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# Stimulant or depressant?
# Resource-related income shocks and conflict

Kai Gehring* Sarah Langlotz† Stefan Kienberger‡

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**Abstract**

We provide evidence on the mechanisms linking resource-related income shocks to conflict, focusing on illegal crops. We hypothesize that the degree of group competition over resources and the extent of law enforcement explain whether opportunity cost or contest effects dominate. Combining temporal variation in international drug prices with spatial variation in the suitability to produce opium, we show that higher prices increase household living standards and reduce conflict in Afghanistan. Analyzing shifts in conflict tactics and using geo-referenced data on drug production networks and territorial control highlight the importance of opportunity costs, and reveal heterogeneous effects consistent with our theory.

*Keywords:* Resources, conflict, illicit economy, geography of conflict, Afghanistan, Taliban

*JEL Classification:* D74, K42, O13, O53

*Corresponding author: University of Zurich, e-mail: *mail@kai-gehring.net*

†University of Goettingen, e-mail: *sarah.langlotz@uni-goettingen.de*

‡University of Salzburg, e-mail: *stefan.kienberger@sbg.ac.at*

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# 1. Introduction

Contests over land and resources play a critical role in internal and external conflict, as acknowledged by key actors in international politics like the United Nations or the World Bank.[1] Hence, resource-related income shocks are a crucial dimension in the economic analysis of conflict (e.g., Berman *et al.* , 2017; Bazzi & Blattman, 2014; Morelli & Rohner, 2015). Yet, we have only begun to understand the micro-foundations behind the resource-conflict-nexus.

This paper provides a new framework and data to explore the mechanisms behind the resource-conflict relationship. We apply this framework to the case of Afghanistan. Since 2001, the Afghan conflict ranks the second deadliest war worldwide, leading to more than three million internal and external refugees.[2] With estimated military and reconstruction costs of 1 billion US Dollars for the United States alone since 2001, it is also one of the most expensive wars.[3] As a consequence, Afghanistan is at the core of several recent contributions in the economics of conflict literature (e.g., Child, 2019; Condra *et al.* , 2018).

The resource we focus on in this paper is opium. In Afghanistan, opium revenues are a crucial source of income (Felbab-Brown, 2013). At least one out of seven Afghans is somehow involved in cultivation, processing, or trafficking (UNODC, 2009). The general framework economists use to understand how (resource-related) income shocks affect violent conflicts can be dated back to contributions by, among others, Grossman (1991), Collier and Hoeffler (2004;

[1] See https://www.un.org/en/land-natural-resources-confl ct/renewable-resources.shtml and https://www.usip.org/sites/default/files/file/08sg.pdf, accessed 08/28/2019.

[2] See https://reporting.unhcr.org/afghanistan, accessed 11/04/2021. According to own calculations based on the Uppsala Conflict Data Program (UCDP), Afghanistan reports about 230,000 battle-related deaths, whereas Syria reached about 330,000 between 2001-2021.

[3] See https://www.bbc.com/news/world-47391821, accessed 11/04/2021. According to the OECD, Afghanistan was always among the top five recipients of bilateral and multilateral official development aid since 2004.

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1998), and Fearon & Laitin (2003). It distinguishes between two major channels: opportunity costs and contest effects. With better outside opportunities, conflict becomes less desirable. If control over a resource becomes more profitable, contests for control intensify conflict.

Dube & Vargas (2013) highlight important differences between resources. More specifically, the higher the labor intensity, the lower the likelihood that higher prices trigger conflict. We use the case of opium-related income changes in Afghanistan to better understand the impact of *de jure* illegal crops on conflict. We demonstrate that in addition to considering differences in labor intensity, it is important to account (i) for the extent to which laws regarding the illegal production and trading of resources are enforced and (ii) for the number of groups competing for resource control.

To measure changes in opium profitability across years and districts, we combine temporal variation in international drug prices over the 2002-2014 period with a new dataset on spatial variation in opium suitability (Kienberger *et al.* , 2017). Our main reduced-form identification strategy exploits the fact that higher international prices have a larger effect in districts with higher suitability, conditional on year-fixed effects that capture aggregate supply changes. We carefully evaluate the risk of omitted variable bias stemming, among others, from potentially endogenous supply shocks that might affect high and low suitability districts differently. Moreover, we assess the risk of remaining biases by exploiting the relationship between depressant drugs like opium and stimulant drugs that are often consumed as complements.

All strategies lead to the same result: higher opium profitability consistently reduces both conflict incidence and intensity. To quantify the size of the effect, we augment this with instrumental variable (IV) estimates using international prices and changes in legal opioid prescriptions in the United States. A 10% increase in opium revenues leads to a decrease in the number of battle-related deaths of about 1.5%. We can use our estimates to predict an alternative conflict path if prices would have remained at higher levels by imposing some assumptions. If heroin prices in 2008 had been as high as in the year 2001, there could have been more than 1800 fewer battle-related deaths in the following year.

Our data allows us to show that this effect seems indeed associated with changes in opportunity costs. First, we use the National Risk and Vulnerability Assessment (NRVA) to show

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that the gains from higher opium profitability affect regular households via increases in food consumption and living standards. Second, we argue that districts, which cultivate opium in its raw form and also process and trade it, can capture a larger share of the added value along the supply chain. This creates more rents to fight about (contest) but also a stronger income effect (opportunity costs). We use geo-referenced data on drug markets, labs, and potential trafficking routes from the United Nations Office on Drugs and Crime (UNODC) and a network-based opium market access measure (similiar to Donaldson & Hornbeck, 2016) to proxy for this added value. We find more added value increases the conflict-reducing effect. Third, we use the SIGACTS (Significant Activities) data from Shaver & Wright (2016) to show that insurgents adapt their tactics by shifting towards less labor-intensive combat activities when better outside opportunities evolve. All these findings verify that, on average, opportunity cost effects dominate contest effects in Afghanistan.

The paper then turns to evaluate implications of our theoretical framework, which suggests that the net effect on conflict is influenced by two factors: (i) whether laws against the production and trading of illegal crops are enforced and (ii) how many groups are fighting for resource control. If laws are enforced, fewer people profit from higher prices, and opportunity cost effects are small. When many groups compete to control lucrative production grounds, higher resource prices are associated with larger contest effects and more conflict. Based on these two dimensions, we distinguish four theoretical scenarios.

Prior evidence on illegal crops (Dell, 2015; Angrist & Kugler, 2008) points to conflict-fueling effects of higher prices in countries like Colombia, where governments enforce laws more widely and several non-state groups are competing for control (e.g., Wright, 2018; Ibanez & Carlsson, 2010; Ibanez & Martinsson, 2013). In such a scenario our framework predicts that higher prices fuel conflict due to weak opportunity costs and strong contest effects. Our main results and existing qualitative evidence suggest that most districts in Afghanistan do not fit the same scenario. The Afghan conflict between 2001 and 2021 was best characterized as a two-sided contest between the Taliban and pro-government forces (see Trebbi & Weese, 2019) including the internationally recognized Western-backed government, international forces, and their allies. Hence, many districts that were not controlled by pro-government forces were held

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by a single insurgent group and are thus described by little competition between groups for resource control and no enforcement of official laws.

To link the districts in Afghanistan to the different combinations of law enforcement and competition between groups for resource control, we use heterogeneity in group control. We proxy for Taliban control with the presence of Pashtuns in a district and with historical Taliban control prior to 2001. Based on Michalopoulos & Papaioannou (2014) and Lind *et al.* (2014), we use distance to major cities and important foreign military bases as a proxy of government control. Districts that neither fall into Taliban or pro-government control are coded as contested.

In line with the predictions from our theoretical framework, we find that the conflict-reducing effect of higher opium prices is the strongest in districts under Taliban control. This suggests that the group is acting as a stationary bandit (Olson, 1993; De La Sierra, 2020) maximizing its revenues from taxing opium farmers. Qualitative evidence documents tax collection and the implementation of conflict-solving mechanisms to minimize violence that would potentially disturb the profitable production process (Peters, 2009). Consistent with our framework, the net effect in the other two scenarios is significantly less negative and turns out to be indistinguishable from zero.

One limitation of this paper is that we measure the effect of prices on *local* conflict. Berman *et al.* (2017) point to the feasibility effect of higher resource prices because insurgents could use the generated income from taxing opium production or trade to finance future fights. Anecdotal evidence suggests that the Taliban pool revenues through the group's central financing committee, which could be used to help finance attacks in other districts.[4] We do not know the extent of this revenue-sharing, but find no conflict-fueling effect when aggregating our data to the larger province level. During our sample period, there is also a negative correlation between prices and conflict at the aggregate country level.

[4] See, e.g., http://www.huffingtonpost.com/joseph-v-micallef/how-the-Taliban-gets-its_b_-8551536.html, accessed 06/14/2018.

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# 2. Literature and theoretical considerations

## A. Contributions to literature

We contribute to various strands of literature. First, our paper relates to the large literature on resource-related income shocks and conflict (e.g., Blattman & Miguel, 2010; Collier & Hoeffler, 2004; Fearon & Laitin, 2003). Studies at the cross-country macro level (e.g., Bazzi & Blattman, 2014; Miguel *et al.* , 2004) and subnational level (e.g., McGuirk & Burke, 2020; Berman *et al.* , 2017; Berman & Couttenier, 2015; Caselli & Michaels, 2013) have not reached a consensus about the direction in which income shocks influence conflict.[5] In addition to providing causal evidence at the subnational level, we augment those studies by exploiting (i) geo-referenced survey data, (ii) within-country differences in the share of value added, and (iii) fine-grained data that allows to distinguish conflict events by type of combat, target, and technology to measure the relative importance of opportunity costs compared to contest effects.

Second, an important strand of literature emphasizes existing cleavages between groups that differ with regard to ideology, ethnicity or religion as a driver of conflict (e.g., Esteban *et al.* , 2012b; Gehring & Schaudt, 2023; Michalopoulos & Papaioannou, 2016; Morelli & Rohner, 2015). In line with one of our arguments, Hodler (2006) highlights that as the number of groups competing for control increases, higher prices tend to lead to more conflict. During our sample period our results support the view that instead of ethnic fractionalization the decisive cleavage in Afghanistan is of an ideological nature between the Taliban and a pro-government alliance.

