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Samuel Bazzi
UC San Diego

Christopher Blattman
Yale University
Center for Global Development

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Center for Global Development
1800 Massachusetts Ave., NW
Washington, DC 20036

202.416.4000
(f) 202.416.4050

www.cgdev.org

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Samuel Bazzi, UCSD

Christopher Blattman, Yale University†

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Abstract: One of the most influential ideas in the study of political instability is that income shocks provoke conflict. "State prize" theories argue that higher revenues increase incentives to capture the state. "Opportunity cost" theories argue that higher prices decrease individual incentives to revolt. Both mechanisms are central to leading models of state development and collapse. But are they well-founded? We examine the effects of exogenous commodity price shocks on conflict and coups, and find little evidence in favor of either theory. Evidence runs especially against the state as prize. We do find weak evidence that the intensity of fighting falls as prices rise—results more consistent with the idea that revenues augment state capacity, not prize-seeking or opportunity cost. Nevertheless, the evidence for any of these income-conflict mechanisms is weak at best. We argue that errors and publication bias have likely distorted the theoretical and empirical literature on political instability.

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† *Blattman* (corresponding author): Assistant Professor, Departments of Political Science and Economics, Yale University, 77 Prospect St., New Haven, CT 06511, christopher.blattman@yale.edu, +1 (203) 903-2528. *Bazzi*: PhD candidate, Department of Economics, UC San Diego, 9500 Gilman Dr. #0534, La Jolla, CA 92093-0534, sbazzi@ucsd.edu, +1 (314) 566-5356.

I. Introduction

Income levels and shocks are among the strongest and most robust correlate of coups, violence and war (Alesina et al. 1996; Blattman and Miguel 2010). At least two canonical theories of conflict argue for a causal link. The first rests on the idea that poverty lowers a citizen's opportunity cost of insurrection. This idea is rooted in classic economic theories of crime (Becker 1968; Ehrlich 1973), and is central to theories of insurrection (Tullock 1971; Grossman 1991; Lichbach 1995). This "opportunity cost" hypothesis drives more than just our conception of war. Domestic conflict and the individual's rational decision to produce or predate sit are central to many of the most prominent formal theories of state formation, political transitions, ethnic struggle, and economic development (e.g. Acemoglu and Robinson 2001; Bates 2008; Esteban and Ray 2008; Besley and Persson 2011; forthcoming). While plausible, evidence for the opportunity cost is thin, sometimes contradictory, and has vociferous opponents.[3] The most persuasive and widely-cited evidence in its favor comes from economic shocks. Miguel et al. (2004) argue that rainfall shocks in Africa lower household incomes, reducing the opportunity cost of conflict.

A second canonical theory argues that states are a prize that can be seized, especially when the institutions that constrain power are weak (Grossman 1995; Bates et al. 2002). This "state prize" logic predicts that the risk of insurrection rises with the value of the prize (Besley and Persson 2011). The strongest evidence comes from the historical analysis of Africa (Reno 1999; Bates 2008) and evidence that natural resource stocks drive conflict (e.g. Fearon 2005; Ross 2006).

This paper takes advantage of export commodity price volatility to test both theories. Commodity prices have three advantages. First, most countries export just a handful of products, and

[3] See Wood (2003) or Blattman and Miguel (2010) for a review.

their price movements are one of the largest determinants of incomes and revenues (Deaton 1999). Second, for most countries, this price volatility is largely exogenous. Third, not all commodities affect household incomes and state rents equally; some prices (like agricultural goods) disproportionately affect households, while others (like oil or minerals) disproportionately affect state rents. As a result, disaggregated price shocks allow us to test between mechanisms. Dube and Vargas (2009) find evidence to this effect in Colombia. They show that attacks increase when the coffee price falls (because real wages decline) but attacks decrease when the oil price falls (as real wages rise).

We collect new and more comprehensive price shocks data than available before, and examine their impact on new and ongoing political instability. We articulate tests that distinguish between the opportunity cost and state prize mechanisms. Finally, we illustrate a practical, concise and systematic approach to robustness testing that minimizes discretion and publication bias.

Our findings cast doubt on these two canonical theories of conflict. We find no evidence that price shocks raise the risk of new instability, even when large, and even in conflict-prone nations. The evidence is least supportive of the state prize motive of conflict. If anything, results run opposite to what is predicted: when oil and mineral prices rise, the probability of conflict actually falls. Coupled with evidence that higher stocks of natural resources have little impact on conflict, it may be time to move the state prize logic aside.

Of course, the absence of evidence is not evidence of absence. For instance, price shocks could have heterogeneous impacts depending on prior power and wealth and resource distributions (Morelli and Rohner 2010; Esteban and Ray 2011). But we argue we can rule out large, robust and homogenous effects. We also explore key sources of heterogeneity and find no evidence that price shocks provoke instability even in weaker, more easily captured, or more fractured states.

Our null results diverge from others. Besley and Persson (2008) find that the incidence of civil wars rises with export prices—evidence consistent with the state as prize. Meanwhile, Brückner and Ciccone (2010) and Savun and Cook (2010) find a seeming opposite result: rising prices dramatically reduce the risk of new conflicts—evidence consistent with the opportunity cost theory. We show these results are sensitive to the conflict measure, sample, and misspecification.

Our findings raise a broader concern of research and publication bias in the conflict literature. Cross-national analysis of civil war suffers from many of the risk factors associated with high publication bias (Ioannidis 2005). Most of all, we worry that the most widely-cited evidence has not been adequately checked for robustness, and that null cases commonly go unpublished.

The right answer is important. The link from income to conflict has become one of the most widely accepted facts in the literature on social unrest. The opportunity cost and state prize ideas are the engines of the mainstream models of state development and political transitions. These theories are also influential in policy. For example, states, militaries, and aid agencies predicate youth employment programs and ex-combatant reintegration on the opportunity cost idea.

Should we conclude that a causal relationship from income shocks to conflict is non-existent? Not yet. We see weak evidence that economic shocks affect the intensity (rather than the onset) of conflict. Rising prices, both agricultural and mineral, lead to fewer battle deaths. The pattern is consistent with evidence from Colombia that conflict intensity falls as coffee prices rise (Dube and Vargas 2009). It is also consistent with the idea that instigating new wars is more difficult than escalating present ones and that, in ongoing conflicts, opportunity cost matters.

Is this a victory for the opportunity cost story? We urge caution. First, the result is still fragile, and requires better intensity data to be sure. Second, the patterns we see are also consistent with an alternative we dub the "state capacity" theory: states collapse into war when they lack the capacity to suppress insurgency or bargain with competitors—capacities linked to state revenues. This revenue-centered approach has a long tradition in political science but has seldom found its way into formal models of political instability. Future research, we argue, should focus on the theoretical predictions that can distinguish between competing accounts, and pursue more micro-level data and experimentation.

II. Competing theories and predictions

A. The opportunity cost of insurrection

In opportunity cost models, a civilian's incentive to rebel rises as household income and economic opportunities decline. Thus we would expect an inverse relation between export price shocks and conflict. Not all price shocks impact household incomes equally, however. Household-produced commodities, especially agricultural goods, affect household incomes directly (Dal Bó and Dal Bó 2011). Revenues from concentrated or capital-intensive commodities, like minerals and fuels, accrue mainly to the state and will affect individual incomes only indirectly, through public goods, transfers, and employment. These indirect effects may be smaller, or offset by relative price changes (as a fall in capital-intensive export prices raises the relative returns to labor). Thus the opportunity cost theory predicts the strongest inverse relationship between insurrection and the prices of labor-intensive commodities (such as annual agricultural crops).

Commodity price shocks should provide a particularly effective. Like rainfall shocks, large price shocks tend to be transitory. Unexpected changes, especially falls, in world commodity supplies push prices temporarily out of equilibrium (Deaton and Laroque 1992; Deaton and Miller 1995). Prices are highly autocorrelated in the short run but the persistence of price shocks tends to be short. Thus most commodity price series resemble a set of brief, unpredictable spikes interspersed by long, shallow troughs.[4] Such transitory shocks ought to augment the opportunity

[4] Evidence from long-run commodity prices suggests that, for some goods, price changes are transitory while, for

cost of insurrection mechanism, since they temporarily reduce the opportunity cost of fighting without affecting the long-term value of state capture (Chassang and Padro-i-Miquel 2009).

B. The state as prize

The state prize logic produces opposing predictions. By this logic, rising commodity prices increase resource rents. This makes the state a more valuable prize, and increases incentives for political violence (Grossman 1995; Bates et al. 2002). While rents may vary most with the stocks of natural resources, theoretical models also emphasize the importance of changing resource values (Bates 2008; Besley and Persson 2008; 2011; forthcoming). One could extend the same argument to coups d'état, which arguably bring the same rents to the coup-makers without the expense and destruction of a long war.

Of course, rising revenues could also make the state easier to defend, helping states buy off opposition, counter insurgents, or strengthen control.[5] The political economy models highlighted above, however, omit this "state capacity" effect or treat the state prize effect as dominant by assumption. We return to this alternative hypothesis in the conclusion.

Meanwhile, by the state prize logic, rents not only vary with the stock and value of resources, but also the ease of capturing such rents. Hence the quality of institutions matters: the less cohesive and inclusive a state's institutions, and the more unaccountable its regime, the more the risk of conflict or coups increase with rents.

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others, price changes have a permanent, stationary component (Ghoshray 2011). Annual permanent changes, however, tend to be small in magnitude. Some large and persistent price changes result from structural breaks (e.g. the cartelization of oil in the 1970s) but large commodity price swings are typically driven by supply shocks and hence are transitory.

[5] The state capacity argument is most frequently raised in cross-national empirical analysis of conflict (e.g. Fearon and Laitin 2003; Smith 2004). Scholars of natural resources and conflict have articulated the state capacity logic non-formally (e.g. Snyder 2006; Ross 2012) but formal incorporation in political economy models remains rare (an exception is Dunning 2008).

Moreover, as with the opportunity cost mechanism, not all commodities are so easily captured or bring equal rents to the state. Any traded commodity is taxable, but some are more easily taxed than others, especially immobile, concentrated commodities with large fixed costs of investment or high switching costs. This includes non-alluvial, capital-intensive mining and petroleum, or "extractive" commodities. It also likely includes lumber, rubber and perennial tree crops like coffee and cocoa. Tree crops are typically high value, require a large initial capital investment, and are easily inspected, making them natural targets for taxation.

Disaggregated commodity shocks thus allow us to distinguish between the state prize and opportunity cost mechanisms. Higher prices of labor-intensive, smallholder-owned, and difficult-to-tax commodities (such as annual crops) should lower the risk of insurrection, while higher prices of capital-intensive or appropriable commodities (such as "extractive" minerals and fuels) should increase the value of the state and make civil conflict more likely—especially in weakly institutionalized states. The effect of higher perennial crop prices on instability is more ambiguous, however, as it arguably raises household incomes and state rents at the same time.

Note that the state prize predictions depend on the belief that price shocks are persistent. The duration of most price shocks is shorter than the average conflict. Such transitory shocks should not affect expected rents. For the state prize effect to hold, opposition leaders must either believe that they can seize the state and appropriate the rents in a short period of time or that the price change is permanent. Beliefs are more important than reality, as the transitory nature of large price shocks is not widely recognized. Until the 1990s, leading forecasters consistently predicted rising prices following positive price shocks (Deaton and Miller 1995; Deaton 1999). Even with a century of price data, it is difficult to distinguish transitory from permanent price changes. If rebel leaders forecast commodity prices no better than econometricians, then price shocks may be perceived as persistent.

III. Data

Our data cover 1957 to 2007 for all countries in Africa, the Middle East, Latin America, and Asia (excluding nations with populations under 1 million).

A. Commodity export price shocks

We develop a new country-specific measure of annual commodity export *price shocks*, S_{it}, for each country i in year t. We use new sources to identify previously unavailable price and export data on 65 legally traded commodities—nearly 50% more data points than standard sources.

We calculate S_{it} as the log difference of a commodity export price index, P_{it}. The index is a geometric average of all commodity export prices. We use U.S. dollar-denominated prices from international markets, and each price is weighted by its lagged share in total national exports.[6] The economy is most sensitive to commodity price shocks in commodity-dependent nations, and so our preferred shock measure multiplies the price difference by the ratio of commodity export values to GDP at the mid-point of the period.[7]

The geometric shock can be disaggregated additively by commodity or commodity class. We distinguish between price shocks to *Annual* agricultural goods (such as oilseeds, food crops, and livestock), S_{Ait}; *Perennial* tree crops (such as cocoa, coffee, rubber or lumber), S_{Pit}; and *Extractive* products (such as iron, oil and gas), S_{Eit}, where $S_{it} = S_{Ait} + S_{Pit} + S_{Eit}$.

