Can social assistance reduce violent conflict and civil unrest? Evidence from a large-scale public works program in Ethiopia

Kalle Hirvonen, Elia Machado, Andrew M. Simons*

11 March 2025

Abstract

We assess how one of the largest public works programs in the world—Ethiopia's Productive Safety Net Program (PSNP)—affected violent conflict and civil unrest. Using difference in differences methods and linking administrative and geocoded conflict event data, we find that the PSNP did not change the risk of violent events, but reduced the likelihood of civil unrest by almost half when compared to non-PSNP districts. These effects are most pronounced during the period of 2014-18, coinciding with widespread protests in Amhara and Oromia, the two most populous regions of Ethiopia. Examining mechanisms, we find evidence that the PSNP fostered greater sympathy and satisfaction with the government, making PSNP households less likely to engage in demonstrations.

*Hirvonen: K.Hirvonen@cgiar.org. International Food Policy Research Institute (IFPRI), and United Nations University World Institute for Development Economics Research (UNU-WIDER), Katajanokanlaituri 6 B, FI-00160 Helsinki, Finland. Machado: Dept. of Earth, Environmental, and Geospatial Sciences, Lehman College, CUNY, Bronx, NY 10468, and Earth and Environmental Sciences Program, The Graduate Center, CUNY, New York, NY 10016. Simons: Dept. of Economics, Fordham University, Bronx, NY 10458. We thank Kate Ambler, Alessandra Hidalgo Aréstegui, Guush Berhane, John Hoddinott, Jordan Kyle, Patricia Justino, Subha Mani, Jean-François Maystadt, Sophie Mitra, and David Rosenkrantz as well as participants at a UNU-WIDER brownbag seminar and the 2024 Agricultural & Applied Economics Association Annual Meeting for useful feedback. This work was supported by the donors who fund the CGIAR Research Initiative on Fragility, Conflict and Migration (FCM), through their contributions to the CGIAR Trust Fund: https://www.cgiar.org/funders/. Part of Hirvonen's time was funded through the UNU-WIDER project, Strengthening Safety Nets in Post-Conflict and Humanitarian Contexts, funded by the Ministry for Foreign Affairs of Finland. The views and opinions expressed are those of the authors only and do not necessarily reflect those of the organizations that funded the study. We do not have a pre-analysis plan as none of our data is primary data collected through surveys or experimental means. We use administrative data on PSNP caseloads sourced from the Ethiopian government, publicly available conflict data from the Armed Conflict Location and Event Data Project (ACLED), and geospatial datasets for environmental/climate data (rainfall, etc.).

Keywords: conflict risk, public works programs, cash transfers, ACLED, PSNP
JEL Codes: D74, I38, H53, Q34, O15

1 Introduction

Violent conflict and civil unrest have increased sharply around the world in the last two decades (Rustad, 2024; OECD, 2021). These upward trends are most pronounced in Africa — a region that hosts 60 percent of the world's poor (World Bank, 2023) — where the number of state-based armed conflict events nearly tripled over the past 15 years (Rustad, 2024), and the number of protests more than quadrupled over the same period (OECD, 2021). During this time, poverty reduction in Africa has been slowest in its fragile and conflict-affected areas (Beegle et al., 2018), likely linked to the enormous economic losses associated with violent conflict and civil unrest (Rohner and Thoenig, 2021; Braithwaite et al., 2014). These trends highlight the growing instability of the region and the urgent need for policies addressing the intertwined challenges of poverty, inequality, violent conflict, and civil unrest.

Recognizing the urgency to address these issues, many countries and international organizations have increased their support to countries highly affected by conflict, often prioritizing cash transfers and other forms of social assistance (World Bank, 2020b; FAO, 2022; IMF, 2022; OECD, 2022). These decisions are grounded in the well-established country-level negative correlation between conflict and per capita income (Collier and Hoeffler, 1998) as well as a growing body of evidence linking persistent poverty and economic shocks to violent conflict and civil unrest (Bellemare, 2015; McGuirk and Burke, 2020; Fetzer, 2020; Cantoni et al., 2024). However, while social assistance and other antipoverty programs are often promoted as tools to reduce violence and civil unrest, evaluations of their effectiveness have focused on the short (Crost et al., 2016; Khanna and Zimmermann, 2017) to medium (Fetzer, 2020; Premand and Rohner, 2024) term, ranging from nine months to six years after the launch of the program, and their impact remains mixed (Rohner and Thoenig, 2021). Moreover, much of the existing research has focused on violent conflict, leaving their potential to mitigate civil unrest underexplored.

This paper addresses these gaps by examining the short and long-term effects of Ethiopia's

Productive Safety Net Program (PSNP), one of the largest and longest-running public works programs in the world, on violent conflict and civil unrest. Launched in 2005 and reaching eight million people, the PSNP provides cash or food transfers to participating households in exchange for labor intensive public works aimed at environmental and infrastructure rehabilitation (Wiseman et al., 2010). The program is relatively well targeted and has been found to improve food security and increase tree cover in the localities where it operates (Berhane et al., 2014; Hoddinott et al., 2024; Hirvonen et al., 2022). We analyze the program's first 15 years (2005–2019), a period marked by sporadic armed insurgencies and a major anti-government protest movement between 2014 and 2018.

In our empirical approach, we link geocoded data on conflict events with digitized PSNP administrative data, and environmental variables to construct annual district-level panel data spanning 1997–2019, permitting us to assess both short and long-term conflict dynamics. We use the definitions for violent events (battles, explosions, or violence against civilians) and civil unrest (protests or riots) as used in our conflict source data, the Armed Conflict Location and Event Data Project (ACLED).[1] Then, using difference-in-differences methods,[2] we find that the PSNP did not significantly alter the risk of violent events in either the short or long term. However, it reduced the likelihood of civil unrest by 2.6 percentage points (95% CI: 1.2; 4.0), or by 47% compared to the mean probability of civil unrest in non-PSNP districts. The reduction in civil unrest is more pronounced in districts with a higher proportion of PSNP beneficiaries. Furthermore, the PSNP decreased the likelihood of a fatality occurring during civil unrest by 1 percentage point (95% CI: 0.2; 1.8), or 52%. An event study plot reveals that the program had no discernible effect on demonstrations in its initial years but led to large reductions in demonstrations between 2014 and 2018, a period of widespread protests against the federal government. Importantly, trend differences before the program

[1] See Table A1 in the appendix for more details.

[2] Government-implemented social assistance programs are often not randomized (Leight et al., 2024), and consequently, most research on this topic employs quasi-experimental techniques, such as difference-in-differences (Fetzer, 2020) or regression discontinuity designs (Crost et al., 2014; Khanna and Zimmermann, 2017).

do not explain these findings, and the results are robust in a variety of checks.[3]

Next, we examine the potential mechanisms through which the PSNP reduces civil unrest. Theoretically, social assistance and other redistributive programs can reduce relative deprivation (Gurr, 1971) and increase support for the incumbent government (Manacorda et al., 2011; Zucco Jr, 2013; De La O, 2013), making civil unrest less likely to occur (Justino and Martorano, 2018). A distinctive feature of the PSNP is that its public works projects are selected and planned by the communities themselves, with technical support from higher administrative levels (MoARD, 2006; Wiseman et al., 2010). This community-driven approach may further enhance trust and legitimacy. Evidence from other contexts suggests that such approaches can strengthen trust in the government by delivering development projects that are responsive to local needs (Olken, 2010; Parks et al., 2019). Moreover, social assistance programs may also function as an insurance mechanism that buffers against real income shocks, which might otherwise fuel violence and unrest (Bellemare, 2015; McGuirk and Burke, 2020; Fetzer, 2020; Cantoni et al., 2024). Finally, public works programs (in contrast to other forms of social assistance) could be unique in how they reduce unrest. Public works activities may keep participants occupied, thereby limiting their availability to engage in demonstrations.

To understand how the PSNP reduced the likelihood of civil unrest, we explore three potential mechanisms. First, examining the months of the year with demonstrations with the months of the year in which public works projects operated, we find no evidence to support the hypothesis that the time constraints imposed by the PSNP public works projects are the mechanism through which the PSNP decreases the likelihood of demonstrations. Second, following the study by Fetzer (2020) of India's National Rural Employment Guarantee Act (NREGA), we examine whether the PSNP serves as an insurance mechanism to mitigate the potential relationship between civil unrest and negative income shocks caused by poor

[3] Our findings are consistent across different outcome variables and are not influenced by a specific calendar year or region of Ethiopia. The results remain robust when controlling for drought shocks, applying an inverse probability treatment weighting approach, and using spatially robust standard errors.

weather outcomes. We do not find evidence that the PSNP serves as an insurance mechanism in this way. Third, using a longitudinal household survey that included questions asking respondents about their "trust in the government" before and after the launch of the PSNP, we find that the PSNP is associated with increased trust in the government to "do what is right for the people". This signals that the program may have fostered greater trust in the government either by reducing economic marginalization and strengthening the social contract, and/or by enhancing government legitimacy through its community-driven public works approach, which aligns development projects with local needs.

