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Nils-Christian Bormann

Yannick I. Pengl

Lars-Erik Cederman

Nils B. Weidmann

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Nils-Christian Bormann is Professor of International Political Studies at the Department of Philosophy, Politics and Economics, Witten/Herdecke University (Address: Alfred-Herrhausen-Strasse 50, 58448 Witten, Germany; email: nils.bormann@uni-wh.de).

Yannick I. Pengl (corresponding author) is Post-Doc at the International Conflict Research Group, ETH Zurich (Address: Haldeneggsteig 4, 8092 Zurich, Switzerland; email: yannick.pengl@icr.gess.ethz.ch).

Lars-Erik Cederman is Professor of International Conflict Research at ETH Zurich (Address: Haldeneggsteig 4, 8092 Zurich, Switzerland; Phone: +41 44 632 67 59; email: cederman@icr.gess.ethz.ch).

Nils B. Weidmann is Professor of Political Science at the Department of Politics and Public Administration, University of Konstanz (Address: Box 84, 78457 Konstanz, Germany; Phone: +49 7531 885 676; email: nils.weidmann@uni-konstanz.de).

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Nils-Christian Bormann

Yannick I. Pengl

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Nils B. Weidmann

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.

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 and related questions of redistribution,[1] 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.[2] 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.[3]

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.[4] 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.[5] Most of this literature assumes that discrepancies between politically included and excluded groups are constant, even calling them an "axiom of politics."[6] Rather than accepting this claim as an assumption, we examine whether and why economic inequality between included and excluded groups changes dynamically over time.

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[1]Piketty 2014; Scheve and Stasavage 2010.

[2]Tilly 1999.

[3]Houle 2015; Baldwin and Huber 2010; Østby 2008; Stewart 2008; Cederman, Weidmann, and Gleditsch 2011.

[4]Alesina, Michalopoulos, and Papaioannou 2016; Michalopoulos and Papaioannou 2013.

[5]Franck and Rainer 2012; Hodler and Raschky 2014.

[6]De Luca, Hodler, Raschky et al. 2018.

We argue that changing patterns of ethno-economic inequality are the result of 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.[7] 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, we show that increasing integration into the world

[7] Rodrik 1999.

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.[8] Rodrik estimates that reducing tariffs leads to distributional effects that exceed GDP growth by a factor greater than ten in African developing countries.[9] 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.[10] 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.[11] Since the vast majority of the global labor force resides in the developing world, global inequality decreases as workers

[8]Hiscox 2001, 1.

[9]Rodrik 1998b, 19.

[10]Rudra 2002; Easterly 2007.

[11]See e.g., Harrison, McLaren, and McMillan 2011.

in China, India, and other emerging markets join the global middle class.[12]

Yet, the debate between globalization skeptics and enthusiasts overlooks an important mediating variable: political institutions. According to Rodrik, domestic "conflict-management institutions" mediate the redistributive effects of trade openness.[13] 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.[14] 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 as well as more specific qualitative studies on how political and institutional forces shape the effects of trade liberalization in developing countries.[15]

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.[16] 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.[17] While economic "inclusiveness"

[12]Milanovic 2013.

[13]Rodrik 1998a, 1999 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.

[14]Bates 1974.

[15]North 1990; Acemoglu and Robinson 2012; Boone 1994; Rudra and Jensen 2011.

[16]Rudra and Jensen 2011.

[17]North 1990; Acemoglu and Robinson 2012.

and the threat of elite capture are central pillars in the recent institutionalist literature,[18] 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 capacity, secure property rights and impartial contract enforcement to civil liberties, equal access to education, and constraints on political elites and rent-seeking coalitions.[19] 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

[18] Acemoglu and Robinson 2012.

[19] North 1990; Acemoglu and Robinson 2012.

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 Mann, infrastructural power refers to "institutional capacity of a central state [...] to penetrate its territories and logistically implement decisions."[20]

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.[21] 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[22]

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.[23] 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

[20]Mann 1993, p.59.

[21]Herbst 2000, ch. 5–6; Migdal 1988.

[22]Migdal 1988; Scott 2009.

[23]See, for example, Harrison, McLaren, and McMillan 2011.

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. Limited control over, and legitimacy among, excluded parts of the population confronts those in power with what Migdal has dubbed "the ruler's dilemma."[24] 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.

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.[25] 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

[24]Migdal 1988.

[25]Kasara 2007.

loyalty or personal connections.[26] This dimension encompasses formal institutional constraints on government leaders and high-ranking bureacurats by, for example, strong and independent judiciaries.[27] 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.[28]

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.[29] 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 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

[26]Weber 1978.

[27]Evans 1995. 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.

[28]Rauch and Evans 2000; Acemoglu and Robinson 2012.

[29]Evans 1995; Rauch and Evans 2000.

insiders and outsiders of patronage networks.[30] 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, by profiting from taxes on import and export goods, and even by creating trade monopolies that benefit their supporters.[31] 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.[32]

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[33] 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[34] Quite the opposite, trade policy tends to create “new rent havens” and “solidify domestic political alliances,” as Boone concludes from her analysis of liberalization policies in Senegal and Côte d’Ivoire.[35]

Beyond these indirect effects, meritocratic bureaucracies can actively shape the mas-

[30]Van de Walle 2009.

[31]Bates 1981 and Bienen 1991, 76-7.

[32]Franck and Rainer 2012; Burgess, Jedwab, Miguel et al. 2015; Hodler and Raschky 2014.

[33]Migdal 1988.

[34]Bienen 1990.

[35]Boone 1994, 462.

sive changes that follow from trade openness.[36] 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 "conflict-management institutions.".[37] 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.[38] Similarly, the Vietnamese government runs programs specifically designed to boost development in ethnic minority regions.[39] 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

[36] Adsera and Boix 2002, 230.

[37] Rodrik 1999, 98-99.

[38] Kanbur 2000; Langer and Stewart 2012.

[39] Kang and Imai 2012.

relative economic status of groups, we resort to estimation using spatial data, as existing research has done.[40] 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.[41] Ethnic groups are considered politically relevant when group members make 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.[42]

To estimate EPR groups' economic trajectories, we combine data on nightlight emissions with information on ethnic settlement regions from the GeoEPR data.[43] 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

[40]Cederman, Weidmann, and Bormann 2015.

[41]Vogt, Bormann, Rúegger et al. 2015.

[42]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.

[43]Vogt, Bormann, Rúegger et al. 2015.

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, specifically in the many less-developed countries in our sample that have unreliable official statistics.[44] Equally relevant for us is that nightlight emissions cannot only be used at the national level, but also to track *subnational* variation in economic outcomes.[45] Investigating the source of horizontal inequality, De Luca et al. rely on changes in total nightlight emissions to demonstrate that a political leader's co-ethnics profit disproportionately from their putative cousin's rule.[46]

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.[47] 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

[44]Henderson, Storeygard, and Weil 2011.

[45]Chen and Nordhaus 2011. Weidmann and Schutte 2017 use fine-grained survey data to show that nightlights predict economic conditions at the household-level well.

[46]De Luca, Hodler, Raschky et al. 2018. For a similar result that focuses on regions but ignores ethnic identity, refer to Hodler and Raschky 2014.

[47]National Geophysical Data Center 2014.

and 2012.

Using the GeoEPR settlement regions described above, we compute the sum of the nightlights emitted from each ethnic region.[48] 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 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.[49] 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.[50] 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.

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[48] 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.

[49] CIESIN et al., 2011.

[50] The 2014 Ethnic Power Relations dataset provides information 139 states in which ethnicity is politically relevant. In 19 states from five different continents, ethnic group settlement areas are not sufficiently distinct to allow us to compute night lights emissions for each group.

Explanatory Variables

We measure globalization using the *trade openness* variable from the World Development Indicators database.[51] 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.[52]

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 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* to capture the macro-historical origins and long-term persistence of effective state institutions.[53] The basic intuition is that today's states' infrastructural power only gradually changes over

[51]World Bank 2019.

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

[53]Borcan, Olsson, and Putterman 2018.

time and is, to a large extent, historically inherited.[54] Like Mann in his definition of infrastructural power, Borcan and colleagues explicitly link state age to similar aspects of institutional capacity: "accumulated state history favors capacity building, taxation and the provision of public goods.".[55] 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.[56] The variable's credible exogeneity to contemporary political events provides another important reason to choose it over over alternative measures such as the tax-to-GDP ratio.

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.[57] 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 political connections"..[58] 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

[54]Dell, Lane, and Querubin 2018 demonstrate the influence of long-established institutions in Vietnam, where the historical legacy of institutional differences persists despite violent decolonization, civil and interstate war. In our case studies of Ethiopia and China, we further highlight the persistence of long-established statehood.

[55]Borcan, Olsson, and Putterman 2018, p.6; Mann 1993.

[56]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.

[57]Coppedge et al. 2019.

[58]Coppedge et al. 2019, p. 176.

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. Expert assessments of institutional quality have been criticized as potentially endogenous to recent economic performance.[59] To ensure that our findings are not due to perceived, yet artificial and potentially endogenous short-term fluctuations, we use the country-specific pre-period value in 1991 for all country-years between 1992 and 2012.[60]

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 differential growth rates between politically excluded and included groups. To account for this possibility, we also run 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 and 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 variation in country's trade openness over time 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. Thus, we need to interact variables across levels, as our multi-level data structure nests groups in countries in years. Specifically, we are interested in how changes in a country-level variable (trade openness) affect changes in a group-level outcome (night lights emissions),

[59]See Glaeser, La Porta, Lopez-de Silanes et al. 2004.

[60]In our Online Appendix, we replace initial values of the meritocracy index variable with its period mean (1992-2012), which does not substantively alter our results.

conditional on group (political status) and country-level (institutional quality) factors.

To accurately assess the effects of these cross-level interactions, we run linear models with a triple interaction along with ethnic group and country-year fixed effects. The interactions test for the heterogeneous effects stipulated above while the fixed effects account for time-invariant omitted variables at the group level and temporal shocks at the country-level. Additionally, country-year fixed effects ensure that all estimates are based on group-level deviations in per capita luminosity from the country-year average. Using an indicator variable for excluded political groups allows us to interpret the estimated effects as changes in the income gap between excluded and included groups, as the average included group forms the base category in each country-year.[61] Our baseline regression specification thus takes the following general form:

$$\begin{aligned} log(y_{ict}) = \beta_1\, Openness_{ct} \times Excluded_{ict} + \\ \beta_2\, Openness_{ct} \times Excluded_{ict} \times \overline{StateCapacity_c} + \\ \beta_3\, Excluded_{ict} \times \overline{StateCapacity_c} + \beta_4\, Excluded_{ict} + \\ + \beta_k\, c_k + \mu_i + \rho_{ct} + \epsilon_{ict} \end{aligned} \tag{1}$$

The outcome variable y is the logarithm of per capita nightlights in group i's settlement area nested in country c at time t.[62] 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

[61]Our modeling strategy makes our results very similar to the operationalization of group-level inequality used by Cederman, Weidmann, and Gleditsch 2011.

[62]Following Weidmann and Schutte 2017, we log-transform the dependent variable to account for its highly skewed distribution.

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. Due to country-year fixed effects, two constitutive terms and one two-way interaction drop from the model ($trade_{ct}$, $\overline{StateCapacity_c}$, and $trade_{ct} \times \overline{StateCapacity_c}$).

Our main focus rests on the cross-level interaction between trade openness, political exclusion, and state capacity. 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. Our hypotheses predict a positive and significant coefficient β_2 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 trade openness effect at different levels of institutional strength, we need to combine all constituent effects that include openness. All else equal, a positive sum $\beta_1 + \beta_2 \times a_c$ indicates increasing trade openness to be associated with disproportionate luminosity gains of excluded groups relative to their included counterparts in a country with institutional quality a_c.[63] Conversely, a negative sum at a_c indicates that excluded groups would grow slower in response to increasing trade than their included counterparts. Given that excluded groups are generally poorer than included groups, faster growth of excluded groups implies catch up and reduced inequality.[64]

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[63]See Appendix (pages A4 to A8) for a more detailed derivation and explanation of this marginal effect.

[64]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 (Table A2 and Figures A3 and A4). We also split our sample at the median values of the institutional moderators to avoid any triple interactions (Table A3).

Multiplicative interaction models potentially suffer two important flaws.[65] First, conventional models assume that the interaction effect is linear and changes at a constant rate along the range of the moderator, even if the data-generating process is non-linear. Second, too few observations and little variation in the treatment variable at extreme values of the moderator may result in unreliable and highly model-dependent point estimates as well as artificially low measures of uncertainty. Hainmueller, Mummolo, and Xu propose a simple binning estimator that addresses both issues by estimating the marginal effects of a treatment variable ($Openness_{ct} \times Excluded_{ict}$) at typically low, intermediate, and high values of a continuous moderator (state antiquity or merit-based appointments).[66] 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).

[Table 1 about here.]

As outlined in Equation 1, we estimate a triple interaction term with one dichotomous (political exclusion) and two continuous variables (trade openness and institutional

[65] Hainmueller, Mummolo, and Xu 2019.

[66] 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.

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 positive estimates 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 larger but with greater uncertainty for meritocracy 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. 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' 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 of changes in trade openness on the relative growth performance of excluded groups (solid line) across the observed percentile range of the two moderators. At low levels of our institutional moderators – where the triple interaction is effectively zero – the marginal effect of trade openness is negative, albeit statistically indistinguishable from zero for meritocratic bureaucracy. With increasing values on either institutional indicator however – and thus greater influence of the triple interaction – the picture changes. At the upper end of the spectrum, the estimated effects become positive and significant.

[Figure 1 about here.]

These results are robust to Hainmueller, Mummolo, and Xu's binning estimator. 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 at the bottom of Table 1).

In substantive terms, an increase in trade openness by one standard deviation of all observed within-country changes (i.e. 17.3 percentage points) in the top tercile of state history (meritocracy) translates into a 12.5% (10.9%) increase in night light emissions for excluded than for included groups. This relationship is reversed in the bottom tercile, where the same change in trade openness is associated with a 5.6% (0.8%) decrease in luminosity for excluded groups compared to groups with access to the state apparatus. 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. Increasing trade widens the gap between included and excluded groups in states with the lowest infrastructural power (left column Figure 1), but we cannot reject the null hypothesis of constant inequality in states where nepotism dictates hiring practices in the bureaucracy (right column Figure 1).

