Globalization, Institutions and Ethnic Inequality

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Abstract. Recent research has shown that inequality between ethnic groups is strongly driven by politics, where powerful groups and elites channel the state's resources towards their constituencies. Most of the existing literature assumes that these politically-induced inequalities are static and rarely change over time. In this paper, we challenge this claim. We argue that economic globalization and domestic institutions interact in shaping inequality between groups. In weakly institutionalized states, gains from trade primarily accrue to political insiders and their co-ethnics. In contrast, politically excluded groups gain ground where a capable and meritocratic state apparatus governs trade liberalization. Using nighttime luminosity data from 1992 to 2012 and a global sample of ethnic groups, we show that the gap between politically marginalized groups and their included counterparts has narrowed over time as economic globalization progressed at steady pace. Our quantitative analysis and four qualitative case narratives show, however, that increasing trade openness is only associated with economic gains accruing to excluded groups in institutionally strong states, as predicted by our theoretical argument. In contrast, the economic gap between ethnopolitical insiders and outsiders remains constant or even widens in weakly institutionalized countries.

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Introduction

Far from being a merely esoteric topic animating academic exchanges, inequality has become the focal point of intense policy debates in recent years. While most of the controversy has concerned the income and wealth discrepancies among individuals (Piketty 2014) and related questions of redistribution (Scheve and Stasavage 2010), there is a growing realization that inequality between ethnic groups is at least as important. Such between-group or "horizontal" differentials constitute special cases of the more general concept of "categorical" inequalities (Tilly 1999). Recent research shows that ethnic inequality is associated with various deleterious outcomes, such as democratic breakdown, bad governance, deficient public goods provision as well as ethnic civil war (Houle 2015; Baldwin and Huber 2010; Østby 2008; Stewart 2008; Cederman, Weidmann, and Gleditsch 2011).

As the consequences of ethnic inequality begin to become clearer, we still know very little about what drives it in the first place. Some of the existing empirical literature identifies static factors such as geographic endowments or long-lasting historical legacies as important determinants of inter-group disparities (Alesina, Michalopoulos, and Papaioannou 2016; Michalopoulos and Papaioannou 2013). Others, however, argue that inequality between groups is the result of political favoritism along ethnic lines, where powerful groups and elites channel the state's resources towards their constituencies (Franck and Rainer 2012; Hodler and Raschky 2014). Most of this literature assumes that discrepancies between politically included and excluded groups are constant, even calling them an "axiom of politics" (De Luca et al. 2018). Rather than accepting this claim as an assumption, we examine whether and why economic inequality between included and excluded groups changes dynamically over time.

We argue that changing patterns of ethno-economic inequality are the result of

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two interrelated mechanisms. First, increasing integration into the global economy has the potential to produce significant welfare gains in most countries. However, ethnic elites in control of the national government influence how gains from trade due to increasing economic globalization are distributed. These gains could be directed to poor and politically marginalized ethnic regions in an effort to reduce economic disparities between groups, or they could be channeled towards the incumbent ethnopolitical elite, thus reinforcing inequality. Which of these strategies prevails, however, depends on a second mechanism: the strength of domestic institutions (Rodrik 1999). Strong state institutions feature *infrastructural power* to widely distribute gains from trade and a *meritocratic bureaucracy* that prevents elite capture and patronage, making it less likely that powerful groups use the state's resources in favor of their own kin. Taken together, these arguments suggest that the effect of globalization on the gap between included and excluded groups is moderated by domestic institutions: Where institutions are weak and prone to ethnic domination, increasing economic openness does not alter preexisting patterns of ethnic favoritism, thereby depriving politically excluded groups from potential gains from trade. On the contrary, strong state institutions enable politically excluded groups to secure significant gains from economic openness and thus to catch up with their countries' average levels of productivity.

To test these arguments, we examine the interplay between domestic institutions and economic globalization and its relationship to inequality between included and excluded groups over the past 25 years. Using remote-sensed nighttime lights to extract a measure of individual ethnic groups' economic trajectories since 1992, we provide a systematic trend analysis of inequality between included and excluded groups, and how it is affected by economic globalization. This pattern of dynamic change cannot be explained by static geographic and historical factors or ethnic favoritism alone. It is also unlikely that decreases in inequality are similar across world regions and individual countries. Relying on a conservative fixed-effects estimator which helps us to distinguish cross-sectional, structural differences from within-group changes in relative economic performance, we show that increasing integration into the world economy is robustly correlated with ethnic inequality. However, whether openness to globalization decreases economic differentials between ethnic groups varies across institutional settings as predicted by our theory. We find support for both the infrastructural power and the meritocratic bureaucracy mechanisms, although the former is stronger and more robust.

In the following, we proceed by spelling out the theoretical mechanisms that link political exclusion, economic globalization and institutions to inequality along ethnopolitical lines. We describe how we generate group-level time-series data from nightlights, and how we analyze these trends in a regression analysis. Finally, we explore our theoretical mechanism in four short case studies before concluding by discussing potential tensions between state-driven economic integration and political equality among ethnic groups.

Explaining Diverging Trends in Ethnic Inequality

Economic globalization, especially in the form of international trade, ranks among the strongest drivers of distributional outcomes (Hiscox 2001, 1). Rodrik (1998b, 19) estimates that reducing tariffs leads to distributional effects that exceed GDP growth by a factor greater than ten in African developing countries. According to globalization skeptics, global markets expose particularly poor and vulnerable segments of the world population to economic fluctuations as social safety nets and regulatory standards yield to the need of keeping up with international competition (Rudra 2002; Easterly 2007). In contrast, globalization optimists argue that trade liberalization benefits export-oriented firms and their poor workers in labor-abundant developing countries and translates into decreasing individual inequality (see e.g. Harrison, McLaren, and McMillan 2011). Since the vast majority of the global labor force resides in the developing world, global inequality decreases as workers in China, India, and other emerging markets join the global middle class (Milanovic 2013).

Yet, the debate between globalization skeptics and enthusiasts overlooks an important mediating variable: political institutions. According to Rodrik (1998a, 1999), domestic "conflict-management institutions" mediate the redistributive effects of trade openness.[1] We apply Rodrik's general intuition about distributional conflict between "social groups" to multiethnic societies that are vulnerable to the political and economic domination of elites from only one or few ethnic groups (Bates 1974). Variation in institutional strength goes a long way towards explaining the distributional effects of trade openness on inequality between ethnopolitical insiders and outsiders. This view builds on prominent theories of economic growth (North 1990; Acemoglu and Robinson 2012) as well as more specific qualitative studies on how political and institutional forces shape the effects of trade liberalization in developing countries (Boone 1994; Rudra and Jensen 2011).

The distinction between ethnopolitical insiders and outsiders is key in examining globalization effects potentially moderated by state institutions. If institutions matter, distributional outcomes are no longer a mere function of factor endowments, relative prices, and comparative advantage (Rudra and Jensen 2011). Instead, the institutional "rules of the game" determine whether there is broad and equitable access to economic opportunities or whether a narrow political and economic elite monopolizes most gains (North 1990; Acemoglu and Robinson 2012). While economic "inclusiveness" and the threat of elite capture are central pillars in the recent institutionalist literature (Acemoglu and Robinson 2012), few studies identify the political and economic elite groups that are in a position to grab disproportionate shares of the economic pie. Studying societies with politically salient ethnic cleavages and unequal access to central state power provides an opportunity to focus on the type of inequality most relevant for analyzing institutional effects — inequality between elite groups and their politically marginalized counterparts.

To understand how state institutions shape the distributional effects of economic globalization across ethnic groups, we first need to identify the most relevant aspects of institutional strength. The political economy literature highlights a whole bundle of growth-enhancing economic and political institutions ranging from fiscal

[1] Rodrik's work generalizes earlier work on the role of state institutions in the trade-fuelled East Asian growth miracles in the 1980s and 1990s and the disappointing performance of many African and Latin American economies during this period (Wade 1990; Amsden 1992; Evans 1995).

capacity, secure property rights and impartial contract enforcement to civil liberties, equal access to education, and constraints on political elites and rent-seeking coalitions (North 1990; Acemoglu and Robinson 2012). Based on this literature, we highlight two central dimensions of institutional strength that may plausibly affect distributional consequences of trade liberalization in multiethnic societies through their impact on the state's ability and political elites' willingness to broadly distribute economic gains respectively.

The first component, *infrastructural power* refers to the state's ability to project its basic functions across the entirety of its territory and population. Where infrastructural power is low, economic gains will not reach peripheral and marginalized ethnic settlement areas. In such situations, even the most well-intentioned state agents are unable to effectively practice redistribution or invest in large-scale development programs. In highly capable states, however, political elites may be able but unwilling to prevent rampant rent-seeking and favoritism. Therefore, we highlight *meritocratic bureaucracy* as the second dimension of institutional strength that provides state agents with the right set of norms and incentives to promote widely shared development. In what follows, we outline what these two dimensions entail and how they matter for the economic fates of politically included and excluded groups during periods of rising economic openness.

Infrastructural Power

Only where the state and its bureaucratic agents are physically present and able to project unequivocal authority can they engage in its basic functions such as of census taking, tax collection, public goods provision and the enforcement of property rights that allow local populations to gain from trade. According to **Mann1993**, infrastructural power refers to "institutional capacity of a central state [...] to penetrate its territories and logistically implement decisions."

While developed countries tend to be relatively uniformly governed across their territories and populations, in today's developing world, the state often fails to fully extend its reach into the home regions of politically unrepresented ethnic minorities populating what is, at least nominally, state territory (see e.g. Herbst 2000, ch. 5–6; Migdal 1988). This may be due to a lack of resources, logistical challenges, or evasion and backlash by local strongmen and communities in the state's periphery (Migdal 1988; Scott 2009).

The implications for peripheral regions' ability to benefit from international trade are clear. Broad-based provision of public goods, such as education, physical infrastructure, and contract enforcement, enables politically underrepresented parts of the population to benefit from international trade and capital flows. Standard economic models predict that these investments will yield the highest returns in the least developed parts of an economy (e.g., Harrison, McLaren, and McMillan 2011). Because group-based political and economic marginalization tend to overlap, politically marginalized groups will enjoy the greatest advantage from public goods and thus be able to catch up with wealthier groups. Where the state's monopoly of violence is contested, or where it lacks the administrative capacity to provide physical infrastructure, public goods, and economic security, local populations are unlikely to gain, no matter how intensively the economic core engages in international trade.

This problem can be expected to apply in particular to groups that are politically marginalized. The state's executive elite lacks the networks into and information about excluded groups that would be needed to adequately govern and respond to group-specific needs (Roessler 2016). Limited control over, and legitimacy among, excluded parts of the population confronts those in power with what Migdal (1988) has dubbed "the ruler's dilemma." Any attempt to build capacity and develop weakly controlled subsets of a country's territory and society risks propping up alternative power centers with dubious loyalty to the central state. Leaders at the helm of infrastructurally weak states are frequently forced to eschew such investments as they may, ultimately, threaten their political survival (Migdal 1988).

A lack of fiscal capacity at the center further exacerbates the problem, since it reduces state elites' incentives to extend trading opportunities to peripheral, ethnically distinct regions with limited state penetration. Broad-based economic growth is of little use to rulers if they cannot tax it, and infrastructurally weak states find it even harder to extract taxes from politically excluded ethnic groups than from the rest of society (Kasara 2007). Based on this reasoning, we derive a first hypothesis:

Hypothesis 1 *Increasing trade openness reduces the income gap between politically excluded and included groups in states with high levels of infrastructural power.*

Meritocratic Bureaucracy

Drawing on Weber's ideal type of the "rational-legal state," the second dimension of state strength refers to state institutions administered by a rule-bound bureaucracy whose members are recruited and promoted on the basis of meritocratic principles rather than loyalty or personal connections (Weber 1978). This dimension encompasses formal institutional constraints on government leaders and high-ranking bureaucrats by, for example, strong and independent judiciaries (Evans 1995).[2] In addition, it comprises informal norms that foster state agents' performance, professionalism, and impartiality. The institutional characteristics of meritocracy therefore limit leaders' and bureacrats' incentives to extract rents to the detriment of powerless groups (Rauch and Evans 2000; Acemoglu and Robinson 2012).

States with independent and meritocratic bureaucracies are in a good position to check elites' attempts to channel the gains from trade into their own pockets and to distribute club goods that benefit primarily their co-ethnics, instead of investing in public goods and market-supporting policies. Moreover, competitive recruitment into the bureaucracy differs from nepotistic hiring in weakly institutionalized states, because it limits the growth of rent-seeking coalitions, undermines preexisting patron-client relationships, and socializes state officials into a culture of professionalism and efficiency (Evans 1995; Rauch and Evans 2000). As a result, political and bureaucratic elites face incentives to implement far-ranging development programs. Since both the availability of rents and the social acceptability of grabbing

[2] Such elite constraints are not necessarily synonymous with democratic rule, as is highlighted by a large literature on autocratic institutions, see e.g. Magaloni (2008).

them are reduced, economic performance, tax revenues as well as merit-based promotions within the state apparatus become the dominant avenues to further one's wealth and status. In short, meritocratic rules and norms align individual state agents' self-interest with the broader goals of effective governance and broad-based economic development.

In contrast, state administrations without such professionalism enable ethnic clientelism, which in turn accounts for large or even increasing inequalities between the ethnic insiders and outsiders of patronage networks (Van de Walle 2009). The absence of meritocratic rules and norms within the state bureaucracy makes excluded groups vulnerable to exploitation. Unchecked elites can benefit from increasing trade openness by granting import and export licences in return for bribes, by manipulating the price of commodities via the control of marketing boards (Bates 1981), by profiting from taxes on import and export goods, and even by creating trade monopolies that benefit their supporters (e.g., Bienen 1991, 76-7). Indeed, where bureaucratic rules and practices do not effectively prohibit such strategies, elites typically reward co-ethnics with public sector appointments, lucrative development contracts, and the disproportionate allocation of state funds to their home region (Franck and Rainer 2012; Burgess et al. 2015; Hodler and Raschky 2014).

Beyond the direct benefits that accrue to co-ethnic supporters, preferential recruitment into public sector jobs sets in motion a vicious circle that rewards political allegiance rather than individual merit (Migdal 1988). Where economic policies and public investment follow the logic of political survival rather than economic productivity, resource allocation becomes inefficient to the point of decreasing economic output. The diminishing economic pie then reinforces rent-seeking even further. Under such conditions, international trade neither yields widely shared welfare gains nor reduces rent-seeking trough the state apparatus as many proponents of liberalization have hoped (Bienen 1990). Quite the opposite, trade policy tends to create "new rent havens" and "solidify domestic political alliances," as Catherine Boone (1994, 462) concludes from her analysis of liberalization policies in Senegal and Côte d'Ivoire.

Beyond these indirect effects, meritocratic bureaucracies can actively shape the massive changes that follow from trade openness (e.g., Adsera and Boix 2002, 230). If political considerations play a role, it is not to reward co-ethnic loyalists but to address potential inequities associated with economic reform in the spirit of Rodrik's (1999, 98-99) "conflict-management institutions." In this respect too, administrative professionalism serves as a precondition of tax and investment policies to compensate globalization losers. For example, in Malaysia, government intervention as a part of the country's development strategy has decreased ethnic inequality considerably (Kanbur 2000; Langer and Stewart 2012). Similarly, the Vietnamese government runs programs specifically designed to boost development in ethnic minority regions (Kang and Imai 2012). Such economic policy-making does not need to reflect egalitarian principles or accountability towards marginalized groups. Instead, local and central bureaucrats foster their status within the state apparatus, buy acquiescence to unequal political representation, and push through the central state's vision of economic development in marginalized ethnic settlement areas. We summarize our theoretical expectations in a second hypothesis:

Hypothesis 2 *Increasing trade openness reduces the income gap between politically excluded and included groups in states with meritocratic bureaucracies.*

Data and Operationalization

Estimating trends in horizontal inequality represents a formidable measurement challenge. Traditional data sources such as surveys are usually designed to capture trends in economic development at the national level, but not at the level of ethnic groups. In those cases where survey-based group-level estimates are available, they cover only few selected years. Since we require continuous group-level measurements over time to capture changes in the relative economic status of groups, we resort to estimation using spatial data, as existing research has done (Cederman, Weidmann, and Bormann 2015). This procedure relies on two kinds of data: (i) a dataset on ethnic groups and their settlement regions, which is combined with (ii) satellite-based data on nightlight emissions to identify wealthy regions. Using these data, we calculate annual estimates of group wealth, which serve as the main outcome measure in the analysis below. In the following, we explain this procedure in more detail.

Measuring Group-level Development using Spatial Data

Our analysis uses a global sample of politically relevant ethnic groups provided by the 2014 version of the Ethnic Power Relations (EPR) project (Vogt et al. 2015). Ethnic groups are considered politically relevant when group members make political claims on behalf of the group in the national political arena, or when the state discriminates against the group politically, for example by denying voting rights to members of that group. Conversely, social and economic discrimination alone do not warrant inclusion into the sample. For each ethnic group, EPR codes the political power status between 1946 and 2013. Most importantly, it distinguishes "included" from "excluded" groups by assessing meaningful access to positions of executive power in the central government, which can change over time.[3]

To estimate EPR groups' economic trajectories, we combine data on nightlight emissions with information on ethnic settlement regions from the GeoEPR data (Vogt et al. 2015). For each EPR group, GeoEPR provides an approximation of the group's settlement region in an electronic format suitable for processing in a Geographic Information System (GIS). Group regions are given as vector polygons, where each polygon indicates the primary settlement area of that group. These polygons are time-variant, as settlement regions can change due to mass migration, forced resettlement, or modification of country borders.

In a second step, we overlay these ethnic regions with global maps of nightlight emissions data. Light emissions have been shown to proxy economic development well in particular in less-developed countries with unreliable official statistics (Henderson, Storeygard, and Weil 2011), which applies to many countries in our sample.

[3]The EPR dataset does not count as political inclusion cases of "token representation" of group representatives who do not in any meaningful way represent their ethnic groups in the executive.

Equally relevant for us is that nightlight emissions can not only be used at the national level, but also to track *subnational* variation in economic outcomes. Chen and Nordhaus (2011) present a global study that compares nightlight emissions to economic output measured at the level of 1-degree (approx. 100 km by 100 km) grid cells. Weidmann and Schutte (2017) use fine-grained survey data to show that nightlights predict economic conditions at the household-level well. Investigating the source of horizontal inequality, De Luca et al. (2018) rely on changes in total nightlight emissions to demonstrate that a political leader's co-ethnics profit disproportionately from their putative cousin's rule.[4]

The work discussed above demonstrates that remote-sensing data can complement, and even improve on, alternative sources of ethnic inequality measures such as surveys. Therefore, we base our analysis entirely on nightlights and compute annual estimates at the level of ethnic groups. More precisely, our method relies on times-series data of nightlight emissions from the Defense Meteorological Satellite Program's Operational Linescan System (DMSP-OLS), provided by the US National Oceanic and Atmospheric Administration. The data come as annual rasters with a resolution of 30 arc seconds, which corresponds to approximately 1 km. We use the "stable lights" version of the data, which removes non-stable light sources such as forest fires (National Geophysical Data Center 2014). For each raster point, the dataset encodes the level of radiation with a value between 0 and 63. Nightlights imagery is available starting in 1992, which is why we limit our analysis to the years between 1992 and 2012.

Using the GeoEPR settlement regions described above, we compute the sum of the nightlights emitted from each ethnic region.[5] This calculation is performed annually for each group, in order to capture variation in luminosity over time as well as changes in the groups' settlement regions. To disentangle changes in luminosity due

[4] For a similar result that focuses on regions but ignores ethnic identity, refer to Hodler and Raschky (2014).

[5] Where group polygons overlap, we additionally divide the sum of nightlights in this region by the number of relevant groups. In other words, where two groups inhabit the same region, they will each receive half of those regions' nightlight emissions.

to population growth from those due to increased economic activity, we compute per capita estimates of group income. To this end, we estimate local group populations by overlaying ethnic settlement areas with disaggregated population data from the Global Rural-Urban Mapping Project's population density dataset (CIESIN et al., 2011). Unfortunately, these population estimates are only available for 1990, 2000, and 2010, which is why we linearly interpolate missing years.

Using our group-level measure of development, Figure A1 in the Appendix shows the global trend in economic inequality between ethnopolitical insiders and outsiders over time. There is a gradual, but clearly discernible decrease in inequality in our sample of 398 ethnic groups in 120 states between 1992 and 2012. This provides evidence for the main motivation of this paper: Inequality between included and excluded groups is clearly not constant over time, which raises the question of how to explain its dynamic evolution.

