

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

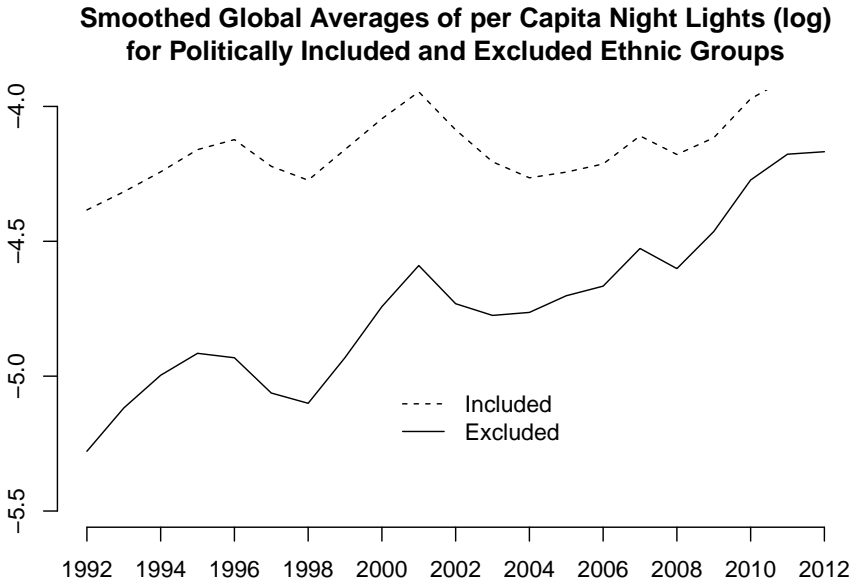


Figure A2: Average Economic Openness and Political Exclusion, 1992–2012.

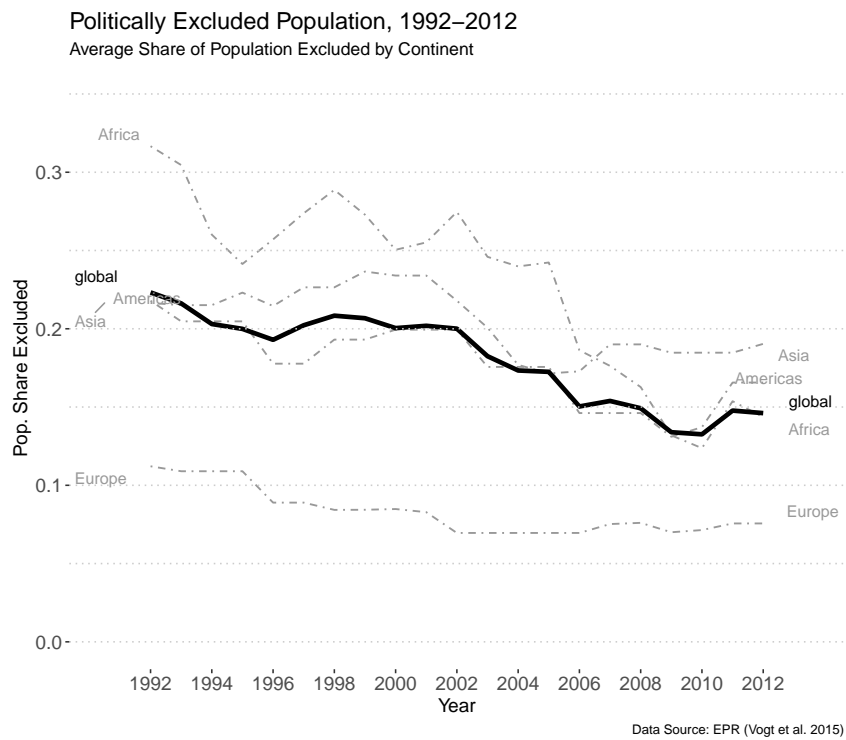
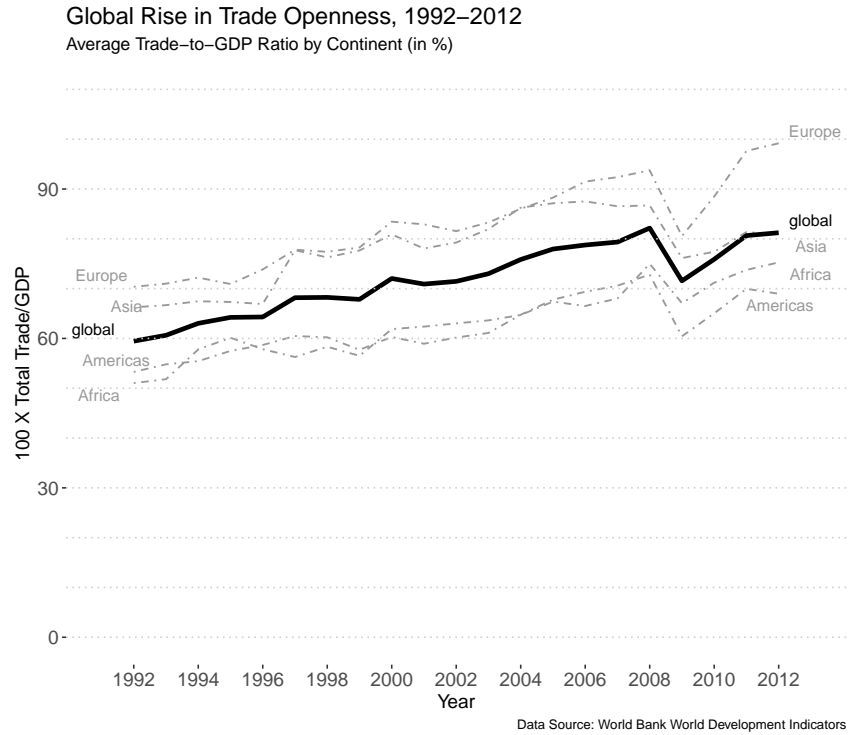


Table A2: Group & Year Fixed Effects

	(1)	(2)	(3)	(4)
Openness	-0.014 (0.233)	0.167 (0.109)	0.136 (0.231)	0.269** (0.087)
Openness × Excluded	-1.559*** (0.281)	0.331 (0.202)	-1.158*** (0.318)	0.278 (0.178)
Openness × Excluded × State History	4.133*** (0.784)		3.296*** (0.735)	
Openness × Excluded × Merit Appoint.		0.512** (0.174)		0.401** (0.150)
Openness × State History	0.186 (0.463)		0.151 (0.502)	
Openness × Merit Appoint.		-0.059 (0.076)		-0.063 (0.091)
State History × Excluded	-2.388*** (0.600)		-1.888*** (0.500)	
Merit Appoint. × Excluded		-0.464** (0.159)		-0.397** (0.146)
GDP p.c. (log)			0.597*** (0.118)	0.671*** (0.151)
Polity IV Score			0.002 (0.003)	0.001 (0.003)
Agric. Share in GDP			-0.003 (0.004)	-0.002 (0.005)
Resource Rents			0.001 (0.003)	0.001 (0.003)
Export Diversification			-0.017 (0.040)	-0.084* (0.042)
GDP × Excluded			0.039 (0.040)	0.108* (0.050)
Polity IV × Excluded			-0.006 (0.004)	-0.003 (0.005)
Agric. Share × Excluded			-0.004 (0.007)	-0.005 (0.006)
Resource Rents × Excluded			-0.0004 (0.003)	-0.004 (0.004)
Export Div. × Excluded			0.082+ (0.048)	0.108+ (0.063)
Excluded	0.959*** (0.221)	-0.158 (0.150)	0.115 (0.651)	-1.644* (0.752)
Conflict Incidence			-0.094 (0.090)	0.026 (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 A3) 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 appointments) in bottom row. Shaded areas indicate 95% confidence intervals.

