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Aiding Partial Liberalization

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Abstract

In many developing countries, especially those heavily reliant on foreign aid, close ties between public officials and firms (cronyism) can often distort governments efforts towards international economic liberalization and subsequent development. Leveraging a difference-in-differences identification strategy, we study how a plausibly exogenous boom to foreign aid in the 1970s and 1980s fostered cronyism (by entrenching authoritarianism) in recipient countries, which then incentivized their governments to subsequently pursue a more partial and hesitant approach to international economic integration after the World Trade Organization's creation. We attribute this partial liberalization to policy choices that protect politically connected commercial interests (political cronies). Our paper reinforces the idea that foreign aid can increase resistance to economic liberalization.

JEL codes: D72, F13, F6, O19, P16, Z12
Keywords: International trade and investment, foreign aid, political economy, World Trade Organization, free trade agreements, rent seeking, Islam, dictatorship

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In many developing countries, politics often distorts efforts towards international economic liberalization.[1] Despite multilateral initiatives at fostering liberalization (Subramanian and Wei 2007), these distortions usually manifest through mechanisms, such as firm lobbying, connections between firms and government officials, and the strategic provision of tariff protection and subsidies (e.g., Grossman and Helpman 1994, Rickard 2018, Atkin and Khandewal 2020). Governments in less democratic settings may be particularly susceptible to the manipulation of foreign investments and trade policies, as these can serve as important sources of rents that are strategically distributed to political insiders (e.g., Zissimos 2017, Gawande and Zissimos 2020, Monroe 2025). For instance, in Tunisia, Rijkers et al. (2017) documented how firms connected to the family of dictator Ben Ali disproportionately benefited from economic policies, especially in sectors subject to authorization and restrictions on foreign direct investment. Collectively, this literature suggests that, when faced with common external pressures to liberalize, developing countries – many of whom receive foreign aid – may adopt a more partial approach to globalization, shaped by the need to placate key political constituencies.

In this paper, we provide systematic evidence that this partial or selective approach towards economic openness is closely tied to how foreign aid can foster political economies (e.g., by entrenching authoritarian governance) conducive to cronyism. We argue that countries that had a greater exposure to aid in the pre-liberalization period were *subsequently* more likely to be reluctant globalizers. While past literature shows that the effect of aid on trade and growth are ambiguous at best (e.g., Werker et al. 2009, Clemens et al. 2012), aid often undermines political incentives for reform and reinforces political closure (e.g, Moyo 2010, Winters and Wright 2010). Notably, a majority of foreign aid is targeted toward authoritarian regimes. A recent OECD (2022) study revealed that, between 2010 and 2019, 79 percent of official development assistance was allocated to autocracies. In such settings, aid likely entrenched rent-seeking structures that later resisted pressures to globalize.

Probing this claim is empirically challenging as foreign aid can be co-determined with a country's stance on globalization and, in some cases, conditioned on trade reforms. To address this, we employ a novel research design that leverages an oil-price induced foreign aid shock in the 1970s and 1980s, which disproportionately affected many poor, non-oil producing Muslim-majority (hereon, Muslim) countries and made them more predisposed to authoritarian politics and cronyism (Werker et al. 2009, Ahmed 2012).[2]

[1] Populist movements have recently led governments in many developed countries (e.g., the United States) to impose restrictions to trade.

[2] In section 3, we argue and provide evidence (see Figure 2) that a reliance on foreign aid *prior* to the

As we elaborate in section 3.1, a decade of high oil prices enriched oil exporters in the Persian Gulf, allowing them to channel some of this new wealth in the form of foreign aid to many non-oil importing Muslim countries. This exogenous variation in aid flows was not driven by religious identify per se, but by geopolitical and economic relationship among Muslim countries. Therefore, categorizing an aid recipient as Muslim refers to their exposure to an aid shock (relative to non-Muslim aid recipients) and *not* to any specific feature of Islam per se. A key advantage of our empirical setting is that this aid was both exogenous and untied to any policy conditionality (Hallwood and Sinclair 1981, Hunter 1984). Using a shift-share difference-in-differences (DD) strategy, we then study patterns of economic integration in these Muslim aid recipients (relative to their non-Muslim counterparts) before and after plausibly exogenous global pressures to liberalize associated with the creation of the World Trade Organization (WTO).[3]

Our empirical strategy interacts plausibly exogenous *global* pressures to liberalize associated with the WTO's creation (our shift variable) with a dummy variable for whether a country is Muslim-majority or not (our share variable). Leveraging a common, global event to identify causal effects has been used in other settings. For example, Woldense and Kroeger (2024) exploit the period around the Cold War's termination (a common geopolitical event) to study authoritarian persistence in Africa. In our context, as we describe in sections 2.2 and 3.3, the establishment of WTO was largely exogenous to political economy conditions in developing countries (particularly, Muslim aid recipients) and importantly, generated common pressures to liberalize across all countries in our sample. Conditional on a battery of country-characteristics and fixed effects, the shift-share interaction term evaluates how pressures to liberalize differentially affects the trajectory of globalization in countries that experienced the aid boom relative to those that did not experience the boom.[4]

Leveraging our DD setup for a sample of non-oil producing aid recipients, we show substantial divergence since 1995 between Muslim and non-Muslim aid recipients in their globalization trajectories.[5] Specifically, Muslim aid-recipients have systematically lagged in relative terms on measures of *de jure* globalization capturing various economic restrictions

WTO's creation may be associated with authoritarian governance and cronyism in aid recipients. Moreover, in Appendix S6, we document how aid recipients (in our treatment group) exhibited characteristics consistent with crony behavior (e.g., higher measures of patronage-based corruption involving ties between governments and firms)

[3]In Appendix S4 we carefully document how non-democratic countries in general tend to be less globalized than democratic ones. Crucially, however, this finding is driven by non-democratic Muslim aid recipients. Among non-democratic non-Muslim countries this 'globalization deficit' is much *less* pronounced.

[4]Borusyak et al. (2025) provide a practical guide to properly using a shift-share research design.

[5]Table S1.2 lists the countries in our estimating sample.

expressed through tariffs, hidden import barriers, taxes on international trade, and investment and capital account restrictions. Using estimates from Gygli et al. (2019), our findings suggest a partial approach to globalization shaves nearly 0.50 percentage points per annum from per capita GDP growth in our treatment group of Muslim aid recipients. We perform a battery of robustness tests that reassure these inferences.

Our findings are robust to various measures of democracy/autocracy, state capacity, alternate classifications of Muslim societies, and outliers (see Appendix S8). We also discount several alternate explanations, such as various geographic determinants of trade (Redding and Venables 2004) (see Appendix S9) and estimate a placebo test that evaluates whether a country's Muslim status is the source of its more partial approach to liberalization (see Table 1, column 6). For the latter, we restrict our analysis to a sample of *non-aid* receiving countries that includes several Muslim countries (e.g., Kuwait, Saudi Arabia) to show that Muslim non-aid receiving countries are statistically indistinguishable in their globalization trajectories compared to non-Muslim non-aid receiving countries. This null result suggests that Muslim countries are *not* inherently less prone to globalization. Rather, Muslim countries that were exposed to a foreign aid shock prior to the WTO's creation exhibit partial liberalization.

In drawing our inferences, there are three main threats to our empirical strategy: violation of the parallel trends assumption, selection on unobservables, and concerns regarding the exogeneity of the WTO's creation. For the first, we perform several exercises to assuage this concern. We decompose our main DD estimates with a flexible specification that interacts our treatment dummy (i.e., whether an aid recipient is Muslim) with the full set of year fixed effects. Our estimates (see Figure 3) reveal that Muslim aid recipients were no different from non-Muslim aid recipients in their level of *de jure* globalization prior to the WTO's creation. We further demonstrate that Muslim aid recipients did not differ in their *trends* prior to the WTO shock based on a "trend-differences" approach (Kahn-Lang and Lang 2020) and show our main DD estimates hold in specifications that control for group-specific time trends and test for pre-trends (see Appendices S7 and S10). We also replicate our findings using a synthetic difference-in-difference estimator (Arkhangelsky et al. 2021) that re-weights the data to account for possible pre-treatment differences across treated (Muslim aid recipients) and non-treated (non-Muslim aid recipients) units (see Appendix S10.1).

Regarding possible selection on unobservables, we employ a test statistic developed by Altonji et al. (2005). This analysis demonstrates that (potential) selection on unobservables is unlikely to bias our inferences (see Appendix S10.2). This test also allows us to evaluate whether unobservables related to the initial decision to join the WTO (or not) unduly affects

our results.

Finally, to reassure the plausible exogeneity of the WTO's creation (the treatment variable in our DD specifications), we study WTO accession patterns and show that neither a country's status as being a Muslim (or not) nor its regime type affects when it joins the WTO (see Appendix S7). Moreover, under the assumption that governments in GATT members exhibit similar stances regarding economic integration (prior to the WTO's creation), we show that our main findings hold when we restrict our analysis to a sample of aid-recipients that were GATT members (see Table 1, column 7). Together, all these exercises reassure our paper's main inference of of a partial approach to globalization in Muslim aid recipients (relative to non-Muslim aid recipients).

We then study channels by unpacking our DD effects using mediation analysis (Barron and Kenny 1986). We demonstrate how the prevalence of nondemocratic institutions (and associated policies) coupled with dependency on foreign aid *prior* to the WTO's creation significantly weakens both the magnitude and statistical significance of our DD coefficient estimates. These results support our argument that aid dependent and less democratic governments may opt to partially liberalize as a means to protect crony actors who may be crucial to the regime's ruling coalition. Together, our main findings suggest that in the presence of external pressures to liberalize, a reliance on foreign aid that can entrench authoritarian governance may incentivize governments to protect regime insiders by pursuing a partial and hesitant approach to international trade and investment liberalization (and associated policies, such as regulatory barriers).

Our paper relates to recent work highlighting how distortions may undermine trade liberalization, particularly in developing countries (Atkin and Khandewal 2020). Our paper's empirical setting linking aid dependency to subsequent trajectories of economic integration following the WTO's creation offers a possible lens to study the positive, but uneven effects of the WTO on trade liberalization (Subramanian and Wei 2007). More generally, our paper ties to a broad literature on the political economy of trade agreements (e.g., Mansfield and Pevehouse 2013, Baccini and and Urpelainen 2014, Baccini 2019).

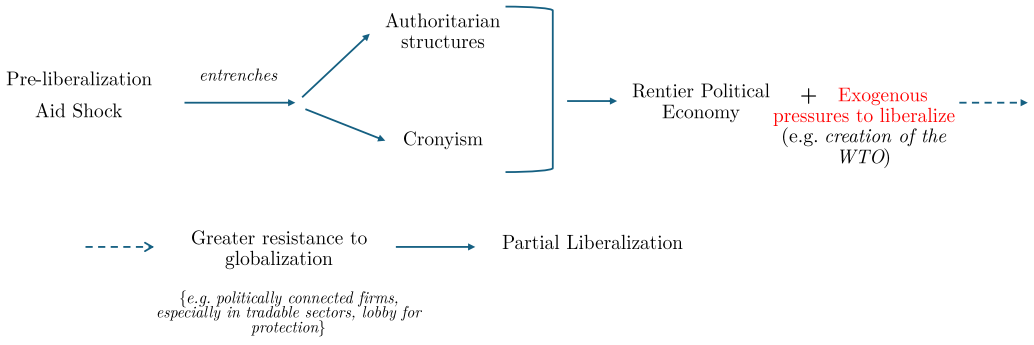
In highlighting the role of authoritarianism and cronyism, our paper also contributes to scholarship on the political economy of dictatorship (e.g., Acemoglu and Robinson 2006, Svolik 2012) and studies linking regime type with trade and investment policies. Existing work suggests that democracies tend to be more "open" to international commerce. For example, Milner and Kubota (2005) argue that democratization in developing countries shifts the median voter's preference towards liberalizing trade policies. In a recent review

article, Gerring et al. (2022, Table 3) show that an overwhelming share of studies document a positive association between democracy and trade and investment liberalization. In the presence of robust authoritarian structures, our findings suggest that partial liberalization may be an effective governing strategy: foreign economic policies can be manipulated to generate rents for connected elites, who, in return may be more inclined to support the regime. Based on existing studies, these forms of political connections may be important in many developing countries, especially those in the Middle East and North Africa region (e.g., Cammett 2007, Diwan et al. 2019, Hertog 2022, Monroe 2025). To the best of our knowledge, our paper presents the first cross-national study (with a causal interpretation) tying foreign aid to partial liberalization across many developing countries.

2 Conceptual framework

Our paper's central argument is predicated on the idea that, when faced with an external impulse to liberalize, aid-exposed countries are more likely to adopt a hesitant or partial approach to globalization. This pattern can be explained by the political economy effect of foreign aid inflows: aid fosters authoritarianism and cronyism. Aid inflows provide an alternative source of rents for incumbent elites, making them less willing to dismantle inefficient economic structures that benefit them. Empirically, this suggests that countries receiving substantial aid before liberalization are more likely to adopt hesitant or partial reforms, as aid weakens reform coalitions and delays the shift toward competitive markets and institutions. The main elements of our argument are developed below and summarized in Figure 1.

Figure 1: *Argument*



2.1 Foreign aid

In many autocracies, foreign rents—including oil revenues and foreign aid—help sustain elite cohesion (Bueno de Mesquita et al. 2003, Kono and Montinola 2009, Ahmed 2012). Morrison (2015) argues that foreign aid functions as a form of nontax revenue, enabling governments to implement policies that prolong their survival. Autocratic leaders, who typically have longer time horizons due to the absence of elections, benefit disproportionately from aid inflows. Unlike democratic leaders, they can stockpile aid to mitigate future crises (Kono and Montinola 2009). Common strategies include redistributing aid to secure elite loyalty and reducing tax collection efforts, which can also favour business elites and reinforce regime stability. Empirical research supports the claim that foreign aid entrenches authoritarianism (Ahmed 2012, Morrison 2015). In a panel of 108 countries, Djankov et al. (2008) find that aid erodes democratic institutions, with its "political curse" potentially exceeding that of oil dependence. Over time, as aid accumulates over time, politics may increasingly exhibit cronyism (e.g., state support for unprofitable and inefficient firms) that can stifle efforts at economic liberalization (Moyo 2010). In this context, foreign aid cushions regimes from pressures for economic reform, enabling governments to sustain patronage networks and resist institutional change. They are also likely to have systematically weaker competition regimes. In the Appendix S12, we show that countries with higher foreign aid inflows have experienced slower improvements in their competition laws since the 1990s, likely due to entrenched cronyism.[6] In short, aid-reliant countries have both the means and the motivation to maintain restrictive economic policies that protect their hold on power.

2.2 Common pressures to liberalize

The successful completion of the 1986 Uruguay Trade Round ushered in the creation of the WTO in 1995. As Preeg (2012) describes, the negotiation process addressed a wide range of issues, including agricultural subsidies, investment protections, and phasing out of various export quotas (e.g., in textiles).[7] The WTO's creation can also be viewed as a broader movement towards economic liberalization, which places competitive pressures on both members and *non-member* countries to liberalize (Bown and McCollough 2007). As Zissimos (2019, 2) emphasizes, the advent of the WTO marked the emergence of a truly "rules-based" system

[6] Using the Competition Law Index (CLI) compiled by Bradford and Chilton (2018), we find that aid-rich countries score systematically lower, with the effect becoming significant after 1992.

[7] This list is not exhaustive of the issues during the negotiation process. See Preeg (2012) for further details.

that created the "strongest dispute settlement system (DSS) in the history of international law." Like its predecessor, the General Agreement on Tariffs and Trade (GATT), the WTO seeks to reduce tariffs among members but goes further by introducing binding provisions − most notably, its DSS − that allow members to challenge discriminatory trade practices (e.g., regulatory barriers, export subsidies, or "dumping" of products, etc.).[8] Adhering to the WTO's commitments can often be quite costly; some estimates suggest as much as a year's development budget for the least developed countries (Zissimos 2019, 8). Despite these costs, even after the WTO's creation, many countries have continued to participate and join preferential trade agreements (PTAs) and bilateral investment treaties (BITs). For example, Mansfield and Pevehouse (2013, Figure 1) show the accelerated rate of growth in PTAs worldwide *after* the WTO's creation, which include agreements with non-WTO member countries. The provisions to liberalize trade and investment in these treaties tend to be more expansive than those contained in the WTO. In short, the period after the WTO's creation (i.e., post-1995) embodies a general, global movement towards economic liberalization for both WTO member and non-member countries.

2.3 Elite cohesion and partial liberalization

Facing external pressure to liberalize, aid-dependent governments—often embedded in rentier political economies—may strategically protect certain groups through selective or partial liberalization. This raises a critical question: which groups are shielded from such liberalization pressures? One prominent explanation emphasizes the role of regime type in guiding these decisions (Milner and Kubota 2005, Gerring et al. 2022). Here, theories of democracy/dictatorship typically model the interaction of two actors − the masses ("poor") and elites − as guiding the autocrat's choice of policies to remain in power (Acemoglu and Robinson 2006, Svolik 2012). These policies typically entail some combination of state repression and the provision of targeted benefits (patronage through economic closure, for example).[9] Crucially, elite cohesion underpins authoritarian resilience in these models. Its absence makes elite defection a common pathway from dictatorship toward democratization (Bueno de Mesquita et al. 2003). Given this importance of elite cohesion in nondemocracies, manipulation of foreign economic policy (e.g., managing external aid receipts or tariffs) can

[8]Several verdicts from the WTO's DSS has compelled member governments to change their domestic laws.