Explanatory Variables

We measure globalization using the *trade openness* variable from the World Development Indicators database (WDI; World Bank 2019). Trade openness is calculated as the share of imports and exports of a country's total annual GDP. Our second explanatory variable captures a group's political status through a dummy variable from the EPR dataset indicating if group representatives are *excluded* from the central government in a given year.[6]

Throughout our observation period, economic globalization was on the rise. The average trade-to-GDP ratio in our sample increased by 36.6% from about 0.59 in 1992 to 0.81 in 2012. This trend was driven by relatively parallel growth rates across world regions (see Figure A2 in the Appendix, top panel) suggesting that rising trade openness similarly affected most countries, regardless of institutional or economic structure. Turning to political exclusion, there was a global trend towards more ethnically inclusive government coalitions with especially fast progress in Africa (see

[6]Political status is always measured on January 1st of a given year, which is why the variable is effectively lagged.

Figure A2 in the Appendix, bottom panel). The significant temporal changes in political exclusion raise the question whether political power is a consequence rather than a cause of group-level development. Trends in group-level luminosity may at least partially be explained by the selective inclusion of groups with particularly high potential for economic growth. We address this potential issue of reverse causation in the empirical section below.

We use two proxies to operationalize our theoretical notion of institutional strength. These indicators mirror the dimensions of infrastructural power and meritocratic bureaucracy discussed above. First, we rely on the *state antiquity index* by Borcan, Olsson, and Putterman (2018) to capture the macro-historical origins and long-term persistence of effective state institutions. The basic intuition is that today's states' infrastructural power only gradually changes over time and is, to a large extent, historically inherited. Borcan, Olsson, and Putterman (2018, p. 6) explicitly link state age to similar aspects of institutional capacity as **Mann1993** does in his definition of infrastructural power: "accumulated state history favors capacity building, taxation and the provision of public goods." More specifically, the state antiquity index codes the degree of centralized statehood on the territory of current-day states for the 110 half centuries between 3500 BCE and 1950 CE. Any form of government beyond the tribal level contributes to these statehood scores. The final index is calculated by aggregating all 110 scores and employing a discount rate of 5% per half decade (Borcan, Olsson, and Putterman 2018).[7] We choose this historical proxy over alternative measures such as the tax-to-GDP ratio to avoid endogeneity problems arising from short-term economic changes and their mechanical impact on these measures.

Second, we use the country-year variable "criteria for appointment decisions in the state administration" from the Varieties of Democracy dataset (V-Dem) to capture the degree of bureaucratic meritocracy (Coppedge et al. 2019). This variable is coded by country experts who assess to what extent "hiring, firing, and promotion in the state administration" are based on "skills and merit" rather than "personal and

[7]The index by Borcan, Olsson, and Putterman (2018) builds on and extends an earlier coding by Bockstette, Chanda, and Putterman (2002) who did not yet include episodes of statehood before the begin of the Common Era.

political connections" (Coppedge et al. 2019, p. 176). The V-Dem codebook instructs country experts to assess "the typical *de facto* (rather than *de jure*) situation obtaining in the state administration." As such, the merit-based appointment indicator plausibly entails informal norms and practices that are not reflected in more legalistic measures of judicial independence, executive constraints, or the rule of law.[8]

Although we estimate a conservative set of fixed effects specifications, we cannot exclude the possibility that time-variant factors correlate with changes in countries' trade openness and at the same time affect growth prospects of politically included groups. To account for this possibility, we add regression models that add a number of important control variables such as GDP per capita, natural resource rents per capita, export diversification, the GDP share of agriculture, political institutions, ethnic groups' involvement in armed conflict, and ethnic demography an where appropriate interact these variables with political exclusion and/or trade openness. We detail these variables and explain their relevance in our online appendix. Table A1 in the online appendix presents summary statistics of the main variables used in this study.

Empirical Strategy

Testing our hypotheses requires an analysis of (i) how within-country variation in trade openness affects group-level nightlights, (ii) how this effect differs between politically excluded and included groups, and (iii) how the difference between excluded and included groups varies between countries with different levels of institutional quality. We therefore run triple interaction models with ethnic group and country-year fixed effects to test for the heterogeneous effects stipulated above while also accounting for time-invariant omitted variables at the group level and temporal

[8]We are aware that expert assessments of institutional quality have been criticized as potentially endogenous to recent economic performance (see e.g. Glaeser et al. 2004). In the Online Appendix, we show that our findings are not due to perceived but artificial short-term fluctuations in the V-Dem Merit-Based Bureaucracy measure. Replacing the time-varying version of the variable with its value in 1991 or the mean across the entirety of our observation period does not substantively alter our results.

shocks at the country-level. Our baseline regression specifications take the following general form:

$$
\begin{aligned}
y_{ict} = {} & \alpha\, \mathit{Excluded}_{ict} + \beta_1 \left(\mathit{Openness}_{ct} - \overline{\mathit{Openness}_c}\right) \times \mathit{Excluded}_{ict} \\
& + \lambda \left(\mathit{Openness}_{ct} - \overline{\mathit{Openness}_c}\right) \times \mathit{Excluded}_{ict} \times Z_{ct} + \tau\, \mathit{Excluded}_{ict} \times Z_{ct} \\
& + \beta_2\, \overline{\mathit{Openness}}_c \times \mathit{Excluded}_{ict} + \mu_i + \rho_{ct} + \epsilon_{ict}
\end{aligned}
\tag{1}
$$

The dependent variable y is the logarithm of per capita nightlights in group i's settlement area nested in country c at time t.[9] The parameters μ_i and ρ_{ct} capture group and country-year fixed effects respectively, while ϵ_{ict} is the error term. We use ethnic group fixed effects (μ_i) to avoid bias from unobserved non-time varying factors at the level of individual ethnic groups and the countries they are situated in. Thus, we ensure that the estimated effects of trade openness on group-level luminosity are not mere artifacts of time-invariant omitted variables such as a group's population share, its more or less favorable geographic location, its deep-rooted cultural heritage, or country-specific trajectories of inter-ethnic relations prior to our period of observation. Country-year fixed effects (ρ_{ct}) account for country-specific temporal shocks, for example in economic performance, political regime, and similar time-varying country-level variables that may correlate with both nightlights and our main independent variables. More specifically, country-year fixed effects ensure that all estimates are based on group-level deviations in per capita luminosity from the country-year average and can thus be interpreted as relative wealth or group-based inequality effects.

Our main focus rests on the cross-level interaction between trade openness and political exclusion. In specifying this interaction, we distinguish within-country from between-country variation in the openness variable by splitting it into a demeaned, within-country component ($\mathit{Openness}_{ct} - \overline{\mathit{Openness}}_c$), and its between-country element, the average unit-value across all years ($\overline{\mathit{Openness}}_c$). We then estimate two in-

[9]We log-transform the dependent variable in line with the recommendation by Weidmann and Schutte (2017) and to account for its highly skewed distribution.

teraction terms: $(Openness_{ct} - \overline{Openness_c}) \times Excluded_{ict}$ and $\overline{Openness}_c \times Excluded_{ict}$.[10] The most important coefficient in this setup is β_1. All else equal, a positive β_1 indicates relatively high levels of trade openness *within specific countries* to be associated with disproportionate luminosity gains of excluded groups relative to their included counterparts in a given country year.[11]

As ethnic exclusion may vary over time, the within-between distinction in the trade openness variable is needed to ensure that only within-country variation in trade contributes to variation in this interaction term. Without that distinction, the interaction term would also pick up level differences between countries in cases where an ethnic group's political status changes during our observation period. Analyzing such level differences would entail the risk of picking up unobserved time-invariant characteristics that correlate with a country's baseline propensity to engage in international trade while also affecting group-level development trajectories.[12] Accordingly, the second interaction term $\overline{Openness}_c \times Excluded_{ict}$ is of less interest for our purposes, and merely conditions the effect of within-group variation in political status on a country's average economic openness. A positive coefficient β_2 implies periods of political exclusion to be associated with more nightlights per capita in relatively open countries than in more closed ones.

The decomposition of the trade variable resembles, but is not equivalent to, "within-between" models proposed by Mundlak (1978) and more recently by Bell and Jones (2015).[13] In contrast to these models, we are not substantively interested in examining the effects of within- and between-unit variation across all predictors in our

[10] Due to the use of country-year fixed effects, our models do not include the constitutive terms $(Openness_{ct} - \overline{Openness_c})$ and $\overline{Openness}_c$ that are constant within country-years.

[11] In the online appendix, we present results from less stringent fixed effects models that still allow to compute marginal effects of trade openness for included and excluded groups separately.

[12] In our robustness checks, we estimate models that use time-invariant versions of the political exclusion dummy (by restricting the sample to groups without temporal variation in status and, alternatively, fixing each group's exclusion variable at its initial value in 1991). Doing so makes the within-between distinction obsolete and the models boil down to conventional fixed effects specifications demeaned by ethnic group and country-year.

[13] See also Dieleman and Templin (2014) and Wooldridge (2009), who refer to this approach as "correlated random effects model."

specification. Instead, we employ the within-transformation selectively to enable an interpretation of interaction effects in terms of within-country increases in economic openness. Apart from that, our models are equivalent to conventional linear models with ethnic group and country-year fixed effects.[14]

Based on our theoretical reasoning, we expect the effect of trade openness on excluded groups to vary with institutional strength measured as state antiquity or meritocratic bureaucracy. To test whether this is indeed the case, we further interact the term $(Openness_{ct} - \overline{Openness_c}) \times Excluded_{ict}$ with the respective institutional proxy Z_{ct} and also include the two-way interaction $Excluded_{ict} \times Z_{ct}$.[15] Our hypotheses predict a positive and significant coefficient λ on the triple interaction term. We expect relatively faster growth of excluded groups at high levels of institutional quality as trade openness increases. To explore size and significance of the effect at different levels of institutional strength, we compute and graphically present marginal effects across the observed ranges of both institutional moderators.[16]

In a recent methodological contribution, Hainmueller, Mummolo, and Xu (2019) point out two weaknesses of multiplicative interaction models. First, conventional models assume that the interaction effect is linear and changes at a constant rate along the range of the moderator. This does not necessarily reflect the data-generating process. In our setup, it may well be the case that the effect of trade openness on relative gains of politically excluded groups first rises and then falls with institutional quality. Second, there are often too few observations and little variation in the treatment variable at extreme values of the moderator. Such lack of common support leads to unreliable and highly model-dependent point estimates as well as artificially low measures of uncertainty. Hainmueller, Mummolo, and Xu (2019) propose a simple binning estimator that addresses both issues by estimating the marginal effects of a treatment variable (in our case $(Openness_{ct} - \overline{Openness_c}) \times Excluded_{ict}$) at typi-

[14]In our online appendix, we employ several additional fixed effects and random effects specifications to ensure the robustness of our results to the choice of the statistical model.

[15]The constitutive term Z_{ct} and its two-way interaction with the within-component of trade openness drop out as a result of country-year fixed effects.

[16]Calculated as $\beta_1 + x \times \lambda$, where x refers to a specific value of infrastructural power or merit-based appointments.

cally low, intermediate, and high values of a continuous moderator (state antiquity or merit-based appointments).[17] We implement this method and present the results both graphically and in formal tests of whether there are statistically significant differences between marginal effects at low, intermediate, and high values of our institutional moderators.

Results

In this section, we put our theoretical arguments to a test. As argued above, states with high levels of *infrastructural power* are more capable of widely distributing gains from trade and those with *meritocratic bureaucracy* will be more likely to withstand attempts of identity-based elite capture. In Table 1 we evaluate the effect of changes in trade openness on the relative growth performance of excluded groups along the two proxies for institutional strength: state antiquity (Models 1 and 3) and the merit-based appointments index (Models 2 and 4).

As outlined in Equation 1, we estimate a triple interaction term with one dichotomous (political exclusion) and two continuous variables (trade openness and institutional strength). The institutional proxies in our base specifications moderate the impact of trade openness on inequality between excluded and included groups in the expected direction. Models 1 and 2 in Table 1 return a positive and statistically significant estimate of the triple interaction. Put differently, the marginal effect of trade openness on excluded groups' nightlights emissions is significantly larger in states with high levels of infrastructural power (Model 1) and merit-based appointments (Model 2). Country-year fixed effects ensure that this marginal effect is estimated relative to the yearly average among politically included groups in the same country. Significantly positive marginal effects thus translate into reduced inequality between excluded and included groups wherever included groups are, on average, richer. Whether the marginal effect of increasing trade openness on excluded groups'

[17] We follow Hainmueller, Mummolo, and Xu's 2019 suggestion and use the median values within the first, second, and third terciles of the moderators as evaluation points.

Table 1: Linear Model of Group-Level Night Lights Emissions, 1992-2012.

	(1)	(2)	(3)	(4)
Within-Country Variation				
Openness (Δ) × Excluded	−0.980**	0.080	−0.860*	0.020
	(0.320)	(0.108)	(0.338)	(0.134)
Openness (Δ) × Excluded × State History	2.262***		2.096**	
	(0.591)		(0.649)	
Openness (Δ) × Excluded × Merit Appoint.		0.267*		0.273*
		(0.110)		(0.125)
Between-Country Variation				
Openness (Ø) × Excluded	−0.038	−0.223	0.549+	0.174
	(0.199)	(0.179)	(0.309)	(0.201)
State History × Excluded	−0.322		−0.500*	
	(0.209)		(0.228)	
Merit Appoint. × Excluded		−0.035		−0.043+
		(0.026)		(0.026)
Within-Group Variation				
Excluded	0.113	0.134	−2.608*	−1.603*
	(0.122)	(0.119)	(1.152)	(0.672)
Observations	6,849	6,454	5,769	5,365
Group-FE	Yes	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes
Binning Tests				
$p(B_L = B_M)$	0.122	0.444	0.162	0.206
$p(B_M = B_H)$	0.001**	0.064+	0.000***	0.033*
$p(B_L = B_H)$	0.001**	0.006**	0.004**	0.003**

+p<0.1; *p<0.05; **p<0.01; **p<0.001
Country-clustered standard errors in parentheses.

relative growth performance indeed turns positive and significant at observed values of institutional strength cannot be assessed from coefficient estimates alone.

The top row of Figure 1 thus plots marginal effects (solid line) across the observed percentile range of the two moderators. The marginal effect of within-country changes in trade openness on the relative growth performance of excluded groups is negative at very low levels of state strength, albeit statistically indistinguishable from zero for meritocratic bureaucracy. With increasing values on either institutional indicator however, the picture changes. At the upper end of the spectrum, the estimated effects become positive and significant. Consistent with our theoretical expectations, increasing trade openness is associated with disproportional luminosity gains by excluded groups and decreasing levels of inequality in states with stronger institutions.

These results are robust to using the binning estimator as proposed by Hainmueller, Mummolo, and Xu (2019). The three vertical point-ranges depict the marginal effect of trade openness on relative nightlight gains for excluded groups at the median of each tercile of our institutional proxies. The point estimates of the binning estimators follow the marginal effect of our linear model almost exactly, and thus reduce concerns about non-linear effects. The marginal effects at typically high values of infrastructural power and meritocratic bureaucracy are positive, statistically significant, and statistically different from the respective marginal effects at typically low and intermediate values of institutional strength (see the p-values from two-sided Wald tests in Table 1). The binning estimates come with somewhat wider confidence intervals than the conventional marginal effect estimates. As a result, the negative point estimate at low levels of the state history index narrowly misses significance at the 95% level (top-left panel in Figure 1). As the binning estimates are less efficient than the marginal effect derived from the linear model in cases where linearity and common support hold (Hainmueller, Mummolo, and Xu 2019, 172), we cautiously suggest that increasing trade openness widens the gap between included and excluded groups in states with the lowest infrastructural power (left column Figure 1). However, we cannot reject the null hypothesis of constant inequality in states

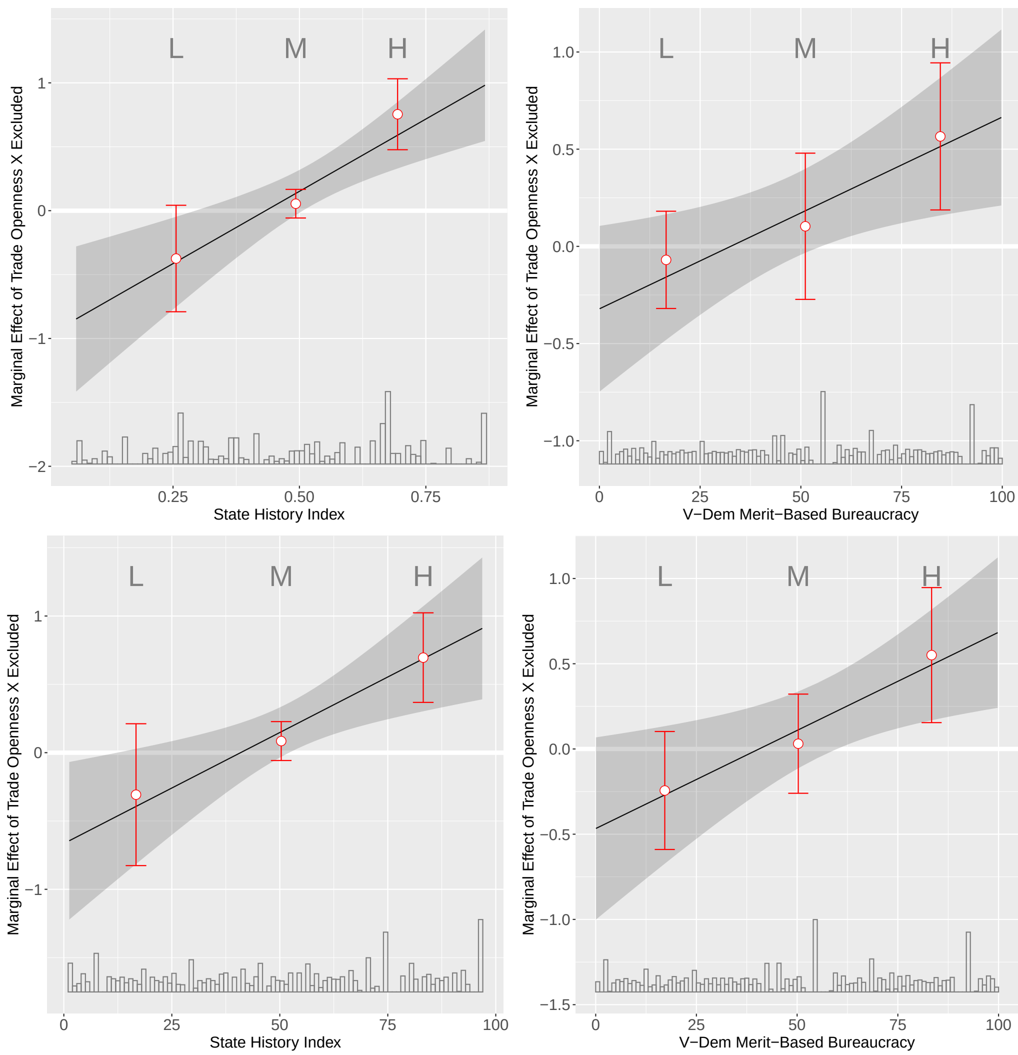


Figure 1: Marginal effects of trade openness on nightlight emissions of excluded groups conditional on state antiquity index (left) and V-Dem Merit-Based Bureaucracy Index (right). Models 1-2 in top row, Models 3-4 with added controls in bottom row. Shaded areas indicate 95% confidence intervals.

where nepotism dictates hiring practices in the bureaucracy (right column Figure 1).

Robustness Checks

A threat to the robustness of our results derives from omitted variables. While our empirical setup is well-suited to minimize bias from unobservables at the group and country-year levels, omitted variables might still affect our results if they co-vary with both within-country changes in trade openness and the average excluded group's economic trajectory. Models 3 and 4 in Table 1 thus include a battery of interactions with control variables. Most interactions with control variables yield statistically insignificant estimates close to zero.[18] Additionally, we probe the sensitivity of our results to unobserved heterogeneity through different fixed effects specifications (Tables A2-A3), by clustering standard errors along country and year (Tables A11-A12), and by estimating mixed effects models with random intercepts by country or group while also including year or country-year fixed effects (Tables A13-A15). Importantly, neither the inclusion of controls nor alternative modeling strategies affect our main results. Coefficient estimates, standard errors, and marginal effects (bottom row in Figure 1) remain very close to our baseline specifications, or become even stronger in the random effects models.

Establishing causality from the type of broad comparative analyses pursued in this paper is difficult. Next to the challenge of omitted variable bias discussed above, we need to consider endogenous ethnic ruling coalitions and other forms of reverse causality. First, political leaders might include those groups into the government that benefit most from trade openness. By selecting economic winners into the ruling coalition, incumbent elites ensure better access to the spoils of increasing economic openness. Kasara (2007), for example, describes this dynamic in Sub-Saharan Africa, where she finds that heads of states tax members of their own ethnic groups more severely than other groups. Such a policy would undermine our account of redistribution in weak states but would not affect our account of strong regimes where

[18] We discuss the rationale for including specific controls in our appendix where Table A5 depicts the full models.

politically excluded groups catch up. Moreover, existing work demonstrates that democratization, and presumably greater ethnic inclusion, preceded the liberalization of trade policies in many developing countries (see Milner and Kubota 2005).

