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Economic Shocks and Conflict: Evidence from Commodity Prices†

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Higher national incomes are correlated with political stability. Is this relationship causal? We test three theories linking income to conflict with new data on export price shocks. Price shocks have no effect on new conflict, even large shocks in high-risk nations. Rising prices, however, weakly lead to shorter, less deadly wars. This evidence contradicts the theory that rising state revenues incentivize state capture, but supports the idea that rising revenues improve counter-insurgency capacity and reduce individual incentives to fight in existing conflicts. Conflict onset and continuation follow different processes. Ignoring this time dependence generates mistaken conclusions about income and instability. (*JEL* D72, D74, O13, O17, O19, Q02, Q34)

How does income affect the risk of political instability? Income levels and growth rates are among the strongest and most robust correlates of coups and civil war.[1] While these correlations could be biased by omitted variables or reverse causality, the link has become one of the most widely accepted facts in the study of social unrest. It is especially influential in policy. States, militaries, and aid agencies predicate stabilization, public works, employment, and postconflict recovery programs on the idea that poor and unemployed men are more likely to fight, riot, and rebel (World Bank 2012).

These views are bolstered by one of the most influential economic theories of conflict: that poverty lowers the opportunity cost of insurrection. The idea comes from economic theories of crime and insurrection, and it is a central mechanism in leading formal theories of political change and state development.[2]

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[1] See Alesina et al. (1996) and Blattman and Miguel (2010). GDP per capita levels have no robust effect on civil war after accounting for country fixed effects (Djankov and Reynal-Querol 2010), but a robust relationship between income changes and conflict risk persists (Brückner 2011).

[2] The opportunity cost of crime and rebellion appears widely in theories of who participates in crime and conflict (Becker 1968; Ehrlich 1973; Hirshleifer 1995; Collier and Hoeffler 1998; Grossman 1991) but is also

A second reason is that states can avert war when they have the revenue to suppress insurgency or buy off opposition. This revenue-centered, state-capacity approach has a long tradition in comparative politics (e.g., Fearon and Laitin 2003). It has seldom, however, found its way into models of political instability.

Rather, to the extent that state revenues enter formal political economy, they usually follow a third, opposing logic: that states are a prize that can be seized, especially when the institutions that constrain power are weak (Bates, Greif, and Singh 2002; Besley and Persson 2010, 2011; Grossman 1995). This theory predicts that insurrection and coups rise with the value of the prize.

Several studies harness exogenous economic shocks to test alternate theories and avoid the bias in simple income-conflict regressions. Some of the strongest support for the opportunity cost mechanism comes from evidence that rainfall and climate shocks increase the risk of conflict onset (Burke et al. 2009; Hsiang, Meng, and Cane 2011; Miguel, Satyanath, and Sergenti 2004). Local climate shocks cannot speak to state-level mechanisms, however, and (in theory) they could affect conflict through channels other than income and opportunity cost. Thus, other sources of causal identification are helpful to round out the evidence and explore mechanisms.

This paper argues that export price shocks can help test the strength of any income-conflict causal relationship, while also helping to distinguish between competing theories. Export price shocks have three advantages that make them an important contribution to the evidence. First, most countries export just a handful of products, and changes in their world price have huge impacts on national income, investment, and spending. Second, export price shocks arguably affect conflict mainly through household incomes and state revenues. Third, some price shocks (such as to annual crops) disproportionately affect household incomes and, hence, the opportunity cost motive. Others (such as oil or mineral stocks or export prices) disproportionately affect state revenues and, hence, the state capacity and state prize motives. What matters is what type of income is affected on balance and how this shapes the incentives of armed actors. We disaggregate trade shocks by commodity, helping us distinguish competing theories.[3]

Such price shocks are an obvious place to look for income volatility, and we are not the first to look at their effect on political instability. The existing studies, however, reach seemingly different conclusions: some find no relationship (e.g., Deaton and Miller 1995); some find that conflict increases as export prices fall (e.g., Brückner and Ciccone 2010; Savun and Cook 2010); and others find the opposite (e.g., Besley and Persson 2008). What explains the inconsistency? We show that one reason is different coverage of commodities and years. A second is that none disaggregate shocks to test mechanisms. Third, conflict is modeled in different and possibly erroneous ways: there is seldom any accounting for the time dependence of conflict; the differences between conflict onset, continuation, and intensity; or

central to broader models of political economy and development when competition between or within groups is key (Acemoglu and Robinson 2001; Besley and Persson 2010, 2011; Esteban and Ray 2008).

[3] Dube and Vargas (2013) take this approach in Colombia. They focus on conflict intensity, and find support for both an opportunity cost and a state prize effect. Violent deaths increase when the coffee price falls but decrease when the oil price falls. This paper takes a parallel cross-national approach.

different definitions of conflict or instability. These omissions turn out to be theoretically and empirically important.

This paper uses trade shocks as an example of how to exploit exogenous variation in income to test theories of political instability. We examine price shocks from 65 globally traded commodities, looking at all developing countries from 1957 to 2007. To do so, we develop new and more complete commodity and country data than previously available, with nearly 50 percent more commodity-country data points. We investigate how alternative conflict measures are constructed, highlight the theoretically salient differences, and argue that more episodic measures of conflict that turn on and off with intensity are (among the data available) the most relevant for testing our theoretical mechanisms. We generate predictions and study the effects of price shocks on the outbreak of new conflicts as well as the persistence and intensity of ongoing ones. In addition to looking at aggregate shocks, we disaggregate shocks into commodity classes according to whether the impact on incomes should disproportionately affect the state or households. Since shocks may have heterogeneous effects, we also test for linear and nonlinear relationships, and for impacts conditional on state susceptibility to conflict. Finally, we illustrate a method of systematic robustness checks designed to minimize the researcher's discretion. This is especially important in the study of conflict where, in addition to the usual specification choices for the right-hand side, there are at least 18 alternative conflict definitions for the left-hand side.

Three findings stand out. First, we see no evidence of an effect of price shocks on the outbreak of new conflict or coups. Whether we look at aggregated or disaggregated shocks, and across all measures of conflict, the relationship is small and insignificant. This is true even for large shocks in the most fragile states.

Second, rising prices of all commodity classes are associated with shorter conflicts and fewer deaths. Effect sizes are large. Using the most episodic measures of conflict, a standard deviation rise in prices (in all commodity classes) roughly doubles the chance that a civil war will end. The statistical association, however, is not robust to using less episodic or low-intensity measures of conflict.

Third, not only do we see no relationship between oil and mineral price shocks and conflict outbreak, but rising oil and mineral prices are actually associated with shorter, less intense conflicts. While somewhat statistically fragile, the direction is the opposite of what the state prize logic would predict.

We draw several conclusions. One is that the state prize logic may not be an empirically important motive for war or coups. This is too strong a conclusion to make from price shocks alone, but combined with recent evidence on resource stocks—especially that large new oil discoveries are unrelated to conflict (Cotet and Tsui 2013)—evidence for a systematic state prize effect is weak indeed.

Second, large economic shocks may not be an important trigger of new conflict. Even very large shocks in the poorest, most politically fractured states are not associated with an increase in conflict risk.[4] Indeed, our evidence joins a growing

[4] Note that it is still possible for trade shocks to have heterogeneous, context-dependent impacts, perhaps because they shape the distribution of power (Esteban and Ray 2011; Esteban and Ray 2008). Our evidence, however, seems to rule out large systematic effects.

body of results that cast doubt on the effect of income on conflict outbreaks.[5] Likewise, economists have found little relation between poverty and terrorism (Krueger 2008) or trade shocks and coups (Deaton and Miller 1995). Like ours, most of these papers challenge the conventional wisdom through attention to theory, time dependence, data measurement and completeness, and robustness checks.

Third, while economic shocks may not trigger new wars, they may play an influential role in existing ones, contributing to the persistence or intensity of war. The direction and magnitude of our results are consistent with recent micro-level evidence that sees more robust relationships between conflict intensity and economic shocks, such as the Dube and Vargas (2014) evidence from Colombia. Berman and Couttenier (2013) examine subnational conflict patterns in 13 African countries and find that trade shocks are associated with conflict outbreaks subnationally, and may influence the location of violence, but are neither strong nor persistent enough to affect the outbreak of nationwide conflict.

These and other papers argue that the evidence from shocks is consistent with the opportunity cost mechanism. We agree, especially then there is a link between falling agricultural prices and conflict length and intensity. Yet the evidence is also consistent with the state capacity theory. We argue this theoretical mechanism deserves much more attention in workhorse political economy models. Other important directions for future work include better quality data on cross-national conflict intensity, and more micro-level case studies that try to distinguish between competing theories.

Finally, our analysis suggests an important methodological lesson for the study of conflict: that the onset and continuation of conflict follow different processes, and that the latter deserves more attention. At the individual level, the decision to rebel is fundamentally different in peacetime, where there is a severe collective action problem to overcome.[6] Shocks are universal, but solutions to the collective action problem in rebellion are not.

I. Competing Theories and Predictions

A. *The Opportunity Cost of Insurrection*

In the economic analysis of crime, rational individuals weigh the relative returns, costs and risks in deciding whether to predate or produce, and rates of crime are predicted to fall when wages and employment rise (e.g., Becker 1968). The same logic has been applied to rebellion, where a civilian's incentive to rebel rises as household income and economic opportunities decline (e.g., Grossman 1991; Hirshleifer 1995). Consequently, many predict an inverse relation between export price shocks and conflict (e.g., Collier and Hoeffler 1998; Miguel, Satyanath, and Sergenti 2004).

[5] Several papers find little correlation between conflict onset and income levels (Djankov and Reynal-Querol 2010), rainfall shocks (Ciccone 2011; Miguel and Satyanath 2011), resource stocks (Cotet and Tsui 2013), and temperature shocks (Dell, Jones, and Olken 2012).

[6] At the country level, states may also be more resilient to shocks in peacetime, and are better able to endogenously respond to price shocks to avoid unrest (Carter and Bates 2011).

In general equilibrium, this prediction rests on household incomes being disproportionately affected by the shock relative to government revenues.[7]

Not all price shocks impact household and state incomes equally, however. Agricultural goods affect household incomes more directly, especially since taxation is typically limited. Revenues from capital-intensive commodities, like minerals and fuels, accrue mainly to the state and will affect individual incomes less directly, through public goods and transfers. 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 (Dal Bó and Dal Bó 2011).[8]

Commodity price shocks should provide a particularly strong source of high frequency income variation. Unexpected changes in world commodity supplies push prices temporarily out of equilibrium (Deaton and Laroque 1992; Deaton and Miller 1995). Prices are autocorrelated in the short run, but the persistence of many price shocks tends to be short. Thus, most commodity price series often resemble a set of brief, unpredictable spikes interspersed by long, shallow troughs.[9] Like transitory rainfall shocks, largely temporary price increases ought to augment the opportunity cost of insurrection mechanism, since they likely reduce the short-term opportunity cost of fighting more than they affect the long-term value of state capture (Chassang and Padró i Miquel 2009). Moreover, existing theories suggest that transitory shocks are sufficient to solve collective action problems and mobilize individuals for political ends (e.g., Acemoglu and Robinson 2001).

B. *The State as Prize*

Rising commodity prices increase resource rents. This may make the state a more valuable prize, and increase incentives to seize it (Grossman 1995; Bates, Greif, and Singh 2002). While rents vary most with resource stocks, theoretical models also emphasize the importance of changing values (Bates 2008; Besley and Persson 2010; Besley and Persson 2011). One can extend the same logic to coups d'état, which bring the same rents to the coup-makers without the costs of a long war. The strongest supportive evidence comes from the historical analysis of African coups and warfare (Bates 2008; Reno 1999), and studies arguing that natural resource stocks drive coups and conflict (Fearon 2005; Ross 2006).

[7] For instance, lower income means that working in the regular economy is worse, but so is predating because there is less to steal and the state is less valuable. See Fearon (2008) for an illustration. The prediction is ambiguous, however, and the common result in many contest models such as Grossman (1991)—that changes in income have no effect on the probability of war—rest mainly on the assumption that changes tax revenues precisely track changes in household income.

[8] This would be mitigated by taxation of rural producers by rebel groups.

[9] For some goods, price changes are transitory, while, for others, price changes have a permanent component (Ghoshray 2011). Year-on-year permanent changes, however, tend to be small in magnitude. Some large and persistent price changes result from structural breaks (e.g., oil cartelization in the 1970s), but large commodity price swings are typically driven by supply shocks and, hence, are transitory. The online Appendix provides further evidence and shows that aside from the persistence of oil and gas price shocks, there is no systematic relationship between commodity class (annual, perennial, extractive) and the permanence of shocks.