[9]In more democratic settings, these theories formally show that patronage is increasingly targeted to the masses through the distribution of a variety of economic and political goods, such as welfare payments and political freedoms/rule of law.

be a prudent strategy for protecting elite incomes.[10] The demand for such protection is likely to be higher among import-competing firms who are also generally more politically influential (Yasar et al. 2011).

Policies of partial economic liberalization can thus provide a viable strategy for sustaining authoritarian regimes. For instance, partial liberalization may forestall democratization by dampening revolutionary threats from the masses. Zissimos (2017) endogenizes trade policy within a model of regime formation and transitions. This model integrates a Heckscher-Ohlin framework of international trade with Acemoglu and Robinson's (2006) model of regime change to identify conditions under which a dictator might employ protectionist policies to prevent a political transition. In equilibrium, distinct policy approaches emerge. One strategy directly protects elite economic interests (e.g., imposing import taxes on elite-controlled sectors), thereby reducing the likelihood of elite defection. This aligns with our argument. Another approach considers domestic factor endowments and their owners. If the masses own the scarce factor, the elites (via the autocrat) may opt to protect sectors employing these scarce factors in order to reduce the incentives to mount a revolution.[11].

Governments have a menu of policy instruments available for protection. Historically, for most developing countries with limited fiscal capacity, tariffs comprised the main instrument. However, as the multilateral trading system has strengthened, tariff levels around the world have fallen precipitously. In response, governments often resort to non-tariff measures and various types of regulations (e.g., domestic content requirements, voluntary export restraints) to protect (certain) economic interests in-lieu of tariffs. Moreover, this trade policy substitution can be strategic: governments can selectively reduce tariffs in some sectors more than others, resulting in tariff dispersion.[12]

This political logic can guide specific international economic policies. For example, governments increasingly sign and implement preferential trade agreements (PTAs). According to Baccini (2019, 76), "the most important change is that modern PTAs not only reduce tariffs but also regulate investment, intellectual property rights, competition policy, government procurement, and many other matters. In other words, PTAs can remove barriers not only at the border but also behind the border, producing what has been referred to as deep integration between countries." Consequently, PTAs can often help introduce and consoli-

[10] Autocrats can also create institutions to share power with elites (see Svolik 2012). Our paper identifies a non-institutional channel via foreign economic policies.

[11] While analytically distinct, these strategies may overlap: an autocrat could simultaneously protect tradeable sectors tied to elites (e.g., steel) and the masses (e.g., textiles)

[12] We provide evidence of such strategic tariff dispersion in our sector-level results in section 5.

date broader economic and political reforms (Baccini and Urpelainen 2014). In this regard, governments in nondemocracies may approach PTAs with caution. Liu and Ornelas (2014) develop a model of endogenous changes in political regime in which participation in PTAs can serve as a commitment device to destroy future protectionist rents. Since such rents are attractive to autocratic groups, PTAs lower their incentives to seek power. In nascent (or unstable) democracies this dynamic can incentivize an incumbent (democrat) to participate in PTAs as a means to consolidate democracy. A corollary to this conjecture portends that autocracies may opt to adopt fewer PTAs, and if they do, ratify those with shallower provisions. Baccini and Chow (2018) provide some empirical support, finding that autocracies sign PTAs with less depth (i.e., strength of their commitments).

Autocracies may also be incentivized to strategically restrict their foreign investment. For example, Gao (2021) develops a model of oligopolistic competition linking globalization in form of increasing foreign direct investment (FDI) with democratization. Rising wages associated with FDI liberalization encourage workers to support democratization, while capitalists (elites) become less willing to support democratization because with increased competition (from inward FDI) they seek protection from the autocrat in the form of FDI restrictions. To the extent that elite cohesion is important for authoritarian stability, autocrats are inclined to restrict FDI, particularly in politically connected industries.

3 Empirical strategy

Our discussion in section two suggests that in the presence of pressures to liberalize, aid dependent countries exhibiting rentier and authoritarian politics may pursue international economic integration partially. However, attempts to empirically evaluate the causal relationship between international economic policy and domestic politics is challenging, particularly from omitted variables and/or reverse causality.[13] To address this concern, we extend a quasi-natural experiment that predisposed a group of governments in Muslim-majority aid recipients to become less democratic and prone to cronyism *prior* to the WTO's creation (Ahmed 2012). We then study how these aid recipients pursued a partial and hesitant approach to international economic liberalization (relative to the governments in non-Muslim aid recipients) *after* the WTO's creation.

[13]On the former, an omitted variable, perhaps culture, could affect both a country's domestic politics and trade policy preferences.

3.1 Quasi-natural experiment

The twin oil crises of 1973 and 1979 produced more than a decade of sky-high oil prices, which filled the government coffers of Gulf oil exporters. These countries – mainly, Saudi Arabia, the United Arab Emirates, and Kuwait – then distributed some of the rents to countries in the developing world, which mainly favored poor, non-oil Muslim countries (Werker et al. 2009).[14] Gulf oil producers were very generous with their foreign aid, doling out over 1.5 percent of their gross domestic product (Neumayer 2003). Notably, this aid was was in the form of block grants and not tied to any policy reforms. According to Hunter (1984): "the largest part of OPEC aid has still consisted of general balance of payment and budgetary support." Hallwood and Sinclair concur: "Most OPEC aid is given on very favourable terms and conditions from the recipient's point of view. A large proportion of this aid is given on a grant basis, otherwise loan terms are with low interest rates and long grace and amortisation periods" (1981, pp. 100–101). And while aid from Western donors has often been tied towards contracts with the donor country, "Arab aid has practically never been tied, with the exception of relatively unimportant specific loans and grants for oil purchases" (Neumayer, 2002, p. 15).

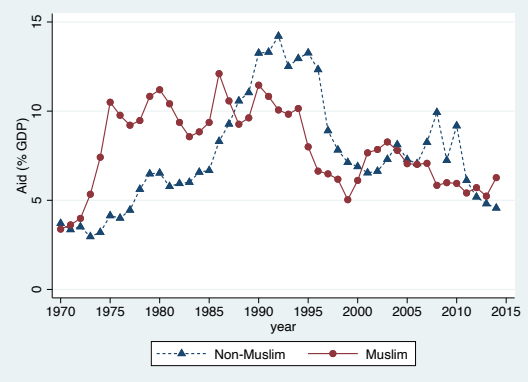
Figure 2a plots the average foreign aid inflows (as a share of GDP) across a sample of non-oil producing aid recipients, differentiated by whether their countries hold significant Muslim (more than 75 percent) populations (Table S1.2 lists the countries in our sample). In the lead up to the first oil price shock (in 1973), Muslim and non-Muslim aid recipients exhibit similar levels of aid dependence. After the first oil price shock in 1973 through the mid-1980s (when the price of oil tanked), Muslim recipients experienced an "aid boom." Using plausibly exogenous variation in world oil prices interacted with a Muslim dummy as an instrumental variable for aid, Ahmed (2012) studies the political consequences, documenting that Muslim aid recipients became less democratic and their (authoritarian) leaders less likely to lose power. Figure 2b graphs this insight: during the 1970s and 1980s, Muslim countries (on average) experienced significant declines in their POLITY scores and remained less democratic relative to non-Muslim aid-recipients in the lead up to the WTO's creation in 1995.[15] In identifying a plausible channel, Ahmed (2012) suggests that foreign aid allowed

[14] Werker et al. (2009) leverage the strong (positive) correlation between oil prices and these aid receipts to construct an instrumental variable to trace how aid can impact the macroeconomy.

[15] Other factors may also account for less democracy in Muslim aid-recipients. Some scholars identify historical factors, such as the expansion of Islam via military conquest (Chaney 2012), the introduction of sharia law in the 12th century (Kuran 2012), and resource endowments, such as oil (Ross 2001). We control for these (time-invariant) factors with country fixed effects. Other scholars point to time-varying country characteristics, such as economic development and geopolitical rivalries (e.g., Cold War politics). We control

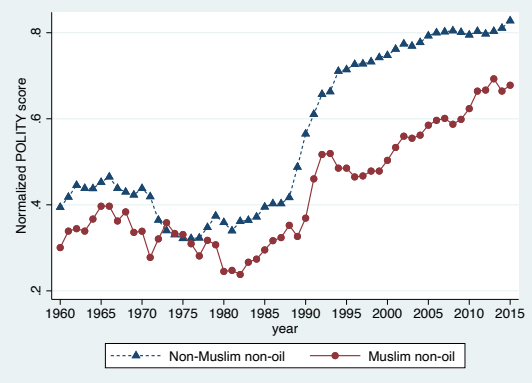
governments in Muslim societies to fund cronyism (e.g., higher wages for public sector employees). Such cronyism seems to have persisted after the end of the oil prices (circa 1985) and extended to the private sector.[16]

Figure 2a: *Foreign aid receipts in non-oil producing Muslim and non-Muslim countries*



Notes: Annual average receipts of foreign aid (% GDP) across Muslim and non-Muslim non-oil producing countries.

Figure 2b: *Democracy in non-oil producing Muslim and non-Muslim aid recipients*



Notes: Annual average of the normalized POLITY score across Muslim and non-Muslim non-oil producing aid recipients. A normalized POLITY score closer to 1 implies a more democratic form of governance.

for these factors (and others - see Appendices S8 and S9)

[16] Appendix A6 documents these patterns.

3.2 Cronyism in Muslim aid recipients

The prevalence of robust authoritarian structures in Muslim aid recipients can make them especially prone to cronyism, which frequently emerges in a "system in which those close to political authorities who make and enforce policies receive favors that have large economic value.[17] These favors allow politically connected agents to earn returns above those that would prevail in an economy in which the factors of production were priced by the market" (Haber 2002, xii).[18] Cronyism is especially prevalent is less democratic environments where governments may actively distort the economy to politically reward a specific set of "connected" firms and/or sectors. In a comparative (qualitative) case study of several MENA economies, Heydemann (2004) describes the pervasiveness of this "network of privilege" in which "cronyism is a key component of a broader system of insider-outsider divisions that also extend to labor markets and other spheres of Arab economies" (Hertog 2019, 39).[19] Detailed country studies from several Muslim aid recipients – such as Jordan (Monroe 2025), Egypt (Hertog 2022), Morocco (Ruckteschler et al. 2022) and those outside the Middle East region, such as Pakistan (Khawja and Mian 2005) – carefully document how these insider-outsider divisions are sustained in commercial activity.

We corroborate these patterns across firms in our sample of aid recipients. In Appendix S6, we carefully study responses by firms to the World Bank's Enterprise Survey (across various years). Our analysis reveals that firms in Muslim aid recipients are prone to experiencing features of cronyism that impact their ease of doing business, regulatory barriers, and notably, their perceptions of patronage-based government corruption. The latter, in particular, hones in on the importance of political connections in commercial activity. For example, firms in Muslim aid recipients report statistically significantly higher perceptions of corruption in their country's public sector compared to firms in non-Muslim aid recipients (Table S6.1, row 3). This finding seems to be a persistent and enduring feature in Muslim aid recipients.[20] Using Political Risk Services ICRG measure of corruption, we observe that over a 30 year period (see Figure S6.1), firms in Muslim aid recipients (on average) *always* report

[17] Cronyism is not a unique feature in Muslim societies. Cronyism, particularly in the form of state patronage to specific societal groups, can occur in other contexts, including in societies where cleavages may be based on ethnicity, religion or geography (e.g., urban vs. rural). Societies with these cleavages often tend to exhibit low income per capita.

[18] Foreign economic policy capture can generate such rents. As Haber (2002, xii) notes "cronies can still be protected from international competition by high levels of trade protection."

[19] Moreover, Hertog (2019, 39) argues this cronyism "is to an important extent explained with a legacy of deep state involvement in the Arab economies." See also Hertog (2022).

[20] Figure S6.1 plots these patterns when aid inflows to Muslim countries started to decline, circa 1985.

higher perceptions of corruption "in the form of excessive patronage, nepotism, job reservations, 'favor-for-favors', secret party funding, and suspiciously close ties between politics and business."[21]

3.3 The plausible exogeneity of the WTO's creation

A crucial component of our empirical strategy is the plausible exogeneity of the WTO's creation to political and economic conditions in Muslim aid recipients. As noted in section 2.2, the WTO's creation ushered in a period of increased pressures to liberalize, for both WTO members and non-members. Moreover, the motives and decisions underlying the WTO's creation was largely orthogonal to economic and political developments in Muslim aid recipients. In Appendix S7 we study this further. Our analysis shows that while many factors might influence a country's decision to join the WTO or not (which we strive to control for in our regressions), a country's religious practices is not a criteria for admission to the organization. Second, after the WTO's creation, Muslim and non-Muslim countries (in our sample of non-oil producing developing countries) have not differed in their propensity to join the organization.The typical Muslim aid recipient acceded to the WTO around the same time as a non-Muslim aid recipient.[22] In the context of our research design, this suggests the WTO may be viewed as a common and plausibly exogenous shock to economic liberalization that has not necessarily differentially targeted non-Muslim countries (relative to Muslim countries).

3.4 Empirical implication

Combining the arguments in sections 2, 3.1, and 3.2, suggest that an foreign aid shock to Muslim (non-oil producing) recipients fostered authoritarianism and cronyism. And when faced with common pressures to liberalize associated with the WTO's creation, these Muslim aid recipients had a greater incentive to engage in partial liberalization. Figure 1 summarizes this conjecture, which also guides our empirical strategy.

[21] Furthermore, as PRS describe in their guide (corresponding to this measure of patronage-based corruption): "In our view these insidious sorts of corruption are potentially of much greater risk to foreign business in that they can lead to popular discontent, unrealistic and inefficient controls on the state economy, and encourage the development of the black market."

[22] We test this formally by regressing a country's year of accession to the WTO on a Muslim dummy. The dummy was statistically insignificant. See Table S7.2.

3.5 Data

Sample. Our research design exploits panel data to compare the level of globalization across Muslim and non-Muslim non-oil producing aid recipients before and after the WTO's creation in 1995. In constructing our sample, aid recipients tend to be developing countries (i.e., is categorized as lower or middle-income by the World Bank) and differentiate oil and non-producers according to British Petroleum's definition. Based on existing studies (e.g., Ahmed 2012, Campante and Yanagizawa-Drott 2015), we categorize a country as being Muslim if at least 75 percent of its population identifies with the Islamic faith.[23] Notably, our sample excludes all oil producing Muslim countries (e.g., Saudi Arabia, Kuwait). We do so because these countries tend to suffer from the well-known resource curse and exhibit pervasive cronyism (Mazaheri 2016), independent of concerns with protecting connected elites in tradeable sectors. Moreover, these oil producers were a primary source of aid received by Muslim non-oil producers (Weker et al. 2009). Our resulting sample, therefore, is a panel of 56 non-oil producing aid recipients from 1970 through 2015. Appendix S1 reports the country sample and summary statistics.

***De jure* globalization.** Our conceptualization of partial liberalization emphasizes the variety of protectionist policies that governments can pursue (e.g., trade taxes, non-tariff measures, capital account restrictions, etc.). Thus, studying one particular measure of liberalization (e.g., trade as a share of GDP) is unlikely to capture this multifaceted process. Cognizant of this, we utilize a composite variable − the KOF Index of Globalization (Dreher 2006) − which carefully measures globalization along its economic, social, and political dimensions for almost every country in the world since 1970.[24] Its comprehensive country, temporal, and topic coverage has made the KOF index the most widely used measure of globalization in the academic literature (see Potrafke 2015 for an extended discussion).

To hone in on the policy dimension, we focus on *de jure* economic globalization (hereon, *de jure* globalization), which we believe best captures our argument. In this regard, we employ a revised version of the KOF Globalization Index, constructed by Gygli et al. (2019), that distinguishes between *de facto* globalization and *de jure* globalization (we describe the KOF index in greater detail in Appendix S1). While *de facto* globalization measures actual

[23] Our results remain robust if we use different percentage cutoffs. Reassuringly, we also verified that our control group of non-oil producing non-Muslim countries were "similar" to our treatment group on various observable characteristics (e.g., per capita GDP, political institutions) prior to the start of our sample period.

[24] In Appendix S1 we provides a more detailed explanation and robust justification for using the KOF index.

international flows and activities, *de jure* globalization measures policies, and conditions that, in principle, enable, facilitate, and foster flows and activities. The *de jure* measure compiles information on trade (regulatory barriers, tariff rates, and membership in trade arrangements) and finance (openness of the capital account, investment restrictions) from a variety of sources and ranges from 0 to 100. An index value closer to 100 implies fewer restrictions on policies and conditions that facilitate cross-border economic exchange. An attractive feature of the index's construction is the ability to make comparisons across countries and over time (for an extended discussion see Gygli et al. 2019).

