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Stimulant or depressant? Resource-related income shocks and conflict

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Abstract:

We provide evidence on the mechanisms linking resource-related income shocks to con- flict, focusing specifically on illegal crops. We hypothesize that the degree of group compe- tition over resources and the extent of law enforcement explain whether opportunity cost or contest effects dominate. Combining temporal variation in international drug prices with spa- tial variation in the suitability to produce opium, we show that in Afghanistan higher prices increase household living standards, and reduce conflict. Using georeferenced data on the drug production network and Taliban versus pro-government control highlights the importance of opportunity cost effects, and reveals heterogeneous effects in line with our theory.

Keywords: Resources, resource curse, conflict, drugs, illicit economy, illegality, geography of conflict, Afghanistan, Taliban

JEL Codes: D74, K42, O13, O53, Q1

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1. Introduction

Contests over land and resources play a critical role for 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 also a crucial dimension in the economic analysis of conflict (e.g., Bazzi & Blattman, 2014; Berman *et al.*, 2017; Morelli & Rohner, 2015; Van der Ploeg & Rohner, 2012). 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, and investigate the role of opium in fueling or abating violence. This ongoing conflict has one of the highest death tolls in recent history and is at the core of several recent contributions (e.g., Child, 2019; Condra & Wright, 2019; Condra *et al.*, 2018; Lind *et al.*, 2014; Sonin *et al.*, 2017).[2]

The framework economists use to understand such conflicts can be dated back to contributions by, among others, Collier and Hoeffler (2004; 1998), Fearon & Laitin (2003) and Grossman (1991). The framework distinguishes the channels through which resources and their value influence conflict in opportunity costs and contest effects. If there are better outside opportunities, conflict becomes less desirable. If control over a resource becomes more profitable, contests for control increase conflict. Empirically, the literature focused on the aggregate country level for many years (e.g., Bazzi & Blattman, 2014; Brückner & Ciccone, 2010; Humphreys, 2005), but recent contributions at the micro-level have discovered large heterogeneities across different commodities and countries (e.g., Berman *et al.*, 2017; Berman & Couttenier, 2015). Dube & Vargas (2013), for instance, highlight differences between resource types; more specifically the higher the labor intensity, the lower the likelihood that higher prices trigger conflict.

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

[2] The Washington Post reports estimates of more than 92.000 casualties since 2001, see https://www.washingtonpost.com/news/worldviews/wp/2015/06/03/149000-people-have-died-in-war-in-afghanistan-and-pakistan-since-2001-report-says/?utm_term=.3810ccdcd9e9, last accessed August 28, 2019.

This paper uses 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 for the extent to which laws regarding the illegal production and trading of resources are enforced and for the number of groups competing for resource control. If laws are enforced, fewer people profit from higher prices and opportunity cost effects are small. When many groups are competing 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, which we then test empirically.

Most existing papers on illegal crops focus on countries like Colombia, where governments enforce laws against the production of those crops and several non-state groups are competing for control (e.g., Ibanez & Carlsson, 2010; Ibanez & Martinsson, 2013; Wright, 2018). We call this the resource-conflict-curse scenario, where, due to weak opportunity costs and strong contest effects, higher prices fuel conflict. In such a context, the empirical literature indeed finds that higher prices are associated with more conflict and violence (Angrist & Kugler, 2008; Dell, 2015; Mejía & Restrepo, 2015). In contrast, the Afghan conflict after 2001 is best characterized as a two-sided contest between one main insurgent group, the Taliban, and the government including the support by Western troops (see Trebbi & Weese, 2016). Government control over the country, and hence law enforcement, is very limited.

The first part of our paper shows that Afghanistan does not fit the resource-conflict-curse scenario. Instead opportunity costs dominate contest effects, and higher prices are on average associated with less conflict. By combining temporal variation in international drug prices with a new dataset on spatial variation in opium suitability (Kienberger *et al.*, 2017), we can measure changes in opium profitability across years and districts. Our main reduced form identification strategy exploits the fact that higher international prices have a larger effect in districts with a higher suitability, conditional on the overall price level. Moreover, we use additional strategies to assess the risk of any remaining bias. These include exploiting the relationship between depressant drugs like opium and stimulant drugs that are often consumed as complements. All strategies lead to the same result: a 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 the differential effect of international prices as well as 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%.

Our data allow us to identify if this effect is indeed associated with changes in opportunity costs. We use different waves of the National Risk and Vulnerability Assessment (NRVA) to show that the gains from higher opium profitability affect regular households or just a small elite. We find that higher prices consistently increase food consumption and living standards, and hence that a higher opium profitability does increase the opportunity costs of fighting for individuals. By exploiting a policy change in the Western military strategy around 2005, we also illustrate that the growing reliance of Afghan households on revenues from opium production amplified the conflict-reducing effect of higher opium prices.

The second part of the paper then exploits subnational variation in value added to further distinguish opportunity costs from contest effects, and highlights the importance of law enforcement and group control over a district. We argue that districts which not only cultivate opium in its raw form, but also process and trade it can capture a larger share of value added along the supply chain. This affects both the intensive margin (higher revenues) as well as the extensive margin (more people benefiting). If the contest effect based on group competition about territorial control dominates, we would expect relatively more fighting in those districts. Using geo-referenced data from the United Nations Office for Drugs and Crime (UNODC) on drug markets, labs, and potential trafficking routes, we proxy for how much value is added along the production chain with binary indices as well as with a network-based opium market access measure that captures how central a district is in the drug production network (Donaldson & Hornbeck, 2016). The results further support that opportunity cost effects dominate potential contest effects, as the conflict-reducing effect of higher prices is larger in those districts with a higher value added.

In the next step, we exploit heterogeneity in group control to distinguish between the three remaining scenarios in our theoretical framework. The scenario with the strongest negative conflict-reducing effect describes districts where laws are not enforced, and where one non-state group is in control. In this case, the higher the prices, the higher the opportunity costs that set an incentive for the group to avoid local conflict that might disturb production, and there are no or only small contest effects. In contrast, if the government enforces laws against drug production, fewer farmers profit from higher prices and some might turn to the Taliban for protection. If several groups compete about controlling lucrative production grounds, higher prices are associated with larger contest effects. Hence, the net effect of higher prices should be more positive in areas under government control and in those which are not controlled by a single group.

To proxy for whether a district is plausibly controlled by the Taliban, we use digitized maps on the homelands of Pashtuns and historical Taliban control prior to 2001. The Taliban were initially a Pashtun ethnic group, making it easier to establish presence in Pashtun districts (see Trebbi & Weese, 2016). Links from before 2001 also make it easier for the Taliban to reestablish their hold on a district. Based on Michalopoulos & Papaioannou (2014) and Lind *et al.* (2014), we use distance to major cities and to the major foreign military bases as a measure of government control and law enforcement. It seems that government control and law enforcement are limited to an area of around two hours travel time around Kabul and the military bases.

Based on these results, we divide the country into three parts resembling the three remaining scenarios in our framework – districts under government control, under Taliban control, and of limited statehood. In line with our expectations, we find that the conflict-reducing effect of higher opium prices is the strongest in districts that are more likely to be controlled by the Taliban after

2001. This suggests the group is acting as a stationary bandit (Olson, 1993; Sánchez De La Sierra, 2019a), which maximizes 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). Also in line with our framework, the net effect in the other two scenarios is significantly less negative, and indistinguishable from zero. The remainder of the paper shows that across a wide range of specifications and different proxies for conflict, the main results are robust to a large battery of robustness tests and alternative specifications.

One limitation of this paper is that we measure the effect of prices on *local* conflict. Berman *et al.* (2017) and Collier *et al.* (2009) point to the feasibility effect of higher resource prices, as insurgents can theoretically use the generated income from taxing opium production or trade to finance future fights. In Afghanistan, anecdotal evidence also suggests that the Taliban pool revenues through the group's central financing committee, which could be used to help finance attacks in other districts.[3] The extent of this revenue-sharing is unknown. While examining spatial spillovers in detail is not the focus of this paper, we still find no conflict-fueling effect when aggregating our data up to the larger province level. Considering the overall time trend in the country, higher prices are associated with less conflict. By imposing some assumptions, we can use our estimates to predict an alternative conflict path if prices would have remained at higher levels. If heroin prices in the year 2009 would have been as high as in the year 2000, there could have been more than 2100 fewer battle-related deaths in that year.

Section 2 discusses the contributions to the literature and relevant theoretical considerations; Section 3 introduces the data; Section 4 explains the empirical strategy. The main results are then presented in Section 5. We investigate channels and mechanisms in Section 5C, distinguish empirically between the three scenarios in Section 6, and discuss sensitivity tests in Section 7.

[3] See, e.g., http://www.huffingtonpost.com/joseph-v-micallef/how-the-Taliban-gets-its_b_8551536.html, accessed June 14, 2018.

Section 8 summarizes and provides policy implications.

2. Literature and theoretical considerations

A. Contributions to literature

We contribute to various strands of literature. First, 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; Brückner & Ciccone, 2010; Hodler, 2006; Miguel *et al.*, 2004) and the subnational level (e.g., Berman *et al.*, 2017; Berman & Couttenier, 2015; Caselli & Michaels, 2013; Harari & La Ferrara, 2018) have not reached a consensus about the direction in which higher prices influence conflict. In addition to providing causal evidence at the subnational level, we augment those studies by exploiting within-country differences in the share of value added and geo-referenced survey data to actually measure the relative importance of opportunity costs compared to contest effects.[4]

Second, an important strand of literature emphasizes existing cleavages between ethnic groups as an important driver of conflict (e.g., Esteban *et al.*, 2012a; Esteban & Ray, 2008; Michalopoulos & Papaioannou, 2016; Morelli & Rohner, 2015; Rohner *et al.*, 2013). In line with one of our arguments, Hodler (2006) highlights that as the number of ethnic groups competing for control increases, higher prices tend to lead to more conflict. We show that during the 2002-2014 period the conflict in Afghanistan was mostly bipolar between pro-Taliban and pro-government groups, with nearly no recorded fights within these two alliances after 2001. We also show that ethnic fractionalization does not influence our results. Thus we do not focus on the behavior of individual

[4] La Ferrara & Guidolin (2007) analyze the effect of conflict on diamond production, i.e., the opposite direction of causality. McGuirk & Burke (2017) emphasize a difference between factor conflict and output conflict. They focus on food, which is relevant as a consumption item in all regions, whereas opium is mostly an export crop in Afghanistan. Gehring & Schneider (2018) show that in established democracies, oil shocks do not lead to violent conflict, but their distribution can foster separatist party success in democracies.

groups within these two alliances (the focus of König *et al.*, 2017, for Congo).

Third, our analysis adds to the scarce causal evidence about the effects of illegal commodities. Despite the importance of the illicit economy, particularly in many developing and conflict-ridden societies, the literature provides very limited evidence on the effects of illegal commodity shocks on conflict. Closely related to our paper is the work by Angrist & Kugler (2008) and Mejía & Restrepo (2015), who find a positive effect of cocaine prices on violence in the Colombian context. As in our paper, Mejía & Restrepo (2015) also exploit heterogeneity in the effect of prices depending on the suitability to grow the illegal crop. Chimeli & Soares (2017) provide evidence that declaring mahogany trade as illegal in Brazil contributed to an increase in violence. 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 problem of establishing a credible government in a poor and economically constrained environment (e.g., Berman *et al.*, 2011a). Law enforcement 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 and affects cultivation only marginally (Ibanez & Carlsson, 2010; Mejía *et al.*, 2015). 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. Our finding that the strongest conflict-reducing effects are in districts controlled by a non-state group, the Taliban, which also provides conflict-solving mechanisms, relates to the literature on the provision of state-like institutions by non-state actors (Sánchez De La Sierra, 2019a,b).

Finally, we add to the growing literature on conflict and violence in Afghanistan (e.g., Child, 2019; Christia, 2012; Condra *et al.*, 2018; Lyall *et al.*, 2013; Sexton, 2016; Trebbi & Weese, 2016). 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. 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 negative impact of Western casualties on opium production over the 2002-2007 period. Our paper augments their findings by relying on a larger sample, a longer time period, a more comprehensive measurement of conflict, and more systematic identification strategies. While Lind *et al.* (2014) argue the Western forces are not involved in actions against opium producers and traffickers, our result on foreign military bases is in line with United Nations Security Council (UNSC) resolutions that describe their involvement.[5]

B. Theoretical considerations

The existing economic literature 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; 2004, Hirshleifer 1995). The first theory hypothesizes that 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 price of fighting becomes more lucrative, fighting will increase with higher resource prices. 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 and how much people 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 illicit crops on conflict, Figure 1 distinguishes four different scenarios that differ along two dimensions: whether laws against the illegal production and trading of those crops are enforced and whether groups are fighting for resource control. Depending on the scenario, opportunity cost effects are more or less likely to dominate contest effects, leading to a net

[5] Officially, the ISAF "is not directly involved in the poppy eradication or destruction of processing facilities, or in taking military action against narcotic producers" (see ISAF mandate: http://www.nato.int/isaf/topics/mandate/index.html, last accessed August 28, 2019). Nevertheless, the 2004 UNSC Resolution 1563 indicates that Western forces were involved in eradication during the 2002-2007 period (see „extending central government authority to all parts of Afghanistan, [...], and of combating narcotics trade and production", http://unscr.com/en/resolutions/doc/1563, last accessed August 28, 2019).

positive or negative effect on conflict.

When the government declares a crop illegal and enforces this law, this leads to at least three relevant differences as compared to the analysis of legal crops and conflict. First, the government as an actor does not profit directly from controlling the resource and from higher prices in the form of taxation. Second, measures of enforcement like eradication weaken the income effect for individual producers, hence decreasing opportunity cost effects. Third, rents will be distributed less equally, with higher rents for non-state groups that are willing to take the risk and compete in the illegal market. The importance of the contest effects then depends on the number of such non-state groups violently competing about controlling resources (Hodler, 2006). If several ethnic groups, insurgents or cartels, who profit from higher prices, fight for control, an increase in resource value leads to stronger contest effects than if one group controls an area (e.g., Esteban *et al.*, 2012a,b).

This suggests that if governments try to enforce laws and multiple groups compete for control, a resource-conflict-curse scenario becomes more likely. In such a case, which the figure refers to as scenario A, higher prices are linked to more conflict. The most reliable existing causal evidence on violence related to illicit crops exists for Mexico (Castillo *et al.*, 2014; Dell, 2015) and Colombia (Angrist & Kugler, 2008; Mejía & Restrepo, 2015), and largely reflects that scenario. Partly due to foreign support and pressure, the governments try to enforce laws in large parts of those 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.

We argue that, at large, Afghanistan, does not fit into this scenario A. Thus, we can distinguish between the three remaining scenarios B to D depending on law enforcement and group competition. Regarding enforcement, governments with limited state capacity might decide against enforcing rules if the costs are higher than the perceived benefits. Governments face a trade-off between the benefit of controlling an area and the risk of losing the support of farmers, who could even turn

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

to insurgents that provide protection of opium fields, processing labs or trafficking routes. It can be rational for a government with limited capacity not to enforce its rules in parts of the country.

Extensive qualitative evidence describes that in Afghanistan, the government is not able or willing to control and strictly enforce laws against opium production in significant parts of the country. In scenario B, with no single group clearly in control, and violent competition between various non-state groups, we expect stronger contest effects. 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.

Still, during our sample period, many districts are reported to be either controlled by pro-government forces or by the Taliban. Thus, many districts will resemble scenario C or D without groups competing for resource control. Scenario C represents districts where state and military capacity are sufficiently strong, so that the government controls a district and decides to enforce bans on opium production. Regarding the net effect of higher prices, we thus expect weak contest effects, but also smaller opportunity cost effects.

In contrast, scenario D describes districts where one insurgent group, which profits from the illegal crop, established control. Trebbi & Weese (2016, p. 5) support the relevance of this scenario, as "insurgent activity in Afghanistan is best represented by a single organized group", which is the Taliban. As production is *de facto* legal in this scenario, the opportunity cost effects related to higher prices are stronger.[6] At the same time, contest effects are smaller as the group has an incentive to act as a stationary bandit who establishes monopolies of violence to sustain taxation contracts (Sánchez De La Sierra, 2019a). More than 65% of the farmers and traffickers in southern Afghanistan stated that the Taliban offer 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. Anecdotal evidence also describes that the Taliban implement conflict-solving mechanisms to minimize violence that would potentially disturb the profitable production process.[7] For these reasons higher prices decrease conflict most notably in this scenario.

Whether the net effect in the country is negative and how large it is depends on the size of opportunity cost effects. Dube & Vargas (2013) show that higher prices of labor-intensive goods reduce conflict more than price increases for more capital-intensive goods. With high labor intensity, more people profit from higher prices, otherwise gains might accrue only to a small elite group. The corresponding distinction in Afghanistan is opium versus wheat production.Opium production is much more labor-intensive than wheat, the main legal alternative (Lind *et al.*, 2014; UNODC, 2013). Mansfield & Fishstein (2016, p. 18) report "opium requiring an estimated 360

[6] This case also resembles the case of legal products with little tensions between groups in a country like Norway. The case of Norway helps to illustrate the importance of accounting for group competition. One reason why Norway is able to profit from its oil resources is that there are little tensions between its regions about the distribution of oil revenues.

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

person-days per hectare, compared to an average of only 64 days for irrigated wheat." Opium revenues are also a crucial source of income (Felbab-Brown, 2013), and according to UNODC (2009), at least one out of seven Afghans is somehow involved in cultivation, processing or trafficking.

While the gross wheat-to-opium per unit price-ratio ranges between 1:4 to 1:27 (UNODC, 2005, 2013), opium production is also more costly. Mansfield & Fishstein (2016) show that whether opium or wheat is more profitable depends on yearly variation in the relative prices, and on the district-specific suitability to produce either of the two crops. Accordingly, a relative decline in opium prices causes marginal producers in some districts but not others to shift towards wheat production and decreases labor demand.[8] Those Afghans owning land lose a complementary source of income in addition to cultivating crops for subsistence. Tenant farmers and cash-croppers do not even have this alternative or back-up option; for them joining the Taliban, who pay a minimal salary, or at least supporting the group with shelter or local expertise, can be the only viable alternative.[9]

In the remainder of the paper we empirically test the predictions from our theoretical considerations. The first main part is to estimate the causal effect of a higher opium profitability on conflict. To understand this relationship, we examine the influence and relevance of opportunity cost effects relative to contest effects. Second, we run some tests to empirically distinguish between the three scenarios in our framework, and show the conditional effect of opium profitability in each case.

[8] 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. The effect of a price increase for wheat on conflict is ambiguous. While the income of few exporting farmers increases, most farmers grow wheat only as a staple crop and households who are net buyers of wheat are negatively affected (Mansfield & Fishstein, 2016).

[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/ (last accessed August 28, 2019) 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, last accessed August 28, 2019).

3. Data description

Conflict data: The UCDP Georeferenced Event Dataset (GED) is our primary source for different conflict indicators.[10] It includes geocoded information, based on media reports, on the "best estimate of total fatalities resulting from an event" (Croicu & Sundberg, 2015; Sundberg & Melander, 2013).[11] As illustrated in Table B.2, 94% 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% of all cases are classified as one-sided conflicts with the Taliban as the perpetrator and civilians as the victims. We differentiate between these different types in Section 7.

In Afghanistan, there are 34 provinces (ADM1, see Figure C.1), further divided into 398 districts (ADM2). Our analysis is conducted at the district level. 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.[12] Using 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 our indicators, Figure G.6 in Appendix G shows a high correlation with a subjective conflict indicator derived from the NRVA household survey. In addition, we show that all our results hold when using the SIGACTS (Significant Activities) data from Shaver & Wright (2016) in a robustness test.

[10] We prefer this 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/problematic for Afghanistan (e.g., Eck, 2012).

[11] An event is defined as "[a]n incident where armed force was [used] by an organized actor against another organized actor, or against civilians, resulting in at least 1 direct death at a specific location and a specific date" (Sundberg & Melander, 2013; Croicu & Sundberg, 2015). These battle-related deaths include dead civilians and deaths of persons of unknown status. For more details see Appendix A. Weidmann (2015) documents some under-reporting of media-based conflict data in areas with low population density compared to the SIGACTS data, which are based on military reports and not publicly available. Media-based datasets could also be downward biased with regard to the intensity of conflict, especially in high conflict areas.

[12] At the country level, the thresholds 25 and 1000 are more common. Berman & Couttenier (2015), in contrast, use a one-BRD threshold at a small grid cell level. District are somewhere in between these two extremes with regard to size. 1000 is thus clearly to high, and a one-BRD threshold might suffer from misreporting and falsely coding conflict.

Opium and wheat suitability index: 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). The left hand side of Figure 2 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, Table F.6 shows the results without weighting.[13] Figure 2 also shows the distribution of wheat suitability on the right hand side. There is a positive correlation of 0.58 between the two, but also clear differences.

Opium suitability (based on Kienberger *et al.*, 2017) Wheat suitability (based on FAO GAEZ)

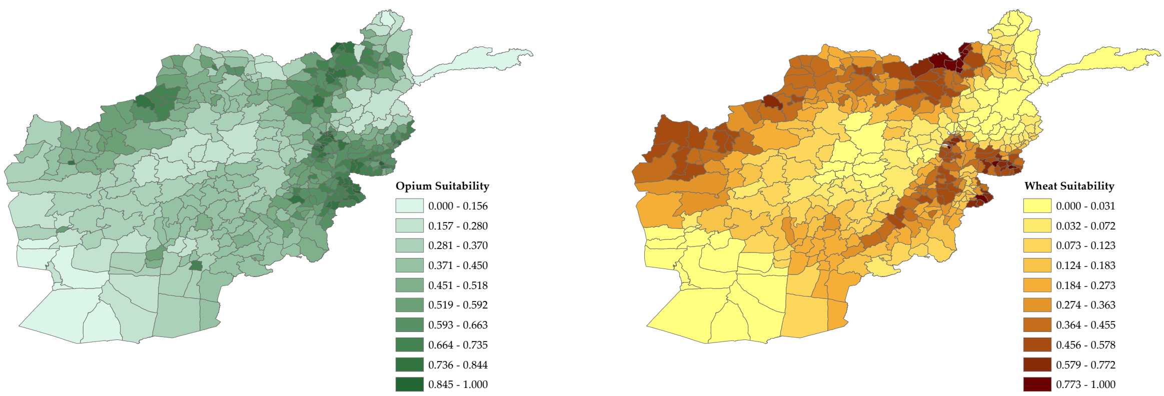

Figure 2: Distribution of opium and wheat suitability across districts (weighted by population)

Drug prices: We use international consumer prices for heroin, as well as for 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

[13] As alternative tests, in Table F.9 we interact the unweighted opium profitability measure with the population density or binary indicators of very low population density (below the 10th or 20th percentile). While the sign of the interaction terms are in line with the expectation that suitability matters more with higher density, they are far from being statistically significant, highlighting that this choice is not essential.

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.[14] The complementary stimulant drugs we consider are cocaine, amphetamine, and ecstasy. We define a complement price index as the average of the three.

Opium cultivation and opium revenues: As a more direct measure, we also 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 is 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 then equals 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 waves 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 official 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 on the household's economic situation.

All these variables, and the ones we use in other parts of the paper, and their sources are described in more detail in Appendix A.

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

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.[15] Thus, our baseline specification focuses on the reduced form intention-to-treat (ITT) effect of opium profitability. Opium profitability combines the temporal variation in prices with the district-specific suitability to grow opium. We also use opium revenues, instrumented with opium profitability, in an IV setting to quantify the size of the effect.

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 (Appendix F, Figure F.2). The outcome variable, $conflict_{d,t}$, is the incidence or the intensity of conflict in district d in year t based on the different thresholds. 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

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

We always control for $wheat\; profitability_{d,t-1}$, defined as wheat price times wheat profitability, since wheat is the main (legal) alternative crop that farmers grow throughout Afghanistan.

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

Afghanistan contributes less than 1% to the global wheat supply, so we can consider the international price as exogenous (as, e.g., Berman & Couttenier, 2015). Our baseline equation includes year-fixed effects τ_t, district-fixed effects δ_d, and province-times-year-fixed effects $\tau_t\delta_p$. Appendix F shows that our results are not affected by the inclusion of control variables, which suggests that the fixed effects capture most biasing variation. We also show that our results remain robust to excluding wheat profitability.

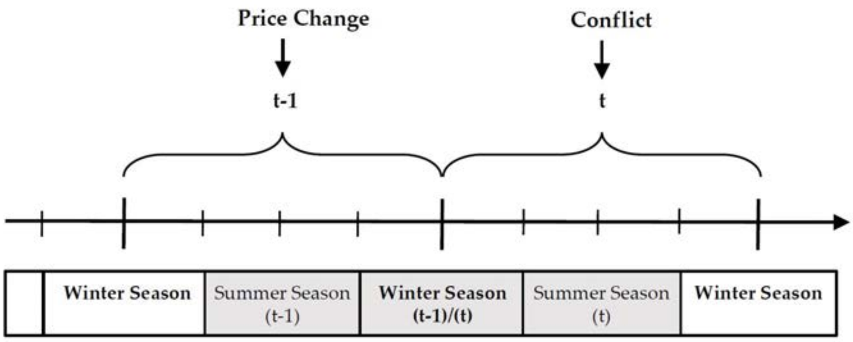

Figure 3: Timing - How price changes in year $t-1$ affect production, revenues, and conflict

There are two main growing seasons for opium in Afghanistan, the winter season starting in fall and the summer season starting around march (Mansfield & Fishstein, 2016). International price changes plausibly influence opium cultivation and revenues in the same and following year, as Figure 3 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.[16]

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

In the following, we discuss (i.) that the movements of prices over our sample period is mostly driven by changes in demand, (ii.) show that international prices of complement drugs, due to

[16] 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 for time for price shocks to affect local production. Appendix E shows that using prices in (*t*) would yield comparable results.

common demand shifters, correlate positively with the international heroin price, (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 4 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.

The graph provides several important insights. First, there are variations between the years, but all prices decline over time. This common pattern suggests that prices are, on average, 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. Second, there is an overall positive correlation between the international heroin price and the complement index (significant at the 1%-level). If drug-specific supply changes would be the decisive influencing factors for the price changes we exploit, we should not observe this co-movement of prices.[17]

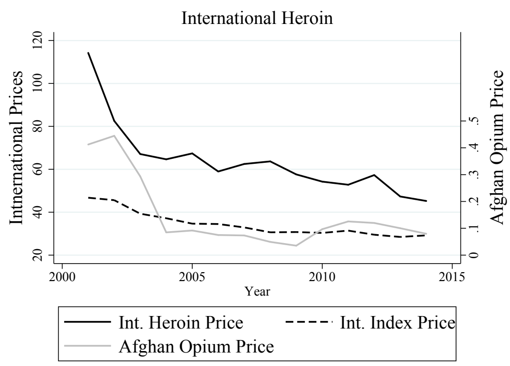

Figure 4: Variation in international and local prices over time

Third, local Afghan prices clearly 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 mean-

[17] Appendix F shows that we can replicate our results using de-trended opium prices, but this eliminates a large share of the economically relevant variation over time.

ingful changes in actual opium revenues at the district level.[18] We can also test directly whether international consumer price changes have statistically 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 3, Table 1 considers lagged effects in column 1, as well as the moving average over (t) and (t-1) in column 2.

Table 1: Effect of international price changes on opium revenues, 2002-2014 period

	Outcome: (t) (1)	**Outcome: (t) + (t-1)** (2)
Opium Profitability (t-1)	2.336	2.489
	(0.827)	(0.749)
Number of observations	5149	5085
Adjusted R-Squared	0.482	0.565

Notes: The dependent variable opium revenues is in logarithms. Column (1) presents lagged effects. Column (2) reports lagged and contemporaneous effects by defining the outcome as the moving average, i.e. (revenues(t)+revenues(t-1))/2. Opium Profitability is defined as the interaction between the normalized drug prices (in logarithms) and the suitability to grow opium. All models include year- and district-fixed effects. Standard errors clustered at the district level are displayed in parentheses.

