Can Development Programs Counter Insurgencies?: Evidence from a Field Experiment in Afghanistan[1]

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We exploit a randomized controlled trial conducted between 2007 and 2011 to understand the effect of Afghanistan's largest development program on insurgency. We find that the program generally reduced insurgent violence, improved economic outcomes, and increased support for the government. However, in areas close to the Pakistani border, the program increased insurgent violence. This effect heterogeneity appears to be driven by the presence of foreign insurgents who are not reliant on the local population for support. The results suggest that while development programs can quell locally-based insurgencies, they may be counterproductive where insurgents are not embedded in the population.

JEL: D74, F35, I38, O15, O18, O19.

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I. Introduction

Over the past two decades, governments and allied forces in countries as diverse as Afghanistan, Iraq, and the Philippines have employed development programs as a tool to counter insurgencies.[2] The use of development programs in this manner reflects the influence among policy-makers and military officers of theories that link the provision of basic services and infrastructure to support for the government and, in turn, to the capability of insurgents to conduct asymmetric warfare. The 2006 U.S. Army/Marine Corps Counterinsurgency Field Manual, for instance, contends that winning the "hearts and minds" of the population by improving access to public goods and services is essential to the effectiveness of counterinsurgency operations (U.S. Army/Marine Corps 2006). This doctrine has informed both the funding of development initiatives by the U.S. military in Afghanistan and Iraq, as well as large outlays of assistance by the U.S. government and other allies to the governments of Afghanistan and Iraq to support the provision of public goods and services.

Despite the increasing use of development programs to counter insurgencies, existing empirical evidence on the effects of aid on violence is ambiguous. A study of U.S. military spending on small-scale infrastructure projects in Iraq indicates that such projects reduced insurgent violence (Berman, Shapiro and Felter 2011), although the effect is only apparent after an increase in troop strength in 2007. Mirroring this finding, Nielsen et al. (2011) conclude, based on a cross-country analysis, that reductions in development assistance increase the probability of the onset of armed conflict. Crost, Felter and Johnston (2016) provide evidence that a conditional cash transfer program in the Philippines leads to a reduction in violence. However, a number of other studies suggest that development programs can have an adverse effect on violence. Evidence from the Philippines, for instance, shows that a government-funded community-driven development program increased insurgent attacks during project preparation, but had no effect in the long run (Crost, Felter and Johnston 2014). Similarly, shipments of U.S. food aid increased the incidence and duration of civil conflicts in recipient countries (Nunn and Qian 2014). The diversity of findings suggests that the effect of development programs on violence may be conditioned by the context and the nature of the intervention (Berman et al. 2013).

In this paper, we identify the effect of development programs on insurgent violence by exploiting the randomized roll-out of Afghanistan's largest development program, the National Solidarity Program (NSP). NSP is a community-driven development program that is funded by foreign donors, executed by the national government, and implemented by local and international NGOs. The program provides block grants of up to $60,000 per village to fund local development projects. In the fall of 2007, 250 of 500 villages in 10 districts across northern, eastern, central, and western

[2] This may include foreign forces, such as the U.S.-led coalition during the 2003-09 Iraq War and the International Security Assistance Force during the 2001-14 Afghanistan War.

Afghanistan were randomly assigned to receive NSP, while the remaining 250 villages did not receive the program until late 2011. Data on insurgent violence is provided by the U.S. military's 'Significant Actions' (SIGACTS) database, which reports dates, locations, and types of incidents in Afghanistan between 2006 and 2014. Data on local economic outcomes, popular support for the government, and related outcomes is provided by three rounds of household surveys conducted across the sample of 500 villages in 2007, 2009, and 2011.

Development programs may reduce violence by impacting the extent to which the local population provides active support (i.e., direct participation in attacks) as well as tacit support (e.g., shelter, sustenance, and intelligence) to insurgent groups. Active support establishes the numerical strength of insurgent forces, while tacit support determines tactical efficacy – i.e., insurgents' ability to attack while limiting casualties and evading capture – and is of particular importance in theaters where insurgents are able to 'blend in' with the local population (Mack 1975). The level of active support is generally determined both by the opportunity costs of joining the insurgency (Grossman 1991; Fearon 2008),[3] as well as by the existence of economic, social, and political grievances (Gurr 1970; Cederman, Weidmann, and Gleditsch 2011). The level of tacit support, on the other hand, is generally determined just by grievances as the local population rarely derives any immediate economic benefits from providing tacit support to insurgents.[4] Development programs may thereby weaken an insurgency by creating opportunities that lessen the economic incentive to actively participate in the insurgency and by addressing perceptions of disenfranchisement that might otherwise encourage residents to provide either tacit or active support to insurgents.[5]

In addition to exacting support through providing economic benefits to populations and/or by exploiting perceptions of exclusion, insurgents may also exact tacit support through intimidation – that is, by punishing the local population either individually or collectively for failing to provide such support. The potential of insurgents to employ intimidation as a tactic complicates the potential effect of development programs on insurgent violence. Specifically, in theaters where intimidation of the local population represents a viable tactic for insurgents and where the numerical strength of insurgent forces remains sufficiently high, insurgents may attempt to counteract any loss in tacit support caused by the implementation of development programs by increasing intimidation. This

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[3] Studies of the insurgency in Iraq have found a negative relationship between unemployment and violence (Berman, et al. 2011; Berman, Shapiro, and Felter 2011; Iyengar, Monten, and Hanson 2011), indicating the importance of economic outcomes in fueling active support during an insurgency. Vanden Eynde (forthcoming) also provides evidence on the effect of economic conditions and violence in the context of the Naxalite conflict in India.

[4] The level of tacit support provided by a population to insurgents may nonetheless affect the probability of receiving government-provided local public goods (Berman, Shapiro, and Felter 2011).

[5] The effectiveness of this approach is, however, conditional on programs delivering perceived benefits and the population assigning credit to the government for delivering those benefits. As noted by Kalyvas (2006), "[t]he main policy implication of this ["hearts and minds"] view is that incumbents need to persuade hostile populations to switch their sympathies through programs of political liberalization, economic development, and civic action" (p. 94).

increase in intimidation may potentially increase violence over what it would have been otherwise, rendering the overall direction of the effect of development programs on insurgent violence ambiguous.

The results of our study indicate that, on average, government-sponsored development projects reduce the number of incidents around villages. The magnitude of this effect is more pronounced in the long run. Development projects also positively impact economic wellbeing, improve attitudes towards the government and allied entities, and has a positive effect on how both men and women perceive the local security situation. The pattern of results appears to provide validation to the "hearts and minds" doctrine articulated in the 2006 U.S. Army/Marine Corps Counterinsurgency Field Manual.

However, there is substantial heterogeneity in treatment effects across the sample. In northern, central, and western Afghanistan, NSP produced a statistically significant and durable reduction in the frequency of violent incidents, while also improving economic outcomes and popular support for the government. In eastern Afghanistan, however, NSP had no effect on violent incidents. While the program improved economic outcomes in eastern Afghanistan, as in the rest of the country, it did not improve perceptions of the government. The heterogeneity in effects between eastern Afghanistan and the rest of the country is principally accounted for by proximity to the Pakistani border. Within the sub-sample of villages in eastern Afghanistan, the effect is conditioned by distance to the Pakistani border, with NSP increasing the number of incidents in the immediate vicinity of the border and decreasing incidents at a sufficient distance from the border. Other differences between districts that border Pakistan and the rest of Afghanistan – such as ethnic composition, initial levels of violence, and opium production – do not condition the effect of NSP on violence.

One of the most salient features of the Afghan districts bordering Pakistan that can explain the heterogeneity in results was that the insurgency in these districts has, since 2007, been increasingly dominated by insurgents either trained in Pakistan or otherwise of foreign extraction, whereas insurgents fighting in northern, central, and western Afghanistan have originated primarily from local communities. Thus, the approach of using development programs to "win hearts and minds" seems to be working in areas where the strength of the insurgency depends on the support of the local population. The approach appears to fail, however, in areas where the insurgency is dominated by external fighters who do not originate from the local communities, are supplied from across the border, and thereby are able to rely on coercion to secure tacit support from the local population. In these areas, the infusion of government-funded projects may not lead to a reduction in violence and could even aggravate conflict by both providing insurgents with ready targets and by presenting a threat to their popular support.

This interpretation of our findings is consistent with Toft and Zhukov (2015), who show that

counterinsurgency tactics in Chechnya decreased violence by nationalist groups that rely on local support, but was ineffective in suppressing violence by Islamist rebels who relied more on external revenue and manpower. The findings for eastern Afghanistan, specifically, are consistent with Crost, Felter and Johnston (2014), who find that insurgents in the Philippines sought to sabotage government-funded projects. However, our results indicate that these adverse effects may not be limited to the period of mobilization, but can persist for years after the start of the program. More generally, the findings align with models of civil conflict that treat insurgencies as the product of interactions among individual actors that respond to incentives (Grossman 1991; Fearon 2008; Gurr 1970; Cederman, Weidmann, and Gleditsch 2011; Bueno de Mesquita 2013).

This paper makes several substantive and methodological contributions to the literature on civil conflict and on the effects of development programs. First, it is the only paper that we are aware of which employs a field experiment to identify the effect of a community-level development program on insurgent violence. Second, it not only draws upon events data on conflict incidents but also uses panel surveys to exact data on perceptions of security, economic outcomes and political attitudes and thereby explores mechanisms behind the effect of development programs on violence. Third, it examines the dynamic effects of development programs on violence by assessing how effects change over the course of four years. Fourth, the paper demonstrates that the effects of public goods provision on insurgent violence strongly depend on the local context and can vary even within a single country. Fifth, the paper implements an analog of 'out-of-sample prediction' by replicating an earlier working paper (Beath, Christia, and Enikolopov, 2012) using an expanded data set that features both additional time periods and additional data from the original period. This replication ameliorates concerns of publication bias due to the lack of a fully-fledged pre-analysis plan, as well as more general concerns relating to the use of pre-analysis plans (Olken, 2015).

The findings also contribute to the literature on the general effectiveness of community driven development programs (Mansuri and Rao 2012, Wong 2012; King and Samii 2014).[6] Evidence on the effects of community-driven development programs in conflict and post-conflict settings is mixed. In Liberia, such programs have been found to increase social cohesion but fail to improve economic wellbeing (Fearon, Humphreys and Weinstein 2009, 2015), yet the opposite has been observed in Sierra Leone (Casey, Glennerster, and Miguel 2012). A study in the Democratic Republic of Congo found few significant effects (Humphreys et al. 2015), while a study in the Philippines observed negative impacts on security (Crost, Felter and Johnston 2014). Our results indicate that a community-driven program, in Afghanistan increased local economic activity and, in

[6] More generally it relates to the literature that examines how political support depends on the provision of material benefits to the population, e.g. in the form of pork barrel spending (see among others. Ferejohn 1974, Shepsle and Weingast 1981, Levitt and Snyder, 1995).

most areas, improved perceptions of government and security.[7]

The paper is divided into seven sections: Section II provides background on the Taliban insurgency and the National Solidarity Program; Section III describes the experiment; Section IV introduces the data sources; Section V presents the methodology and results; Section VI discusses the results, both on security outcomes and on the underlying mechanisms of economic wellbeing and attitudes towards the government driving those outcomes; and Section VII concludes.

II. Background

The insurgency that began after the fall of the Taliban regime in 2001 is composed of a mix of both local and foreign fighters, but has generally deployed guerilla tactics enabled by the support from local communities (subsection II.A). As the largest development program in Afghanistan, the National Solidarity Programme has potentially served a counter-insurgency function in building government legitimacy (subsection II.B).

A. Structure of the Insurgency

Afghanistan's post-2001 insurgency has been centered on the neo-Taliban, which was formed by members of the original Taliban who dispersed across Afghanistan and into Pakistan following the defeat of the regime in 2001 (Giustozzi 2012). The neo-Taliban has been composed of three distinct groups of fighters: a core-group of highly motivated, full-time Afghan insurgents; a wider group of young Afghan men alienated by government corruption, angry at the presence of foreign military forces, or attracted by financial benefits of the insurgency; and a small group of foreign insurgents who serve as mid-level commanders, trainers, and financiers (Rohde 2007).