To our knowledge, this study is among the first to assess how social assistance programs influence the likelihood of both violent conflict and civil unrest. In addition, our data, spanning 15 years since the inception of the PSNP, allow us to capture the short- and long-term dynamics of these relationships. This enables us to contribute to two related yet distinct strands of this literature: i) examining nonviolent political participation as it relates to the social contract between governments and citizens, and ii) the impact of social assistance and other antipoverty programs on violent conflict.

Regarding the first theme, our study presents new evidence on how social assistance programs influence civil unrest. To our knowledge, it is among the first to analyze the impact of a large-scale government transfer program on political instability.[4] Taking a broader view, the findings presented here contribute to research on how government-implemented programs and service provision can strengthen the social contract between governments and citizens, helping prevent civil unrest (Devarajan and Ianchovichina, 2018; Justino and Martorano, 2018, 2019; Justino, 2025). For example, during the Syrian revolution in 2011-2012, the risk of violence was lower in sub-districts with a higher government provision of electricity (De Juan and Bank, 2015). Cross-country evidence from Europe suggests that government

[4]Two unpublished studies analyze the impact of government transfer programs on protest participation. Justino and Martorano (2016) use difference-in-differences with propensity score matching to analyze Mexico's Oportunidades program, finding that it reduced protest participation after the 2008 financial crisis by 2.4 percentage points – a 39% decrease relative to the 2008 national average. Aguero and Fasola (2022) study South Africa's Old Age Pension program using a fuzzy regression discontinuity design and find no significant effect on protest participation.

spending cuts are associated with an increased number of protests (Ponticelli and Voth, 2020). In Latin America, beneficiaries of large-scale conditional cash transfer programs have been found to be more likely to support the incumbent government (Manacorda et al., 2011; Zucco Jr, 2013; De La O, 2013).

In our setting, we find that the PSNP's dampening effect on civil unrest was concentrated in the period between 2014 and 2018 that was marked by major anti-government protests, driven by widespread feelings of economic and political marginalization (Human Rights Watch, 2014; Abbink, 2016; Dias and Yetena, 2022; Makahamadze and Fikade, 2022). This temporal specificity suggests that the PSNP's impact on civil unrest was shaped by the heightened political and economic turbulence during this period, since protests were largely absent outside this timeframe and concentrated in non-PSNP areas during the unrest. This finding aligns with emerging literature demonstrating that the apparent effects of interventions can heavily depend on the prevailing temporal context when impacts are measured (Kroft and Notowidigdo, 2016; Casey et al., 2023; Fiala et al., 2025). It underscores the value of longitudinal analysis — and data that cover a longer period of time — to be able to capture the dynamic interplay between social programs and civil unrest in a variety of contexts or time frames.

On the second theme, there is mixed evidence on the impact of social assistance and other antipoverty programs on violent conflict. In the short term, programs implemented in active conflict areas may escalate violence if insurgent groups perceive that a new government initiative undermines their legitimacy among the local population. For instance, Premand and Rohner (2024) evaluated an unconditional cash transfer program in Niger and found that it led to short-term increases in violence from terrorist groups, including Boko Haram and other jihadist organizations, seeking to disrupt the program. Similarly, Khanna and Zimmermann (2017) reported that India's NREGA caused short-term increases in the violence of Maoist insurgents. Crost et al. (2014) assessed a large development program without a transfer component in the Philippines, documenting a significant increase in conflict casualties where the program was implemented compared to ineligible municipalities. This surge was primarily due to incidents initiated by insurgents during the early stages of the program, aimed at undermining government support.

Although these studies show that the short-term impacts of such social assistance programs can sometimes exacerbate violence, there is also evidence suggesting that, in the long term, these types of programs can reduce violence and civil unrest. For example, looking at the longer-term impacts of NREGA, Fetzer (2020) found that the program serves as an insurance mechanism, keeping rural incomes above a reservation level during periods of drought, making it difficult for insurgents to recruit rural farmers. Crost et al. (2016) studied a conditional cash transfer program in the Philippines and found a significant reduction in violent conflict events nine months after the launch of the program. Their analysis highlights how the program reduced the influence of insurgent groups in areas where it was operational. Meanwhile, Berman et al. (2011) documented how improved service provision reduced insurgent violence in Iraq. They suggested that these improvements came only after the coalition forces had a better understanding of communities' needs. In Afghanistan, Beath et al. (2013) found that a large-scale development program without transfer components positively changed attitudes toward government, influenced perceptions of well being, and reduced violence in many areas, but not in areas with the highest levels of initial violence.

We contribute to this literature via a null result on the PSNP's effect on violent conflict. The previous literature has often found increases in violence in the short term when insurgent or rebel groups may be challenging/attacking the new program, and decreases in violence in the longer term as the programs became established and responsive to local needs. In the case of Ethiopia's PSNP we find a null result on violent conflict in the short and long term. This null finding may be explained by contextual factors. During the study period, the armed insurgencies in Ethiopia were relatively small in scale and rebel groups were not strong enough to recruit fighters from rural areas en masse, as was the case with the Maoists in India (Khanna and Zimmermann, 2017; Fetzer, 2020). In addition, there were no influential terrorist groups that attempted to sabotage seemingly successful social assistance programs, such as the case of Boko Haram sabotaging the government-led cash transfer program in Niger (Premand and Rohner, 2024).

The remainder of the paper is composed of seven sections. Section 2 describes how and why the PSNP was launched, the political and governance context of Ethiopia, and the conflict dynamics in our study period (1997-2019). Section 3 describes the district-level panel data set used for our main analysis. Section 4 details our difference-in-differences approach along with the event study specification. Section 5 describes our results, section 6 describes the various robustness checks, and section 7 explores potential explanatory mechanisms of our results, incorporating evidence from a longitudinal household survey on trust toward the government. In section 8, we conclude the paper.

2 Setting

2.1 The PSNP

With a population of over 110 million, Ethiopia is the second most populous country in Africa (World Bank, 2021). Rainfed agriculture forms a major component of the national economy, providing a livelihood to approximately 80 percent of the population. Ethiopia's history is characterized by catastrophic droughts that triggered large-scale famines in the 1970s and 1980s. The 1990s and early 2000s were characterized by localized food shortages that were typically addressed by *ad hoc* requests for humanitarian food aid (De Waal, 2017).

Launched in 2005, the PSNP is a multiyear initiative aimed at improving food security through a more sustainable approach than the repeated emergency humanitarian appeals that characterized the 1990s and early 2000s (Wiseman et al., 2010; De Waal, 2017). Although the program is primarily funded by international partners (World Bank, 2018), it is managed and implemented by the Ethiopian government. Approximately 80-85 percent of the households included in the PSNP receive food or cash payments in return for labor-intensive public works carried out over a six-month period outside of the main agricultural season, the other 15-20 percent of the households with limited labor capacity (e.g., pregnant and lactating women, elderly) receive unconditional transfers. The real value of the transfer amounts has varied across years, but on average, it has amounted to approximately 15 percent of household consumption (Hirvonen and Hoddinott, 2021).[5]

The PSNP combines geographic and community level targeting. Districts were chosen for the program based on how often they had requested and received emergency food aid before the program's 2005 launch (MoARD, 2006; World Bank, 2020a). Within these districts, communities identify the most food-insecure households to receive PSNP assistance (Simons, 2022). Studies based on household data from PSNP areas indicate that the program is generally well targeted at the community level (Coll-Black et al., 2011). However, a recent assessment of the geographic targeting suggest that many impoverished and food-insecure districts are not covered by the PSNP (World Bank, 2020a).

When the program began, it served 192 districts[6] with 4.8 million beneficiaries in the four highland regions of Amhara, Oromia, Southern Nations, Nationalities and Peoples' Region (SNNP) and Tigray, along with smaller, mainly urban areas in the east such as Dire Dawa and Harari (World Bank, 2020a). Since its inception, the number of PSNP's beneficiaries has increased in the highland districts where it started, and the program has expanded to include Ethiopia's lowland regions of Afar and Somali. By 2019, the PSNP had grown to cover over 300 districts, offering support to around eight million people (World Bank, 2020a). To date, none of the districts that entered the program have exited from it (World Bank, 2020a).

Evaluations of the PSNP have found it to reduce food insecurity (Gilligan et al., 2009; Berhane et al., 2014), lessen the negative impacts of drought shocks (Knippenberg and Hoddinott, 2017), and to have modest effects on agricultural productivity (Hoddinott et al.,

[5]In 2019, the average annual transfers per household amounted to approximately \$124 (Berhane et al., 2020).

[6]We use the term district to mean *woreda*, the third-level administrative division within the country.

2012; Gazeaud and Stephane, 2023). The PSNP public works projects, which include soil and watershed conservation, hillside terracing, and reforestation activities have resulted in increased tree cover in PSNP districts (Hirvonen et al., 2022).