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).

[18] To put this into perspective, some reports indicate that 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/politics/news/afghanistan-the-making-of-a-narco-state-20141204, accessed August 28, 2019. In Appendix F in Table F.10 we replace revenues with opium cultivation in hectares. The estimations do not include province-times-year-fixed effects, as the actual district level cultivation data from which revenues are calculated is gathered at the province level.

C. Visualizing the identification strategy

Our treatment variable is the interaction term $opium\ profitability_{d,t-1} = drug\ price_{t-1} \times suitability_d$. The main effects of the two variables constituting the interaction term ($drug\ price_{t-1}$, $opium\ suitability_d$) are 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 E.3 shows that a lead term $opium\ profitability_{t+1}$ turns out to be very close to zero and insignificant.

Figure 5 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 immediately 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, however, 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 a higher suitability experience a much higher increase in conflict when prices and opium 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. Overall shocks on the quantity of opium supplied from Afghanistan that influence the international (heroin) price

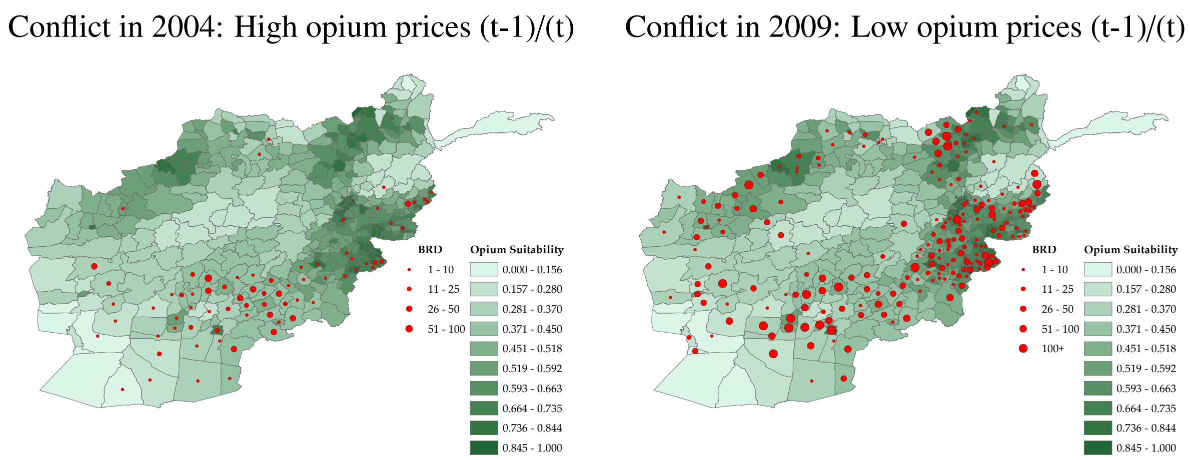

Figure 5: Intensity of conflict in districts with high and low suitability to grow opium

p^O_{t-1} are captured by the year-fixed effects τ_t. They 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 that Figure 5 suggests a negative effect of opium profitability on conflict, we are most concerned about a potential downward bias of the coefficient.

Consider eradication campaigns as an example. They 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, Felbab-Brown, 2013; Mejía *et al.*, 2015), 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.3 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 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 τ_t, (see Appendix F). Moreover, Appendix F shows that the results hold when including district level time-varying covariates, capturing climate conditions as well as other baseline covariates frequently used in the literature such as luminosity (as a proxy for development) and population.

Regarding changes over time, we would be concerned if by coincidence 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 for reasons unrelated to opium (see, e.g., Christian & Barrett, 2017). We alleviate this concern in five different ways. First, Appendix F shows the results with de-trended opium prices, which exhibit less variation, but support the main finding. Second, we randomize prices across years and find that random assignment yields no significant relationship with coefficients being distributed around zero. Third, 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. Fourth, Section 7 uses the increase in legal opioid prescriptions in the United States, which affect heroin prices in a plausibly exogenous way, in an IV approach. Fifth, Table F.17 controls for high- and low-suitability-specific (alternatively high- and low-production-specific) linear time trends. 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.

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

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

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

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 β 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 that the impact of common demand shifters is sufficiently strong, and 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 β and whether b^O is an upper or lower bound estimate.

The assumptions regarding the effect of common demand shifters and the exogeneity of supply for complements are plausibly fulfilled for opium. Drugs are classified as stimulants (uppers) or depressants (downers). Regarding the first assumption, experts agree that there is a high share of polydrug users (see also Jofre-Bonet & Petry, 2008); users that combine a stimulant and a depressant (EMCDDA, 2016). As heroin is as depressant, we use the prices of the three important stimulants: cocaine, amphetamine, and ecstasy (EMCDDA, 2016).[19] The trend in prices presented in Figure 4, but also correspondence with experts, validate that changes in demand have the largest impact on drug price changes. Regarding the second assumption, there is no evidence suggesting that ecstasy and amphetamines are produced in Afghanistan (UNODC, 2013), and cocaine is exclusively produced in South America. Thus, district-specific supply shocks in Afghanistan are not systematically related to those for the complements.

To sum up; if there would be a remaining bias related to OV_{t-1}, there is 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. Thus 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 β.

[19] 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).

5. Results

A. Main results - reduced form using opium profitability

We now turn to our main results in Table 2. Column 1 uses conflict intensity, measured as the log of battle-related deaths (BRD), and columns 2 to 5 the conflict incidence with smaller and larger BRD thresholds. Panel A reports results using the local opium price. Panel B uses the international heroin price to compute opium profitability (our baseline specification). Panel C reports results using the complement price index.

The regression coefficients are very much in line with the suggestive graphical evidence in Figure 5. 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 scarce, 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.

The results are also 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 in 2009. The size of this effect is similar to Mejía & Restrepo (2015), who find that in Colombia a 10% change in cocaine revenue leads to a 5% *increase* in homicides, but points in the opposite direction. We take this as evidence that, relating back to our theoretical considerations, Afghanistan does indeed not reflect the resource-conflict-curse scenario A in Figure 1, where higher prices fuel conflicts due to contest effects. Rather, the effect of higher opportunity costs of fighting seems to dominate contest effects on average.

Table 2: 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)
Number of observations	5174	5174	5174	5174	5174
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 equal across all panels. Standard errors are in parentheses (clustered at the district level). See Table F.5 in the Appendix for estimates using cocaine prices.

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.[20]

[20] For wheat, the main legal alternative crop, we find positive coefficients in most regressions (see Table F.14). However, contrary to opium price-related shocks, the point estimates of wheat price-related shocks sometimes switch signs and turn negative. Two reasons are that, contrary to opium, wheat is relatively less labor intensive and it is often also imported from abroad. Most households are net buyers of wheat (Mansfield & Fishstein, 2016), and are thus negatively affected by price increases.

B. Instrumental variable specifications using opium revenue

To put the results and their size into perspective, we also examine the direct effect of district-level opium revenues on conflict. As explained above, computing revenues requires the use of cultivation estimates – partly estimated based on provincial level data - and regional yields. Thus, these estimations exclude province-times-year-fixed effects that cause weak IV problems, but still include district and year fixed effects. While IV estimations can help with measurement error, one potential issue here is that measurement error could also be non-random if, for instance, the data collection was affected by the risk of conflict. Thus, we regard the IV results mostly as a quantification that accompanies our main results.

In addition to providing a simple OLS estimate, we first instrument the endogenous variable opium revenues with our opium profitability measure. We then introduce a second instrument, the interaction of legal opioid prescriptions in the United States with the suitability to grow opium. Prescribed opioids can affect heroin prices in two ways. Heroin prices could increase if a larger legal opioid supply led to a net increase in demand for heroin. The net effect on heroin prices could also be negative if more users substitute heroin with a legal opioid. Empirically, substitution seems to dominate, as a larger supply of legal opioids strongly correlates with lower heroin prices (r=-0.83). Based on this strong correlation, we use legal opioid prescription interacted with the suitability as our second instrument.[21] Technically, while the reduced form approach in Table 2 presents the ITT effect, Table 3 identifies the local average treatment effect (LATE) for compliers. Having two IVs enables us to compare the two LATEs, as well as estimating an over-identification test when they are jointly included.

Panel A of Table 3 presents the OLS and IV results, and panel B reports the first stage estimates. The OLS effect is negative and statistically significant, but rather small in size. The first

[21] Note that the empirical literature is divided about the general causal relationship between legal opioids and heroin use (Compton *et al.*, 2016).

stage estimates in columns 2 and 3 reveal that both instruments work well, with their effect being statistically significant at the 1%-level. Both are strong instruments as indicated by the Kleibergen-Paap F-statistic exceeding the critical threshold of ten, proposed by Staiger & Stock (1997). When we include both IVs in one regression, the coefficients keep their sign but the second instrument dominates. This is not surprising, when considering the strong correlation, and given that the change in prescriptions causes part of the change in heroin prices. The Hansen J test in the joint specification does not reject the overidentifying restrictions.

Turning to the second stage results presented in panel A, instrumenting with opium profitability leads to a negative coefficient that is larger than the OLS coefficient and remains statistically significant. The estimate suggests that a 10% revenue increase leads to a decrease in BRD of about 1.5%. Using legal opioid supply instead of heroin prices leads to a very similar, but slightly larger, effect. A 10% increase is associated with a decrease in BRD of about 1.93%. Using both instruments in column 4 leads to a coefficient that is virtually identical with column 3. The LATE using legal opioid supply, which is clearly exogenous to the Afghan conflict, is very similar in size to the effect using the international heroin prices. This also further assures us about the validity of our main approach.[22] Applying two different approaches, the reduced form and IV, we consistently find that higher prices are causing a reduction in conflict. This validates that, on average, opportunity costs of fighting dominate contest effects in Afghanistan.

[22] Figure F.5 shows the partial leverage plot of the corresponding first stage regression. We also show IV results for a different timing in Appendix F.

Table 3: OLS, 1st and 2nd stage IV results for Opium Revenue (t-1), 2002-2014 period

	OLS (1)	**IV** (2)	**IV** (3)	**IV** (4)
		Panel A: OLS and IV 2nd stage		
(log) Revenue (t-1)	-0.011	-0.153	-0.193	-0.192
	(0.005)	(0.083)	(0.086)	(0.086)
Kleibergen-Paap F stat.		16.379	13.047	8.141
Hansen J p-val.				0.220
		Panel B: 1st Stage		
Opium Profitability (t-1)		2.922		0.149
		(0.722)		(0.726)
Legal Opioids (t-1)*Suitabillity			-15.384	-14.915
			(4.259)	(5.581)

Notes: Models include 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. Note that the supply of legal opioids is strongly negatively correlated with heroin prices. Standard errors are in parentheses (clustered at the district level).

C. Opportunity versus contest effects

C.1. Opportunity costs at the household level

All tests above provide an indication of the importance of opportunity cost effects. An important question remaining is to what degree households and farmers benefit from a higher opium profitability. Given the high labor intensity of opium relative to wheat as its main alternative, 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 an Afghan nationally-representative household survey, the National Risk and Vulnerability Assessment (NRVA). We construct several indicators of household living standards, in accordance with the literature. This allows us to analyze whether opium profitability translates into better living standards, which would provide evidence for the opportunity cost hypothesis.

Figure 6 plots the coefficients for opium profitability for six different regression models with the outcome variable indicated in the legends. We find evidence that dietary diversity and food expenditures increase with a higher opium profitability.[23] We also consider indicators that are not as volatile as food consumption. In years following high opium prices, households in districts with a 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 a higher opium profitability. The corresponding regression results are presented in Table F.19.[24]

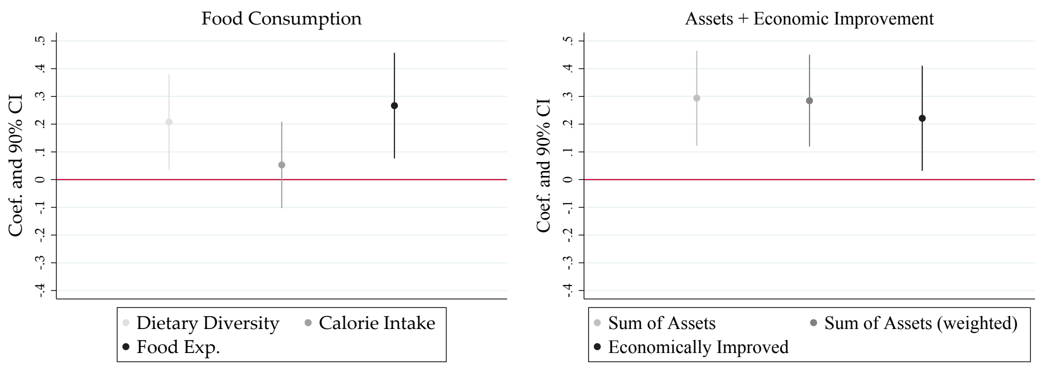

Figure 6: Effect of opium profitability (t-1) on standard of living indicators in (t)

C.2. Opportunity costs and contest effects conditional on value added

This section uses differences in value added between districts as a second test that the resource-conflict scenario A does not describe the situation in Afghanistan during our sample period. In addition to showing that the average effect of opium profitability is negative, we use 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.

[23] This suggests that quality of food consumption improves. We also construct food expenditure adjusted for spatial price differences using the Paasche or Laspeyres price indexes, since households in different districts face different prices. The results are robust to this choice as can be seen in Table F.19.

[24] Results are also robust when accounting for household survey weights as presented in Figure F.6.

Hence, higher prices are associated with a relatively 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 high value added districts.

Using UNODC reports, we geo-referenced 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 7.a) 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.

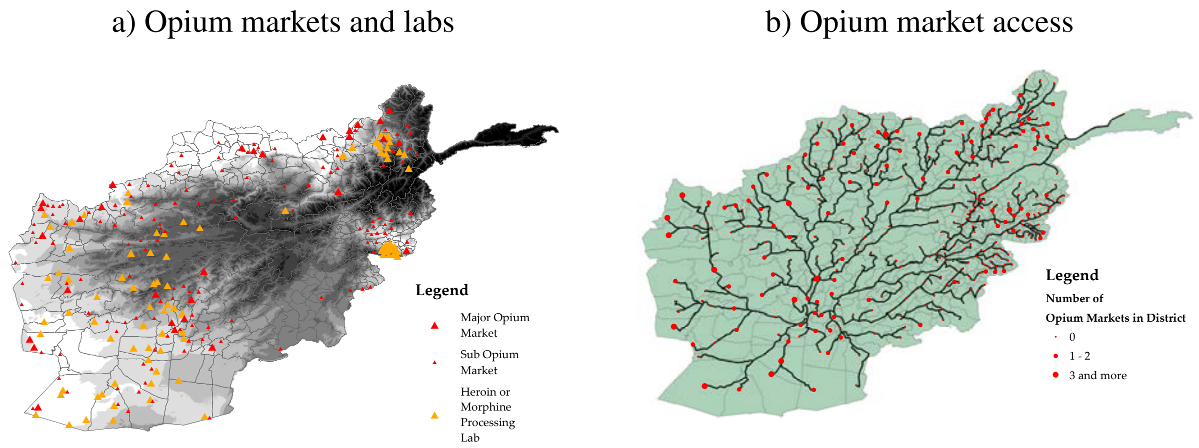

Figure 7: Value added and market access, measures for Table 4

Notes: Left side – Opium markets, heroin and morphine processing labs.
Right side – Opium market access. 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.

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 θ 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 7.b) visualizes this approach.

Table 4, 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.

Table 4: 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)
	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)

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 in the column heading. The partial bivariate product of the suitability times X are captured in the fixed effects as both are time-invariant. For definitions of the variables X please see Appendix A. The number of observations is 5,174 in every regression, the adjusted R-squared varies between 0.649 and 0.652. Standard errors are in parentheses (clustered at the district level).

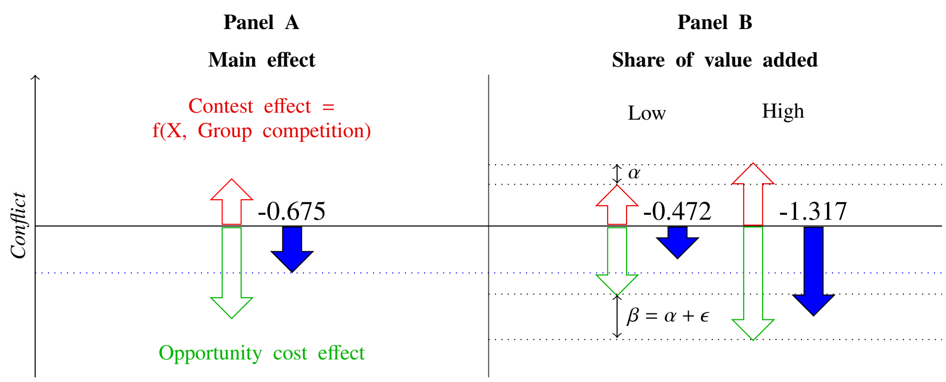

Figure 8: Opportunity cost effects dominate contest effects (2002-2014)

Notes: Panel A: Opportunity cost effects dominate contest effects on average. Refers to Table 2, panel B, column 1. Panel B: Opportunity costs effects increase more in the share of value added than contest effects. Refers to Table 4, panel A, column 1.

Figure 8, panel A, shows the average main effect, and panel B visualizes the marginal effects of opium profitability in districts with lower and higher value added, based on Table 4, panel A, column 1. They indicate that 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.

6. Scenarios based on law enforcement and group competition

Our theoretical framework in Figure 1 distinguishes between four scenarios. We showed that Afghanistan does not resemble the resource-conflict-curse scenario A. This leaves three remaining scenarios. In scenario B, neither the government nor the Taliban have firm control over a district. As laws against the illegal production and trading of opium are not enforced, we expect large contest effects to be offset by large opportunity cost effects. Scenarios C and D describe districts either controlled by pro-government forces or by the Taliban. In scenario C, where the government enforces laws, we expect weaker opportunity cost effects, but also weak contest effects. We expect the strongest conflict-reducing effect in scenario D, where districts are strictly controlled by one insurgent group, the Taliban. In this case, the opportunity cost effects should be large, and contest effects small.

To proxy for government control, we measure whether a district is within a specific proximity to a major Western military base or to one of the five largest cities.[25] 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 for the presence of state institutions. We use the road- and terrain-adjusted travel time as the most precise measure in the main analysis, and linear distance as a robustness test in Appendix F.

We use two proxies for Taliban control. First, whether a district has been controlled by the Taliban in years prior to 2001 (Dorronsoro, 2005). We expect that due to the common past and

[25] Another option is to use an interaction with distance directly. However, we do not expect distance to have a linear effect. Computing a cut-off this way would require interacting with linear distance and a squared distance term. However, given that opium profitability is already an interaction, this would be a quadruple interaction. The data do not have enough power to estimate this precisely.

existing networks, the Taliban will, all else equal, find it easier to expand their power again in those districts. Second, Trebbi & Weese (2016, p. 5) argue that support for the Taliban as the main insurgent group is best explained by ethnic boundaries. We exploit the fact that, even though they also feature members from different ethnicities, the Taliban were initially a Pashtun group. For all those variables, we use a variety of different sources, ranging from maps provided by experts at the UN to data from the American military, satellite pictures and newspaper reports or information from Weidmann *et al.* (2010) on whether Pashtuns are present in a district. For each dimension of control, we create three binary variables for the centroid of a district being within one, two, or three hours travel time, respectively. Figure 9 visualizes the data. Appendix H documents the steps involved in the construction and all sources in detail.

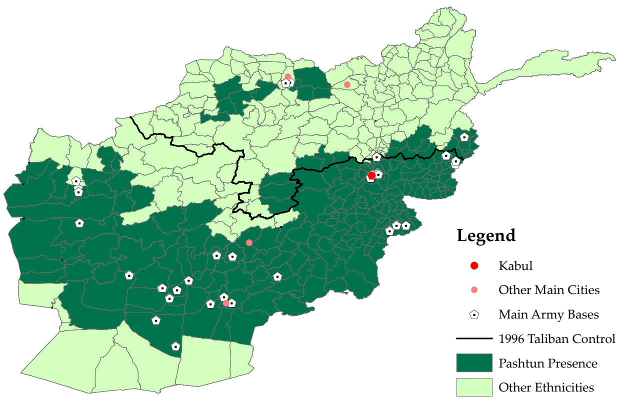

Figure 9: Pashtun presence, main army bases, and major cities

Notes: Presence of the ethnic group Pashtun (Source: GREG). Red dots indicate the capital Kabul and other major cities: Hirat, Kandahar, Kunduz, Mazari Sharif. White symbols with black dots indicate the location of a foreign military base, for which we could track location, opening and closing date (sources in detail in the Appendix A). The area south of the thick black line was controlled by the Taliban prior to 2001 (Dorronsoro, 2005).

Table 5 shows the corresponding regression results. In each specification, we interact *opium profitability* with one of the proxy variables. We interpret a significant interaction effect as a sign that the relative size of opportunity cost versus contest effects changes due to law enforcement and group competition. Panel A begins by showing the results 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 a higher opium profitability is significantly more positive. Thus, as predicted, a higher degree of law enforcement seems to lower the opportunity cost effect.

Panel B starts by evaluating whether there are also 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. This is in line with qualitative evidence. 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). Although the official government claims that "poppy cultivation only takes place in areas controlled by the Taliban", and a US counter-narcotics official reports that "(president) Karzai had Taliban enemies who profited from drugs, but he had even more supporters who did."[26]

[26] See, http://www.rollingstone.com/politics/news/afghanistan-the-making-of-a-narco-state-20141204, accessed August 28, 2019. The same source also reports a case where a drug trafficker possessed a letter of safe passage from a counter-narcotics police leader, and a new director of an anti-corruption agency was revealed to be a formerly convicted drug trafficker.

Table 5: Government versus Taliban control, 2002-2014 period

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Law enforcement – Government control					
	Proximity to military bases			Proximity to Kabul		
Interaction with	**Travel Time 3D**			**Travel Time 3D**		
	1 if ≤ 1	**1 if ≤ 2**	**1 if ≤ 3**	**1 if ≤ 1**	**1 if ≤ 2**	**1 if ≤ 3**
Opium Profitability (t-1)	-0.952	-0.930	-0.767	-0.831	-0.893	-0.826
	(0.319)	(0.350)	(0.426)	(0.308)	(0.315)	(0.325)
Opium Profitability (t-1)*X	1.780	1.170	0.364	2.510	1.685	0.588
	(0.467)	(0.419)	(0.498)	(0.892)	(0.671)	(0.508)
	Panel B: Limited and No Law Enforcement – Other main cities and Taliban control					
	Proximity to other cities			Potential Taliban control		
Interaction with	**Travel Time 3D**			**Pashtun**	**Former Territory**	
	1 if ≤ 1	**1 if ≤ 2**	**1 if ≤ 3**	**Presence**	**All**	**w/o north**
Opium Profitability (t-1)	-0.698	-0.616	-0.612	0.312	-0.207	-0.221
	(0.301)	(0.309)	(0.330)	(0.365)	(0.372)	(0.365)
Opium Profitability (t-1)*X	0.731	-0.389	-0.207	-1.723	-1.013	-1.063
	(1.080)	(0.579)	(0.502)	(0.412)	(0.477)	(0.491)

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 in the column heading, as well as province-times-year- and district-fixed effects. The other main 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.20 in the appendix for estimates based on linear distance. See Table F.21 in the appendix for the NRVA definition of Taliban control proxied by Pashtun presence. Standard errors are in parentheses (clustered at the district level).

The second part of panel B shows the results for interactions with the measures of Taliban control. As predicted, the interaction terms are consistently negative and significant in all three specifications. This suggests not only that opportunity costs are higher as people in Taliban areas profit from higher prices, but also that competition about resource control is limited in those areas. Again, this is in line with qualitative evidence. A local farmer in such a district describes that "the Taliban have a court there to resolve people's problems," and that "the security situation is good for the people living there."[27] Other sources verify the link between the Taliban and the drug production process, with the group sometimes even providing seeds, tools, and fertilizer. A local

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

Taliban leader is described as "just one of dozens of senior Taliban leaders who are so enmeshed in the drug trade"[28] and that the one "drug cartel is the Taliban."[29]

a) Scenarios B, C and D b) Marginal effect of Opium Profitability

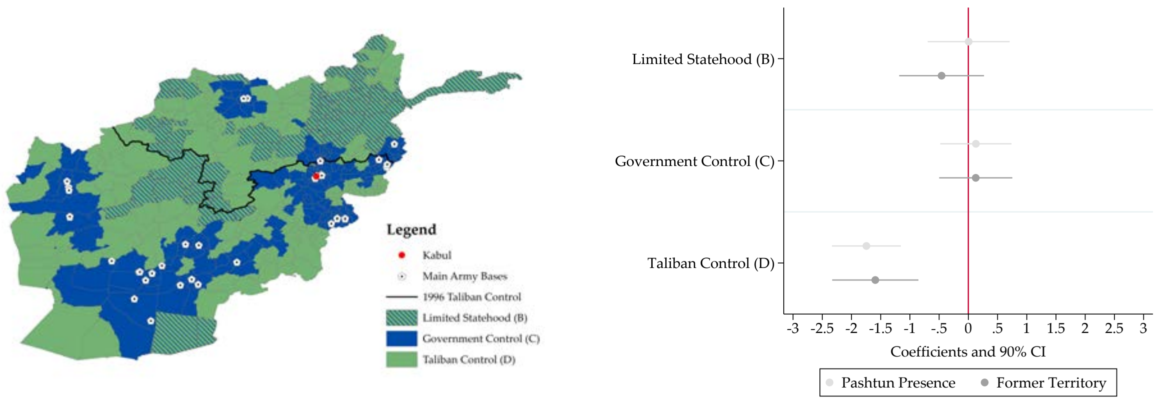

Figure 10: Treatment effect conditional on group control

Notes: Taliban control are districts that were either under Taliban control prior to 2001 or feature a significant share of the Pashtun ethnic group. We classify government control as districts within two hours travelling distance from a military base or the capital Kabul, based on the results in Table 5. A district that fulfills conditions for both Taliban and government control is coded as government control. The right hand side visualizes this using prior control; in Figure G.4 we show the same map using Pashtun presence to determine Taliban control. The left hand side shows the marginal effects for opium profitability corresponding to the three types of districts. Corresponding regression results are in Table F.22.

As a next step, we aim to use these insights to empirically distinguish the effect of opium profitability in the three relevant scenarios in our framework. To do that, we categorize districts accordingly. We code them as under government control if 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 it is either within the area they used to control prior to 2001 or if Pashtuns are present in the district, and if travel time to Kabul or a military base is more than 2 hours. The remaining districts are coded as the ones with limited statehood. Figure 10. b) displays the resulting geographic division on a map.

We use this division to code binary identifiers, and run a regression to compute the marginal effect of opium profitability on conflict for districts in each category. Figure 10. a) plots marginal

[28] See https://www.nytimes.com/2017/10/29/world/asia/opium-heroin-afghanistan-taliban.html and https://thediplomat.com/2016/10/how-opium-fuels-the-talibans-war-machine-in-afghanistan/, last accessed August 28, 2019.

[29] See, https://qz.com/859268/americas-failed-war-on-drugs-in-afghanistan-is-threatening-to-doom-its-war-on-terror-as-well/, last accessed August 28, 2019.

effects. The results are in line with what our theoretical framework predicts. While opium profitability has the largest conflict-reducing effect in districts under Taliban control, the coefficients estimates in districts under government control or with limited statehood are indistinguishable from zero and from each other.[30]

7. Further results and sensitivity analysis

This section explores further results and the sensitivity of our main findings. The most important tests are explained here with tables and figures reported in Appendix E. All other sensitivity tests along with corresponding tables and figures are presented in Appendix F.

