flows between neighbors through legal ports of entry. There are several key reasons for this. First, as authors like Carter & Poast (2020) highlight, border fortifications lead to a diversion of illicit flows from un-permitted routes across the land border to ports of entry. Second, to the extent that border walls increase the costs and difficulty of engaging in illicit trade, this serves to only heighten the potential profits associated with it and to increase the financial reward for organizations that can successfully continue to smuggle. Consequently, the set of actors that can continue to profitably engage in cross-border illicit trade (and are not driven out of it by enhanced security measures) are the subset of illicit actors that are most capable, who will also enjoy higher prices for their services. The upshot here is that the market for cross-border illicit trade becomes more profitable and attracts organizations with greater capabilities following border fortification, which leads to significant increases in cross-border illicit trade flows.

Illicit trade flows are notoriously difficult to measure. It is not hard to fathom why, as the actors involved in illicit trade have strong incentives to shield their activities from view. Nonetheless, scholars have long worked on measures that tap into cross-border illicit flows, with a number of scholars following Bhagwati (1964) in using patterns in the "mis-invoicing" of imports and exports in international trade statistics, e.g., (Berger & Nitsch 2008). While idea that mis-invoicing, or differences between Mexico's reported exports to the United States and the U.S.'s reported imports from Mexico, for example, reflects illicit flows is intuitive, it is true that these differences can also reflect other factors. For instance, Schultz (2015) argues that missing values and discrepancies in international trade statistics often reflects a lack of cooperation between the exporting and importing governments, while Stone (2008) uses missing economic data as an instrument for low bureaucratic capacity. Despite the inherent noise in these measures, Berger & Nitsch (2008) and others report statistical relationships consistent with the idea the mis-invoicing reflects illicit activities, such as strong statistical connections between mis-invoicing of products and the level of corruption in a country's government.

Three factors create a unique opportunity to make inferences about illicit trade flows. First, the the precipitous global rise of border walls during the 21st century provides an opportunity to see how patterns of reporting changed among neighbors after wall construction. As noted above, by the late 2010s, there were over 50 borders has walls under construction or finished, making what was a relatively rare occurrence at the turn of the century much more common. Second, the post-Cold War globalization of the world economy and the accompanying easing of both capital flows and trade changed the economic landscape in a way that altered the importance of mis-invoicing. In the mid-1960s and the two decades that followed, scholars tended to focus on how mis-invoicing related to actors wanting to avoid costs associated with tariffs or capital controls; however, after the mid-1990s cross-border exchange had been greatly liberalized, as barriers such as tariffs and capital controls were sharply decreased or even eliminated in many countries. This made the use of mis-invoicing to skirt transaction costs like tariffs, e.g., the under-invoicing of imports, much less worthwhile. However, as observers such as Naim (2006) detail at length, illicit trade flows and money laundering have become a huge business globally, and mis-invoicing of trade flows remains an attractive way to move goods and money across borders that can "blend in" with the very flows of goods that states and international organizations work to facilitate.

Finally, the availability of detailed global international trade data at the product level across more than five decades provides unique opportunities to measure changes in invoicing discrepancies across the spectrum of traded commodities over time. This data allows us to depart from prior practice, where scholars have calculated the gap in reported imports and corresponding exports across country-pairs, or more recently calculated differences across commodities within country-pairs for half a decade or so. Rather, we use detailed UNCOMTRADE data, which records several measures of reported imports and exports at three fine-grained commodity categories for all trading country-pairs from 1962–2018. Furthermore, rather than calculating simple reporting differences at the dyadic level or at the commodity level as has been done in prior work, we estimate measures that calculate inequality in reporting discrepancies within dyad-year, analogous to a GINI coefficient for reporting discrepancies at the commodity level. Thus, if a dyad exhibits reporting discrepancies across many or most commodities, which may reflect a lack of cooperation, bureaucratic capacity or efforts to evade capital controls or tariffs, our measure would not flag such a case as likely to represent illicit flows.

We find that when a state erects a border wall, illicit trade subsequently increases. Specifically, we show that the inequality in reporting discrepancies across all goods traded between the importing and exporting state significantly increases after the importer erects a border wall. This is the case despite the fact that aggregate import flows decrease, a finding that we replicate using different data than Carter & Poast (2020). Our estimates are conservative, as all models condition out any country-level factors for both importer and exporter with country-year fixed effects in addition to including directed-dyadic fixed effects. Accordingly, the estimated effect of border barrier construction is identified off of variation within each directed-dyad before and after border fortification. We of course also include a number of other theoretically merited control variables that vary across directed dyads across time.[1]

[1] Nicely, all country-level factors, such as the corruption of the exporter, are conditioned out by the country-year fixed effects. This eliminates many potential inferential problems that might arise from the endogeneity of country-level variables and border walls.

The paper proceeds as follows. First, we outline our theoretical expectations over how border walls should affect illicit trade flows. Next, we discuss our strategy for measuring illicit trade, taking care to clarify how and why it differs from prior treatments. Then we outline our research design and identification strategy, and move on to report and discuss our main results. We then conduct additional tests and sensitivity analysis to further probe both the plausibility of our argument and the robustness of our estimated coefficients. Finally, we conclude with policy implications and some potentially fruitful routes for future research.

Border Walls and Border Security

There have been two recent waves of research on border walls. The first wave of work highlighted the contemporary boom in wall-construction and sought to identify its causes. The second wave seeks to assess both the political and economic consequences of border walls. Our research bridges these two waves as we highlight an unwanted consequence of border wall construction, increased illicit trade, which is also widely argued to be a driver of wall construction itself.

The literature on the determinants of border fortifications exhibits remarkable consistency in that pretty much all recent research identifies illicit cross-border flows as central to the contemporary increase in wall-building. In one of the earlier papers with data on border fortifications, Rosière & Jones (2012) argue that gaining control over immigration flows was a key motivation behind recent wall construction efforts. Using different data sets and research designs, both Hassner & Wittenberg (2015) and Carter & Poast (2017) demonstrate that border income disparity between neighbors is a key determinant of where walls are built. Hassner & Wittenberg (2015) provide a cross-sectional analysis that helps us understand why the U.S. has invested much more in border security and walling on its southern border than its northern border, a finding that Carter & Poast (2017) also establish in pooled regression models. However, the demonstration in Carter & Poast (2017) that within-dyad variation in cross-border income disparities across time also explains *when* barrier construction happens (e.g., why did the U.S. start fortifying its border with Mexico in the 1990s and not in prior decades) starkly highlights the key role that cross-border wealth disparities and the economic incentives that accompany them play in border fortification. As all of this work highlights, stark cross-border income inequality affects incentives to illegally migrate across the border, or to engage in the cross-border smuggling of illicit goods. Border walls are built in response to the unwanted cross-border movement of people and goods, usually from relatively poor to wealthy neighbors. However, no research of which we are aware investigates what effect, if any, the global rise of barrier construction has had on illicit flows.

Several recent papers assess the possible effects of border walls on the spread of militancy and political violence. Avadan & Gelpi (2017) analyze the effect of walls on transnational terrorist attacks, summarizing theoretical reasons which suggest that the relationship could be either positive or negative. Using matching methods that match "similar" observations across both dyads and years, they find that barriers reduce transnational terrorism. Using high-dimensional fixed effects count models that identify estimates by comparing pre-wall observations to post-wall observations within-dyad (and condition out all country-level confounders for both states), Carter & Ying (2020) find no significant relationships between border wall construction and the cross-border flows of transnational terrorist attacks. In a related vein, Linebarger & Braithwaite (2020) find that border walls have limited utility in stemming the spread of insurgency, as the conditions under which walls might be effective, i.e., non-rugged terrain and the presence of state institutions and infrastructure, are the mirror-opposite of the conditions that militants flourish in, i.e., low state capacity and rugged terrain. In short, there is not consensus over the effects border barriers have on the cross-border movement of political violence and insurgency.

