Title

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Publication Date

2020-10-23

DOI

10.26085/C3QG6W

Peer reviewed

Series Name: WPS
Paper No.: 139
Issue Date: 23 Oct 2020

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ESOC Working Paper No. 21
September 17, 2020

ABSTRACT

Scholars of the resource curse argue that reliance on primary commodities destabilizes governments: price fluctuations generate windfalls or periods of austerity that provoke or intensify civil conflict. Over 350 quantitative studies test this claim, but prominent results point in different directions, making it difficult to discern which results reliably hold across contexts. We conduct a meta-analysis of 46 natural experiments that use difference-in-difference designs to estimate the causal effect of commodity price changes on armed civil conflict. We show that commodity price changes, on average, do not change the likelihood of conflict. However, there are cross-cutting effects by commodity type. In line with theory, we find price increases for labor-intensive agricultural commodities reduce conflict, while increases in the price of oil, a capital-intensive commodity, provoke conflict. We also find that price increases for lootable artisanal minerals provoke conflict. Our meta-analysis consolidates existing evidence, but also highlights opportunities for future research.

Keywords: resource curse, armed conflict, commodity prices, meta-analysis

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Author Acknowledgements
We are grateful to Emily Allendorf, Albert Na, Jacquelyn Nguyen, Jinqi Song, and Pei Yin Teo for excellent research assistance. The study pre-analysis plan is registered with EGAP at http://egap.org/registration/4993 (further details and code registered at OSF at https://dx.doi.org/10.17605/OSF.IO/DY9UF). Deviations from the PAP are described in Appendix H. We thank the many authors included in this meta-analysis for generously sharing data, replication code, and other statistics. We thank Alex Coppock, Chad Hazlett, Macartan Humphreys, Dan Posner, Anouk Rigterink, Michael Ross, Renard Sexton, Tara Slough, and Yael Zeira as well as seminar participants at APSA, CEGA, ESOC, UCLA, and UCSB for their helpful comments. Blair and Christensen received support from the Project on Resources and Governance.

Citation Information
Blair, G., D. Christensen, and A. Rudkin. (2020). *Do Commodity Price Shocks Cause Armed Conflict? A Meta-Analysis of Natural Experiment* (ESOC Working Paper No. 21). Empirical Studies of Conflict Project. Retrieved [September 17, 2020], from http://esoc.princeton.edu/wp21.

Half of all countries depend economically on primary commodities such as crude oil and wheat, a 20-year high (UNCTAD 2019).[1] Policymakers worry that such dependence stymies economic growth and leaves countries vulnerable to price shocks; the UN warns that commodity-dependent states will not meet its Sustainable Development Goals.

Decades of social science research underlie these concerns. Scholars argue that these countries experience three maladies: macroeconomic shocks from volatile commodity prices (Gelb 1988); reduced state capacity and accountability (Mahdavy 1970); and armed conflict (Collier and Hoeffler 2004).

We focus on whether changes to the value of primary commodities cause armed civil conflict in producing regions, a claim which has inspired an outpouring of theoretical and empirical work. Since 2002, we count over 350 empirical papers that study the relationship between armed civil conflict and the value of primary commodities, a body of work that has collectively generated over 20,000 citations (see Appendix Figure A.1). We examine work that studies three outcomes related to armed civil conflicts: onset (start of conflict), incidence (presence of conflict), and intensity (number of battles or fatalities).

The increased attention has led to debate about when, or even whether, commodity price shocks affect armed conflict.[2] Prominent studies offer contradictory accounts: Dube and Vargas (2013), for example, find that violence increases in Colombia's oil-producing municipalities as the international price of oil rises. By contrast, Bazzi and Blattman (2014: 1) state that "[p]rice shocks have no effect on new conflict, even large shocks in high-risk nations." However, studies often examine different sets of commodities, outcomes, and countries, which may explain apparently incongruous findings.

We conduct a formal meta-analysis of natural experiments.[3] We proceed in four steps. First, we conduct an expansive literature search that yields over 3,300 study records. Second, we screen studies on research-design and topical grounds: the 46 included studies (102 estimates) quantitatively analyze the effect of plausibly exogenous variation in world commodity prices on armed civil conflict using a generalized difference-in-difference design with unit and time fixed effects. Third, we standardize estimates to place coefficients on a common scale. When needed, we reanalyze study data to increase uniformity (e.g., when authors report coefficients from probit models). Finally, we use two standard meta-analytic techniques to evaluate prominent hypotheses about whether and which primary commodity prices affect armed conflict.

When we pool studies across commodity types, we find no effect. The same is true when we restrict attention to estimates that bundle together multiple types of commodities. It does not appear that commodity price increases uniformly generate windfalls that make the state or other territory a prize worth fighting for.

[1] UNCTAD defines primary commodities as goods that are "largely unprocessed or unrefined," which includes "farming, forestry, fishing, and the extractive industries" (UNCTAD 2018). A country is classified as dependent when these commodities account for over 60 percent of exports.

[2] Studies in our corpus rely on a two-way fixed effects estimation, leveraging changes in prices. Researchers commonly refer to these changes as shocks, which can be positive or negative.

[3] The panel research designs we rely on are referred to alternatively as "natural experiments" and "quasi-experiments." We use the first for consistency.

The overall null effect comprises cross-cutting effects for different commodities. First, price increases for agricultural commodities reduce the likelihood of armed conflict, while price increases for oil and gas have the opposite effect. These divergent results match theoretical predictions that price increases for labor-intensive commodities such as agricultural goods generate employment and, thus, raise the opportunity cost of fighting (Dal Bó and Dal Bó 2011). By contrast, higher prices for capital-intensive goods like oil and gas boost the returns to fighting without offsetting opportunities for legal employment. Second, we find that price increases for artisanal minerals such as alluvial diamonds and gold increase the likelihood of armed conflict. This supports arguments that such commodities are especially "lootable" (shorthand for features that reduce the costs that rebels pay to appropriate production) and, thus, likely to provoke conflict when prices increase (Snyder and Bhavnani 2005; Rigterink 2020).

Meta-analyses remain rare in political science, especially for observational work. We count just five meta-analyses published in the top three political science journals between 1999–2018 (see Appendix I). Only one synthesizes exclusively observational research. A recent meta-analysis, O'Brochta (2019), studies questions similar to our own.[4] We note several key differences: most importantly, the analysis omits all studies in our sample by excluding work on commodity prices and does not screen studies based on their research design (see Appendix A.4). O'Brochta is particularly interested in how different analysis decisions affect authors' findings. By contrast, we attempt to standardize the analysis across our studies in order to test theoretical claims about how effects vary by commodity type.

1. Commodity Prices and Conflict: Theoretical Predictions

The outpouring of empirical research on primary commodities and conflict builds on rationalist, economic theories of civil war. Keen (1998: 11) argues that "internal conflict persisted not so much *despite* the intentions of rational people, as *because* of them. The apparent 'chaos' of civil war can be used to further local and short-term interests. These are frequently economic." In short, economic interests often motivate people to form and join armed groups that challenge the state (for a critique, see Kalyvas 2003).

Control of natural resources is among the most common economic explanations for conflict (for a review, see Ross 2004). Well-known formal models predict that the likelihood of armed conflict increases with the value of primary commodities (e.g., Besley and Persson 2011). The prediction about natural resources builds on a more general insight: increasing the value of the "prize" to be won by controlling the state induces conflict over who governs (see also Fearon and Laitin 2003; Garfinkel and Skaperdas 2007).[5] Laitin (2007: 22) offers a simple summary of these arguments: "If there is an economic motive for civil war in the past half-century, it is in the expectation of collecting the revenues that ownership of the state avails." This, Laitin argues, accounts for the strong empirical association between oil and civil war, but the logic extends to other primary commodities that generate government revenues and should be most apparent when these commodities command high prices, leading to the first hypothesis that has been commonly tested in the empirical literature:

[4] Ahmadov (2014) conducts a meta-analysis on oil wealth and democracy, another aspect of the resource curse.

[5] We note two more specific variants of the rapacity hypothesis: (1) rebels sell "booty futures" to finance rebellion (Ross 2004); or (2) "greedy outsiders" (neighboring states or foreign firms) finance rebellions (Humphreys 2005).

(H1) **Rapacity**: Increases in the prices of primary commodities raise the likelihood of conflict in places producing those commodities.

A number of scholars argue, on the other hand, that commodity price increases should have no — or even a negative — effect on armed conflict. Governments, they argue, use the revenues generated by rising primary commodity prices to build state capacity and, thus, deter would-be challengers. Models of autocratic politics argue that autocrats use resource revenues to buy off or eliminate potential challengers, limiting instability (Bueno de Mesquita and Smith 2010).

These first two effects — sometimes termed the "state prize" and "state capacity" effects — do not depend on which commodities generate windfalls. Yet, a growing body of work argues that commodity prices have varied effects, depending on how different commodities are produced. Prominently, Dal Bó and Dal Bó (2011) predict that price increases for labor-intensive commodities reduce armed conflict. Higher prices for such commodities generate gainful employment, raising the opportunity cost of conflict and drawing would-be combatants into the productive sector. By contrast, higher prices for capital-intensive commodities lower the opportunity cost of conflict. The returns to appropriation rise, for example, as oil theft becomes more lucrative, without offsetting increases in legal employment. These arguments produce a second, commonly tested hypothesis:

(H2) **Opportunity Cost**: Increases in the prices of labor-intensive (capital-intensive) primary commodities lower (raise) the likelihood of conflict in places producing those commodities.

Commodities also vary in their "lootability," characteristics that affect the costs armed groups or the state pay to appropriate production. Lootable primary commodities have a high value-to-weight ratio, require few specialized inputs like high-skill labor or physical capital to produce, and cannot be easily defended (Snyder and Bhavnani 2005). Artisanally-mined diamonds are exemplary: small, precious stones can be easily transported; unskilled labor is the primary input; and alluvial diamond fields can cover large areas, making them costly to fortify (Rigterink 2020: 92). Scholars have argued that higher prices for lootable commodities provoke conflict, providing a third hypothesis:

(H3) **Lootability**: Increases in the prices of lootable primary commodities raise the likelihood of conflict in places producing these commodities.[6]

Testing (H2) and (H3) requires information about whether a particular primary commodity is labor-intensive or lootable. Though we planned to classify commodities along these dimensions, authors rarely directly measure either feature.[7] Instead, we follow the literature in associating these features with particular types of commodities (see Table 1 and Appendix A.9). We note three challenges. First, this classification does not capture heterogeneity within types (e.g., crops can vary in capital intensity). Second, differences across commodity types that the literature attributes to lootability and labor- and capital-intensity could

[6] Our pre-analysis plan discussed but did not register H3.

[7] We also planned to code commodity "taxable" commodities, but authors did not consistently code this feature, and we could not independently code "taxability" for most commodity-country pairs.

