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Ethnopolitical Exclusion and Resource Conflict

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Abstract: Why do natural resources fuel ethnic conflict in some ethnic homelands but not others? This article argues that the answer turns on the boundary between active discrimination and passive powerlessness. Among excluded groups, lootable resources raise conflict risk only where the state actively discriminates, not where groups are merely powerless; among included groups, the same resources predict lower risk. No rival account reproduces this positive/null/negative gradient. The boundary governs whether these groups fight, not what they fight over: petroleum pushes discriminated and powerless groups alike toward territorial conflict, wherever the oil sits. Two-way fixed-effects estimates on 859 ethnic groups in 150 countries (1960–2020) establish both patterns. The conflict-generating mechanism thus lies between a group and its state, so country-level resource-curse measures, averaged across the nation, can miss the within-country boundary that actually divides conflict from peace.

Keywords: ethnic conflict; natural resources; political exclusion; resource curse; ethnic power relations

Reproducibility Package: All data, code, and documentation necessary to reproduce every empirical result in this article are openly available at https://doi.org/10.17605/OSF.IO/S28Y4.

WHY do natural resources fuel ethnic conflict in some homelands but not others? This article argues that the answer turns on the political relationship between an ethnic group and the state. The same resources raise the risk of conflict for groups the state actively discriminates against, leave it unchanged for those it merely sidelines, and lower it for those it shares power with, a pattern no rival account reproduces. A second question concerns not whether these groups fight but what they fight over.

The mechanism operates bilaterally on the state-group relationship (Cederman, Wimmer, and Min 2010; Cederman, Gleditsch, and Buhaug 2013; Wimmer, Cederman, and Min 2009). Resources beneath a group's homeland are an asset the state seeks and from which the group is denied a share. When the state has revealed coercion as its preferred posture toward the group, the institutional channel for negotiated revenue-sharing has closed, and armed mobilization becomes the residual channel the group can use to claim a share or halt the extraction. Powerless groups face neither side of this bargain; included groups face the opposite, with the institutional channel open and rents flowing through it.

Building on Asal et al. (2016)'s group-level resource-exclusion interaction and Alcorta, Swedlund, and Smits (2020)'s Sub-Saharan exclusion-conflict boundary, this article isolates the resource-conflict boundary *within* the excluded category on a global panel and identifies it against rival accounts using a joint sign pattern across the three political-status categories. Only the contested-homeland account predicts

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the full pattern: amplification under active discrimination, null under mere powerlessness, and sign reversal under inclusion. Rebel-financing (Collier and Hoeffler 2004) predicts uniform amplification (resources finance rebels regardless of political status); grievance accounts (Buhaug, Cederman, and Gleditsch 2014; Cederman et al. 2013; Gurr 1993) predict amplification under any form of exclusion and silence on included groups. Neither rival reproduces the positive/null/negative gradient the data show; Table 1 in the *Theory: Contested Homelands* section lays the three sets of predictions side by side. The same account carries a companion prediction about the *form* of conflict. If the discrimination boundary that governs onset is a single mechanism, it should also govern what excluded groups fight over, concentrating petroleum's territorial pull among the groups the state singles out. Whether it does is the article's second question.

I test this argument using the GROWup Research Front End 2021 dataset, a geocoded ethnic-group-year panel integrating annual expert-coded political status data from the Ethnic Power Relations (EPR) project (Wimmer et al. 2009), group-level resource endowments from Peace Research Institute Oslo Grid (PRIO-GRID) geological surveys, and conflict onset data from the Uppsala Conflict Data Program (Gleditsch et al. 2002; Vogt et al. 2015). The sample covers 859 EPR-relevant groups across 150 countries from 1960 to 2020. The main specification is a linear probability model (LPM) with two-way fixed effects for country and year and the country-year control set. The within-excluded asymmetry holds across a spec curve, full leave-one-out diagnostics, alternative outcome codings, and triangulation against an independent production-based resource dataset (Denly et al. 2022).

The companion prediction about conflict form does not hold. Petroleum tilts discriminated and powerless groups alike toward territorial conflict, so what excluded groups fight over follows the geography of the resource (Hunziker and Cederman 2017; Le Billon 2001), not the discrimination boundary. I reconcile the two results by treating discrimination and geography as separate mechanisms: discrimination governs whether excluded groups fight, and geography governs what they fight over.

The *Theory: Contested Homelands* section develops the contested-homeland argument and derives the hypotheses. The *Research Design* section describes the research design. The *Results* section presents the empirical results and robustness checks. The *Discussion and Conclusion* section discusses theoretical and policy implications, scope conditions, limitations, and future research. An online supplement provides the full robustness battery, data construction details, and additional results.

Theory: Contested Homelands

Consider the Kurds of northern Iraq, concentrated in petroleum-rich territories near Kirkuk, systematically excluded from Ba'athist executive power, and by the 1980s subject to active state discrimination: their decades of armed insurgency intensified alongside rising oil revenues, exemplifying the resource-under-exclusion configuration this article theorizes. The contested-homeland argument identifies why resources can have opposite effects depending on whether the resident group shares state power: for excluded groups, resource wealth raises the prize of territorial

control without providing state-mediated access to rents, making armed contestation the only available route.

The country-level association between natural resource wealth and civil war risk is well established but contested (Humphreys 2005): fixed effects on the country panel attenuate the oil-conflict association to insignificance (Cotet and Tsui 2013), and instrumenting resource dependence renders it statistically insignificant (Brunnschweiler and Bulte 2009). Country-level estimates average across the ethnic groups inhabiting resource-rich territory, conflating configurations where the mechanism plausibly operates (excluded minorities atop the reserves) with those where it does not (dominant majorities controlling both state and rents). The subnational turn relocates analysis to spatial cells but does not systematically condition on the political status of the resident group (Berman et al. 2017; Buhaug and Gates 2002; Lujala 2010), and dyadic tests of exclusion theory have established that the center-periphery axis matters for conflict onset without yet combining that finding with the resource-status interaction this article isolates (Buhaug, Cederman, and Rød 2008).

Wimmer et al. (2009) establish, in a configurational analysis of the full postwar cross-national record, that conflict risk tracks the *mode of ethnic incorporation into state power* rather than the presence of ethnic heterogeneity: it is political exclusion, not ethnic diversity per se, that generates armed conflict. The EPR framework operationalizes this finding by coding each politically relevant group's annual access to central executive power, distinguishing DISCRIMINATED groups (those facing active state hostility through civil-rights restrictions and targeted repression) from POWERLESS groups, which are politically marginal but not actively targeted (Cederman et al. 2010, 2013; Wucherpfennig et al. 2012). The political salience of these distinctions is itself a product of institutional context (Posner 2005) and colonial inheritance patterns (Müller-Crepon 2020; Wucherpfennig, Hunziker, and Cederman 2016). This discrimination-powerlessness boundary is what the contested-homeland argument turns on.