3.6 Specification

To examine why Muslim aid recipients may be prone to partial liberalization in the presence of common pressures to liberalize, we compare differences in globalization in the post-WTO period relative to the pre-WTO period between Muslim and non-Muslim countries. We estimate variations of the following DD regression specification:

$$G_{it} = \alpha + \beta(Muslim_i \times Post_t) + X_{it}\theta + Y_t + C_i + \epsilon_{it} \quad (1)$$

In equation (1), G_{it} is the level of globalization in country i in year t. $Muslim_i \times Post_t$ is the interaction between an indicator variable equal to 1 if the country is Muslim-majority (and zero if otherwise) and a post-WTO dummy equal to 1 from 1995 onwards (and zero otherwise).[25] This interaction term evaluates how a "shift" to increased pressures to liberalize (associated with the WTO's creation) *differentially* affects the "share" of countries in our data that are Muslim relative to non-Muslim aid recipients.

In equation (1), X_{it} is a vector of time-varying country characteristics, such as log GDP per capita and population. In several specifications − particularly in our evaluation of competing explanations − we also include the interaction of various initial country characteristics, X_i (e.g., timing since the Neolithic Revolution, fixed geographic drivers of trade, etc.) and our post-WTO dummy. C_i are country fixed effects that account for any time-invariant differences across countries. Y_t are year fixed effects that account for any perturbations that apply to all countries in a given year (e.g., world interest rate, the end of the Cold War).

Importantly, as long as we control for year and country fixed effects, we automatically control for any independent effects of a country being Muslim or not (with each country

[25] In Appendix S3, we amend equation (1) with a "flexible specification" that allows us to evaluate the parallel trends assumption underlying DD regression. Figure 2 plots the coefficients estimates from this flexible specification.

fixed effect) and the timing of the WTO's creation (with a fixed effect for each year). To the extent that a country's decision to join the WTO (or not) is endogenous to the outcomes we study, we also control for these factors (all pre-treatment and interacted with $Post_t$). Finally, we conservatively cluster our standard errors at the country level. The coefficient of interest, β, measures the observed change in globalization in Muslim countries (relative to non-Muslim countries) after the WTO shock (relative to before).

4 Evidence

4.1 Baseline estimates

Table 1 reports our estimates from our baseline specification.[26] In column (1) we estimate a parsimonious model that only includes country and year fixed effects. The negative and statistically significant coefficient on $Muslim_i \times Post_t$ implies that Muslim countries experienced smaller increases in *de jure* globalization (relative to non-Muslim countries) after the WTO's creation (relative to before). In the remaining columns in Table 1, we control for factors that might affect patterns of globalization. In column (2), we control for a country's "timing since the Neolithic Revolution" interacted with $Post_t$ to capture the potential long-run effect of state development on globalization.[27] Prior studies find that countries with longer state histories (associated with an earlier transition to settled agriculture) tend to exhibit less democratic institutions today (Hariri 2015). Adding this control both increases the coefficient size and statistical significance of $Muslim_i \times Post_t$ compared to our benchmark estimate in column (1).

[26] Appendix S2 we plot our raw data which "motivates" and importantly corroborates our inferences from Table 1.

[27] The effect of $Muslim_i \times Post_t$ remains robust in specifications that control for various measure of *contemporary* state development, such as fiscal capacity, regime durability/consolidation, and government stability. These results are reported in Table S8.1

Table 1: *Globalization across Muslim and non-Muslim countries*

	KOF Globalization Index, *de jure*						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
						Placebo	GATT members
Muslim x Post WTO	-5.395**	-8.762***	-8.981***	-7.716***	-7.359**	-0.081	-7.028*
	(2.394)	(2.349)	(2.369)	(2.344)	(3.046)	(3.864)	(3.628)
Controls:							
GDP per capita, natural log			✓	✓	✓	✓	✓
Total population, natural log				✓	✓	✓	✓
Arab conquest (x Post)					✓	✓	✓
Country fixed effects	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓	✓
Observations	2,176	2,176	2,176	2,176	2,176	1,220	1,674
R^2	0.827	0.837	0.845	0.849	0.849	0.963	0.854

Notes: Estimation via OLS. Robust standard errors, clustered by country reported in parentheses. *, **, *** = significant at 10, 5, and 1 percent respectively. Coefficients for a constant, country year fixed effects, and control variables are not reported. Years since Agricultural Transition and Arab Conquest vary across country but not year. In columns 1-5, the sample includes non-oil producing, aid receiving countries. Column 6 reports a placebo regression in which the estimating sample includes non-aid receiving countries. The sample includes traditional foreign aid donors and oil exporting countries. In column 7, the sample is restricted to non-oil producing, aid recipients who were members of the GATT.

Our main DD effect remains robust when accounting for several standard confounders. Column (3) controls for a country's GDP per capita (in log units), which captures the potential role of economic development and market size on *de jure* globalization.[28] Accounting for per capita income may be considered "dirty" since it is post-treatment. In column (4) we control for a country's population size (in log units), which may proxy for market size. While adding this control slightly diminishes the effect on $Muslim_i \times Post_t$, it remains statistically significant and larger in magnitude (coefficient = -7.7) compared to column (1).

Finally, our main DD result remains robust when controlling for democracy.[29] In column (5) we do *not* use a *contemporary* measure of democracy which is likely to be endogenous with trade and investment policies. Rather, we control for a pre-treatment covariate that has been associated with authoritarian structures in many contemporary Muslim countries: the percentage of a modern country's territory conquered by Arab armies during the expansion of Islam (Chaney 2012).[30] Recent work suggests Arab conquest introduced governing and institutions (e.g., sharia law, an alliance between the state, clergy, and military) that set conquered territories on a long-run trajectory of pernicious political economy and authoritarian political institutions in the contemporary era (Blaydes and Chaney 2016); and this in turn may differentially affect each country's economic policies after the WTO's creation, such as the role of legal institutions (e.g., Kuran 2011, Gutmann and Voigt 2015).[31]

The estimated coefficients on $Muslim_i \times Post_t$ in columns (1) to (5) are negative and statistically significant. Moreover, accounting for confounders strengthens both the estimated effect's magnitude and statistical significance. The coefficient on $Muslim_i \times Post_t$ is substantively meaningful. For example, averaging the estimated DD effect across columns (2) to (5) suggests that Muslim countries experienced smaller increases (about 8.2 index points less) in *de jure* globalization relative to non-Muslim countries after the WTO shock (relative to before). This 8 point difference is equivalent to 19 percent of the average level of *de jure* globalization across our sample. This has significant welfare implications. For instance, estimates from Gygli et al. (2019, Table 5) imply an 8 point reduction in *de jure* globalization is associated with a 0.49 percent decline in annual economic growth.

[28] For example, higher income countries may enjoy comparative advantage in industries that benefit from more liberal economic policies (e.g., higher returns to capital from fewer capital and investment controls).

[29] Our main findings also hold when controlling for several measures of contemporary state capacity (see Table S8.1). These measures tend to be correlated with democracy.

[30] Since this percentage is specific to each modern country and time-invariant, we interact it with $Post_t$ to capture its differential effect on *de jure* globalization after the WTO's creation.

[31] Thus, Arab conquest plausibly captures the subsequent effect of Islamic law and the triparite governing coalition described in section 2.2.

Placebo test. Skeptics may worry that our DD estimates are driven by a country's Muslim status rather than it's dependency on foreign aid (prior to the WTO's creation). To allay this concern, we estimate a placebo regression for a sample of non-aid recipients that is comprised of traditional foreign aid donors (e.g,. Germany, Norway, United Kingdom, United States) and oil producers, many of whom doled out aid and are Muslim (e.g, Kuwait, Saudi Arabia). For this sample, column 6 shows the Muslim non-aid recipients are are statistically indistinguishable in their globalization trajectories compared to non-Muslim non-aid recipients.This null result suggests that Muslim countries are *not* inherently less prone to globalization. Rather, Muslim countries that were exposed to a foreign aid shock prior to the WTO's creation exhibit partial liberalization.

Endogenous accession to the WTO. While our arguments and statistical analysis seeks to bolster our claim that the (1) WTO's creation comprised exogenous pressures to liberalize for all countries (section 2.2) and (2) Muslim non-aid recipients acceded to the WTO around the same time as non-Muslim aid recipients (see Appendix S7), skeptics may still worry that country's *decision* to apply for and eventually accede to the WTO may be endogenous to the government's stance (likely, favorable) to liberalization. To address this concern, we re-estimate our baseline specification for a sample of aid dependent countries that were members of the GATT, under the notion that these countries shared similar views towards economic integration (see column 7).[32] Indeed, these countries quickly acceded to the WTO. The typical Muslim and non-Muslim aid-recipient entered the WTO in 1997 and 1996, respectively; and this one year difference is not statistically significant (based on a t-test of the group means, see Appendix S7). Reassuringly, the coefficient on $Muslim_i \times Post_t$ in column 7 is similar in magnitude to those in columns 1-5, but is estimated with slightly less precision with a p-value of 0.06 (likely, due to the smaller estimating sample).

Robustness. Our main finding on $Muslim_i \times Post_t$ in columns 1-5 remains robust in specifications that varies the size of the treatment group, for example by increasing and decreasing the threshold for qualifying as being Muslim to 60 and 80 percent respectively and dropping individual Muslim countries and all countries in the MENA region from the treatment group (see Appendix S8). The latter addresses concerns that particular countries

[32]This sample drops 13 countries from our main sample (columns 1-5). Of these 13 countries, 7 were non-Muslim aid-recipients.

(outliers) might unduly drive the main findings. Our main finding also holds when we use the trade component of the KOF *de jure* index as the dependent variable.[33] This is reassuring, as one would expect the WTO shock to affect trade related policies.

Our results also remain robust when controlling for a battery of potential confounders associated with being Muslim and/or predispositions towards globalization (e.g., geography, political instability). These results, reported in Appendix S9, allow us to discount these competing explanations. Finally, we probe whether our findings are generalizable to aid-dependent non-democracies more broadly. In Appendix S4, we show that nondemocratic countries tend to be less globalized (consistent with our arguments in section 2). Crucially, however, this globalization deficit is predominantly due our (treatment) group of Muslim aid-recipients (who tended to be less democratic due to their aid boom in the 1970s and 1980s). Among non-Muslim aid-recipients, a globalization deficit is less pronounced and not statistically significant.

4.2 Parallel trends

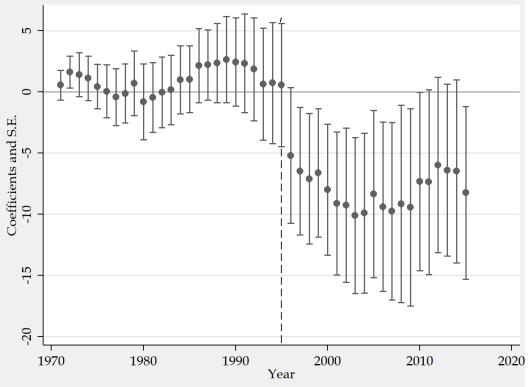
To unpack the average effects presented in Table 1, we estimate a flexible DD specification which interacts $Muslim_i$ with *each* year fixed effect (see Appendix S3). Performing this exercise is helpful in capturing how the relationship between a country's Muslim status and *de jure* globalization evolves over time and also probes whether the parallel trends assumption is violated. Figure 3 plots the coefficient estimates and corresponding 95 percent confidence intervals for the interactions. Two important insights emerge. First, there are no systematic differences in *de jure* globalization between Muslim and non-Muslim countries prior to the WTO shock. It is only after the WTO shock that *de jure* globalization in Muslim countries experiences smaller increases relative to non-Muslim countries. Second, the magnitude of the (negative) interaction effects increases for about 7 years after the shock (i.e., through 2002) and is strongly persistent thereafter. This supports our conjecture that Muslim countries have partially liberalized their policies relative to non-Muslim countries after being exposed to the common globalization shock in 1995.

This interpretation is substantively important as it allows us to rule out other global shocks, such as the Cold War's termination around 1990. For example, if the period surrounding the end of the Cold War affected the subsequent trajectory of economic liberalization in Muslim societies (relative to non-Muslim societies), one would expect Figure 3 to

[33] In this model, the coefficient on $Muslim_i \times Post_t$ is -6.51 with a corresponding p-value$<$0.05.

demonstrate statistically significant coefficient estimates around 1990.[34] Figure 3 does *not* exhibit this pattern, implying that neither Cold War's termination nor *any* other global shock *prior* to 1995 (e.g., structural adjustment policies in the 1980s) are driving our findings.

Figure 3: *The difference in de jure economic globalization between Muslim and non-Muslim aid recipients, over time*



Notes: Each (coefficient) point is that year's fixed effect (Y_t) interacted with $Muslim_i$ on *de jure* globalization based on estimation of equation (2), with the corresponding 95 percent confidence interval. Standard errors are clustered at the country level. The regression controls for *Years since Agricultural transition*$_i \times Post_t$, the log of GDP per capita, country and year fixed effects.

Finally, in Appendix S10, we offer the following additional analyses to bolster our causal inferences: (1) an innovative and alternate "trend differences" strategy to evaluate the parallel trends assumption (Kahn-Lang and Lang 2020), (2) an approach that accounts for possible pre-treatment differences in globalization between Muslim and non-Muslim countries using a synthetic difference-in-differences estimator (developed by Arkengelsky et al. 2021), (3) specifications that control for pre-trends and country-specific time trends, (4) estimation via dynamic DD which allows us to rule out pre-trends (Chaisemartin and D'Haultfoeuille 2020), and a (5) statistical test to discount biases from possible selection on *unobservables* based on Altonji et al. (2005).

5 Evaluating channels

Our conceptual framework suggests that partial liberalization may stem from two underlying conditions: (1) the prevalence of nondemocratic institutions and (2) the provision of rents to

[34] As an additional check, we also control for $Muslim_i$ interacted with a post Cold War dummy (plus our baseline controls). In this specification, $Muslim_i \times Post_t$ is equal to -8 with a p-value<0.01, while $Muslim_i \times PostColdWar_t$ equals -0.92 and is statistically insignificant (p-value=0.54).

maintain elite cohesion. This suggests that institutions and dependency on foreign aid are mediating variables. To evaluate these conjectures formally, we use a four-step procedure developed by Baron and Kenny (1986) to statistically decompose a potential mediation process.

5.1 Institutions

We start by accessing the potential mediating role of political institutions. To guide our analysis, consider the diagram:

$$M \rightarrow A \rightarrow G$$

where M, A, and G refer to whether a country is Muslim (or not), its quality of democratic institutions, and level of globalization respectively. In the diagram, A is the hypothesized mediating variable. The first step involves examining the overall relationship ($M \rightarrow G$), which we have done in Table 1 by regressing $Muslim_i \times Post_t$ on a country's level of *de jure* globalization. The following two steps probe the intermediate steps. The second step tests the significance of the first step ($M \rightarrow A$) by regressing $\overline{Polity_i} \times Post_t$ on $Muslim_i \times Post_t$, where $\overline{Polity_i}$ is a country's average level of democracy (normalized polity score) prior to 1995. In the third step, we regress a country's level of globalization on $\overline{Polity_i} \times Post_t$ to test the significance of the second intermediating relationship, $A \rightarrow G$. The fourth step conducts a multiple regression with M and A predicting G.

The purpose of the steps 1-3 is to verify that there exists a zero-order relationship among the variables. If one or more of these relationships are not statistically significant, one cannot usually conclude whether mediation is not possible or likely. If there are significant relationships from steps 1 through 3, one proceeds to step 4. In this fourth step regression model, some form of mediation is present if the effect of the mediator (A) remains significant after controlling for M. If M is *not* significant when A is controlled, the findings support full mediation.

Table 2 reports these four steps sequentially.[35] Column (1) reports our baseline specification. In column (2), we estimate the first intermediate step, $M \rightarrow A$. In comparison to their average democracy scores prior to the WTO's creation, the coefficient estimate suggests that Muslim countries (after 1995) tend to be less democratic than their non-Muslim counterparts (after 1995). In column (3), the *positive* effect on $\overline{Polity_i} \times Post_t$ implies that countries

[35]Since various time-varying country controls are likely endogenous with whether a country is Muslim and its level of democracy, we estimate parsimonious models with country and year fixed effects only.

with *less* democratically oriented political institutions prior to the WTO are associated with *lower* levels of globalization after the WTO's creation compared to before. These findings support the second intermediate step, $A \rightarrow G$. Following the reasoning in Barron and Kenny (1986), since the estimated relationships are statistically significant across columns 1-3 one can safely infer that mediation is likely.

Column 4 supports this conclusion: the specification provides evidence of full mediation. Controlling for a country's quality of democratic institutions (prior to the WTO) effectively "kills" the effect of $Muslim_i \times Post_t$ on globalization. The estimated coefficient of -2.01 is smaller in magnitude than our benchmark estimates in Table 1 and is no longer statistically significant. This finding also holds when we use the "polyarchy" measure of democracy from the Varieties of Democracy data as a mediating variable: it also kills the effect of $Muslim_i \times Post_t$ on globalization.

