Where there's a will, there's a way: Border walls and refugees

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Abstract

Over the last decade, there has been a notable surge in the movement of refugees across international borders, posing significant challenges for the international community. In response, various policy measures have been implemented, including the construction of border walls, with the aim of impeding refugee influx. However, scholars have expressed doubts regarding the effectiveness of these fortifications, suggesting that walls merely redirect migrants to alternative routes, discourage return migration, or alter migrants' cost-benefit calculations. Despite these concerns, there has been a lack of rigorous testing to support or refute these claims beyond case-specific evidence. This paper addresses this research gap by thoroughly examining the arguments surrounding the impact of border fencing on refugee flows. We conduct a systematic, cross-national test of these arguments with a two-way fixed-effects estimator, an equivalence test, and a recently developed matching estimator designed for use on time-series cross-sectional data. Our results strongly support those who are skeptical of the impact of walls. We consistently demonstrate either that border fencing has not had any causal impact on refugee flows between 1970 and 2017 or that the statistical state-of-the-art is incapable of discerning that true effect. In either scenario, the evidence suggests that border fences fail to deliver the anticipated outcomes. These findings hold significant implications as violence-driven refugee flows persist, underscoring that while walls may serve as politically attractive tools for populist leaders, their actual deterrent effects are highly questionable at best.

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Introduction

Refugees have long represented both a humanitarian tragedy and a political challenge for states in regions affected by political violence. However, the international community has recently faced a significant upsurge in refugee flows across international borders. According to the UNHCR, the number of refugees under their mandate has nearly doubled between 2010 and 2020 to more than 26 million (Karasapan, 2020).[1] This figure does not include the millions of internally displaced persons who have been forced from their homes but have not crossed a border.

Growing concerns over migration and refugees correspond to the rising importance of border control (Simmons & Kenwick, 2022). As the diversity, intensity, and pace of migration rose post-Cold War, migration politics became a hot-button issue and increasingly wedded to national security concerns, particularly in the Global North (Adamson, 2006). These concerns extend to refugees and often lead states to build border walls. The Arab Spring, followed by the ongoing Syrian civil war and the European migration 'crisis,' reinvigorated debates in the developed world about controlling access to territory. Indeed, states have built 61 new border walls since 1945, 14 of which have been built since 2012.[2] In all 14 cases, leaders cited migration concerns as the primary justification for their construction (Benedicto & Brunet, 2018), which accords with observations of a positive correlation between the uptick in global refugee flows and walls' proliferation.[3]

Of course, walls serve myriad purposes, such as preventing militancy (Linebarger & Braithwaite, 2020) and smuggling (Carter & Poast, 2017; Vallet, 2020). Moreover, states may have multiple and potentially overlapping rationales for wall construction. For example, Turkey supposedly intends for its new wall on the Iranian border to reduce anticipated refugee flows from Afghanistan,[4] but the wall may serve other purposes such as precluding the spread of militancy (Linebarger & Braithwaite, 2020), non-state actors (Hassner & Wittenberg, 2015), terrorism (Avdan & Gelpi, 2017), or pandemics

[1] Refugees are individuals who are outside their country of nationality and unable to return due to a 'well-founded fear of being persecuted for reasons of race, religion, nationality, membership of a particular social group or political opinion' (UNHCR, 2007).

[2] We use 'barrier,' 'wall,' and 'fence' interchangeably below.

[3] https://www.dw.com/en/as-migration-is-rising-so-are-border-barriers/a-58848161

[4] https://www.euronews.com/2021/08/20/turkey-builds-a-border-wall-to-stop-refugees-from-afghanistan

(Kenwick & Simmons, 2020).

The recent acceleration of border barrier construction is especially striking for two reasons. First, walls come with a hefty price tag: The Trump administration infamously asked Congress for $23 billion for enhanced border security and $18 billion for the southern border wall (Ainsley, Ainsley). Controversy over funding the wall led to a 35-day government shutdown in December 2018. Second, most extant scholarship finds that walls are ineffective. The prevailing wisdom among political geographers holds that walls do not impede migration because they can be circumvented (Dear, 2013a) and do not fully enclose the border (Vallet, 2020). Walls may even backfire, by discouraging return migration and motivating more migrants to apply for asylum (Schon & Leblang, 2021). If walls work at all, they do so when reinforced with other measures such as surveillance, razor wire, heat sensors, movement detectors, drones, and patrol personnel (Hassner & Wittenberg, 2015). In other words, where there is a will, there is a way.

Despite these issues, numerous politicians extol walls' ability to impede territorial access and tout them as a palliative for migratory pressures. Most notably, Donald Trump argued that a wall on the US-Mexico border was necessary 'to make it very hard [for migrants] to come in,'[5] while British politicians justified the 'Great Wall of Calais' as the best way to prevent 'illegals' from seeking asylum in the UK.[6] And in recent years, Greece has extended its border wall to deter migrants from entering the EU.[7] Implicit and explicit in these justifications is the assumption that, despite the cost, *border walls work and keep people out*.

We argue that a lack of systematic large-N studies permits this disconnect between scholarship and political practice. While scholars have made important strides in documenting the ineffectiveness of walls in stemming migration, much extant scholarship is based on case studies of particular borders (Jones, 2012) and micro-analyses of specific border segments (Chambers et al., 2021). Schon & Leblang (2021) makes a valuable step toward systematic, large-N scrutiny of walls' effects on migration. Our paper uses an alternative approach to extend this line of inquiry that circumvents the

[5] https://www.nytimes.com/2017/01/25/us/politics/refugees-immigrants-wall-trump.html

[6] https://www.bbc.com/news/uk-37421525

[7] https://www.theguardian.com/world/2020/oct/20/greece-extends-wall-on-turkish-border-as-refugee-row-deepens

problems inherent to an instrumental variables framework.[8]

This paper offers the first broadly comparative systematic empirical examination of walls' effects on refugee flows. We draw on work that shows how walls restrict terrorism and trade flows to develop and test arguments for the effect of border fences on refugee flows. We conduct three analyses to test these arguments: 1) a two-way fixed-effects estimator, 2) a statistical equivalence test, and 3) a recently developed statistical matching estimator for time-series cross-sectional data (Imai, Kim & Wang, 2021).

Our results corroborate the scholarly skepticism over walls' effectiveness: Border fences do not appear to have the effect that leaders frequently use to justify their expensive construction. Specifically, we fail to reject the hypothesis that border fences have not affected refugee flows between 1970 and 2017. However, we do not assert that our null hypothesis is true. It is not possible to accept a null hypothesis; one can either reject or fail to reject the null. Instead, at a minimum, we can state with confidence that the current statistical state-of-the-art is incapable of discerning any such effect. As a result, we flip the burden of proof on leaders who argue that border fencing is a 'solution' to the humanitarian crisis created by refugee flows.

Refugees and border walls

The number of refugees has nearly doubled in the last decade. As of early 2020, there were nearly 26 million refugees living in foreign states. The Syrian civil war fueled the refugee crisis, propelling an outpouring of 6.6 million refugees, most of whom still reside in refugee camps (Karasapan, 2020).

While some states in the developing world, such as Germany, have opened their doors to millions of refugees and sought to integrate them into society (Brücker, Jaschke & Kosyakova, 2019), migration has generally been framed as a 'crisis,' especially in the cases of Mediterranean migrants to the EU and Central American migrants to the US. Many other nations—including Turkey, Hungary, and the United States—have sought instead to keep refugees out of their territory by constructing border

[8] Notable challenges to IV approaches include satisfying the exclusion restriction and inconsistent parameter estimates due to weak instruments. See, e.g. Sovey & Green (2011).

walls and fences. While governments cite a host of reasons for constructing border barriers—ranging from drug trafficking and smuggling to terrorism—controlling migration often tops the list of professed policy motivations (Benedicto & Brunet, 2018; Rosière & Jones, 2012). In addition, leaders may score domestic brownie points by constructing barriers even if they have minimal utility. These diversionary tactics can bolster leaders' flagging popularity and increase their chances for political survival (Linebarger & Braithwaite, 2022).

Animosity toward refugees neglects the infusion of human capital resulting from refugee inflows (Betts et al., 2017; Taylor et al., 2016). Contrary to fears that refugees drain host state's welfare systems, studies find that refugee populations actively contribute to the host economy, bringing with them new skills and assets (Cali & Sekkarie, 2015; Taylor et al., 2016; Alloush et al., 2017), in addition to micro-level benign effects (Taylor et al., 2016). Unfortunately, however, the objective economic dividends may not be readily evident, and perceptions of refugees as fiscal burdens may persist, particularly if policy hurdles prevent refugees' seamless integration into the host's labor market (De Haas et al., 2019).

Several other factors underpin motivations to limit refugee inflows. First, refugees fleeing conflict and repression raise concerns over the transmission of conflict across borders and the worsening of tensions with neighboring states (Echevarria & Gardeazabal, 2016). Second, they can also shift demographics and may compound existing grievances (Saideman & Ayres, 2000). Third, states may fear that economic migrants disguised as refugees will exploit the asylum system (Neumayer, 2005). Finally, a large volume of refugees will present a challenge as the adjudication of claims to asylum is a lengthy process during which time states must accommodate refugees on their soil.

To be sure, this restrictive stance mostly exists in the Global North (Abdelaaty, 2021; Blair, Grossman & Weinstein, 2022a). While Global North states have increased their policy restrictiveness, Global South states have liberalized over time Abdelaaty (2021); Blair, Grossman & Weinstein (2022a). This liberalization has invited a growing number of refugees (Blair, Grossman & Weinstein, 2022b). However, anti-migrant sentiment persists in the Global South (Buehler, Fabbe & Han, 2020), which provides the same impetus for leaders to build walls, as has been seen in southern Africa.

Despite the presumption that walls are effective, many scholars argue that walls do not impede migration. Next, we review these arguments and present countervailing perspectives.

Theorizing the impact of border fences on refugee inflows

Arguments against the effect of walls

We lean on the robust consensus among political geographers that walls are ineffective at inhibiting migratory flows (Vallet, 2020), and the notion of adaptation by agents.

First, rather than restricting flows, walls may have unintended spatial effects on migration, by redirecting migrants to un-fenced stretches of the border. Most long borders are not completely fenced (Cannon, 2016; Sterling, 2009), and they are also difficult to guard, especially with difficult terrain (Linebarger & Braithwaite, 2020). Thus, walls may not deter migrants, but instead shift migrants to unguarded and more dangerous routes. This type of displacement effect comports with the 'funnel effect' or the 'balloon effect,' whereby deterrent measures, such as enhanced surveillance infrastructure, amplify the physiological difficulty of traversing difficult terrain and force migrants into tougher routes (Chambers et al., 2021).