Aggregate effects: Our results do not rule out that local Taliban forces use part of the revenue extracted from the opium business to finance anti-government conflict and attacks. Local revenues could partly be used for violent operations if there are relevant targets within a district. Of course, revenues need not fully remain within the district, and could be pooled to enable countrywide operations. Figure 11 does not indicate such a mechanism at large scale. On average, an increase in opium revenue correlates with a decrease in casualties. Table E.2 shows a regression aggregating all our data at the provincial level and again finds a negative coefficient for opium profitability. We would expect the opposite pattern if higher revenues in one district were causing more conflict in neighboring districts of other provinces.

[30] Our theoretical framework makes no explicit prediction about the net size of the effect in each scenario. It predicts that the most negative effect occurs in scenarios where one group, that also benefits from higher prices, is in control, and that the net effect in scenarios B and C should be more positive. Figure F.7 in Appendix E validates that, in contrast to the results in C.1. Note that if the heroin price yields an upper bound estimate, the small positive effect in scenario C might be slightly overestimated. As all marginal effects are estimated using the same prices, assessing differences between scenarios is still feasible.

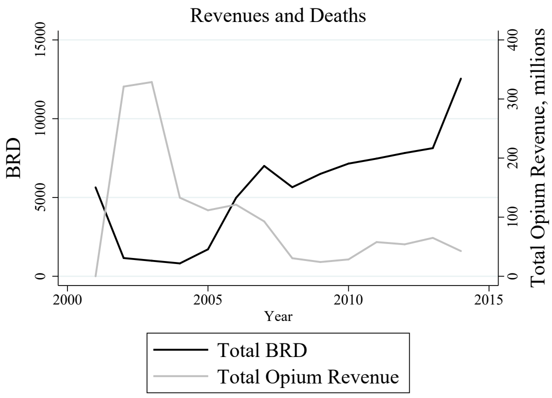

Figure 11: Correlation between opium revenues and battle-related deaths at country level

Counter-factual when freezing prices: In Figure 12, we fix price levels at the year 2000 to compute a hypothetical counterfactual if prices would not have fallen as much as they did over our sample period. To do so, we use the β estimate from the baseline specification in Table 2, panel B, column 1 on conflict intensity. More precisely, for a given t we calculate $\hat{BRD}_{t,2000} = \sum_d BRD_{d,t} + \beta \times suitability_d \times BRD_{d,t} \times \frac{price_{2000} - price_{t-1}}{price_{t-1}}$. The figure illustrates that without the strong decline in heroin prices, there would have been an increase in battle-related-deaths, but to a smaller extent than the drastic rise that the country actually experienced.

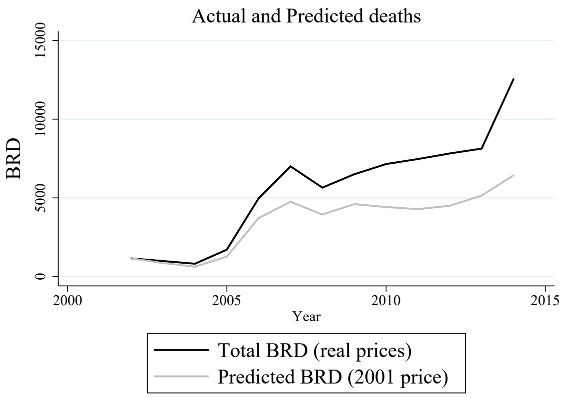

Figure 12: Counterfactual number of battle-related deaths when fixing prices at higher initial level

Timing of shocks: We consider different lag structures by including opium profitability in periods $t + 1$, t, and $t - 1$ at the same time (either all three or in groups by two) in Table E.3, with $t + 1$ testing for pre-trends. Table E.3 shows that opium profitability in t and $t - 1$ is related to a conflict-reducing effect, while the lead effect of international opium prices in *t+1* interacted with the suitability to grow opium has no significant effect on conflict. This is reassuring and supports the causal order and mechanism that we hypothesize.Table E.4 shows that including the contemporaneous and lagged variables individually yields very similar coefficients, with slightly larger coefficients for our preferred timing ($t-1$).

Types of fighting: Table B.2 shows that almost all events reported by UCDP are conflicts between the Taliban and the Afghan government, i.e., two-sided violence involving the state. Table E.5 compares our baseline results to a more distinct analysis of distinguishing actors and deaths per conflict side. Column 2 shows that casualties caused by Taliban violence against civilians – about 4% of all casualties – exert a smaller and statistically insignificant negative effect. For the majority of violent events, which are conflicts involving Taliban and pro-government groups, there is a clear negative and significant effect on total casualties (column 3) and casualties individually for each side (column 4 and 5). Very importantly, we compare our results using conflict data based on military reports. Table E.6 reports the reduced form results when using conflict indicators from the SIGACTS dataset. For the three different types of events, direct fire, indirect fire, and IED (both normal and in logs), we find the same pattern as for the different UCDP GED conflict measures that we apply in our baseline regressions.

Outcome variable (conflict onset and ending): We consider heterogeneous effects of opium profitability on onset and ending of conflict events. Bluhm *et al.* (2016) point to the importance of differentiating between the probability of switching from one conflict state to another as, for instance, from peace to conflict versus from conflict to peace.[31] Thus, we also measure the effects for conflict incidence (panel A), onset (panel B), and ending (panel C) in separate models. Results are presented in Table E.7. Panel A verifies our main finding with a linear probability model by showing similar results when using conditional logit. In panel B, we find that opium profitability

[31] Berman & Couttenier (2015), for instance, argue that conflict persistence is very low at their level of analysis (a cell equivalent to 55 times 55 km at the equator) compared to country level data. Consequently, they do not include the lagged dependent and rather estimate separate models for onset and ending. We report transition probabilities of the different conflict intensities from peace to war in Table B.4.

consistently reduces the likelihood of a conflict onset for conflict measured up to a threshold of 25 battle-related deaths. For conflict ending, we only find a significantly positive effect for smaller conflicts. These results indicate that a positive income shock and more opium cultivation raise the likelihood that an ongoing small conflict ends, and reduces the likelihood that conflicts break out.

Ethnic fractionalization: Table E.8 examines whether the conflict-reducing effect is stronger or weaker depending on how many ethnic groups are present. We compute both a binary indicator for whether one or more groups are present, as well as a continuous variable counting the number of groups in a district. This is done based on ethnic homelands from Weidmann *et al.* (2010), as well as using survey based ethnicity measures. 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.

Sensitivity analysis: Appendix F discusses all other tests in detail. We show that our results are robust to (i.) modifications of the treatment variable, like using the unweighted suitabilities and de-trended price data, (ii.) modifications of the empirical model, as including different sets of fixed effects, (iii.) replacing revenues with cultivation, (iv.) adjusting the timing in the IV analyses and instrumenting opium profitability rather than revenues, (v.) different choices on how to cluster standard errors, (vi.) 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, (vii.) and to dropping potential outliers like border districts, the two southern provinces Kandahar and Helmand, and to leave out each year and province one at a time. Finally, to rule out problems caused by non-linear trends in the time series (see Christian & Barrett, 2017), we randomize the time-varying variable (international heroin price) across years, as well as the district-specific suitability across districts, and find no evidence for problematic trends in these placebo tests.

8. Conclusion

This paper provides new evidence on the mechanisms linking resource-related income shocks to conflict, thus adding to one of the key strands of the conflict literature (e.g., Berman *et al.*, 2017; Berman & Couttenier, 2015; Brückner & Ciccone, 2010; Morelli & Rohner, 2015). For this purpose, we focus on Afghanistan, which is an exemplary case of a conflict-ridden country with a weak labor market, limited state capacity, and difficulties to form stable governing coalitions between existing groups. By employing new data and a variety of identification strategies, we provide new insights on the conflict in Afghanistan, that allow for a better understanding of conflict more broadly.

Overall, our reduced form results show that, on average, 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. These results are robust to a battery of sensitivity tests, including IV estimations that exploit legal opioid prescriptions in the United States. 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 results add to the literature in several ways. First, we verify the insight from Dube & Vargas (2013) and Dal Bó & Dal Bó (2011) on the role of the commodity's relative labor intensity and show that opium, which is a highly labor intensive crop, indeed matters for household living standards. Second, our results augment the scarce literature on the effect of illegal resource-shocks (e.g., Angrist & Kugler, 2008; Chimeli & Soares, 2017; Mejía & Restrepo, 2015). We document that the degree to which the illegality of a crop influences conflict decisively depends on law enforcement and the degree to which groups compete for control (Hodler, 2006). Third, we add to studies on countries with ongoing conflicts like Iraq (Berman *et al.*, 2011a; Condra & Shapiro, 2012), Mexico (Dell, 2015), or Colombia (Wright, 2018).

While we do not claim that our findings can explain the conflict in Afghanistan in all its complexity, they augment our prior understanding based on the existing insights (e.g., Bove & Elia, 2013; Child, 2019; Condra *et al.*, 2018; Lyall *et al.*, 2013). Although we make no strong claims beyond our observation period, the findings are in line with the spread of conflict in Afghanistan in the last years, which featured falling prices and thus lower opium profitability. We use results at the province level and country level to verify that higher opium revenues do not, on average, spill over and fuel conflict in other districts. Based on our local estimates, we predict a counter-factual scenario of the country with higher opium prices and a significantly smaller increase in conflict.

At the same time, it is too simplistic to conclude that opium production should not be considered a potential problem. Instead, we aim to highlight the importance of understanding the underlying trade-offs in order to derive sound policy measures. 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 that strict law enforcement measures in production countries have little to no effect on cultivation (Clemens, 2008; Ibanez & Carlsson, 2010; Mejía *et al.*, 2015), as long as drug demand from consumer countries remains high.

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Online appendix

A. Definition and sources of the variables

Any Lab: We count all types of heroin laboratories. This variable takes on the value 1 if there is at least one lab in a district i, and 0 otherwise. As described in Appendix H, we georeference maps from UNODC reports regarding drug markets, labs, and trafficking routes, assign coordinates to the labs, and later compute district averages. Source: UNODC (2006/07, 2014, 2016).

Any Military Base: This variable takes on the value 1 if there is at least one open military base in a district i in year t, and 0 otherwise. The approach is described in detail in Appendix 35. Note that we are most likely not capturing all existing locations, as we did not receive the exact information about opening and closing for all military bases. Opening and closing dates were coded with the available information; if there was no information about shutting down a base we assume it is still active. Source: For the more well-known bases, we use Wikipedia's GeoHack program; for the less well-documented bases, we use Wikimapia and Google Maps satellite data.

Battle-Related Deaths (BRD): This variable measures the best (most likely) estimate of total fatalities resulting from an event, with an event being defined as "[an] incident where armed force was [used] by an organized actor against another organized actor, or against civilians, resulting in at least 1 direct death at a specific location and a specific date." A direct death is defined as "a death relating to either combat between warring parties or violence against civilians." Note that the Uppsala Conflict Data Program Georeferenced Event Dataset (UCDP GED) only includes BRD of events that belong to a dyad ("two conflicting primary parties or party killing unarmed civilians") that reached in total at least 25 BRD within one year. If the dyad generated events with less then 25 BRD in the previous or subsequent years, they are still counted if the dyad had reached the 25 BRD threshold in another year. We construct a continuous measure (log of BRD) and binary outcomes from all BRD of any party or any type of violence (state-based, non-state or one-sided violence). To capture the lowest level of conflict in a binary measure, we classify a district-year observation with at least five BRD *small conflict*. We then increase the threshold to 10 for the next level of conflict intensity (*low conflict*). In analogy to the threshold used in macro level analyses, we call a district-year observation *conflict* if there are more than 25 BRD. At the top, we take a threshold of 100 BRD for the most severe level of violence what we call *war*. Since UCDP GED provides information on the parties and the type of violence we also construct specific outcome measures according to those categories. Besides different measures of incidence, we also construct measures on onset and ending. We define conflict onset as the incidence of a conflict in a district, where there was no conflict in the previous year ($Conflict_{i,t} = 1|Conflict_{i,t-1} = 0$). Years of ongoing conflict are set to missing. In analogy, a conflict ending is defined when conflict persisted in the previous year but not in the current year ($Conflict_{i,t} = 0|Conflict_{i,t-1} = 1$). We also set the ending variable missing for observations which have been at peace in the previous year and remained in peace in the current year, following the standards in the literature. Source: UCDP GED (Sundberg & Melander, 2013; Croicu & Sundberg, 2015).

Calorie Intake: We use a questionnaire in which women self report amounts, frequencies, and sources of a large set of food items, to construct measures on calorie intake and food insecurity. We multiply amounts consumed with kcal values for that food item to get total household calorie intake. Total household daily calorie intake is divided by the number of members that were resident and ate at least dinner regularly in the household during the last seven days to get per capita measures. Source: For kcal values, we use the CSO & The World Bank (2011). The questionnaire responses are from the NRVA women's questionnaire (CSO, 2005, 2007/08, 2011/12).

Consumer Price Index (CPI): Source: For the Euro area (19 countries), we draw data from the OECD (2016); for the remaining countries (2010 = 100), we use the World Bank (2016).

Dietary Diversity: This variable varies between 0 and 8, with eight indicating a high food diversity. According to Wiesmann *et al.* (2009, p. 5) "Dietary diversity is defined as the number of different foods or food groups eaten over a reference time period, which in my case is one week, not regarding the frequency of consumption." We classify the different food items from the survey into eight food groups as explained in Wiesmann *et al.* (2009). These groups are staples, pulses, vegetables, fruit, meat/fish, milk/dairy, sugar, and oil/fat. Source: NRVA (CSO, 2005,2007/08,2011/12).

Distance/Proximity/Travel Time to Kabul (capital) and Kandahar, Kunduz, Jalalabad, Hirat, and Mazari Sharif (next five largest cities): For the proximity to Kabul and other main cities we define binary indicators for the distance being smaller than 75 km (1 if < 75) or smaller than 100 km (1 if < 100). In analogy to these categories, we construct indicators for the travel time to Kabul or one of the other main cities falling below 2 or 3 h. We use the shapefiles provided by the Afghan statistical authority on the 398 Afghan districts. Note that the shapefiles available at www.gadm.org do not reflect the current status of administrative division in Afghanistan, and instead we use the one from Empirical Studies of Conflict (ESOC Princeton, https://esoc.princeton.edu/files/administrative-boundaries-398-districts). To compute the distances, we first create the centroid of each district polygon. To compute road distances we combined road shapefiles from the official Afghan authorities with street maps from open street map, which were improved by voluntary contributors to close gaps in the official maps. 3D-distances were computed using elevation data from the US Geological Survey (https://www.usgs.gov/land-resources/eros/coastal-changes-and-impacts/gmted2010?qt-science_support_page_related_con=0#qt-science_support_page_-related_con, accessed July 9, 2018). We add the elevation information to the shapefile containing the roads, and then compute and save three-dimensional distances. We then use the network analyst in ArcGIS to set up a network between all district centroids, clipping centroids that do not overlap with a street in that district that is closest with regard to the as-the-bird-flies distance. Then, we compute the most efficient routes using road distances in two- and three-dimensions. The distances are saved in a matrix and exported in a table that is further processed in Stata. For the variable "distance to other main cities" we use the minimum distance to any of the five cities. For travel time we use the distinction of roads in three classes (motorways, rural, urban), and assign commonly used values for average traveling speed for that road type based on three sources. Sources: The first source is UNESCAP (http://www.unescap.org/sites/default/files/2.4.Afghanistan.pdf, p. 14, last accessed August 28, 2019) which assumes that the speed on motorways is 90 km/h and on urban roads 50 km/h. The second source is IRU (https://www.iru.org/apps/infocentre-item-action?id=560&lang=en) which states no limits except for urban areas with 50 km/h. The 3rd source is WHO (http://apps.who.int/gho/data/view.main.51421, last accessed August 28, 2019) reporting 90 km/h for rural. We choose the following average traveling speeds, assuming that no strictly enforced limits and little traffic on motorways (120 km/h), and accounting for some (90km/h-10km/h) and moderate traffic in cities (50-20 km/h). Thus our main choice is the following. Motorways: 120 km/h, rural: 80 km/h, urban: 30 km/h. These choices are not perfect, but we verify that our results hold with other variations as well.

Drug Prices (International): Variables are normalized so that prices vary between 0 and 1. We use data on average prices per gram across all available countries in Europe for the following drugs: amphetamines, cocaine, ecstasy, heroin (brown). To construct the average price of alternative drugs we use a mean of the three stimulant drugs amphetamines, cocaine, and ecstasy. For the analysis we convert all drug prices into constant 2010 euros per gram. We then normalize the prices by using a linear min-max function such that all prices vary between 0 and 1. Source: European Monitoring Center for Drugs and Drug Addiction (EMCDDA).

Economically Improved: This variable refers to the question "How do you compare the overall economic situation of the household with 1 year ago?" A value of 1 indicates much worse, 2 slightly worse, 3 same, 4 slightly better, and 5 much better. This is a self-reported measure. Source: NRVA (CSO, 2005, 2007/08, 2011/12).

Ethnic Groups: We record the majority and minority ethnic groups on a district level. We have used the GIS-coordinates of all ethnic groups. Source: The "georeferencing of ethnic groups" (GREG) dataset Weidmann *et al.* (2010). It relies on maps from the classical "Soviet Atlas Narodov Mira" from 1964, and is very extensively used for the construction of ethnolinguistic fractionalization indices. GREG is a georeferenced dataset containing the coordinates of the group boundaries of 1120 ethnic groups. One advantage and disadvantage of the data is that it is capturing group locations in the 1960s. This is an advantage as it ensures that the boundaries are not endogenous to changes during our period of observation. It is partly a disadvantage if groups and countries changed over time. In Afghanistan, the country boundary did not change. Ethnic group populations certainly change to some degree over time, so that all variables more precisely capture the historic homelands of ethnic groups rather than the current settlement areas. An alternative definition measures the number of different native languages (including Pashtun) present in a district using NRVA 2003 houshold survey.

Ethnically Mixed: We construct two measures of whether a district is ethnically mixed. The first variable is the number of ethnic groups; the second is a binary indicator, which takes a value of 1 if the number of ethnic groups is larger than 1, and 0 otherwise. For more extensive methodology, see Ethnic Groups.

Ethnic Trafficking Route: The variable takes on the value of 1 if there is a potential trafficking route leading from a district to at least one unofficial border crossing point without crossing the ethnic homeland of another group. The underlying intuition is that trafficking is cheaper and significantly easier to conduct, and the accruing additional profits are higher, if there is no need to cross the area of other ethnic groups to transport over the border. Source: For data on unofficial border crossings, we used the UNODC; for information about the homelands of ethnic groups, we used the (GREG) dataset (Weidmann *et al.*, 2010).

Events (ACLED): ACLED differentiates between different types of fighting. We define an event to be non-violent if it belongs to one of the types: "Non-violent rebel activity", "Riots/Protests", or "Non-violent activity by a conflict actor". Violent events are those that involve a battle between government and rebels or involve violence against civilians. We construct the variable "All Events", which includes both types of events and a variable "Violent Events" referring to the latter category only. Finally we also construct a variable that included only "Violence against Civilians". For all event variables we take the logarithm. From ACLED (Raleigh *et al.*, 2010).

Food Expenditures (Paasche/Laspeyres): Precise food amounts were merged with local prices to estimate household food expenditure. We show three food expenditure measures, which are all measured in constant 2011 prices, i.e., prices of the 2011/12 survey wave. Only food items that appear in all three waves are included to build the measure. The first measure "Food Exp. 2011 Prices" does not account for spatial price differences. "Food Exp. 2011 Prices, Paasche" and "Food Exp. 2011 Prices, Laspeyres" adjust for spatial price differences, since households in different districts face different prices. Missing values of district prices are replaced by the province median, which in case of missing values has been replaced by the national median price. For close to all reported food items, prices have been given in the district questionnaire. Prices vary at the district level. Following the literature, we include food items from all possible sources, i.e., purchased food or food in form of gifts etc. Information on food and drinks consumed outside the house (from the male survey section) are also included in the total food expenditure measures (adjusted for inflation and regional price differences depending on the measure). Expenditures are measured in per capita terms by dividing the total household food expenditure with the number of households (resident and ate at least dinner regularly in the household during the last seven days). We use the section on food consumption from the NRVA women's questionnaire as this section offers precise amounts per food item. Source: NRVA women's and male's questionnaire and district questionnaire (CSO, 2005, 2007/08, 2011/12).

Inflation, GDP Deflator: We use a GDP deflator for the United States with 2010 as the base year. Source: World Bank (2016).

Insecurity/Violence Shock: The share of sampled households per district that have experienced a shock due to insecurity/violence. At the household level, the variable takes on the value of 1 if the household has experienced an insecurity/violence shock. Source: NRVA survey (CSO, 2005, 2007/08, 2011/12).

Legal Opioids: Since most single publications do not cover our whole sample period, we want to cross-verify the numbers using a variety of sources. Source: A main source is the US CDC Public Health surveillance report 2017 (https://stacks.cdc.gov/view/cdc/47832, last accessed August 28, 2019). Other important sources were Manchikanti *et al.* (2012); Kenan & Mack (2012); Dart *et al.* (2015).

Local Opium Price: We utilize reports of (monthly) province level dry opium prices by farmers and by traders as well as country-wide yearly data on fresh opium farm-gate prices weighted by regional production. The province level opium prices of farmers and traders are highly correlated, with a correlation coefficient close to 1 (0.998). The correlation between the country level farm-gate price and the province level farm-gate price is 0.66, significant at the 1%-level. While the province level prices are only available from 2006 to 2013 and for a subset of provinces, they are still very helpful in identifying whether international prices are correlated with local prices. We use the country-wide yearly data on fresh opium farm-gate prices in Afghanistan interacted with the suitability as one proxy for opium profitability in our regressions in Table 2, panel A. Source: Annual Afghanistan Opium Price Monitoring reports (UNODC).

Luminosity: We use this variable as a proxy for GDP and development (Henderson *et al.*, 2012). The yearly satellite data are cloud-free composites made using all the available smooth resolution data for calendar years. The products are 30 arc second grids, spanning -180 to 180 degrees longitude and -65 to 75 degrees latitude. A number of constraints are used to select the highest quality data for entry into the composites: Data are from the center half of the 3000 km wide OLS swaths. Lights in the center half have better geolocation, are smaller, and have more consistent radiometry. Sunlit data and glare are excluded based on the solar elevation angle, Moonlit data based on a calculation of lunar illuminance. Observations with clouds are excluded based on clouds identified with the OLS thermal band data and NCEP surface temperature grids. Lighting features from the aurora have been excluded in the northern hemisphere on an orbit-by-orbit manner using visual inspection. Source: Version 4 DMSP-OLS nighttime lights time series, National Oceanic and Atmospheric Administration-National Geophysical Data Center (NOAA/NGDC, https://www.ngdc.noaa.gov, last accessed August 28, 2019). We take the logarithm.

Markets (Major/Sub) and Sum of all Markets: The first variable takes on the value 1 if there is at least one major or sub-market in district i, and 0 otherwise. The second variables counts the sum of all opium markets in a district (both sub and major). Source: UNODC reports on drug markets, labs, and trafficking routes (e.g., UNODC 2006/07, 2014, 2016).

Market Access: Market access for a district i is computed as $MA_i = \sum_{j=1}^{N} dist_{i,j}^{-\theta} W_j$. W_j is the importance of district j proxied using either the number of opium markets or mean luminosity (or population). $dist_{i,j}$ are the distances between the district and the other districts and θ is the factor discounting other districts that are further way. We use a factor of 1, as in Donaldson & Hornbeck (2016). To take account of the topography and mountainous terrain in Afghanistan, we compute distances using the two-dimensional road network (Market Access 2D) as well as a three-dimensional road network when adjusting for elevation (Market Access 3D).

Mixed/Taliban Territory 1996: The binary indicator on Taliban Territory that we create takes on the value 1 if a district belongs to the territory that was occupied or under the control of the Taliban in 1996, and 0 otherwise. A second indicator (Taliban Territory 1996 - No North) takes on a value of 1 if the district is exclusively occupied by the Taliban and is characterized by no presence of the Northern Alliance. We use an existing map which indicates the territory of the Taliban in 1996 as well as the territory of other major groups of the Northern Alliance (Dschunbisch-o Islami, Dschamiat-i Islami, Hizb-i Wahdat). We georeferenced the map and aligned it with the district boundaries; in many cases, the division was quite clearly aligned or overlapping with a district boundary, in the other cases we chose the closest district boundary. We classify a district as a Mixed Territory if it is part of the Taliban 1996 territory and part of the territory of any of the three groups belonging to the Nothern Alliance. Source: The map is from Dorronsoro (2005), and more details can be found in Giustozzi (2009).

Opium Cultivation and Revenues: This variable measures opium cultivation in hectares. Data at the district level is an estimate from the data at the province level. We use logged values for opium cultivation and for revenues. From opium cultivation and the respective yields we were able to calculate actual opium production at the district-year level. We also constructed opium revenues by multiplying opium production in kg with the fresh opium farm-gate prices at harvest time in constant 2010 EU/kg. Source: Annual Opium Poppy Survey (UNDCP, 2000) and Afghanistan Opium Survey (UNODC, 2001-2014).

Opium Suitability: This is an index with possible values ranging between 0 and 1 which acts as a proxy for potential of opium production based on exogenous underlying information about land cover, water availability, climatic suitability, and soil suitability. The environmental as well as climatic suitability to cultivate opium poppy (Papaver somniferum) is characterized by different factors such as the prevailing physio-geographical and climatic characteristics using climatic suitability based on the EcoCrop model from Hijmans *et al.* (2001). The factor determined to be most important by experts is land cover (S1, 0.41 – the sum of the weights equals 1.0), followed by water availability (S2, 0.28) and climatic conditions (S3, 0.21) respectively. This is in line with additional studies previously carried out by UNODC and described in the World Drug Report (2011) for Myanmar. The data and the index itself was modeled on a $1km^2$ resolution and then aggregated to the district units by an area weighted mean approach. The original indicator values were normalized using a linear min–max function between a possible value range of 0 and 100 to allow for comparison and aggregation. Only the land cover indicator was normalized integrating expert judgments through an Analytical Hierarchy Process (AHP) approach. The four indicators were then subsequently aggregated applying weighted means (weights were verified through expert consultations building on the AHP method). None of the input factors constituting the index is itself to a major degree affected by conflict, which is the outcome variable. Consequently, the index values by district can be considered as exogenously given.

We weight the opium and wheat suitabilities with the (lagged) population distribution within the districts. This is helpful as, for instance, the south features large desert areas and at the same time concentrated areas with dense population, and accounting for the suitability in uninhabited desert areas might be misleading (although our results are not significantly affected by this choice).

Source: The index was developed in the context of a study in collaboration with UNODC; and is described in detail in a publication in a geographical science journal (Kienberger *et al.*, 2017).

Pashtun: Our binary indicator takes on the value 1 if Pashtuns are present to any degree in a district i, regardless of whether they were the majority group, and 0 otherwise. The GREG polygons can contain more than one ethnic group. For more extensive methodology, see Ethnic Groups.

Population: This is a minimally-modeled gridded population data that incorporates census population data from the 2010 round of censuses. Population estimates are derived by extrapolating the raw census estimates to a series of target years and are provided for the years 2000, 2005, 2010, 2015, and 2020. We use the ipolated data from 2000 till 2015. We then take the logarithm. Source: The Center for International Earth Science Information Network - CIESIN - Columbia University. 2016. Gridded Population of the World, Version 4 (GPWv4): Administrative Unit Center Points with Population Estimates. Palisades, NY: NASA Socioeconomic Data and Applications Center (SEDAC) http://dx.doi.org/10.7927/H4F47M2C, last accessed August 28, 2019.