As an extension, we now probe whether any particular institutional arrangement in Muslim aid-recipients might be relevant. In particular, Menaldo (2013) argues the prevalence of monarchies in many *contemporary* Muslim societies (relative to non-Muslim societies) has created a "monarchical political culture" that "has promoted cohesion among regime insiders, such as ruling families and other political elites and ... has helped monarchs consolidate their authority and foster political stability" (Menaldo 2013, 709). To evaluate this possibility, we use data on autocratic regimes from Geddes, Franz, and Wright (2014) to determine the proportion of years a country was ruled by a monarch prior to 1995 ($\overline{Monarch_i}$), interacted with $Post_t$ (see Table 2).[36] Columns 5 and 6 show that countries that tended to be ruled by a monarch prior to 1995 were no more (or less) to be globalized after the WTO's creation. Moreover, controlling for $\overline{Monarch_i} \times Post_t$ does not affect the statistically significant effect of $Muslim_i \times Post_t$ on a country's level of *de jure* globalization (column 6). These findings together with those in columns 3 and 4 suggest that a broad configuration of autocratic institutions (e.g., constraints on the executive, degree of electoral participation and regulation) that we capture with the normalized POLITY index effectively "mediates" the post-WTO globalization deficit in Muslim societies (columns 3 and 4) rather than a specific type of autocratic regime in these societies (columns 5 and 6).

[36] Consistent with Menaldo, a higher share of Muslim countries tend to be ruled by a monarch compared to their non-Muslim counterparts.

Table 2: *The mediating role of institutions and foreign aid*

	KOF Globalization	Norm. POLITY x Post WTO	KOF Globalization					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Muslim x Post WTO	-5.395	-0.252		-2.01		-6.212		-3.075
	(2.394)**	(0.065)***		(2.451)		(2.391)**		(2.282)
Normalized POLITY			15.65	13.435				10.707
x Post WTO			(5.713)***	(6.027)**				(6.058)*
Monarchy x Post WTO					1.796	6.435		
					(5.358)	(5.459)		
Foreign aid x Post WTO							-0.388	-0.365
							(0.139)***	(0.123)***
Country fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Observations	2,176	2,176	2,176	2,176	2,176	2,176	2,176	2,176
R^2	0.834	0.94	0.83	0.835	0.82	0.83	0.832	0.845

Notes: Estimation via OLS. Robust standard errors, clustered by country reported in parentheses. **, *** = significant at 5 and 1 percent respectively. A constant is not reported. In columns 1, 3-8 the dependent variable is the KOF globalization index, *de jure*.

5.2 Foreign aid

In columns 7 and 8 of Table 2, we examine the mediating role of aid dependency. Using data from the World Bank's World Development Indicators, we interact a country's average level of aid (% GDP) prior to 1995 ($\overline{Aid_i}$), interacted with $Post_t$.[37] In column 5, the negative and statistically significant effect on $\overline{Aid_i} \times Post_t$ implies that countries that were more dependent on foreign aid prior to 1995 were less globalized after the WTO's creation. This effect remains robust in a specification that also controls for $\overline{Polity_i} \times Post_t$ (column 6). In this specification, the estimated effect on $\overline{Polity_i} \times Post_t$ is smaller in magnitude and statistical significance (relative to the coefficient estimate in column 4).

Following the reasoning in Barron and Kenny (1986), these effects suggest that foreign aid and political institutions mediate the effect of $Muslim_i \times Post_t$ on globalization. Moreover, the smaller effect of $\overline{Polity_i} \times Post_t$ on globalization (relative to column 4) suggests that foreign aid may be mediating the effect of institutions as well. This inference is consistent with arguments that dependency on foreign aid can buttress rentier political structures (Ahmed 2012).

5.3 Pre-WTO policy choices

In Appendix S11, we further show that *policies* associated with non-democratic politics (e.g., adoption of shallower trade agreements) in Muslim societies *prior* to the WTO's creation significantly weakens both the magnitude and statistical significance of our DD coefficient estimates. Together, our results in Table 2 and Appendix S11 suggest the prevalence of authoritarian political institutions and associated policies largely mediate the widening globalization deficit in Muslim societies (compared to non-Muslim countries) after the WTO's creation.

6 Conclusion

Politics often distorts attempts at economic liberalization. We evaluate this conjecture in a novel context by tracing how a boom to foreign aid fostered cronyism and entrenched less democratic forms of governance in a group of poor, non-oil producing Muslim-majority countries. In doing so, these aid recipients became more predisposed to exhibit cronyism; and

[37] Consistent with Ahmed (2012) and Figure 2, Muslim countries exhibited higher shares of aid (% GDP) relative to non-Muslim countries prior to 1995.

when facing subsequent pressures to liberalize their economies (associated with the creation of the WTO) adopted a more partial and hesitant approach towards economic globalization. This pattern we, argue is rooted in politics in which trade and investment policy closure and regulatory restrictions can generate rents that can be supplied to favored business and politically connected actors (cronies). And these elites are in turn prone to support the incumbent (and predominantly, less democratic) regime.

Cognizant of concerns from unobserved heterogeneity and reverse causality, we employ a difference-in-differences research design to draw causal inferences. We leverage the timing of the WTO's establishment in 1995 as a plausibly exogenous global shock to economic liberalization to study patterns of globalization across Muslim aid recipients (our treatment group) relative to non-Muslim aid recipients (our control group).

Using a difference-in-differences identification strategy, our analysis suggests Muslim aid recipients countries experienced significantly smaller increases in *de jure* globalization (compared to non-Muslim aid recipients) after the WTO's creation (compared to the period before). This finding is robust, in particular to concerns with parallel trends and several competing explanations (e.g., geographic drivers of trade, political instability). In investigating why these aid recipients partially liberalized, we statistically demonstrate the importance of two mediating variables: nondemocratic institutions and dependency on foreign aid. Our analysis of channels suggests the prevalence of crony political economies – stemming from pre-existing dependency on foreign aid – may have incentivized governments to view trade and related foreign economic policies as a means to generate rents for important commercial elites. Moreover, since our treatment group of aid recipients tend to exhibit less democratic politics (see Figure 1b), distributing rents to elites through cronyism likely bolstered the incumbent regime's political durability.

Our paper offers at least two substantive implications that may generalize beyond our treatment group of aid recipients. First, in the wake of global pressures to liberalize, political factors may be influential in the speed and depth of economic reforms that countries undertake. Second, this partial approach to globalization may differentially affect firms and interests within countries. In particular, crony firms tend to be the main beneficiaries of protection.

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Appendix S1: Data

S1.1 Description of the KOF index

The argument we develop in the paper emphasizes the variety of foreign economic *policies* governments can pursue to protect politically relevant interest groups. As we argue in section 2, in pursuit of this objective, governments can remove (or create) regulations and restriction that provide a source of rents for entrenched interest groups, particularly those in foreign aid-dependent contexts.

To capture the multi-faceted dimensions of international economic integration, we follow Dreher (2006) and Gygli et al. (2019) in conceptualizing globalization as a "process of creating networks of connections among actors at intra- or multi-continental distances, mediated through a variety of flows including people, information and ideas, capital, and goods. Globalization is a process that erodes national boundaries, integrates national economies, cultures, technologies and governance, and produces complex relations of mutual interdependence."

Since our conceptual framework identifies policy choices, we focus our empirical analysis on *de jure* economic globalization (hereon, *de jure* globalization). We employ a revised version of the KOF Globalization Index, constructed by Gygli et al. (2019) that distinguishes between *de facto* globalization and *de jure* globalization. While *de facto* globalization measures actual international flows and activities, *de jure* globalization measures policies, and conditions that, in principle, enable, facilitate and foster flows and activities. In practice, *de jure* globalization is often a prerequisite for *de facto* globalization. As Gygli et al. (2019, 564) observe "tariffs need to be reduced or abolished to promote international trade. Infrastructure such as internet access needs to be available to exchange information and ideas. International agreements need to be signed and embassies built to enable political collaboration. When *de jure* globalization has occurred, *de facto* globalization proceeds. Goods and services need to be traded, information exchanged, and policies in line with international agreements implemented."

Our measure of *de jure* globalization compiles information on trade (regulatory barriers, tariff rates, and membership in trade arrangements) and finance (openness of the capital account, investment restrictions) from a variety of sources and ranges from 0 to 100. An index value closer to 100 implies fewer restrictions on policies and conditions that facilitate cross-border economic exchange. An attractive feature of the index's construction is the ability to make comparisons across countries and over time (see Gygli et al. 2019 for further details).

The trade dimension uses variables on trade regulation, trade taxes, tariff rates and free trade agreements. Trade regulation includes the average of two subcomponents: prevalence of non-tariff trade barriers and compliance costs of exporting. The variable trade taxes measures the income of taxes on international trade as a share of total income in a country. The variable tariff rates refers to the unweighted mean of tariff rates. The variables trade regulation, trade taxes and tariff rates are calculated as the inverse of the normalized values

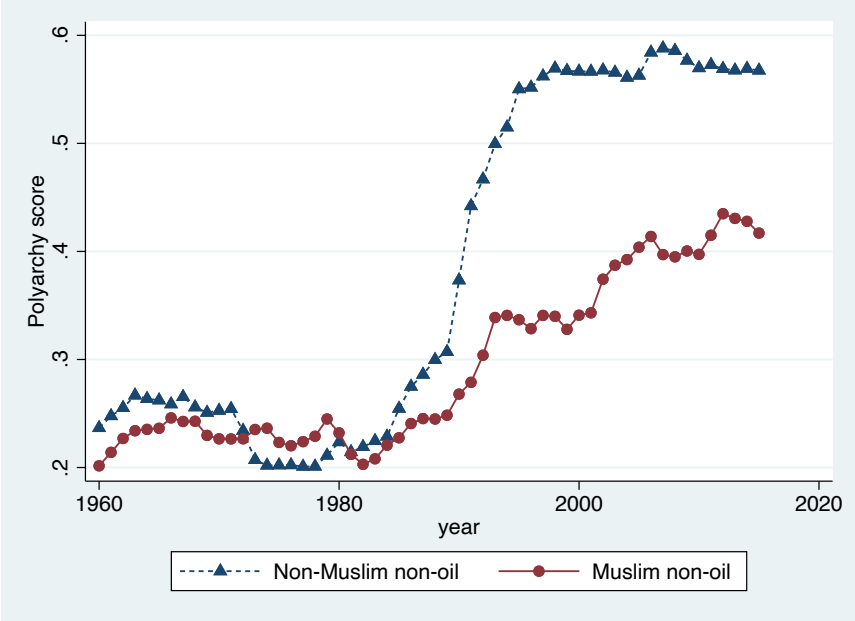
such that higher values relate to a higher level of *de jure* trade globalization. Free trade agreements refer to the stock of multilateral and bilateral free trade agreements.

The finance dimension measures the openness of a country to international financial flows and investments. The IMF's Annual Report on Exchange Arrangements and Exchange Restrictions (AREAER) is the primary source for most measures of *de jure* financial globalization. It measures the openness of the capital account of a country using the most widely used index based on the AREAER reports: the Chinn-Ito index. The second variable measures investment restrictions based on the World Economic Forum (WEF) Global Competitiveness Report. To account for policies that are potentially favorable to capital flows, the index also includes the number of international treaties which covers bilateral investment agreements and treaties with investment provisions. It does not include information on the strength of treaty commitments ("depth").

S1.2 Democracy across Muslim and non-Muslim non-producing developing countries

Figure S1.1 plots the average level of democracy using the polyarchy score (from the V-DEM data set) across Muslim and non-Muslim non-oil producing developing countries. The score ranges from 0 to 1, where a value closer to 1 corresponds to a greater quality of (electoral) democracy.

Figure S1.1: *Democracy in non-oil producing Muslim and non-Muslim countries*



Notes: Annual average of polyarchy score (from Varieties of Democracy Data set, Coppedge et al. 2020) across Muslim and non-Muslim non-oil producing countries.

S1.3 Summary statistics and sample

Tables S1.1 and S1.2 report summary statistics for our sample of non-oil producing developing countries.

Table S1.1: *Summary statistics*

	Non-Muslim					Muslim				
	N	Mean	SD	Min	Max	N	Mean	SD	Min	Max
KOF de jure	1567	46.269	14.215	9.422	85.829	781	36.91	11.648	13.832	67.917
GDP per capita, log	1749	7.395	0.967	4.754	9.596	874	6.911	0.922	5.481	9.35
Population, log	2068	15.72	1.109	13.169	18.431	1120	15.954	1.473	11.334	19.057
Arab Conquest	2376	0.004	0.023	0	0.153	1180	0.496	0.441	0	1
Agricultural transition	2417	3.601	1.86	1	8	1251	5.86	2.813	2.9	10.5
FTA Depth Index	2417	1.482	0.798	0.327	5	1251	1.182	0.607	0.227	2.286
Deep FTAs, Average	2417	2.312	0.753	1	3.913	1251	1.628	0.489	1	2.5
Deep FTAs, Max No.	2417	4.594	1.664	2	7	1251	3.141	0.857	1	4
Distance from Coast	2376	266.757	348.036	12.252	1675.81	1251	360.473	375.613	26.24	1180.26
Foreign Aid (% of GDP)	1652	7.342	11.207	-0.643	147.059	885	8.033	8.378	0.003	57.828
Trade Restrictiveness Index, Overall	2204	0.167	0.078	0.031	0.401	1251	0.111	0.058	0.005	0.235
Trade Restrictiveness Index, Manufact.	2204	0.118	0.099	0.009	0.42	1251	0.089	0.069	0.002	0.257
Real Market Potential, RV (log)	2417	15.187	1.054	13.271	18.588	1251	14.845	1.153	13.179	17.282
Real Market Potential, HM (log)	2417	13.363	0.793	11.965	14.968	1251	13.365	0.889	12.185	15.169

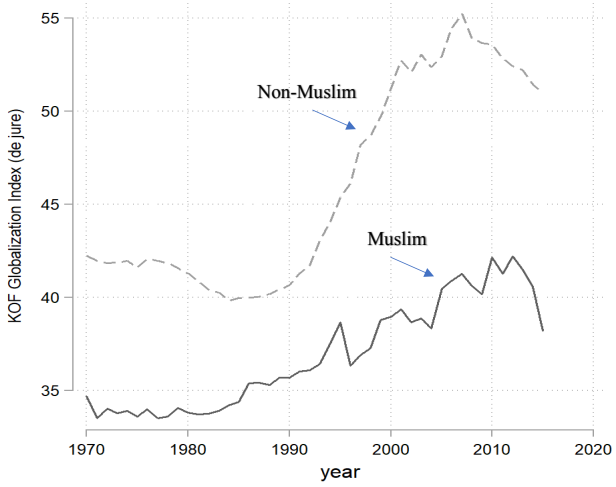
Table S1.2: *Sample of non-oil producing aid recipients*

Muslim	Non-Muslim	
Afghanistan	Armenia	Malawi
Albania	Bolivia	Mongolia
Bangladesh	Botswana	Mozambique
Burkina Faso	Bulgaria	Nicaragua
Djibouti	Chile	Panama
Egypt	Cote d'Ivoire	Paraguay
Gambia	Dominican Republic	Philippines
Guinea	El Salvador	Poland
Jordan	Eritrea	Serbia & Montenegro
Lebanon	Ghana	South Africa
Mali	Guatemala	Sri Lanka
Morocco	Guinea-Bissau	Tanzania
Niger	Guyana	Togo
Pakistan	Haiti	Uganda
Senegal	Honduras	Ukraine
Sierra Leone	Hungary	Uruguay
Somalia	Jamaica	Zambia
Sudan	Kenya	Zimbabwe
Tunisia	Liberia	
Turkey	Madagascar	

Appendix S2: Motivating trends

To motivate our analysis, Figure S2.1 plots the evolution of the *de jure* component of the KOF Index of Economic Globalization between Muslim and non-Muslim countries. The figure highlights two stylized features. First, Muslim countries have always lagged their non-Muslim comparators in terms of their policies regarding economic globalization. Second, since 1995 there has been a greater divergence in the globalization trajectories between Muslim and non-Muslim countries. Prior to 1995, the KOF index was about 7 index points (on average) lower in Muslim countries compared to non-Muslim countries. After 1995, this difference more than doubled to around 15 index points and corresponds to a "difference-in-differences" of 8 index points.

Figure S2.1: *Average annual level of globalization in Muslim and non-Muslim countries*



Notes: Annual group average of KOF globalization index across Muslim and non-Muslim non-oil producing countries.

Appendix S3: Flexible specification

In equation (1), conditional on our controls, our identification strategy relies on the interaction effect, $Muslim_i \times Post_t$, being exogenous with respect to globalization (G_{it}). There are two specific challenges we confront in relying on this assumption. First, if there are country characteristics that influence globalization and also shape the relationship between the WTO shock and globalization then this would violate the exogeneity assumption. Second, if Muslim countries were on a different trend in terms of their globalization prior to the WTO shock (relative to non-Muslim countries) then the assumption would be violated. We address the first concern by including country and year fixed effects in our benchmark specifications. Furthermore, we evaluate (and discount) several country characteristics that may be both correlated with a country's level of globalization and the WTO shock, such as market potential, geographic, and historical characteristics (see Appendix S9).