The contested-homeland argument extends the internal-colonialism tradition (Hechter 1975) by locating the conflict-generating mechanism in the bilateral state-group relationship rather than in the periphery's ethnic distinctiveness alone. I argue that valuable territory in an ethnic group's settlement area raises the stakes of political conflict under exclusion, and that these elevated stakes translate into armed conflict primarily when the state has adopted an actively adversarial posture toward the group. The mechanism draws on a core insight from the sociology of state formation: states construct coercive administrative reach through the extraction and administration of bounded populations, and natural resource deposits concentrate both fiscal incentive and administrative problem in a single geographic site (Mann 1986; Tilly 1992). When an ethnically distinct group inhabits that site, the state must resolve a fundamental organizational choice between incorporating the group as a political constituency and treating it as a subject of extraction. The EPR classification identifies which resolution the state has made: the discriminated status marks active coercion and revenue denial; the powerless status marks administrative indifference. The ethnic boundary, once stabilized by coercive state action, reorganizes group resources along the same categorical line the state drew. Wimmer (2008) identifies this as the key dynamic linking state strategies to ethnic boundary hardening.[1]

"Valuable territory," as used here, denotes land whose economic returns generate a bilateral extraction contest between a central state seeking to capture the rents and a resident group denied a share of them; extractive resources are the cleanest empirical case because their rents accrue to concentrated, physically located extraction sites that the state must coercively secure against the resident population. This connects the argument to Roessler (2011)'s analysis of how African rulers trade off coup risk against civil-war risk through targeted ethnic exclusion. Exclusion is a deliberate strategic choice, not an institutional accident. The contested-homeland account adds a sharper boundary condition: peripheral extraction generates conflict not whenever the periphery is ethnically distinct but specifically where the state has chosen coercive over accommodative incorporation. The mechanism operates simultaneously on both sides of a potential conflict, creating a compounding of incentives that is absent either when valuable territory is present without exclusion or when exclusion is present without contested territorial value.

The asymmetric bargain at the heart of the argument can be stated compactly. The state has a material incentive to maintain control over resource-rich territory (Boone 2014; Herbst 2000; Scott 1998); when the resident group is excluded, the state faces reduced institutional constraints on extraction, and active discrimination signals that coercion has displaced negotiation as the state's preferred posture. The excluded group, denied formal access to revenue redistribution and facing a state that has committed to coercion, retains armed mobilization as the only remaining channel to claim a share of rents or to halt extraction, either by forcing inclusion (governmental conflict) or by establishing autonomous territorial control (territorial conflict). Powerless groups face neither side of this bargain: an indifferent state lacks the coercive-extraction incentive that triggers the mechanism, and a powerless group retains a theoretical path to negotiated accommodation. This requires that state repression be selectively applied rather than universal. The documented politics of ruling coalitions supports this premise: elites repress specific groups in response to perceived threats while tolerating others (Roessler 2011). The EPR discrimination indicator captures this selectivity through observed behavior rather than inferred preference: powerless groups are coded as such precisely because the state has not invested coercive infrastructure against them. When the state subsequently shifts posture, later EPR coding waves reclassify the group as discriminated; the lag-sensitivity analysis in the *Robustness* section holds the within-excluded asymmetry across one- to three-year lags of the exclusion variable, addressing the resulting backward-coding concern.

The argument's discriminating power against rival accounts comes from the joint sign pattern across the three political-status categories, not from any single category alone. Table 1 makes this explicit. A pure rebel-financing account (Collier and Hoeffler 2004) predicts that lootable resources elevate conflict risk wherever they occur, including in included groups, because the mechanism is rebel-side capacity. A pure grievance account (Cederman et al. 2013; Gurr 1993) predicts that any form of political exclusion (discriminated or powerless) is sufficient to mobilize when resources are present, because resources amplify relative deprivation; it has no prediction on included groups. The contested-homeland argument predicts that the lootable effect operates specifically through coercive state-group bargaining, which

Table 1: Three Accounts of the Resource-Conflict Relationship: Predicted Sign of Lootable × Status Interaction by Political Category.

	Contested-Homeland (This Article)	Rebel-Financing (Collier and Hoeffler 2004)	Grievance (Gurr 1993)
Discriminated	+ (large)	+	+
Powerless	0	+	+
Included	−	+	0
Joint pattern	$+ / 0 / -$	$+ / + / +$	$+ / + / 0$

Notes: Predictions for the coefficient on lootable resources interacted with political-status category in a group-year panel regression of armed conflict onset. The contested-homeland account uniquely predicts a negative coefficient for included groups; rebel-financing and grievance accounts predict positive or null effects respectively. The within-excluded asymmetry (discriminated $\neq$ powerless) and the included sign reversal (negative on lootable for included groups) together distinguish the contested-homeland account from the two leading alternatives in observational terms.

is present under discrimination, attenuated under mere powerlessness, and absent under inclusion (where institutional substitution dominates). Only the contested-homeland account generates the joint prediction of positive-discriminated, null-powerless, and negative-included coefficients.

Two implications of Table 1 deserve emphasis. First, territorial fixity predicts that petroleum should elevate conflict risk for excluded groups because the state wants the geographically fixed rents. Rebel-financing accounts predict null on petroleum because mobile insurgents cannot loot it. Second, the negative coefficient for included groups is an active prediction, not merely a null: resource rents accessible through institutional participation raise the group's material stake in the settlement, so armed mobilization would forfeit that access.

Two grievance variants differ in their included-group predictions. A Gurr-style relative-deprivation account (Gurr 1993) predicts null for included groups; Cederman et al. (2013)'s horizontal-inequalities (HI) variant predicts null-or-attenuated and can accommodate the within-excluded asymmetry, but only because HI mobilization requires both political exclusion and a deprivation-relevant cleavage, leaving the discriminated/powerless distinction unforced. The contested-homeland account is distinguished from Gurr-style grievance by the included-group sign reversal, from HI-style grievance by the within-excluded asymmetry, and from both jointly by the full $+/0/-$ pattern. A strict rebel-financing reading shifts the included-group row to $+/+/0$ (included groups do not produce rebels); even under that reading the account is contradicted by the within-excluded asymmetry, which rebel financing predicts to be uniformly positive.

The article accordingly focuses on extractive resources, where the bilateral territorial contest concentrates into geographically discrete, capital-intensive sites whose rents flow to central state authorities rather than to dispersed local producers (Knutsen et al. 2017; Sánchez de la Sierra 2020).

A corollary concerns the type of conflict the mechanism produces. Petroleum and concentrated mineral deposits are spatially fixed: their value can only be captured by controlling the specific territory where extraction occurs, not by capturing

the central state (Caselli, Morelli, and Rohner 2015; Le Billon 2001; Toft 2014). The territorial imperative this creates has been documented in resource-rich secessionist movements from Aceh to the Niger Delta (Aspinall 2007). The implication follows directly from Buhaug (2006)'s framework linking relative capability to rebel objective: groups whose homelands contain fixed-rent resources have a substantive reason to prefer secession over center-seeking conflict. Lootable resources, in contrast, are mobile and fungible and can support either form of insurgency (Lujala, Gleditsch, and Gilmore 2005; Olsson, 2007). The account therefore predicts a corresponding shift in conflict form: petroleum-rich excluded homelands should produce territorial rather than governmental armed conflict, with the lootable interaction carrying no equivalent prediction.

The argument yields three testable hypotheses. H1a and H1b trace the boundary the mechanism turns on, identifying it on either side of the discrimination divide; H2 takes up the resource-type margin and asks whether the same boundary governs the form of conflict. Splitting H1 into two facets makes the failure modes explicit: a finding that the within-excluded asymmetry holds without the included sign reversal, or vice versa, would partially support the bargaining account but not its full prediction.