Table 1: Commodity Classifications and Predicted Effect Direction from Each Hypothesis

	Characteristics		Predicted Direction		
Commodity Type	Labor-intensive	Lootable	(H1)	(H2)	(H3)
Pooled (average of commodities)	Mix	Mix	+	+/-	+/0
Agriculture	✓		+	–	0
Artisanal Minerals	✓	✓	+	–	+
Commercial Minerals			+	+	0
Oil & Gas		✓	+	+	+/0
Bundle of Multiple Types	Mix	Mix	+	+/-	+/0

be confounded by other unmeasured characteristics. Third, once oil is extracted and in transport, it takes on some lootable features: long stretches of pipeline are costly to defend and can be attacked with few specialized inputs. The lootability of oil, thus, varies along its supply chain.

The literature on natural resources and conflict suffers from what Humphreys (2005: 510) calls "an embarrassment of mechanisms." We test many prominent claims, but not all. The studies we examine focus on (nearly) contemporaneous effects of commodity price changes on conflict and, thus, do not speak to processes that unfold over long periods: Mahdavy (1970), for example, argues that oil wealth reduces domestic taxation and, over the long term, undermines state capacity; Collier and Hoeffler (2004) note long-standing grievances in resource-rich regions.

2. Research Design

2.1 Data Collection

To generate the most complete universe of studies, we combine three approaches: (1) we run keyword searches on Google Scholar; (2) include studies citing prominent early works; and (3) publicly solicit recent and unpublished work. This yields 3,346 studies (see Table 2).

Our topical filter requires that studies include a quantitative analysis where armed conflict is the dependent variable and commodity prices are an independent variable. Among 376 relevant studies, our research design filter retains 46 natural experiments that leverage plausibly exogenous price variation. These studies represent 201 countries and 10,926 unique country-years.[8] Included countries are on average 40% as wealthy, somewhat more unequal, two-thirds more prone to conflict, and somewhat less democratic than the world at large; they more closely resemble those countries that experienced an intra-state conflict in the post-war period (see Appendix B.3). Identification relies on the inclusion of unit and time fixed effects to absorb time-invariant confounds and global shocks.[9] This second filter increases the internal validity of

[8] In Appendix B.2, we quantify the data overlap between studies by calculating the "effective number" of countries (138), and country-years (8,796). No particular country or country-year has outsized influence.

[9] A burgeoning literature studies causal identification in two-way fixed effects models and highlights the additive constant-effects functional form assumption (e.g., Imai and Kim 2020).

included studies. We retain one estimate per paper for every commodity and conflict type (onset, incidence, and intensity) following pre-specified rules (see Appendix A.5).

Table 2: Stages of Filtering and Number of Studies Selected

	Criteria	Studies	Estimates
Search	Keyword, Citation Network, or Public Call	3,346	
Topical Filter	DV: Armed Civil Conflict, IV: Commodity Price	376	
Research Design Filter*	Leverages Plausibly-Exogenous Variation in Commodity Prices	46†	
Partial R	Information to Compute Partial R	46	102
Included in Meta-analysis	Statistics to Standardize Effect Size	37	88

* second filter also requires that the study uses a fixed effects panel model; † two working papers were abandoned

Together, these two filters ensure the conceptual comparability of study estimates. We take two additional steps to ensure that the estimates are numerically comparable. First, we standardize all estimates to address potential differences in the scales of the conflict outcomes (e.g., binary or count) and price variables (e.g., in different currencies). Our standardized effects are expressed in terms of standard deviation changes in the prices and conflict variables:

$$\widehat{\beta}_{std} = \widehat{\beta} \times \frac{sd(\text{Price})}{sd(\text{Conflict})}$$

Following Mummolo and Peterson (2018), we residualize the variables using the unit and time fixed effects before computing the standard deviations. More commonly reported pooled standard deviations often overstate the variation used to estimate $\hat{\beta}$ in a two-way fixed effects model. We compute these statistics (or receive them from authors) for 37 studies (see Table A.3).

Second, we ensure that all studies use a common functional form:

$$\text{Conflict}_{it} = \delta_i + \gamma_t + \beta \ \text{Prices}_{it} + \kappa X_{it} + \varepsilon_{it} \tag{1}$$

where i indexes the authors' cross-sectional unit (which we use to cluster the standard errors) and t indexes their temporal unit. X_{it} includes the other time-varying controls included in the authors' original specification. This overcomes non-comparability that arises from the use of models with non-linear link functions (e.g., logistic regression) or the choice of fixed effects (e.g., using year fixed effects where the temporal unit in the panel is month). We acquire replication data for 32 studies and estimate this model; we confirm the remaining 5 estimate a similar linear model.[10]

These standardization steps exclude nine papers for which we lack the necessary statistics (see Table A.6). We can, however, compute an alternate measure of effect size, the partial r (ρ_p), which requires

[10] Two studies incorporate additional fixed effects to improve causal identification: Gehring et al. (2018) add province-year effects; McGuirk and Burke (2018) add country-by-time effects. Dropping these studies does not affect our results.

only the t-statistic (t) and degrees of freedom (df): $\rho_p = t/\sqrt{t^2+df}$ (see Appendix B.4). Reassuringly, the distribution of ρ_p does not change with the inclusion of these nine studies.

2.2 Meta-analysis

We first estimate the fixed effects meta-analysis model (Rosenthal and Rubin 1982), which is a precision-weighted average of the standardized estimates ($\widehat{\beta}_{std}$s from Equation 1, with weights equal to the inverse of the standardized variance).[11] Under minimal assumptions, this model consistently estimates the average effect for the studies in our sample (Rice et al. 2018). We also compute the random effects meta-analysis model (DerSimonian and Laird 1986).[12] This model assumes that the true effects differ across studies, but that these are drawn from a common (normal) distribution.

The fixed and random effects models both recover quantities of interest: the former provides an efficient estimator for the average effect within our sample of studies, while the latter provides both an estimated mean and variance of true effects, which permits generalization to out-of-sample studies. For numerical reasons, the standard errors from the random effects model will always be weakly larger. We estimate both models for each type of commodity. We also present a pooled effect, which averages our estimates across commodity types, giving equal weight to each commodity type.[13]

Our estimates pool across conflict types (incidence, onset, and intensity).[14] In Appendix C we show that coefficient estimates are stable when re-estimating our models while leaving out each conflict type; and additionally, that conflict type is not significant when entered as a moderator.[15]

We take steps to mitigate publication bias and assess whether it skews our estimates. We include working papers. We also perform several diagnostic tests: p-curves, funnel plots, and meta-regression analysis (see Appendix H). These find no evidence of publication bias. Prominent papers in this literature have published null results (e.g., Bazzi and Blattman 2014), ameliorating concern that only positive findings escape the file drawer. We also find that our results are not driven by outliers in effect size or precision (Appendix G).

3. Results

In Table 3, when we pool our study estimates, we find no overall effect (fixed effects: $= -0.001$, $p = 0.619$; random effects: 0.004, p = 0.223). In the top panel of Figure 1, we display these estimates along with 90%

[11] The fixed effects meta-analysis model, a precision-weighted mean of study estimates, is distinct from the similarly-named identification strategy used in the studies we analyze.

[12] We pre-registered a Bayesian random effects model with study and country hierarchies. We could not, however, fit this model given an insufficient number of studies within most countries (see Appendix J).

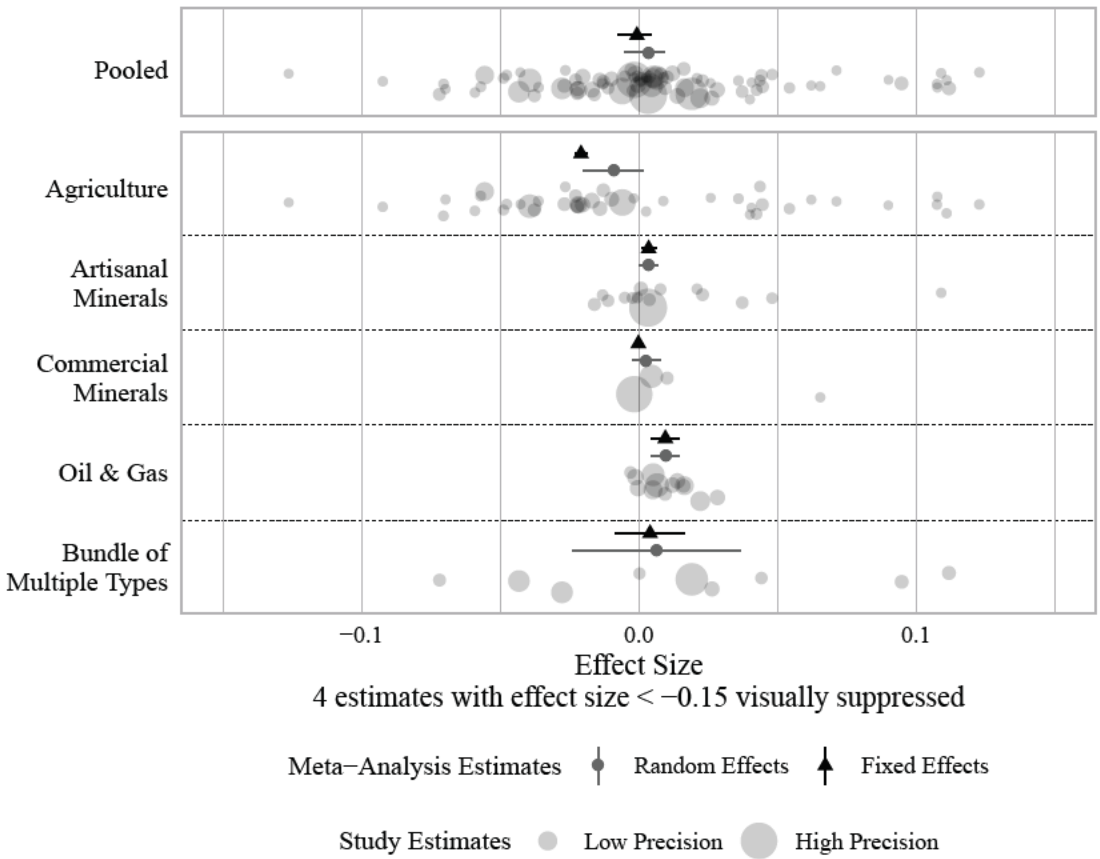
[13] This approach avoids over-weighting commodity types that have received more scholarly attention. We bootstrap confidence intervals and p-values using the bias-corrected percentile method.

[14] We planned to present separate estimates for center-seeking and territorial conflict, but found studies did not consistently differentiate these outcomes. We found too few studies of coups to analyze studies on that outcome.

[15] Further, in some studies, country is the areal (i.e., spatial) unit; others use sub-national divisions. This choice does not appear to influence authors' estimates (see Appendix D). Exclusion of studies with time-varying commodity weights also does not influence results (see Appendix E).

confidence intervals and the raw data from each study.[16] We also see no effect for bundles that include multiple commodity types. We find little support for H1: windfalls from commodity prices do not generally make producing states or regions more or less attractive targets for attacks.