Robustness Checks

Establishing causality from the type of broad comparative analyses pursued in this paper is difficult. One 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 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.[67] Most

[67] We interact country-year controls with exclusion due to the multi-level nature of our data. Our primary interest is the effect of country-level variables on group-level outcomes conditional on groups' political status. Thus, the necessary control is at the cross-level interaction between exclusion and time-varying, country-level controls such as economic development. Country-year fixed effects already account for country-level constituent terms.

interactions with control variables yield statistically insignificant estimates close to zero, and thus do not exert a meaningful effect on the luminosity gains of excluded groups.[68] Another concern is that the demographic dominance of the largest ethnic group rather than institutional quality drives our findings. We therefore rerun our baseline models and add an additional triple interaction between trade, exclusion, and the population share of the country's largest group (Table A5 and Figure A7). Doing so slightly weakens our baseline findings, but trade continues to be significantly more beneficial for excluded groups at high rather than low values of both institutional moderators. Additionally, we probe the sensitivity of our results to unobserved heterogeneity through different fixed effects specifications (Tables A2-A4) and by clustering standard errors along country and year (Tables A10-A11). 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.

A final concern of unobserved heterogeneity arises from the multilevel interaction of two time-varying factors: $exclusion_{ict}$ and $trade_{ct}$. Although country-year fixed effects seem to account for time-varying confounders at the country-level, the interaction with $exclusion_{ict}$ allows cross-country variation in trade openness, and thus potentially omitted cross-country confounders that interact with $exclusion_{ict}$, to creep in through the back-door.[69] To fully isolate the effect of changes in trade openness and to guard ourselves against this source of omitted variable bias, we demean $trade_{ct}$, and thus split it into a within-country and a between-country term. Reassuringly, we find that our main results are robust to this specification, and that within rather than between-country changes in trade openness drive the catch-up effects of excluded ethnic groups (Table A4, Figures A5 & A6).

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

[68] We discuss the rationale for including specific controls in the Appendix.

[69] We explain this concern in greater detail in the Appendix, pp.A9-A12.

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.[70] Such a policy would undermine our account of redistribution in weak states but would not affect our account of strong regimes where 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.[71]

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. 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.[72] 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

[70]In some African states, for example, government leaders tax members of their own ethnic groups more severely than other groups. See Kasara 2007.

[71]See Milner and Kubota 2005.

[72]Hodler and Raschky 2014 first used this approach.

contexts 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 statistically indistinguishable from zero. All coefficients remain substantively small and/or point in the direction that strengthens rather than weakens our interpretation.[73] In addition, ethnopolitical upgrades and downgrades are extremely rare and occur in less than 1.5% of all group years in our sample. Overall, we do not find any evidence for the strategic inclusion or exclusion of groups based on recent economic performance, 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).[74] 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.[75]

[Table 2 about here.]

[73]The positive interaction between the pre-upgrade dummy and state history in Model 5 suggest that, if anything, the strategic inclusion of economically rising groups is more common under strong institutions. Thus, our estimate might understate the true catch-up effect.

[74]We repeat the same strategy with a linear time trend over the three years prior to a group's change in power status (Table A6, Models 1 and 2).

[75]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 A6, 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 A7, Models 1 and 2). Neither of these tests alters our conclusions.

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.[76] 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 triple interactions suggesting that trade benefits poorer groups in states with high infrastructural power or meritocratic bureaucracies.[77] As the bottom-left panel row in Figure 2 shows, poorer groups grow relatively faster as trade openness increases but only in countries with a long history of statehood or medium to high scores of bureacuratic meritocracy. These findings rule out alternative accounts based on endogenous ethnic coalition building yet do not directly investigate the distributional effects between ethno-political insiders and outsiders as stipulated in our theoretical argument.

[Figure 2 about here.]

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,[78] we disagree with the extreme view that political elites can ignore institutions regardless of

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[76]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.

[77]Note that the time-invariant constitutive term of initial nightlights drops from the model.

[78]Pepinsky 2014.

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.[79] One of our measures of state strength, the state antiquity index, predates current developments and makes short-term changes running from ethnic inequality to 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 the period mean instead of initial values (Table A7, Model 4). Finally, 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 A7, 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.[80] For example, political leaders representing industrial interests may close off their economy to shield their allies from global competition while hurting domestic farmers and their representatives who would benefit from closer integration into the world economy.[81] 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.[82] We find limited 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 slightly lowers our

[79]Doner, Ritchie, and Slater 2005.

[80]Adsera and Boix 2002.

[81]Bates 1981.

[82]De Boef and Keele 2008.

confidence in the meritocratic bureaucracy specification (Table A8, Figure A12). Nonetheless, the difference between marginal effects at typically high vs. typically low values of the meritocracy variable remain significant at the 5% interval. 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 $t-1$.[83] Although this bias is likely to be small since our data covers more than 20 years for most groups,[84] 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. Just more than half of the effect occurs instantaneously, while most of the remaining part unfolds over the next 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 (left column) but increases in Mozambique and Iraq (right). Finally, the four narratives help us validate our measurement of ethnic inequality by comparing nightlights emissions to alternative data sources.

[Figure 3 about here.]

[83]Nickell 1981.

[84]Beck and Katz 2011.

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.[85] 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.[86]

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.[87] 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.[88] 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 minorities remain at lower absolute levels.[89] 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

[85]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.

[86]For example, Knight 2014; Clapham 2018.

[87]Sautman 1998.

[88]Fuchang, Chengwei, and Yuan 2016, 10-11.

[89]Hannum and Wang 2012, 158-160.

with economic development.[90]

We now turn to Ethiopia, which "identifies itself as a developmental state" and "is actively engaged in driving developmental efforts."[91] 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.[92] 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 derive 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.[93]

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.[94] China places 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.

[90]Office of the High Commissioner of Human Rights 2018.

[91]Kedir 2014, 11.

[92]Hill and Tsehaye 2015, xvi.

[93]Clapham 2018, 1155.

[94]Fukuyama 2011, 110-39.

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."[95] 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.[96] Although many groups criticize the central government for favoring the Tigry over other ethnic groups, the appointment of Oromo Prime Minister Abiy Ahmed demonstrates that power sharing does not only exist on paper.[97] 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.[98] 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,[99] while benefiting from privatization reforms,[100] and rewarding

[95]Clapham 2018, 1157.

[96]Verhoeven 2016.

[97]Pilling and Barber 2019.

[98]Hanlon and Mosse 2010, 2.

[99]Nuvunga and Sitoe 2013, 118.

[100]Hanlon and Mosse 2010, 3.

their Tonga and Makonde co-ethnics.[101] 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,[102] country experts agree that embezzlement, ethnic patronage, and corruption are common in Mozambique and facilitated by weak state institutions.[103] 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. 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 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.[104] 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.

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[101]Orre and Rønning 2017, 21.

[102]Hanlon and Mosse 2010, 7-10.

[103]For example, Stasavage 1999; Orre and Rønning 2017, X.

[104]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.

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.[105] 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."[106] 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."[107] 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.[108] 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 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,[109] but the Sunnis were marginalized and

[105]Lynch 2014, 12.

[106]Dodge 2013, 245.

[107]International Crisis Group 2011.

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

[109]O'Driscoll 2017, 323-4.

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.[110] 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,

[110] See Piketty 2014.

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.[111] 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.[112] 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.[113] In his anthropological study of peoples in the Southeast Asian highlands, James Scott reminds us that ethnic minorities rarely greet such state-building projects with much enthusiasm.[114] In the most extreme cases, developmental schemes in ethnic minority regions may trigger

[111]Stewart 2008.

[112]Puddington and Roylance 2016, 14-15.

[113]Doner, Ritchie, and Slater 2005.

[114]Scott 2009.

armed conflict between the "sons of the soil" and the state.[115]

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 inter-ethnic 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,[116] 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.

[115]Weiner 1978.

[116]Scheve and Stasavage 2010.

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Tables

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

	(1)	(2)	(3)	(4)
Openness × Excluded	-0.791^{**}	-0.0002	-0.763^{**}	0.019
	(0.240)	(0.098)	(0.262)	(0.122)
Openness × Excluded × State History	1.904^{***}		2.001^{***}	
	(0.544)		(0.556)	
Openness × Excluded × Merit Appoint.		0.211^{*}		0.243^{*}
		(0.103)		(0.099)
State History × Excluded	-1.301^{**}		-1.390^{**}	
	(0.464)		(0.456)	
Merit Appoint. × Excluded		-0.176^{+}		-0.235^{**}
		(0.089)		(0.088)
GDP × Excluded			0.030	0.051^{+}
			(0.035)	(0.031)
Agric. Share × Excluded			-0.006^{+}	-0.004
			(0.003)	(0.003)
Polity IV × Excluded			-0.002	-0.005
			(0.005)	(0.004)
Resource Rents × Excluded			-0.003	-0.003
			(0.002)	(0.003)
Export Conc. × Excluded			0.050	0.058^{+}
			(0.032)	(0.032)
Excluded	0.526^{**}	-0.007	0.186	-0.667
	(0.189)	(0.086)	(0.567)	(0.476)
Conflict Incidence			-0.084	0.022
			(0.085)	(0.027)
$p(B1 = B2)$	0.018	0.073	0.006	0.136
$p(B2 = B3)$	0.002	0.036	0.003	0.006
$p(B1 = B3)$	0.001	0.003	0.000	0.004
Group-FE	Yes	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes
Observations	6,849	5,887	5,769	4,954

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

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

	(5)	(6)	(7)	(8)
Openness × Excluded	−0.797**	0.021		
	(0.253)	(0.100)		
Openness × Excl. × State History	1.927***			
	(0.559)			
Openness × Excl. × Merit Appoint.		0.226*		
		(0.104)		
Openness × Initial Night Lights			−0.237	0.307**
			(0.276)	(0.105)
Openness × Initial NL × State History			1.462**	
			(0.477)	
Openness × Initial NL × Merit Appoint.				0.176**
				(0.055)
State History × Excluded	−1.285**			
	(0.472)			
Merit Appoint. × Excluded		−0.191*		
		(0.093)		
Excluded	0.509*	−0.033		
	(0.198)	(0.089)		
Pre-Upgrade Dummy	−0.059	0.0005		
	(0.095)	(0.041)		
Pre-Upgrade Dummy × State History	0.216			
	(0.319)			
Pre-Upgrade Dummy × Merit Appointments		0.052		
		(0.039)		
Pre-Downgrade Dummy	−0.0005	−0.057		
	(0.259)	(0.128)		
Pre-Downgrade Dummy × State History	−0.198			
	(0.448)			
Pre-Downgrade Dummy × Merit Appointments		0.093		
		(0.090)		
$p(B1 = B2)$	0.038	0.024	0.883	0.000
$p(B2 = B3)$	0.002	0.031	0.000	0.237
$p(B1 = B3)$	0.001	0.002	0.003	0.000
Country-Year FE	Yes	Yes	Yes	Yes
Ethnic Group FE	Yes	Yes	Yes	Yes
Controls	No	No	No	No
Observations	6,471	5,564	6,112	5,326

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

Figures

Figure 1: Marginal effects of interactions in Table 1 from Models 1-2 (top) and 3-4 (bottom). All plots display two types of marginal effects of changes in trade openness on nightlight emissions for excluded groups conditional on the state antiquity index (left) and V-Dem Merit-Based Bureaucracy Index (right). The black lines indicate continuous marginal effects computed directly from the linear model with 95% confidence intervals (shaded areas). The vertical point-ranges display the marginal effects of trade openness along with 95% CIs at the median of each tercile of the institutional proxies.

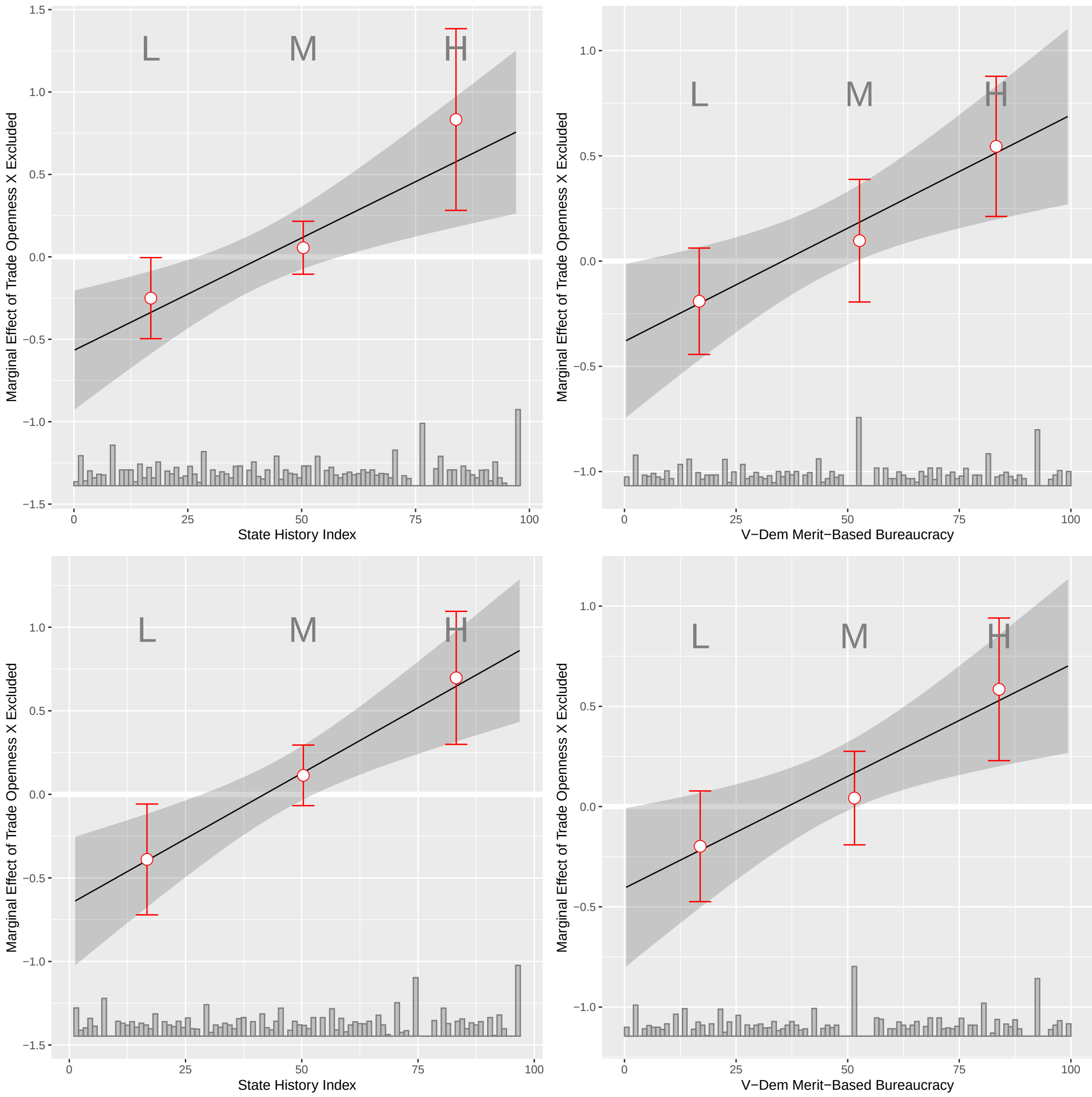


Figure 2: Marginal effects of interactions in Table 2 from Models 5-6 (top) and 7-8 (bottom). All plots display two types of marginal effects of changes in trade openness on nightlight emissions conditional on the state antiquity index (left) and V-Dem Merit-Based Bureaucracy Index (right). The black lines indicate continuous marginal effects computed directly from the linear model with 95% confidence intervals (shaded areas). The vertical point-ranges display the marginal effects of trade openness along with 95% CIs at the median of each tercile of the institutional proxies.

