Crony Globalization: Evidence from a Natural Experiment

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Abstract

We exploit the World Trade Organization's creation in 1995 as a natural experiment to study the politics of partial liberalization in predominantly Muslim-majority societies. Embedding this plausibly exogenous shock to economic liberalization in a difference-in-differences framework, we show Muslim societies have systematically lagged behind in relative terms (to non-Muslim countries) on measures of *de jure* globalization that capture various restrictions expressed through tariffs, hidden import barriers, and investment and capital account restrictions. We then explore channels, attributing the presence of a globalization deficit to the prevalence of nondemocratic politics and rentier political economies in Muslim societies. We further compile detailed sector-level data from several North African countries, finding slower tariff liberalization in sectors penetrated by political cronies. Our findings suggest that partial liberalization may be a strategy for regime stability in many Muslim societies.

JEL codes: D72, F13, F6, O19, P16, Z12
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There is broad evidence to suggest that Muslim-majority (hereon, Muslim) societies suffer from a long-term development disadvantage in terms of poor economic and political outcomes relative to non-Muslim societies (Kuran 2018).[1] While past work on this development deficit has considered the effects of external rents, both oil (e.g., Ross 2001) and non-oil (e.g., foreign aid and remittances, see Ahmed 2012), there has been insufficient emphasis on the political salience of domestically generated rents from foreign economic policy capture.[2] Partial liberalization in international trade and investment (and associated policies, such as regulatory barriers) may be an important source of such rents.

Governments in less democratic settings may be particularly receptive to the manipulation of the foreign investment and trade policies. In Tunisia, for example, Rijkers et al (2017) document how firms connected to the dictator Ben Ali's family disproportionately benefited from economic policies, especially in sectors subject to authorization and restrictions on foreign direct investment.[3] In this paper, we provide more systematic evidence that many Muslim societies' hesitant and partial approach towards economic may be tied to the politics of regime durability in these societies. Trade and investment policy closure and regulatory restrictions generate unearned rent streams that can be passed on to favored businesses and politically connected actors. Governments may choose a wide array and mix of policies with respect to trade, investment, capital account, and regulations in fostering this type of "crony globalization." In doing so, garnering support from commercial elites can be crucial for both the maintenance and durability of authoritarian regimes (e.g., Acemoglu and Robinson 2006, Rijkers et al 2017, Zissimos 2017, Gawande and Zissimos 2020).

While prior literature has furnished both case study and cross-country evidence on the politics of economic reform (e.g., Cammett 2007, Diwan et al 2019), our paper systematically demonstrates, for the first time, that Muslim societies are especially prone to crony globalization. Cognizant that rents from oil production may obscure valid inferences (e.g., oil wealth can foster cronyism independently of a country's international economic exposure, see Arezki and Bruckner 2011), we conservatively limit our analysis to non-oil producing developing countries. Thus, our paper's causal estimates purposefully purge the direct ef-

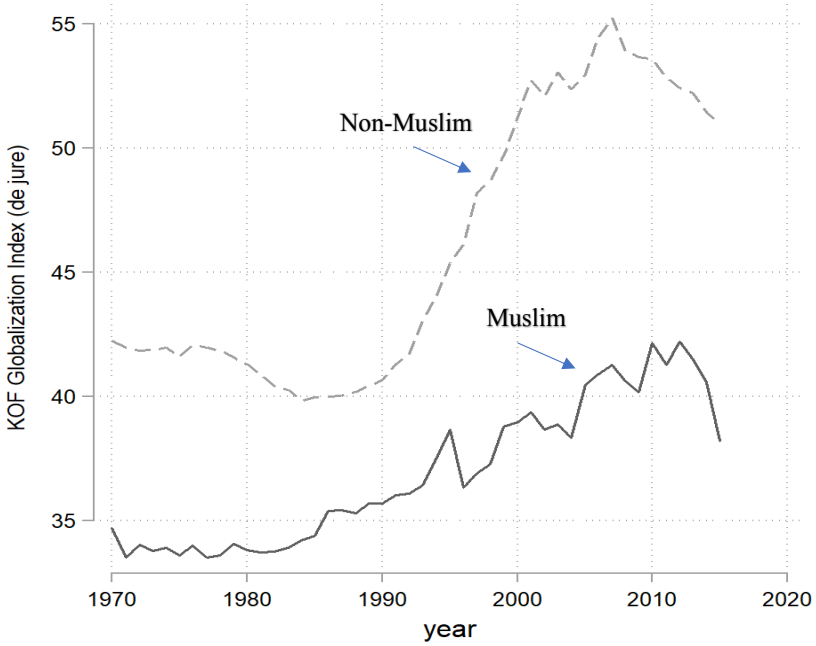
[1] Existing work has also emphasized several predominantly fixed or time-invariant characteristics of Muslim societies, including their history (e.g., Chaney 2012) and cultural norms (e.g., Fish 2002). Our analysis accounts for these explanations (and other plausible time-invariant factors) with both country fixed effects and robustness checks that evaluate these competing explanations (see section 4.3).

[2] In practice, these policies span various areas (e.g., trade, investment, regulatory barriers, etc.) and can reinforce each other. Accordingly, we analyze a composite measure of these policies (i.e., the KOF globalization index).

[3] Similarly, Ruckteschler et al (forthcoming) demonstrate how politically connected firms in Morocco enjoyed protection from non-tariff measures following the adoption the EU-Morocco FTA.

fects of oil production on various forms of pernicious political economy (e.g., corruption, rent seeking). We complement our cross-national evidence with novel sector-level data from Egypt and Tunisia to document slower tariff liberalization in sectors penetrated by political cronies.

Figure 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.

Since governments may choose a mix of policies in trade, investment, and regulations to protect elite interests, our main analysis focuses on a broad measure of globalization that captures the multifaceted scope of international economic policies.[4] To motivate our analysis, Figure 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, throughout the sample period, Muslim countries have always lagged behind their non-Muslim comparators in terms of their regulatory approach to economic globalization. Second, since 1995 there has been a greater divergence in the evolution of KOF index between Muslim and non-Muslim countries.[5] Prior to 1995, the KOF index was about

[4]In section 5, we also study various components of globalization, including a country's overall restrictiveness to trade, "depth" of trade agreements, and sector-specific tariff levels (the latter in Egypt and Tunisia).

[5]First developed and introduced in Dreher (2006), the KOF index is the most widely used measure of

7 index points (on average) lower in Muslim countries compared to non-Muslim countries. After 1995, this difference has nearly doubled to around 15 index points. Together, these patterns provide suggestive evidence that Muslim countries seem to have fallen behind their non-Muslim counterparts in terms of their *de jure* engagement with economic globalization. Our paper presents more systematic evidence of this divergence and provides evidence of a plausible channel via political cronyism.