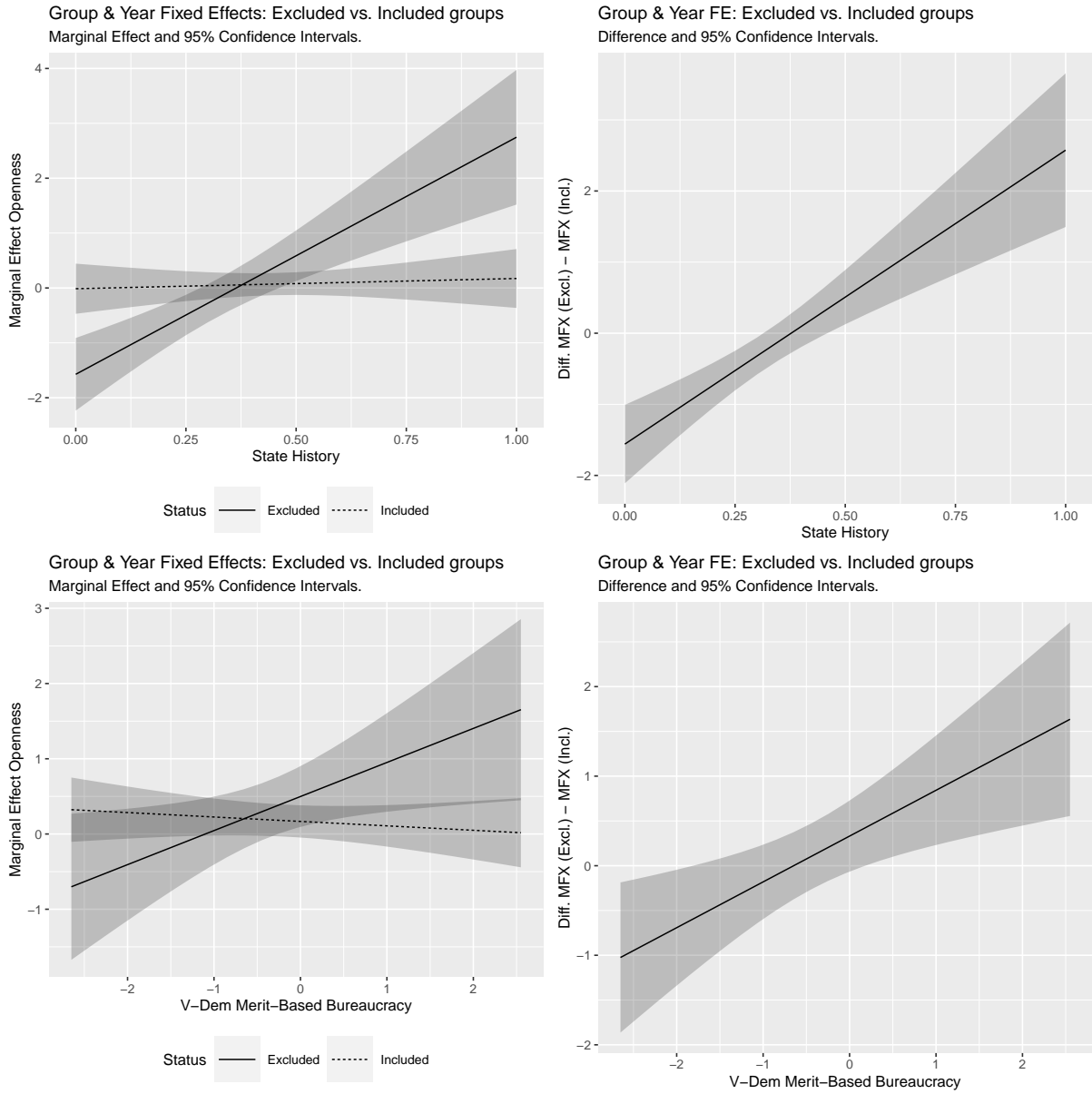


Figure A4: Group & Year Fixed Effects & Controls: Marginal effects of trade openness on nightlight emissions of excluded and included groups across observed range of state antiquity index (top-left) and V-Dem Merit-Based Bureaucracy index (bottom-left). Difference in marginal effect between excluded 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.