The insurgency has also incorporated a number of smaller armed groups that operated independently of the neo-Taliban. The most prominent of these has been the extremist Haqqani Network,[8] which established a sanctuary in the Kurram Agency of Pakistan's Federally Administered Tribal Areas, has been active in the eastern border provinces, and included many Pakistani and Uzbek operatives (Dressler 2012). The Pakistani Taliban (Tehrik-i-Taliban Pakistan) also established bases in Afghanistan's eastern provinces, which were used to launch cross-border attacks on Pakistan (Khan 2012). In addition to these larger groups, the insurgency has also featured hundreds

[7] As such, the effects of the intervention potentially extended beyond the provision of basic services in so far as civil conflict results not only in loss of life and physical destruction, but also reduces investment in physical (Collier, 1999; Blattman and Miguel 2010) and human (Leon, 2012) capital.

[8] According to Dressler (2012) "[t]he Haqqanis have long disregarded Mullah Omar's public orders to avoid civilian casualties, mounting spectacular suicide attacks and assassinations of key security and political figures in Kabul and elsewhere" (p. 11) and "emplace sophisticated improvised explosive devices (IEDs) to restrict U.S. forces' ability to interfere with Haqqani operations in southeastern Afghanistan" (p. 15).

of local militias that regularly switched allegiances between the neo-Taliban and the government (Foschini 2014).[9]

The insurgency – and particularly operations by the Haqqani Network and the Tehrik-i-Taliban Pakistan – has been facilitated by the porous border between Afghanistan and the tribal areas of Pakistan, which has enabled the insertion of materiel and foreign-based fighters into Afghanistan (Johnson and Mason 2007; Jones 2008; and Giustozzi and Ibrahimi 2012) and has served as a sanctuary (Jones 2008, Bumiller 2010). The "Parrot's Beak" at the intersection of Pakistan's Kurram Agency and the Afghanistan provinces of Nangarhar, Logar, and Paktia has witnessed particularly high levels of cross-border flows and insurgent activity (Dorronsoro 2009).[10]

While the Haqqani Network and the Tehrik-i-Taliban Pakistan have been more geographically concentrated and cohesive, the neo-Taliban has generally functioned as a decentralized network and large-scale operations or even well-planned tactical operations have been rare (Giustozzi 2012). Commanders exercised autonomy, but drew on a limited template of unsophisticated modes of attack. Initially, tactics drew heavily on those developed during the anti-Soviet jihad, such as ambushing patrols or attacking International Security Assistance Force (ISAF) outposts with RPGs and small arms fire. After 2007, however, improvised explosive devices (IEDs) ordinarily followed by RPGs and small arms fire were increasingly used to attack patrols and convoys (O'Hanlon 2010).

The ability of the neo-Taliban to undertake such attacks has often been dependent on support provided to insurgents by local communities. In a 2011 report on the state of the insurgency, the U.S. Department of Defense, for instance, noted that, "[t]he majority of insurgent fighters and commanders operate in or near their home districts, and low-level insurgent fighters are often well-integrated into the local population" (U.S. Department of Defense 2011, p. 59). To ensure support, the neo-Taliban leadership sought to limit civilian casualties and allowed commanders to moderate the movement's strict edicts (Giustozzi 2012).

While local insurgents predominated across Afghanistan, non-local fighters – whether foreign fighters or insurgents from other parts of the country – concentrated in certain strategic areas. Due to its adjacency to the supply route linking Afghanistan with the Kurram Agency in Pakistan, non-local fighters became particularly numerous in the eastern province of Nangarhar (Dressler 2012; Foschini 2014; Mansfield 2011). Non-local fighters provided technical expertise, but also tended to

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[9] Another group, the Hizb-i-Islami Gulbuddin, led by the former Prime Minister of Afghanistan, Gulbuddin Hekmatyar, predominated in the northeastern provinces.

[10] Dressler (2012) notes that the Haqqani Network is active in "the districts of Hisarak, Sherzad, Chaparhar, and Jalalabad", which enables "the network to project force into the provincial capital of Jalalabad, transit east to Kabul, or smuggle men and materials into northern Laghman and Kapisa provinces" (p. 30). Dressler (2012) further notes that Nangarhar has been used particularly by the Haqqani Network to traffic Uzbek fighters representing the Islamic Movement of Uzbekistan "through eastern Afghanistan to the country's northern provinces" (p. 30).

be "more ideological in nature and less tolerant of local norms" (U.S. Department of Defense 2011, p. 59). The differences in approach between local and non-local insurgents were particularly apparent in the treatment of development projects. While local insurgents generally tolerated such projects on account of their connections with local communities – even allowing girls' schools to operate (Foschini 2011) – non-local insurgents instead regularly threatened to kill anyone who cooperated with the Afghan government or foreign aid groups (Rohde 2007).

B. The National Solidarity Program

In 2003, less than two years after the fall of the Taliban, the Afghan government inaugurated NSP as a means to deliver services and infrastructure to the country's rural population and to build representative institutions for village governance.[11] NSP has now been implemented in over 32,000 villages across all of Afghanistan's 34 provinces at a cost of over $1 billion, making it the largest development program in the country. NSP is managed by the Government of Afghanistan, but is funded by the World Bank and a consortium of bilateral donors and is implemented by NGOs contracted by district.

NSP employs the community-driven modality of project delivery and implements two major interventions in participating villages: (i) creates a gender-balanced Community Development Council (CDC) through a secret ballot election; and (ii) disburses block grants to finance local development projects selected by the CDC in consultation with the village community.[12] As NSP implementation combines both of these interventions, we cannot isolate the effect of elected institutions from that of monetary resources. Block grants are valued at $200 per household up to a village maximum of $60,000 and average of around $33,000 in the villages in our sample.[13] The four most popular categories of projects financed by NSP are irrigation (25 percent of projects in our sample); water supply (21 percent); road infrastructure (19 percent of projects); and electricity (10 percent). During implementation, a signboard is usually affixed to the project to indicate that it is funded by the central government through NSP (Figure A.3 in the Online Appendix). From a

[11] The current President of Afghanistan, Ashraf Ghani, wrote that "[t]he intent of [NSP] was to address the process of democratization from the ground level up, in parallel to the process of constitution making and rule writing at the center" (Ghani and Lockhart 2009), p. 206 – 208.

[12] The CDC is responsible for compiling the list of candidate projects; preparing budgets for the proposed projects for the approval of the central NSP office in Kabul; and selecting the relevant contractors and designating the laborers that will work on each project. Randomized variation was induced in the method by which community development councils were elected (Beath et al. 2016) and in how development projects were selected (Beath, Christia, and Enikolopov 2017). There is no significant difference in security incidents along either of these dimensions.

[13] Villages must contribute at least 10 percent of project costs on top of the NSP grant, which they largely do in the form of labor.

random sample of villagers interviewed during program implementation, 70 percent identified the central government as the project sponsor.[14]

NSP takes approximately three years to complete all the projects in participating villages. The process of facilitating CDC elections usually takes about six months, after which an average of twelve months elapse before project implementation starts. During this period, CDCs and villagers design projects, submit proposals, receive funds, and, if necessary, procure contractors. Once started, project construction lasts an average of nine months (Beath, Christia, and Enikolopov 2013b). The average number of implemented projects in a village is three and projects may be implemented at the same time. Figure A.4 in the Online Appendix presents a detailed timeline of NSP implementation. The implementation of the program in the control villages began in October 2011.

In the aftermath of the fall of the Taliban regime, NSP was the primary vehicle for projecting the authority of the new administration to the countryside and, as such, served an implicit state-building function in establishing the government as a benevolent provider of public goods and services. As the insurgency gained strength after 2006, military and political advisors and media commentators expressed interest in NSP as a means by which to stem the expansion of neo-Taliban control across rural Afghanistan. American counterinsurgency experts, for instance, urged increased funding for the program, arguing that it would improve security "by building an Afghan state through Afghan means" (Nagl, Exum and Humayun 2009). A Washington Monthly article in 2007 similarly trumpeted NSP-funded projects as "the schools the Taliban won't torch" and, as such, a means of projecting state authority and development projects into the countryside without invoking the ire of insurgents.

III. Description of the Experiment

To assess the effects of NSP on the insurgency, the study exploits the randomization of the program over 500 villages in ten districts across Afghanistan (subsection III.A). Although these villages were selected purposively, their characteristics are broadly representative of characteristics of villages across Afghanistan (subsection III.B). Of these 500 villages, half were assigned to receive NSP using a matched-pair cluster randomization procedure, with which implementing partners overwhelmingly complied (subsection III.C).

[14] Another 22 percent of respondents indicated that they were provided by NGOs and about 2 percent of villagers indicated that they were provided either by sub-national government bodies, local leaders, foreigners or villagers themselves (Beath et al. 2010).

A. Sample Selection

Between 2007 and 2011, NSP implementation was randomized across 500 villages spanning 10 rural districts across Afghanistan.[15] The 10 districts were selected based on three criteria: (i) absence of previous activity by NSP (which reduced the number of eligible districts from 398 to 74); (ii) conduciveness of security conditions to household survey administration (which further reduced the number of eligible districts to 34); and (iii) minimum of 65 villages (which further reduced the number of eligible districts to ten).[16] The first criterion excluded villages closer to big cities and provincial centers, which were more likely to receive NSP before 2007. The second criterion, excluded provinces in southern Afghanistan characterized by extremely high levels of violence at baseline, but did not exclude regions with moderately high levels of violence.

In each district, 50 sample villages were selected by the assigned NGO, with the understanding that a randomization algorithm would be used to select 25 of these villages for NSP mobilization. In addition, in each district, NGOs chose another 15 communities of particular humanitarian and/or political importance to receive NSP and not be included in the experiment. These villages were usually more easily accessible from the district center, rendering the villages in the sample more remote.

B. Sample Characteristics

Although not a random sample of districts in Afghanistan, the ten sample districts represent the country's geographic, ethnic, and economic diversity and cover the western, central, northern, and eastern regions of the country (see Figure 1).[17] The ten sample districts include five districts in which the Tajik ethnicity predominates, four districts in which the Pashtun ethnicity predominates, and one district in which the Hazara ethnicity predominates, as well as two districts with Uzbek and Turkmen communities. NGOs assigned to implement NSP in the ten sample districts represented a

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[15] Other works based on the randomization of NSP have examined its effect on women's rights (Beath, Christia, Enikolopov 2013a) and governance (Beath, Christia, Enikolopov 2015). Results of the analysis of the effect of the program on a range of economic, institutional, and social outcomes in the form of a report are available in Beath, Christia and Enikolopov (2013b). Results on the program's effects on security are original to this paper.

[16] Ultimately, the number of districts satisfying the three criteria was eleven, but one of the districts was excluded due to contractual complications between NSP and the NGO assigned to the district. Part A of the Online Appendix provides more details on district selection.

[17] The ten selected districts encompassed four districts in Herat province (Adraskan; Chisht-e Sharif; Fersi; and Gulran) in the western region; one district in Ghor province (Daulina) and one district in Daykundi province (Sang Takht) in the central region; one district in Balkh province (Balkh) and one district in Baghlan province (Khost Wa Firing) in the northern region; and two districts in Nangarhar province (Hisarak and Sherzad) in the eastern region.

mix of international and local NGOs that reflected the diversity of NGOs contracted to implement NSP across Afghanistan.[18]

There also exists substantial variation in the ten sample districts in the initial level of violence and opium production (Table 1). The two districts in the eastern region – Hisarak and Sherzad districts in Nangarhar province – provide diversity in geographic characteristics relevant to the insurgency given that they border Pakistan, with one of the districts – Hisarak – lying adjacent to the neo-Taliban supply route that connects the Kurram Agency to Logar, Nangarhar, and Paktia provinces.[19] Between January 2006 and December 2014, security conditions in the ten districts were generally representative of conditions in districts across Afghanistan, with the exception of districts in the southern provinces, which experienced much higher levels of violence (Figure 2).