Ruggedness: We calculate the average ruggedness index for every district. While ruggedness refers to the variance in elevation, we also use raw elevation data. Source: Elevation data from NASA Shuttle Radat Topography Mission (SRTM) data set. The data on terrain ruggedness is the same that was used in Nunn & Puga (2012), although we use it on a more disaggregated level. The dataset and a detailed documentation are available at http://diegopuga.org/data/rugged/, last accessed August 28, 2019.

Sigacts Conflict Data: These variables measure SIGACTS (Significant Activities) in a given district based on military reports; SIGACTS are defined as direct fire (DF), indirect fire (IDF), and improvised explosive device (IED) events. DF attacks can be defined as close combat events that are characterized by the use of weapons like small arms or rocket-propelled grenades. IDF attacks, including mortars and rockets, can be heard within a large area, but are less precise when being launched from great distances. While DF and IDF involve fighters, IEDs involve less risk for the perpetrators. IEDs can be placed around roads and directed against moving targets, for instance pro-government convoys. Source: We use data from Shaver & Wright (2016).

Southern Provinces: Dummy variable which we assign a value of 1 for districts located in one of the two provinces Kandahar and Hilmand, and 0 otherwise.

Sum of Assets (weighted): The number of assets the households possess over a set of assets that is constant over 3 survey waves. This set consists of Radio/Tape, Refrigerator, TV, VCR/DVD, Sewing Machine, Thuraya (any phone), Bicycle, Motorcycle, Tractor/Thresher, and Car. Sum of Assets weighted is the sum of asset weighted by the proportion of households not possessing the specific item. Source: NRVA (CSO, 2005, 2007/08, 2011/12).

Vegetation Health Index (VHI): We compute an index that captures inter-annual variations in drought conditions, the vegetation health index (VHI) of FAO (Van Hoolst *et al.*, 2016). VHI is a composite index joining the Vegetation Condition Index (VCI) and the Temperature Condition Index (TCI, Kogan 1995). Low values of VHI represent drought conditions. This is a combination of low values of the observed VCI (relatively low vegetation) and higher values of the TCI (relatively warm weather). For details see Van Hoolst *et al.* (2016). The VHI is calculated from data of Advanced Very High Resolution Radiometer (AVHRR) sensors on board of the National Oceanic and Atmospheric Administration (NOAA) and Meteorological Operational Satellite (METOP) satellites. It is superior to simply using precipitation data, which do not directly measure drought conditions, require assumptions about the linearity of the effect and, in particular in Afghanistan, have severe limitations in terms of quality and resolution. The index is based on earth observation data and is available on a monthly basis with a resolution of 1 km^2. As cultivation and harvest times differ within Afghanistan, we use the yearly average. The remote sensing based index is operationally used to monitor drought conditions in the Global Early Warning System (GEWS), low VHI values indicate drought conditions. For a similar approach to a VHI, see Harari & La Ferrara (2018).

Wheat Price (International): Prices are period averages in nominal US dollars with 2005 as the baseline. We use benchmark prices, representative of the global market. They are determined by the largest exporter of a given commodity. Source: International Monetary Fund (IMF) Primary Commodity Prices database (IMF, 2005-2017).

Wheat Suitability: Seven different soil quality ratings (SQs) are calculated and are combined in a soil unit suitability rating (SR, %). The SR represents the percentage of potential yield expected for a given crop/Land Utilization Type (LUT) with respect to the soil characteristics present in a soil map unit of the HWSD and is depending on input/management level. The FAO-GAEZ (2012) model provides for each crop/LUT a comprehensive soil suitability evaluation for all the soil units contained in the Harmonized World Soil Database (HWSD). This is done by the use of individual soil quality ratings. Source: Global Agro-ecological Zones

(GAEZ v3.0) by the Food and Agriculture Organization of the United Nations (FAO-GAEZ 2012). Details are provided on http://www.fao.org/nr/gaez/about-data-portal/agricultural-suitability-and-potential-yields/en/, last accessed August 28, 2019. Go to the section "Agro-ecological suitability and productivity" to find the suitability we use and access the data portal for downloads.

B. Descriptive statistics

Table B.1: Descriptive statistics

	Observations	Mean	Stand. Dev.	Min.	Max.
(log) BRD	5174	1.11	1.54	0.00	8.20
Small Conflict	5174	0.31	0.46	0.00	1.00
Low Conflict	5174	0.23	0.42	0.00	1.00
Conflict	5174	0.14	0.34	0.00	1.00
War	5174	0.03	0.18	0.00	1.00
(log) Taliban-Civilians BRD	5174	0.08	0.37	0.00	4.14
(log) Taliban-Government BRD	5174	1.05	1.52	0.00	8.20
(log) Government BRD caused by Taliban	5174	0.53	0.94	0.00	8.03
(log) Taliban BRD caused by Government	5174	0.77	1.33	0.00	6.39
(log) Wheat Profitability	5174	-0.48	0.46	-2.11	0.01
(log) Opium Profitability - Int. Heroin	5174	-1.52	0.66	-4.61	-0.00
(log) Opium Profitability - Local Opium	5174	-1.04	0.70	-4.61	0.01
(log) Opium Profitability - Int. Complement	5174	-1.30	0.56	-3.17	-0.00
(log) Opium Profitability - Int. Cocaine	5174	-1.15	0.67	-4.61	-0.00
Opium Suitability	5174	0.53	0.18	0.00	1.00
Wheat Suitability	5174	0.55	0.23	0.00	1.00
(log) Cultivation	5174	1.38	2.15	0.00	6.91
(log) Opium Revenue	5149	4.26	5.83	0.00	16.98
Luminosity	4776	0.49	3.03	0.00	58.01
Vegetation Health Index (VHI)	5173	124.08	23.20	51.28	191.99
(log) Population	5174	3.96	1.24	0.44	9.58
Ruggedness in 1000	5148	299.18	216.54	4.48	877.01
Any Military Base	5174	0.04	0.20	0.00	1.00
Market Access - Opium Market 2D	5174	4.47	1.10	2.24	11.23
Market Access - Opium Market 3D	5174	2.63	0.69	1.33	6.93
Market Access - Luminosity 2D	5174	6.51	4.86	1.85	41.26
Market Access - Luminosity 3D	5174	6.47	4.84	1.85	41.24
Major/Sub Market	5174	0.27	0.44	0.00	1.00
Sum of all Markets	5174	0.40	0.85	0.00	8.00
Any Lab	5174	0.13	0.34	0.00	1.00
Ethnic Trafficking Route	5174	0.52	0.50	0.00	1.00
Distance to Kabul: Linear	5148	277.05	181.54	0.00	817.64
Distance to Kabul - Road 2D	5174	345.03	212.05	0.00	959.78
Distance to Kabul - Road 3D	5174	347.47	213.08	0.00	964.48
Travel Time to Kabul - 2D	5174	7.53	5.91	0.00	28.40
Travel Time to Kabul - 3D	5174	7.57	5.94	0.00	28.45
Pashtuns	5174	0.74	0.44	0.00	1.00
Ethnic Groups - 1 if Mixed	5174	0.59	0.49	0.00	1.00
Ethnic Groups - Number	5174	1.93	0.97	1.00	5.00
Mixed Territory 1996	5174	0.04	0.20	0.00	1.00
Taliban Territory 1996	5174	0.58	0.49	0.00	1.00

Notes: The sample is based on the specification in Table 2, column 1.

Table B.2: Type of violence and fighting parties

	Frequency (1)	Percent (2)
	Conflict Dyads	
Government of Afghanistan - Taliban	14,853	93.93
Taliban - Civilians	614	3.88
Government of United States of America - al-Qaida	125	0.97
	Type of violence	
State-based violence	15,084	95.39
Non-state violence	631	3.99
One-sided violence	98	0.62

Notes: Summary on types of violence in Afghanistan provided by UCDP GED between 2002-2014.

Table B.3: Balancing tests - high and low opium suitable districts

	Mean Values per Group		**P-Value**
	High Suitability	Low Suitability	
Ruggedness in 1000	286.052	342.550	0.000
Distance to Kabul - Linear	248.425	371.647	0.000
Distance to Kabul - Road 2D	311.787	454.068	0.000
Distance to Kabul - Road 3D	314.037	457.126	0.000
Travel Time to Kabul - Road 2D	6.560	10.693	0.000
Travel Time to Kabul - Road 3D	6.597	10.755	0.000
Pashtuns	0.780	0.602	0.000
Mixed Ethnic Groups	0.538	0.742	0.000
Number Ethnic Groups	1.830	2.247	0.000
Mixed Territory 1996	0.030	0.075	0.000
Taliban Territory 1996	0.593	0.527	0.000
Ethnic Trafficking Route	0.557	0.409	0.000
BRD 2000	14.308	11.075	0.172
Luminosity 2000	0.160	0.213	0.322
(log) Population 2000	3.974	2.654	0.000
Wheat Suitability	0.609	0.371	0.000

Notes: Sample based on Table 1, column 1. To assign a districts to low or high suitability, we use a cut-off of 0.4. In Table F.16 we control for an interaction of all the variables (above the separating line) with a time trend or with time-fixed effects.

Table B.4: Markov transition matrix

	1 if 0	1 if >0	1 if >10	1 if >25	1 if >100
1 if 0	87.49	7.55	2.46	1.85	0.64
1 if >0	36.86	35.41	15.81	9.76	2.17
1 if >10	23.46	30.19	19.81	23.27	3.27
1 if >25	19.90	13.21	16.64	36.54	13.70
1 if >100	19.25	7.55	4.15	28.68	40.38

Notes: Sample based on Table 2, column 1.

C. Geographical overview

Afghanistan and its neighboring states

Notes: Opium is reported to be mostly trafficked through Iran, Pakistan as well as through Turkmenistan according to UNODC.

Elevation and mountainous terrain in Afghanistan

Notes: The central and north-eastern part of Afghanistan feature the most mountainous terrain. Mountains are correlated with opium suitability, for instance very high altitude areas with a lot of snow are obviously unsuitable, but generally opium can be produced in many places as our map for the suitability indicator shows. We will run regressions with and without the border districts, as well as regressions controlling for elevation or ruggedness (in a flexible way interacted with year dummies) to account for potentially time-varying effects of these factors. Source for elevation data: (U.S. Geological Survey (USGS) Global Multi-resolution Terrain Elevation Data 2010 (GMTED2010), available at https://lta.cr.usgs.gov/GMTED2010, accessed 06.04.2018). Source for ADM1 administrative data is http://www.gadm.org, last accessed August 28, 2019.

Figure C.1: ADM1 level (provinces) of Afghanistan

Notes: The figure plots the 34 provinces (ADM1 level). Source: Central Statistical Office Afghanistan (http://afghanag.ucdavis.edu/country-info/about-afghan.html, last accessed August 28, 2019).

D. Identification using complement prices

In this section, we discuss to which extent we can use additional information on complement prices to learn something about the potential impact of omitted variables. Specifically, we are interested in the biasing effect of an omitted variables OV_{t-1} that varies over time. This bias would affect the opium estimate (b^O) if the omitted variable bias correlates with opium prices and impacts conflict different in low and high suitability districts. Given that the bias would work through affecting the price, and local demand plays a negligible role for overall profits, OV_{t-1} will be any events or shocks that influence opium prices by affecting opium supply.

The underlying idea of using a complement to opium is that a supply shock affects the price of opium and its complement in the opposite direction. Therefore, an omitted variable will also have opposite effects on the estimates regressing the respective prices interacted with suitability on conflict. Section D.1 begins by deriving conditions under which we can exploit this mechanism to learn something about the sign and the bounds of the true underlying relationship between prices and conflict. In Section D.2, we then illustrate the theoretical results using Monte Carlo simulations. Section D.3 finally shows the corresponding proofs and how under stronger assumptions the true parameter is even bounded by the estimates using the opium price and the complement price. We will illustrate everything with regard to opium prices and the respective complements, but the underlying theory and strategy can be applied to any other goods that fulfill the conditions outlined below.

D.1. Theoretical predictions

Set Up: We begin by setting up the price equations:

$$p^o_{t-1} = \eta DS_{t-1} - q^o_{t-1} + \bar{\omega} q^c_{t-1} + \varepsilon^o_{t-1} \tag{6}$$

$$q^o_{t-1} = OV_{t-1} + \xi^o_{t-1} \tag{7}$$

$$p^c_{t-1} = \eta DS_{t-1} - q^c_{t-1} + \bar{\omega} q^o_{t-1} + \varepsilon^c_{t-1} \tag{8}$$

$$q^c_{t-1} = \xi^c_{t-1} \tag{9}$$

Variables p^o_{t-1} and p^c_{t-1} denote the price of respectively opium and the complement at year $t-1$. Prices are a function of common demand shifters (DS_{t-1}) and supply shocks (OV_{t-1}, ξ_{t-1}). The omitted variable (OV_{t-1}) only affects opium supply so that $\rho(q^c_{t-1}, OV_{t-1}) = 0$. The parameter $\bar{\omega} \in [0,1]$ indicates the degree to which the two prices react to changes in the supply of the complement. A higher $\bar{\omega}$ indicates that each price reacts stronger to each others supply. To simplify notation, the coefficient of the omitted variable in equation (7) is set to unity. In that case, $\eta > 0$ directly measures the effect of the common demand shifters relative to the omitted variable. Both ξ_{t-1} and ε_{t-1} can be interpreted as (product specific) error terms. Substituting supply in the price equation gives us the following:

$$p^o_{t-1} = \eta DS_{t-1} - OV_{t-1} + \tilde{\varepsilon}^o_{t-1} \tag{10}$$

$$p^c_{t-1} = \eta DS_{t-1} + \bar{\omega} OV_{t-1} + \tilde{\varepsilon}^c_{t-1} \tag{11}$$

with $\tilde{\varepsilon}^o_{t-1} \equiv \varepsilon^o_{t-1} - \xi^o_{t-1} + \bar{\omega}\xi^c_{t-1}$ and $\tilde{\varepsilon}^c_{t-1} \equiv \varepsilon^c_{t-1} - \xi^c_{t-1} + \bar{\omega}\xi^o_{t-1}$. We are interested in estimating the effect of prices on conflict, which we assess using the following regressions:

$$c_{d,t} = \beta(p^o_{t-1} \times s_d) + \gamma(OV_{t-1} \times s_d) + \delta_d + \tau_t + u_{d,t} \tag{12}$$

$$c_{d,t} = b^o(p^o_{t-1} \times s_d) + \delta_d + \tau_t + \nu_{d,t} \tag{13}$$

$$c_{d,t} = b^c(p^c_{t-1} \times s_d) + \delta_d + \tau_t + \upsilon_{d,t} \tag{14}$$

Equation (12) is the true regression and equations (13) and (14) are both two regressions that do not include the unobserved OV_{t-1}. The outcome variable $c_{d,t}$ denotes conflict at year t in district d and s_d the opium suitability in district d. All regressions include district (δ_d) and year (τ_t) fixed effects.

We can use those equations to derive the following estimates:

$$\text{plim } \hat{b}^o = \beta + \tilde{\gamma} \tag{15}$$

$$\text{plim } \hat{b}^c = \theta\beta - \alpha\bar{\omega}\tilde{\gamma} \tag{16}$$

where:

$$\tilde{\gamma} \equiv -\gamma\frac{\sigma^2_{OV}}{\sigma^2_{p^o}}, \; \theta \equiv \frac{\eta^2\sigma^2_{DS} - \bar{\omega}(\sigma^2_{OV} + \sigma^2_{\xi^o} + \sigma^2_{\xi^c})}{\sigma^2_{p^c}}, \; \alpha \equiv \frac{\sigma^2_{p^o}}{\sigma^2_{p^c}}$$

To make sense of the estimates, we assume $\theta > 0$. Using the definition of θ this restriction is equivalent to:

$$\eta^2 > \bar{\omega}\frac{\sigma^2_{OV} + \sigma^2_{\xi^o} + \sigma^2_{\xi^c}}{\sigma^2_{DS}} \tag{17}$$

In other words, we impose that the demand side relative to the supply side must be large enough to discipline θ. As we argue in the main part of the paper, this seems justified for drug prices over the last decades. Since both α and $\bar{\omega}$ are positive, we see that OV_{t-1} affects the estimates in opposite direction. Hence a larger $\tilde{\gamma}$ increases the opium estimate, while it decreases the complement estimate. We will exploit this mechanism in the next section to draw inference on our true estimate β.

Results: In this section we discuss how the asymptotic properties of the estimates behave. More specifically, we derive sufficient conditions to both determine the sign of β and its upper or lower bound. Whenever we refer to $\hat{b}^o$ or $\hat{b}^c$ we refer to its probability limit unless otherwise noted.

First, we can conclude that if we observe both estimates ($\hat{b}^o$ and $\hat{b}^c$) having the same sign, also the true estimate β must have the same sign. More formally:

Corollary 1.

$$\beta < 0 \; if \, \hat{b}^o, \hat{b}^c < 0 \tag{18}$$

$$\beta > 0 \; if \, \hat{b}^o, \hat{b}^c > 0 \tag{19}$$

Proof. We prove $\beta < 0$ if $(\hat{b}^o, \hat{b}^c < 0) \wedge (\theta > 0)$ as the second statement follows a similar proof. Since $\hat{b}^o < 0$ from (15) it must be that $(\beta < 0) \vee (\tilde{\gamma} < 0)$. Then if $\tilde{\gamma} < 0$ we must have from (16) that $\theta\beta < 0$ and since $\theta > 0$ it must be that $\beta < 0$. □

This implies that if both the estimates using opium and complements ($\hat{b}^o$ and $\hat{b}^c$) are negative, as we observe in Afghanistan, then also the opium estimate without omitted variable bias $\hat{\beta}$ must be negative.

Second, we can proof that if both $\hat{b}^o$ and $\hat{b}^c$ have the same sign and $\hat{b}^c$ is further away from zero, $\hat{b}^o$ tends to be biased towards zero. More formally:

Corollary 2.

$$\beta < \hat{b}^o < 0 \; if \, (\hat{b}^c < \hat{b}^o < 0) \tag{20}$$

$$\beta > \hat{b}^o > 0 \; if \, (\hat{b}^c > \hat{b}^o > 0) \tag{21}$$

Proof. We again prove the first statement as the second follows a similar proof. First, from Corollary 1 we know that $\beta < 0$ if $\hat{b}^c, \hat{b}^c < 0$. Then from (15) we conclude that for $\beta < \hat{b}^c < 0$ it must be true that $\tilde{\gamma} > 0$. Knowing that $\hat{b}^c < \hat{b}^o$ and $\beta < 0$, $\tilde{\gamma} > 0$ is satisfied by construction. □

In other words, the opium estimate $\hat{b}^o$ bounds the true estimate as either an upperbound or a lowerbound if the estimates ($\hat{b}^o, \hat{b}^c$) are respectively negative or positive and the complement estimate $\hat{b}^c$ is higher in absolute value than $\hat{b}^c$.

From Table 2 we know that, apart from being both negative, the complement estimate is more negative than the opium estimate. From Corollary 2 we henceforth learn that our main estimate is biased towards zero in the presence of omitted variable bias. This in turn implies that our estimates are conservative and that the magnitude of the true effect is (potentially) larger.

D.2. Simulation

We now propose a Monte Carlo approach to illustrate how the asymptotic properties regarding sign and bounds of true β behave in the finite sample setup. In the simulation, we vary γ, the impact of the omitted variable on the outcome variable, in three different ways. First we draw γ from the normal distribution ($\gamma \sim N(0, 1)$) since we do not know the direction of the bias. In the second scenario, we analyze the upward bias, and in the third scenario the downward bias. We simulate a very general data generating process where we assume for the common demand shifters $DS_t \sim N(0, 1)$, and for the omitted variable $OV_t \sim N(0, 1)$. All error terms are also drawn from the standard normal distribution with mean zero and variance equal to one. For the opium suitabillity, we assume $s_d \sim U[0, 1]$. Finally, we set the impact of the demand shifters on price (η) so that the inequality from Remark 1 holds.

We generate the outcome variable, the conflict in district d at time t using the following

$$c_{dt} = \alpha + \beta(p^o_{t-1} \times s_d) + \gamma(OV_{t-1} \times s_d) + \tau_t + \delta_d + u_{d,t} \tag{22}$$

with $u_{d,t} \sim \mathcal{N}(0, 1)$. In each round, we draw 1,000 observations, clustered in 100 districts with 20 time periods, and compute all variables. For each row, we then estimate the following three equations which refer to equations (12 – 14):

$$c_{d,t} = \alpha + \beta(p^o_{t-1} \times s_d) + \gamma(OV_{t-1} \times s_d) + \tau_t + \delta_d + u_{d,t}, \tag{23}$$

$$c_{d,t} = \alpha + b^o(p^o_{t-1} \times s_d) + \tau_t + \delta_d + v_{d,t}, \tag{24}$$

$$c_{d,t} = \alpha + b^c(p^c_{t-1} \times s_d) + \tau_t + \delta_d + \upsilon_{d,t}. \tag{25}$$

We set true β equal to zero to analyze the most worrisome case, in which the true effect is insignificant, but due to the omitted variable bias, we may falsely interpret the effect as statistically significant. We want to understand how we can reduce the overrejection of the true null hypothesis that $\beta = 0$ by using information obtained from estimating both b^o and b^c. We are considering three rejection rules:

Rule 1: p-value of $\hat{b^o}$ smaller than 5%.

Rule 2: $\hat{b^o}$ and $\hat{b^c}$ are of the same sign ($\hat{b^o} \cdot \hat{b^c} > 0$).

Rule 3: $\hat{b^o}$ and $\hat{b^c}$ are of the same sign, and the p-values of both $\hat{b^o}$ and $\hat{b^c}$ are smaller than 5%.

Table D.1: Simulation results

	Unknown bias $\gamma \sim N(0,1)$ (1)	Upward bias $\gamma = -1$ (2)	Downward bias $\gamma = 1$ (3)
	A. Average estimates		
$\hat{\beta}$	0.000	-0.000	0.000
$\hat{b^o}$	0.000	0.077	-0.077
$\hat{b^c}$	-0.001	-0.077	0.076
	B. Rejection rates		
$p(\hat{\beta} = 0) < 0.05$	0.052	0.055	0.056
Rule 1: significance of b^o			
$p(\hat{b^o} = 0) < 0.05$	0.505	0.709	0.717
Rule 2: same sign of b^o and b^c			
$\hat{b^o} \cdot \hat{b^c} > 0$	0.345	0.250	0.248
Rule 3: same sign and significance of both b^o and b^c			
$p(\hat{b^o} = 0) < 0.05 \wedge p(\hat{b^c} = 0) < 0.05 \wedge \hat{b^o} \cdot \hat{b^c} > 0$	0.052	0.066	0.071

Notes: Simulations with 10,000 repetitions. In each repetition, regressions are based on a dataset with 20 years and 100 districts. $\hat{\beta}$ is the estimate of the impact of opium profitability on conflict obtained from the true regression, i.e. one that controls for the omitted variable (true β equals 0). $\hat{b^o}$ is the estimate of the impact of opium profitability on conflict using the opium price only (without controlling for omitted variable), and $\hat{b^c}$ using the complement price (without controlling for the omitted variable). The first column shows the results when the impact of the omitted variable on conflict (γ) is drawn from a standard normal distribution. The second column shows the results for an upward bias ($\gamma = -1$) and the third column show the results for a downward bias ($\gamma = 1$). Panel A displays the average values of the estimates. Panel B shows the rejection rate of the true hypothesis based on the selected rejection rules. p( ) indicates that the coefficient estimates are significantly different from zero at the 5%-level.

The first rule is the naive rule indicating how often we would falsely reject the null hypothesis if we would ignore the presence of omitted variable bias. The second rule captures the idea of upper and lower bounds of the true estimate. The rejection rate under this rule shows the likelihood that true parameter β lies outside bounds of the $\hat{b^o}$ and $\hat{b^c}$ estimates. We showed in Corollary 3 that β must be bounded by $\hat{b^o}$ and $\hat{b^c}$. Since this is an asymptotic result, we want to show how often this holds in a finite sample. In the third rule, we impose additional restriction on the statistical significance of $\hat{b^o}$ and $\hat{b^c}$.

We repeat the simulation 10,000 times and in each round we store the estimates ($\hat{\beta}$, $\hat{b^o}$ and $\hat{b^c}$) and the corresponding standard errors and p-values. We show how likely it is that we would falsely reject the null hypothesis given the results we obtain in a finite sample. We then compare rejection rates obtained by imposing our three rules to the rejection rate based on p-value of the unbiased estimator (p-value of $\hat{\beta}$ smaller than 5%). In this case, the true hypothesis should be rejected in 5% of cases.

Table D.1 shows the mean estimates and rejection rates in the three scenarios. In column 1, γ is drawn from a standard normal distribution. Column 2 shows the upward bias scenario and column 3 the downward bias scenario. While the true estimates $\hat{\beta}$ have mean zero, the average estimates of $\hat{b^o}$ and $\hat{b^c}$ are substantially different from zero in the upward and downward bias scenarios. Moreover, we can see that, on average, $\hat{b^o}$ and $\hat{b^c}$ estimates have opposite signs. In the unknown bias scenario, omitted variable bias differs from zero in every repetition, but has an expected value of zero. Thus, the average estimates $\hat{b^o}$ and $\hat{b^c}$ in column 1 are close to zero.

Panel B in Table D.1 shows the rejection rates for different rejection rules. If we observed the omitted variable we would reject the null hypothesis in 5% of cases. We find that in more than 50% of the cases $\hat{b^o}$ is significantly different from zero. This implies that in at least half of the cases we would incorrectly find a statistically significant impact of opium profitability. The results for rule 2 show that the likelihood of β being outside of the bounds is quite high (35% in the unknown bias scenario, and 25% in both the upwards and downwards bias scenarios). However, if we would impose an additional restriction on both estimates being statistically significant, we would reject the true null hypothesis in roughly 5% of cases. Hence, the probability of rejecting true null hypothesis is more or less equal to the probability of rejecting the true null hypothesis based on unbiased estimates of the true β. We can therefore say with confidence that the true estimate is unlikely to be zero when both the opium estimate and the complement estimates are significant and have the same sign.

We further illustrate simulation results in Figures D.1-D.3. We focus on two scenarios: the graphs on the left side present results for the upward bias, and the graphs on the right side show results for the downard bias. In Figure D.1, we plot the distribution of the estimates. We can clearly see that the estimates based on the opium price and estimates based on the complement price are biased in the opposite directions to each other as stated in Corollary 3.

In Figures D.2-D.3, we show how we improve our inference using complement prices. Given our different rejection rules, the dark red points indicate $\hat{b^o}$ estimates for which we falsely reject the null hypothesis and the light red points the estimates for which we do not reject. Figure D.2 shows the results for rule 1. If we would make our rejection decisions based on p-value of $\hat{b^o}$ only, we would falsely reject the nul hypothesis in many cases. Under this rule, the overrejection is largely driven by the bias. For example, most of the false rejections in the upward bias scenario come from estimates that are greater than zero. The rejection of few negative estimates may be attributed to the noise generated by the error terms. Figure D.3 shows that we can substantially reduce the overrejection of the true hypothesis by conditioning on the statistical significance and the sign of

$\hat{b}^o$ and $\hat{b}^c$ following Collorary 3. We achieve this by reducing the number of false rejections of estimates driven by the bias. In the upward bias scenario, we substantially reduce the number of false rejections for positive estimates. The rejection rate of negative estimates remains similar as in rule 1, because these rejections are driven by the noise and not the omitted variable bias. Thus, we showed that in the finite sample, the additional information obtained from using complement prices allow us to reduce the occurance of false rejections of the null hypothesis, which are driven by omitted variable bias. The rejection rate is similar to that obtained by estimating true β, because the remaining rejections are driven by the noise generated by the error terms.