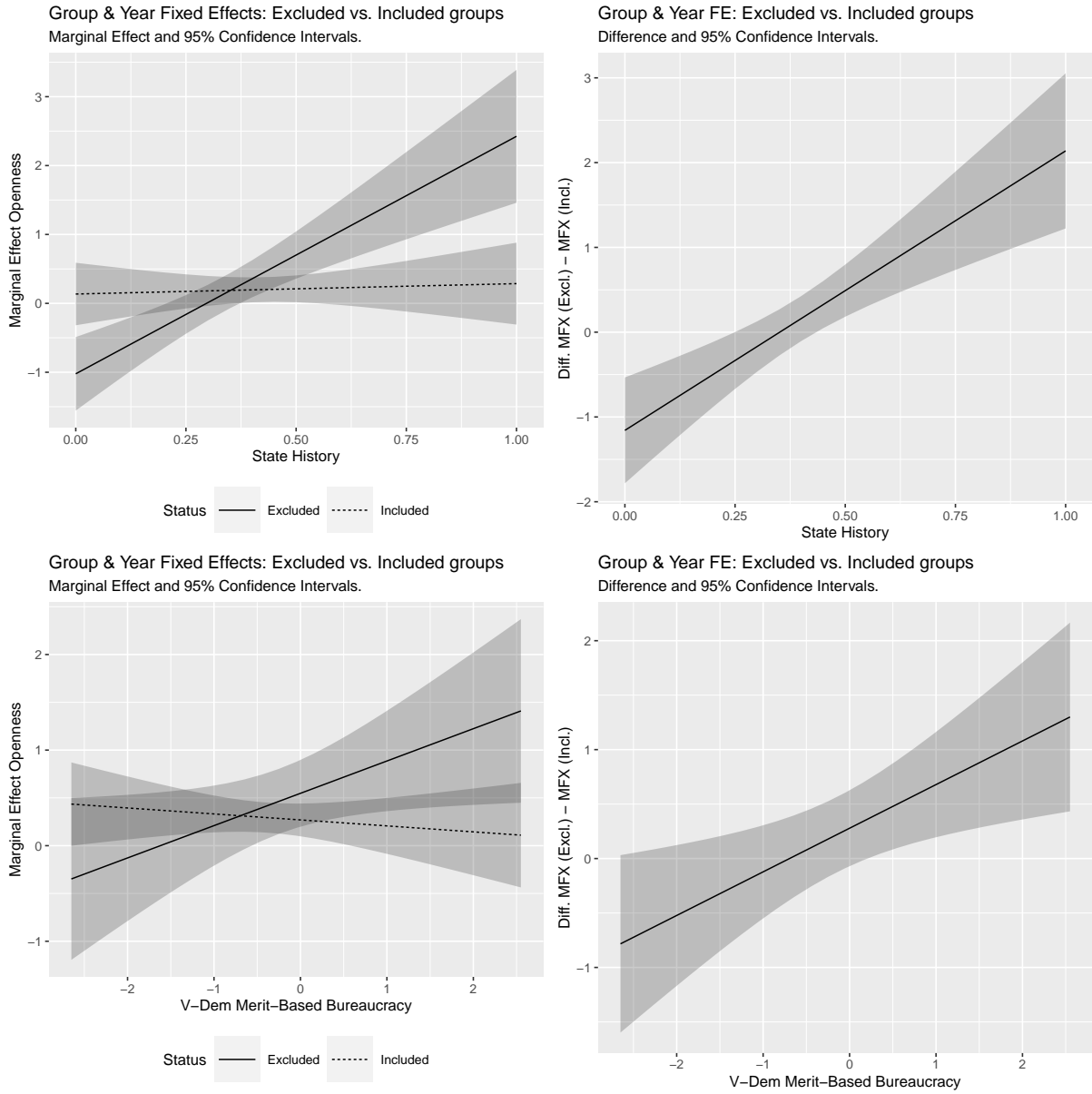


Table A3: Split Sample at Median of Institutional Moderators

	State Age		Merit. Bureauc.	
	(1)	(2)	(3)	(4)
Openness \times Excluded	-0.235*	0.450**	-0.200	0.440**
	(0.109)	(0.147)	(0.135)	(0.134)
GDP \times Excluded	-0.050	0.191**	-0.017	0.150**
	(0.035)	(0.055)	(0.038)	(0.043)
Agric. Share \times Excluded	-0.009*	-0.006	-0.007 ⁺	0.002
	(0.004)	(0.005)	(0.004)	(0.005)
Polity IV \times Excluded	-0.002	-0.001	-0.010*	0.001
	(0.007)	(0.005)	(0.004)	(0.010)
Resource Rents \times Excluded	-0.001	-0.007*	-0.001	0.003
	(0.003)	(0.003)	(0.003)	(0.004)
Export Div. \times 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 endogenous 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 de-

composing 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 (0.222)	0.036 (0.101)	NA (NA)	NA (NA)
Excluded	0.641 (0.544)	0.701** (0.241)	0.236 (0.344)	0.237 (0.154)
Merit Appointments		0.202+ (0.119)		NA (NA)
Openness (Δ) \times Excluded	-1.586*** (0.294)	0.539** (0.192)	-0.976** (0.320)	0.099 (0.101)
Openness (Δ) \times Excluded \times State History	4.321*** (0.697)		2.260*** (0.589)	
Openness (Δ) \times Excluded \times Merit Appoint.		0.582*** (0.147)		0.262* (0.103)
Openness (Δ) \times State History	0.051 (0.446)		NA (NA)	
Openness (Δ) \times Merit Appoint.		-0.095 (0.070)		NA (NA)
Excluded \times State History	-1.237 (1.742)		-0.627 (1.116)	
Openness (\emptyset) \times Excluded \times State History	1.908 (2.867)		0.595 (1.907)	
Excluded \times Merit Appoint.		0.001 (0.062)		0.019 (0.043)
Openness (\emptyset) \times Excluded \times Merit Appoint.		-0.188** (0.058)		-0.096* (0.040)
Openness (\emptyset)	NA (NA)	NA (NA)	NA (NA)	NA (NA)
Openness (\emptyset) \times Excluded	-0.940 (0.895)	-1.093** (0.354)	-0.264 (0.598)	-0.419+ (0.248)
Openness (\emptyset) \times State History	NA (NA)		NA (NA)	
Openness (\emptyset) \times Merit Appoint.		-0.217 (0.155)		NA (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 appointments) in bottom row. Shaded areas indicate 95% confidence intervals.