Findings over the economic effects of border walls have been stronger than those in the security realm. Two recent studies show that the erection of border walls has significant negative effects on legal trade flows (Carter & Poast 2020, Kamwela, van Bergeijk et al. 2020). In a study of post-Cold War trade flows Kamewla, van Bergeijk et al. (2020) find that barriers decrease trade between 46 and 73 percent relative to what would be expected without a wall. Carter & Poast (2020) similarly find that trade is reduced by around 30 percent after a wall is built. Of course, the finding that border barriers reduce legal trade is not necessarily intuitive as walls are not built to block movement through ports of entry and legal checkpoints. Carter & Poast (2020) argue that how barriers and increased border security affects illicit trade is the key to understanding why steeper transaction costs depress legal trade. In the only related study that analyzes how wall construction affects illicit activity, Germansky, Grossman & Wright (2019) show that the construction of the border barrier by Israel on its boundary with Palestinian territories simply diverted illicit activity, but did not reduce it. This is a particularly interesting case, as Israel is a case where the border area was highly securitized and state capacity was quite high across the entire area and time period. Thus, we would expect the addition of a border wall to have particularly strong effects in such a case, e.g., (Linebarger & Braithwaite 2020). Missing from the literature is evidence over how border walls affect illicit cross-border trade flows across the large number of (heterogeneous) cases. We help to fill that gap here, employing the latest high-dimensional fixed effects estimators developed in the trade economics literature to aid in identifying estimates of the effects of border barrier construction on illicit flows.

Why Border Walls Boost Illicit Trade

Leaders erect border walls to stem the unwanted cross-border flow of goods and people, or at least to appear as if they are doing so. Our claim is that if it is indeed leaders' intention to reduce illicit flows with border fortification (Hassner & Wittenberg 2015, Carter

& Poast 2017), these efforts backfire or fail. Our logic is straightforward. If a border barrier does not raise the costs of doing illicit business enough to push all smugglers, drug traffickers, and others engaged in illicit trade out of business, it has several pernicious (and presumably unwanted) effects. In short, border fortifications foster a market for illicit trade that demands more capable and sophisticated actors. To the degree that border security measures "succeed" in raising the costs and risks of illicit cross-border activities, they also push all but the most capable and organized actors out of the business and leave them with higher prices and profits. We outline our argument in more detail in what follows.

When states militarize and fortify their borders, this increases the risks and costs for actors involved in illicit cross-border trade. However, unless the barrier and heightened security measures that accompany fortification push all actors out of illicit cross-border trade, only the most capable, organized and sophisticated organizations can continue to operate. Thus, rather than sharing the market with numerous individuals and small actors, larger and more capable organizations will take control of a greater share of the market. The increased costs and risks associated with doing business only heighten the appeal of market share for larger organizations, as the upshot is higher prices and profits. An illustration of how these more capable groups gain and hold "market share" is seen in the evolution of Mexican drug cartels during the late 1980s and early 1990s following three major developments: the diversion of Colombian cocaine from routes through South Florida to routes over the U.S.-Mexico land border as a result of the U.S.'s massive militarized "interdiction" program during the 1980s, the subsequent focus on militarizing and policing flows on the U.S.-Mexico border, and the increase in legal goods flowing across the border following the NAFTA agreement. Moisés Naím notes how large Mexican cartel groups quickly adapted by, above all, "maintaining control at all costs over their respective border-crossing corridors", which involved the use of extreme violence against anyone thought as a competitor and a continual stream of massive bribes to officials (Naim 2006, 75). Naím goes on to outline how these groups started to use their size, capabilities, and control of key territory to partake in all forms of illicit cross-border trade. He notes that "in what amounted to a total recomposition of the game, product expertise was traded for functional specialty" as "the Mexican groups focused on controlling the border and took part directly or indirectly in the movement of a wide range of goods across it" (Naim 2006, 75–76).

One important consequence of the increased costs and risks associated with moving illicit goods across a fortified border is that illicit flows are increasingly diverted from illegal border crossing points to ports of entry (Carter & Poast 2020). This "substitution" of illicit trade into legal ports of entry occurs for several reasons. First, ports of entry are designed to *facilitate* legal flows. While state efforts at un-permitted crossing points along a fortified border are aimed to staunch or block flows, the opposite is true of ports of entry. States do of course attempt to filter out illicit goods at ports of entry. However, this is a difficult task for several reasons, all of which make ports of entry attractive to actors involved in cross-border illicit trade that are sophisticated enough to move goods through them. Even when high-capacity states step up monitoring and enforcement at ports, there are limits to how intensively they can do so without legal commerce grinding to a halt. For instance, most estimates suggest that the U.S. Customs and Border Control inspects less than 5% of containers that enter through ports of entry, despite the greatly increased worries about inspections among policy-makers following the 9/11 attacks. This is not a new phenomenon either, as Peter Andreas nicely highlights how the U.S. government's efforts to stop cocaine shipments to South Florida in the 1980s, i.e., the interdiction strategy, served to divert it to the Mexican land-border, which led to better organized and more dangerous organizations exporting illicit goods across the border (Andreas 2000, 42–50). He summarizes the outcome, noting that "[i]n other words, the main impact of the U.S. interdiction strategy was to create more business for Mexican smuggling organizations and more work for law enforcement" (Andreas 2000, 45).

In sum, moving illicit goods through ports of entry necessitates greater capability and more sophisticated methods than simply crossing the land border un-permitted by foot or vehicle. As Andreas (2000, 95) notes in his seminal treatment of the U.S.-Mexico border: "the intensified border control campaign has transformed the once relatively simple act of crossing the border into a more complex system of illegal practices." He goes on to highlight that individuals used to easily cross the border illegally on their own, but that now the vast majority must hire "professional smugglers". The trends are no different for illicit goods. As moving these goods across un-permitted points on the land border safely and undetected has increased in difficulty, more sophisticated methods have been adopted by more capable organized actors.

Research Design and Data

Assessing the effects of border barriers on illicit trade flows is not a trivial task. We first discuss our statistical approach to identifying estimates of border walls on illicit flows and subsequently outline how we propose to measure illicit trade flows.

Given the large number of plausible confounders that could adversely affect any estimated relationship between border fortification and illicit trade, especially at the country-level, we follow recent approaches in the international trade literature by estimating high-dimensional fixed effect models (Baldwin & Taglioni 2006, Gowa & Hicks 2013). These models include country-year fixed effects for both the importer and exporter, thus conditioning out any country-level variables, time-varying or not, that might affect illicit trade flows. Thus, variables such as the level of crime in the exporting country in a given year, or the corruption level in each country's police force or customs units, are accounted for without error. In addition, we follow the international trade literature including directed dyadic fixed effects, so that all fixed features that affect both the importing and exporting relationship between two states are accounted for.[2] Thus, if we take the United States and Canada as an example, the directed dyadic fixed effects pick up any fixed features that affect of Canada's exporting relationship with the United States as well as any fixed features that affect Canada's importing relationship with the United States.

This fixed effects strategy leaves only variation for time-varying dyadic variables to explain. Thus, any estimated coefficients (which must be for time-varying dyadic variables) are identified off of within-directed dyad variation, conditional on all of the other variables and fixed effects. This is ideal here as our key variable of interest, the presence of physical border barriers, is a time-varying dyadic variable. We also hasten to note that the inclusion of country-year and directed dyadic fixed effects and the fact that we cluster standard errors by dyad in all models provides a high hurdle for any of our regressors to have significant effect on illicit trade flows.

Measuring Illicit Trade Flows

Quantifying the size and scope of illicit trade flows has been a perennial concern for economists and political scientists alike. As cross-border legal trade has expanded, so too has the market for illicit trade activity. The UN estimates the cost of illicit trade to be approximately 2.2 trillion dollars a year or 3 percent of global GDP (UNCTAD 2019). The UN argues that illicit trade deprives governments of necessary revenue and crowds out legitimate business. As an example, black tea imported into Afghanistan and smuggled to Pakistan is believed to be one-third of Pakistan's tea market. The total value of the licit tea market is worth an estimated 610 million dollars a year as of 2014. The evasion of sales tax via smuggled tea costs Pakistan's government approximately 84 million dollars in lost tax revenue annually

[2]Additionally, as is the case in the international trade literature, these country-year fixed effects can be thought of as multi-lateral resistance terms, ensuring that our estimates for how a variable affects illicit flows reflect an actor making the comparison of the potential profits and risks to illicit trade for a given destination country relative to the average profits and risks for all other possible destinations.

(Baloch 2014). Despite the proposed size and scope of the illicit market, measuring illicit trade has proven to be quite difficult due to the covert nature of such activity.

One method of measuring illicit trade is derived from discrepant reporting of trade statistics. This method was popularized by Bhagwati (1964) in his study of tariff evasion in Turkey using trade discrepancies. He argued that one country's reports of imported goods should be equal to what its partner reports in exports but this is rarely the case. Matching records are very rare in trade data with only 0.03% of reported flows in the official UN trade statistics agreeing, and illicit trade is one of many possible explanations for these discrepancies in trade reports[3]. Since this groundbreaking work, mirrored measurements have been deployed to investigate a variety of different phenomenon including tariff evasion, smuggling, and capital flight.