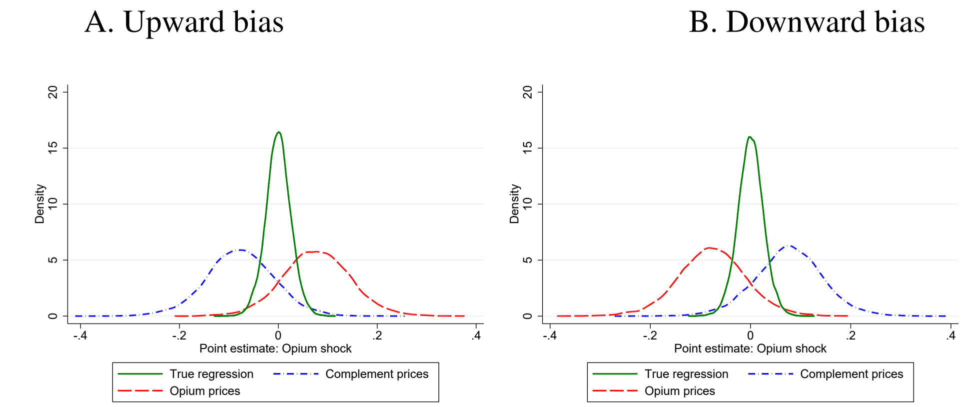


Figure D.1: Distribution of estimates

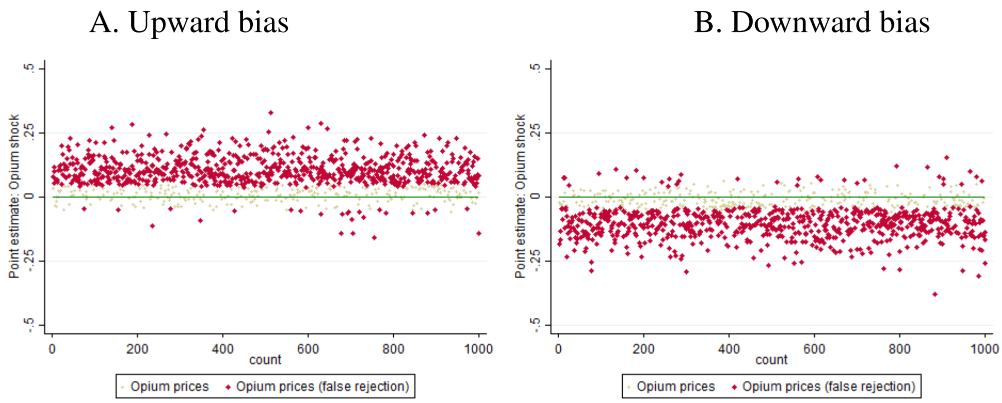


Figure D.2: Rule 1 - Significance of $\hat{b^o}$

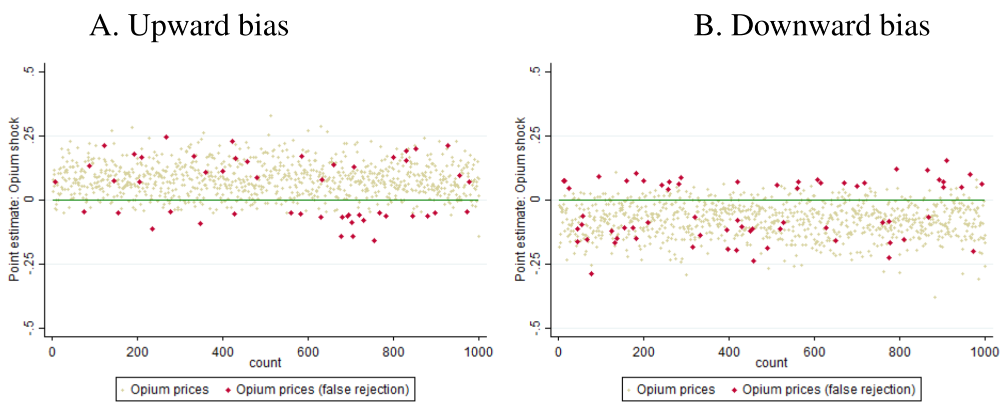


Figure D.3: Rule 3 - Same sign and significance of both $\hat{b^o}$ and $\hat{b^c}$

Notes: Figure D.1 shows the distribution of point estimates (kernel density): $\hat{\beta}$ (green), $\hat{b^o}$ (red), and $\hat{b^c}$ (blue) based on the simulation with 10,000 repetitions. Figures D.2 and D.3 show point estimates: $\hat{b^o}$ that did not lead to the rejection of the null hypothesis (light red), and $\hat{b^o}$ that led to the rejection of the null hypothesis (dark red) based on the simulation with 1,000 repetitions. In each repetition, regressions are based on a dataset with 20 years and 100 districts. The dark red points indicate cases of rejecting true null hypothesis based on different rejection rules. Figure D.2 shows rejections under the following condition: $\hat{b^o}$ is significantly different from 0 at the 5%-level. Figure D.3 shows the rejections under the following condition: $\hat{b^o}$ and $\hat{b^c}$ are of the same sign and they are both significantly different from zero at the 5%-level.

D.3. Derivations and other results

Derivations: We briefly go over the derivations of the estimators stated in (15) and (16). The derivation of $\hat{b}^o$ is akin to the omitted variable bias formula. For the derivation of $\hat{b}^c$ we use both price equations (10) and (11)

$$\begin{aligned}
\hat{b}^o &= \frac{cov(c_{d,t}, p^o_{t-1} \times s_d)}{var(p^o_{t-1} \times s_d)} && (26)\\
&= \beta \frac{cov(p^o_{t-1} \times s_d, p^o_{t-1} \times s_d)}{var(p^o_{t-1} \times s_d)} + \frac{cov(OV_{t-1} \times s_d, p^o_{t-1} \times s_d)}{var(p^o_{t-1} \times s_d)} && (27)\\
&= \beta + \frac{cov(OV_{t-1}, p^o_{t-1})}{var(p^o_{t-1})} && (28)\\
&= \beta + \frac{cov(OV_{t-1}, p^o_{t-1})}{var(OV_{t-1})} \frac{var(OV_{t-1})}{var(p^o_{t-1})} && (29)
\end{aligned}$$

hence,

$$\text{plim } \hat{b}^o = \beta - \gamma \frac{\sigma^2_{OV}}{\sigma^2_{p^o}} \tag{30}$$

$$\begin{aligned}
\hat{b}^c &= \frac{cov(c_{d,t}, p^c_{t-1} \times s_d)}{var(p^c_{t-1} \times s_d)} && (31)\\
&= \beta \frac{cov(p^o_{t-1} \times s_d, p^c_{t-1} \times s_d)}{var(p^c_{t-1} \times s_d)} + \frac{cov(OV_{t-1} \times s_d, p^c_{t-1} \times s_d)}{var(p^c_{t-1} \times s_d)} && (32)\\
&= \beta \frac{cov(\eta DS_{t-1} - OV_{t-1} + \tilde{\varepsilon}^o_{t-1}, \eta DS_{t-1} + \bar{\omega} OV_{t-1} + \tilde{\varepsilon}^c_{t-1})}{var(p^c_{t-1})} && (33)\\
&\qquad + \frac{cov(OV_{t-1}, p^c_{t-1})}{var(p^c_{t-1})} &&\\
&= \beta \frac{\eta^2 var(DS_{t-1}) - \bar{\omega} var(OV_{t-1}) + cov(\tilde{\varepsilon}^o_{t-1}, \tilde{\varepsilon}^c_{t-1})}{var(p^c_{t-1})} && (34)\\
&\qquad + \frac{cov(OV_{t-1}, p^c_{t-1})}{var(OV_{t-1})} \frac{var(OV_{t-1})}{var(p^c_{t-1})} &&
\end{aligned}$$

hence,

$$\text{plim } \hat{b}^c = \frac{\eta^2 \sigma^2_{DS} - \bar{\omega}(\sigma^2_{OV} + \sigma^2_{\xi^o} + \sigma^2_{\xi^c})}{\sigma^2_{p^c}} \beta + \bar{\omega} \gamma \frac{\sigma^2_{OV}}{\sigma^2_{p^c}} \tag{35}$$

Where the explicit variances of the prices are as followed:

$$\sigma^2_{p^o} = \eta^2\sigma^2_{DS} + \sigma^2_{OV} + \sigma^2_{\varepsilon^o} + \sigma^2_{\xi^o} + \bar{\omega}^2\sigma^2_{\xi^o} \tag{36}$$

$$\sigma^2_{p^c} = \eta^2\sigma^2_{DS} + \bar{\omega}^2\sigma^2_{OV} + \sigma^2_{\varepsilon^c} + \sigma^2_{\xi^c} + \bar{\omega}^2\sigma^2_{\xi^o} \tag{37}$$

Other results: In this subsection we state the conditions under which β is bounded. We start with a remark of auxiliary value.

Remark 1. *If $\hat{b}^o > \hat{b}^c$ then it must be that:*

$$\tilde{\gamma} > \frac{(\theta - 1)\beta}{1 + \alpha\bar{\omega}}$$

If $\hat{b}^o < \hat{b}^c$ then it must be that:

$$\tilde{\gamma} < \frac{(\theta - 1)\beta}{1 + \alpha\bar{\omega}}$$

So that:

$$\tilde{\gamma} > 0 \;\; if \;\; (\beta < 0) \wedge (\hat{b}^o > \hat{b}^c)$$

$$\tilde{\gamma} < 0 \;\; if \;\; (\beta > 0) \wedge (\hat{b}^o < \hat{b}^c)$$

We can now state the cases in which β is between $\hat{b}^o$ and $\hat{b}^c$:

Corollary 3.

$$\beta \in (\hat{b}^c, \hat{b}^o) \; if \begin{cases} \hat{b}^c < \hat{b}^o, \beta < 0, \theta > \bar{\theta} \\ \hat{b}^c < \hat{b}^o, \beta > 0, \theta < \bar{\theta}, \tilde{\gamma} > 0 \end{cases}$$

$$\beta \in (\hat{b}^o, \hat{b}^c) \; if \begin{cases} \hat{b}^o < \hat{b}^c, \beta < 0, \theta < \bar{\theta}, \tilde{\gamma} < 0 \\ \hat{b}^o < \hat{b}^c, \beta > 0, \theta > \bar{\theta} \end{cases}$$

where:

$$\bar{\theta} \equiv 1 + \frac{\alpha\bar{\omega}\tilde{\gamma}}{\beta}$$

Proof. As the following proof can be easily extended, we prove the following case:

$$\beta \in (\hat{b}^c, \hat{b}^o) \text{ if } \hat{b}^c < \hat{b}^o, \tilde{\gamma} > 0, \beta < 0, \theta > \bar{\theta}$$

From (15) we observe that $\beta < \hat{b}^o$ if $\tilde{\gamma} > 0$. From (16) we solve $\beta > \hat{b}^c$ for θ and arrive to $\theta > \bar{\theta}$ if $\beta < 0$. Finally Remark 1 shows that in two cases a restriction $\tilde{\gamma}$ follows by construction and is henceforth superfluous. □

This results states that as long as θ is sufficiently diciplined, the true estimate β is between our two estimates $\hat{b}^o, \hat{b}^c$

Finally, we end this subsection by stating the complete result: β having the same sign and being bounded by the two estimates ($\hat{b}^o$ and $\hat{b}^c$). This result is a combination of the sufficient conditions stated in Corrolaries 1 and 3.

Corollary 4.

$$\hat{b}^c < \beta < \hat{b}^o < 0 \; if \; (\hat{b}^c < \hat{b}^o < 0) \land (\theta > \bar{\theta}) \tag{38}$$

$$\hat{b}^o < \beta < \hat{b}^c < 0 \; if \; (\hat{b}^c < \hat{b}^o < 0) \land (\theta < \bar{\theta}) \land (\tilde{\gamma} < 0) \tag{39}$$

$$0 < \hat{b}^o < \beta < \hat{b}^c \; if \; (0 < \hat{b}^c < \hat{b}^o) \land (\theta > \bar{\theta}) \tag{40}$$

$$0 < \hat{b}^c < \beta < \hat{b}^o \; if \; (0 < \hat{b}^c < \hat{b}^o) \land (\theta < \bar{\theta}) \land (\tilde{\gamma} > 0) \tag{41}$$

where:

$$\bar{\theta} \equiv 1 + \frac{\alpha\bar{\omega}\tilde{\gamma}}{\beta}$$

E. Further results

Regressions at the province level

Table E.1: Effect of income shocks on opium revenues, at the province level 2002-2014

	Outcome: (t)	**Outcome: (t) and (t-1)**
	(1)	(2)
Opium Profitability (t-1)	5.885	5.461
	(3.203)	(3.078)
Number of observations	442	442
Adjusted R-Squared	0.608	0.678

Notes: Models include province- and year-fixed effects. The dependent variable opium revenues in (t) is in logarithms. Standard errors clustered at the province level are displayed in parentheses.

Table E.2: Additional results using normalized drug prices, at the province level 2002-2014

	Local Opium Price	**International Heroin Price**	**Complement Price**
	(1)	(2)	(3)
Opium Profitability (t-1)	-0.290	-0.717	-1.101
	(0.299)	(0.566)	(0.648)
Number of observations	442	442	442
Adjusted R-Squared	0.722	0.723	0.726

Notes: Models include province- and year-fixed effects. The dependent variable is the log of BRD in (t). Opium Profitability is defined as the interaction between the normalized drug prices (in logarithms) as indicated in the column heading and the suitability to grow opium. Standard errors are in parentheses (clustered at the province level).

Different timing of the shocks

Table E.3: Leads and lags, 2002-2014

	Not including: Wheat Profitability (1)	**Including Wheat Profitability** (2)
	Panel A: Including t-1, t, and t+1	
Opium Profitability (t+1)	-0.066	-0.045
	(0.251)	(0.252)
Opium Profitability (t)	-0.660	-0.609
	(0.320)	(0.321)
Opium Profitability (t-1)	-0.773	-0.459
	(0.289)	(0.329)
Number of observations	4776	4776
Adjusted R-squared	0.648	0.649
	Panel B: Including t and t+1	
Opium Profitability (t+1)	-0.122	-0.116
	(0.254)	(0.251)
Opium Profitability (t)	-1.141	-0.875
	(0.311)	(0.324)
Number of observations	4776	4776
Adjusted R-squared	0.648	0.649
	Panel C: Including t-1 and t	
Opium Profitability (t)	-0.273	-0.266
	(0.249)	(0.249)
Opium Profitability (t-1)	-0.668	-0.405
	(0.286)	(0.303)
Number of observations	5174	5174
Adjusted R-squared	0.649	0.649

Notes: Models include province-times-year- and district-fixed effects. The dependent variable is the logarithm of BRD in (t). Wheat profitabillity includes both leads and lags. Standard errors are in parentheses (clustered at the district level).

Table E.4: Timing of shocks, 2002-2014

	(log) BRD	**1 if** ≥ 5	**1 if** ≥ 10	**1 if** ≥ 25	**1 if** ≥ 100
	(1)	(2)	(3)	(4)	(5)
			Panel A: Contemporaneous effect		
Opium Profitability (t)	-0.608	-0.168	-0.161	-0.130	-0.021
	(0.246)	(0.075)	(0.074)	(0.066)	(0.033)
Adjusted R-Squared	0.649	0.501	0.483	0.454	0.309
			Panel B: Lagged effect		
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

Notes: Models include province-times-year- and district-fixed effects. The dependent variable is conflict in (t) operationalized as indicated in the column heading. Standard errors are in parentheses (clustered at the district level).

Types of fighting

Table E.5: Types of fighting, 2002-2014

Conflict Actor:	**All**	**Taliban vs. Civil.**	**Taliban vs. Gov.**		
BRD:	**Any**	**Talib.&Civil.**	**Talib.&Gov.**	**Taliban**	**Government**
	(1)	(2)	(3)	(4)	(5)
Opium Profitability (t-1)	-0.675	-0.098	-0.677	-0.539	-0.521
	(0.296)	(0.069)	(0.302)	(0.187)	(0.274)
Number of observations	0.649	0.200	0.658	0.555	0.596

Notes: Models include province-times-year- and district-fixed effects. The dependent variable is the log of BRD in (t) for a specific type of conflict operationalized as indicated in the column heading. Standard errors are in parentheses (clustered at the district level).

Table E.6: Main results using SIGACTs conflict data, 2002-2014 period

	DF	**IDF**	**IED**	**(log) DF**	**(log) IDF**	**(log) IED**
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Local Opium Price					
Opium Profitability (t-1)	-45.696	-5.094	-12.263	-0.464	-0.312	-0.441
	(33.597)	(1.721)	(5.267)	(0.107)	(0.090)	(0.087)
Adjusted R-Squared	0.444	0.545	0.576	0.809	0.737	0.774
	Panel B: International Heroin Price (Baseline)					
Opium Profitability (t-1)	-58.929	-3.009	-16.402	-0.747	-0.371	-0.654
	(32.579)	(1.953)	(7.176)	(0.223)	(0.152)	(0.197)
Adjusted R-Squared	0.442	0.543	0.574	0.808	0.736	0.773
	Panel C: International Complement Price					
Opium Profitability (t-1)	-96.148	-7.921	-26.719	-1.099	-0.618	-1.006
	(54.144)	(2.821)	(10.144)	(0.269)	(0.193)	(0.231)
Adjusted R-Squared	0.444	0.544	0.578	0.810	0.737	0.776

Notes: Models include province-times-year- and district-fixed effects. The dependent variable is conflict in (t) operationalized as indicated in the column heading (DF - Direct Fire, IDF - Indirect Fire, IED - Improvised Explosive Devices). 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 equal across all panels (5174). Standard errors are in parentheses (clustered at the district level).

Outcome variable (onset and ending)

Table E.7: Conditional logit - incidence, onset and ending, 2002-2014

	1 if ≥ 5	**1 if** ≥ 10	**1 if** ≥ 25	**1 if** ≥ 100
	(1)	(2)	(3)	(4)
		Panel A: Incidence		
Opium shock (t-1)	-6.376	-6.823	-6.849	-3.148
	(1.764)	(2.519)	(3.249)	(6.672)
Number of observations	4407	3510	2431	806
Pseudo R-Squared	0.350	0.272	0.272	0.213
		Panel B: Onset		
Opium shock (t-1)	-4.505	-6.076	-5.729	-1.719
	(1.686)	(2.375)	(3.092)	(5.601)
Number of observations	2953	2739	1995	714
Pseudo R-Squared	0.170	0.136	0.149	0.149
		Panel C: Ending		
Opium shock (t-1)	4.053	0.445	-0.446	-9.784
	(1.698)	(2.430)	(2.939)	(8.124)
Number of observations	1931	1195	730	207
Pseudo R-Squared	0.105	0.077	0.102	0.161

Notes: Conditional logit model with year- and district-fixed effects. The dependent variable is conflict onset/ending in (t) operationalized as indicated in the column heading. Standard errors are in parentheses (clustered at the district level).

Effect of Ethnic Fractionalization

Table E.8: No difference conditional on ethnic diversity

Interaction with	Ethnic Groups (GREG)		Ethnic Groups (NRVA)	
	1 if Mixed	Number	1 if Mixed	Number
	(1)	(2)	(3)	(4)
Opium Profitability (t-1)	-0.763	-0.977	-0.403	-0.380
	(0.370)	(0.568)	(0.380)	(0.530)
Opium Profitability (t-1)*X	0.130	0.114	-0.524	-0.179
	(0.421)	(0.280)	(0.423)	(0.223)

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 drug prices (in logarithms) and the suitability to grow opium. Regressions include interactions of the opium profitability with a variable X as indicated in in the column heading. For definitions of the variables X please see Appendix A. Standard errors are in parentheses (clustered at the district level).

F. Sensitivity analysis

Modifications of the treatment variable: We use multiple modifications of our treatment variable, both by replacing drug prices and crop suitabilities with alternative measures. Tables F.1 and F.2 are equivalent to our main results presented in Table 2 apart from the fact that drug prices are not normalized in Table F.1 and in Table F.2 the prices are not in logarithms. Table F.3 shows the results with de-trended opium prices, which exhibit less variation, but support the main finding. In Table F.4, we use the deviation of the international prices from their long-term mean.[32] This is a first attempt to rule out that our results are driven by the long-term negative trend in international drug prices as visible in Figure 4. We find our results to be unaffected by all these choices. With regard to the suitability, we replace the population-weighted suitability for opium and wheat with an unweighted version (see Table F.6). Weighting is important as population density differs strongly across Afghanistan, but causes potential bad control problems due to endogenous migration. While the wheat profitability turns insignificant, the results for opium profitability remain unaffected for all specifications.

Finally, we dichotomize the levels of the interaction. This reduces the complexity of the DiD-like interpretation. In panel A of Table F.7 we dichotomize the suitability based on the sample median. This allows us to interpret a price increase for two groups of districts, i.e., suitable (above the median) and less suitable (below the median). In panel B both variables are dichotomized based on the respective sample median. The coefficient in panel A indicates that a 10% increase in prices leads to about a 2.3% decrease in battle-related deaths in districts with a high suitability. Panel B finds that changing from a low- to a high-price-period reduces deaths by about 50% in districts with high suitability. Across all columns, the results are robust to this adaption.

Empirical model: First, we show our main results with a less restrictive set of fixed effects in Table F.8 for the different prices in panels A to D. The results using only district- and year-fixed effects all point in the same direction, with somewhat smaller coefficients. Again, all four prices consistently indicate a negative effect, both when looking at conflict intensity and incidence. The fact that the effects in our baseline specification become larger in absolute terms, when using province-times-year-fixed effects, suggests that the fixed effects succeed in eliminating biasing variation. In Table F.9 we test for population-density-specific heterogeneity of the main effect. The table shows that using the unweighted opium profitability measure interacted with the population

[32] Specifically, we use the mean over the entire observation period. Due to data restrictions we cannot calculate the mean over a longer term.

density yields a negative coefficient, in line with the expectation that suitability matters more with higher density. We also run a placebo test by interacting opium profitability (unweighted) with a dummy variable taking value one for districts with a very low population density (below the 10th or 20th percentile). The interaction terms turn out to be positive, indicating that the negative effect is weaker when population density is very low. Note that in both types of tests, the interaction terms are far from being statistically significant.

Outcome (cultivation and revenue): In Sections 4 and 5, we show that there is a strong effect of prices on opium revenues. Table F.10 also supports the positive effect of a higher opium profitability on opium cultivation (in hectares), with positive coefficients that are marginally insignificant in column 1 and significant at the 5%-level in column 2 when considering both periods that are most likely affected by the price change. This is not surprising as opium revenues are affected through changes in price and quantity produced, and cultivation only by the latter.

We then turn to the baseline IV results. To account for the different timing as shown in Figure 3, we show the second and first stage results when we replace revenue in $t-1$ with the moving average of revenue in $t-1$ and t in Table F.11 and Table F.12. The two IVs, opium profitability and legal opioid prescription, are again strong as indicated by the F-statistic. Having alternative sources of exogenous variation also enables us to compare the LATE of the different IVs. We find that the local effects do not differ much either in terms of magnitude or with regard to statistical significance.

Standard errors: In a next step, we use different choices on how to cluster standard errors. In the baseline models we used the district level, allowing for serial correlation over time within a district. In Table F.13 we use two-way clustering, i.e., district and year clusters in panel A and province and year clusters in panel B (Cameron *et al.*, 2011). Clustering at the province level is problematic as the number of clusters might be too small, which can lead to the over- or under-rejection of the null hypothesis (Cameron & Miller, 2015). Instead, we use the wild-cluster bootstrap method with the null imposed with 1000 replications and Webb's weights (Webb, 2013), which has been shown to provide valid inference even for few clusters. Figure F.2 plots the distribution of the bootstrap estimates. The null hypothesis of no effect is rejected both when using the international heroin price or the complement price index at least at the 5%-level.

Covariates and trends: Our specifications so far only include wheat profitability and different fixed effects as covariates. It is natural to first compare these results to results without those main covariates. In Table F.14, we find our results to remain robust when we exclude wheat profitability, with slightly more negative coefficients. Table F.14 also shows the tables including the coefficient of wheat profitability for comparison. To account for the persistence of conflict, we include the lagged dependent variable in a next step. Opium profitability remains negative in all columns and statistically significant in columns 1 to 3 (see Table F.15). In Table F.16 (panel A) we add a baseline set of pre-determined covariates such as luminosity and population as well as an exogenous measure of droughts, the VHI. In further specifications (panel B), we also allow for time-varying effects of time-invariant district-specific control variables.[33] One concern with our specification is that the time trends in prices interact not only with opium suitability, but also with other district characteristics. One way to model this is by adding interactions between these characteristics and a time trend. Another more flexible way is to interact the time invariant control variables with year dummies (panel C). This last specification allows for fully flexible trends interacting with a wide range of district features. The coefficients of our treatment variable are remarkably stable, ranging from -0.675 in the baseline (column 1, panel B, Table 2) to -0.694 in the most flexible specification for conflict intensity. They also remain significant with p-values below at least 0.1 for all conflict proxies (with the sole exception of the category "war").

Sensitivity to outliers: In Table F.18 we drop potential outliers. In panel A we exclude all border districts from the specification as they could be either very different to other districts or shocks in neighboring countries could affect border districts in a different fashion. For instance, we expect a large share of trafficking to occur close to the border. This could drive the results if international price increases would not reach the average farmer but only the traders, which are closer to the final customer along the supply chain. We find that our results are not driven by this particular group of districts. In panel B, we drop the two southern provinces Kandahar and Helmand and find our results to remain robust to this choice. These provinces are of specific interest for a number of reasons. First, the Taliban had their origins in the southern region and are thus likely to still have a strong support base here. Second, these provinces are known to be the largest producers of opium. Third, because of their direct connection to Pakistan, which is not only important in relation to trafficking routes but is also a major base of military support for the Taliban.

Apart from these rather obvious heterogeneous groups, we systematically investigate whether results are driven by a particular province or year. Figure F.3 reports the coefficients and the 90%

[33] The set of time-invariant covariates includes Ruggedness, Ethnic Trafficking Route, Pashtuns, Mixed Ethnic Groups, Taliban Territory 1996, Mixed Territory 1996, Distance Linear, Distance 2D and 3D, Travel Time 2D and 3D (all distances to Kabul).

confidence intervals when we drop each year or province one at a time. All coefficients remain stable and within a narrow band.

Randomization: One of the important points raised by Christian & Barrett (2017) is that non-linear trends in the time series of Bartik/shift-share like instruments can be problematic. We address this by looking at prices of different drugs and different versions of these prices (de-trended, log vs. non-log). To further rule out that the results are driven by non-linear trends, we implement further randomization placebo tests. We first randomize the time-varying variable (international heroin price) across years, and in a second specification randomize the district-specific suitability across districts. We would be worried if any of these specifications would create a negative effect similar in magnitude to our treatment effect. Figure F.4 plots the distribution of the coefficients generated by 5'000 randomizations per test along with the actual coefficient. We can also use this to conduct a randomization inference (RI) exercise, in which we compare how many of the random draws generate coefficients that are more negative than ours in order to compute an RI p-value. Reassuringly, we find that if the treatment was randomized according to the two different strategies, the simulated coefficients are always centered around zero. The p-values computed using two-sided symmetric randomization inference are 0.021.