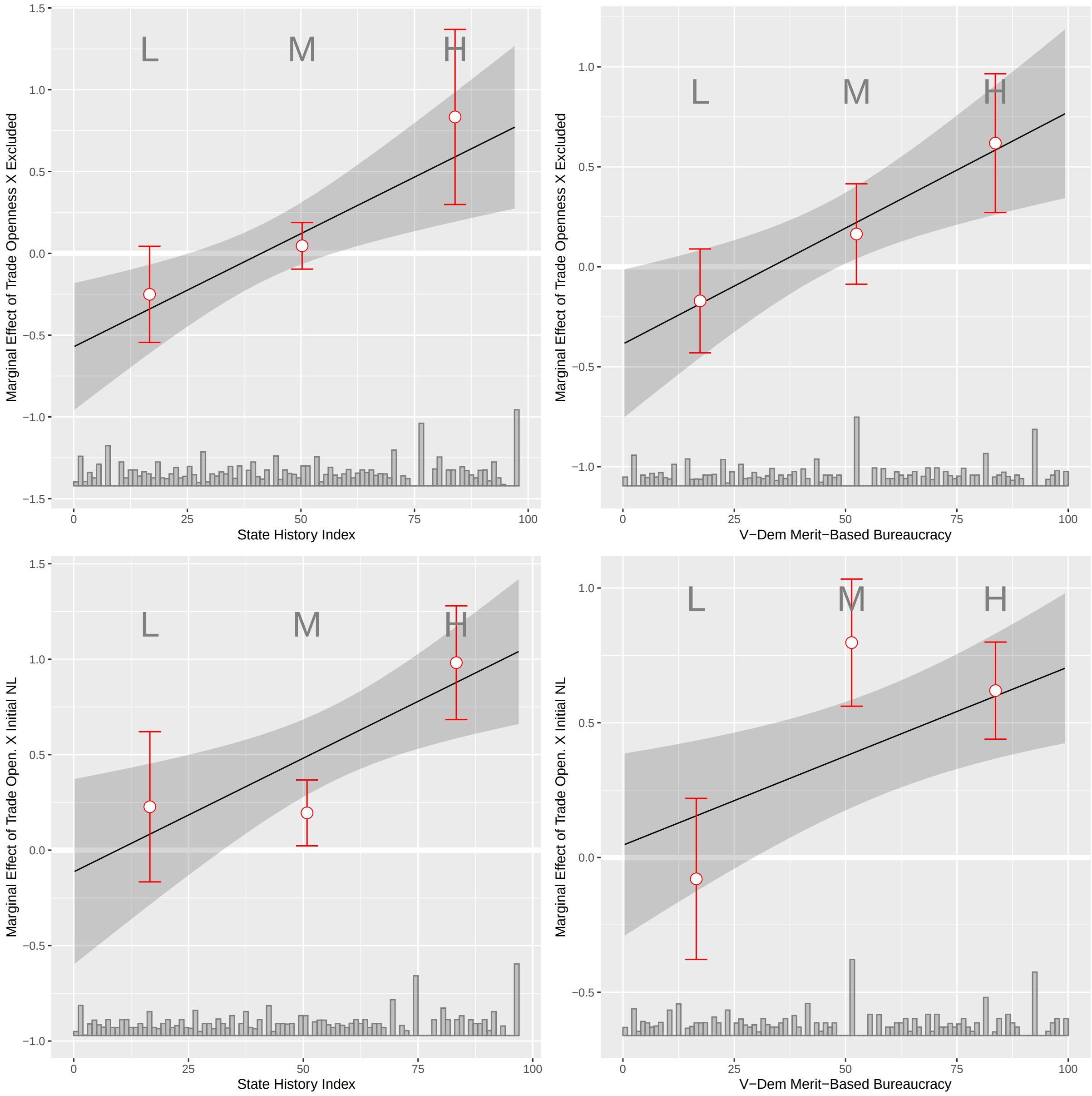
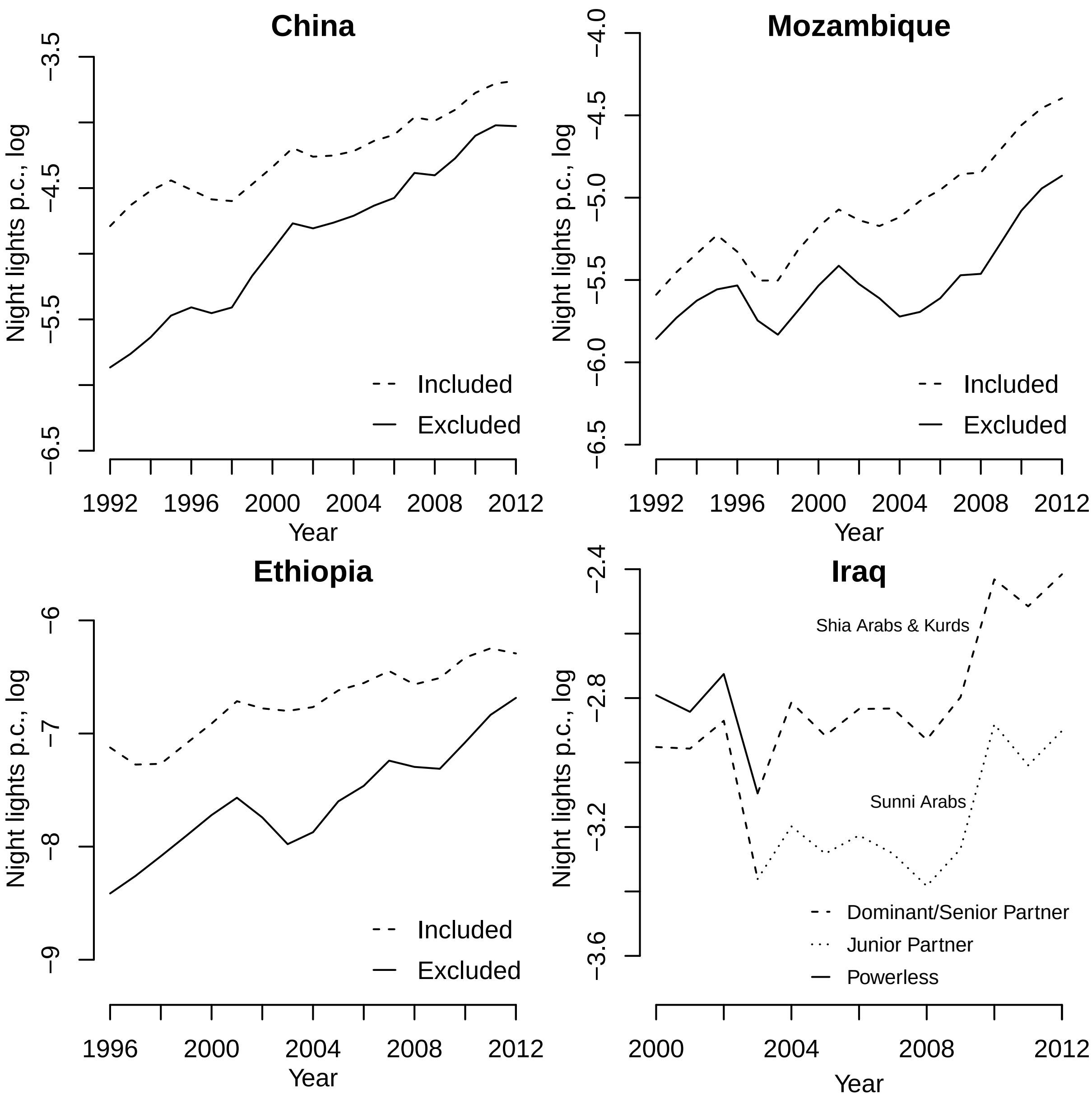


Figure 3: Trends of Ethnic Inequality in Selected Cases, 1992–2012.



Online Appendix for "Globalization, Exclusion and Ethnic Inequality"

Table A1: Summary Statistics

Statistic	N	Mean	St. Dev.	Min	Max
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	0.042	0.327	0	3
Pre-Downgrade Trend	6,909	0.022	0.239	0	3
Trade Openness	6,909	0.650	0.340	0.0002	2.204
Log(GDP p.c.)	6,814	12.162	2.011	7.229	16.581
Polity IV	6,775	2.267	6.481	−10.000	10.000
Agric. Share	6,765	17.416	12.383	0.551	65.175
Resource Rents	6,867	8.588	10.280	0.001	68.778
Export Diversification	6,122	3.232	1.244	1.336	6.411
State History	6,849	0.477	0.222	0.058	0.867
Merit-Based Appointments	5,887	0.446	1.043	−1.981	2.520
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 Dictatorship	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

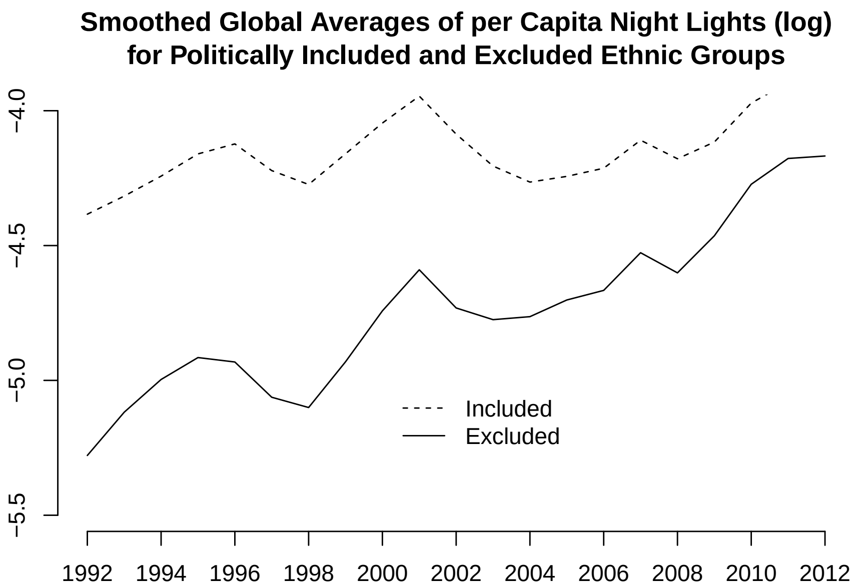


Figure A2: Average Economic Openness and Political Exclusion, 1992–2012.

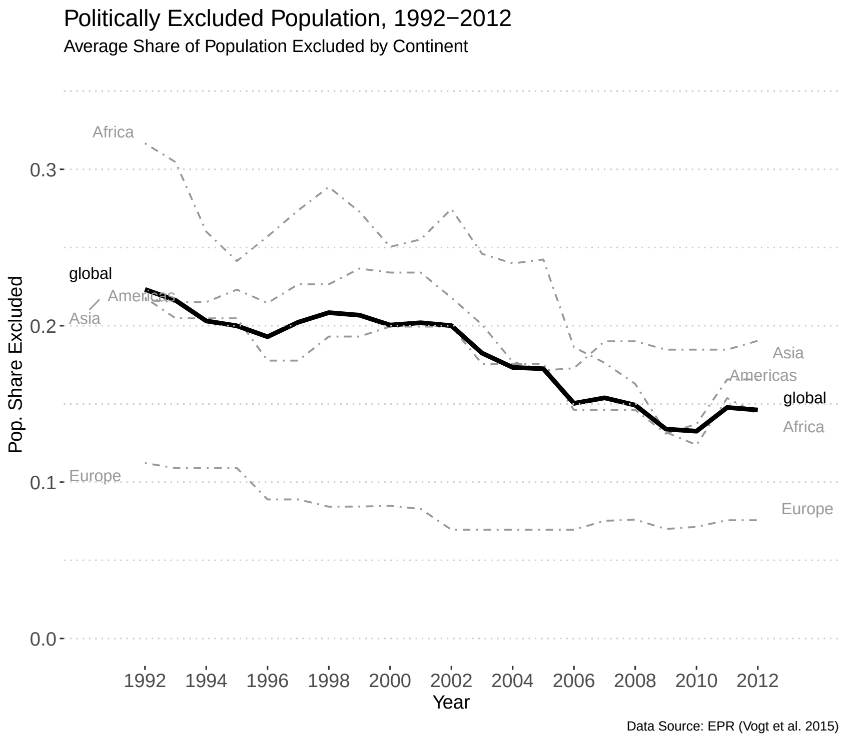


Table A2: Group & Year Fixed Effects

	(1)	(2)	(3)	(4)
Openness	−0.014	0.167	0.136	0.269**
	(0.233)	(0.109)	(0.231)	(0.087)
Openness × Excluded	−1.559***	0.331	−1.158***	0.278
	(0.281)	(0.202)	(0.318)	(0.178)
Openness × Excluded × State History	4.133***		3.296***	
	(0.784)		(0.735)	
Openness × Excluded × Merit Appoint.		0.512**		0.401**
		(0.174)		(0.150)
Openness × State History	0.186		0.151	
	(0.463)		(0.502)	
Openness × Merit Appoint.		−0.059		−0.063
		(0.076)		(0.091)
State History × Excluded	−2.388***		−1.888***	
	(0.600)		(0.500)	
Merit Appoint. × Excluded		−0.464**		−0.397**
		(0.159)		(0.146)
GDP p.c. (log)			0.597***	0.671***
			(0.118)	(0.151)
Polity IV Score			0.002	0.001
			(0.003)	(0.003)
Agric. Share in GDP			−0.003	−0.002
			(0.004)	(0.005)
Resource Rents			0.001	0.001
			(0.003)	(0.003)
Export Diversification			−0.017	−0.084*
			(0.040)	(0.042)
GDP × Excluded			0.039	0.108*
			(0.040)	(0.050)
Polity IV × Excluded			−0.006	−0.003
			(0.004)	(0.005)
Agric. Share × Excluded			−0.004	−0.005
			(0.007)	(0.006)
Resource Rents × Excluded			−0.0004	−0.004
			(0.003)	(0.004)
Export Div. × Excluded			0.082+	0.108+
			(0.048)	(0.063)
Excluded	0.959***	−0.158	0.115	−1.644*
	(0.221)	(0.150)	(0.651)	(0.752)
Conflict Incidence			−0.094	0.026
			(0.090)	(0.039)
Group-FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Country-Year FE	No	No	No	No
Controls	No	No	Yes	Yes
Observations	6,849	5,887	5,769	4,954