Figure 1: Effects of Commodity Prices on Conflict by Commodity Type



Yet, this reflects cross-cutting effects by commodity type. Consistent with our second hypothesis, we find that rising prices for oil and gas (capital-intensive commodities) increase armed conflict. Both fixed and random effects estimates are 0.01 and significant at the one-percent level. How large are these standardized effects in real-world terms? From 1998 to 2000, crude oil prices increased 115%. Our meta-estimate, when applied to the context studied by Carreri and Dube (2017), implies a 16.5% increase in paramilitary attacks in Colombia's oil-producing municipalities (see Appendix F).

By contrast, we find that price increases for agricultural commodities — which are labor-intensive relative to other types — reduce armed conflict: the fixed effects estimate is -0.021 and significant. Applied to the context studied in Guardado (2018), our estimate implies that the 190% increase in coffee prices from 1993 to 1998 drove a 55% reduction in attacks in coffee-producing areas in Peru and Colombia.

There does appear to be heterogeneity in the effect estimates for agricultural commodities ($\hat{\tau} = 0.0011$), which is reflected in the smaller estimate from the random effects model, -0.009 with $p = 0.165$. Some

[16] Confidence intervals for the pooled effect are not centered due to our bootstrapping procedure.

authors argue that particular crops are more capital intensive and, thus, exacerbate conflict when prices rise: for example, Crost and Felter (2019: 3) report that price increases for bananas only exacerbate conflict where production occurs on large plantations, not where smaller-scale, labor-intensive production predominates (see also, Gehring et al. 2018).

Table 3: Meta-Analysis Estimates of the Effect of Commodity Price Changes on Armed Conflict

	Fixed Effects Meta-Analysis			Random Effects Meta-Analysis				
Commodity type	Estimate	Std. Err.	p-value	Estimate	Std. Err.	p-value	Between-study variance ($\widehat{\tau}^2$)	N
Pooled	-0.001	0.004	0.619	0.004	0.005	0.223	0.0005	88
Agriculture	-0.021	0.001	0.000	-0.009	0.007	0.165	0.0011	45
Artisanal Minerals	0.004	0.002	0.027	0.004	0.002	0.071	0.0000	16
Commercial Minerals	-0.000	0.001	0.896	0.003	0.003	0.402	0.0000	4
Oil	0.010	0.003	0.001	0.010	0.003	0.001	0.0000	13
Multiple	0.004	0.008	0.592	0.006	0.018	0.726	0.0014	10

H2 and H3 do not generate a clear prediction for artisanal minerals, which are both labor-intensive and lootable. Across 13 estimates, we find a small but significantly positive effect of 0.004 with no evidence of heterogeneity, suggesting that lootability offsets the opportunity-cost mechanism.

Finally, we do not find any effect for commercial minerals.[17] We are wary of over-interpreting a null result from four studies. However, this could indicate that lootability is a necessary condition: if it is prohibitively costly to appropriate production, then realistic price increases would not induce fighting. The difficulty of operating a commercial mine (e.g., hiring engineers, refining or shipping ore) may dissuade rebels from fighting over these operations (Christensen 2019). The same is not necessarily true of oil, which may be cheaper to loot through attacks on pipelines.

4. Discussion

While on average commodity prices do not affect conflict, this masks cross-cutting effects by commodity type. We find, in line with theory, that price increases in labor-intensive (capital-intensive) commodities prevent (provoke) conflict. We also find evidence that price increases for lootable commodities lead to conflict.

A meta-analysis not only reveals what we have learned, it also identifies gaps in our knowledge. While we find no evidence of publication bias, some regions and commodities are over-represented in our sample of studies (see Figure 2). The 16 estimates for artisanal minerals largely come from three regions: the three estimates from South America come from Colombia; the Asia estimate comes from Myanmar. Artisanal mining is not confined to these places: the World Bank estimates that 14 million people work in artisanal and small-scale mining in Africa and Latin America and over 26 million people in East and South Asia.

[17] Artisanal and commercial mining can colocate (occur in close proximity), complicating efforts to separately estimate effects for both commodity types. This should generate a convergence in our estimates for commercial and artisanal mining.

Figure 2: Evidence Gap Map (Number of Estimates) by Commodity Type and Continent

	Africa	Asia	N. America	S. America	Multiple
Agriculture	N = 14	9	3	2	17
Artisanal Minerals	12	1		3	
Commercial Minerals	4				
Oil & Gas	4	2		3	4
Multiple	4				6

We have a rich set of theoretical predictions about factors that moderate the relationship between commodity prices and conflict. Yet, we found the measures needed to evaluate these moderators lacking. Future research should directly measure features such as capital intensity, illegality, lootability, and taxability. We also expect new insights will come from more comparisons of the same commodity or crop where the input mix or scale of production vary (e.g., Crost and Felter 2019; Rigterink 2020).

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Supporting Information

Do Commodity Price Shocks Cause Armed Conflict?
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Following text to be published online.

Contents

A. Data Collection and Filtering

A.1 Keyword Search

We run searches on Google Scholar with three different sets of keywords: (1) *(price OR prices) AND (conflict OR war) AND (resource OR commodity)*; (2) *("price shock" OR "price shocks") AND (conflict OR war OR protest)*; and (3) *(price OR prices) AND ("resource curse") AND (conflict OR war OR violence OR protest)*. The Google Scholar searches were performed April–May 2018.

A.2 Citation Network Search

We additionally use Google Scholar to search all papers citing one of three papers we identified as central to this literature: Collier and Hoeffler (2004), Dube and Vargas (2013), and Bazzi and Blattman (2014). The first two network searches were completed April–May 2018 and the final search was completed December 2018.

A.3 Topical Filter

We restrict attention to complete, English-language papers, including both working papers and published works. To pass the topical filter, a paper must use real-world data to empirically study the relationship between commodity prices and a measure of intra-state armed conflict, broadly (inclusive of civil conflict, coups, and organized or violent crime).

Figure A.1: Number of Studies on Commodity Prices and Conflict over Time

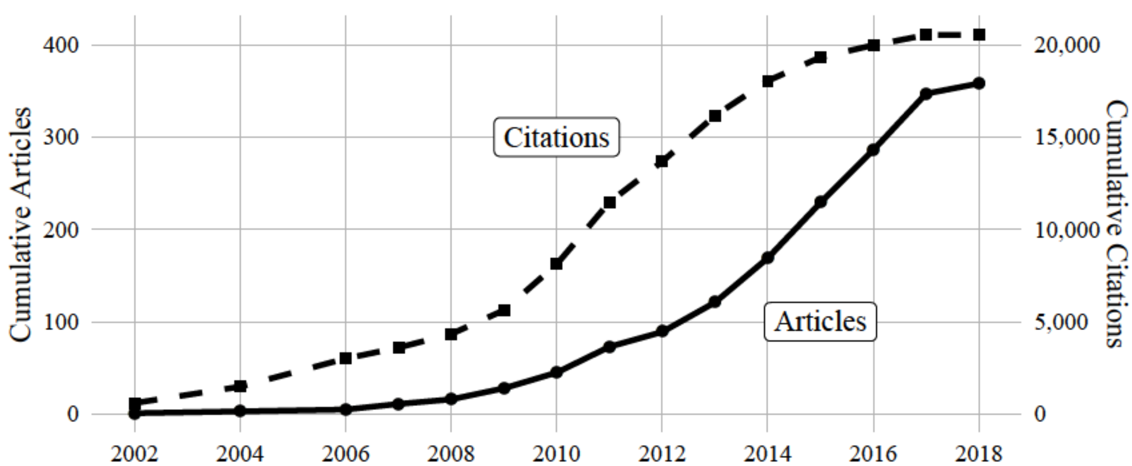


Figure A.1 displays counts of English-language peer-reviewed articles or working papers published between 2002 and 2019 in which the dependent variable was a measure of armed conflict and the key independent variable was a commodity value. No papers prior to 2002 were identified by our searches. Solid line: cumulative number of articles; dashed line: cumulative number of citations for counted articles.

A.4 Research Design Filter

At this stage we include only those studies which use a credible strategy to identify a causal relationship between resource commodity prices and armed civil conflict. Authors must argue that their price measure is plausibly exogenous to the area of study conditional on the fixed effects and other included covariates.

Papers need not including any particular "control" variables; may use non-linear link functions (e.g. logistic or probit regression); may estimate the relationship as part of a simultaneous estimation; may instrument prices as part of an instrumental variables identification strategy; may use contemporary or time lagged prices; may include single commodity prices, multiple separate commodity prices, or a combined index of commodity prices; may span multiple countries or regions within a single country; and may operate at any level of geographic or temporal aggregation.

This filter is intended to ensure that authors credibly claim that $\mathbb{E}[\varepsilon_{it}|\text{Prices}_{it}, X_{it}] = 0$, where i indexes i indexes a geographic unit (grid cell, district, province, country), t, time (month, quarter, year), and X_{it} includes covariates (if any).

At this stage we also excluded several studies which examined protests or coups as outcomes.

A.5 Rules for Selecting among Specifications

If a single paper presents multiple estimates for the same commodity and conflict type, then we use the following rules to select among specifications:

- Papers may employ *multiple specifications*, including and excluding covariate adjustment. When this occurs, we will select specifications that hew closest to our general model to improve direct comparability between studies.

- Papers sometimes employ *multiple standard error calculations*. We prefer standard error calculations that are justified based on the research design or, if none are justified by the design, are heteroskedasticity-robust and clustered at the appropriate geographic level of variation.

- Papers may include *multiple levels of geographic aggregation*. In general, we prefer to extract the estimates based on the lowest level of geographic aggregation for which full data is available, unless authors raise a clear preference for a different level of geographic aggregation on the basis of theory, data integrity, or causal identification.

- Some papers use *multiple temporal lags*. Where authors report a single temporal lag in their main paper and offer others as "extra specifications" or "robustness checks," we will collect the main temporal lag. In cases where authors report multiple temporal lags in the same model, we will prefer an aggregation of all studied temporal lags into a single effect estimate; if one is not available, we calculate one.

- Some papers report *different model link functions*. If a paper reports models which otherwise are identical on the above criteria, but differ in the link function they use, we select linear models, including linear probability models, in order to, when possible, compare estimated effect sizes on a common scale.

Our PAP pre-specifies a longer set of rules to handle other contingencies.

A.6 Extracting Data from Studies

In this section, we describe the data items extract from each of the included studies. Where replication archives are publicly available or have been provided, we generate these data ourselves. Where information is unavailable, we consult study authors or extract it from the text of the study. The items extracted include:

- **Basic study metadata**: Author information, date of publication, citation count at time of data collection, venue of publication, and other basic study metadata.

- **Estimate metadata**: A list of all relevant "control" covariates; the functional form of the included estimate; a list of countries and county-years included; a list of commodities contributing to the price or treatment variable; the type of conflict studied (incidence, intensity, onset); whether the conflict data originates from ACLED, UCDP, or other sources; the number of unit- and time-fixed effects; overall number of observations.

- **Study effect and uncertainty**: The regression coefficient β, its corresponding t-statistic, standard error, and p-value.

- **Standardization information**: The residualized standard deviations necessary to standardize the regression coefficient for comparison, described in Section 2.1.