Fisman & Wei (2004) investigate trade discrepancies – missing trade – between Hong Kong and China. They find that an increase in tariffs is associated with increased discrepancies in trade reports. These findings regarding tariff evasion using mirrored measurements have since been shown in the context of Germany and Eastern Europe (Javorcik & Narciso 2008). In their study, Javorcik & Narciso (2008) find that product differentiation increases tariff evasion as determining the unit price and classification becomes more difficult. The results for increased trade discrepancy in light of increased tariffs have been shown in India (Mishra, Subramanian & Topalova 2008), North America (Stoyanov 2009), Mozambique (van Dunem & Arndt 2006), Tanzania and Kenya (Levin & Widell 2014), Tunisia (Rijkers, Baghdadi & Raballand 2017) and Brazil (Kume, Piani & Miranda 2011). A cross-national study using data for 2004 finds tariff evasion to be more pronounced in poorer countries (Jean & Mitaritonna 2010). Further, tariff evasion – measured in trade discrepancies – has been found to be facilitated by the presence of dense economic networks (Rotunno & Vézina 2012) as well as through intermediaries (Feenstra, Hai, Woo & Yao 1999).

[3]All transactions at the SITC1 three digit commodity code level for 1962-2018.

The literature on smuggling has often focused on specific commodities such as antiquities where the incentive for tariff evasion can be separated from smuggling. Fisman & Wei (2009) leverage the fact that in the United States cultural object imports face no tariffs but often face export restrictions from their country of origin. The gap in trade in antiquities is highly correlated with levels of perceived corruption in the exporting country. In a study of smuggling of legal and illegal goods across the US-Mexico border, Buehn & Eichler (2009) use trade discrepancies to measure the rate of mis-invoicing of legal goods.

Mirrored measurements of trade have also been used to measure capital flight. Bhagwati, Krueger & Wibulswasdi (1974) finds evidence of capital flight from less developed countries using a mirrored measurement of trade. Similar findings come from a mirrored measurement study of trade in India suggesting 21 billion dollars in capital flight from 1971 to 1986 (Rishi & Boyce 1990). In a study of 33 Sub-Saharan Africa from 1970 to 2004 that measures capital flight using trade discrepancies with more developed partners (Ndikumana & Boyce 2010). A limitation to the studies of capital flight and trade mis-invoicing is that disentangling the incentives for firms to mis-invoice to evade capital controls and the incentives incentives to mis-invoice to evade tariffs using this mirrored measurement of trade data is quite difficult.

For the purposes of this study, we develop a measurement of trade flow discrepancy that continues to build upon the important work noted above. We capture the discrepancy between the reported value of exports from State A to State B and the reported value of imports from State A into State B. Previous studies in the rate of tariff evasion in developing economies often make the assumption that more developed counterparts are accurately recording trade flows (Javorcik & Narciso 2008, Javorcik & Narciso 2017). As an example, Javorcik & Narisco (2017) in their study of WTO accession and tariff evasion compare the rate of tariff discrepancies from fifteen countries entering into the WTO with three exporting economies with lower levels of corruption: the United States, France, and Germany. The underlying assumption of such a measurement is that the United States, France, and Germany report closer to the 'true' value of dyadic trade flows. With our focus on contiguous dyads, this measurement of deviation from the 'true' value is not a feasible strategy. The data collected by Carter & Poast (2017) on the construction of physical border barriers contain dyads with jointly high levels of perceived corruption (ex. Uzbekistan-Kyrgyzstan, Bangladesh-Myanmar) where such a baseline to measure deviation from 'truth' does not exist. Accordingly, we develop our measurement remaining agnostic to the truthfulness of dyadic reporting parties.

Another limitation to previous measurements has been the scope of the mirrored measurement. Our present task requires a cross-national multi-year measurement. As noted above, previous studies have limited their scope to a particular country, specific commodity, or a short time period, our measurement expands on previous work by building a cross-national measurement using commodity-level data that extends from 1962-2018.

The mirror statistics measurement of trade flow discrepancy encapsulates several possible illicit incentives for erroneous reporting enumerated by Collins (2019), including over-invoicing of imports; under-invoicing of imports; over-reporting of exports; under-invoicing of exports. Within our measurement there are both illicit financial flows as well as trade flows. Incorrectly expressing the value of goods entering into a country could be used to avoid capital controls and keep money abroad. This goal could be achieved by over-invoicing imports or stating that an import is worth more than the true value of the good. A canonical example of this process is the $973 bucket exported from the Czech Republic to the United States. This bucket has been pointed to as evidence of illicit financial flows (Sikka 2003), although this assessment has been disputed (Forstater 2018). A similar effect could be achieved by under-invoicing exports or stating that an export is worth less than the true value of the export. These strategies also help actors involved in illicit networks to evade capital controls.

While it is possible to obtain the total discrepancy in reported trade value between two states, it is equally important to recognize that these totals are composed of discrepancies at the product level. Formally, when state i exports a commodity k to state j, state i reports a value v_{ik} for the exported units of commodity k and state j reports a value v_{jk} for the imported units. The aggregate discrepancy in reported trade flows between state i and j d_{ij} is given by

$$d_{ij} = \sum_{k=1}^{K} v_{ik} - v_{jk} \tag{1}$$

where $k \in K$ commodities. In a dyad where one state consistently over-invoices transactions while the other consistently under-invoices them, $d_{ij} \approx 0$, so this is not a sufficient statistic to estimate the true rate of mis-invoicing in a dyad. Taking the sum of $|v_{ik} - v_{jk}|$ instead better captures the level of total level of mis-invoicing, but we are particularly interested in deliberate (if not malicious) mis-invoicing. To capture this specific source of mis-invoicing, we must examine patterns of reporting discrepancies rather than aggregate levels.

In order to evade tariffs, under-invoicing the value of imports may successfully obfuscate the true value of a good. For example, stating the unit-value of women's athletic clothing as being lower than the true value. Under-invoicing goods may allow for an actor to side-step caps on strictly controlled imported goods. Actors engaged in illicit trade flows can also mis-specify the products entering into the country by classifying the "true" product as a similar product taxed at a lower rate. For example, imports of chicken to Russia were often mis-classified as turkey, this error resulted in a decline in the tariff rate from 25% to turkey's tariff rate of 10% (Afontsev et al. 2004). Similar mis-classification strategies have also been identified in Fisman & Wei (2004). Finally, by over-reporting the value of exports, actors can take advantage of export credits. Our measurement of illicit trade does not discriminate between these different forms of trade flows. For our purposes, we are more concerned about levels of mis-invoicing than distinguishing between types.

In constructing this mirror statistics-based measurement, we use the United Nations Comtrade dataset. These data and their varying aggregations have been discussed at length in other contexts[4] but it is useful to consider how varying aggregations of UN Comtrade data may change the types of discrepancy observed in our calculations of illicit trade flows. For our purposes, we use the SITC1 coding scheme for trade data as it remains consistent through the full period of our data from 1962 - 2018. The data are classified with increasing specificity as the number of digits in a code increase. Accordingly, a one-digit code represents the broadest classification of a good while a five-digit code is the most granular representation of a good. Figure 1 demonstrates the structure of the SITC 1 coding for types of salted fish with the five-digit code differentiating between salted and dried cod and other forms of salted fish.

```
0: Food and live animals chiefly for food
  03: Fish, crustacean and molluscs, and preparation thereof
    035: Fish, dried, salted or in brine; smoked fish
      0350: Fish, dried, salted or in brine; smoked fish
        03502: Cod (not in fillets) dried, whether or not salted
        03503: Fish (excluding cod) dried, salted or in brine
```

Figure 1: Structure of commodity codes for salted cod.

The example provided in Figure 1 also exposes an issue in constructing a measurement of illicit flows with highly disaggregated data: whether erroneous classification is due to human error or due to attempts to evade the detection of the value of goods entering into a country. Error may occur at highly disaggregated commodity codes because of the inability of a customs agent to distinguish between `salted cod 03502` and `salted pollak 03503`. In constructing this measurement of illicit trade flows, we need to balance our need for specificity while also weighing the possibility for human error in determining the goods entering into a country. Other work has found that as commodities are aggregated upwards towards broader commodity classification, discrepancy in reporting values is reduced (Fisman & Wei 2004). For the purposes of our measurement, we select the three and four

[4]See Thies & Peterson (2015), Kim et al. (2019) for extensive discussions on this topic.

digit commodity codes as they provide a sufficient aggregation upwards to reduce persistent human error but still provide sufficient specificity that we can know the types of commodities that have higher discrepancies.[5] Figure 2 presents a histogram of reporting discrepancies for 38,694,649 transactions at the SITC three digit commodity code level for all directed dyads and commodities from 1962-2018 in 2019 US Dollars.