Second, walls may backfire and increase refugee inflows. How does this backfire occur? (Schon & Leblang, 2021: 2622) contend that walls deter *return* because they increase the costs of return and re-migration. So, barriers lead people to apply for asylum if they cannot stay with permanent status, thereby increasing overall refugee flows.

Third, if walls are one instrument among many in the state's arsenal, then they likely work in conjunction with other instruments of border control and deterrence policies (Helbling & Leblang, 2019; Linebarger & Braithwaite, 2020). While Hassner & Wittenberg (2015) argue that other policies bolster walls' effectiveness, taken to the extreme, this argument suggests that walls have no independent effects on their own. Additionally, traditional drivers of refugee flows such as foreign policy concerns, economic interests, and humanitarian norms may dwarf the effects of walls (Blair, Grossman & Weinstein, 2022a; Moorthy & Brathwaite, 2019).

Fourth, ethnic kin networks can facilitate adaptation, by allowing refugees to revise routes of travel (Helbling & Leblang, 2019). These networks also provide information about the de jure rights and de facto conditions of potential destination countries (Blair, Grossman & Weinstein, 2022b). Information networks have always existed for migrant populations, but mobile phone technology and internet penetration substantially facilitate the transmission of information about policies (Blair, Grossman & Weinstein, 2022a). All told, information flows add to the gravitational pull of migrant diasporas (Blair, Grossman & Weinstein, 2022a).

Fifth, states often face pressures to pursue contradictory policies, some of which undermine the effectiveness of walls. For example, the U.S. has opened new legal ports of entry to funnel licit goods and increased border fortifications simultaneously. Private interest groups have a stake in maintaining economic efficiency and preventing congestion (Dear, 2013b), which conflicts with the goals of an effective barrier.

Sixth, and related, private and public agencies in neighboring states have existing transborder ties, which create pressures to keep the border partially open. Criminal syndicates also facilitate refugee adaptation as these organizations forge ties across the border and have a stake in facilitating illicit flows (Bove & Böhmelt, 2016).[9] Corruption on either or both sides of the border deflates the utility of the fence by expanding the pool of beneficiaries from unauthorized border crossings (Sviatschi, 2018a,b).

Finally, walls may be less germane where states permit entry of migrants without documentation. While there is variation among states on entry requirements, most states' de jure policies explicitly permit entry without visas, and in some cases they even allow refugees to cross at otherwise unofficial points along the border (Blair, Grossman & Weinstein, 2022a).

These factors lead us to specify our null hypothesis:

H_1 (null): Border walls have no overall impact on refugee flows to destination states.

[9] This is less relevant for the modal refugee, who flees in the Global South where reliance on smugglers for crossing borders is uncommon. We thank an anonymous reviewer for this excellent point.

Arguments for the effect of walls

Despite widely-shared skepticism that walls work, we now present several interrelated causal mechanisms through which walls affect refugee flows.

The wider body of work on migration and border control has shown that other policies do, under some conditions, reduce migratory flows. Policies may override the effects of pull factors such as proximity, a healthy economic outlook and thriving job market, common ties such as shared cultural and linguistic heritage (Fitzgerald, Leblang & Teets, 2014). Tighter controls may also redirect migrants to nearby states with laxer policies or deflect them into other categories (Brekke, Røed & Schøne, 2017; Schon & Leblang, 2021). If walls are one tool in the state's toolkit, then it stands to reason that walls reduce refugee inflows too.

At their core, border barriers are tools of deterrence, coercion, and immigration enforcement (Schon & Leblang, 2021). In the extreme, one can imagine that a border wall might make passage physically impossible, but a more likely scenario is that travelers will exploit alternative avenues of access. To the extent that tunnels, smugglers, and other mechanisms allow migrants to sidestep the barrier, its effects will be muted. Instead, as with any policy of deterrence or coercion, walls are bargaining tools. They affect perceptions of the costs of migration, the probability that one will evade capture, and the resolve of their opponent to continue making the act of migration (and their status as refugees) costly and unattractive. In short, border walls affect refugees directly and indirectly.

The first set of mechanisms posits that walls directly thwart entry for migrants arriving by land, the modal case for refugees (Karasapan, 2020). This mechanism will affect migrants who seek to traverse international borders without documentation and/or authorized supervision, where states refuse entry without documentation.

In contrast, walls are less consequential for economic migrants equipped with travel, work, student, investment, and other visas, permits, or visa waivers. These migrants use supervised air, land, or maritime ports for entry. Likewise, where de jure policies allow refugees to cross without visas, walls would be of lesser import (Blair, Grossman & Weinstein, 2022a).

As a first direct mechanism, border fencing increases the costs of the migration journey for potential refugees. These costs may include increased travel through difficult terrain to circumvent the wall, increased costs in bribes or payments to smugglers to help them across, or increased wait times associated with remapping and redrawing access routes. Concomitantly, border fencing increases the risks of interceptions and apprehension by state authorities, thereby injecting greater uncertainty into smuggling operations. For migrants, these additional risks entail a higher price for the journey, as smugglers shift the added costs onto prospective migrants (Roberts et al., 2010).

Second, barriers increase the probability that refugees will be detained or captured and repatriated (Coleman & Kocher, 2011: 229). States combine border fences with border patrols and larger policing efforts to prevent access. Border walls force migrants to move in exposed or narrow spaces that facilitate their capture and detention.

In addition to presenting logistical impediments, border walls dissuade entry through their indirect, symbolic role. Walls create perceptions that the state is tough on migration and these perceptions matter. For example, a media firestorm emerged in the first few months of the Biden Administration that claimed that perceptions of Biden's supposed 'softness' on immigration unleashed a 'surge' of migrants from Central America (Ponnuru, Ponnuru). Even if this perception does not accurately reflect the facts on the ground, beliefs about the government's overall stance on migration significantly shape migratory pressures.

Insofar as a wall captures the state's general 'border orientation,' á la Simmons & Kenwick (2022), it can steer migrants away. While this may not be as relevant for refugees who lack the time to strategically adapt to policies, it can parallel the deterrent effects of harsh asylum determination policies (Thielemann, 2009). The urgency of flight for refugees fleeing conflict and persecution may limit the resources and time necessary for strategic adaptation, but nevertheless, just as stringent policies propel spatial deflection to states perceived to be more welcoming (Brekke, Røed & Schøne, 2017). That is, migrants may be driven off to 'greener pastures'—more welcoming destination countries—if they perceive the walls to be indicative of an environment hostile to newcomers.

For all three of these reasons, we expect the erection of border walls to cause potential refugees either to seek alternative destinations or to remain in their homeland because both alternatives will become relatively more attractive than attempting migration to the wall-constructing destination country.

H2: Border walls decrease refugee inflows for destination states.

Empirical analysis: Three tests

To test these competing hypotheses, we investigate whether border fence construction has an appreciable association with the number of refugees in a destination state in a given year. In our analyses, we fail to reject the null hypothesis that border fences are uncorrelated with refugee arrivals. While one cannot accept the null hypothesis in a classic null hypothesis significance test, we show that our tests cannot detect a significant association. This process involves several steep inferential hurdles for which we proclaim no miraculous solution; causal inference on observational data is difficult. Accordingly, we present *three* tests to corroborate our results: a two-way fixed effects estimator, a statistical equivalence test, and a new, nonparameteric matching technique. Taken together, our evidence suggests that the impact of border fences is so imperceptibly small as to render the material and social costs of their construction and policing irresponsible.

Main variable definitions

Previous studies on the determinants of refugee flight use UN High Commissioner for Refugees (UNHCR) refugee stocks data (e.g. Uzonyi, 2015). However, Shaver et al. (2022) show that these data make refugee flows appear rarer than they are in reality, which introduces bias. A large number of individual claimants attempt to gain asylum in a given year, complicating accurate data collection by the UNHCR (Crisp, 1999). As such, only including reported refugees ignores this relevant population, which walls could affect. Moreover, refugee stocks may change for reasons unrelated to yearly refugee arrivals, such as births and deaths among the existing population, as well as resettlement.

Accordingly, our dependent variable is the inverse hyperbolic sine-transformed (IHS) asylum seeker *arrival rate*, which we calculate as the number of asylum applications and prima facie refugee arrivals in a country-year, normalized by population (Blair, Grossman & Weinstein, 2022b). These data also come from the UNHCR (UNHCR, UNHCR). We use the inverse hyperbolic sine transformation because it behaves similarly to the natural logarithm and is defined at 0.

We use Avdan & Gelpi (2017)'s data to derive our border wall measure.[10] The original data includes all border walls built from 1945 to 2007, and we extend this variable to 2015. To do so, we relied upon several sources, including the World Population Review's list of countries with border walls,[11] Wikipedia's list of updated border walls,[12] and the Transnational Institute's report for the European Union and Schengen zone(Benedicto & Brunet, 2018). Additionally, we cross-checked our updated list of border barriers with scholarly pieces that have recently assembled and examined data on border barriers (Carter & Poast, 2020; Linebarger & Braithwaite, 2020).

The original Avdan & Gelpi (2017) data is directed-dyadic, and it records the building and target state of border barriers, the year that construction began; and, for defunct barriers, the year that the barrier was dismantled. In this paper, however, we investigate whether states with border walls dampen refugee inflows. To do so, we convert this data into a monadic variable that measures whether a given state has a barrier on any of its borders in a given year. Our primary independent variable takes on the value of 1 for a fenced country-year and 0 otherwise. This binary approach is standard in the published literature on the effects of border walls (see e.g. Avdan & Gelpi, 2017; Carter & Poast, 2020; Hassner & Wittenberg, 2015; Linebarger & Braithwaite, 2020).

For example, for the United States-Mexico dyad, our monadic dataset would yield '1' for the US and '0' for Mexico because the US is the builder and Mexico is the target. There are cases of countries that are builders on one frontier and targets on another. For example, Turkey built a wall with Syria in 2013 and is constructing another against Iran, but it has also been the target of walls erected by Bulgaria in 2014, and Greece in 2012. Our variable shows a '1' for Turkey from 2013 through 2017 because it is the building state for those years. Finally, there are a handful of cases where a state dismantles a wall and constructs a new one. Going back to the Bulgaria-Turkey example, Bulgaria

[10]While walls are variegated in structure and length, we make no distinction among different types of barriers. However, our results are robust to using Simmons & Kenwick (2022)'s more fine-grained border orientation measure. See, appendix table D.VII.