> *H1a (within-excluded asymmetry): Among politically excluded ethnic groups, the association between lootable resources in the group's homeland and armed conflict onset is concentrated in groups subject to active state discrimination and is absent in groups that are merely politically powerless.*
>
> *H1b (included-group sign reversal): Among politically included ethnic groups, lootable resources in the group's homeland are associated with* lower *armed conflict onset risk. Together with H1a, this completes the joint sign pattern across the three political-status categories.*
>
> *H2 (discrimination-governed conflict form): Among excluded groups, petroleum's tilt toward territorial rather than governmental conflict falls most heavily on those subject to active state discrimination, mirroring the onset gradient of H1a.*

Petroleum is the paradigmatic point resource: geographically fixed, infrastructure-dependent, and impossible to capture through a mobile insurgency, which makes control of its territory, rather than of the center, the natural object of conflict. Lootable minerals are diffuse and fungible and carry no equivalent directional prediction across conflict type. H2 is the single-mechanism reading of that tilt: if the boundary that governs onset also governs form, petroleum's territorial pull should concentrate among the discriminated rather than extend to the merely powerless. *The Petroleum and Conflict-Type Substitution (H2)* section tests whether it does.

Research Design

Data and Variables

The analysis draws on the GROWup Research Front End (RFE) 2021 dataset, a geocoded ethnic-group-year panel assembled at ETH Zurich that integrates political

status, geographic, and conflict data at the group level (Vogt et al. 2015). The unit of observation is an ethnic group within a country in a given year. The effective estimation sample consists of EPR-relevant groups: those whose interests a significant political actor claims to represent in national politics or that face systematic state discrimination in the political domain. Membership follows from EPR relevance rather than from an explicit filter on political status; observations missing the control variables are removed by listwise deletion. The sample comprises 859 EPR-relevant groups across 150 countries, 1960–2020, or 38,255 group-years with the group-level covariates (the lean sample). I add country-year controls (log GDP per capita, log population, V-Dem polyarchy, V-Dem rule of law [Coppedge et al. 2024], and own group war history) following Asal et al. (2016), Cederman et al. (2010), and Denly et al. (2022). The estimation sample with these controls is 37,665 group-years across 842 groups in 146 countries; the difference is concentrated in a small set of countries lacking the V-Dem indices or a GDP-per-capita series (the online supplement). GDP per capita is from the World Development Indicators (WDI) (World Bank 2024), backfilled with the Maddison Project Database 2023 (Bolt and van Zanden 2023) for pre-1990 gaps; remaining unrecoverable cases (119 group-years) are documented in the online supplement.

Conflict data originate from the Uppsala Conflict Data Program (UCDP)/PRIO Armed Conflict Dataset, as coded by the GROWup team at the group level (Gleditsch et al. 2002). Political status data come from the EPR dataset (Wimmer et al. 2009), which codes each group's access to central executive power annually on the basis of expert assessment. Resource data derive from the PRIO-GRID static geological survey layer (Balestri 2012; Lujala 2009; Lujala et al. 2005; Lujala, Rød, and Thieme 2007; Tollefsen, Strand, and Buhaug 2012), a global grid coding mineral and hydrocarbon deposit presence from geological surveys. The grid data are aggregated to ethnic group settlement areas using area-weighted means, so that each group's resource indicator reflects the endowment of the territory where its members actually reside rather than the country as a whole.

Conflict onset. The outcome is a binary indicator equal to one in the first calendar year of a new armed conflict episode between the ethnic group and the state. An armed conflict episode requires at least 25 battle-related deaths in a calendar year, following the standard UCDP threshold. The onset coding captures only the first year of each new episode, distinguishing initiation from continuation; groups in ongoing conflicts do not contribute an onset observation in subsequent years. The unconditional conflict onset rate is 0.64 percent across all group-years, reflecting the rarity of ethnic conflict onset even in this group-focused sample. As a robustness check, I replicate the main results using the Drop-Ongoing (DO) rather than the default Keep-Ongoing (KO) GROWup episode-coding option; results are substantively identical (Table A3 in the online supplement).

Resource presence. I construct two binary resource indicators from PRIO-GRID static geological surveys. The lootable resource indicator equals one if any of the following deposits are present within the group's settlement area: secondary or primary diamond deposits, vein or placer gold deposits, or gem deposits. These resources are termed "lootable" because they can be extracted by artisanal or semi-industrial

methods and converted through informal markets, potentially generating revenue for nonstate actors (Le Billon 2001; Ross 2004). The petroleum indicator equals one if any hydrocarbon deposits are present in the group's territory. Both resource variables are coded from static geological surveys rather than annual production data, ensuring that the onset of conflict cannot retroactively cause resource presence.

The primary specification applies inclusive coding: PRIO-GRID cells that are missing are treated as zeros, retaining all 38,255 group-years and following the standard approach in the PRIO-GRID literature. Missingness in the static resource layers is substantial; I adopt an inclusive coding convention that treats missing-as-zero, which biases the estimated interaction downward and does not vary systematically with political exclusion. (The online supplement documents the missingness rates, coding rationale, and exclusion-on-missingness regression.) The result is robust to alternative coding: conservative coding with an alternative oil-field count (no missing values) confirms the result (Table A1, column 3, in the online supplement).

Political exclusion. I code a group as politically excluded if it holds any EPR status other than meaningful access to executive power: that is, DISCRIMINATED, POWERLESS, or SELF-EXCLUSION. Discriminated groups (5,397 group-years; 14.1 percent of the sample) face active state-imposed repression or formal discrimination in public life. Powerless groups (16,585 group-years; 43.4 percent) are politically marginal but are not documented as facing active state hostility. Self-excluding groups (600 group-years; 1.6 percent) have excluded themselves from central state power, controlling territory they have declared independent of the central government; I drop these observations from the sample in a robustness specification (Table A2, column 2, in the online supplement) to confirm that their inclusion does not drive the main result.

A potential concern with the discrimination-powerlessness boundary is retroactive coding endogeneity, because expert coder judgments may be influenced by observed mobilization.[2] *The Lootable-Resource Gradient (H1a and H1b)* section discusses this further in context.

Controls. All specifications include the group's relative population share among politically relevant ethnic groups, the group's relative population balance among politically relevant groups in the country, the number of years since the group's last conflict onset, and a binary indicator for prior conflict history. These covariates follow the standard controls in the EPR conflict literature (Cederman et al. 2013). The main estimating equation is presented in equation (1), where μ_i and λ_t denote country and year fixed effects, respectively, $\mathbf{X}_{igt}$ collects the control variables, and ε_{igt} is the error term clustered at the country level:

$$\begin{aligned}\Pr(\text{onset}_{igt}) = \alpha &+ \beta_1 \text{Lootable}_{ig} \times \text{Excl}_{igt} + \beta_2 \text{Oil}_{ig} \times \text{Excl}_{igt} \\ &+ \beta_3 \text{Lootable}_{ig} + \beta_4 \text{Oil}_{ig} + \beta_5 \text{Excl}_{igt} + \mathbf{X}'_{igt}\boldsymbol{\gamma} + \mu_i + \lambda_t + \varepsilon_{igt}. \quad (1)\end{aligned}$$

The status indicator Excl_{igt} enters equation (1) as a single category for compactness; the results decompose it into separate discriminated- and powerless-group

interactions with each resource, holding politically included groups as the omitted base.

Identification Strategy

The central identification challenge is separating the resource-exclusion interaction from other characteristics of conflict-prone groups that may correlate with both resource endowment and political status. Two-way country and year fixed effects absorb time-invariant country-level confounders (institutions, geography, and colonial legacies) and global annual shocks (commodity price cycles, Cold War end, and post-9/11 security environment). Residual identification exploits within-country variation in which groups inhabit resource-rich territories and which groups are politically excluded: 82 of 146 estimation-sample countries contain at least one relevant ethnic group with a resource-rich homeland and at least one without, and 57.1 percent of country-years with more than one relevant group exhibit within-country variation in the lootable indicator (40.9 percent for petroleum). The identifying contrast is cross-group comparison within the same country and year.