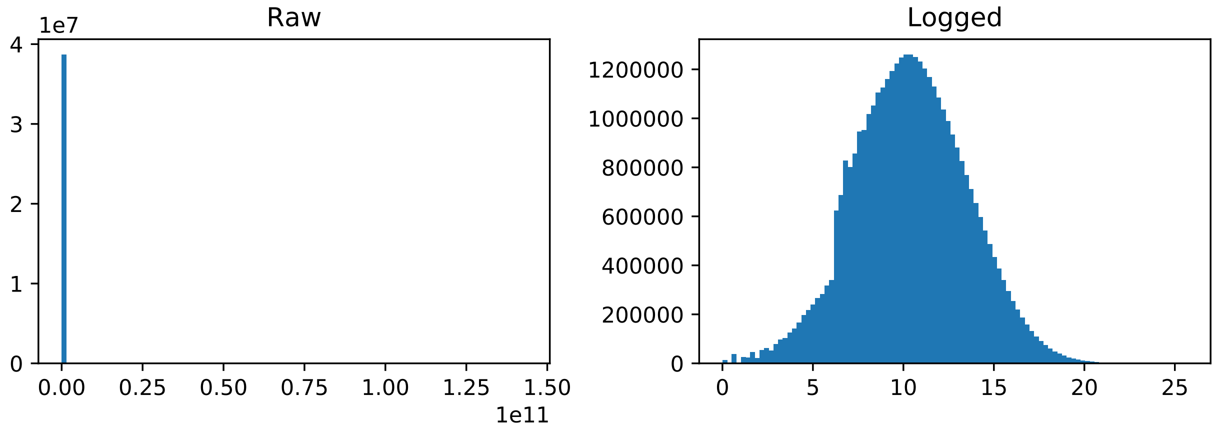


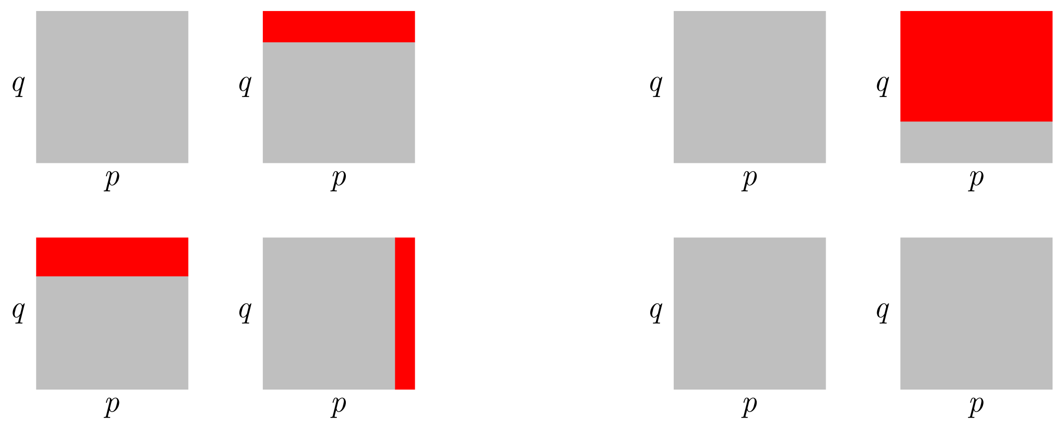
Figure 2: Histograms of reporting discrepancies at the directed-dyad-commodity-year level

We further refine our measurement by adopting an understanding of trade flow reporting discrepancies from Forstater (2018) and extend her conceptualization to develop a systematic measure of reporting discrepancies. While she is interested in different sources of misreporting (both the price and the quantity) to understand the different *methods* by which illicit flows occur, we are interested in detecting total illicit flows. She notes that concentrated discrepancies and widespread marginal differences in reporting can both indicate illicit activity, but via different mechanisms.

We wish to separate genuine reporting error — due to causes such as the inclusion of shipping cost in the value of goods by one reporter and not the other, the inconsistent attribution of trade to a trade partner, or when a shipment leaves the exporter in one year and arrives at the importer the next — from deliberate falsification of flow reporting due

[5]The use of four digit commodity codes is required for our analysis of differentiated and undifferentiated goods, so we generate our measure at both specificity levels.

to illicit trade flows. Reporting error would manifest as (semi) random measurement error; while each individual reported commodity flow may be slightly inaccurate, on average they will be accurate. In contrast, smuggling would appear as large differences concentrated in specific commodities that are advantageous for the purposes of smuggling, with one partner consistently under reporting these commodities or failing to report them at all. Figure 3a provides a simplified illustration of the former case, while Figure 3b presents the latter.[6]



(a) Frequent marginal discrepancies

(b) Concentrated large discrepancy

Figure 3: Patterns of reporting irregularities adapted from (Forstater 2018). p and q represent reported prices and quantities, respectively, while red regions denote differences from the higher reported figure.

Measuring illicit economic activity in this manner is preferable to approaches that look for irregularities in pre-specified baskets of goods that are susceptible to smuggling because which goods are susceptible to smuggling can vary greatly across contexts. A concentration-based conceptualization of illicit economic exchange also underestimates the level of deliberate mis-invoicing occurring in a relationship. As Forstater (2018) notes, widespread marginal discrepancies across commodities can indicate either small-scale illicit exchange, or benign reporting error due to any number of sources. By assuming that large and concentrated discrepancies represent a clearer signal of mis-invoicing, we are looking for

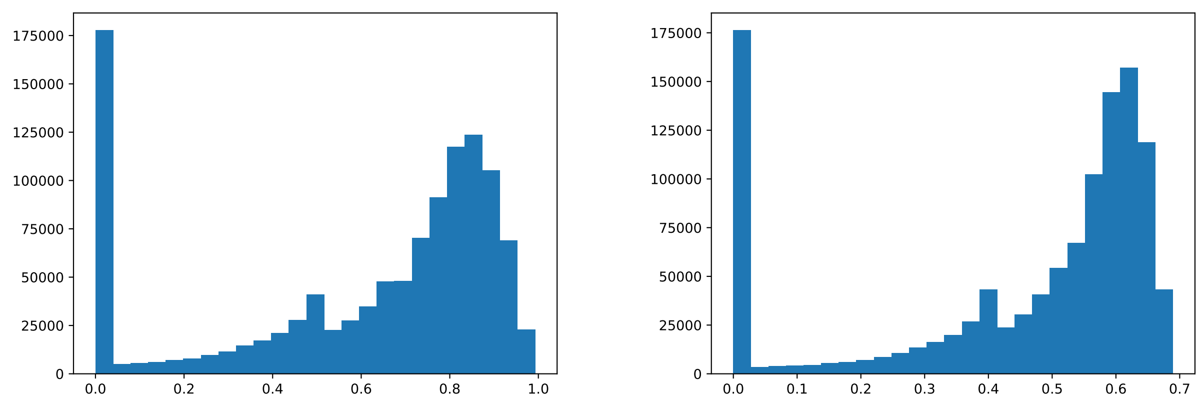
[6] While mis-invoicing can occur in the reporting of both prices and quantities, the Comtrade data only report the total value of all transactions in a given commodity-year for all observations. Most observations include the reported quantity, but 11.86% do not, so we focus on reported transaction values rather than disaggregating to price and quantity misreporting.

only the most egregious cases.

Accordingly, we require a measure that can capture the degree of concentration of discrepancies in a trading relationship. The Gini coefficient is a commonly used measure of inequality that does exactly this. It is given by

$$G = \frac{\sum_{i=1}^{n} \sum_{j=1}^{n} |x_i - x_j|}{2n^2\bar{x}} \tag{2}$$

where x is the vector of commodity-level discrepancies in a given directed-dyad-year. In a dyad with no reporting discrepancies, the Gini coefficient will have a value of 0. Conversely, in a dyad with one discrepant commodity and otherwise harmonious statistics $\lim_{n\to\infty} G(x) = 1$, so a Gini coefficient near 1 would indicate concentrated discrepancies in a small number of commodities and widespread agreement among other commodities.