Cognizant that omitted variables and endogeneity may unduly bias the pattern in Figure 1, we leverage the timing of the World Trade Organization's (WTO) creation as a natural experiment and employ a difference-in-differences (DD) research design to draw causal inferences. Our identification strategy leverages the timing of the WTO's establishment in 1995 as an exogenous "shock" to economic liberalization, and investigate whether Muslim countries' (our treatment group) engagement with processes of economic globalization differed substantively after WTO's establishment relative to the non-Muslim cohort (our control group).

The establishment of WTO was a fairly universal shock, since it similarly affected both Muslim and non-Muslim recipients (the average year of joining the WTO was the same for Muslim and non-Muslim recipients, i.e. 1995). The WTO's founding ushered a period of regulatory harmonization, proliferation of preferential trade agreements (PTAs) and investment treaties, and the push for "deeper" reforms (e.g., Preeg 2012). Using the KOF index of economic globalization developed by Dreher (2006) and Gygli et al (2019) and controlling for country and year fixed effects, our empirical analysis suggests substantial divergence since 1995 between Muslim and non-Muslim countries in terms of their engagement with globalization. Specifically, Muslim countries have systematically lagged behind in relative terms on measures of *de jure* globalization capturing various economic restrictions expressed through tariffs, hidden import barriers, taxes on international trade, and investment and capital account restrictions. Our statistical analysis is careful to account for the determinants of a country's decision to join the WTO and other potential confounding factors (e.g., measures of market potential, geographic determinants of globalization, historical and institutional factors).

There are two threats to our inferences: violation of the parallel trends assumption and selection on unobservables. On the former, we perform several exercises to assuage this concern. We decompose our main DD estimates with a flexible specification that regresses our

globalization in the academic literature. We employ the revised (second) version of the index from Gygli et al (2019), which has separate measures of de facto and de jure globalization. See section 3 for further discussion.

treated variable (i.e., whether a country is Muslim) on each year fixed effect. Our estimates (see Figure 2) reveal that Muslim countries were no different from non-Muslim countries in their level of *de jure* globalization prior to the WTO's creation. We further demonstrate that Muslim countries did not differ in their *trends* prior to the WTO shock based on an approach advanced in Kahn-Lang and Lang (2020) and show our main DD estimates hold in specifications that control for group-specific time trends. On the latter, we employ a test statistic developed by Altonji et al (2005) to demonstrate that (potential) selection on unobservables is unlikely to bias our inferences. Together, these exercises reassure our finding that a globalization deficit emerged in Muslim countries (relative to non-Muslim countries) after the WTO's creation.

We then study channels and present two sets of results. First, we probe why Muslim societies seem to be distinctive from non-Muslim societies by unpacking the institutional and policy characteristics that may drive the differential patterns of *de jure* globalization after the WTO's establishment. We show that Muslim countries tend to exhibit autocratic politics, are reliant on foreign rents (e.g., foreign aid), exhibit greater restrictions on trade (based on composite measures from the World Bank) and have adopted fewer preferential trade agreements that lack strong commitments to liberalization (i.e., less depth). These characteristics in Muslim societies are consistent with our conceptual framework where governments in non-democracies reward regime supporters with rents via partial trade liberalization (see section 2). As we document, in these settings, governments are likely to implement regulatory barriers and engage in shallower PTAs (i.e., those with less stringent commitments) in order to protect politically connected actors (cronies).

Our second set of results provides novel within country evidence from Egypt and Tunisia tying slower tariff liberalization in sectors penetrated by cronies. Our analysis reveals that crony sectors benefit from higher levels of tariff protection than non-crony sectors − on both the extensive and intensive margins − and, importantly, these differences have persisted after the WTO's creation. Together, our cross-national and within-country evaluation of channels provides evidence that partial liberalization may stem from policy decisions to protect politically connected and important regime supporters in many Muslim societies.

In addition to introducing a globalization deficit as a potential source of economic and political underperformance in Muslim societies (Kuran 2018), our paper contributes to broader literatures in political economy. Our paper speaks to scholarship on the political economy of dictatorship (Wintrobe 1998, Acemoglu and Robinson 2006). While much of this literature has focused on domestic economic and political factors, recent work suggests that

international economic integration, particularly capital flows, can affect the stability of nondemocratic governance (Ahmed 2020, Gao forthcoming). Our findings suggest that policies related to international trade may also affect regime stability in nondemocracies.[6]

Our paper relates to recent work highlighting how distortions may undermine trade liberalization, particularly in developing countries (Atkin and Khandewal 2020, Arezki et al 2021). Our findings emphasize how foreign economic policies can be manipulated to generate rents for elites, and these elites in turn are more inclined to support the regime. Based on prior work this connection between the commercial interests of elites and their support for the regime may be particularly relevant in many Muslim-majority societies, such as Indonesia (Fisman 2001), Pakistan (Khwaja and Mian 2005) and those in the Middle East and North Africa (Cammett 2007). Notably, our analysis shows how cronyism extends beyond Middle Eastern countries. Finally, our paper contributes to scholarship documenting how trade agreements and international organizations more broadly can affect economic and political reforms (e.g., Pevehouse 2005, Lui and Ornelas 2014, Baccini and Urpelainen 2014, Baccini 2019).

2 Conceptual framework

Our paper's central argument is predicated on the idea that governments may have an incentive to strategically and partially liberalize international economic policy to protect the economic interests of elites.[7]

2.1 Elite defection and political transitions

Prominent theories of democracy/dictatorship model the interaction of two actors − the masses ("poor") and elites − as guiding the dictator's choice of policies to remain in power (e.g., Wintrobe 1998, Bueno de Mesquita et al 2003, Acemoglu and Robinson 2006, Svolik 2012). These policies typically entail some (optimal) combination of state repression and the provision of targeted benefits (patronage). In dictatorships, the latter tends to be targeted to the elite.[8] Depending on the context, elites may comprise members of the same class (e.g.,

[6] As we discuss in the next section, Zissimos (2017) provides a formal treatment for how endogenous trade policy can affect authoritarian stability.

[7] This strategy is not exclusive to dictatorships. Governments in democracies may also protect specific private interests (industries, firms, etc.) to strengthen their electoral prospects (Grossman and Helpman 1994).

[8] 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

landholders, industrialists), occupations (e.g., the military), ethnic, and/or religious groups.

In these accounts, elite cohesion is crucial to authoritarian resilience. Without it, elite defection comprises a plausible and empirically prevalent pathway from dictatorship to (more) democracy. For example, O'Donnell and Schmitter (1986) and Collier (1999) emphasize conflict among elites as a potential source of political liberalization. Democracy arises when some subset of the authoritarian coalition (the "soft-liners") joins with the disenfranchised (masses). In selectorate theory, Bueno de Mesquita et al (2003) articulate a model that explicitly connects elite defection to the dictator's ability to supply targeted economic and political benefits to members in his "winning coalition." The model's comparative statics show that a reduction in targeted benefits (e.g., imposition of tariffs to protect sectors important to elite interests) weakens the loyalty of elites to the autocrat; which in turn, heightens the likelihood of defection to another challenger. This challenger may be another would-be dictator or could be possibly be a more representative government that can credibly supply benefits to the defecting elite.[9] More recently, Svolik's (2012) theory of authoritarian politics starts with the empirical observation that elite defection (coups) comprises more than two out of every three regime transitions since World War II.[10]

2.2 Partial liberalization in dictatorships

The importance of elite cohesion in dictatorships suggests the manipulation of foreign economic policy may be a prudent political strategy to protect the income (or "rents") of elites.[11] This protection − which, we refer to as partial liberalization − could entail various instruments, including trade taxes (tariffs), non-tariff barriers, export subsidies, regulatory barriers, exchange rate controls, investment restrictions, among many others.[12] Partial liberalization may also forestall democratization by dampening revolutionary threats from the masses. Zissimos (2017), for example, endogenizes trade policy in a model of regime formation and transitions. The model combines a Heckcher-Ohlin model of international trade and trade policy with Acemoglu and Robinson's (2000) model of regime formation to delineate

political freedoms/rule of law.