- **Effect size interpretation**: We extract details about commodity price series and conflict base rates for the two studies explored in Appendix F.

A.7 PRISMA 2009 Flow Diagram

We include the PRISMA 2009 flow diagram to describe transparently our data collection process.

Figure A.2: PRISMA 2009 Flow Diagram

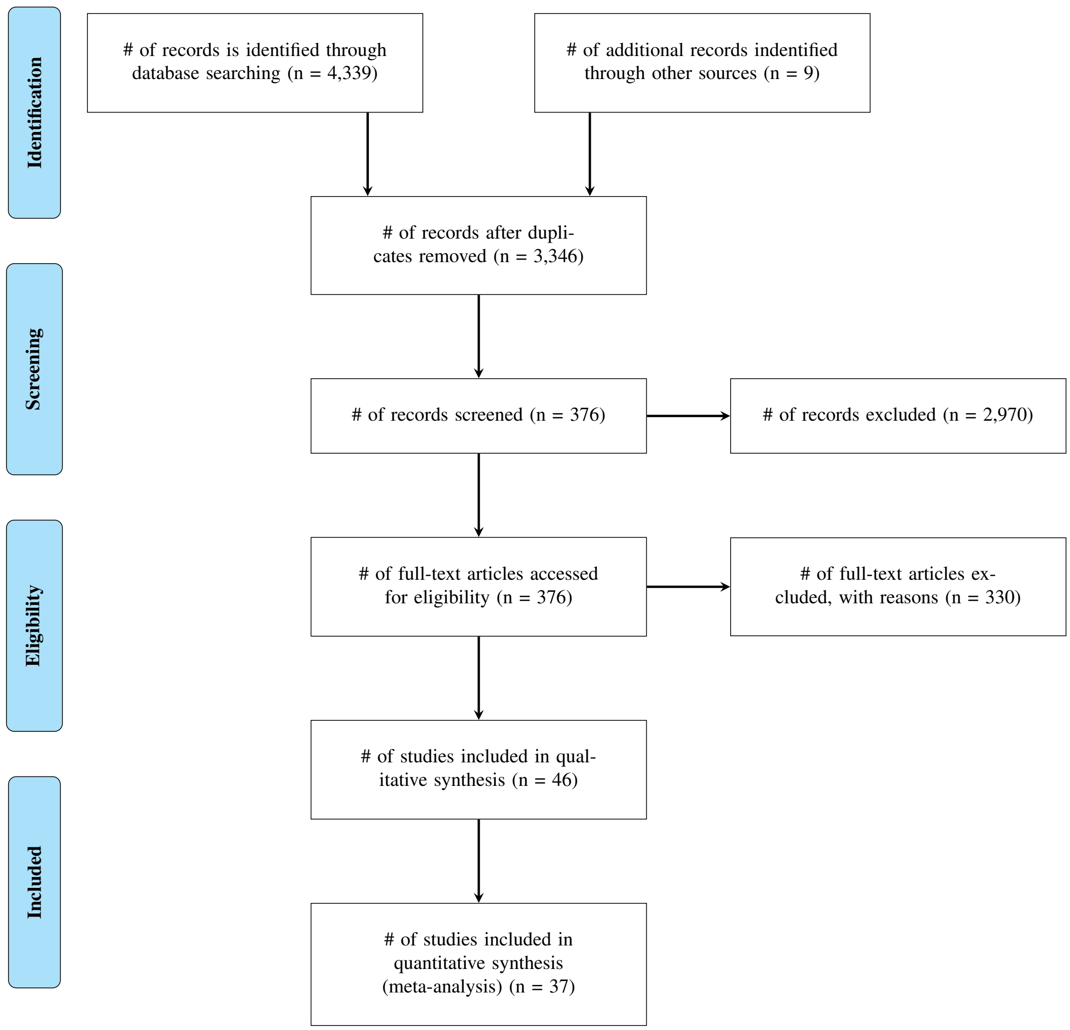


Figure A.2 displays the meta-analysis data collection flow. "Records excluded" were excluded either because the study was not in English, the study title or abstract did not reflect the topical filter described in Section 2.1, or the papers appeared to be incomplete or unavailable. "Full-text articles excluded" were excluded due to a failure to meet the research design filter described in Section 2.1. In addition, two working papers identified during searches were deemed by their authors to be abandoned. The drop in studies from the qualitative synthesis to the quantitative synthesis is described in Section 2.1 and a complete list of papers excluded at that stage is available below in Appendix B.4.

A.8 Coding Commodity Type

Table A.1: Commodities by Type

Commodity Type	Commodities
Agriculture	Agricultural oils, bananas, barley, beef, cattle, cavendish bananas, cereals, chocolate, cocoa bean, coconut oil, coffee, cotton, dairy, fish, groundnuts, lacatan bananas, maize, meat, nuts, olive, opium, orange, palm oil, rice, saba bananas, sorghum, soybean oil, soybeans, staple crops, sugar, sunflower, tea, tobacco, wheat
Artisanal Minerals	Aluminum, copper, gold, iron ore, jade, lead, manganese, nickel, oil, palladium, phosphate, secondary diamonds, silver, tantalum, tin, tungsten, wolframite, zinc
Commercial Minerals	Aluminum, coal, copper, gold, iron ore, lead, nickel, phosphate, platinum, primary diamonds, silver, tantalum, tin, zinc
Oil	Natural gas, oil

Table A.1 enumerates the set of commodities that are coded in each commodity type. We exclude studies that focus on drugs from the study (including Millán-Quijano 2015). We include a single study that focuses on opium farming, Gehring et al. (2018), per its in-text argument characterizing opium as a labor-intensive agricultural commodity. Additionally, one included study, Dagnelie et al. (2018), is explicitly a study of artisanal minerals, but includes oil as one of many minerals in its mineral price index.

A.9 Coding Estimates as Commercial or Artisanal

We code mineral commodities as artisanal or commercial according to the following rule set, blind with respect to study outcomes:

- Study text: Where a study (or cited data set) explicitly states that data represents commercial, government-sanctioned, officially recorded mining, the estimate is commercial. Where a study explicitly states that data represents illegal, artisanal, or informal mining, the estimate is artisanal.
- Country-commodity and commodity level estimates: Where no determination can be made, we source information at the country-commodity level to assess whether academic, industry, or NGO reports substantiate the presence of an ASM sector in the country-commodity and cite those. If information is available to substantiate the presence of an ASM sector, the estimate is artisanal. If not, the estimate is commercial. In cases where multiple commodities are combined in the same estimate, we make an effort to determine which commodities dominate the index and code the estimate accordingly.

Table A.2 enumerates coding decisions for each study with minerals.

Table A.2: Coding Mineral Studies as Artisanal or Commercial

Citation	Artisanal	Commercial	Coding Citation
Berman et al. (2017)		✓	"Hence, small-scale mines, and those that are illegally operated, are not included in our sample" (Berman et al. 2017: 1570).
Christensen (2019)		✓	"These databases do not include artisanal or illegal mines" (Christensen 2019 SI 29).
Christensen et al. (2019a)	✓		"Directly working in artisanal and small-scale mining: ... 14,000" (Hilson and Maconachie 2017: 444).
Dagnelie et al. (2018)	✓		"It is estimated that more than half of the cassiterite and coltan production and more than 90 percent of gold production and export is 'informal"' (Geenen 2012: 322). "The key minerals produced by artisanal miners in [Eastern Congo] are cassiterite, coltan, tungsten, copper, cobalt, gold and diamonds. Minerals which we were told have a history of production include... semi-precious stones, ... iron, and platinum" (PACT 2010: 20).
Dube and Vargas (2013)	✓		"ASM activity in Colombia is significant, representing 72 per cent [sic] of the country's total gold production" (Fritz et al. 2018: 9).
Idrobo et al. (2014)	✓		"We also have a set of variables with information about the presence.. of illegal mines at the municipal level" (Idrobo et al. 2014: 92).
Jensen et al. (2017)	✓	✓	"Primary diamonds depend on deep shaft mining techniques that require heavy equipment, supplies, engineering expertise and organization. As such, we consider that they are less lootable. Secondary diamonds in contrast can be extracted by relatively unskilled workers collecting diamonds from the surface" (Jensen et al. 2017: 11).
Maystadt et al. (2014)	✓		See entry under Dagnelie et al. (2018).
Parker and Vadheim (2017)	✓		"Estimates of the number of artisanal miners in the five eastern provinces are rough but ranged from 710,000 to 860,000 in 2007. The World Bank estimates that artisans produce 90% of the minerals exported from the country" (Parker and Vadheim 2017: 5). "Export data are a less reliable measure of gold output because approximately 98% of gold mined in the eastern DRC is smuggled" (Parker and Vadheim 2017: 11).
Rigterink (2020)	✓	✓	"There exist two types of diamonds, primary and secondary diamonds, which are chemically identical yet differ in their labor-intensiveness, 'lootability,' and potential for government revenue." (Rigterink 2020: 92) "[D]iamond propensity... [is] able to capture potential for small-scale diamond production, even if this goes unrecorded by the production statistics" (Rigterink 2020: 100).
Tolonen (2015)		✓	"large-scale mineral mines in Africa, from IntierraRMG" (Tolonen 2015: 11).

Table A.2 specifies the coding of estimates as artisanal or commercial minerals based on the above-specified coding rules.

B. Studies Used in Meta-analysis

B.1 Included Studies

Table A.3: Studies Included in Meta-Analysis

Citation	Year	N	Continent	Oil	Com. Min.	Art. Min.	Ag.	Mult.
Brückner and Ciccone (2010)	2010	1	Africa					✓
Cotet and Tsui (2013)	2013	1	Multiple	✓				
Jablonski and Oliver (2013)	2013	3	Multiple	✓			✓	
Dube and Vargas (2013)	2013	4	S. America	✓		✓	✓	
Basedau and Pierskalla (2014)	2014	1	Africa	✓				
Idrobo et al. (2014)	2014	1	S. America			✓		
Maystadt et al. (2014)	2014	1	Africa			✓		
Maystadt and Ecker (2014)	2014	1	Africa				✓	
Bazzi and Blattman (2014)	2014	8	Multiple				✓	✓
Fjelde (2015)	2015	1	Africa				✓	
O'Trakoun (2015)	2015	1	Multiple				✓	
Raleigh et al. (2015)	2015	1	Africa				✓	
Ahrens (2015)	2015	2	Africa				✓	✓
Berman and Couttenier (2015)	2015	2	Africa				✓	
Knorr (2015)	2015	9	Multiple				✓	
Aguirre (2016)	2016	1	Africa					✓
Dube et al. (2016)	2016	1	N. America				✓	
Van Weezel (2016)	2016	1	Africa				✓	
Berman et al. (2017)	2017	1	Africa		✓			
Andersen et al. (2017)	2017	2	Multiple	✓				
Calí and Mulabdic (2017)	2017	2	Multiple				✓	✓
Carreri and Dube (2017)	2017	2	S. America	✓				
Christian (2017)	2017	2	N. America				✓	
Jensen et al. (2017)	2017	2	Africa		✓	✓		
Gong and Sullivan (2017)	2017	3	Africa				✓	
Parker and Vadheim (2017)	2017	8	Africa			✓		
Dagnelie et al. (2018)	2018	1	Africa			✓		
Guardado (2018)	2018	1	S. America				✓	
Ciccone (2018)	2018	2	Africa					✓
Hong and Yang (2018)	2018	2	Asia	✓				
Fetzer and Kyburz (2018)	2018	3	Africa	✓				
Gehring et al. (2018)	2018	4	Asia				✓	
McGuirk and Burke (2018)	2018	4	Africa				✓	
Christensen (2019)	2019	1	Africa		✓			
Christensen et al. (2019a)	2019	1	Asia			✓		
Crost and Felter (2019)	2019	5	Asia				✓	
Rigterink (2020)	2020	2	Africa		✓	✓		

Table A.3 lists the studies included in the meta-analysis along with the year of publication ("Year"); the number of estimates extracted from the study that are included in meta-analyses ("N"); the continent(s) analyzed in the study's data ("Continent"); and the set of commodities included in the study's data analysis (✓ indicates a commodity type is included).