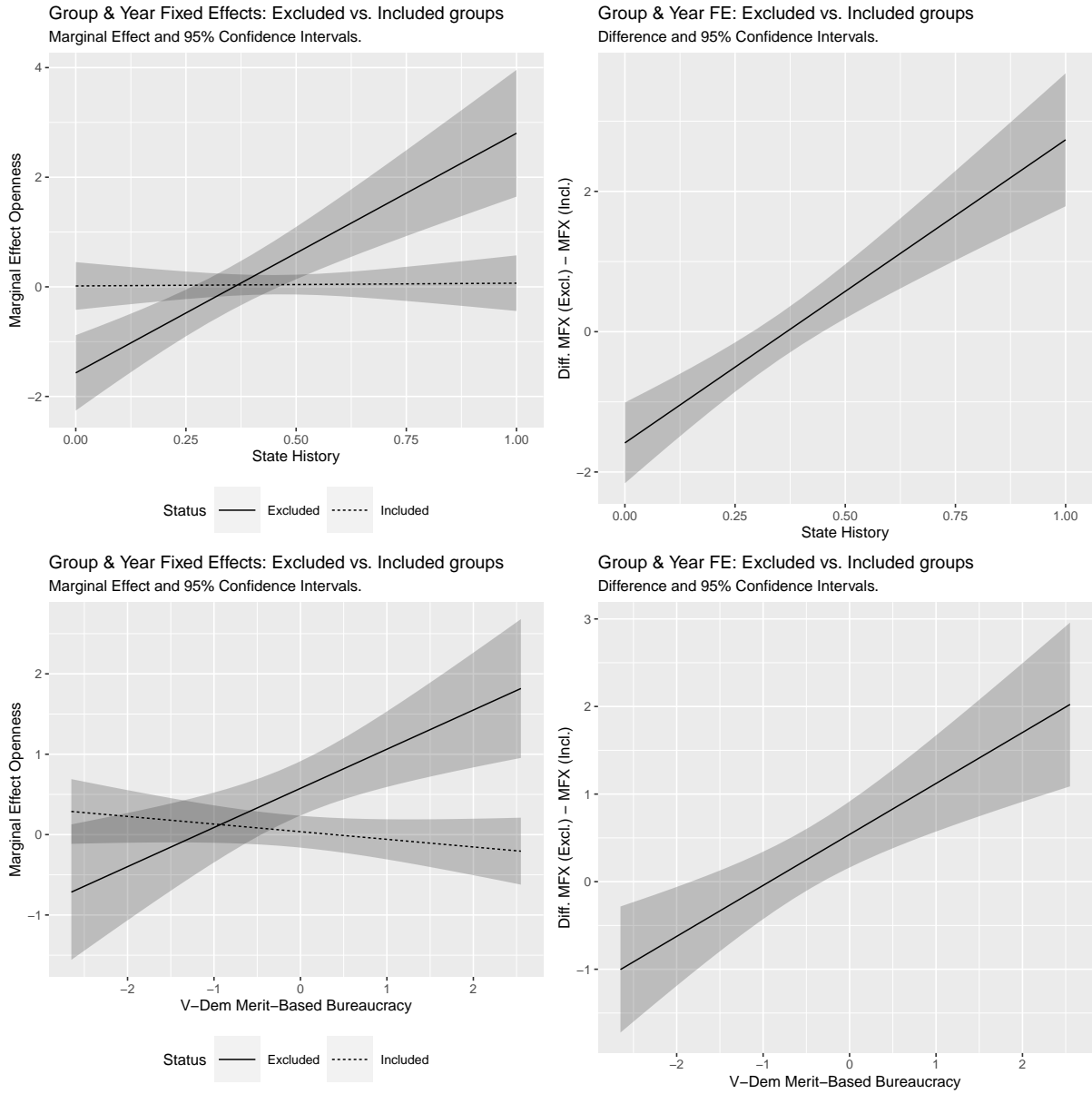
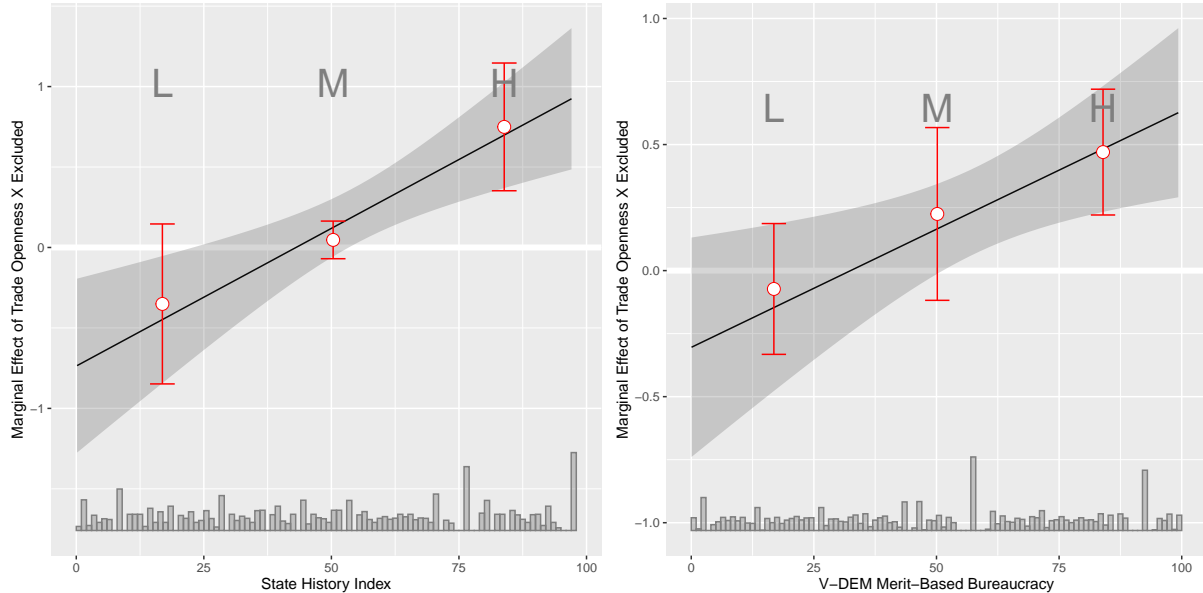


Figure A6: Within-Between Models with Group & Country-Year Fixed Effects: Marginal effects of trade openness on nightlight emissions of excluded groups conditional on state antiquity index (left) and V-Dem Merit-Based Bureaucracy Index (right). Binning estimates (Hainmueller, Mummolo, and Xu 2019) as 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 portfolios 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 but 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 or 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** (0.325)	-0.653* (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 (0.468)	1.136** (0.408)
Openness × Excluded	-1.216* (0.505)	
State History × Excluded		-0.120 (0.090)
Merit Appointments × Excluded	-0.447 (0.453)	-1.109** (0.368)
Max. Group Size × Excluded	0.723** (0.263)	0.559* (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.