To assess how the study sample compares to the population of rural Afghanistan, we compare the baseline characteristics of the sample households with the characteristics of households surveyed by the nationally-representative 2007–08 National Risk and Vulnerability Assessment. Between the two samples, there are no significant differences in the age of respondents or in their income levels (Table A.1 in the Online Appendix). Respondents in sample villages were more likely to be engaged in agriculture; have worse access to medical services; and enjoy better access to electricity, although the magnitude of these differences is small. Overall, there is a high degree of overlap in the distribution of key variables between the sample and the overall population of rural Afghanistan.

C. Assignment of Treatment and Compliance

Of the 50 sample villages selected in each district by implementing NGOs, 25 were assigned to the treatment group using a matched-pair randomization procedure in the following four stages:

1. *Village Clusters***.** To minimize potential for spillovers between treated and untreated units, villages located within 1 kilometer were grouped in village clusters. Of the 500 sample villages, 107 were assigned to 41 village clusters. The number of villages in each village cluster ranged from two to six.
2. *Matched Pairs***.** In each district, the 50 sample villages were paired into 25 groups of two using an optimal greedy matching algorithm which matched villages to ensure similarity based on background characteristics provided that the villages were not in the same village cluster (King et

[18] NGOs facilitating the implementation of NSP in the two Nangarhar districts also facilitated NSP in other districts in the sample. The implementation of NSP in Sherzad district was facilitated by the international NGO, the Internal Rescue Committee (IRC), which also facilitated NSP in Gulran district in Herat province. The implementation of NSP in Hisarak district was facilitated by the international NGO, People-in-Need, which also facilitated NSP in Balkh district in Balkh province and Khost Wa Firing in Baghlan province.

[19] Technically, the only district in the sample that borders Pakistan is Sherzad, but Hisarak, is less than 10 km away from the border across Sherzad, which makes it also easily accessible from Pakistan.

al. 2007). The matching used pre-baseline data provided by Afghanistan's Central Statistics Organization (village population) and geographic characteristics derived from village coordinates (distance to river, distance to major road, altitude, and topography).

3. *Assignment of Treatment***.** In each matched pair, one village was randomly assigned to receive NSP, such that the clusters of villages were assigned the same treatment status.[20]
4. *Violations of Clustering Restrictions***.** In a few districts, the large number of clustered villages precluded the co-assignment of all the villages in a cluster to the same treatment status. For cases in which assignment of treatment status was not possible without a violation of the clustering restriction, the number of violations was minimized through a simulation approach.[21]

The randomization procedure achieved statistical balance between treatment and control groups. Table 2 presents means, normalized differences,[22] and *t*-statistics for several variables at baseline. Among the variables listed, mean differences never exceed 13 percent of the standard deviation.[23]

Villages assigned to the treatment group received NSP following the administration of a baseline survey in September 2007. The remaining 250 villages assigned to the control group did not receive NSP until after the endline survey, which concluded in September 2011. Available information indicates that compliance with treatment assignments was very high, with just five control villages receiving NSP and eight treatment villages not receiving NSP.[24] Most of these cases appear to have arisen due to inadvertent language errors in village names. The analysis employs the initial treatment assignment and thus follows the intention-to-treat approach.

IV. Data

Data for the study comes from two sets of sources: (i) data on the date, location, and type of security incidents across Afghanistan between 2006 and 2014 is provided by ISAF (subsection IV.A); (ii) data from three rounds of household level data collected across the 500 sample villages immediately before, during, and following NSP implementation provides information necessary to explore the mechanisms behind overall effects (subsection IV.B).

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[20] The assignment was performed after we conducted the baseline survey, but before we processed the data. As such, baseline survey results could not be affected by the assignment, but were also not available to be used in the matching procedure.

[21] We generated 1,000 random assignments for each district and chose the one with the minimum number of cluster restriction violations. In the resulting assignment the clustering restriction was violated in 17 village clusters (covering 44 villages).

[22] Following Imbens and Wooldridge (2009), normalized differences are differences divided by pooled standard errors.

[23] Figure A5 in the Online Appendix summarizes normalized differences between treatment and control villages at baseline. Figures A6 and A7 show that there is no difference in security incidents between treatment and control villages at baseline. We also check that our results are robust to controlling for baseline values of the outcome variables (or variables most closely related to the outcome variables).

[24] We used a variety of checks to verify compliance with treatment assignments, including administrative records of NSP, reports from monitors, and analysis of midline and endline data.

A. Security Incidents

Data Source

The study uses a subset of the U.S. Department of Defense's Combined Information Data Network Exchange (CIDNE) database, which includes information on all 'Significant Activity' (SIGACT) field reports by ISAF units operating across Afghanistan. U.S. and partner military forces routinely logged both the geo-referenced location, date, time, and type of insurgent activities as they operated.[25] The dataset was recently declassified and was made available by Shaver and Wright (2016). The CIDNE database is the most reliable source of information on security incidents in Afghanistan as it is compiled primarily to inform risks toward security personnel and is not subject to the general biases that afflict data based on media reports (Weidmann 2016).[26] The dataset contains information on 246,334 security incidents that occurred in Afghanistan between January 2006 and December 2014.

Our analysis focuses on 908 incidents that occurred within a 15 km radius from the centers of sample villages from January 2006 until September 2011, when the mobilization of villages in the control group started. Of these incidents, 131 occur before the start of implementation of NSP in October 2007 and are used to measure the baseline level of violence. The remaining 777 incidents are used to assess the impact of NSP. In this subsample, exchange of fire account for 45 percent of incidents; improvised explosive devices (IED) account for 43 percent (of which 24 percent were explosions and 19 percent were IEDs that were found and cleared); while the remaining 12 percent of incidents include assassinations, mine strikes, surface-to-air fire, sabotage, and unexploded ordinance.

The data used in the earlier working paper (Beath, Christia, and Enikolopov 2012) was also based on the CIDNE database, but was significantly smaller both in terms of time period covered and types of incidents included in the dataset. In particular, it included only incidents that occurred up until March 2010 and only incidents related to IEDs and mine strikes (which, as was mentioned above, constitute about 35 percent of all security incidents). As a result of adding new types of incidents, the number of incidents in our sample between October 2007 and March 2010 increased from 133 to 314. Extending the time period from March 2010 to September 2011 increases the number of incidents by another 463 cases. The updated CIDNE database thereby allows us apply the

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[25] Similar data have been used in academic research on conflict in Afghanistan (Lyall, Blair, and Imai 2013; Blair, Lyall, and Imai 2014; Hirose, Imai, and Lyall 2014). Scholars have drawn from analogous datasets to assess the effects of aid on security in Iraq (Shaver and Tenorio, 2015; Berman, Shapiro, and Felter 2011; Iyengar, Monten, and Hanson 2011) and the Philippines (Crost, Felter and Johnston 2014).

[26] As the database is for military use and compiled based on reports by troops that have no active role in NSP planning or implementation, and are not necessarily aware of program activities, it is highly unlikely that NSP could bias incident reporting.

specifications in Beath, Christia, and Enikolopov (2012) to a set of data covering roughly six times the number of incidents.

A potential drawback of the CIDNE data is that it is based on reports from ISAF troops, who may have incentives to manipulate information. For example, military forces might underreport incidents near NSP villages to provide a better image for the program. Although we cannot eliminate this possibility, the extent of such misreporting is likely to be limited. First, the data is based on (at the time) classified field reports intended for internal use by ISAF forces. The database was collected primarily to inform risks toward security personnel on the ground and, as such, there was an inherent incentive for troops to report incidents accurately. The aggregate data was declassified only several years later. In addition, we are not aware of any public claims that link NSP activity with reported security incidents against ISAF personnel, so there is no evidence that this data was ever used to promote the program's public image. Second, NSP is a purely civilian program facilitated by NGOs. ISAF forces played no role in NSP activities and were not informed of implementation activities, which makes it unlikely that reporting of incidents was correlated with NSP implementation.[27]

Constructed Indicators

Measures of violence are constructed for three periods: (i) January 2006 - September 2007, which predates the implementation of NSP and establishes the existence of pre-treatment balance between treatment and control villages; (ii) October 2007 - September 2009, which coincides with the election of CDCs and commencement of project implementation; (iii) October 2009 - September 2011, which coincides with project completion. In the results, we do not examine the effect of the program after September 2011 since it coincides with the start of implementation of NSP in the control villages.

In the benchmark specifications, we follow the approach in Beath, Christia, and Enikolopov (2012) and focus on the extensive margin of violence by measuring whether there is at least one security incident within a specific radius from the center of a sample village in each period. We test the robustness of the results by looking at the intensive margin through measuring the number of security incidents within the same radii from village centers. In the benchmark analysis, we consider all types of security incidents together, while additional analyses consider different types of incidents separately.

As we do not have any priors on the radius at which NSP will affect security incidents, we employ summary indices that combine information for fifteen different radii ranging from 1 km to 15 km.

[27] Even if ISAF presence was higher around NSP villages that might actually increase the likelihood of incidents getting reported by ISAF units, as these incidents would be more likely to be observed.

In particular, we construct fifteen variables for the occurrence of security incidents within a specific radius and then aggregate them in a single summary index using three alternative methods of aggregation. In the benchmark specification, we use a summary index similar to that of Kling, Liebman and Katz (2007), which is defined to be the equally weighted average of z-scores of the individual indicators.[28] We also check the robustness of the results to indices constructed as the first principal component of the individual indicators (with factor loadings computed based on the data from the control group only) and the covariance-weighted sum of the individual indicators (Anderson 2008). Summary indices were not used in Beath, Christia, and Enikolopov (2012), which relied only on the analysis of 15 separate indicators.

B. Survey-Based Measures

Data Source

Survey-based measures come from a panel of baseline, midline, and endline surveys:

(i) Data from the baseline survey was collected between August and September 2007, which predated the implementation of NSP in the 250 treatment villages.[29] The baseline survey was administered to 13,899 male villagers and male and female village leaders across the 500 sample villages.

(ii) Data from the midline survey was collected between May and October 2009 and followed CDC elections and project selection, but predated the completion of 84 percent of projects implemented in the treatment villages. The midline survey was administered to 14,889 male and female villagers and village leaders across 474 of the sample villages.

(iii) The endline survey was conducted between May and October 2011 and followed the completion of 99 percent of projects implemented in treatment villages, but predated NSP implementation in control villages. The survey covered 13,811 male and female villagers and village leaders across 441 of the sample villages.

Constructed Indicators

The midline and endline surveys are used to construct the following groups of measures of security perceptions, economic wellbeing, and attitudes toward the government and allied actors:

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[28] The z-scores are calculated by subtracting the control group mean and dividing by the control group standard deviation. Thus, each component of the index has a mean equal to 0 and a standard deviation equal to 1 for the control group.

[29] The survey consisted of four instruments: (i) a male household questionnaire administered to ten randomly-selected male heads of household in each village; (ii) a male focus group questionnaire administered to a group of village leaders in each village; (iii) a female focus group questionnaire administered to the wives or other relatives of the village leaders; and (iv) a female individual questionnaire administered individually to the participants of the female focus group. Further information on the survey instruments is provided in Section B of the Online Appendix. Table A.2 in the Online Appendix lists the number of respondents for each instrument and survey.

(i) Perceptions of the security situation in and around the sample villages are given by three sets of indicators: (a) security perceptions of male respondents; (b) security perceptions of female respondents; (c) security incidents in and around villages reported by respondents. Summary statistics for these indicators are reported in Panel B of Table 3.

(ii) Economic welfare in the sample villages is assessed by four sets of indicators: (i) individual economic outcomes; (ii) provision of public goods; (iii) perceptions of economic welfare; and (iv) rate of net migration. Summary statistics for these indicators are reported in Panel C of Table 3.

(iii) Attitudes towards the government and allied forces are assessed by a single set of indicators. Summary statistics for these indicators are reported in Panel D of Table 3.