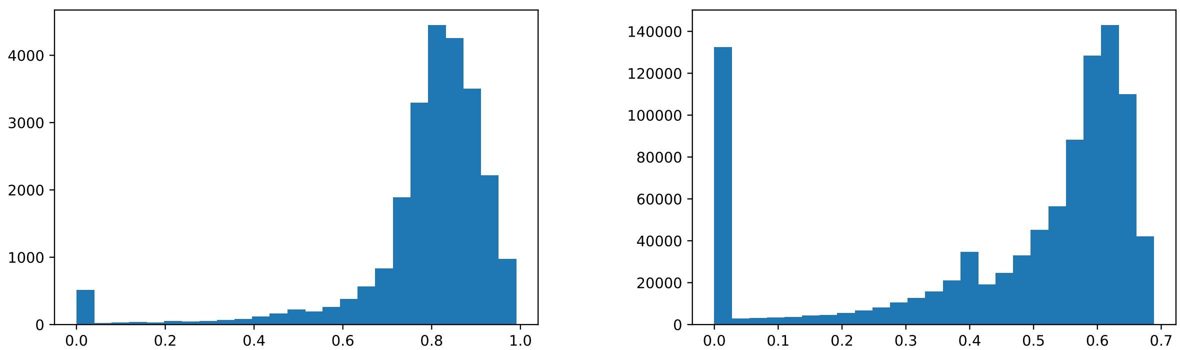


(a) Raw

(b) Logged

Figure 4: Histograms of Gini coefficients of discrepancies at the three digit commodity code level.

The distribution of Gini coefficients for all directed-dyad-years is heavily biased towards 0, as seen in Figure 4a. This bias is due to the fact that most directed-dyad-years trade relatively few commodities. If we examine all directed dyads globally from 1962 to the present, many trade fewer than 10 commodities in a given year, as the median is 14 commodities at the three digit level. 172,566 directed-dyad-years (15.22% of observations) trade only one commodity, which will necessarily yield a Gini coefficient of 0. Another peak is noticeable at $G = 0.5$ due to the many directed-dyad-years that trade only two commodities. In this case, if one commodity has no discrepancy, any discrepancy in the second commodity will result in a Gini coefficient of 0.5.



(a) Raw

(b) Logged

Figure 5: Histograms of Gini coefficients of discrepancies at the three digit commodity code level in contiguous directed-dyads

However, given that our interest is in how border walls affect trade patterns between states that see the construction of a barrier, our primary analyses include only contiguous dyads, similarly to (Carter & Poast 2020), where the median directed dyad trades 95 commodities. As seen in Figure 5, the higher rate of trade between neighbors ($\rho = 0.19$ between contiguity and number of commodities traded) results in a different distribution of Gini coefficients with far fewer equal to 0 and no noticeable peak at 0.5. This focus on contiguous directed dyads is helpful in that it puts the focus on what are typically each state's most important trading partners, e.g., Canada and Mexico for the United States. Nonetheless, we also control for the total value of all traded commodities in each regression model to account for the possibility that reporting discrepancies that are the result of bureaucratic or human error likely increase in the total value of traded goods.

Measuring Border Barriers

We use the data collected by (Carter & Poast 2017) as the basis of our measure of physical border barriers. The original data provides coverage up to 2013, by which time there were over 30 border walls globally. Given that more than 20 additional walls have been constructed since 2013, we have updated the data through 2018 to ensure that our analysis includes these important new cases.[7]

Additional Variables

We also control for several dyadic-level variables that are widely found to affect two states' trading relationship and several variables that tap into the nature and quality of states' political relationship. The idea here is to condition out the character of two states' time-varying economic and political relationship so we can plausibly identify the effect of border walls on illicit flows. While there are of course a number of potential confounders at the country-level, such as the level of governmental corruption or crime in either the importer or exporter, all such variables are soaked up by the country-year fixed effects. This is a great advantage as it allows us to focus our attention solely on time-varying dyadic variables.

One key aspect of states political relationship that is known to affect trade is whether they are allies or not (Gowa 1994). We measure alliance using the defensive alliance measure from (Gibler 2008). We also include a measure of whether both states in a dyad are democracies, as regime type has also been found to affect trade flows (Bliss & Russett 1998, Morrow, Siverson & Tabares 1998, Mansfield, Milner & Rosendorff 2000) and is also widely known to affect patterns of conflict e.g., (Russett 1994).[8] We also include a measure of whether the two states are embroiled in a territorial dispute with each other, as territorial

[7] We use X, Y and Z to code post-2013 cases, and also update the data pre-2013 using these sources to ensure consistency.

[8] We code a dyad as democratic if both states have Polity scores of at least six.

disputes are known to influence trade (Simmons 2006) and have also been found to increase reporting discrepancies in international trade flows (Schultz 2015). We use data on territorial disputes collected by (Huth 1996) and updated by (Huth, Croco & Appel 2011) and (Carter, Wellhausen & Huth 2019).

We also include several variables that are important to understanding two states' trading relationship. We use data collected by (Mansfield & Milner 2012) that measures whether two states have a preferential trade agreement (PTA) in place. Their data covers the period 1945–2010, and we update this data through 2018 taking care to follow their coding procedures. We also include a measure of whether the two states share a common currency, using data from Glick & Rose which we update through 2018. Observers such as (Naim 2006) suggest that common currencies facilitates the illicit flow of money, which makes this an important control. We also measure whether both states are GATT/WTO members, using data from Goldstein, Rivers & Tomz, which we update through 2018.

Results

Tables 1 and 2 show results over the effect of border barriers on the Gini coefficient in reporting discrepancies within a directed dyad. While table 1 presents results for data disaggregated by 3 digit commodity codes, table 2 presents results disaggregated by 4 digit commodity codes. We first discuss the results for 3 digit commodity codes, then briefly discuss the 4 digit results as they are very similar.

We report the results from six models in table 1, all of which use the 3 digit commodity classification. Models I and II are OLS models where the dependent variable is the unlogged Gini coefficient (i.e., Figure 5a), models III and IV are OLS models where the dependent variable is the logged Gini coefficient (i.e., Figure 5b), while models V and

VI and Poisson models with the unlogged Gini coefficient again as the dependent variable.[9] The models in Table 2 are identically specified, except in that they all use the trade data disaggregated at the 4-digit level. All models include the log of the total value of all traded commodities, *Log of Total Trade*, to account for the fact that discrepancies tend to increase in the raw value (and quantity) of trade. Thus, we want to condition out this effect, as it likely is reflective of bureaucratic errors, or a simple inability to be as consistently accurate in reporting as the volume of traded goods increase.

We find that the presence of a *Border Barrier* significantly increases the concentration of reporting discrepancies across all six models in Table 1. Model I, which is OLS on the unlogged Gini coefficient, includes only *Border Barrier* as well as *Log of Total Trade*, while Model II adds a number of variables common in the trade literature to this specification. Models III and IV are specified in the same way with the only difference being that we log the dependent variable. This is again true for the Poisson regressions in Models V and VI. In all models, the only other variable that we find statistically significant results for besides *Border Barrier* is the *Log of Total Trade*. Recall that all models include both importer-year and exporter-year fixed effects in addition to directed dyadic fixed effects. Thus, it is not possible that any omitted fixed or time-varying country-level factors in either of the two states or any fixed features of the directed dyadic trading relationship adversely affect these results.

Table 2 shows the results of models that are identical to those in Table 2, except in that we use the 4 digit level commodity data. The results for the effect of a *Border Barrier* are very similar, as we again find that reporting discrepancies are more concentrated in particular commodities when the border is fortified (relative to the period prior to border fortification). The only differences in Table 2 are that we find the presence of

[9]While Poisson models are most commonly applied to count data, they are increasingly applied to any non-negative dependent variable, especially dependent variables with a number of zeros. All that is required is (just as with OLS) correct specification of the conditional mean, and there is no requirement to specify distributional assumptions. See Correia, Guimarães & Zylkin (2020) for a straightforward discussion.