Nevertheless we explore this selection logic. To understand how strategic ethnic coalition formation could undermine our findings, consider a government that invites groups with positive growth in nightlights emissions into the ruling coalition. Wealthier and faster growing groups would now be included, and we would observe a widening of the gap between included and excluded groups completely unrelated to the distributive effects of international trade. A similar dynamic would occur if governments strategically exclude groups with low economic growth. Following Hodler and Raschky (2014), we provide a rough test of this logic in Models 5 and 6 in Table 2 by including dummy variables that indicate if an ethnic group will be upgraded to or downgraded from the central government in the following year. If the selection logic operated, the upgrade dummy should be positive, while the downgrade dummy should take a negative sign.

Importantly, this process needs to be more common in weakly than in strongly institutionalized states to undermine our finding that inequality between included and excluded groups stays constant or widens in weaker states. If the selection effect operated equally in all states, we would overestimate the effect of trade openness on excluded groups' nightlights emissions in states with weaker institutions but underestimate it in stronger ones. The strategic inclusion of economically rising groups in strongly institutionalized should exert a downward bias on our finding that excluded groups catch up with included groups as a result of rising trade openness. To test this effect, we interact the upgrade and downgrade dummies with our institutional proxies.

In all models, the coefficients on the dummy variables as well as the interaction terms remain substantively small and statistically insignificant for downgrades. We find weak evidence that governments of states with greater infrastructural power include groups with higher growth in the preceding year, whereas the opposite dynamic plays out in states at the low end this institutional indicator (Model 5).[19] We find no such effect for the meritocratic bureaucracy proxy (Model 6). If at all, the selection logic operates against our findings. Yet the selection effects are weak, and the results for changes in trade openness in Models 5 and 6 remain practically indistinguishable from our baseline models (see Figure 2, top row).[20] Of course, pre-upgrade and downgrade dummy terms capture effects of observed past performance rather than expectations about future economic growth. However, we doubt that governments are able to accurately predict growth performance of sub-national regions inhabited by ethnically distinct groups.[21]

While these results are encouraging, it is still possible that political elites strategically select their coalition partners, and that this selection is a function of the groups' economic performance and potential to benefit from trade. To rule out that our results are driven by this mechanism, we drop information on the political status of groups entirely and replace it with their initial nightlights emissions in 1992.[22] Rather than estimating the differences between excluded and included groups, we investigate the relative changes in group-level nightlights emissions between initially poorer and wealthier groups in response to changes in trade openness at different levels of institutional strength.

Models 7 and 8 in Table 2 again display positive and statistically significant coefficients on the triple interaction terms.[23] As the bottom row in Figure 2 suggests,

[19]The upgrade dummy alone signifies the effect for which the institutional proxies take a value of zero, or for very weak states. The positive effect of the interaction between the upgrade dummy and the state antiquity suggests that as infrastructural power grows, governments tend to include economically better-performing groups.

[20]We repeat the same strategy with a linear time trend over the three years prior to a group's change in power status (Table A7, Models 1 and 2).

[21]We perform two additional tests to limit potential biases arising from strategic selection of ethnic coalition partners. First, we estimate models that drop all groups that experience a change in their power status from the analysis (Table A7, Models 3 and 4). Second, we assign each ethnic group its initial exclusion value (in 1991) which we keep constant across all observation years (Table A8, Models 1 and 2). Neither of these additional tests alters our conclusions.

[22]We inverted the coding of the initial nightlights variable so that poorer groups have higher values and vice versa. This facilitates comparison of the estimated effects to our original model, where excluded groups take a higher value than included ones.

[23]Note that any interaction between initial nightlights and group/country-constant variables are

Table 2: Robustness Tests of Group-Level Night Lights Emissions, 1992-2013.

	(5)	(6)	(7)	(8)
Within-Country Variation				
Openness (Δ) × Excluded	-0.976^{**}	0.085		
	(0.322)	(0.109)		
Openness (Δ) × Excl. × State History	2.250^{***}			
	(0.596)			
Openness (Δ) × Excl. × Merit Appoint. (Ø)		0.268^{*}		
		(0.110)		
Openness (Δ) × Initial Night Lights × -1			-0.237	0.299^{**}
			(0.276)	(0.104)
Openness (Δ) × Initial NL × -1 × State History			1.462^{**}	
			(0.477)	
Openness (Δ) × Initial NL × -1 × Merit Appoint.				0.209^{**}
				(0.073)
Between-Country Variation				
Openness (Ø) × Excluded	-0.044			
	(0.187)			
State History × Excluded	-0.354			
	(0.220)			
Merit Appoint. × Excluded		-0.025		
		(0.026)		
Within-Group Variation				
Excluded	0.131	0.001		
	(0.113)	(0.046)		
Pre-Upgrade Dummy	-0.281^{+}	-0.079		
	(0.142)	(0.065)		
Pre-Upgrade Dummy × State History	0.643^{+}			
	(0.359)			
Pre-Upgrade Dummy × Merit Appoint.		0.014		
		(0.059)		
Pre-Downgrade Dummy	0.063	-0.009		
	(0.127)	(0.050)		
Pre-Downgrade Dummy × State History	-0.247			
	(0.271)			
Pre-Downgrade Dummy × Merit Appoint.		0.077		
		(0.052)		
Observations	6,832	6,438	6,112	5,719
Country-Year FE	Yes	Yes	Yes	Yes
Ethnic Group FE	Yes	Yes	Yes	Yes
Controls	No	No	No	No
Binning Tests				
$p(B1 = B2)$	0.137	0.356	0.883	0.000^{***}
$p(B2 = B3)$	0.001^{**}	0.079^{+}	0.000^{***}	0.988
$p(B1 = B3)$	0.001^{**}	0.005^{**}	0.003^{**}	0.002^{**}

$^{+}p<0.1$; $^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$
Country-clustered standard errors in parentheses.

poorer groups grow relatively faster as trade openness increases but only in states with intermediate or high levels of meritocratic bureaucracy or a long history of statehood. In states with weak institutions, the relationship between richer and poorer groups does not change as trade openness increases. Although this finding does not directly test our political logic of elite enrichment and ethnic favoritism in weakly institutionalized states, it rules out alternative accounts based on endogenous ethnic coalition building.

Other forms of reverse causality are possible. Rapacious political elites may enrich themselves and their coethnics, and while doing so, weaken or override existing institutions. Although it is plausible that elites craft or destroy institutions at times (e.g., Pepinsky 2014), we disagree with the extreme view that political elites can ignore institutions regardless of their initial strength. In contrast, our argument stipulates that only institutions that are weak to begin with are vulnerable to elite capture. We ground our argument in historical institutionalist work that traces the origin of institutions to elite bargains at critical junctures but identifies institutional constraints on elite action after the critical juncture (e.g., Doner, Ritchie, and Slater 2005). One of our measures of state strength, the state antiquity index, predates current developments and makes short term changes running from ethnic inequality onto bad institutions less plausible. As far as the more contemporary measure of merit-based appointments in the state administration is concerned, our results remain robust to using initial values or the mean across our observation period rather than yearly varying values (Table A8, Models 4 and 5). In addition, both institutional interactions remain significant if included in the same model making it unlikely that they capture the same underlying dimension of institutional strength (Table A8, Model 3).

Another concern of reverse causality arises from political elites who anticipate the distributional effects of trade openness. These elites could fine-tune the degree of openness to benefit themselves and their followers (cf. Adsera and Boix 2002). For example, political leaders representing industrial interests may close off their econ-

accounted for by the group fixed effects as initial nightlights are time-invariant.

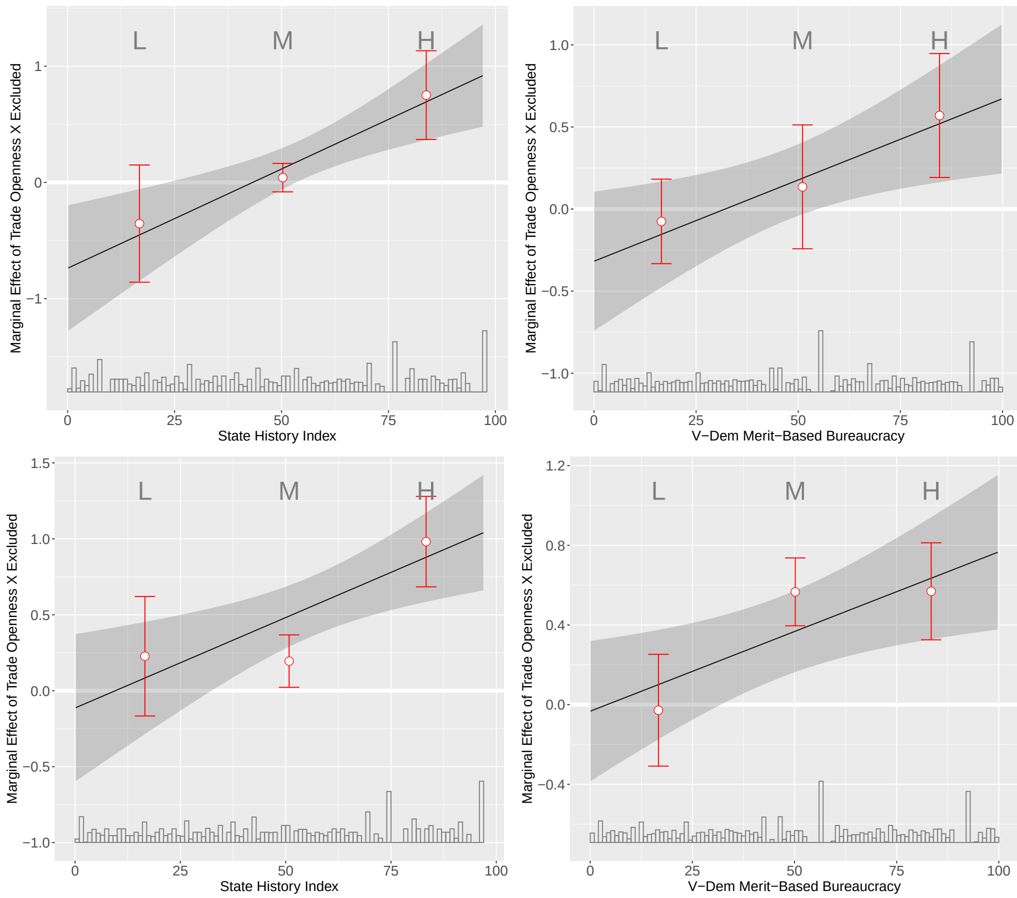


Figure 2: Marginal effects of trade openness on nightlight emissions of excluded groups with dummies for changes in political status (top) and poor groups (bottom). All plots derive from the models in Table 2 and condition on the state antiquity index (left) and V-Dem Merit-Based Bureaucracy Index (right). Shaded areas indicate 95% confidence intervals.

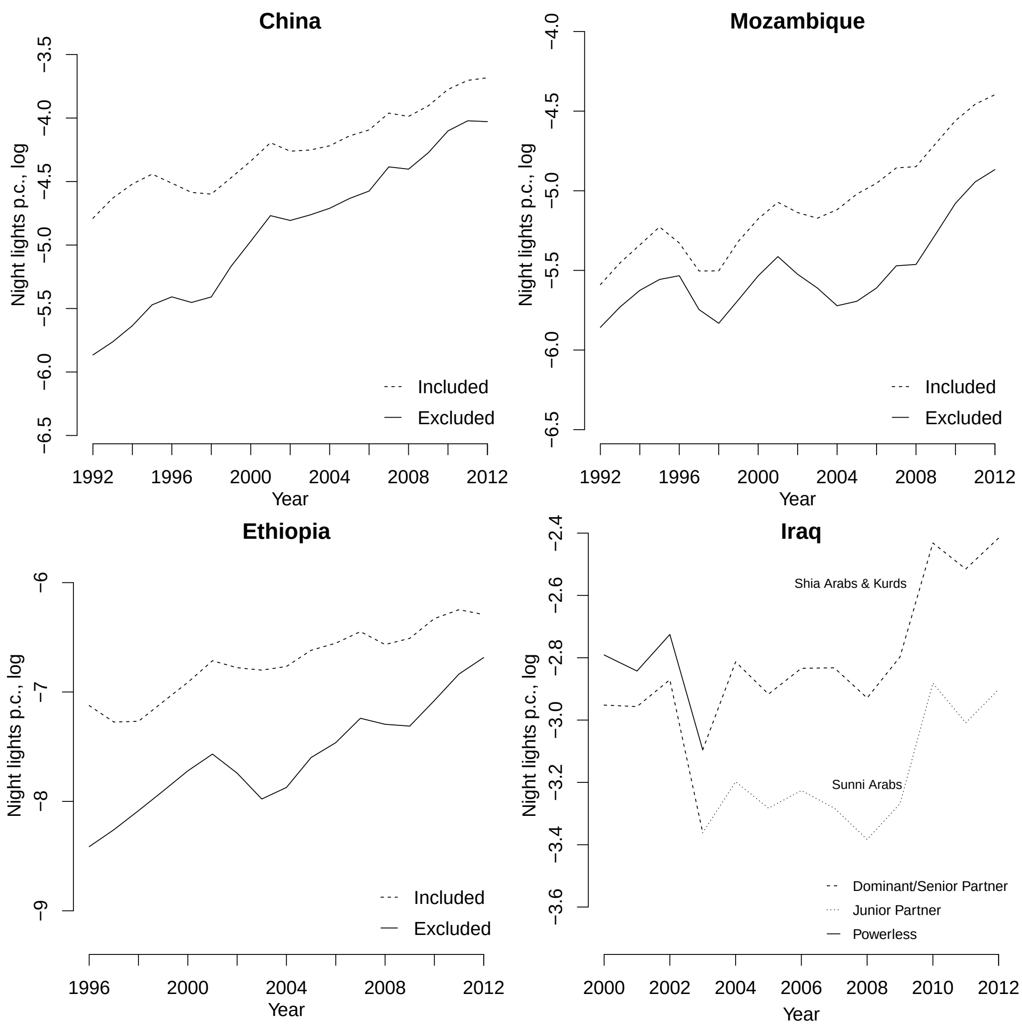
omy to shield their allies from global competition while hurting domestic farmers and their representatives who would benefit from closer integration into the world economy (e.g., Bates 1981). However, this argument does not explain why members of excluded ethnic groups, who have no say over political decisions, would ever benefit from increasing integration into the world economy.

To probe the temporal dynamics in our models, we estimate autoregressive distributed lag (ADL) models which include contemporaneous and lagged indicators of the explanatory and the lagged outcome variable (De Boef and Keele 2008). We find no support for including lagged explanatory variables but evidence for serial correlation. Adding a lagged dependent variable does not change the results of the state antiquity model but lowers our confidence in the meritocratic bureaucracy specification (Table A9, Figure A11). Yet, as our models also include group fixed effects, this introduces bias due to the fact that both the LDV and the error term at time t depend on the error term at $t-1$ (Nickell 1981). Although this bias is likely to be small since our data covers more than 20 years for most groups (Beck and Katz 2011), we cannot exclude the possibility that this bias drives the lower confidence in the meritocratic bureaucracy models. These temporal specifications do allow us to estimate the period over which increasing trade openness affects the gap between excluded and included groups occurs. Just more than half of the effect occurs instantaneously, while most of the remaining part dissipates over four years.

Illustrative Case Examples

To explore the postulated institutional mechanisms in greater detail, we buttress our quantitative findings with narratives tracing ethnic inequality in China, Iraq, Ethiopia and Mozambique. We select these four states because they experienced increased trade openness over the last two decades while diverging in the quality of their institutional endowments and the makeup of ethnic government coalitions. Whereas China and Ethiopia feature stronger state institutions, Iraq and Mozambique represent weakly institutionalized neopatrimonial regimes. Figure 3 showcases trends of ethnic inequality in these four countries. In line with our theoretical argument, the developmental gap between included and excluded groups decreases in Ethiopia and China (top row) but increases in Mozambique and Iraq (bottom). Finally, the four narratives help us validate our measurement of ethnic inequality by comparing night-lights emissions to alternative data sources.

Figure 3: Trends of Ethnic Inequality in Selected Cases, 1992–2012.



We first focus on the strongly institutionalized cases, which deepened their integration into the world economy during the period of interest. According to World

Bank data, China increased its trade-to-GDP ratio by roughly 43 percentage points whereas Ethiopia's more than doubled between 1992 and 2012.[24] At the same time, both countries carried out impressive developmental programs to lift many of their citizens out of poverty, and financed public investments to improve the economic welfare of officially recognized ethnic groups irrespective of their representation in the central government (e.g., Knight 2014; Clapham 2018).

The Chinese government already started to address ethnic inequality in the 1980s by implementing affirmative action policies designed to increase education levels among ethnic minorities (Sautman 1998). Efforts aimed at reducing regional disparities between the prosperous coastal and economically backward central and western areas, home to multiple minority groups, are complemented by the Ethnic Minority Development Fund, that has grown ten-fold between 2001 and 2014 (Fuchang, Chengwei, and Yuan 2016, 10-11). According to Chinese census data these efforts have paid off and minority groups have experienced relatively faster growth in education and urbanization levels than the majority Han Chinese although remaining at lower absolute levels (Hannum and Wang 2012, 158-160). Yet, while many minorities have made economic progress, political inequality has in some cases increased due to the Chinese government's authoritarian methods of development. Internment camps for Uyghurs in Xinjang and large-scale persecution of Tibetans constitute unacceptable human rights abuses that cannot be justified with economic development (Office of the High Commissioner of Human Rights 2018).

We now turn to Ethiopia, which "identifies itself as a developmental state" and "is actively engaged in driving developmental efforts" (Kedir 2014, 11). Mirroring the Chinese developments, poverty fell most in the two Ethiopian regions of Tigray and the State of the Southern Nations, Nationalities and Peoples where it was highest in 1996 according to micro survey data (Hill and Tsehaye 2015, xvi). These reductions in regional inequality match the catch-up of poorer ethnic groups documented by nightlights emissions of regionally concentrated ethnic groups (see Figure 3), and de-

[24]The difference in growth rates derives from China's higher starting point in 1992. Even in 2012, China's overall trade-to-GDP ratio of 49% was still ahead of Ethiopia's level of 39%. Recall though that our primary interest is in changes in trade openness rather than levels.

rive from Ethiopia's increasing integration into global markets, which enabled many small scale farmers to benefit from rising world food prices. At the same time, the Ethiopian government invested in redistribution, the development of education and health services, and infrastructure projects (Clapham 2018, 1155).

The Chinese and Ethiopian states were able to implement these inequality-reducing policies thanks to capable institutions deriving from a long history of statehood and bureaucratic traditions. These descriptions fit the top ranks the two countries take in the state antiquity index, which we use in our empirical analysis above. Despite the political dominance of the Han Chinese in China, competitive recruitment into the bureaucracy, decentralized decision-making, and local elections restrict ethnic favoritism (Fukuyama 2011, 110-39). Thus, China ends up only in the second quartile of the distribution of the meritocratic appointments index, behind the economically more advanced states of Europe, North America, and East Asia. Even though corruption is widespread, Chinese bureaucrats need to fulfill development targets set by the central government, some of which are directly measuring minorities' economic well-being. Local elections further check bureaucrats' attempts to favour co-ethnics.

While Ethiopia has fewer institutional constraints on state agents than China, the country "has made a reputation for itself among donors as a reasonably honest and efficient user of the aid that it receives" (Clapham 2018, 1157). Ethiopia's institutional strength mainly builds on the extensive bureaucracy that can implement government reforms throughout the country's territory. While individual corruption is widespread, the multiethnic recruitment into the state's ruling party, the Ethiopian Peoples Revolutionary Democratic Front (EPRDF), and ethnic federalism guard against the most blatant forms of ethnic favoritism (Verhoeven 2016). Although many groups criticize the central government for favoring the Tigry over other ethnic groups, the recent appointment of an Oromo Prime Minister demonstrates that power sharing does not only exist on paper (Pilling and Barber 2019). Ethiopia's rank in the lower half of the meritocratic appointments index fits with this description, and points towards a stronger role of infrastructural power in this case.

In contrast to the two developmental success stories, ethnic inequality has increased in Mozambique and Iraq. In both countries, government officials far less restrained by state institutions favored their coethnics in distributing public funds. In Mozambique, the former independence movement Frelimo began its rule in 1975 with an ambitious state-driven development program (Hanlon and Mosse 2010, 2). After 15 destructive years of civil war, a peace agreement in 1992 between Frelimo and the opposition movement Renamo attracted foreign aid and investment inflows. Under the liberalization paradigm of the Washington Consensus that saw a rise in the country's trade-to-GDP ratio by a factor of 1.77, Frelimo's leaders dominated government institutions (Nuvunga and Sitoe 2013, 118), while benefiting from privatization reforms (Hanlon and Mosse 2010, 3), and rewarding their Tonga and Makonde coethnics (Orre and Rønning 2017, 21). In contrast, Renamo's Shona supporters feel marginalized and deprived of the promises made in the 1992 peace agreement. Despite anti-corruption efforts by President Guebuza in the 2000s (Hanlon and Mosse 2010, 7-10), country experts agree that embezzlement, ethnic patronage, and corruption are common in Mozambique and facilitated by weak state institutions (e.g. Stasavage 1999; Orre and Rønning 2017, X). Not surprisingly, Mozambique ranks near the bottom in the state antiquity index and in the lower half of the meritocratic appointments index.