To address the issue of multiple hypotheses testing, we construct summary indices that combine multiple measures of the same concept using the three methods discussed above. We report the results for the individual indicators in the Online Appendix.

Correlation between Security Incidents and Security Perceptions

Self-reported measures of security from the surveys are likely to be generally less reliable than the CIDNE data given that survey bias (Bertrand and Mullainathan 2001) is likely to be affect security-related questions in areas experiencing an insurgency. However, self-reported measures may capture intimidation and other insurgent activities that are generally not captured in the CIDNE dataset. The correlation between the CIDNE data and the three survey-based measures of security is statistically significant for men, but not for the security perceptions of women or for self-reported security incidents (Table A.6 in the Online Appendix). On the assumption that the CIDNE data on security incidents is less likely to be affected by survey bias, the security perceptions of male villagers appear to represent the most reliable measure of the underlying security situation. The finding that perceptions of female are not as informative is not entirely surprising, since the travel of women beyond their home compound is generally limited across most parts of Afghanistan given prevailing cultural norms (Beath, Christia and Enikolopov 2013a). The lack of correlation between the number of self-reported security incidents and the number of incidents reported in the CIDNE data seems to be driven by noticeable underreporting of security incidents in the survey data, as evident from the comparison of the respective numbers in Panels A and C in Table 3.

Attrition

Although we have a balanced panel of treatment and control villages for measures of security incidents, not all of the villages and households from the baseline survey were included in the midline and endline surveys and, as such, the survey-based measures are subject to two forms of attrition.

Due to the deterioration in security conditions, 26 villages (11 treatment and 15 control villages) could not be surveyed during the midline survey and 59 villages (33 treatment and 26 control villages) could not be surveyed at endline. Although there are no statistically significant differences in attrition between treatment and control groups, the attrition cannot be considered random as it was caused by insecurity. The potential effect of village-level attrition on the estimates for survey-based measures is discussed further in Subsection V.F.

While midline and endline enumerators were instructed to locate previously interviewed respondents (or, failing that, members of the same household), only 65 percent of midline and 59 percent of endline male villager respondents were among baseline respondents. [30] There is no statistically significant difference in individual-level attrition between control and treatment villages at either point. As with village level attrition, we discuss in greater detail the potential effect of individual-level attrition on the estimated effects for survey-based measures in Subsection V.F.

V. Specification and Results

To identify the effect of NSP on insurgent violence, we employ a benchmark OLS specification and use a variation of this specification to test for effect heterogeneity (subsection V.A). We find that NSP reduces the probability of security incidents, although effects are conditioned by geography (subsection V.B). We also that NSP similarly affects perceptions of security by male villagers (subsection V.C). To examine whether these effects are – per counterinsurgency doctrine – driven by the delivery of public goods and services and subsequent shifts in popular sentiment, the study examines effects on economic and attitudinal outcomes. We find that NSP positively affects economic well-being of households (subsection V.D) and improves villagers' attitudes towards government representatives and allied actors (subsection V.E). The results are robust to alternate specifications and do not appear to be driven by attrition in the survey data (subsection V.F).

A. Specifications

Benchmark Specification

To ensure that the estimation represent an out-of-sample test of Beath, Christia, and Enikolopov (2012), the above benchmark specification is the same as for the earlier study but is extended to cover a longer time period and incorporates additional robustness tests. Specifically, the benchmark specification for estimating the effect of the program on outcomes of interest is the following OLS model:

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[30] Table A.3 in the Online Appendix provides detailed information on individual-level attrition for different waves of the survey. The predominant reason for enumerators not being able to interview baseline respondents was that the person was away from home on the day that the survey team visited the village, as it was the time of harvest. Differences between treatment and control groups in individual-level attrition are not statistically significant.

$$Y_{tv} = \alpha + \sum_{s=1}^{s=T} \beta_{\tau} \cdot (T_v \times \tau_s) + \varphi_{pt} + \varepsilon_{tvi} \quad (1)$$

where Y_{tv} is the outcome of interest for village *v* at period *t*; T_v is the village treatment dummy; τ_t is the dummy for time period; and φ_{pt} are matched pair-survey fixed effects that account for the assignment of treatment using pair-wise matching (Bruhn and McKenzie 2009). Estimations cover two periods: midline (t = 1) or endline (t = 2). For indicators constructed at the household level, the relevant outcome is Y_{tvi}, where *i* indexes household. For measures of security incidents, we also report results controlling for the baseline level of violence. In the benchmark specification, we report standard errors that are clustered at the village cluster level in order to account for correlation between residuals within clusters of villages due to the non-independence of treatment assignment. We check for the robustness of these results using standard errors that account for spatial and serial correlation (Conley 1999; Hsiang 2010), as well as randomization inferences (Young 2016).

This specification follows the pre-analysis plan for the general assessment of NSP registered at the EGAP web portal in 2012.[31] The pre-analysis plan, however, was limited to survey-based measures and did not include the heterogeneity analysis described below.

Effect Heterogeneity

As discussed in Subsection III.B, sample districts differ along a number of dimensions. To estimate whether the effect of NSP varies based on pre-existing characteristics, we use the following specification:

$$Y_{tv} = \alpha + \sum_{s=1}^{s=T} \beta_{\tau} \cdot (T_v \times \tau_s) + \sum_{s=1}^{s=T} \gamma_{\tau} \cdot (X_d \times T_v \times \tau_s) + \varphi_{pt} + \varepsilon_{tvi} \quad (2)$$

where: X_d is a variable that measures characteristics. As treatment was stratified at the district level, we focus on heterogeneity of effects with respect to district-level characteristics that may potentially condition the effect of NSP on outcomes of interest. We focus on the same variable that was used in Beath, Christia, and Enikolopov (2012) – proximity to Pakistan, as measured by a binary variable for the two districts in Nangarhar province. But we also check whether the heterogeneity effects hold once we condition on three additional district-level variables:(i) share of population in the district that identifies with the Pashtun ethnic group (measured by the baseline survey); (ii) opium production in 2006-07 (measured by the number of hectares in the district under poppy cultivation

[31] The plan was registered in February 2012 with ID 20120220AA. More broadly, our experimental design follows the 2008 document that described the hypotheses and planned methodology of the impact evaluation of NSP available at https://www.researchgate.net/publication/228594654.

per UNODC 2014); and (iii) initial level of violence in the district (measured as the logarithm of the number of security incidents within a 15 km radius around sample villages in the district between January 2006 and September 2007).

B. Effect on Security Incidents

Average Effect

The implementation of NSP results in a lower probability of security incidents both at midline and endline. These effects are even stronger if we control for baseline levels of violence. (Columns 1 and 2 of Table 4). The magnitude of the effect is higher in the time period between the midline and endline surveys, which coincided with the completion of NSP-funded projects, although the difference between the coefficients is not statistically significant. The disaggregated measures further indicate that the effect of NSP on security incidents is consistently negative for all radii larger than 4 km and is statistically significant at the 5 percent level for radii between 8 and 10 km at midline and between 7 and 13 km at endline (Figure 3; Table A.4 in the Online Appendix).

Effect Heterogeneity

The proximity of a district to the Pakistani border has a statistically significant effect on how NSP affects security incidents (Columns 2 and 6 of Table 4). In the eight sample districts that do not border Pakistan, the effect of NSP on security incidents is negative and statistically significant at both midline and endline, whereas the effect of NSP is very small in magnitude and not statistically significant for the two districts that border Pakistan. These differences in effects between districts are not driven by differences in the share of Pashtuns, opium production or pre-treatment level of violence (columns 5-6 of Table 4).[32] Moreover, the effect in the two districts that border Pakistan becomes consistently positive (although still not statistically significant) once we control for other district characteristics.

Disaggregation of the effects by radii further indicates that, in districts that do not border Pakistan, the program consistently decreases violence for all radii in both time periods, with the reduction in violence consistently statistically significant at the 5 percent level at endline for radii between 4 and 13 km from the village centers (see Figure 4 and Table A.5 in the Online Appendix). In districts bordering Pakistan, there is a reduction in incidents at the 9km radius but an increase in incidents at endline at the 2 km radius. The latter result suggests that the treatment villages themselves become targets of the attacks.

[32] Note that all the measures of characteristics except for the dummy for proximity to Pakistan are demeaned, so the coefficients in the first three rows correspond to the districts with average values of these measures.

To further examine effect heterogeneity, we control for distance to the Pakistani border within the sub-sample of villages in the two districts that border Pakistan, all of which are within 50 kilometers of the border. Despite a significant reduction in the sample size, the effect is statistically significant and indicates that the effect of NSP on security incidents strongly depends on the distance from the border (Table 6). In the immediate vicinity of the Pakistan border, NSP increases security incidents. This effect becomes smaller with distance and flips for villages located approximately 35-40 km from the border, which is slightly more than the median distance in this subsample.

Overall, the results indicate that NSP decreased violence both in the first two years after the start of NSP implementation and in the following two years, during which all NSP projects were completed. The decrease in violence, however, was observed only in areas that do not border Pakistan and there was no significant effect of the program on security in areas bordering Pakistan. Moreover, in the immediate vicinity of the Pakistani border, NSP increases the number of security incidents.

C. Effect on Perceptions of Security

Average Effect

The effect of NSP on perceptions of security by male and female villagers is strongly positive at midline, but attenuates in size and becomes only weakly statistically significant at endline (Columns 1 and 3 of Table 6). These effects hold for most of the constituent individual indicators, such as whether male respondents believe the security situation has improved in the last two years (Table A.7 in the Online Appendix). The fact that the questions about security perceptions were formulated in relative terms compared to the situation two years ago probably explains why the effect is observed at midline, but not at endline. There is no statistically significant evidence of a general effect of NSP on security incidents reported by villagers (Column 5 of Table 6), which, as noted above, is potentially driven by underreporting of security incidents in the survey.

Effect Heterogeneity

As with security incidents, the effects on perceptions of security by male villagers are conditioned by proximity to Pakistan (Column 2 of Table 6). In the two districts that border Pakistan, NSP has no effect on the probability of male respondents reporting an improvement in the local security situation at midline. There is no evidence of significant heterogeneity of effects with respect to perceptions of female respondents and self-reported security incidents.

D. Effect on Economic Outcomes

Average Effect

NSP has a weakly positive impact on individual economic outcomes at midline and has no effect at endline (Column 1 of Table 6). NSP has a strong positive effect on villagers' access to public goods

at both midline and endline (Column 3 of Table 6),[33] while positively impacting villagers' perceptions of their economic wellbeing and reducing net migration at both midline and endline (Columns 5 and 7 of Table 6). The midline result is driven by a statistically significant increase in reported household income (which also appears at endline) and a decrease in employment in subsistence agriculture and animal husbandry (Panel A of Table A.8 in the Online Appendix). The other constituent indicator – household consumption – shows no statistically significant changes in either period. For access to public serves and goods (such as time spent collecting water and the usage of electricity), the effects are generally stronger at endline than midline, although as the effect on water quality is stronger at midline, the overall effect on the summary index is stable over time (Panel B of Table A.8). Analysis of the effect for the constituent indicators of economic perceptions demonstrates that, while NSP improves the perceptions of female respondents at midline and endline, the effect for male respondents is confined to the midline (Panel C of Table A.8).

Effect Heterogeneity[34]

At midline, there are no significant differences between districts bordering Pakistan and the rest of the sample (Columns 2, 4, 6, and 8 of Table 6). At endline, the positive effect on public goods provision is stronger in districts bordering Pakistan, but the effect on individual economic outcomes and net migration in these districts is significantly smaller than in the rest of the sample. The effect of NSP on individual economic outcomes in the border districts is negative at endline as a result of a reduction in household consumption and an increase in employment in subsistence agriculture and animal husbandry (Panel A of Table A.8). There is thus no evidence of heterogeneity in the results at midline and some mixed results on heterogeneity at endline. Overall, NSP appears to have a positive effect on economic outcomes both in districts bordering Pakistan and in the rest of the country.

