

Do Commodity Price Shocks Cause Armed Conflict?

A Meta-Analysis of Natural Experiments*

Graeme Blair[†]
Darin Christensen[‡] and
Aaron Rudkin[§]

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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 conflict. 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 international commodity price changes on armed conflict. We show commodity price changes, on average, do not change conflict risks. However, this overall effect comprises cross-cutting effects by commodity type. In line with theory, we find price increases in labor-intensive agricultural commodities reduce conflict, while increases in the price of oil, a capital-intensive commodity, provoke conflict. We also find that prices changes for lootable artisanal minerals provoke conflict. Our meta-analysis consolidates existing evidence, but also highlights gaps for future research to fill.

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[†] Assistant Professor of Political Science, UCLA, graeme.blair@ucla.edu, <https://graemeblair.com>

[‡] Assistant Professor of Public Policy and Political Science, UCLA, darinc@luskin.ucla.edu, <https://darinchristensen.com>

[§] Ph.D. candidate in political science, UCLA, rudkin@ucla.edu, <https://rudkin.ca>.

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 civil conflict (Collier and Hoeffler 2004).² Dependency theorists further emphasize the loss of autonomy that results from reliance on commodity exports (e.g., Cardoso and Faletto 1979).

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).³ We examine work that studies three outcomes related to armed civil conflicts: onset (start of conflict), incidence (presence of conflict), or intensity (number of battles or fatalities).

The increased attention has led to debate about when, or even whether, commodity price shocks affect armed conflict.⁴ 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 for 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.” Studies often examine different sets of commodities and countries. Thus, findings that initially appear incongruous may reflect systematic variation across commodities and contexts.

We conduct a formal meta-analysis of natural experiments. We proceed in four steps. First, we conduct an expansive literature search that yields over 3,300 study records. Second, we screen studies on substantive and research-design grounds. The 46 included studies (102 estimates) quantitatively analyze the effect of plausibly exogenous variation in world commodity prices on armed civil conflict. 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 types of, 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 commodities uniformly generate windfalls that make the state or other territory a prize worth fighting for.

¹UNCTAD defines primary commodities as goods that are “largely unprocessed or unrefined,” which includes “farming, forestry, fishing, and the extractive industries” (UNCTAD 2018).

²A smaller set of works (not covered by this meta-analysis) consider interstate disputes.

³We did not find matching studies before 2002.

⁴Studies 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.

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 profit from 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 G). Only one synthesizes exclusively observational research. A recent meta-analysis, O’Brochta (2019), studies questions similar to our own.⁵ 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 joined 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).⁶ 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. This generates the first hypothesis that has been commonly tested in the empirical literature:

⁵Ahmadov (2014) conducts a meta-analysis of studies of oil wealth and democracy, another aspect of the resource curse.

⁶We 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.⁷ 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. This provides a third hypothesis:

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

Testing (H2) and (H3) requires information about whether a particular primary commodity is labor-intensive or lootable. These characteristics are rarely directly measured. 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 their capital intensity). Second, differences across commodity types that the literature attributes to lootability and labor- and capital-intensity could be confounded by other unmeasured characteristics. Third, once oil is extracted and in transport, it takes on some lootable features: long, fixed stretches of pipeline are

⁷Snyder and Bhavnani (2005: 565) provide a concise definition: “high-value goods with low economic barriers to entry.” For an encompassing description, see Le Billon (2009: 17–18).

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

Commodity Type	Characteristics		Predicted Direction		
	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

costly to defend and can be attacked with few specialized inputs. The lootability of oil, thus, varies along its supply chain.⁸

The literature on natural resources and conflict suffers from what Humphreys (2005: 510) calls “an embarrassment of mechanisms.” Here, we test many prominent claims, but not all. The studies we examine focus on the (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 all studies citing Collier and Hoeffler (2004), Dube and Vargas (2013), or Bazzi and Blattman (2014); and (3) publicly solicit additional papers to capture recent and/or unpublished work. This yields 3,346 study records (see Table 2).