Like Mozambique, Iraq typifies a weak state, but its ethnic power relations, trade openness, and ethnic inequality exhibit greater dynamics than observed in the other three cases.[25] In the final years of Saddam Hussein's rule, the politically dominant Sunni Arab regions emitted slightly fewer nightlights per capita than the excluded Shi'a and Kurdish areas. We attribute this reversal of included and excluded groups' economic status before 2003 to the lingering consequences of the first Gulf War and the protection of the Kurdish region by the US-enforced no-flight zone that enabled de facto Kurdish autonomy and cross-border trade with Turkey. The Iraq War in

[25]The more dynamic narrative renders the use of the state antiquity index slightly less useful. Iraq ranks in the upper half of the index, even if its infrastructural power has been destroyed by the 1st and 2nd Gulf Wars, and the Iraq civil war. Note that these deviations work against our hypothesis in the quantitative analysis.

2003 reversed the ethnic power relations when the US military installed a multi-ethnic power-sharing coalition, in which Shi'a and Kurds held the senior government positions.[26] Part of the Sunni's subsequent economic demise can be explained by the destruction wrought by the 2003 invasion and the subsequent civil war that negatively affected oil production and in fact decreased Iraq's trade-to-GDP ratio until 2008.

The first national elections after the end of Saddam Hussein's rule were held in December 2005 and brought Nouri al-Maliki, a Shi'a Arab, to power. While Maliki initially promised to build bridges between the country's three major ethnic groups, he later adopted an explicitly ethnonationalist agenda that prioritized his political allies and Shi'a coethnics while discriminating against the Sunni populations (Lynch 2014, 12). During his first few years in office, Shi'a Arabs and Kurds mostly stagnated economically as measured by nightlight emissions. In the absence of increasing earnings from oil exports, political insiders could not benefit too much, even as Maliki and his allies began to undermine state institutions.

From his first day in office, "Maliki slowly built a shadow state that circumvented both the existing governing elite and democratic oversight of the exercise of power" (Dodge 2013, 245). The "lawlessness that prevailed until 2008" rendered possible "widespread corruption which spread like a virus throughout state institutions" and enabled officials to embezzle "billions of dollars ...from state coffers, owing mostly to gaps in public procurement" (International Crisis Group 2011). As a result Sunni areas also grew slower than Shi'a and Kurdish regions during periods of relative stability and increasing integration into the global economy after 2008.[27] This growth in ethnic inequality stemmed directly from Maliki's overt ethnic nepotism and the widespread embezzlement of state resources enabled by weak institutions. This development fits well with Iraq's decline on the meritocratic appointments index by 15

[26] In Iraq, we break up the category of included groups by considering the distinction between senior and junior power-sharing partners, since the main line of division runs through the governing coalition. Yet, the main logic of a power difference resulting in ethnic inequality remains. Moreover, the case implies that we might underestimate the effect of exclusion on ethnic inequality in weakly institutionalized states where even included junior partners fall further behind.

[27] Between 2009 and 2012, Iraq's trade-to-GDP ratio rose by more than 10 percentage points.

places between 2005 and 2012, even if its starting point in 2005 was already in the lowest quintile. The Kurds used their autonomy and influence in Baghdad to resist some of Maliki's encroachments on their share of oil resources (O'Driscoll 2017, 323-4), but the Sunnis were marginalized and fell further behind. Thus, the lack of strong state institutions enabled elite capture of the gains of trade and ethnic favoritism in both Mozambique and Iraq.

Conclusion

Motivated by the realization that extreme inequality poses an urgent challenge to development policy and the stability of ethnically divided societies, this study demonstrates that inequality between ethnic insiders and outsiders has been slowly decreasing since the end of the Cold War (Figure A1). While inequality levels remain substantial, such a decrease is striking because it contrasts sharply with the increase in levels of individual inequality in developed economies (see Piketty 2014). However, changes in ethnic inequality are themselves unevenly distributed across the globe. As our case descriptions reveal, some cases deviate from the overall trend and have exhibited increases in economic inequality along ethnopolitical lines.

In this study, we argue that these different trajectories derive from variation in two important dimensions of individual states' institutional strength that govern the between-group distribution of gains and losses from rising levels of international trade — infrastructural power and meritocratic bureaucracy. Ethnic power relations assume a central role where state institutions are weak and exploited by ethno-centric elites. Politically marginalized groups fail to catch up or fall even further behind where the state lacks physical presence and clientelist networks absorb most gains from economic openness. In contrast, excluded groups stand a better chance of narrowing the gap to political insiders in more effectively governed states. Our empirical analysis shows that increasing trade openness disproportionately benefits excluded groups in polities with a longer history of centralized statehood, and to a slightly lesser extent, in states with meritocratic hiring and promotion practices in the bureaucracy.

What do these findings imply for the outcomes commonly associated with ethnic inequality? The more ethnopolitical and ethno-economic cleavages reinforce each other, the higher the potential for distributional conflict between groups, which in turn undermines governance, public goods provision and political stability. The combination of ethno-economic inequality and ethnopolitical exclusion has been shown to be particularly conflict-prone (Stewart 2008). This does not bode well for the development prospects of weakly institutionalized countries. In such settings, increasing trade openness is likely to exacerbate divisions between the ethnic insiders and outsiders in political patronage networks. In the African context, these adverse effects may be partially counteracted by the clear trend towards ethnically more inclusive government coalitions (see Figure A2 in the appendix). However, a substantial number of groups remains excluded from political power. Moreover, a mere broadening of the patronage coalition is unlikely to compensate for the lack of long-term development strategies and effective political institutions.

Yet it would be a mistake to embrace the observed inequality reduction in strongly institutionalized states as an unambiguously benign process. Without political representation, the groups that benefit the most in economic terms are rarely able to influence the overall development path. Whether this is a price worth paying remains debatable. In fact, few fast-growing Asian countries with relatively strong state institutions live up to high standards of human rights and democracy (Puddington and Roylance 2016, 14-15). In particular, China's policies towards Muslim Uyghurs and Buddhist Tibetans rank among the most blatant violations of human rights globally. Clearly, the developmental strategies chosen by some strong-state governments are part of a broader, nationalist state-building agenda (Doner, Ritchie, and Slater 2005). In his anthropological study of peoples in the Southeast Asian highlands, Scott (2009) reminds us that ethnic minorities rarely greet such state-building projects with much enthusiasm. In the most extreme cases, developmental schemes in ethnic minority regions may trigger armed conflict between the "sons of the soil" and the state (Weiner 1978).

Thus, strong institutions do not guarantee that economic globalization translates into politically *and* economically inclusive development. Against this backdrop, future research needs to consider specific development programs and their consequences in terms of group-level inequalities, overall prosperity, and political stability. This agenda should include more fine-grained survey and census data to identify the effects of specific economic policy reforms on the income distribution and interethnic relations not only between, but also within, subnational geographic regions. To match the insights gained by students of class conflict and redistribution in developed states (e.g. Scheve and Stasavage 2010), a clearer focus on distributional conflict between politically salient identity groups is needed to reveal what works and what does not in efforts to realize the developmental potential of trade in multi-ethnic societies. For now, this study provides a clearer picture of the global changes in ethnic inequality and how trade openness and domestic institutions may shape this process.

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Online Appendix for "Globalization, Exclusion and Ethnic Inequality"

Table A1: Summary Statistics

Statistic	N	Mean	St. Dev.	Min	Max
Within-Group Variation					
Log(Night Lights p.c.)	6,909	−4.319	1.846	−11.525	0.426
Excluded	6,909	0.407	0.491	0	1
Conflict Incidence	6,909	0.058	0.234	0	1
Pre-Upgrade Dummy	6,892	0.009	0.093	0.000	1.000
Pre-Downgrade Dummy	6,892	0.004	0.067	0.000	1.000
Pre-Upgrade Trend	6,909	0.051	0.343	0	3
Pre-Downgrade Trend	6,909	0.027	0.249	0	3
Within-Country Variation					
Trade Openness (Δ)	6,909	0.003	0.151	−0.745	0.797
Log(GDP p.c.) (Δ)	6,814	−0.053	0.309	−1.196	0.834
Polity IV (Δ)	6,775	0.015	2.610	−12.714	12.115
Agric. Share (Δ)	6,765	0.410	4.736	−19.948	36.534
Resource Rents (Δ)	6,867	0.171	4.293	−23.101	32.309
Export Diversification (Δ)	6,122	−0.155	0.490	−2.480	1.797
Between-Country Variation					
Trade Openness (∅)	6,909	0.647	0.303	0.133	1.731
Log(GDP p.c.) (∅)	6,814	12.215	1.992	7.412	16.399
Polity IV (∅)	6,909	2.201	5.833	−10.000	10.000
Agric. Share (∅)	6,909	16.853	11.205	0.825	54.455
Resource Rents (∅)	6,909	8.639	9.370	0.006	48.152
Export Diversification (∅)	6,146	3.379	1.160	1.268	6.118
State History	6,849	0.477	0.222	0.058	0.867
Merit-Based Appointments	6,454	0.389	1.023	−2.610	2.544
Max Group. Size	6,909	0.591	0.250	0.160	0.981
Executive Constraints	6,559	4.550	2.059	1.000	7.000
Party-Based Autocracy	6,909	0.216	0.411	0	1
Personalist Autocracy	6,909	0.189	0.392	0	1
Military Dictatortship	6,909	0.033	0.179	0	1
Monarchy	6,909	0.016	0.126	0	1

Figure A1: Global Trend in Ethnic Inequality between Included and Excluded Groups

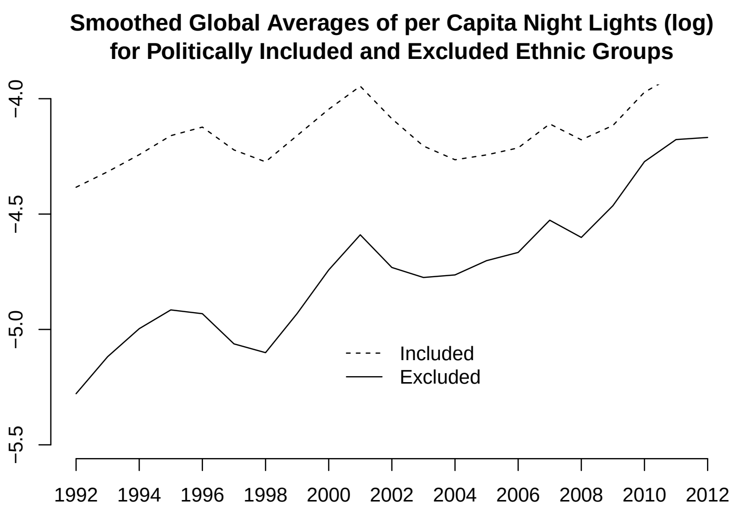

Figure A2: Average Economic Openness and Political Exclusion, 1992–2012.

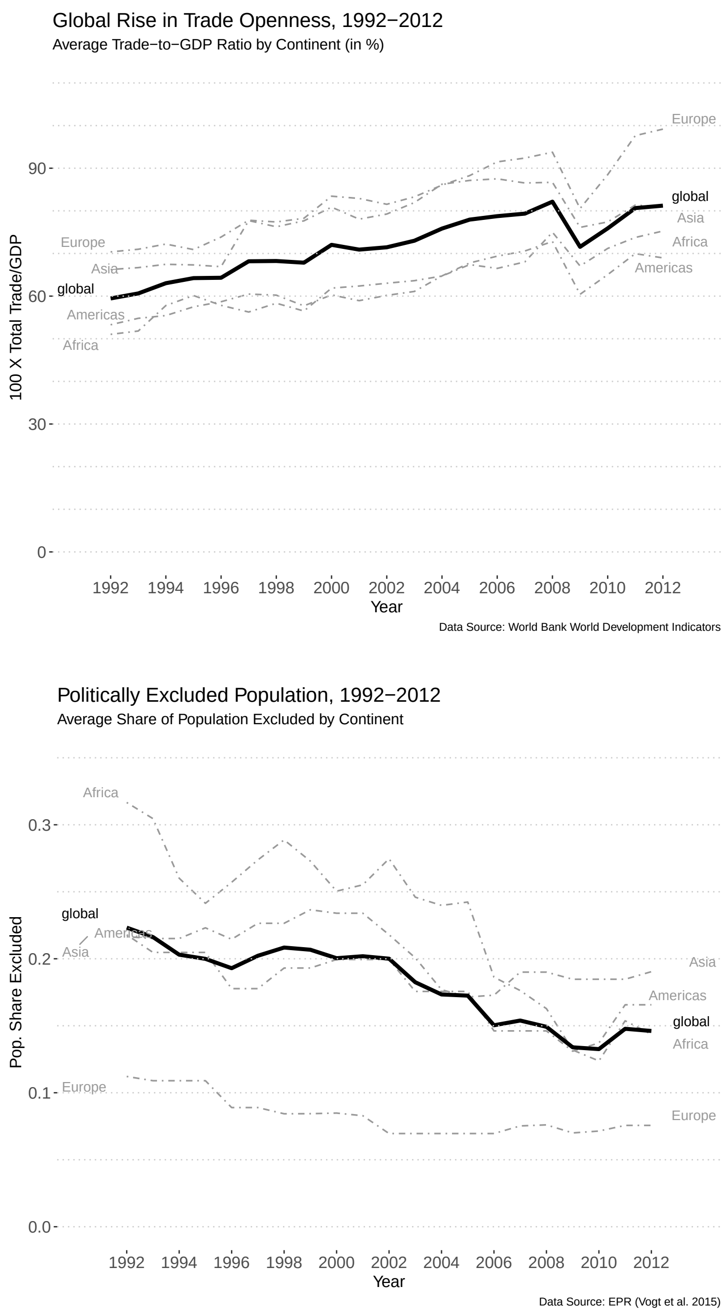

Table A2: Different Fixed Effects: State History

	(1)	(2)	(3)	(4)
Openness (Δ)	0.742	−0.081	0.020	
	(0.774)	(0.256)	(0.229)	
Openness (Δ) × Excluded	−1.126	−1.559***	−1.603***	−0.980*
	(1.330)	(0.448)	(0.304)	(0.396)
Openness (Δ) × Excluded × State History	5.613*	4.420***	4.351***	2.262**
	(2.550)	(0.961)	(0.723)	(0.732)
Openness (Δ) × State History	−1.569	0.103	0.037	
	(1.557)	(0.521)	(0.459)	
Openness (∅)	1.470**			
	(0.469)			
Openness (∅) × Excluded	−0.754	−0.021	−0.236	−0.038
	(0.736)	(0.506)	(0.313)	(0.247)
State History	2.333*			
	(0.999)			
State History × Excluded	−1.676	−0.323	−0.203	−0.322
	(1.303)	(0.431)	(0.277)	(0.259)
Excluded	0.732	−0.118	0.244	0.113
	(1.068)	(0.363)	(0.197)	(0.151)
Country FE	No	Yes	–	–
Group FE	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	–
Country-Year FE	No	No	No	Yes
Controls	No	No	No	No
Observations	6,849	6,849	6,849	6,849

Country-clustered standard errors in parentheses.
Significance codes: $^{+}$p<0.1;, *p<0.05; **p<0.01; ***p<0.001

Different fixed effects specifications: Our theoretical argument predicts that the effect of increasing trade openness on group-level nightlights differs between politically excluded and included groups, and that this difference varies across institutional context. Our baseline models include ethnic group and country-year fixed effects and only identify the difference in marginal effects between excluded and included groups as well as its interaction term with the respective institutional proxy. We believe that this modelling strategy more effectively deals with omitted variables and unobserved heterogeneity than potential alternatives while at the same time focusing attention on those coefficients that are of interest for our theoretical argument.

In order to systematically motivate this approach and show that our results are

Table A3: Different Fixed Effects: Merit-Based Appointments

	(1)	(2)	(3)	(4)
Openness (Δ)	0.110	−0.013	0.009	
	(0.379)	(0.116)	(0.093)	
Openness (Δ) × Excluded	1.471*	0.593*	0.592**	0.080
	(0.630)	(0.252)	(0.191)	(0.108)
Openness (Δ) × Excluded × Merit Appoint.	0.761	0.471*	0.682***	0.267*
	(0.479)	(0.223)	(0.176)	(0.110)
Openness (Δ) × Merit Appoint.	0.001	0.002	−0.113	
	(0.306)	(0.095)	(0.070)	
Openness (∅)	1.812***			
	(0.442)			
Openness (∅) × Excluded	−1.376*	−0.284	−0.556	−0.223
	(0.612)	(0.430)	(0.378)	(0.179)
Merit-Based Appointments	0.722***	0.054	0.016	
	(0.129)	(0.053)	(0.034)	
Merit-Based Appointments × Excluded	−0.417	−0.180	−0.069+	−0.035
	(0.312)	(0.112)	(0.041)	(0.026)
Excluded	0.769	0.0004	0.387	0.134
	(0.515)	(0.282)	(0.241)	(0.119)
Country FE	No	Yes	–	–
Group FE	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	–
Country-Year FE	No	No	No	Yes
Controls	No	No	No	No
Observations	6,454	6,454	6,454	6,454

Country-clustered standard errors in parentheses.
Significance codes: $^{+}$p<0.1;, *p<0.05; **p<0.01; ***p<0.001

robust to alternative modelling strategies, we run additional models with less stringent fixed effects.

- *Year fixed effects only.* These models identify all constitutive terms of the triple interaction and allow to compute marginal effects for both excluded and included ethnic groups. The first column in Tables A2 and A3 shows coefficient estimates, whereas the top row in Figures A3 and A5 plots marginal effects and the difference between excluded and included groups. Unobserved heterogeneity between countries and/or ethnic groups may lead to biased estimates in these models.

- *Country and year fixed effects.* Time-invariant country-level terms such as the state antiquity index and the between-country component of trade openness drop out. It remains possible to compute marginal effects for both excluded and included ethnic groups. The second column in Tables A2 and A3 shows coefficient estimates, whereas the bottom row in Figures A3 and A5 plots marginal effects and the difference between excluded and included groups. Unobserved heterogeneity between ethnic groups within the same country may lead to biased estimates in these models.

- *Group and year fixed effects.* This model estimates the same set of coefficients as the previous model but now also accounts for time-invariant differences between ethnic groups. Again, the effects for both included and excluded effects are estimated. The third column in Tables A2 and A3 shows coefficient estimates, whereas the top row in Figures A4 and A6 plots marginal effects and the difference between excluded and included groups. Time-varying country-level variables that correlate with trade openness and the growth differential between excluded and included groups may bias results.

- *Group and country-year fixed effects (our baseline models).* The additional inclusion of country-year fixed effects nets out all temporal shocks and time-varying variables at the country level. The corresponding terms (e.g. the within component of trade openness and its interaction with the institutional moderator) drop out. The fourth column in Tables A2 and A3 shows coefficient estimates, whereas the bottom row in Figures A4 and A6 plots the difference in marginal effects between excluded and included groups.

- *Group and country-year fixed effects (split samples).* Finally, we simplify our baseline models by splitting our sample at the median of the respective institutional moderator instead of estimating triple interactions. Results in Table A4 show that the interaction between within-country changes in trade openness and political exclusion is negative (but insignificant) at below-median values of institutional quality but gets positive and significant in countries/country-years above the median of state antiquity or merit-based appointments.

Across the board, results from these models lead to similar conclusions as our baseline specifications. Increases in trade openness are associated with relatively faster nightlight growth of politically excluded groups the higher the respective country's institutional quality. If anything, our baseline models lead to more conservative estimates than alternative fixed effects specifications.