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

Group and year fixed effects. Regression equation A1 represents a fully specified triple interaction model that includes all three constitutive terms ($Openness_{ct}$, $Excluded_{ict}$, $\overline{StateCapacity_c}$), the three possible two-way interactions between them, and the triple interaction. We add group (μ_i) and year fixed effects (ρ_t) to account for time-invariant differences between groups and yearly shocks equally affecting all groups in our sample. As the country-level institutional moderator is time-invariant, its coefficient (β_3) cannot be estimated as a consequence of group fixed effects. Table A2 reports coefficient estimates and standard errors from this model (Columns 1 and 2 without controls, Columns 3 and 4 with controls). We are interested in whether temporal variation in trade openness at the country-level differentially affects included and excluded groups at given values of the institutional moderator.

$$\begin{aligned} log(y_{ict}) = \beta_1\, Openness_{ct} + \beta_2\, Excluded_{ict} + \beta_3\, \overline{StateCapacity_c} + \\ \beta_4\, Openness_{ct} \times Excluded_{ict} + \\ \beta_5\, Openness_{ct} \times \overline{StateCapacity_c} + \\ \beta_6\, Excluded_{ict} \times \overline{StateCapacity_c} + \\ \beta_7\, Openness_{ct} \times Excluded_{ict} \times \overline{StateCapacity_c} + \\ + \beta_k\, c_k + \mu_i + \rho_t + \epsilon_{ict} \end{aligned} \tag{A1}$$

$$\frac{d_y}{d_x}\,(Incl.) = \beta_1 + \beta_5 a_c \tag{A2}$$

$$\frac{d_y}{d_x}\,(Excl.) = \beta_1 + \beta_4 + \beta_5 a_c + \beta_7 a_c \tag{A3}$$

$$\frac{d_y}{d_x}\,(Excl.) - \frac{d_y}{d_x}\,(Incl.) = \beta_4 + \beta_7 a_c \tag{A4}$$

This requires (i) calculating marginal effects of trade openness on logged nightlights of included and excluded groups at value a_c of the institutions variable and then, (ii) calculating the difference between these two marginal effects at value a_c. The marginal effect for included groups is defined as the partial derivative of the dependent variable with respect to trade openness with $Excluded_{ict}$ set to zero and $\overline{StateCapacity_c}$ set to a_c. This boils down to the sum of β_1 and the product $\beta_5 a_c$ (Equation A2). The marginal effect for excluded groups is the same partial derivative but now with the exclusion dummy set to one, which implies adding β_4 and $\beta_7 a_c$ to $\beta_1 + \beta_5 a_c$ (Equation A3). The difference in marginal effects between excluded and included groups is therefore simply $\beta_4 + \beta_7 a_c$. This difference can be interpreted as the effect of increasing trade openness on the nightlights gap between the average included and excluded group. Wherever included groups are, on average, richer than excluded ones, and $\beta_4 + \beta_7 a_c$ is positive (negative), increasing trade openness narrows (widens) the economic gap between included and excluded groups.

Figures A3 and A4 plot the marginal effects for included and excluded groups (Equations A2 and A2) as well as the difference between these marginal effects (Equation A4) across the observed ranges of our two institutional moderators (Figure A3 is based on models without control variables, whereas Figure A4 includes them). Across all four specifications, the marginal effect of trade openness on excluded group's nightlights is increasing with institutional quality, while the effect for included groups remains constant and very close to zero. As a result, the difference in marginal effects between excluded and included groups is negative at low values of institutional quality, increases along the range of our institutional moderators, and becomes positive and significant at high values. As explained above, we interpret these patterns as evidence that temporal increases in trade openness narrow the economic gap between ethnopolitical insiders and outsiders in strongly institutionalized states but have no effect or even widen ethnic inequality under weak institutions.

Relationship to our baseline models. The additional inclusion of country-year fixed effects in our baseline models nets out all temporal shocks and time-varying variables at the country level. The constitutive terms and two-way interactions without any variation below the country-level accordingly drop from the model ($\beta_1\, Openness_{ct}$ and $\beta_5\, Openness_{ct} \times \overline{StateCapacity_c}$). The only remaining terms relevant for computing marginal effects of trade openness are now $\beta_4\, Openness_{ct} \times Excluded_{ict}$ and $\beta_7\, Openness_{ct} \times Excluded_{ict} \times \overline{StateCapacity_c}$. In other words, the model with group and country-year fixed effects more directly gets at the difference in marginal effects between excluded and included groups, as the average included group in a given country-year now serves as baseline category. Separate marginal effects for excluded and included groups can no longer be derived; only their relative difference at institutional value a_c which, as before, boils down to $\beta_4 + \beta_7 a_c$.

Figure A3: Group & Year Fixed Effects: Marginal effects of trade openness on night-light emissions of excluded and included groups across observed range of state antiquity index (top-left) and V-Dem Merit-Based Bureaucracy index (bottom-left). Difference in marginal effect between excluded and included groups (right). Based on Table A2. Model 1 (state antiquity) in top row. Model 2 (merit apppointments) in bottom row. Shaded areas indicate 95% confidence intervals.

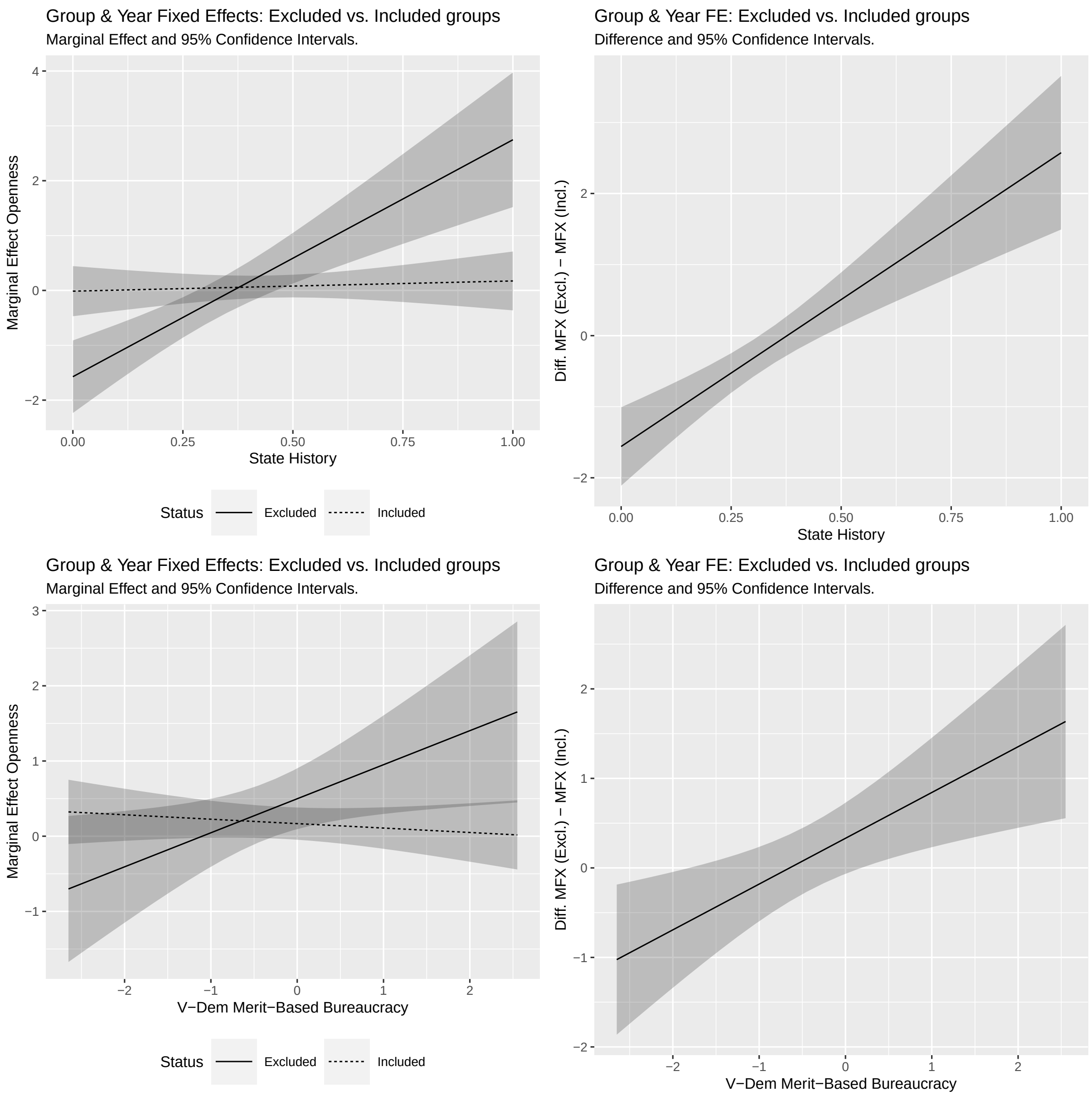


Figure A4: Group & Year Fixed Effects & Controls: Marginal effects of trade openness on nightlight emissions of excluded and included groups across observed range of state antiquity index (top-left) and V-Dem Merit-Based Bureaucracy index (bottom-left). Difference in marginal effect between excluded and included groups (right). Based on Table A2. Model 3 (state antiquity) in top row. Model 4 (merit appointments) in bottom row. Shaded areas indicate 95% confidence intervals.

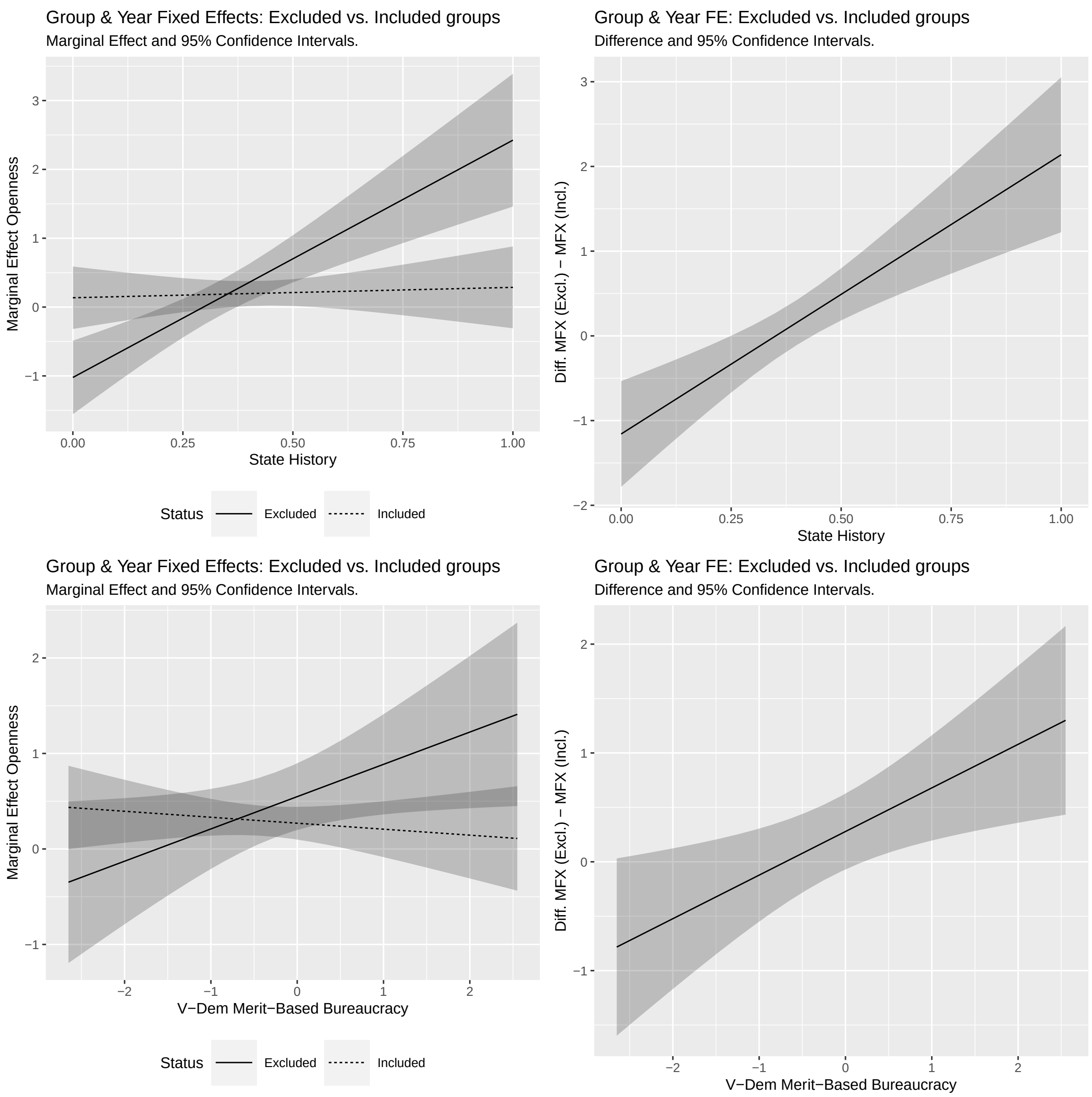


Table A3: Split Sample at Median of Institutional Moderators

	State Age		Merit. Bureauc.	
	(1)	(2)	(3)	(4)
Openness × Excluded	−0.235*	0.450**	−0.200	0.440**
	(0.109)	(0.147)	(0.135)	(0.134)
GDP × Excluded	−0.050	0.191**	−0.017	0.150**
	(0.035)	(0.055)	(0.038)	(0.043)
Agric. Share × Excluded	−0.009*	−0.006	−0.007+	0.002
	(0.004)	(0.005)	(0.004)	(0.005)
Polity IV × Excluded	−0.002	−0.001	−0.010*	0.001
	(0.007)	(0.005)	(0.004)	(0.010)
Resource Rents × Excluded	−0.001	−0.007*	−0.001	0.003
	(0.003)	(0.003)	(0.003)	(0.004)
Export Div. × Excluded	0.007	0.076+	0.025	0.047
	(0.037)	(0.044)	(0.026)	(0.046)
Excluded	0.876	−2.745***	0.429	−2.245***
	(0.545)	(0.747)	(0.565)	(0.481)
Conflict Incidence	0.058	−0.145	0.036	−0.001
	(0.059)	(0.098)	(0.044)	(0.020)
Group-FE	Yes	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes	Yes
Controls	No	No	No	No
Observations	2,978	2,791	2,733	2,221