Our analysis centers on the four highland regions of Amhara, Oromia, SNNP, and Tigray. This geographic focus is due to three main reasons. First, the PSNP has been active in these regions the longest, providing a more extensive time frame to study its impact on conflict. In addition, these highland regions experienced a simultaneous program rollout (in early 2005). Second, despite the program's expansion into other regions, the highland regions have continued to be a primary focus. In 2019, over 70 percent of all PSNP beneficiaries were from these four highland regions. Third, the quality of implementation has been considerably higher in the four highland regions compared to the two lowland arid regions that joined the PSNP later (Sabates-Wheeler et al., 2013; Lind et al., 2022).

2.2 Political and governance context

After a long and brutal civil war that started in 1974, Ethiopian and Eritrean rebel forces overthrew the Derg military junta in 1991. A transitional government was formed, after which the Ethiopian People's Revolutionary Democratic Front (EPRDF) gained control of the federal government in Addis Ababa. The EPRDF was a multiparty alliance of the Tigray People's Liberation Front, the Amhara Democratic Party, the Oromo Democratic Party, and the Southern Ethiopian People's Democratic Movement. After gaining power, the EPRDF established a federal governing structure formed of regional states divided along ethno-linguistic lines.[7]

The EPRDF era was characterized by impressive development gains in multiple domains. The GDP per capita tripled between 1991 and 2019, crop yields doubled, and the poverty headcount rate fell from 69 percent in 1995 to 27 percent in 2015 (World Bank, 2024).

[7]During our study period, the country was sub-divided into nine regional states and two chartered cities. This study focuses on the highland regions—Amhara, Oromia, SNNP, and Tigray—that together host approximately 65 percent of the total population in Ethiopia.

The infant mortality rate decreased by 70 percent and child stunting fell from 68 percent in 1992 to 27 percent in 2019 (World Bank, 2024). However, at the same time, political freedoms were severely limited. Assessing the degree of political rights and civil liberties, the Freedom House classified Ethiopia as *partly free* between 1995 and 2010, and thereafter as *not free* (Freedom House, 2024). Since 2002, Ethiopia has consistently ranked in the bottom 20 percent of the World Press Freedom Index (Reporters Without Borders, 2024). Meanwhile, the Human Rights Watch reports serious human rights violations throughout our study period (see, e.g., Human Rights Watch, 1998, 2001, 2016).

2.3 Conflict dynamics, 1997-2019

During the EPRDF's rule, there were three main factors that contributed to conflict events in the highland regions. First, ethnically based armed groups such as the Oromo Liberation Front (OLF, based in the Oromia region) and Ogaden National Liberation Front (ONLF, based in the Somali region) initially joined the EPRDF-led transitional government. However, they left shortly after, citing harassment and obstacles to their regional campaigns (Human Rights Watch, 1998). Since then, these groups, together with their allies, have engaged in sporadic armed insurgencies against the federal government and their respective state governments (Human Rights Watch, 1998).

A second factor was the Eritrean—Ethiopian War from May 1998 to June 2000 (Human Rights Watch, 2001). The war began over a border dispute, primarily centered on the town of Badme and its surrounding areas, which both countries claimed as their own. The conflict escalated into a full-scale war with extensive fighting along the disputed border. We exclude conflict events involving the Eritrean military in this analysis as these were international war events, rather than within-country conflict events, which is the focus of this study.[8]

Finally, between 2014 and 2018, a series of anti-government protests took place in the two most populous regions, Amhara and Oromia (Human Rights Watch, 2016). These protests

[8] In section 6 we show that our results are robust to also including events involving foreign actors (Figure 7).

were initially sparked by the 2014 announcement of the Addis Ababa Master Plan, which proposed expanding the capital's boundaries at the expense of farmers in Oromia (Human Rights Watch, 2016). However, the underlying grievances driving the unrest were rooted in widespread feelings of economic and political marginalization (Human Rights Watch, 2014; Abbink, 2016; Dias and Yetena, 2022; Makahamadze and Fikade, 2022). In April 2014, protests began in several towns across Oromia against the 2014 Addis Ababa Master Plan. By the end of 2015, the protests had spread to nearly all zones of Oromia, and by July 2016, they had also reached the Amhara region (Human Rights Watch, 2017b).[9] The protest wave ended in 2018 when Prime Minister Hailemariam Desalegn resigned and Abiy Ahmed, originally from the Oromia region, was sworn in as the new prime minister. The EPRDF was dissolved in December 2019.[10]

3 Data

We generated an annual data series of PSNP beneficiary counts at the district level by digitizing the program's yearly planning documents provided by the Ethiopian Ministry of Agriculture. These planning documents date from 2005 (the year of the PSNP's inception) through 2019 (we made the formal data request in 2020). All variables were aggregated at the district level using the 2007 administrative boundaries from Ethiopia's Central Statistical Agency as a baseline. This was the closest census data available to the program's onset.[11] The data set used in our analysis comprises 617 districts, with 247 districts having received PSNP support since the program's inception in 2005.

[9] On October 2, 2016, a stampede at the Oromo cultural festival (Irreecha) in the town of Bishoftu resulted in the deaths of dozens, possibly hundreds, of people. The stampede was triggered by security forces using tear gas and firearms in response to anti-government chants (BBC News, 2016; Human Rights Watch, 2017a). This incident marked a significant turning point, sparking severe riots in the region and leading to a nationwide state of emergency, which imposed further restrictions on freedom of speech and resulted in hundreds of arrests (Human Rights Watch, 2017a).

[10] Our study period does not include the civil war in 2020-2022 primarily fought in the Tigray region and the subsequent widespread armed conflict in the Amhara region.

[11] Any increase in the number of PSNP-eligible highland districts since the census year resulted from the administrative division of districts (Wiseman et al., 2010). To address this, we merged the newly created child districts back with their parent districts as of 2007, including their respective PSNP beneficiary counts.

The conflict variables for violent and civil unrest were calculated by totaling the number of events in each district per year, based on data from the Armed Conflict Location and Event Dataset (ACLED) (Raleigh et al., 2010). ACLED categorizes conflict events into violent events and demonstrations (see Table A1 in the appendix). Violent events include battles, explosions/remote violence, and violence against civilians. Demonstrations—–referred to interchangeably as civil unrest in this paper—–include protests and riots. In addition to conflict events, ACLED also records the number of reported fatalities from each event.[12]

After removing events categorized as strategic developments (88 observations),[13] events involving Eritrea and other foreign actors (94 observations), and those with the lowest geocoded precision (35 observations) which location cannot be assigned to a specific district,[14] there were 3,358 conflict events recorded in the four highland regions between 1997 and 2019. Out of these, 1,522 (45%) are classified as violent events and 1,836 (55%) as demonstrations.[15] The number of recorded fatalities during the same period related to these conflict events was 13,974.

Figures 1a and 1b show the number of conflict events and fatalities in the highland regions during the study period. The year 2014 clearly marks a distinct change in conflict dynamics. Between 1997 and 2013, there were typically fewer than 100 conflict events recorded in the

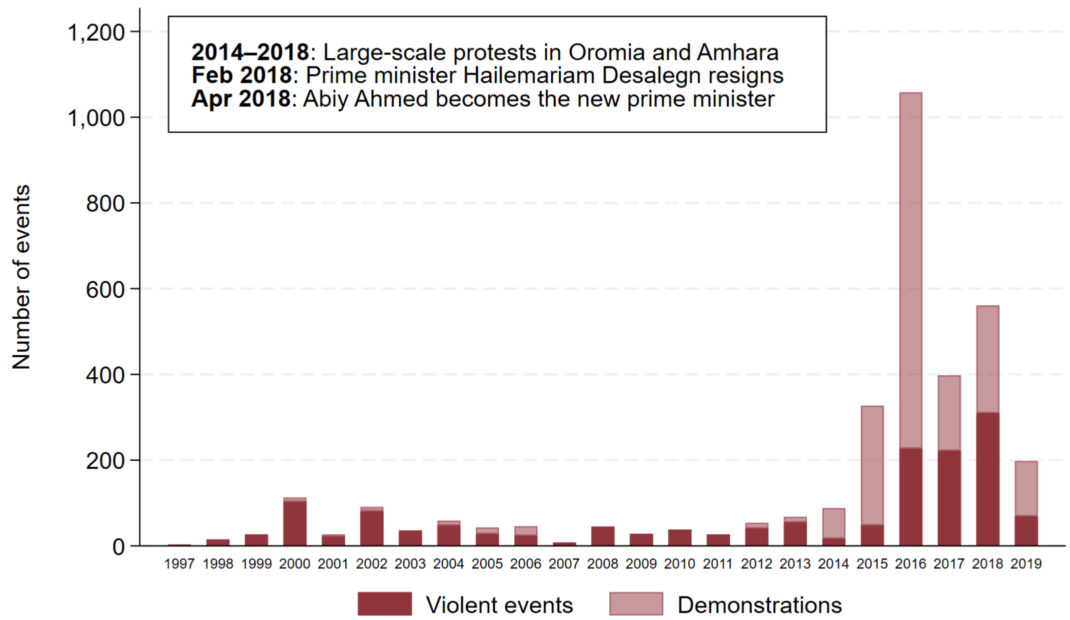
[12] The Uppsala Conflict Data Program (UCDP), a commonly used source in conflict research, is not suitable for our analysis. Unlike ACLED, the UCDP applies a stricter conflict definition, requiring armed force between two organized parties—one of which must be the government—resulting in at least 25 battle-related deaths within a calendar year (Sundberg and Melander, 2013). In addition, the UCDP only includes incidents involving armed force used by organized actors against either other organized actors or civilians, with at least one direct fatality at a specific time and place. The UCDP's data lacks sufficient variation for our purposes, with conflict-related fatalities occurring in only 1.6 percent of district-year observations during the study period, and does not differentiate between event types, such as demonstrations, which restricts its applicability for analyzing civil unrest.