Not all countries may be equally vulnerable, however. 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.

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 nonalluvial, 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, especially marketing boards. Note that prices shocks to any taxable crop will affect state revenues. Our assumption is that this effect is disproportionately large for minerals/fuels over perennial crops and especially annual crops.

Disaggregated commodity shocks should help us distinguish between the state prize and opportunity cost mechanisms. Higher prices of labor-intensive, smallholder-owned, and difficult-to-tax commodities (such as annually harvested agricultural 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.[10] The effect of higher perennial crop prices on instability is more ambiguous, however, as it arguably directly raises household incomes and state rents at the same time.

Note, however, that the incentives for state capture increase with the belief that price shocks are persistent. The duration of most price shocks is shorter than the average conflict, meaning that price shocks are an incomplete test of the state prize idea. One reason it is still a reasonable test, however, is that the transitory nature of many price shocks is not widely recognized, even among professional forecasters (Deaton 1999). Beliefs are also more important than reality. If rebel leaders forecast commodity prices no better than econometricians, then price shocks may be perceived as persistent, thereby strengthening the incentives to predate under the state prize logic.

[10] One caveat is that a shock to mineral and fuel prices is not a truly clean test of the state prize mechanism. In principle, a rise in capital-intensive good prices is also consistent with the opportunity cost theory. Dal Bó and Dal Bó (2011) integrate social conflict into a general equilibrium model of trade where there are three sectors: capital-intensive, labor-intensive, and an appropriation sector that is labor intensive relative to the economy. In this framework, with well-functioning markets, rising prices of the capital-intensive good should cause that industry to expand, the labor-intensive industry to contract, and make labor relatively more abundant, lowering wages and, hence, the opportunity cost of appropriation. Overall this strengthens our expectation of a reduced-form positive relationship between mineral resource prices and conflict, even if it clouds the mechanism at work. This general equilibrium mechanism, however, would be moderated by labor market conditions that limit the responsiveness of wages in the lowest income countries to changes in the demand for labor in other sectors—e.g., large amounts of nonmarket labor, of highly elastic labor supply, or downward nominal wage rigidity (Behrman 1999; Kaur 2013). We find the state prize logic more persuasive in developing countries, but a positive correlation between mineral/fuel prices and conflict would not eliminate the possibility that opportunity cost also plays a role.

C. *Revenues and State Capacity*

Of course, rising revenues could also make the state easier to defend, helping states buy off opposition, counter insurgents, or strengthen control. This state capacity logic is seldom discussed in the economic conflict literature, but is more common in political science. Fearon and Laitin (2003), for instance, argue that per capita income is a proxy for state administrative, military, and police capabilities. They associate financially weak central governments with weak local policing and inept and corrupt counterinsurgency practices. Others look at oil regimes and argue that resource windfalls allow the state to strengthen its apparatus of security and control and suppress or buy off rebellion (Ross 2012; Snyder 2006).

These arguments are generally case-based, and formal incorporation in political economy models remains rare (an exception is Dunning 2008). More commonly, political economy models ignore this state capacity effect or assume the state prize effect is dominant (e.g., Besley and Persson 2010).[11] Capacity will also be linked to capital goods and technologies that are typically imported and expensive (from guns to transport to surveillance technology). Finally, counterinsurgency, co-option and repression include buying off opposition leaders, public goods spending, and other strategic payoffs that increase with wealth.

If true, the predictions from the state capacity theory are opposite to those of the state prize effect: rising oil and mineral prices (and perhaps those of perennial crops) should lower the risk of new wars and shorten ongoing ones, especially in poorer and more weakly institutionalized states.

II. Data

A. *Commodity Price Shocks*

We construct a new database of commodity price shocks, one with nearly 50 percent more commodity-country data than previous sources.[12] 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, due mainly to data availability).

Our focus is on export price shocks, though prices of food and imports are considered. 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.

We calculate S_{it} as the annual difference in each country's log commodity export price index. Each country's price index is a geometric average of all commodity

[11] If modeled in a general equilibrium framework, richer governments would also face higher wages, moderating the capacity advantage of more wealth. But so long as there is inequality (as one might expect in oil or mineral regimes), there will be poor people to mobilize (Fearon 2008).

[12] The increase arises mainly from additional commodities, but also from year coverage. For export shares and quantities, we start with the United Nations Commodity Trade Statistics Database and fill in missing countries and years using regional and country statistical yearbooks. For United States or world prices, we start with the International Monetary Fund's International Financial Statistics, and, to fill in missing commodities, we use a variety of sources including the US Bureau of Labor Statistics, Geological Survey, and Department of Agriculture, as well as various commodity-specific studies. We detail sources and construction in the online Appendix.

export prices weighted by lagged export shares. The geometric construction allows us to decompose the shock additively into commodity classes. We use US dollar-denominated prices from international markets. In our preferred measure, each price is weighted by its share in total national exports in years $t - 2$ to $t - 4$. Results are mostly unchanged by the use of an arithmetic average or fixed weights (from either the initial year in the data or the midpoint of the period).

Finally, the economy is most sensitive to commodity price shocks in commodity-dependent nations, and so our shock measure multiplies the price difference by the ratio of commodity export values to GDP at the midpoint of the period.[13]

Weighting the shock based on export importance and major export products has the advantage of ensuring pass-through of the shock to the economy, and varying national income. But we will not capture the effect of trade shocks on poorly integrated peripheries, where some conflicts originate. Hence, export price shocks will help us estimate local average treatment effects of income changes to the households or states that receive the revenues from traded commodities.

There are three potential sources of endogeneity in this shock, however. 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.

Second, not all nations are price-takers. Nations that produce a large 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, we omit from a nation's price shock any products where they produce more than a 10 percent share of global exports. We also consider 3 percent and 20 percent thresholds, yielding similar results.

Third, commodity prices can affect real incomes through consumption. A rise in the price of food or fuel exports raises the incomes of farmers or laborers who produce them, thus increasing the opportunity cost of rebellion. But this food and fuel price rise also decreases real incomes of all households (reducing the opportunity cost of rebellion). Food prices have been analyzed elsewhere (Arezki and Brückner 2011; Bellemare 2013), and we focus this paper on export commodity price shocks alone. But these prices cannot necessarily be ignored.[14] We discuss alternative ways of dealing with this issue in the empirical strategy below.

[13] To calculate X/GDP, we take the average of the ratio in the years 1978 to 1982, or 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). Results do not change if we use a moving average.

[14] If food and fuel prices co-move with export prices, we will tend to underestimate the opportunity cost effect. Indeed, our country export price shocks have a correlation of 0.3 with a broad *Food and fuel price shock* of 42 commodities. Estimates vary, but food represents 50 to 70 percent of household expenditure in developing countries, and fuels represent roughly 7 to 12 percent (Bacon, Bhattacharya, and Kojima 2010; Banerjee and Duflo 2007). Remaining consumption is largely nontraded goods or services such as education, health, and housing. Manufactures are a very small proportion of household consumption on average. Based on these approximate shares, we calculate this shock as $6/7$ food prices (with equal weight given to all food commodities in our dataset) and $1/7$ fuel prices (with equal weight given to all fuel commodities).

B. *Insurrection and Internal Conflict*

Although there are six common approaches to measuring internal war, most papers consider just one, usually without considering the theoretical or empirical differences across measures. There are four major datasets: (i) UCDP/PRIO; (ii) Fearon and Laitin (2003); (iii) Sambanis (2004); and (iv) the Correlates of War, or COW (Sarkees and Wayman 2010). All four use a threshold of 1,000 battle deaths to define a *Civil war*.[15] The UCDP/PRIO dataset also codes smaller *Civil conflicts* of at least 25 battle deaths per year, leading to 3 common UCDP/PRIO conflict constructions: low intensity (25 deaths) conflict years, *cumulatively* high-intensity (1,000 death) years, and high-intensity years only. These competing measures differ on when to code the start, what counts as a war, and how to treat breaks in violence (Sambanis 2004).[16]

For our purposes, the theoretically most important difference is how lulls in conflict are treated. The UCDP/PRIO and COW measures are more episodic and tend to capture variations in conflict intensity—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 ongoing war. Table 1 lists summary statistics. The differences in the datasets are evident simply by comparing means of incidence. Comparing the most to the least episodic measures, UCDP/PRIO high-intensity civil war and Fearon and Laitin civil wars are coded in 7 percent and 20 percent of country-years, respectively.

A second distinction is time dependence in conflict. Studies tend 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)—often with no theoretical guidance for one choice over the other. Some also look at conflict *ending* separately from onset, by disaggregating conflict into onset and continuing years. As a result, these datasets and coding choices give researchers considerable latitude—at least 18 choices of a dependent variable.

When considering economic shocks, and given our theoretical mechanisms, we believe the more episodic UCDP/PRIO and COW measures are theoretically most relevant, as they capture the ebb and flow of incentives for war as incomes rise or fall. Also, as we show in the next section, it makes the most theoretical and statistical sense to examine onset and endline separately and to discard incidence (or treat it as a special case). Nonetheless, we also take an approach we believe ought to be commonplace: we examine all conflict measures and are cautious of giving prominence to any relationship that fails to produce consistent signs, magnitudes, and robustness.

Finally, in addition to looking at indicators for passing a death threshold, we also consider the underlying battle death data itself to measure conflict intensity (Lacina and Gleditsch 2005). Although the quality of data on deaths is poor, this is arguably

[15] This is an annual threshold in the case of UCDP/PRIO and cumulatively over the life of the war in the case of Sambanis. We use a dataset provided by James Fearon, which extends his the original one with Laitin. In this dataset, civil wars include those conflicts with over 1,000 battle deaths cumulatively and an annual average of at least 100 per year.

[16] These differences do not reflect measurement error so much as different definitions of war. Indeed, relative to most cross-national measures one could argue war is accurately measured.

Table 1—Summary Statistics

			Incidence		Onset		Ending	
Variable	Dataset	Definition	Obs.	Mean	Obs.	Mean	Obs.	Mean
Civil wars	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 1 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.

the most theoretically relevant variable when considering the opportunity cost or state prize and capacity mechanisms.

C. *Coups*

Following Deaton and Miller (1995), we also examine coups d'état, since the state prize theory arguably applies to both conflict and coups—one the mode of capture from the periphery, and one from the center. We consider a measure of actual and attempted *Coups* (Powell and Thyne 2011) capturing any illegal and overt attempt by the military or elites to unseat a sitting executive. We also consider a broader measure of unconstitutional changes in leadership, *Irregular leader exit*, which only includes *successful* coups, assassinations, and revolts—from the Archigos leader transition dataset (Goemans, Gleditsch, and Chiozza 2009).

III. Empirical Strategy

A. *Econometric Model of Conflict*

Cross-national conflict specifications vary, but typically take the form:

$$C_{it} = \alpha + \mathbf{X}_i \boldsymbol{\beta} + \mathbf{S}_{it} \boldsymbol{\theta} + \varepsilon_{it}, \tag{1}$$

where C is an indicator for a conflict event (either onset or incidence) for country i in year t, $\mathbf{X}$ is a vector of time-invariant country characteristics, $\mathbf{S}$ is a vector of time-varying country characteristics (i.e., shocks), and ε is an idiosyncratic error term. Researchers employ both linear and nonlinear (e.g., logit) estimators. For clarity of exposition, we discuss the linear model here.

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 $\mathbf{X}$ vector is often endogenous and so introduces bias, which we address here through the use of country fixed effects.

Second, the time-dependence of time-varying shocks is typically ignored, introducing bias. Commodity price shocks, for example, are negatively autocorrelated and can take at least one period to impact incomes (see online Appendix). This leads to correlation between the current shock and the error term. To guard against bias, our baseline $\mathbf{S}_{it}$ vector 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 econometric concerns. Conceptually, shocks could have a larger effect in ongoing conflicts. For instance, if initiating a conflict has a fixed cost, or is subject to a coordination problem, new conflicts could be less sensitive to price shocks than existing ones, on average. The more serious concern is econometric, however, as ignoring dynamics biases the estimated effect of shocks on conflict. Conflict is highly persistent, and both 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):

$$\text{(2a)} \qquad Onset_{it} = \alpha_{Oi} + \tau_{Ot} + \alpha_{Oi} \times t + \mathbf{S}_{it}\boldsymbol{\theta}_O + \mathbf{Z}_{it}\boldsymbol{\beta}_O + \varepsilon_{Oit}$$

$$\text{(2b)} \qquad Ending_{it} = \alpha_{Ei} + \tau_{Et} + \alpha_{Ei} \times t + \mathbf{S}_{it}\boldsymbol{\theta}_E + \mathbf{Z}_{it}\boldsymbol{\beta}_E + \varepsilon_{Eit}$$

where α_i and τ_t are country and year fixed effects, $\alpha_i \times t$ are country-specific time trends, $\mathbf{Z}_{it}$ is a vector of time-varying covariates, and ε_{it} is an idiosyncratic error term.[17] Coefficients in the dynamic model confirm that the simple conflict incidence regression is both biased and unnecessarily constrained (not shown). The approach provides results consistent with duration analysis.