A principal remaining threat is reverse causation from resource wealth to exclusion status: governments might preemptively exclude resource-rich groups in anticipation of extraction or to forestall resource-backed secessionism. When I regress the annual exclusion indicator jointly on both resource variables, conditioning on relative group size, relative balance, and peace years with country and year fixed effects, neither lootable resources ($p = 0.25$) nor petroleum ($p = 0.65$) predicts exclusion, inconsistent with widespread strategic targeting. Because the contested-homeland mechanism turns on active *discrimination* rather than exclusion in general, I repeat the test with discrimination as the outcome: neither lootable ($p = 0.09$) nor petroleum ($p = 0.22$) predicts discrimination, and both point estimates are negative, so resource-rich groups are, if anything, *less* likely to be actively discriminated against. The discrimination boundary is thus not assigned in anticipation of resource wealth; resource presence and political status can be treated as conditionally exogenous to one another rather than jointly determined. A complementary pre-trend check addresses the behavioral analog: across the observed transitions from inclusion into exclusion, conflict onset rates in the five years before exclusion begins are statistically indistinguishable by resource status (1.21 percent for resource-rich groups and 1.44 percent for resource-poor; difference $p = 0.80$), with no evidence of anticipatory mobilization.

Two-way fixed effects, reverse-causality nulls on both resource variables for both exclusion (lootable: $p = 0.25$; petroleum: $p = 0.65$) and discrimination (lootable: $p = 0.09$; petroleum: $p = 0.22$), pre-trend equivalence across resource-status cells, lag-sensitivity stability, and Oster (2019) bounding ($\hat{\delta}^*$=5.32, far above the δ^*=1 benchmark at which unobserved selection would have to equal observed selection to nullify the estimate) collectively support a conditional-correlational reading: resource-rich homelands predict elevated conflict risk under exclusion, robust to substantial selection on unobservables. Two residual limitations warrant acknowledgment. The design cannot rule out time-varying country-group factors that jointly predict resource status, exclusion, and conflict; and because resource

variables are coded from geological surveys rather than production data, exogenous variation in resource income over time cannot be exploited.

A third scope limitation is EPR relevance-selection endogeneity. The EPR project codes only groups that are politically relevant (those a significant political actor claims to represent nationally or that face state-led political discrimination), which means the estimation sample excludes many small ethnic communities that have never mobilized. If resource-rich groups are systematically more likely to cross the relevance threshold regardless of their formal political status, the sample is a nonrandom draw weighted toward communities that have already posed credible threats. This article cannot resolve this selection concern with available data; the within-excluded asymmetry should be read as characterizing groups that have already achieved political salience, not as a universal claim about all ethnic minorities.

All specifications are LPMs with two-way fixed effects (Bergé 2018) and standard errors clustered by country.[3] Inference does not depend on LPM asymptotics: a wild cluster bootstrap (Roodman et al. 2019) (999 Rademacher draws) returns $p = 0.005$ and a conditional logit yields log-odds 1.541 ($p<0.001$); see Table A2 in the online supplement.

Results

Descriptive Evidence

Discriminated groups in lootable resource-rich territories experience conflict onset at 2.28 percent annually, more than double the 0.97 percent rate for similar groups without resources (H1a). Included groups show the reverse: 0.23 percent with lootable resources versus 0.42 percent without. This sign reversal is what rival accounts cannot jointly generate. Figure 1 summarizes rates across all six cells; 82 of 146 estimation-sample countries provide the within-country identifying variation the two-way fixed-effects estimator requires.

The Lootable-Resource Gradient (H1a and H1b)

Table 2 disaggregates excluded groups by the state's posture: actively discriminated groups (14.1 percent of the sample) versus merely powerless groups (43.4 percent). The lootable × discriminated coefficient is positive in 100 percent of the 24 analytic specifications in Figure 2 and reaches $p < 0.05$ in 88 percent of them, with a median estimate of 0.015. In the joint model (Table 2, column 1) the lootable × discriminated coefficient is 0.015 (SE = 0.005, $p = 0.004$) while lootable × powerless is 0.002 (SE = 0.002, $p = 0.45$); the Wald test of equality returns $z = 2.54$, $p = 0.011$. The result is similar without the country-year controls (0.017, Wald $p = 0.009$; $N = 38{,}255$). Lagged-exclusion specifications (Table A5 in the online supplement) return $p = 0.014$ at $k = 1$ and $p = 0.020$ at $k = 3$, with the Wald p-value rising gradually at longer lags as effective sample size shrinks. The result is not a knife-edge product of any single specification; the specification curve shows that the

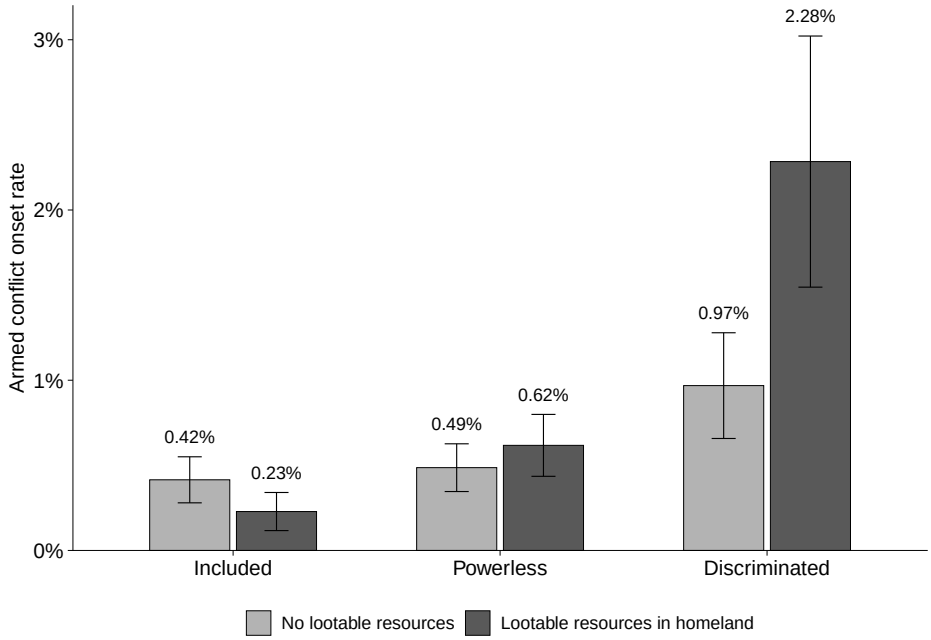

Figure 1: Conditional conflict onset rates by EPR exclusion status and lootable resource presence. Group-years 1960–2020 ($N = 37{,}655$). Self-excluded group-years (600 observations) are dropped from this figure because the contested-homeland mechanism does not apply to voluntary withdrawal from the central political settlement; they are retained in the primary estimation sample (see *The Lootable-Resource Gradient (H1a and H1b)* section). Bars show raw conditional means; error bars show 95 percent confidence intervals. The discriminated $\times$ lootable cell sits well above all others, and the included $\times$ lootable cell sits *below* the included $\times$ no-resources cell, previewing the H1b sign reversal.

within-excluded asymmetry holds across the full universe of plausible analytic choices (Section A.5 in the online supplement).