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[6] The lagged export share is the average of shares in $t-2$ to $t-4$. Time-varying weights provide a sharper measure of the price shock, but we also examine results using fixed weights from 1980. Our main source of export shares come from UNSD (2010). Since the UN database contains many gaps, we supplement it with data from regional and country statistical yearbooks. Small gaps in export weight data were interpolated geometrically. When export data are missing at the beginning of the period, such as 1955-1965, we use the earliest weights data available. Detailed data sources and construction are described in a data appendix available from the authors.

[7] To calculate *X/GDP*, we take the average of the ratio in the years 1978 to 1982, and the nearest five years to 1980. Export values come from the same database as the export shares (UNSD 2010), and GDP comes from the World Development Indicators (World Bank 2009).

There are two potential sources of endogeneity in this shock. The first arises from the fact that the unobserved variables that drive conflict risk (such as poverty or weak institutions) can also reduce a nation's export diversity. Lower diversification increases price volatility and can create spurious correlation between shocks and conflict. To the extent these unobserved variables are time-invariant, country fixed effects control for them.

The second source of endogeneity is that not all nations are price-takers. Nations that produce a significant share of world output (e.g. cocoa in Cote d'Ivoire) are potential "price-makers" in that adverse supply shocks will increase world prices. If world prices rise in anticipation of conflict (because of lower supply) there will be a spurious positive correlation between conflict and lagged price shocks. To avoid this bias, we omit from a nation's price shock any products where they produce more than a 10% share of global exports. We also consider 3% and 20% thresholds.

B. Insurrection and internal conflict

Although there are six common measures of internal war, most papers examine just one. There are four major datasets, produced by: UCDP/PRIO (Gleditsch et al. 2002); Fearon and Laitin (2003); Sambanis (2004); and the Correlates of War, or COW (Sarkees and Wayman 2010). All four use a threshold of 1000 battle deaths to define a year of *Civil war*. The UCDP/PRIO dataset also codes smaller *Civil conflicts* of at least 25 battle deaths per year, leading to three common UCDP/PRIO conflict constructions: low intensity (25 deaths) conflict years, *cumulatively* high-intensity (1000 death) years, and high-intensity years only.

These competing measures show surprisingly little consensus on how to code a war. The datasets disagree on when to code the start, what counts as a war, how to treat breaks in violence, and a host of other factors (Sambanis 2004). For our purposes, the most theoretically important difference is how lulls in conflict are treated. The UCDP/PRIO dataset codes conflict mechanically and episodically; if battle deaths fall below the threshold in a given year, the year is coded as a zero. The other three measures code a war more politically, attempting to measure the start date of a war as the beginning of hostilities and treating lulls in conflict as years of ongoing war. Thus UCDP/PRIO is useful for capturing variation in intensity, while the others are suited to more decisive changes in violence.

Table 1 lists summary statistics. Most empirical work tends to focus on either the *onset* of new conflict events (an indicator equaling one in the year a new conflict begins, with years of ongoing conflict coded as zeros or dropped), or the *incidence* of conflict (an indicator equaling one in years of a new or ongoing war).

Together these datasets give the researcher at least 6 choices of dependent variable for onset, and 12 if we include incidence—a considerable amount of latitude. The differences in the datasets are evident simply by comparing means of incidence. UCDP/PRIO high-intensity civil war and Fearon and Laitin civil wars are coded in 7% and 20% of country-years, respectively.

We take the approach we believe ought to be standard: we consider all conflict measures and regard skeptically any relationship that fails to produce consistent signs, magnitudes, and robustness without an ex-ante theoretical rationale for preferring one measure over another.

Finally, in addition to looking at indicators for passing a death threshold, we also consider the underlying battle death data itself (Lacina and Gleditsch 2005) as a measure of conflict intensity.

C. Coups

We also examine two different measures of coups d'état. The first, a measure of actual or attempted *Coups*, comes from Powell and Thyne (2011). It is an indicator for any illegal and overt attempt by the military or elites within the state to unseat the sitting executive. We also consider a broader measure of unconstitutional changes in leadership, or *Irregular leader exit*, which includes successful coups, assassinations, and revolts. The measure comes from the Archigos dataset (Goemans et al. 2009), which identifies the effective head of state for all nations between 1875 and 2004 as well as their manner of gaining and losing power.

IV. Empirical strategy

Cross-national conflict specifications vary, but typically look like this:

(1) $C_{it} = \alpha + \boldsymbol{X_i\beta} + \boldsymbol{S_{it}\theta} + \varepsilon_{it}$,

where C is an indicator for a conflict event (either onset or incidence) for country i in year t, $\boldsymbol{X}$ is a vector of relatively time-invariant country characteristics, $\boldsymbol{S}$ is a vector of time-varying country characteristics (like price or rainfall shocks), and ε is an i.i.d. error term. Researchers employ both linear and non-linear (e.g. logistic) regressions, and for clarity of exposition we discuss our empirical strategy using the linear model. Our analysis will employ both.

If we are interested in the coefficient on an exogenous, time-varying shock, there are several problems with the standard approach. First, the time-invariant $\boldsymbol{X}$ vector is often endogenous and so introduces bias. Second, the time-dependence of time-varying shocks is typically ignored, also introducing bias. Commodity prices, for example, are negatively autocorrelated and can take several periods to impact incomes (Deaton and Miller 1995). This leads to correlation between the current shock and the error term. Our $\boldsymbol{S_{it}}$ includes the current shock and two lags.

The third and most serious problem comes when C is an indicator for conflict incidence—equaling one for a year of new conflict and a year of ongoing conflict. This approach constrains shocks to have the same effect on conflict onset as continuation/ending. This raises both conceptual and empirical concerns. Conceptually, shocks could have a larger effect in ongoing conflicts. For instance, if initiating a conflict has a fixed cost, or faces a coordination problem, new conflicts could be less sensitive to price shocks than existing ones on average.

The more serious concern is empirical, however, as ignoring dynamics biases the estimated effect of shocks on conflict. Conflict is highly persistent, and current and lagged conflict are affected by current and lagged shocks. If we omit the lagged dependent variable we introduce a large correlation between the error term, the dependent variable, and the shocks.

While one solution is to use a dynamic model, mathematically it is identical to modeling onset and ending separately, on split samples (Beck and Katz 2011):

(2a) $Onset_{it} = \alpha_{Oi} + \tau_{Ot} + \boldsymbol{S}_{it}\boldsymbol{\theta}_O + \chi_{Oi} + \varepsilon_{Oit}$

(2b) $Ending_{it} = \alpha_{Ei} + \tau_{Et} + \boldsymbol{S}_{it}\boldsymbol{\theta}_E + \chi_{Ei} + \varepsilon_{Eit}$

where α_i and τ_t are country and year fixed effects, and χ_i is a country-specific error.[8] Coefficients in the dynamic model confirm that the simple conflict incidence regression is both biased and unnecessarily constrained (regressions not shown).

There are three ways to estimate equations 2a and 2b: logit regression without fixed effects, fixed effects logit, or a fixed effects linear model. We prefer fixed effects, as they control for unobserved country characteristics that lead a country to be more conflict-prone as well as less diversified and more resource-dependent. While logit has a slight efficiency advantage, it does not sit well with fixed effects, nor does it have easily interpretable magnitudes with fixed effects (Beck 2011). This paper reports both models, but focuses on the linear model for ease.

Finally, to estimate the effect of battle *Deaths*, we consider two specifications:

(3a) $Deaths_{it} = \alpha_{Di} + \tau_{Dt} + \boldsymbol{S}_{it}\boldsymbol{\theta}_D + \pi_{Dt}First_{it} + \delta_{Dt}Duration_{it} + \varepsilon_{Dit}$

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[8] Equation (2a) includes all peace years (where *C* equals 0) and the first year of onset (which is equivalent to the common practice of modeling onset and recording years of ongoing conflict as missing). Equation (2b) treats all years of ongoing conflict as a zero and the year of ending as a 1. We include year fixed effects to eliminate potential bias from the co-movement of global shocks and global conflict, and cluster standard errors by country. The use of country-specific time-trends has no material effect and in the absence of a theoretical rationale we omit them.

(3b) $Deaths_{it} = \alpha_D + \tau_{Dt} + \boldsymbol{S}_{it}\boldsymbol{\theta}_D + \beta_{Dt}Deaths_{it-1} + \pi_{Dt}First_{it} + \delta_{Dt}Duration_{it} + \varepsilon_{Dit}$

which are estimated only on years of ongoing conflict. To allow for heterogeneous impacts over the lifespan of a conflict, we include an indicator for the first year of conflict (*First*) and a count variable for the length of the conflict (*Duration*), though the results are not particularly sensitive to their exclusion. Because both price shocks and battle deaths may be time-dependent, we consider specification (3b), which includes a lagged dependent variable and omits country fixed effects.[9] We also consider the natural log of *Deaths* as a dependent variable.

V. Results

A. The impact of price shocks on new conflicts and coups

A large trade and development literature shows that commodity shocks have an enormous impact on national income, investment, and public expenditures (Deaton 1999). Our commodity data are broadly consistent with this literature: a one standard deviation shock increases per capita GDP growth in our sample by 1.4 percentage points per year (a 36% increase over mean growth) and by 2.0 percentage points in Africa (an increase of 76% relative to mean growth).[10]

Impact of the aggregate and disaggregated price shocks.—Table 2 displays the results of a linear regression of coups and conflict onset on price shocks (equation 2a). All shocks are standardized to have mean zero and unit standard deviation. In addition to the regression estimates, each column lists the sum of the three shock coefficients and the p-value on this sum. To interpret

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[9] In the presence of lagged dependent variables, fixed effects can produce attenuation bias, especially over short panels (Nickell 1981). In the full panel used to estimate equations (2a) and (2b), the bias is quite small of order $1/T$ where T is the number of years (at least 40 in the most countries). In equations (3a) and (3b), however, the median panel length is $T=6$.

[10] Figures are based on a regression of current-price GDP data from the World Development Indicators on the current shock and two lags. The appendix provides details.

magnitude, the columns also list the change in conflict risk associated with the sum of all shocks.

Overall, we see no evidence of a consistent, robust relationship between commodity price shocks and political instability. The point estimates on S_t, S_{t-1}, and S_{t-2} shift in sign and magnitude depending on the lag or the measure of conflict onset, and tend to be small relative to their standard errors. Only one of the 24 coefficients is statistically significant at the 5% level. The standard errors are large, however, and so we cannot rule out large effects in either direction.

Table 3 disaggregates the shock into annual, perennial and extractive commodities. The opportunity cost mechanism predicts an inverse relationship between agricultural (annual) price shocks and onset, but we see little evidence of a relationship. Of the 24 agricultural price coefficients, more than half have the "wrong" sign and just one is significant at the 5 percent level (for coups). Of the six war variables, only one of the six sums runs in the predicted direction.

The state prize mechanism predicts we should see a positive relationship between the prices of easily-captured commodities and conflict onset/coups, especially for minerals and fuels, but also potentially for perennial crops as well. We see no consistent or statistically significant relationships with conflict onset and coups. Most of the signs are the opposite of what the theory predicts. Looking at the sum of mineral shocks on conflict ending, two of these sums are positive and significant, suggesting that rising prices actually lower the risk of war (though, as we see below, this significance is fragile to conflict measure and specification).

How to reconcile this null finding with previous results?—These results contrast with previous evidence on price shocks and social conflict. For instance, Bruckner and Ciccone (2010) find a large and robust inverse relationship between war onset and export price in sub-Saharan Africa. Besley and Persson (2008) find a positive relation between export prices and war incidence. The Besley and Persson data were not available for replication, but we reconcile our analysis to the Bruckner and Ciccone (BC) results in Table 4, making changes cumulatively in Columns 1 to 8 until the results in Table 2 match standardized BC results (Column 8). Looking leftwards from the BC result in Column 8, the result appears to be dependent on the sample of years (1983 onwards alone); to the use of a particular estimator (instead of a standard logit, ordinary least squares, or fixed effects estimator)11; to a price shock containing fewer commodities; and, most of all, a unique coding and older version of the UCDP/PRIO war measure. Moving rightwards to columns 9 to 14, even using the BC sample, estimator, and price shock, the result is weakly robust to the use of the standard UCDP/PRIO codings or other conflict datasets. The results are not robust at all, however, with the use of an OLS or logit estimator, or a less restrictive sample (i.e. using new commodity data).