In addition to looking at aggregate shocks, we distinguish between price shocks to *Annual* agricultural goods (such as oilseeds, food crops, and livestock) that likely

[17] 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 0 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 accounts for secular changes in conflict risk that may vary across countries and offers a flexible way to incorporate import price shocks as discussed below.

accrue to households, S_{Ait}; *Perennial* tree crops (such as cocoa, coffee, rubber, or lumber), which are ambiguous, S_{Pit}; and *Extractive* products (such as iron, oil, and gas), S_{Eit}, which are more likely to accrue to states. Note that $S_{it} = S_{Ait} + S_{Pit} + S_{Eit}$ because of the geometric construction of the underlying price series

There are three common ways to estimate equations (2a) and (2b): logit regression without fixed effects, conditional fixed effects logit, or a fixed effects linear probability model. We examine all, and find little substantive difference. We show results from all estimators but our preferred, default specification is the fixed effects linear probability model (LPM). Fixed effects control for unobserved country characteristics that lead a country to be more conflict-prone as well as less diversified and more resource-dependent. The most common drawback to fixed effects—increasing the noise to signal ratio and biasing estimates toward zero—is less problematic here, since our independent variables of interest, $\mathbf{S}_{it}$, contain considerable within-country variation.[18] We prefer the LPM to simplify interpretation of the coefficients, and because it allows us to estimate a more flexible specification that includes country-specific time trends and consumption shocks, an advantage that outweighs the minor gains from limited dependent variable techniques (Beck 2011). Results are robust to more parsimonious specifications.

Last, to estimate the effect of prices on deaths (D), we use two specifications:

$$\text{(3a)} \quad D_{it} = \tau_{Dt} + \mathbf{S}_{it}\boldsymbol{\theta}_D + \pi_{Dt}1st_{it} + \delta_{Dt}Duration_{it} + \mathbf{Z}_{it}\boldsymbol{\beta}_D + \varepsilon_{Dit}$$

$$\text{(3b)} \quad D_{it} = \tau_{Dt} + \mathbf{S}_{it}\boldsymbol{\theta}_D + \beta_{Dt}D_{it-1} + \pi_{Dt}1st_{it} + \delta_{Dt}Duration_{it} + \mathbf{Z}_{it}\boldsymbol{\beta}_D + \varepsilon_{Dit}$$

that 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 (1*st*) 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.[19] We also consider the natural log of *Deaths* as a dependent variable. In all specifications of equations (2a)–(3b), we cluster standard errors at the country level.[20]

[18] The within-country variation in our aggregate commodity price shock variable, S_{it}, is ten times larger than the between-country variation.

[19] In the presence of lagged dependent variables, fixed effects can lead to bias over short panels. The bias has order $1/T$, where T is the number of years. This bias is small when estimating the larger panels using equations (2a) and (2b), but when estimating equations (3a) and (3b), the median panel length is $T = 6$. Moreover, to estimate (3a) and (3b) we use a maximum likelihood interval regression estimator because we observe a range of battle deaths rather than a fixed number (see below). This interval estimator cannot accommodate country fixed effects as it is subject to an incidental parameters problem. Instead, we include regional dummies to sweep out regional effects.

[20] Our key conclusions remain unchanged when we take a less conservative approach to inference, by imposing (i) an AR(4) structure on the correlation of error terms within country, or (ii) homoscedasticity (i.e., nonrobust standard errors). Results available upon request.

B. *Time-Varying Confounders*

We are most concerned with time-varying factors correlated with both export price shocks and conflict.[21] As we note above, prices of a broad basket of consumption goods are positively correlated with individual country export prices. The inclusion of year fixed effects will reduce this bias, by removing any general association between conflict and food/fuel price shocks in a given year. We may still be worried, however, about country-specific reactions to the same shock.

We thus want to include a country-specific annual shock to real consumption arising from changing world prices. Our preferred method is to interact the country-invariant *Food and fuel price shock* described above with country fixed effects (in addition to using year fixed effects and allowing trends in conflict to vary across countries). This approach has several advantages. Data on actual consumption baskets are available for only a small number of countries, making a country-specific shock impossible to construct. The usual alternative, an import price index, has several problems. Country-specific import weights are endogenous to price movements, production (not consumption) patterns, and wealth. More importantly, the composition of developing country imports does not reflect household consumption. Imports to developing countries are 70 percent manufactures, on average (Baxter and Kouparitsas 2006), despite being a negligible part of average household expenditures. An import price index better reflects purchases by firms, elites, and governments. Thus, import shocks are less theoretically relevant.

C. *Price Shocks and Income*

Our analysis focuses on the reduced form impact of export commodity shocks on conflict. This presumes a "first-stage" relationship between the price shocks and income. Most countries export just one to three major products, and historically changes in the world price of these exports have huge impacts on national income, investment, and spending (Blattman, Hwang, and Williamson 2007; Deaton and Miller 1995). We confirm this relationship in our data in the online Appendix. Briefly, our analysis shows that a standard deviation (SD) increase in prices raises per capita GDP growth by 1 percentage point per year in developing countries in the baseline—a 65 percent increase over average growth rates. If we control for time fixed effects (and hence commodity price shocks affecting many countries similarly), then the effect of price shocks declines but is still robust—a standard deviation increase raises growth rates by 22 to 36 percent depending on the specification. These effects are most robust when we disaggregate by commodity class—annual crop and fuels/mineral prices are most strongly associated with growth rates.

GDP growth is not the ideal measure, since we are really interested in household incomes and state revenues. We also find that positive export price shocks are associated with large and statistically significant increases in government and household expenditures (see online Appendix).

[21] Recall that we account for price makers in such a way that reduces the scope for rainfall shocks at home, for example, to affect world prices of commodities in large agricultural goods exporters.

We do not, however, claim that the only effect of price shocks is upon incomes, and, hence, we do not adopt an instrumental variables (IV) approach. We believe the main effect is upon incomes, but can't exclude other effects that could affect conflict on the margin: through inequality, changed migration patterns, changed land use and conflict, and so forth.[22] Moreover, IV estimates would enlarge standard errors and bias us towards the null.[23]

IV. Results

A. *The Impact of Aggregate Shocks on New Conflicts and Coups*

Table 2 displays the results of a linear regression of conflict onset on aggregate price shocks (equation (2a)). Price shocks have mean zero and unit standard deviation. In addition to the individual coefficients, the table also displays the sum of the three shock coefficients, its p-value, and (to interpret magnitudes) the change in conflict risk associated with this sum. In panel A, the table displays regression results without controlling for the consumption shock, and panel B includes it.

Looking at aggregate price shocks, we see no evidence of a large, consistent, robust relationship between price shocks and political instability, with or without the consumption shock. The point estimates on S_t, S_{t-1}, and S_{t-2} shift in sign depending on the lag or the measure of conflict onset, and are small relative to their standard errors. None of the 48 coefficients in panels A or B are significant at the 5 percent level, and only four are significant at the 10 percent level. Finally, relative to the mean of the dependent variables (recorded at the base of the table), the standard errors are generally small, for the most part ruling out large effects.

Of course, this aggregate shock could conflate opposing effects, biasing results toward the null. Table 3 disaggregates the shock into annual, perennial, and extractive commodities. The table displays only the sum of the current and lagged shocks (coefficients on individual lags are in the online Appendix). The opportunity cost mechanism predicts an inverse relationship between agricultural (annual) price shocks and onset, but we see little evidence of this relationship. Without the consumption shock (panel A), the sum of annual agricultural shocks is close to zero, is not robust, and for more than half the onset measures has the "wrong" sign. Controlling for the consumption shock (panel B), the signs on the sum are negative just half the time, and the coefficients are not statistically significant.

Meanwhile, the state prize mechanism (and the general equilibrium approach to opportunity cost) predicts a positive effect of the price of easily captured commodities on conflict onset/coups, especially minerals and fuels. The same is true of the general equilibrium version of the opportunity cost theory. In both panels A and B,

[22] Sarsons (2013) offers evidence against the excludability of rainfall in income-conflict analysis.

[23] In unreported results (available upon request), we estimate the IV regression and confirm that: (i) the estimates are much noisier than the reduced form, and (ii) there remains no evidence of a systematic relationship between export price shocks and conflict.

Table 2—Impacts of Aggregate Export Price Shocks on Conflict and Coup Onset

	Dependent variable: Indicator for onset							
	UCDP/PRIO Civil War data			Other Civil War datasets			Coups	
	Low (1)	High cum. (2)	High (3)	FL (4)	S (5)	COW (6)	Archigos (7)	PT (8)
Panel A. No consumption shocks								
Price shock, t	−0.0002	0.0019	0.0006	0.0006	−0.0008	0.0017	0.0012	0.0007
	(0.0025)	(0.0017)	(0.0015)	(0.0012)	(0.0014)	(0.0019)	(0.0024)	(0.0026)
Price shock, $t-1$	0.0051	0.0014	0.0003	−0.0005	−0.0007	0.0025	−0.0022	−0.0008
	(0.0033)	(0.0018)	(0.0012)	(0.0015)	(0.0016)	(0.0017)	(0.0032)	(0.0033)
Price shock, $t-2$	−0.0014	−0.0007	−0.0004	0.0011	−0.0012	0.0015	−0.0041	−0.0054
	(0.0027)	(0.0014)	(0.0011)	(0.0011)	(0.0016)	(0.0018)	(0.0022)*	(0.0032)*
Sum of all shocks	0.003	0.003	0.001	0.001	−0.003	0.006	−0.005	−0.005
p-value of sum	[0.527]	[0.395]	[0.836]	[0.600]	[0.433]	[0.115]	[0.315]	[0.276]
Impact of shocks on risk (%Δ)	0.082	0.118	0.028	0.067	−0.124	0.201	−0.106	−0.092
Observations	4,106	4,352	4,748	4,088	4,092	4,398	4,647	5,079
R^2	0.108	0.142	0.086	0.108	0.086	0.068	0.054	0.070
Number of countries	117	117	117	114	117	116	114	117
Mean of dependent variable	0.042	0.022	0.019	0.018	0.022	0.029	0.048	0.06
Panel B. With consumption shocks								
Price shock, t	−0.0007	0.0005	−0.0008	0.0018	−0.0004	0.0001	0.0040	0.0015
	(0.0026)	(0.0016)	(0.0015)	(0.0017)	(0.0018)	(0.0021)	(0.0032)	(0.0033)
Price shock, $t-1$	0.0052	0.0001	−0.0004	−0.0002	−0.0005	0.0026	0.0006	0.0003
	(0.0036)	(0.0020)	(0.0013)	(0.0016)	(0.0018)	(0.0021)	(0.0038)	(0.0041)
Price shock, $t-2$	−0.0013	−0.0027	−0.0019	0.0007	−0.0004	0.0025	−0.0023	−0.0042
	(0.0030)	(0.0014)*	(0.0011)*	(0.0012)	(0.0018)	(0.0019)	(0.0025)	(0.0033)
Sum of all shocks	0.003	−0.002	−0.003	0.002	−0.001	0.005	0.002	−0.002
p-value of sum	[0.580]	[0.469]	[0.268]	[0.442]	[0.733]	[0.184]	[0.735]	[0.743]
Impact of shocks on risk (%Δ)	0.077	−0.094	−0.159	0.129	−0.059	0.179	0.047	−0.040
Observations	4,106	4,352	4,748	4,088	4,092	4,398	4,647	5,079
R^2	0.246	0.256	0.159	0.272	0.227	0.198	0.128	0.134
Number of countries	117	117	117	114	117	116	114	117
Mean of dependent variable	0.042	0.022	0.019	0.018	0.022	0.029	0.048	0.06

Notes: All regressions use a linear probability model and include year fixed effects, country fixed effects, and country-specific time trends. Robust standard errors in parentheses, clustered by country. All regressions in panel B also include interactions of country fixed effects and terms capturing the contemporaneous, once-lagged, and twice-lagged sum of price shocks for a bundle of imported foods, oil, and gas commodities common across countries. Dataset abbreviations are as follows: FL = Fearon and Laitin, S = Sambanis, COW = Correlates of war, and PT = Powell and Thyne.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

roughly half the signs on extractive shocks are in the wrong direction again. None are statistically significant in panels A or B.[24]

[24] These results do not change if we consider other functional forms, a quadratic of the price shock, or the absolute value of the price shock (results available upon request).