Table A4: Split Sample at Median of Institutional Moderators

	State Age		Merit. Bureauc.	
	Low	High	Low	High
Openness (Δ) × Excluded	−0.244	0.417*	−0.147	0.459*
	(0.205)	(0.177)	(0.213)	(0.205)
Openness (∅) × Excluded	−0.158	0.237	−0.072	−0.249
	(0.197)	(0.946)	(0.195)	(0.328)
Excluded	0.133	−0.310	0.024	0.182
	(0.160)	(0.606)	(0.096)	(0.219)
Group-FE	Yes	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes	Yes
Controls	No	No	No	No
Observations	3,500	3,349	3,243	3,211

Country-clustered standard errors in parentheses.
Significance codes: $^{+}p<0.1$;, $^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$

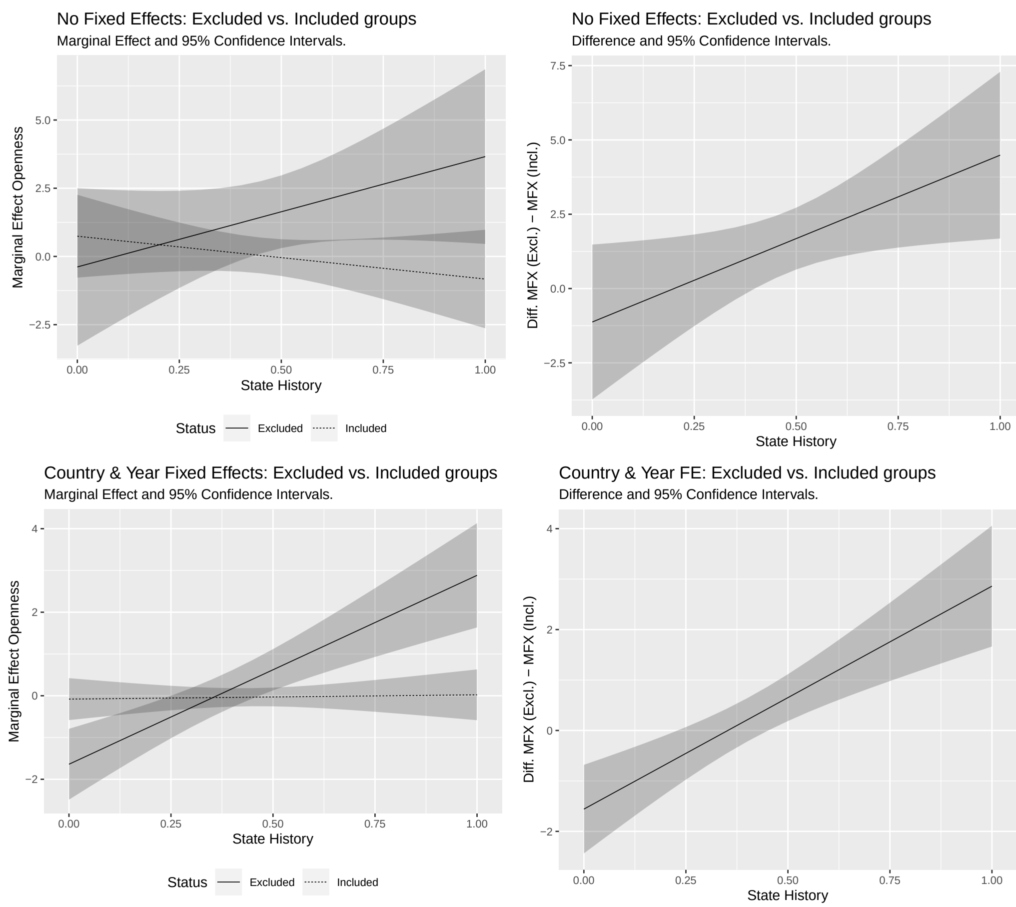

Figure A3: Marginal effects of trade openness on nightlight emissions of excluded and included groups across observed range of state antiquity index (left). Difference in marginal effect between excluded and included groups (right) Based on Table A2. Model 1 (year fixed effects) in top row. Model 2 (country & year fixed effects) in bottom row. Shaded areas indicate 95% confidence intervals.

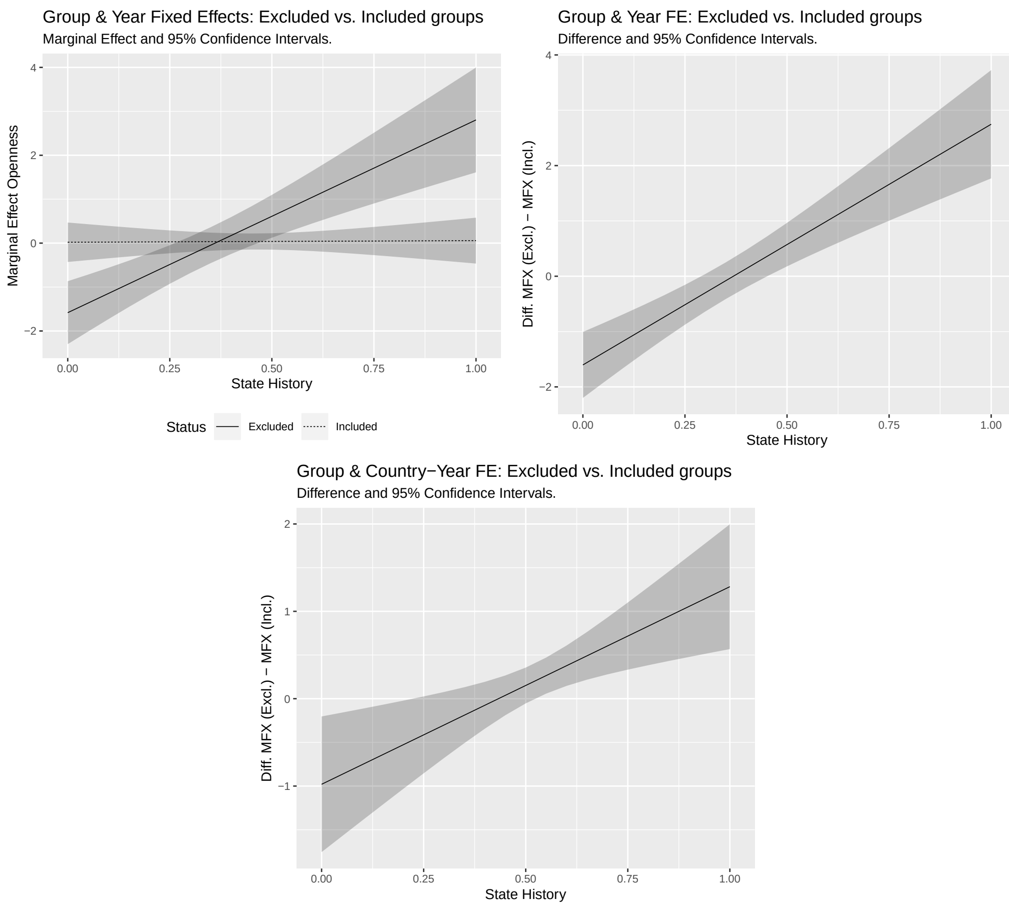

Figure A4: Marginal effects of trade openness on nightlight emissions of excluded and included groups across observed range of state antiquity index (top left). Difference in marginal effect between excluded and included groups (top right and bottom) Based on Table A2. Model 3 (grouo and year fixed effects) in top row. Model 2 (group & country-year fixed effects) in bottom row. Shaded areas indicate 95% confidence intervals.

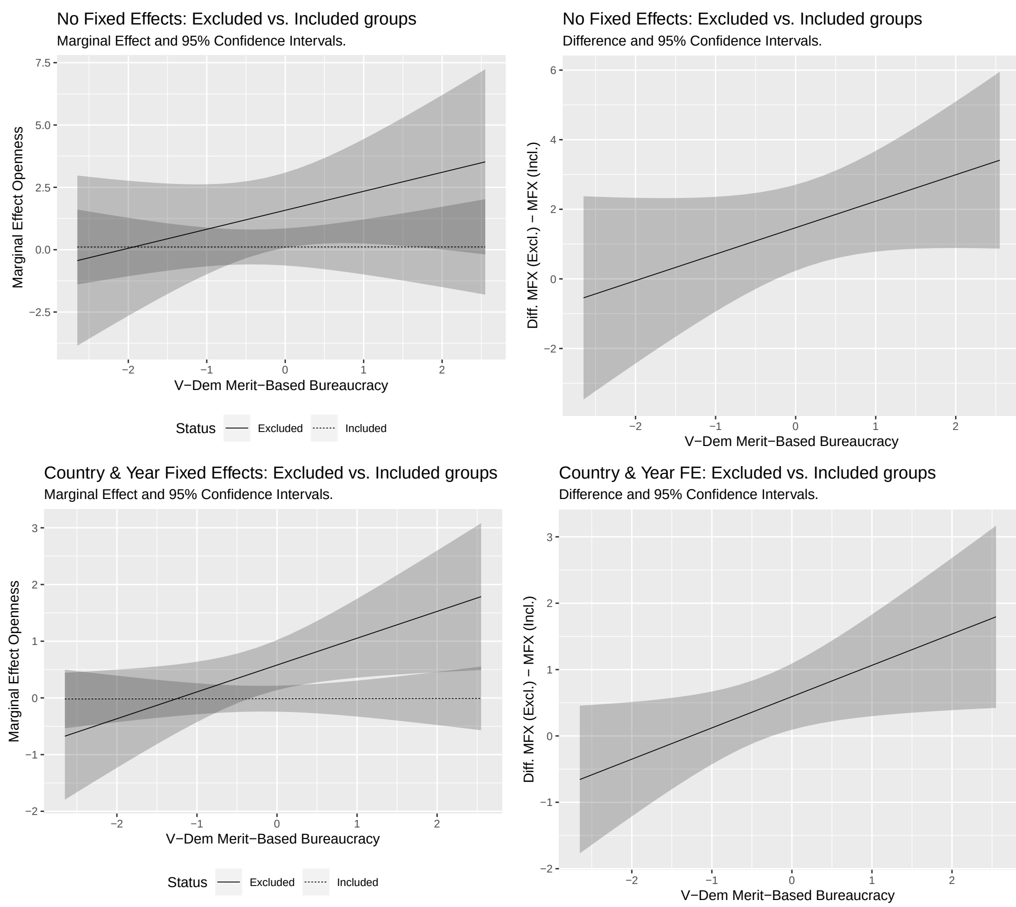

Figure A5: Marginal effects of trade openness on nightlight emissions of excluded and included groups across observed range of V-Dem Merit-Based Bureaucracy index (left). Difference in marginal effect between excluded and included groups (right) Based on Table A3. Model 1 (year fixed effects) in top row. Model 2 (country & year fixed effects) in bottom row. Shaded areas indicate 95% confidence intervals.

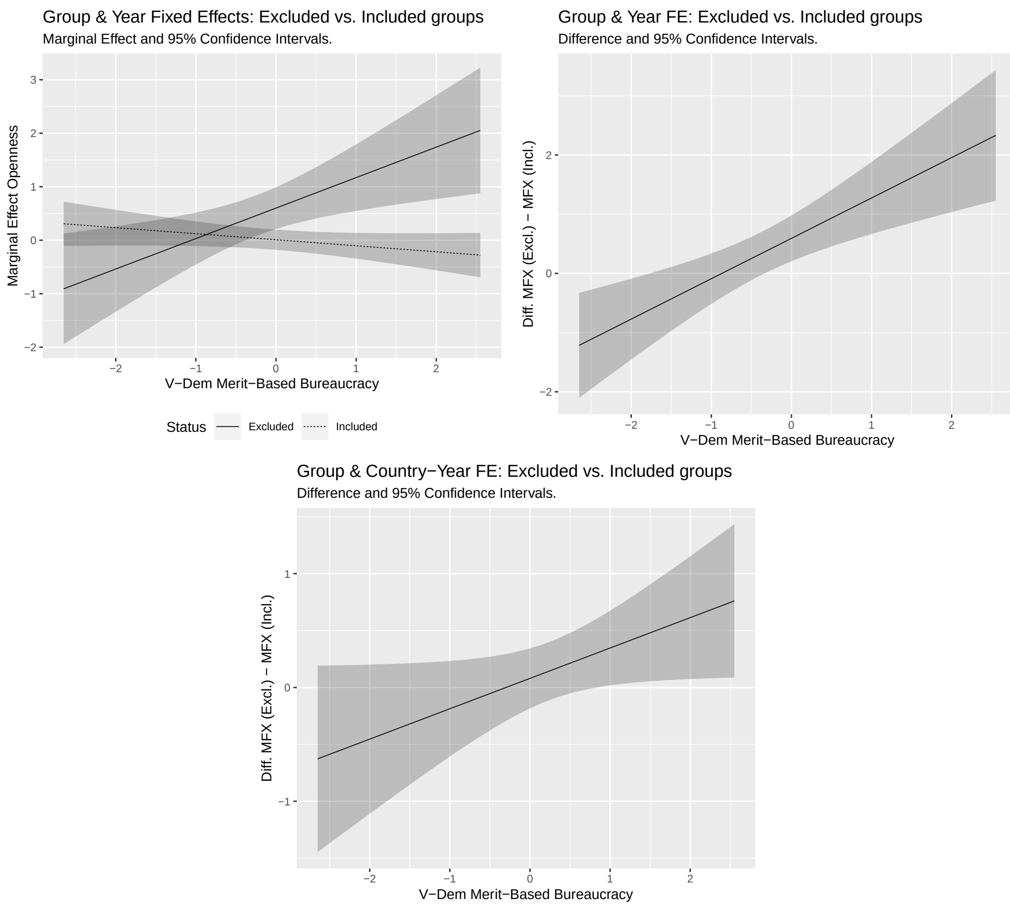

Figure A6: Marginal effects of trade openness on nightlight emissions of excluded and included groups across observed range of V-Dem Merit-Based Bureaucracy index (top left). Difference in marginal effect between excluded and included groups (top right and bottom). Based on Table A3. Model 3 (group and year fixed effects) in top row. Model 2 (group & country-year fixed effects) in bottom row. Shaded areas indicate 95% confidence intervals.

Omitted variable bias: To account for the potential of omitted variable bias, we estimate additional models interacting exclusion not only with trade but also with the within and between-country components of the following control variables:

- *GDP per capita in PPP US$ (log)* (World Bank 2019): Richer countries are more open to trade and achieve more extreme distributions of wealth. Richer countries might achieve equality between different groups because they have the means to redistribute. Yet, a greater level of income also enables greater levels of economic inequality.

- *Natural Resource Rents per capita in PPP US$ (log)* (World Bank 2019): Countries with a higher dependency on natural resources frequently suffer from the resource curse. Lower state capacity, capture of valuable government offices by specific ethnic groups, and a heightened risk of ethnic armed conflict are common consequences with important implications for ethnic inequality.

- *Agricultural Share of GDP* (World Bank 2019): Countries that rely on agricultural production to a large extent greater vulnerability to changes in world market prices and might thus see greater fluctuation in ethnic inequality. Moreover, political elites might strategically include groups from agriculturally productive parts of the country (e.g. Kasara 2007).

- *Polity IV Regime Index* (Marshall, Jaggers, and Gurr 2011): While political scientists and economists broadly agree that regime type affects economic inequality, which way the effect runs is disputed. Political elites in democratic elites tend to face greater constraints in using their power to their own advantage but authoritarian leaders might find it easier to implement welfare transfers (Albertus and Menaldo 2016).

- *Export Diversification* Henn, Papageorgiou, and Spatafora (2013): Countries exporting one or few commodities experience greater vulnerability to changes in world market prices and might thus see greater fluctuation in ethnic inequality in reaction to increases or decreases in international trade. Moreover, political elites will find it easier to control trade on few rather than on many commodities which will exacerbate the impact of weak institutions.

In addition, we control for ongoing armed conflict at the ethnic group level:

- *Ongoing Armed Conflict* (Gleditsch et al. 2002; Themnér and Wallensteen 2014; Wucherpfennig et al. 2012): Ongoing armed conflict at the ethnic group level inhibits and destroys economic activity and trade and could at the same time affect ethnic inequality.

With the exception of the Polity variable, whose estimated coefficient is negative and significant, these controls exhibit the expected sign. Faster growth seems to benefit excluded groups whereas increasing shares of agriculture or other natural resources in national income point in the opposite direction. The conflict dummy is negatively signed yet fails to reach significance. More importantly, however, the inclusion of these variables does not affect our main results (Table A5).

Table A5: Linear Model of Group-Level Night Lights Mechanisms, 1992-2012.

	(1)	(2)	(3)	(4)
Within-Country Variation				
Openness (Δ) × Excluded	−0.980**	0.080	−0.860*	0.020
	(0.320)	(0.108)	(0.338)	(0.134)
Openness (Δ) × Excluded × State History	2.262***		2.096**	
	(0.591)		(0.649)	
Openness (Δ) × Excluded × Merit Appoint.		0.267*		0.273*
		(0.110)		(0.125)
GDP (Δ) × Excluded			0.064	0.061
			(0.064)	(0.075)
Polity IV (Δ) × Excluded			−0.009*	−0.008*
			(0.004)	(0.004)
Agric. Share (Δ) × Excluded			−0.004	−0.003
			(0.004)	(0.004)
Resource Rents (Δ) × Excluded			−0.005	−0.004
			(0.003)	(0.003)
Export Diversification (Δ) × Excluded			0.010	−0.008
			(0.034)	(0.029)
Between-Country Variation				
Openness (∅) × Excluded	−0.038	−0.223	0.549+	0.174
	(0.199)	(0.179)	(0.309)	(0.201)
State History × Excluded	−0.322		−0.500*	
	(0.209)		(0.228)	
Merit Appoint. × Excluded		−0.035		−0.043+
		(0.026)		(0.026)
GDP (∅) × Excluded			0.133*	0.092*
			(0.057)	(0.037)
Polity IV (∅) × Excluded			0.012	0.006
			(0.010)	(0.007)
Agric. Share (∅) × Excluded			0.040	−0.003
			(0.027)	(0.013)
Resource Rents (∅) × Excluded			0.004	−0.0004
			(0.003)	(0.005)
Export Diversification () × Excluded			0.135*	0.086+
			(0.061)	(0.047)
Within-Group Variation				
Excluded	0.113	0.134	−2.608*	−1.603*
	(0.122)	(0.119)	(1.152)	(0.672)
Conflict Incidence			−0.077	0.031
			(0.078)	(0.024)
Observations	6,849	6,454	5,769	5,365
Group-FE	Yes	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes
Binning Tests				
$p(B1 = B2)$	0.122	0.444	0.162	0.206
$p(B2 = B3)$	0.001**	0.064+	0.000***	0.033*
$p(B1 = B3)$	0.001**	0.006**	0.004**	0.003**

+p<0.1; *p<0.05; **p<0.01; **p<0.001
Country-clustered standard errors in parentheses.

Omitted variable bias (cont.): Finally, ethnic demography may be an omitted variable correlating with our proxies of institutional quality and group-level development. In countries with a clear majority group, state and institution-building may be less challenging than in ethnically more fragmented societies. In addition, politically powerful majority groups may be less hesitant to invest in economically backward minority areas. We therefore re-run our models adding an additional triple interaction multiplying trade openness with exclusion and the population share of the country's largest ethnic group. Accounting for ethnic dominance does not substantively alter our conclusions. The coefficients of the additional interaction term point in the expected direction but do not undermine our findings (Table A6).

Table A6: Controlling for Size of Largest Group.

	(1)	(2)
Within-Country Variation		
Openness (Δ) × Excluded	−1.215**	−0.513+
	(0.371)	(0.283)
Openness (Δ) × Excl. × State History	2.057***	
	(0.503)	
Openness (Δ) × Excl. × Merit Appoint.		0.218*
		(0.087)
Openness (Δ) × Excl. × Max. Group Size	0.553	0.981*
	(0.365)	(0.423)
Between-Country Variation		
Openness (∅) × Excluded	−0.016	−0.162
	(0.171)	(0.174)
State History × Excluded	−0.295	
	(0.237)	
Merit Appointments × Excluded		−0.022
		(0.029)
Max. Group Size × Excluded	−0.087	−0.232
	(0.204)	(0.174)
Within-Group Variation		
Excluded	0.130	0.199
	(0.153)	(0.160)
Country-Year FE	Yes	Yes
Ethnic Group FE	Yes	Yes
Observations	6,849	6,454

Country-clustered standard errors in parentheses.
Significance codes: +p<0.1;, *p<0.05; **p<0.01; ***p<0.001

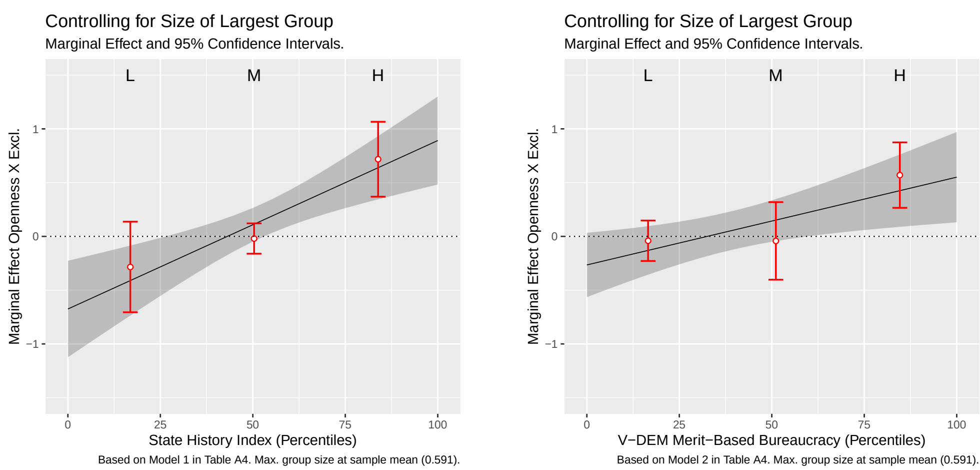

Figure A7: Marginal effects of trade openness on nightlight emissions of excluded groups conditional on state antiquity index (left) and V-Dem Merit-Based Bureaucracy Index (right). Binning estimates (Hainmueller, Mummolo, and Xu 2019) as points on top. Size of largest ethnic group set to sample mean. Based on Table A6. Shaded areas and error bars indicate 95% confidence intervals.