In short, the large and robust relationship seems to be a function of a very specific sample, model and dependent variable choice, a limitation common to the conflict literature and one that is detrimental to our theoretical and empirical understanding of conflict processes.

***A systematic approach to robustness analysis.*—**We advocate a systematic approach to robustness analysis, one that allows researchers to judge the sensitivity of any estimate to arbitrary choices in the dependent and independent variables and the empirical model, without burdening a paper with dozens of tables.

For each of the six onset measures, we estimate ten models, each by a linear and logit regression, for a total of 120 specifications. We selected our ten models based on ex-ante specification of plausible alternatives, from which we selected our preferred specification prior to analysis. Specifically, we: (1) start with our main specification; (2) drop the *X*/*GDP* rescale; (3) include all price-makers; (4) use a 3% price-maker cutoff instead of 10%; (5) use a 20% price-maker cutoff;

[11] The authors use an instrumental variable estimator equivalent to a least squares estimator that does not use the typical small-sample adjustment.

(6) replace time-varying weights with fixed 1980 weights; (7) censor the price shock at the 1st and 99th percentile; (8) eliminate year fixed effects; (9) eliminate country fixed effects; and (10) include country-specific time trends. We decompose the shock into the three classes, and examine the size and significance of the sum (giving us 360 potential sums and p-values).

Figure 1 graphs the results. Figure 1(i) displays the distribution of p-values. A purely random relationship between price shocks and conflict would produce a simple uniform distribution (indicated by the horizontal line). Our actual results resemble such a uniform distribution.

We plot these p-values against the magnitude of the coefficients in terms of their impact of the risk of conflict in Figure 1(ii), using only the linear model. We plot a kernel regression line for each commodity type. None of the sums for annual agricultural commodities or extractive minerals display a consistent direction of impact, let alone a robust relationship. Only three appear to cross the p=0.05 threshold (the vertical line) and all are perennial shocks (the one commodity family with a theoretically ambiguous effect on conflict). One of the three perennial shock coefficients indicate a large increase in conflict risk, however, and two a large decrease. The logit model (discussed below) performs worse on average.

Impact heterogeneity: Could shocks have robust impacts in weak, fractured or more captured states?—Not all societies are equally vulnerable to income shocks. One reason we do not see robust results could be the fact that we do not take the relevant fragilities into account. Indeed, our theory suggests the expected value of state capture is not simply a function of rents and revenues, but whether these can be appropriated by the leader. Where executive power is checked, the incentives for a violent overthrow will be reduced overall, and be resilient to changes in state revenue. Hence the state prize effect will be strongest in centralized, less competitive regimes.

We first look at the effect of price shocks under different regimes, using Polity IV regime data.[12] We start with the impact of shocks in *Non-democracies*. We also look a subcomponent of autocracy—*Low Executive Constraints*, a below-median score in the Polity measure of the extent of institutional constraints on the decision-making powers of the chief executive. While autocratic or unconstrained regimes may be more attractive to capture, these states can also be durable and resistant to conflict. Empirically-speaking, we may prefer a measure of weak non-democratic states, vulnerable to revolt. A body of cross-national evidence suggests that *Anocracies* are the least stable type, and so we consider them in addition to non-democracies (Vreeland 2008). More fragile still, state-failure forecasting suggests that extremely polarized anocracies, or *Partial democracies with factionalism* (*PDF) regimes*, are the most unstable—over 30 times more likely to collapse than autocracies (Goldstone et al. 2010).

Figure 2 performs the same graphical robustness analysis as in Figure 2 but on regime subsamples. Panel (i) graphs annual shocks and (ii) graphs extractive shocks. We see no evidence of a robust relationship, and hence no support for the opportunity cost or state prize effects in weaker or more centralized or unconstrained regimes. Looking at annual shocks, a small number of point estimates are significant for non-democracies and unconstrained executives, but these results are fragile and point in the opposite direction predicted by the opportunity cost theory. Looking at extractive shocks, none of the point estimates are significant at conventional levels. Estimates for unconstrained executives and PDF regimes point in the "right" direction for the state prize theory (higher prices are associated with more conflict) but are not robust.

Finally, other scholars emphasize that latent social conflict and societal fractures are a crucial

[12] The 21-point Polity index runs from Autocracy (-10) to Democracy (+10), and is calculated using measures of executive constraints, the openness and competitiveness of executive recruitment, and the regulation and competitiveness of political participation (Marshall et al. 2009).

risk factor. If shocks accentuate inter-group conflict via mutual fears, the existence and depth of the social cleavage undoubtedly raises the risk of conflict. Robust conflict risk factors include ethnic dominance and ethnic polarization (Fearon et al. 2007; Esteban and Ray 2011). Absolute and relative levels of deprivation (i.e. inequality and poverty) are also robust predictors of conflict, and associated with weak states and grievances (Blattman and Miguel 2010). Figure 3 performs the same analysis as in Figure 2 for three other high-risk sub-samples: countries with high (above median) levels of ethnic polarization, above median levels of inequality, and below median levels of GDP per capita.[13] It also looks at sub-Saharan Africa (SSA) alone. Again we see no evidence that the shocks are more robust in these high-risk cases.

B. Conflict continuation and ending

***Impact of the aggregate and disaggregated price shocks.*—**We perform identical analysis for conflict ending (i.e. peace onset) in Table 5, using equation 2b. The results are larger and more robust than with conflict onset, though not uniformly so. Just 3 of the 18 price shock coefficients are significant at the 5% level, and two of the six sums are positive and significant at the 10% level. Here, the relationship between shocks and conflict ending is more consistently positive (rising prices lead to rising incomes, which lower the probability of a conflict continuing) but the magnitudes vary dramatically and in one case—using Fearon and Laitin data—the sum still flips sign. The relationship is most robust using the more episodic, high intensity measures from UCDP/PRIO and the COW measure.

We see a similar pattern in disaggregated shocks (Table 6). Virtually all the coefficients and

[13] For each country we take the earliest available year of data for these measures, and construct an indicator that equals one if the country is above the median level in our sample. Polarization figures come from Montalvo and Reynal-Querol (2005). For inequality we use Gini coefficients from UNU-WIDER (2008). GDP per capita come from World Bank (2009).

sums are positive (save for Fearon and Laitin), some significantly so. Just 8 of the 36 shock coefficients are significant at the 5% level. Only the sums of perennial price shocks are statistically significant, and again only for the high-intensity UCDP/PRIO and COW measures. The annual and extractive shocks point in the same direction and have similarly large magnitudes.

These findings sit uncomfortably with the theoretical predictions. The state prize mechanism predicts the opposite relation between shocks and conflict ending—higher prices should intensify the struggle for the state. And the robust effect of perennial shocks is only consistent with the opportunity cost mechanism to the extent that tree crops affect household incomes more than annual crops. But our theoretical priors (that these commodities are most taxed) predicted otherwise. Before dwelling on these results, are they robust enough to merit rationalization?

Systematic robustness and heterogeneity analysis.—Figure 4 applies the robustness analysis to ending. The distribution of p-values is skewed towards the left now, with roughly one in four logit sums of shock coefficients significant at the 5% level, and one in six using the linear probability model (Panel i). The effects are more uniformly positive, though the sign on the extractive shock has a greater tendency to flip (Panel ii).

Which specifications tend to produce more significant results? We systematically analyse p-values in Table 7, using each robustness regression, by commodity group (annual, perennial, extractive) as an observation, and regressing the p-value on a series of indicators for the dependent variable used, or the specification employed. We examine onset and ending separately. Positive coefficients imply the variable or specification choice reduces statistical significance, while negative coefficients imply that statistical significance is improved.[14] Our main specification (Equa-

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[14] The data are not independently distributed, and so we should take the standard errors with caution (even if we want to cluster on the dependent variable, there are too few clusters to give meaningful analysis). But the sign and

tion 2) provides the most efficient estimates. The exception is the logit versus linear model choice—logit is less efficient on average with onsets, but more efficient with ending.

The high-intensity war measures from UCDP-PRIO and COW are more sensitive to price shocks than other war measures. The robustness of these results, however, is sensitive to model assumptions such as the use of fixed export commodity weights, different price-maker cut-offs, or the absence of fixed effects.[15] Nevertheless, there may be a signal inside this noise. The evidence suggests an effect of shocks on conflict ending, and it is possible that price variation affects intensity of conflict, and so we complete our analysis with a look at battle deaths.

Before doing so, note that focusing on regime subsamples (Figure 5) or on other risk factors (Figure 6) does not appear to significantly improve the performance of the shock measures—different risk factors have different effects, sometimes running in the opposite of the predicted direction. Overall, few of the regressions pass conventional thresholds for statistical significance.

C. Conflict intensity and battle deaths

Most of the theoretical models we examined do not analyze or predict the onset of conflict, but rather the degree of arming and fighting. Higher commodity prices are associated with decreased abilities to recruit and fight in the opportunity cost and "state capacity" cases, and increased incentives to recruit and fight (in the state prize case). The fact that we see the most robust results with the most episodic measures of conflict suggests that price shocks may be influencing the intensity of ongoing conflicts rather than the onset of new ones or complete conflict ending. If so, an ending indicator would measure this relationship with significant error, leading to large

magnitude of coefficients is informative.

[15] For an example of the underlying robustness checks, see the appendix (Appendix Table 2) for an illustration with one of the most robust dependent variables, UCDP/PRIO cumulatively high intensity warfare, using the logit model. The magnitudes of the sums of shocks are consistently large, but robustness is highly sensitive to simple model changes.

coefficients imprecisely estimated. Rather, we should be looking at intensity directly.

We turn to the sole available data cross-nationally on conflict intensity, from battle deaths. Table 8 displays results from equation 3a (with country fixed effects but without lagged battle deaths) and equation 3b (with a lagged dependent variable, but without fixed effects).[16]

Overall, the dynamic results suggest a large (but somewhat fragile) inverse relationship between price shocks and intensity—higher prices mean less battle deaths, mainly in the current year. A standard deviation rise in current export prices is associated with 475 fewer battle deaths in the static linear specification (Column 1), and 803 fewer deaths in the dynamic specification (Column 2). The latter represents a 15% decrease in average battle deaths. Only the dynamic specification is statistically significant, however. Given the high, positive autocorrelation of battle deaths, the static model is arguably biased towards zero, and (as with conflict incidence above) we prefer to account for rather than ignore dynamics that we know to bias results. Both results are displayed so as not to ignore the sensitivity to dynamics.

In some conflicts, only total and not annual battle deaths are known, and so the death estimates do not vary over time. If we omit these years from the dynamic specification (Column 3) the size and significance of the coefficient falls, suggesting that the omitted conflicts have high average deaths and large falls in prices, or few average deaths and little changes in prices.

We repeat the same three regressions using the natural log of battle deaths as a dependent variable (Columns 4 to 6). The contemporaneous shock is inversely associated with battle deaths in all three specifications, though only at the 10% level of significance when estimating the static

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[16] The battle deaths dataset estimates a high, low, and "best" estimate of battle deaths. Roughly a third of country-years do not have a best estimate, only a range. Thus we estimate equations (3a) and (b) using interval regression, taking the "best estimate" as a point estimate when available but using the high-low interval otherwise. The lagged dependent variable cannot be an interval, however, and here we use the average of the high and low estimates. We use a linear lagged dependent variable in log specification as well, so as not to lose the first year of all conflicts.

model (Column 4) or the model omitting years without annualized death data (Column 6).

Note that, in all specifications, only the contemporaneous shock is negative and significant. Lagged shocks have no systematic relationship with current battle deaths—the magnitude and even sign changes easily, and none are significant. The sum of the contemporary shock and three lags are generally negative but never significantly so.

Table 9 relates battle deaths to disaggregated price shocks. The negative effect of current price increases on battle deaths is robust only for the dynamic specification with all years of data (Columns 2 and 5). The logarithmic and static specifications are generally weaker or not statistically significant. Omitting years without annual deaths data reduces the size and significance of all relationships.

The inverse relationship between current price shocks and current battle deaths holds for all three commodity families. The relationship is strongest for annual crop prices and extractive commodity prices. Perennial crops have the same effect but it is smaller and less robust.

The result complements evidence from Colombia that increases in the coffee price are associated with fewer military attacks in coffee-producing regions (Dube and Vargas 2009). It runs against the same paper's finding, however, that an oil price increase also increases attacks.