E. Effect on Attitudes toward Government

Average Effect

At midline and endline, NSP improves perceptions of government representatives, NGO employees, and ISAF soldiers, with the magnitude of the effect being higher at midline (Column 9 of Table 7). At midline, attitudes improve toward all types of government representatives and allied actors, except for members of the police (Table A.9 in the Online Appendix). At endline, the effect

[33] The measure of access to public goods corresponds to measures of "utilities" in the NSP impact evaluation final report (Beath, Christia and Enikolopov 2013b). The report also shows a positive effect of the program on services (female counseling, education and health services), but no effect on infrastructure outcomes (access to irrigation and villagers' mobility).

[34] We focus on the comparison of districts with respect to their proximity to Pakistan in the heterogeneity analysis for survey-based measures as this is the only dimension for which we see a conditioning effect on security incidents.

is weaker for all actors except central government officials and ISAF soldiers and loses statistical significance completely for members of parliament and NGO employees.

Effect Heterogeneity

The positive effect of the program on attitudes toward the government, NGOs and foreign military forces is limited to districts that do not border Pakistan. In the border districts, the effect is negative at midline and statistically significant at the 5 percent level for spatially correlated standard errors while it is not statistically significant at endline (Column 2 of Table 7). The effect heterogeneity results hold for all individual indicators, except attitudes toward the district governor and ISAF forces (Table A.9 in the Online Appendix).[35]

F. Robustness of Results

Alternative Outcome Measures

In the benchmark specification, we use the methodology of Kling, Liebman and Katz (2007) to construct summary indices. Tables A.10-A.12 in the Online Appendix provide evidence that the results are robust to using alternative indices, that are constructed either as the first principal component of the individual indicators (with factor loadings computed based on the data from the control group only) or as the covariance-weighted sum of the individual indicators (Anderson 2008). The results for the security measures are also robust but less precise if we use the number of security incidents instead of a binary measure of occurrence of security incidents (see Table A.13 in the Online Appendix).

Alternative Inferences Approaches

We use two alternative inference approaches to gauge the statistical significance of our results. First, we use standard errors that account for spatial and serial correlation instead of clustering. Spatial correlation among villages is assumed to be declining linearly up to a distance cutoff and to be zero after that cutoff (Conley 1999; Hsiang 2010). We use a cutoff at 100km and we check that the results are similar using cutoffs of 50km and 150km. The results on the effect of NSP on security incidents prove to be even more statistically significant is we use standard errors that account for spatial and serial correlation (Table A.14 in the Online Appendix) and similar in statistical significance for survey-based measures (Table A.15 in the Online Appendix).

In addition, we use randomization inference (Young 2016). In particular, we generate 10,000 placebo treatment assignments following the same procedure as in the actual treatment assignment (that is,

[35] There is no significant difference between the two groups of districts in the effects of NSP on attitudes toward ISAF soldiers at midline, but at endline there is a positive effect of NSP on attitudes towards ISAF soldiers in the districts that do not border Pakistan.

taking into account matched pairs and village clusters). For each of the placebo treatment assignments, we estimate treatment effects using specifications (1) and (2) in Subsection V.A and obtain the share of simulated *t*-statistics that are higher in absolute values than the original *t*-statistics, which can be interpreted as an analogue of the two-sided *p*-values based on the distribution of simulated treatment effects. P-values based on the permutation tests are very similar to the *p*-values based on the clustered standard errors or even smaller (Table A.16 in the Online Appendix).

Attrition in Survey-Based Measures

Unlike the results for security incidents that are based on the balanced panel, the results based on the survey data may be biased due to attrition at both the village and individual level, especially since attrition is driven primarily by security considerations (Subsection IV.B). While there were no significant differences in attrition between treatment and control villages in either the midline or endline surveys (Columns 1 and 2 of Table A.17 in the Online Appendix), the results may be biased if the determinants of attrition were also correlated with the treatment effect. At the village-level, attrition in the midline survey was correlated with violence before the start of the program, while poorer villages were more likely to attrit in the endline survey (Columns 3 and 4 of Panel A of Table A.17). At the individual level, attrition was higher for younger and poorer respondents, as well as non-Pashtuns (Panel B of Table A.17). The predicted probability of villages and individuals remaining in the panel is, however, uncorrelated with treatment status (Columns 5 and 6 of Panel B of Table A.17). For effect heterogeneity, the treatment effect is smaller for villages and individuals that were, according to their observable characteristics, more likely to suffer attrition (Table A.18 in the Online Appendix). Thus, the bias due to attrition was, if anything, likely to attenuate treatment effects.

We also assess the effect of attrition on our results by calculating lower and upper bounds using the non-parametric approach developed by Lee (2009). The results for the effect of the program on access to public goods, economic perceptions, and attitudes toward the government are robust to both village and individual attrition in both survey waves (Table A.19). However, the lower bound for individual-level attrition on the effect on security perceptions at midline is no longer statistically significant. Importantly, for all the effects that were statistically significant in the benchmark analysis, the difference between lower and upper bound is very small. Thus, the analysis suggests that attrition has a very limited effect on the estimated treatment effects for the survey-based measures.

VI. Discussion

While NSP generally reduces violent incidents, effects are conditioned by context. In areas bordering Pakistan in which the insurgency was dominated by fighters that did not depend on local communities for support, insurgents targeted development projects as a means of repelling

government authority (subsection VI.A). While the results appear to be robust to publication bias concerns and do not appear to be over-stated due to spillovers, caution is nonetheless warranted in extending the results to other development programs that do not share the characteristics of NSP and/or to other areas that do not share the characteristics of the sample (subsection VI.C).

A. Summary and Interpretation of Results

The effects of NSP on security and related outcomes in participating villages differed substantially by geography. At a sufficient distance from Pakistan (more than 40km), NSP appears to have initiated a process that is consistent with the "winnings hearts and minds" paradigm and which ultimately led to a reduction in violence. Specifically, the increased access of villagers to public goods enabled by NSP, improved villagers' perceptions of their economic situation and, in turn improved villagers' attitudes toward the government and allied actors, which led to a reduction in security incidents. The decline in security incidents resulting from the presence of NSP occurred at a distance from villages, which suggests that villages were used as staging areas for attacks, rather than being targets of the attacks themselves. Importantly, the positive effects of NSP on economic outcomes and violence are persistent and observed up to four years after the start of the program.

In areas close to the border with Pakistan, however, the effects of NSP on local security are very different. NSP improved the access of villagers to public services at both midline and endline to the same or even greater extent as in other sample districts, which improved villagers' perceptions of their economic situation and, at least at midline, improved individual economic outcomes. However, the economic benefits induced by NSP in these regions were not associated with a change in how villagers view government representatives and allied actors and the program did not lead to a reduction in security incidents. Instead, there is even evidence of an increase in security incidents associated with NSP in the immediate proximity to the Pakistani border. The increase in security incidents associated with NSP in these districts is observed in the immediate proximity of villages, which indicates that the treatment villages became targets of attacks, rather than merely staging areas for attacks.

In the eight non-border districts, NSP appears to have reduced violence primarily by reducing active (rather than tacit) support for the insurgency. A reduction in tacit support for the insurgency should generally be associated with increased flows of information between the population and counter-insurgent forces, which should lead to an increase in the proportion of IEDs that are found and cleared.[36] However, in the non-border districts, there is no discernable difference in the NSP-related increase on the number of IEDs that were found and cleared as opposed to the number of exploded

[36] As tacit support includes the harboring of insurgents in addition to the sharing of intelligence, it is feasible that the population may reduce the former while not increasing the latter. As such, the interpretation here is limited to only the intelligence-sharing aspect of tacit support.

IEDs (Table A.20 in the Online Appendix). Moreover, while increased information sharing would be expected to reduce the number of security incidents in areas near villages, the NSP-related reduction in violence does not appear to be stronger in the immediate vicinity of villages (Figures 4 and 5). As such, NSP appears to be reducing insurgent activity generally, rather than specifically increasing the sharing of intelligence between the local population and the ANSF and ISAF.

In the two districts that border Pakistan, NSP delivered economic benefits, yet popular support for the government did not increase and insurgents increased attacks in and around villages which received NSP. While it is impossible to precisely identify the characteristics of the border regions that induced this distinct effect, the finding that the adverse effect decreases with the distance to the border even within the two eastern districts and that we do not find any evidence that the effect depends on other distinguishing characteristics of these two districts – such as ethnic composition, intensity of opium production, or levels of pre-existing violence – suggests that proximity to the Pakistan border is the main characteristic that drives the difference. The finding that the effect does not depend on the levels of pre-existing violence is especially important, since it allows us to refute the alternative interpretation of the heterogeneity of results that was proposed in Beath, Christia, and Enikolopov (2012). In addition, the finding that there were no substantial differences in the effect of NSP on economic outcomes suggests that the divergence in the treatment effect was driven not by the differences in implementation of NSP, but by the differences between the structure of the insurgency in the areas bordering Pakistan and the insurgency in central, northern, and western Afghanistan.

Various qualitative studies and news reports indicate that the proximity of southern Nangarhar (where the two border districts in the sample are located) to one of the insurgency's major supply routes and the districts' adjacency to Pakistan's restive Kurram Agency resulted in an influx of foreign or otherwise non-local insurgents (Mansfield 2011; Dressler 2012; Foschini 2011).[37] Reports indicate that, in contrast to local insurgents, non-local insurgents commonly attacked projects and people working with government and allied actors, including NSP-funded projects and NGO staff and villagers associated with NSP (Foschini 2011).[38] In southern Nangarhar, the ability of local insurgents to mount a prolonged strategy of attacking projects and implementing staff was aided by the adjacency of the two border districts to Pakistan, which gave insurgents the ability to retreat, regroup and to mount cross-border attacks and thereby negated the importance of the support of

[37] Dressler (2012) notes that the Haqqani Network is active in "the districts of Hisarak, Sherzad, Chaparhar, and Jalalabad", which enables "the network to project force into the provincial capital of Jalalabad, transit east to Kabul, or smuggle men and materials into northern Laghman and Kapisa provinces" (p. 30). Dressler (2012) further notes that Nangarhar has been used particularly by the Haqqani Network to traffic Uzbek fighters representing the Islamic Movement of Uzbekistan "through eastern Afghanistan to the country's northern provinces" (p. 30).

[38] Foschini (2011) notes that "[t]he more brutal acts of violence – like the murder of two local Community Development Council workers in Rodat district of Nangarhar ... are usually ascribed to foreign, or anyway not local, insurgents".

the local population to insurgent operations (Buhaug and Gates 2002; Salehyan 2007, 2008; Crost, Felter and Johnston 2014; Martinez 2015). Absent the tactical constraint imposed by a need for the support of the local population (Kalyvas 2006; Kalyvas and Kocher 2009), insurgents have used indiscriminate violence to intimidate communities collaborated with the government and allied agencies.[39] The importance of cross-border attacks is fully consistent with the finding that the adverse effect of NSP on security incidents is limited to a relatively narrow area within 35-40km from the Pakistani border.

The difference in the effects of NSP on security incidents between the areas bordering Pakistan and the remaining areas seems to be a product of two different insurgency structures. In the districts in central, western, and northern Afghanistan, those participating in the insurgency are mainly local to the communities in which they reside and undertake attacks against targets a number of kilometers away from their home. NSP, by providing economic benefits to the community, increased the opportunity costs to villagers of participating in the insurgency and improved villagers' perceptions of the government. As a result, NSP reduced the number of villagers actively participating in the insurgency, which, in turn, decreased the number of attacks by local insurgents on targets away from their village. In the areas of eastern Afghanistan that border Pakistan, the influence of non-local fighters has been more pronounced. Non-local insurgents are less dependent on local support and so their optimal policy is often to attack local infrastructure and intimidate local leaders as a means of limiting government control of an area.[40] Thus, improvements in economic wellbeing caused by NSP, instead of winning support for the government and lowering violence as happens in areas where the insurgency is local, attracted coercion and increased violence.