Table 1: Illicit Trade and Border Walls: Level 3 Trade Data

	Model I (OLS)	Model II (OLS)	Model III (OLS)	Model IV (OLS)	Model V (Poisson)	Model VI (Poisson)
Border Barrier	0.022**	0.027**	0.015**	0.019**	0.037**	0.041**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Log of Total Trade	0.060**	0.059**	0.040**	0.039**	0.103**	0.098**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Democratic Dyad		0.005		0.004		0.009
		(0.01)		(0.01)		(0.02)
Defensive Alliance		-0.005		-0.003		-0.006
		(0.01)		(0.00)		(0.01)
Territorial Dispute		-0.004		-0.003		-0.005
		(0.01)		(0.01)		(0.01)
Preferential Trade Agreement		-0.009		-0.006		-0.016
		(0.01)		(0.01)		(0.02)
Common Currency		0.021		0.014		0.037
		(0.02)		(0.01)		(0.02)
Both in GATT/WTO		-0.013		-0.013		-0.036
		(0.02)		(0.02)		(0.03)
Constant	-0.281**	-0.249**	-0.134**	-0.101**	-2.087**	-1.969**
	(0.04)	(0.05)	(0.03)	(0.03)	(0.07)	(0.08)
Dependent Variable	Gini	Gini	Ln Gini	Ln Gini	Gini	Gini
Country-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Directed Dyadic Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
N	30641	24388	30641	24388	30615	24378

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

a *Defensive Alliance* significantly lowers the concentration of reporting discrepancies. This finding suggests that allied states experience fewer instances of reporting discrepancy that are suggestive of illicit trade flows. Additionally, we find in Model VI that when two trade

Table 2: Illicit Trade and Border Walls: Level 4 Trade Data

	Model I (OLS)	Model II (OLS)	Model III (OLS)	Model IV (OLS)	Model V (Poisson)	Model VI (Poisson)
Border Barrier	0.023**	0.024**	0.016**	0.017**	0.039**	0.036**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Log of Total Trade	0.061**	0.059**	0.040**	0.039**	0.105**	0.097**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Democratic Dyad		0.003		0.003		0.003
		(0.01)		(0.01)		(0.01)
Defensive Alliance		-0.009*		-0.006*		-0.014**
		(0.01)		(0.00)		(0.01)
Territorial Dispute		-0.001		-0.001		-0.001
		(0.01)		(0.01)		(0.01)
Preferential Trade Agreement		-0.011		-0.008		-0.019
		(0.01)		(0.01)		(0.02)
Common Currency		0.027		0.019		0.048**
		(0.02)		(0.01)		(0.02)
Both in GATT/WTO		-0.013		-0.012		-0.032
		(0.02)		(0.01)		(0.03)
Constant	-0.269**	-0.221**	-0.125**	-0.083**	-2.085**	-1.931**
	(0.04)	(0.04)	(0.03)	(0.03)	(0.08)	(0.08)
Dependent Variable	Gini	Gini	Ln Gini	Ln Gini	Gini	Gini
Country-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Directed Dyadic Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
N	30463	24316	30463	24316	30437	24308

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

partners have *Common Currency*, this increases the concentration of reporting discrepancies across commodities. This finding is consistent with the argument of Naim (2006), although this coefficient fails to reach conventional levels of significance in the other specifications.

Differentiated Goods and Illicit Trade

In Tables 1 and 2 we present evidence using several different modeling approaches and two different data sets in support of the argument that border fortification increases illicit trade flows. In this section, we further probe the relationship between walls and trade by further disaggregating the 4-digit data into differentiated versus undifferentiated products.

As pointed out by Rauch (1999) in his seminal treatment, cross-border trade in highly differentiated products, e.g., footwear, lumber, medications, tends to rely much more heavily on interpersonal networks. In contrast, trade in undifferentiated products, such as oil, gold, iron ore, are much less dependent on interpersonal trading networks as these goods have global reference prices and do not vary nearly as much in price or characteristics across borders (see Rotunno & Vézina (2012) for a good discussion). Two key differences between differentiated goods and undifferentiated goods have important implications for the effect of walls on illicit flows.

First, if border walls push smugglers to increasingly employ misinvoicing to facilitate illicit flows as we argue above, highly differentiated goods that have more country-specific or localized pricing will be more attractive options (Mishra, Subramanian & Topalova 2008, Javorcik & Narciso 2008, Rotunno & Vézina 2012). Simply put, the prices of undifferentiated goods with known regional or global reference prices are harder to manipulate or distort. Illicit traders will prefer to work with goods for which prices are more highly variable, which should make mis-invoicing more difficult to detect.

Second, as Rauch (1999) points out, international trade in differentiated products relies much more heavily on interpersonal cross-border networks. This is important as smuggling organizations can leverage these existing networks in several ways to move illicit goods across a border. First, gaining the cooperation of or control over a business that is embedded in an existing interpersonal trading network provides a relatively seamless method of accessing an established cross-border trading relationship. For instance, (Naim 2006, 65–67) describes a medium sized Mexican company in a border town that specializes in construction materials that had slowly become engaged in illicit-cross border trade. Materials used in the construction industry, such as building stones or lumber, are highly differentiated products that tend to rely on interpersonal trading networks. Businesses embedded in interpersonal cross-border trading networks have established trust that can facilitate infiltration of existing trade, something which often takes years to establish (Rotunno & Vézina 2012). The ability of illicit traders to coopt the interpersonal cross-border networks that facilitate trade in differentiated goods interacts with the fact that prices for these products are more variable and less standardized to make this portion of cross-border trade ideal for smuggling.

To investigate these ideas, we disaggregate trade into differentiated and undifferentiated products at the 4-digit level and replicate the models estimated in Table 2.[10] We use the conservative classification scheme developed by Rauch (1999) to ensure that we adhere to the strictest coding of an undifferentiated good in his classification scheme.[11] Specifically, we separately calculate Gini coefficients for differentiated and undifferentiated products[12] and then replicate the six models reported in Table 2, which leads to the twelve estimated models in Table 3. Everything else about all models is identical to what is presented above, with the only difference being that we separately estimate regressions for Gini coefficients using differentiated goods, and Gini coefficients using undifferentiated goods, i.e., Models I-A and I-B.

The results across all model specifications demonstrate that trade in highly differentiated products drives the estimated relationship between *Border Barrier* and concentrated discrepancies in reported trade. Across all models, our models using the Gini coefficient over

[10]Following conventional practices in the literature, we use 4-digit commodity data. Less disaggregated coding choices would group commodities that are differentiated and undifferentiated into the same category.

[11]See the appendix for results using Rauch's less-restrictive classification scheme.

[12]Here we combine both reference priced and formal exchange commodities as they are both undifferentiated good types. See the appendix for models that separate commodities into all three categories.

Table 3: Differentiated Goods, Illicit Trade and Border Walls: Level 4 Trade Data

	Model I-A (OLS)	Model I-B (OLS)	Model II-A (OLS)	Model II-B (OLS)	Model III-A (OLS)	Model III-B (OLS)	Model IV-A (OLS)	Model IV-B (OLS)	Model V-A (Poisson)	Model V-B (Poisson)	Model VI-A (Poisson)	Model VI-B (Poisson)
Border Barrier	0.036** (0.01)	0.007 (0.01)	0.035** (0.01)	0.003 (0.01)	0.025** (0.01)	0.006 (0.01)	0.023** (0.01)	0.004 (0.01)	0.061** (0.02)	0.018 (0.02)	0.056** (0.01)	0.013 (0.02)
Log of Total Trade	0.046** (0.00)	0.067** (0.00)	0.043** (0.00)	0.068** (0.00)	0.031** (0.00)	0.045** (0.00)	0.029** (0.00)	0.045** (0.00)	0.086** (0.00)	0.121** (0.00)	0.078** (0.00)	0.122** (0.01)
Democratic Dyad			-0.001 (0.01)	0.006 (0.01)			-0.000 (0.01)	0.006 (0.01)			-0.001 (0.02)	0.015 (0.02)
Defensive Alliance			-0.003 (0.01)	-0.007 (0.01)			-0.002 (0.00)	-0.003 (0.01)			-0.007 (0.01)	-0.008 (0.01)
Territorial Dispute			0.003 (0.01)	0.009 (0.01)			0.002 (0.01)	0.007 (0.01)			0.003 (0.02)	0.020 (0.02)
Preferential Trade Agreement			0.001 (0.01)	-0.004 (0.01)			0.001 (0.01)	-0.003 (0.01)			0.005 (0.02)	-0.006 (0.02)
Common Currency			0.002 (0.03)	0.019 (0.02)			0.001 (0.02)	0.016 (0.01)			0.006 (0.03)	0.038 (0.02)
Both in GATT/WTO			-0.018 (0.02)	0.006 (0.03)			-0.015 (0.02)	0.001 (0.02)			-0.043 (0.03)	0.003 (0.05)
Constant	-0.048 (0.04)	-0.429** (0.04)	0.015 (0.05)	-0.444** (0.05)	0.009 (0.03)	-0.247** (0.03)	0.058 (0.04)	-0.255** (0.03)	-1.805** (0.08)	-2.460** (0.09)	-1.648** (0.09)	-2.494** (0.10)
Dependent Variable	Gini Diff Goods	Gini Undiff Goods	Gini Diff Goods	Gini Undiff Goods	Ln Gini Diff Goods	Ln Gini Undiff Goods	Ln Gini Diff Goods	Ln Gini Undiff Goods	Gini Diff Goods	Gini Undiff Goods	Gini Diff Goods	Gini Undiff Goods
Country-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Directed Dyadic FE	Yes	Yes	Yes	Yes	Yes	Yes.	Yes	Yes	Yes	Yes	Yes	Yes
N	29917	29158	23902	23563	29917	29158	23902	23563	29869	29081	23873	23533

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

differentiated products, i.e., the Models with A in their label, mirror those reported in Tables 1 and 2, while the results for undifferentiated products, i.e., the Models with B in their label, show no significant relationship with border fortification (although the coefficients all remain positive). This evidence lends additional nuance and credibility to the finding that border walls increase illicit flows. We now further probe the plausibility of our findings by exploring the temporal dynamics that underlie them.