VI. Discussion and conclusions

A. Implications for our theories of political instability and conflict

***The state is not a prize?*—**Warlord politics and the state prize logic lie at the center of the most influential models of conflict, state development, and political transitions in economics and political science. Yet we see no evidence for this idea in economic shocks, even when looking at the friendliest cases: fragile and unconstrained states dominated by extractive commodity revenues. Indeed, we see the opposite correlation: if anything, higher rents from commodity prices weakly lower the risk and length of conflict.

Perhaps shocks are the wrong test. Stocks of resources could matter more than price shocks (especially if shocks are transitory). But combined with emerging evidence that war onset is no more likely even with rapid increases in known oil reserves (Humphreys 2005; Cotet and Tsui 2010) we regard the state prize logic of war with skepticism.[17] Our main political economy models may need a new engine.

Naturally, an absence of evidence cannot be taken for evidence of absence. Many of our conflict onset and ending results include sizeable positive and negative effects.[18] Even so, commodity price shocks are highly influential in income and should provide a rich source of identifiable variation in instability. It is difficult to find a better-measured, more abundant, and plausibly exogenous independent variable than price volatility. Moreover, other time-varying variables, like rainfall and foreign aid, exhibit robust correlations with conflict in spite of suffering similar empirical drawbacks and generally smaller sample sizes (Miguel et al. 2004; Nielsen et al. 2011). Thus we take the absence of evidence seriously.

***Do resource revenues drive state capacity?*—**State prize models assume that rising revenues raise the value of the capturing the state, but have ignored or downplayed the effect of revenues on self-defense. We saw that a growing empirical political science literature takes just such a revenue-centered approach, illustrating that resource boom times permit both payoffs and repres-

[17] Ross (2012) argues that oil stocks increase political instability in some cases—with onshore oil, and in lower income nations only—not from a state prize effect, but rather because of separatist incentives and insurgent financing.

[18] Aside from the general principle that one cannot prove the null hypothesis, there are other reasons why cross-national data and methods could bias us towards the null. First, like all cross-national analyses of conflict the dependent variable is subject to error. A second limitation is unobserved heterogeneity in impacts—we measure a number of high-risk subsamples but these too are error prone, and some remain unobserved (e.g. the state's dependence on primary commodity revenues and rents). A third challenge is that the time lag between price shocks and conflict onset may be heterogeneous with and across countries.

sion, and that stocks of lootable or extractive resources can bring political order and stability. This countervailing effect is most likely with transitory shocks, as current revenues are affected while long term value is not.

Our findings are partly consistent with this state capacity effect. For example, conflict intensity is most sensitive to changes in the extractive commodities rather than the annual agricultural crops that affect household incomes more directly. The relationship only holds for conflict intensity, however, and is somewhat fragile. We do not see a large, consistent or robust decline in conflict or coup risk when prices fall. A reasonable interpretation is that the state prize and state capacity effects are either small or tend to cancel one another out.

Opportunity cost: Victory by default?—Finally, the inverse relationship between prices and war intensity is consistent with opportunity cost accounts, but not exclusively so. As we noted above, the relationship between intensity and extractive commodity prices is more consistent with the state capacity view. Moreover, we shouldn't mistake an inverse relation between individual aggression and incomes as evidence for the opportunity cost mechanism. The same correlation is consistent with psychological theories of stress and aggression (Berkowitz 1993) and sociological and political theories of relative deprivation and anomie (Merton 1938; Gurr 1971). Micro-empirical work will be needed to distinguish between these mechanisms.

Other reasons for a null result.—Ultimately, however, the fact that commodity price shocks have no discernible effect on new conflict onsets, but some effect on ongoing conflict, suggests that political stability might be less sensitive to income or temporary shocks than generally believed. One possibility is that successfully mounting an insurgency is no easy task. It comes with considerable risk, costs, and coordination challenges.

Another possibility is that the counterfactual is still conflict onset. In poor and fragile nations, income shocks of one type or another are ubiquitous. If a nation is so fragile that a change in prices could lead to war, then other shocks may trigger war even in the absence of a price shock. The same argument has been made in debunking the myth that price shocks led to fiscal collapse and low growth in developing nations in the 1980s.[19]

B. A general problem of publication bias?

More generally, these findings should heighten our concern with publication bias in the conflict literature. Our results run against a number of published results on commodity shocks and conflict, mainly because of select samples, misspecification, and sensitivity to model assumptions, and, most importantly, alternative measures of instability.

Across the social and hard sciences, there is a concern that the majority of published research findings are false (e.g. Gerber et al. 2001). Ioannidis (2005) demonstrates that a published finding is less likely to be true when there is a greater number and lesser pre-selection of tested relationships; there is greater flexibility in designs, definitions, outcomes, and models; and when more teams are involved in the chase of statistical significance. The cross-national study of conflict is an extreme case of all these. Most worryingly, almost no paper looks at alternative dependent variables or publishes systematic robustness checks. Hegre and Sambanis (2006) have shown that the majority of published conflict results are fragile, though they focus on time-invariant regressors and not the time-varying shocks that have grown in popularity.

We are also concerned there is a "file drawer problem" (Rosenthal 1979). Consider this decision rule: scholars that discover robust results that fit a theoretical intuition pursue the results;

[19] A case literature illustrates the damaging impacts of commodity volatility in the 1980s (Bevan et al. 1993; Van de Walle 2001; Bates 2008) but subsequent cross-national analysis finds little to support a causal relationship, as those countries that did not suffer price shocks fell into crisis as well (Deaton and Miller 1995; Deaton 1999).

but if results are not robust the scholar (or referees) worry about problems with the data or empirical strategy, and identify additional work to be done. If further analysis produces a robust result, it is published. If not, back to the file drawer. In the aggregate, the consequences are dire: a lower threshold of evidence for initially significant results than ambiguous ones.[20]

We ourselves are guilty of this offence—the data in this paper were initially collected and a draft produced (but not distributed) in 2003. An absence of robust findings diminished the exigency to publish. Moreover, informal discussions with conflict and commodity/trade scholars suggest we are not the first to look at the effect of prices on political instability and fail to pursue or publish counterintuitive null results. Null results, it was feared, could be attributed to incomplete datasets, the absence of disaggregated shocks data, or unobserved heterogeneity. This paper tackles each and finds only a fragile relation between shocks and intensity.

C. Implications for future research

We see several directions for future research. First, better intensity data is needed at the cross-national level, as are more country case studies like that of Colombia. Second, it is crucial to test competing theories than to simply identify reduced-form effects. Theorists should focus on the predictions that distinguish between competing accounts, rather than confirming evidence alone. Third, researchers should test theories at the appropriate level. The state capacity mechanism has predictions for the effect of economic shocks on state finances, redistribution, and military expenditures. The individual motive to rebel, meanwhile, is best tested at the micro-level.

Yet our aim is broader than challenging the result between price shocks and conflict. Indeed,

[20] Country cases and micro-level data offer other risks. Cases may be unintentionally selected because of their propensity to yield the predicted result (e.g. a conflict with commodity-exploiting rebels). Cases that fail to reject the null may also be subject to publication bias.

we find the existing theories and evidence highly plausible, and would not be surprised to see robust evidence of the opportunity cost or state prize mechanisms emerge. We have misgivings, however, because of the potential extent of research and publication bias, and the fragility of past results. It is probably unrealistic to expect the profession to specify non-experimental tests and empirical models ex-ante, let alone pre-register hypotheses and methods. Nevertheless, aside from the obvious recommendation that null findings be pursued and published, there are a few steps that can be taken that minimize the risk of research and publication bias, such as splitting datasets into training and testing samples. A long literature in comparative politics highlights the danger of selection bias in country cases, and as more are pursued, systematic selection will become more important (Geddes 1990). For existing studies, we offer and suggest a simple and concise approach to systematic robustness checks. Empirical studies of conflict are sufficiently important for academic theory and real-world policy that no less should be expected.

VII. Appendix

A. Price shocks and income growth

A link from price shocks to instability presupposes an impact of shocks on income. Appendix Table 1 illustrates the reduced-form relationship between shocks and GDP growth.

We use current-price GDP data (World Bank 2009). PPP or other constant-price GDP data (such as that from the Penn World Tables) would only give the indirect effect of the price shock on output quantities, whereas we are interested in the effect on actual incomes. If we use constant-price income data then, as one would expect, the effect of price shocks on income runs in the same direction but is smaller and less statistically significant.

We estimate the effect of price shocks with and without country fixed effects (columns 1 and 2). Without fixed effects we see whether countries with consistently more volatile commodities have lower average growth rates. All three lags are positive and statistically significant. The sum of these shocks is 36% of the mean growth rate across all countries, implying a one standard deviation rise in export commodity prices increases GDP by more than a third. With fixed effects we see whether deviations in growth from the country-specific average growth rate are explained by deviations in price shock from the average shock. Only the lagged price shock is significant, and the overall effect on growth halves. Are these impacts large? in columns 3 and 4 we benchmark our price shocks to a shock with proven relevance—rainfall in Africa (Miguel and Satyanath forthcoming). Income appears to be more sensitive to a standard deviation change in rainfall than commodity prices (at least in Africa), but the impact of both shock types is large.

B. Robustness analysis: An example

Figures 1 and 4 in the paper illustrate the results of nine different models for each of the 6 main conflict measures. For illustrative purposes, Appendix Table 2 displays the underlying robustness checks for one of the most robust dependent variables, UCDP/PRIO cumulatively high intensity warfare, using the (in this case) higher-powered logit model. The magnitudes of the sums of shocks are consistently large, but robustness is highly sensitive to simple model changes.

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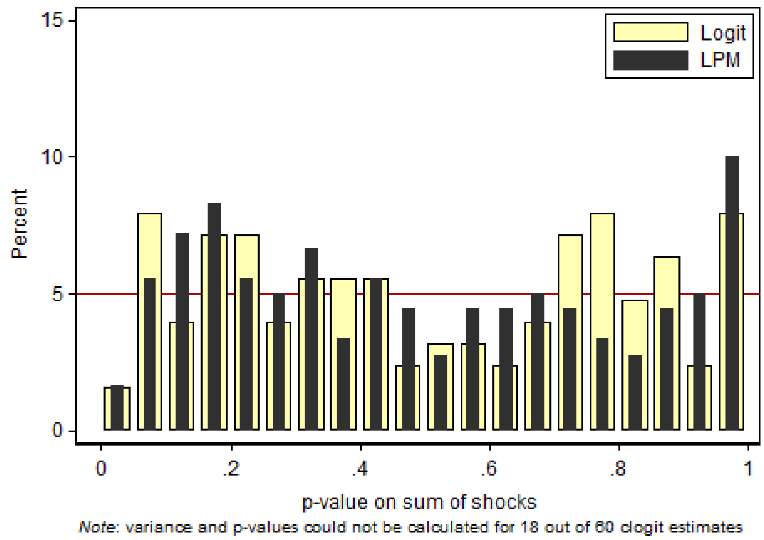
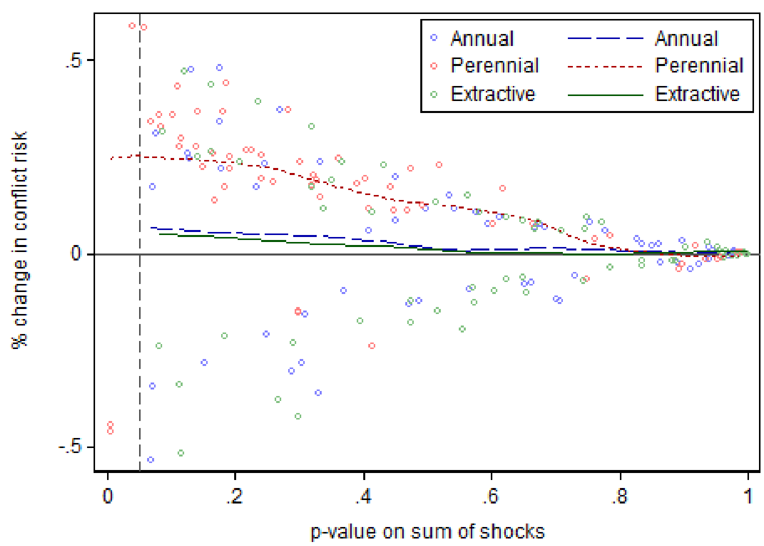
Figure 1: Robustness analysis for disaggregated price shocks and conflict onset

i. Distribution of p-values from 6 dependent variables and 20 alternative models



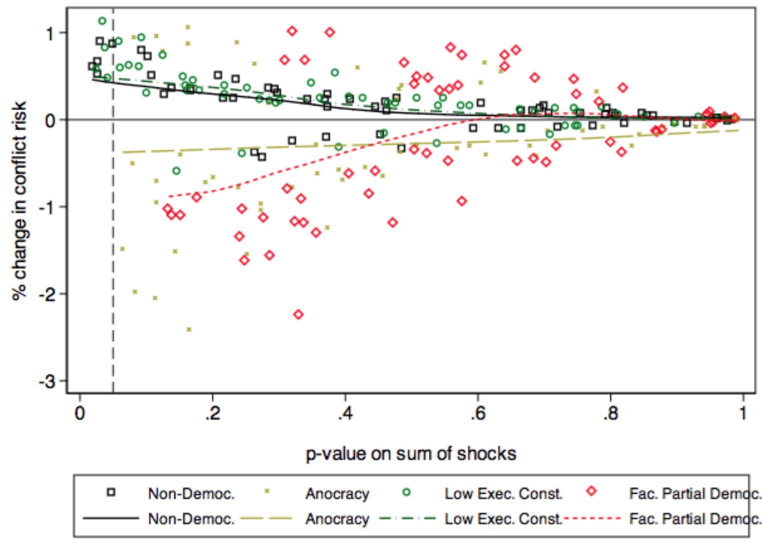
ii. Change in conflict risk plotted against p-values (with locally-fitted line)