Table A7: Endogeneity of Political Status to Economic Performance?

	(1)	(2)	(3)	(4)
Within-Country Variation				
Openness (Δ) × Excluded	−0.972**	0.082	−0.625**	0.216*
	(0.326)	(0.109)	(0.230)	(0.102)
Openness (Δ) × Excl. × State History	2.234***		1.894***	
	(0.604)		(0.480)	
Openness (Δ) × Excl. × Merit Appoint.		0.265*		0.180^{+}
		(0.111)		(0.091)
Between-Country Variation				
Openness (∅) × Excluded	−0.040	−0.213		
	(0.190)	(0.181)		
State History × Excluded	-0.514^{+}			
	(0.309)			
Merit Appointments × Excluded		−0.028		
		(0.028)		
Within-Group Variation				
Excluded	0.185	0.137		
	(0.117)	(0.134)		
Pre-Upgrade Trend	−0.091	−0.023		
	(0.058)	(0.025)		
Pre-Upgrade Trend × State History	0.236			
	(0.171)			
Pre-Upgrade Trend × Merit Appointments		0.011		
		(0.022)		
Pre-Downgrade Trend	0.033	−0.009		
	(0.079)	(0.031)		
Pre-Downgrade Trend × State History	−0.141			
	(0.162)			
Pre-Downgrade Trend × Merit Appointments		0.038		
		(0.026)		
Country-Year FE	Yes	Yes	Yes	Yes
Ethnic Group FE	Yes	Yes	Yes	Yes
Observations	6,849	6,454	5,715	5,373

Country-clustered standard errors in parentheses.
Significance codes: $^{+}p<0.1$; $^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$

Endogeneity of ethnic groups' power status or institutional quality: The potential endogeneity of ethnic groups' political power status to previous or anticipated economic performance is perhaps the most serious threat to inference in our empirical setup. In addition to controlling for pre-upgrade and pre-downgrade dummies (Table 2 in the main text), we perform additional robustness checks addressing this issue. We first follow Hodler and Raschky (2014) and replace dummy variables with a linear trend over the three years prior to an ethnic group's upgrade to or down-

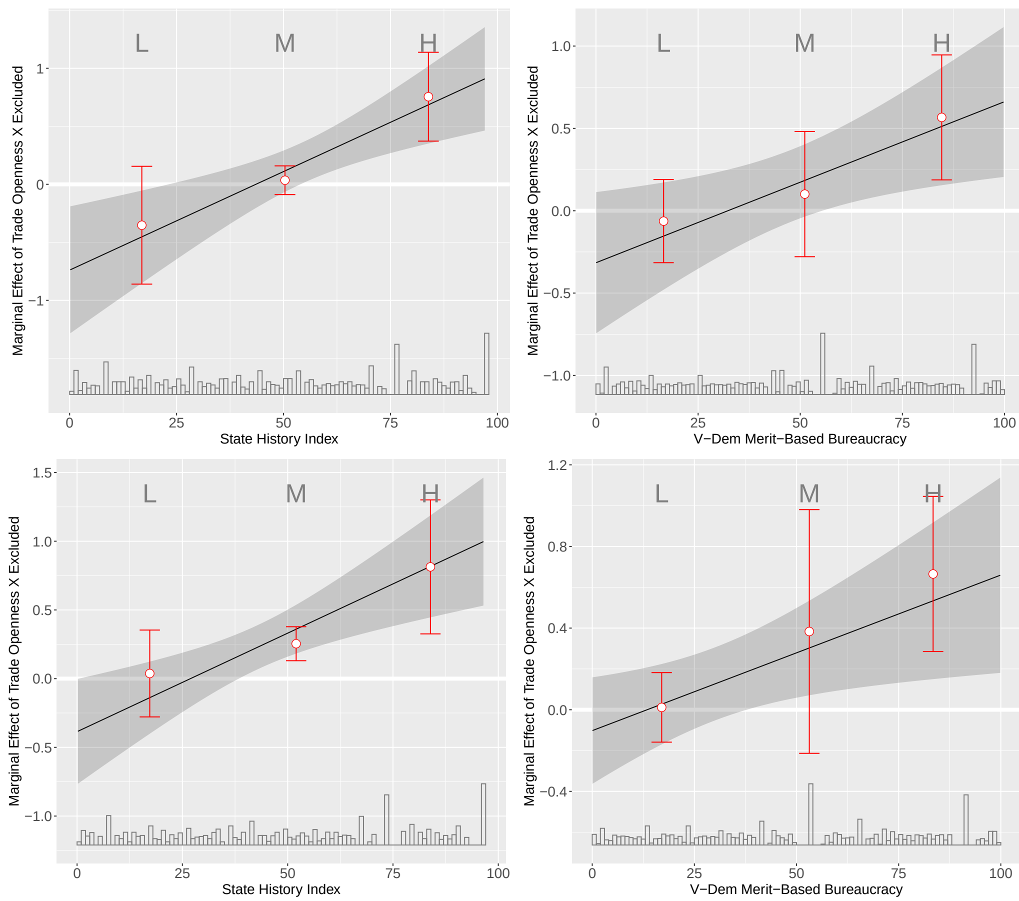

Figure A8: Marginal effects of trade openness on nightlight emissions of excluded groups across percentiles of state antiquity index (left) and V-Dem Merit-Based Bureaucracy Index (right). Binning estimates (Hainmueller, Mummolo, and Xu 2019) as points on top. Based on Table A7. Models 1-2 in top row, models 3-4 with constant power status in bottom row. Shaded areas and error bars indicate 95% confidence intervals.

grade from the ethnic government coalition. If governments strategically include economically rising groups and exclude groups with weaker growth performance, we would expect a positive coefficient on the pre-upgrade trend and a negative one on the pre-downgrade trend. To explain away our findings, the inclusion of groups already on the rise has to be more common in weakly than in strongly institutionalized countries. Therefore, we interact the pre- and post-dummies with our institutional proxies (Table A7, columns 1 and 2). The coefficients on the trend variables and their interaction terms remain substantively small and statistically indistinguishable from zero. The results for our main terms of interest in these specifications remain practically indistinguishable from our baseline models (for marginal effects and binning estimates, see top row of Figure A8).

Second, we run models that restrict the sample to ethnic groups that saw no change in political status between 1992 and 2012 (Models 3 and 4 in Table A7). As the marginal effects and binning plots in the bottom row of Figure A8 suggest, our result holds in this subsample of ethnic groups with more plausibly exogenous political status.

Third, we keep the complete sample but fix each group's political status at its initial value in 1991 (Models 1 and 2 in Table A8). The state age interaction term remains large and significant but the one with merit-based appointments gets smaller and loses statistical significance. The more robust binning estimates suggest, however, that at high values of bureaucratic meritocracy, the marginal effect of trade openness on excluded groups' relative economic fortunes remains positive, significant, and significantly different from the marginal effects at low and intermediate values of the moderator (see Wald tests in Column 2 of Table A8 and top-right panel of Figure A9). Taken together, these results make it highly unlikely that our results are a mere artifact of any endogeneity of political power to previous economic performance.

The expert-coded V-Dem proxy of merit-based appointments in the state bureaucracy may be endogeneous to country experts' perceptions of recent economic performance or ethnic power relations. This may bias our results if annual variation in institutions correlates with temporal changes in trade openness or political exclusion and coders assign worse institutional values to countries where excluded ethnic groups lose ground in economic terms. To rule out this inferential threat, we run models with time-invariant versions of the V-Dem meritocracy variable. Model 4 in Table A8 assigns each country its period mean (1992-2012) whereas Model 5 uses the initial value in 1991. Coefficients and Wald tests in Models 4 and 5 in Table A8 as well as the marginal effects and binning plots in Figure A10 show that our results remain robust to using more plausibly exogenous versions of our second institutional proxy.

Finally, we want to make sure that merit-based appointments are not a predetermined corollary of our historical state capacity measure but have an independent effect in moderating the distribution of gains from trade across ethnic groups. We therefore include both institutional proxies in the same model. The coefficients on the interaction terms become slightly smaller but remain statistically significant (Model 3 in Table A8). The marginal effect of trade openness on excluded groups' relative economic performance increases along the range of both institutional moderators, is positive and significant at high values of both moderators, and remains significantly different from the effect at low values of both moderators (bottom row in Figure A9.)

Table A8: Additional Robustness Checks

	(1)	(2)	(3)	(4)	(5)
Within-Country Variation					
Openness (Δ) × Excluded (91)	−0.730+	0.154			
	(0.375)	(0.110)			
Openness (Δ) × Excluded (91) × State History	1.957**				
	(0.686)				
Openness (Δ) × Excluded (91) × Merit Appoint.		0.129			
		(0.124)			
Openness (Δ) × Excluded			−0.833**	0.120	0.095
			(0.288)	(0.104)	(0.108)
Openness (Δ) × Excluded × State History			1.898***		
			(0.516)		
Openness (Δ) × Excluded × Merit Appoint.			0.179*		
			(0.081)		
Openness (Δ) × Excluded × Merit Appoint. (∅)				0.280*	
				(0.113)	
Openness (Δ) × Excluded × Merit Appoint. (91)					0.281**
					(0.102)
Between-Country Variation					
Openness (∅) × Excluded			−0.228	0.051	−0.392
			(0.171)	(0.227)	(0.243)
Merit Appoint. × Excluded (91)		−0.080**			
		(0.027)			
State History × Excluded			−0.126		
			(0.122)		
Merit Appoint. × Excluded			−0.020		
			(0.024)		
Merit Appoint. (∅) × Excluded				0.062	
				(0.061)	
Merit Appoint. (91) × Excluded					−0.031
					(0.040)
Within-Group Variation					
Excluded			0.189	−0.075	0.220
			(0.128)	(0.176)	(0.153)
Group-FE	Yes	Yes	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No
Observations	6,445	6,068	6,394	6,909	5,887
Binning Tests					
$p(B1 = B2)$	0.431	0.941	0.116(S) 0.220(M)	0.758	0.638
$p(B2 = B3)$	0.009	0.042	0.076(S) 0.290(M)	0.017	0.029
$p(B1 = B3)$	0.012	0.040	0.009(S) 0.019(M)	0.001	0.001

Binning tests in Model 3 for both state age (S) and meritocracy (M) moderator.
Country-clustered standard errors in parentheses.
Significance codes: $^{+}$p<0.1;, *p<0.05; **p<0.01; ***p<0.001

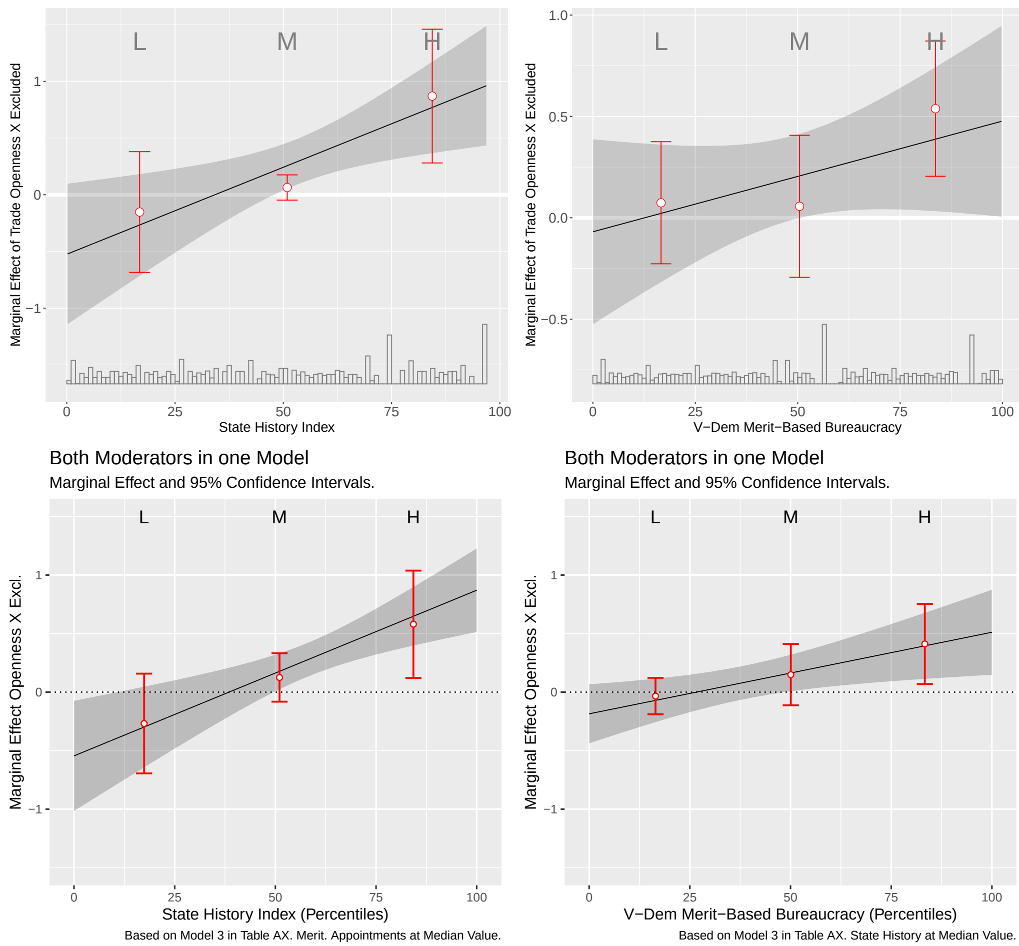

Figure A9: Marginal effects of trade openness on nightlight emissions of excluded groups across percentiles of state antiquity index (left) and V-Dem Merit-Based Bureaucracy Index (right). Binning estimates (Hainmueller, Mummolo, and Xu 2019) as points on top. Based on Table A8. Models 1-2 with initial values of the group-level political exclusion variable in top row; Model 3 including both institutional moderators in bottom row. Shaded areas and error bars indicate 95% confidence intervals.

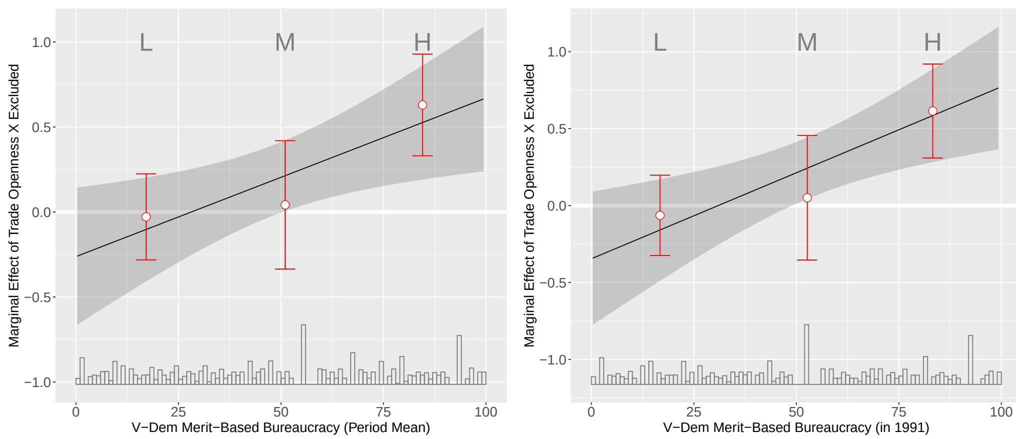

Figure A10: Marginal effects of trade openness on nightlight emissions of excluded groups across percentiles of period mean (1992-2002, left) and initial value (1991, right) of the V-Dem Merit-Based Bureaucracy Index. Binning estimates (Hainmueller, Mummolo, and Xu 2019) as points on top. Based on Table A8 (Models 4-5). Shaded areas and error bars indicate 95% confidence intervals.

Dynamic specifications: We run two additional model specifications to explore the temporal dynamics of our main effects. The first two columns in Table A9 implement an autoregressive distributed lag (ADL) models that adds one-year lags of all predictors and the dependent variable to our baseline specifications. The lags of our explanatory variables fail to reach statistical significance both individually and jointly. Wald tests of joint significance of all lagged explanatory variables yield p-values of 0.98 (Model 1) and 0.45 (Model 2). Failing to reject the null hypothesis of no difference leads us to adopt the more restrictive partial adjustment model with a lagged outcome variable (Models 3 and 4) (De Boef and Keele 2008, 187). The positive and statistically significant effects of the lagged nightlights indicator point towards serial correlation in the data. The main variables continue to be positive, but the triple interaction with the meritocratic appointment index fails to reach statistical significance in Model 4. Accordingly, we cannot reject the null of no catch-up effect of excluded groups at high levels of the meritocratic appointment index, even if the general marginal effect in the right panel of Figure A11 still points in the right direction. In contrast, the estimated interaction effect for state antiquity remains robustly different from zero at low and high levels of the index (left panel).

Although the estimated effects of the triple interactions in Models 3 and 4 halve in size relative to our main specifications, this does not mean that serial correlation was responsible for 50% of the reported effect size in the main paper. Rather the inclusion of the lagged outcome variable in Models 3 and 4 allows us to estimate the short versus long-term effects of our variables of interest. The effect reported in Table A11 is the instantaneous effect of trade openness on ethnic inequality at different levels of institutional strength. To compute long-run effects, we need to calculate the long-run multiplier, a combination of the the short-term effect and the estimated effect of the lagged outcome variable (De Boef and Keele 2008, 191). For the partial adjustment model this is $\frac{\beta}{1-\alpha}$, where β is the effect of the triple interaction, and α the estimated coefficient of the lagged outcome variable.[28] For state antiquity

[28] In the ADL model the long-run multiplier effect would be $\frac{\beta_0+\beta_1}{1-\alpha}$, where β_0 captures the contemporaneous effect of our triple interaction, and β_1 the one-year lag effect.

Table A9: Autoregressive Distributed Lag and Partial Adjustment Models.

	(1)	(2)	(3)	(4)
Within-country variation				
Openness (Δ) × Excluded	−0.513**	−0.012	−0.591**	0.003
	(0.182)	(0.074)	(0.187)	(0.066)
Openness (Δ) × Excl. × State History	1.091**		1.232***	
	(0.351)		(0.326)	
Openness (Δ) × Excl. × Merit Appoint.		0.015		0.107
		(0.070)		(0.065)
Openness (Δ) × Excluded (t-1)	−0.123	0.014		
	(0.223)	(0.086)		
Openness (Δ) × Excl. × State History (t-1)	0.214			
	(0.403)			
Openness (Δ) × Excl. × Merit Appoint. (t-1)		0.126		
		(0.081)		
Between-country variation				
Openness (∅) × Excluded	−0.007	−0.094	−0.008	−0.119
	(0.105)	(0.119)	(0.105)	(0.105)
Openness (∅) × Excluded (t-1)	0.022	0.020		
	(0.074)	(0.086)		
State History × Excluded	−0.001		−0.046	
	(0.157)		(0.113)	
State History × Excluded (t-1)	−0.015			
	(0.138)			
Merit Appoint. (∅) × Excluded		−0.046*		−0.012
		(0.022)		(0.018)
Merit Appoint. (∅) × Excluded (t-1)		0.065		
		(0.042)		
Within-group variation				
Exclusion	−0.019	0.043	−0.011	0.050
	(0.113)	(0.078)	(0.083)	(0.071)
Exclusion (t-1)	−0.029	−0.040		
	(0.116)	(0.066)		
Night Lights (log, t-1)	0.446***	0.435***	0.442***	0.436***
	(0.055)	(0.066)	(0.053)	(0.064)
Ethnic Group FE	Yes	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes	Yes

Country-clustered standard errors in parentheses.
Significance codes: $^{+}$p<0.1;, *p<0.05; **p<0.01; ***p<0.001

Table A10: Short and Long-Run Effects from Dynamic Moels

Moderator at 90th percentile Static effect from baseline models	State History 0.714		Merit Appointments 0.584	
Dynamic Model Long-run effect	ADL 0.617	LDV 0.595	ADL 0.477	LDV 0.364
First year	49.30%	55.80%	3.60%	56.40%
Second year	28.10%	24.70%	54.50%	24.60%
Third year	12.50%	10.90%	23.70%	10.70%
Fourth year	5.60%	4.80%	10.30%	4.70%
Fifth year	2.50%	2.10%	4.50%	2.10%

Based on coefficient estimates from Table A9

55% of the effect occur instantaneously, 25% occur in year 2, 11% in year 3, and 4.8% in year 4 (based on Model 3 in Table A9. As the estimated effect size of the lagged outcome variable is almost identical in Model 4, so is the distribution of the effect over time: 56% in year 1, 25% in year 2, 11% in year 3, and 4.7% in year 4.[29] Thus, slightly more than half of the effect of trade openness along our institutional proxies arrives in the short run, while the other half plays out over roughly four to five years.