[11]`https://worldpopulationreview.com/country-rankings/countries-with-border-walls`

[12]`https://en.wikipedia.org/wiki/Border_barrier#List_of_current_barriers`

had a wall in place from 1949 until 1989. In the dyadic version of the data, the Bulgaria-Turkey border would be coded as '1' from 1949 to 1989, '0' until 2014, and '1' from 2014 and on. Our data accounts for these 'treatment reversals.'

It merits noting that the building state may not necessarily be the destination state. In fact, states on the periphery of the EU (e.g. Hungary, Poland) or just outside its borders (e.g. Turkey) are countries of transit for migrants fleeing to more attractive states, such as Germany. Such transit states may well encounter refugee influxes amassed on their walled borders as regional magnet states pull refugees. The effects of walls on transit migration lies beyond the scope of our study.

Finally, most states have not built border fences: Only 370 of our country-year observations are 'treated.' The rare-events nature of border fences increases uncertainty in estimated effects. We acknowledge this issue, and our multiple tests and robustness checks look to use the most credible methods possible to infer an effect. Absent finding an effect under these conditions, our approach still remains a scientific, evidence-based method for determining whether expensive and divisive border fences 'work.'

Test 1: Two-way fixed effects model

If we are interested in the effect of a time-varying shock, there are two problems with the standard linear regression model. First, there are country-specific, time-invariant variables that are often endogenous and will introduce bias. Second, there is time-dependence in the time-varying border fence variable. Border fences are negatively autocorrelated: if a country builds a border fence in a given year, it is unlikely to build one in subsequent years. Moreover, this variable is likely to take at least one year to impact refugee flows. This produces a correlation between border fence construction in the current year and the error term.

The two-way fixed effects estimator is the standard method for estimating causal effects under these conditions. The country fixed-effects control for unobserved, unit-specific and time-invariant confounders when estimating causal effects from observational data, while the year fixed effects account for any common shocks that influence refugee flows, such as a global economic downturn

(Angrist & Pischke, 2008). Accordingly, in the first part of our analysis, we estimate the following linear regression model with country and time fixed effects,

$$Y_{it} = a_i + \gamma_t + \beta X_{i,t} + \sigma^T Z_{i,t} + e_{it}$$

X_{it} is the treatment variable—an indicator for whether a country has a physical border fence in year t. Y_{it} measures the log number of refugees in country i in year t. Z_{it} represents the set of time-varying covariates including log per-capita GDP, an indicator for whether bordering states are experiencing a major episode of political violence, an international conflict indicator, Varieties of Democracy (V-Dem) liberal democracy index, and whether a state is an EU member. We control for EU membership because the Schengen zone is a unique case of free mobility and because it is positively correlated with refugee demand and border fence construction. In addition, we also include a GDP shock variable (and its lag) that measures whether a given country has experienced a sharp decline in its economic production. To create the GDP shock variable, we calculate the change in GDP for each country-year ($GDP_t - GDP_{t-1}$), and then define the bottom 15% of these changes as GDP shocks (Nielsen et al., 2011). The GDP data come from the World Bank Development Indicators and the violence data come from the Center for Systemic Peace Major Episodes of Political Violence data (Marshall, 2019).

The results of this initial analysis is in table I.[13] We specify four models. In Model (1), we include all of the independent variables and use two-way fixed effects to identify the effect of building a border fence on refugee flows. In Model (2), we control for lagged refugee flows. In Model (3), we add a country-specific linear time trend. In Model (4), we include lagged refugee flows, unit fixed effects, and a country-specific time trend. We cluster standard errors by country in each of the models.

In each model, we fail to reject the null hypothesis that border walls do not affect refugee flows into states that build them.[14] Although the estimated coefficient is positive in each model, the estimated standard errors are sufficiently large to cast serious doubt on any appreciable effect of border walls.

[13]The country-years in our sample are found in appendix table A.I.

[14]We explore heterogeneity in these effects in appendix E. In appendix C, we replicate this analysis with a binary dependent variable, and the results are consistent.

Instead, we find some evidence that international conflict, violent conflicts in neighboring states, EU membership, and liberal democracy are associated with refugee flows. Finally, and unsurprisingly, Models (2)–(4) estimate a strong, positive association between lagged refugees and contemporaneous refugees.

To be sure, this effect may be unique to the Global North; Global South countries are liberalizing their policies (Abdelaaty, 2021; Blair, Grossman & Weinstein, 2022a) and attracting more refugees (Blair, Grossman & Weinstein, 2022b). The Global South hosts 85% of the world's displaced persons, so these states likely approach policy-making from a different perspective (Abdelaaty, 2021). Thus, using walls to restrict refugee flows might only be a strategy in the Global North. We test whether the effect of border walls systematically differs between the North and South in appendix B. To do so, we replicate the analysis in table I on global North and South samples. In all cases, we report insignificant effects of walls, though the parameter estimates are negative in the global North and positive in the global South.

Table I in here

Robustness: Dyadic models

We use a monadic approach to draw these conclusions, but recent studies use a dyadic unit-of-analysis (Blair, Grossman & Weinstein, 2022b; Linebarger & Braithwaite, 2020). One benefit of dyadic models is that one can account for cross-border connections between sending and receiving countries. As a robustness check, we conduct such a dyadic analysis in appendix D. In these models, we include many of the same control variables as in table I, as well as dyadic variables such as contiguity, capital distance, and existing migrant stocks.[15]

The results are in appendix table D.V. Model (1) is a dyadic replication of table I, and we again fail to reject the hypothesis of no effect. In addition, we interact the fences variable with Blair, Grossman & Weinstein (2022b)'s displacement policy liberality index (Model 2) and a similar index from the

[15] We also restrict the sample to contiguous dyads. The findings in appendix table D.VI are consistent.

DEMIG policy database (Model 3) (DEMIG, 2015) to measure whether policy restrictiveness moderates the effect of the walls. In Model (2), we report a significant negative interaction, but in Model (3) we estimate an insignificant interaction. The former sample includes Global South states and the latter includes Global North states, which suggests that the moderating effect is sample-dependent. This result provides further evidence that border fences do not work in the Global North.

Test 2: Equivalence test

It could be the case that border fences prevent inflows of refugees, but our analysis is underpowered to detect such an effect. Moreover, in a classic null hypothesis statistical test, it is not possible to accept a null hypothesis; one can either reject the null if the test statistic falls outside the critical region or fail to reject it. In the latter case, the researcher can claim that the test did not detect a significant effect, but they cannot claim that the null hypothesis is true.

In response, we conduct an *equivalence test* to infer whether our estimates and their uncertainty (i.e. confidence interval) in table I fall within a region of 'practical equivalence' around zero. In other words, while we cannot statistically argue that an effect is zero, we can 'statistically reject effects large enough to be deemed worthwhile' (Lakens, 2017: 355). Pharmacologists developed equivalence tests to warrant claims that a new, cheaper drug works as well as existing, more expensive drugs (Hauck & Anderson, 1984). This goal resembles our analysis in this paper: we do not claim that border fences have a Platonically zero effect on refugee admission. Rather, we claim that the effect of border fences is so small or imperceptible as to render their expensive construction, maintenance, and security useless.

To conduct an equivalence test, one first must specify a range of effect sizes that is worthwhile to examine. Scholars typically specify their effect sizes in terms of Cohen's d and use Cohen's rules of thumb for specifying effect sizes: large effects are roughly 0.8, medium effects are 0.5, small effects are 0.3, and negligible effects are 0.1. We follow Kruschke (2018), who recommends that scholars use a standardized negligible effect size ($0.1 \times SD_Y$) as their region of practical equivalence (ROPE). We specify an upper (δ_u) and lower (δ_l) equivalence bound based on our smallest effect size of interest (i.e. 0.28 in this case). Then, we test two composite, one-sided null hypotheses: 1) $H_1 \leq -\delta_l$ and 2) $H_2 \geq \delta_u$. If we reject both of these hypotheses at a given level of significance ($\alpha = 0.05$), we conclude that the observed effect falls within the equivalence bounds and is practically equivalent to zero (Seaman & Serlin, 1998). If the coefficient is statistically significant and the narrow confidence intervals (i.e. $1 - 2\alpha$) include or exceed the ROPE, we reject the hypothesis of practical equivalence.

In Figure 1, we report equivalence tests for Table I. Three results emerge. The results in all four models are practically equivalent to zero because the entire confidence interval lies within the equivalence bounds and intersects zero. These results provide additional evidence that border fences do not appear to affect refugee flows. Regardless of whether the effect is either equivalent to or indistinguishable from zero, this analysis raises the burden of proof on policymakers who claim that fences are effective. To be sure, this test is not a panacea. Equivalence tests require us to specify effect sizes that we consider 'worthwhile,' which presents a researcher degree-of-freedom. Be that as it may, these results are robust to effect sizes that are smaller than negligible.

Figure 1 in here

Test 3: Nonparametric matching

The results in Table I and Figure 1 suggest that border fences do not inhibit refugee flows. However, states do not randomly assign border fence construction, which poses a threat to causal identification. Accordingly, one must make several assumptions to substantiate this causal interpretation.

First, in linear fixed-effects models, most scholars acknowledge that they must assume strict exogeneity of the disturbance term to causally identify β. Scholars use a variety of regression, matching, and weighting techniques to satisfy this assumption. Unfortunately, causal inference presents more complications in time-series cross-sectional data that add additional, often unseen, complexity to identification.

One must also assume that 1) past treatments do not directly affect current outcomes, and 2) past outcomes do not affect the current treatment. This latter assumption is what (Imai & Kim, 2019: 470) call the 'no carryover effects' assumption. Therefore, we must assume, in our example, that previously constructed border fences do not directly affect contemporaneous refugee flows and that previous refugee flows do not affect whether a state constructs a border fence in given year. These assumptions are unrealistic in our empirical context, In fact, obvious violations of these assumptions *motivate* this study. Moreover, Imai & Kim (2021) show that the two-way fixed-effects estimator is incapable of adjusting simultaneously for unobserved unit-specific and unobserved time-specific confounders.