Third, our analysis adds to the scarce causal evidence on the effects of illegal commodities. Despite the importance of the illicit economy, particularly in many developing and conflict-ridden societies, the literature provides limited evidence on the effects of illegal commodity shocks on conflict. Closely related to our paper is the work by Angrist & Kugler (2008), who find a positive effect of cocaine prices on violence in the Colombian context. Chimeli & Soares

[5] Most of these papers focus on developing countries. Gehring & Schneider (2020) show that in established democracies, resource shocks are less likely lead to violent conflict, but their distribution can foster separatist party success in democracies.

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(2017) provide evidence that declaring mahogany trade as illegal contributed to an increase in violence in Brazil. We show that *de jure* illegality only affects the impact of price changes when it is enforced by the government.

This connects our paper to studies about the challenges of establishing a credible government in a poor and economically constrained environment (e.g., Berman *et al.* , 2011). Law enforcement by the government in these environments can lead to a conflict with the producers and create support for cartels or rebel groups. Moreover, eradication measures are often found to be ineffective (Mejía *et al.* , 2015; Ibanez & Carlsson, 2010). Our results highlight that the Afghan government is either unwilling or unable to enforce laws concerning opium production in districts beyond Kabul and the reach of foreign military bases. The Taliban profit from opium production and trafficking, setting an incentive for them to provide conflict-solving mechanisms in the districts they control. This helps to explain that we find the strongest conflict-reducing effect in districts controlled by the Taliban, relating to the literature on the provision of state-like institutions by non-state actors (Sánchez de la Sierra 2020; 2021).

Finally, we add to the growing literature on conflict and violence in Afghanistan (e.g., Child, 2019; Condra *et al.* , 2018; Trebbi & Weese, 2019; Langlotz, 2021; Lyall *et al.* , 2013). Wright (2018) argues that the tactics of rebel groups depend on their own and the state's capacity, as well as on outside options available to civilians – all potentially affected by income shocks. We add evidence along this line. Two studies address opium production and conflict in Afghanistan. Bove & Elia (2013) show a negative correlation between conflict and opium prices for a sample of 15 out of 34 provinces over the 2004-2009 period. Lind *et al.* (2014) find a positive impact of Western casualties on opium production over the 2001-2007 period. Our paper augments their findings by focusing on the link from opium profitability to conflict, using a longer period, more comprehensive measures of conflict, and new identification strategies.



## B. Theoretical considerations

The literature on the economics of conflict distinguishes between two main mechanisms that link (resource-related) income shocks to conflict: opportunity costs effects (e.g., Grossman, 1991), and the contest model (e.g., Collier & Hoeffler 1998). The first theory hypothesizes that

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with a significant rise in income, the opportunity costs of fighting increase, leading to, all else equal, less violence. The second theory predicts that, as the gains from fighting become more lucrative, higher resource prices will increase violence. Whether the net effect on conflict is positive or negative depends on the relative size of these effects. The size of opportunity cost effects depends on how many people and how much they profit from higher prices. The size of contest effects is influenced by the extent of violent group competition about resource control.

To analyze the impact of illegal resources on conflict, we develop a new theoretical framework. Figure 1 distinguishes four scenarios that differ along two dimensions: (i) whether laws against the production, processing, and trading of those resources are enforced and (ii) whether multiple groups involved in the illegal business are fighting for resource control. Depending on the combination of these two dimensions, opportunity cost effects are more or less likely to dominate contest effects. Hence, the predicted net effect on conflict can turn out to be relatively more positive, mixed, or negative.

[Figure 1 here].

If laws are actually enforced, this leads to some important differences of illegal compared to legal resources. First, the government as an actor does not profit directly from controlling the resource and from higher prices in the form of taxation. Second, enforcement measures like eradication campaigns reduce the positive income effect of higher prices for individual producers, leading to smaller opportunity cost effects. Third, only groups willing to take the risk will compete in an illegal market. If such groups (e.g., warlords, insurgents, or cartels) who profit from higher prices fight for control, an increase in the resource value is associated with strong contest effects (Hodler, 2006). If only one group controls an area, we would expect only small or no contest effects (e.g., Esteban *et al.* , 2012a).

This suggests that if governments try to enforce laws and multiple groups compete for control, opportunity cost effects will be small and contest effects large. Hence, a conflict-fueling effect is the most likely result, in line with a resource-conflict-curse. Scenario A in Figure 1 describes such a case where higher prices are linked to more conflict.

Many developing countries are, however, described by limited state capacity. Governments might decide against enforcing rules if the costs are higher than the perceived benefits. They face a trade-off between the benefit of controlling an area and the risk of losing the support of

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those involved in the production or trading of the illegal resource since they might even turn to insurgents that provide protection of illegal income-generating activities. Therefore, it can be rational for a government with a limited capacity not to enforce laws in parts of the country.

We expect stronger contest effects in scenario B, with no single group clearly in control and violent competition between various groups. At the same time, as laws are not enforced, the contest effects are more likely offset by stronger opportunity cost effects, since more farmers benefit from higher prices. Thus, as illustrated in Figure 1, the net effect is mixed.

Scenarios C and D reflect areas without intense group competition for resource control. Scenario C represents regions where state and military capacity are sufficiently strong so that the government controls the territory and enforces its laws against illegal resource production. We thus expect small contest effects, but also smaller opportunity cost effects. For that reason, the overall effect is more mixed, as in scenario B.

In contrast, scenario D describes regions where one insurgent group is in control and profits from the illegal activity. Opportunity cost effects will be larger as more people profit from higher prices when there is no enforcement. Contest effects are smaller as the group has an incentive to act as a stationary bandit that establishes monopolies of violence to sustain taxation contracts (De La Sierra, 2020). Scenario D predicts a conflict-reducing effect.

Countries can be thought of as comprising different geographical areas that correspond to these scenarios. The average effect on conflict will depend on the prevalence of the respective areas. Most prior causal evidence on illegal resources exists for Mexico (Dell, 2015; Castillo *et al.* , 2020) and Colombia (Angrist & Kugler, 2008), and shows on average a conflict-fueling effect of illegal resource production and prices. This suggests that a high share of geographical areas seem to fit scenario A. Partly due to foreign support and pressure, the governments try to enforce laws in large parts of the countries. Thus, fewer farmers are profiting from higher prices, and the opportunity cost effect is less pronounced. At the same time, there is a strong contest effect as several non-state groups are competing to control resource production.

During our observation period, Afghanistan differs from Mexico and Colombia in several ways. First, state capacity is much weaker and enforcement of laws is restricted to certain areas. Second, opium as the main illegal crop is considerably more labor-intensive than widely

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available alternatives. Following the logic of Dube & Vargas (2013), opportunity cost effects will thus be highly relevant when production shifts from other crops towards opium. Third, a significant number of areas are controlled by one insurgent group, the Taliban, which supports and profits from opium production. Hence, compared to Mexico and Colombia, there should be more areas where opportunity cost effects are likely to be relatively high and where group competition is less intense.

The average net effect remains an empirical question. In Section 5, we begin by estimating the average effect of higher opium profitability on conflict in Afghanistan. We then examine the relevance of opportunity cost effects relative to contest effects in Section 6. In Section 7, we make use of heterogeneity in territorial control to link the different theoretical scenarios described above to geographical areas in Afghanistan.

# 3. Data description

**Conflict data:** We use two different conflict datasets, one largely based on media and the other based on military reports. The UCDP Georeferenced Event Dataset (GED) is our primary source. It includes geocoded information on the "best estimate of total fatalities resulting from an event" (Croicu & Sundberg, 2015; Sundberg & Melander, 2013). As illustrated in Table B.4, 95% of the events covered by UCDP in our sample period are fights between the Afghan government and the Taliban (so-called state-based violence). Less than 4% are classified as one-sided conflicts with the Taliban as the perpetrator and civilians as the victims.

Afghanistan consists of 34 provinces (ADM1, see Figure C.3), further divided into 398 districts (ADM2). We define conflict incidence as a binary conflict measure using thresholds of 5, 25, 50, and 100 battle-related deaths (BRD), and conflict intensity as the log of the number of BRD per district-year. The use of different thresholds, each somewhat arbitrary, along with a continuous measure of BRD, alleviates concerns about specifying when a conflict becomes relevant and ensures transparency. To verify the reliability of these indicators, Figure G.9 shows a high correlation with a subjective conflict indicator derived from the NRVA household survey. In addition, our second main source is the SIGACTS (Significant Activities) data from Shaver

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& Wright (2016) (see details in Section E.5).[6]

**Opium and wheat suitability:** We exploit a novel dataset measuring the suitability to grow opium based on exogenous underlying information about land cover, water availability, climatic suitability, and soil suitability. Conceptually, the index developed by Kienberger *et al.* (2017) in collaboration with UNODC is comparable to other suitability indices by the Food and Agricultural Organization (FAO). Figure G.1a plots the distribution of the opium suitability index across Afghan districts. An index of one indicates perfect suitability, and an index of zero means a district is least suitable for growing opium. Given that opium is a "renewable" resource, this suitability can also be understood as the actual "resource" that varies across districts. We weight the suitability with the pre-determined population density, to account for areas that are potentially hard to reach and not populated. This does not affect our results (see Table F.10). We also account for an index of wheat suitability as wheat is the main legal alternative (Lind *et al.* , 2014; UNODC, 2013), illustrated in Figure G.1b. We see a positive correlation (r=0.58) between the two, but also clear differences.

**Drug prices:** We use international consumer prices for heroin and complement drugs from the European Monitoring Center for Drugs and Drug Addiction (EMCDDA). Heroin, a "depressant" drug that reduces arousal, is an opiate derived from morphine extracted from the opium poppy. To capture global changes in demand, we take the mean of all monthly prices for each country and then compute the average across countries to eliminate the effects of country-specific shocks. Local Afghan price data are taken from the annual Afghanistan Opium Price Monitoring reports by UNODC.[7] The complementary stimulant drugs we consider are cocaine, amphetamine, and ecstasy. We define a complement price index as the average of the three.