B.2 Data Concentration

In this section, we assess the concentration of our data. Our study sample represents a raw total of 201 countries and 10,926 country-years. In order to assess whether "streetlighting" (a focus on particular countries, time periods, or data sources) contributes to a sample that is more concentrated or effectively smaller than it first appears, we generate two common diagnostic indices: Herfindahl-Hirschman (HH) Indices, and "effective" counts. HH indices are a measure of concentration from 0 to 1 calculated by taking the proportion of the total data in a given group (p_i), squaring, and summing across groupings ($\sum p_i^2$). In a sample where data is equally distributed across contexts and studies, the HH index will be $\frac{1}{N}$. The inverse of the HH index constitutes an "effective number", which takes values between 0 and N on the same scale as the raw data: higher effective numbers indicate more even distributions of data.

We generated these statistics for four quantities in our data: number of countries, number of country-years, number of data source-countries, and number of data source-country-years. "Data source" is a rough coding of the source of data used in a given study: "ACLED" or "UCDP" for two canonical data sets of conflict, or "other" for other data sources or original data gathered by authors. We do not distinguish between different revisions or subsets of ACLED or UCDP.

We conclude that our data displays low overall concentration along any of these groupings.

Table A.4: Concentration of Data

Data Grouping	Raw Count	HH Index	Effective Number
Countries	201	0.00722	138
Country-years	10,926	0.00011	8,796
Data source-country	488	0.00368	272
Data source-country-year	29,069	0.00007	15,255

Table A.4 shows dimensions of our sample. Raw counts refer to the number of unique groupings in our sample. HH Indices are a measure of data concentration equal to $\sum p_i^2$ where p_i is the proportion of total data in each grouping which span from 0 to 1. Effective Numbers are a measure of data uniqueness equal to $\frac{1}{\sum p_i^2}$ which spans from 0 to N.

B.3 Study Contexts

In this section, we compare the characteristics of countries included in our study sample to a global sample.

To construct Table A.5, we take our included studies and extract each country-year pair. We join the country-year to common country characteristics. We, first, aggregate to the study-level the average value of those characteristics and present the distribution across studies. (We label this the "Included Sample.") Next, we take countries present in our sample and look at a fixed year, 1995 (the mean year of our included sample). For this "Weighted Sample," we weight by the number of studies including a country. We compare our study distribution to two reference distributions: a sample of "Target Countries" which experienced an intra-state conflict post-1945; second, a sample of all countries. Both comparison groups are analyzed for the year 1995, the mean year of our included sample.

Table A.5: Characteristics of Included Country-Years

	Included Sample			Weighted Sample			Target Countries in 1995			All Countries in 1995		
Variable	Mean	p25	p75	Mean	p25	p75	Mean	p25	p75	Mean	p25	p75
GDP per capita	3,949	1,632	5,839	5,571	562	4,520	2,743	567	3,221	9,421	886	8,540
Gini Coefficient	45.28	43.60	45.72	39.51	33.11	43.53	41.23	35.30	45.70	38.22	31.55	43.98
War	0.19	0.09	0.13	0.13	0.00	0.00	0.26	0.00	1.00	0.11	0.00	0.00
Polity	1.14	-0.82	2.24	1.09	-5.45	7.00	0.63	-6.00	6.00	2.37	-5.00	8.00
Year	1995	1992	2004	1995			1995		1995			

Table A.5 compares country-years included in our sample to a global sample. "War" refers to the presence of an inter- or intra-state war in a state. "Included Sample" columns represent the mean, first quartile, and third quartile across studies. Each study's estimate is the mean of all country-year pairs included in in the sample. "Weighted Sample" columns represents a distribution of all in-sample countries in 1995 (the mean year in our sample), weighted by the number of studies including the country. "Target Countries in 1995" represents a distribution of the same quantities across those countries with at least one intra-state conflict in the post-1945 period, for the year 1995. "All Countries in 1995" represents a distribution of the same quantities across all global countries for the year 1995.

In comparison to countries with intra-state conflicts after 1945, our sample is somewhat more wealthy per capita, approximately as unequal, somewhat less conflict prone, and slightly more democratic. In comparison to a broader sample of all global countries, our sample is 40% as wealthy per capita, somewhat more unequal, two-thirds more prone to conflict, and somewhat less democratic.

B.4 Studies where Standardization is Not Feasible

Table A.6: Studies Not Included in Meta-Analysis

Citation	Year	N	Continent	Oil	Com. Min.	Art. Min.	Ag.	Mult.
Arezki and Brückner (2011)	2011	2	Multiple	✓				
Arezki and Gylfason (2013)	2013	1	Africa					✓
Musayev (2014)	2014	1	Multiple					✓
Morgan and Reinhardt (2015)	2015	1	Multiple					✓
Tolonen (2015)	2015	1	Africa		✓			
Abidoye and Calí (2015)	2015	3	Africa	✓				✓
Galperin (2016)	2016	2	Asia				✓	
Mamo (2018)	2018	1	Africa					✓
Giménez-Gómez and Zergawu (2018)	2018	2	Multiple					✓

Table A.6 lists studies for which we were unable to standardize estimates, because we lack sufficient statistics or replication data for the authors' estimation sample. All study authors were contacted about inclusion in this meta-analysis.

The main quantity of interest, a standardized regression coefficient, can only be obtained with access to replication data or ancillary information from study authors.[18] Thus, for some studies which would otherwise be included in the meta-analysis (listed in Table A.6), we are unable to obtain a standardized regression coefficient, which is necessary to render the meta-analysis scale-free and numerically comparable across studies. In this section, we assess the consequence of excluding studies for this reason by using a substitute quantity of interest. The best available substitute is a measure derived from the t-statistic: partial r (ρ_p). Raw t-statistics are commonly reported in papers or can be readily derived from reported quantities. However, because t-statistics are formed from the ratio of the the coefficient of interest and its standard error, a large t-statistic can emerge from either a large coefficient, high precision, or both. ρ_p scales the t-statistic to between -1 and 1 by penalizing t-statistics with greater degrees of freedom, as follows:[19]

$$\rho_p = \frac{t}{\sqrt{t^2 + df}}$$

The calculation of ρ_p for all studies allows us to address a threat to our main presented results: the results of studies without available replication data could differ systematically from those with available replication data. By demonstrating that the ρ_p statistics of those papers without available replication data are indistinguishable from those with available replication data, we offer evidence that our main analyses are not threatened by study exclusion.

The choice to privilege our main results over ρ_p results reflects a tradeoff between the standardized regression coefficient offering more comparable estimates and the partial r results offering broader inclusion of studies.

[18] Specifically, it requires the residualized standard deviations of the variables of interest.

[19] This definition is widely known but covered in Cooper et al. (2009) and Aloe (2014) and others.

Table A.7: Difference in Partial R (ρ_p) for Included and Excluded Studies

	$\rho_p = \alpha + \beta\mathbb{1}(\text{Excluded}) + \varepsilon$				
Commodity Type	Sample Mean ($\hat{\alpha}$)	Excluded ($\hat{\beta}$)	p-value	N	Partial R Only
Agriculture	-0.07	-0.04	0.91	47	2
Artisanal Minerals	-0.00			16	0
Commercial Minerals	0.19	-0.28	0.53	5	1
Oil	0.25	-0.30	0.30	16	3
Multiple	-0.01	0.03	0.89	18	8

Table A.7 compares studies included in our analyses with those studies which were eligible for inclusion but where standardization data could not be obtained, using ρ_p statistics which can be calculated using available materials. No evidence is found to support the claim that excluded studies differ systematically from included studies.

C. Analysis of Outcome Type

The studies in our corpus measure conflict in three ways: onset (start of conflict), incidence (presence of conflict), or intensity (number of battles or fatalities). In our meta-analysis of the effects of commodity prices on conflict, we pool across these different measures. In this section, we examine whether this pooling is justified, or whether effects vary by conflict type. Over half of the estimates in our meta-analysis use measures of intensity, and the remainder are evenly split between measures of incidence and onset. We offer two diagnostics to assess the impact of this decision.

C.1 Leave-one-out Analysis by Outcome Type

First, we re-estimate Table 3 using the same methods while leaving out each of the three conflict types in turn. The results produce stable coefficient estimates that yield substantively similar conclusions to our main results. Results are subject to greater uncertainty owing to the reduced number of observations, and so loss of power in some cases yields loss of significance. When dropping intensity, the coefficient of the "Commercial Minerals" meta-estimate becomes significant despite only retaining 3 studies: we do not draw a substantive conclusion from this.