To interrogate the credibility of the null findings above, we adopt the nonparametric matching methods that Imai & Kim (2019); Imai, Kim & Wang (2021) develop to estimate causal effects in time-series cross-sectional data. This nonparametric matching approach clarifies the exact counterfactual outcomes that one uses to estimate causal effects. To be specific, one first finds a set of control observations for each treated observation that has the same treatment history up to the pre-specified number of years. This group of control observations is called a *matched set*, and one then uses any of the standard matching or weighting techniques to further refine this match set. This refinement step ensures that treated and control observations are as balanced as possible across the covariates of interest.

This method matches treated units that were previously untreated to never-treated units. Both the treated and untreated units have the same treatment history in a pre-specified lag window, but the control units remain untreated in the period the treated unit receives the treatment. In our example, Botswana is a control unit until 1997, the year in which it builds a border fence. If we specify a lag window of 4 periods, then the method matches Botswana with control units that share an identical treatment history from 1993–1996 but do not build border fences in 1997.

Then, one simply applies the difference-in-differences estimator to estimate the causal effect of interest. In most cases (such as this one), the causal estimand of interest is the average treatment effect of border fences among the treated (ATT). The differences-in-differences estimator is equivalent to the linear two-way fixed effects estimator if there are only two time periods and the treatment occurs only in the second period. Imai, Kim & Wang (2021)'s estimator is a useful extension because it generalizes to more than two time periods and each unit may receive the treatment multiple times.

To be specific, we define the average treatment effect of building a border fence on the number of refugees as,

$$\delta(F, L) = E\{Y_{i,t+F}(X_{it} = 1, X_{it} = 0, \{X_{i,t-l}\}_{l=2}^{L})\} - \\ Y_{i,t+F}(X_{it} = 0, X_{it} = 0, \{X_{i,t-l}\}_{l=2}^{L}) | X_{it} = 1, X_{it-1} = 0\}$$

In this expression, we choose F as the number of leads, which represents the refugee inflow at F time periods after the state built a border wall. In this set-up, $F = 1$ would be the effect of building a border fence on the refugee inflow one year in the future. We also set L, which is the length of the lag window. An advantage of this method over the two-way fixed effects estimator is that it provides a straightforward way to estimate the treatment effect effect for an arbitrary number of future periods. For example, $\delta(1, 4)$ is the ATT of building a border fence on refugee flows one year in the future while assuming that the potential outcomes depend on the four previous years of treatment history.

The choice of F and L is important because we must assume that the potential (control) outcome for country i at time $t + F$ depends neither on the treatment status of other countries in that year (i.e. $X_{i',t'}$) nor on its own treatment status after L years (i.e. $\{X_{i,t-l}\}_{L+1}^{t-1}$). In other words, we must assume no spillover effects—the potential outcomes of one country cannot affect the potential outcomes of others—but we can allow for some carryover effects. We conduct two robustness checks below to assuage concerns over potential violations of this assumption.[16]

Finally, following Imai, Kim & Wang (2021), we make the same parallel trends assumption as the differences-in-differences framework. In other words, after controlling for treatment, outcome, and covariate histories, the difference between the treatment and control group is constant over time. This assumption is important because the causal quantity of interest—the average treatment effect on the treated—is computed by taking the estimated differences in the outcome between the treat-

[16]Linebarger & Braithwaite (2022) find evidence of global diffusion of fortification, and one country's fence could push migrants elsewhere, thereby triggering an additional impetus to build fences. For example, during the height of the migration 'crisis' from 2015 to 2017, walls channeled migrants in southeastern Europe to other countries, which were then inspired to follow suit by walling off their borders.

ment group and the control group. The control observations are meant to approximate the values that the treated group *would have taken* in the absence of the treatment. As such, the difference between the treated and control groups approximates the difference between the treated group and its counterfactual outcome in the absence of the treatment. If the parallel trends assumption does not hold, then the analyst exaggerates the difference between the treatment and control groups, which leads to an upward biased effect. In this case, we find a null effect; so, even if the parallel trends assumption does not hold, then we would be exaggerating a null finding. A violation of the parallel trends assumption would therefore make it more difficult for us to find a null effect in this analysis.

Figure 2 in here

The results of the nonparametric matching estimator are similar to those in Table I. In Figure 2, we present the average treatment effect of building a border fence on refugee flows for those states that have already built a border fence. In this analysis, we estimate the effect of border fences up to ten periods in the future because Blair, Grossman & Weinstein (2022b) find that policies persist for up to 10 years after the treatment. We also assume that the potential outcomes of fence construction only depend on the four previous years of treatment history. In addition, we include a similar set of covariates as Table I to create the matched sets. Specifically, we match on refugee flows, log per-capita GDP, nearby conflict, EU membership, and log population, as well as four lags of each. We use mahalanobis matching to refine the matched sets because it provides the best covariate balance.[17] A list of country-years in the treatment group can be found in Appendix table **??**.

The results are in Figure 2. First, we find further evidence that border fence construction does not appear to affect the volume of refugee flows, even up to ten years in the future. For skeptics of Table I, this figure should provide added confidence in the result. In fact, all the estimated ATT point estimates are positive, indicating even more strongly that border walls fail to restrict refugee flows.

These results conform with extant findings. Ironically, barriers may incentivize more sophisticated groups to begin smuggling; and those groups are often involved in other forms of organized

[17] See, Appendix Figure F.4 for a balance test plot.

crime (Tinti & Reitano, 2018: 32). The end result is that barriers not only animate innovation by opportunist smugglers but also change the landscape of criminal activity, empowering more established and networked criminal organizations to operate. Thus, the weight of the evidence remains against policymakers who argue in favor of border walls, even after accounting for the potential endogeneity of border walls to previous refugee flows, and even after accounting for the potentially time-variant effects of border wall construction.

Robustness: Generalized synthetic control and panel event study

We must assume both parallel trends and homogeneous treatment effects in this analysis, both of which likely do not hold. We conduct two further robustness checks to assuage concerns over these potential violations. The results are in appendix H.

First, we estimate a generalized synthetic control model, which does not require the parallel trends assumption (Xu, 2017). To do so, we estimate counterfactual refugee arrivals for treated countries in post-treatment periods. The treatment effect is the difference between observed and counterfactual refugee arrivals for treated countries. Appendix figure H.5 provides another null finding: Border fences are associated with an insignificant increase in refugee arrivals (ATT: -0.058, 95% CI: $(-1.215, 1.097)$). Second, we conduct a panel event study, which a related approach (Sun & Abraham, 2021). This estimator also accommodates variation in treatment assignment but is robust to dynamic post-treatment effects. The results in appendix table H.X, and they are consistent with the other analyses (ATT: -0.078, 95% CI: $(-0.262, 0.106)$).

Conclusion

We began this article by drawing attention to the ongoing global refugee crisis, which has gained renewed momentum due to the continued civil conflict in a number of countries and has garnered increased attention after Russia's 2022 invasion of Ukraine. Getting 'tough' on border control, of which building fences is a visual manifestation, has lent some world leaders an appealing tactic for mollifying public anxiety about a migration deluge.

To be sure, our null findings do not negate that leaders may get domestic bang for their buck by building walls. But, such benefits do not hinge on the objective effectiveness of borders in impeding entry by those deemed undesirable. Rather, the iconic value of walls as symbols of state power and markers of national identity feed populist agendas (Brown, 2010), irrespective of walls' utility in monitoring transborder movement. And these incentives are not the province of right-wing and anti-immigrant leaders. Exerting territorial control is a cherished part of state sovereignty, and inextricably linked to statehood (Rosenberg, 2022; Rudolph, 2005). Thus, even as globalists may bemoan the lack of a borderless world (Sassen, 1996), it comes as no surprise that when faced with the prospect of uncontrolled and unauthorized flows, states respond by erecting physical impediments across their frontiers. All told, walls may be futile but serve other purposes, helping leaders ease public anxieties over insecurity, reaffirm sovereignty, enrich vested interests, as well as partake in security theater (Andreas, 2011).

The problem, of course, is that many scholars argue that walls hold scant utility (Vallet, 2020: 10). Regardless, the received wisdom has not stopped leaders from claiming that their states should expend enormous resources to build border walls *because they believe they will hold back migrants*. Our results corroborate this longstanding wisdom, demonstrating that while building fences may be an effective symbolic rhetorical ploy in the public relations toolbox, they do not mitigate refugee inflows. The two-way fixed effects models show insignificant effects of border fences on refugee inflows. The equivalence tests estimate these effects as statistically equivalent to zero. And the non-parametric matching model corroborates these findings, showing that fence construction has no discernible effect on the volume of refugees, even up to ten years into the future. To reiterate, we cannot be certain whether border walls truly have zero effect on refugee flows or whether our estimate is too noisy to discern that true effect. Regardless, this uncertainty flips the burden of proof back onto the leaders clamoring for expensive walls that are unlikely to achieve their stated policy aim.

Our paper lays the groundwork for understanding the effects of fences in stemming refugee inflows. Readers may speculate about whether these effects are heterogeneous and contingent upon country characteristics, other border and migration policies, and the nature of frontiers. We remain agnostic about these possibilities. The two-level fixed effects design accommodates unit-level heterogeneity and we explore some possible moderators in the Appendix, but future work should go further.

This analysis is not the final verdict and lends itself to several interpretations. For example, earlier scholarship finds that walls may be effective in conjunction with other instruments of control at the border (Hassner & Wittenberg, 2015). This validates scholars' exhortations for a multifaceted approach to border security (Vallet & David, 2014; Vallet, 2020). So, walls may do little on their own, but they might become more effective when combined with measures such as increased surveillance, policing, and biometric technologies. Nevertheless, the adaptability of agents may mean that actors find means to circumvent even high-tech barriers.

Our study also points to a number of other research avenues. For example, one possible explanation of our findings is that fences may simply displace flows to porous sections of the border. Another possibility is that border walls, while high profile, may simply add nothing to modern states' sophisticated toolbox of refugee and asylum policies. A third mechanism is that individual agents—be they individual refugees or criminal entrepreneurs—are adaptable and canny. Our results are consistent with the notion that as states build walls, migrants adapt and innovate and find alternate pathways to cross borders.

Another line of future inquiry could examine the conditions under which barriers might be rendered more effective. While anecdotal evidence seems to cast doubt on the utility of virtual fences, to our knowledge, scholars have not empirically probed how different border control policies work together in averting unwanted access. A related conjecture is that the structure of the border fence itself can bear on its effectiveness. Another angle of investigation would build upon recent work showing that the nature of terrain conditions the effects of fences vis-á-vis militancy (Linebarger & Braithwaite, 2020).