Changes in US military strategy that increases reliance on opium revenues: We validate the importance of opportunity costs by using an important policy change in the foreign coalition's military strategy. This helps us to verify the importance of the opium economy in providing jobs. More importantly, this sheds some light on the effectiveness of nation building efforts and foreign military interventions, linking our study to the literature on nation building, as for instance Berman *et al.* (2011a) for Iraq and Dell & Querubin (2018) for Vietnam. These studies often consider a distinction between strategies focusing on the use of firepower and military force, and strategies based on winning "hearts and minds" by investing money and providing services and public goods like security. Obviously, each conflict is different, but nonetheless studying the successes and failures often can provide important lessons for the future and other contexts.

In Afghanistan, the coalition forces initially provided strong financial support to existing warlords and local strongholds from roughly 2001 to 2005 to build a strong anti-Taliban coalition. Rough estimates speak of several "hundred thousands of men" being armed as part of local militias, and more than 60% of provincial governors being "leaders of armed groups and most of the remaining ones had links to the latter" (Giustozzi, 2009, p. 91). Around 2005, the coalition switched their strategy towards a nation-building approach that attempted to pacify and "clean" Afghan politics. In this process, intense pressure on the Afghan government forced political leaders and governors to abandon their connection, as well as their support for militias, causing many trained and armed men to lose their main source of income (Giustozzi, 2009, p. 94 ff.). This change in strategy also coincides with the resurgence of the Taliban.

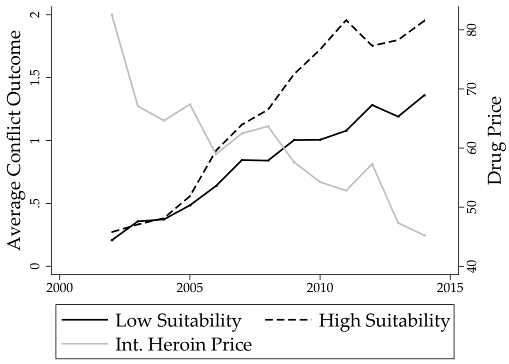


Figure F.1: Variation of conflict across high and low suitability districts over time

Notes: To assign a districts to low or high suitability, we use a cut-off of 0.4. See Appendix F, Figure F.8 for an alternative cut-off of 0.3. Inferences do not depend on this choice.

There is an analogy to the order of events in Iraq, where the de-Baathification process dissolved the Iraqi army and stopped all senior and mid-level party officials from joining the new army and security services. Various experts assess that this "drove many of the suddenly out-of-work Sunni warriors into alliances with a Sunni/anti-American insurgency" that later joined forces like ISIS. They speak of the "pervasive role played by members of Iraq's former Baathist army" and estimate that "25 of ISIS's top 40 leaders once served in the Iraqi military."[34] Figure F.1 shows the correlation between prices and conflict in low and high suitability areas. It is clearly visible that, around the approximate timing of this change, the relationship between drug profitability and conflict becomes much stronger in high suitability districts. Dissolving the militias eliminates many reasonably paid jobs, which increases the reliance on income from the opium economy. Hence, the results contrast Berman *et al.* (2011b), who use survey results for two years and find no effect of unemployment. It provides further evidence for the importance of the opportunity cost mechanism in Afghanistan. This also highlights an important trade-off between "cleaning" the state and non-state armed groups as well as fighting the production of an illegal resource at the

[34] See http://time.com/3900753/isis-iraq-syria-army-united-states-military/, https://www.reuters.com/investigates/special-report/mideast-crisis-iraq-islamicstate/, https://www.independent.co.uk/news/world/middle-east/how-saddam-husseins-former-military-officers-and-spies-are-controlling-isis-10156610.html and http://nationalpost.com/news/world/how-the-catastrophic-american-decision-to-disband-saddams-military-helped-fuel-the-rise-of-isil, last accessed August 28, 2019. A detailed report about "Lessons of De-Baathification in Iraq" is by Sissons and Al-Saiedi, available at https://www.ictj.org/publication/bitter-legacy-lessons-de-baathification-iraq, last accessed August 28, 2019.

same time.

Modifications of the treatment variable: Drug prices

Table F.1: Non-normalized drug prices, 2002-2014

	(log) BRD	**1 if** ≥ 5	**1 if** ≥ 10	**1 if** ≥ 25	**1 if** ≥ 100
	(1)	(2)	(3)	(4)	(5)
	Panel A: Local Opium Price				
Opium Profitability (t-1)	-0.644	-0.166	-0.165	-0.143	-0.079
	(0.200)	(0.059)	(0.056)	(0.052)	(0.030)
Number of observations	5174	5174	5174	5174	5174
Adjusted R-Squared	0.649	0.501	0.483	0.454	0.311
	Panel B: International Heroin Price (baseline)				
Opium Profitability (t-1)	-2.103	-0.503	-0.550	-0.465	-0.183
	(0.835)	(0.256)	(0.233)	(0.206)	(0.108)
Adjusted R-Squared	0.649	0.501	0.483	0.453	0.310
	Panel C: Complement Price				
Opium Profitability (t-1)	-4.023	-1.016	-0.982	-0.870	-0.371
	(1.337)	(0.399)	(0.364)	(0.329)	(0.176)
Adjusted R-Squared	0.650	0.502	0.484	0.454	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 drug prices (in logarithms) and the suitability to grow opium. Standard errors are in parentheses (clustered at the district level).

Table F.2: International heroin price, price not in logarithms, 2002-2014

	(log) BRD	**1 if** ≥ 5	**1 if** ≥ 10	**1 if** ≥ 25	**1 if** ≥ 100
	(1)	(2)	(3)	(4)	(5)
Opium Profitability (t-1)	-6.970	-1.665	-1.781	-1.438	-0.553
	(2.232)	(0.696)	(0.618)	(0.537)	(0.277)
Number of observations	5174	5174	5174	5174	5174
Adjusted R-squared	0.649	0.501	0.483	0.453	0.310

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 international price (prices are not in logarithms) and the suitability to grow opium. Standard errors are in parentheses (clustered at the district level).

Table F.3: International heroin price, de-trended prices, 2002-2014

	(log) BRD (1)	**1 if** $\geq$ 5 (2)	**1 if** $\geq$ 10 (3)	**1 if** $\geq$ 25 (4)	**1 if** $\geq$ 100 (5)
Opium Profitability (t-1)	-6.799	-1.566	-1.882	-1.265	-0.482
	(2.232)	(0.681)	(0.606)	(0.541)	(0.248)
Number of observations	5174	5174	5174	5174	5174
Adjusted R-Squared	0.647	0.499	0.481	0.452	0.310

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 a product of the de-trended heroin price with opium suitability. Standard errors are in parentheses (clustered at the district level).

Table F.4: International heroin price, in deviations, 2002-2014

	(log) BRD (1)	**1 if** $\geq$ 5 (2)	**1 if** $\geq$ 10 (3)	**1 if** $\geq$ 25 (4)	**1 if** $\geq$ 100 (5)
Opium Profitability (t-1)	-6.197	-1.434	-1.567	-1.387	-0.620
	(3.136)	(0.875)	(0.887)	(0.747)	(0.350)
Number of observations	5174	5174	5174	5174	5174
Adjusted R-Squared	0.649	0.501	0.483	0.453	0.310

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 international price deviations (from the mean) and the suitability to grow opium. Standard errors are in parentheses (clustered at the district level).

Table F.5: Main results using cocaine prices, 2002-2014 period

	(log) BRD (1)	**1 if** $\geq$ 5 (2)	**1 if** $\geq$ 10 (3)	**1 if** $\geq$ 25 (4)	**1 if** $\geq$ 100 (5)
Opium Profitability (t-1)	-0.461	-0.116	-0.124	-0.102	-0.026
	(0.199)	(0.059)	(0.057)	(0.051)	(0.025)
Adjusted R-Squared	0.650	0.501	0.484	0.454	0.310

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 equal across all panels. Standard errors are in parentheses (clustered at the district level). Compare with main results in Table 2.

Modifications of the treatment variable: Suitability

Table F.6: Unweighted suitabilities, 2002-2014

	(log) BRD (1)	**1 if** ≥ 5 (2)	**1 if** ≥ 10 (3)	**1 if** ≥ 25 (4)	**1 if** ≥ 100 (5)
Opium Profitability (t-1)	-0.988	-0.249	-0.244	-0.188	-0.031
	(0.290)	(0.093)	(0.088)	(0.077)	(0.041)
Number of observations	5174	5174	5174	5174	5174
Adjusted R-squared	0.650	0.501	0.483	0.453	0.310

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 international prices (in logarithms) and the unweighted suitability to grow opium (in analogy for wheat). Standard errors are in parentheses (clustered at the district level).

Modifications of the treatment variable: Dyadic Diff-in-Diff

Table F.7: Diff-in-Diff - Dyadic treatment, 2002-2014

	(log) BRD (1)	**1 if** ≥ 5 (2)	**1 if** ≥ 10 (3)	**1 if** ≥ 25 (4)	**1 if** ≥ 100 (5)
	Panel A: Suitability dichotomized				
Opium Profitability (t-1)	-0.229	-0.052	-0.041	-0.042	-0.017
	(0.087)	(0.027)	(0.026)	(0.022)	(0.013)
Number of observations	5174	5174	5174	5174	5174
Adjusted R-Squared	0.648	0.500	0.482	0.452	0.311
	Panel B: Suitability and Heroin Price dichotomized				
Opium Profitability (t-1)	-0.397	-0.107	-0.090	-0.071	-0.029
	(0.117)	(0.038)	(0.035)	(0.030)	(0.018)
Number of observations	5174	5174	5174	5174	5174
Adjusted R-Squared	0.649	0.501	0.483	0.453	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. Suitability (for opium and wheat) dichotomized according to the sample median in panel A. Suitability (for opium and wheat) and international prices (for heroin and wheat) dichotomized according to the sample median in panel B. Standard errors are in parentheses (clustered at the district level).

Adaptions of empirical model

Table F.8: Main results using normalized drug prices, district- and year-fixed effects, 2002-2014

	(log) BRD (1)	**1 if** ≥ 5 (2)	**1 if** ≥ 10 (3)	**1 if** ≥ 25 (4)	**1 if** ≥ 100 (5)
			Panel A: Local Opium Price		
Opium Profitability (t-1)	-0.280	-0.097	-0.078	-0.026	0.000
	(0.091)	(0.028)	(0.026)	(0.021)	(0.012)
Number of observations	5174	5174	5174	5174	5174
Adjusted R-Squared	0.562	0.422	0.410	0.389	0.264
			Panel B: International Heroin Price (baseline)		
Opium Profitability (t-1)	-0.451	-0.132	-0.122	-0.050	-0.010
	(0.209)	(0.064)	(0.063)	(0.051)	(0.023)
Adjusted R-Squared	0.561	0.421	0.409	0.389	0.264
			Panel C: Complement Price		
Opium Profitability (t-1)	-0.707	-0.217	-0.172	-0.091	-0.028
	(0.222)	(0.068)	(0.064)	(0.050)	(0.023)
Adjusted R-Squared	0.563	0.423	0.410	0.390	0.264

Notes: Models include 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. Standard errors are in parentheses (clustered at the district level).

Table F.9: Population-density-specific heterogeneity, 2002-2014

Interaction with	**Continuous** (1)	**<10 percentile** (2)	**<20 percentile** (3)
Opium Profitability (t-1)	-0.978	-1.019	-1.032
	(0.295)	(0.283)	(0.308)
Opium Profitability (t-1)*X	-0.021	0.413	0.198
	(0.075)	(1.319)	(0.940)

Notes: Models include province-times-year- and district-fixed effects. The dependent variable is the log of battle-related deaths in (t). Regressions include interactions of the opium profitability with different indicators of population density (X) as indicated in in the column heading. Column 1 includes a continuous measure on population density, columns 2 and 3 include binary indicators taking a value of one if the population density is below the 10th or 20th percentile. Opium Profitability is defined as the interaction between the normalized international prices (in logarithms) and the unweighted suitability to grow opium (in analogy for wheat). Standard errors are in parentheses (clustered at the district level).

Outcome and timing (cultivation and revenue)

Table F.10: Effect of income shocks on opium cultivation, 2002-2014

	Outcome: (t)	**Outcome: (t)+(t-1)**
	(1)	(2)
Opium Profitability (t-1)	0.483	0.705
	(0.307)	(0.308)
Number of observations	5174	5174
Adjusted R-Squared	0.399	0.488

Notes: The dependent variables opium cultivation is in logarithms. Column (1) presents lagged effects. Column (2) reports lagged and contemporaneous effects by defining the outcome as the moving average, i.e. (revenues(t)+revenues(t-1))/2. Opium Profitability is defined as the interaction between the normalized heroin price (in logarithms) and the suitability to grow opium. Standard errors clustered at the district level are displayed in parentheses.

Table F.11: IVs for revenue in (t)+(t-1), 2002-2014

	(log) BRD (1)	**1 if** ≥ 5 (2)	**1 if** ≥ 10 (3)	**1 if** ≥ 25 (4)	**1 if** ≥ 100 (5)
			Panel A: Opium Price Shock (t-1) as IV		
(log) Revenue:(t)+(t-1)	-0.173	-0.049	-0.045	-0.020	-0.004
	(0.099)	(0.030)	(0.030)	(0.022)	(0.010)
Number of observations	5085	5085	5085	5085	5085
Kleibergen-Paap F stat.	11.047	11.047	11.047	11.047	11.047
			Panel B: Legal Opioids (t-1) as IV		
(log) Revenue:(t)+(t-1)	-0.252	-0.075	-0.060	-0.032	-0.011
	(0.133)	(0.039)	(0.036)	(0.025)	(0.011)
Kleibergen-Paap F stat.	6.672	6.672	6.672	6.672	6.672

Notes: Models include year- and district-fixed effects. The dependent variable is conflict in (t) operationalized as indicated in the column heading. Opium revenues is operationalized as the moving average between (t) and (t-1). Standard errors are in parentheses (clustered at the district level).

Table F.12: Corresponding 1st stage results for revenues (t)+(t-1), 2002-2014

	Opium Profitability (1)	**Legal Opioids** (2)
Opium Profitability (t-1)	2.489	
	(0.749)	
Legal Opioids (t-1)		-11.489
		(4.448)

Notes: Models include year- and district-fixed effects. The dependent variable is opium revenue or cultivation as indicated in the panel heading. The corresponding IVs are indicated in the column heading. Standard errors are in parentheses (clustered at the district level).

Standard errors

Table F.13: Standard errors clustered at different levels, 2002-2014

	(log) BRD (1)	**1 if** ≥ 5 (2)	**1 if** ≥ 10 (3)	**1 if** ≥ 25 (4)	**1 if** ≥ 100 (5)
			Panel A: Clustered at district and year level		
Opium Profitability (t-1)	-0.675	-0.167	-0.191	-0.147	-0.040
	(0.325)	(0.092)	(0.080)	(0.074)	(0.051)
Number of observations	5174	5174	5174	5174	5174
Adjusted R-Squared	0.649	0.501	0.484	0.453	0.310
			Panel B: Clustered at province and year level		
Opium Profitability (t-1)	-0.675	-0.167	-0.191	-0.147	-0.040
	(0.365)	(0.103)	(0.103)	(0.082)	(0.044)
Number of observations	5174	5174	5174	5174	5174
Adjusted R-Squared	0.649	0.501	0.484	0.453	0.310

Notes: Models include province-times-year- and district-fixed effects. The dependent variable is conflict in (t) operationalized as indicated in the column heading. Standard errors are clustered as indicated in the panel heading.

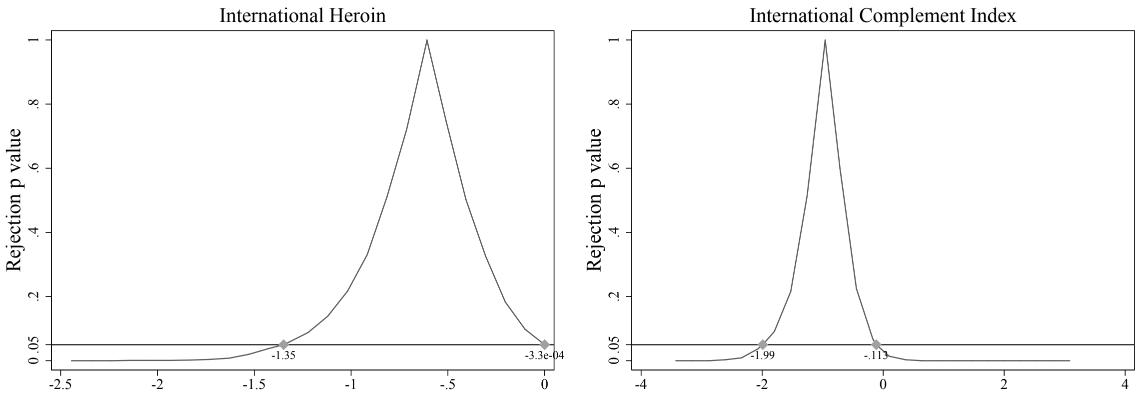


Figure F.2: Wild Bootstrap (province level clustered se, 95% confidence intervals)

Notes: Figures show the distribution of bootstrap estimates. The dependent variable is the (log) of BRD. Regressions correspond to Table 2 column 1 (panel B and C). The number indicate the left and right 95%-confidence interval. The test of the the null hypothesis at the 5%-level is whether this intervall contains 0.

Covariates and trends

Table F.14: Wheat Profitability, 2002-2014

	(log) BRD (1)	**1 if** ≥ 5 (2)	**1 if** ≥ 10 (3)	**1 if** ≥ 25 (4)	**1 if** ≥ 100 (5)
			Panel A: Wheat Profitability included		
Opium Profitability (t-1)	-0.675	-0.167	-0.191	-0.147	-0.040
	(0.296)	(0.090)	(0.085)	(0.075)	(0.037)
Wheat Profitability (t-1)	0.307	0.088	0.077	0.034	-0.010
	(0.123)	(0.039)	(0.036)	(0.031)	(0.019)
Number of observations	5174	5174	5174	5174	5174
Adjusted R-Squared	0.649	0.501	0.484	0.453	0.310
			Panel B: Wheat Profitability excluded		
Opium Profitability (t-1)	-0.923	-0.238	-0.253	-0.175	-0.031
	(0.279)	(0.085)	(0.079)	(0.069)	(0.030)
Number of observations	5174	5174	5174	5174	5174
Adjusted R-Squared	0.649	0.501	0.483	0.453	0.310

Notes: Models include province-times-year- and district-fixed effects. The dependent variable is conflict in (t) operationalized as indicated in the column heading. Standard errors are in parentheses (clustered at the district level).

Table F.15: Dynamics - lagged dependent, 2002-2014

	(log) BRD (1)	**1 if** ≥ 5 (2)	**1 if** ≥ 10 (3)	**1 if** ≥ 25 (4)	**1 if** ≥ 100 (5)
Opium Profitability (t-1)	-0.455	-0.140	-0.160	-0.102	-0.021
	(0.252)	(0.084)	(0.076)	(0.065)	(0.032)
Dependent (t-1)	0.236	0.114	0.153	0.228	0.207
	(0.023)	(0.019)	(0.023)	(0.027)	(0.040)
Number of observations	5174	5174	5174	5174	5174
Adjusted R-Squared	0.670	0.508	0.495	0.481	0.340

Notes: Models include province-times-year- and district-fixed effects. The dependent variable is conflict in (t) operationalized as indicated in the column heading. Standard errors are in parentheses (clustered at the district level).

Table F.16: Including covariates, 2002-2014

	(log) BRD (1)	**1 if** ≥ 5 (2)	**1 if** ≥ 10 (3)	**1 if** ≥ 25 (4)	**1 if** ≥ 100 (5)
	Panel A: Baseline covariates				
Opium Profitability (t-1)	-0.595	-0.177	-0.188	-0.132	-0.014
	(0.275)	(0.086)	(0.082)	(0.070)	(0.038)
Wheat Profitability (t-1)	0.282	0.093	0.077	0.028	-0.019
	(0.129)	(0.041)	(0.037)	(0.032)	(0.019)
VHI (t)	0.000	-0.000	-0.000	0.000	-0.000
	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)
Luminosity (t-2)	0.018	0.005	0.002	-0.003	-0.000
	(0.020)	(0.006)	(0.006)	(0.005)	(0.003)
(log) Population (t-2)	1.417	-0.478	0.037	0.611	0.789
	(3.472)	(0.911)	(0.900)	(0.959)	(0.308)
Number of observations	5173	5173	5173	5173	5173
Adjusted R-Squared	0.649	0.501	0.483	0.453	0.311
	Panel B: Baseline covariates, time-invariant covariates×trend				
Opium Profitability (t-1)	-0.659	-0.176	-0.192	-0.173	-0.032
	(0.268)	(0.084)	(0.081)	(0.068)	(0.039)
Wheat Profitability (t-1)	0.262	0.090	0.080	0.025	-0.017
	(0.130)	(0.041)	(0.037)	(0.032)	(0.020)
VHI (t)	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.002)	(0.001)	(0.000)	(0.000)	(0.000)
Luminosity (t-2)	0.014	0.004	0.001	-0.004	-0.001
	(0.020)	(0.006)	(0.006)	(0.005)	(0.003)
(log) Population (t-2)	-0.657	-0.891	-0.712	-0.224	0.854
	(3.475)	(0.950)	(0.957)	(0.931)	(0.381)
Number of observations	5147	5147	5147	5147	5147
Adjusted R-Squared	0.654	0.504	0.487	0.461	0.316
	Panel C: Baseline covariates, time-invariant covariates×time dummies				
Opium Profitability (t-1)	-0.744	-0.206	-0.218	-0.187	-0.042
	(0.288)	(0.089)	(0.087)	(0.073)	(0.042)
Wheat Profitability (t-1)	0.271	0.089	0.081	0.032	-0.020
	(0.142)	(0.043)	(0.041)	(0.036)	(0.022)
VHI (t)	-0.000	-0.001	-0.000	0.000	-0.000
	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)
Luminosity (t-2)	0.010	0.003	0.000	-0.005	-0.000
	(0.020)	(0.006)	(0.006)	(0.005)	(0.003)
(log) Population (t-2)	-0.860	-0.847	-0.753	-0.345	0.816
	(3.520)	(0.985)	(0.974)	(0.938)	(0.379)
Number of observations	5147	5147	5147	5147	5147
Adjusted R-Squared	0.655	0.504	0.487	0.462	0.313

Notes: Models include province-times-year- and district-fixed effects. The dependent variable is conflict in (t) operationalized as indicated in the column heading. The set of time-invariant covariates includes Ruggedness (categorized in five quantiles), Distance Linear, Distance 2D and 3D, Travel Time 2D and 3D (all distances to Kabul), Pashtuns, Mixed Ethnic Groups, Number of Ethnic Groups, Taliban Territory 1996, Mixed Territory 1996, and Ethnic Trafficking Route. Standard errors are in parentheses (clustered at the district level).

Table F.17: Production and opium-suitability-specific trends, 2002-2014

	Based on production			**Based on opium suitability**		
Percentile	**50**	**75**	**90**	**50**	**75**	**90**
	(1)	(2)	(3)	(4)	(5)	
Opium Profitability (t-1)	-0.499	-0.521	-0.492	-0.426	-0.606	-0.737
	(0.208)	(0.211)	(0.209)	(0.363)	(0.342)	(0.314)
Adjusted R-Squared	0.564	0.562	0.562	0.650	0.649	0.649

Notes: Models include province-times-year- and district-fixed effects as well as production- or suitability-specific time trends based on the percentile as indicated in the column heading. The dependent variable is the log of battle-related deaths in (t). Standard errors are in parentheses (clustered at the district level).

Outlier analysis

Table F.18: Drop potential outliers, 2002-2014

	(log) BRD	**1 if** ≥ 5	**1 if** ≥ 10	**1 if** ≥ 25	**1 if** ≥ 100
	(1)	(2)	(3)	(4)	(5)
			Panel A: No border districts		
Opium Profitability (t-1)	-0.601	-0.160	-0.161	-0.146	-0.014
	(0.304)	(0.098)	(0.096)	(0.086)	(0.055)
Number of observations	3718	3718	3718	3718	3718
Adjusted R-Squared	0.678	0.523	0.512	0.483	0.341
			Panel B: No Southern provinces (Kandahar and Hilmand)		
Opium Profitability (t-1)	-0.674	-0.174	-0.215	-0.118	-0.007
	(0.311)	(0.096)	(0.091)	(0.078)	(0.033)
Number of observations	4732	4732	4732	4732	4732
Adjusted R-Squared	0.620	0.479	0.458	0.407	0.254

Notes: Models include province-times-year- and district-fixed effects. The dependent variable is conflict in (t) operationalized as indicated in the column heading. In panel A, all border districts are excluded and in panel B all districts in the two provinces Kandahar and Hilmand are excluded. Standard errors are in parentheses (clustered at the district level).

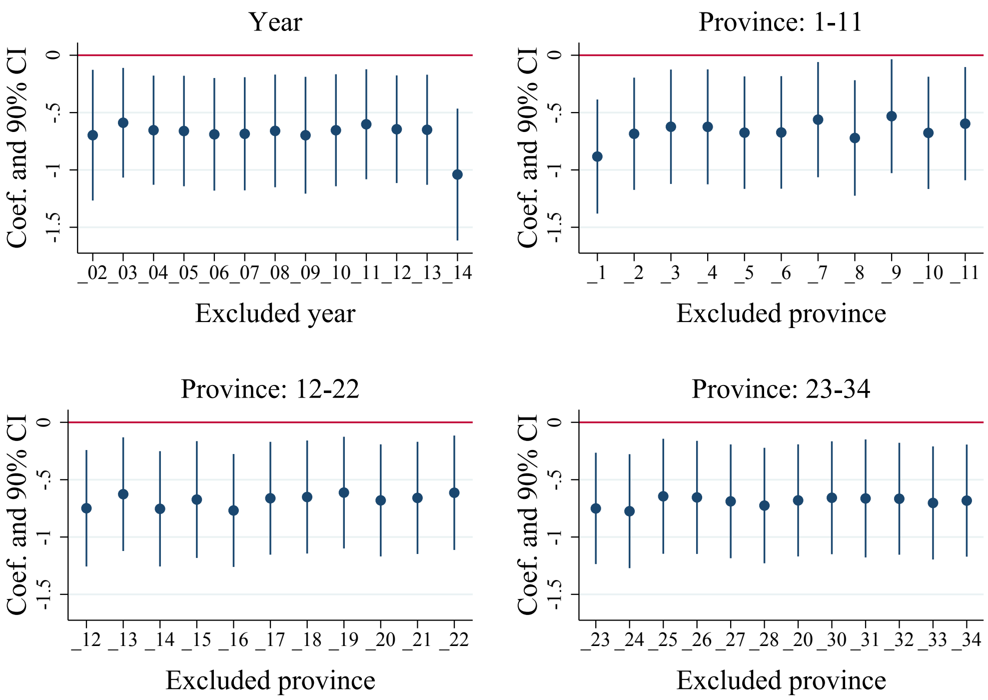


Figure F.3: Leave one out - year and province

Notes: This figure shows results for 47 separate regressions in analogy to panel B's column (1) of Table 2, where we leave out one year or one province at the time. This also alleviates concerns whether particular outliers in the cross-sectional variation drive our result.