The sign reversal for included groups. A more demanding test of the same logic uses politically *included* groups as the omitted base. Column 2 of Table 2 reports this specification: the coefficient on lootable resource presence (the marginal effect for included groups) is -0.004 (SE $= 0.001$, $p = 0.005$). The lootable $\times$ discriminated coefficient is $+0.016$ ($p = 0.003$), and lootable $\times$ powerless is $+0.003$ ($p = 0.15$), implying marginal lootable effects of -0.004 for included, $+0.012$ for discriminated, and -0.001 for powerless groups. The cross-category Wald test of equality returns $p = 0.012$. The estimated coefficient for H1b identifies the *sign* of the relationship, which is what the joint sign-pattern test requires. The magnitude itself is bounded by the low baseline conflict rate among included groups: the 0.4 percentage-point shift against a 0.42-percent base rate limits effect-size interpretation but not the discriminating test against rival accounts that predict positive or null effects on included groups. A status-shuffle placebo permutes the discriminated and powerless labels among groups within each country-year (500 replications) while holding the within-country political composition constant. The observed negative lootable coefficient for included groups falls at the lower tail of this within-country null

Table 2: H1a (Within-Excluded Decomposition) and H1b (Included-Group Sign Reversal).

	Dependent variable: Conflict onset (KO)	
	(1) Joint Model (Excluded Base)	(2) Sign Reversal (Included Base)
Panel A: Key interactions		
Lootable × discriminated	0.015***	0.016***
	(0.005)	(0.005)
Lootable × powerless	0.002	0.003
	(0.002)	(0.002)
Lootable × self-excluded		0.016
		(0.028)
Oil × discriminated	−0.002	−0.002
	(0.004)	(0.004)
Oil × powerless	0.007*	0.007**
	(0.004)	(0.004)
Panel B: Status indicators and main effects		
Discriminated	0.004	0.008**
	(0.003)	(0.003)
Powerless	−0.002	0.001
	(0.002)	(0.002)
Self-excluded		0.031
		(0.027)
Lootable (included base)	−0.003*	−0.004***
	(0.002)	(0.001)
Oil	0.001	0.001
	(0.002)	(0.002)
Panel C: Controls		
Relative group size, relative balance, peace years, own war history, log GDP per capita, log population, V-Dem polyarchy, V-Dem rule of law.		
Panel D: Fit statistics		
Wald test (H_0: Discrim. = Powerless)	$p = 0.011$	$p = 0.012$
Observations	37,665	37,665
Country FE	Yes	Yes
Year FE	Yes	Yes

Notes: Linear probability model with country and year fixed effects. Sample: 859 groups in 150 countries, 1960–2020; listwise deletion on the augmented control set (log GDP per capita, log population, V-Dem polyarchy, V-Dem rule of law, own group war history) leaves 37,665 group-years (842 groups, 146 countries). GDP per capita is from WDI where available and backfilled with the Maddison Project Database 2023 (Bolt and van Zanden 2023) where WDI coverage is sparse; the remaining 119 group-years with no available GDP-per-capita series are dropped by listwise deletion. Standard errors clustered by country (146 clusters) in parentheses. Controls also include relative group size, relative balance, and peace years. Column (1) tests H1a (within-excluded asymmetry): excluded groups enter as separate discriminated/powerless indicators, and the Wald test compares their lootable interactions; the omitted base in this column comprises politically included and self-excluded groups. Column (2) tests H1b (included-group sign reversal): politically included groups alone are the omitted base, so the row "Lootable (included base)" reports the marginal effect of lootable resources for included groups; self-excluded groups are interacted with both resource indicators to retain the full political-status gradient. The Wald tests compare the Lootable × Discriminated and Lootable × Powerless coefficients within each column's joint model.
$^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

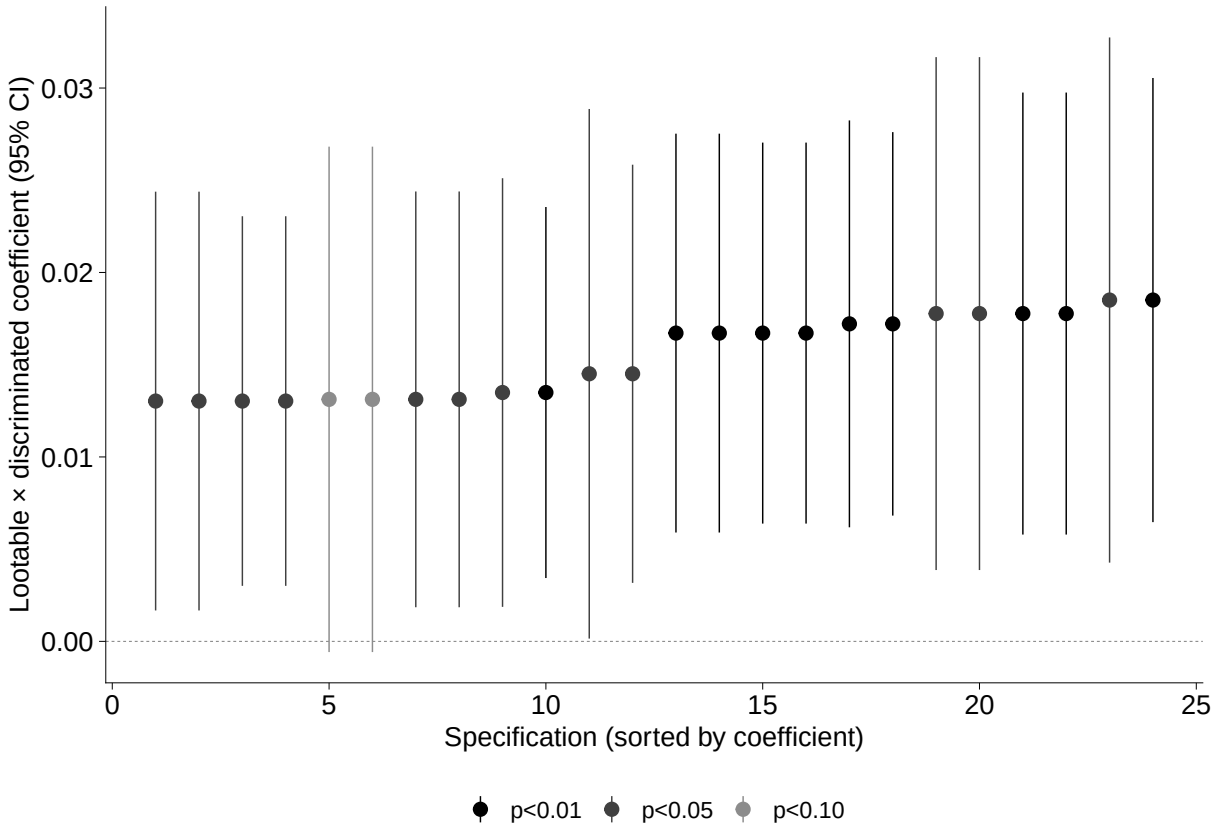

Figure 2: Specification curve for the lootable × discriminated interaction across 24 analytic choices. Coefficients sorted by magnitude; vertical bars are 95 percent confidence intervals. All specifications use the article's primary inclusive resource coding; conservative-coding specifications are excluded due to mechanical collinearity (the online supplement). 100 percent of specifications are sign-positive; 88 percent reach $p < 0.05$; median estimate is 0.015.

($p_{\text{perm}} = 0.032$, one-sided), consistent with a real signal rather than a within-country sorting artifact.