We beckon scholars to examine these and related questions. They are ripe for exploration and timely, given that border walls remain very much in vogue.

Replication data

Replication data: The datasets and code for the empirical analysis in this article, along with the online appendix, can be found at http://www.prio.org/jpr/datasets. All analyses were conducted using R (Version 4.1.3).

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Appendix

Contents

A Two-way fixed-effects sample

state	From	To
Afghanistan	2003	2015
Albania	1995	2015
Algeria	1976	2015
Angola	1977	2015
Argentina	1972	2015
Armenia	1997	2015
Australia	1989	2015
Austria	1970	2015
Azerbaijan	2000	2015
Bahrain	2004	2015
Bangladesh	1978	2014
Belarus	1996	2015
Belgium	1972	2015
Benin	1984	2015
Bolivia	1987	2015
Bosnia and Herzegovina	1998	2015
Botswana	1970	2015
Brazil	1972	2015
Bulgaria	1991	2015
Burkina Faso	1986	2015
Burundi	1971	2015
Cambodia	1994	2015
Cameroon	1983	2015
Canada	1980	2015
Central African Republic	1970	2015
Chad	1994	2015
Chile	1972	2015
China	1981	2015
Colombia	1974	2015
Comoros	2001	2001
Congo	1990	2015
Costa Rica	1972	2015
Croatia	1998	2015
Cuba	1979	2013
Cyprus	1982	2015
Czech Republic	1995	2015
Denmark	1971	2015
Djibouti	1980	2015
Dominican Republic	1972	2015
Ecuador	1977	2013
Egypt	1970	2015
El Salvador	1979	2015
Eritrea	1995	2012
Estonia	1998	2015
Ethiopia	1970	2015
Fiji	2008	2014
Finland	1981	2015
France	1972	2015
Gabon	1990	2015
Gambia	1990	2015
Georgia	1999	2015
Germany	1992	2015
Ghana	1990	2015
Greece	1970	2015
Guatemala	1980	2015
Guinea	1990	2015
Guinea-Bissau	1990	2015
Haiti	2009	2015
Honduras	1978	2015
Hungary	1989	2015
Iceland	1992	2015
India	1971	2015
Indonesia	1978	2015
Iran	1980	2013
Iraq	1990	2015
Ireland	1991	2015
Israel	1999	2015
Italy	1970	2015
Ivory Coast	1971	2015
Jamaica	1992	2000
Japan	1978	2015
Jordan	1978	2015
Kazakhstan	1997	2015
Kenya	1970	2015
Kuwait	1985	2015
Kyrgyz Republic	2000	2015
Latvia	1998	2015
Lebanon	1972	2015
Lesotho	1977	2014
Liberia	1984	2014
Libya	1994	2013
Lithuania	1997	2015
Luxembourg	1995	2015
Macedonia	1999	2015
Madagascar	2000	2015
Malawi	1988	2015
Malaysia	1977	2015
Mali	1986	2015
Malta	1992	2015
Mauritania	1989	2014
Mauritius	2001	2001
Mexico	1972	2015
Moldova	1999	2015
Mongolia	2006	2015
Morocco	1996	2015
Mozambique	1996	2015
Namibia	1992	2015
Nepal	1989	2015
Netherlands	1980	2015
New Zealand	1992	2015
Nicaragua	1980	2015
Niger	1991	2015
Nigeria	1978	2015
Norway	1980	2015
Oman	2001	2015
Pakistan	1980	2015
Panama	1974	2015
Papua New Guinea	1982	2014
Paraguay	1994	2015
Peru	1972	2015
Philippines	1975	2015
Poland	1991	2015
Portugal	1980	2015
Qatar	1980	2015
Russian Federation	1992	2015
Rwanda	1972	2015
Saudi Arabia	1982	2015
Senegal	1971	2015
Sierra Leone	1982	2013
Singapore	1976	2012
Slovakia	1995	2015
Slovenia	1997	2015
Solomon Islands	2012	2012
Somalia	1978	2015
South Africa	1995	2015
South Korea	1978	2015
Spain	1981	2015
Sri Lanka	2000	2015
Sudan	1970	2015
Suriname	1992	1992
Swaziland	1976	2015
Sweden	1981	2015
Switzerland	1972	2015
Syria	1980	2007
Tajikistan	1994	2015
Tanzania	1970	2015
Thailand	1975	2015
Togo	1982	2015
Trinidad and Tobago	2007	2015
Tunisia	1995	2015
Turkey	1970	2015
Turkmenistan	1996	2011
Uganda	1970	2015
Ukraine	1997	2015
United Arab Emirates	1979	2013
United Kingdom	1979	2015
United States of America	1973	2015
Uruguay	1983	2015
Uzbekistan	1996	2005
Vanuatu	2008	2008
Venezuela	1972	2013
Vietnam	1986	1992
West Germany	1970	1990
Yemen, Rep.	1992	2015
Zambia	1970	2015
Zimbabwe	1980	2015

Table A.I: The country-years in the two-way fixed-effects analysis.

B Global North/South analysis

Table B.II: Two-way fixed-effects analysis on the Global North

	Dependent variable:			
	IHS Refugee Flow			
	(1)	(2)	(3)	(4)
Border Fence	−0.061	−0.054	0.062	0.031
	(0.116)	(0.067)	(0.035)	(0.026)
Lag IHS Refugees		0.683^{**}	0.448^{**}	0.455^{**}
		(0.077)	(0.070)	(0.077)
Log GDPc	−0.145	0.020	−0.056	−0.092
	(0.260)	(0.126)	(0.181)	(0.076)
Negative GDP Shock	0.007	0.054	0.019	−0.068
	(0.050)	(0.044)	(0.050)	(0.046)
GDP Shock (t-1)	−0.039	−0.012	−0.033	−0.028
	(0.084)	(0.064)	(0.047)	(0.054)
Nearby Conflict	0.006	−0.016	0.025	−0.005
	(0.056)	(0.045)	(0.041)	(0.038)
International Conflict	−0.100	0.039	−0.010	0.032
	(0.104)	(0.050)	(0.049)	(0.053)
Liberal Democracy	−0.241	0.066	−0.097	0.009
	(0.268)	(0.135)	(0.105)	(0.122)
EU Member	0.392	0.149	0.040	0.017
	(0.268)	(0.118)	(0.112)	(0.106)
Country FE	✓	✓	✓	✓
Year FE	✓	✓	✓	X
Country Time Trend	X	X	✓	✓
Observations	1,119	1,088	1,088	1,088
R^2	0.961	0.980	0.985	0.984
Adjusted R^2	0.957	0.978	0.983	0.982
Residual Std. Error	0.771 (df = 1027)	0.547 (df = 995)	0.486 (df = 957)	0.492 (df = 1001)
Note:				$^{\dagger}p<0.1$; $^{*}p<0.05$; $^{**}p<0.01$

Table B.III: Two-way fixed-effects analysis on the Global South

	Dependent variable:			
	IHS Refugee Flow			
	(1)	(2)	(3)	(4)
Border Fence	0.180	0.055	0.034	0.170
	(0.327)	(0.072)	(0.139)	(0.134)
Lag IHS Refugees		0.671**	0.427**	0.452**
		(0.068)	(0.106)	(0.107)
Log GDPc	0.096	0.037	−0.077	0.068
	(0.234)	(0.076)	(0.069)	(0.068)
Negative GDP Shock	−0.074	−0.017	−0.009	0.011
	(0.054)	(0.035)	(0.033)	(0.036)
GDP Shock (t-1)	−0.050	−0.002	0.014	0.029
	(0.067)	(0.038)	(0.034)	(0.033)
Nearby Conflict	0.072	0.086**	0.115**	0.062*
	(0.059)	(0.026)	(0.036)	(0.030)
International Conflict	−0.158	−0.076	−0.084	−0.036
	(0.093)	(0.041)	(0.047)	(0.041)
Liberal Democracy	0.452	0.137	0.119	−0.096
	(0.281)	(0.073)	(0.090)	(0.055)
Country FE	✓	✓	✓	✓
Year FE	✓	✓	✓	X
Country Time Trend	X	X	✓	✓
Observations	2,576	2,482	2,482	2,482
R^2	0.795	0.926	0.941	0.938
Adjusted R^2	0.781	0.921	0.934	0.932
Residual Std. Error	1.053 (df = 2407)	0.616 (df = 2317)	0.562 (df = 2206)	0.570 (df = 2250)

Note: †p<0.1; *p<0.05; **p<0.01

C Alternative dependent variable

In this appendix, we replicate the models in table I with an alternative dependent variable. Our alternative variable is whether a country experienced a refugee 'shock' in a given year. We define a refugee shock as a refugee inflow that is in the top 25% of all refugee inflows in a given year. The results are in table C.IV. We report the same null finding as above in all four models. These results corroborate our argument that border fences either have a null or an imperceptibly small effect that runs counter to the claims of policymakers.

Table C.IV: Replication of table I with binary DV

	Dependent variable:			
	Refugee Shock			
	(1)	(2)	(3)	(4)
Border Fence	0.024	0.022	−0.016	−0.016
	(0.031)	(0.016)	(0.013)	(0.013)
Lag Refugee Shock		0.427**	0.213**	0.212**
		(0.048)	(0.048)	(0.049)
Log GDPc	0.099*	0.056*	0.054*	0.036
	(0.039)	(0.024)	(0.022)	(0.019)
Negative GDP Shock	0.018	0.016	0.011	0.008
	(0.009)	(0.011)	(0.011)	(0.010)
GDP Shock (t-1)	0.035*	0.021	0.017	0.014
	(0.014)	(0.012)	(0.011)	(0.011)
Nearby Conflict	0.016	0.009	0.004	−0.0005
	(0.014)	(0.009)	(0.010)	(0.009)
International Conflict	−0.028	−0.018	−0.012	−0.019
	(0.024)	(0.015)	(0.015)	(0.017)
Liberal Democracy	−0.020	−0.004	−0.015	−0.011
	(0.038)	(0.024)	(0.027)	(0.028)
EU Member	0.080	0.034	−0.030	−0.029
	(0.048)	(0.021)	(0.030)	(0.032)
Country FE	✓	✓	✓	✓
Year FE	✓	✓	✓	X
Country Time Trend	X	X	✓	✓
Observations	3,695	3,570	3,570	3,570
R^2	0.753	0.806	0.839	0.834
Adjusted R^2	0.738	0.794	0.821	0.818
Residual Std. Error	0.224 (df = 3486)	0.197 (df = 3365)	0.184 (df = 3215)	0.186 (df = 3259)
Note:				†p<0.1; *p<0.05; **p<0.01

D Dyadic analysis

In this appendix, we conduct a dyadic analysis of the effect of border walls on refugee flows. This analysis is similar to recent work on the effect of displacement policies on refugee arrivals (Blair, Grossman & Weinstein, 2022b). The dependent variable is the same as the main analysis. However, we make two changes. We first assess the effect of a border fence on the bilateral flow of refugees, rather than on the arrival of all refugees in a given destination state. Then, we replicate this analysis with Simmons & Kenwick (2022)'s border orientation variable to test whether our results are robust to a more fine-grained border measure.