Table 3—The Impact of Disaggregated Export Price Shocks on Conflict and Coup Onset

	Dependent variable: Indicator for onset							
	UCDP/PRIO Civil War data			Other Civil War datasets			Coups	
	Low (1)	High cum. (2)	High (3)	FL (4)	S (5)	COW (6)	Archigos (7)	PT (8)
Panel A. No consumption shocks								
Annual crop shock								
Sum of all price shock coefficients	0.004	0.003	0.0002	0.001	−0.005	0.008	−0.002	−0.006
p-value of sum	[0.593]	[0.541]	[0.965]	[0.839]	[0.205]	[0.127]	[0.813]	[0.357]
Impact of shocks on risk (%Δ)	0.098	0.116	0.008	0.031	−0.245	0.268	−0.04	−0.097
Perennial crop shock								
Sum of all price shock coefficients	0.004	0.006	0.006	0.004	−0.001	0.003	−0.007	0.003
p-value of sum	[0.513]	[0.162]	[0.087]*	[0.316]	[0.790]	[0.589]	[0.276]	[0.774]
Impact of shocks on risk (%Δ)	0.097	0.269	0.321	0.227	−0.052	0.093	−0.136	0.045
Extractive crop shock								
Sum of all price shock coefficients	0.005	0.003	−0.0002	0.002	−0.003	0.008	−0.009	−0.01
p-value of sum	[0.573]	[0.469]	[0.954]	[0.65]	[0.584]	[0.117]	[0.179]	[0.136]
Impact of shocks on risk (%Δ)	0.108	0.146	0.011	0.085	−0.134	0.292	−0.196	−0.173
Observations	4,106	4,352	4,748	4,088	4,092	4,398	4,647	5,079
R^2	0.109	0.143	0.087	0.108	0.086	0.069	0.055	0.072
Number of countries	117	117	117	114	117	116	114	117
Mean of dependent variable	0.042	0.022	0.019	0.018	0.021	0.029	0.047	0.059

(*Continued*)

Finally, our results contrast with previous evidence on price shocks and conflict. Brückner 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 the opposite. The latter data are not available for replication, but we reconcile our results to Brückner and Ciccone in the online Appendix. Their inverse relationship between shocks and conflict is driven by a unique coding of the UCDP/PRIO war measure and the use of an older version of that dataset, and is not robust to alternate or updated codings of conflict. If their coding of conflict is used, the result is also sensitive to the sample of years (1983 onward) and the use of a particular small sample adjustment to the standard errors. The coding of the shocks measure is inconsequential.

A Systematic Approach to Robustness Analysis.—Because the choice of dependent variables and model specification offer an unusually large amount of discretion to the researcher, we propose 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 or requiring millions of regressions (as in Sala-i-Martin 1997).

Table 3—The Impact of Disaggregated Export Price Shocks on Conflict and Coup Onset (*Continued*)

	Dependent variable: Indicator for onset							
	UCDP/PRIO Civil War data			Other Civil War datasets			Coups	
	Low (1)	High cum. (2)	High (3)	FL (4)	S (5)	COW (6)	Archigos (7)	PT (8)
Panel B. With consumption shocks								
Annual crop shock								
Sum of all price shock coefficients	0.003	−0.005	−0.006	0.001	−0.003	0.006	0.008	−0.002
p-value of sum	[0.713]	[0.226]	[0.108]	[0.790]	[0.485]	[0.263]	[0.457]	[0.850]
Impact of shocks on risk (%Δ)	0.073	−0.213	−0.287	0.054	−0.152	0.208	0.157	−0.0281
Perennial crop shock								
Sum of all price shock coefficients	0.005	0.003	0.003	0.006	0.001	0.001	−0.002	0.005
p-value of sum	[0.489]	[0.503]	[0.351]	[0.225]	[0.892]	[0.794]	[0.723]	[0.669]
Impact of shocks on risk (%Δ)	0.112	0.137	0.182	0.326	0.030	0.046	−0.0505	0.0769
Extractive crop shock								
Sum of all price shock coefficients	0.005	−0.003	−0.005	0.004	−0.001	0.009	0.001	−0.007
p-value of sum	[0.540]	[0.525]	[0.267]	[0.400]	[0.885]	[0.137]	[0.908]	[0.520]
Impact of shocks on risk (%Δ)	0.122	−0.122	−0.253	0.201	−0.038	0.297	0.022	−0.111
Observations	4,106	4,352	4,748	4,088	4,092	4,398	4,647	5,079
R^2	0.247	0.257	0.160	0.273	0.228	0.199	0.129	0.136
Number of countries	117	117	117	114	117	116	114	117
Mean of dependent variable	0.042	0.022	0.019	0.018	0.021	0.029	0.047	0.059

Notes: All regressions use a linear probability model and include year fixed effects, country fixed effects, and country-specific time trends. Robust standard errors in parentheses, clustered by country. All regressions in panel B also include interactions of country fixed effects and terms capturing the contemporaneous, once-lagged, and twice-lagged sum of price shocks for a bundle of imported foods, oil, and gas commodities common across countries. Dataset abbreviations are as follows: FL = Fearon and Laitin, S = Sambanis, COW = Correlates of war, and PT = Powell and Thyne.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

For each of the 6 conflict onset measures, we estimate 11 models, each by a linear and logit regression. Specifically, we (i) start with our main specification, then make the following changes one at a time: (ii) drop the consumption shock;[25] (iii) drop the X/GDP rescale of price indices; (iv) include all price-makers; (v) use a 3 percent price-maker cutoff instead of 10 percent; (vi) use a 20 percent price-maker cutoff; (vii) replace time-varying weights with fixed 1980 weights; (viii) censor the price shock at the first and ninety-ninth percentile; (ix) exclude country-specific time trends; (x) eliminate year fixed effects; and (xi) eliminate country fixed effects. We also decompose the aggregate shock into the three classes, and examine the size

[25] The logit estimators do not converge when saturated, such as when using country-specific time trends or the country-specific consumption shock. Thus, these terms are omitted from the logit analysis, and only the first nine models are estimated for logit case, for a total of 120 models.

and significance of the sum (giving us 378 potential sums and p-values). An example of the results in table form is in the online Appendix. We focus on the figures here because they allow us to concisely, transparently, and intuitively communicate the results of 132 regressions better than tables. The relevant models will vary by question or application, but this general approach to robustness is one that could be applied widely in conflict or other cross-national analysis.

Figure 1 graphs the results. Panel A displays the distribution of p-values. A robust result is generally one that has $p < 0.05$ for the main specification and is robust to small changes in specification and measurement, especially theoretically arbitrary ones. Given the large number of reasonable permutations, this corresponds to a highly left-skewed distribution of p-values. Our preferred specification is not significant, however, and this figure demonstrates that small changes do not alter this fact. It also shows that what significant results we do occasionally observe are not much better than chance, since the distribution is closer to a uniform distribution of p-values—the distribution one might expect from a purely random relationship between price shocks and conflict (indicated by the horizontal line).

We then plot these p-values against the magnitude of the coefficients in terms of their impact of the risk of conflict in Figure 1(panel B), using only the linear model (the logit model, not shown, performs no better). Only two estimates cross the $p = 0.05$ threshold (the vertical dashed line). We fit a nonparametric kernel regression line for each commodity class to assess the average direction of effects. Annual crop prices and conflict display the hypothesized inverse relationship, but on average it is not large and almost all estimates are far from conventional levels of statistical significance. The relationship between extractive commodities and conflict, meanwhile, is volatile but on average close to zero and not robust.

Impact Heterogeneity.—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 revenues, but whether the leader can appropriate revenues. 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.[26] We start with the impact of shocks in *Nondemocracies*. We also look at 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, we may prefer a measure of weak nondemocratic 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 nondemocracies (Vreeland 2008). More

[26] 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 competitiveness of political participation (Marshall, Gurr, and Jaggers 2012).

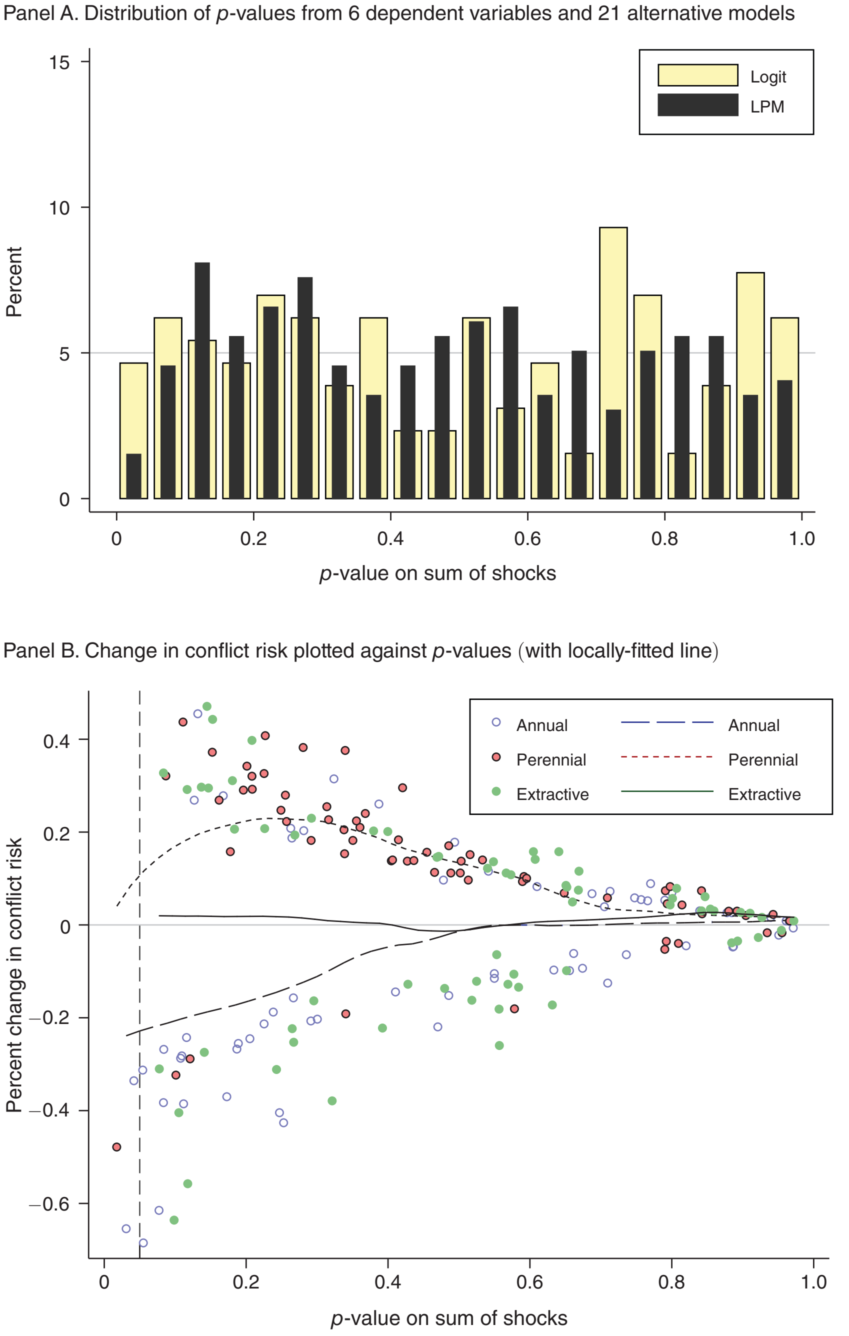


Figure 1. Robustness Analysis for Disaggregated Price Shocks and Conflict Onset

Notes: We estimate p-values and changes in conflict risk using 11 models, each by linear and logit regression, for 6 conflict measures, with shocks disaggregated by commodity. The frequency diagram (panel A) has bins of width 0.05, and a horizontal line represents the uniform distribution. The scatter plot (panel B) 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.

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). We specified these subgroups before doing the analysis based on our assessment of the most theoretically relevant risk factors.[27] We report every subgroup analyzed.