[29]These are the relative effect size distributions over time. The overall effect of increasing trade openness on the gap between excluded and included groups is smaller along the range of the meritocratic appointment index than along the state antiquity index.

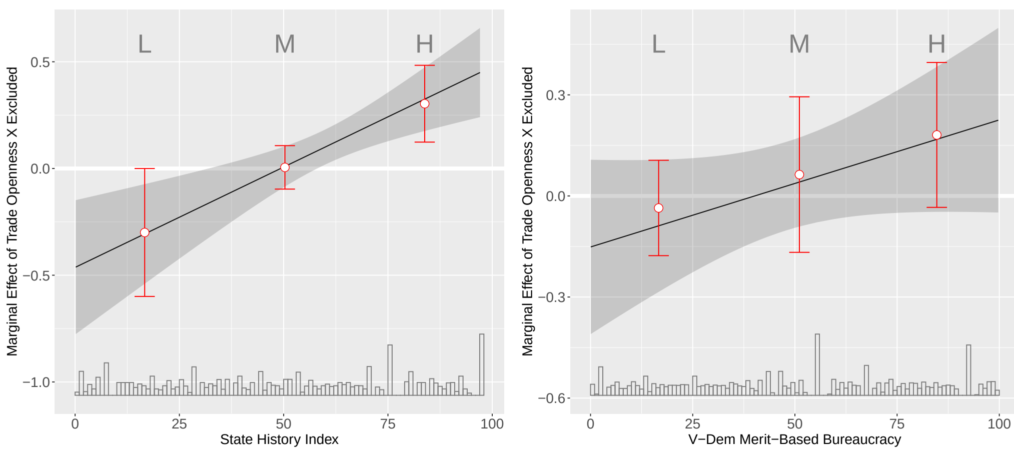

Figure A11: Marginal effects of trade openness on nightlight emissions of excluded groups across percentiles of state antiquity index (left) and V-Dem Merit-Based Bureaucracy Index (right). Binning estimates (Hainmueller, Mummolo, and Xu 2019) as points on top. Based on Models 3 and 4 in Table A9. Shaded areas and error bars indicate 95% confidence intervals.

Two-way clustered standard errors: Tables A11 and A12 replicate Tables 1 and 2 with standard errors clustered on both country and year. The coefficient estimates as well as marginal effect and binning plots in Figures A12 and A13 show that all results remain robust.

Table A11: Replication of Table 1 in Main Text with 2-way Clustered Standard Errors.

	(1)	(2)	(3)	(4)
Within-country variation				
Openness (Δ) × Excluded	−0.980**	0.080	−0.860*	0.020
	(0.293)	(0.110)	(0.338)	(0.134)
Openness (Δ) × Excluded × State History	2.262***		2.096**	
	(0.560)		(0.649)	
Openness (Δ) × Excluded × Merit Appoint.		0.267*		0.273*
		(0.116)		(0.125)
GDP (Δ) × Excluded			0.064	0.061
			(0.064)	(0.075)
Polity IV (Δ) × Excluded			−0.009*	−0.008*
			(0.004)	(0.004)
Agric. Share (Δ) × Excluded			−0.004	−0.003
			(0.004)	(0.004)
Resource Rents (Δ) × Excluded			−0.005	−0.004
			(0.003)	(0.003)
Export Conc. (Δ) × Excluded			0.010	−0.008
			(0.034)	(0.029)
Between-country variation				
Openness (∅) × Excluded	−0.038	−0.223	0.549+	0.174
	(0.198)	(0.181)	(0.309)	(0.201)
State History × Excluded	−0.322		−0.500*	
	(0.204)		(0.228)	
Merit Appoint. × Excluded		−0.035		−0.043+
		(0.029)		(0.026)
GDP (∅) × Excluded			0.133*	0.092*
			(0.057)	(0.037)
Polity IV (∅) × Excluded			0.012	0.006
			(0.010)	(0.007)
Agric. Share (∅) × Excluded			0.040	−0.003
			(0.027)	(0.013)
Resource Rents (∅) × Excluded			0.004	−0.0004
			(0.003)	(0.005)
Export Conc. () × Excluded			0.135*	0.086+
			(0.061)	(0.047)
Within-group variation				
Excluded	0.113	0.134	−2.608*	−1.603*
	(0.128)	(0.121)	(1.152)	(0.672)
Conflict Incidence			−0.077	0.031
			(0.078)	(0.024)
$p(B1 = B2)$	0.122	0.444	0.162	0.206
$p(B2 = B3)$	0.001	0.064	0.000	0.033
$p(B1 = B3)$	0.001	0.006	0.004	0.003
Group-FE	Yes	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes
Observations	6,849	6,454	5,769	5,365

Standard errors clustered on country and year in parentheses.
Significance codes: +p<0.1;, *p<0.05; **p<0.01; ***p<0.001

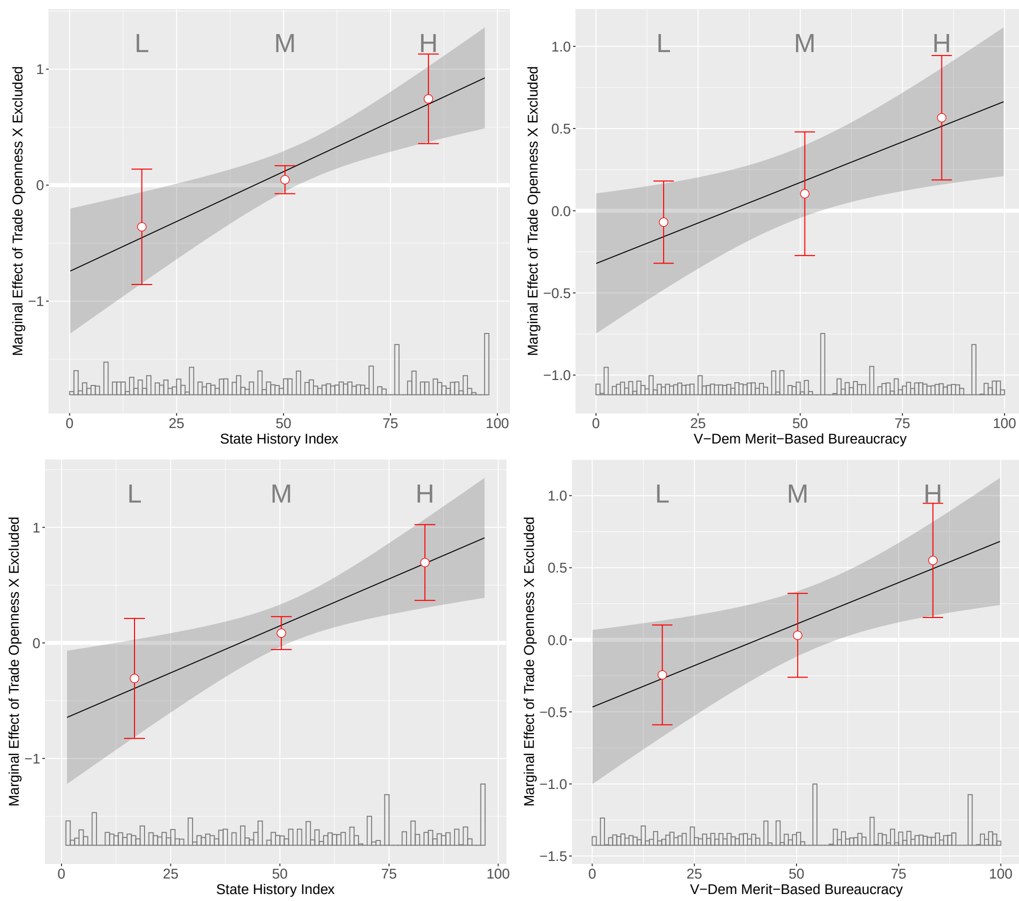

Figure A12: Marginal effects of trade openness on nightlight emissions of excluded groups across percentiles of state antiquity index (left) and V-Dem Merit-Based Bureaucracy Index (right). Binning estimates (Hainmueller, Mummolo, and Xu 2019) as points on top. Based on Table A11. Models 1-2 with in top row, models 3-4 in bottom row. Shaded areas and error bars indicate 95% confidence intervals.

Table A12: Replication of Table 2 in Main Text with 2-way Clustered Standard Errors.

	(1)	(2)	(3)	(4)
Within-Country Variation				
Openness (Δ) × Excluded	-0.976^{**}	0.085		
	(0.297)	(0.111)		
Openness (Δ) × Excl. × State History	2.250^{***}			
	(0.572)			
Openness (Δ) × Excl. × Merit Appoint. (∅)		0.268^{*}		
		(0.116)		
Openness (Δ) × Initial Night Lights			-0.237	0.299^{*}
			(0.285)	(0.111)
Openness (Δ) × Initial NL × State History			1.462^{**}	
			(0.468)	
Openness (Δ) × Initial NL × Merit Appoint.				0.209^{**}
				(0.071)
Between-Country Variation				
Openness (∅) × Excluded	-0.044			
	(0.186)			
State History × Excluded	-0.354			
	(0.219)			
Merit Appoint. × Excluded		-0.025		
		(0.027)		
Within-Group Variation				
Excluded	0.131	0.001		
	(0.114)	(0.048)		
Pre-Upgrade Dummy	-0.281^{+}	-0.079		
	(0.153)	(0.074)		
Pre-Upgrade Dummy × State History	0.643^{+}			
	(0.317)			
Pre-Upgrade Dummy × Merit Appointments		0.014		
		(0.061)		
Pre-Downgrade Dummy	0.063	-0.009		
	(0.146)	(0.052)		
Pre-Downgrade Dummy × State History	-0.247			
	(0.301)			
Pre-Downgrade Dummy × Merit Appointments		0.077		
		(0.049)		
$p(B1 = B2)$	0.137	0.356	0.883	0.000
$p(B2 = B3)$	0.001	0.079	0.000	0.988
$p(B1 = B3)$	0.001	0.005	0.003	0.002
Country-Year FE	Yes	Yes	Yes	Yes
Ethnic Group FE	Yes	Yes	Yes	Yes
Controls	No	No	No	No
Observations	6,832	6,438	6,112	5,719

Standard errors clustered on country and year in parentheses.
Significance codes: $^{+}p<0.1$;, $^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$

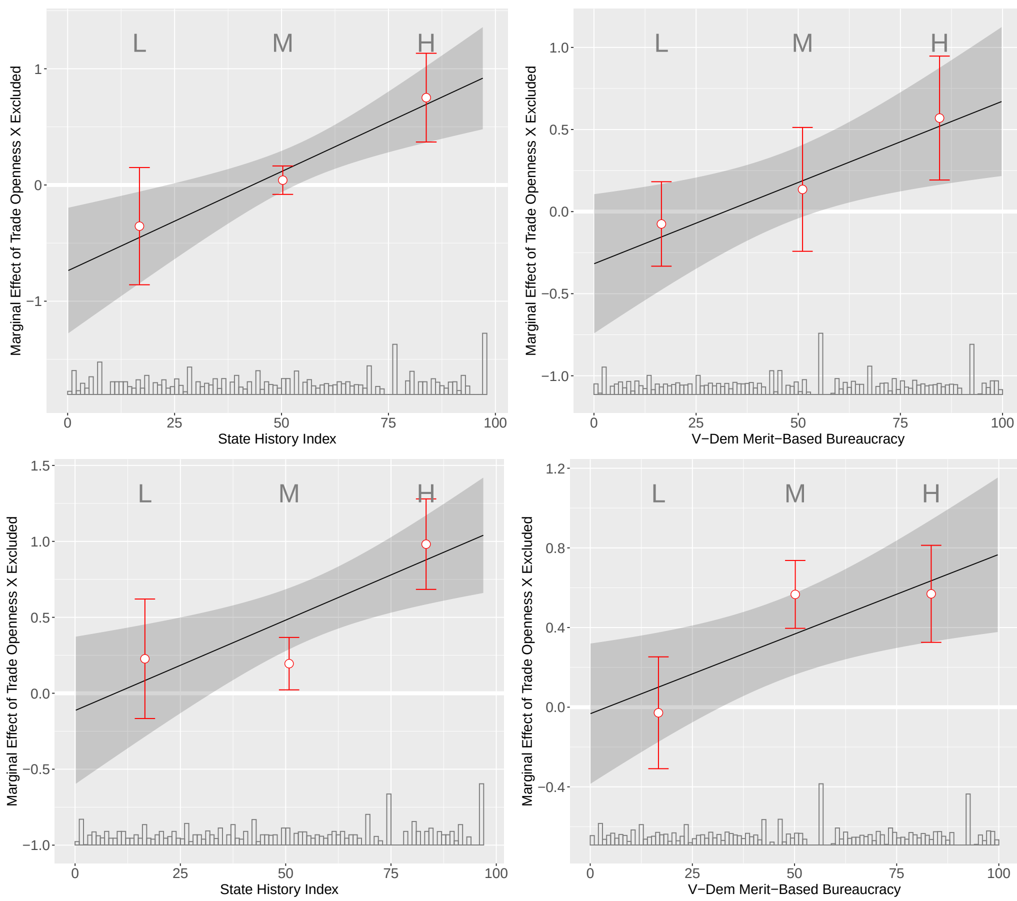

Figure A13: Marginal effects of trade openness on nightlight emissions of excluded groups (top) and interacted with the inverse of initial nightlights (bottom) across percentiles of state antiquity index (left) and V-Dem Merit-Based Bureaucracy Index (right). Binning estimates (Hainmueller, Mummolo, and Xu 2019) as points on top. Based on Table A12. Models 1-2 in top row, models 3-4 in bottom row. Shaded areas and error bars indicate 95% confidence intervals.

Table A13: Country Random Intercepts

	(1)	(2)	(3)	(4)
Within-Country Variation				
Openness (Δ)	−0.064	−0.012	−0.130	0.169
	(0.217)	(0.080)	(0.258)	(0.108)
Openness (Δ) × Excluded	−1.571***	0.598***	−0.858*	0.463**
	(0.334)	(0.112)	(0.379)	(0.158)
Openness (Δ) × Excluded × State History	4.448***		2.643***	
	(0.622)		(0.739)	
Openness (Δ) × Excluded × Merit Appoint.		0.476***		0.232+
		(0.102)		(0.126)
Openness (Δ) × State History	0.070		0.731	
	(0.430)		(0.544)	
Openness (Δ) × Merit Appoint.		0.003		0.045
		(0.069)		(0.089)
Between-Country Variation				
Openness (∅)	1.211*	1.257*	0.808*	0.894*
	(0.513)	(0.526)	(0.367)	(0.369)
Openness (∅) × Excluded	−0.022	−0.286***	−0.459***	−0.634***
	(0.069)	(0.068)	(0.091)	(0.092)
State History	2.391**		0.994+	
	(0.740)		(0.525)	
State History × Excluded	−0.331**		0.271+	
	(0.103)		(0.140)	
Merit-Based Appointments		0.078**		0.017
		(0.028)		(0.035)
Merit-Based Appointments × Excluded		−0.182***		−0.083*
		(0.021)		(0.032)
Within-Group Variation				
Excluded	−0.114	0.004	2.021***	1.950***
	(0.072)	(0.050)	(0.409)	(0.410)
Country-RE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes
Observations	6,849	6,454	5,769	5,365

Standard errors in parentheses.
Significance codes: $^{+}p<0.1$;, $^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$

Random effects specifications: We estimate three additional types of random effects specifications as an alternative to account for unobserved heterogeneity between countries and/or ethnic groups. We prefer our fixed effects models discussed in the main paper and Appendix as they do not require the strong assumption that the random effects are uncorrelated with the independent variables.

- First, we run models with random intercepts by country and year fixed effects.

Table A13 presents results from models with (Columns 3 and 4) and without control variables and their interactions (Columns 1 and 2). Figures A14 and A15 plot the corresponding marginal effects.

- Second, we estimate random intercepts by ethnic group while maintaining year fixed effects. Table A14 presents results from models with (Columns 3 and 4) and without control variables and their interactions (Columns 1 and 2). Figures A16 and A17 plot the corresponding marginal effects.

- Third, we estimate random intercepts by ethnic group while also including country-year fixed effects. Table A15 presents results from models with (Columns 3 and 4) and without control interactions (Columns 1 and 2). Figure A18 plots the corresponding marginal effects.

All 12 of these random effects models support our hypotheses and show stronger effects than our baseline specifications.

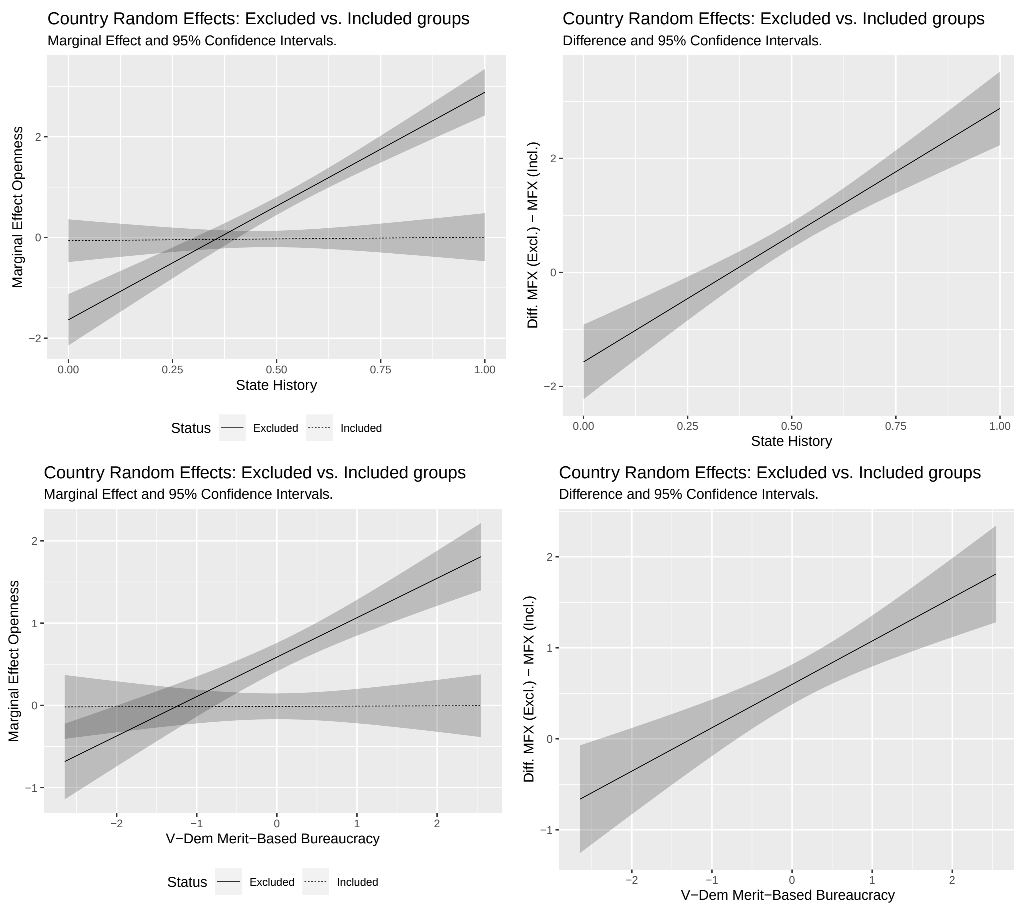

Figure A14: Marginal effects of trade openness on nightlight emissions of excluded and included groups across observed range of state antiquity index (top left). Difference in marginal effects between excluded and included groups across state antiquity (top right). Same for V-Dem Merit-Based Bureaucracy (bottom row). Based on Models 1 and 2 in Table A13 (models with country random intercepts).