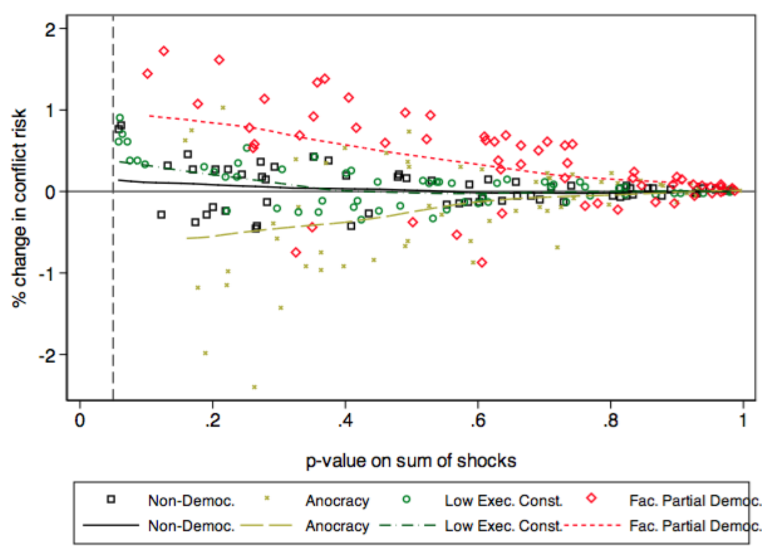
Notes: We estimate p-values and changes in conflict risk (based on the sum of current and lagged price shocks) using 10 models, each by linear and logit regression, for six measures of conflict, with shocks disaggregarted by commodity. The frequency diagram (Panel i) has bins of width 0.05 and a horizontal line represents the uniform distribution. The scatter plot (Panel ii) displays results of the linear model alone, with a vertical line at p=0.05. Fitted lines come from a kernel regression by commodity class. Consistent, robust results should be uniform in direction and below or close to p=0.05.

Figure 2: Disaggregated price shocks and conflict onset – Regime subsamples

i. "Annual agricultural" price shocks and conflict onset



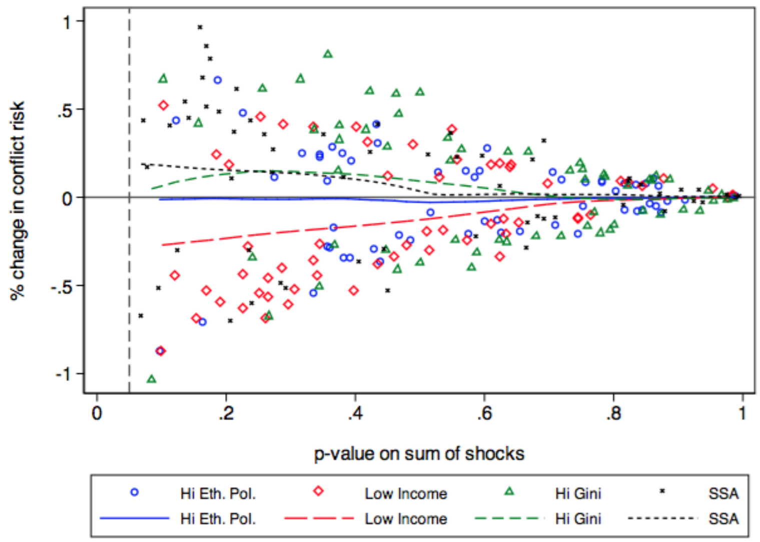
ii. "Extractive" price shocks and conflict onset



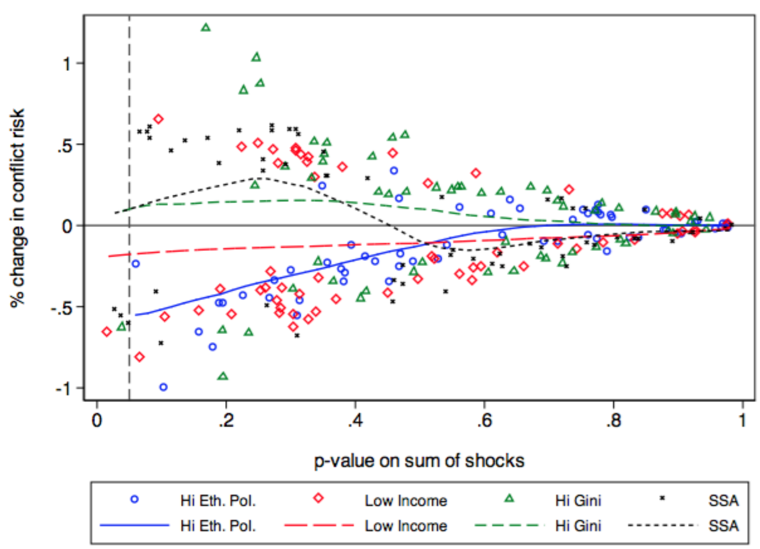
Notes: We estimate p-values and changes in conflict risk (based on the sum of current and lagged price shocks) using 10 models, each by linear and logit regression, for six measures of conflict, by regime type. Panel (i) examines annual crop prices and Panel (ii) prices of extractive (mineral and fuel) commodities. In each panel a vertical line at p=0.05. Fitted lines come from a kernel regression by regime type. Consistent, robust results should be uniform in direction and below or close to p=0.05.

Figure 3: Disaggregated price shocks and conflict onset – "High risk" subsamples

i. "Annual agricultural" price shocks and conflict onset



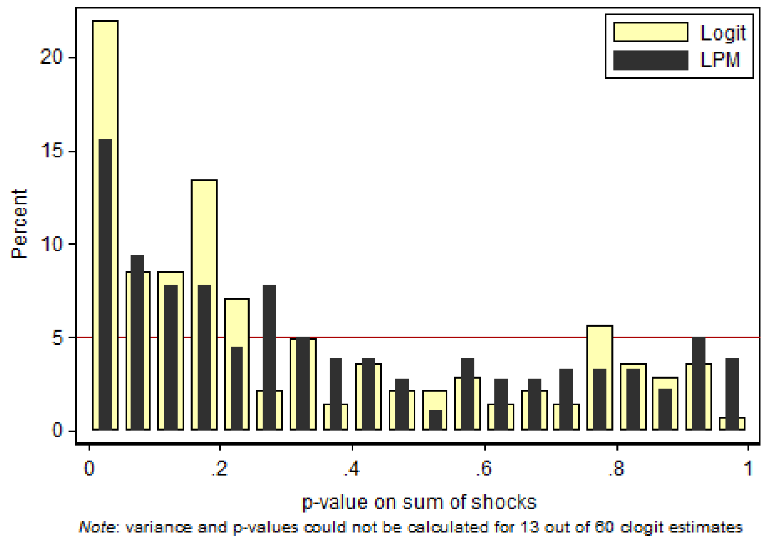
ii. "Extractive" price shocks and conflict onset



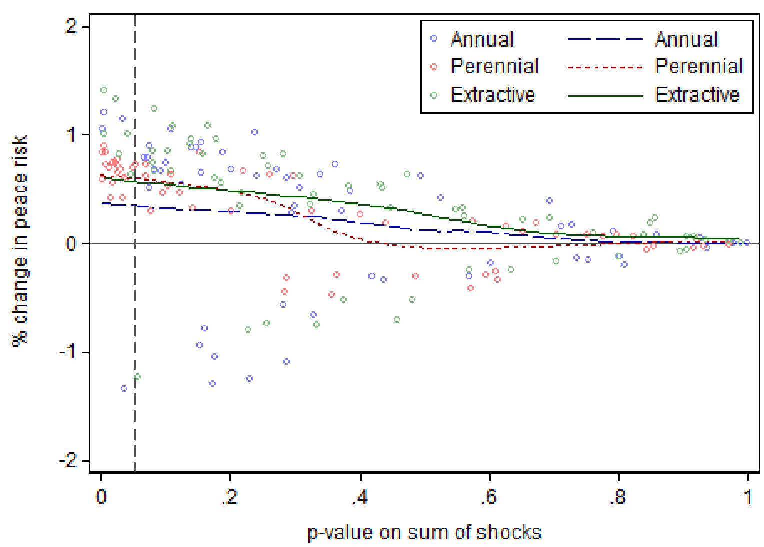
Notes: We estimate p-values and changes in conflict risk (based on the sum of current and lagged price shocks) using 10 models, each by linear and logit regression, for six measures of conflict, by conflict risk factor. Panel (i) examines annual crop prices and Panel (ii) prices of extractive (mineral and fuel) commodities. In each panel a vertical line at p=0.05. Fitted lines come from a kernel regression by conflict risk factor. Consistent, robust results should be uniform in direction and below or close to p=0.05.

Figure 4: Robustness analysis for disaggregated price shocks and conflict ending

i. Distribution of p-values from 6 dependent variables and 20 alternative models



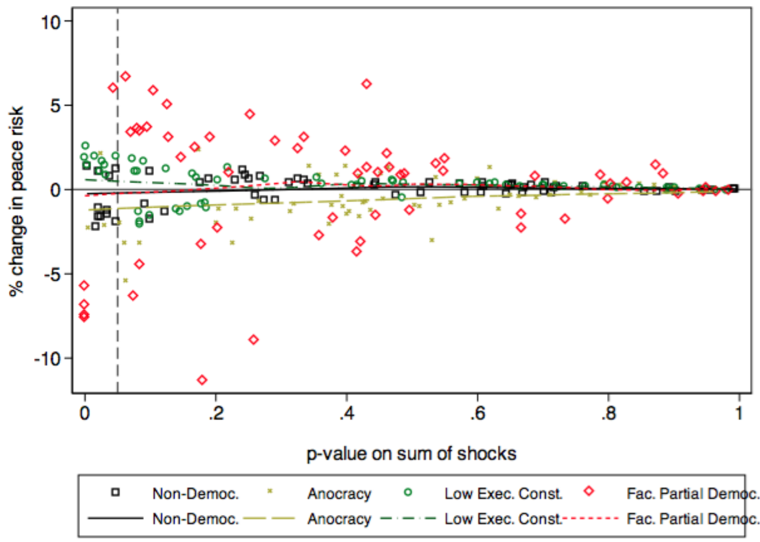
ii. Change in conflict risk plotted against p-values (with locally-fitted line)



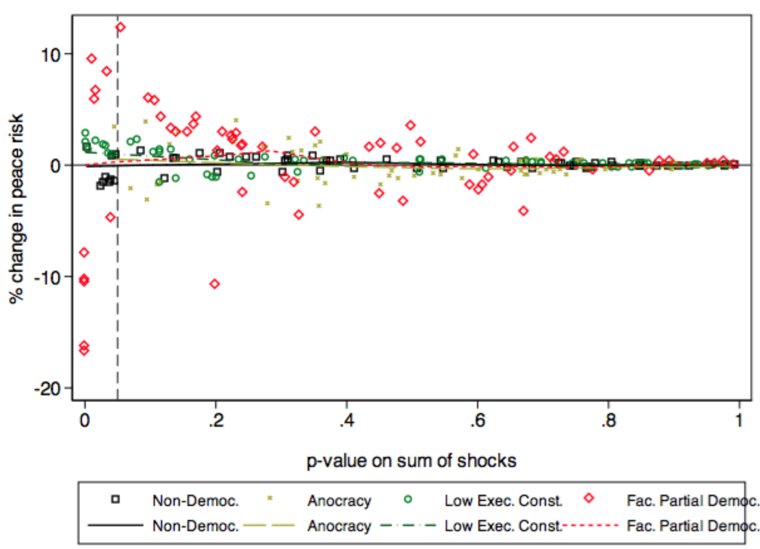
Notes: We estimate p-values and changes in conflict risk (based on the sum of current and lagged price shocks) using 10 models, each by linear and logit regression, for six measures of conflict, with shocks disaggregarted by commodity. The frequency diagram (Panel i) has bins of width 0.05 and a horizontal line represents the uniform distribution. The scatter plot (Panel ii) displays results of the linear model alone, with a vertical line at p=0.05. Fitted lines come from a kernel regression by commodity class. Consistent, robust results should be uniform in direction and below or close to p=0.05.