Figure 2 performs the same graphical robustness analysis as in Figure 1 but on regime subsamples. To address concerns about reverse causality, we define the year t sample as countries with the given regime type in $t - 3$.[28] Panel A graphs annual shocks and panel B graphs extractive shocks. We see no evidence of a robust relationship, and, hence, little 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 nondemocracies 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, even without a correction for multiple hypothesis testing. Estimates for unconstrained executives and PDF regimes point in the "right" direction for the state prize theory (higher prices correlate with more conflict) but are not robust.

Finally, other scholars emphasize that latent social conflict and societal fractures are a crucial 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. Commonly cited conflict risk factors include ethnic dominance (Fearon, Kasara, and Laitin 2007; Goldstone et al. 2010) and ethnic polarization (Esteban and Ray 2011; Esteban, Mayoral, and Ray 2012). Absolute and relative levels of deprivation (i.e., inequality and poverty) are also commonly cited predictors of conflict, and are associated with weak states and grievances, especially in the case literature (Blattman and Miguel 2010). Figure 3 performs the same analysis as in Figure 2 for three other high-risk subsamples: countries with high (above median) levels of ethnic polarization, above median levels of inequality, and below median levels of GDP per capita.[29] It also looks at sub-Saharan Africa (SSA) alone. The results in Figure 3 look very similar to those from Figure 1 (panel B), and are no more robust in spite of being estimated on more plausibly relevant subsamples.

B. *Conflict Continuation and Ending*

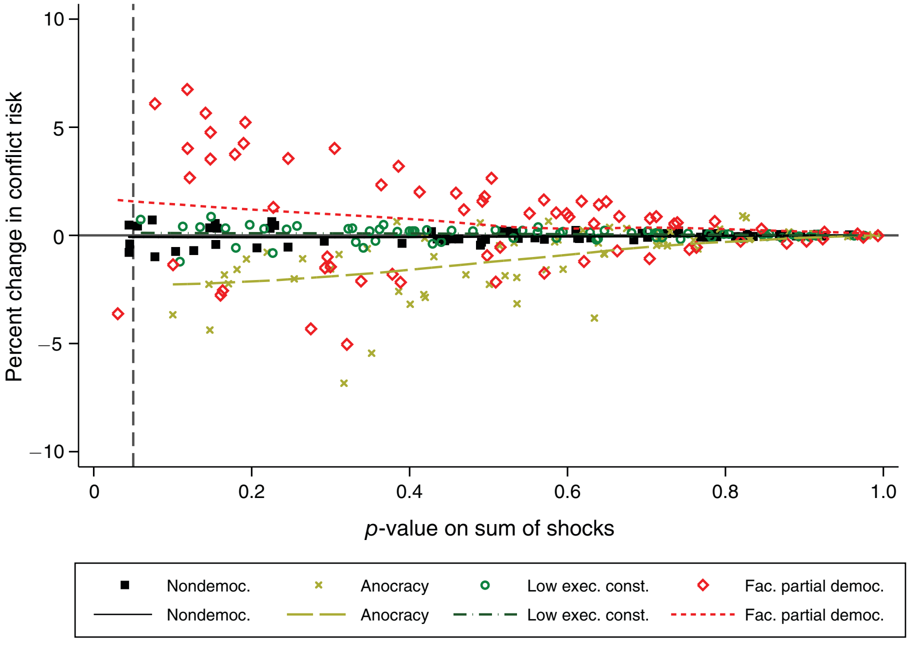
We perform identical analysis for conflict ending (i.e., peace onset) in Tables 4 and 5, using equation (2b). Table 4 examines the aggregate shock. Ignoring the

[27] Another form of subgroup analysis suggested to us uses typologies of conflict (e.g., center- and autonomy-seeking conflicts, or identity-based conflicts versus others). We have limited the analysis here to pre-specified subgroups only, and leave this for future research.

[28] Longer lags diminish the sample size dramatically. However, regime type is not associated with lagged shocks (results available upon request). This suggests that a deep lag of regime type can be used as a predetermined source of risk for our purposes.

[29] 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. Polarization figures come from Montalvo and Reynal-Querol (2005). For inequality we use Gini coefficients from UNU-WIDER (2008). GDP per capita comes from World Bank (2009).

Panel A. "Annual agricultural" price shocks and conflict onset



Panel B. "Extractive" price shocks and conflict onset

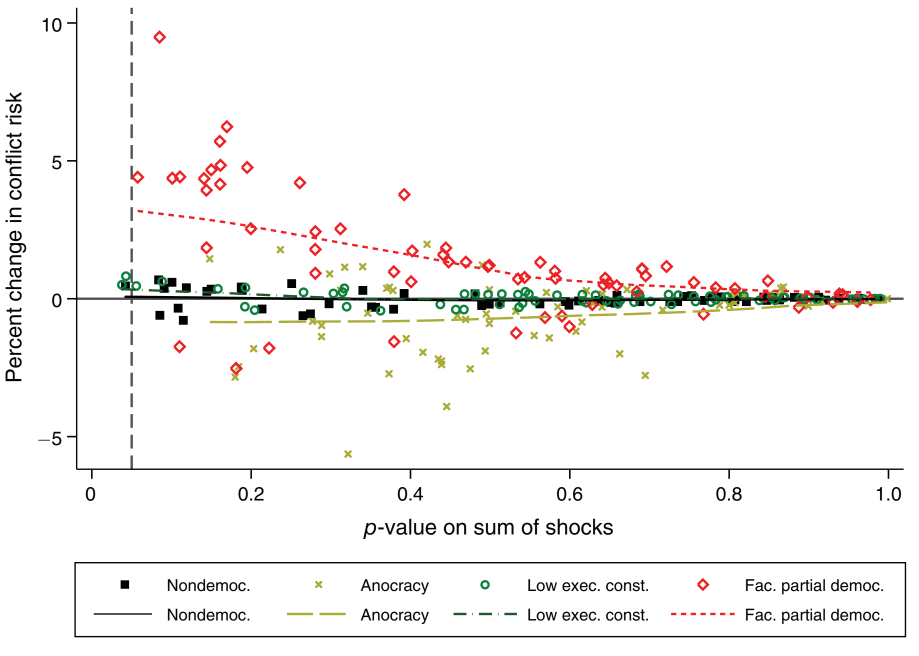
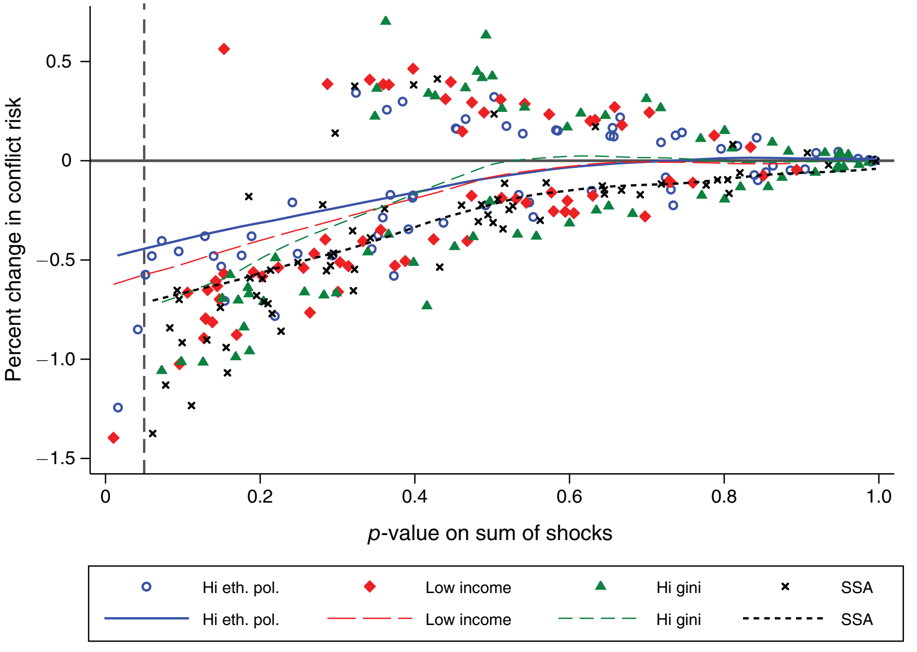


Figure 2. Disaggregated Price Shocks and Conflict Onset—Regime Subsamples

Notes: We estimate p-values and changes in conflict risk (based on the sum of current and lagged price shocks) using 11 models, each by linear and logit regression, for 6 measures of conflict, by regime type. Panel A examines annual crop prices, and panel B prices of extractive (mineral and fuel) commodities. In each panel, there is 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 $p = 0.05$.

Panel A. "Annual agricultural" price shocks and conflict onset



Panel B. "Extractive" price shocks and conflict onset

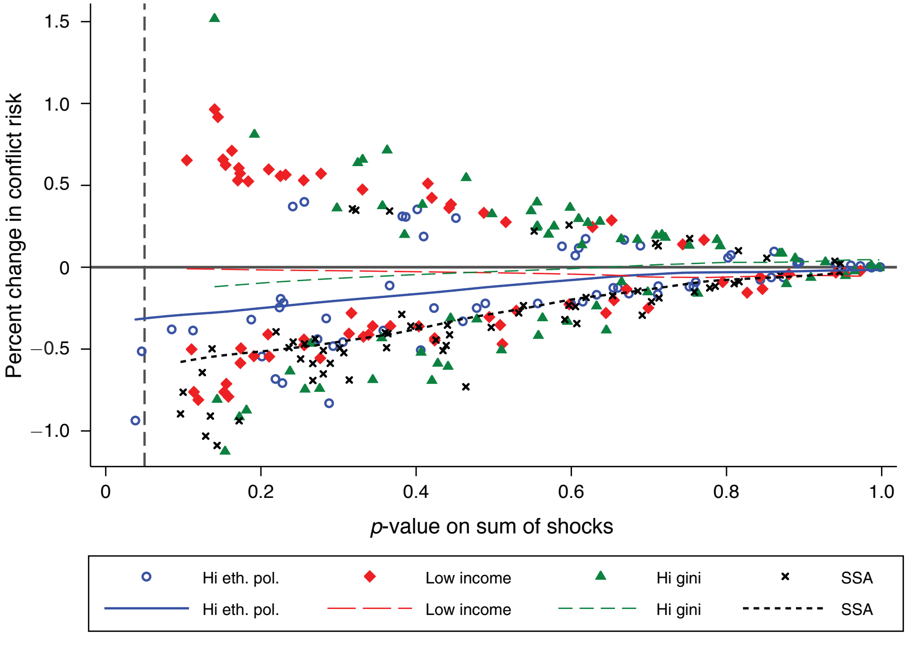


Figure 3. Disaggregated Price Shocks and Conflict Onset—"High Risk" Subsamples

Notes: We estimate p-values and changes in conflict risk (based on the sum of current and lagged price shocks) using 11 models, each by linear and logit regression, for 6 measures of conflict, by conflict risk factor. Panel A examines annual crop prices and panel B prices of extractive (mineral and fuel) commodities. In each panel, there is 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 4—IMPACTS OF AGGREGATE PRICE SHOCKS ON CONFLICT ENDING

	Dependent variable: Indicator for ending					
	UCDP/PRIO Civil War data			Other Civil War datasets		
	Low (1)	High cum. (2)	High (3)	FL (4)	S (5)	COW (6)
Panel A. No consumption shocks						
Price shock, t	0.0119	0.0284	0.0378	−0.0131	−0.0168	0.0644
	(0.0181)	(0.0184)	(0.0378)	(0.0180)	(0.0143)	(0.0287)**
Price shock, $t-1$	−0.0002	0.0310	−0.0155	−0.0085	0.0103	0.0650
	(0.0265)	(0.0211)	(0.0534)	(0.0141)	(0.0176)	(0.0338)*
Price shock, $t-2$	−0.0344	−0.0031	0.1060	−0.0112	−0.0194	0.0273
	(0.0264)	(0.0252)	(0.0428)**	(0.0148)	(0.0151)	(0.0403)
Sum of all shocks	−0.023	0.056	0.128	−0.033	−0.026	0.157
p-value of sum	[0.617]	[0.176]	[0.211]	[0.223]	[0.385]	[0.053]**
Impact of shocks on risk (%Δ)	−0.141	0.515	0.503	−0.554	−0.295	0.821
Observations	995	749	353	1,013	907	665
R^2	0.207	0.255	0.355	0.256	0.283	0.293
Number of countries	83	52	42	56	61	59
Mean of dependent variable	0.161	0.109	0.255	0.059	0.088	0.191
Panel B. With consumption shocks						
Price shock, t	0.0338	0.0366	0.0843	0.0045	0.0098	0.0502
	(0.0281)	(0.0191)*	(0.0648)	(0.0255)	(0.0270)	(0.0507)
Price shock, $t-1$	0.0418	0.0324	−0.0581	−0.0072	0.0314	0.0743
	(0.0378)	(0.0194)	(0.0745)	(0.0216)	(0.0246)	(0.0471)
Price shock, $t-2$	−0.0084	0.0255	0.1240	0.0052	−0.0081	0.0106
	(0.0235)	(0.0284)	(0.0516)**	(0.0159)	(0.0156)	(0.0506)
Sum of all shocks	0.067	0.095	0.150	0.003	0.033	0.135
p-value of sum	[0.241]	[0.020]**	[0.139]	[0.948]	[0.488]	[0.194]
Impact of shocks on risk (%Δ)	0.418	0.864	0.589	0.043	0.376	0.708
Observations	995	749	353	1,013	907	665
R^2	0.296	0.322	0.467	0.302	0.319	0.336
Number of countries	83	52	42	56	61	59
Mean of dependent variable	0.161	0.109	0.255	0.059	0.088	0.191

Note: All regressions use a linear probability model and include year fixed effects, country fixed effects, and country-specific.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

consumption shock (panel A), the effect on conflict ending is ambiguous: the sum of the coefficients is positive half the time and negative the other, and is only statistically robust in one instance—the COW measure.