[9] In a potential transition to a democratic regime, the provision of benefits may not be targeted exclusively to elites. Rather, the benefits could be a strengthening in property right protections that improves the economic welfare of the elites and masses (e.g., by spurring more private investment and innovation).

[10] This empirical pattern in turn shapes Svolik's formal theory detailing how autocrats maintain elite cohesion.

[11] Dictators may also want to manipulate policies regarding foreign capital (Ahmed 2020, Gao forthcoming).

[12] As we describe in the next section, our measure of (*de jure*) globalization strives to capture these multiple dimensions of protection.

conditions under which elites (the dictator) may pursue protectionist policies to prevent a political transition.[13] In equilibrium, various policy options are viable. One policy entails directly protecting the economic interests of elites (e.g., via trade taxes on products from sectors controlled by the elites), thus lowering the likelihood of elite defection. This is consistent with our arguments above. Another policy choice considers a country's (relative) factor endowments and their owners. This can affect the incidence and strength of revolutionary threats: if the masses own the scarce factor, the elites (dictator) may opt to protect sectors employing these scarce factors in order to reduce the incentives to mount a revolution.[14]

Governments have a menu of policy instruments available for protection. Historically, for most developing countries with limited fiscal capacity, trade taxes (or tariffs) comprised the main instrument (Besley and Persson 2011).[15] However, as the multilateral trading system has strengthened since World War II, tariff levels around the world have fallen precipitously. In response, governments often to resort non-tariff barriers and various types of regulations (e.g., domestic content requirements, voluntary export restraints) as a means to protect (certain) economic interests in-lieu of tariffs.

In an effort to counteract these policies from their trading partners, governments increasingly sign and implement preferential (free) trade agreements (PTAs) (Maggi and Rodriguez-Clare 2007). According to Baccini (2019, 76), "the most important change is that modern PTAs not lonely reduce tariffs but also regulate investment, intellectual property rights, competition policy, government procurement, and many other matters. In other words, PTAs remove barriers not only at the border but also behind the border, producing what has been referred to as deep integration between countries." As a consequence, PTAs often help introduce and consolidate broader economic and political reforms (e.g., Pevehouse 2005, Baccini and Urpelainen 2014, Liu and Ornelas 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

[13] Acemoglu and Robinson (2006, Chapter 10) do present a model of globalization and trade liberalization and political transitions. However, since globalization is exogenous in their model, they do not consider the choice by governments over trade policy.

[14] While analytically distinct, these strategies could overlap: a dictator could protect (certain) tradeable sectors tied to elites (e.g., steel) and the masses (e.g., textiles).

[15] Countries at an early stage of development tend not to invest in domestic fiscal capacity. As Besley and Persson (2011, 41-43) state: "Arguably, trade taxes and income taxes are two polar opposite cases. To collect trade taxes requires being able to observe trade flows at major shipping ports. Although such tax allocations may encourage smuggling, it is a much easier proposition than collecting income taxes. The latter requires major investments in enforcement and compliance structures throughout the economy. ... High-income countries tend to depend more on income taxes, whereas middle- and, in particular low-income countries depend more on trade taxes."

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 FTAs as a means to consolidate democracy. A corollary to this conjecture portends that dictatorships may opt to adopt fewer FTAs, 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).

Dictatorships may also have incentive to strategically restrict their foreign investment. As with trade policy formation (e.g., Zissimos 2017, Gawande and Zissimos 2020), autocrats may also weigh the economic and political interests of the masses and elites. For example, Gao (forthcoming) develops a model of oligopolistic competition linking globalization in form of increasing potential foreign direct investment (FDI) to 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 dictator in the form of FDI restrictions. To the extent that elite cohesion is important for autocratic stability, dictators are inclined to restrict FDI, particularly in politically connected industries.

2.3 Empirical implications

Our conceptual framework suggests that partial economic liberalization may be a viable strategy for nondemocratic regimes to generate rents for politically connected (relevant) elites. In doing so, these governments are in stronger position to limit elite defection and stay in power. Applied to Muslim societies, this generates several empirical implications. First, liberalization is likely to be partial in Muslim countries, which can be characterized as being slower and potentially divergent relative to non-Muslim countries. Second, the presence of partial liberalization may stem from several channels: (a) the prevalence of (pre-existing) rentier structures in Muslim societies; (b) the adoption of fewer and shallower trade agreements; and (c) the differential (greater) protection of politically connected firms (cronies).

3 Empirical strategy

Attempts to empirically evaluate the causal relationship between international economic policy and domestic politics and how it might differ across Muslim and non-Muslim countries

is challenging, particularly from omitted variables and/or reverse causality.[16] To address these concerns, we employ a difference-in-differences (DD) research design that leverages the timing of the World Trade Organization's (WTO) creation in 1995 as an exogenous and common shock to trade liberalization and economic liberalization more broadly (we elaborate below). We then study whether patterns of economic globalization differed substantively across Muslim and non-Muslim countries after the WTO's establishment.

3.1 An exogenous shock to globalization: The WTO's creation

A crucial component of our empirical strategy is the exogeneity of the WTO's creation to political and economic conditions in Muslim countries. 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 tackled many issues, including those related to agricultural subsidies, investment protections, phasing out of various export quotas (e.g., in textiles), and concerns with state sovereignty (initially, a concern of the United States).[17] Importantly, the motives and decisions underlying the WTO's creation was largely orthogonal to economic and political developments in Muslim countries. Second, after the WTO's creation, the Muslim and non-Muslim countries (in our sample of non-oil producing developing countries) have not differed in their propensity to join the organization.[18] In the context of our research design, this suggests the WTO may be viewed as a common shock that has not necessarily differentially targeted non-Muslim countries (relative to Muslim countries).

The WTO's creation can also be viewed as a broader movement towards economic liberalization. Like its predecessor, the General Agreement on Trade and Tariffs (GATT), the WTO strives to reduce tariffs among member countries. However, unlike the GATT, the WTO introduced several provisions − most notably, its dispute settlement body (DSB) − that allows member countries to challenge policies in other countries that discriminate in trade (e.g., regulatory barriers, export subsidies, "dumping" of products, etc.).[19] Relatedly, 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,

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

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

[18] We tested this formally by regressing a country's year of accession to the WTO on a Muslim dummy. The dummy was statistically insignificant.