[13] According to documentation from ACLED (2023), 'strategic developments' represent crucial junctures in periods of political violence (e.g., recruitment drives, peace talks, high-level arrests) but are not collected and recorded in the same cross-comparable fashion as 'political violence' and 'demonstration' events. As such, the 'strategic developments' event type is primarily used for understanding conflict context, and not necessarily a marker of conflict itself.

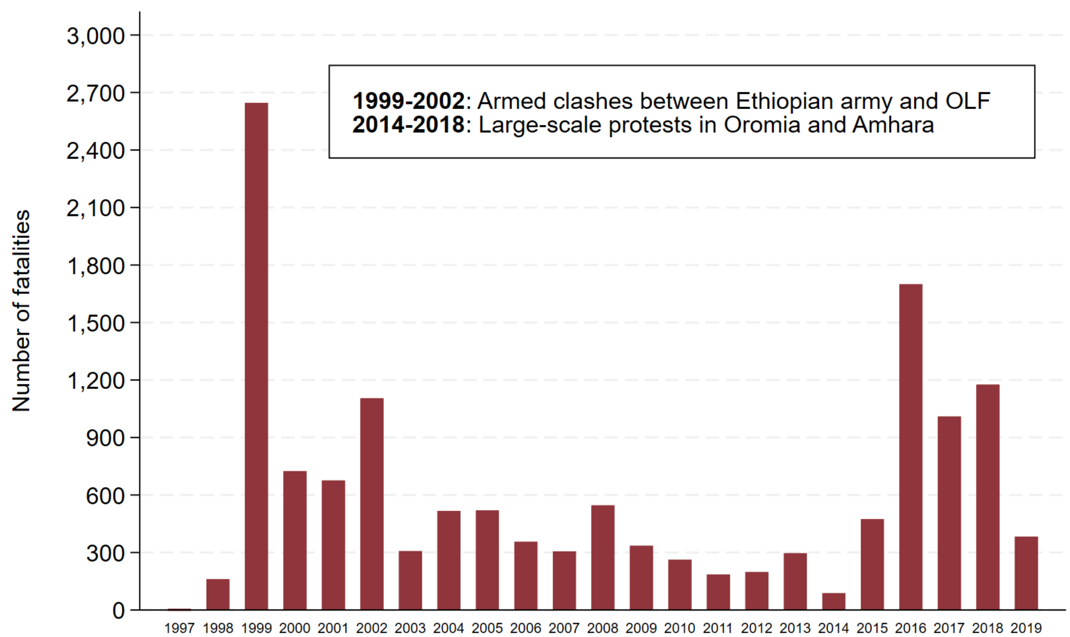
[14] Geo-precision ranges from 1 (highest precision) to 3 (lowest precision) in the ACLED data set. A conflict event is assigned code 3 if a large (non-specific) region is mentioned in the original conflict reporting (like 'border area', 'forest', or 'sea').

[15] Violent actors involved in violent events were primarily the Ethiopian military forces, police, ethnic militia, and armed opposition groups. In contrast, virtually all demonstration events involved either the Ethiopian military force, the police, rioters, or demonstrators.

Figure 1: Conflict in highland Ethiopia, by year



(a) Conflict events in highland Ethiopia, by year



(b) Conflict-related fatalities in highland Ethiopia, by year

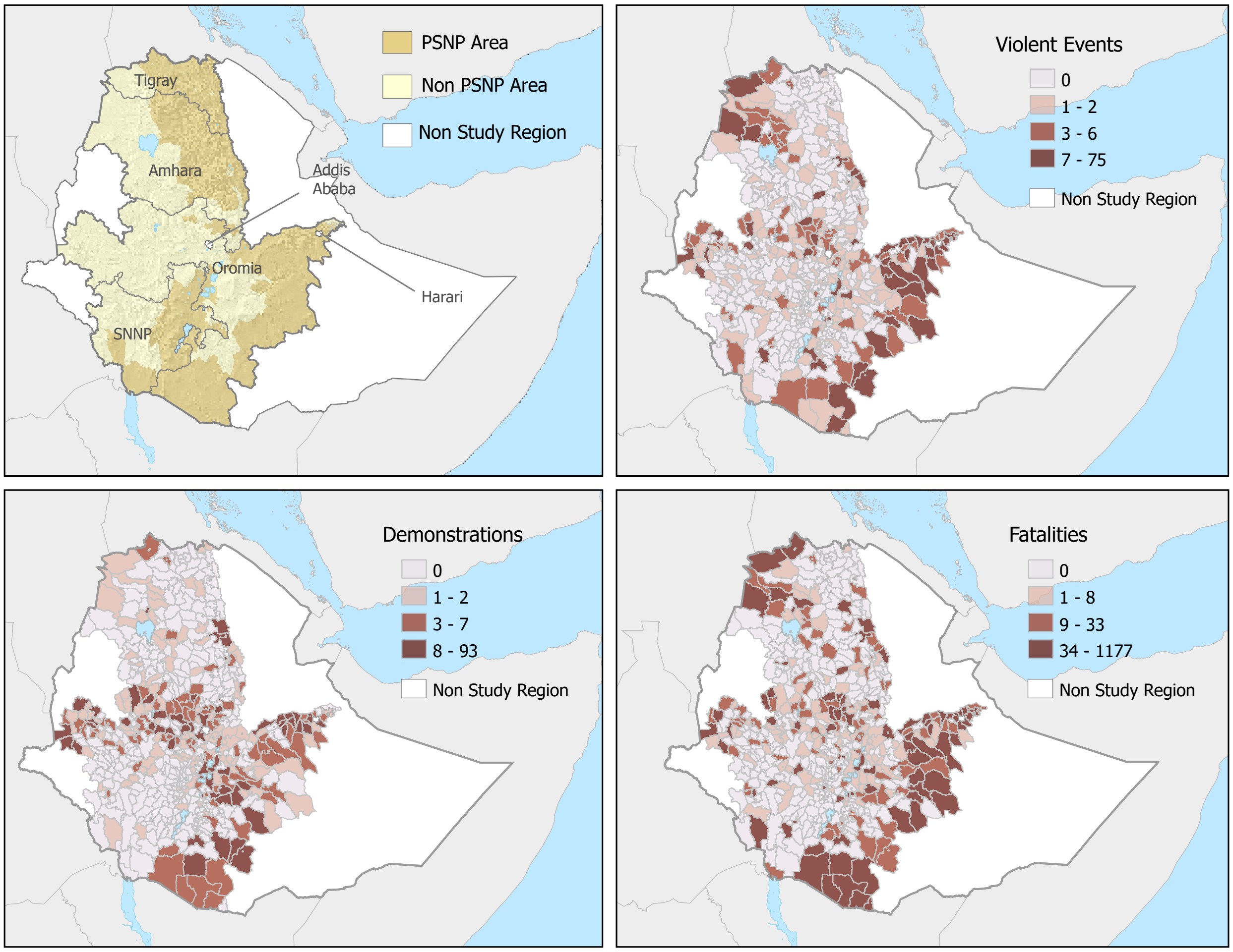
Ethiopian highlands. More than 85 percent of the events during this period were violent events. In 2014, large-scale protests began in the Oromia region, which eventually spread to the Amhara region. Between 2014 and 2019, two-thirds of the recorded events were demonstrations. Fatality counts are more evenly distributed across years. Apart from the protest period, 1999-2002 stands out. This period was characterized by active fighting between the OLF and Ethiopian military forces.

Figure 2 shows the spatial distribution of the number of violent events, demonstrations and fatalities by district in Ethiopia. Between 1997 and 2019, violent events, and fatalities were largely concentrated in districts located in the north-western and south-eastern parts of the Ethiopian highlands, while no events were recorded in many districts in the north-eastern and south-western regions of the highlands. Demonstrations largely occurred in the central and south-eastern parts of the Ethiopian highlands.