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[6] We prefer these two datasets over the Armed Conflict Location & Event Data Project (ACLED). ACLED is only available for the 2004-2010 period, thus reducing the sample by half, and is reported to be partly unreliable for Afghanistan (Eck, 2012)

[7] The bulk of heroin consumed in Europe is brown heroin. White heroin is much more

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**Opium cultivation and opium revenues:** As a more direct measure, we compute district-level opium revenues. Due to illegality and the local circumstances, these data are extremely hard to collect. We use information on actual opium cultivation and opium yields, retrieved from the annual UNODC Opium Survey reports, which are based on survey questionnaires and remote sensing methods. We then calculate actual opium production in kilograms at the district-year level by multiplying opium cultivation – partly extrapolated from province-level data – with the respective yields that vary by year and region. Opium revenues equal opium production multiplied with the yearly Afghan farm-gate prices for fresh opium at harvest time in constant 2010 Euro/kg. For the regression analysis, we take the logarithm of the revenues.

**Survey data:** We use the National Risk and Vulnerability Assessment (NRVA) survey conducted in 2005, 2007/08, and 2011/12 from the Central Statistics Organization (CSO) to test the relevance of opportunity costs at the household level. The surveys include between 21,000 and 31,000 households, covering between 341 and 388 of the 398 districts in Afghanistan. We harmonize the data from three different waves to construct indicators based on food consumption and expenditures, household assets, and a self-reported measure of the household's economic situation. All variables and their sources are described in more detail in Appendix A.

# 4. Identification strategy

## A. Estimating equation and identification

We are interested in the effect of opium revenues on conflict. However, district-level opium revenues rely on local price, cultivation, and yield data which all exhibit considerable measurement error and are sometimes missing.[8] Thus, our baseline specification focuses on the reduced

expensive and consumed less often, which is also why price data is only available for a few selected countries. Where available, both prices have a correlation of 0.49.

[8] As stated by the UNODC (2015, p. 63) "[d]istrict estimates are derived by a combination of different approaches. They are indicative only, and suggest a possible distribution of the

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form intention-to-treat (ITT) effect of opium profitability, which combines temporal variation in prices (in logarithms) with district-specific suitability to grow opium. We also use opium revenues, instrumented with opium profitability, in an IV setting to quantify the effect size.

Our baseline equation at the district-year level over the 2002 to 2014 period is:

$$conflict_{d,t} = \beta\, opium\ profitability_{d,t-1} + \zeta\ wheat\ profitability_{d,t-1} + \tau_t + \delta_d + \tau_t\delta_p + \varepsilon_{d,t}. \quad (1)$$

Standard errors are clustered at the district level, but results are robust to different choices including the use of province-level clusters and a wild-cluster bootstrap approach. $conflict_{d,t}$, is the incidence or the intensity of conflict in district $d$ in year $t$ based on the different thresholds from UCDP GED. Our "treatment" variable $opium\ profitability_{d,t-1}$ measures the relative extent of the shock induced by world market price changes in $t-1$ conditional on the exogenous district-specific suitability to grow opium in district $d$. More specifically, $opium\ profitability_{d,t-1}$ is defined as:

$$opium\ profitability_{d,t-1} = price_{t-1} \times opium\ suitability_d. \quad (2)$$

Mansfield & Fishstein (2016) show that whether opium or wheat is more profitable to produce in a specific district and year depends on yearly variation in the relative prices, and on the district-specific suitability to produce either of the two. Looking at gross profits, opium seems clearly more profitable (UNODC, 2005, 2013), but its production is also more costly, such that net returns are more comparable. According to UNODC (2004), between 80% to 90% of landowners and farmers decide on their own what they plant, which will usually be the most profitable crop. Given that in this framework relative price changes are the ones that matter for the producers, our baseline specification controls for $wheat\ profitability_{d,t-1}$ (defined as the international wheat price times a wheat suitability index). Wheat is the main (legal) alternative crop that farmers can grow throughout Afghanistan. Afghanistan contributes less than 1% to the global wheat supply, so we can consider the international wheat price as exogenous (as, e.g., Berman & Couttenier, 2015).

estimated provincial poppy area among the districts of a province." Assuming the measurement error is normal, this would bias our estimations towards zero. In case the precision of estimates is also affected by conflict and suitability, however, the bias is hard to predict.

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Variation in $opium\ profitability_{d,t-1}$ over time comes from changes in opium prices, which affect the opportunity costs of fighting. First, opium production is much more labor-intensive than wheat. Mansfield & Fishstein (2016, p. 18) report "opium requiring an estimated 360 person-days per hectare, compared to an average of only 64 days for irrigated wheat." Accordingly, a relative decline in opium prices causes marginal producers in some districts to shift towards wheat production. The decrease in labor demand in the agricultural sector comes along with decreased opportunity costs of fighting. The extent to which the producers shift depend on the respective suitabilities and prices.

Second, for a given level of production, lower prices reduce the profits of those involved in the production, processing, and trading. The effect of these price changes will be, on average, more pronounced in high suitability districts. Afghans who own land and produce for subsistence receive less income, but still have their subsistence production. Tenant farmers, cash-croppers, or those involved in processing and trading might be affected more severely. Even absent strong ideological support, joining the Taliban for a minimal salary or supporting them through shelter or local expertise can be the only viable alternative to make a living.[9] The effects of price changes on labor demand and profits are in line with the opportunity cost logic. With lower prices, the likelihood of fighting for or indirectly supporting the Taliban increases.

Our baseline equation includes year-fixed effects $\tau_t$, district-fixed effects $\delta_d$, and province-times-year-fixed effects $\tau_t\delta_p$. Afghanistan is a major supplier of opium, hence endogenous changes in supply in the country affect the world market prices. The year-fixed effects absorb

[9] Bove & Elia (2013, p. 538) write that "in Afghanistan individuals may choose between opium cultivation and joining an anti-government group." Several sources speak of ten US Dollar per month as the wage offered by the Taliban (more than in the official army), e.g., https://www.wired.com/2010/07/taliban-pays-its-troops-better-than-karzai-pays-his/ and Afghan officials are cited as wanting to turn "ten-dollar-Taliban" around (https://www.cleveland.com/world/index.ssf/2009/08/afghan_leaders_move_toward_-rec.html, accessed 08/28/2019).

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changes in overall supply while our strategy relies on the heterogeneous effect conditional on opium suitability. Appendix F shows that our results are not affected by the inclusion of control variables, which suggests that the fixed effects capture the most biasing variation. We also show that our results remain robust to excluding wheat profitability.

[Figure 2 here]

There are two main opium growing seasons in Afghanistan. The winter season starts in fall and the summer season around March (Mansfield & Fishstein, 2016). International price changes plausibly influence opium cultivation and revenues in the same and following year, as Figure 2a illustrates. To account for that, and to prevent problems caused by reverse causality, our preferred specification tests for the effect of $opium\ profitability$ on conflict one year later.[10]

## B. Changes in international prices, local prices, and local revenues

In the following, we discuss (i) that an important part of the movements of prices over our sample period is driven by changes in demand, (ii) show that international prices of complement drugs, due to common demand shifters, correlate positively with the international heroin price,

[10] Price changes in $(t-1)$ are most likely affecting cultivation decisions in summer$(t-1)$, winter$(t-1)/(t)$, and summer$(t)$, and thus also labor demand and revenues in $(t)$. Caulkins *et al.* (2010, p. 9) suggest that "the largest driver of changes in hectares under poppy cultivation is not eradication or enforcement risk, but rather last year's opium prices." Taking contemporaneous prices in $(t)$ is problematic with yearly price and conflict data. Using the price in $(t)$ would introduce reverse causality, as price changes later in the year can be affected by earlier conflict. Moreover, we need to allow time for price shocks to affect local production. Appendix E shows that using prices in $(t)$ would yield comparable results. Also note that while we estimate the average yearly effect, Wright (2018) and Sonin & Wright (2023) use sub-yearly variation to study how price fluctuations within a year affect the temporal distribution and tactics of conflict. It is possible that the short-term reaction differs from the average effect.

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(iii) demonstrate that international prices translate into economically relevant changes in the local price in Afghanistan and, (iv) establish that they affect opium revenues at the district level in Afghanistan. Figure 2b displays the variation in the international prices of heroin, the complement price index, and the local Afghan opium price. The local price is the most direct measure, but also most likely endogenous to opium supply shocks in Afghanistan.

Endogenous supply from Afghanistan matters for prices as the country itself is a major producer of opium. Data on illicit products is notoriously unreliable, but UNODC estimates suggest Afghanistan provided 93% of the world's non-pharmaceutical opium in 2007 (UNODC, 2007) and still more than 80% in 2019 (UNODC, 2020b). During our estimation period, UNODC suggests Afghan production first increases strongly until about 2007, and then decreases sharply for several years. Overall, a clear insight from those production estimates is that overall Afghan supply varies strongly over time and between years. This is in line with Mansfield & Fishstein (2016), highlighting that depending on relative price changes of opium compared to alternatives, farmers adjust their production choice from year to year.

Figure 2b provides several important insights about opium prices. First, there is variation across individual years, but all prices decline over time. This trend in all prices does not follow the strong upward and downward fluctuations in Afghan supply, which suggests that prices are more strongly driven by common demand factors. Interviews with experts at EMCDDA support this view; there is no agreement on the reasons, but the emergence of new synthetic or legal alternatives might be a factor, rather than changes in the supply of an individual drug.

Third, local Afghan prices follow a similar pattern as the international heroin price, which is reassuring. This indicates that, despite end-customer market prices being multitudes higher than local prices, international price changes can also translate into economically meaningful changes in actual opium revenues at the district level.[11]

We can also test directly whether international consumer price changes have statistically

[11] To put this in perspective, an amount of opium worth 600 US Dollar can have a street value of more than 150,000 US Dollar (see http://www.rollingstone.com/polit cs/news/afghanistan-the-making-of-a-narco-state-20141204, accessed 08/28/2019).