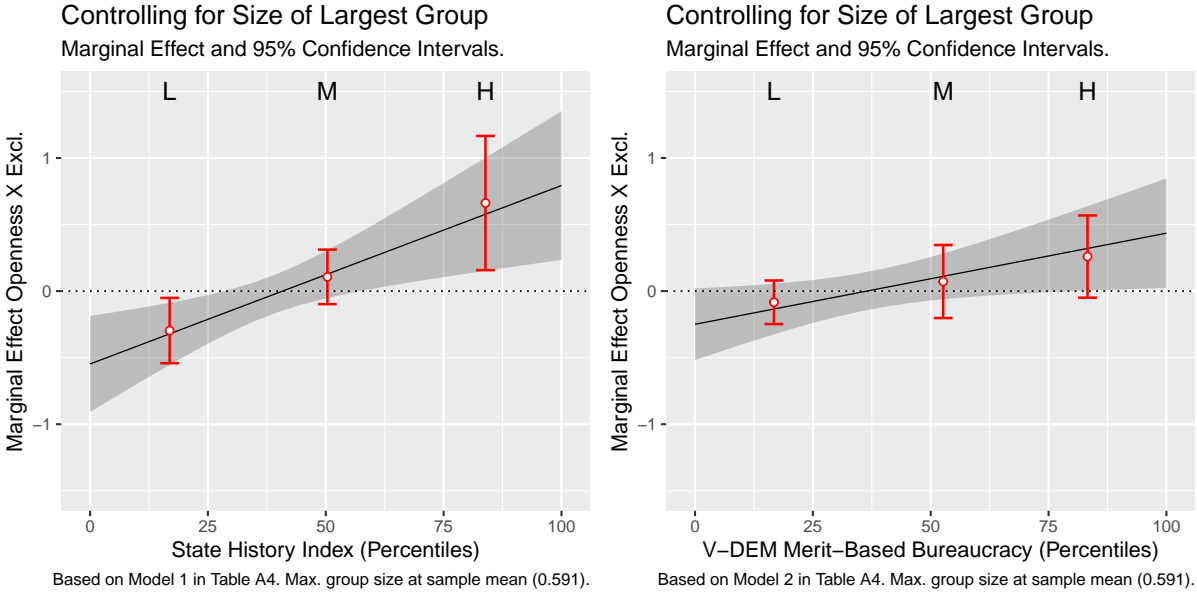


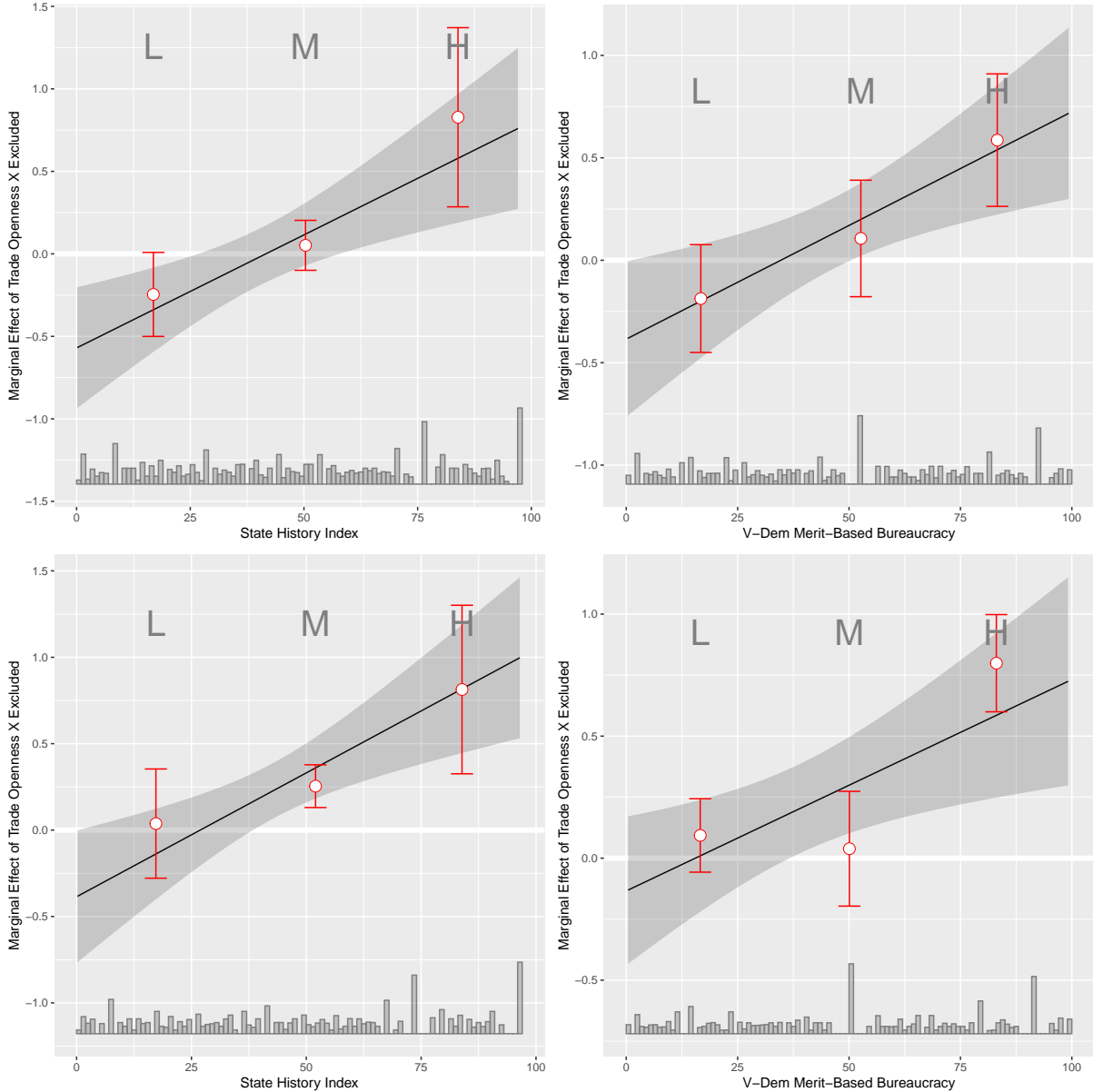
Table A6: Endogeneity of Political Status to Economic Performance?

	(1)	(2)	(3)	(4)
Openness \times Excluded	-0.792** (0.240)	0.001 (0.099)	-0.625** (0.230)	0.216* (0.099)
Openness \times Excl. \times State History	1.904*** (0.541)		1.894*** (0.480)	
Openness \times Excl. \times Merit Appoint.		0.213* (0.105)		0.189* (0.088)
State History \times Excluded	-1.368** (0.507)			
Merit Appointments \times Excluded		-0.170+ (0.102)		
Excluded	0.541** (0.199)	-0.014 (0.094)		
Pre-Upgrade Trend	-0.019 (0.036)	0.0001 (0.014)		
Pre-Upgrade Trend \times State History	0.080 (0.116)			
Pre-Upgrade Trend \times Merit Appointments		0.017 (0.017)		
Pre-Downgrade Trend	0.024 (0.077)	-0.012 (0.035)		
Pre-Downgrade Trend \times State History	-0.121 (0.151)			
Pre-Downgrade Trend \times 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 \times Excluded (91)	-0.730 ⁺ (0.375)	0.187 (0.118)		
Openness \times Excluded (91) \times State History	1.957** (0.686)			
Openness \times Excluded (91) \times Merit Appoint.		0.179 (0.126)		
Openness \times Excluded			-0.787*** (0.206)	0.082 (0.111)
Openness \times Excluded \times State History			0.007** (0.003)	
Openness \times Excluded \times Merit Appoint.			0.011*** (0.003)	
Openness \times Excluded \times Merit Appoint. (\emptyset)				0.173 (0.108)
State History \times Excluded			-0.007** (0.002)	
Merit Appoint. \times Excluded			-0.006* (0.003)	
Merit Appoint. (\emptyset) \times Excluded				-0.025 (0.108)
Excluded			0.548** (0.184)	-0.088 (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.