The HI variant of grievance theory (Cederman et al. 2011, 2013) can accommodate the within-excluded asymmetry on its own; the included-group sign reversal is what it cannot generate. The joint sign pattern across the three categories (positive amplification under discrimination, null under powerlessness, sign reversal under inclusion) matches Table 1 only for the contested-homeland account; rebel-financing and grievance accounts each generate at least one prediction that the data contradicts. Self-excluded groups (1.6 percent of group-years) are retained in the estimation sample but not interpreted under the joint sign-pattern test, because voluntary withdrawal from the central political settlement does not trigger the contested-homeland mechanism; Table A2, Column 2, in the online supplement shows that the result is unchanged when they are dropped. Across the three categories, only the contested-homeland account matches the observed $+/0/-$ pattern; rebel-financing is contradicted in two cells, grievance variants in at least one.

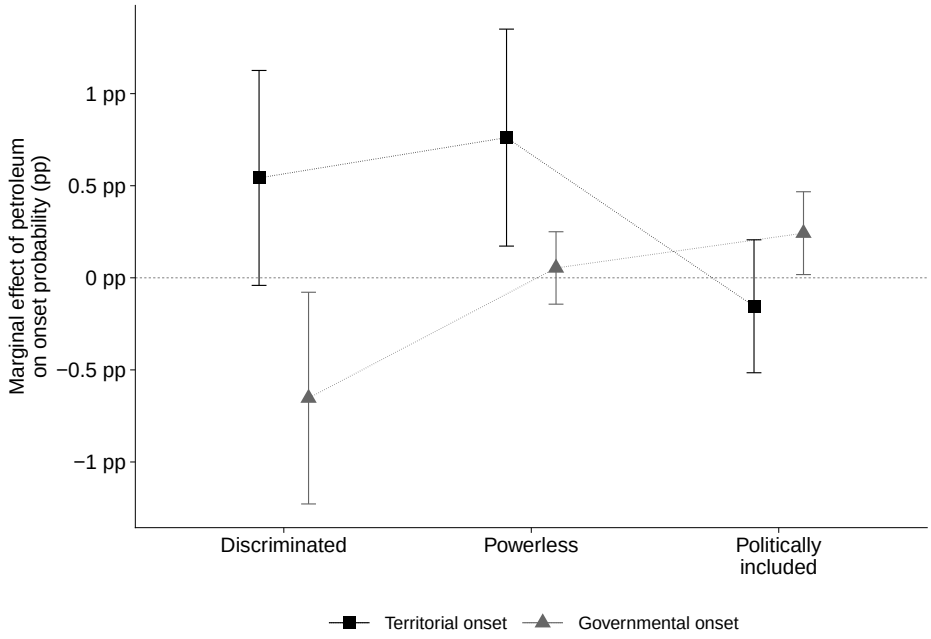

Figure 3: Marginal effect of petroleum on conflict onset, by conflict type and political status. Points report the *full marginal effect* (main effect plus the relevant interaction) of moving from petroleum-absent to petroleum-present, evaluated separately for discriminated, powerless, and politically included groups, with 95 percent confidence intervals. Estimates come from the three-category territorial and governmental models that underlie Table 3; included groups are the omitted base, so each point is the sum of the petroleum main effect and that status's petroleum × status interaction. The conflict-type substitution is visible for both excluded categories: for discriminated groups petroleum raises territorial onset (+0.5 pp) and lowers governmental onset (−0.7 pp, $p = 0.026$); for powerless groups it raises territorial onset (+0.8 pp, $p = 0.011$) while leaving governmental onset essentially unchanged (+0.1 pp). The two excluded categories shift alike, so the substitution tracks resource geography, not the discrimination boundary that governs onset. For *included* groups the pattern reverses: petroleum is associated with slightly lower territorial onset (−0.2 pp) and higher governmental onset (+0.2 pp, $p = 0.037$), consistent with infighting over the distribution of central-state resource revenue.

Petroleum and Conflict-Type Substitution (H2)

H2 tests the framework on a different margin, and I run it with exactly the three-category decomposition used for H1. Where H1 asks whether *lootable* resources raise conflict onset differently across discriminated, powerless, and included groups, H2 asks whether *petroleum*, a geographically fixed point resource, shifts the *type* of conflict (territorial vs. governmental) across the same three categories. If the discrimination boundary that organizes onset also organizes conflict form, the petroleum substitution should be concentrated among discriminated groups, as the onset effect is.

It does not. Table 3 estimates the petroleum and lootable interactions jointly across the three categories, with politically included groups as the base. Petroleum moves discriminated and powerless groups toward territorial conflict in equal measure (Fig. 3), and a Wald test cannot separate them ($p = 0.39$; Fig. 4). Among

Table 3: Territorial versus Governmental Conflict Onset, by Political Status (H2).

	(1) Territorial Onset	(2) Governmental Onset
Panel A: Petroleum (point resource) interactions		
Petroleum × discriminated	0.007^{*}	-0.009^{***}
	(0.004)	(0.003)
Petroleum × powerless	0.009^{**}	-0.002
	(0.004)	(0.002)
Panel B: Lootable (mobile resource) interactions		
Lootable × discriminated	0.005	0.010^{**}
	(0.004)	(0.004)
Lootable × powerless	0.001	0.001
	(0.002)	(0.001)
Panel C: Main effects (politically included base)		
Petroleum	-0.002	0.002^{**}
	(0.002)	(0.001)
Lootable	-0.001	-0.002
	(0.002)	(0.001)
Panel D: Petroleum conflict-type substitution (terr. − gov.)		
Discriminated	0.016^{***}	(0.005), $p = 0.003$
Powerless	0.011^{***}	(0.004), $p = 0.004$
Wald (H_0: discriminated = powerless substitution)		$p = 0.39$
Controls: relative group size, relative balance, peace years, own war history, log GDP/capita, log population, V-Dem polyarchy and rule of law.		
Observations	37,665	37,665
Country FE	Yes	Yes
Year FE	Yes	Yes

Notes: Linear probability models with country and year fixed effects, estimated jointly with discriminated and powerless interactions; the omitted base comprises politically included and self-excluded groups. Sample: 859 groups in 150 countries, 1960–2020; listwise deletion on the augmented control set (with Maddison Project 2023 backfill) leaves 37,665 group-years (842 groups, 146 countries). Standard errors clustered by country (146 clusters) in parentheses. *Territorial onset* = first year of conflict over autonomy or secession; *Governmental onset* = first year of conflict over capture of the central state. The petroleum conflict-type substitution (Panel D) is estimated in a stacked specification with equation-specific country and year fixed effects, so that the cross-equation covariance enters the standard error and Panel D reproduces the difference between the Panel A coefficients. It is positive and significant for both discriminated and powerless groups, and a Wald test cannot distinguish the two ($p = 0.39$): unlike the lootable–onset gradient in Table 2, the petroleum substitution does *not* depend on the discrimination boundary.
$^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