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and economically significant effects at the local Afghan level. We use the empirical model as defined in equation 1, but with the revenues from opium cultivation as the dependent variable. Corresponding to Figure 2a, Table E.1 considers lagged effects in column 1 and the moving average over (t) and (t-1) in column 2. In line with our proposed mechanism, external price changes, measured by the interaction of the international heroin price with the suitability to grow opium, lead to an increase in local opium revenues in the same and following year. The results are significant at the 1%-level in both columns. Quantitatively, a 1% increase in the international heroin price leads to about a 2.4% increase in revenues for those districts where opium suitability reaches one (perfect suitability). For districts characterized by the mean suitability, 0.53, the effect would roughly decrease by half (0.53 * 2.40 = 1.27).

## C. Visualizing the identification strategy

The treatment variable $opium\ profitability$ is the interaction between $drug\ price_{t-1}$ and $suitability_d$ with the two levels' effects being captured by district-fixed and year-fixed effects. Thus, the setting resembles a difference-in-difference approach, with price changes having a stronger effect on profitability in high suitability districts. While there is no pre-treatment period in our setting, we can test whether lead terms have significant effects to check violations of the identifying assumptions. Table F.1 shows that a lead term $opium\ profitability_{t+1}$ turns out to be very close to zero and to be insignificant.

Figure 2c illustrates the variation used for identification with two maps at the district level, showing opium suitability and the distribution of conflict across Afghanistan for two selected years. 2004 followed a year of high prices and opium profitability was higher (left graph). 2009, in contrast, was a year of lower prices (right graph). It becomes clear that lower prices are associated with more widespread and more intense conflict, whereas higher prices are associated with less conflict. This suggests a negative effect of higher prices, but other events in those years could bias such a simple inference procedure. Our identification, however, relies on the differential effect of prices conditional on suitability. This intuition becomes clear when comparing the relative change in conflict for different levels of opium suitability. Districts with higher suitability experience a much higher increase in conflict when prices and opium

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profitability decline. This is most evident in the north, northeast, and east.

## D. Potential biases

The biggest concern for causal identification of the effect of $opium\ profitability_{d,t-1}$ on conflict is the impact of opium supply side shocks in Afghanistan on heroin prices. As discussed in Section 4B, supply fluctuates strongly across individual years, and does not seem to be the major cause of the overall decline in prices. Changes in the aggregate Afghan opium supply, that influences the international (heroin) price $p^O_{t-1}$, are captured by the year-fixed effects $\tau_t$. $\tau_t$ thus also capture, for instance, problematic omitted variables $OV_{t-1}$ like yearly changes in political institutions, eradication campaigns, climate, or changes in foreign military strategy, which could cause such supply shocks. Province-times-year-fixed effects $\tau_t\delta_p$ account flexibly for omitted variables $OV_{p,t-1}$, for instance, changes in sub-national province-level institutions. Identification in our setting relies only on within-province variation in a particular year due to differences in how the price affects opium profitability depending on opium suitability. Problematic omitted variables $OV_{d,t-1}$ would need to affect both opium supply and hence $p^O_{t-1}$, as well as $conflict_{d,t}$. The effect on both would also need to differ between districts within provinces depending on high and low $opium\ suitability_d$. Given the negative effect of opium profitability on conflict in Figure 2c, we are most concerned about a potential downward bias.

Consider eradication campaigns, that can decrease supply and hence increase the heroin price, and could at the same time raise the likelihood of conflict. If eradication occurred more often in low suitability areas, this would lead to a downward bias. If it was more common in high suitability areas, it would cause an upward bias. Based on the notorious ineffectiveness of eradication policies (see, Mejía *et al.* , 2015; Felbab-Brown, 2013), the bias would most likely be small, but there could be other unobserved factors that have a similar effect.

Generally, any problematic biases would either need to result from cross-sectional differences between high and low suitability districts that have an effect which varies over time, or changes over time whose effect varies by suitability. Regarding the first possibility, Table B.5 shows that low and high suitability districts differ in some covariates $X_d$, like the distance to Kabul, elevation, and ruggedness. For instance, high suitability districts are on average closer

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to Kabul. This could become problematic if, inversely to the declining prices, overall conflict would increase over time, but districts closer to Kabul would be affected less by this increase. This would cause a downward bias. We capture any such bias to the extent that it is based on observable differences by interacting the complete set of time-invariant covariates $X_d$ with a linear time trend or flexibly with time-fixed effects $\tau_t$. Moreover, we will show that the results hold when including district-level time-varying covariates capturing climate conditions and other aspects frequently used in the related literature such as luminosity and population.

Regarding changes over time, we would be concerned if by coincidence other long-term trends in prices correlate with long-term trends in conflict that are driven by omitted variables and differ between low and high suitability districts (see, e.g., Barrett & Christian, 2017). We address and evaluate those concerns in multiple ways. First, Appendix E shows that trends between low and high suitability districts begin to diverge more after an exogenous change in Western policy around 2005 increased the reliance of the local population on opium revenues. Second, Appendix F shows the results with de-trended opium prices, which exhibit less variation, but support the main finding. Third, we control for high- and low-suitability-specific (alternatively high- and low-production-specific) linear time trends. Fourth, we randomize prices across years and find that random assignment yields no significant relationship with coefficients being distributed around zero. Fifth, Section 5B exploits the increase in legal opioid prescriptions in the United States, which affect heroin prices in a plausibly exogenous way, as an instrument. Finally, the next section explains how we can exploit the relationship of opium with complement drugs to assess the remaining risk of such a bias.

## E. Identification using changes in complement prices

Assume that equation 3 is the "true" regression, but we estimate the "short" equation 4 in the sense Angrist & Pischke (2008) use "true" and "short." Corresponding to equation 2, *opium profitability* is defined as opium price $p^O_{t-1}$ times opium suitability $s_d$.

$$c_{d,t} = \beta \times p^O_{t-1} \times s_d + \gamma \times s_d \times OV_{t-1} + \tau_t + \delta_d + u_{d,t}, \tag{3}$$

$$c_{d,t} = b^O \times p^O_{t-1} \times s_d + \tau_t + \delta_d + \epsilon_{d,t}, \tag{4}$$

$$c_{d,t} = b^C \times p^C_{t-1} \times s_d + \tau_t + \delta_d + \upsilon^C_{d,t}. \tag{5}$$

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Our main estimating equation 1 corresponds to equation 4, both do not capture the effect of omitted variables $OV_{t-1}$ in the true equation 3. This means that $b^O$ (O=Opium) could be biased and deviate from $\beta$ iff $\gamma \neq 0$ and $\rho = corr(p^O_{t-1}, OV_{t-1}) \neq 0$. $OV_{t-1}$ could thus be time-varying factors that affect overall opium prices through changing opium supply, whose effects on conflict differs between low and high suitability districts. For simplicity, we do not display wheat profitability and province-times-year fixed effects.

Now think about a drug that constitutes a complement to opium. The prices of both opium and a complement depend on the following factors: (i) changes in demand for various reasons, to which we refer to as common demand shifters, (ii) changes in the supply of opium, and (iii) changes in the supply of the complement. Common demand shifters move prices in the same direction, and as consumption largely takes place outside of the producing countries, they are not affected much by supply shocks in Afghanistan. However, due to the negative cross-price elasticity, changes in supply move $b^O$ and $b^C$ (C=Complement) in opposite directions. For that reason, using the estimate $b^C$ from equation 5 helps to inform us about a potential bias of $b^O$ related to any variable $OV_{t-1}$.

Appendix D provides the formal proofs. The requirements for this approach to be informative about potential biases are (i) that the impact of common demand shifters is sufficiently strong, and (ii) that supply side shocks to the complements are exogenous to district-level differences in conflict in Afghanistan. Using both estimates then provides information about the sign of $\beta$ and whether $b^O$ is an upper or lower bound estimate.

These two assumptions are plausibly fulfilled. Regarding the first assumption of common demand shifters, drugs can be classified as stimulants (uppers) or depressants (downers). Experts agree that there is a high share of polydrug users (see e.g., Jofre-Bonet & Petry, 2008); users that combine a stimulant and a depressant (EMCDDA, 2016).[12] As heroin is as depres-

[12] For instance, Leri *et al.* (2003, p. 8) conclude that the "prevalence of cocaine use among heroin addicts not in treatment ranges from 30% to 80%," making it a "strong" complement. This can take place in form of "speed-balling" (mixing heroin and cocaine), consuming the two jointly or with a time lag (e.g., weekend versus workday drug consumption).

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sant, we use the prices of the three important stimulants: cocaine, amphetamine, and ecstasy (EMCDDA, 2016). The trend in prices (Figure 2b) and correspondence with experts validate that demand changes have the largest impact on prices. Regarding the second assumption of exogenous complement supply, cocaine is exclusively produced in South America and there is no evidence of ecstasy and amphetamines production in Afghanistan (UNODC, 2013b). Thus, they should be exogenous to district-specific supply shocks in Afghanistan.

To sum up; if there was a remaining bias related to $OV_{t-1}$, there would be a risk to over-reject the hypothesis that the effect of opium profitability is zero. We can show that if both estimates, $b^C$ and $b^O$ have the same sign and are statistically significantly different from zero, over-rejection is drastically reduced even in the presence of a bias. If $b^C$ is further away from 0 than $b^O$, $b^O$ provides an upper bound estimate of $\beta$.

# 5. Results

## A. Main results - reduced form

We now turn to our main results in Table 1. The regression coefficients are very much in line with the suggestive graphical evidence in Figure 2c. Already when using the interaction with local opium prices, which are more likely to be endogenous, all five coefficients are negative. When turning to our baseline specification with international heroin prices in panel B, the negative effect of opium profitability on conflict intensity and incidence is more pronounced. The only insignificant coefficient is for conflict likelihood based on more than 100 BRD. These high-scale events are extremely rare, but this is also an indication that they are caused by other influencing factors. The first four coefficients for conflict intensity and likelihood are all significant at the 5%- to 10%-level.

[Table 1 here].

The results are of an economically meaningful size. In column 1, a 10% increase in the international heroin price translates to 6.75% fewer battle-related deaths in perfectly suitable districts. A back-of-the-envelope calculation suggests that if heroin prices in 2008 would have been as high as in 2001, a difference of 79%, there would have been overall 1,896 fewer deaths

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in 2009. Not many areas in Afghanistan seem to reflect scenario A of Figure 1, where higher prices fuel conflict. Instead, the effect of higher opportunity costs of fighting seems to dominate contest effects on average.