When we account for the consumption shock (panel B), the aggregate price shock coefficients tend to increase in size and have a more consistently positive sign. This is what we would expect if rising consumer prices (i.e., falling real wages) increase individual incentives to rebel (as omitting this consumption shock thus biases our coefficients downwards)—evidence itself that is suggestive of the opportunity cost mechanism at work. The percentage change in "risk of peace" represented by the sums is extremely large, especially for the most episodic measures of civil

Table 5—The Impact of Disaggregated Commodity Price Shocks on Conflict Ending

	Dependent variable: Indicator for ending					
	UCDP/PRIO Civil War data			Other Civil War datasets		
	Low (1)	High cum. (2)	High (3)	FL (4)	S (5)	COW (6)
Panel A. No consumption shocks						
Annual crop shock						
Sum of all price shock coefficients	−0.047	0.069	0.222	−0.046	−0.029	0.232
p-value of sum	[0.425]	[0.300]	[0.138]	[0.165]	[0.442]	[0.004]***
Impact of shocks on risk (%Δ)	−0.297	0.631	0.871	−0.772	−0.331	1.213
Perennial crop shock						
Sum of all price shock coefficients	0.012	0.075	0.190	−0.017	−0.026	0.173
p-value of sum	[0.778]	[0.029]**	[0.023]**	[0.597]	[0.364]	[0.005]***
Impact of shocks on risk (%Δ)	0.071	0.682	0.745	−0.285	−0.291	0.905
Extractive crop shock						
Sum of all price shock coefficients	−0.038	0.079	0.206	−0.043	−0.021	0.268
p-value of sum	[0.578]	[0.252]	[0.250]	[0.265]	[0.643]	[0.004]***
Impact of shocks on risk (%Δ)	−0.238	0.718	0.807	−0.717	−0.235	1.406
Observations	995	749	353	1,013	907	665
R^2	0.212	0.259	0.379	0.260	0.286	0.309
Number of countries	83	52	42	56	61	59
Mean of dependent variable	0.161	0.109	0.255	0.087	0.08	0.191

(*Continued*)

war: UCDP/PRIO and COW. By these measures, a standard deviation increase in commodity prices almost doubles the chance that a civil war will end. However, few of these sums are significant at conventional levels. Only the UCDP/PRIO measure of war is (more or less) consistently significant.

Results are more robust if we look at disaggregated export price shocks, in Table 5. Comparing results without and with the consumption shock (panel A to panel B), we again see sums of coefficients becoming larger and consistently positive once the endogeneity is reduced. This is true across all commodity classes. Again, the percentage change in conflict risk represented by these results is also very large, with the exception of the least episodic Fearon and Laitin measure of war. These impacts are large and robust for only two measures of war, UCDP/PRIO and COW, and so must be treated with caution. But both are the two most episodic measures of civil war and, hence, likely to be the most responsive. A one standard deviation increase in prices in any commodity class is associated with a doubling of the likelihood the civil war ends. Before dwelling on these results, are they robust enough to merit rationalization?

Systematic Robustness and Heterogeneity Analysis.—Figure 4 applies the robustness analysis to conflict endings. The distribution of p-values is skewed toward the left now, with roughly 20 percent significant at the 5 percent level and 30 percent

Table 5—The Impact of Disaggregated Commodity Price Shocks on Conflict Ending (*Continued*)

	Dependent variable: Indicator for ending					
	UCDP/PRIO Civil War data			Other Civil War datasets		
	Low (1)	High cum. (2)	High (3)	FL (4)	S (5)	COW (6)
Panel B. With consumption shocks						
Annual crop shock						
Sum of all price shock coefficients	0.062	0.113	0.198	−0.006	0.030	0.249
p-value of sum	[0.366]	[0.076]*	[0.147]	[0.894]	[0.632]	[0.028]**
Impact of shocks on risk (%Δ)	0.388	1.033	0.777	−0.107	0.345	1.303
Perennial crop shock						
Sum of all price shock coefficients	0.075	0.100	0.184	0.011	0.016	0.164
p-value of sum	[0.206]	[0.027]**	[0.105]	[0.776]	[0.688]	[0.032]**
Impact of shocks on risk (%Δ)	0.467	0.914	0.723	0.191	0.184	0.861
Extractive crop shock						
Sum of all price shock coefficients	0.096	0.133	0.205	0.0001	0.051	0.284
p-value of sum	[0.266]	[0.045]**	[0.206]	[0.999]	[0.499]	[0.029]**
Impact of shocks on risk (%Δ)	0.599	1.216	0.804	−0.001	0.580	1.486
Observations	995	749	353	1,013	907	665
R^2	0.452	0.484	0.673	0.459	0.498	0.513
Number of countries	83	52	42	56	61	59
Mean of dependent variable	0.161	0.109	0.255	0.087	0.08	0.191

Notes: All regressions use a linear probability model and include year fixed effects, country fixed effects, and country-specific time trends. Robust standard errors in parentheses, clustered by country. All regressions in panel B also include interactions of country fixed effects and terms capturing the contemporaneous, once-lagged, and twice-lagged sum of price shocks for a bundle of imported foods, oil, and gas commodities common across countries. Dataset abbreviations are as follows: FL = Fearon and Laitin, S = Sambanis, COW = Correlates of war, and PT = Powell and Thyne.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

at the 10 percent level of significance or more (panel A). The effects are more uniformly positive (panel B).

Which specifications tend to produce more significant results? We take the *p*-values from Figure 4 and analyze their correlates in Table 6, using each robustness regression (by commodity class) as an observation. We regress the *p*-value on indicators for the dependent variable used or the specification employed, examining war onset and ending separately. Positive coefficients imply the variable or specification choice reduces statistical significance, while negative coefficients imply that statistical significance is improved. The data are not independently distributed, and so we should view 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 magnitude of coefficients are informative.

First, note that our preferred specification (equation (2)) provides some of the most efficient estimates. Thus, the absence of any robust result in Tables 2 and 3

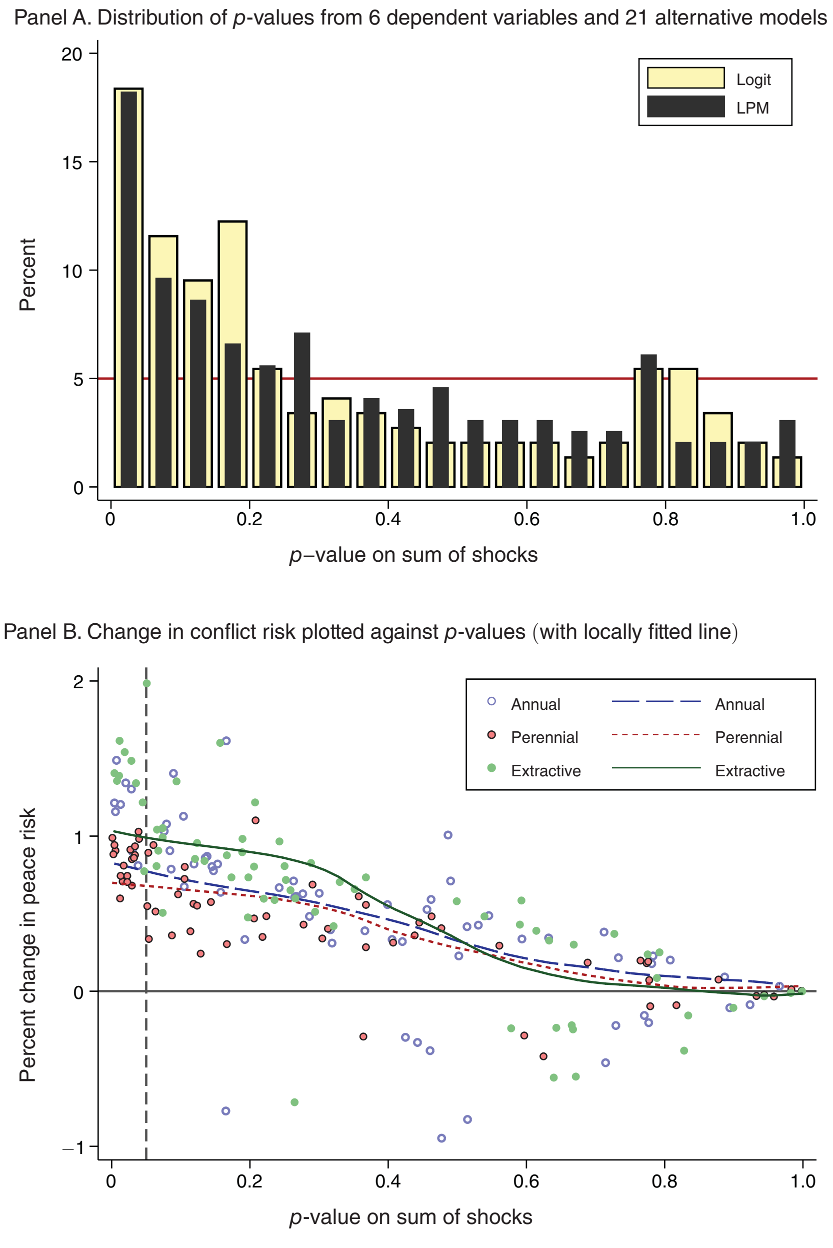


FIGURE 4. ROBUSTNESS ANALYSIS FOR DISAGGREGATED PRICE SHOCKS AND CONFLICT ENDING

Notes: We estimate p-values and changes in conflict risk using 11 models, each by linear and logit regression, for 6 measures of conflict, with shocks disaggregated by commodity. The frequency diagram (panel A) has bins of width 0.05, and a horizontal line represents the uniform distribution. The scatter plot (panel B) 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.

(shocks on conflict onset) is unlikely to change in any alternative specification. The somewhat robust estimates presented in Tables 4 and 5 provide a best-case scenario. The exception is the logit versus linear probability model choice—logit is less efficient, on average, with onsets, but slightly more efficient with ending.

Table 6—Impact of Model Assumptions on p-Values in Robustness Analysis

Covariate	Dependent variable: p-value on sum of price shocks[a] War onset	War ending
Logit model	0.0395	−0.0170
	(0.0314)	(0.0278)
Annual crop price shock	−0.0075	−0.0117
	(0.0352)	(0.0328)
Perennial crop price shock	−0.0727	−0.1040
	(0.0338)**	(0.0326)***
Not including consumption shocks	0.0096	−0.0009
	(0.0814)	(0.0563)
Not including country fixed effects	−0.0397	0.0179
	(0.0606)	(0.0548)
Not including year fixed effects	0.0531	−0.0247
	(0.0664)	(0.0432)
Not including country-specific time trends[b]	0.0105	−0.0043
	(0.0622)	(0.0576)
Including fixed weights	−0.0384	0.2495
	(0.0680)	(0.0622)***
Not including exports/GDP adjustment	0.0154	−0.0065
	(0.0671)	(0.0545)
Censoring price outliers	−0.0167	0.0616
	(0.0621)	(0.0578)
Including all price-makers	−0.0471	0.0605
	(0.0685)	(0.0568)
Using 3 percent price-maker cutoff	−0.0453	0.1895
	(0.0626)	(0.0602)***
Using 20 percent price-maker cutoff	−0.0029	0.0219
	(0.0699)	(0.0538)
COW dependent variable (DV)	−0.3402	−0.2896
	(0.0385)***	(0.0391)***
Fearon and Laitin DV	−0.0985	0.0804
	(0.0470)**	(0.0558)
UCDP/PRIO High cum. DV	−0.1541	−0.2400
	(0.0441)***	(0.0395)***
UCDP/PRIO High DV	−0.2918	−0.1627
	(0.0447)***	(0.0443)***
Sambanis DV	−0.0814	0.0503
	(0.0593)	(0.0546)
Constant	0.6824	0.4254
	(0.0527)***	(0.0496)***
Observations[c]	327	351
R^2	0.214	0.343

Notes: Robust standard errors in parentheses.