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

Group and country-year fixed effects (split samples). As additional test, 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 A3 show that the interaction between within-country changes in trade openness and political exclusion is negative 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.

Fixed effects and temporal variation in the interaction terms. We include group fixed effect in all models to ensure that effects are only identified from temporal variation in trade openness within countries (group fixed effects nest country fixed effects as groups are nested within countries). This strategy faces limitations when interacting trade openness with time-varying moderators (such as political exclusion and, in its raw form, the VDEM meritocracy variable).

Consider the case of a hypothetical country A with a constant trade-to-gdp ratio of 0.8 and constantly high institutional quality 1 throughout our observation period. A constantly excluded ethnic group i ($Excluded_{ict} = 1$ in all observation years) is unproblematic. The two-way interaction term $Openness_{ct} \times Excluded_{ict}$ and the triple interaction $Openness_{ct} \times Excluded_{ict} \times \overline{StateCapacity_c}$ remain constantly valued at 0.8. As group fixed effects demean all variables with respect to the group-specific period average, both of these interactions are effectively zero across all years and do not contribute any variation to our estimates.

The situation is different for ethnic group j which is politically included in the first half of our observation period, but excluded thereafter. Both interaction terms are 0 for the first half of years but rise to 0.8 in the second half of years. The period mean of both interaction terms is 0.4 and demeaning by fixed effects thus implies a sharp increase from -0.4 to 0.4 with the onset of political exclusion in the second half of our observation period. All variation now comes from within-group changes in political status rather than from within-country changes in trade openness. Similar problems may arise by including the time-variant version of our meritocracy variable. As such, temporal variation in political exclusion and meritocracy may contaminate tests of our hypothesis that *increasing* trade openness differentially affects excluded and included groups at different *levels* of institutional quality. We do not expect group j's exclusion from political power to be associated with large and sudden economic gains just because country A has comparatively high levels of trade openness. Nor do we expect small temporal increases in institutional quality to massively benefit excluded groups in open as compared to closed economies.

Temporal variation in the moderators not only complicates the interpretation of estimates as evidence for or against our hypotheses, but also compromises the inferential benefits of our fixed effects strategy. In the example of group j above, all identifying variation comes from temporal changes in exclusion interacted with *levels* of trade. As a result, cross-country variation in trade openness creeps back into the model and we face, at least partially, the same concerns about unobserved heterogeneity as in specifications without fixed effects. In addition, year-to-year changes in political status may understandably be seen as more endogeneous than increasing trade openness during a global wave of economic integration. Much the same applies to temporal changes in VDEM-based institutional variables which may, on top, suffer from measurement error or even ex-post rationalizations of recent economic performance or inequality trends by country experts (Glaeser, La Porta, Lopez-de Silanes et al. 2004).

We address these problems in various ways. First, we keep the value of the VDEM meritocracy variable constant across all specifications—either at the initial value for each country (e.g. Table 1) or at the country-specific period mean between 1992 and 2012 (Model 4 Table A7 below). Second, we run models that use a subsample of ethnic groups with no temporal changes in power status (Models 3 and 4 in Table A6 below) or assign each group its initial value of exclusion (Models 1 and 2 in Table A7 below). We keep the potentially problematic time-varying exclusion dummy in our main specifications, as year-to-year changes in political status are rare (they occur in only 1.3% of the group-years in our sample.)

We perform one additional test that minimizes the problems laid out above by decomposing the trade openness variable into its between-country (country-specific period mean) and within-country components (difference between country-year value and country-specific period mean). We then run models that include both the within and the between component as constitutive terms and their interactions with the (minimally) time-varying political exclusion dummy and our institutional moderators (constant state history and time-varying meritocracy). This results in the following specification where $Openness_{ct}(\Delta)$ denotes the within-country component in trade and $Openness_{c}()$ refers to the country specific period mean:

$$\begin{aligned} log(y_{ict}) = \; & \beta_1 \, Openness_{ct}(\Delta) + \beta_2 \, Excluded_{ict} + \beta_3 \, StateCapacity_{ct} + \\ & \beta_4 \, Openness_{ct}(\Delta) \times Excluded_{ict} + \\ & \beta_5 \, Openness_{ct}(\Delta) \times StateCapacity_{ct} + \\ & \beta_6 \, Excluded_{ict} \times StateCapacity_{ct} + \\ & \beta_7 \, Openness_{ct}(\Delta) \times Excluded_{ict} \times StateCapacity_{ct} + \\ & \beta_8 \, Openness_{c}() + \\ & \beta_9 \, Openness_{c}() \times Excluded_{ict} + \\ & \beta_{10} \, Openness_{c}() \times StateCapacity_{ct} + \\ & \beta_{11} \, Openness_{c}() \times Excluded_{ict} \times StateCapacity_{ct} + \\ & \mu_i + \rho_{(c)t} + \epsilon_{ict} \end{aligned} \tag{A5}$$

We estimate this model with group fixed effects (μ_i) and either year (ρ_t) or country-year fixed effects (ρ_{ct}) Table A4 reports the resulting coefficient estimates (Columns 1 and 3 with state history and columns 2 and 4 with meritocracy as institutional moderator). Coefficients and standard errors that cannot be estimated due to the group or country-year fixed effects are labelled as NA. The relevant marginal effects, and differences in marginal effects can be calculated in exactly the same way as specified above in equations A2, A3, and A4. The key difference is that, now, only within-country variation in trade openness over time contributes to these estimates, regardless of any temporal variation in exclusion and/or meritocracy scores. Figures A5 and A6 summarize these quantities of interest and can be directly compared to Figure A3 and 1 (top panels), respectively. The marginal effects for excluded groups and their difference to those for included groups increase even faster across the range of institutional quality than before, especially for the meritocracy moderator. The last row in Table A4 (Columns 2 and 4) indicates why this may be the case: the triple interaction between the between component of openness, exclusion, and meritocracy is negative and significant at about half the size of the interaction term with the within component. Where between-country variation contributes identifying variation to the interaction term(s), as in the more conventional models presented above, it may thus partially offset the effects based on within-country variation alone.

Table A4: Within-Between Decomposition of Trade Openness

	(1)	(2)	(3)	(4)
Openness (Δ)	0.016	0.036	NA	NA
	(0.222)	(0.101)	(NA)	(NA)
Excluded	0.641	0.701**	0.236	0.237
	(0.544)	(0.241)	(0.344)	(0.154)
Merit Appointments		0.202+		NA
		(0.119)		(NA)
Openness (Δ) × Excluded	−1.586***	0.539**	−0.976**	0.099
	(0.294)	(0.192)	(0.320)	(0.101)
Openness (Δ) × Excluded × State History	4.321***		2.260***	
	(0.697)		(0.589)	
Openness (Δ) × Excluded × Merit Appoint.		0.582***		0.262*
		(0.147)		(0.103)
Openness (Δ) × State History	0.051		NA	
	(0.446)		(NA)	
Openness (Δ) × Merit Appoint.		−0.095		NA
		(0.070)		(NA)
Excluded × State History	−1.237		−0.627	
	(1.742)		(1.116)	
Openness (∅) × Excluded × State History	1.908		0.595	
	(2.867)		(1.907)	
Excluded × Merit Appoint.		0.001		0.019
		(0.062)		(0.043)
Openness (∅) × Excluded × Merit Appoint.		−0.188**		−0.096*
		(0.058)		(0.040)
Openness (∅)	NA	NA	NA	NA
	(NA)	(NA)	(NA)	(NA)
Openness (∅) × Excluded	−0.940	−1.093**	−0.264	−0.419+
	(0.895)	(0.354)	(0.598)	(0.248)
Openness (∅) × State History	NA		NA	
	(NA)		(NA)	
Openness (∅) × Merit Appoint.		−0.217		NA
		(0.155)		(NA)
State History	NA		NA	
	(NA)		(NA)	
$p(B1 = B2)$	—	—	0.125	0.158
$p(B2 = B3)$	—	—	0.001	0.289
$p(B1 = B3)$	—	—	0.001	0.003
Group-FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	—	—
Country-Year FE	No	No	Yes	Yes
Controls	No	No	No	No
Observations	6,849	5,887	6,849	5,887

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

Across the board, results from alternative fixed effects models, the split sample analysis, and the within-between approach lead to similar conclusions as our baseline specifications. Within-country 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 these alternative specifications.

Figure A5: Within-Between Models with Group & Year Fixed Effects: Marginal effects of within-country changes in trade openness on nightlight emissions of excluded and included groups across observed range of state antiquity index (top-left) and V-Dem Merit-Based Bureaucracy index (bottom-left). Difference in marginal effect between excluded and included groups (right). Based on Table A4. Model 1 (state antiquity) in top row. Model 2 (merit apppointments) in bottom row. Shaded areas indicate 95% confidence intervals.

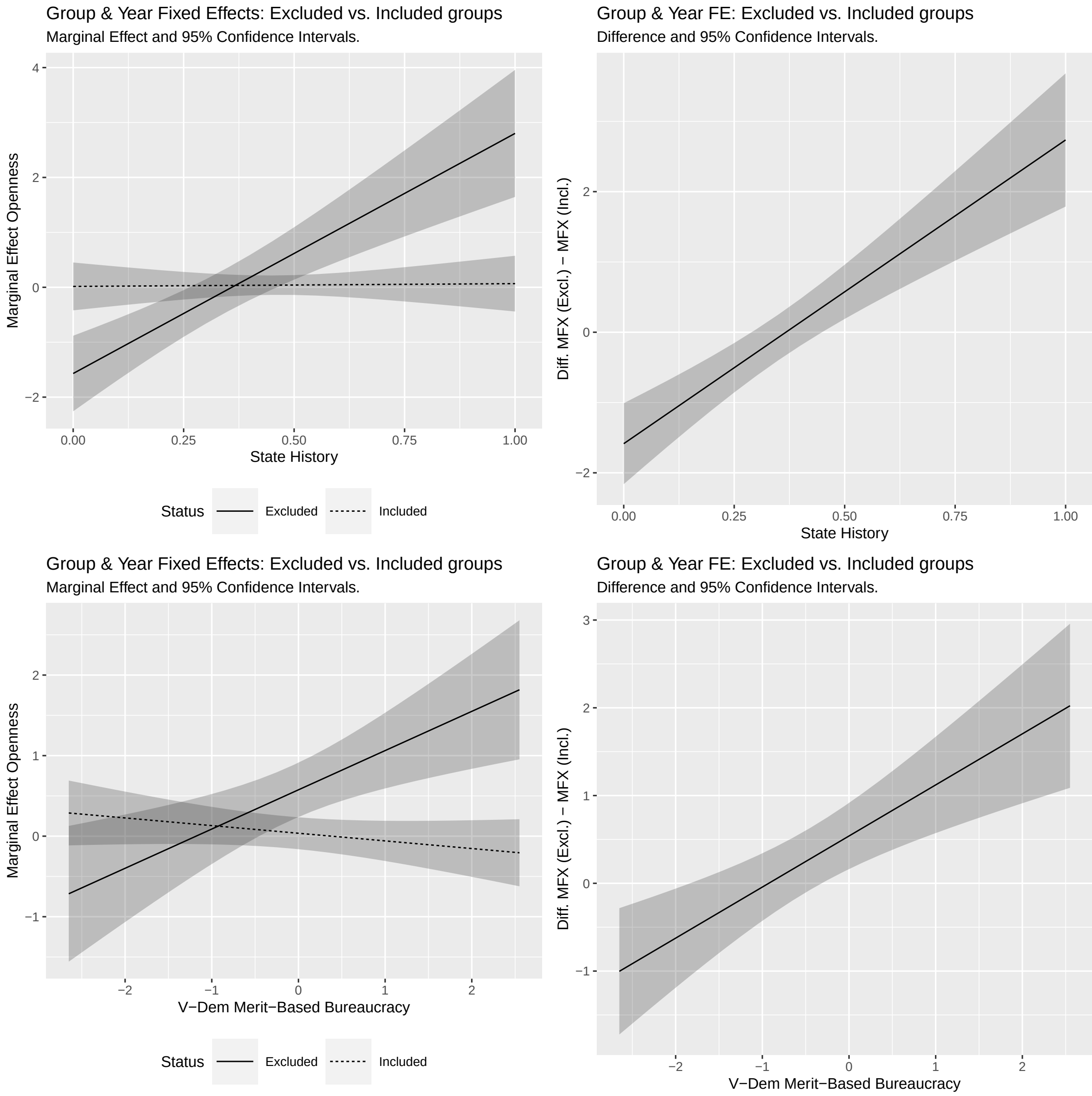
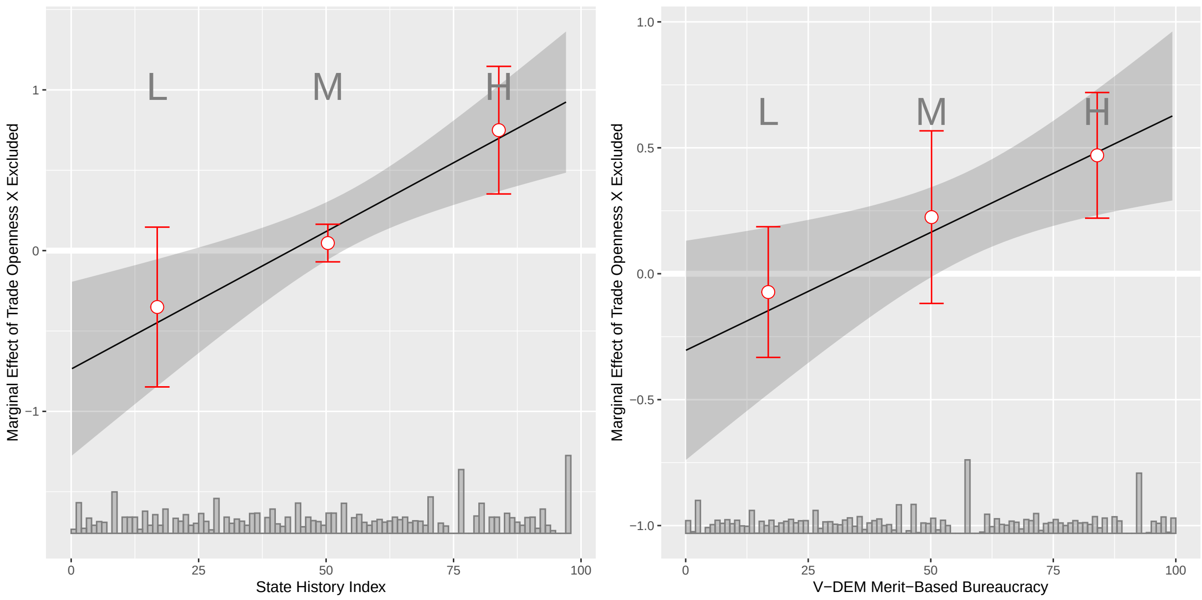