Figure 5: Disaggregated price shocks and conflict ending – Regime subsamples

i. "Annual agricultural" price shocks and conflict ending



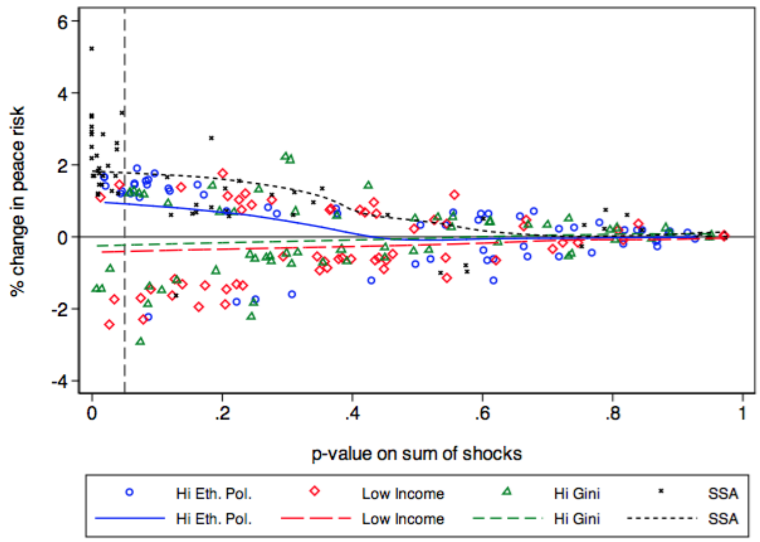
ii. "Extractive" price shocks and conflict ending



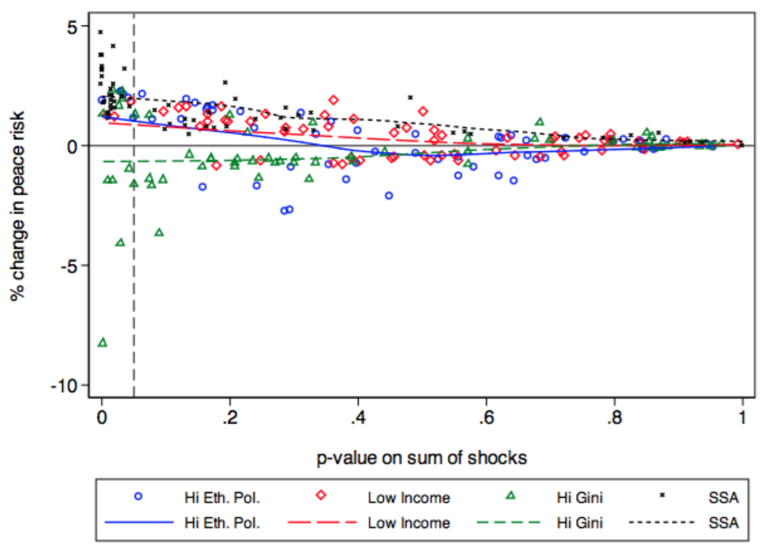
Notes: We estimate p-values and changes in conflict risk (based on the sum of current and lagged price shocks) using 10 models, each by linear and logit regression, for six measures of conflict, by regime type. Panel (i) examines annual crop prices and Panel (ii) prices of extractive (mineral and fuel) commodities. In each panel a vertical line at p=0.05. Fitted lines come from a kernel regression by regime type. Consistent, robust results should be uniform in direction and below or close to p=0.05.

Figure 6: Disaggregated price shocks and conflict ending–"High risk" subsamples

i. "Annual agricultural" price shocks and conflict ending



ii. "Extractive" price shocks and conflict ending



Notes: We estimate p-values and changes in conflict risk (based on the sum of current and lagged price shocks) using 10 models, each by linear and logit regression, for six measures of conflict, by conflict risk factor. Panel (i) examines annual crop prices and Panel (ii) prices of extractive (mineral and fuel) commodities. In each panel a vertical line at p=0.05. Fitted lines come from a kernel regression by conflict risk factor. Consistent, robust results should be uniform in direction and below or close to p=0.05.

Table 1: Summary statistics

			(1)	(2)	(3)	(4)	(5)	(6)
			Incidence		**Onset**		**Ongoing**	
Variable	**Dataset**	**Definition**	**N**	**Mean**	**N**	**Mean**	**N**	**Mean**
Civil War	UCDP/PRIO (Any)	Low and high intensity battle deaths	5,101	0.20	4,048	0.04	995	0.16
	UCDP/PRIO (High CML)	High intensity battle deaths - *cumulative*	5,101	0.15	4,294	0.02	749	0.11
	UCDP/PRIO (High)	High intensity battle deaths	5,101	0.07	4,690	0.02	353	0.25
	Fearon & Laitin (FL)	High intensity war	5,101	0.20	4,032	0.02	1,013	0.06
	Sambanis (S)	High intensity war	4,984	0.18	4,037	0.02	906	0.09
	Correlates of War (COW)	High intensity war	5,101	0.13	4,379	0.03	665	0.19
Battle Deaths	PRIO Battle Deaths (High)	High estimate for annual battle fatalities	1,030	7,319				
	PRIO Battle Deaths (Low)	Low estimate for annual battle fatalities	1,030	1,478				
	PRIO Battle Deaths (Best)	Best estimate for annual battle fatalities	701	4,030				
Coups	Archigos	Irregular exit of leader			4,647	0.05		
	Powell & Thyne (PT)	Actual or attempted coups			5,079	0.06		

Notes: *UCDP/PRIO (Any)* includes both minor armed conflicts (resulting in between 25 and 999 battle deaths per year) and wars (resulting in 1,000 or more battle deaths per year). *UCDP/PRIO (High CML)* includes only wars but also takes into account the temporal dimension of the conflict; specifically, it takes a value of one in any given year if there were either 1,000 or more battle deaths in that year or if the conflict since its onset had exceeded the threshold of 1,000 battle-related deaths. *UCDP/PRIO (High)* includes only wars and does not take into account the temporal dimension of the conflict.

Table 2: Impacts of aggregate price shocks on conflict and coup onset

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Indicator for onset							
	UCDP/PRIO Civil War			**Other Civil War**			**Coups**	
	Low	**High Cum.**	**High**	**FL**	**S**	**COW**	**Archigos**	**PT**
Price Shock, t	-0.0006	0.0018	0.0009	0.0010	-0.0004	0.0016	0.0007	0.0004
	(0.0023)	(0.0017)	(0.0016)	(0.0011)	(0.0013)	(0.0019)	(0.0023)	(0.0026)
Price Shock, t-1	0.0045	0.0008	0.0001	-0.0008	-0.0011	0.0023	-0.0026	-0.0011
	(0.0031)	(0.0018)	(0.0014)	(0.0015)	(0.0017)	(0.0018)	(0.0031)	(0.0032)
Price Shock, t-2	-0.0015	-0.0008	-0.0000	0.0014	-0.0018	0.0013	-0.0045	-0.0058
	(0.0025)	(0.0015)	(0.0013)	(0.0013)	(0.0015)	(0.0019)	(0.0022)**	(0.0033)*
All Shocks (sum)	0.00234	0.00179	0.000964	0.00161	-0.00321	0.00515	-0.00638	-0.00649
p-value of sum	0.620	0.531	0.747	0.492	0.307	0.173	0.194	0.198
Impact of all shocks on risk (%Δ)	5.47%	8.16%	5.03%	8.91%	-14.90%	17.80%	-13.30%	-10.80%
Observations	4,048	4,294	4,690	4,032	4,037	4,379	4,647	5,079
R-squared	0.017	0.012	0.012	0.014	0.015	0.021	0.016	0.021
Number of Countries (clusters)	117	117	117	114	117	116	114	117
Mean of Dependent Variable	0.0427	0.0219	0.0192	0.0181	0.0216	0.0290	0.0480	0.0601

All regressions include year and country fixed effects (coefficients not displayed)
Robust standard errors in parentheses, clustered by country
**** p<0.01, ** p<0.05, * p<0.1*

Table 3: The impact of disaggregated commodity price shocks on conflict and coup onset

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Indicator for Onset							
	UCDP/PRIO Civil War			**Other Civil War**			**Coups**	
	Low	**High Cum.**	**High**	**FL**	**S**	**COW**	**Archigos**	**PT**
Annual Crop Price Shock, t	-0.0004	0.0014	0.0020	0.0016	-0.0001	0.0029	0.0017	0.0013
	(0.0032)	(0.0026)	(0.0020)	(0.0014)	(0.0017)	(0.0023)	(0.0035)	(0.0034)
Annual Crop Price Shock, t-1	0.0042	0.0019	0.0003	-0.0016	-0.0025	0.0015	0.0000	0.0026
	(0.0058)	(0.0028)	(0.0016)	(0.0019)	(0.0023)	(0.0026)	(0.0053)	(0.0050)
Annual Crop Price Shock, t-2	-0.0008	-0.0016	-0.0011	0.0014	-0.0018	0.0032	-0.0048	-0.0111
	(0.0033)	(0.0020)	(0.0020)	(0.0016)	(0.0018)	(0.0024)	(0.0029)*	(0.0045)**
Perennial Crop Price Shock, t	-0.0030	0.0025	0.0029	0.0004	-0.0013	0.0010	0.0019	0.0004
	(0.0036)	(0.0021)	(0.0020)	(0.0032)	(0.0036)	(0.0037)	(0.0028)	(0.0031)
Perennial Crop Price Shock, t-1	0.0054	0.0003	0.0003	0.0021	0.0020	0.0035	-0.0056	-0.0005
	(0.0057)	(0.0020)	(0.0020)	(0.0030)	(0.0034)	(0.0029)	(0.0038)	(0.0043)
Perennial Crop Price Shock, t-2	0.0026	0.0033	0.0036	0.0024	-0.0010	-0.0009	-0.0028	0.0031
	(0.0035)	(0.0024)	(0.0023)	(0.0024)	(0.0022)	(0.0027)	(0.0041)	(0.0052)
Mineral, Oil & Gas Price Shock, t	-0.0001	0.0029	0.0003	0.0015	-0.0003	0.0018	0.0001	0.0003
	(0.0032)	(0.0024)	(0.0024)	(0.0016)	(0.0019)	(0.0029)	(0.0035)	(0.0040)
Mineral, Oil & Gas Price Shock, t-1	0.0068	0.0006	0.0001	-0.0015	-0.0018	0.0036	-0.0045	-0.0038
	(0.0044)	(0.0023)	(0.0020)	(0.0022)	(0.0024)	(0.0027)	(0.0041)	(0.0045)
Mineral, Oil & Gas Price Shock, t-2	-0.0041	-0.0019	-0.0004	0.0018	-0.0029	0.0016	-0.0070	-0.0085
	(0.0039)	(0.0022)	(0.0018)	(0.0019)	(0.0023)	(0.0027)	(0.0031)**	(0.0046)*
Sum of all annual crop shocks	0.00298	0.00168	0.00118	0.00142	-0.00448	0.00756	-0.00312	-0.00722
p-value of sum	0.667	0.675	0.777	0.593	0.248	0.125	0.688	0.249
Impact of annual shocks on risk (%Δ)	6.98%	7.67%	6.15%	7.85%	-22.40%	26.07%	-7.80%	-12.03%
Sum of all perennial crop shocks	0.00504	0.00608	0.00685	0.00489	-0.000276	0.00356	-0.00656	0.00303
p-value of sum	0.408	0.112	0.102	0.218	0.952	0.493	0.274	0.727
Impact of perennial shocks on risk (%Δ)	11.80%	27.76%	35.68%	27.02%	-1.38%	12.28%	-16.40%	5.05%
Sum of all oil and gas shocks	0.00256	0.00174	-2.82e-05	0.00172	-0.00497	0.00695	-0.0114	-0.0120
p-value of sum	0.709	0.673	0.995	0.623	0.292	0.207	**0.096***	**0.096***
Impact of oil and gas shocks on risk (%Δ	6.00%	7.95%	-0.15%	9.50%	-24.85%	23.97%	-28.50%	-20.00%
Observations	4,048	4,294	4,690	4,032	4,037	4,379	4,647	5,079
R-squared	0.018	0.013	0.013	0.015	0.016	0.022	0.017	0.023
Number of Countries (clusters)	117	117	117	114	117	116	114	117
Mean of Dependent Variable	0.0427	0.0219	0.0192	0.0181	0.0200	0.0290	0.0400	0.0600