[a] The dependent variable is the p-value on the sum of all three price shocks (disaggregated by commodity type) for three commodity types (annual, perennial and extractive) in 126 alternative regressions based on 6 alternative dependent variables, 2 estimators (linear and logit) and 10 alternative models. Each independent variable is an indicator for the estimator, model assumption, or dependent variable.

[b] Linear probability model only. The logit estimates exclude these, or the estimation will not converge.

[c] The logit estimator does not converge in 16 (11) cases for onset (ending).

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

The episodic, high-intensity war measures from UCDP-PRIO and COW are more sensitive to price shocks than other measures. Using these dependent variables alone, the positive impact of prices on war ending is robust. The nonrobust observations in Figure 4 are driven primarily by other, less episodic measures of civil war.[30] This suggests that a finer measure of episodic conflict, in particular the intensity of the warfare, may produce more robust results. Note, however, 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.

C. *Conflict Intensity* (*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. The fact that we see the most robust results with the most episodic measures of ongoing conflict suggests that price shocks may be influencing the intensity of existing war rather than the onset of new ones or complete conflict ending. If so, the ending indicators above would measure this latent relationship with error, leading to large coefficients imprecisely estimated. Rather, we should be looking at intensity directly.

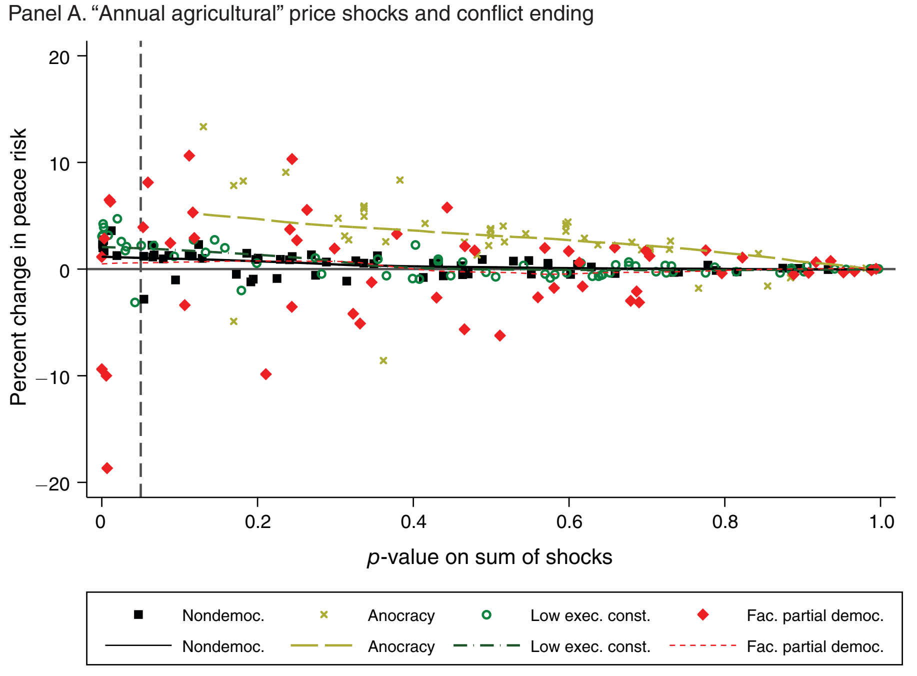
We turn to the sole available cross-national data on conflict intensity, from battle deaths. Table 7 displays results from equation (3a) (without lagged battle deaths) and equation (3b) (with a lagged dependent variable).[31] Estimates in panel B include the consumption shock.

Overall, the dynamic results suggest a large (but somewhat fragile) inverse relationship between current-year price shocks and intensity—higher prices are associated with fewer battle deaths, mainly in the current year. A standard deviation rise in current export prices is associated with 377 fewer battle deaths in the static linear specification (column 1), and 754 fewer deaths in the dynamic specification (column 2). The latter represents a 15 percent 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 toward 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

[30] The robustness of most results, however, generally suffers if we use fixed export commodity weights or a lower price maker cut-off. These differences do not reflect measurement error so much as a different definitions of war. Indeed, relative to most cross-national measures one could argue war is relatively accurately measured. See the online Appendix for an illustration of the underlying robustness checks with one of the most robust dependent variables, UCDP/PRIO cumulatively high intensity warfare. The magnitudes of the sums of shocks are consistently large, but robustness is somewhat sensitive to simple model changes.

[31] 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 (3b) 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.



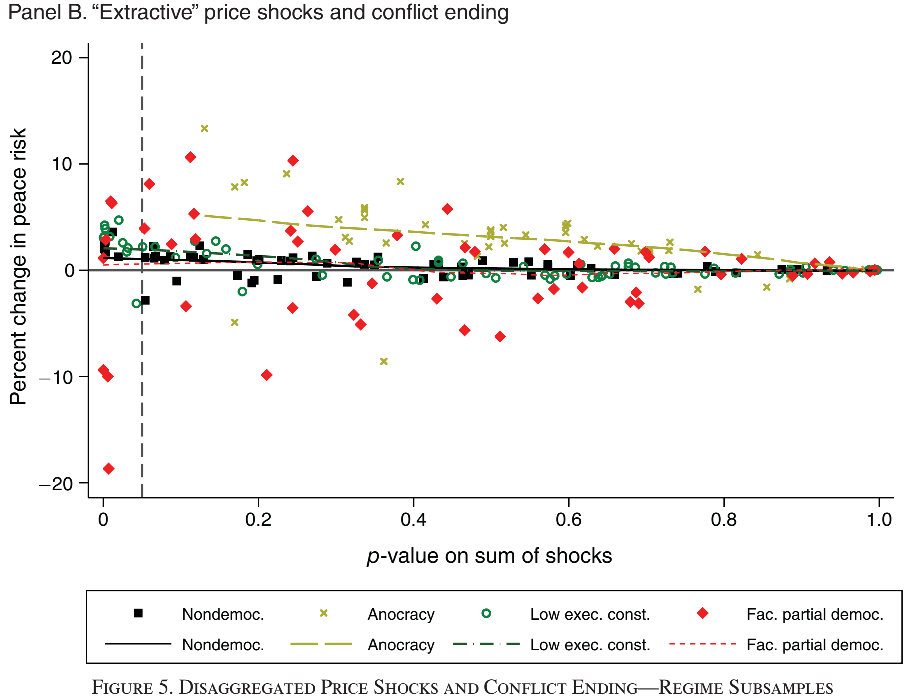
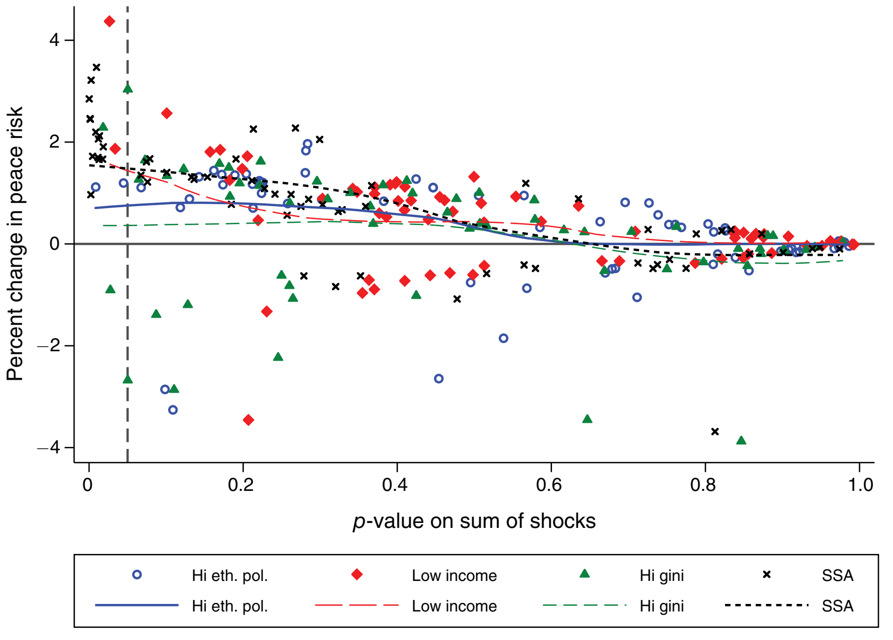


Figure 5. Disaggregated Price Shocks and Conflict Ending—Regime Subsamples

Notes: We estimate p-values and changes in conflict risk (based on the sum of current and lagged price shocks) using 11 models, each by linear and logit regression, for 6 measures of conflict, by regime type. Panel A examines annual crop prices and panel B prices of extractive (mineral and fuel) commodities. In each panel, there is 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 $p = 0.05$. One extreme outlier is removed from each figure for presentational purposes.

Panel A. “Annual agricultural” price shocks and conflict ending



Panel B. “Extractive” price shocks and conflict ending

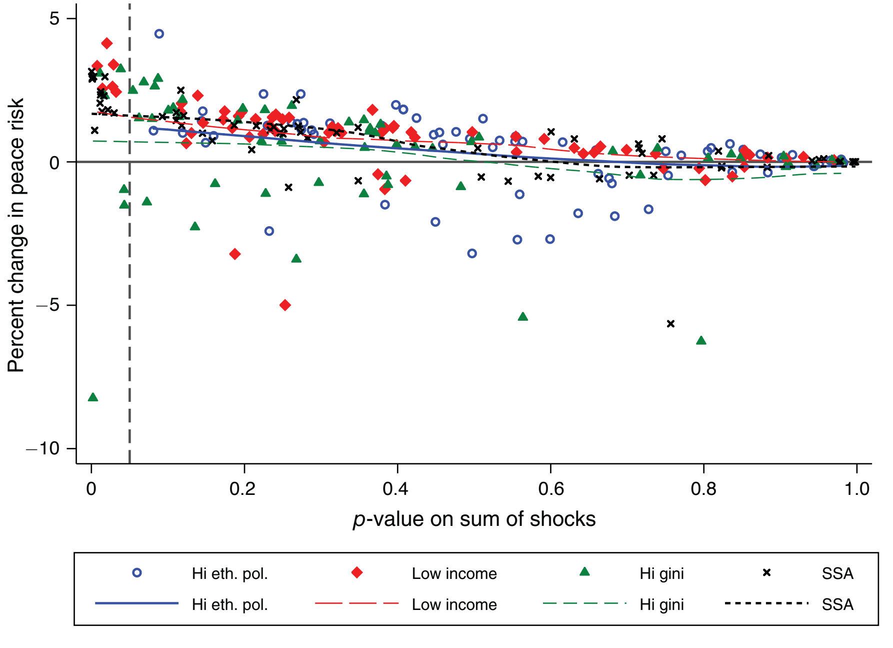


Figure 6. Disaggregated Price Shocks and Conflict Ending—“High Risk” Subsamples

Notes: We estimate p-values and changes in conflict risk (based on the sum of current and lagged price shocks) using 11 models, each by linear and logit regression, for 6 measures of conflict, by conflict risk factor. Panel A examines annual crop prices, and panel B prices of extractive (mineral and fuel) commodities. In each panel, there is 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 $p = 0.05$. One extreme outlier is removed from each figure for presentational purposes.