The heterogeneity over the effect of development programs on conflict identified by this study reinforces the diversity of results in the existing literature. Recent studies have, for instance, contrastingly observed that large-scale food aid provided to governments of low-income countries can increase conflict (Nunn and Qian 2014), that small-scale development projects delivered by the military can improve security in countries afflicted by violent insurgencies (Berman, Shapiro, and Felter 2011), that community-driven development programs can increase conflict if insurgents employ violence strategically in response to program implementation (Crost, Felter and Johnston 2014), and that conditional cash transfers may reduce violence (Crost, Felter and Johnston 2016). These results and our own underline the importance of the local context in conditioning the effects

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[39] Foschini (2011) reports that in southern Nangarhar, "cross-border raids constitute an important tool for insurgents to intimidate or punish not-supportive local communities, as exemplified by the most famous of these instances when, two months ago, gunmen stormed a wedding party in Dur Baba [in south-eastern Nangarhar], killing the groom and eight other guests, apparently in retaliation for the defiant attitude of the district governor".

[40] Over time, the strategy has proved successful, with Hisarak falling out of government control for at least seven months over 2013-14 (Mahbob, 2013).

of development interventions and the danger inherent in governments and allied organizations seeking to apply one-size-fits-all approaches to countering insurgencies (Berman et al. 2013).

B. Methodological Concerns

Model Selection

Experimental results that involve multiple hypothesis testing and heterogeneity analysis can be viewed with skepticism unless they adhere to a pre-analysis plan that obviates opportunities for the manipulation of the model selection process. In the absence of a fully-fledged pre-analysis plan, we replicate the results using the exact specifications employed for an earlier working paper (Beath, Christia, and Enikolopov, 2012), which had access to an earlier form of the dataset that covered a more limited temporal span and a narrower set of incident types.

The findings of this study are fully consistent with those of Beath, Christia, and Enikolopov (2012). This not only indicates that the observed effects persist over time, but also makes it much less likely that the results are artifacts of multiple hypotheses testing or unrestricted heterogeneity analysis. This approach, which is similar in spirit to out-of-sample testing of econometric models, allows for credible estimation of the effects, and also ameliorates some of the concerns related to the rigid observance of detailed pre-analysis plans (Olken, 2015).

Spillovers

Externalities in insurgent violence between villages may affect the integrity of estimates of the effect of NSP on violence in two ways. On the one hand, an NSP-induced increase in government support in one village may reduce violence not only in and around that village, but also in other villages that have not received NSP.[41] Such positive spillovers from treatment to control villages will downwardly bias the estimated effect of NSP on violence. On the other hand, an NSP-induced increase in government support in one village may displace violence to neighboring villages if insurgents relocate to control villages that are deemed to be more supportive. Such negative spillovers from treatment to control villages would upwardly bias the estimated effect of the program on violence.

The design of the experiment attempted to reduce the potential for such spillovers by ensuring that villages that were close to each other received the same treatment status (subsection III.C). However, clustering might not mitigate this bias if the aforementioned security externalities are sufficiently strong. In this case, larger geographical units, such as districts, would represent the proper unit of analysis. Although we were unable to perform this analysis in the context of this field experiment, we consider that the potential for negative spillovers is unlikely, at least in the eight non-

[41] This is a particular concern in areas where insurgents undertake attacks well beyond their home village.

border districts. As our estimates of the effects of NSP on security incidents in the eight non-border districts indicate that insurgents frequently undertook attacks well beyond their village, positive spillovers are more feasible and may downwardly bias the estimates.

External Validity

The external validity of our findings is limited by the specific characteristics of the intervention and the regions we consider. Compared to other development interventions in Afghanistan and other conflict and post-conflict environments, NSP is distinguished by the large volume of resources it provides to communities, emphasis on community participation,[42] and the affiliation of the program with the government. In so far as such differences may condition how the local population and insurgents respond to a development program, the results discussed above may not necessarily apply to other types of interventions (e.g. interventions managed by bilateral aid agencies and those implemented by foreign militaries, such as projects funded by Provincial Reconstruction Teams and the Commanders Emergency Response Program studied in Berman, Shapiro, and Felter 2011).

The extrapolation of the study's findings to other regions of Afghanistan – such as the southern provinces – should be similarly informed by an understanding of the differences between those regions and the ten sample districts. As noted in subsection III.B, the sample districts are generally representative of the rural population of Afghanistan and, as such, the results for districts that do not border Pakistan should generalize to other areas in northern, central, and western Afghanistan. In some districts in southern Afghanistan – such as those bordering Pakistan – the conditions that resulted in the adverse effect of NSP on local security in southern Nangarhar would appear to be present and, as such, our findings may be applicable. In other areas of southern Afghanistan that experienced high levels of violence and where non-local fighters and cross-border sanctuaries were less relevant, considerable caution should be exercised in applying our findings to reach conclusions over the effects of development programs on local security.

VII. Conclusion

In this paper, we identify how Afghanistan's largest development program – the National Solidarity Program (NSP) – affected insurgent violence. To study potential channels behind the effect, we also examine the effects of the program on villagers' economic welfare and level of support for the government and allied actors. Random assignment of NSP across 500 villages in central, western, northern, and eastern Afghanistan enables causal inference over the effects of the program on the outcomes of interest, as well as the exploration of how context conditions the treatment effect. Concerns of whether the results are driven by model selection are addressed by replicating the

[42] NSP incorporates a range of participatory processes – such as the election of Community Development Councils and participatory selection of sub-projects – to ensure that members of community are involved in program implementation.

results of an earlier working paper that had access to a much narrower dataset of security incidents and did not include information from the last wave of the survey.

Estimates indicate that on average, NSP positively affects security and, in particular, reduces the probability of observing security incidents near villages that have received the program. This effect is not limited to short-term effects and is observed up to four years after the start of the program. The program also improves economic welfare and attitudes toward all levels of government, NGOs, and foreign forces. However, the effects are strongly conditioned by geography. In the two sample districts that border Pakistan, economic improvements induced by NSP did not translate into either improved attitudes toward the government or an improvement in security. In fact, treatment villages in the immediate vicinity of the Pakistani border were more likely to suffer attacks. In all other regions, NSP improved economic outcomes and attitudes toward the government and reduced violence. The difference in findings between the two sets of districts is not explained by variation in the districts' ethnic composition, initial level of violence, or levels of opium production.

These results demonstrate that development programs can contribute to preventing the spread of violent conflict, but only in areas in which insurgents are embedded in local communities. By improving the access of the population to basic services, development programs can improve the local population's perceptions of the government and, in turn, discourage them from providing active or tacit support to the insurgency, which in turn results in a persistent reduction in violence. However, in areas dominated by non-local fighters and proximate to cross-border safe havens, improvements in economic welfare delivered by development programs do not appear to translate into a reduction in violence. Rather, such programs may exacerbate insecurity by providing opportunities for non-local insurgents to attack development projects. This finding is consistent with the results in Crost, Felter and Johnston (2014), but shows that the adverse effects on security can persist during and after project implementation. This interpretation of the mechanisms behind the heterogeneity of results, however, is not based on micro-level data on the composition and activity of insurgents. Exploiting this type of data would be a promising avenue for future research, although obtaining such information is obviously difficult (see Johnston et al 2016 for a rare exception of using micro-level data on terrorist organization).

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Figure 1. Ten Sample Districts

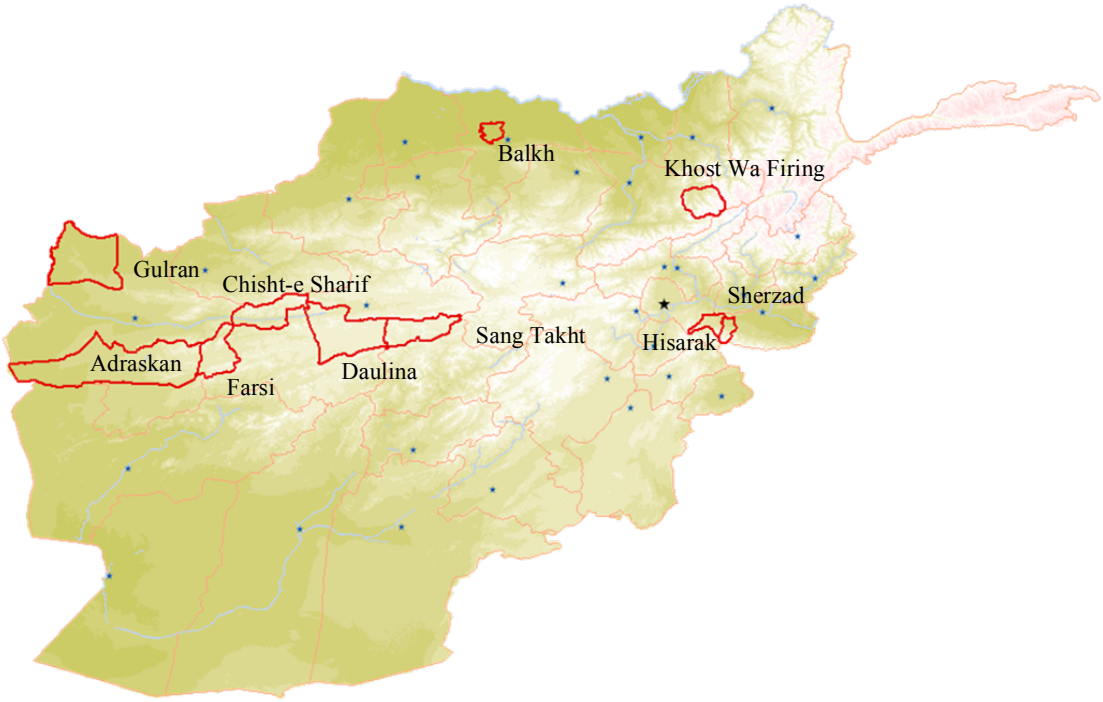
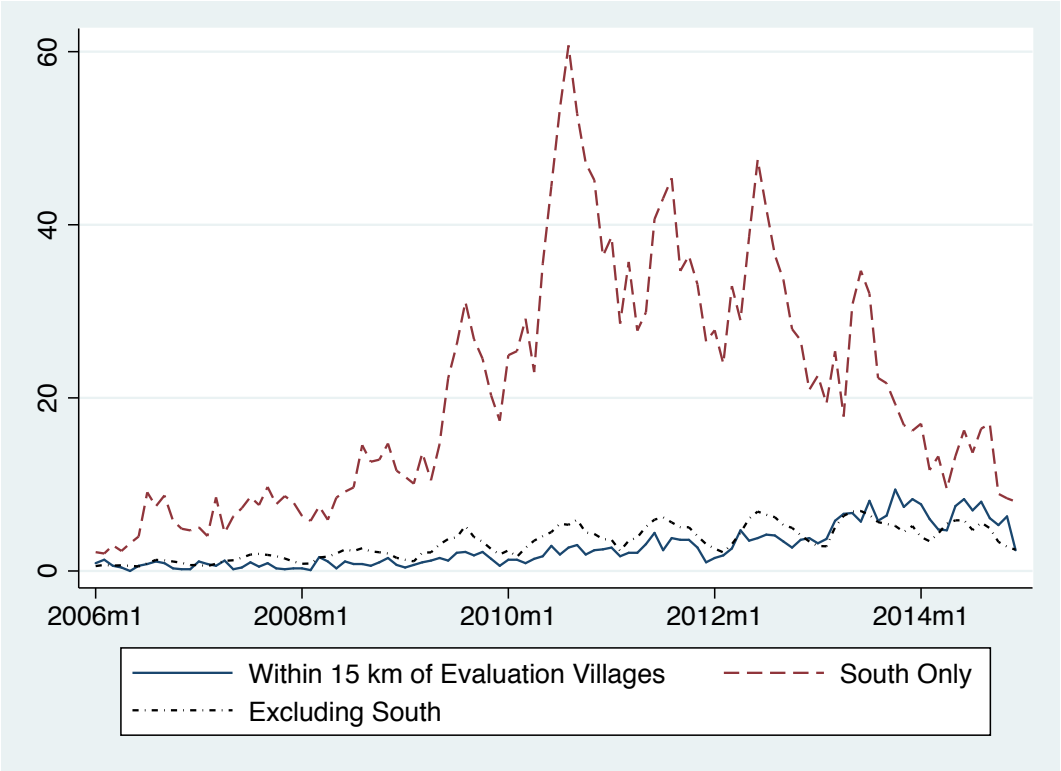
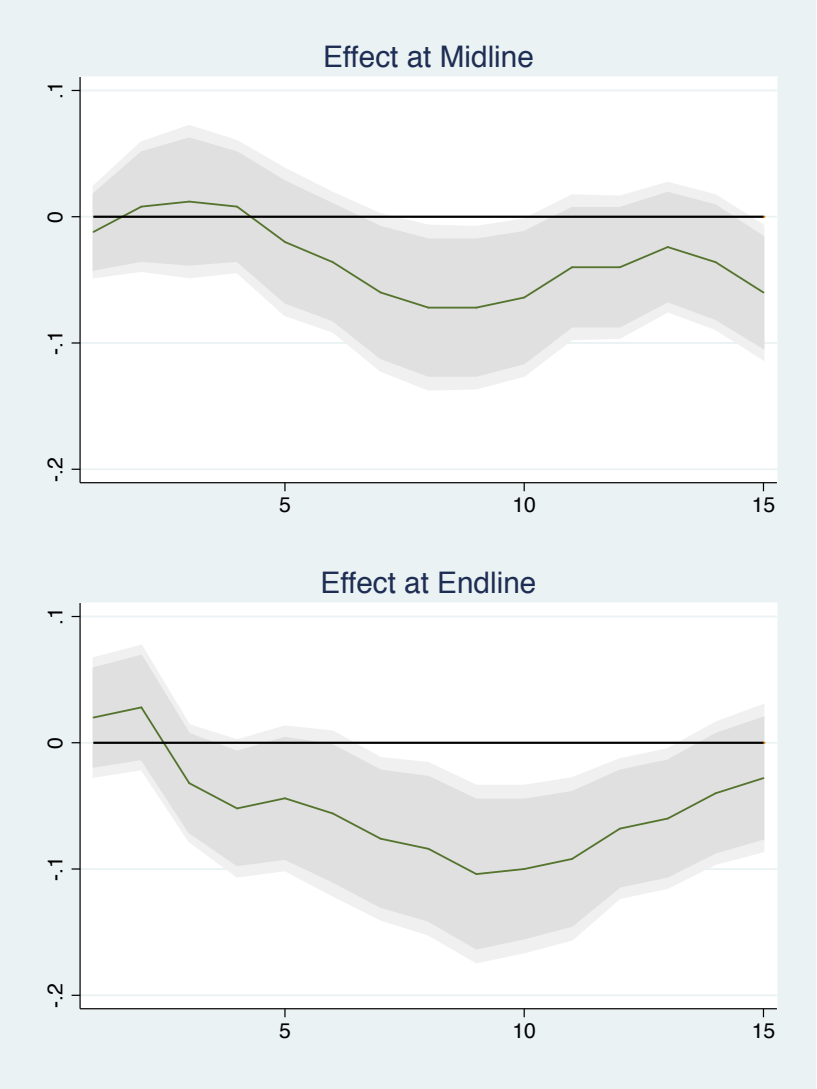