To further examine the risk of this estimate being biased, we now turn to the results using our complement prices. Panel C shows that when using the complement price index the coefficients are more negative in each column, and significant at least at the 5%-level. As Section 4E explains, knowing that both estimates have the same sign, and are statistically significantly different from zero, drastically reduces the risk of false rejection due to omitted variable bias. The fact that the estimates using the complement prices are always more negative indicates that the $opium\ profitability_{d,t-1}$ coefficients in panel B are (marginally) upward biased, and thus provide an upper bound of the true negative effect.[13]

## B. Instrumental variable specifications

Additionally, we run two IV specifications, which are described in more detail in Appendix E.2. The treatment variable in both IV specifications is opium revenues, derived from provincial-level data and regional yields. The first instrument is the opium profitability measure that we use as the treatment variable in the main reduced form. As a second instrument, we employ the interaction of exogenous changes in legal opioid prescriptions in the United States with the local suitability to grow opium. If users substitute heroin with a legal opioid due to changes in the availability of the alternative, this creates an exogenous change in heroin demand.

Table E.2 shows that the OLS effect of opium revenues is negative and statistically significant, in line with our prior results. The first stages work well according to standard metrics for both instruments separately, as well when included jointly. The second stage estimate using both instruments suggests that a 10% revenue increase leads to a decrease in BRD of about

[13] Contrary to opium price-related shocks, point estimates of wheat profitability sometimes switch signs and turn negative (see Table F.16). Wheat is relatively less labor intensive and often imported from abroad. Most households are net buyers of wheat (Mansfield & Fishstein, 2016) and thus negatively affected by price increases.

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1.92%. Overall, while the IV results need to be treated with caution due to measurement error in the revenue variable, they support the causal interpretation of our prior reduced-form results.

# 6. Opportunity costs and contest effects

## A. Opportunity costs at the household level

All tests above provide an indication of the importance of opportunity cost effects, but to what degree do households and farmers benefit from higher opium profitability? Given the high labor intensity of opium relative to its main alternative wheat, we would expect that higher prices not only benefit a small elite but also larger shares of the population. To examine this dimension, we use different waves of a nationally-representative household survey, the NRVA. We construct several indicators of household living standards, in accordance with the literature.

Figure 3 plots the coefficients for opium profitability for six separate regression models with the outcome variable indicated in the legends. We find evidence that dietary diversity and food expenditures increase with higher opium profitability. We also consider indicators that are not as volatile as food consumption. In years following high opium prices, households in districts with higher opium suitability also benefit more from the price increases in terms of assets that they hold. The last indicator "Economically Improved" is a self-reported measure, which turns out to be affected in the same direction as the other indicators of living standards. If households are better off economically, there is less need to fight, as the opportunity costs of fighting indeed increase with higher opium profitability. This supports the importance of an opportunity cost mechanism.

[Figure 3 here].

## B. Opportunity costs and contest effects conditional on value added

This section uses additional sub-national variation to further validate that opportunity cost effects dominate contest effects in Afghanistan. Districts, which feature not only raw production but also intermediate steps along the value-chain (like trading, processing, or trafficking), can obtain a higher share of the value added. Hence, higher prices are associated with a relatively

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stronger effect on opium profitability and higher gains from fighting in those districts. If there is widespread competition between different groups about resource control, we expect opium profitability to be more conflict-fueling, and thus have a more positive effect on conflict in high value added districts. In contrast, if the opportunity cost effect of higher prices dominates, we expect the effect to be even more negative in districts with a high value added.

[Figure 4 here].

Using UNODC reports, we geo-reference data on whether a district contains opium markets, a heroin or morphine lab, or whether it is crossed by potential drug trafficking routes to proxy for value added. Markets create additional jobs and revenue, profit margins are higher further up the production chain, and trafficking routes allow raising income through taxation or road charges. Figure 4a shows the locations of markets and labs; Appendices A and H provide all sources. There is no reliable information about yearly changes, but it is plausible that with little eradication efforts and limited state capacity, most locations remain relevant throughout the sample period. We create four cross-sectional indicators, measuring the existence and number of markets, the existence of any processing lab, and whether a district is on a plausible trafficking route that would not need to cross areas of other ethnic groups.

As a second proxy for value added, we use a slightly adapted market access approach based on Donaldson & Hornbeck (2016). The assumption is that a district that is more central in the opium production network can also extract a higher share of value added. We also compute a "regular" market access variable using luminosity as a proxy for the economic importance of a district as an end-consumer market. This serves as a placebo test, and also tests whether sales in the country itself are important enough to have a potentially significant effect on conflict.

Market access for a district $i$ is computed as $MA_i = \sum_j W_j dist_{i,j}^{-\theta}$. $W_j$ is the importance of district $j$ proxied using either the number of drug markets or mean luminosity, $dist_{i,j}$ are the distances between the district and the other districts, and $\theta$ is the factor discounting other districts that are further away. We use a factor of one as in Donaldson & Hornbeck (2016). To take account of transportation costs and the often mountainous terrain in Afghanistan, we compute distances using the two-dimensional road network (Market Access 2D) as well as roads adjusted for elevation (Market Access 3D). Figure 4b visualizes this approach.

[Table 2 here].

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Table 2 presents results on the heterogeneity analysis based on district-level differences in value added. Panel A indicates that the link between the conflict-reducing effect of a higher opium profitability is significantly more pronounced in those districts that account for a potentially larger share of the value chain. Panel B shows that using the opium market access measures also yields a significant negative interaction effect, further supporting that opportunity cost effects dominate contest effects. In contrast, there is no effect when instead weighting by luminosity, supporting that our opium market access measure does not pick up something that is simply location-specific. It also suggests that Afghanistan plays no crucial role as an end-consumer market. Figure F.8, panel A, visualizes the average main effect, and panel B the marginal effects of opium profitability in districts with lower and higher value added. When the share of value added is greater, the increase in opportunity cost effects is larger, in absolute terms, than the increase in contest effects.

# 7. Law enforcement and group competition

Our framework in Figure 1 distinguishes four scenarios with differing predictions of the net effect of illegal resource profitability on conflict. In this section, we explore to what extent districts in Afghanistan resemble those combinations of law enforcement and competition between groups for resource control.

Out of the four scenarios, we argue that – both theoretically and in light of the prior results – Afghan districts do not seem to resemble scenario A where laws are enforced and multiple groups fight violently about resource control. Empirically, the average negative effect and the additional evidence in Table 2 would be in sharp contrast to a scenario where contest effects are stronger than opportunity cost effects. Qualitative evidence describes the Afghan government as unable or unwilling to control and strictly enforce laws against opium production in large

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parts of the country.[14] Researchers describing their fieldwork in Badakhshan "observed neither restrictions to poppy farmers nor any repercussions or a need to hide the fields from outsiders," and in areas supposedly controlled by the government, "officials at all levels are benefiting from the proceeds from drug trafficking" (Kreutzmann, 2007, p. 616).

While the official government claims that "poppy cultivation only takes place in areas controlled by the Taliban," a US counter-narcotics official reports that "(president) Karzai's Taliban enemies finance themselves from the drug trade, so do many of his supporters." We assume no Afghan districts are best described by scenario A based on the prior quantitative and the qualitative evidence.[15]

To link our scenarios to within-country heterogeneity, we aim to distinguish which actor controls a district and assign districts to either government control or Taliban control. The remaining areas where neither group is likely to be in clear control can be thought of as contested. Contested means that there are potentially multiple groups competing for resource control, which could be local strongmen, warlords, or groups linked to a particular ethnicity. However, the UCDP GED data classifies all groups as Taliban or pro-government, even when their actual allegiances are less clear and might change over time. Hence, we cannot directly identify

[14] Our theoretical framework assumes that the official government chooses whether to enforce laws, but is not one of the groups fighting for control over drug production and trade. However, government agents might profit individually from the drug business, which is most likely in areas where the state capacity and law enforcement are limited.

[15] See, http://www.rollingstone.com/politcs/news/afghanistan-the-making-of-a-narco-state-20141204 and https://www.reuters.com/article/uk-afghanistan-drugs-idUKN2444215920080724, accessed 08/29/2023. The first source reports a case of a drug trafficker possessing a letter of safe passage from a counter-narcotics police leader, and a new director of an anti-corruption agency who was revealed to be a formerly convicted drug trafficker.

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conflict within these alliances.

Our approach is thus to focus on districts most likely to be actually controlled by the government and those most likely controlled by the Taliban. A variety of geo-referenced data is used to create a series of proxy variables, which are shown in Figure 5a. We expect that laws about the illicit nature of opium production are, if anywhere, only enforced in districts under government control. To proxy for which districts are under government control, we measure whether a district is within a specific proximity to a major Western military base or one of the five largest cities. For the capital Kabul, which also hosts several military bases, we code a separate indicator. We follow Michalopoulos & Papaioannou (2014), who use distance as a measure of government influence, and Lind *et al.* (2014), who propose it as an indicator for law enforcement and the presence of state institutions. We use road- and terrain-adjusted travel time in the main analysis and linear distance as a robustness test in Appendix F.

We use two proxies for Taliban control. First, whether the Taliban have controlled a district in years prior to 2001 (Dorronsoro, 2005). We expect that due to the common past and existing networks, the Taliban will, all else equal, find it easier to expand their power again in those districts. Second, Trebbi & Weese (2019) argue that ethnic boundaries best explain support for the Taliban as the main insurgent group. Thus, we exploit the fact that the Taliban were initially a Pashtun group (even though they also feature members from different ethnicities).

For those proxy variables, we use various sources, ranging from maps provided by experts at the U.N. to data from the American military, satellite pictures and newspaper reports. We also use information from the Ethnologue dataset (Gordon, 2005) and the geo-referencing of ethnic groups' dataset (GREG, Weidmann *et al.* , 2010) on whether Pashtuns are present in a district. For both Taliban and pro-government control, we create binary variables for the centroid of a district being within two or three hours travel time, respectively.