Table A.8: Leave-one-out Results by Conflict Type

	Fixed Effects Meta-Analysis			Random Effects Meta-Analysis				
Commodity type	Estimate	Std. Err.	p-value	Estimate	Std. Err.	p-value	Between-study variance ($\widehat{\tau}^2$)	N
Panel A: Original Table 3								
Pooled	-0.001	0.004	0.619	0.004	0.005	0.223	0.0005	88
Agriculture	-0.021	0.001	0.000	-0.009	0.007	0.165	0.0011	45
Artisanal Minerals	0.004	0.002	0.027	0.004	0.002	0.071	0.0000	16
Commercial Minerals	-0.000	0.001	0.896	0.003	0.003	0.402	0.0000	4
Oil	0.010	0.003	0.001	0.010	0.003	0.001	0.0000	13
Multiple	0.004	0.008	0.592	0.006	0.018	0.726	0.0014	10
Panel B: Dropping Intensity								
Pooled	0.002	0.005	0.404	0.008	0.006	0.124	0.0005	41
Agriculture	-0.022	0.001	0.000	-0.014	0.009	0.120	0.0008	15
Artisanal Minerals	0.008	0.009	0.382	0.012	0.012	0.316	0.0003	6
Commercial Minerals	0.005	0.002	0.015	0.005	0.002	0.019	0.0000	3
Oil	0.010	0.004	0.008	0.010	0.004	0.008	0.0000	9
Multiple	0.005	0.008	0.481	0.014	0.020	0.474	0.0016	8
Panel C: Dropping Onset								
Pooled	-0.036	0.033	1.000	-0.043	0.030	1.000	0.0044	68
Agriculture	-0.017	0.002	0.000	-0.010	0.008	0.203	0.0013	37
Artisanal Minerals	0.003	0.002	0.037	0.003	0.002	0.050	0.0000	15
Commercial Minerals	-0.000	0.001	0.862	0.002	0.003	0.476	0.0000	3
Oil	0.010	0.003	0.001	0.010	0.003	0.001	0.0000	11
Multiple	-0.093	0.066	0.156	-0.180	0.180	0.317	0.0394	2
Panel D: Dropping Incidence								
Pooled	0.002	0.007	0.826	0.008	0.007	0.024	0.0007	67
Agriculture	-0.025	0.002	0.000	-0.005	0.008	0.511	0.0012	38
Artisanal Minerals	0.004	0.002	0.025	0.004	0.002	0.025	0.0000	11
Commercial Minerals	-0.002	0.001	0.165	0.021	0.032	0.515	0.0015	2
Oil	0.008	0.005	0.068	0.008	0.005	0.068	0.0000	6
Multiple	0.004	0.008	0.592	0.006	0.018	0.726	0.0014	10

Table A.8 is estimated in the same manner as Table 3. Panels B–D drop one of the three conflict types (intensity, onset, and incidence) while retaining the other two.

C.2 Outcome Types as Moderators

Next, we run a random-effects meta-analysis regression with outcome types as moderators. We conduct a joint test of significance across the three outcome types. The results are presented in Table A.9.

Table A.9: Conditional Effects by Outcome Type

Outcome Type	Estimate	Standard Error	p-value	N
Incidence	-0.001	0.006	0.828	24
Intensity	-0.003	0.005	0.628	54
Onset	0.004	0.008	0.619	24

Table A.9 presents estimates of the conditional effect of outcome type on the random effects meta-analysis estimates of the effect of commodity prices on conflict. The p-value on the Q test for joint significance of the three outcome types is 0.913. We cannot reject the null of no difference between all pairs of outcome types.

We find that there is little variation in average effects across outcome types. We cannot reject the null hypothesis of no differences in effects.

D. Analysis by Choice of Areal Unit

Some of our included studies use country as their areal unit; others use sub-national areal units (e.g., provinces, grid cells). This analysis choice could produce effect heterogeneity.

We only have enough estimates to make comparisons for the agriculture and oil and gas commodity types. In the top panel of Table A.10, we meta-analyze estimates from studies that take country as the areal unit; in the bottom panel, estimates from studies using sub-national areal units. We see very little difference in the fixed effects estimates for either commodity type. In the case of oil and gas, the result is the same sign and a comparable magnitude. The agriculture estimates are identical. The sign of the estimates from random effects analysis are also comparable. In short, we do not find substantial differences driven by this analysis choice.

Table A.10: Results with Sample Split Based on Areal Unit

	Fixed Effects Meta-Analysis			Random Effects Meta-Analysis				
Commodity type	Estimate	Std. Err.	p-value	Estimate	Std. Err.	p-value	Between-study variance ($\widehat{\tau}^2$)	N
Panel A: Studies with Country as Areal Unit								
Agriculture	-0.021	0.005	0.000	-0.021	0.005	0.000	0.0000	19
Oil	0.005	0.007	0.512	0.005	0.007	0.512	0.0000	4
Panel B: Studies with Sub-National Areal Unit								
Agriculture	-0.021	0.001	0.000	-0.001	0.009	0.878	0.0015	26
Oil	0.011	0.003	0.001	0.011	0.003	0.001	0.0000	9

Table A.10 is estimated in the same manner as Table 3. Each panel isolates effects for studies that use country as the areal unit (top) or a sub-national areal unit (bottom). We discard artisanal minerals, commercial minerals, and multiple commodity-type studies (these had one or fewer studies in one category, so we could not make comparisons between the two study types).

E. Robustness to Exclusion of Time-Varying Weights

Studies in our corpus include commodity prices as a treatment variable, typically interacted with a unit-varying interaction term (e.g., a dummy indicating the presence of resource activity; a weight variable indicating the relative intensity of resource activity or the relative contribution of a commodity to a country's economic output). Some studies choose fixed interaction terms, while others use time-varying interaction terms (typically a slowly moving average of lagged export shares of a commodity). For more discussion of the choice to use time-varying or fixed interaction terms, see Ciccone (2018). Because this analysis choice could impact our results, we re-estimate a version of our main results excluding those studies that use time-varying weights (which total 10 estimates). Dropping these studies only affects the Pooled, Agriculture, and Multiple categories below. We see no differences in the substantive or statistical interpretation of our results without these estimates.

Table A.11: Results Dropping Two Studies Using Time-varying Weights

	Fixed Effects Meta-Analysis			Random Effects Meta-Analysis				
Commodity type	Estimate	Std. Err.	p-value	Estimate	Std. Err.	p-value	Between-study variance ($\hat{\tau}^2$)	N
Panel A: Excerpted Main Results (Table 3)								
Pooled	-0.001	0.004	0.619	0.004	0.005	0.223	0.0005	88
Agriculture	-0.021	0.001	0.000	-0.009	0.007	0.165	0.0011	45
Multiple	0.004	0.008	0.592	0.006	0.018	0.726	0.0014	10
Panel B: Time-Varying Weights Dropped								
Pooled	-0.002	0.004	0.627	0.002	0.005	0.433	0.0005	78
Agriculture	-0.021	0.001	0.000	-0.010	0.007	0.128	0.0011	40
Multiple	0.002	0.008	0.779	0.001	0.023	0.973	0.0017	5

Table A.11 is estimated in the same manner as Table 3, with estimates from Bazzi and Blattman (2014) and Calí and Mulabdic (2017) dropped.

F. Effect Size Interpretation

Our standardized meta-estimates, described above, represent the standard deviation change in outcomes of interest per standard deviation change in treatment. We provide a mapping from these standardized meta-estimates to real-world cases by selecting representative studies and using the process described below.

We select a study and then time interval within the study period: $[\underline{t}, \bar{t}]$. We use commodity price data to calculate the rate of growth (decline) in prices over that period. To capture the change in the treatment variable (typically an interaction of prices and past production) associated with this price swing (δ_x), we take the average change in treatment (net of other predictors) over the time interval for treated units ($D_i = 1$). Treated units are those with production of the commodity of interest.

Next, we calculate a base rate of conflict, $\bar{y}_0$, by taking the mean of the conflict variable (y_{it}) among control units in the immediate pre-period ($\underline{t} - 1$). Control units are observations with no production for the commodity of interest.

We scale the unstandardized regression coefficient, β by δ_x and take its ratio to the base conflict rate $\bar{y}_0$. This is an effect interpretable as the percentage increase over baseline conflict associated with the chosen commodity volatility, τ_{pct}. We scale this percentage by the ratio of our meta-estimate for the commodity category, β_{meta} to the standardized regression coefficient from the paper's model, β_{std}, resulting in a scaled effect size τ_{scaled}. Mathematically:

$$
\begin{aligned}
\delta_x &= \mathbb{E}[x_{it} \mid D_i = 1,\, t = \bar{t}] - \mathbb{E}[x_{it} \mid D_i = 1,\, t = \underline{t}] \\
\bar{y}_0 &= \mathbb{E}[y_{it} \mid D_i = 0,\, t = \underline{t} - 1] \\
\tau_{\text{pct}} &= 100 \times \left(\frac{\beta \times \delta_x}{\bar{y}_0} \right) \\
\tau_{\text{scaled}} &= \tau_{\text{pct}} \times \frac{\beta_{\text{meta}}}{\beta_{\text{std}}}
\end{aligned}
$$

Values are provided below for the two studies cited in-text:

Table A.12: Effect Size Interpretation, Selected Studies

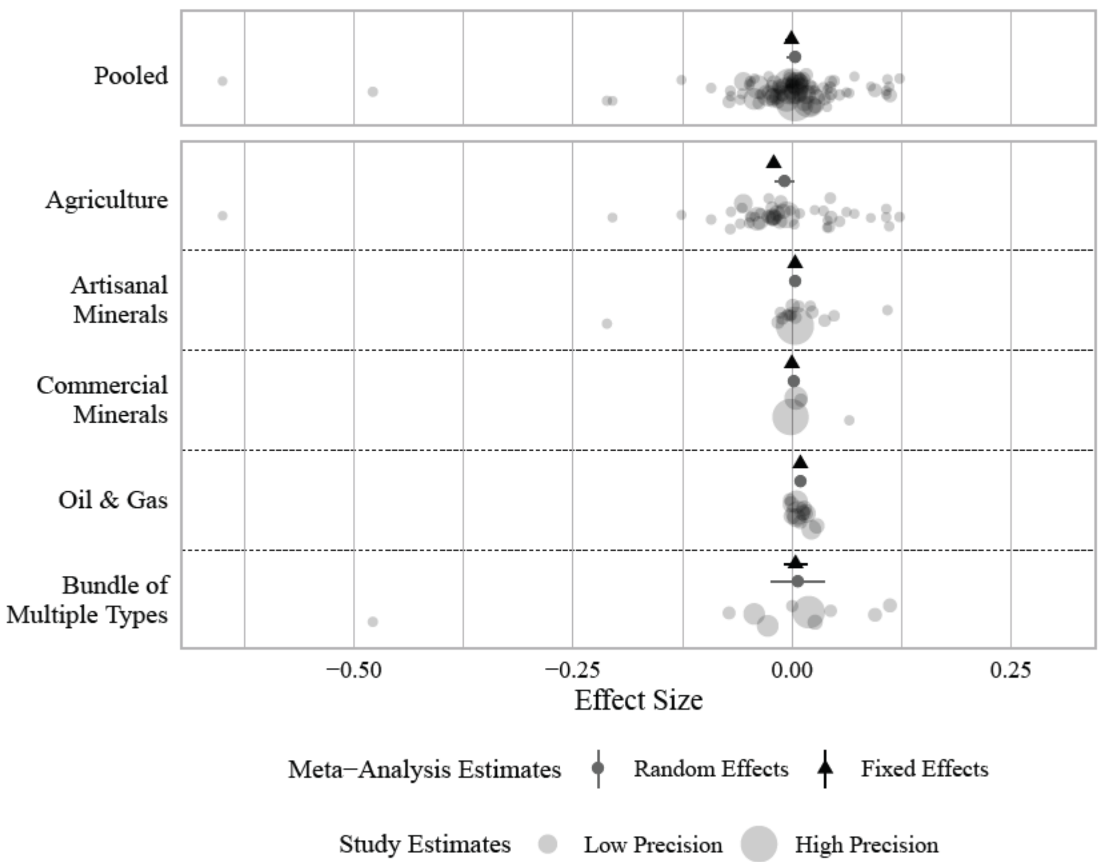
Study	Years	β	δ_{price} (%)	δ_x	$\bar{y}_0$	τ_{pct}	β_{std}	β_{meta}	τ_{scaled}
Carreri and Dube (2017) Paramilitary Attacks $\sim$ Oil Prices	1998-2000	0.031	115.95%	0.780	0.049	48.604%	0.028	0.01	16.469%
Guardado (2018) Attacks $\sim$ Coffee Prices	1993-1998	-0.055	190.64%	1.06	0.099	-58.331%	-0.022	-0.02	-55.018%

G. Impact of Outlier Studies

G.1 Impact of Studies With Outlier Effect Magnitudes

In Figure 1, four studies with negative outlier effect sizes in the range $[-0.61, -0.15]$ are visually suppressed, although they are incorporated into the analysis. We first present a version of this figure without the visual suppression (Figure A.3).