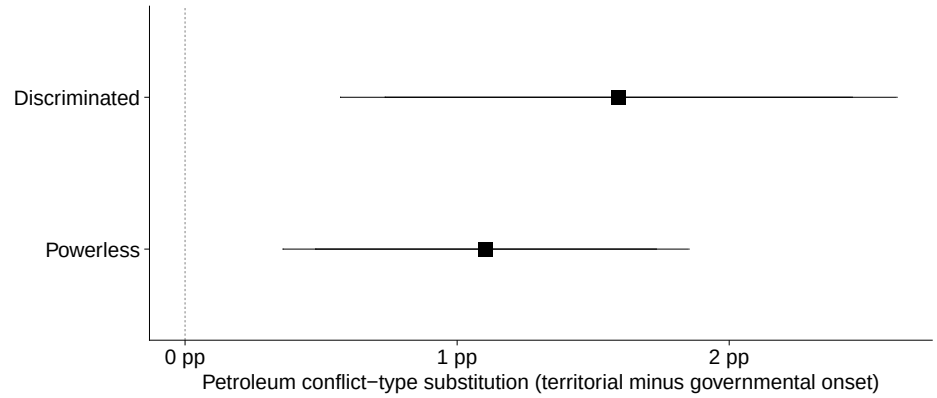

Figure 4: Petroleum conflict-type substitution, by political status. Each point is the cross-equation difference between petroleum's effect on territorial onset and its effect on governmental onset (the substitution quantity reported in Table 3, Panel D), estimated jointly in a stacked specification so that the cross-equation covariance enters the SE. A positive value means petroleum shifts mobilization *toward* territorial and *away from* governmental conflict. Thick bars are 90 percent confidence intervals and thin bars are 95 percent confidence intervals. The substitution is positive for both discriminated (+1.6 pp) and powerless (+1.1 pp) groups, and a Wald test cannot distinguish them ($p = 0.39$): unlike the lootable–onset gradient of H1, the petroleum substitution does not depend on the discrimination boundary.

discriminated groups it raises territorial onset (0.007, SE = 0.004, $p = 0.06$) and lowers governmental onset (−0.009, SE = 0.003, $p = 0.006$), a cross-equation substitution of 0.016 (SE = 0.005, $p = 0.003$); among powerless groups the figures are nearly identical (territorial 0.009, SE = 0.004, $p = 0.01$; substitution 0.011, SE = 0.004, $p = 0.004$).[4] Petroleum reshapes the form of conflict for excluded groups, but it does so regardless of discrimination: powerless groups, never singled out by the state, make the same territorial shift as the discriminated. This is the reverse of what a single mechanism predicts, and H2 is rejected.

Reconciling this substitution with the onset gradient of H1 is the task of the next section.

Two Mechanisms: Onset and Form

The two results describe two different forces. Discrimination decides whether a resource-rich excluded group fights: lootable wealth and a closed institutional exit turn grievance into armed onset for discriminated groups, and leave it dormant for the merely powerless. Geography decides what the fighting is over: a spatially fixed deposit makes control of the territory, rather than capture of the center, the rational prize for any excluded group, coerced or not. Discrimination selects onset and geography selects form.

The resource type bears this out. Lootable wealth, easily monetized and mobile, underwrites a contest for the revenue-distributing center. For discriminated groups it raises *governmental* onset (0.010, SE = 0.004, $p = 0.02$) rather than territorial, and for powerless groups it raises neither. Spatially fixed petroleum instead makes the territory itself the prize.

The channel tracks the resource, not the group's treatment, and it even reverses under inclusion. Among politically included groups petroleum is associated with *lower* territorial onset (-0.002, $p = 0.40$) and *higher* governmental onset (0.002, SE $= 0.001$, $p = 0.04$). Groups that already hold a share of central power contest oil rents *within* the governing coalition rather than secede, the "infighting" the EPR literature documents and UCDP codes as governmental (Fig. 3). The same territorial pull appears at the interstate level in Caselli et al. (2015)'s finding that oil deposits lying close to, and asymmetrically across, national borders predict interstate conflict; Hunziker and Cederman (2017) find it among excluded groups as well, though without the status decomposition that shows it to be geographic rather than coercion-specific.

The geographic reading rests on UCDP's conflict-type coding, which is not fully independent of the covariates: coders classify onset as territorial or governmental after the fact, partly on the geographic character of the contested zone, so petroleum-rich excluded groups may receive territorial codes partly because the disputed territory is visibly oil-bearing. Two features of the design address this. First, UCDP's incompatibility code reflects the parties' stated aims (sovereignty over territory vs. control of the central government), not the resource endowment per se; the geographic fixity of petroleum creates the territorial incentive, but the UCDP coder records the incompatibility that follows from it, which is exactly the causal channel the theory describes. Second, and decisively, a pure coding-endogeneity artifact would bias both conflict-type coefficients in the same direction (upward), whereas the data show positive territorial and negative governmental coefficients for the same groups in the same country-years: a systematic misclassification cannot produce opposite signs simultaneously.

Together, the two mechanisms fit the full pattern: the onset gradient that runs along the discrimination boundary, the petroleum substitution that does not, and its reversal under inclusion. Because the two run on different inputs, they can appear together or apart. A discriminated group over a diamond field meets the first condition but not the second; an excluded group over an oil field it cannot loot meets the second but not the first. Where active discrimination and a fixed deposit coincide, both channels are live at once, and mobilization is both most likely to occur and most likely to turn secessionist. Those homelands are where the sharpest cases sit, and separating the two mechanisms there is the open problem this leaves for future work.

Robustness

The full robustness battery is in the online supplement; the finding holds across an organized 24-specification spec curve (Fig. 2; 100 percent sign-positive, 88 percent at $p < 0.05$, responding to Hegre and Sambanis [2006]), alternative outcome codings (Table A3 in the online supplement), a conservative oil-field count, triangulation against Denly et al. (2022)'s independent production-based resource data (Wald $p = 0.044$ under lean controls, $p = 0.058$ augmented; Table A6 in the online supplement), and a wild cluster bootstrap with 999 Rademacher draws that returns $p = 0.005$ for the main interaction (Table A2 in the online supplement). Lagging

the exclusion indicator one to three years before observed mobilization preserves the within-excluded asymmetry (Wald $p = 0.014$ at $k = 1$, $p = 0.020$ at $k = 3$; Table A5 in the online supplement), bounding within-year EPR backward-coding as a mechanical source. Discriminated and powerless groups differ on 10 of 13 observables (Table A7 in the online supplement); adding interactions between lootable resources and the four most imbalanced covariates weakens the main coefficient by 18 percent but preserves significance ($p = 0.034$; Table A8 in the online supplement). The attenuated estimate of 0.0125 represents a 1.25 percentage-point annual elevation in conflict-onset probability, which remains substantively large relative to the dependent variable's low baseline rate. Across all 859 leave-one-group-out (LOGO) specifications (Table A4 in the online supplement), the lootable $\times$ discriminated coefficient stays in $[0.0138, 0.0163]$ and reaches $p < 0.05$ in every one. Trimming the top 1–2 percent of observations by Cook's distance yields estimates of 0.0153–0.0160, all within the LOGO range, confirming that no small cluster of influential conflict episodes drives the result. The LOGO and Cook's distance signals concentrate in paradigmatic cases the theory predicts (the Kurds in Iran, the Karenni in Myanmar, the Tutsi-Banyamulenge in eastern DRC, the Africans of Zimbabwe, and the Baluchis of Pakistan), expected if the mechanism operates only where the boundary condition holds most strongly.

Discussion and Conclusion

The contested-homeland argument is a theory of conflict onset, and the evidence for it is the within-excluded asymmetry. Lootable resources amplify conflict only where the state has revealed coercion through active discrimination, and not where exclusion is merely passive powerlessness (H1a); the Wald test of equality between the discriminated and powerless interactions rejects at the 5 percent level, and the signal is concentrated in the cases the boundary condition selects most strongly. The same resources predict *lower* conflict for politically included groups, whose institutional channels remain open (H1b).