Randomization

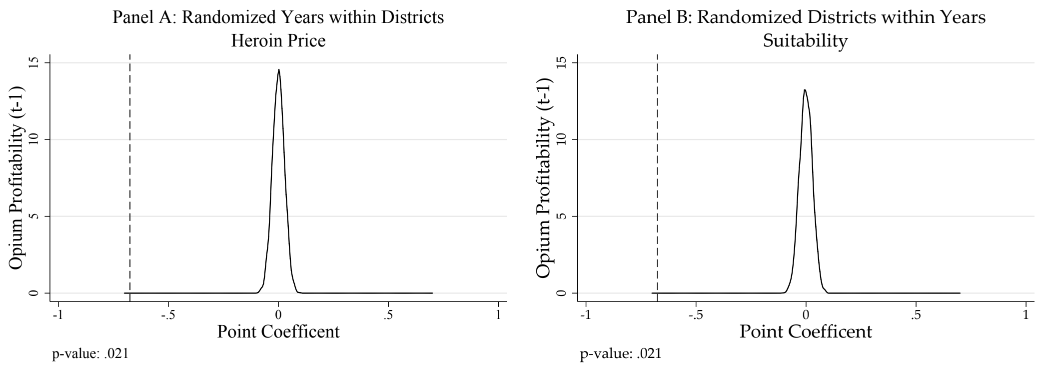


Figure F.4: Randomization: Heroin Price and Opium Suitability

Notes: This figure plots the distribution of the coefficients generated by 5'000 randomizations, with panel A randomly reordering prices across years within districts and multiplying with the actual suitability and panel B reordering the suitability across districts and multiplying with the actual price in the respective yes. Based on the regression model in Panel B's column (1) of Table 2. For this placebo test, we want to see whether the randomized coefficients are centered around zero, and what share of the draws turn out to be more negative than the actual treatment coefficient. This share is used to compute the randomization inference p-value shown in the bottom of the graph.

Partial leverage plot of first stage result presented in Table 3

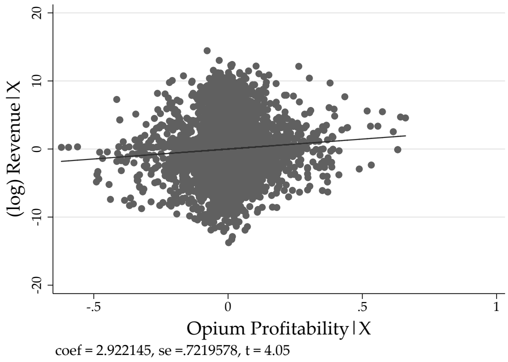


Figure F.5: 1st stage IV results for (log) Revenue

Tables and robustness for regressions at the household level

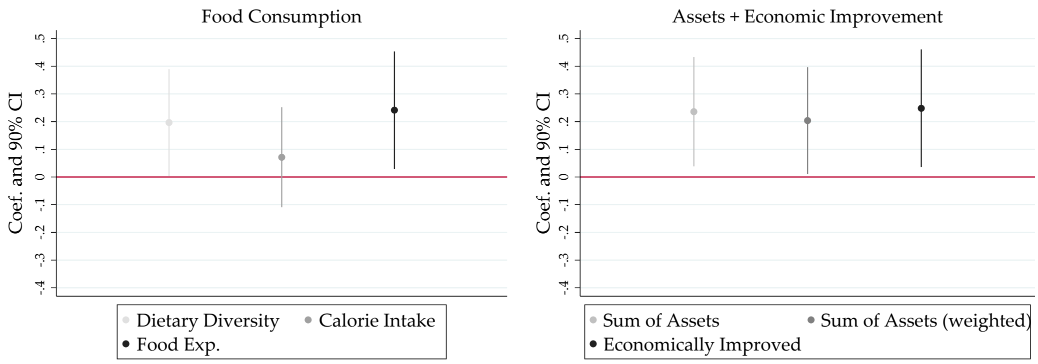


Figure F.6: Effect of Opium Profitability (t-1) on living standard indicators in (t) based on food consumption, expenditures an assets, accounting for household survey weights

Notes: The figure shows results of 6 separate regressions in analogy to Table F.19. The difference is that we include household survey weights in the regressions. Results are also robust to using robust standard errors rather than clustering at the district-year level.

Table F.19: Living standards at the household level, 2005-2012

	(1)	(2)	(3)
		Panel A: Food consumption	
	Dietary Diversity	**Calorie Intake**	**Food Insecurity**
Opium Profitability (t-1)	0.571	143.915	698.905
	(0.289)	(256.750)	(303.057)
Number of observations	72224	71634	72643
Adjusted R-Squared	0.371	0.139	0.225
		Panel B: Food expenditures	
	Food Exp.	**Food Exp. Paasche adj.**	**Food Exp. Laspeyres adj.**
Opium Profitability (t-1)	698.905	788.172	750.822
	(303.057)	(312.228)	(314.647)
Number of observations	72643	72643	72635
Adjusted R-Squared	0.225	0.196	0.217
		Panel C: Assets	
	Sum of Assets	**Sum of Assets weighted**	**Economically improved**
Opium Profitability (t-1)	0.925	0.614	0.431
	(0.327)	(0.217)	(0.225)
Number of observations	72447	66620	70670
Adjusted R-Squared	0.323	0.336	0.249

Notes: Models include province-times-year- and district-fixed effects. The dependent variable in (t) is 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. Standard errors are in parentheses (clustered at the district-year level).

Robustness for Table 5

Table F.20: Government control and law enforcement, 2002-2014 period

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Military bases and Kabul					
Interaction with	**Proximity to military bases**			**Proximity to Kabul**		
	Linear distance			**Linear distance**		
	1 if ≤ 75	**1 if ≤ 100**	**1 if ≤ 125**	**1 if ≤ 75**	**1 if ≤ 100**	**1 if ≤ 125**
Opium Profitability (t-1)	-1.258	-1.115	-0.963	-0.826	-0.782	-0.650
	(0.382)	(0.431)	(0.478)	(0.308)	(0.313)	(0.329)
Opium Profitability (t-1)*X	1.489	0.896	0.440	1.693	0.712	0.334
	(0.452)	(0.489)	(0.527)	(0.800)	(0.667)	(0.621)
	Panel B: Main cities and the Taliban					
Interaction with	**Proximity to other cities**			**Taliban dominance w/o government controlled districts**		
	Linear Distance			**Pashtun**	**Former territory**	
	1 if ≤ 75	**1 if ≤ 100**	**1 if ≤ 125**	**Presence**	**All**	**w/o north**
Opium Profitability (t-1)	-0.685	-0.535	-0.545	1.400	0.466	0.376
	(0.327)	(0.345)	(0.361)	(0.468)	(0.656)	(0.598)
Opium Profitability (t-1)*X	-0.014	-0.463	-0.366	-2.461	-1.325	-1.250
	(0.527)	(0.456)	(0.407)	(0.509)	(0.752)	(0.684)

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. Regressions include interactions of the opium profitability with a variable X as indicated in in the column heading. The other main cities are Kandahar, Kunduz, Jalalabad, Hirat, and Mazari Sharif (next five largest cities). Standard errors are in parentheses (clustered at the district level).

Robustness for Table 5

Table F.21: Alternative proxy for Taliban Control (based on NRVA)

Interaction with	**Any Pashtuns**	**Share Pashtuns**
	(1)	(2)
Opium Profitability (t-1)	-0.062	-0.283
	(0.403)	(0.359)
Opium Profitability (t-1)*X	-1.157	-1.005
	(0.433)	(0.577)

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 drug prices (in logarithms) and the suitability to grow opium. Regressions include interactions of the opium profitability with a variable X as indicated in in the column heading. For definitions of the variables X please see Appendix A. For this table the different measures on ethnic groups are derived from the NRVA 2003, which is not nationally representative, but serves as suitable a proxy for ethnic group distribution. Standard errors are in parentheses (clustered at the district level).

Table for Figure 10

Table F.22: Regressions results corresponding to Figure 10

Measure of Taliban control:	Former Taliban area (1)	Pashtun Presence (2)
Effect of opium profitability under		
Limited Statehood (B)	0.006	-0.458
	(0.427)	(0.441)
Government Control (C)	0.129	0.127
	(0.371)	(0.381)
Taliban Control (D)	-1.745	-1.592
	(0.358)	(0.449)

Notes: The dependent variable is the log of battle-related deaths in (t). Standard errors are in parentheses (clustered at the district level). Which district is associated with which scenario is based on the conditions defined in the paper about Taliban or government control. The coefficients are the marginal effects of Opium Profitability conditional on being in one of those three scenarios. Opium Profitability is the interaction between the normalized drug prices (in logarithms) and the suitability to grow opium. All regressions include province-times-year- and district-fixed effects.

Survey results conditional on degree of group competition - omitting districts around Kabul. Figure F.7 shows that within that area within 50 km around Kabul, the effect of a higher opium profitability on household food consumption and assets are also much more heterogeneous and on average more negative. This is in line with more government effort with respect to eradication, which can affect a significant share, but not all producers, and thus increase the variance and decrease the average positive impact of higher prices.

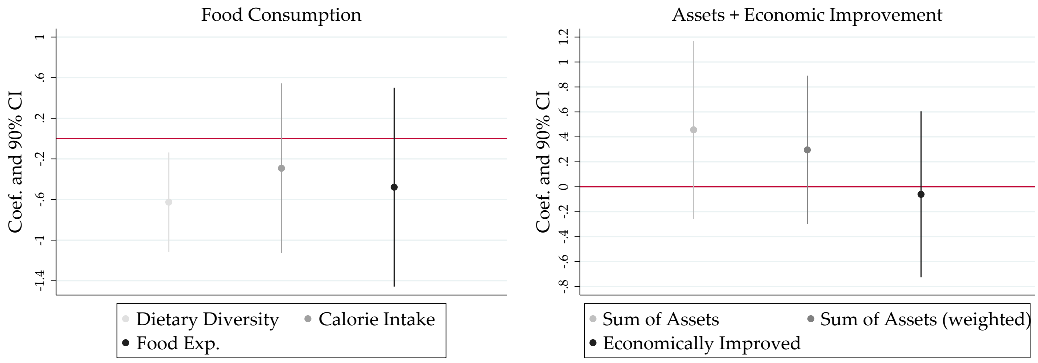


Figure F.7: Effect of Opium Profitability (t-1) on standard of living indicators in (t)

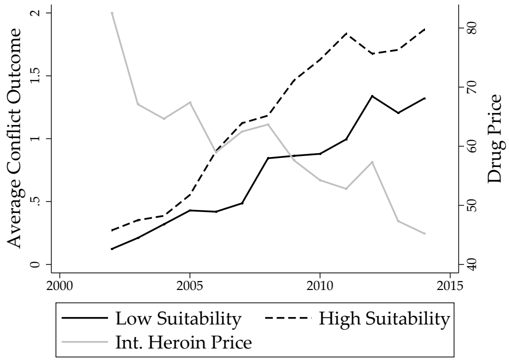


Figure F.8: Variation of conflict across high and low suitable districts over time

Notes: To assign a district to low or high suitability, this figure use an alternative cut-off of 0.3.

Robustness for Figure F.1.

G. Additional maps

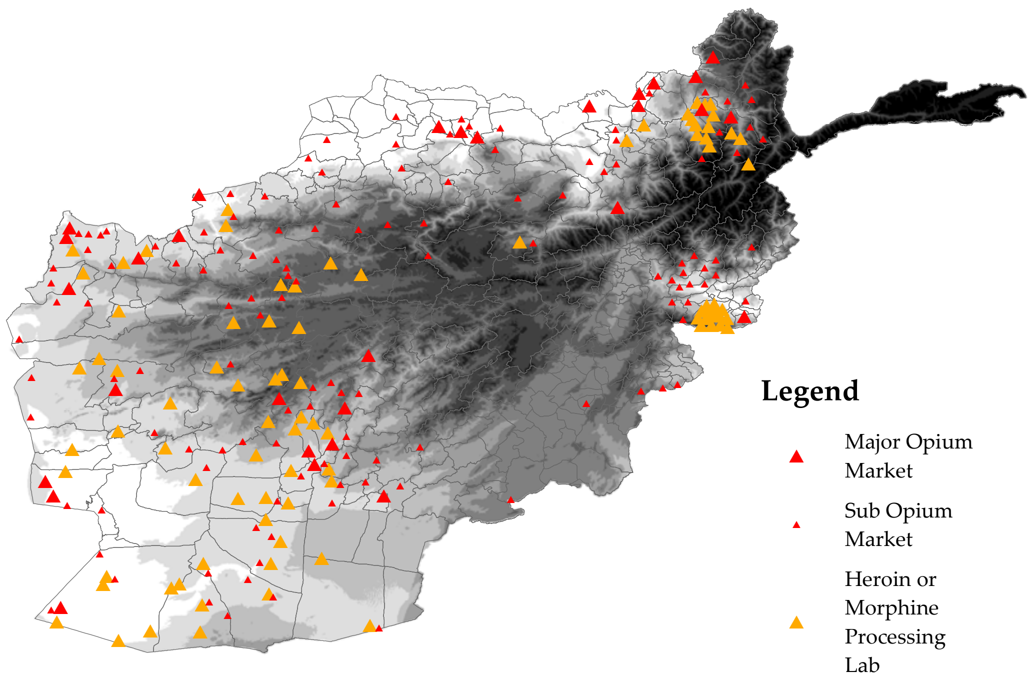


Figure G.1: Opium suitability and the location of opium markets and processing labs

Notes: Opium suitability is from Kienberger *et al.* (2017) and is weighted by population. Opium market and lab information based on UNODC.

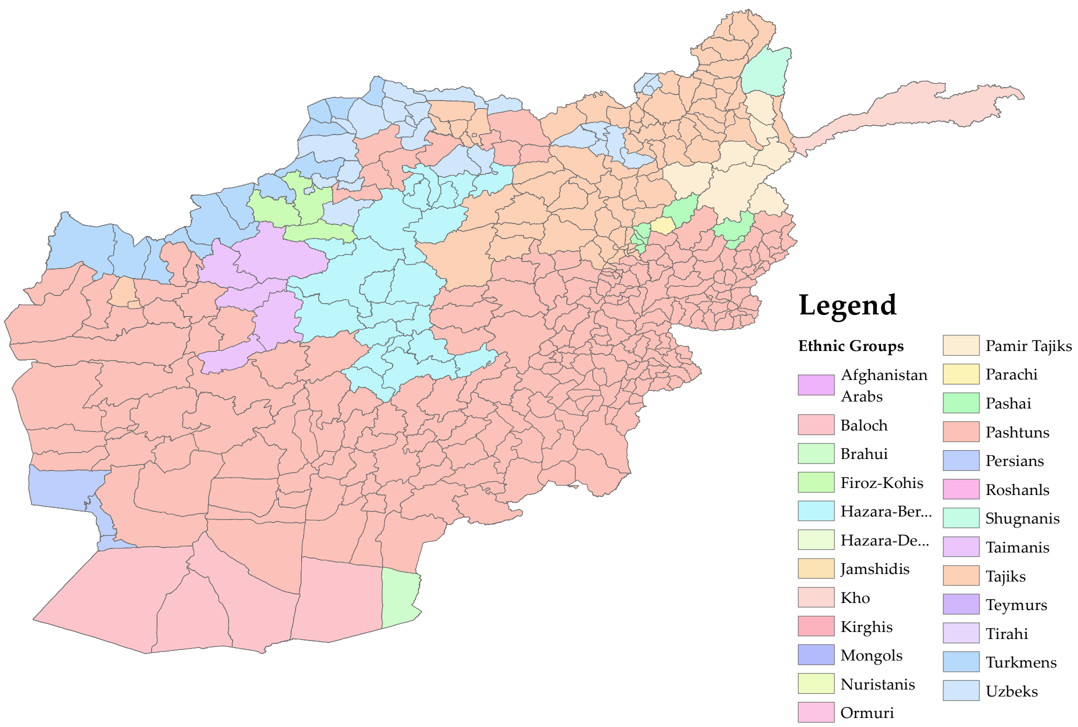


Figure G.2: Distribution of ethnic groups (homelands)

Notes: Distribution of ethnic groups (homelands) in Afghanistan. Note that these are partly overlapping polygons, i.e. some districts feature more than one group even though this is not visible in the map, but we account for this in later estimations. Source: GREG (Weidmann *et al.*, 2010).

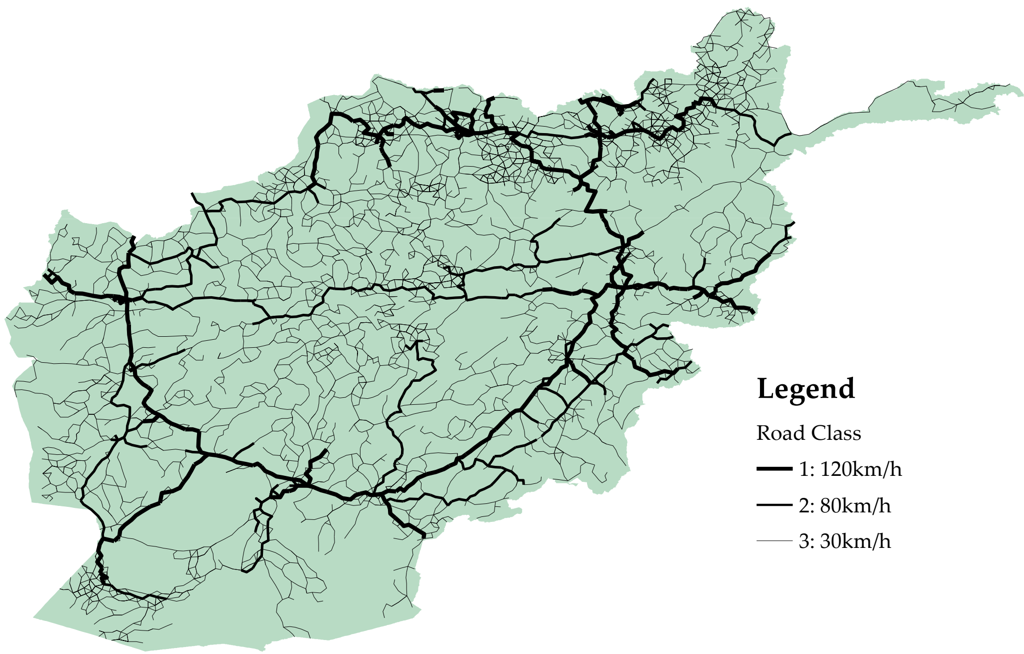


Figure G.3: The road network

Notes: The road network in Afghanistan distinguishing in highways (assumed speed 120 km/h), rural roads (ass. speed 90 km/h), and urban roads (ass. speed 50 km/h). The distinction in road types and the choice of average speed is not decisive for our results.

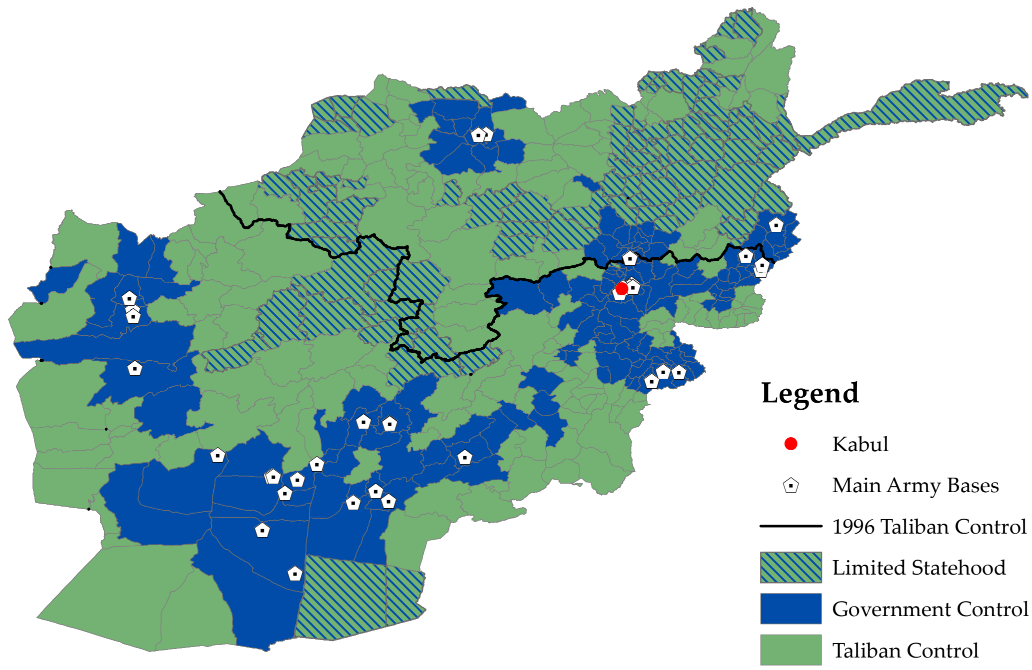


Figure G.4: Control across districts (Pashtun)

Notes: See Figure 10 for the same map using former Taliban territory to determine Taliban control.

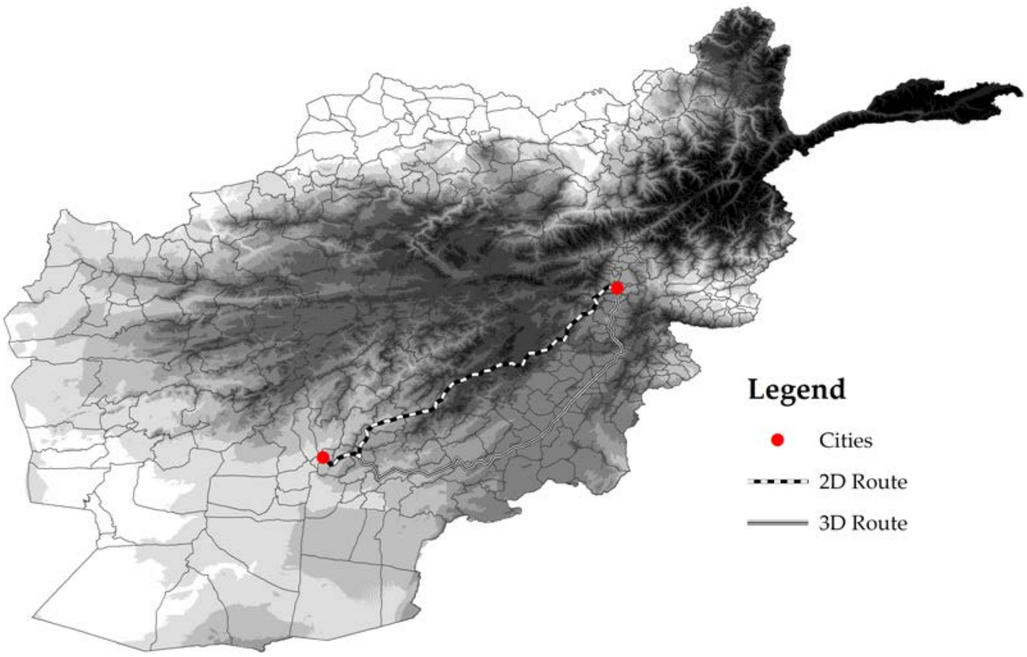
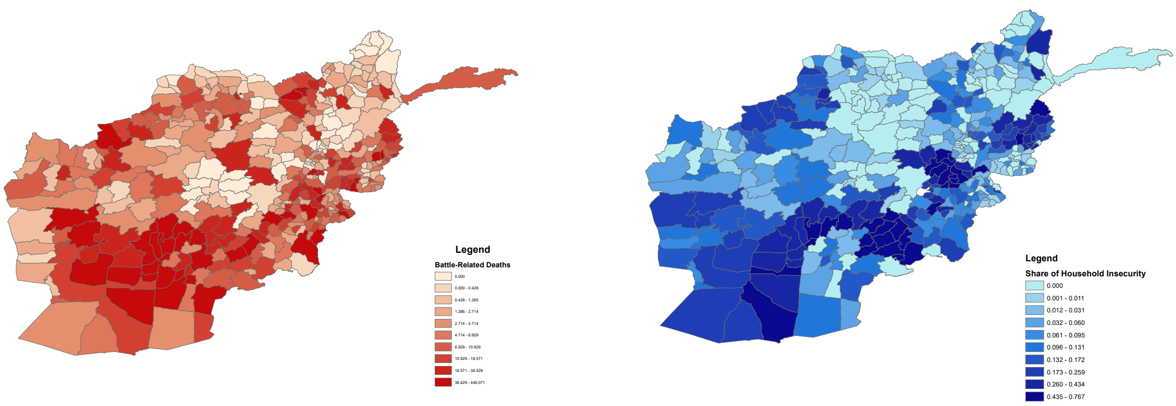


Figure G.5: Elevation and distance

Notes: The intensity of black indicates the elevation in Afghanistan. The white-black dashed line shows the shortest road distance between to district centroids. The second white/black line indicates the shortest distance when accounting for elevation differences along the roads. In particular the central part of Afghanistan is very mountainous, which can have a large effect on transportation costs and travel time.



Number of battle-related deaths (UCDP GED) Share of households experiencing insecurity shock (NRVA)

Figure G.6: Distribution of objective (BRD) and subjective (NRVA) conflict indicators (2002-2014)

The figure below is an excerpt from a book by Dorronsoro (2005). We geo-reference the green area as the area formerly under Taliban control, and the other three polygons as not under Taliban control.