Figure 2: Number of Security Incidents



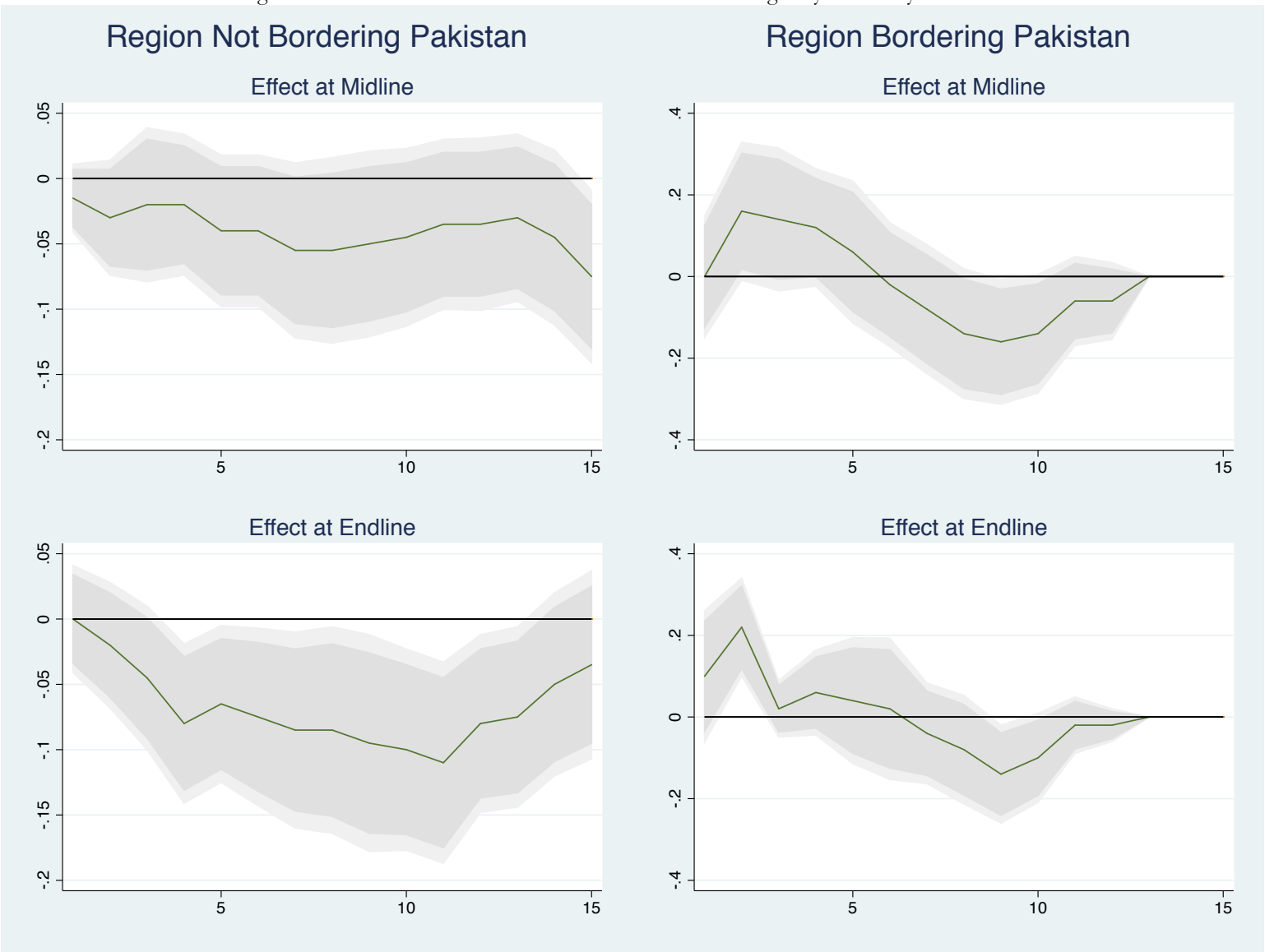
Note: The figure shows the average number of incidents within 15km of evaluation villages in each of the 10 sample districts, as well as the average number of incidents per district in the southern area of Afghanistan, and in all the areas of Afghanistan except the southern part

Figure 3: Treatment Effect for Different Radii around Villages



Notes: The figure plots estimated treatment effects for the occurrence of security incidents within a certain radius of a village, where the radius changes from 1km to 15km. Lighter (darker) shaded area shows 5% (10%) confidence intervals based on standard errors adjusted for clustering at the village-cluster level.

Figure 4: Treatment Effect for Different Radii around Villages by Proximity to Pakistan.



Notes: The figures plot estimated treatment effects for the probability of having a security incident within a certain radius of a village, where the radius changes from 1km to 15km. Lighter (darker) shaded area shows 5% (10%) confidence intervals based on standard errors adjusted for clustering at the village-cluster level.

Table 1. Characteristics of Sample Districts

Region	Province	District	Opium Production in 2006-07 (Hectares)	Number of Incidents from January 2006 until Start of NSP	Pashtun Share of Population	Borders Pakistan
Northern	Baghlan	Khost Wa Firing	249	1	0.02	No
Northern	Balkh	Balkh	987.5	30	0.65	No
Central	Daykundi	Sang Takht	1	0	0.00	No
Central	Ghor	Duleena	65.5	0	0.00	No
Western	Heart	Adraskan	147.5	19	0.90	No
Western	Heart	Chishti Sharif	21	3	0.08	No
Western	Heart	Fersi	55.5	2	0.03	No
Western	Heart	Gulran	16	4	0.24	No
Eastern	Nangarhar	Hesarak	289	5	1.00	Yes
Eastern	Nangarhar	Sherzad	647	80	0.96	Yes

Note: Although Hesarak does not directly border Pakistan, it is separated by only a narrow strip of Sherzad district.

Table 2. Statistical Balance between Treatment and Control Groups.

Variable	Mean Level in Control Group	Mean Level in Treatment Group	Normalized Difference	*t*-Statistics
Number of Households in Village	110.06	120.15	0.09	1.14
Number of People in Household	9.87	9.76	-0.02	-0.42
Age of Respondent	43.27	43.77	0.04	1.10
Respondent Speaks Pashtu as Mother Tongue	0.29	0.28	-0.02	-0.28
Respondent is Unemployed	0.05	0.04	-0.03	-0.62
Respondent is Employed in Subsistence Agriculture or Husbandry	0.66	0.65	-0.01	-0.22
Respondent Received no Formal Education	0.72	0.72	0.01	0.34
Respondent Owns Land	0.66	0.67	0.02	0.28
Household Has Access to Electricity	0.13	0.15	0.04	0.59
Male Health Worker is Available to Treat Villagers	0.10	0.13	0.08	1.33
Female Health Worker is Available to Treat Villagers	0.08	0.10	0.07	1.10
Main Source of Drinking Water is Unprotected Spring	0.27	0.27	0.00	0.00
Dispute among Villagers Occurred in Past Year	0.38	0.37	-0.02	-0.27
No Problems are Experienced in Meeting Household Food Needs	0.45	0.46	0.02	0.36
Household Borrowed Money in Past Year	0.48	0.48	-0.02	-0.33
Respondent Reports Attending Meeting of Village Council in Past	0.34	0.36	0.03	0.68
Respondent Believes that Women Should be Members of Council	0.42	0.44	0.05	0.89
Assets	-0.01	0.00	0.01	0.15
Natural Log of Income	8.67	8.63	-0.07	-1.15
Natural Log of Consumption	11.10	11.06	-0.06	-0.91
Occurance of Security Incidents within 2 km of Village between January 2006 and Start of NSP	0.05	0.07	0.07	0.75
Occurance of Security Incidents within 5 km of Village between January 2006 and Start of NSP	0.16	0.18	0.04	0.47
Occurance of Security Incidents within 10 km of Village between January 2006 and Start of NSP	0.29	0.28	-0.03	-0.30
Occurance of Security Incidents within 15 km of Village between January 2006 and Start of NSP	0.44	0.45	0.02	0.27

Table 3. Summary Statistics for Outcome Variables.