To address the second challenge, we estimate the fully flexible specification given by:

$$G_{it} = \alpha + \Gamma_t(Muslim_i \times Year_t) + X_{it}\theta + Y_t + C_i + \epsilon_{it} \quad (2)$$

This specification allows us to investigate whether Muslim countries were trending differently in terms of levels of globalization relative to non-Muslim countries prior to the WTO shock. In equation (2), G_{it} is the level of globalization in country i in year t. $Muslim_i \times Year_t$ are interactions between *each* year fixed effect and the Muslim indicator variable ($Muslim_i$). C_i and Y_t are country and year fixed effects, respectively. The vector of estimated interaction coefficients, Γ_t, shows the relationship between being a Muslim country and its level of globalization in each year (t) of our panel. If, for example, Muslim countries were not on a different trend in terms of their level of globalization prior to the WTO shock then we would expect the coefficients to be more or less constant and statistically indistinguishable from zero for the years prior to 1995. However, if Muslim countries engaged in partial liberalization after the WTO shock (as we hypothesize), then we would expect the coefficients to become more negative as we move further into the post-shock period. Equation (2) is also advantageous in discerning whether other global "shocks" (e.g., the Cold War's termination circa 1990) might also affect subsequent trajectories of globalization.

Appendix S4: Regime type and globalization

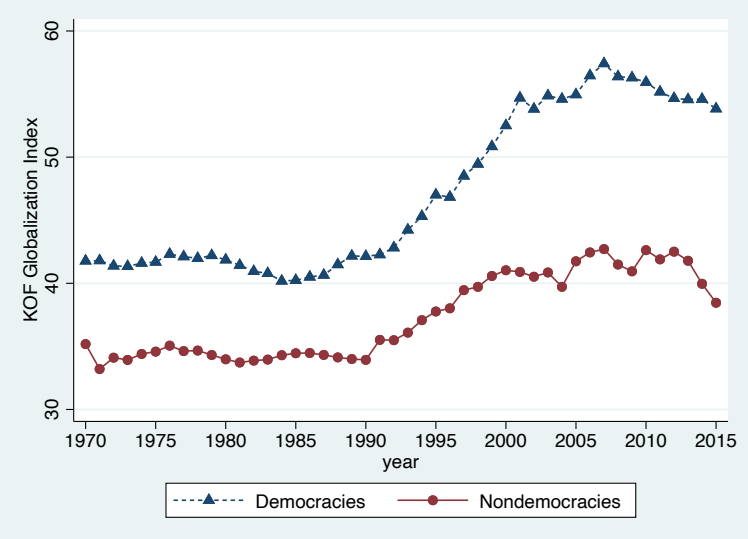
Our paper's theory is predicated on the notion that nondemocratic politics may incentivize governments to pursue a more hesitant and partial approach to globalization. While this argument is not necessarily exclusive to Muslim societies, we will subsequently demonstrate that Muslim non-democracies are a very influential group of societies in our analysis. In particular, we show the globalization deficit that less democratic regimes exhibit is largely due to the inclusion of non-democratic Muslim societies in our analysis. Importantly, our analysis reveals that among nondemocratic non-Muslim societies the globalization deficit is less pronounced and in fact *not* statistically significant.

S4.1 Globalization deficit in autocracies

We first show that autocracies tend to exhibit lower levels of globalization relative to democracies. For our sample of non-oil producing developing countries, Figure S4.1 plots the annual average level of the KOF *de jure* globalization index across groups of democracies and non-democracies based on their normalized polity scores. In this figure, a country is classified as a democracy if its average *pre-WTO* normalized POLITY score is greater than or equal to 0.75 (which corresponds to a +7 or higher on the POLITY index). A nondemocracy is a country with a normalized polity score below 0.75.

The figure reveals two stark observations. First, nondemocratic countries tend to be less globalized than democratic societies. Second, this divergence (globalization deficit) widens further after the WTO's creation in 1995.

Figure S4.1: *Globalization by regime type (based on normalized polity score)*



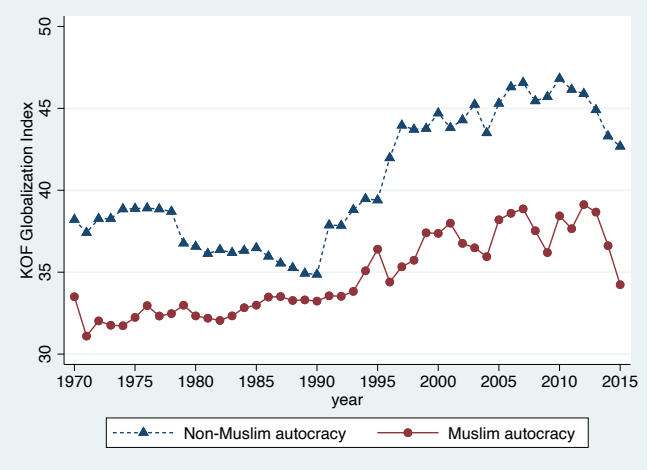
Notes: Annual group average of the KOF *de jure* economic globalization index across democratic and non-democratic non-oil producing countries. A country is democratic if its average pre-WTO normalized POLITY score is greater than or equal to 0.75. A nondemocracy is a country with a average pre-WTO normalized POLITY score below 0.75.

S4.2 The importance of Muslim non-democracies

The patterns in Figure S4.1 suggest that nondemocratic societies (on average) tend to be less globalized relative to democratic countries (on average). This suggests that perhaps our paper's argument holds more broadly across *all* non-democracies (i.e., both Muslim and non-Muslim autocracies).

Trends. Figure S4.2 provides an initial evaluation of this conjecture by comparing the average level of the KOF *de jure* globalization index across Muslim and non-Muslim *autocracies* in our sample. The figure shows that since 1970, Muslim autocracies are less globalized than non-Muslim autocracies and this gap seems to increase *after* the WTO's creation in 1995.

Figure S4.2: *Globalization across Muslim and non-Muslim autocracies*



Notes: Annual group average of the KOF *de jure* economic globalization index across Muslim and non-Muslim nondemocracies. A country is included in this sample of nondemocracies if its average pre-WTO normalized POLITY score is less than 0.75.

Regression analysis. In Table S4.1, we undertake more rigorous statistical analysis of the pattern in Figure S4.2. We estimate our baseline difference-in-differences regression (given by equation 1 in the main text) and account for our baseline controls (e.g., log GDP per capita) and country and year fixed effects. The variables of interest are the pre-WTO country average of our democracy measure interacted with a post-WTO dummy (that is equal to 1 on and after 1995 and 0 prior to 1995).

In column 1, the statistically significant effect on Democracy x Post WTO implies that more democratic countries prior to 1995 became more globalized after the WTO's creation in 1995 (relative to less democratic societies). This inference is consistent with the pattern in Figure S4.1 which shows a growing globalization deficit between nondemocratic and democratic countries. However, if we "separate" this effect from Muslim countries (column 2), the effect on democracy x post WTO declines in magnitude while the Muslim effect is negative and statistically significant. In this specification, "democracy in Muslim countries

x post WTO" extracts the "Muslim" effect. As robustness, in columns 3 and 4 we replicate the analysis using the VDEM measure of democracy. In columns 3 and 4, the direction, magnitude, and statistical significance of these coefficient estimates are similar to those in columns 1 and 2.

Table S4.1: *Globalization across nondemocratic Muslim and non-Muslim countries*

	KOF Globalization Index, *de jure*			
Pre-WTO average x Post WTO dummy	(1)	(2)	(3)	(4)
Democracy (POLITY) x Post WTO	13.07	11.706		
	(5.336)**	(4.982)**		
Democracy (POLITY) in Muslim countries x Post WTO		-10.56		
		(2.923)***		
Democracy (VDEM) x Post WTO			11.907	9.983
			(6.718)*	(6.791)
Democracy (VDEM) in Muslim countries x Post WTO				-14.761
				(4.761)***
F-test: sum of coefficients = 0		0.04		0.3
... p-value		0.842		0.588
Baseline controls	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations	2,176	2,176	2,176	2,176
R^2	0.846	0.851	0.841	0.851

Notes: Estimation via OLS. Robust standard errors, clustered by country reported in parentheses. *, **, *** = significant at 10, 5, and 1 percent respectively. The unit of observation is country-year. All specifications include baseline controls, country and year fixed effects. These coefficients and a constant are not reported. Baseline controls include: log GDP per capita, log total populations, Years since Agricultural Transition x Post WTO, and Arab conquest x Post WTO.

Together, the results in columns 1-4 suggest that Muslim countries are a highly influential group of countries in our sample and importantly, the relationship between regime type (autocracy) and globalization after the WTO's creation is *driven* by these countries. Furthermore, a F-test of the sum of coefficients on democracy x post WTO and democracy in Muslim countries x Post WTO reveals the sum is not statistically different from zero, which suggests the effects nullify each other and that separating out the Muslim effect effectively "kills" the democracy x post WTO effect.

Appendix S5: Foreign aid in Muslim societies

Figure 2 suggests that non-oil producing Muslim countries tend to be more dependent on foreign aid relative to their non-Muslim counterparts. In this appendix we corroborate this observation with more stringent econometric analysis.

The patterns in Figure 2 suggest that prior to 1990, Muslim countries were more aid dependent which may be attributable to generous aid disbursements from oil producing countries in the Persian Gulf (Ahmed 2012). Figure 2 also shows that by the end of the Cold War (circa 1990) average aid disbursements in non-Muslim countries had caught up. Below we examine whether the end of the Cold War and period thereafter marked a significant shift in the allocation of foreign aid across Muslim and non-Muslim countries. To do so, we estimate a difference-in-differences specification where we interact whether a recipient country is a Muslim with a post Cold War dummy (equal to 1 from 1990 onwards and 0 in the period before). If this interaction effect is statistically *insignificant* this implies that foreign aid inflows across Muslim and non-Muslim recipients were *not* different.

Table S5.1: *Foreign aid across Muslim and non-Muslim countries*

	Foreign aid (% GDP)		
	(1)	(2)	(3)
Muslim countries	8.631	8.608	9.306
	(1.281)***	(1.514)***	(1.431)***
Muslim x Post Cold War		0.038	
		(1.861)	
Muslim x Post WTO			-1.459
			(1.827)
Year fixed effects	Yes	Yes	Yes
No. observations	2,194	2,194	2,194
R^2	0.143	0.143	0.144

Notes: Estimation via OLS. Robust standard errors, clustered by country reported in parentheses. *, **, *** = significant at 10, 5, and 1 percent respectively. The unit of observation is country-year. Regressions include year and country fixed effects. These coefficients and a constant are not reported. Post-Cold War is equal to 1 after 1990 and zero otherwise. Post WTO is equal to 1 for observations after 1995 and zero otherwise.

We first estimate a specification without a Muslim × Post Cold War interaction term. Column 1 in Table S5.1 shows that over the entire sample period, Muslim countries on receive about 8.6 percentage points (relative to GDP) more aid than non-Muslim countries. This difference is statistically significant. In column 2, we control for Muslim × Post Cold War interaction. In this specification, the interaction effect is statistically no different from zero; implying the end of the Cold War did not affect aid flows to Muslim countries relative to non-Muslim countries. In column 2, the coefficient on *Muslim* remains positive, similar in magnitude to column 1, and statistically significant. This implies a country's recipient status (as Muslim or not) rather than the Cold War's termination more robustly explains the variation in aid flows across Muslim and non-Muslim countries. Finally, in column 3, we

examine whether the period after the WTO's creation in 1995 might (differentially) explain aid flows across Muslim and non-Muslim countries. In this specification the coefficient on Muslim × Post WTO is essentially nil and not statistically significant. In contrast, the coefficient on the *Muslim* country dummy – which measures the effect of foreign aid in Muslim societies *before* 1995 – is 9.31 and highly statistically significant. Together, this leads to an important inference: Muslim countries were significantly more reliant on foreign aid (relative to non-Muslim countries) in the period *prior* to the WTO's creation.

Substantively, the (null) findings on the interaction terms in columns 2 and 3 generate useful inferences. In particular, they imply that after 1990, differences in aid flows are unlikely to affect the DD effects we find (with Muslim × Post WTO) in the paper's main findings related to patterns of globalization. Rather, Muslim countries received significantly higher levels of foreign aid relative to non-Muslim countries *before 1995.* These findings reinforce our argument in section 2.2, that prior to the WTO's creation, Muslim countries were highly exposed and reliant on foreign aid which created an environment conducive to entrenching rentier political structures (Ahmed 2012).

Appendix S6: Cronyism in Muslim societies

In this appendix, we provide statistical evidence from surveys conducted among firms (managers) that Muslim societies tend to exhibit higher (perceived) levels of crony activity.

Evidence from firm surveys. We leverage responses from the World Bank Enterprise Surveys (WBES). The WBES are nationally representative firm-level surveys, with top managers and owners of businesses interviewed using a globally comparable questionnaire that covers a broad range of business environment topics as well as firms' characteristics and performance measures. All information collected through the WBES— raw granular data, the WBES indicators at the firm and economy level—are made publicly available upon completion of the surveys through the World Bank's website and data portal.[38] From the WBES, we utilize three questions: a firm's overall "ease of doing business" in that country (based on a country ranking 1-190, where a *higher* value implies *greater strains* on commerce); a 6-point rating of the regulatory environment faced by firms in that country (1=least regulations, 6=greatest regulations); a measure of corruption in the public sector (e.g., bribery for contracts, role of political connections/patronage networks) perceived by private firms in that country (1=low corruption, 36=highest corruption). We regress responses from these questions (aggregated to the country level) on a dummy variable equal to 1 if the country is Muslim and zero if otherwise.

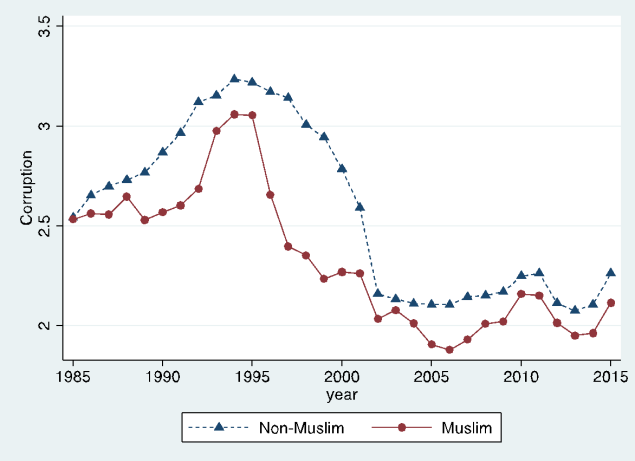
Table S6.1 reveals that that Muslim countries tend to be associated with greater burdens on doing business and regulatory restrictions, and higher perceived corruption in the public sector. For instance, in the first row, the typical Muslim countries tend to rank 127 places higher than their non-Muslim counterparts on their ease of doing business (recall, a higher rank implies greater strains on doing business). In the second and third rows, the positive and statistically significant effects imply that firms in Muslim countries (relative to those in non-Muslim countries) believe they operate in more regulated business environments and where government corruption is perceived to be more pervasive.

Table S6.1: *Cronyism in Muslim societies*

		Muslim countries	No. obs
(1)	Ease of doing business	127.607	117
	(Lower = greater ease)	(13.049)***	
(2)	Business regulatory environment	3.277	380
	(1=Low, 6=High)	(0.107)***	
(3)	Corruption in public sector	5.263	393
	(1=Low, 36=High)	(2.241)**	

Notes: Estimation via OLS. Robust standard errors, clustered by country reported in parentheses. *, **, *** = significant at 10, 5, and 1 percent respectively. The unit of observation is country-year. Regressions include year fixed effects. These coefficients and a constant are not reported.

Figure S6.1: *Patronage-based corruption in Muslim and non-Muslim countries*



Notes: Annual group average of the ICRG corruption score across Muslim and non-Muslim non-oil producing countries. A higher corruption score implies greater controls on the corruption (i.e., lower perceptions of patronage-based corruption).

Political connections. In addition to the statistical associations above, we also show that Muslim societies are more prone to government corruption that involves political connections (patronage-based corruption). To do so, we leverage the International Country Risk Guide's (ICRG) measure of corruption which is "concerned with actual or potential corruption in the form of excessive patronage, nepotism, job reservations, 'favor-for-favors', secret party funding, and suspiciously close ties between politics and business." As they state in their guide: "In our view these insidious sorts of corruption are potentially of much greater risk to foreign business in that they can lead to popular discontent, unrealistic and inefficient controls on the state economy, and encourage the development of the black market."

The ICRG's corruption measure ranges from 1 to 6, where a score of 1 equals the greatest degree of patronage-based corruption and a score of 6 equal to the lowest level. This corruption score is also advantageous as ICRG has recorded each country's corruption score since 1984 using a consistent methodology (across countries and over time). We calculate and plot the annual average across Muslim and non-Muslim countries. Figure S6.1 shows that on average, Muslim countries have always scored lower on the ICRG corruption measure. This pattern implies that Muslim countries are more prone to exhibit patronage-based corruption which includes various form of cronyism.

[38] As of 2024, the website contains a total of 355 WBES collected through a consistent methodology across the world, 12 Informal Sector Enterprise Surveys covering 38 cities, and other surveys, along with the cross-economy databases.