To assess how far government control reaches beyond main cities and Western military bases, we conduct additional regressions in Table 3. In each specification, we interact *opium profitability* with one of the proxy variables for government control. We interpret a significant interaction effect as a sign that the relative size of opportunity cost versus contest effects changes due to laws enforced when governments control a district. Columns 1-4 show results

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for proximity to military bases and the capital Kabul. In all specifications, the main effect for opium profitability remains negative. The interaction terms indicate that government enforcement plays an important role, but only within a limited range. For districts within a travel time of less than two hours, the effect of higher opium profitability is significantly more positive.[16] Thus, as predicted, a higher degree of law enforcement seems to lower the opportunity cost effect. Columns 5-6 evaluate whether there are differences related to the distance to other cities except for Kabul. Proximity to other cities has no significant effect, suggesting that outside a limited range around Kabul and military bases, there is *de facto* little law enforcement.

[Figure 5 and Table 3 here]

As a next step, we aim to use these insights to empirically distinguish the effect of opium profitability conditional on who controls a district. To do that, we create a categorical control measure that assigns districts to one of three categories. Based on the prior results, we code them as under government control if the travel distance to Kabul or the next military base is below two hours. For Taliban control, there are two options. The insurgents are coded as controlling a district if the district is either within the area they used to control prior to 2001 or if a non-negligible share of Pashtuns are present in the district and travel time to Kabul or a military base is more than two hours. The remaining districts are coded as contested. Figure 5b displays the resulting geographic categorization on a map.

If pro-government forces control districts, the government is more likely to enforce laws, so we expect weaker opportunity cost effects. As there is little competition about control, there should also be small contest effects. The prediction for the net effect is mixed. If neither the government nor the Taliban have firm control over a district, laws regarding opium production are not enforced, but there might be different factions fighting for control. The predicted net effect is again mixed, so it remains an empirical question. Based on the relative size of opportunity cost and contest effects, we expect the strongest conflict-reducing effect in districts strictly controlled by one insurgent group, in this case the Taliban.

[16] In line with our expectation, the effect is even more positive for a distance of less than one hour and less positive for longer distances.

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We then run regressions that interact our opium profitability measure with the categorical control variables to compute the marginal effect of opium profitability on conflict for districts in each category. Figure 5c plots marginal effects. The results are in line with the predictions of our theoretical framework. The effect of opium profitability in districts under government control or in contested districts is indistinguishable from zero. Those districts would resemble scenarios B and C, where opportunity cost and contest effects are either both small or both large. In contested areas, laws regarding opium production are not enforced, but there might be different factions fighting for control. In government-controlled areas, laws are enforced, leading to weaker opportunity cost effects. As there is little competition about control as well, contest effects are small as well.

In contrast, according to both proxies, we find a large and statistically significant negative effect in districts under Taliban control. This resembles the predicted conflict-reducing effect in scenario D. As production is *de facto* legal in this scenario, the opportunity cost effects related to higher prices are stronger. If one group is in control that profits from uninterrupted production, contest between groups should be weak. Indeed, more than 65% of the farmers and traffickers in southern Afghanistan stated that the Taliban offered to protect opium production and trafficking (Peters, 2009). UNODC (2013, p. 66) states that "[i]n some provinces, notably those with a strong insurgent presence, some or all farmers reported paying an opium tax," in the form of a land or road tax. A local farmer describes that "the Taliban have a court there to resolve people's problems" and "the security situation is good for the people living there."[17]

# 8. Further results and sensitivity analysis

This section summarizes further results and the sensitivity of our main findings. All tests along with corresponding tables and figures are explained in detail in Appendices E and F.

[17] See http://www.rollingstone.com/politics/news/afghanistan-the-making-of-a-narco-state-20141204, accessed 08/28/2019.

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**Further results (Appendix E):** First, we consider aggregate effects at the province level to rule out that higher revenues in one district were causing more conflict in neighboring districts of other provinces. Figure E.2 points to the negative effect as for the district-level regressions.

Second, in Figure E.3, we fix price levels at those of the year 2001 to compute a hypothetical counterfactual if prices would not have fallen as much as they did over our sample period. The figure suggests that without the strong decline in heroin prices, battle-related-deaths would have increased considerably less.

Third, increased opportunity costs due to better outside options could not only affect the intensity and frequency of violence, but also influence the types of violent activities the actors engage in. We use the concept of rational agent decision-making to explore how insurgents adjust their strategies in response to better outside options and reduced labor supply due to higher opium profits. In Section E.5 we discuss findings based on the SIGACTS dataset. We find evidence that as outside opportunities improve, insurgents shift from labor-intensive combat to less labor-intensive types, resulting in a decline in direct fire and events with casualties. Moreover, while attacks on harder targets like military bases or foreign forces decrease, there is no significant decline in attacks on softer targets, such as civilians. Although the share of technology-intensive attacks, measured by improvised explosive devices (IED), shows a positive coefficient, it is not statistically significant. These findings shed light on the role of opportunity cost effects in shaping the conflict dynamics in Afghanistan.

**Sensitivity analysis (Appendix F):** We probe the sensitivity of our results in various ways. First, in Table F.1 we test for pre-trends and different lag structures by including opium profitability in periods $t + 1$, $t$, and $t - 1$, either at the same time or in groups of two. Opium profitability in $t$ and $t - 1$ are both associated with a conflict-reducing effect, but there are no signs of any problematic pre-trends. Table F.2 shows that including the contemporaneous and lagged variables individually yields very similar coefficients.

Second, we analyze heterogeneous effects of opium profitability on onset and ending of conflict events. Bluhm *et al.* (2021) point to the importance of differentiating between the probability of switching from one conflict state to another. Thus, we estimate the effects for conflict incidence (panel A), onset (panel B), and ending (panel C) in separate models using

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conditional logit, presented in Table F.3. Panel A verifies our main finding and the results in panels B-C indicate that a positive income shock and more opium cultivation reduces the likelihood that conflicts break out, and raises the likelihood that an ongoing small conflict ends.

Third, Table F.4 examines whether the conflict-reducing effect is stronger or weaker depending on how many ethnic groups are present. All interactions turn out to be insignificant, supporting the notion that after 2001 conflicts occurred mainly between pro-government and pro-Taliban factions, not between ethnicities.

We continue by showing that our results are also robust to (i) modifications of the treatment variable, like using unweighted suitabilities and de-trended price data, (ii) modifications of the empirical model, including different sets of fixed effects, (iii) using cultivation instead of revenues, (iv) different choices on how to cluster standard errors, (v) leaving out wheat suitability, adding a baseline set of pre-determined covariates such as luminosity and population, as well as an exogenous measure of droughts, the VHI, and allowing for time-varying effects of time-invariant district-specific control variables, (vi) and to dropping potential outliers like border districts, the two southern provinces Kandahar and Helmand, and leaving out each year and province one at a time. Finally, to evaluate potential problems caused by non-linear trends in the time series (see Barrett & Christian, 2017), we randomize the time-varying variable (international heroin price) across years, as well as the district-specific suitability across districts. We find no evidence for such problematic trends in these placebo tests.

# 9. Conclusion

This paper provides new evidence on the mechanisms linking resource-related income shocks to conflict, thus adding to key strands of the conflict literature (e.g., Berman *et al.* , 2017; Berman & Couttenier, 2015; Morelli & Rohner, 2015). Our study highlights that the effect of resource-related income shocks critically depends on the type of resource and the context.

There can be important differences between legal and illegal resources. First, the opportunity cost effects of higher prices can be lower for illegal resources, but only if governments enforce laws and counteract increases in production through eradication and other actions. From an industrialized-country perspective, enforcement of laws might be regarded as the standard.

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In developing countries, where most conflicts occur, the capacity of the state to do so is often limited. Second, competition among groups active in illicit production, processing, and transport can be more violent than in a legal product market. However, the extent to which this moderates the contest effects of higher prices depends on whether and how many groups are competing for control in an area. Our theoretical framework allows forming priors about the net effect of illegal resource shocks and testing those empirically.

We focus on Afghanistan, an exemplary case of a conflict-ridden country with a weak labor market, limited state capacity, and difficulties forming stable governing coalitions between existing groups. Based on various identification strategies, our main reduced-form results consistently show that a 10% rise in international heroin prices decreases the number of battle-related deaths by about 7% in districts with the highest possible suitability to grow opium. Our analyses indicate that our baseline specification using heroin prices is – if one is worried about potential omitted variable bias – most likely an upper bound of the true negative effect. Our theoretical framework helps to understand spatial heterogeneity in this effect across districts. In line with our theoretical predictions, the most significant reduction in conflict is observed in areas where laws are not enforced and without multiple groups competing for resource control.

We do not argue that our findings can explain the conflict in Afghanistan in all its complexity, but they augment our prior understanding based on the existing insights (e.g., Child, 2019; Condra *et al.* , 2018; Bove & Elia, 2013; Lyall *et al.* , 2013). While we make no causal claims beyond our observation period (2002-2014), the findings align with the spread of conflict in Afghanistan in recent years, which featured falling prices and thus lower opium profitability. We use results at the province and country levels to verify that higher opium revenues do not, on average, spill over and fuel conflict in other districts.

Nonetheless, illegal drugs like opium constitute a serious problem for various reasons. In order to derive sound policy measures, it is important to understand the underlying trade-offs. In a context with weak labor markets and few outside opportunities, depriving farmers of their main source of income by enforcing rules through eradication measures has to be weighted against the impact on households and the risk of fueling conflict. Our results show that households are indeed negatively affected by lower opium prices. Most available evidence suggests

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that strict law enforcement measures in production countries have little to no effect on cultivation (Mejía *et al.* , 2015; Ibanez & Carlsson, 2010; Clemens, 2008), as long as drug demand from consumer countries remains high.