	Control Group				Treatment Group			
	Mean Level	Standard Deviation	Mean Level	Standard Deviation	Mean Level	Standard Deviation	Mean Level	Standard Deviation
	Midline		Endline		Midline		Endline	
Panel A								
Security Incidents								
Occurance of Security Incidents within 2 km of a Village	0.12	0.32	0.12	0.33	0.12	0.33	0.15	0.36
Occurance of Security Incidents within 5 km of a Village	0.32	0.47	0.30	0.46	0.30	0.46	0.26	0.44
Occurance of Security Incidents within 10 km of a Village	0.53	0.50	0.57	0.50	0.46	0.50	0.47	0.50
Occurance of Security Incidents within 15 km of a Village	0.67	0.47	0.71	0.46	0.61	0.49	0.68	0.47
Panel B								
Economic Outcomes								
Natural Log of Annual Household Income	7.04	0.64	7.31	0.74	7.08	0.64	7.36	0.69
Natural Log of Annual Household Consumption	7.51	0.66	7.68	0.70	7.51	0.63	7.68	0.67
Respondent is Unemployed	0.06	0.24	0.20	0.40	0.06	0.25	0.19	0.39
Respondent is Employed in Subsistence Agriculture and Husbandry	0.59	0.49	0.17	0.38	0.55	0.50	0.18	0.38
Access to Public Goods								
Primary Source of Drinking Water is Protected Source	0.37	0.48	0.32	0.47	0.41	0.49	0.37	0.48
Estimated Hours Spent Collecting Water in Past Week	2.23	0.99	1.03	0.69	2.29	0.95	0.99	0.62
Number of seasons in Past Year Water Was of Poor Quality	0.88	1.27	1.06	1.26	0.74	1.17	1.00	1.22
Number of seasons in Past Year Water Was Not Available	0.44	0.61	0.68	0.71	0.39	0.60	0.63	0.70
Logarithm of Hours of Electricity in Past Month	1.39	2.28	2.45	2.45	1.49	2.29	2.78	2.43
Economic Perceptions								
Perceived Improvement in Household's Situation in Past Year (Male)	0.35	0.48	0.15	0.35	0.41	0.49	0.16	0.37
Expected Improvement in Household's Situation Next Year (Male)	0.26	0.44	0.10	0.31	0.30	0.46	0.12	0.33
Perceived Improvement in Household's Situation in Past Year (Female)	0.29	0.45	0.17	0.38	0.34	0.47	0.21	0.41
Expected Improvement in Household's Situation Next Year (Female)	0.38	0.48	0.17	0.38	0.43	0.49	0.21	0.41
Ln(Net Number of Families Migrating to the Village)	0.37	2.09	0.09	2.01	0.86	1.99	0.56	1.94
Panel C								
Security Perception by Male Respondents								
Security in and around Village Has Improved in Past Two Years	0.62	0.49	0.53	0.50	0.66	0.48	0.57	0.49
Security in and around Village Has Deteriorated in Past Two Years	0.13	0.34	0.21	0.41	0.12	0.33	0.20	0.40
Security Perception by Female Respondents								
Compared to Two Years Ago Women Feel More Safe in Working for NGOs or the Government or Attending Training Courses	0.29	0.45	0.30	0.46	0.33	0.47	0.34	0.47
Compared to Two Years Ago Women Feel Less Safe in Working for NGOs or the Government or Attending Training Courses	0.17	0.38	0.30	0.46	0.13	0.34	0.28	0.45
Compared to Two Years Ago Teenage Girls Feel More Safe when Traveling to and from School or Socializing	0.29	0.46	0.32	0.47	0.33	0.47	0.34	0.47
Compared to Two Years Ago Teenage Girls Feel Less Safe when Traveling to and from School or Socializing	0.21	0.41	0.32	0.47	0.18	0.38	0.29	0.45
Self-Reprted Security Incidents								
Village has Experienced Attack in the Past Year	0.04	0.19	0.07	0.26	0.03	0.18	0.07	0.25
Village has Experienced Attack by Anti-Government Elements in the Past Year	0.03	0.17	0.06	0.23	0.03	0.17	0.04	0.21
Respondent Household has been Affected by Insecurity in Village during the Past Year	0.01	0.12	0.03	0.17	0.02	0.14	0.04	0.19
Respondent Household has been Affected by Insecurity on Roads around District during the Past Year	0.02	0.15	0.06	0.24	0.03	0.16	0.06	0.24
Panel D								
Attitudes toward Government, Civil Society, and ISAF Soldiers								
District Governor Acts for the Benefit of All Villagers	0.59	0.49	0.57	0.50	0.65	0.48	0.59	0.49
Provincial Governor Acts for the Benefit of All Villagers	0.65	0.48	0.60	0.49	0.71	0.46	0.62	0.49
Central Government Officials Act for the Benefit of All Villagers	0.64	0.48	0.55	0.50	0.69	0.46	0.58	0.49
President of Afghanistan Acts for the Benefit of All Villagers	0.76	0.43	0.69	0.46	0.80	0.40	0.72	0.45
Members of Parliament Act for the Benefit of All Villagers	0.50	0.50	0.40	0.49	0.56	0.50	0.41	0.49
Government Judges Act for the Benefit of All Villagers	0.46	0.50	0.34	0.47	0.51	0.50	0.37	0.48
National Police Act for the Benefit of All Villagers	0.71	0.46	0.74	0.44	0.72	0.45	0.76	0.43
NGO Employees Act for the Benefit of All Villagers	0.64	0.48	0.59	0.49	0.68	0.46	0.60	0.49
ISAF Soldiers Act for the Benefit of All Villagers	0.26	0.44	0.17	0.37	0.29	0.45	0.20	0.40

Table 4. Effect on Security Incidents

	Occurrence of at Least One Security Incident					
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Effect at Midline	-0.071	-0.083	-0.087	-0.090	-0.130	-0.107
	(0.042)*	(0.038)**	(0.047)*	(0.044)**	(0.059)**	(0.057)*
Treatment Effect at Endline	-0.119	-0.131	-0.151	-0.154	-0.192	-0.169
	(0.045)***	(0.043)***	(0.053)***	(0.052)***	(0.060)***	(0.057)***
Proximity to Pakistan × Treatment Effect at Midline			0.081	0.035	0.294	0.121
			(0.105)	(0.085)	(0.165)*	(0.164)
Proximity to Pakistant × Treatment Effect at Endline			0.164	0.117	0.369	0.195
			(0.082)**	(0.074)	(0.142)***	(0.142)
Pashtun Share of Population × Treatment Effect at Midline					-0.459	-0.224
					(0.271)*	(0.266)
Pashtun Share of Population × Treatment Effect at Endline					-0.349	-0.114
					(0.235)	(0.239)
Opium Production × Treatment Effect at Midline					-0.036	-0.029
					(0.027)	(0.026)
Opium Production ×Treatment Effect at Endline					-0.040	-0.032
					(0.029)	(0.027)
Initial Level of Violence × Treatment Effect at Midline					0.119	0.082
					(0.060)*	(0.054)
Initial Level of Violence × Treatment Effect at Endline					0.079	0.043
					(0.053)	(0.050)
Occurrence of at Least One Security Incident at Baseline		0.433		0.430		0.421
		(0.059)***		(0.060)***		(0.063)***
Matched Pair-Survey Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,000	1,000	1,000	1,000	1,000	1,000
R-squared	0.831	0.861	0.832	0.862	0.835	0.863
Treatment Effect at Midline in Districts in Close Proximty to Pakistan			-0.006	-0.055	0.164	0.014
			(0.094)	(0.073)	(0.129)	(0.125)
Treatment Effect at Endline in Districts in Close Proximty to Pakistan			0.013	-0.037	0.176	0.026
			(0.063)	(0.052)	(0.109)	(0.110)

Note: Dependent variables are unweighted average of measures for different radii (Kling, Leibman, and Katz, 2007). Midlne referes to the period from the start of the program in October 2010 until the completion of the Midline survey in September 2009; Endline refers to the period from the completion of the midline survey until the completion of the endline survey in September 2011. Measures of the share of Pashtuns, opium production, and the initial level of violence are demeaned. Standard errors corrected for spatial correlation and serial correlation in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5: Effect on Security as Function of Distance to Pakistan

	Occurrence of at Least One Security Incident	
	Regions Bordering Pakistan	
	(1)	(2)
Treatment Effect at Midline	1.042	0.679
	(0.356)***	(0.316)**
Treatment Effect at Endline	0.902	0.539
	(0.263)***	(0.306)*
Distance to Pakistan × Treatment Effect at Midline	-29.056	-20.076
	(10.152)***	(9.344)**
Distance to Pakistan × Treatment Effect at Endline	-24.559	-15.579
	(7.761)***	(8.991)*
Distance to Pakistan	-18.827	-17.842
	(10.800)*	(7.365)**
Dependent Variable at Baseline		0.389
		(0.102)***
Matched pair-survey fixed effects	Yes	Yes
Observations	200	200
R-squared	0.867	0.896

Note: Dependent variables are unweighted average or measures for different radii (Kling, Leibman, and Katz, 2007). Midline referes to the period from the start of the program in October 2010 until the completion of the Midline survey in September 2009; Endline refers to the period from the completion of the midline survey until the completion of the endline survey in September 2011. Distance is measured in thousands of kilometers. Robust standard errors adjusted for clustering at the village-cluster level in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6. Effect on Perceptions of Security (Survey-Based Measures)

	Security Perception by Male Respondents		Security Perception by Female Respondents		Self-Reported Security Incidents	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Effect at Midline	0.080	0.098	0.086	0.098	0.002	-0.004
	(0.023)***	(0.026)***	(0.028)***	(0.032)***	(0.028)	(0.033)
Treatment Effect at Endline	0.046	0.046	0.018	0.006	0.010	0.004
	(0.025)*	(0.027)*	(0.026)	(0.026)	(0.027)	(0.032)
Proximity to Pakistan × Treatment Effect at Midline		-0.107		-0.084		0.033
		(0.050)**		(0.058)		(0.045)
Proximity to Pakistan × Treatment Effect at Endline		0.000		0.211		0.033
		(0.077)		(0.139)		(0.038)
Matched Pair-Survey fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,962	8,962	7,909	7,909	8,989	8,989
R-squared	0.342	0.342	0.319	0.319	0.331	0.331
Treatment Effect at Midline in Districts in Close Proximty to Pakistan		-0.009		0.014		0.029
		(0.043)		(0.049)		(0.030)
Treatment Effect at Endline in Districts in Close Proximty to Pakistan		0.046		0.216		0.038
		(0.072)		(0.137)		(0.020)*

Note: Dependent variables are unweighted average of the corresponding individual indicators (Kling, Leibman, and Katz, 2007). Robust standard errors adjusted for clustering at the village-cluster level in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 7. Effect on Economic Outcomes and Public Goods Provision (Survey-Based Measures)

	Economic Outcomes								Attitudes toward Government, Civil Society, and ISAF Soldiers	
	Individual Economic Outcomes		Access to Public Goods		Economic Perceptions		Ln(Net Number of Families Migrating to the Village)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment Effect at Midline	0.030	0.022	0.053	0.064	0.109	0.101	0.419	0.340	0.096	0.124
	(0.015)*	(0.018)	(0.028)*	(0.029)**	(0.016)***	(0.018)***	(0.184)**	(0.203)*	(0.021)***	(0.023)***
Treatment Effect at Endline	0.015	0.030	0.123	0.078	0.083	0.078	0.481	0.622	0.056	0.063
	(0.017)	(0.019)	(0.029)***	(0.025)***	(0.020)***	(0.020)***	(0.197)**	(0.217)***	(0.018)***	(0.020)***
Proximity to Pakistan × Treatment Effect at Midline		0.045		-0.063		0.050		0.511		-0.170
		(0.031)		(0.093)		(0.035)		(0.479)		(0.049)***
Proximity to Pakistan × Treatment Effect at Endline		-0.092		0.283		0.032		-0.774		-0.043
		(0.037)**		(0.122)**		(0.069)		(0.466)*		(0.053)
Matched pair-survey fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,990	8,990	8,990	8,990	8,992	8,992	873	873	8,982	8,982
R-squared	0.145	0.146	0.366	0.369	0.212	0.212	0.658	0.662	0.239	0.240
Treatment Effect at Midline in Districts in Close Proximty to Pakistant		0.067		0.001		0.151		0.408		-0.046
		(0.026)**		(0.089)		(0.030)***		(0.208)*		(0.043)
Treatment Effect at Endline in Districts in Close Proximty to Pakistant		-0.062		0.360		0.110		-0.076		0.020
		(0.032)*		(0.120)***		(0.066)*		(0.205)		(0.049)

Note: Dependent variables (except in columns 7-8) are unweighted average of the corresponding individual indicators (Kling, Leibman, and Katz, 2007). Robust standard errors adjusted for clustering at the village-cluster level in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.