As the foregoing analysis indicates, conflicts were relatively rare events in highland Ethiopia during the study period. Across the 617 districts in our data set, the likelihood of observing a violent event, demonstration, or fatality is approximately five percent for each type of event between 1997 and 2019. This produces a heavily right-tailed distribution, raising the risk that a few very large values disproportionately influence the regression estimates. To address this concern, we convert our outcome variables into binary ones, assigning a value of 1 if a conflict event took place in a district in a specific year.[16] However, we later show that our results are robust to using the non-converted outcome variables.

[16] A low incidence of conflict events is a common feature in this literature. For instance, Premand and Rohner (2024) report that, on average, only 6% of their household sample in Niger was exposed to a conflict in a given year, while Crost et al. (2016) find that the typical village in their sample from the Philippines recorded just 0.07 conflict events annually. Similarly, Khanna and Zimmermann (2017) report an average of 0.08 conflict incidents per month in Indian districts between January 2005 and March 2008. In our case, the average district in the Ethiopian highlands records 0.09 violent events, 0.15 demonstrations, and 0.77 fatalities per year during the study period.

Figure 2: Spatial distribution of conflict type by district (1997-2019)



Note: a map of Ethiopia showing the highland study region and the spatial distribution of each type of conflict event. Top-left: the study region with PSNP districts (district boundaries not shown) in light brown and non-PSNP districts in beige. Top-right: violent events by district between 1997 and 2019. Bottom-left: demonstrations by district between 1997 and 2019. Bottom-right: fatalities (from conflict) by district between 1997 and 2019. Water bodies are only shown in the study region. Source: authors' compilation based on data from ACLED (2023).

In additional analyses, we use the Ethiopian Rural Household Survey (ERHS) and several spatial data products (see Table A2). These data sets are introduced in the relevant sections below.

4 Methods

We first use a static two-way fixed effect specification (Roth et al., 2023) to estimate the impact of the PSNP on conflict:

$$Y_{d,t} = \beta(D_d * T_t) + \alpha_d + \delta_t + \epsilon_{d,t}, \tag{1}$$

where $Y_{d,t}$ is a binary variable obtaining value 1 if district d experienced a conflict event in year t. We estimate Equation (1) separately using three different conflict event types: violent event, demonstration, and a fatality. D_d is a binary variable capturing PSNP districts. T_t is a binary variable obtaining value 1 if the year is on or after the launch of the PSNP, thus obtaining value 1 if district d is observed in 2005-2019 and zero if before 2005. The terms α_d and δ_t are fixed effects in districts and years, respectively, and absorb non-interacted variables D_d and T_t. This is a non-staggered difference in differences setting: districts are either treated in 2005-2019 or never treated during this period. The treatment effect is estimated as β.

Considering the changing conflict dynamics over time (Figures 1a and 1b), we then turn to an event-study approach (Miller, 2023) to model the PSNP's impact on conflict risk in district d in year t:

$$Y_{d,t} = \left[\sum_{j \in \{-8,\ldots,0,\ldots,14\}}^{j} \beta_j \ (D_{d,t-j}) \right] + \alpha_d + \delta_t + \epsilon_{d,t}, \tag{2}$$

where α_d and δ_t are district and year fixed effects, as in Equation 1. $D_{d,t-j}$ are binary variables indicating that the district was a given number of years away from 2005, the year marking the launch of the PSNP.[17] We observe conflict events from period $t = -8$ (1997) to period $t = 14$ (2019). If $j > 0$, β_j coefficients capture the dynamic effects of the PSNP over time since its launch. If $j < 0$, β_j coefficients quantify the dynamic effects of PSNP before

[17]We use the user-written command *eventdd* in Stata by Clarke and Tapia-Schythe (2021) to estimate Equation 2.

its launch, which we use to assess pre-treatment trends. Statistically significant treatment effects before the launch of the PSNP would cast doubt on the parallel trend hypothesis (Roth et al., 2023). As we will show later, we cannot reject the null hypothesis that the conflict dynamics of two groups followed a parallel trend in the pre-treatment period.

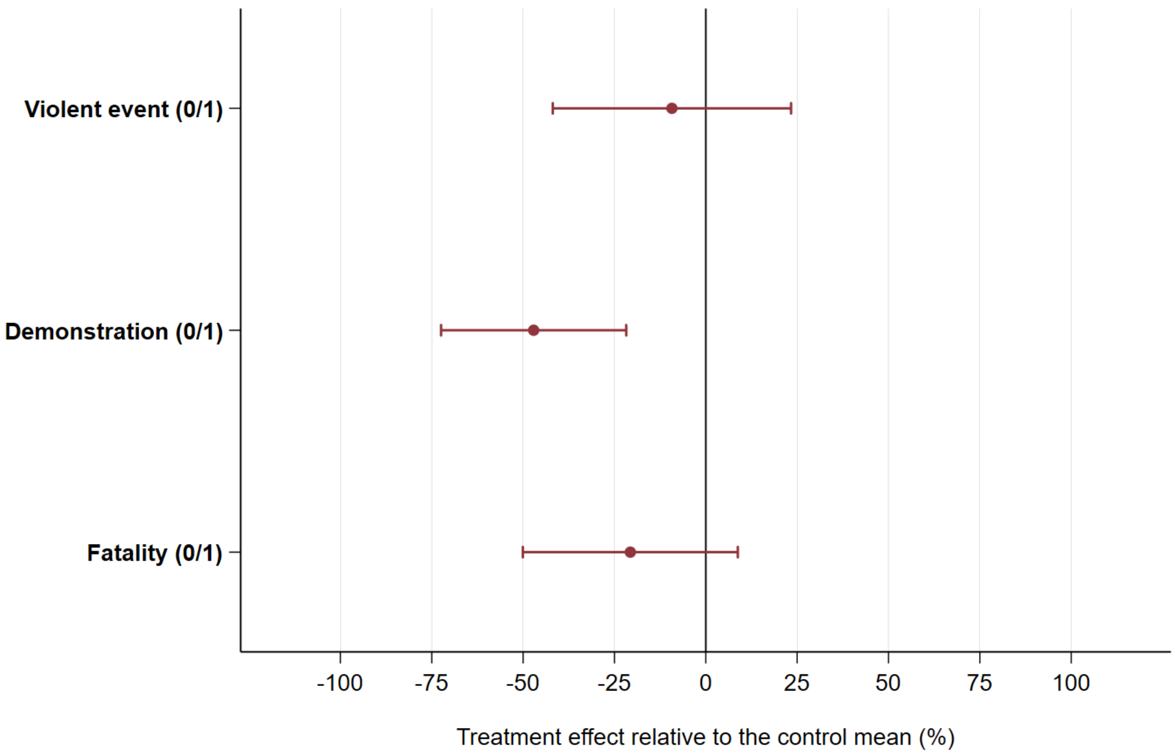
In all specifications, we cluster our standard errors at the district level — i.e.„ the level of the treatment (Abadie et al., 2023). Below we demonstrate that our findings are robust to applying standard errors that are robust to both spatial autocorrelation and heteroskedasticity (Conley, 1999).

5 Results

In Figure 3 we present our main findings expressed in percentage terms. These quantify the treatment effect relative to the mean in the non-PSNP districts. (Table B1 in the appendix presents the unstandardized treatment effects based on Equation (1)). The estimated treatment effect on the number of violent events is negative, but not statistically different from zero ($p = 0.577$). For demonstrations, the treatment effect is negative and statistically significant ($p < 0.01$). The estimated treatment effect is -0.026 (95% CI: $-0.040; -0.012$), indicating that the PSNP reduces the likelihood of a demonstration by 2.6 percentage points (column 2 of Table B1). Compared to the mean probability of a demonstration in non-PSNP districts during the study period (1997-2019), this translates into a 47 percent reduction in the probability of a protest (Figure 3). The corresponding treatment effect for fatality risk is also negative, but considerably smaller and not statistically different from zero ($p = 0.168$).