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# Main tables of the paper

Table 1: Main results, 2002-2014 period

| | **(log) BRD** (1) | **1 if** ≥ 5 (2) | **1 if** ≥ 10 (3) | **1 if** ≥ 25 (4) | **1 if** ≥ 100 (5) |
|---|---|---|---|---|---|
| | **Panel A: Local opium prices** | | | | |
| Opium Profitability (t-1) | -0.346 | -0.096 | -0.094 | -0.076 | -0.042 |
| | (0.107) | (0.033) | (0.032) | (0.029) | (0.018) |
| Adjusted R-Squared | 0.649 | 0.501 | 0.483 | 0.453 | 0.311 |
| | **Panel B: International heroin prices (baseline)** | | | | |
| Opium Profitability (t-1) | -0.675 | -0.167 | -0.191 | -0.147 | -0.040 |
| | (0.296) | (0.090) | (0.085) | (0.075) | (0.037) |
| Adjusted R-Squared | 0.649 | 0.501 | 0.484 | 0.453 | 0.310 |
| | **Panel C: International complement price index** | | | | |
| Opium Profitability (t-1) | -0.947 | -0.249 | -0.237 | -0.203 | -0.086 |
| | (0.308) | (0.094) | (0.086) | (0.076) | (0.041) |
| Adjusted R-Squared | 0.651 | 0.502 | 0.484 | 0.455 | 0.311 |

*Notes*: Models include province-times-year- and district-fixed effects. The dependent variable is conflict in (t) operationalized as indicated in the column heading. Opium Profitability is defined as the interaction between the normalized drug prices (in logarithms) and the suitability to grow opium. The number of observations is 5,174 across all panels. Standard errors are in parentheses (clustered at the district level). See Table F.9 for estimates using cocaine prices.

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Table 2: Opportunity costs conditional on value added, 2002-2014 period

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | **Panel A: Value added, based on opium markets, labs, trafficking** | | | |
| **Interaction with** | **Any Market** | **Number of Markets** | **Any Processing Lab** | **On Ethnic Traff. Route** |
| Opium Profitability (t-1) | -0.472 | -0.480 | -0.590 | 0.105 |
| | (0.314) | (0.306) | (0.312) | (0.358) |
| Opium Profitability (t-1)*X | -0.845 | -0.521 | -0.502 | -1.734 |
| | (0.416) | (0.255) | (0.557) | (0.487) |
| Adjusted R-Squared | 0.650 | 0.651 | 0.649 | 0.652 |
| | **Panel B: Value added based on market access approach** | | | |
| **Interaction with** | **Opium Market 2D** | **Opium Market 3D** | **Luminosity 2D** | **Luminosity 3D** |
| Opium Profitability (t-1) | 1.489 | 1.496 | -0.902 | -0.899 |
| | (1.141) | (1.130) | (0.434) | (0.434) |
| Opium Profitability (t-1)*X | -0.470 | -0.474 | 0.035 | 0.035 |
| | (0.232) | (0.231) | (0.041) | (0.041) |
| Adjusted R-Squared | 0.651 | 0.651 | 0.649 | 0.649 |

*Notes*: Models include province-times-year- and district-fixed effects. The dependent variable is the log of battle-related deaths in (t). Opium Profitability is defined as the interaction between the normalized international heroin price (in logarithms) and the suitability to grow opium. Opium Market 2D and 3D range between two and about twelve hours, thus computing the marginal effects of opium profitability conditional on market access yields almost always negative effects. Regressions include interactions of the opium price with a variable X as indicated in the column heading. The partial bivariate product of the suitability times X are captured in the fixed effects as both are time-invariant. Variables X are defined in Appendix A. The number of observations is 5,174 in every regression. Standard errors are in parentheses (clustered at the district level).

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Table 3: Government control and law enforcement, 2002-2014 period

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Proximity to | Military bases | | Kabul | | Other cities | |
| **Interaction with** | **Travel Time 3D** | | | | | |
| | **1 if** ≤ 2 | **1 if** ≤ 3 | **1 if** ≤ 2 | **1 if** ≤ 3 | **1 if** ≤ 2 | **1 if** ≤ 3 |
| Opium Profitability (t-1) | -0.930 | -0.767 | -0.893 | -0.826 | -0.616 | -0.612 |
| | (0.350) | (0.426) | (0.315) | (0.325) | (0.309) | (0.330) |
| Opium Profitability (t-1)*X | 1.170 | 0.364 | 1.685 | 0.588 | -0.389 | -0.207 |
| | (0.419) | (0.498) | (0.671) | (0.508) | (0.579) | (0.502) |
| Adjusted R-Squared | 0.652 | 0.650 | 0.650 | 0.650 | 0.649 | 0.649 |

*Notes*: The dependent variable is the log of battle-related deaths in (t). Opium Profitability is defined as the interaction between the normalized international heroin price (in logarithms) and the suitability to grow opium. Regressions include interactions of the opium profitability with a variable X as indicated in the column heading as well as province-times-year- and district-fixed effects. Other cities are Kandahar, Kunduz, Jalalabad, Hirat, and Mazari Sharif, the next five largest cities). Travel time is road and terrain-adjusted travel distance. See Table F.22 in the Appendix for estimates based on the linear distance. The number of observations is 5,174 in all columns. Standard errors are in parentheses (clustered at the district level).

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# Main figures of the paper

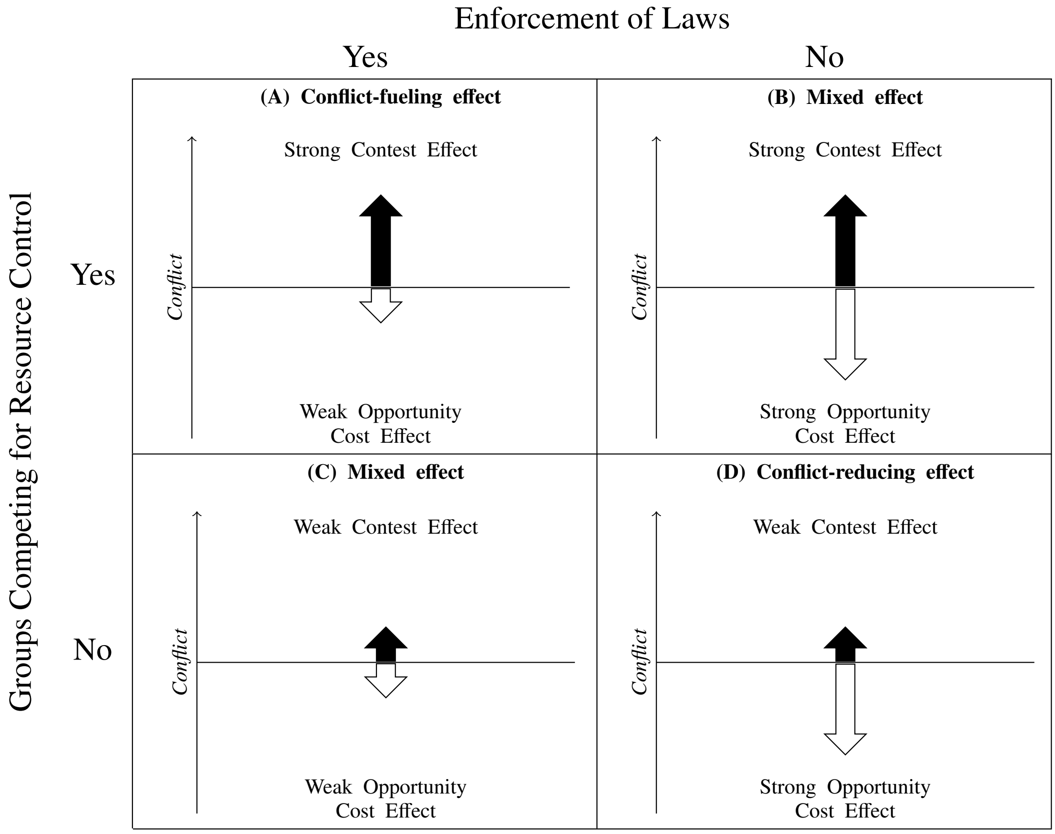


Figure 1: Scenarios for illegal resources: Law enforcement and group competition

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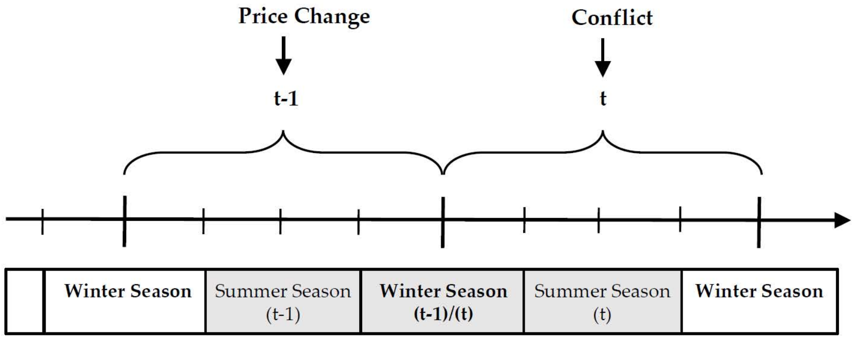
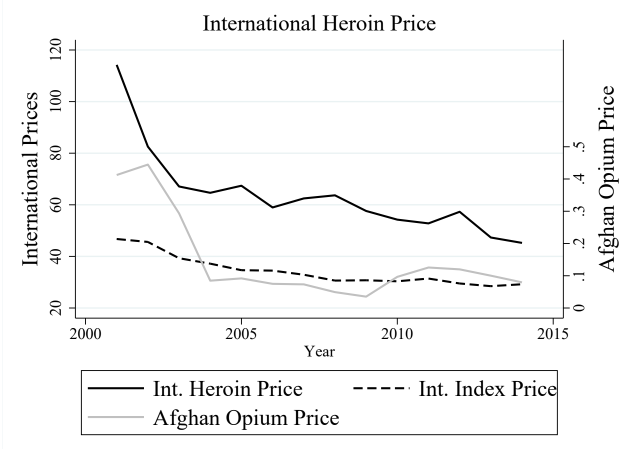
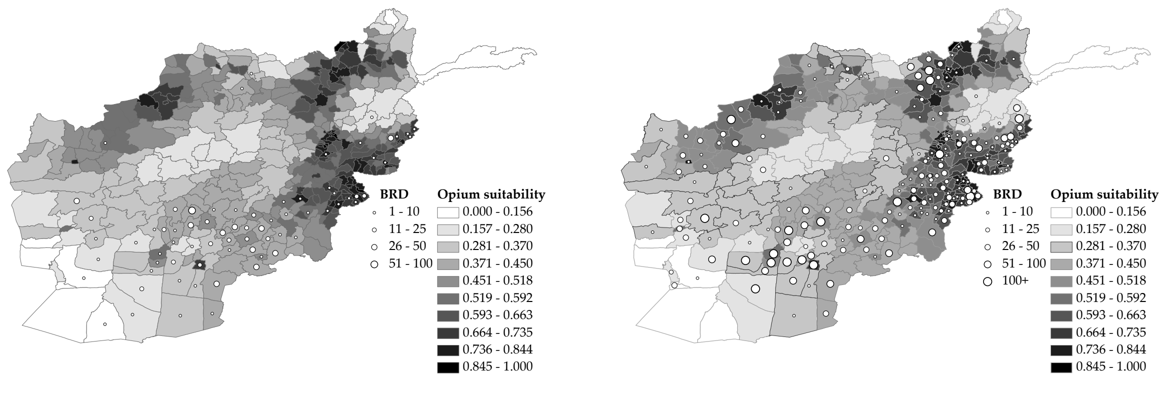
(a) Timing: How price changes in year $t-1$ affect production, revenues, and conflict