Our relevance 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.⁹ 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). Most often, identification relies on the inclusion of unit and time fixed effects to absorb time-invariant confounds and global shocks.¹⁰ This second filter increases the internal

⁸By contrast, commercial minerals with low weight-to-value ratios (e.g., gold) are often transported with armored vehicles or helicopters to minimize opportunities for looting.

⁹In 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.

¹⁰A 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).

validity of included studies and increases the comparability of included designs. We retain one estimate per paper for every commodity and conflict type (onset, incidence, and intensity) following pre-specified rules discussed in 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	
Relevance Filter	DV: Conflict, IV: Commodity Value	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

We take two additional steps to ensure estimates are comparable. First, we standardize all estimates included in the meta-analysis to achieve a comparable scale. This avoids, for example, differences that arise from measuring prices in different currencies. Our standardized effects are expressed in terms of standard deviation changes in the predictor and outcome variables:

$$\hat{\beta}_{std} = \hat{\beta} \times \frac{sd(\text{Prices})}{sd(\text{Conflict})}$$

Following Mummolo and Peterson (2018), we residualize the prices and conflict 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, listed in Table A.3.

Second, for 32 of these 37 studies we acquire replication data and impose a common functional form:

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

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. Reanalysis 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). For the five studies that could not be reanalyzed due to data availability, we confirm that their analysis broadly conforms to Equation 1.¹¹

These two standardization steps exclude nine papers for which we cannot acquire the statistics needed to rescale the effect size and standard error (see Table A.6).¹² For these 9 studies and the 37 noted above,

¹¹Gehring et al. (2018) includes province-year fixed effects rather than simply year fixed effects.

¹²We also dropped two working papers authors classified as abandoned.

we can, however, compute an alternate measure of effect size, the partial r (ρ_p), which requires only the t -statistic (t) and degrees of freedom (df): $\rho_p = t / \sqrt{t^2 + df}$ (see Appendix B.4 for details). The distribution of ρ_p does not change depending on the inclusion of these nine studies; these exclusions are unlikely to affect our results.

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 ($\hat{\beta}$ s from Equation 1, with weights equal to the inverse of the standardized variance). 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).¹³ This model assumes that the true effects differ across studies, but that these are drawn from a common (normal) distribution. The estimator recovers both the mean and variance (τ^2) of that distribution, which is useful in generalizing to studies not included in this meta-analysis, because we can characterize the distributions generating study effects. We note two differences between the fixed and random effects models that arise when $\hat{\tau}^2 > 0$: first, the standard errors from the random effects model will always be weakly larger; second, the relative weight placed on imprecise estimates will be greater in the random effects model.

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. 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.¹⁴

Our estimates pool across conflict types (incidence, onset, and intensity). 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.

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-curve, funnel plot, and meta-regression analysis), which we describe in Appendix F. 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.