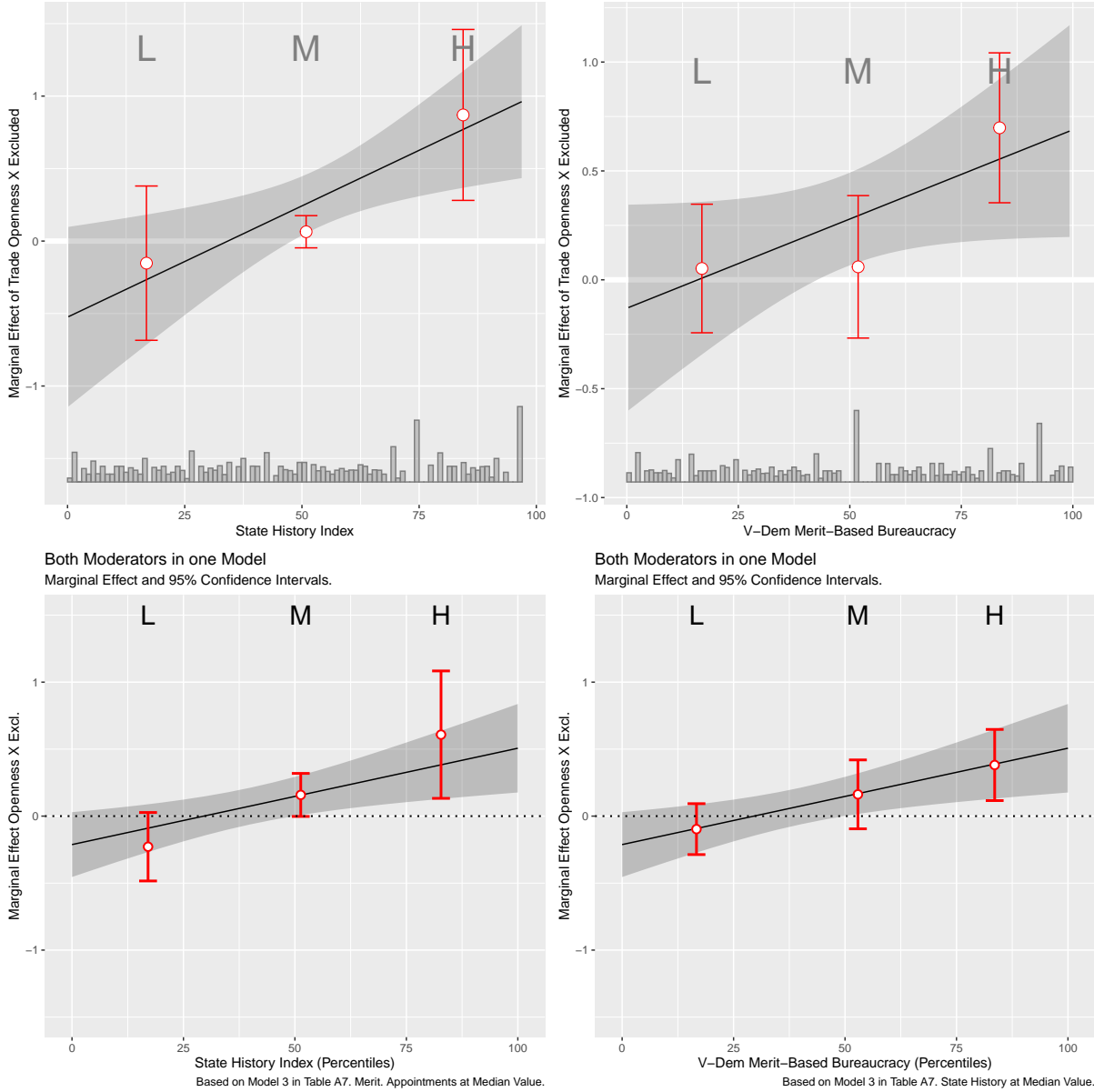
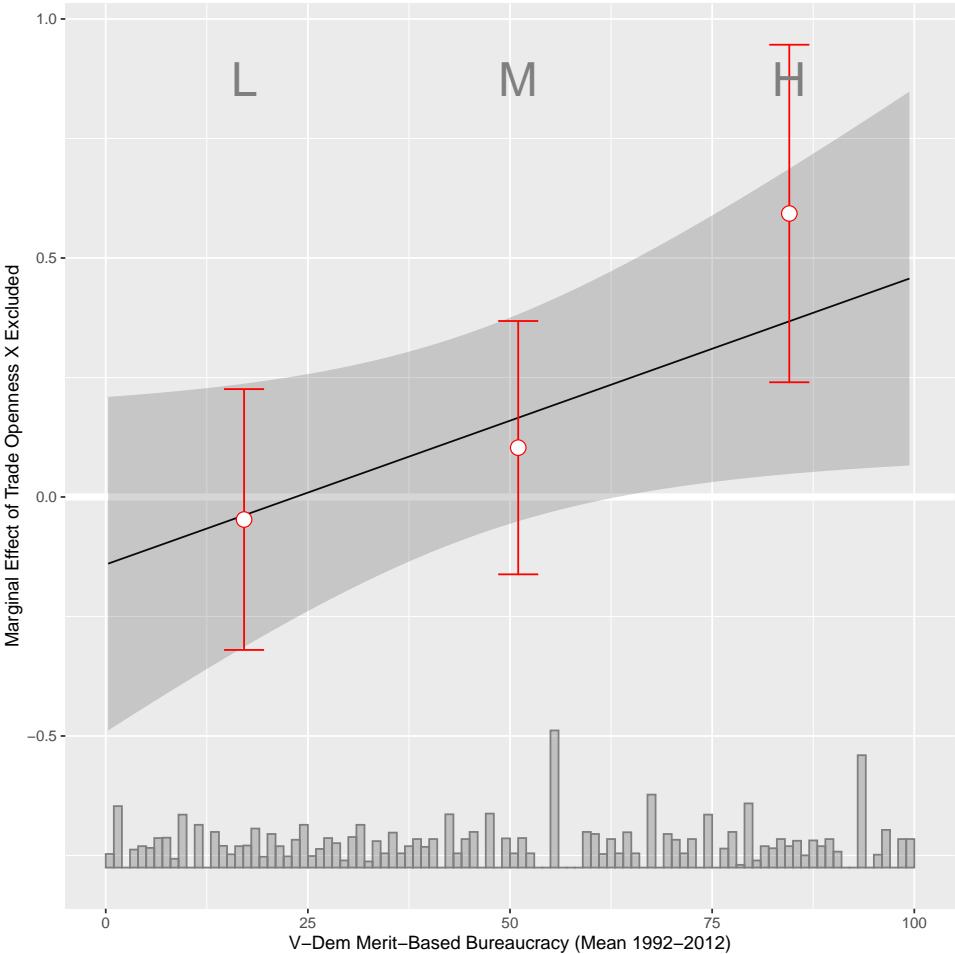


Figure A10: Marginal effect of trade openness on nightlight emissions of excluded groups across percentiles of the period mean (1992-2012) of the V-Dem Merit-Based Bureaucracy Index. Binning estimates (Hainmueller, Mummolo, and Xu 2019) as points on top. Based on Model 4 in Table A7. Shaded areas and 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 90th 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

¹¹⁷In 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 \times Excluded	-0.393** (0.124)	-0.042 (0.057)	-0.410** (0.138)	-0.032 (0.059)
Openness \times Excl. \times State History	0.884** (0.279)		0.904** (0.275)	
Openness \times Excl. \times Merit Appoint.		0.013 (0.062)		0.089 (0.056)
Openness \times Excluded (t-1)	-0.030 (0.143)	-0.005 (0.071)		
Openness \times Excl. \times State History (t-1)	0.053 (0.284)			
Openness \times Excl. \times Merit Appoint. (t-1)		0.078 (0.067)		
State History \times Excluded	-0.469* (0.209)		-0.519* (0.220)	
State History \times Excluded (t-1)	-0.044 (0.192)			
Merit Appoint. (\emptyset) \times Excluded		-0.088+ (0.052)		-0.067 (0.051)
Merit Appoint. (\emptyset) \times Excluded (t-1)		0.075 (0.048)		
Exclusion	0.198+ (0.118)	0.004 (0.056)	0.214* (0.107)	-0.004 (0.050)
Exclusion (t-1)	0.003 (0.115)	-0.021 (0.054)		
Night Lights (log, t-1)	0.446*** (0.055)	0.433*** (0.069)	0.443*** (0.053)	0.426*** (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