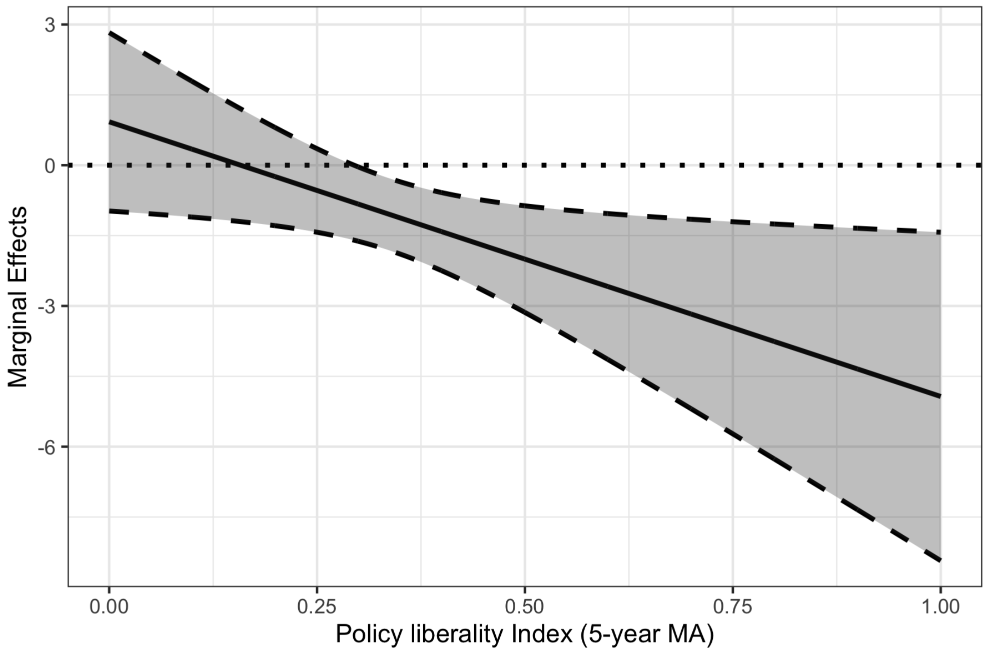
Main dyad analysis

The results in table D.V support the main findings above with a twist. In Model (1), we estimate a null effect of border fences that is near zero. However, it may be the case that a state's migration policy restrictiveness moderates the effect of border fences on refugee inflows. To test this proposition, we interact the border fence variable with Blair, Grossman & Weinstein (2022b)'s measure of displacement policy liberality (five-year moving average). In Model (2), we find a null effect for countries with low levels of policy liberality, but the effect of border walls becomes negative and significant for states at higher levels. The results of this interaction are in figure D.1a. While this analysis has identification issues, we infer that border fences only affect refugee arrivals in countries with relatively liberal refugee policies. However, the policy variable is only available for developing countries (Blair, Grossman & Weinstein, 2022a).[18]

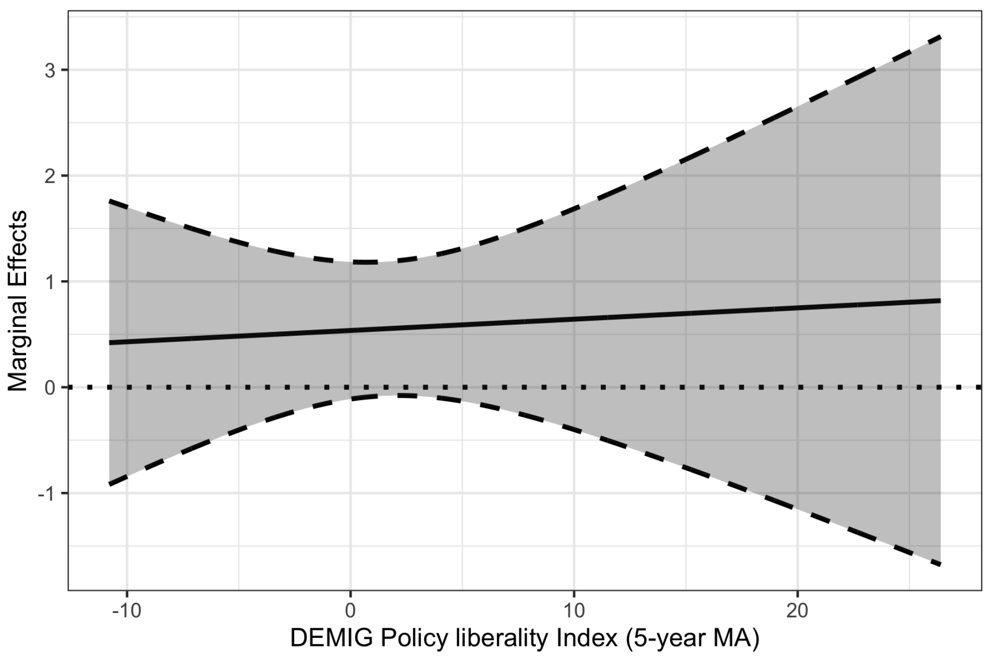
To discern whether this finding is unique to the Global South, we interact the border fence variable with an aggregated migration policy liberality index from the DEMIG Policy database (five-year moving average) (DEMIG, 2015). The DEMIG data record information on immigration policy changes for 45 mostly Global North countries from 1945 to 2013.[19] The results are in Model (3) and figure D.1b, and they contradict those in Model (2). We estimate an insignificant interaction between border fences and the DEMIG policy liberality variable, which suggests that the significant result in Model (2) is a function of the Global South sample. Future work should investigate how border walls interact with other migration and refugee policies to affect arrivals.

[18]Afghanistan, Angola, United Arab Emirates, Armenia, Azerbaijan, Burundi, Benin, Burkina Faso, Bangladesh, Bahrain, Bhutan, Botswana, Central African Republic, Côte d'Ivoire, Cameroon, Congo–Kinshasa, Congo–Brazzaville, Comoros, Cape Verde, Cyprus, Djibouti, Algeria, Egypt, Eritrea, Ethiopia, Gabon, Georgia, Ghana, Guinea, Gambia, Guinea-Bissau, Equatorial Guinea, India, Iran, Iraq, Israel, Jordan, Kazakhstan, Kenya, Kyrgyzstan, Kuwait, Lebanon, Liberia, Libya, Sri Lanka, Lesotho, Morocco, Madagascar, Maldives, Mali, Mozambique, Mauritania, Mauritius, Malawi, Namibia, Niger, Nigeria, Nepal, Oman, Pakistan, Qatar, Rwanda, Saudi Arabia, Sudan, Senegal, Sierra Leone, Somalia, South Sudan, São Tomé & Príncipe, Eswatini, Seychelles, Syria, Chad, Togo, Tajikistan, Turkmenistan, Tunisia, Turkey, Tanzania, Uganda, Uzbekistan, Yemen, South Africa, Zambia, and Zimbabwe.

[19]Argentina, Australia, Austria, Belgium, Brazil, Canada, Chile, China, Czech Republic, Czechoslovakia, Denmark, Finland, France, German Democratic Republic, Germany, Greece, Hungary, Iceland, India, Indonesia, Ireland, Israel, Italy, Japan, Luxembourg, Mexico, Morocco, Netherlands, New Zealand, Norway, Poland, Portugal, Russia, Slovak Republic, Slovenia, South Africa, South Korea, Spain, Sweden, Switzerland, Turkey, Ukraine, United Kingdom, United States of America, and Yugoslavia.



(a) Global South policy liberality (Blair, Grossman & Weinstein, 2022b).



(b) DEMIG policy liberality (DEMIG, 2015).

Figure D.1: This figure plots the marginal effect of border fences on refugee arrivals at different levels of policy liberality.

Table D.V: Dyadic models of border fences on refugee admission

	Dependent variable:		
	IHS Refugee Flow		
	(1)	(2)	(3)
Border Fence	−0.031	0.926	0.536
	(0.419)	(0.970)	(0.331)
Policy Liberality Index (5-year MA)		−1.124*	
		(0.536)	
DEMIG Policy Liberality (5-year MA)			−0.042**
			(0.014)
IHS Migrant Stock	0.048**	0.059**	0.095**
	(0.009)	(0.017)	(0.023)
IHS GDPc Ratio	−1.451**	−1.479**	−1.279**
	(0.151)	(0.272)	(0.358)
IHS GDPc Ratio (sq)	0.295**	0.257**	0.390**
	(0.043)	(0.071)	(0.085)
Contiguous	0.410**	0.935**	0.062
	(0.130)	(0.202)	(0.238)
Common Language	0.288**	0.201	0.065
	(0.065)	(0.130)	(0.162)
Colonial Tie	0.663**	−0.631	−0.190
	(0.152)	(0.429)	(0.334)
IHS Distance	−0.736**	−0.653**	−0.378**
	(0.046)	(0.104)	(0.101)
Lib. Democracy Origin	−0.310	0.317	1.514
	(0.461)	(0.672)	(1.182)
Lib. Democracy Dest.	−1.028**	−2.455**	−0.209
	(0.186)	(0.598)	(0.603)
IHS Population Origin	−1.718**	−2.597**	−0.572
	(0.369)	(0.821)	(0.999)
IHS Population Dest.	0.623**	−0.588	1.296*
	(0.191)	(0.774)	(0.637)
Near Violence Dest.	−0.036*	0.023	−0.110*
	(0.017)	(0.042)	(0.045)
Civil War Origin	0.274**	0.289**	0.234**
	(0.038)	(0.057)	(0.086)
International War Dest.	−0.002	0.046	0.166*
	(0.029)	(0.084)	(0.081)
International War Origin.	0.121*	0.076	0.310**
	(0.058)	(0.144)	(0.111)
Fence X Policy Liberality Index (5-year MA)		−5.856*	
		(2.631)	
Fence X DEMIG Policy Liberality (5-year MA)			0.011
			(0.049)
Origin FE	✓	✓	✓
Dest. FE	✓	✓	✓
Year FE	X	✓	✓
Observations	41,236	5,669	6,945
R^2	0.448	0.471	0.560
Adjusted R^2	0.443	0.453	0.548
Residual Std. Error	1.356 (df = 40894)	1.575 (df = 5482)	1.284 (df = 6759)

Note: †p<0.1; *p<0.05; **p<0.01

Contiguous dyads analysis

Given the instrumental nature of Hypothesis 2, it may be the case that the effect of border fences on refugee admission may only be relevant for dyads that share a land border. We replicate Model (1) in table D.V to test the effect of border fences on contiguous dyads. The results are in table D.VI, and they are consistent with the other findings in this analysis.