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

Mansfield and Pevehouse (2013, Figure 1) show the number of PTAs worldwide grown and at a faster rate after the WTO's creation. 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.

3.2 Specification

To examine why Muslim societies may be prone to crony globalization (as suggested by Figure 1), we follow an estimation strategy that is similar to the difference-in-differences (DD) approach. We compare differences in globalization in the post-WTO period relative to the pre-WTO period between Muslim and non-Muslim countries. Our baseline specification is:

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

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 "shock" dummy that take a value equal to 1 from 1995 onwards. 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 rates, oil prices). 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) and the timing of the WTO's creation. 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).

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 and fixed geographic and historical characteristics.

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 year fixed effects and a 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. Moreover, if Muslim countries engaged in partial liberalization after the WTO shock, then we would expect the coefficients to become more negative as we move further into the post-shock period.

3.3 Data

Sample. Our research design exploits panel data to compare the level of globalization across Muslim and non-Muslim non-oil producing countries before and after the WTO's creation in 1995 (our shock variable). Based on existing studies, we categorize a country as being Muslim if at least 75 percent of its population identifies with the Islamic faith (e.g., Ahmed 2012, Campante and Yanagizawa-Drott 2015).[20] Notably, we exclude oil producing Muslim countries (e.g., Saudi Arabia, Kuwait, etc.) from our analysis. We do so because these countries tend to suffer from the well-known resource curse and exhibit pervasive cronyism (Arezki and Bruckner 2011), independent of concerns with protecting connected elites in

[20] Our results remain robust if we use different percentage cutoffs.

tradeable sectors. Thus, by restricting our analysis to non-oil producing countries our estimated effects are unlikely to be biased in our favor. Moreover, since our treatment group of non-oil producing Muslim countries are all developing countries, we only include non-oil producing non-Muslim countries (our control group) that are developing countries as well.[21] Our resulting sample, therefore, is a panel of 56 non-oil producing developing countries from 1970 through 2015.

De jure globalization. Our conceptualization of partial liberalization emphasizes the variety of protectionist policies governments may pursue (e.g., trade taxes, non-tariff measures, capital account restrictions, regulatory barriers, etc.) in an increasingly globalized world economy. 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.[22] 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 a discussion).

To hone in on the policy dimension, we focus our analysis on *de jure* economic globalization (hereon, *de jure* globalization). Here, 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.[23] 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.[24] Our measure of *de jure* glob-

[21] We also verified that our control group of nonoil 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.

[22] 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."

[23] This distinction has substantive economic implications. Gygli et al (2019, Table 5), for example, show that *de jure* economic globalization is robustly associated with economic growth, while *de facto* economic globalization exhibits a weaker association.

[24] 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

alization 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.[25] 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).

4 Results

4.1 Baseline estimates

Table 1 reports estimates from our baseline specification in equation (1). In column (1) we estimate a parsimonious model that only includes country and year fixed effects. The coefficient on $Muslim_i \times Post_t$ is negative and precisely estimated and suggests 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 successively 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. Prior studies find that longer state histories (associated with an earlier transition to settled agriculture) can affect long-run economic development and political institutions (e.g., Hariri 2015, Borcan et al 2018).[26] Adding this control both increases the coefficient size and statistical significance

line with international agreements implemented."

[25]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 uses 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 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").

[26]The long-run effect of state history may be particularly important for many of the countries in our treatment group. For instance, Hariri (2015) shows the longer state histories of many Muslim states in

of $Muslim_i \times Post_t$ on *de jure* globalization compared to our benchmark estimate in column (1).

Our main DD effect remains robust when accounting for several (potential) confounding factors. In columns (3) and (4) we control for two standard time-varying country characteristics. 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.[27] Accounting for this variable may be considered "dirty" since its likely 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 $Muslimi \times Post_t$, it remains statistically significant and larger in magnitude (coefficient = -7.7) compared to column (1). Finally, in column (5) we control for a confounder specific to Muslim societies: the percentage of a modern country's territory conquered by Arab armies during the expansion of Islam following the death of Prophet Muhammad.[28] Recent work suggests Arab conquest introduced governing and economic institutions that set conquered territories on a long-run trajectory of pernicious political economy and less representative political institutions in the contemporary era (Chaney 2012, Blaydes and Chaney 2016, Ahmed 2021); and this in turn may differentially affect each country's economic policies after the WTO shock.

The estimated coefficients on the interaction of Muslim and the post-WTO shock dummy in columns (1) to (5) are consistently negative and statistically significant. Moreover, accounting for confounders strengthens both the estimated effect's magnitude and statistical precision (significance). The coefficient on $Muslim_i \times Post_t$ is substantively meaningful. For instance, averaging the estimated DD effect across columns 2-5 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 index point difference is equivalent to 19 percent of the average level of *de jure* globalization across our sample and has significant welfare implications. Using estimates from Gygli et al (2019, Table 5), an 8 index point reduction in *de jure* globalization is associated with a 0.49 percent decline in annual economic growth.

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

the Middle East and North Africa (MENA) region are robustly correlated with less democratic political institutions.

[27]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).

[28]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.

	KOF Globalization Index, *de jure*				
	(1)	(2)	(3)	(4)	(5)
Muslim x Post WTO	-5.395**	-8.762***	-8.981***	-7.716***	-7.359**
	(2.394)	(2.349)	(2.369)	(2.344)	(3.046)
Controls:					
Years since Agricultural Transition (x Post)	No	Yes	Yes	Yes	Yes
GDP per capita, natural log	No	No	Yes	Yes	Yes
Total population, natural 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	2,176	2,176	2,176	2,176	2,176
R-squared	0.827	0.837	0.845	0.849	0.849

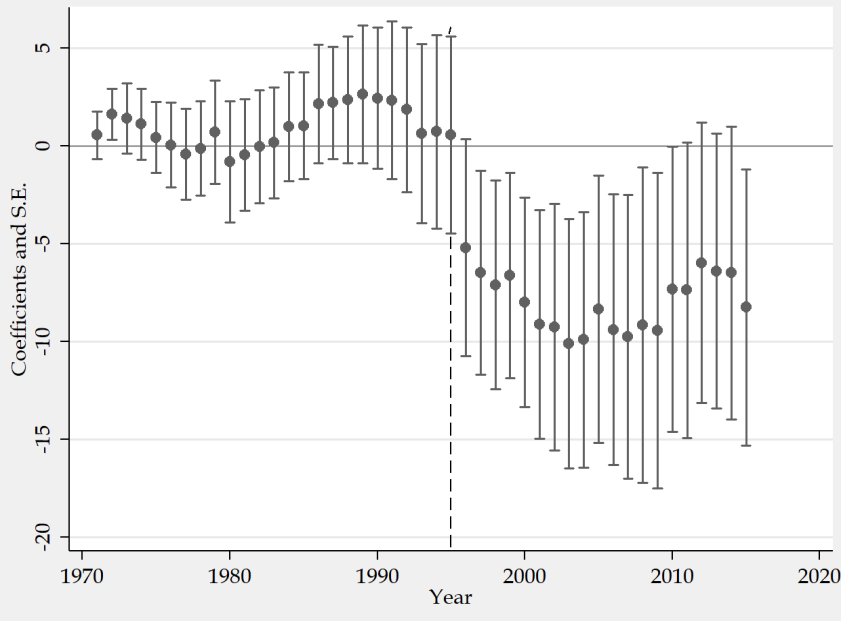
Notes: Estimation via OLS. Robust standard errors, clustered by country reported in parentheses. *, **, *** = significant at 10, 5, and 1 percent respectively. The unit of observation is country-year. Years since Agricultural Transition and Arab Conquest vary across country but not year.