Appendix S7: Accession to the WTO

In this appendix, we evaluate whether a country's status as being Muslim (or not) or regime type influences its accession to the WTO. Our analysis shows that that the typical Muslim country in our sample acceded (joined) the WTO around the same time as non-Muslim countries. A country's regime type is not a robust determinant of when a country joins the WTO.

Table S7.1: *Year of WTO accession Muslim and non-Muslim aid recipients*

Muslim countries	Year of accession to:		Non-Muslim countries	Year of accession to:				
	GATT	WTO		GATT	WTO		GATT	WTO
Afghanistan		2016	Armenia		2003	Malawi	1964	1995
Albania		2000	Bolivia	1990	1995	Mongolia		1997
Bangladesh	1972	1995	Botswana	1987	1995	Mozambique	1992	1995
Burkina Faso	1963	1995	Bulgaria		1996	Nicaragua	1950	1995
Djibouti	1994	1995	Chile	1949	1995	Panama		1997
Egypt	1970	1995	Cote d'Ivoire	1963	1995	Paraguay	1994	1995
Gambia	1965	1996	Dom. Republic	1950	1995	Philippines	1979	1995
Guinea	1994	1995	El Salvador	1991	1995	Poland	1967	1995
Jordan		2000	Eritrea		X	Serbia		X
Lebanon		X	Ghana	1957	1995	South Africa	1948	1995
Mali	1993	1995	Guatemala	1991	1995	Sri Lanka	1948	1995
Morocco	1987	1995	Guinea-Bissau	1994	1995	Tanzania	1961	1995
Niger	1963	1996	Guyana	1966	1995	Togo	1964	1995
Pakistan	1948	1995	Haiti	1950	1996	Uganda	1962	1995
Senegal	1963	1995	Honduras	1994	1995	Ukraine		2008
Sierra Leone	1961	1995	Hungary	1973	1995	Uruguay	1953	1995
Somalia		X	Jamaica	1963	1995	Zambia	1982	1995
Sudan		X	Kenya	1964	1995	Zimbabwe	1948	1995
Tunisia	1990	1995	Liberia		2016			
Turkey	1951	1995	Madagascar	1963	1995			

Notes: The Table lists countries along with their year to the GATT and WTO accession. The symbol 'X' indicates that the country only has a working party status and is not currently a WTO member. An empty cell indicates the country was not GATT member.

Muslim vs. Non-Muslim classification. We first evaluate whether a country's status as Muslim (or not) affects whether and when it accedes to the WTO. We use a country's year of accession to the WTO (see Table S7.1) to calculate the average year of joining the WTO across Muslim and non-Muslim countries. The typical Muslim and non-Muslim country joined the WTO in 1997 and 1996, respectively. This one year difference is not statistically significant (based on a t-test of the group means). We complement this with cross-sectional regression analysis, where we regress a country's year of accession to the WTO on country characteristics. The coefficient estimate (=1.13) in column 1 of Table S7.2 implies that Muslim countries tend to join the WTO about one year after a non-Muslim country.[39] But this

[39]Since the constant is equal to 1996, the coefficient of 1.13 implies the typical (average) Muslim country joined the WTO in 1997.

one year difference is not statistically significant.

Table S7.2: *Determinants of WTO accession*

	Year of accession to the WTO			
	(1)	(2)	(3)	(4)
Muslim	1.128		0.488	1.114
	(1.746)		(0.725)	(1.811)
Democracy (POLITY)		0.787	1.142	
		(0.778)	(0.738)	
Democracy (V-DEM)				-0.063
				(1.929)
Constant	1996.333	1995.275	1994.91	1996.37
	(0.706)***	(0.414)***	(0.493)***	(1.207)***
No. countries	49	47	47	49
R^2	0.01	0.01	0.02	0.01

Notes: Robust standard errors in parentheses. *, **, *** = significant at 10, 5, and 1 percent respectively. The number of observations is less than the full sample (56) since countries that have not acceded to the WTO (indicated with "X" in Table S7.1) and/or do not have a POLITY (or V-DEM) score are not included in the statistical analysis.

Regime type. In columns 2-4 of Table S7.2, we evaluate whether a country's quality of democracy (among our sample of poor non-oil producing countries) affects when it joins the WTO. The coefficient estimate in column 2 (=0.787) implies that a one point increase in a country's POLITY score (i.e., a movement in the direction of greater democracy) is associated with delaying a country's entry into the WTO. However, the effect is statistically insignificant which suggests that movements in the quality of democracy do not seem to affect WTO accession (at least in our sample of countries). This null effect holds in a specification that also controls for whether a country is Muslim or not (column 3). In column 4, we examine the association of a different measure of democracy based on the "polyarchy" variable in the V-DEM dataset. The negative coefficient (=-0.063) suggests that countries that are more democratic join the WTO earlier (relative to less democratic countries). However, the effect is statistically indistinguishable from zero. On balance, the null effects in Table S7.2 suggest that neither a country's status as Muslim (or not) nor its quality of democracy seems to affect when its joins the WTO.

Appendix S8: Robustness checks

Contemporary measures of state development/capacity. Below, we show that our main finding is robust to other measures of contemporary state development: tax revenues (% GDP) as a measure of fiscal capacity (data from the World Bank's World Development Indicators), regime durability as a measure of political institutional development and consolidation (using "DURABLE" variable from the POLITY data set), and government stability which measures the risk of government (in)stability (data from ICRG). For each of these variables, we calculate a country's pre-WTO average and interact it with a post-WTO dummy. Across all 3 specifications, the Muslim x Post WTO interaction is similar in magnitude and statistically significant. In column 1, countries with higher tax ratio are more likely to be globalized. This is unsurprising, as countries with higher tax ratios tend to be higher income as well as democratic; factors that are associated with more globalized economic policies. In columns 2 and 3, neither regime durability nor government stability seem to be affect levels of *de jure* globalization.

Table S8.1: *Globalization across Muslim and non-Muslim countries, controlling for contemporary measures of state development*

	KOF Globalization Index, *de jure*		
	(1)	(2)	(3)
Muslim x Post WTO	-7.27	-7.518	-7.187
	(3.322)**	(3.112)**	(3.050)***
Additional controls (x Post WTO)			
Tax ratio	25.903		
	(12.769)**		
Regime durability		-0.033	
		(0.057)	
Government stability			0.999
			(0.892)
Baseline controls	Yes	Yes	Yes
Country FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	1973	2,176	2,126
R^2	0.856	0.849	0.849

Notes: Estimation via OLS. Robust standard errors, clustered by country in parentheses. ** = significant at 5%. The unit of observation is the country-year. Tax ratio is a country's pre-WTO average of tax revenues (% GDP). Regime durability is a country's pre-WTO average of its DURABLE measure from POLITY. DURABLE measures a country's institutions change by more than 3 points on the POLITY index from the previous year. Government stability is a country's pre-WTO average. This variable lies on a 0 to 12 point index, where a higher value corresponds to greater government stability. Baseline controls include log GDP per capita and total population. These coefficients, a constant, and year and country fixed effects are not reported.

Muslim classification. Our findings are not sensitive to how we classify Muslim countries. In the paper's main analysis, we follow Ahmed (2012) and Campante and Yanagizawa-Dott (2015) and classify a country as Muslim if at least 75 percent of its population identify with the Islamic faith. Below, we replicate Table 1 using percentages below and above the 75 percent cutoffs. Table S8.2 shows the main findings hold when the Muslim population threshold is reduced to 60 percent. Table S8.3 shows the main findings hold when the Muslim population threshold is increased to 80 percent.

Table S8.2: *Globalization across Muslim and non-Muslim countries, with at* ***least 60%*** *of population identifying as Muslim*

	KOF Globalization Index, *de jure*				
	(1)	(2)	(3)	(4)	(5)
Muslim x Post WTO	-5.185**	-8.735***	-8.952***	-7.756***	-7.653**
	(2.427)	(2.363)	(2.384)	(2.346)	(3.047)
Controls:					
Years since Agricultural Transition (x Post)	No	Yes	Yes	Yes	Yes
GDP per capita, log	No	No	Yes	Yes	Yes
Total population, log	No	No	No	Yes	Yes
Arab conquest (x Post)	No	No	No	No	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	2,148	2,148	2,148	2,148	2,148
R^2	0.827	0.839	0.846	0.850	0.850

Notes: Estimation via OLS. Robust standard errors, clustered by country reported in parentheses. *, **, *** = significant at 10, 5, and 1 percent respectively. The unit of observation is country-year. Years since Agricultural Transition and Arab Conquest vary across country but not year.

Table S8.3: *Globalization across Muslim and non-Muslim countries, with at* ***least 80%*** *of population identifying as Muslim*

	KOF Globalization Index, *de jure*				
	(1)	(2)	(3)	(4)	(5)
Muslim x Post WTO	-4.027	-7.814***	-8.153***	-6.792***	-5.840*
	(2.409)	(2.450)	(2.459)	(2.462)	(3.417)
Controls:					
Years since Agricultural Transition (x Post)	No	Yes	Yes	Yes	Yes
GDP per capita, log	No	No	Yes	Yes	Yes
Total population, log	No	No	No	Yes	Yes
Arab conquest (x Post)	No	No	No	No	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	2,056	2,056	2,056	2,056	2,056
R^2	0.828	0.838	0.846	0.849	0.849

Notes: Estimation via OLS. Robust standard errors, clustered by country reported in parentheses. *, **, *** = significant at 10, 5, and 1 percent respectively. The unit of observation is country-year. Years since Agricultural Transition and Arab Conquest vary across country but not year.

Excluding Muslim countries. Skeptics may worry that our findings are driven by particular Muslim countries or perhaps countries from the entire Middle East and North Africa (MENA) region. Table S8.4 shows this is not the case. In this table, each row represents our the coefficient on Muslim x Post WTO from our main specification (equation 1) when we drop observations from each Muslim country (one-by-one) from our of non-oil producing sample. The final row reports a highly constrained specification where we drop *all* countries from the MENA region. Many of these countries tend to exhibit highly robust and durable authoritarian political structures (Chaney 2012, Menaldo 2013). Reassuringly, in this specification the effect of Muslim x Post WTO remains negative and statistically significant. This implies that Muslim countries *outside* the MENA region globalize less after the WTO's creation (relative to non-Muslim countries).

Table S8.4: *Globalization across Muslim and non-Muslim countries, dropping Muslim countries (one-by-one)*

Effect on Globalization index, de jure				
	Muslim x Post WTO			
Excluded country	Coefficient	SE	Observations	R^2
	(1)	(2)	(3)	(4)
Albania	-7.595**	(3.268)	2,140	0.850
Bangladesh	-7.357**	(3.366)	2,131	0.838
Burkina Faso	-5.774*	(2.921)	2,130	0.851
Egypt	-7.279**	(3.052)	2,130	0.850
Gambia	-9.351***	(2.656)	2,130	0.854
Guinea	-7.230**	(3.255)	2,146	0.844
Jordan	-6.762**	(3.040)	2,135	0.850
Lebanon	-7.653**	(3.047)	2,148	0.850
Mali	-7.311**	(3.062)	2,130	0.847
Morocco	-7.334**	(3.040)	2,130	0.849
Niger	-7.354**	(3.072)	2,130	0.846
Pakistan	-7.464**	(3.051)	2,130	0.848
Senegal	-8.249**	(3.236)	2,130	0.848
Sierra Leone	-7.342**	(3.491)	2,130	0.846
Sudan	-6.338**	(3.116)	2,130	0.854
Tunisia	-7.350**	(3.047)	2,130	0.847
Turkey	-7.335**	(3.041)	2,130	0.848
All countries in the MENA region	-6.835**	(3.059)	2,015	0.851

Notes: Estimation via OLS. Robust standard errors, clustered by country reported in parentheses. *, **, *** = significant at 10, 5, and 1 percent respectively. The unit of observation is country-year. Each row reports the coefficient on Muslim x Post WTO (on the KOF globalization index, de jure) in a sample that excludes observations from the indicated country in the "Excluded country." All specifications control for Years since Agricultural Transition x Post WTO, GDP per capita (log), total population (log), Arab conquest x Post WTO, country and year fixed effects. These coefficients and a constant are not reported.

Appendix S9: Evaluating competing explanations

It is plausible that our main results in Table 1 may be driven by other factors that differentially affect *de jure* globalization (across Muslim and non-Muslim countries) after the WTO's creation. If these competing explanations "matter" one would expect them to *weaken* the statistical effect of $Muslim_i \times Post_t$ on globalization. We consider two broad categories of explanations: geographic determinants of trade (e.g., market potential, distance to ports, etc.) and measures of political stability (e.g., civil unrest, ethnic fragmentation). In this appendix, we evaluate these explanations by controlling for their interactive effect (with $Post_t$) in our baseline specification given by equation (1). Our analysis shows $Muslim_i \times Post_t$ to remain robust in these regressions. This suggests these competing explanations are *not* driving the globalization deficit in Muslim societies.

Geographic determinants of trade. Workhorse models of international trade demonstrate that markets (populations) more distant from the coast or navigable rivers tend to engage in less trade. We consider four standard measures. Columns 1-2 in Table S9.1 show that countries with a greater share of its surface area or population within 100 kilometers of the sea or river exhibit higher levels of *de jure* globalization after the WTO shock. Columns 3-4 show that landlocked countries and those whose centroid is farther from a coast or navigable river exhibit lower levels of *de jure* globalization after the WTO shock. These effects are consistent with existing models. Across all four specifications, the effect of $Muslim_i \times Post_t$ remains highly statistically significant (p-value$<$0.01) with a relatively stable coefficient estimate hovering between -7 to -8.1.

Geography may also affect export capacity and market potential (Head and Mayer 2004, Redding and Venables 2004). Columns 5-8 control for several measures of market potential (interacted with $Post_t$) stemming from work in economic geography. While the coefficient on $Muslim_i \times Post_t$ is reduced slightly, our main DD effect remains statistically significant. In these specifications, only Head and Mayer's (2004) measure of real market potential is a robust determinant of a country's level of *de jure* globalization after the WTO's creation.

Table S9.1: *Robustness to geographic drivers of trade*

	KOF Globalization Index, de jure							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Muslim x Post WTO	-7.555***	-8.145***	-7.188***	-7.908***	-7.167***	-7.311***	-6.898***	-6.640**
	(1.949)	(1.940)	(2.247)	(2.197)	(2.331)	(2.583)	(2.349)	(2.608)
Additional controls: (x Post WTO)								
Share of surface area within 100 km of sea or river	0.0994***							
	(0.0295)							
Share of population within 100 km of sea or river		0.0972***						
		(0.0281)						
Distance from coast or navigable river			-0.00629*					
			(0.00332)					
Dummy for landlocked countries				-5.166*				
				(2.708)				
Log of real market potential (Head and Mayer)					4.169***			
					(1.376)			
Log of foreign market potential (Head and Mayer)						-0.997		
						(2.763)		
Log of real market potential (Redding and Venables)							1.526	
							(0.983)	
Log of foreign market potential (Redding and Venables)								-2.866
								(2.840)
Observations	2,130	2,130	2,176	2,176	2,176	2,176	2,176	2,176
R^2	0.861	0.861	0.854	0.854	0.857	0.849	0.851	0.850

Notes: Robust standard errors, clustered by country in parentheses. *, **, *** = significant at 10, 5, and 1 percent respectively. All specifications include baseline controls (years since agricultural transition x Post, log GDP per capita), country and year fixed effects. These coefficients and a constant are not reported.

Table S9.2: *Robustness to measures of political in(stability)*

	KOF Globalization Index, de jure						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Muslim x Post WTO	-7.716***	-7.484***	-7.597***	-7.183***	-7.588***	-6.794***	-5.227**
	(2.344)	(2.300)	(2.385)	(2.205)	(2.406)	(2.474)	(2.234)
Controls: (x Post WTO)							
Occurrences of civil unrest	No	Yes	No	No	No	No	No
Likelihood of civil unrest	No	No	Yes	No	No	No	No
War	No	No	No	Yes	No	No	No
Cross-border conflict, ICRG	No	No	No	No	Yes	No	No
External conflict risk, ICRG	No	No	No	No	No	Yes	No
Civil war risk, ICRG	No	No	No	No	No	No	Yes
Observations	2,176	2,176	2,176	2,176	2,176	2,176	2,176
R^2	0.849	0.850	0.849	0.854	0.849	0.851	0.857

Notes: Robust standard errors, clustered by country in parentheses. *, **, *** = significant at 10, 5, and 1 percent respectively. All specifications include baseline controls (years since agricultural transition x Post, log GDP per capita), country and year fixed effects. These coefficients and a constant are not reported. Additional controls are the country average values of the variables in the pre-treatment period (i.e., prior to 1995) and their interaction with the post-WTO indicator variable.

Political stability. Governments experiencing or facing a heightened risk of political instability (e.g., civil unrest, interstate state) may be less inclined to pursue policies that liberalize cross-border economic exchange. This concern may be particularly acute in many Muslim societies which are prone to experiencing civil unrest and interstate war (Kuran 2018). To the extent that heightened political instability is correlated with our Muslim dummy, failing to account for such unrest may comprise omitted variable bias. In Table S9.2 we control

for several measures of intrastate and interstate violence, each interacted with $Post_t$. These measures include both realized (e.g., incidence) and perceived (e.g., risk) types of political instability. Across all specifications, our estimated effect of $Muslim_i \times Post_t$ on *de jure* globalization remains negative and statistically significant.