Dyadic border orientation analysis

Finally, we acknowledge that our border fence measure is coarse and does not account for the variance in border fortifications. To address this issue, we use Simmons & Kenwick (2022)'s border orientation measure to replicate the dyadic analysis in the previous subsections. Simmons & Kenwick (2022) use an item-response theory model to estimate a given state's border orientation on each of its borders.[20] In Model (1), we replicate Model (1) in table D.V. In Models (2) and (3), we include the DEMIG policy liberality variable (2) and its interaction with the border orientation variable (3). The results are in table D.VII. All three models corroborate the findings in table D.V and throughout the manuscript. We do not report a significant association between border orientation and refugee admissions.

[20] See, (Simmons & Kenwick, 2022: 860–61) for more details on the measurement model.

Table D.VI: Dyadic models of border fences on refugee admission: contiguous dyads

	Dependent variable:
	IHS Refugee Flow
Border Fence	0.026
	(0.644)
Policy Liberality Index (5-year MA)	0.023
	(0.038)
IHS Migrant Stock	−0.658
	(0.655)
IHS GDPc Ratio	−0.078
	(0.153)
IHS GDPc Ratio (sq)	−0.162
	(0.341)
Common Language	−1.480*
	(0.615)
Colonial Tie	−0.166
	(0.290)
IHS Distance	−0.460
	(1.751)
Lib. Democracy Origin	0.435
	(1.167)
Lib. Democracy Dest.	2.584
	(1.678)
IHS Population Origin	−4.213**
	(1.600)
IHS Population Dest.	−0.039
	(0.106)
Near Violence Dest.	0.606**
	(0.125)
Civil War Origin	−0.092
	(0.165)
International War Dest.	0.431
	(0.288)
Origin FE	✓
Dest. FE	✓
Year FE	✓
Observations	2,325
R^2	0.650
Adjusted R^2	0.606
Residual Std. Error	1.661 (df = 2066)
Note:	†p<0.1; *p<0.05; **p<0.01

Table D.VII: Replication of the dyadic analysis with the border orientation measure from Simmons & Kenwick (2022).

	Dependent variable:		
	IHS Refugee Flow		
	(1)	(2)	(3)
Border Orientation	0.116	0.220	−0.041
	(0.207)	(0.240)	(0.316)
DEMIG Policy Liberality (5-year MA)		−0.077	−0.111
		(0.058)	(0.084)
IHS Migrant Stock	0.028	0.088	0.074
	(0.047)	(0.063)	(0.066)
IHS GDPc Ratio	−0.816	2.030	2.043
	(0.761)	(1.873)	(1.853)
IHS GDPc Ratio (sq)	−0.291	−0.081	−0.062
	(0.199)	(0.643)	(0.631)
Common Language	−0.066	−1.662*	−1.583
	(0.488)	(0.839)	(0.884)
Colonial Tie	−1.732**	−4.236**	−4.328**
	(0.480)	(0.580)	(0.643)
IHS Distance	0.862**	1.661	1.338
	(0.307)	(1.435)	(1.509)
Lib. Democracy Origin	−2.234	−7.736	−8.157
	(1.961)	(4.147)	(4.206)
Lib. Democracy Dest.	2.335	3.850	3.904
	(1.386)	(2.348)	(2.364)
IHS Population Origin	4.413	2.715	2.888
	(2.806)	(3.185)	(3.140)
IHS Population Dest.	−5.055	6.573	6.152
	(3.279)	(5.102)	(4.886)
Near Violence Dest.	−0.005	−0.170	−0.150
	(0.129)	(0.255)	(0.257)
Civil War Origin	0.689**	−0.433	−0.489
	(0.190)	(0.422)	(0.435)
International War Dest.	−0.192	0.135	0.068
	(0.296)	(0.274)	(0.273)
International War Origin.	0.338	0.230	0.281
	(0.352)	(0.326)	(0.335)
Fence X DEMIG Policy Liberality (5-year MA)			0.062
			(0.073)
Origin FE	✓	✓	
Dest. FE	✓	✓	
Year FE	X	✓	
Observations	1,503	454	454
R^2	0.716	0.842	0.843
Adjusted R^2	0.666	0.801	0.802
Residual Std. Error	1.399 (df = 1279)	0.946 (df = 359)	0.944 (df = 358)
Note:			†p<0.1; *p<0.05; **p<0.01

E Heterogeneous effects

The analysis in Table I provides evidence that border fences do not affect the flow of refugees to states that build them. However, once may be concerned that estimating a single effect assumes a constant treatment effect across countries. If this assumption does not hold, then we would be left in the dark as to whether certain outlier countries are driving our null finding. Instead, it could be the case that border fences affect the flow of refugees for most countries but this effect is masked.

To investigate this possibility, we replicate Model (1) in Table I with a random effects specification that allows the effect of border fences to *vary* across countries. The results reveal a null finding that is consistent across most countries in our data set. The full model results are in Table E.VIII. In Figure E.2, we present the estimated effect of border fences for all countries. The Figure shows a strong null effect for nearly every country of interest, regardless of region. Few countries, such as Pakistan, Iran, and Slovenia, report a significant negative effect of border fences, and several other countries—like Jordan and Turkey—report significant positive effects. Taken together, these results provide further evidence we cannot identify a significant effect of border fences in general and that this null finding is not driven by heterogeneity across countries.

Ideally, we would use the new estimator proposed by Callaway & Sant'Anna (2021) to further explore this heterogeneity. The Callaway & Sant'Anna (2021) estimates group-time average treatment effects to account for effects that are heterogeneous across time. However, we cannot use this estimator on our data because 14 of our 24 cohorts—a group of countries that build fences in the same year— only have a single cross-section in them. We cannot use this estimator under these conditions. Future work should explore potential violations of the constant effects assumption.

In addition, we test whether any outlier countries may be affecting the effects in table I. We conduct a leave-one-out analysis that sequentially drops countries and reanalyzes Model (1). The results are in figure E.3, and they show consistent null findings for models that leave out every country other than Botswana. When we leave out Botswana, we report a positive and significant effect of border fences, which still contradicts claims that fences 'work' and decrease refugee flows.

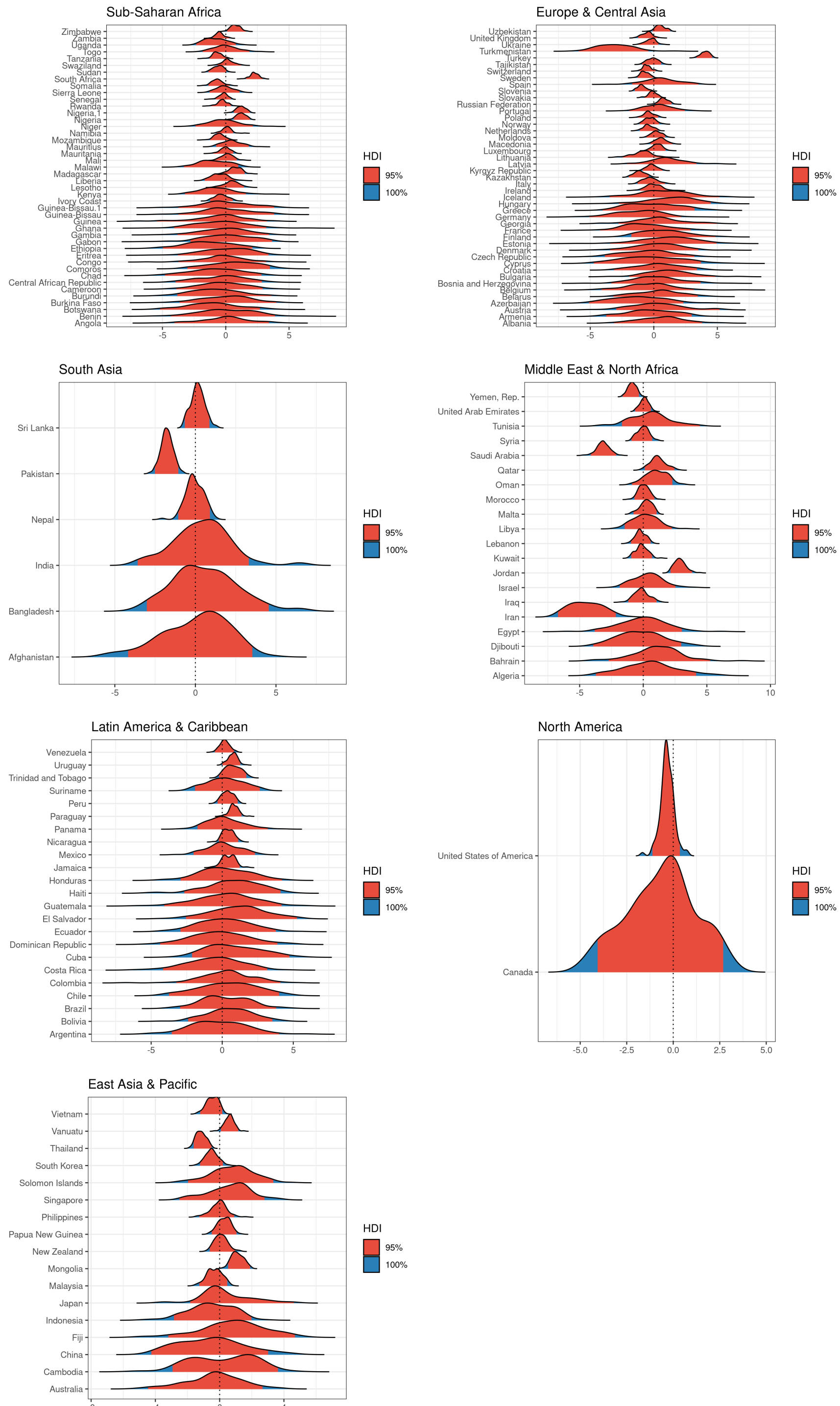


Figure E.2: Investigating treatment effect heterogeneity. Results from a random effects model with varying effects of border fences by country.