Our main finding on $Muslim_i \times Post_t$ remains robust in specifications that varies the size of the treatment group, for example by increasing and decrease the threshold for qualifying as being Muslim to 60 and 80 percent and dropping individual countries from the treatment group (see Appendix C). The latter addresses concerns that particular outlier countries might unduly drive the main findings. As we discuss shortly, our results are also robust to controlling for a battery of potential confounders associated with being Muslim and/or predispositions towards globalization (e.g., geography).

Flexible specification. To unpack the average effects presented in Table 1, we next provide more fine-grained evidence based on estimating equation (2) that interacts $Muslim_i$ with *each* year fixed effect. Performing this exercise is helpful in capturing how the relationship between a country's Muslim status and *de jure* economic globalization evolves over time and also probes whether the parallel trends assumption is violated. We plot the coefficient estimates and corresponding 95 percent confidence intervals for the interactions in Figure 2. Several important insights emerge from this exercise. As Figure 2 shows, 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. Noticeably,

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 governments in Muslim countries have partially liberalized their policies relative to non-Muslim countries after being exposed to the common globalization shock.

Figure 2: *The difference in de jure economic globalization between Muslim and non-Muslim countries, over time*



Notes: Each point refers to the corresponding year fixed effect (Y_t) interacted with $Muslim_I$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 $Agriculturaltransition_i \times Post_t$, the log of GDP per capita, country and year fixed effects.

Competing explanations. It is plausible that our main results may be driven by unaccounted factors associated with differences between Muslim and non-Muslim countries that may differentially affect *de jure* globalization after the WTO-shock. In Appendix D, we evaluate these explanations by controlling for their interactive effect (with $Post_t$) in our baseline specification given by equation (1). 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) and societal factors (e.g., ethnic fragmentation). Our analysis shows the effect of $Muslim_i \times Post_t$ remains robust in specifications that account for these (potential) competing explanations.

4.2 Parallel trends

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 2 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.

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 B1 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 ($Muslim_i \times Post_t$). 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.

4.3 Selection on unobservables

Despite our attempts to control for many observable factors (e.g., the historical and geographic drivers of globalization, market potential, per capita income, time-invariant charac-

teristics with country fixed effects), the estimates in Table 1 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.[29]

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 $Postt$, 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.

5 Evaluating channels

Guided by our conceptual framework in section 2, we now probe channels to evaluate why Muslim countries have partially liberalized after the WTO's creation. We first examine the importance of political institutions and rents. We then show that Muslim countries have adopted specific policies, particularly trade agreements with less stringent (shallow) commitments and exhibit higher tariff rates and non-tariff barriers that may provide them

[29] 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.

greater scope for protectionism after the WTO shock. Building on these insights, we then provide within-country evidence from Egypt, Morocco, and Tunisia that protected (crony) sectors have benefited from protectionist policies in the wake of each country's adoption of free trade agreements. Our findings suggest that societies where foreign rents are pervasive incentivizes their governments to protect connected elites (cronies) with partial globalization. Many Muslim societies feature these characteristics: reliance on rents and crony capitalism.

5.1 Political institutions and rents

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 maintain elite cohesion. Our discussion was broad, with implications that could apply to Muslim and non-Muslim societies. In this section, we probe the veracity of our framework and its relevance in Muslim non-oil producers.

Table 2: *The mediating role of political institutions and rents*

	KOF Globalization Index, de jure							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Muslim x Post WTO					-7.098***	-6.519***	-7.264***	-8.376***
					(2.325)	(2.284)	(2.097)	(2.395)
Controls: (x Post WTO)								
Foreign Aid (% of GDP)	-0.285**				-0.220*			
	(0.130)				(0.128)			
Democracy measure (CGV)		12.05***				10.15**		
		(4.419)				(4.080)		
Democracy measure (BMR)			8.300*				7.411	
			(4.641)				(4.520)	
Measure of checks and balances				0.0114**				0.0169***
				(0.00487)				(0.00610)
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,176	2,176	2,176	2,176	2,176	2,176	2,176	2,176
R-squared	0.842	0.848	0.844	0.839	0.851	0.856	0.854	0.852

Notes: Robust standard errors, clustered by country in parentheses. *, **, *** = significant at 10, 5, and 1 percent respectively. The control variables are the pre-period (i.e., before 1995) average value interacted with *Post* WTO_t.

We first explore the relationship between rents and *de jure* globalization. Since our sample is comprised of non-oil producing countries, we use a country's dependence on foreign aid (as a share of GDP) to proxy for its reliance on rents.[30] We interpret a greater reliance on foreign aid as an indicator of robust rentier structures; an inference that is applicable in many non-oil producing Muslim societies (Ahmed 2012). Column (1) in Table 2 shows that countries more reliant on foreign aid have experienced smaller gains in *de jure* globalization after the WTO shock compared to less aid dependent countries (after the WTO shock).[31] With respect to political institutions, columns (2) to (4) employ several different measures of democracy to show that countries with a higher quality of democratic institutions experienced larger gains in *de jure* globalization after the WTO's creation.[32]

Together, the results in columns (1) to (4) suggest that countries more reliant on rents and those with less democratic institutions have exhibited smaller improvements in *de jure* globalization after the WTO-shock (compared to before). This is consistent with our conceptual framework emphasizing how autocracy and reliance on rents can be conducive for partial liberalization. However, the greater prevalence of dictatorship and rentier structures in many Muslim countries may affect our inferences. To investigate this, we introduce $Muslim_i \times Post_t$ as an additional control in these specifications. The results in columns (5) to (8) are informative.

First, while foreign aid exerts a strong negative impact on the trajectory of *de jure* globalization after the WTO's creation (column 1), this effect is substantially weakened with the inclusion of $Muslim_i \times Post_t$ (column 5). This suggests $Muslim_i \times Post_t$ is partly capturing the impact of these countries greater reliance on foreign aid. This is consistent with Ahmed's (2012) findings that surges in foreign aid during the 1970s and 1980s generated a rentier political economy in many non-oil producing Muslim countries. As part of this new political equilibrium, governments increasingly distributed rents to buy political stability.[33]

[30] Our measure of aid is the pre-shock country average.