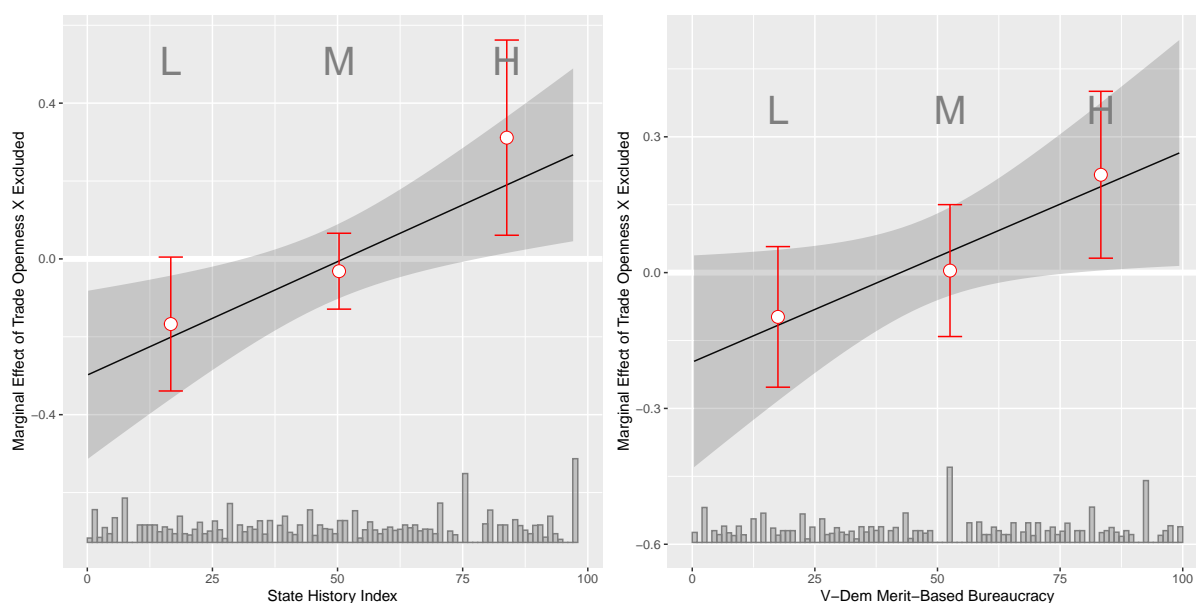
Table A9: Short and Long-Run Effects from Dynamic Models

Moderator at 90 th percentile Static effect from baseline model	State History 0.635		Merit Appointments 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.



¹¹⁸These 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** (0.230)	-0.0002 (0.098)	-0.763** (0.234)	0.019 (0.116)
Openness × Excluded × State History	1.904** (0.556)		2.001** (0.531)	
Openness × Excluded × Merit Appoint.		0.211+ (0.106)		0.243* (0.105)
GDP × Excluded			0.030 (0.034)	0.051 (0.033)
Agric. Share × Excluded			-0.006* (0.003)	-0.004 (0.003)
Polity IV × Excluded			-0.002 (0.005)	-0.005 (0.004)
Resource Rents × Excluded			-0.003 (0.002)	-0.003 (0.003)
Export Conc. × Excluded			0.050 (0.033)	0.058+ (0.033)
State History × Excluded	0.526** (0.181)	-0.007 (0.087)	0.186 (0.503)	-0.667 (0.484)
Merit Appoint. × Excluded			-0.084 (0.084)	0.022 (0.030)
Excluded	-1.301* (0.461)		-1.390** (0.450)	
Conflict Incidence		-0.176+ (0.100)		-0.235* (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.