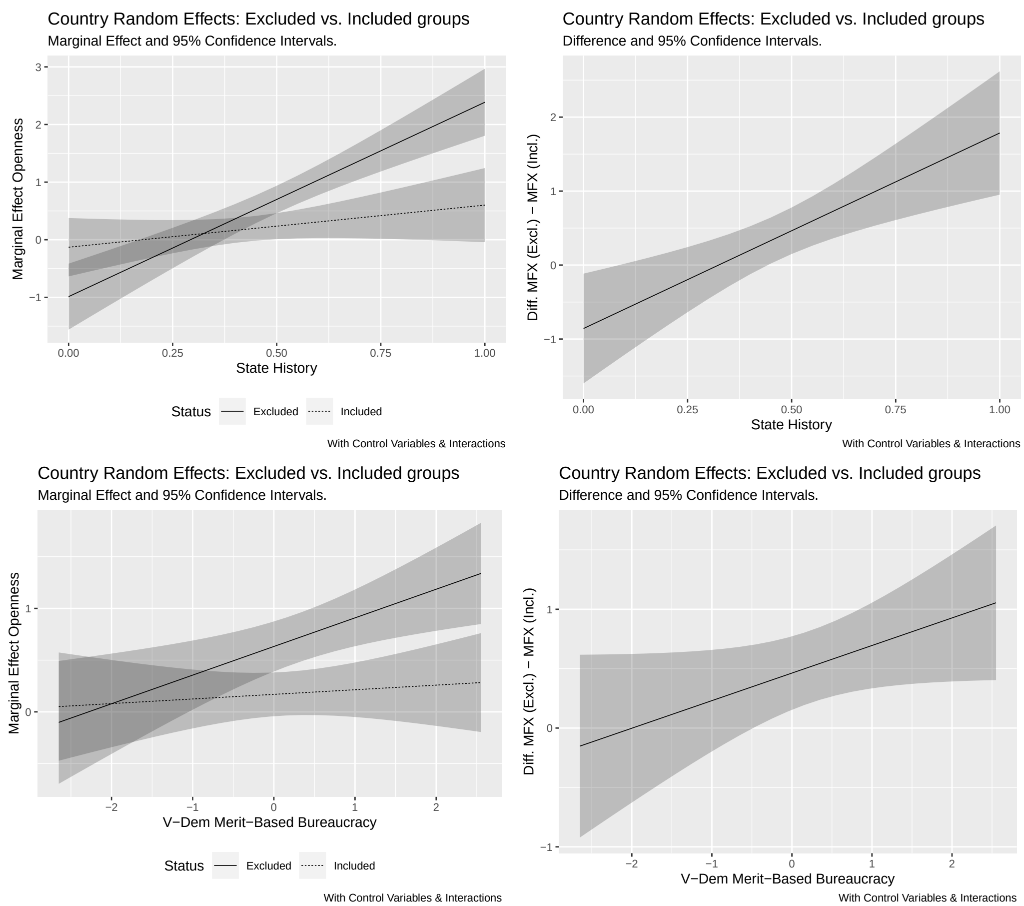

Figure A15: Marginal effects of trade openness on nightlight emissions of excluded and included groups across observed range of state antiquity index (top left). Difference in marginal effects between excluded and included groups across state antiquity (top right). Same for V-Dem Merit-Based Bureaucracy (bottom row). Based on Models 3 and 4 in Table A13 (models with country random intercepts and control variables).

Table A14: Group Random Intercepts

	(1)	(2)	(3)	(4)
Within-Country Variation				
Openness (Δ)	0.027	0.009	0.083	0.211***
	(0.101)	(0.037)	(0.111)	(0.045)
Openness (Δ) × Excluded	−1.607***	0.596***	−1.003***	0.281***
	(0.158)	(0.052)	(0.168)	(0.068)
Openness (Δ) × Excluded × State History	4.365***		2.927***	
	(0.294)		(0.329)	
Openness (Δ) × Excluded × Merit Appoint.		0.686***		0.469***
		(0.047)		(0.054)
Openness (Δ) × State History	0.023		0.255	
	(0.202)		(0.234)	
Openness (Δ) × Merit Appoint.		−0.111***		−0.084*
		(0.032)		(0.037)
Between-Country Variation				
Openness (∅)	1.543***	1.570***	0.571*	0.849***
	(0.314)	(0.310)	(0.248)	(0.251)
Openness (∅) × Excluded	−0.245**	−0.560***	0.154	−0.247+
	(0.092)	(0.091)	(0.137)	(0.136)
State History	1.261**		1.398***	
	(0.415)		(0.358)	
State History × Excluded	−0.241*		−0.517***	
	(0.108)		(0.133)	
Merit-Based Appointments		0.032*		0.014
		(0.015)		(0.017)
Merit-Based Appointments × Excluded		−0.073***		−0.093***
		(0.018)		(0.023)
Within-Group Variation				
Excluded	0.259**	0.388***	−2.073***	−1.502***
	(0.081)	(0.061)	(0.452)	(0.415)
Group RE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes
Observations	6,849	6,454	5,769	5,365

Standard errors in parentheses.
Significance codes: +p<0.1;, *p<0.05; **p<0.01; ***p<0.001

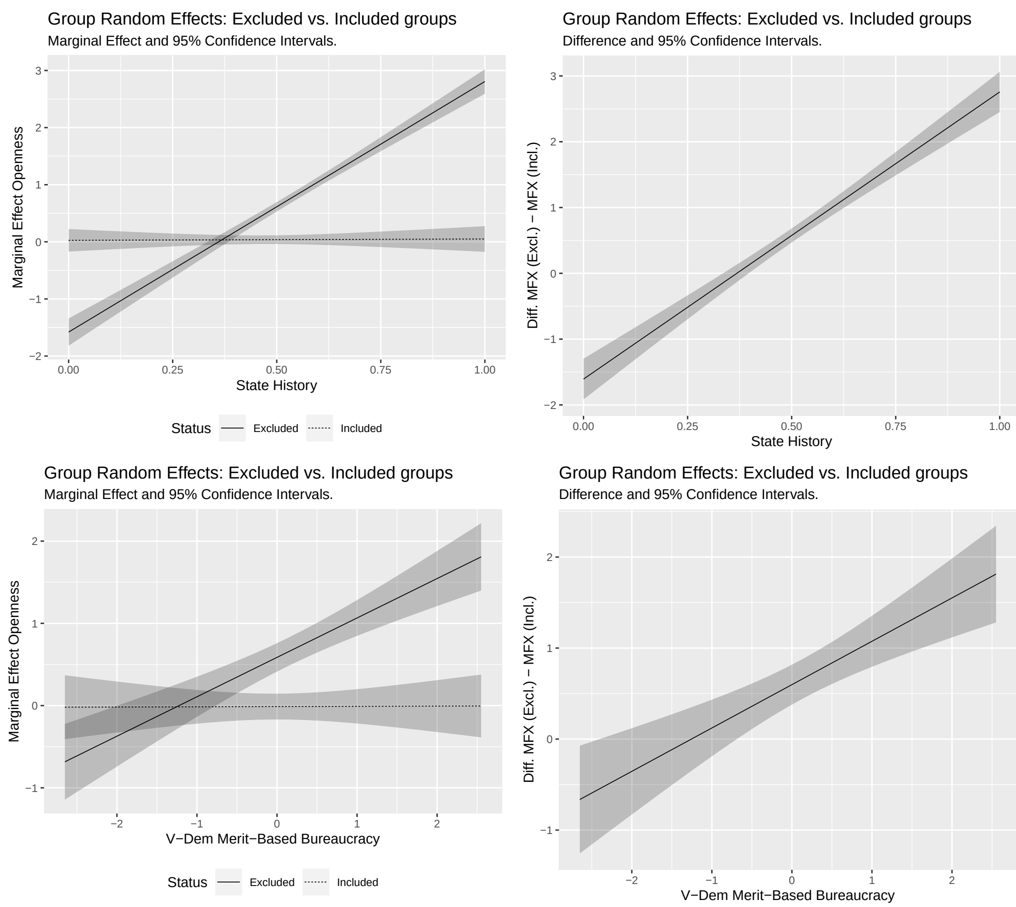

Figure A16: Marginal effects of trade openness on nightlight emissions of excluded and included groups across observed range of state antiquity index (top left). Difference in marginal effects between excluded and included groups across state antiquity (top right). Same for V-Dem Merit-Based Bureaucracy (bottom row). Based on Models 1 and 2 in Table A14 (models with group random intercepts).

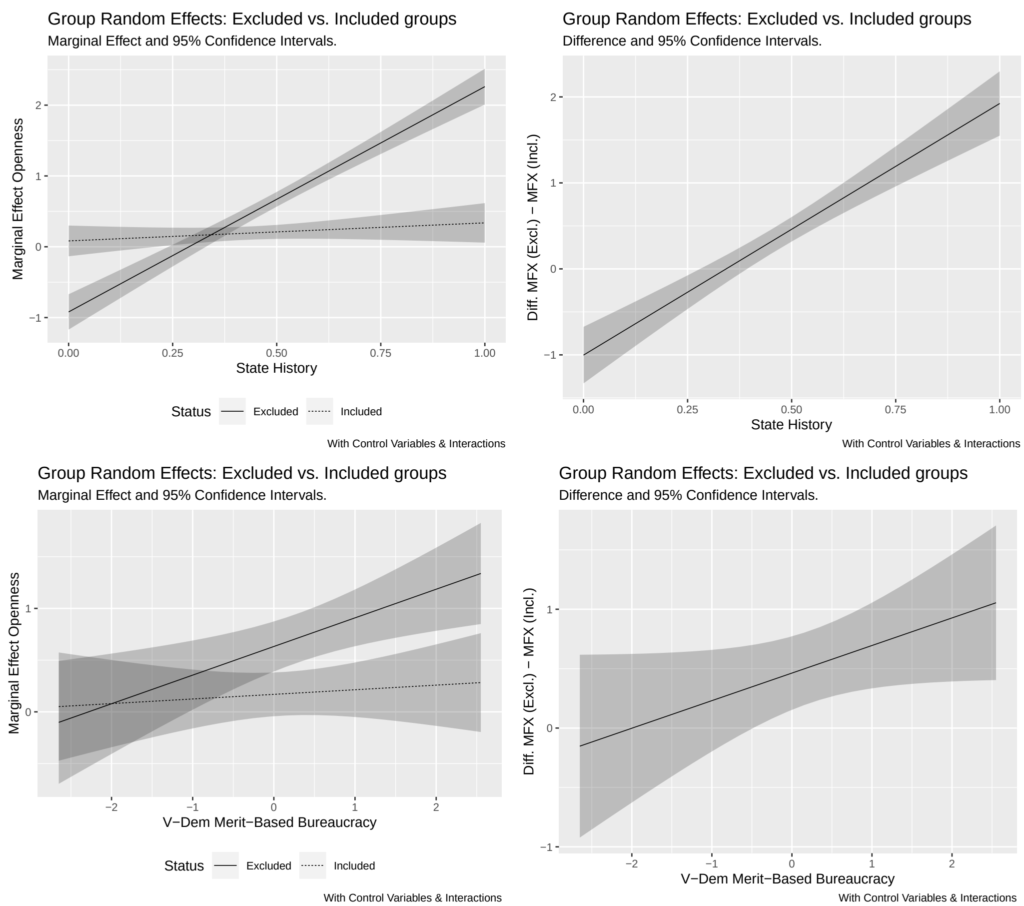

Figure A17: Marginal effects of trade openness on nightlight emissions of excluded and included groups across observed range of state antiquity index (top left). Difference in marginal effects between excluded and included groups across state antiquity (top right). Same for V-Dem Merit-Based Bureaucracy (bottom row). Based on Models 3 and 4 in Table A14 ((models with group random intercepts and control variables).

Table A15: Group Random & Country-Year Fixed Effects

	(1)	(2)	(3)	(4)
Within-Country Variation				
Openness (Δ) × Excluded	−0.978***	0.078	−0.854***	0.044
	(0.170)	(0.053)	(0.191)	(0.075)
Openness (Δ) × Excluded × State History	2.261***		2.113***	
	(0.333)		(0.391)	
Openness (Δ) × Excluded × Merit Appoint.		0.267***		0.279***
		(0.048)		(0.058)
Between-Country Variation				
Openness (∅) × Excluded	−0.029	−0.210*	0.366*	0.081
	(0.095)	(0.091)	(0.160)	(0.154)
State History × Excluded	−0.334**		−0.469***	
	(0.106)		(0.137)	
Merit-Based Appointments × Excluded		−0.039*		−0.050*
		(0.017)		(0.024)
Within-Group Variation				
Excluded	0.102	0.117*	−1.992***	−1.290**
	(0.081)	(0.059)	(0.540)	(0.488)
Group RE	Yes	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes
Observations	6,849	6,454	5,769	5,365

Standard errors in parentheses.
Significance codes: $^{+}p<0.1$;, $^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$

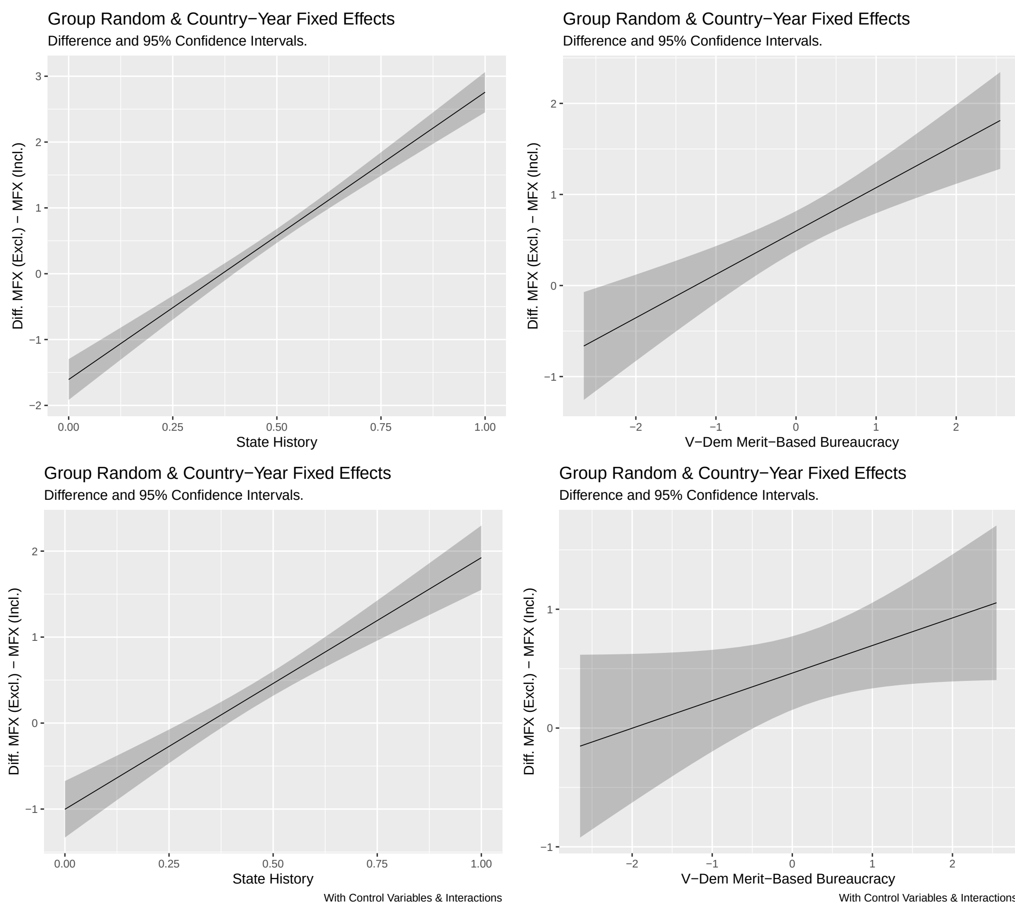

Figure A18: Marginal effects of trade openness on nightlight emissions of excluded relative to included groups across observed range of state antiquity index (left column) and V-Dem Merit-Based Bureaucracy (right column). Top row based on Models 1 and 2 in Table A15 (models with group random intercepts and country-year fixed effects). Bottom row based on Models 3 and 4 in Table A15 (group random intercepts, country-year fixed effects, control interactions).

Alternative measures of state institutions. Table A16 replaces state age and merit-based appointments with the ordinal executive constraints measure from Polity IV (Model 1, Marshall, Jaggers, and Gurr (2011)) and dummies for differently institutionalized authoritarian regime types as defined by Geddes, Wright, and Frantz (2014) (Model 2). Figures A19 and A20 display the associated marginal effects. The executive constraints interaction remains small and insignificant, consistent with our notion that the relevant dimensions of institutional strength are different from formal democratic constraints. In party-based regimes, the effect is positive and significant. The effect for more weakly institutionalized personalist dictatorships is negative, significantly smaller than the effect in party-based regimes, yet not significantly different from zero. In both monarchies and military regimes, increasing trade openness is associated with a catch-up effect of excluded groups but this effect is orders of magnitude smaller in monarchies than in party-based regimes and the large effect in monarchies fails to reach conventional significance levels (Figure A20).

Table A16: Linear Model of Group-Level Night Lights Mechanisms with alternative Institutional Moderators

	(1)	(2)
Within-country variation		
Openness (Δ) × Excluded	−0.080	0.080
	(0.203)	(0.140)
Openness (Δ) × Excl. × Exec. Constraints	0.029	
	(0.044)	
Exec. Constraints × Excluded	−0.005	
	(0.017)	
Openness (Δ) × Excl. × Personalist		−0.206
		(0.264)
Personalist × Excluded		−0.008
		(0.100)
Openness (Δ) × Excl. × Party		0.403
		(0.260)
Party × Excluded		−0.062
		(0.049)
Openness (Δ) × Excl. × Military		0.591
		(0.504)
Military × Excluded		0.018
		(0.055)
Openness (Δ) × Excl. × Monarchy		0.003
		(0.143)
Personalist × Excluded		−0.026
		(0.030)
Between-country variation		
Openness (Ø) × Excluded	0.044	0.021
	(0.221)	(0.234)
Within-group variation		
Exclusion	−0.035	−0.040
	(0.220)	(0.165)
Group-FE	Yes	Yes
Country-Year FE	Yes	Yes
Observations	6,559	6,909

*p<0.05; **p<0.01; ***p<0.001
Country clustered standard errors in parentheses.
Baseline category in Model 2: non-autocracies.

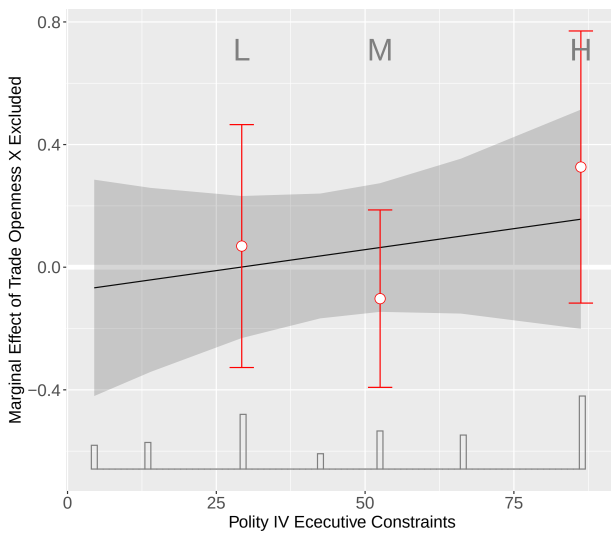

Figure A19: Marginal effects of trade openness on nightlight emissions of excluded groups across percentiles of Polity IV Executive Constraints. Binning estimates (Hainmueller, Mummolo, and Xu 2019) as points on top. Based on Table A12. Shaded areas and error bars indicate 95% confidence intervals.

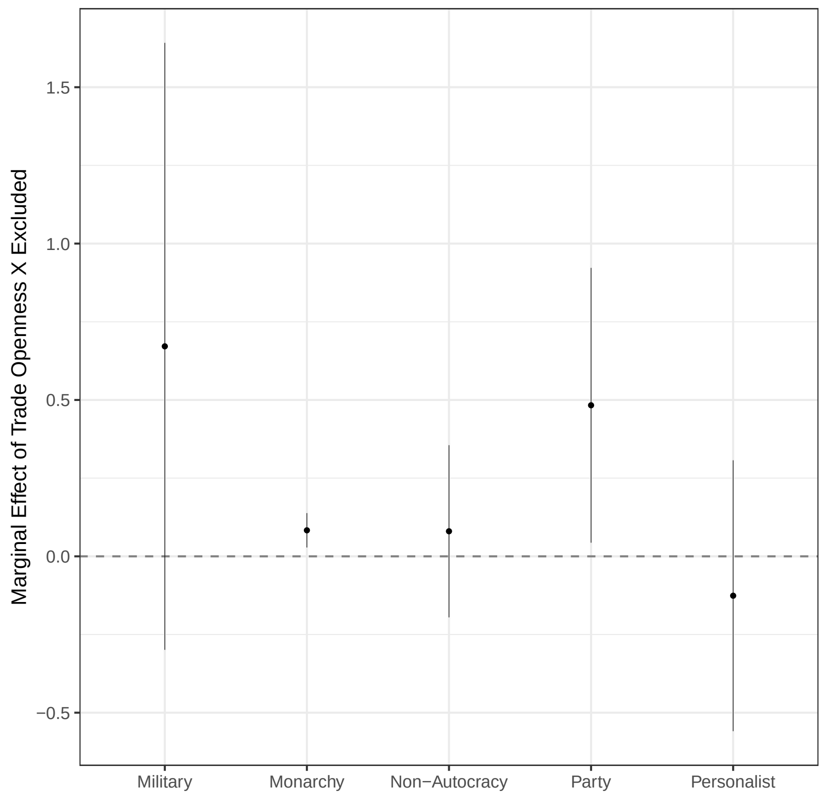

Figure A20: Marginal effects of trade openness on nightlight emissions of excluded groups across military dictatorships, monarchies, democracies, party-based autocracies, and personalist dictatotships . Binning estimates (Hainmueller, Mummolo, and Xu 2019) as points on top. Based on Table A12. Error bars indicate 95% confidence intervals.

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