[31] For example, the coefficient estimate implies that countries where foreign aid comprises 10 percent of its national income exhibit a level of *de jure* globalization that is nearly 3 index points lower after the WTO's creation than countries that do not receive any aid.

[32] Our measures of democracy are the pre-treatment period average for each country. In column (2), we use a dichotomous measure of democracy constructed by Chiebub, Ghandi, and Vreeland (2010). This variable is based on four key dimensions: (a) elected chief executive; (b) elected legislature; (c) presence of more than one party in competition for major offices; (d) alternation in power under electoral rules identical to the ones that brought the incumbent to office. In column (3), we use Boix, Miller, and Rosato's (2012) continuous measure of democracy. BMR rely on a variety of sources to measure two central dimensions for democracy: contestation and participation (and involves a minimal suffrage requirement). In column (4), we use a measure of checks and balances from the Database of Political Institutions compiled by the World Bank (available at: https://datacatalog.worldbank.org/dataset/wps2283-database-political-institutions).

[33] Indeed, when the level of foreign aid declined, many of these Muslim recipients experienced political

Thus, in the wake of pressures to liberalize their economies (from the WTO's creation), it seems plausible that governments in Muslim countries might pursue partial and selective international economic policies (e.g., tariff reductions, removal of regulatory barriers and capital controls) to continue supplying rents.

Second, the interpretation of political institutions is more nuanced. Columns (6) to (8) show that democracy does not trump the Muslim effect. While the coefficient on $Muslim_i \times Post_t$ is slightly weakened with the CGV measure of democracy (column 6), it remains robust to the inclusion to all three measures (CGV, BMR, and checks and balances) and those not reported in table (e.g., POLITY, an "aggregate measure" constructed by Acemoglu et al 2019). That said, the measures of democracy generally remain strong predictors of *de jure* globalization but are unable to dislodge the Muslim effect. On balance, the results in columns (6) to (8) do not understate the importance of politics but they also suggest the well-known democratic of Muslim societies does not offer a complete explanation for their globalization deficit. Moreover, the results in columns (1) and (5) suggest that rents may also matter in explaining the globalization deficit. For instance, if Muslim societies are mostly "limited access societies" (North et al 2012), our findings may be capturing the importance of role of rents in sustaining these political orders, whether they are democratic or dictatorial. In short, politics might still be important in (partially) explaining the prevalence of partial liberalization in Muslim societies, but for reasons that are not easily explained away by democracy-autocracy measures.

5.2 Policy choices

Our conceptual framework also identified choices over policies as plausible pathways for governments 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.[34] 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

instability (Ahmed et al 2021).

[34]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.

depth. To measure these aspects of PTA adoption, we draw on information from the DESTA database (Dur et al 2014).

Table 3 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, $Muslimi \times Post_t$ 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 4 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). Across these specifications, the coefficient on Muslim is negative, quite stable, and statistically significant.

Table 3: *Policy decisions and globalization*

	KOF Globalization Index, de jure						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Consistent sample				Full sample	
Muslim x Post WTO	-7.772***	-5.864***	-5.094*	-4.902*	-4.163	-4.803*	-3.824
	(2.442)	(2.128)	(2.649)	(2.684)	(2.631)	(2.661)	(2.576)
Controls: (x Post WTO)							
Overall Trade Restrictiveness, manufacturing		58.70***					
		(13.07)					
Overall Trade Restrictiveness, all sectors			42.07**				
			(16.80)				
Number of deep FTAs, maximum				2.660***		2.401***	
				(0.802)		(0.800)	
Depth of FTAs, average					7.004***		5.676***
					(1.672)		(1.733)
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,089	2,089	2,089	2,089	2,089	2,176	2,176
R-squared	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 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 4: *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	Yes	Yes	Yes	Yes	Yes	Yes
Longitude	Yes	Yes	Yes	Yes	Yes	Yes
Regional fixed effect	No	Yes	Yes	Yes	Yes	Yes
Foreign Aid (% of GDP)	No	No	Yes	Yes	Yes	Yes
Remittances (% of GDP)	No	No	Yes	Yes	Yes	Yes
Log of GDP per capita	No	No	No	Yes	Yes	Yes
Democracy indicator	No	No	No	No	Yes	Yes
Total trade (% of GDP)	No	No	No	No	No	Yes
Countries	56	56	56	56	56	56
R-squared	0.208	0.333	0.396	0.396	0.397	0.407

Notes: Robust standard errors in parentheses. ** = significant at 5 percent. Foreign aid (% GDP), remittances (% GDP), log GDP per capita, democracy indicator (CGV) and total trade (% GDP) are country averages. The dependent variable, "Depth of trade agreements", is drawn from Dur et al (2014) and where a higher value corresponds to more depth (i.e., stricter PTA commitments).

5.3 Within-country evidence

Our analysis in the previous sub-sections suggests the adoption of shallower trade agreements and prevalence of greater trade barriers may (partially) explain why Muslim countries have experienced a smaller increase in *de jure* globalization (relative to non-Muslim countries) after the WTO's creation (compared to before). We draw on these insights to study how trade liberalization (after the adoption of a new PTA) affects cronyism at a more fine-grained within-country level. We compile and map information on trade protectionist measures and political connections across sectors. Discerning the latter can be particularly challenging as political connections are not as readily apparent in countries with less transparent reporting practices and greater informalities in economic transactions.

To address these challenges, we draw on novel data from Egypt and Tunisia that varies at the sector-level and crucially identifies political connections (cronies). Our analysis focuses on studying patterns of protection across crony and non-crony firms/sectors following the adoption of PTAs with the European Union after the WTO's creation. This therefore offers us an opportunity to study patterns of trade protectionist measures following a post-WTO

"PTA shock."

5.3.1 Data

Our main analysis focuses on politically connected actors in Egypt, drawing on data compiled by Eibl and Malik (2016). Construction of this data involved a three-step procedure. First, crony firms are identified from Roll's (2010) list of Egypt's financial and economic core elites and supplemented with addition information guided by the commonly used definition of politically connected firms proposed by Faccio (2006). More specifically, a firm is classified as being politically connected if the owner or top manager is a member of parliament, cabinet official (minister), head of state, or connected with regime insiders through marital ties and business interests. This approach is conservative as it only identifies firms as politically connected if there is a clear and documented link. Second, this information on crony firms is combined with detailed information on the list of products manufactured by these companies. Unfortunately, this information is not compiled by any statistical agency and required gathering data from a number of sources, including company websites, press archives and Orbis. Third, each product was then assigned to its respective sector using the most detailed 4-digit International Standard Industrial Classification (ISIC) developed by UNCTAD. Together, this three step procedure allows us to generate an ordinal variable, *Crony Activity*, which increases by one unit for every additional politically connected actor in a sector.[35] Figure E1 in Appendix E illustrates our data construction.