All regressions include year and country fixed effects (coefficients not displayed)
Robust standard errors in parentheses, clustered by country
**** p<0.01, ** p<0.05, * p<0.1*

Table 4: Example of sensitivity to specification - Reconciliation with Bruckner-Ciccone (BC) Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	Indicator for Civil War Onset (UCDP/PRIO High measure)								**Alternate Dep. Var. (using restricted sample and BC estimator)**					
	Base result from Table 2	**Use 3% Price-Maker Cutoff and drop X/GDP Adjustment**	**Sub-Saharan Africa (SSA) Only**	**Sub-Saharan Africa (SSA) Only and Year > 1983**	**Restrict years to BC sample**	**Use more lmited index of commodities**	**Use older coding of dependent variable**	**Use BC estimator (BC 2010 result)**	**UCDP/ PRIO High**	**UCDP/ PRIO High CML**	**UCDP/ PRIO Any**	**FL**	**S**	**COW**
Price Shock, t	0.0005	-0.0024	-0.0043	-0.0124	-0.0091	-0.0085	-0.0108	-0.0108	-0.0085	0.0108	0.0163	-0.0059	-0.0104	0.0185
	(0.0015)	(0.0085)	(0.0035)	(0.0072)*	(0.0067)	(0.0078)	(0.0060)*	(0.0056)*	(0.0074)	(0.0063)*	(0.0093)*	(0.0086)	(0.0085)	(0.0127)
Price Shock, t-1	0.0005	0.0100	0.0001	-0.0031	0.0017	-0.0025	-0.0055	-0.0055	-0.0025	-0.0115	-0.0149	-0.0172	-0.0092	-0.0054
	(0.0011)	(0.0110)	(0.0032)	(0.0055)	(0.0049)	(0.0059)	(0.0073)	(0.0069)	(0.0056)	(0.0067)*	(0.0113)	(0.0084)**	(0.0067)	(0.0065)
Price Shock, t-2	-0.0003	0.0084	-0.0066	-0.0082	-0.0062	-0.0078	-0.0171	-0.0171	-0.0078	0.0039	-0.0094	0.0021	0.0000	0.0026
	(0.0011)	(0.0163)	(0.0045)	(0.0067)	(0.0073)	(0.0098)	(0.0081)**	(0.0077)**	(0.0093)	(0.0069)	(0.0077)	(0.0095)	(0.0064)	(0.0103)
Sum of all shocks	0.000658	0.0161	-0.0109	-0.0237	-0.0136	-0.0188	-0.0333	-0.0333	-0.0188	0.00322	-0.00809	-0.0211	-0.0195	0.0158
p-value of sum	0.801	0.542	0.184	0.110	0.366	0.211	**0.0654***	**0.0524***	0.187	0.743	0.608	0.249	**0.0696***	0.374
Impact of all shocks	3.43%	84.40%	-50.50%	-91.60%	-49.90%	-61.60%	-117.90%	-117.90%	-61.60%	9.43%	-12.30%	-93.10%	-64.00%	37.30%
Observations	4,690	4,722	1,768	1,005	809	820	814	814	820	732	686	662	689	731
R-squared	0.089	0.089	0.104	0.080	0.099	0.118	0.098	0.012	0.004	0.009	0.009	0.012	0.005	0.008
Number of Countries (cluster	117	118	45	45	39	39	39							
Mean of Dependent Variable	0.0192	0.0191	0.0215	0.0259	0.0272	0.0305	0.0283	0.0283	0.0305	0.0342	0.0656	0.0227	0.0305	0.0424

All regressions include year and country fixed effects (coefficients not displayed)
Robust standard errors in parentheses, clustered by country
**** p<0.01, ** p<0.05, * p<0.1*

Table 5: Impacts of aggregate price shocks on conflict ending

	(1)	(2)	(3)	(4)	(5)	(6)
	Indicator for ending					
	UCDP/PRIO Civil War			**Other Civil War**		
	Low	**High Cum.**	**High**	**FL**	**S**	**COW**
Price Shock, t	0.0100	0.0429	0.0892	-0.0026	-0.0061	0.0410
	(0.0199)	(0.0203)**	(0.0432)**	(0.0174)	(0.0153)	(0.0262)
Price Shock, t-1	0.0123	0.0347	-0.0337	-0.0021	0.0255	0.0564
	(0.0270)	(0.0284)	(0.0525)	(0.0216)	(0.0181)	(0.0286)*
Price Shock, t-2	0.0010	0.0012	0.1091	-0.0218	-0.0022	0.0152
	(0.0299)	(0.0242)	(0.0423)**	(0.0154)	(0.0232)	(0.0383)
Sum of all shocks	0.0233	0.0787	0.165	-0.0265	0.0172	0.113
p-value of sum	0.635	**0.091***	0.154	0.367	0.609	**0.0999***
Impact of all shocks on risk (%Δ)	14.50%	71.90%	64.50%	-44.70%	19.70%	59.00%
Observations	995	749	353	1,013	906	665
R-squared	0.060	0.097	0.154	0.048	0.060	0.155
Number of Countries (clusters)	83	52	42	56	60	59
Mean of Dependent Variable	0.161	0.109	0.255	0.0592	0.0872	0.191

All regressions include year and country fixed effects (coefficients not displayed)
Robust standard errors in parentheses, clustered by country
**** p<0.01, ** p<0.05, * p<0.1*

Table 6: The impact of disaggregated commodity price shocks on conflict ending

	(1)	(2)	(3)	(4)	(5)	(6)
	Indicator for ending					
	UCDP/PRIO Civil War			**Other Civil War**		
	Low	**High Cum.**	**High**	**FL**	**S**	**COW**
Annual Crop Price Shock, t	-0.0007	0.0615	0.1538	-0.0107	-0.0182	0.0556
	(0.0272)	(0.0328)*	(0.0702)**	(0.0234)	(0.0229)	(0.0421)
Annual Crop Price Shock, t-1	0.0242	0.0391	-0.0530	-0.0106	0.0299	0.0759
	(0.0407)	(0.0435)	(0.0840)	(0.0311)	(0.0281)	(0.0422)*
Annual Crop Price Shock, t-2	-0.0062	0.0007	0.1670	-0.0180	0.0038	0.0111
	(0.0380)	(0.0342)	(0.0599)***	(0.0201)	(0.0319)	(0.0532)
Perennial Crop Price Shock, t	0.0140	0.0483	0.0944	0.0055	0.0072	0.0781
	(0.0174)	(0.0207)**	(0.0391)**	(0.0168)	(0.0166)	(0.0272)***
Perennial Crop Price Shock, t-1	0.0426	0.0318	0.0093	-0.0029	0.0167	0.0433
	(0.0189)**	(0.0189)*	(0.0467)	(0.0148)	(0.0128)	(0.0280)
Perennial Crop Price Shock, t-2	-0.0094	-0.0011	0.0731	-0.0180	-0.0056	0.0111
	(0.0223)	(0.0207)	(0.0307)**	(0.0129)	(0.0165)	(0.0272)
Mineral, Oil & Gas Price Shock, t	0.0074	0.0575	0.1326	-0.0068	-0.0150	0.0392
	(0.0296)	(0.0310)*	(0.0711)*	(0.0247)	(0.0228)	(0.0399)
Mineral, Oil & Gas Price Shock, t-1	0.0088	0.0506	-0.0668	0.0003	0.0403	0.0935
	(0.0467)	(0.0482)	(0.0980)	(0.0358)	(0.0300)	(0.0427)**
Mineral, Oil & Gas Price Shock, t-2	0.0186	0.0110	0.2093	-0.0243	0.0037	0.0307
	(0.0449)	(0.0371)	(0.0736)***	(0.0232)	(0.0367)	(0.0563)
Sum of all annual crop shocks	0.0173	0.101	0.268	-0.0393	0.0154	0.143
p-value of sum	0.792	0.154	0.108	0.329	0.727	0.101
Impact of annual shocks on risk (%Δ)	10.70%	92.50%	105.00%	-66.30%	17.70%	74.60%
Sum of all perennial crop shocks	0.0471	0.0791	0.177	-0.0153	0.0182	0.133
p-value of sum	0.200	**0.053***	**0.048****	0.611	0.573	**0.011****
Impact of annual shocks on risk (%Δ)	29.30%	72.30%	69.30%	-25.90%	20.90%	69.40%
Sum of all oil and gas shocks	0.0347	0.119	0.275	-0.0308	0.0289	0.163
p-value of sum	0.652	0.110	0.165	0.481	0.558	0.102
Impact of annual shocks on risk (%Δ)	21.60%	108.80%	107.90%	-52.10%	33.20%	85.50%
Observations	995	749	353	1,013	906	665
R-squared	0.073	0.101	0.173	0.053	0.064	0.165
Number of Countries (clusters)	83	52	42	56	60	59
Mean of Dependent Variable	0.161	0.109	0.255	0	0.0872	0.191

All regressions include year and country fixed effects (coefficients not displayed)
Robust standard errors in parentheses, clustered by country
**** p<0.01, ** p<0.05, * p<0.1*

Table 7: Impact of model assumptions on p-values in robustness analysis

	P-value on sum of price shocks †	
	War Onset	War Ending
Logit model	0.041	-0.072
	(0.033)	(0.027)***
Annual crop price shock	0.028	-0.030
	(0.039)	(0.034)
Perennial crop price shock	-0.143	-0.098
	(0.040)***	(0.033)***
Not including country fixed effects	-0.015	0.040
	(0.067)	(0.048)
Not including year fixed effects	0.043	-0.030
	(0.067)	(0.046)
Including Fixed weights	0.059	0.250
	(0.076)	(0.065)***
Not including exports/GDP adjustment	0.018	-0.037
	(0.063)	(0.047)
Censoring price outliers	-0.020	0.082
	(0.062)	(0.045)*
Including all price-makers	-0.047	0.038
	(0.066)	(0.041)
Using 3% price-maker cutoff	-0.065	0.051
	(0.065)	(0.050)
Using 20% price-maker cutoff	-0.013	0.088
	(0.067)	(0.045)*
COW Dependent Variable (DV)	-0.313	-0.346
	(0.043)***	(0.044)***
Fearon & Laitin DV	-0.056	-0.056
	(0.049)	(0.058)
UCDP/PRIO High Cum. DV	-0.079	-0.314
	(0.049)	(0.044)***
UCDP/PRIO High DV	-0.180	-0.259
	(0.050)***	(0.047)***
Sambanis DV	-0.166	0.149
	(0.076)**	(0.064)**
Observations ‡	288	303
R-squared	0.203	0.428

Robust standard errors in parentheses, no clustering
*** p<0.01, ** p<0.05, * p<0.1

† The dependent variable is the p-value on the sum of all three price shocks (disaggregated by commodity type) for 3 commodity types (annual, perennial and extractive) in 108 alternative regressions based on 6 alternative dependent variables, two estimators (linear and logit) and 9 alternative models. Each indepedent variable is an indicator for the estimator, model assumption, or dependent variable.
‡ The logit estimator does not converge in all 324 cases

Table 8: The impact of aggregated and disaggregated commodity price shocks on battle deaths