Table 7—The Impact of Aggregated Commodity Price Shocks on Battle Deaths

	Dependent variable: No. of battle deaths			Dependent variable: ln(battle deaths)		
	Static (no lagged DV) (1)	Dynamic (with lagged DV) (2)	Omitting nonannual deaths data (3)	Static (4)	Dynamic (5)	Omitting nonannual deaths data (6)
Panel A. No consumption shocks						
Price shock, t	−377.2	−754.1	−437.6	−0.158	−0.203	−0.158
	(512.0)	(280.2)***	(315.5)	(0.112)	(0.076)***	(0.104)
Price shock, $t-1$	−50.7	436.7	205.3	−0.086	−0.027	−0.104
	(460.4)	(432.6)	(305.5)	(0.132)	(0.131)	(0.103)
Price shock, $t-2$	77.5	25.0	161.7	−0.134	−0.135	−0.089
	(604.5)	(395.1)	(559.7)	(0.152)	(0.124)	(0.143)
Duration	−66.1	−42.9	−14.9	0.007	0.009	0.012
	(50.8)	(26.5)	(19.4)	(0.015)	(0.013)	(0.015)
Indicator for first year of conflict	−2,508.5	422.4	540.4	−1.278	−0.923	−0.912
	(750.0)***	(649.1)	(511.1)	(0.205)***	(0.210)***	(0.251)***
Lagged battle deaths		0.736	0.909		0.0001	0.0001
		(0.138)***	(0.025)***		(0.0000)***	(0.0000)***
Sum of all shocks	−350.4	−292.4	−70.6	−0.378	−0.365	−0.351
p-value of sum	[0.813]	[0.731]	[0.942]	[0.303]	[0.210]	[0.228]
Impact of all shocks on risk (%Δ)	−0.068	−0.057	−0.018	−0.054	−0.052	−0.052
Observations	1,009	1,009	690	1,009	1,009	690
Mean of dependent variable	5,159	5,159	4,016	7.065	7.065	6.706
Number of countries	82	82	74	82	82	74

(*Continued*)

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 small 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 conventional significance levels when estimating the dynamic model (column 5).

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 change easily, and none are significant. The sum of the shock and two lags are generally negative, but never significantly so.

Table 8 relates battle deaths to disaggregated price shocks, including country-specific consumption shocks (see online Appendix for results without this shock). Coefficients on annual crop and extractive commodity shocks are at least as large and typically larger. The negative effect of current price increases on linear battle deaths is robust only for the dynamic specification with all years of data (column 2). The logarithmic specifications are generally more statistically significant. Omitting years without annual deaths data reduces the size and significance with linear battle deaths. The inverse relationship between current price shocks and

Table 7—The Impact of Aggregated Commodity Price Shocks on Battle Deaths (*Continued*)

	Dependent variable: No. of battle deaths			Dependent variable: ln(battle deaths)		
	Static	Dynamic	Omitting nonannual deaths data	Static	Dynamic	Omitting nonannual deaths data
Panel B. With consumption shocks						
Price shock, t	−374.5	−779.1	−320.7	−0.200	−0.248	−0.196
	(430.3)	(315.7)**	(337.6)	(0.093)**	(0.073)***	(0.101)*
Price shock, $t-1$	−249.4	357.3	42.2	−0.154	−0.081	−0.181
	(459.4)	(432.6)	(318.0)	(0.122)	(0.121)	(0.102)*
Price shock, $t-2$	−27.1	−33.5	153.1	−0.148	−0.143	−0.096
	(586.2)	(381.5)	(550.2)	(0.141)	(0.113)	(0.131)
Duration	−59.7	−41.2	−14.0	0.008	0.010	0.011
	(51.1)	(26.9)	(19.5)	(0.014)	(0.013)	(0.015)
Indicator for first year of conflict	−2562.5	384.0	514.0	−1.289	−0.934	−0.932
	(746.9)***	(654.2)	(471.3)	(0.198)***	(0.204)***	(0.240)***
Lagged battle deaths		0.732	0.904		0.0001	0.0001
		(0.139)***	(0.026)***		(0.0000)***	(0.0000)***
Sum of all shocks	−651.0	−455.3	−125.4	−0.502	−0.472	−0.474
p-value of sum	[0.631]	[0.563]	[0.895]	[0.110]	[0.056]*	[0.069]*
Impact of all shocks on risk (%Δ)	−0.126	−0.088	−0.031	−0.071	−0.067	−0.071
Observations	1,009	1,009	690	1,009	1,009	690
Mean of dependent variable	5,159	5,159	4,016	7.065	7.065	6.706
Number of countries	82	82	74	82	82	74

Notes: All regressions use a maximum likelihood interval regression model and include year and region fixed effects. Robust standard errors are clustered by country. All regressions in panel B also include interactions of region fixed effects and terms capturing the contemporaneous, once-lagged, and twice-lagged sum of price shocks for a bundle of imported foods, oil, and gas commodities common across countries.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

current battle deaths holds for all three commodity classes. The relationship is strongest for annual crops and extractive commodities. Perennial crops have the same effect, but it is smaller and less robust. These results complement evidence from Colombia that rises in the coffee price are associated with fewer military attacks in coffee-producing regions (Dube and Vargas 2014). It runs against the same paper's finding, however, that oil price increases raise attacks.

V. Discussion

We draw several conclusions from these results. First, we see little support for the idea of a state as a prize, even when looking at the most amenable 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 temporary shocks are the wrong test of the state prize theory. Stocks of resources could matter more than price shocks. But combined with emerging evidence that war onset is no more likely even with

TABLE 8—THE IMPACT OF DISAGGREGATED COMMODITY PRICE SHOCKS ON BATTLE DEATHS, WITH IMPORT SHOCK

	Linear battle deaths			Natural log of battle deaths		
	Static (1)	Dynamic (2)	Omitting nonannual deaths data (3)	Static (4)	Dynamic (5)	Omitting nonannual deaths data (6)
Annual crop price shock, t	−782.0	−1,174.3	−799.6	−0.266	−0.315	−0.227
	(679.8)	(482.0)**	(574.2)	(0.154)*	(0.130)**	(0.157)
Annual crop price shock, $t-1$	−369.5	290.0	−114.4	−0.187	−0.107	−0.227
	(544.2)	(518.5)	(415.2)	(0.148)	(0.146)	(0.131)*
Annual crop price shock, $t-2$	−726.2	−331.8	−280.4	−0.278	−0.223	−0.184
	(742.7)	(467.8)	(669.0)	(0.187)	(0.147)	(0.183)
Perennial crop price shock, t	−184.2	−489.6	−81.6	−0.178	−0.215	−0.169
	(462.6)	(306.6)	(274.0)	(0.096)*	(0.083)***	(0.090)*
Perennial crop price shock, $t-1$	−26.1	412.2	215.0	−0.120	−0.067	−0.133
	(441.1)	(361.0)	(273.1)	(0.110)	(0.105)	(0.093)
Perennial crop price shock, $t-2$	491.1	391.1	542.2	−0.032	−0.034	−0.010
	(552.8)	(415.8)	(509.8)	(0.127)	(0.110)	(0.112)
Mineral, oil & gas price shock, t	−582.4	−1,176.5	−613.1	−0.271	−0.344	−0.266
	(659.2)	(491.0)**	(585.3)	(0.136)**	(0.104)***	(0.155)*
Mineral, oil & gas price shock, $t-1$	−402.5	492.8	−133.0	−0.215	−0.109	−0.260
	(726.1)	(695.3)	(523.7)	(0.182)	(0.184)	(0.152)*
Mineral, oil & gas price shock, $t-2$	−363.7	−371.6	−194.6	−0.294	−0.290	−0.218
	(988.0)	(569.3)	(811.3)	(0.230)	(0.179)	(0.218)
Duration	−57.8	−40.8	−13.6	0.008	0.010	0.011
	(51.3)	(27.6)	(19.7)	(0.015)	(0.013)	(0.015)
Indicator for first year of conflict	−2,647.8	294.9	416.2	−1.309	−0.955	−0.951
	(765.7)***	(656.4)	(471.4)	(0.199)***	(0.203)***	(0.239)***
Lagged battle deaths		0.729	0.900		0.0001	0.0001
		(0.1369)***	(0.0263)***		(0.0000)***	(0.0000)***
Annual crop shock						
Sum of all price shock coefficients	−1,878	−1,216	−1,194	−0.730	−0.645	−0.638
p-value of sum	[0.309]	[0.278]	[0.376]	[0.094]*	[0.067]*	[0.079]*
Impact of shocks on risk (%Δ)	−0.364	−0.236	−0.297	−0.103	−0.091	−0.095
Perennial crop shock						
Sum of all price shock coefficients	280.8	313.7	675.6	−0.330	−0.316	−0.312
p-value of sum	[0.823]	[0.693]	[0.421]	[0.267]	[0.213]	[0.197]
Impact of shocks on risk (%Δ)	0.0544	0.0608	0.168	−0.047	−0.045	−0.047
Extractive crop shock						
Sum of all price shock coefficients	−1,349	−1,055	−940.7	−0.780	−0.743	−0.743
p-value of sum	[0.547]	[0.437]	[0.564]	[0.104]	[0.048]*	[0.065]*
Impact of shocks on risk (%Δ)	−0.261	−0.205	−0.234	−0.110	−0.105	−0.111
Observations	1,009	1,009	690	1,009	1,009	690
Mean of dependent variable	5,159	5,159	4,016	7.065	7.065	6.706
Number of countries	82	82	74	82	82	74

Notes: All regressions use a maximum likelihood interval regression model and include year and region fixed effects. Robust standard errors are clustered by country.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

rapid increases in known oil reserves (Cotet and Tsui 2013; Humphreys 2005), we regard the state prize logic of war (and the general equilibrium predictions of the opportunity cost theory) with skepticism, at least as a systematic or average description of the causes of war.

If anything, the evidence tips in the other direction, toward a "state capacity" effect. State prize models assume that rising revenues raise the value of capturing the state, but have ignored or downplayed the effect of revenues on self-defense. We noted that a growing empirical political science literature takes just such a revenue-centered approach, illustrating that resource boom times permit both payoffs and repression, and that stocks of lootable or extractive resources can bring political order and stability (Smith 2004). This countervailing effect is most likely with transitory shocks, as current revenues are affected, while long-term value is not. Our findings are consistent with this view. For example, conflict intensity is most sensitive to changes in the extractive commodities rather than the annual agricultural crops that directly affect household incomes. 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.

Finally, the inverse relationship between prices and war intensity is consistent with opportunity cost accounts, but not exclusively so. The relationship between intensity and extractive commodity prices is more consistent with the state capacity view. Moreover, the inverse relation between individual aggression and incomes is consistent with theories of relative deprivation (e.g., Gurr 1971). Micro-empirical work is needed to distinguish between these mechanisms.

Ultimately, however, the fact that commodity price shocks bear little systematic relation to new conflict onsets but have some effect on ongoing conflict, suggests that political stability might be less sensitive to income or temporary shocks than generally believed. 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 comprehensive, and plausibly exogenous source of high frequency income variation than price volatility. One explanation for these null results is that successfully mounting an insurgency is no easy task. It comes with considerable risk, costs, and coordination challenges. If these are met, as in an ongoing conflict, then price shocks may have a material, robust relationship with mobilization into war. Otherwise the effect may be indirect and weak.

As we noted above, however, export commodity price shocks help us estimate a local average treatment effect on households and states that are somewhat integrated into export trade. Many conflicts originate in poorly integrated peripheries, and external shocks (to export prices or food prices) may not provide meaningful variation in local incomes. Other local shocks to income, such as climate, will help identify opportunity cost and related individual mechanisms in these more isolated places (Harari and La Ferrara 2013).

A final possibility is that the counterfactual is still conflict onset. In fragile nations, income shocks are ubiquitous. If a nation is so fragile that a change in prices leads 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.[32]

We see several directions for future research. First, more quantitative country case studies such as Colombia are needed. Second, better cross-national intensity data is required. Third, it is crucial to test competing theories rather than simply to identify reduced-form effects. Theorists should focus on the predictions that distinguish between competing accounts, rather than confirming evidence alone.

Our aim is broader than questioning the result between price shocks and conflict. Beyond the usual concerns about a "file drawer problem" of unpublished statistically insignificant results (Rosenthal 1979), we are concerned that the literature on political instability has a high risk of publication bias. Ioannidis (2005) demonstrates that a published finding is less likely to be true when (i) there is a greater number and lesser preselection of tested relationships, (ii) there is greater flexibility in designs, definitions, outcomes, and models, and (iii) more teams are involved in the chase of statistical significance. Perhaps as a result, 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 amenable to testing for equilibrium changes in conflict as we do here.

It is unrealistic to expect the profession to ex ante specify nonexperimental tests and models, let alone preregister hypotheses and methods. Nevertheless, there are a few steps that can be taken that minimize the risk of research and publication bias, such as a systematic look at alternative dependent variables, rigorous robustness checks, splitting datasets into training and testing samples, and systematic selection of country cases. For existing studies, we offer a simple approach to robustness checks. Empirical studies of conflict are sufficiently important for academic theory and real world policy that no less should be expected.

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[32] A case literature illustrates the damaging impacts of commodity volatility in the 1980s (Bates 2008; Bevan, Collier, and Gunning 1993; Van de Walle 2001), 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).

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