Temporal Effects

The results in Tables 1 and 2 establish that when two states' mutual border is fortified, this increases the reporting discrepancies associated with illicit trade flows. Here we further probe this finding by analyzing the temporal dynamics behind this effect. Theoretically, we would expect smugglers and other actors engaged in illicit trade to build the capacity to move illicit goods through ports of entry over time as the barrier remains in place. In other words, we expect the effect of a border barrier on illicit trade flows to increase across time, as opposed to being constant. In Table 4 we include an interaction between *Border Barrier* and *Log Years Barrier Present*, which measures the logged number of years since the barrier has been in place.[13] We follow Carter & Goemans (2018) in logging the time since border barrier variable. The log transformation suggests that the effect of additional years wanes in importance as time marches on.[14]

The results in Tables 4 and 5 demonstrate the effects of border fortification on illicit trade increase across time. Specifically, the interaction between *Border Barrier* and *Log Years Barrier Present* is positive and significant across all models, although the significance falls to the .10 level in three of the four OLS models with the logged Gini coefficient as the dependent variable, i.e., Models III and IV in Table 4 and Model III in Table 5. The estimate

[13] Before logging the temporal variable, the first year is recorded with a value of 1.

[14] See the appendix for similar results using an unlogged *Years Since Border Barrier*.

Table 4: Temporal Effects of Illicit Trade and Border Walls: Level 3 Trade Data

	Model I (OLS)	Model II (OLS)	Model III (OLS)	Model IV (OLS)	Model V (Poisson)	Model VI (Poisson)
Border Barrier	-0.010	0.004	-0.006	0.001	-0.018	-0.004
	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)
Log Years Barrier Present	-0.008	-0.006	-0.005	-0.004	-0.013	-0.010
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Border Barrier x Log Years Barrier Present	0.025**	0.019*	0.016**	0.014*	0.042**	0.035**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Log of Total Trade	0.060**	0.059**	0.040**	0.039**	0.103**	0.098**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Democratic Dyad		0.007		0.005		0.013
		(0.01)		(0.01)		(0.02)
Defensive Alliance		-0.004		-0.003		-0.006
		(0.01)		(0.00)		(0.01)
Territorial Dispute		-0.004		-0.003		-0.006
		(0.01)		(0.01)		(0.01)
Preferential Trade Agreement		-0.009		-0.006		-0.016
		(0.01)		(0.01)		(0.02)
Common Currency		0.022		0.015		0.040*
		(0.02)		(0.01)		(0.02)
Both in GATT/WTO		-0.014		-0.014		-0.037
		(0.02)		(0.02)		(0.03)
Constant	-0.285**	-0.252**	-0.136**	-0.104**	-2.095**	-1.978**
	(0.04)	(0.05)	(0.03)	(0.03)	(0.07)	(0.08)
Dependent Variable	Gini	Gini	Ln Gini	Ln Gini	Gini	Gini
Country-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Directed Dyadic Fixed Effects	Yes	Yes	Yes	Yes.	Yes	Yes
N	30641	24388	30641	24388	30615	24378

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

for *Border Barrier* alone represents the effect of the border fortification in the first year it is built, i.e., *Log Years Barrier Present* equals zero, and is small and statistically insignificant across all specifications.[15] Thus, the effect of border fortification on reporting discrepancies is not significant in the very first year, although the effect does significantly increase across time subsequently.[16]

In Tables 6 and 7 we break down the effect of the time since a border barrier is constructed by decade. Thus, while *Log Years Barrier Present* produces a smooth and continuous (increasing) effect across time, it is possible that the temporal effect of border wall construction shifts in discontinuous ways across time. In the following analysis, we produce a factor variable that takes a value of one each decade of age for each border barrier in the data and then interact each category with *Border Barrier*. Thus, the variable *Ten to Twenty Years* indicates that a border wall is over a decade old, but less than two decades old.[17] The excluded category in all models is the first decade, where a wall is between 1 and 10 years old.

The results across all models in Tables 6 and 7 suggest that the effect of *Border Barrier* on illicit trade is strongest in the second and third decade of a border barrier, waning subsequently. While the interaction between *Border Barrier* and *Forty to Fifty Years* or *Fifty Plus Years* are significant in some of the specifications, the interactions with *Ten to Twenty Years* and *Twenty to Thirty Years* are consistently significant at the .05 level across all specifications. The estimate for the *Border Barrier* variable indicates the effect in the first decade (the excluded category), and is positive in all models, but only modestly significant in Model VI in Table 6. These results provide further evidence that the effect of border walls

[15] The individual estimate for *Log Years Barrier Present* is not very meaningful, as this variable does not take positive values in the absence of a border wall, i.e., *Border Barrier* equals zero.

[16] It should be noted that there are only 120 first year observations, which is a function of 60 border walls and two directed dyads per wall.

[17] In the interest of space, we do not show the estimates for the individual decade indicators, as they are not meaningful. Note that a border barrier is never ten to twenty years old in a case where it has not been build, i.e., *Border Barrier* is equal to zero.

Table 5: Temporal Effects of Illicit Trade and Border Walls: Level 4 Trade Data

	Model I (OLS)	Model II (OLS)	Model III (OLS)	Model IV (OLS)	Model V (Poisson)	Model VI (Poisson)
Border Barrier	-0.008	0.002	-0.005	-0.001	-0.011	-0.008
	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Log Years Barrier Present	-0.010	-0.007	-0.007	-0.004	-0.018	-0.008
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Border Barrier x	0.026**	0.019*	0.017**	0.014**	0.043**	0.033**
Log Years Barrier Present	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Log of Total Trade	0.061**	0.059**	0.040**	0.039**	0.105**	0.098**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Democratic Dyad		0.005		0.004		0.007
		(0.01)		(0.01)		(0.01)
Defensive Alliance		-0.009*		-0.006		-0.014**
		(0.01)		(0.00)		(0.01)
Territorial Dispute		-0.001		-0.001		-0.002
		(0.01)		(0.01)		(0.01)
Preferential Trade Agreement		-0.011		-0.008		-0.019
		(0.01)		(0.01)		(0.02)
Common Currency		0.029		0.020		0.050**
		(0.02)		(0.01)		(0.02)
Both in GATT/WTO		-0.014		-0.013		-0.033
		(0.02)		(0.01)		(0.03)
Constant	-0.272**	-0.224**	-0.127**	-0.085**	-2.092**	-1.940**
	(0.04)	(0.04)	(0.03)	(0.03)	(0.08)	(0.08)
Dependent Variable	Gini	Gini	Ln Gini	Ln Gini	Gini	Gini
Country-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Directed Dyadic Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
N	30463	24316	30463	24316	30437	24308

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

on illicit trade is not immediate, but rather grows after a wall is about a decade old, wanes slightly after two decades and then decreases further after thirty years have passed.

Conclusion

Our research has sought to bridge a gap in the burgeoning literature on the causes and consequences of border wall construction by linking a common justification – combating illicit trade – to the effects of border wall construction. We argue that border walls increase the levels of illicit trade as border walls divert illicit trade activity from land routes to ports of entry. The increased cost of using ports of entry pushes out less professionalized illicit trade networks and replaces them with more robust and professionalized ones. In order to systematically test this argument, we develop a novel measurement of illicit trade using mirrored reporting from over five decades of cross-national fine-grained commodity level trade data. We find a robust positive relationship between border wall construction and subsequent increases in the concentration of discrepant trade reports.