Our data begins after the WTO's creation, which precludes us from studying how the WTO shock affected patterns of protection (tariff rates) across crony and non-crony firms. Instead, we exploit each country's adoption of its trade agreement with the EU as a plausibly shock to liberalization that was largely orthogonal to its domestic political economy. For example, the impetus for the EU to sign a PTA with Egypt was determined outside of Egypt's domestic political arena and was an outcome of high-level geopolitical concerns that linked trade and security in the post-9/11 period (Adly 2019).[36]

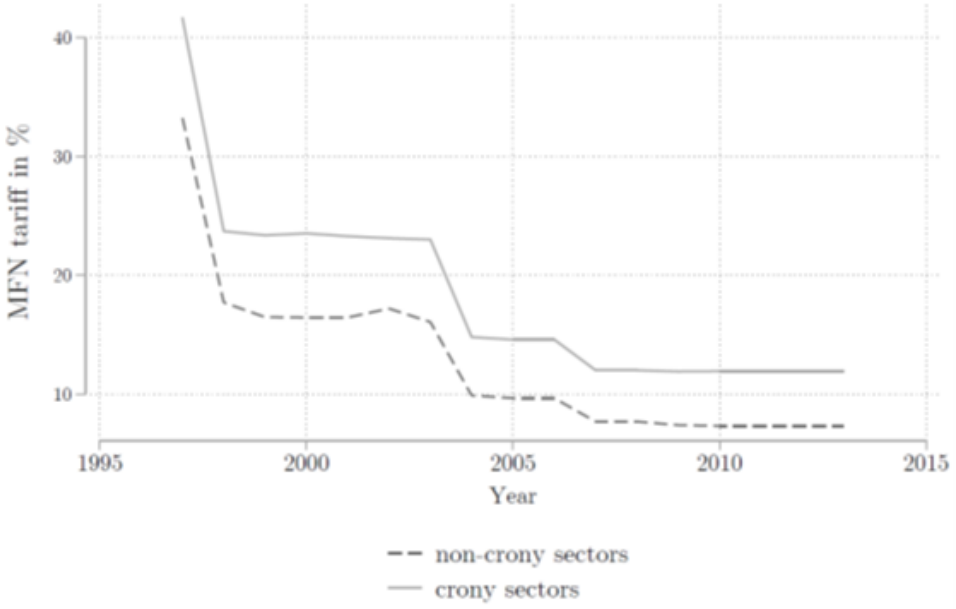
[35] Our data from Tunisia is constructed in similar manner, albeit from different sources.

[36] "Exogenous" reasons also affected the EU's decision to sign FTAs with Morocco and Tunisia. For example, the main impetus for the EU to sign a PTA with Morocco stemmed from geo-political objectives to link security and stability in the Mediterranean with trade cooperation as part of the Barcelona process (Al-Khouri 2008).

5.3.2 Protection in politically connected sectors

We begin our analysis by examining patterns in tariff rates across crony and non-crony sectors in Egypt. In Figure 3, we first plot the average tariff rates across sectors that have at least one crony firm (*crony sector*) and those without any. The figure suggests that crony sectors tend to enjoy higher tariff protection, and notably this favoritism continued after the implementation of the Egypt-EU PTA in 2004. While suggestive, the pattern in Figure 3 could be driven by unobserved heterogeneity and omitted variables. Moreover, the figure does not necessarily reveal any information about the intensive margin: whether sectors with more intensive penetration by cronies exhibit greater tariff protection. To address these concerns, we probe whether sectors with more active cronies predicts higher tariff levels over time while controlling for a number of sectoral characteristics.

Figure 3: *MFN tariff rate in Egypt in crony and non-crony sectors*



Notes: Annual average tariffs in sectors with any crony activity ("crony") and those without any.

To evaluate the effect of political connections on tariffs, we estimate a Prais-Winston specification with an AR1 error structure, estimated using OLS and robust standard errors clustered by sector. As tariff levels are likely to be affected by their level in the previous period, the adjustment of the error structure is important to control for this serial correlation. Given the limited number of time periods in our data, we prefer the Prais-Winsten specification to a lagged dependent variable. Moreover, considering the downward trend of tariffs

in the MENA region during the 2000s (World Bank 2009), we include time and sector fixed effects at the ISIC-2 level. Since our explanatory variable measuring political connections does not vary over time, we refrain from using lower-level fixed effects for different sectors.

Table 5: *Crony activity and trade protection in Egypt*

	MFN tariff rate (%)					
	(1)	(2)	(3)	(4)	(5)	(6)
Crony Activity	2.867***	2.795***	3.092***	3.317***	2.519***	2.094***
	(0.223)	(0.213)	(0.222)	(0.218)	(0.217)	(0.297)
Controls						
Establishments	Yes	Yes	Yes	Yes	Yes	Yes
Employees	Yes	Yes	Yes	Yes	Yes	Yes
Output to GDP	No	Yes	Yes	Yes	Yes	Yes
Value added to GDP	No	Yes	Yes	Yes	Yes	Yes
Output concentration	No	No	Yes	Yes	Yes	Yes
Imports	No	No	No	Yes	Yes	Yes
Fixed effects						
Period	No	No	No	No	Yes	Yes
Sector	No	No	No	No	No	Yes
Observations	22,767	21,912	21,9121	21,355	21,355	21,355

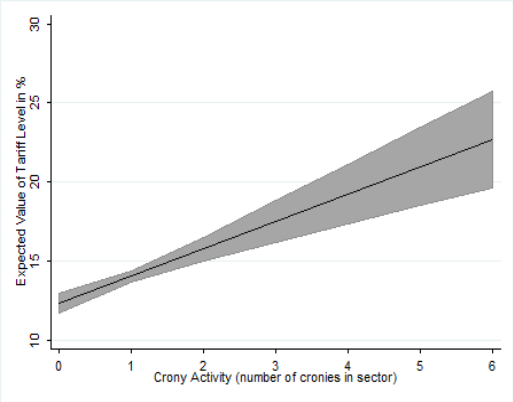
Notes: Estimation via Prais-Winston regressions with AR-1 error. Robust standard errors, clustered by sector in parentheses. *, **, *** = significant at 10, 5, and 1 percent respectively. The dependent variable is annual MFN tariff rates and the main variable of interest is the total number of cronies active in a sector (Crony Activity). Analysis is carried out at the sector-year level with 119 ISIC-4 manufacturing sub-sectors. Estimations are carried out on an unbalanced panel over the period, 2002-2010. The following controls are included: log of the total number of enterprises (Establishments), log of the total number of employees (Employees), the share of output and value-added to GDP, output per enterprise as a ratio of total output (Output concentration), and the log of total imports.