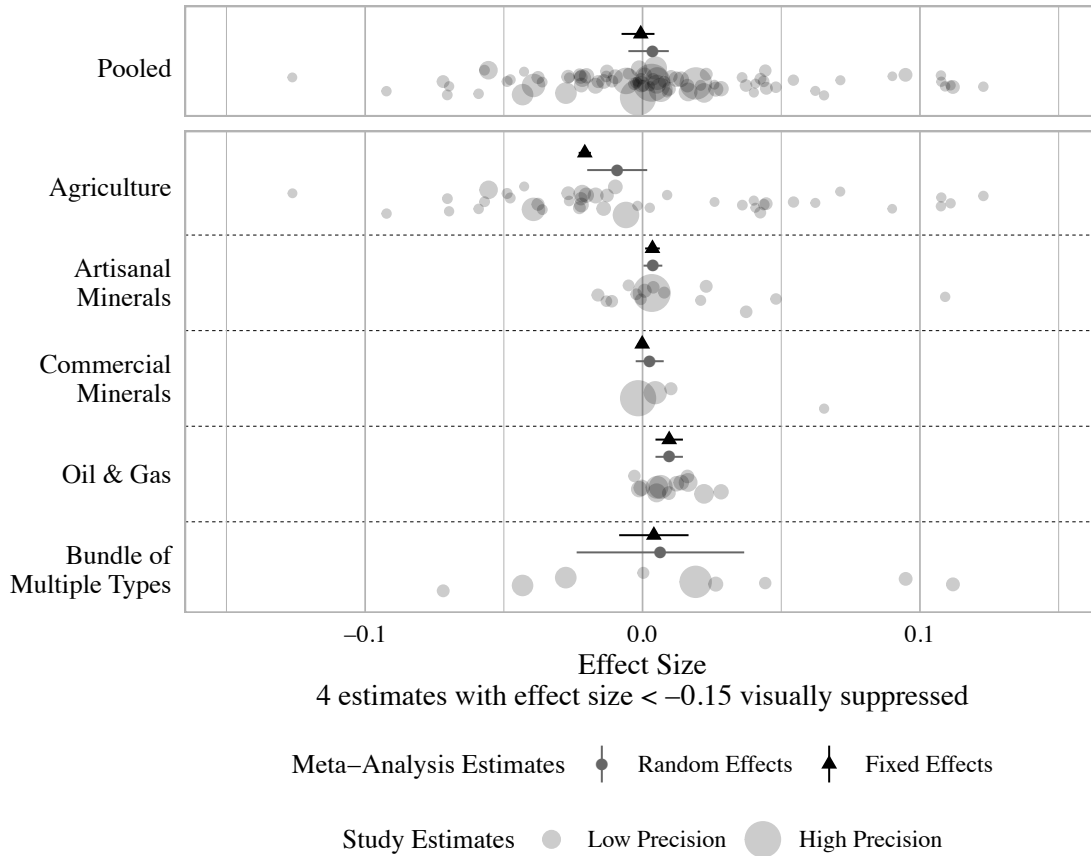
¹³We pre-registered a hierarchical Bayesian random effects model with study and country hierarchies. We could not, however, fit this model given an insufficient number of studies within most countries (see Appendix H).

¹⁴This avoids over-weighting commodity types that received more scholarly attention. We bootstrap confidence intervals and p-values using the bias-corrected percentile method.

3. Results

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% confidence intervals and the raw data from each study (see numerical estimates in Table 3).¹⁵ We also see no effect for bundles that include multiple types of commodities. 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 D).

By contrast, we find that price increases for agricultural commodities — which are labor-intensive relative to other classes — reduce armed conflict: the fixed effects estimate is -0.021 and significant.

¹⁵The confidence intervals for the pooled effect are not centered due to our bootstrapping procedure.

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 $p = 0.165$. When we more closely inspect some of the positive estimates for agricultural commodities, we find authors arguing that these particular crops are more capital intensive: for example, Gehring et al. (2018) argue that wheat production in Afghanistan is relatively capital intensive. 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.

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

Commodity type	Fixed Effects Meta-Analysis			Random Effects Meta-Analysis				N
	Estimate	Std. Err.	p-value	Estimate	Std. Err.	p-value	Between-study variance ($\hat{\tau}^2$)	
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 in true effects, suggesting that lootability offsets the opportunity-cost mechanism.

Finally, we do not find any effect for commercial minerals.¹⁶ We are wary of over-interpreting a null result with just 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 tons of 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.

¹⁶Artisanal and commercial mining can occur in close proximity. In these contexts, colocation complicates efforts to separately estimate effects for both commodity types. This should generate a convergence in our estimates for commercial and artisanal mining.

A meta-analysis not only reveals what we have learned, it also identifies gaps in our knowledge — areas where new research can make a large marginal contribution to the field. First, 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 one estimate from Asia comes from Myanmar. Artisanal mining is not confined to these places: The World Bank estimates that roughly 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.

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

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

Second, we have a rich set of theoretical predictions about factors that moderate the relationship between commodities' prices and conflict. Yet, we often lack the measures needed to evaluate the scope conditions or predictions of these models. Collecting better data on moderators enables improved tests of theory. Future research should directly measure features such as capital intensity, illegality, lootability, and taxability. Progress has been made by studying heterogeneity across commodities. We expect additional insights will come from 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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