	(1)	(2)	(3)	(4)	(5)	(6)
	Linear Battle Deaths			**Natural Log of Battle Deaths**		
	Static	**Dynamic**	**Omitting non-Annual Deaths Data**	**Static**	**Dynamic**	**Omitting non-Annual Deaths Data**
Price Shock, t	-475.4	-803.5	-430.3	-0.187	-0.225	-0.181
	(500.2)	(273.9)***	(304.9)	(0.108)*	(0.071)***	(0.103)*
Price Shock, t-1	-86.8	416.7	218.5	-0.103	-0.040	-0.128
	(455.0)	(406.9)	(303.0)	(0.130)	(0.125)	(0.108)
Price Shock, t-2	111.7	29.0	234.7	-0.155	-0.158	-0.137
	(607.1)	(358.8)	(552.7)	(0.152)	(0.120)	(0.138)
Duration	-69.4	-38.6	-27.9	0.002	0.006	0.007
	(37.7)*	(20.9)*	(14.4)*	(0.011)	(0.010)	(0.012)
Indicator for first year of conflict	-2,926.4	329.9	420.9	-1.333	-0.934	-0.871
	(856.0)***	(625.3)	(476.6)	(0.192)***	(0.198)***	(0.251)***
Lagged Battle Deaths		0.743	0.916		0.0001	0.0001
		(0.140)***	(0.025)***		(0.0000)***	(0.0000)***
Country fixed effects	Yes	No	No	Yes	No	No
Sum of all shocks	-450.4	-357.8	22.93	-0.059	-0.424	-0.446
p-value of sum	0.756	0.623	0.981	0.850	0.121	0.126
Impact of all shocks on risk (%Δ)	-0.087	-0.0694	0.0057	-0.009	-0.060	-0.067
Observations	1,009	1,009	690	1,009	1,009	690
Mean of Dependent Variable	5159	5159	4016	6.897	7.065	6.706
Number of Countries (clusters)	82	82	74	82	82	74

All regressions include year fixed effects (coefficients not displayed).
Robust standard errors in parentheses, clustered by country
**** p<0.01, ** p<0.05, * p<0.1*

Table 9: The impact of aggregated and disaggregated commodity price shocks on battle deaths

	(1)	(2)	(3)	(4)	(5)	(6)
	Linear Battle Deaths			**Natural Log of Battle Deaths**		
	Static	**Dynamic**	**Omitting non-Annual Deaths Data**	**Static**	**Dynamic**	**Omitting non-Annual Deaths Data**
Annual Crop Price Shock, t	-796.2	-1,152.1	-809.5	-0.209	-0.252	-0.156
	(837.1)	(461.6)**	(555.3)	(0.189)	(0.144)*	(0.175)
Annual Crop Price Shock, t-1	-156.8	369.6	100.9	-0.130	-0.063	-0.165
	(554.6)	(502.0)	(417.5)	(0.156)	(0.150)	(0.138)
Annual Crop Price Shock, t-2	-570.1	-250.1	-167.3	-0.280	-0.233	-0.217
	(778.1)	(431.7)	(671.6)	(0.200)	(0.154)	(0.191)
Perennial Crop Price Shock, t	-279.7	-504.1	-131.2	-0.169	-0.197	-0.157
	(464.2)	(275.9)*	(272.6)	(0.109)	(0.089)**	(0.099)
Perennial Crop Price Shock, t-1	73.8	449.8	351.3	-0.084	-0.035	-0.093
	(410.7)	(330.8)	(261.0)	(0.114)	(0.107)	(0.098)
Perennial Crop Price Shock, t-2	583.6	417.6	565.5	-0.041	-0.051	-0.051
	(575.1)	(404.4)	(504.8)	(0.135)	(0.115)	(0.123)
Mineral, Oil & Gas Price Shock, t	-693.8	-1,206.1	-756.7	-0.250	-0.311	-0.239
	(789.6)	(427.0)***	(528.9)	(0.159)	(0.100)***	(0.153)
Mineral, Oil & Gas Price Shock, t-1	-140.3	602.4	152.1	-0.139	-0.048	-0.177
	(733.5)	(655.1)	(476.6)	(0.193)	(0.188)	(0.157)
Mineral, Oil & Gas Price Shock, t-2	-160.6	-261.0	-8.6	-0.296	-0.301	-0.252
	(1,032.6)	(531.8)	(823.9)	(0.244)	(0.185)	(0.221)
Duration	-69.4	-39.1	-29.9	0.002	0.006	0.006
	(37.9)*	(21.3)*	(14.1)**	(0.011)	(0.010)	(0.012)
Indicator for first year of conflict	-3,017.3			-1.352		
	(877.5)***			(0.192)***		
Lagged Battle Deaths		0.740	0.912		0.0001	0.0001
		(0.139)***	(0.024)***		(0.0000)***	(0.0000)***
Country fixed effects	Yes	No	No	Yes	No	No
Sum of all annual crop shocks	-1523	-1033	-875.9	-0.619	-0.549	-0.538
p-value of sum	0.459	0.626	0.512	0.372	0.157	0.189
Impact of annual shocks on risk (%Δ)	-0.295	-0.200	-0.218	-0.088	-0.078	-0.080
Sum of all perennial crop shocks	377.8	363.3	785.6	-0.294	-0.284	-0.303
p-value of sum	0.766	0.335	0.350	0.218	0.103	0.121
Impact of annual shocks on risk (%Δ)	0.0732	0.0704	0.196	-0.042	-0.040	-0.045
Sum of all oil and gas shocks	-994.8	-864.7	-613.2	-0.684	-0.660	-0.668
p-value of sum	0.685	0.498	0.701	0.211	0.301	0.278
Impact of annual shocks on risk (%Δ)	-0.193	-0.168	-0.153	-0.097	-0.093	-0.100
Observations	1,009	1,009	690	1,009	1,009	690
Mean of Dependent Variable	515	5159	4016	7	7	6
Number of countries (clusters)	82	82	74	82	82	74

All regressions include year fixed effects (coefficients not displayed).
Robust standard errors in parentheses, clustered by country
**** p<0.01, ** p<0.05, * p<0.1*

Appendix Table 1: Commodity price shocks and GDP per capita growth

	(1)	(2)	(3)	(4)
	Income per capita growth rate †			
	All non-OECD countries		**Sub-Saharan Africa**	
Price Shock, t	0.0036	0.0011	0.0021	0.0006
	(0.0011)***	(0.0014)	(0.0029)	(0.0035)
Price Shock, t-1	0.0066	0.0035	0.0104	0.0047
	(0.0014)***	(0.0016)**	(0.0018)***	(0.0018)**
Price Shock, t-2	0.0038	0.0009	0.0077	0.0026
	(0.0013)***	(0.0013)	(0.0026)***	(0.0026)
Rainfall shock, t			0.0091	0.0062
			(0.0025)***	(0.0027)**
Rainfall shock, t-1			0.0082	0.0050
			(0.0024)***	(0.0029)*
Rainfall shock, t-1			0.0048	0.0015
			(0.0023)**	(0.0022)
Fixed effects	No	Yes	No	Yes
Sum of price shocks	0.0140	0.00546	0.0203	0.00788
p-value of sum	**0.000*** **	0.119	**0.000*** **	0.129
Impact of price shocks (%Δ)	35.9%	14.0%	72.9%	28.3%
Sum of rainfall shocks			0.0221	0.0127
p-value of sum			**0.001*** **	**0.055***
Impact of rainfall shocks (%Δ)			79.3%	45.6%
Observations	4,254	4,254	983	983
Number of countries (clusters)	113	113	40	40
R-squared	0.016	0.059	0.043	0.091
Mean annual growth rate	0.0391	0.0391	0.0278	0.0278

Robust standard errors in parentheses, clustered by country

† *The dependent variable is the log change in current price GDP per capita data from the WDI (World Bank 2011)*

**** p<0.01, ** p<0.05, * p<0.1*

Appendix Table 2: Robustness analysis on ending using UCDP/PRIO (Cumulative High) dependent variable, logit estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Ending Using UCDP/PRIO (High CML)									
	Baseline	**Not Including Exports/GDP Scale Adjustment**	**Including All Price-Makers**	**Using 3% Price-Maker Cutoff**	**Using 20% Price-Maker Cutoff**	**With Fixed 1980 Weights**	**Censoring Price Outliers**	**Dropping Time Fixed Effects**	**Dropping Country Fixed Effects**	**Absolute Value of Shocks**
Sum Annual Crop Price Shock, t	0.7872	0.7382	0.4592	0.8632	0.5629	0.6934	0.7872	0.8297	0.5583	1.3177
	(0.3682)**	(0.3194)**	(0.2876)	(0.5220)*	(0.3196)*	(0.7341)	(0.3682)**	(0.2486)***	(0.2981)*	(0.4672)***
Sum Annual Crop Price Shock, t-1	0.3624	0.5181	0.2170	0.3343	0.4581	0.2180	0.3624	0.5559	0.3136	0.0643
	(0.3065)	(0.2824)*	(0.2904)	(0.6060)	(0.2867)	(0.4281)	(0.3065)	(0.2951)*	(0.3628)	(0.5273)
Sum Annual Crop Price Shock, t-2	0.0234	0.1448	0.0691	-0.6972	-0.0380	-0.0450	0.0234	-0.0163	-0.2652	0.0063
	(0.4086)	(0.3834)	(0.3446)	(0.6515)	(0.3853)	(0.3341)	(0.4086)	(0.2476)	(0.3577)	(0.4217)
Sum Perennial Crop Price Shock, t	0.6013	0.5342	0.4840	0.6986	0.5485	0.0600	0.6013	0.5552	0.4561	0.1278
	(0.2382)**	(0.1935)***	(0.1918)**	(0.3400)**	(0.2390)**	(0.1175)	(0.2382)**	(0.1506)***	(0.1888)**	(0.1933)
Sum Perennial Crop Price Shock, t-1	0.4904	0.5550	0.2744	0.5595	0.5317	-0.0816	0.4904	0.4934	0.3610	0.0460
	(0.2660)*	(0.2371)**	(0.2514)	(0.4547)	(0.3286)	(0.0805)	(0.2660)*	(0.2102)**	(0.2300)	(0.3311)
Sum Perennial Crop Price Shock, t-2	-0.0489	-0.0398	0.0505	0.0188	-0.0062	-0.0339	-0.0489	-0.0378	-0.1328	-0.2385
	(0.3065)	(0.3026)	(0.2612)	(0.4084)	(0.3063)	(0.3079)	(0.3065)	(0.2015)	(0.2326)	(0.2733)
Sum Mineral, Oil & Gas Price Shock, t	0.8510	0.9018	0.4570	1.0147	0.6658	0.3115	0.8510	0.8193	0.4983	-1.2098
	(0.4224)**	(0.3601)**	(0.3062)	(0.5796)*	(0.3554)*	(0.2326)	(0.4224)**	(0.2859)***	(0.3110)	(0.6004)**
Sum Mineral, Oil & Gas Price Shock, t-1	0.6054	0.5616	0.3830	0.2946	0.4950	0.2475	0.6054	0.7068	0.4666	-0.7524
	(0.3383)*	(0.3194)*	(0.2726)	(0.7220)	(0.3281)	(0.2604)	(0.3383)*	(0.3257)**	(0.3862)	(0.6686)
Sum Mineral, Oil & Gas Price Shock, t-2	0.0223	0.2101	0.1667	-0.5796	0.0975	-0.0661	0.0223	0.0594	-0.3121	-0.2633
	(0.4435)	(0.4079)	(0.3538)	(0.5845)	(0.3980)	(0.2020)	(0.4435)	(0.2628)	(0.3644)	(0.4971)
Sum of all annual crop shocks	1.173	1.401	0.745	0.500	0.983	0.866	1.173	1.369	0.607	1.388
p-value of sum	**0.088***	**0.033****	0.183	0.710	0.137	0.338	**0.088***	**0.018****	0.321	**0.087***
Sum of all pereannial crop shocks	1.043	1.049	0.809	1.277	1.074	-0.0555	1.043	1.011	0.684	-0.0646
p-value of sum	**0.049****	**0.017****	**0.045****	0.160	**0.063***	0.893	**0.049****	**0.004*****	0.107	0.876
Sum of all oil and gas shocks	1.479	1.673	1.007	0.730	1.258	0.493	1.479	1.585	0.653	-2.226
p-value of sum	**0.081***	**0.024****	0.159	0.604	0.116	0.374	**0.082***	**0.013****	0.332	**0.047****
Observations	644	644	644	644	644	644	644	644	622	644

All regressions include year and country fixed effects (coefficients not displayed)
Robust standard errors in parentheses, clustered by country
**** p<0.01, ** p<0.05, * p<0.1*