The ACLED provides the fatalities occurring at each violent event or demonstration. In Figure B1 in the appendix, we present a disaggregated analysis of fatalities by event. These regression results suggest that the PSNP reduced the likelihood that a fatality at a demonstration occurred by 52 percent ($p = 0.01$), however, it did not affect the likelihood that a fatality at a violent event occurred ($p = 0.973$). While this sub-analysis is indicative

Figure 3: Impact of PSNP on conflict risk, by conflict type

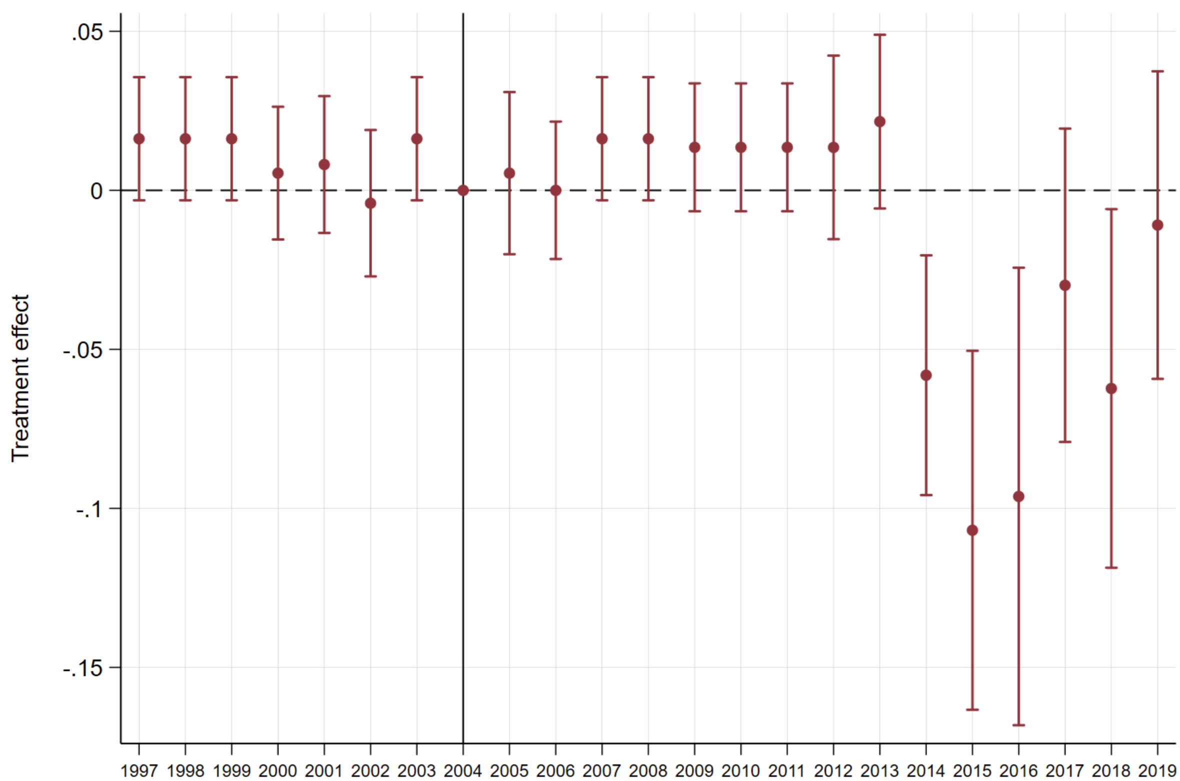


Note: 14,191 observations. Difference in differences estimates based on two-way fixed effects. The outcome variable is binary (0/1), obtaining value 1 if a violent event / demonstration / fatality occurred in the district in the given year. Solid dots mark the treatment effects relative to the control mean and capped bars are the corresponding 95%–confidence intervals based on standard errors clustered at the district level.

of the effect of the PSNP on reducing fatalities at demonstrations, it is important to note that this result is based on relatively small cell sizes. Fatalities from violent events and demonstrations in control areas were reported in 3.7 and 1.9 percent of districts per year, respectively. Because these are rare events, we interpret them in a cautious manner.

After finding an overall reduction in the likelihood of demonstrations due to the PSNP, we next seek to understand the dynamics of how the likelihood of demonstrations evolved since the PSNP began in 2005. Figure 4 shows the plot of the event study based on the estimation of Equation (2) when the outcome variable is a binary variable that captures demonstrations. The treatment effects during the pre-protest period (2005-2013) are small and not statistically different from zero. However, the treatment effects are all negative in

Figure 4: Impact of PSNP on likelihood of a demonstration, by year

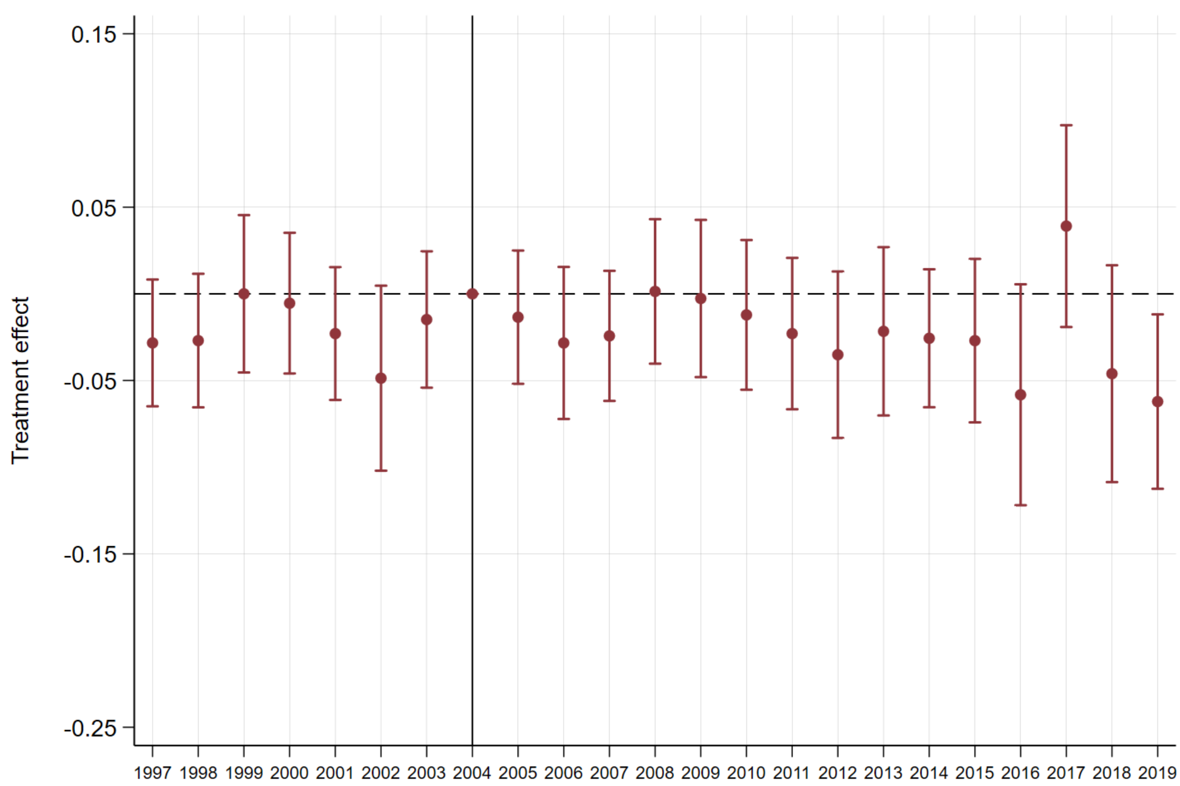


Note: 14,191 observations. Difference in differences event study plot based on two-way fixed effects. PSNP was launched in 2005. The outcome variable is binary, obtaining value 1 if a demonstration occurred in the district in the given year. Solid dots mark the estimated coefficients and vertical capped bars are 95%–confidence intervals based on standard errors clustered at the district level.

the protest period (2014-2018) and mostly statistically significant. The magnitudes of these effects are sizable. For example, in 2014, the average control district had a 10.3 percent likelihood of experiencing a demonstration. Meanwhile, the impact estimate for 2014 is a reduction of 5.8 percentage points, implying that the PSNP reduced the likelihood of a demonstration by 56 percent.

Figure 5 shows the corresponding event-study plot for violent events. In line with the results reported in Figure 3, none of the coefficients appear statistically significant at the 5%-critical level. Figure B2 in the appendix shows the event study plot for fatalities. Apart from 2017, the treatment effects are trending negative during the protest period but are mostly not statistically different from zero.

Figure 5: Impact of PSNP on likelihood of a violent event, by year



Note: Event study plot. PSNP was launched in 2005. The outcome variable is binary, obtaining value 1 if a violent event occurred in the district in the given year. Solid dots mark the estimated coefficients and vertical capped bars are 95% confidence intervals based on standard errors clustered at the district level.