Table 5 shows that on the intensive margin, tariffs in Egypt tend to be significantly higher in sectors with more active cronies. In column (1), we control for two measures of sector size: the number of establishments and employees. The positive and precisely estimated coefficient on crony activity implies that a sector with an additional politically connected firm enjoys an additional 2.9 percentage point of tariff protection. In columns (2) and (3), this effect holds when controlling for several measure of a sector's output, such as its share of output to GDP. It is plausible that failing to account for a sector's competition from abroad may overstate the estimated effect of cronyism. This does not seem to the case, as the coefficient on crony activity increases in magnitude when also accounting for a sector's

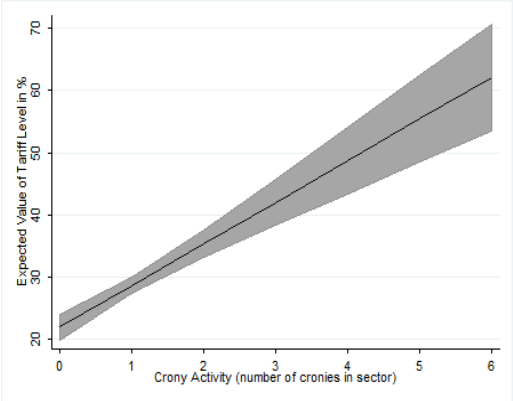
import penetration (column 4). Finally, we account for unobserved heterogeneity that varies over time (column 5) and over time and at the sector level (column 6). While controlling for these fixed effects reduces the estimated effect on crony activity, it nevertheless remains positive and statistically significant.

Figure 4: *Effect of cronyism on tariff protection on the intensive margin*

A: *Egypt*



B: *Tunisia*



In Figure 4a, we graph the expected value of tariffs across sectors with greater crony penetration associated with our most conservative specification (associated with column 6 in Table 5). The figure suggests that sectors with the greatest crony penetration enjoy three times greater tariff protection than a non-crony sector. In a similar vein, Figure 4b provides additional evidence from Tunisia following the implementation of its respective PTA with the EU: sectors with more active cronies tend to exhibit higher tariff levels over time compared to non-crony sectors (while also controlling for sector and time fixed effects). These patterns in Egypt and Tunisia offer two substantive implications. First, liberalization has affected both crony and non-crony firms but has not closed the gap in protection between crony and

non-crony firms. Second, crony firms continue to enjoy preferential protection in the wake of liberalization.

6 Conclusion

Globalization is often viewed as propelling economic and possibly political liberalization. This paper raises some skepticism. We present evidence that many Muslim societies have adopted a more hesitant and partial approach towards economic globalization, plausibly due to their pre-existing rentier political economies and predisposition to cronyism. We argue that 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. We empirically evaluate this argument cross-nationally and with novel sector-level data on cronyism from Egypt and Tunisia.

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 an exogenous global shock to economic liberalization, and investigate whether Muslim countries' (our treatment group) engagement with processes of economic globalization differed substantively after WTO's establishment relative to the non-Muslim cohort (our control group).

Our analysis suggests Muslim countries experienced significantly smaller increases in *de jure* globalization (compared to non-Muslim countries) 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 Muslim countries have partially liberalized, our analysis of channels reveals two plausible reasons. First, the prevalence of rentier political economies may have incentivized governments to view trade and related foreign economic policies as a means to generate rents for important commercial elites. Second, this policy preference was reflected in government decisions to adopt fewer and, notably, shallower preferential trade agreements that provide greater opportunities and scope to pursue protectionist measures (e.g., regulatory barriers, imposition of non-tariff measures, etc.). Moreover, since many Muslim countries exhibit less democratic politics, distributing rents to elites through cronyism likely bolstered the incumbent regime's political durability. Our analysis of politically connected sectors in provides further substantiation: crony sectors continue to enjoy greater

and preferential protection (e.g., higher tariff rates, access to greater non-tariff measures) in the wake of recent trade agreements with the EU. Together our findings strong suggest the globalization deficit in many Muslim societies may have their roots in politics.

Our paper offers at least two substantive implications that may be applicable beyond Muslim societies. 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 and industries tend to be the main beneficiaries of protection, often through a variety of government policies (e.g., tariffs, non-tariff measures, regulatory barriers). The preferential protection that cronies receive in foreign economic policy may be particularly pervasive in countries with less democratic politics.

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Appendix A: Data

Table A1: *Summary statistics*

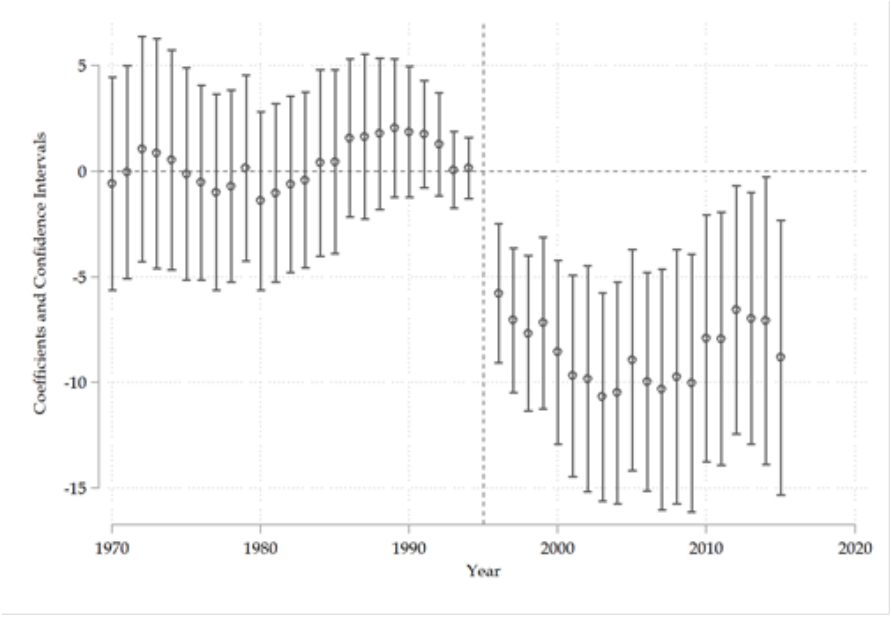
	Non-Muslim					Muslim				
	N	Mean	SD	Min	Max	N	Mean	SD	Min	Max
KOF de jure	1567	46.269	14.215	9.422	85.829	781	36.91	11.648	13.832	67.917
GDP per capita, log	1749	7.395	0.967	4.754	9.596	874	6.911	0.922	5.481	9.35
Population, log	2068	15.72	1.109	13.169	18.431	1120	15.954	1.473	11.334	19.057
Arab Conquest	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 A2: *Sample of non-oil producing developing countries*

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 B: Additional figures

Figure B1: *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 $Muslim_I$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.

Appendix C: Additional results

Table C1: *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-squared	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 C2: *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-squared	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.

Table C3: *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-squared
	(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

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 D: Evaluating competing explanations

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 D1 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 on 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.

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 D2 we control for several measures of intrastate and interstate violence, each interacted with POSTt. These measures include both realized (e.g., incidence) and perceived (e.g., risk) types of political instability. Across all the specifications, our estimated effect of $Muslim_itimesPost_t$ on *de jure* globalization remains negative and statistically significant.

Table D1: *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-squared	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 D2: *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-squared	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.

Appendix E: Political connections and trade data

Figure E1: *Overview - Mapping political connections to sector-level trade data*