References

Head, Keith and Thierry Mayer. 2004. "Market Potential and the Location of Japanese Investment in the European Union", *Review of Economics and Statistics*, 86(4), 959?972.

Redding, Stephen and Anthony J. Venables. 2004. "Economic geography and international inequality", *Journal of International Economics*, 62: 53-82.

Appendix S10: Potential threats to causal inference

There are two main threats to our causal inferences: violation of the parallel trends assumption and endogenous selection. In this appendix we perform a number of exercises to mitigate these concerns.

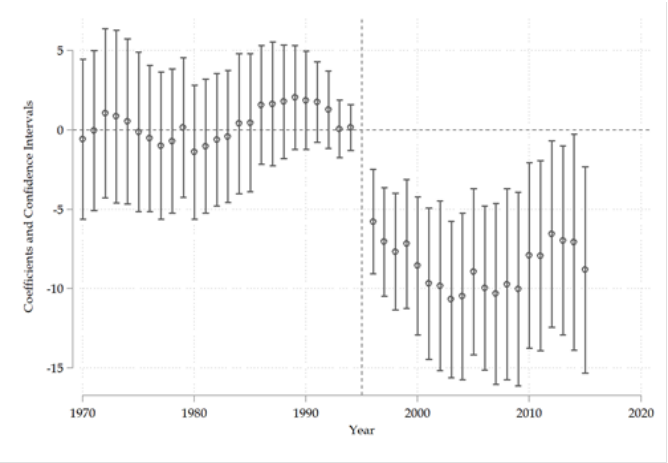
We first consider additional tests for violations of the parallel trends. In section 4.3, we decomposed our main DD estimates with a flexible specification that interacts our treatment dummy (i.e., whether a country is Muslim) on the full set of year fixed effects. Our estimates (presented in Figure 3) reveal that Muslim countries were no different from non-Muslim countries in their level of *de jure* globalization prior to the WTO's creation. In this appendix, we conduct additional evaluations that demonstrate that (1) Muslim countries did not differ not in their "trend differences" prior to the WTO's creation based on an approach advanced in Kahn-Lang and Lang (2020) and (2) show our main DD estimates hold in specifications that account for group-specific time trends as well in specifications that statistically account (i.e., "re-weight" the data) for possible pre-treatment differences in globalization trajectories between Muslim and non-Muslim countries using the synthetic difference-in-differences estimator developed by Arkengelsky et al. (2021).

Second, we probe possible concerns with endogenous selection with several approaches. One concern is that accession to the WTO is not exogenous; rather, it reflects an explicit policy decision of governments. For instance, Figure 3 in the main text suggests there may be possible pre-trends soon before and after the 1990. In this regard, we test for possible pre-trends with leads, country-specific trends, and employ a new methodology to help rule out any pre-trends. Finally, to address concerns about potential bias associated with selection on unobservables, we employ a test statistic developed by Altonji et al. (2005) and demonstrate that (potential) selection on unobservables is unlikely to bias our inferences. This test also allows us to evaluate whether unobservables related to the initial decision to join the WTO (or not) unduly affects our results. Together, these findings reassure our causal interpretation: the globalization deficit widened in Muslim countries (relative to non-Muslim countries) after the WTO's creation.

S10.1 Parallel trends

Testing for "trend differences." The causal interpretation of our results is bolstered if the parallel trends assumption is not violated: in the absence of the treatment (WTO-shock), the difference between the treatment (Muslim) and control (non-Muslim) group is constant over time. While there are no formal tests per se for this assumption, there are several specification tests to account for differential trends across treated and non-treated units. We conduct several exercises that reassures us that the parallel trends assumption is unlikely to be violated. First, our flexible specification reveals that Muslim and non-Muslim countries did not differ in their levels of *de jure* globalization prior to the WTO shock. As Figure 3 in the main paper shows, while the difference in the *de jure* globalization index between Muslim and non-Muslim countries is positive, the magnitude is very small (about 1-2 index points) and statistically indistinguishable from zero.

Figure S10.1: *Testing for trend differences based on Kahn-Lang and Lang (2020)*



Notes: Each point refers to the corresponding year fixed effect (Y_t) interacted with $Muslim_i$ on *de jure* globalization based on the procedure described in Kahn-Lang and Lang (2020), with the corresponding 95 percent confidence interval. Standard errors are clustered at the country level. The regression controls for Years since $Agriculturaltransition_i \times Post_t$, the log of GDP per capita, country and year fixed effects.

Our second exercise, tests for differences in trends of *de jure* globalization in the pre-shock period between Muslim and non-Muslim countries. Following the approach in Kahn-Lang and Lang (2020), we use the year prior to the treatment (i.e., in our case 1994) as the base year and estimate the differences between our control (non-Muslim) and treatment (Muslim) groups in each previous year relative to the base year. This allows us to test the null hypothesis that outcomes prior to the treatment year exhibited parallel trends. Conditional on our baseline controls (i.e., log GDP per capita, time since the Neolithic transition, country and year fixed effects), we fail to reject the null of equal trends (see Figure S10.1 for a visual inspection).

Our third approach includes a linear time trend as well as the linear trend interacted with our dummy for the treatment group ($Muslim_i$) in our main specification. Including these additional trends does not affect the negative and statistically effect on our main DD interaction ($Muslim_i \times Post_t$).[40] Furthermore, the interaction between the linear time trend and $Muslim_i$ is statistically insignificant. Together, these findings show that even if there was a difference in the pre-trend for Muslim and non-Muslim countries, our main DD effect continues to hold even controlling for this "trend difference" in the pre-WTO shock period in our main specification.

Synthetic controls. The analysis above employs several strategies to suggest that the parallel trends assumption is unlikely to be violated. That said, skeptics may also worry that the units in our treatment (Muslim countries) and control (non-Muslim countries) groups

[40]Results available upon request.

differ systematically prior to our treatment (i.e., the WTO's creation). One strategy to mitigate this concern is to construct a "synthetic control" that reweights units in the control group to closely align (match) the trajectory of globalization in the treated group (prior to the treatment) and re-estimate our main DD regression. To do so, we employ the synthetic difference-in-differences (SDID) estimator developed by Arkhangelsky et al. (2021). For our purposes the SDID estimator is appropriate since our research design uses just one treatment administered in one time period (i.e., the advent of the WTO in 1995), rather than a staggered treatment administered over multiple periods, and possibly administered to different units in the treatment group (Imai et al. 2021).

The SDID approach combines the attractive features of synthetic controls (SC) and difference-in-differences (DID). As Arkhangelsky et al. (2021, 4089) note: "Like SC, our method reweights and matches pre-exposure trends to weaken the reliance on parallel trend like assumptions. Like DID, our method is invariant to additive unit-level shifts and allows for valid large-panel inference." Whereas conventional SC approaches reweight units (i.e., countries) only, the SDID approach reweights units on the temporal dimension as well. This improves the overall fit and precision (efficiency) of the DID estimates. Specifically, "unit weights are designed so that the average outcome for the treated units is approximately parallel to the weighted average for control units. Time weights are designed so that the average posttreatment outcome for each of the control units differs by a constant from the weighted average of the pretreatment outcomes for the same control units. Together, these weights make the DID strategy more plausible" (Arkhangelsky et al. 2021, 4090).

Table S10.1: *Globalization across Muslim and non-Muslim countries using synthetic difference-in-differences (SDID)*

	KOF Globalization Index, *de jure*				
	(1)	(2)	(3)	(4)	(5)
Muslim x Post WTO	-5.708***	-6.467**	-7.170**	-7.109**	-7.109**
	(2.049)	(3.184)	(3.083)	(3.100)	(3.100)
Controls:					
Years since Agricultural Transition (x Post)		✓	✓	✓	✓
GDP per capita, natural log			✓	✓	✓
Total population, natural log				✓	✓
Arab conquest (x Post)					✓
Country fixed effects	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓

Notes: Estimation via OLS using Arkhangelsky et al.'s (2021) SDID estimator. Bootstrapped standard errors reported in parentheses. *, **, *** = significant at 10, 5, and 1 percent respectively. Years since Agricultural Transition and Arab Conquest vary across country but not year.

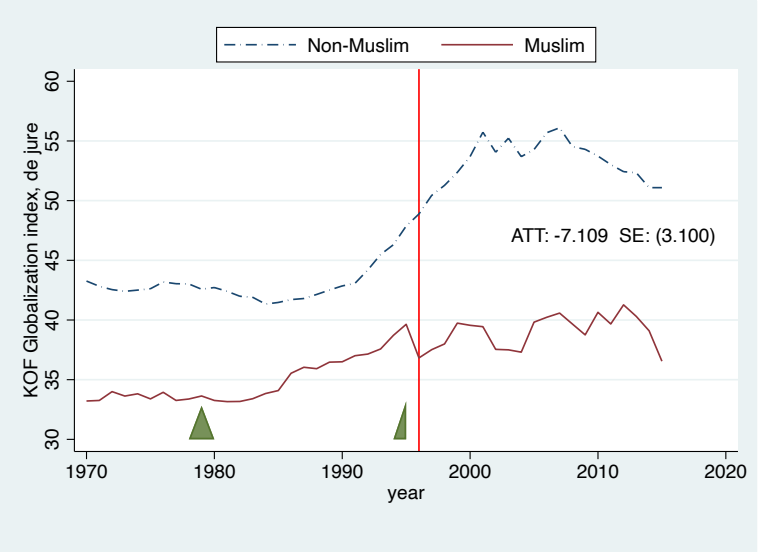
In Table S10.1 we re-estimate our main DD specifications (corresponding to Table 1 in the main text) using the SDID estimator.[41] Across all the specifications, the estimated treatment effect is negative and statistically significant, which implies that Muslim countries

[41] Estimation via SDID requires a balanced panel of country-year observations. This means several coun-

globalized less relative to non-Muslim countries after the WTO's creation. The coefficient estimates are slightly smaller in magnitude relative to those reported in Table 1, suggesting that re-weighting countries in the control group (marginally) improves the precision of our main DD estimates. That said, our substantive interpretation is largely unchanged. To see this, Figure S10.2 plots the trajectory of globalization in Muslim and our synthetic control of non-Muslim countries corresponding to our full model (i.e., column 5, Table S5.1). Prior to 1995, the average level of globalization in Muslim countries is about 6.5 to 7 index points less than average in the synthetic control of non-Muslim countries. After the WTO's creation, this difference grows to around 14 index points. The corresponding difference-in-difference is around 6.5 to 7 index points; which is similar to the average treatment effect on the treated (ATT) of -7.109.

Another attractive feature of the SDID estimator is the ability to transparently plot the weights for countries in our control group. We do so in Figure S10.3 where countries with larger circles receive greater weight in constructing the synthetic control. For instance, Bolivia, Uruguay, and Zambia play a more prominent role in constructing the synthetic control relative to Chile and Honduras.

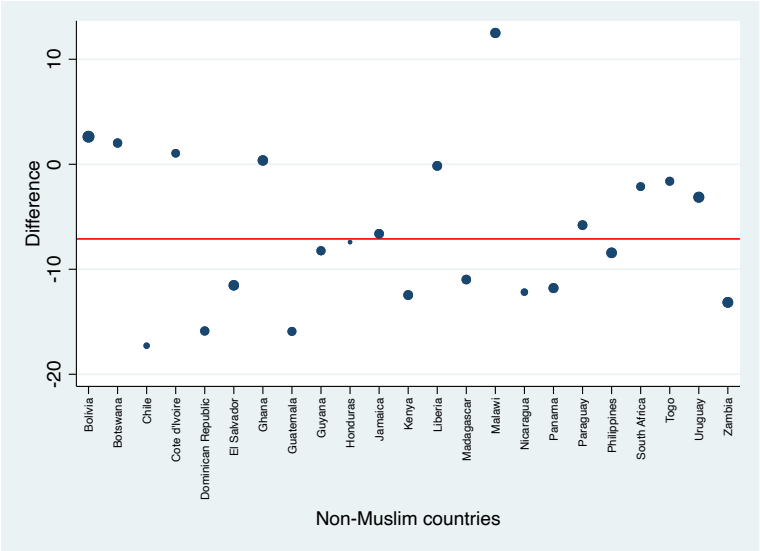
Figure S10.2: *Globalization trajectory, Muslim versus non-Muslim synthetic control*



Notes: Figure plots trajectory of *de jure* globalization between Muslim and synthetic control of non-Muslim countries. The plot accounts for baseline controls, country and year fixed effects corresponding to the specification in column 5 of Table S10.1. In the figure (from left to right), the first triangle corresponds to the year (in the pre-treatment) where the difference between globalization in Muslim and non-Muslim countries is the greatest. The second triangle corresponds to the treatment year (=1995).

tries in the treatment (Muslim) and control (non-Muslim) groups are excluded in the analysis. Despite this smaller sample size, as Table S5.1 shows the SDID estimates are close in magnitude and statistical significance to the benchmarks results in Table 1.

Figure S10.3: *Country weights to construct synthetic control*



Notes: Each point refers to the weight each country in the control group (non-Muslim countries) receives in the specification in column 5 of Table S5.1.

S10.2 Endogenous selection

Testing for pre-trends. While we are careful to document that Muslim countries do not differ in the likelihood (timing) of accession to the WTO (see Appendix S7), a country's accession to the WTO is a policy choice and these choices are unlikely to be plausibly exogenous.[42] We estimate several additional specifications to mitigate concerns about possible (endogenous) pre-trends. We first include leads, where we interact a Muslim dummy with the indicator variables for the years *prior* to the WTO's creation (e.g., Muslim dummy x dummy variable for year 1994, etc.). We then estimate a specification with country-specific trends.

The regression specifications in Table S10.2 show that the paper's main finding remains robust when we control for various leads and country-specific trends. The leads interact whether a country is Muslim with a dummy variable for each year before (up to 5 years) the WTO's creation. These leads are neither individually nor jointly significant suggesting that Muslim countries did not exhibit a different globalization trajectory (relative to non-Muslim countries) in the years preceding the WTO's creation. This inference is consistent with the patterns in Figure 3 of our paper. Moreover, when we control for country-specific time trends (see columns 6 and 7) we observe a larger effect on $Muslim_i \times Post_t$ compared to our baseline estimates in Table 1.

[42]For instance there is some evidence in Figure 4 of possible pre-trends in the years soon and after 1990.

Table S10.2: *Testing for pre-trends*

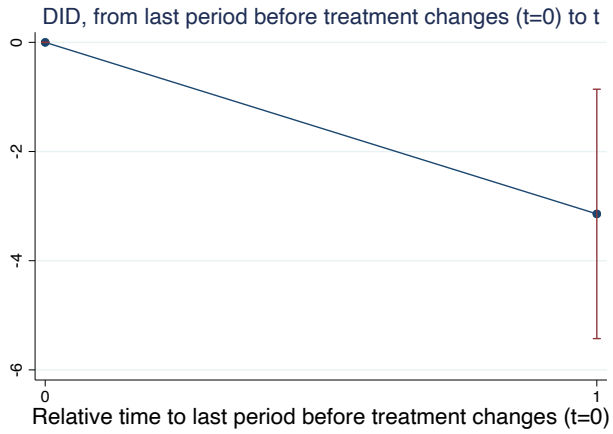
	KOF Globalization Index, *de jure*						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Muslim x Post WTO	-7.356	-7.362	-7.303	-7.213	-7.106	-9.428	-9.709
	(3.085)***	(3.122)**	(3.156)**	(3.190)**	(3.214)**	(1.923)***	(2.498)***
Muslim x Year 1994	0.055	0.049	0.108	0.197	0.305		-1.237
	(1.643)	(1.716)	(1.776)	(1.827)	(1.862)		(2.006)
Muslim x Year 1993		-0.11	-0.052	0.037	0.145		-1.271
		(1.491)	(1.554)	(1.609)	(1.648)		(1.703)
Muslim x Year 1992			1.078	1.166	1.274		-0.016
			(1.329)	(1.390)	(1.435)		(1.505)
Muslim x Year 1991				1.563	1.669		0.517
				(1.207)	(1.259)		(1.218)
Muslim x Year 1990					1.891		0.527
					(1.019)		(1.126)
Country FE x Year trend						Yes	Yes
F-statistic: Joint significance of leads		0.04	1.09	1.23	1.28		0.83
... p-value		0.961	0.363	0.309	0.287		0.527
Baseline controls	✓	✓	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓
Observations	2,176	2,176	2,176	2,176	2,176	2,176	2,176
R^2	0.849	0.849	0.849	0.849	0.849	0.924	0.924

Notes: Estimation via OLS. Robust standard errors, clustered by country reported in parentheses.*, **, *** = significant at 10, 5, and 1 percent respectively. All specifications control for baseline controls (see Table 1), country and year fixed effects. These coefficients and a constant are not reported. Baseline controls include: log GDP per capita, log total population, years since agricultural transition x Post WTO, Arab conquest x Post WTO.