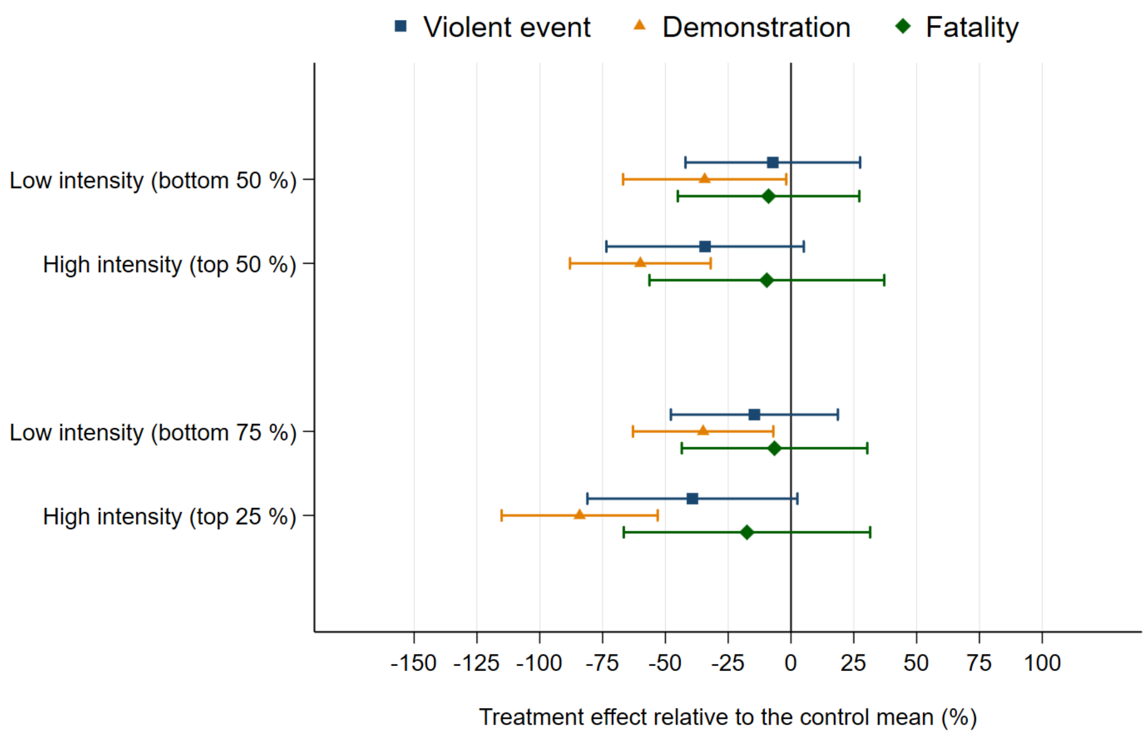
Finally, we explore whether the program's impact varied depending on the intensity of program participation. To do this, we calculate the share of PSNP beneficiaries in each district relative to the district's population at the start of the PSNP in 2005. Based on this measure we divide districts into high and low caseload intensity groups. We then use these binary indicators (high/low intensity) to replace the original treatment variable and re-run the regression analysis. We do this using both the median and the top 25-percent of the caseload shares as cutoff thresholds. Figure 6 shows that the estimated treatment effects are generally stronger in districts with higher caseload intensity, particularly when using the top 25-percent threshold. However, as before, only the treatment effects on demonstrations are statistically different from zero. This finding strengthens our confidence that the program itself, not some other unobserved factor, is driving the results, as the impact is more pronounced in districts with a greater intensity of program participation.

6 Robustness

We assess the robustness of these findings in several ways. First, the key identifying assumption is that, in the absence of PSNP, the average conflict risk among the PSNP and non-PSNP districts would have followed parallel trends. Following standard practice in the literature (Roth et al., 2023), we show and statistically test for differences in trends prior to the launch of the PSNP in 2005. For all three outcomes, we cannot reject the null hypothesis that the two groups were on a parallel trend prior to 2005. However, as shown in Figure 1a, the 1997-2004 period was characterized with a relatively low number of violent events and demonstrations, possibly making the assessment of pre-trends somewhat problematic. Therefore, we check if our results are robust to using a matching approach to construct treatment and control groups that are similar prior to the launch of the PSNP.[18] To this end, we used a propensity score matching algorithm (Rosenbaum and Rubin, 1983) to match the PSNP and non-PSNP districts based on their pre-program characteristics that

[18] For more details about the matching approach, see appendix C.

Figure 6: Estimates by PSNP caseload intensity



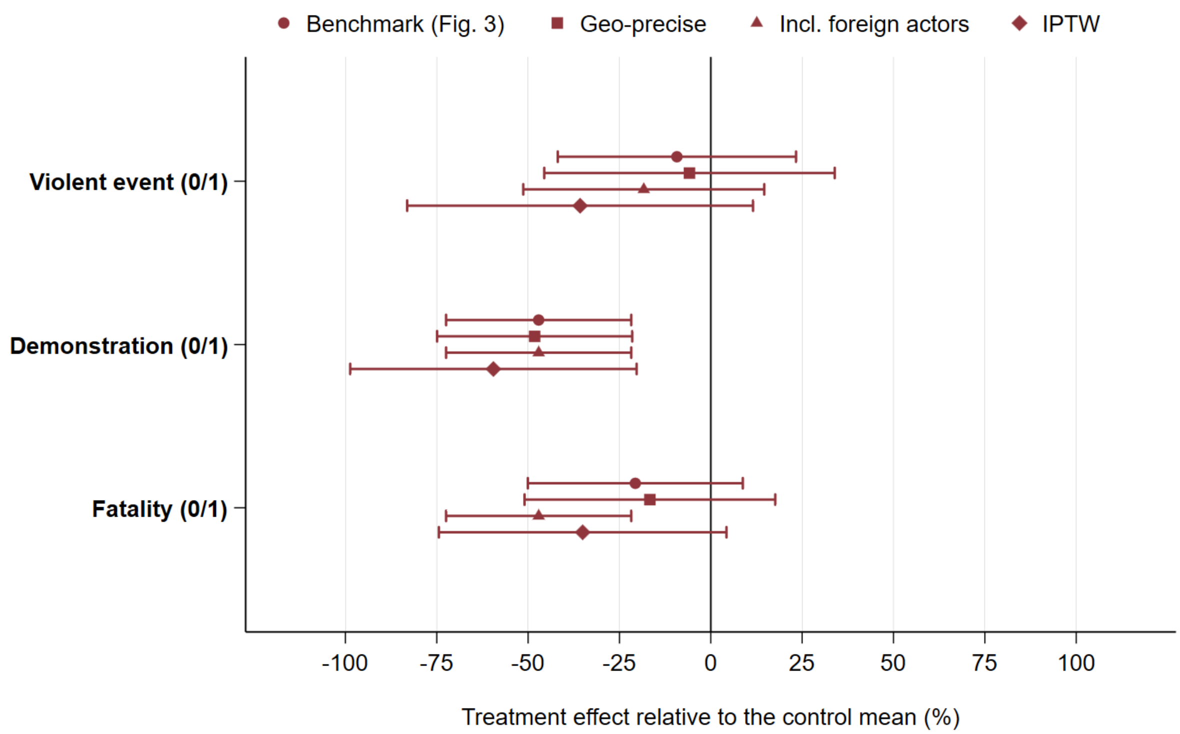
Note: N = 14,191 (23 years × 617 districts). Solid dots mark the treatment effects relative to the control mean and capped bars are the corresponding 95%–confidence intervals based on standard errors clustered at the district level. Caseload intensity is based on the share of PSNP beneficiaries in each district relative to the district's population in 2005.

predict districts' inclusion into the program (Hirvonen et al., 2022; World Bank, 2020a) using several variables. These include the mean normalized difference vegetation index (NDVI) in 2000-2004 and its squared term; the mean and standard deviation of annual rainfall in 1995-2004 from the Climate Hazards Group InfraRed Precipitation with Station (CHIRPS) (Funk et al., 2015) (Figure C1, panel B); mean elevation from the Shuttle Radar Topography Mission (SRTM), a global digital elevation model (DEM) of the world (USGS EROS, 1996) from which we also derived the slope (Figure C1, panel D); and mean population density per km^2 in 2005 from the Gridded Population of the World, 2005 (GPW) (CIESIN, 2016) (Figure C1, panel C).

We then use these district-level propensity scores (PS) to calculate inverse probability treatment weights (IPTW) (Abadie, 2005; Joffe et al., 2004): 1/PS for the treated (PSNP) districts and 1/(1-PS) for the untreated (non-PSNP) districts. We define the area of common support (Figure C2) as districts for which the estimated propensity score is within the interval [0.1; 0.9] (Crump et al., 2009). Restricting the data to districts within this common support yields a subset of 313 districts, primarily located just inside and outside of the 'PSNP boundary' (see Figure C3). Using this subset (N = 7,199; 313 districts $\times$ 23 years) and applying the inverse probability treatment weights to estimate Equation (1), we find that the results remain robust (Figure 7).

Second, a related concern is that the two groups of districts may be subject to different shocks during the treatment period. The primary income shock in this agro-pastoralist context (with negligible access to irrigation) is related to drought. We therefore verify that the results are robust to controlling for weather conditions using different data sources and methods to construct our weather variables. First, we used the CHIRPS (version 2) annual rainfall data with 0.05 resolution (Funk et al., 2015) and aggregated the rainfall data to the district level by taking its mean within each district. We use these data to construct an annual rainfall Z-score variable with zero mean and SD of one. Negative Z-score values indicate that the annual rainfall was below the long-term mean in the district. Second, we

Figure 7: Impact of PSNP on likelihood of a conflict, only considering highest geo-precision events, including foreign actors, or applying inverse probability treatment weighting (IPTW) method



Note: N = 14,191 (23 years × 617 districts), except for IPTW, N = 7,199 (23 years × 313 districts), after restricting the data to common support. Solid dots mark the treatment effects relative to the control mean and capped bars are the corresponding 95%–confidence intervals based on standard errors clustered at the district level. 'Benchmark (Fig. 3)': reproduces the estimates in Figure 3; 'Geo-precise': only includes ACLED events coded with the highest geo-precision marker; 'Incl. foreign actors': includes ACLED events involving both domestic and foreign actors; 'IPTW': estimates based on inverse probability treatment weighting method.

use the the 12-month lag SPEI (SPEI-12) (Vicente-Serrano et al., 2010) for each district at the end of each December, to account for district specific changes in the climatic water balance from the long-run average during the whole calendar year. Our findings are robust to controlling for contemporaneous and lagged rainfall and drought conditions, see Tables D1 and D2.