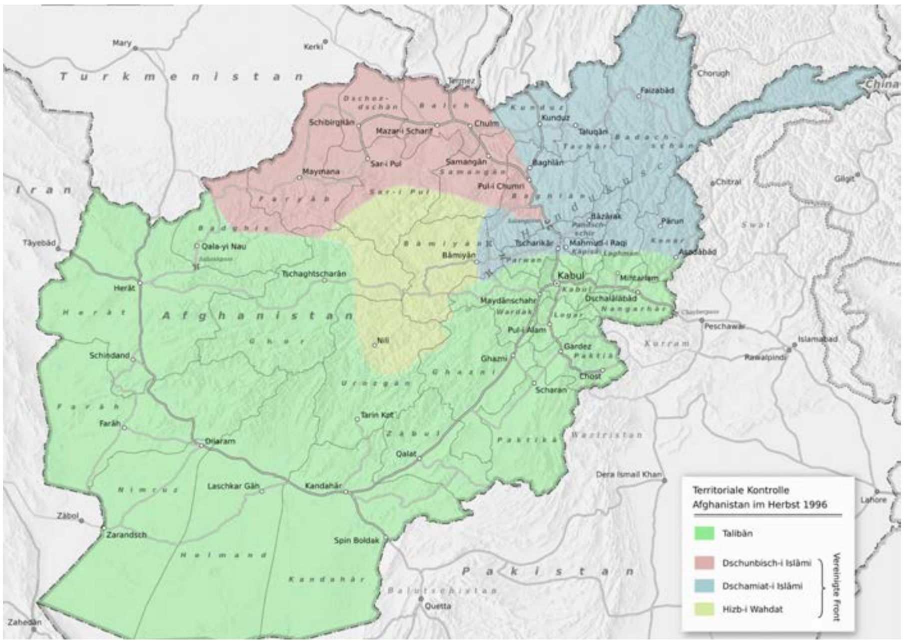
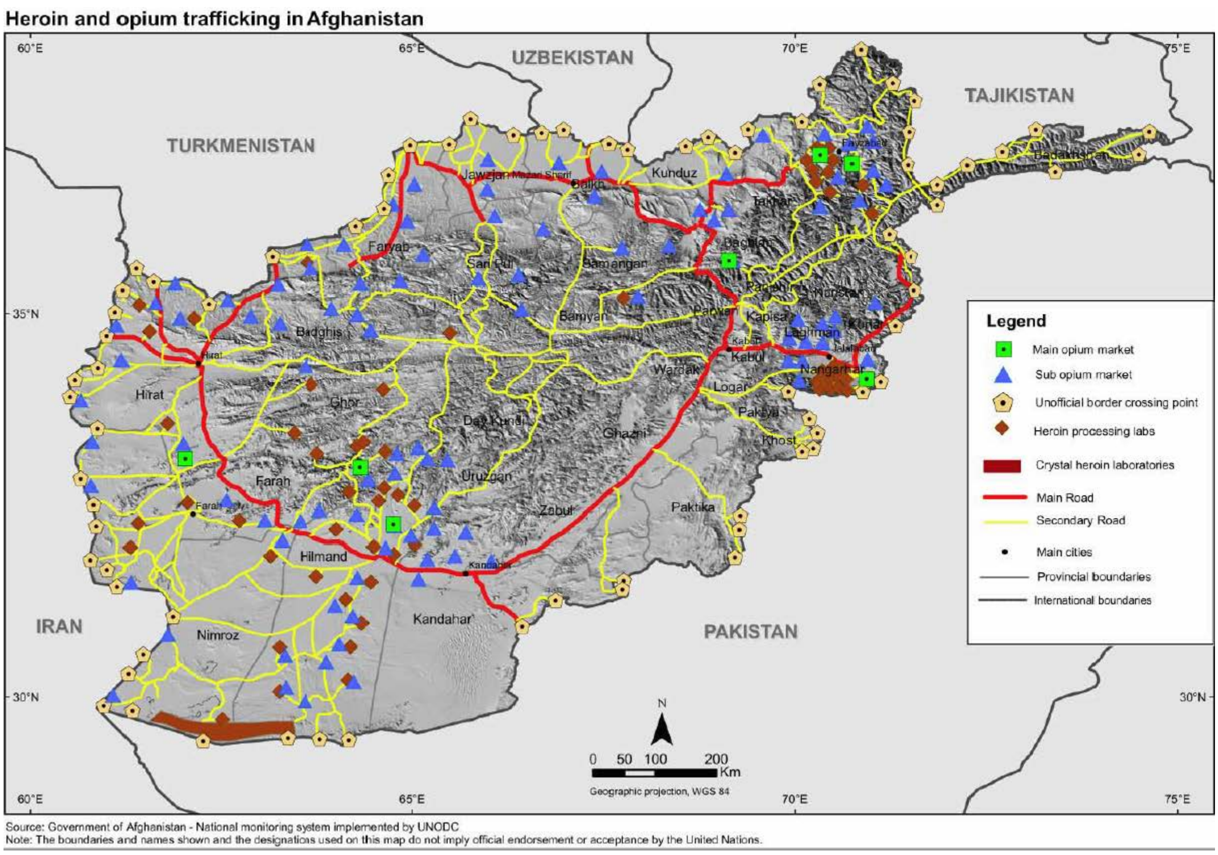


Figure G.7: Political control in Afghanistan in the fall of 1996

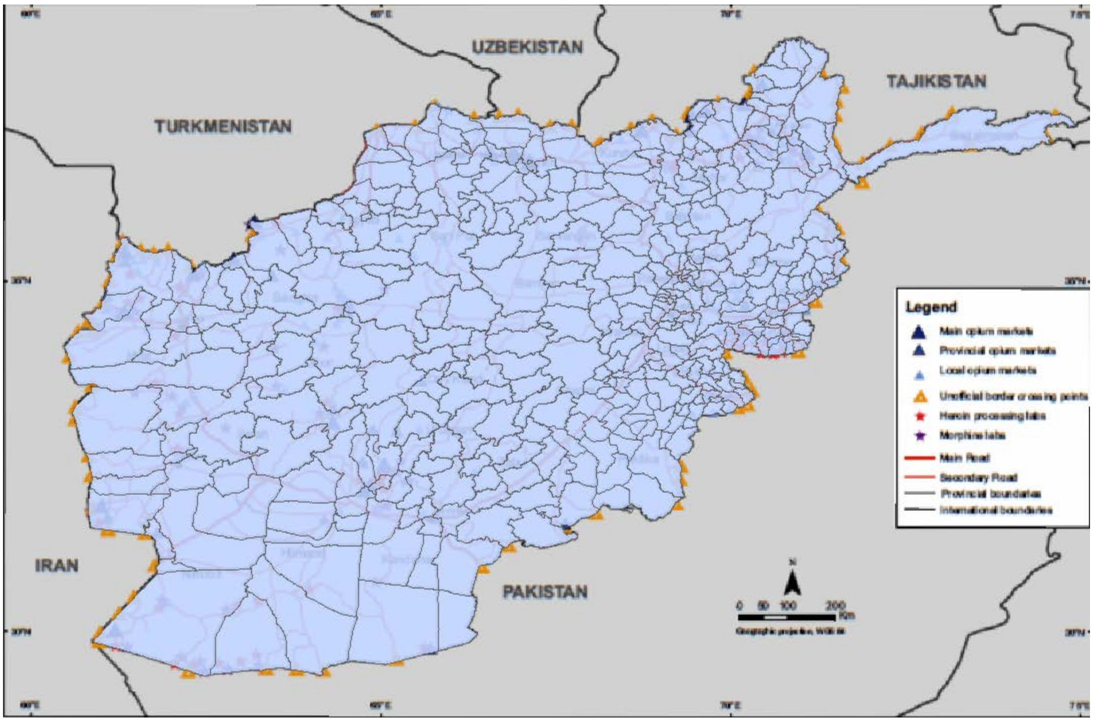
H. Data coding and map generation

Processing and trafficking: There is little to no information that is publicly available on trafficking routes that might be used to smuggle opium through and out of the country. Nevertheless, the UN Office on Drugs and Crime creates and contains spatial maps in its public reports. We digitize a UNODC map from 2007 (about the middle of our sample period) by taking image files of the maps themselves and georeferencing specific points on the images (border points) to a geographically accurate projection of Afghanistan. This process was continued until the map and the images matched perfectly. We then digitized the data contained in the image about the important roads used for trafficking, and the other variables such as main opium markets and heroin processing labs. We verified locations with other UNODC reports.



Original UNODC map (2007)

Map making process: The source of the original map is UNODC's 2007 Afghanistan Opium Survey. The map depicts major and secondary roads, main cities, opium markets, border crossing points, and processing labs. We also used the 2009 Afghanistan Opium Survey to cross-validate the data points. In almost all cases, there were no changes between the two years. In case the 2009 map identifies additional markets or labs we added these as data points. Given that the location of illegal markets and labs will always contain some measurement error and could be moved over time, our aim is to code variables that measure the potential for a trafficking route, border crossing, market or lab. This means that the indicators that we create are time-invarying, also due to the availability of data. We interact the binary indicators extracted from the map with an exogenous variable, so that the interaction term can be interpreted as causal under relatively mild circumstances.



Superimposed maps

In the next step of the process, we match the borders of the image and the georeferenced (Co-ordinate system GCS WGS 1984) shapefile for Afghan authorities.[35] This way, we are accurately overlaying the data points and not simply making an educated guess as to where to place the points. Below are the two final digitized maps based on the UNODC data, overlaid with the district data. The binary indicators that we use in Section 5 on heterogeneous effects are coded as one if the respective feature is present within the boundaries of the district polygon at least once. Alternatively, we use the number of feature per district, e.g., for opium markets.

[35] ESOC Princeton, https://esoc.princeton.edu/files/administrative-boundaries-398-districts, last accessed August 28, 2019.

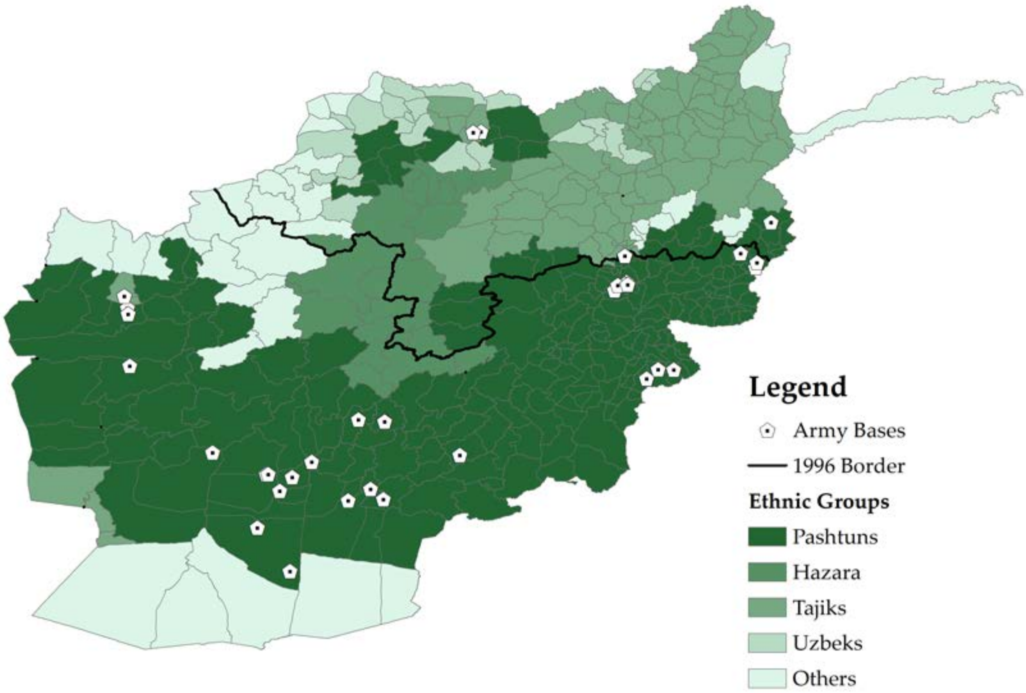


Figure H.1: Final map 2

Notes: The map shows the four major ethnic groups in Afghanistan in different shades of green (Source: GREG). The white symbols with the black dots indicate the location of a foreign military base, for which we could track location, opening and closing date (sources in detail in Appendix 35). The area south of the thick black line was controlled by the Taliban prior to 2001. (Dorronsoro, 2005).

Major military bases: This section describes how we determine the locations of major known military bases in Afghanistan. There are nearly 400 foreign military bases in Afghanistan, but most bases release no official information as to their geographic location for security reasons. In order to find this information, we compile data from different sources about the most relevant bases to include. We then pinpoint, with latitude and longitude coordinates, the exact location of these bases. Since some are now closed, some data points record past base locations. We rely on information from Wikipedia's GeoHack program for the more well-known bases and on news articles, Wikimapia and Google Maps satellite data for the less well-documented ones. News articles were useful in this case because they are often allowed to publish the district in which these bases are located; from there, we were able to look for these bases by referencing photos of the bases (if available) with available satellite data to verify their location. Below, we show the table with the locations of the about 50 bases that we could identify. The exact locations are blackened out for confidentiality reasons, even though we are convinced none of this information is confidential and could be misused or endanger soldiers. Without access to confidential NATO and US military information this is the best data we could assemble. It is certainly not a complete list of bases, which introduces considerable measurement error to the indicator variable we create based on it. At the same time, we have no reason to expect this measurement error to be non-normal.

OBJECTID *	Base Name	Installation Type	Militaries Present	Lat	Lon	District
1	Delaram	FOB	USMC	[redacted]	[redacted]	Delaram
2	Leatherneck	Camp	USMC	[redacted]	[redacted]	Nahri Saraj
3	Kabul International Airport	Camp	ISAF, Turkish Army, US Army, USMC, USAF, Mongolian Armed Forces	[redacted]	[redacted]	Kabul
4	Kandahar Airfield	Airfield	RAF, USAF, US Army	[redacted]	[redacted]	Kandahar
5	Shindand Airbase	Airbase	USAF, AAF	[redacted]	[redacted]	Shindand
6	Bagram Airfield	Airfield	US Army, USAF	[redacted]	[redacted]	Bagram
7	Bastion	Camp	British Army, RAF, Royal Navy (RN), Royal Marines (RM), USMC, Estonian Land Forces, Danish Defence, Tonga Defence Services	[redacted]	[redacted]	Nahri Saraj
8	Price	MOB	RM, British Army, Danish Defence, US Army, USMC	[redacted]	[redacted]	Nahri Saraj
9	Lashkar Gah	MOB	British Army, RM	[redacted]	[redacted]	Lashkargah
10	Eggers	Camp	NATO, US Army, USMC, US Air Force, Australian Army, New Zealand Army, French Army, Turkish Army, Mongolian Armed Forces	[redacted]	[redacted]	Kabul
11	Salerno	FOB	US Army, USAF, US Navy	[redacted]	[redacted]	Khost (Matun)
12	Chapman	FOB	US Special Operation Command, US Army, CIA	[redacted]	[redacted]	Khost (Matun)
13	Marmal	Camp	German Army, German Navy, German Air Force, Royal Netherlands AF, Swedish Air Force, US Army, Mongolian Armed Forces	[redacted]	[redacted]	Mazar-e Sharif
14	Dwyer	Camp	USMC, British Army, RM	[redacted]	[redacted]	Garmsir
15	Rhino	Camp	USMC, US Navy, US Army, USAF, SASR	[redacted]	[redacted]	Garmsir
16	Holland	Camp	Australian Army, New Zealand Army, US Army, Royal Netherlands Army, ANA	[redacted]	[redacted]	Tarin Kot
17	Black Horse	Camp	US Army, Canadian Army	[redacted]	[redacted]	Kabul
18	Dogan	Camp	<Null>	[redacted]	[redacted]	Kabul
19	Invicta	Camp	Italian Army	[redacted]	[redacted]	Kabul
20	Julien	Camp	Canadian Army	[redacted]	[redacted]	Kabul
21	Julien	Camp	Canadian Army	[redacted]	[redacted]	Kabul
22	Phoenix (Qargha)	Camp	US Army	[redacted]	[redacted]	Kabul
23	Souter	Camp	British Army	[redacted]	[redacted]	Kabul
24	Warehouse	Camp	Canadian Army	[redacted]	[redacted]	Kabul
25	Pucino	Camp	USSOCOM	[redacted]	[redacted]	Khost (Matun)
26	Clark	Camp	US Army	[redacted]	[redacted]	Mandozayi
27	Blessing	Camp	US Army, USMC	[redacted]	[redacted]	Waygal
28	Bostick	FOB	US Army	[redacted]	[redacted]	Nari
29	Joyce	FOB	US Army	[redacted]	[redacted]	Sarkani
30	Wright	Camp	US Army	[redacted]	[redacted]	Asadabad
31	Albert	Camp	US Army	[redacted]	[redacted]	Bagram
32	Blackjack	Camp	US	[redacted]	[redacted]	Bagram
33	Bulldog	Camp	US	[redacted]	[redacted]	Bagram
34	Civilian	Camp	US	[redacted]	[redacted]	Bagram
35	Cunningham	Camp	US	[redacted]	[redacted]	Bagram
36	Gibraltar	Camp	US	[redacted]	[redacted]	Bagram
37	Warrior	Camp	US	[redacted]	[redacted]	Bagram
38	Pratt	Camp	US Army	[redacted]	[redacted]	Mazar-e-Sharif
39	Spann	Camp	US Army	[redacted]	[redacted]	Mazar-e-Sharif
40	Baker	Camp	Australian Army	[redacted]	[redacted]	Daman
41	Nathan Smith	Camp	Canadian Army, US Army	[redacted]	[redacted]	Kandahar
42	Hadrian	Camp	Royal Netherlands Army	[redacted]	[redacted]	Deh Rawod
43	Russell	Camp	Australian Army	[redacted]	[redacted]	Tarin Kot
44	Hamidullah	FOB	USMC, British Army, RM	[redacted]	[redacted]	Sangin
45	Arena	Camp	Italian Army, Italian Air Force, US Army	[redacted]	[redacted]	Hirat
46	Stone	Camp	Carabinieri, US Army	[redacted]	[redacted]	Hirat
47	Vianini	Camp	Italian Army	[redacted]	[redacted]	Hirat
48	Losano	Camp	RNLAF, US Army, USAF	[redacted]	[redacted]	Kandahar
49	Lagman	FOB	US Army, US Navy, Romanian Army, ANA	[redacted]	[redacted]	Qalat
50	Shorabak	Camp	ISAF, US, Britain, Denmark, Estonia, Tonga	[redacted]	[redacted]	Lashkargah
51	Pasab (Wilson)	FOB	US Army	[redacted]	[redacted]	Panjwayi

Main bases and relevant information (1/2)

Opened	Closed	Field9	Notes	Shape *
2009	2014	<Null>	<Null>	Point
2008	2014	<Null>	Regional Command Southwest Headquarters	Point
2001	.	Open	ISAF Headquarters, ISAF Joint Command Headquarters, Headquarters for RC-Capital	Point
2001	.	Open	RC-S headquarters	Point
2004	2014	<Null>	<Null>	Point
2001	.	Open	Largest US base in Afghanistan, RC-East Headquarters	Point
2006	2014	<Null>	Main British base and formerly home to Task Force Helmand	Point
2006	2014	<Null>	<Null>	Point
2006	2014	<Null>	<Null>	Point
2006	2014	<Null>	NATO Training Mission – Afghanistan Headquarters	Point
2003	2013	<Null>	<Null>	Point
2001	.	Open	Major CIA and Special Operations counter-insurgency outpost	Point
2005	.	Open	<Null>	Point
2007	2009	<Null>	<Null>	Point
2001	2002	<Null>	First Marine land base in Afghanistan	Point
2006	2013	<Null>	<Null>	Point
2008	2013	<Null>	<Null>	Point
2002	2015	<Null>	<Null>	Point
2006	2012	Close unk, camp was open in 2012	<Null>	Point
2003	2005	<Null>	Reopened as a Counterinsurgency Academy in April 2007	Point
2007	.	Open	Reopened as a Counterinsurgency Academy in April 2008	Point
.	.	Open	Opening unknown	Point
2007	2014	Slated to close in 2014	<Null>	Point
2002	2014	Slated to close in 2014, Canada withdrew all troops at this time	<Null>	Point
2002	2013	<Null>	<Null>	Point
.	.	Open unk, close unk	<Null>	Point
2002	2011	<Null>	<Null>	Point
2006	2012	<Null>	<Null>	Point
2002	2013	Close unk, camp was open in 2013	<Null>	Point
2001	.	Close unk	<Null>	Point
2004	2012	Close unk, camp still open 2012	Located in/related to Bagram Airfield	Point
.	2012	Open unk, close unk, camp still open 2012	Located in/related to Bagram Airfield	Point
.	2012	Open unk, close unk, camp still open 2012	Located in/related to Bagram Airfield	Point
2003	2012	Close unk, camp still open 2012	Located in/related to Bagram Airfield	Point
2004	2012	Close unk, camp still open 2012	Located in/related to Bagram Airfield	Point
2002	2012	Close unk, camp still open 2012	Located in/related to Bagram Airfield	Point
.	.	Open	Located in/related to Bagram Airfield, opening date unknown	Point
.	2014	Open unk	<Null>	Point
.	2014	Open unk, between 2001 and 2004	<Null>	Point
2006	2015	<Null>	Located in/related to Kandahar Airfield	Point
2003	2013	<Null>	<Null>	Point
.	2013	Open unk, task force Uruzgan started 2006	<Null>	Point
2005	2013	<Null>	<Null>	Point
2007	2014	<Null>	<Null>	Point
2012	.	Open	<Null>	Point
Before/in 2008	2014	<Null>	<Null>	Point
Before/in 2006	2012	<Null>	<Null>	Point
.	.	Open unk, close unk	Located in/related to Kandahar Airfield	Point
2004	2014	<Null>	<Null>	Point
2005	.	Open	ISAF logistics hub	Point
.	2014	Open unk, slated to close in 2014	<Null>	Point

Main bases and relevant information (2/2)

This table shows the available data for about 50 bases that we deemed to be the most important foreign bases in Afghanistan over the last 15 years. We list the name, type, location (coordinate system CGS WGS 1984), militaries present (countries of origin), district in which the base is located, date opened and closed (a "." in the opened or closed section means there is either no data for closure time or that the base is still open. See Field 9 for explanatory notes in these cases), and

general notes of interest.

Confirming the location of these districts using satellite imagery

Example: “Base Blackhorse”

This is an example of the Wikimapia satellite imagery, which we used to locate bases. This is an image of Base Blackhorse, now closed. We were able to locate this as Base Blackhorse by first searching for the military base on wikimapia which offered two possible locations (approximately 9 miles away from each other) where the base could be. After we discovered in a news report that the base was located next to an Afghan National Army base, which was itself located on the site of the Pul-e-Charkhi-Prison, we were able to determine the definitive location of the prison and thus the location of the base.

Definitions and explanation of how each base was found. Below, we have laid out the definitions for what each type of base exists in Afghanistan and explained how we determined the specific locations for each base we included. The base definitions are important to know because the type of base is a good indicator of its size. Though this was not the only criteria we used to determine whether or not a specific base should be represented on the map, it was important for weeding out those that are not included (for example, we included no firebases on account of their temporary nature and generally small size). Below this, we provide more detail about specific bases whose locations we were not able to get from the GeoHack database, in which bases are supposed to have had multiple confirmations. These bases were found using satellite data and through available news reports, photos and satellite imagery. All definitions below are adapted or directly taken fromWikipedia to provide a rough idea about the types of military bases that exist in Afghanistan. We do not rely on the distinctions and simply code whether there is an open base or not.

Additional information about bases (from wikipedia):

- Definition FOB - A forward operating base (FOB) is any secured forward military position, commonly a military base, that is used to support tactical operations. A FOB may or may not contain an airfield, hospital, or other facilities. The base may be used for an extended period of time. FOBs are traditionally supported by Main Operating Bases that are required to provide backup support to them. A FOB also improves reaction time to local areas as opposed to having all troops on the main operating base.

- Definition MOB - A MOB is a term used by the United States military defined as a permanently manned, well protected base, used to support permanently deployed forces, and with robust sea and/or air access.

- Definition COP - A combat outpost is a detachment of troops stationed at a distance from the main force or formation, usually at a station in a remote or sparsely populated location, positioned to stand guard against unauthorized intrusions and surprise attacks; the station is occupied by troops, it is usually a small military base or settlement in an outlying frontier, limit, political boundary or in a foreign country.

- Definition Firebase - A temporary military encampment to provide artillery fire support to infantry operating in areas beyond the normal range of fire support from their own base camps.

- Definition Camp - A semi-permanent facility for the lodging of an army. Camps are erected when a military force travels away from a major installation or fort during training or operations, and often have the form of large campsites.
- Definition Base - A facility directly owned and operated by or for the military or one of its branches that shelters military equipment and personnel, and facilitates training and operations. In general, a military base provides accommodations for one or more units, but it may also be used as a command center, a training ground, or a proving ground. In most cases, a military base relies on some outside help in order to operate. However, certain complex bases are able to endure by themselves for long periods because they are able to provide food, water and other life support necessities for their inhabitants while under siege.

All locations are taken from Wikimedia's GeoHack program if available. We do not consider Firebases and COPs, which are smaller and often temporary outposts. In addition, we found or updated the information for the following cases:

1. COP/FOB Zangabad has been coded as FOB Pasab. This was the most likely location for a forward operating base close the Zhari/Panjwayi district border. Exact location determined as such using Wikimapia satelite imagery. It is coded as being in the district of Panjwayi.
2. Camp/FOB Hadrian location determined using Wikimapia satellite imagery.
3. Camp Russell location determined using Wikimapia satellite imagery in relation to Camp Holland.
4. Camp Arena, Camp Vianini, and Camp Stone are each in roughly the same area. Using Wikimapia imagery, we assume that Camp Arena, the only camp with an Italian Air Force presence, is located at the airfield in Hirat. Camp Vianini and Camp Stone were assigned their locations using Wikimapia imagery as well. We believe Camp Vianini to be at the location we chose based on the fact that an Italian artillery regiment was attacked at that location and we believe the Italian Army was the only major force at Camp Vianini. Camp Stone, which has multiple country forces at its location, is expected to be south of the airport and Camp Arena, according to Wikimapia data.
5. Camp Blackhorse determined using Wikimapia and various sources citing the camp to be adjacent to the Pul-e-Charkhi ANA compound.
6. Camp Clark determined using Wikimapia satellite imagery.

7. Camp Warehouse determined using Wikimapia satellite imagery.

8. Camp Phoenix location determined using google maps and Wikimapia satellite data.

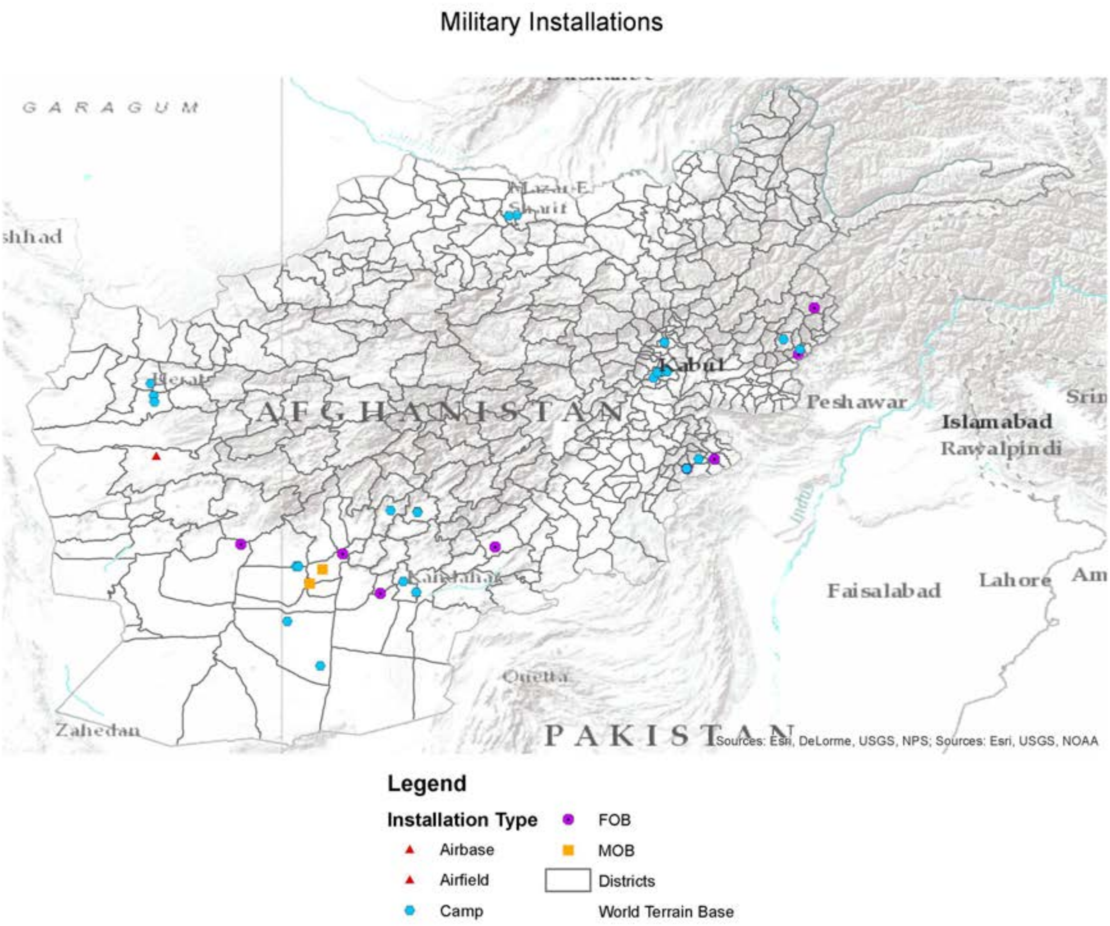
9. Camp Invicta located using Wikimapia satellite data.

10. FOB Hamidullah located using Wikimapia satellite data. In Wikimapia, the location is described as FOB Nolay, the previous name of the base.

11. Camp Blessing located using Wikimapia satellite data.

12. FOB Joyce located using satellite data and with news articles stating that FOB Joyce is within/very close to the village of Serkanay.

13. Camp Wright located using Wikimapia and Google Maps satellite data; it is listed as 'USA Army Base" on the Wikimapia site.



Final Map of Located Military Installations

This map shows the geographic location of the bases that we identified. Some bases are not visible in this view as a result of closely overlapping with other bases, in which case the map displays only one symbol.