Table E.VIII: Random slopes model

	Dependent variable:
	IHS Refugee Flow
Border Fence	0.176
	(0.435)
Log GDPc	−0.272***
	(0.085)
Negative GDP Shock	−0.013
	(0.099)
GDP Shock (t-1)	0.021
	(0.099)
Nearby Conflict	0.803***
	(0.061)
International Conflict	−0.051
	(0.112)
Liberal Democracy	−0.401***
	(0.146)
EU Member	2.755***
	(0.247)
Log Population	0.008***
	(0.001)
Constant	6.126***
	(0.840)
Country RE	✓
Year RE	✓
Observations	6,795
Log Likelihood	-17,206.180
Akaike Inf. Crit.	34,442.370
Bayesian Inf. Crit.	34,544.730
Note:	*p<0.1; **p<0.05; ***p<0.01

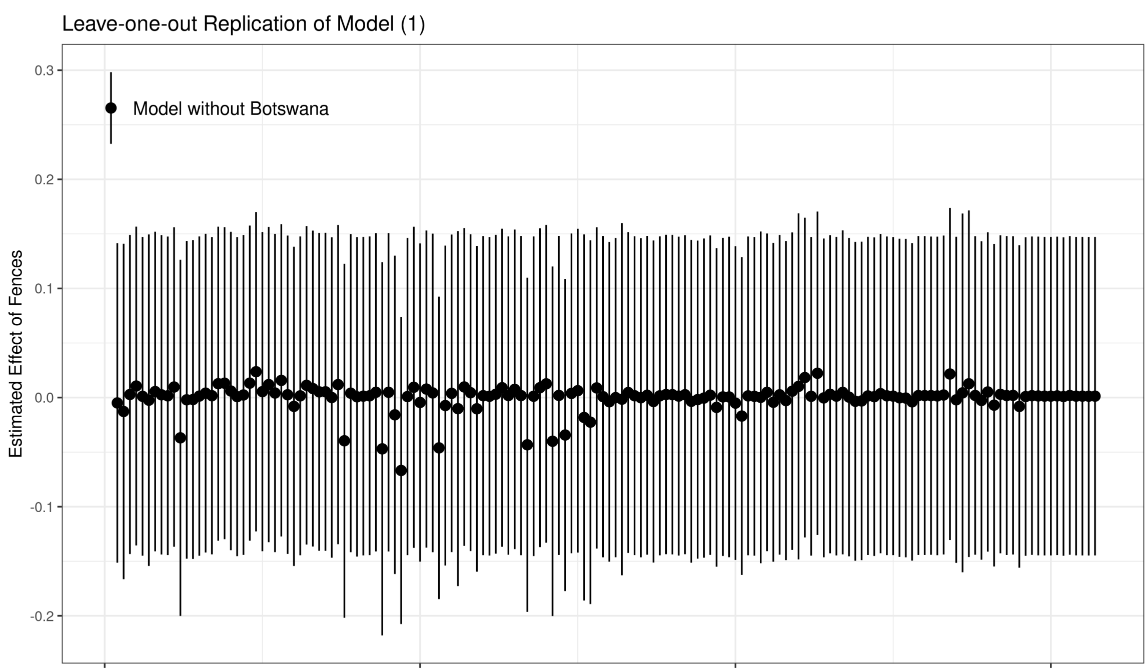


Figure E.3: Leave-one-out analysis of model (1) in table I

F Balance test

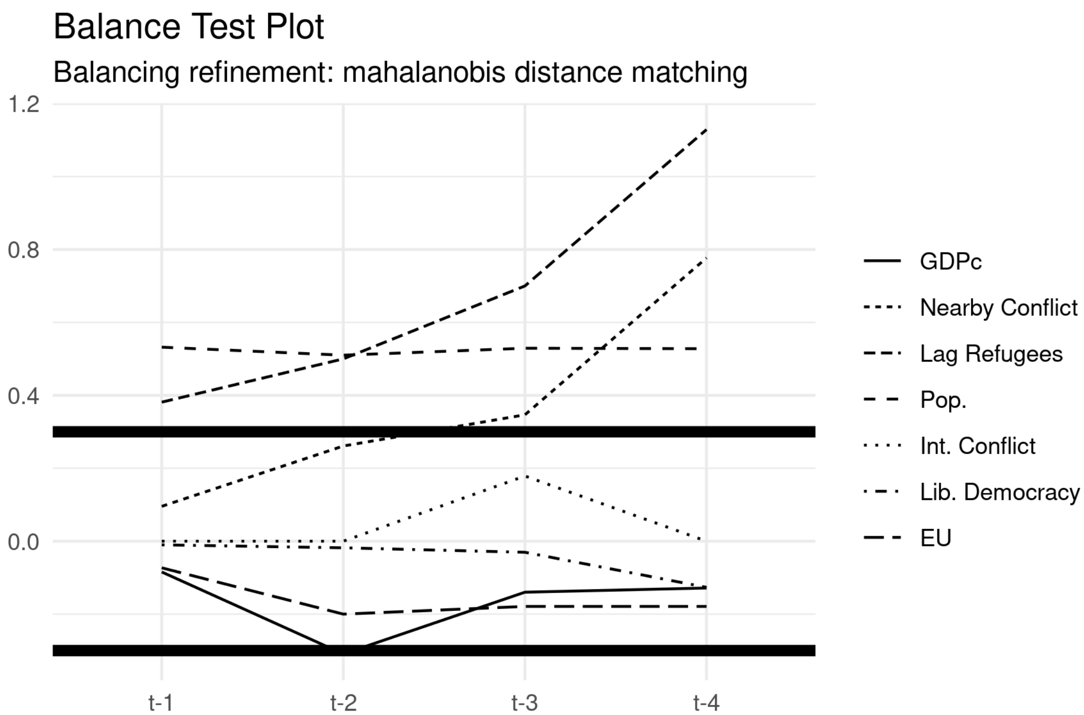


Figure F.4: Balance test plot for main analysis. In each period, we take the average of the difference between the values of the covariates for the treated units and the weighted average of the control units. The results are re-weighted in terms of standard deviations. The horizontal lines are at +/- 0.3, which represents a good threshold for assessing balance.

G Treated units

Country	Year
Botswana	1997
China	2006
India	1986
Iran	2000
Jordan	2006
Kazakhstan	2006
Kuwait	1991
Lithuania	2005
Myanmar	2009
Nigeria	1981
Pakistan	2005
Russian Federation	1991
Saudi Arabia	2006
South Africa	1975
Spain	1993
Thailand	2001
Turkmenistan	2001
United Arab Emirates	2007
United States of America	2005
Uzbekistan	1999

Table G.IX: The twenty country-years that are treated units in the main analysis.

H Generalized synthetic control and dynamic effects analysis

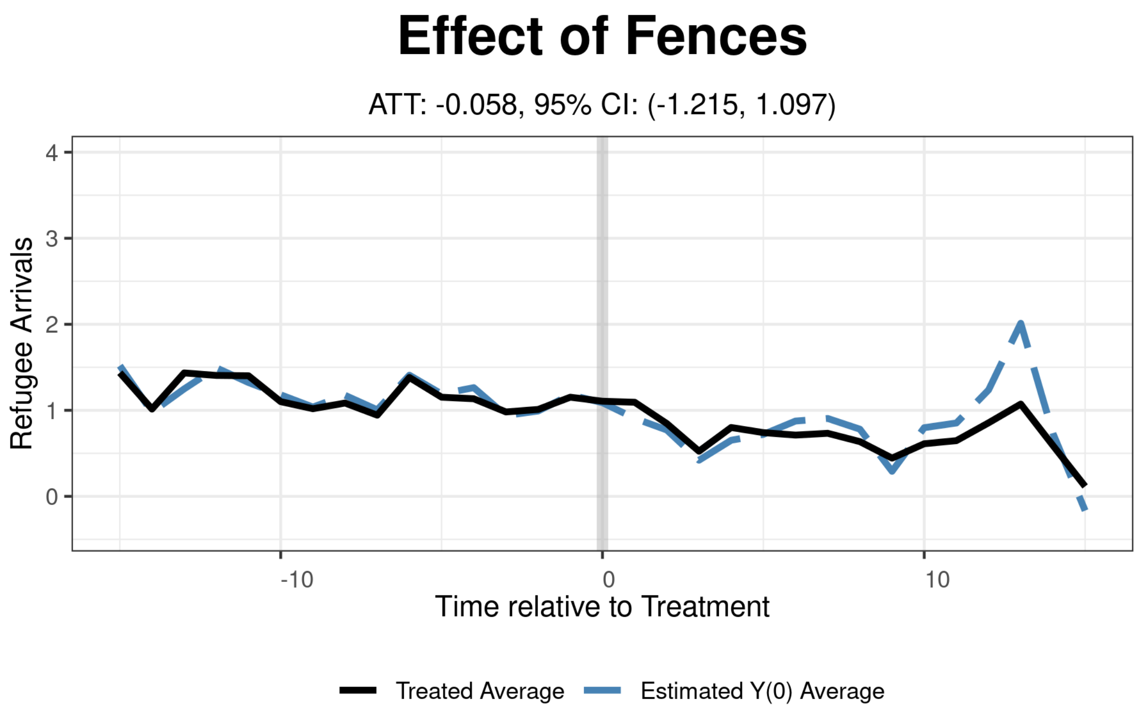


Figure H.5: This figure plots observed refugee arrivals (solid line) against imputed refugee arrivals in the absence of a border fence for treated countries (dashed line).

	Estimate	Standard Error
Effect of Fence on Refugee Arrivals	-0.078	0.092

Table H.X: This table presents the estimated effect of border fences using the Sun & Abraham (2021) estimator. This estimate averages the treatment effects for each cohort and then takes a weighted average across the cohorts. A cohort is defined as a year in which a country first fences its border. We must drop 14 of the 26 cohorts because they only have a single cross-section.