Figure A6: Within-Between Models with Group & Country-Year Fixed Effects: Marginal effects of trade openness on nightlight emissions of excluded groups conditional on state antiquity index (left) and V-Dem Merit-Based Bureaucracy Index (right). Binning estimates (Hainmueller, Mummolo, and Xu 2019) as points on top. Based on Table A4 (Model 3 left, Model 4 right). Shaded areas and error bars 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 (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, Wallensteen, Eriksson et al. 2002; Themnér and Wallensteen 2014; Wucherpfennig, Metternich, Cederman 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.

Most controls exhibit the expected sign. Faster growth and diversified export protfolios seem to benefit excluded groups whereas increasing shares of agriculture in national income points in the opposite direction. Note however, that only the export diversification interaction reaches statistical significance. The conflict dummy is negatively signed bit insignificant. The democracy and resource rent interactions remain close to zero, insignificant, and switch signs between specifications. More importantly, however, the inclusion of these variables does not affect our main results (Table 1 in the main text).

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 ro titular nation, 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 A5).

Table A5: Controlling for Size of Largest Group.

	(1)	(2)
Openness × Excluded	−1.024**	−0.653*
	(0.325)	(0.256)
Openness × Excl. × State History	1.763**	
	(0.546)	
Openness × Excl. × Merit Appoint.		0.164*
		(0.082)
Openness × Excl. × Max. Group Size	0.508	1.136**
	(0.468)	(0.408)
Openness × Excluded	−1.216*	
	(0.505)	
State History × Excluded		−0.120
		(0.090)
Merit Appointments × Excluded	−0.447	−1.109**
	(0.453)	(0.368)
Max. Group Size × Excluded	0.723**	0.559*
	(0.263)	(0.222)
Country-Year FE	Yes	Yes
Ethnic Group FE	Yes	Yes
Observations	6,849	5,887

Standard errors clustered on country and year 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 A5. Shaded areas and error bars indicate 95% confidence intervals.

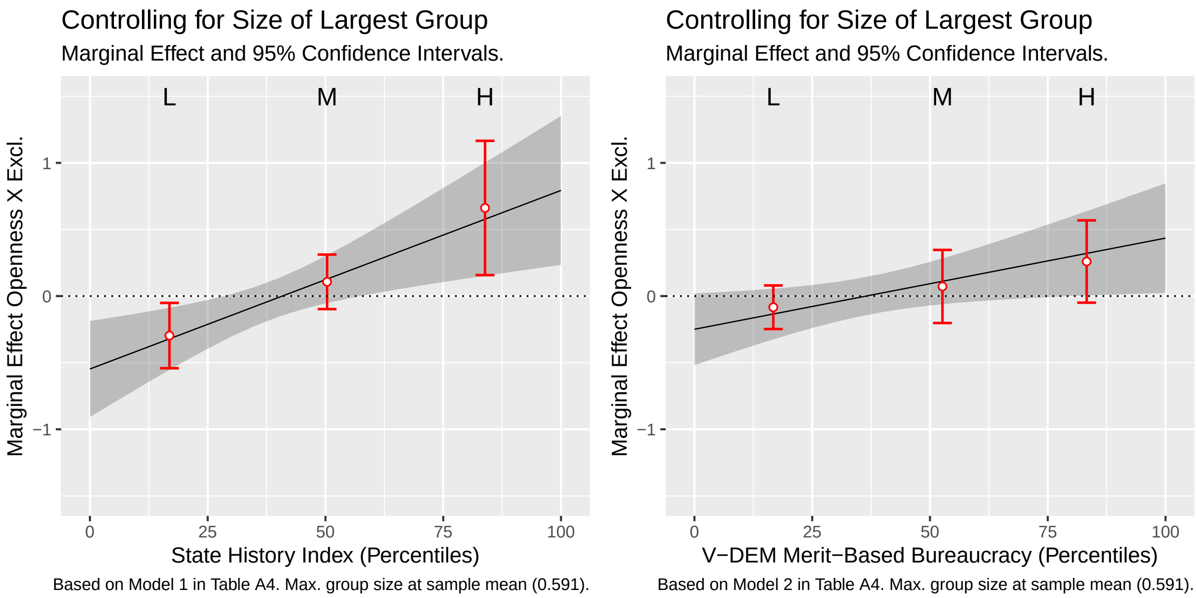


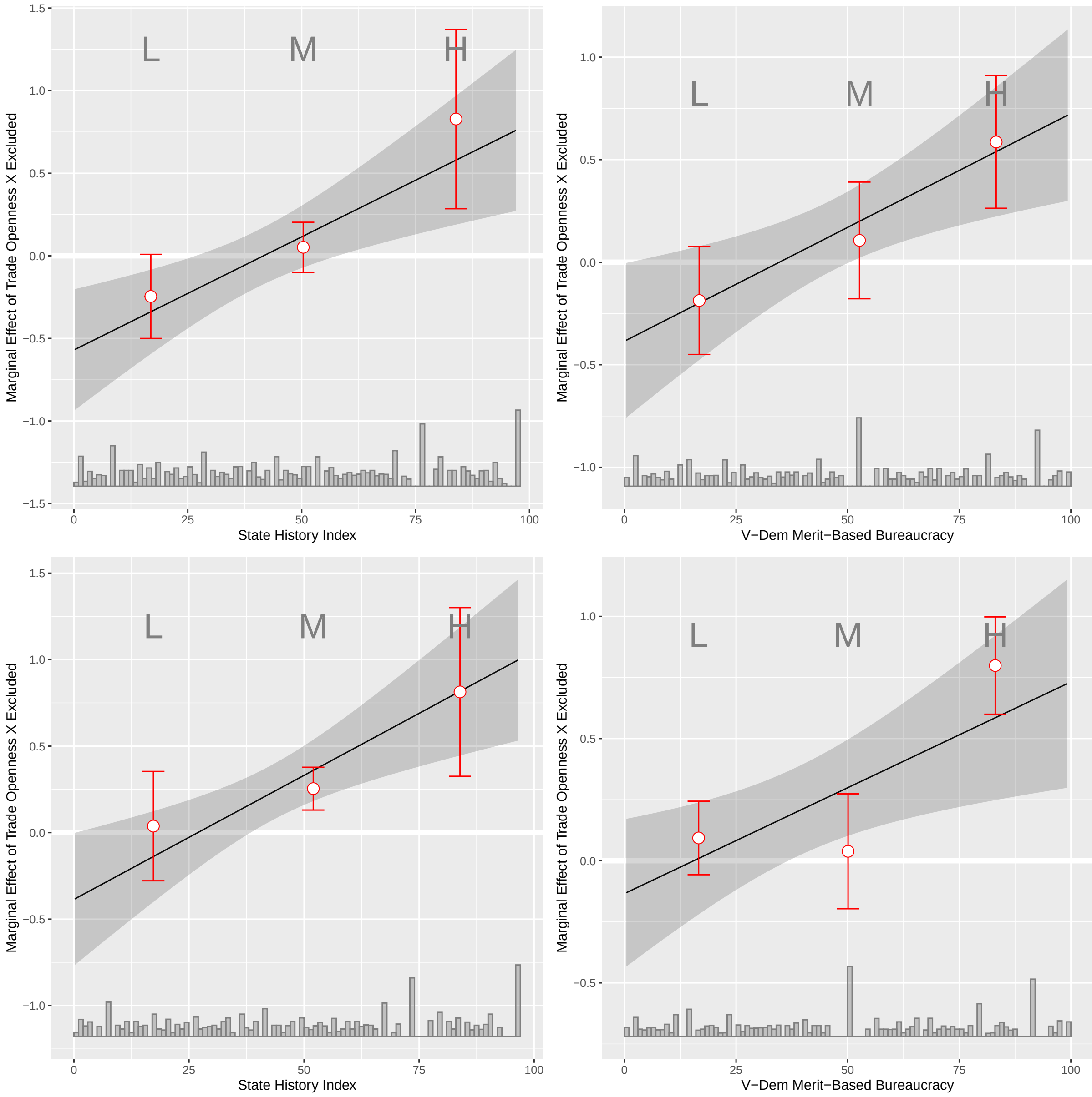
Table A6: Endogeneity of Political Status to Economic Performance?

	(1)	(2)	(3)	(4)
Openness × Excluded	-0.792^{**}	0.001	-0.625^{**}	0.216^{*}
	(0.240)	(0.099)	(0.230)	(0.099)
Openness × Excl. × State History	1.904^{***}		1.894^{***}	
	(0.541)		(0.480)	
Openness × Excl. × Merit Appoint.		0.213^{*}		0.189^{*}
		(0.105)		(0.088)
State History × Excluded	-1.368^{**}			
	(0.507)			
Merit Appointments × Excluded		-0.170^{+}		
		(0.102)		
Excluded	0.541^{**}	-0.014		
	(0.199)	(0.094)		
Pre-Upgrade Trend	-0.019	0.0001		
	(0.036)	(0.014)		
Pre-Upgrade Trend × State History	0.080			
	(0.116)			
Pre-Upgrade Trend × Merit Appointments		0.017		
		(0.017)		
Pre-Downgrade Trend	0.024	-0.012		
	(0.077)	(0.035)		
Pre-Downgrade Trend × State History	-0.121			
	(0.151)			
Pre-Downgrade Trend × Merit Appointments		0.040		
		(0.025)		
Country-Year FE	Yes	Yes	Yes	Yes
Ethnic Group FE	Yes	Yes	Yes	Yes
Observations	6,849	5,887	5,715	4,893

Standard errors clustered on country and year 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 downgrade 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 (exclusion) of groups already on the rise has to be more common in weakly (strongly) institutionalized countries. Therefore, we interact the pre- and post-trends with our institutional proxies (Table A6, columns 1 and 2). The coefficients on the trend variables and their interaction terms remain substantively small and statistically indistinguishable from zero in the state history model. Note that we only observe 59 upgrades to and 31 downgrades from political power in our sample (i.e. in less than 1% and 0.5% of all group-years). The results for our main terms of interest in these specifications remain practically indistinguishable from our baseline models (for marginal

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 A6. 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.



effects and binning estimates, see top row of Figure A8).

Nonetheless, we want to rule out that temporal changes in political power status drive any of our findings, and therefore implement two additional specifications. First, 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 A6). As the marginal effects and binning plots in the bottom row of Figure A8 suggest, our result hold 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 A7). 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 A7 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.

Model 4 in Table A7 assigns each country the period mean across all sample years of the VDEM meritocracy proxy instead of using the 1991 value. We want to make sure that our results are not due a somewhat arbitrary choice of how to make this variable time-invariant. The Wald tests of the difference between the high and medium and low bins (bottom of in Table A7) as well as the marginal effects and binning plots in Figure A10 show that our results remain robust to using pre-period values 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 A7). 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, although only at the 10% level for meritocracy (bottom row in Figure A9).

Table A7: Additional Robustness Checks

	(1)	(2)	(3)	(4)
Openness × Excluded (91)	-0.730^{+}	0.187		
	(0.375)	(0.118)		
Openness × Excluded (91) × State History	1.957^{**}			
	(0.686)			
Openness × Excluded (91) × Merit Appoint.		0.179		
		(0.126)		
Openness × Excluded			-0.787^{***}	0.082
			(0.206)	(0.111)
Openness × Excluded × State History			0.007^{**}	
			(0.003)	
Openness × Excluded × Merit Appoint.			0.011^{***}	
			(0.003)	
Openness × Excluded × Merit Appoint. (Ø)				0.173
				(0.108)
State History × Excluded			-0.007^{**}	
			(0.002)	
Merit Appoint. × Excluded			-0.006^{*}	
			(0.003)	
Merit Appoint. (Ø) × Excluded				−0.025
				(0.108)
Excluded			0.548^{**}	−0.088
			(0.184)	(0.114)
$p(B1 = B2)$	0.431	0.973	0.028(S) 0.067(M)	0.195
$p(B2 = B3)$	0.009	0.009	0.096(S) 0.018(M)	0.024
$p(B1 = B3)$	0.012	0.006	0.011(S) 0.015(M)	0.007
Group-FE	Yes	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes	Yes
Controls	No	No	No	No
Observations	6,445	5,660	5,838	6,909

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 A7. 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.