The conflict-type results mark where that boundary stops. Petroleum shifts discriminated and powerless groups toward territorial rather than governmental conflict by the same amount, and the two cannot be told apart, so the discrimination boundary that governs onset does not govern the form of conflict. Discrimination selects onset and geography selects form. Separating these two channels, which the resource-conflict literature has largely run together, is a distinct contribution.

What this changes for the resource-curse debate. Country-level studies aggregate over all group-state dyads within a country, mechanically mixing the discriminated-group positive, the powerless-group null, and the included-group negative into a single country mean. The cross-national resource-curse literature has spent two decades looking for the country-level institutional or economic feature that determines whether resource wealth produces conflict; the country panel is the wrong unit for that question. The same country, in the same year, with the same national institutions and commodity prices, produces conflict from resource-rich territories of discriminated groups and quiescence from resource-rich territories of included

groups. The Mehlum et al. (2006) framework and its successors locate the "curse" in country-mean institutional quality; the mechanism documented here is also institutional but operates at the group-state level. Country-mean institutional quality cannot determine whether a specific group's relationship with the central state is one of institutional inclusion or revealed coercion. One country can house both configurations simultaneously, and the resource-conflict association partitions along that internal boundary. Country-level prescriptions (transparency initiatives, sovereign wealth funds, and Extractive Industries Transparency Initiative [EITI] accession) target the country-mean and cannot operate on the within-country group-state dyad where the conflict mechanism actually operates.

The deeper payoff comes from joining two literatures that have developed largely in isolation from one another. The resource-conflict tradition treats the resource endowment as the causal variable and the polity as background; the ethnopolitical-conflict tradition built on EPR (Cederman et al. 2010; Wimmer et al. 2009) treats group-state political status as the causal variable and resources as background. Neither alone predicts the sign pattern documented here. Asking instead how the two interact at the group-state level (whether a resource sits in the homeland of a group the state actively coerces, merely sidelines, or shares power with) is what renders the resource-conflict association legible: the same endowment is a *casus belli*, a nonevent, or a deterrent depending on the political relationship in which it is embedded.

The fusion also sharpens what each tradition has missed. For the ethnopolitical-conflict literature, political status is not a uniform treatment: its conflict consequences scale with the material stakes in a group's territory, so that the same act of discrimination is far more dangerous over an oil field or a diamond district than over agricultural land. And for the study of armed conflict more broadly, grievance and opportunity emerge not as rival explanations but as complementary inputs to one decision: active discrimination supplies both the grievance and the closure of institutional exit, while lootable resources supply the financing and the prize. The contested homeland is where the two coincide.

The group-level mechanism changes how several country-level results should be read rather than overturning them. Null or unstable resource-curse coefficients in cross-national panels (e.g., Brunnschweiler and Bulte 2009; Cotet and Tsui 2013) are exactly what offsetting group-level effects (positive for discriminated homelands, null for powerless ones, negative for included ones) should produce once averaged within countries; the country-panel null is evidence *for* a heterogeneous group-level mechanism, not against a resource effect. I do not claim prior findings are wrong; I claim they are aggregates of a mechanism that becomes interpretable only at the dyadic level.

Finally, the mechanism is narrow in scope. Most of the world's group-state dyads hold no significant lootable or point resources, and for them the resource-amplification channel is simply absent; conflict there, where it arises, runs through the other determinants the EPR literature identifies, and the null coefficients for these resource-poor cells confirm that resources do not matter where the theory says they should not. I do not claim that resources drive ethnic conflict in general (the large majority of ethnic conflicts involve no resource stakes), only that where

extractive resources and active discrimination coincide in a homeland, they interact to raise conflict risk sharply.

Scope conditions. The mechanism does not operate everywhere. Two scope conditions are worth stating explicitly. First, the contested-homeland logic presumes a state that retains coercive capacity over the territory in question; where the state has effectively withdrawn (collapsed states, hinterlands with no functional state presence), the mechanism's first-order prediction (that armed mobilization is the residual channel against state coercion) becomes incoherent because there is no state to contest. Cross-border refugee flows and spillover from neighboring conflicts (Salehyan and Gleditsch 2006) may also blur the dyadic mechanism in regions of overlapping civil wars; the present design absorbs cross-country contamination through country fixed effects but does not model it directly. Second, where institutional revenue-sharing is genuinely operative (federations with constitutionally fixed transfers, autonomy agreements with revenue-allocation clauses), the included-group prediction may apply partially to formally excluded groups, attenuating the within-excluded asymmetry. Empirically, the leave-one-group-out diagnostic in the *Robustness* section confirms that the result is not driven by the cases where these scope conditions most plausibly apply, and the LPM coefficient distribution across all 859 groups stays within a narrow range.

Policy implications. For analysts working in conflict early-warning, the result identifies a high-risk configuration that is actionable in the sense that it can be observed in advance: a resource-rich ethnic homeland where the state's political relationship with the resident group is coded as *actively discriminatory* rather than *merely powerless*. Standard early-warning systems flag resource-rich excluded groups as elevated-risk cases regardless of the discrimination-powerlessness boundary; the results here imply this aggregation hides substantial heterogeneity. Within excluded groups, the risk concentration is on the discriminated side. An early-warning analyst monitoring Sudan in 2008, for example, would have correctly elevated the Nuer homeland's oil-rich territory above the average excluded-group baseline, because both criteria were satisfied: active discrimination and resource extraction in the homeland. The diagnostic value lies in the joint condition: neither resource extraction alone nor discrimination alone is sufficient.

The result does not support the further causal inference that any given intervention will reduce risk; translating the diagnostic into intervention requires identification strategies the present design cannot supply.

Limitations. The two-way fixed-effects design cannot account for time-varying country-group confounders, and the static coding of resource variables means the result identifies an endowment-and-status pattern rather than an income-shock mechanism. The EPR discrimination-powerlessness boundary rests on expert assessment; replication with alternative coding schemes would strengthen confidence in the heterogeneity estimate.

Future research. The diagnostic finding that PRIO's deposit catalog misses 1,475 group-years of active production (421 of them in politically excluded homelands) suggests that closing the data gap with production-based sources may reveal additional within-excluded heterogeneity in specific resource sectors that the present panel cannot detect. As group-level production data improve, sub-group analysis and cross-national comparison of revenue-sharing institutions and their effect on the resource-exclusion-conflict nexus are natural extensions the present design enables.

Notes

1 The mechanism can also be stated in rationalist terms. Discrimination forecloses credible commitment to any future power-sharing arrangement, converting the extraction contest into an armed-conflict equilibrium (Fearon 1995; Powell 2006; Walter 2009). The sociological and rationalist accounts are complementary: the former explains why coercive exclusion becomes the state's chosen posture; the latter explains why, once adopted, it escalates to armed conflict.

2 Reclassification rates from *powerless* to *discriminated*: 0.19 percent more than five years before first conflict onset; 0.64 percent within five years. The interaction of near-conflict timing with lootable status is null ($\hat{\beta} = 0.004$, $p = 0.33$), so the reclassification spike is not concentrated among resource-rich groups.

3 Recent heterogeneous-staggered difference-in-difference concerns (Callaway and Sant'Anna 2021; Goodman-Bacon 2021; Roth et al. 2023) do not bind here: the treatment variable is a time-varying group-year status, not a discrete onset date, and the identifying contrast is between groups with different status-resource combinations in the same country-year rather than between cohorts treated at different times.

4 The coefficients reported here are the petroleum $\times$ status interaction terms from the territorial and governmental onset models (Table 3). Figure 3 plots the implied marginal effects, which add the petroleum main effect to each interaction, so its point estimates differ slightly from the interaction coefficients quoted here.

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