Our manuscript has numerous important contributions to the study of illicit trade and borders. First, this paper provides the first, as far as we know, systematic investigation of the effect of border walls on illicit flows of trade. Our finding that border walls increase the concentration of misreporting in trade data is consistent with dyad specific findings (e.g. (Getmansky, Grossman & Wright 2019)). Importantly, border walls are an extreme form of border securitization but a variety of less severe and less costly policy options exist that may disrupt land routes. The effect of these policies (e.g. increasing border patrols) on illicit trade flows should also be systematically investigated.

Second, we develop a new comprehensive measure of illicit economic activity derived from mirror statistics. Mirror statistics are a straightforward concept that operate based on the assumption that both parties involved in an international transaction should report equal. Where previous work has focused on discrepancies in reported trade between a single pair of countries (Fisman & Wei 2004), between one country and a number of trade partners (Bhagwati 1964), or in trade of a specific commodity (Fisman & Wei 2009). In contrast with work that has sought to uncover the specific mechanisms by which illicit economic exchange occurs, we seek to detect illicit trade in the aggregate.

We create a mirror statistics-derived measure of irregularities in economic exchange between two trading partners. Drawing on insights from Forstater (2018), we argue that patterns of discrepancies in mirror statistics between trading partners can be broadly classified into two different categories: unintentional reporting errors and intentional misinvoicing. Reporting errors are a form of measurement error, and should reduce the efficiency of estimates of true levels of trade, but not bias in. In contrast, misinvoicing is a deliberate attempt to bias estimates of trade. We seek to distinguish between these two scenarios by looking for small numbers of commodities that report widely discrepant mirror statistics in contrast with trade portfolios characterized by high levels of minimal discrepancy across many commodities.

In order to distinguish between these two patterns of reporting discrepancies, we calculate a Gini coefficient of mirror statistics in each directed-dyad-year. The Gini coefficient is a measure of concentration, so directed-dyad-years with higher Gini coefficients are characterized by large discrepancies in a handful of commodities, while those with low Gini coefficients represent directed-dyad-years with either (functionally) no reporting discrepancies, or widespread by negligible discrepancies. This measure cannot provide insight into the precise mechanism by which illicit exchange occurs, but does capture total levels of illict activity. This measure can also be used in analyses of how factors such as shared regime type, common free trade agreement membership, or military hostility affect levels of illicit economic exchange between states. This study only examines states with shared borders, where long travel times are unlikely to drive discrepancies due to departures and arrivals falling on opposite sides of a calendar year (Forstater 2018), so other studies that use this measure should control for the distance between members of a dyad.

Third, we find that the subsequent increase in illicit trade flows arise from a concentration in discrepancies from reports of differentiated goods. We argue that if border walls push smugglers to a misinvoicing strategy at ports of entry, then differentiated goods become easier to smuggle as the price of differentiated goods is easily distorted. Further, the licit trade of differentiated goods has been linked to dense interpersonal networks that have been shown to translate to illicit networks. Our findings are consistent with previous work linking differentiated goods with increased rates of smuggling (Javorcik & Narciso 2008, Rotunno & Vézina 2012). The disaggregation of trade flows into differentiated and undifferentiated products can be fruitfully exploited to investigate the consequences of border wall construction on the movement of licit trade that requires intensive use of interpersonal networks.

Finally, our research has important implications for policymakers. Our findings suggests that border walls are not an effective means of reducing illicit trade but rather a mechanism to divert cross-border smuggling to more sophisticated methods. Given the steady pattern of enhanced economic globalization, the commensurate growth of illicit trade, and a global pandemic the heightened demand for border walls is unlikely to go away. Policymakers need to consider whether building a border wall to deter illicit trade is worth the investment given the consequences.

Table 6: Decade Effects of Illicit Trade and Border Walls: Level 3 Trade Data

	Model I (OLS)	Model II (OLS)	Model III (OLS)	Model IV (OLS)	Model V (Poisson)	Model VI (Poisson)
Border Barrier	0.010	0.018	0.007	0.012	0.015	0.024*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Border Barrier x Ten to Twenty Years	0.144**	0.143**	0.100**	0.098**	0.258**	0.245**
	(0.05)	(0.05)	(0.04)	(0.04)	(0.07)	(0.07)
Border Barrier x Twenty to Thirty Years	0.098**	0.100*	0.065**	0.071*	0.178**	0.187**
	(0.04)	(0.05)	(0.03)	(0.04)	(0.06)	(0.07)
Border Barrier x Thirty to Forty Years	0.049	0.015	0.031	0.015	0.064	0.037
	(0.04)	(0.04)	(0.03)	(0.02)	(0.05)	(0.04)
Border Barrier x Forty to Fifty Years	0.055	0.058	0.033	0.045*	0.093	0.119**
	(0.04)	(0.04)	(0.03)	(0.03)	(0.06)	(0.05)
Border Barrier x Fifty Plus Years	0.090*	0.026	0.056*	0.020	0.155**	0.060
	(0.05)	(0.04)	(0.03)	(0.03)	(0.06)	(0.05)
Log of Total Trade	0.061**	0.059**	0.040**	0.039**	0.104**	0.098**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Democratic Dyad		0.007		0.005		0.013
		(0.01)		(0.01)		(0.02)
Defensive Alliance		-0.004		-0.003		-0.006
		(0.01)		(0.00)		(0.01)
Territorial Dispute		-0.004		-0.003		-0.006
		(0.01)		(0.01)		(0.01)
Preferential Trade Agreement		-0.008		-0.006		-0.015
		(0.01)		(0.01)		(0.02)
Common Currency		0.020		0.013		0.034
		(0.02)		(0.01)		(0.02)
Both in GATT/WTO		-0.012		-0.012		-0.033
		(0.02)		(0.02)		(0.03)
Constant	-0.286**	-0.254**	-0.137**	-0.106**	-2.098**	-1.984**
	(0.04)	(0.05)	(0.03)	(0.03)	(0.07)	(0.08)
Dependent Variable	Gini	Gini	Ln Gini	Ln Gini	Gini	Gini
Country-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Directed Dyadic Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
N	30641	24388	30641	24388	30615	24378

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

Table 7: Decade Effects of Illicit Trade and Border Walls: Level 4 Trade Data

	Model I (OLS)	Model II (OLS)	Model III (OLS)	Model IV (OLS)	Model V (Poisson)	Model VI (Poisson)
Border Barrier	0.009	0.015	0.007	0.010	0.014	0.020
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Border Barrier x Ten to Twenty Years	0.142**	0.120**	0.106**	0.086**	0.288**	0.211**
	(0.06)	(0.05)	(0.04)	(0.04)	(0.09)	(0.07)
Border Barrier x Twenty to Thirty Years	0.098**	0.093*	0.064**	0.065*	0.168**	0.172**
	(0.04)	(0.05)	(0.03)	(0.04)	(0.06)	(0.06)
Border Barrier x Thirty to Forty Years	0.044	0.026	0.029	0.020	0.052	0.037
	(0.04)	(0.03)	(0.02)	(0.02)	(0.05)	(0.04)
Border Barrier x Forty to Fifty Years	0.057	0.071**	0.033	0.053**	0.075	0.125**
	(0.04)	(0.04)	(0.03)	(0.03)	(0.06)	(0.05)
Border Barrier x Fifty Plus Years	0.089*	0.032	0.057*	0.023	0.152**	0.072
	(0.05)	(0.04)	(0.03)	(0.03)	(0.06)	(0.05)
Log of Total Trade	0.061**	0.059**	0.040**	0.039**	0.105**	0.098**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Democratic Dyad		0.005		0.004		0.006
		(0.01)		(0.01)		(0.01)
Defensive Alliance		-0.009*		-0.006*		-0.014**
		(0.01)		(0.00)		(0.01)
Territorial Dispute		-0.001		-0.001		-0.001
		(0.01)		(0.01)		(0.01)
Preferential Trade Agreement		-0.010		-0.007		-0.018
		(0.01)		(0.01)		(0.02)
Common Currency		0.028		0.019		0.044*
		(0.02)		(0.01)		(0.02)
Both in GATT/WTO		-0.013		-0.012		-0.029
		(0.02)		(0.01)		(0.03)
Constant	-0.273**	-0.226**	-0.127**	-0.087**	-2.095**	-1.947**
	(0.04)	(0.04)	(0.03)	(0.03)	(0.07)	(0.08)
Dependent Variable	Gini	Gini	Ln Gini	Ln Gini	Gini	Gini
Country-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Directed Dyadic Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
N	30463	24316	30463	24316	30437	24308

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

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