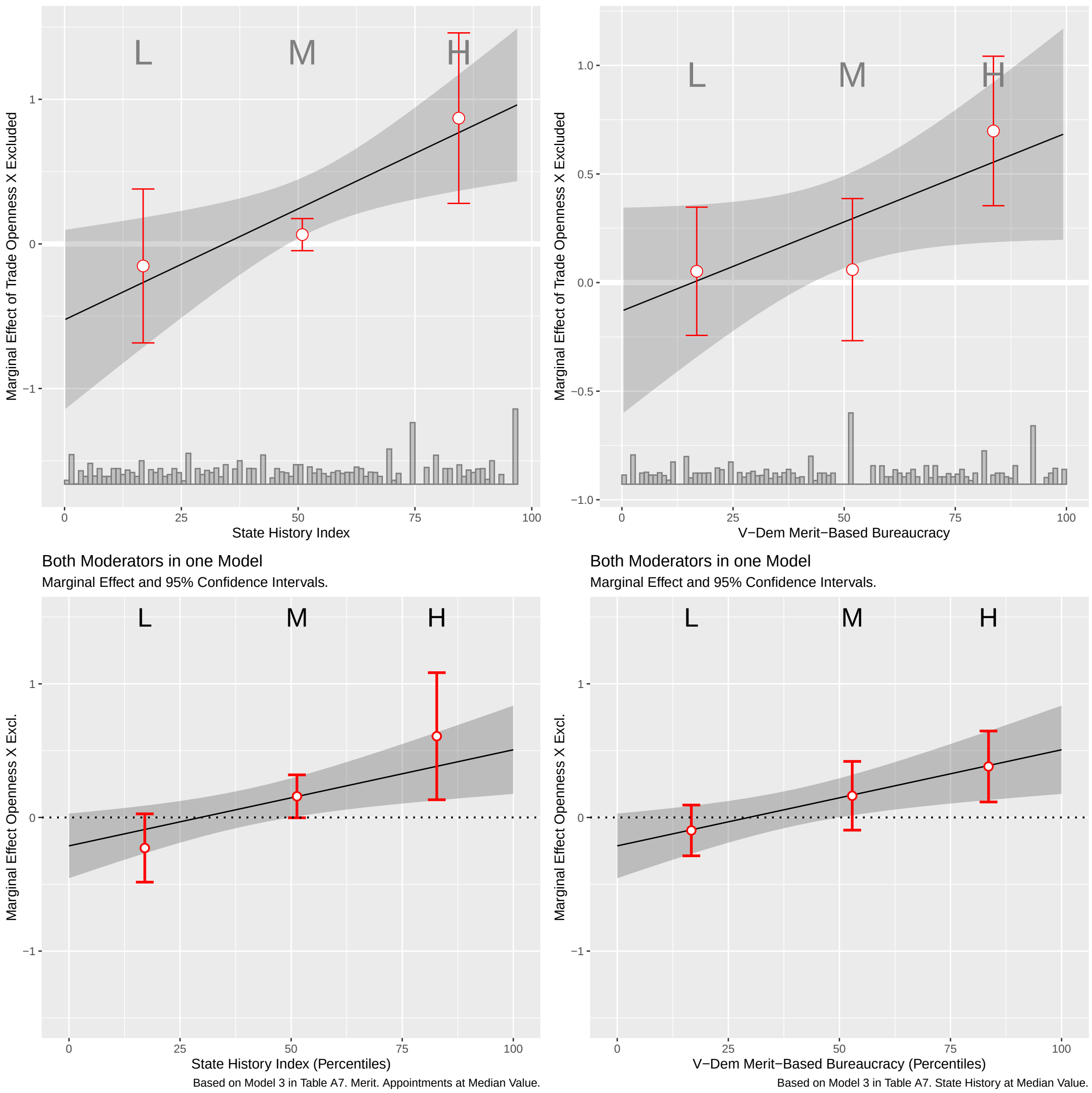
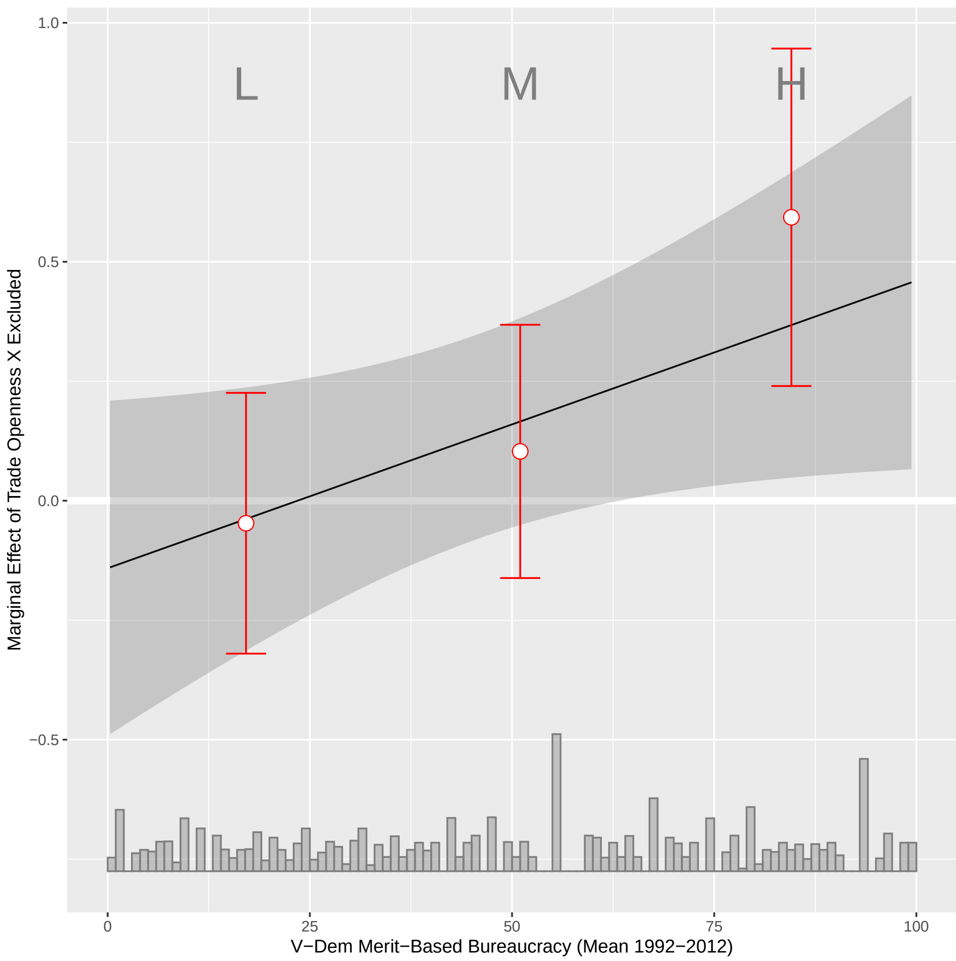


Figure A10: Marginal effect of trade openness on nightlight emissions of excluded groups across percentiles of the period mean (1992-2012) of the V-Dem Merit-Based Bureaucracy Index. Binning estimates (Hainmueller, Mummolo, and Xu 2019) as points on top. Based on Model 4 in Table A7. Shaded areas and 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 A8 implement an autoregressive distributed lag (ADL) models that adds one-year lags of all predictors and the dependent variable to our baseline specifications. None of the lags of our explanatory variables in Models 1 and 2 reach statistical significance, although in Model 2, the lagged triple interaction terms is much larger than the contemporaneous one. Wald tests of joint significance of all lagged explanatory variables yield p-values of 0.98 (Model 1) and 0.03 (Model 2). Failing to reject the null hypothesis of no difference in one case leads us to also 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. The binning estimates in the right panel of Figure A11 however still indicate a positive and significant marginal effect at high levels of meritocracy that is significantly different from those at medium and low levels at the 10% and 5% intervals, respectively. The estimated interaction effect for state antiquity remain different from zero at low ($p<0.1$) and high levels ($p<0.05$) 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 coefficient on the variable of interest, and α the estimated coefficient of the lagged outcome variable.[117] The long-run dynamic effects in the ADL and LDV models are somewhat smaller than the static effects in our baseline models (Models 1-2 in Table 1), especially for the meritocracy moderator. The dynamic models also allow to calculate how the long-run effects materialize over time. For state antiquity set to the 90$^{\text{th}}$ percentile of the observed distribution 55.7% of the effect of trade openness on excluded groups occur instantaneously, 24.67% occur in year 2, 10.93% in year 3, and 4.84% in

[117] In the ADL model the long-run multiplier effect is $\frac{\beta_0+\beta_1}{1-\alpha}$, where β_0 captures the contemporaneous effect of a variable of interest, and β_1 the one-year lag effect.

Table A8: Autoregressive Distributed Lag and Partial Adjustment Models.

	(1)	(2)	(3)	(4)
Openness × Excluded	−0.393**	−0.042	−0.410**	−0.032
	(0.124)	(0.057)	(0.138)	(0.059)
Openness × Excl. × State History	0.884**		0.904**	
	(0.279)		(0.275)	
Openness × Excl. × Merit Appoint.		0.013		0.089
		(0.062)		(0.056)
Openness × Excluded (t-1)	−0.030	−0.005		
	(0.143)	(0.071)		
Openness × Excl. × State History (t-1)	0.053			
	(0.284)			
Openness × Excl. × Merit Appoint. (t-1)		0.078		
		(0.067)		
State History × Excluded	−0.469*		−0.519*	
	(0.209)		(0.220)	
State History × Excluded (t-1)	−0.044			
	(0.192)			
Merit Appoint. (Ø) × Excluded		−0.088+		−0.067
		(0.052)		(0.051)
Merit Appoint. (Ø) × Excluded (t-1)		0.075		
		(0.048)		
Exclusion	0.198+	0.004	0.214*	−0.004
	(0.118)	(0.056)	(0.107)	(0.050)
Exclusion (t-1)	0.003	−0.021		
	(0.115)	(0.054)		
Night Lights (log, t-1)	0.446***	0.433***	0.443***	0.426***
	(0.055)	(0.069)	(0.053)	(0.067)
Ethnic Group FE	Yes	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes	Yes
Observations	6,472	5,561	6,520	5,604

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

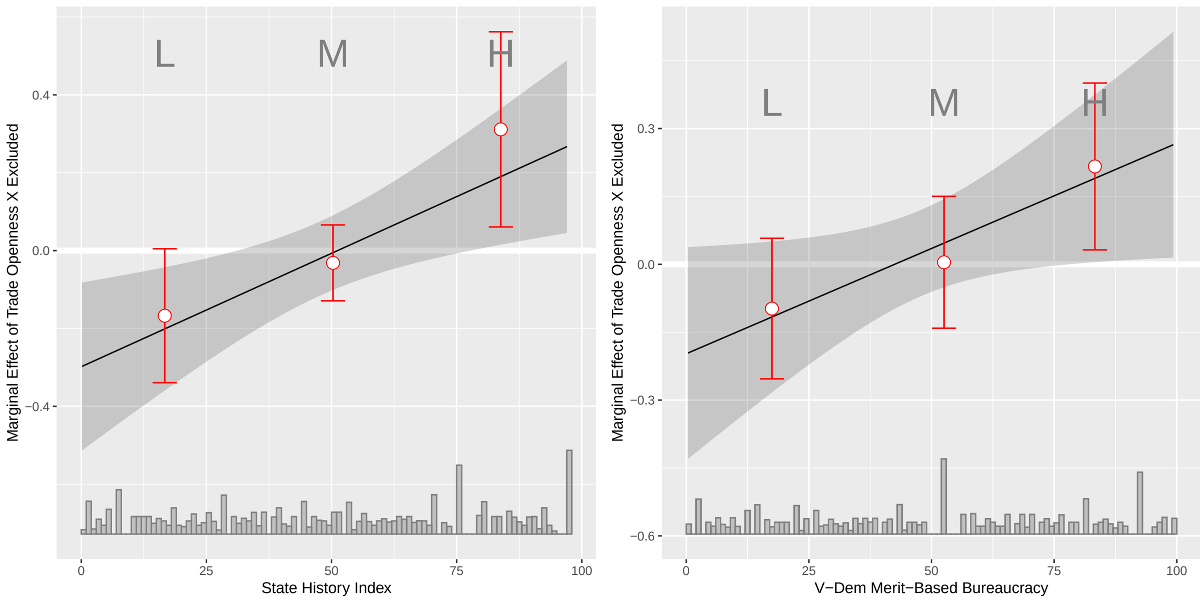
Table A9: Short and Long-Run Effects from Dynamic Models

Moderator at 90th percentile	State History		Merit Appointments	
Static effect from baseline model	0.635		0.398	
Dynamic Model	ADL	LDV	ADL	LDV
Long-Run Effect	0.503	0.479	0.219	0.238
First Year	53.5%	55.7%	-7.83%	57.4%
Second Year	25.75%	24.67%	61.18%	24.45%
Third Year	11.49%	10.93%	26.47%	10.42%
Fourth Year	5.13%	4.84%	11.45%	4.44%
Fifth Year	2.29%	2.14%	4.95%	1.89%

Based on coefficient estimates from Table A9

year 4 (based on Model 3 in Table A8. 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: 57.4% in year 1, 24.45% in year 2, 10.42% in year 3, and 4.44% in year 4.[118] 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.

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 A8. Shaded areas and error bars indicate 95% confidence intervals.



[118]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.

Table A10: Replication of Table 1 in Main Text with 2-way Clustered Standard Errors.