Ruling out parallel trends. In a recent paper, Chaisemartin and D'Haultfoeuille (2020) develop an estimator to account for/rule out parallel trends. This multiple period dynamic estimator provides a flexible strategy to "control" for pre-trends across (multiple) treatment groups (versus controls) across multiple treatment periods (t $\geq$ 1).[43] In implementing this estimator, our paper's empirical setup is simple: we have one treatment period (post-1995) and one treated group (Muslim countries). This means our treatment is the (average) "difference" across one time period (before and after 1995). With this estimator, our DID is reduced to -3.142 with a standard error of 1.165. The resulting 95 percent confidence (from -5.427 to -0.858) does not contain zero and is therefore statistical significant at the 5 percent level. We graph the corresponding DID effect in Figure S10.4.

[43]When implemented across multiple treatment periods, the estimator generates "event-study" like DID treatment effects.

Figure S10.4: *Average (DID) treatment effect from dynamic DID estimator*



Notes: Average treatment effect for Muslim countries based on dynamic DID estimator developed by Chaisemartin et al. (2019)

Selection on unobservables. Despite our attempts to control for many observable factors (e.g., the historical and geographic drivers of globalization, fixed effects, etc.) our main DD estimates may still be biased by unobservable factors correlated with selection into the WTO and subsequent patterns of globalization. To assess the likelihood that selection on unobservables biases our inferences, we calculate a test statistic derived from Altonji et al. (2005) that quantifies how much stronger selection on unobservables, relative to selection on observables, must be to explain away the full estimated effect. We follow an empirical application from Nunn and Wantchekon (2011) that "compares" the regression coefficient on $Muslim_i \times Post_t$ from estimating equation (1) with a restricted set of controls ($\hat{\beta}^R$) against another with a full set of controls ($\hat{\beta}^F$). We then calculate the ratio: $\hat{\beta}^F/(\hat{\beta}^R\text{-}\hat{\beta}^F)$, where a value less than 1 implies selection on unobservables is greater than selection on observables. In interpreting this ratio, Nunn and Wantchekon (2011, 3238) state: "The intuition behind the formula is straightforward. First, consider why the ratio is decreasing in ($\hat{\beta}^R\text{-}\hat{\beta}^F$). The smaller is the difference, the less the estimate is affected by selection on observables, and the stronger selection on unobservables needs to be (relative to observables) to explain away the entire effect. Next, consider the intuition behind $\hat{\beta}^F$ in the numerator. The larger $\hat{\beta}^F$, the greater is the effect that needs to be explained away by selection on unobservables, and therefore the higher is the ratio."

We estimated various restricted regressions and report ratios associated with a parsimonious specification that controls for per capita GDP, the interaction of Years since the Agricultural Transition and $Post_t$, and country and year fixed effects (i.e., this corresponds to column 3 in Table 1). We consider two sets of full covariates: the baseline set of controls from equation (1) corresponding to column 5 in Table 1 and a second, adding to this the geographic determinants of trade (e.g., share of a country's territory within 100km of a river or sea, landlock dummy, measure of real market potential) all interacted with $Post_t$. Performing this exercise yields two ratios of 4.53 and 5.80 (the latter associated with the

second "full covariate" model). Taking the lower value implies that to attribute the entire OLS estimate to selection effects, selection on unobservables would have to be at least four times greater than selection on observables. In our view, this inference makes it less likely that the estimated effect of $Muslim_i \times Post_t$ is fully driven by unobservables.

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Kahn-Lang, Ariella and and Kevin Lang. 2020. "The Promise and Pitfalls of Differences-in-Differences: Reflections on 16 and Pregnant and Other Applications", *Journal of Business & Economic Statistics*, 38(3): 613-620.

Appendix S11: Pre-WTO policy choices

As we argued in section 2, policy choices comprise pathways for governments in less democratic settings to partially liberalize. One policy dimension is a country's overall stance on tariffs. To capture this, we use the overall trade restrictiveness index (OTRI) in manufacturing and all sectors compiled by the World Bank, where a higher index corresponds to a greater commitment to reduce tariffs.[44] Another policy dimension relates to the number and strength of commitments (depth) of preferential trade agreements (PTAs) adopted by governments. If governments are hesitant to liberalize, they may opt for fewer PTAs and those with less depth. To measure these aspects of PTAs, we draw on information from the DESTA database (Dür et al. 2014).

Table S11.1 evaluates whether these policy choices shaped a country's *de jure* globalization after the WTO shock. To capture this differential effect, we interact a country's average value on these measures in the pre-shock period (i.e., prior to 1995) and our post-WTO shock, $Post_t$. We re-estimate our baseline specification given by equation (1) with these interactive policy measures as additional controls. Two important patterns emerge. First, countries that adopted more favorable policy stances towards trade liberalization (e.g., signed more PTAs) experience larger gains in *de jure* globalization after the WTO shock (compared to before). Second, the estimated effect on $Muslim_i \times Post_t$ weakens, both in magnitude and statistical significance. For instance, the coefficient estimate on $Muslim_i \times Post_t$ in column (4) is 40 percent smaller compared to our benchmark estimate in column (1) that does not control for policy choices. Moreover, $Muslim_i \times Post_t$ is no longer statistically significant.

Together, these two patterns suggest that policy choices may be important mediating factors. Substantively, it implies that our "Muslim effect" is likely capturing the differential policy choices these governments chose (relative to non-Muslim countries) in the pre-WTO period and the subsequent effect it had after the WTO's creation. Table S11.2 provides additional evidence that governments in Muslim countries pursued PTAs with less stringent commitments towards liberalization prior to the WTO's creation. We regress the average depth of a country's PTAs in the pre-WTO period on a Muslim dummy and control for a series of confounding factors (e.g., geographic factors, average receipts of rents, per capita GDP, a democracy indicator, etc.). Across these specifications, the coefficient on Muslim is negative, quite stable, and statistically significant.

[44]The Overall Trade Restrictiveness Index (OTRI) summarizes the trade policy stance of a country by calculating the uniform tariff that will keep its overall imports at the current level when the country in fact has different tariffs for different goods. In a nutshell, the OTRI is a more sophisticated way to calculate the weighted average tariff of a given country, with the weights reflect the composition of import volume and import demand elasticities of each imported product.

Table S11.1: *Economic policy choices and globalization*

	KOF Globalization Index, de jure						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Consistent sample				Full sample	
Muslim x Post WTO	-7.422**	-5.793**	-5.008	-4.068	-4.435	-4.115	-4.325
	(3.099)	(2.421)	(3.043)	(3.520)	(3.197)	(3.507)	(3.153)
Policy controls: (x Post WTO)							
Overall Trade Restrictiveness, manufacturing		58.666***					
		(12.029)					
Overall Trade Restrictiveness, all sectors			42.986**				
			(16.776)				
Number of deep FTAs, maximum				2.702***		2.424***	
				(0.807)		(0.806)	
Depth of FTAs, average					7.067***		5.793***
					(1.702)		(1.730)
Baseline controls							
Log population	-7.518*	-7.506**	-7.539*	-0.684	-8.952**	-1.932	-7.905*
	(4.258)	(3.626)	(3.945)	(4.099)	(3.943)	(4.057)	(4.002)
Log GDP per capita	4.191*	3.876*	3.762*	3.987**	3.896**	4.341**	4.229**
	(2.223)	(1.984)	(2.044)	(1.799)	(1.821)	(1.872)	(1.927)**
Years since Agricultural Transition x Post WTO	1.213**	0.872*	1.010**	1.400***	1.322**	1.225***	0.955*
	(0.511)	(0.485)	(0.490)	(0.410)	(0.517)	(0.444)	(0.536)
Arab conquest x Post WTO	-1.019	-0.211	-0.268	-2.302	1.034	-1.817	1.604
	(3.218)	(2.806)	(3.127)	(3.533)	(3.606)	(3.599)	(3.590)
Country fixed effects	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓	✓
Observations	2,089	2,089	2,089	2,089	2,089	2,176	2,176
R^2	0.850	0.868	0.857	0.863	0.866	0.859	0.861

Notes: Robust standard errors, clustered by country in parentheses. *, **, *** = significant at 10, 5, and 1 percent respectively. Overall Trade restrictiveness (manufacturing, all sectors), and the number and depth of FTAs are country averages prior to the WTO's creation. The "policy control" variables are the pre-period (i.e., before 1995) average value interacted with $Post\ WTO_t$. In columns 1-4, the sample is held constant. We refer to this as a consistent sample.

Table S11.2: *Depth of trade agreements in Muslim and non-Muslim countries prior to 1995*

	Depth of Free Trade Agreements					
	(1)	(2)	(3)	(4)	(5)	(6)
Muslim	-0.607**	-0.718**	-0.714**	-0.714**	-0.714**	-0.670**
	(0.276)	(0.312)	(0.325)	(0.313)	(0.318)	(0.291)
Controls						
Latitude	0.011**	0.014	0.019**	0.019*	0.019**	(0.018)**
	(0.005)	(0.009)	(0.009)	(0.010)	(0.009)	(0.009)
Longitude	0.003	-0.0003	-0.0003	-0.0003	-0.0003	-0.0009
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Foreign Aid (% of GDP)			0.020	0.020	0.020	0.013
			(0.013)	(0.017)	(0.018)	(0.018)
Remittances (% of GDP)			-0.039**	-0.039**	-0.039*	-0.044*
			(0.018)	(0.020)	(0.020)	(0.023)
Log of GDP per capita				-0.002	-0.006	-0.069
				(0.153)	(0.0173)	(0.157)
Democracy indicator					0.048	0.129
					(0.382)	(0.371)
Total trade (% of GDP)						0.004
						(0.004)
Fixed effects for regional FTA						
Americas		-0.810**	-0.595*	-0.594*	-0.607	-0.673
		(0.302)	(0.335)	(0.352)	(0.375)	(0.390)
Asia		-0.097	-0.129	-0.129	-0.139	-0.104
		(0.702)	(0.693)	(0.705)	(0.754)	(0.75)
Europe		-0.311	-0.302	-0.299	-0.303	-0.297
		(0.559)	(0.644)	(0.773)	(0.774)	(0.794)
Intercontinental		-0.511**	-0.370	-0.369	-0.369	-0.320
		(0.243)	(0.228)	(0.261)	(0.265)	(0.248)
Countries	56	56	56	56	56	56
R^2	0.208	0.333	0.396	0.396	0.397	0.407
Adjusted R^2	0.162	0.235	0.278	0.262	0.246	0.241

Notes: Robust standard errors in parentheses. ** = significant at 5 percent.The dependent variable, "Depth of trade agreements", is drawn from Dür et a.l (2014) and where a higher value corresponds to more depth (i.e., stricter PTA commitments). Foreign aid (% GDP), remittances (% GDP), log GDP per capita, democracy indicator, and total trade (% GDP) are country averages. Following Dür et al. (2014) we take into account within region FTAs (e.g., NAFTA for the Americas) with fixed effects at continent level (i.e., "Americas", "Asia", etc.). Intercontinental refers to FTAs involving countries across different continents. Since very few countries in Africa and Oceania participate in within-region FTAs (Dür et al. 2014) we omit fixed effects for Africa and Oceania.

Appendix S12: Foreign aid and competition law

With the global push towards a market-oriented economy since the late 1980s, countries across the world improved their legal regimes to fair competition in the market by prohibiting and penalizing anti-competitive practices (e.g. entry barriers, abuse of dominance, etc.). With more entrenched cronyism in aid-rich countries, we expect that these countries are likely to have witnessed less improvement in the legal regimes governing market competition. To empirically substantiate this, we use the Competition Law Index (or the CLI) constructed by Bradford and Chilton (2018), which provides a systematic measure of the intensity of competition-related regulations across countries and over time.[45] The Competition Law Index (CLI) measures the scope and strength of its competition laws over time. It tracks both the legal powers given to regulators and the actual content of the laws, combining these factors into a single score that reflects how strictly competition is regulated in that country based on formal laws. This score indicates how much regulatory pressure businesses may face when operating in the market. The CLI score, normalized to fall between 0 and 1, increases when competition laws ban a wider range of business practices or offer stronger remedies. On the other hand, the score decreases if the laws include more exceptions, defenses, or loopholes that reduce their overall impact.[46]

The change in competition regime is evident in the dataset: in 1990 only 51 countries had a competition law but, by 2010, 126 countries had some competition provision in their legal regimes. Given the analytical priors described in section 2.1, we expect that aid-rich countries are likely to have made relatively slower progress in improving their competition laws. To empirically probe this, we run a fixed effects model that regresses the mean CLI score on net ODA per capita, flexibly estimating its marginal effect of foreign aid over time. The model includes country fixed effects and standard errors are clustered by country. Figure S12.1 shows the resulting coefficient plot that maps out the effect of net ODA per capita on CLI over time. As is clear, the effect of aid is indistinguishable from zero till 1991. However, the aid effect becomes negative and statistically significant from 1992 onwards. This is consistent with our priors: as countries sought to expand the scope and stringency of their competition laws after 1990, aid-rich countries saw a disproportionately slower improvement in their competition laws. This slower progress is probably rooted in greater resistance from entrenched crony actors to open up the market to greater competition. Figures S12.2 and S12.3 summarize the CLI for our sample of treated (Muslim) and control (non-Muslim) countries. As Figure S12.2 shows, the average CLI score for the Muslim sample is noticeably lower than the non-Muslim group. The same empirical pattern holds for a principal components (PCA) measure of the 36 variables included in the CLI (see figure S12.3).[47]

[45] Data on the index is accessible via the website: www.comparativecompetitionlaw.org.

[46] Note that the CLI measures the law in the books. Prohibitions are treated as increasing the scope or stringency of competition law and defenses as reducing the scope or stringency of the law.

[47] The PCA score places greater weight on individuals based on the extent of overall variation they explain.

Figure S12.1: *Effect of aid per capita on the competition index, over time*

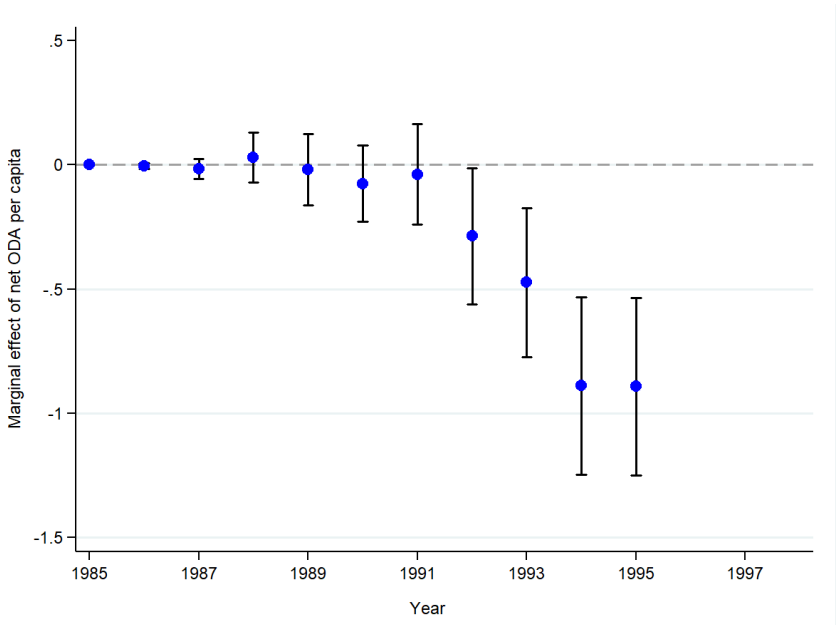


Figure S12.2: *Average CLI score across Muslim and non-Muslim countries*

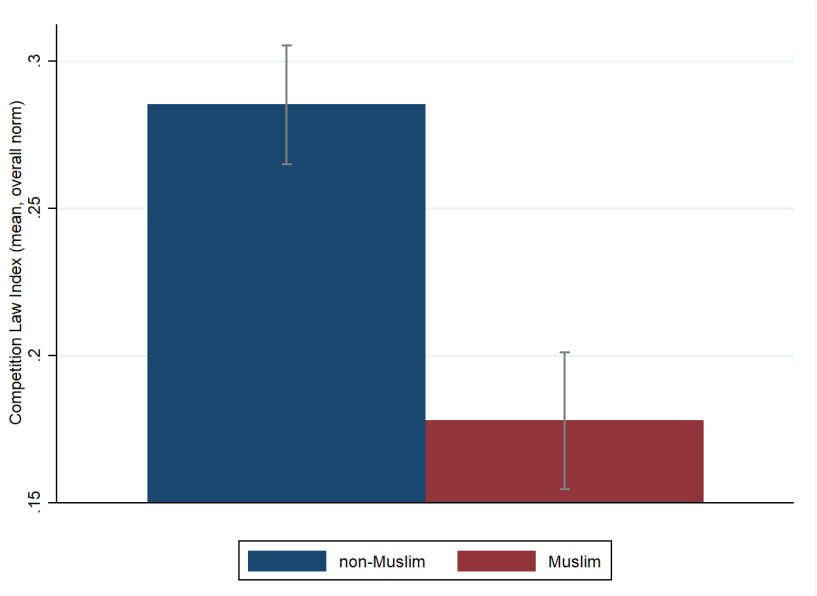


Figure S12.3: *Average CLI score (PCA) across Muslim and non-Muslim countries*