Figure A.3: Effects of Commodity Prices on Conflict by Commodity Type, No Suppression of Outliers



Next, we investigate whether these outliers are driving substantive effects. Table A.13 reproduces the main results (Table 3) with the study effect sizes censored to two intervals: first, the interval ± 0.15, which effectively replaces the study effect sizes of the four visually suppressed studies with -0.15. The four studies censored in this fashion include one study of Artisanal Minerals, two Agricultural studies, and one study of multiple commodity types. The substantive interpretation of results is unchanged, although the precision of the random effect artisanal minerals estimate is improved.

Finally, we censor study effect sizes to the interval ± 0.10, which affects eleven studies. When censored in this manner, the substantive results are generally unchanged, but the random effects estimate of artisanal minerals moves from being positive and significantly different from zero to positive but indistinguishable from zero. The fixed effects estimate of the same category remains significant.

Table A.13: Meta-Analysis Estimates with Outlier Effects Winsorized

Commodity type	Fixed Effects Meta-Analysis Estimate	Std. Err.	p-value	Random Effects Meta-Analysis Estimate	Std. Err.	p-value	Between-study variance ($\widehat{\tau}^2$)	N
Panel A: Effects Winsorized to ± 0.15								
Pooled	-0.001	0.004	0.618	0.004	0.005	0.185	0.0005	88
Agriculture	-0.021	0.001	0.000	-0.009	0.007	0.184	0.0011	45
Artisanal Minerals	0.004	0.002	0.025	0.004	0.002	0.028	0.0000	16
Commercial Minerals	-0.000	0.001	0.896	0.003	0.003	0.402	0.0000	4
Oil	0.010	0.003	0.001	0.010	0.003	0.001	0.0000	13
Multiple	0.004	0.008	0.571	0.008	0.018	0.673	0.0014	10
Panel B: Effects Winsorized to ± 0.10								
Pooled	-0.001	0.004	0.623	0.004	0.004	0.223	0.0004	88
Agriculture	-0.021	0.001	0.000	-0.009	0.006	0.127	0.0009	45
Artisanal Minerals	0.004	0.002	0.024	0.004	0.003	0.120	0.0000	16
Commercial Minerals	-0.000	0.001	0.896	0.003	0.003	0.402	0.0000	4
Oil	0.010	0.003	0.001	0.010	0.003	0.001	0.0000	13
Multiple	0.004	0.008	0.590	0.006	0.018	0.719	0.0013	10

Table A.13 is estimated in the same manner as Table 3, except with the included study effect magnitudes clipped according to the panel header.

G.2 Impact of Studies with Highly Precise Standard Errors

In Figure 1, it is clear that several studies are highly precise (large circles), which reflects that they have small standard errors. Both the fixed effects and random effects meta-analysis estimators incorporate the precision of each estimate, weighting estimates in proportion to the squared standard error.

We investigate whether our results are driven by these highly precise estimates by censoring the study standard errors to the 90% percentile of the distribution of standard errors within commodity type. Table A.14 presents a reproduction of the main results (Table 3) with the study standard errors censored (replaced with the 90% percentile value).

The only substantive change to the results comes in the estimates of the effects for artisanal minerals, which move from positive and statistically different from zero to positive but indistinguishable from zero. The change is due to the very small standard error in Rigterink (2020).

Table A.14: Meta-Analysis Estimates with High Weights Clipped by Commodity

	Fixed Effects Meta-Analysis			Random Effects Meta-Analysis				
Commodity type	Estimate	Std. Err.	p-value	Estimate	Std. Err.	p-value	Between-study variance ($\hat{\tau}^2$)	N
Pooled	-0.001	0.004	0.795	0.004	0.005	0.165	0.0005	88
Agriculture	-0.019	0.002	0.000	-0.009	0.007	0.168	0.0011	45
Artisanal Minerals	0.005	0.004	0.235	0.005	0.005	0.275	0.0001	16
Commercial Minerals	0.001	0.001	0.658	0.003	0.003	0.392	0.0000	4
Oil & Gas	0.010	0.003	0.002	0.010	0.003	0.002	0.0000	13
Multiple	-0.006	0.010	0.552	0.006	0.019	0.740	0.0015	10

H. Evidence on Publication Bias

H.1 *p*-curve

p-curves are a diagnostic tool which assume the presence of a publication filter wherein only studies presenting "significant" p-values are published (Simonsohn et al. 2014). Assuming no true effect is present, this filter would produce "false positive" results 5% of the time, and the distribution of *p*-values below 0.05 would be uniform. Conversely, if the distribution of *p*-values below 0.05 is highly non-uniform, and skewed heavily toward very small *p*-values, then is evidence against the null hypothesis that the published effects emerged due to chance. We observe ample evidence to reject the null hypothesis.

Figure A.4: p-curve of Included Studies

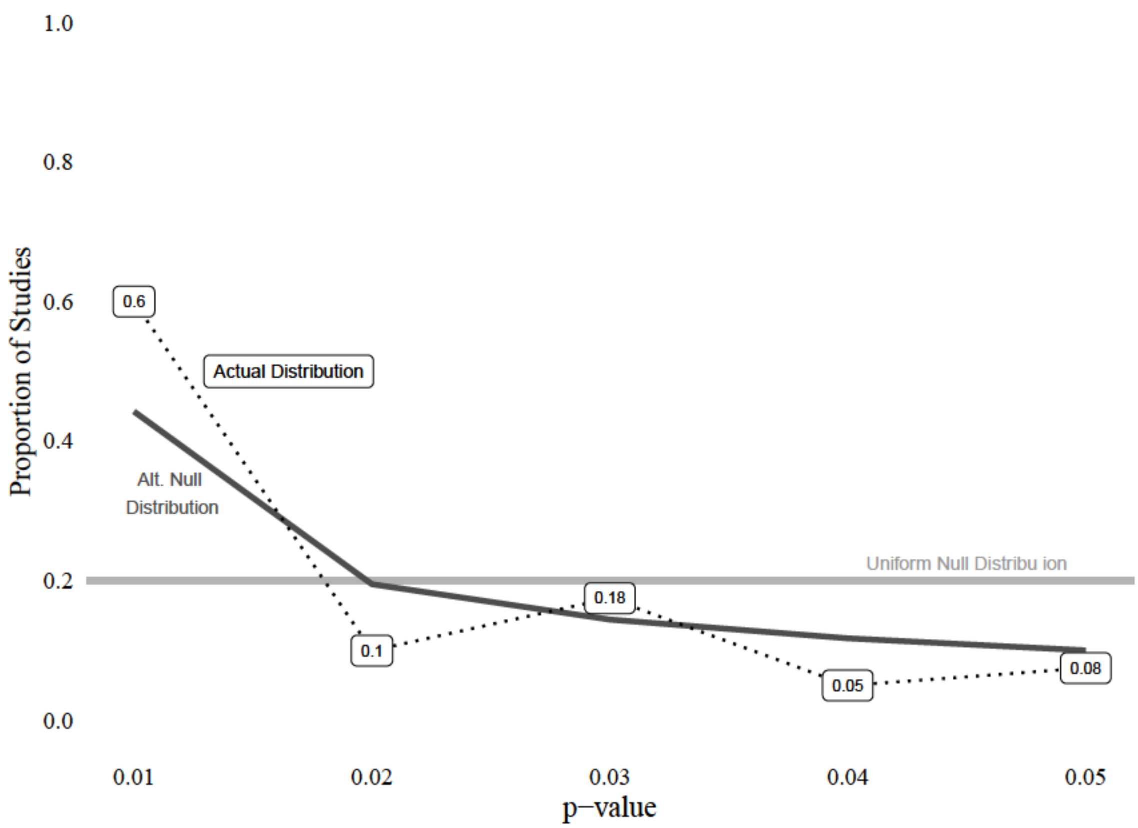


Figure A.4 displays the distribution of observed studies in blank compared to a two reference distributions: in light grey, a uniform null distribution in which there is no true effect and publication bias exists; and in dark grey (dashed line), a null distribution in which the true effect is large enough to be 33 percent powered. The presence of a greater-than-expected number of studies with $p \leq 0.01$ and fewer near the $p = 0.05$ publication threshold offers evidence against the null distributions (Simonsohn et al. 2014).

H.2 Funnel Plots

Funnel plots are a visual diagnostic tool to assess the relationship between study precision and effect size (Egger et al. 1997). Assuming a true effect μ, the distribution of observed effects should vary symmetrically about μ and by a degree proportionate to study precision. If the distribution of observed effects is non-symmetric, there is evidence that publication bias impacted data availability in our corpus.

Figure A.5: Funnel Plots by Commodity Type

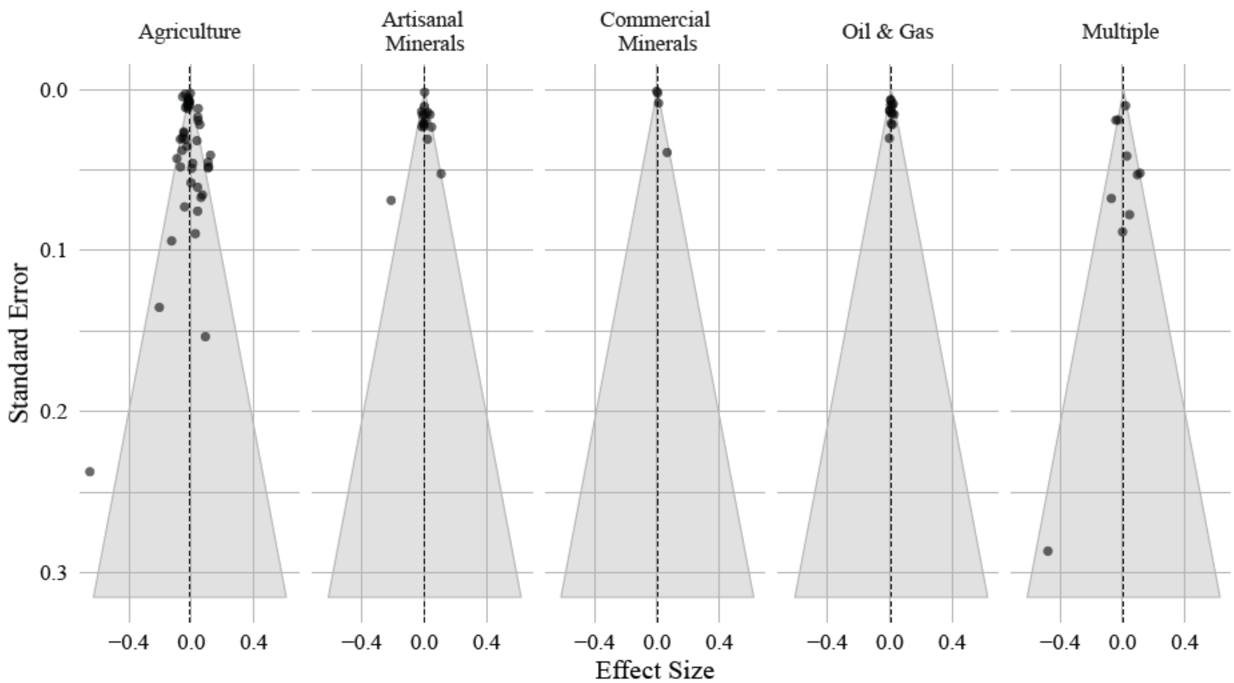


Figure A.5 displays funnel plots, which depict the relationship between coefficient estimate and standard error as a graphical diagnostic of publication bias. The presence of pronounced visual asymmetry (with high density on one one side of the vertical axis and not the other) would indicate evidence of publication bias. The presented results show little overall asymmetry, although there are a limited number of outliers in the Agriculture and Multiple commodity types (Egger et al. 1997).