	(1)	(2)	(3)	(4)
Openness × Excluded	−0.791**	−0.0002	−0.763**	0.019
	(0.230)	(0.098)	(0.234)	(0.116)
Openness × Excluded × State History	1.904**		2.001**	
	(0.556)		(0.531)	
Openness × Excluded × Merit Appoint.		0.211+		0.243*
		(0.106)		(0.105)
GDP × Excluded			0.030	0.051
			(0.034)	(0.033)
Agric. Share × Excluded			−0.006*	−0.004
			(0.003)	(0.003)
Polity IV × Excluded			−0.002	−0.005
			(0.005)	(0.004)
Resource Rents × Excluded			−0.003	−0.003
			(0.002)	(0.003)
Export Conc. × Excluded			0.050	0.058+
			(0.033)	(0.033)
State History × Excluded	0.526**	−0.007	0.186	−0.667
	(0.181)	(0.087)	(0.503)	(0.484)
Merit Appoint. × Excluded			−0.084	0.022
			(0.084)	(0.030)
Excluded	−1.301*		−1.390**	
	(0.461)		(0.450)	
Conflict Incidence		−0.176+		−0.235*
		(0.100)		(0.107)
$p(B1 = B2)$	0.018	0.073	0.006	0.136
$p(B2 = B3)$	0.002	0.036	0.003	0.006
$p(B1 = B3)$	0.001	0.003	0.000	0.004
Group-FE	Yes	Yes	Yes	Yes
Country-Year FE	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes
Observations	6,849	5,887	5,769	4,954

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

Two-way clustered standard errors: Tables A10 and A11 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.

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 A10. Models 1-2 with in top row, models 3-4 in bottom row. Shaded areas and error bars indicate 95% confidence intervals.

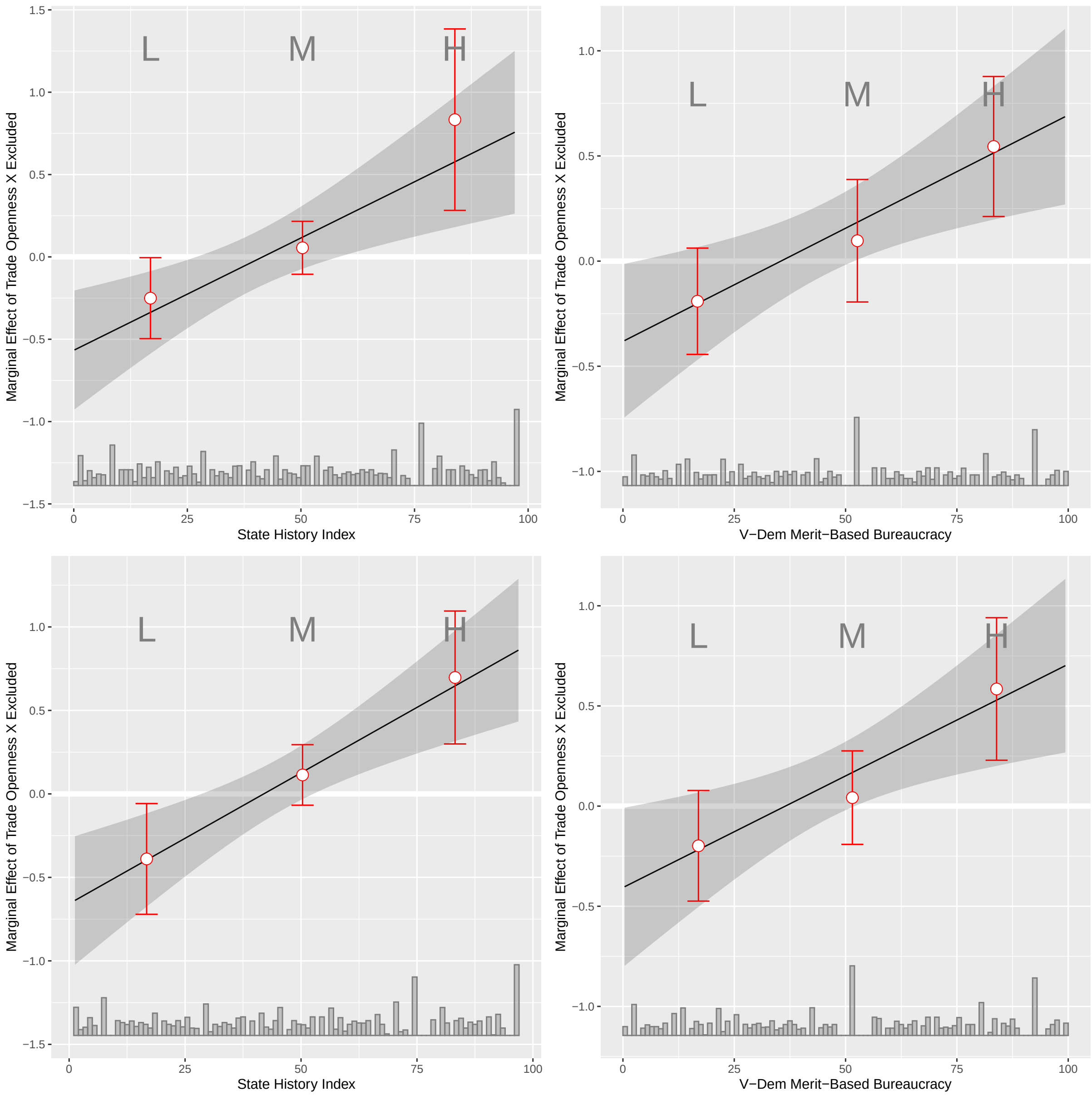
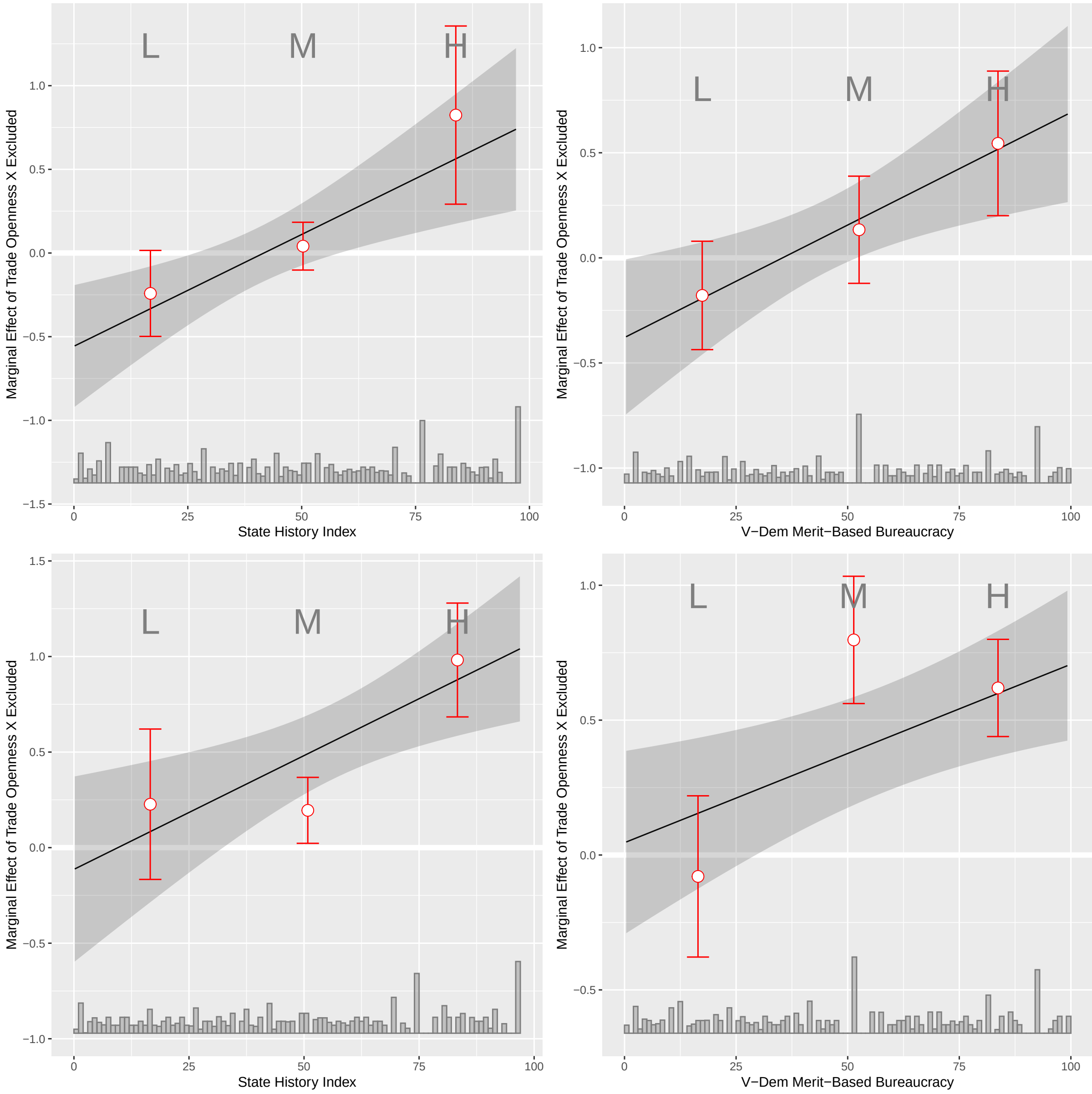


Table A11: Replication of Table 2 in Main Text with 2-way Clustered Standard Errors

	(1)	(2)	(3)	(4)
Openness × Excluded	−0.797**	0.021		
	(0.241)	(0.097)		
Openness × Excl. × State History	1.927**			
	(0.561)			
Openness × Excl. × Merit Appoint.		0.226*		
		(0.107)		
Openness × Initial Night Lights			−0.237	0.307*
			(0.285)	(0.113)
Openness × Initial NL × State History			1.462**	
			(0.468)	
Openness × Initial NL × Merit Appoint.				0.176**
				(0.053)
State History × Excluded	−1.285*			
	(0.457)			
Merit Appoint. × Excluded		−0.191^{+}		
		(0.104)		
Excluded	0.509*	−0.033		
	(0.184)	(0.090)		
Pre-Upgrade Dummy		0.052		
		(0.040)		
Pre-Upgrade Dummy × State History		0.093		
		(0.091)		
Pre-Upgrade Dummy × Merit Appointments	0.216			
	(0.325)			
Pre-Downgrade Dummy	−0.198			
	(0.450)			
Pre-Downgrade Dummy × State History	−0.059	0.0005		
	(0.094)	(0.040)		
Pre-Downgrade Dummy × Merit Appointments	−0.0005	−0.057		
	(0.251)	(0.121)		
$p(B1 = B2)$	0.028	0.041	0.883	0.000
$p(B2 = B3)$	0.002	0.048	0.000	0.237
$p(B1 = B3)$	0.001	0.004	0.003	0.000
Country-Year FE	Yes	Yes	Yes	Yes
Ethnic Group FE	Yes	Yes	Yes	Yes
Controls	No	No	No	No
Observations	6,471	5,564	6,112	5,326

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 A11. Models 1-2 in top row, models 3-4 in bottom row. Shaded areas and error bars indicate 95% confidence intervals.



Alternative measures of state institutions. Table A12 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 A14 and A15 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 (i.e. the most strongly institutionalized autocracies), the effect is positive and larger than for any other regime type. However, neither the estimate for party-based regimes nor its difference to the other regime types reach conventional significance levels (Figure A15).

Table A12: Linear Model of Group-Level Night Lights Mechanisms, 1992-2013.

	(1)	(2)
Openness × Excluded	0.086	0.019
	(0.192)	(0.098)
Openness × Excl. × Exec. Constraints	−0.015	
	(0.032)	
Exec. Constraints × Excluded	0.003	
	(0.032)	
Openness × Excl. × Personalist		0.072
		(0.212)
Personalist × Excluded		−0.057
		(0.215)
Openness × Excl. × Party		0.157
		(0.125)
Party × Excluded		−0.173
		(0.128)
Openness × Excl. × Military		0.159
		(0.279)
Military × Excluded		−0.072
		(0.151)
Openness × Excl. × Monarchy		0.024
		(0.086)
Personalist × Excluded		−0.047
		(0.072)
Exclusion	−0.058	−0.034
	(0.208)	(0.083)
Group-FE	Yes	Yes
Country-Year FE	Yes	Yes
Observations	6,559	6,909

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

Figure A14: 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.

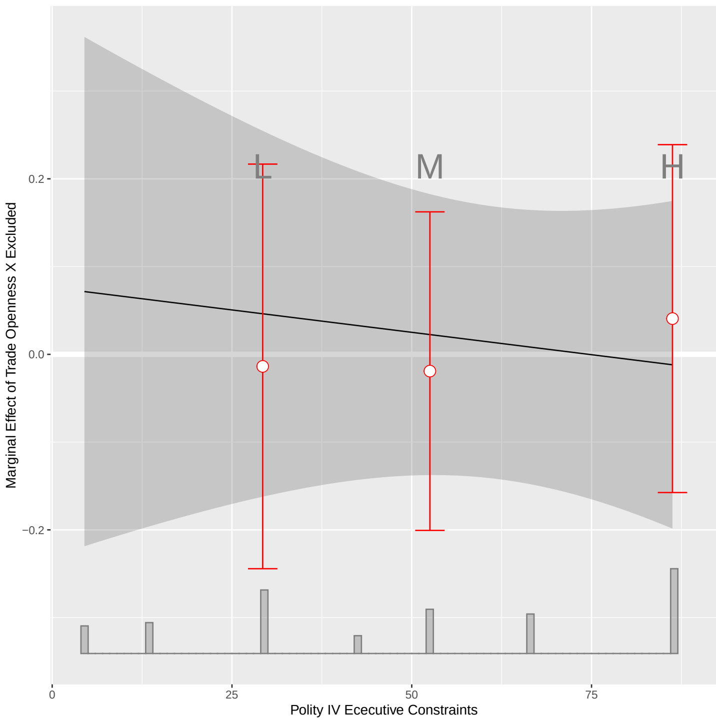


Figure A15: Marginal effects of trade openness on nightlight emissions of excluded groups across military dictatorships, monarchies, democracies, party-based autocracies, and personalist dictatotships . Binning estimates (Hainmueller, Mummolo, and Xu 2019) as points on top. Based on Table A12. Error bars indicate 95% confidence intervals.