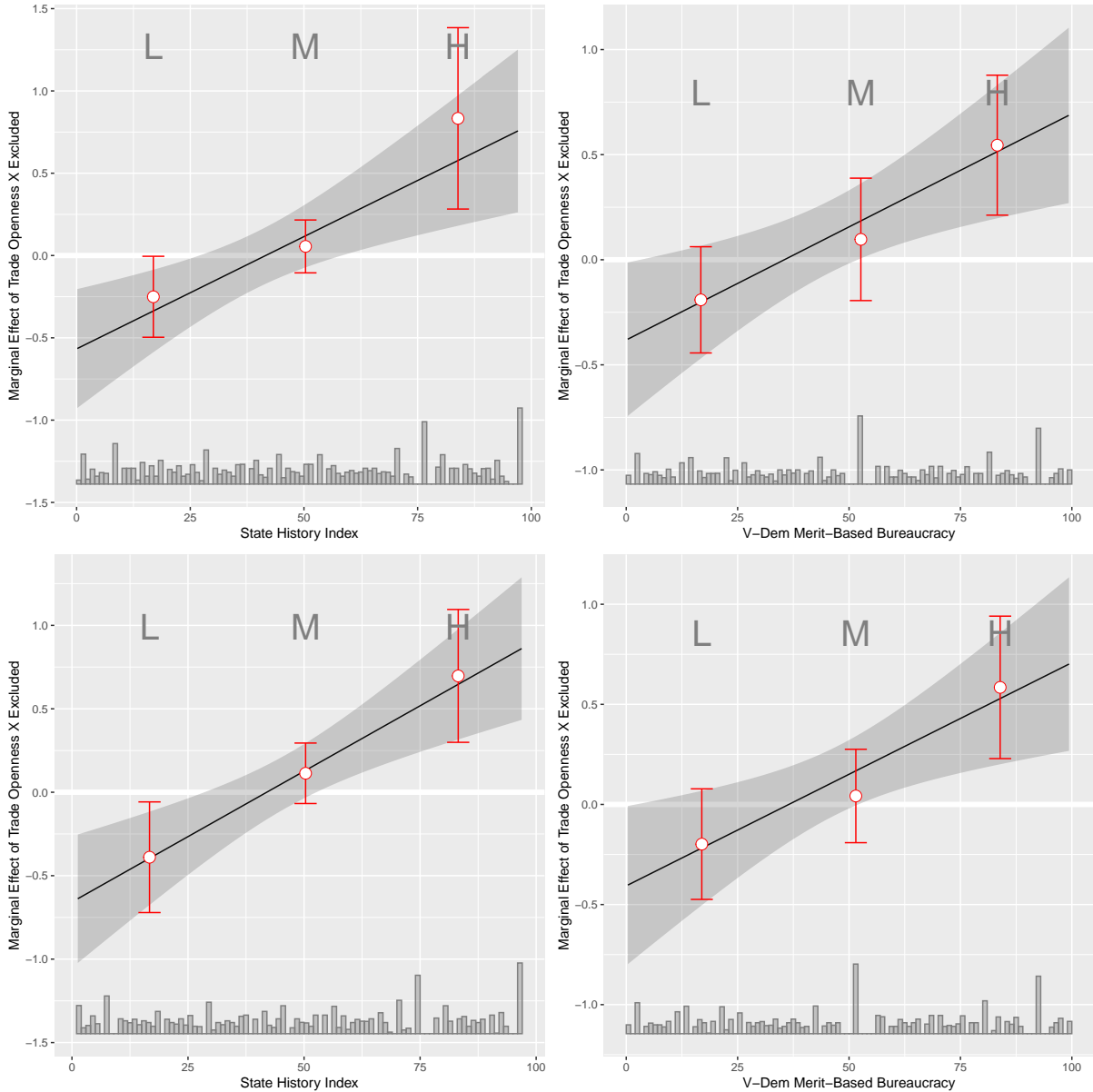
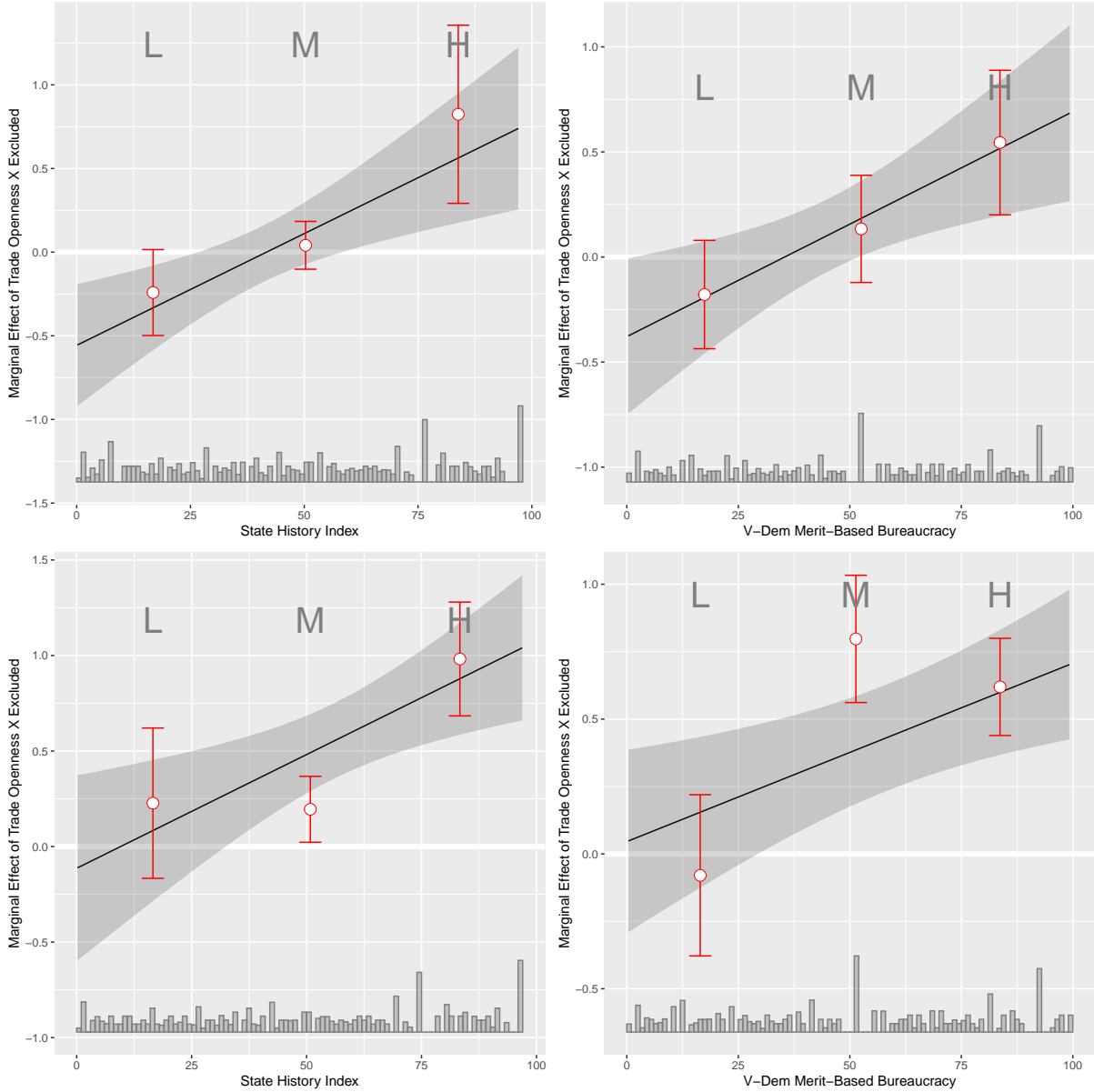


Table A11: Replication of Table 2 in Main Text with 2-way Clustered Standard Errors

	(1)	(2)	(3)	(4)
Openness × Excluded	-0.797** (0.241)	0.021 (0.097)		
Openness × Excl. × State History	1.927** (0.561)			
Openness × Excl. × Merit Appoint.		0.226* (0.107)		
Openness × Initial Night Lights			-0.237 (0.285)	0.307* (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* (0.184)	-0.033 (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 (0.094)	0.0005 (0.040)		
Pre-Downgrade Dummy × Merit Appointments	-0.0005 (0.251)	-0.057 (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 (0.192)	0.019 (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 (0.208)	-0.034 (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.

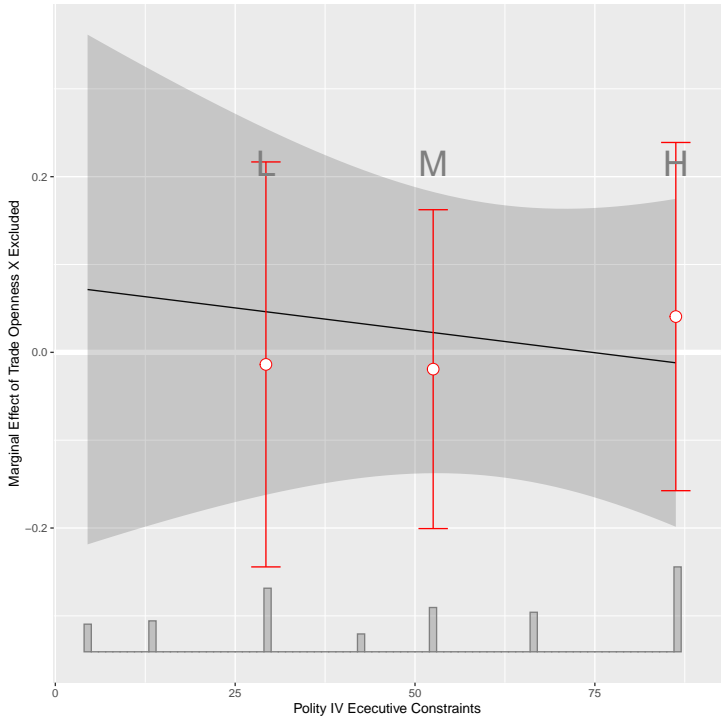


Figure A15: Marginal effects of trade openness on nightlight emissions of excluded groups across military dictatorships, monarchies, democracies, party-based autocracies, and personalist dictatotships . Binning estimates (Hainmueller, Mummolo, and Xu 2019) as points on top. Based on Table A12. Error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