H.3 Meta-regression

Meta-regression analysis offers a regression equivalent to the funnel plot: an empirical examination of the relationship between effect size and study precision (Christensen et al. 2019b). The ability to reject the null hypothesis of no relationship would suggest publication bias, but we find no evidence to do so.

Table A.15: Meta-regression to Assess Possible Publication Bias

Commodity Type	Estimate	Std. Error	p-value	N
Pooled	-0.001	0.001	0.277	102
Agriculture	0.000	0.000	0.330	47
Artisanal Minerals	-0.005	0.015	0.736	16
Commercial Minerals	-0.002	0.002	0.410	5
Oil	0.000	0.000	0.244	16
Multiple	0.001	0.001	0.467	18

Table A.15 presents effect estimates from a regression of effect size on statistical precision (Christensen et al. 2019b). If more precise results differ from less precise results, then publication bias may distort meta-estimates. Results reveal no evidence to reject a null hypothesis of no relationship, suggesting publication bias is not affecting result integrity.

I. Published Meta-Analyses in Political Science

We conducted a search of five top political science journals for meta-analyses to evaluate how common the method is in the field. We searched the full-text of the archives of the *American Political Science Review*, *American Journal of Political Science*, *Journal of Politics*, *Quarterly Journal of Politics*, and *Political Analysis*. We searched the journal's own archive in each case; we did not restrict the date range. We searched for three keywords: "systematic review," "meta," and "research synthesis." From the records we found (904), we manually filtered based on title to exclude items that represented the journal's front matter, back matter, volume information, notes from the editor, etc. (696 were retained). We then gathered the abstracts. For those we could not obtain an abstract for, we downloaded the full text PDF (117). We searched the abstracts and the first two pages of abstract-less PDFs for the three search terms (27 matched). We then manually evaluated whether the paper included a meta-analysis or systematic review in the paper or supplementary materials (5 did).

In terms of the methods of the studies that were analyzed, one meta-analysis studied only experimental work (Kalla and Broockman 2018); one studied only observational work (Doucouliagos and Ulubaşoğlu 2008); and three studied a mixture (Lau et al. 1999; Lau et al. 2007; Aarøe et al. 2017).

To assess how our sample size of estimates compares to this set of meta-analyses, we collected the number of studies and estimates where it was possible to do so from the main text, supplementary materials, or replication data. Doucouliagos and Ulubaşoğlu (2008) studied 483 estimates from 84 studies; Lau et al. (2007) 294 estimates from 111 studies; Lau et al. (1999) 117 estimates from 52 studies; Kalla and Broockman (2018) 49 studies; Aarøe et al. (2017) 66 estimates from 16 studies.

J. Deviations from Preanalysis Plan

In this section, we outline changes from our preanalysis plan. We note four changes and their justifications.

J.1 Analyses by Outcome Type

We planned to analyze our data by outcome type separately, estimating models for conflict onset, intensity, duration, and coups. We found an insufficient number of studies of conflict duration (0) and of coups (5), so exclude those types from our analyses. However, we found many studies of conflict incidence, which we include in our analyses. We did not predict theoretically substantial differences between onset and intensity outcomes, and for this reason we pool our primary analyses across the three outcome types (onset, intensity, and incidence). We demonstrate in Appendix C.2 that there is no overall difference in the effects of commodity prices on conflict by outcome type and in Appendix C.1 that when we drop any of the outcome types our results by commodity type are substantively unchanged.

J.2 Testing How Effects Vary by Commodity Type and Conflict Type

Our pre-registered analysis strategy involved fitting a hierarchical Bayesian model with heterogeneity by commodity type (e.g. labor and capital intensity and taxability of commodities). In collecting our data, we found that studies did not consistently code these dimensions. As a result, we did not include factor intensity or taxability as a moderator in our analysis. Instead, we considered heterogeneity by commodity type, which we found was possible to code ourselves. We link commodity types to theoretical mechanisms in our presentation of the results, but note that commodity type could be confounded by other unmeasured characteristics of specific commodities.

We planned to fit this model separately for six conflict outcomes: onset, intensity, and duration of armed conflict; coups; and armed conflicts in which fighting was over control of the state and those in which fighting was over control of a territory. We found that there were an insufficient number of studies focused on coups (fewer than 5); similar to the problem for commodity types, few studies coded the incompatibility of the conflict (center-seeking or territorial). As a result, we dropped these three outcomes from our analyses.

J.3 Testing Lootability Hypothesis

We did not originally intend to test theories surrounding lootability. As such, our tests of H3 should be regarded as exploratory.

J.4 Meta-Analysis Estimators

We adopt two standard meta-analysis estimators, the fixed effects and random effects estimators. We estimate the random effects estimator using the random effects maximum likelihood model. This was a deviation from our preregistered analysis plan, which called for the use of a hierarchical Bayesian model with two nested hierarchical levels for study and country.

In the data we collected, there were many fewer studies per country than we expected. Based on guidance in Gelman et al. (2014) we determined that this model would be inappropriate; hierarchical models with sufficiently few units (e.g. a single digit number of J) in any level of the hierarchy produce nonconvergent or biased parameter estimates, or become highly sensitive to choice of prior (Gelman et al. 2014: 128). For instance, attempting to fit a model with a hierarchy by region (e.g. with upper level groups equal to Africa, Asia, Americas, Multi-Region) would bias τ under a uniform prior distribution (Gelman et al. 2014: 129) and render inference highly sensitive to prior parameterization under an inverse-gamma prior distribution (Gelman et al. 2014: 130).

We also conducted a research design diagnosis via Monte Carlo simulation (Blair et al. 2019) using the structure of our data in terms of the number of studies and number per region. We found both convergence difficulties and bias in estimates of τ^2. On the basis of these challenges, we decided to depart from pre-registered analyses in favor of the fixed and random effects models discussed in our main results.

K. PRISMA Checklist

Table A.16: PRISMA Checklist

#	Checklist item	Page
1	Identify the report as a systematic review, meta-analysis, or both.	1
2	Provide a structured summary including, as applicable: background; objectives; data sources; study eligibility criteria, participants, and interventions; study appraisal and synthesis methods; results; limitations; conclusions and implications of key findings; systematic review registration number.	1
INTRODUCTION		
3	Describe the rationale for the review in the context of what is already known.	2
4	Provide an explicit statement of questions being addressed with reference to participants, interventions, comparisons, outcomes, and study design (PICOS).	Sec. 1
METHODS		
5	Indicate if a review protocol exists, if and where it can be accessed (e.g., Web address), and, if available, provide registration information including registration number.	1
6	Specify study characteristics (e.g., PICOS, length of follow-up) and report characteristics (e.g., years considered, language, publication status) used as criteria for eligibility, giving rationale.	5
7	Describe all information sources (e.g., databases with dates of coverage, contact with study authors to identify additional studies) in the search and date last searched.	5, 5
8	Present full electronic search strategy for at least one database, including any limits used, such that it could be repeated.	Pg. 5, Sec. 2.1
9	State the process for selecting studies (i.e., screening, eligibility, included in systematic review, and, if applicable, included in the meta-analysis).	5, Sec. 2.1
10	Describe method of data extraction from reports (e.g., piloted forms, independently, in duplicate) and any processes for obtaining and confirming data from investigators.	Sec. 2.1
11	List and define all variables for which data were sought (e.g., PICOS, funding sources) and any assumptions and simplifications made.	Sec. A.6
12	Describe methods used for assessing risk of bias of individual studies (including specification of whether this was done at the study or outcome level), and how this information is to be used in any data synthesis.	Sec. A.4
13	State the principal summary measures (e.g., risk ratio, difference in means).	Sec. 2.1
14	Describe the methods of handling data and combining results of studies, if done, including measures of consistency (e.g., I2) for each meta-analysis.	Sec. 2.2
15	Specify any assessment of risk of bias that may affect the cumulative evidence (e.g., publication bias, selective reporting within studies).	Sec. 2.2 & H
16	Describe methods of additional analyses (e.g., sensitivity or subgroup analyses, meta-regression), if done, indicating which were pre-specified.	Sec. 2.2
RESULTS		
17	Give numbers of studies screened, assessed for eligibility, and included in the review, with reasons for exclusions at each stage, ideally with a flow diagram.	6
18	For each study, present characteristics for which data were extracted (e.g., study size, PICOS, follow-up period) and provide the citations.	Table A.3
19	Present data on risk of bias of each study and, if available, any outcome level assessment (see item 12).	Sec. A.4
20	For all outcomes considered (benefits or harms), present, for each study: (a) simple summary data for each intervention group (b) effect estimates and confidence intervals, ideally with a forest plot.	TBA
21	Present results of each meta-analysis done, including confidence intervals and measures of consistency.	Sec. 3
22	Present results of any assessment of risk of bias across studies (see Item 15).	Sec. H
23	Give results of additional analyses, if done (e.g., sensitivity or subgroup analyses, meta-regression [see Item 16]).	Sec. 3
DISCUSSION		
24	Summarize the main findings including the strength of evidence for each main outcome; consider their relevance to key groups (e.g., healthcare providers, users, and policy makers).	9
25	Discuss limitations at study and outcome level (e.g., risk of bias), and at review-level (e.g., incomplete retrieval of identified research, reporting bias).	8
26	Provide a general interpretation of the results in the context of other evidence, and implications for future research.	9
27	Describe sources of funding for the systematic review and other support (e.g., supply of data); role of funders for the systematic review.	1

Table A.16 reports the page numbers for each item of the PRISMA checklist (Moher et al. 2009). See `prisma-statement.org`. Item labeled TBA will be added upon publication.

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