(b) Variation in international and local prices over time




(c) Intensity of conflict in districts with high and low suitability to grow opium




Conflict in 2004: High opium prices (t-1)/(t) Conflict in 2009: Low opium prices (t-1)/(t)

Figure 2: From opium prices to production changes and conflict

*Notes*: The figures show a) the timing from a price change to a possible change in production and then conflict, b) price changes over our sample period, the effect of price changes on estimated production, and c) an illustration of the differential effect of price changes conditional on opium suitability.

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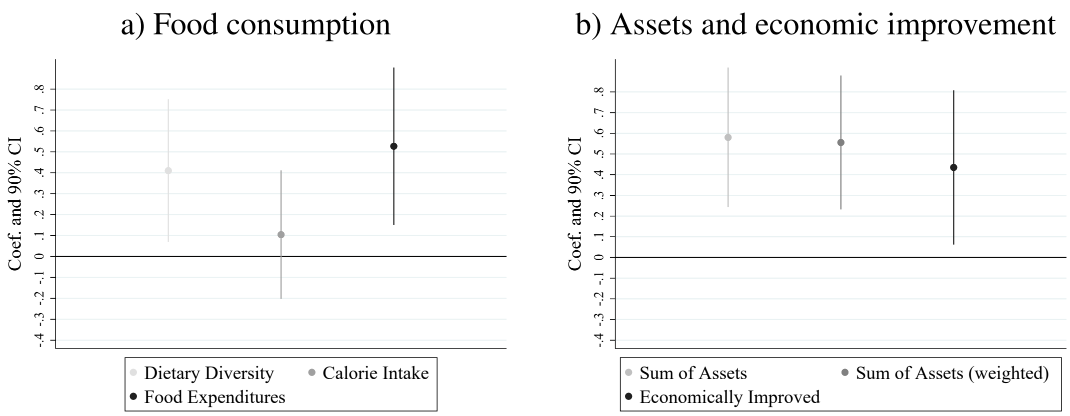
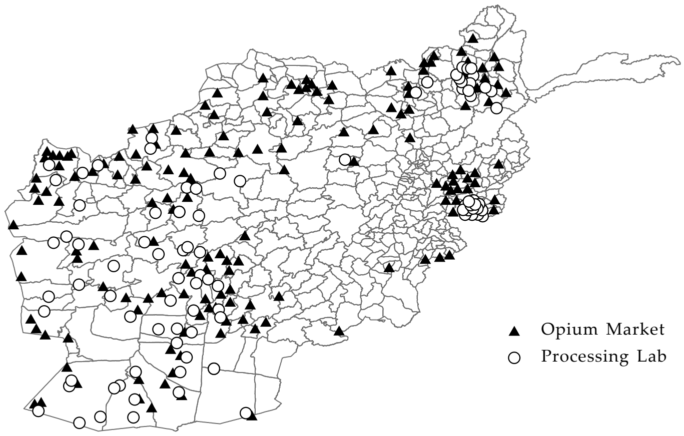



Figure 3: Effect of opium profitability (t-1) on standard of living indicators in (t)

*Notes*: The figure plots coefficient estimates (along with 90% confidence intervals) of opium profitability for six separate regressions with outcomes as indicated in the legends. The outcome variables are based on the NRVA household survey, details on their construction can be found in Appendix A. Outcomes are standardized to have a mean value of zero and a standard deviation of one. All regressions follow the baseline regression equation with international heroin prices in Table 1, panel B. Corresponding regression results are displayed in Table F.21.

(a) Opium markets and labs




(b) Opium market access

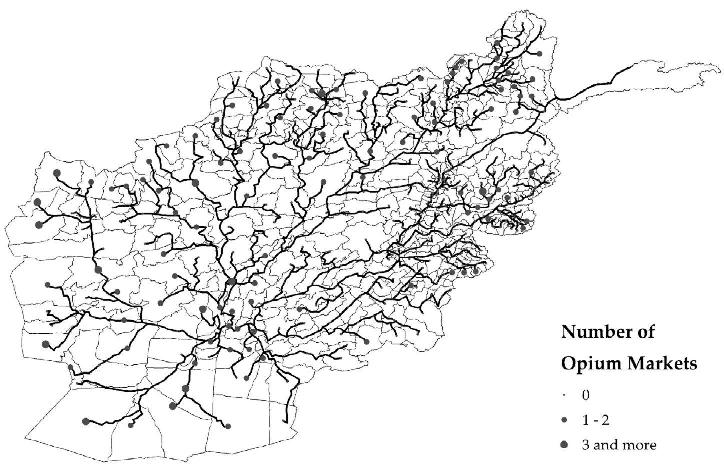



Figure 4: Proxies for value added and computation of market access measures

*Notes*: a) Triangles indicate opium markets and circles indicate heroin and morphine processing labs. b) Dots indicate district-specific centroids, black lines are the shortest road connections to the other centroids in the network. Opium market access is computed for every district, leading to individual optimal road connections. Distances are used as weights and multiplied with the importance of the respective network members, e.g., the number of drug markets. Sources: UNODC (2016), Open Street Map and Afghanistan Information Management Service (AIMS), processed with ArcGIS.

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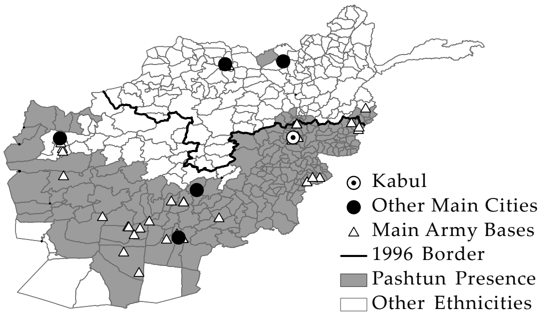
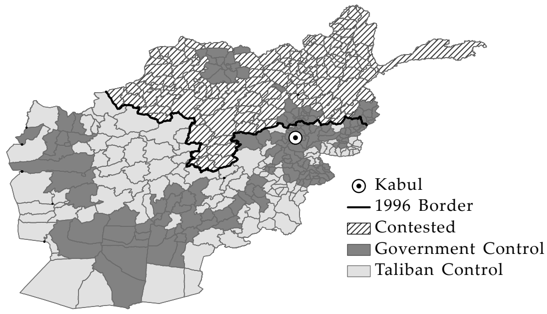
a) Geo-referenced data




b) Territorial control classification




c) Marginal effect of Opium Profitability

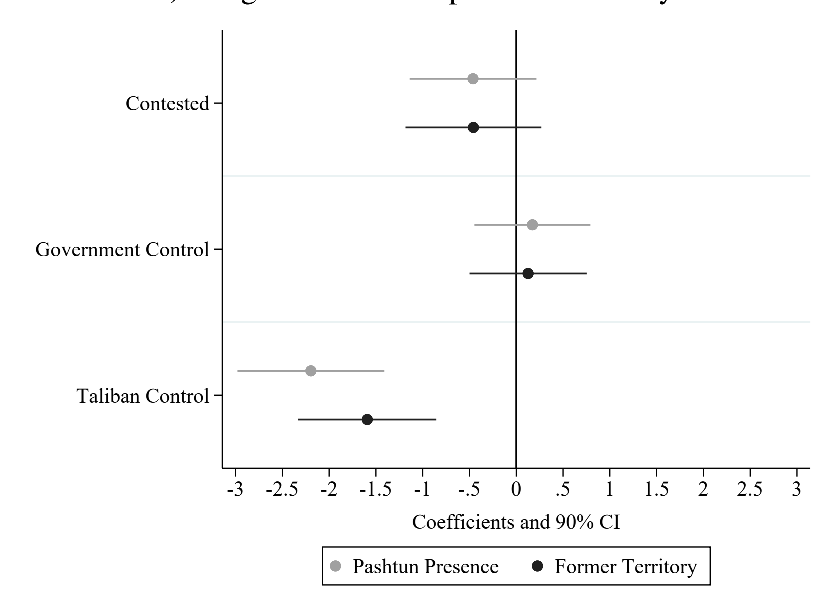



Figure 5: Treatment effects conditional on group control

*Notes*: a) Presence of Pashtuns (Ethnologue). Dots indicate Kabul and other major cities: Hirat, Kandahar, Kunduz, Mazari Sharif. Triangles indicate locations of foreign military bases as defined in Appendix A. The area south of the 1996 border was controlled by the Taliban before 2001 (Dorronsoro, 2005), which we refer to as former territory. b) Based on the results in Table 3, districts are considered under government control if they are within a 2-hour travel distance from a military base or Kabul. Districts under Taliban control are proxied by former territory. All other districts are coded as contested. Figure G.8 shows the map using Pashtun presence to define Taliban control. c) Marginal effects of opium profitability for the three types of districts following the baseline model in Table 1, panel B, adding an interaction with territorial control. Corresponding regression results are in Table F.24.