Third, the use of geocoded conflict data may create non-negligible spatial dependencies across districts, in which case using standard errors clustered at the district level may not be valid. To address this, we computed Conley (1999) standard errors that are robust to both spatial autocorrelation and heteroskedasticity. The Conley approach is based on a weighting matrix that places more weight on observations located closer to each other. Using district centroid coordinates and being agnostic of the appropriate distance where correlation between points becomes negligible, we experimented with distance cut-offs between 100 and 1,000 kilometers at 100 kilometer intervals. Figure D1 shows that the p-values remain below the 5% critical value across all distance cut-offs when the outcome variable is a binary variable capturing demonstrations.

Fourth, the results are robust to various re-configurations of the outcome variables such as using a continuous count of conflict events that occurred rather than a binary measure of whether a conflict event occurred (Table D3), only considering conflict events coded with the highest geo-precision marker (Figure 7), and including events involving foreign actors (Figure 7). Though note that in this last analysis the likelihood of fatalities is negative and statistically significantly different than zero ($p<0.01$).

Finally, our results are not driven by one particular calendar year, or region. The estimated effects remain stable when we omit one year at a time (Figures D2, D3, D4), or one region at a time (Figure D5) from the data set.

7 Potential mechanisms

In this section, we examine three hypotheses of potential mechanisms to explain how participation in the PSNP reduced the likelihood of demonstrations. First, we assess whether the time required to participate in public works makes households too busy to participate in demonstrations. Second, we investigate whether weather shocks predict civil unrest and, if so, whether the PSNP weakens this relationship, similar to the mechanism found in Fetzer (2020). Third, we explore whether PSNP households are more content with the government, which could explain why individuals in PSNP districts were less likely to participate in demonstrations.

7.1 Are PSNP households too busy to take part in demonstrations?

The PSNP public works are intentionally scheduled outside of the cropping season, and PSNP communities in the highland regions typically conduct public works projects between January and June. Using ACLED data, which includes the calendar month of each event, we can assess treatment effects by month. If participation in public works were influencing the observed patterns, we would expect to see the negative impacts on demonstrations concentrated during the public works season when participants are occupied with public works activities. However, our analysis does not show clear patterns (Figure E1), suggesting that work commitments during this period are not responsible for the observed decrease in the likelihood of demonstrations.

7.2 Does PSNP weaken the weather-civil unrest link?

Building on Fetzer (2020), we explore whether civil unrest escalates during or after weather shocks and if the PSNP attenuates this relationship. We model demonstration risk at the district level as a function of rainfall (or drought) shocks, the presence of the PSNP, and the interaction between rainfall shocks and the PSNP indicator variable. For more details, see

Appendix E.2. The results, presented in Table E1, indicate that the estimated impacts of rainfall and drought shocks on the likelihood of demonstrations do not differ meaningfully between PSNP and non-PSNP districts. This suggests the PSNP does not modify the link between weather shocks and civil unrest.

7.3 Are PSNP households more content with the government?

Our final testable hypothesis is whether the PSNP fostered greater sympathy and satisfaction toward the ruling party (the EPRDF), making PSNP households less likely to engage in demonstrations. To explore this hypothesis, we turn to the Ethiopian Rural Household Survey (ERHS), a longitudinal household survey conducted in 21 villages across the four highland regions between 1989 and 2009 (the survey was discontinued after 2009). For more details about the ERHS, see appendix section A.2. The final two survey rounds were conducted in 2004 and 2009 and asked respondents, whether they agreed or disagreed with the statement: 'I believe that the government does what is right for the people'. We use the household heads' responses to this question to assess whether households participating in the PSNP are more likely to agree with this statement than non-PSNP households. If they are, it suggests that PSNP households are more content with the government than other households. However, an important concern with this reasoning is that PSNP households may be inclined to respond more positively to questions about the government out of fear of losing benefits, especially if they doubt the confidentiality of the survey.[19] To address this concern, we compare the results against responses to an additional statement that focuses on the ability (rather than intentions) of the federal government: 'I am confident in the ability of government officials to do their job.'. A respondent motivated by fear would likely respond positively to both questions, while only responding positively to the first statement ('government does what is right for the people') would indicate increased contentment rather

[19] This is a plausible hypothesis given that the household-level targeting of the PSNP operates at the community level and the relevant community level task forces and appeals committees have traditionally been closely connected to the ruling party (Lavers, 2021; Cochrane and Tamiru, 2016).

than a response motivated by fear.

We create a binary indicator obtaining value 1 if the respondent agreed or strongly agreed with the statement and zero otherwise. Using an ANCOVA specification, we then regress this binary variable $T_{i,t=1}$ observed in 2009 (after the PSNP was launched) against an indicator variable capturing PSNP households in 2009:

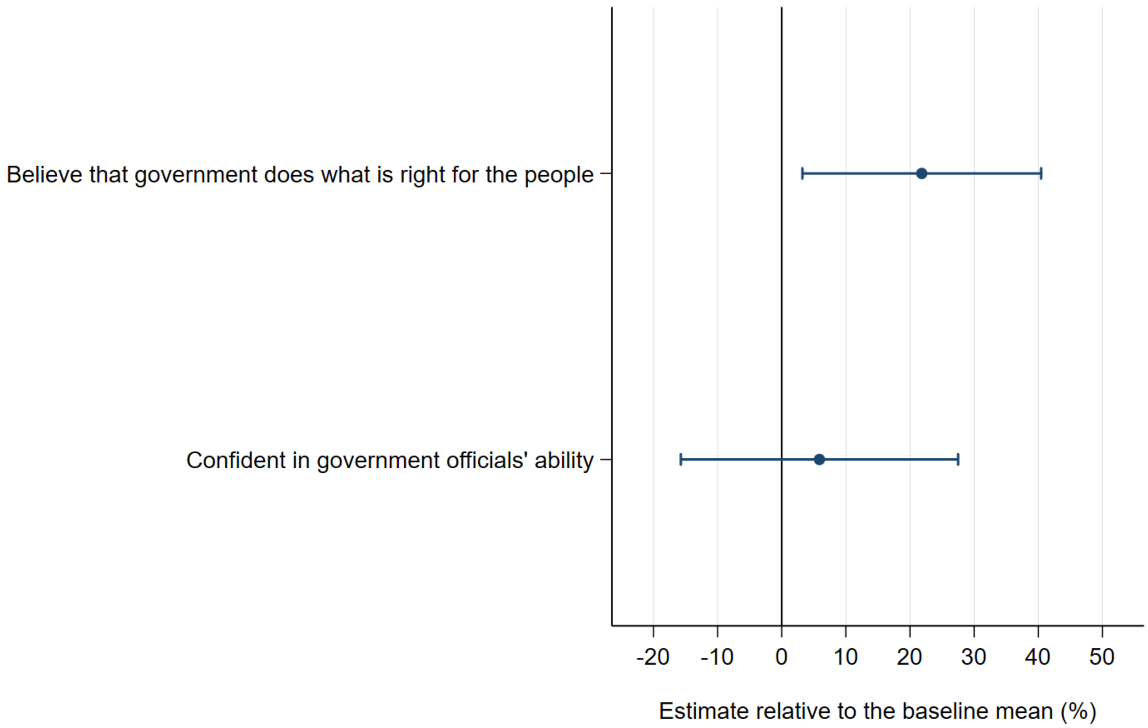
$$T_{i,t=1} = \pi(PSNP_{i,t=1}) + \zeta(T_{i,t=0}) + \gamma(X_{i,t=0,1}) + \epsilon_{i,t=1}, \tag{3}$$

where $PSNP_{i,t=1}$ obtains value 1 if the household participates in the PSNP in year 2009, and zero otherwise. $T_{t=0}$ is the outcome variable observed in 2004 (before the PSNP was launched), vector X contains the control variables, including the characteristics of the respondent observed in 2009 when the question was asked: age, sex, and level of education. Other control variables include key household characteristics observed in 2004: log per capita consumption and household size, as well as binary variables that capture administrative regions. The unit of observation in these regressions is the household observed in 2009. Moreover, PSNP participation is defined at the household level and, therefore, we do not cluster standard errors but adjust them for heteroskedasticity.

Estimating Eq. 3, we find that PSNP households are 8.7 percentage points more likely to agree with the statement "I believe that the government does what is right for the people" than other households ($p < 0.05$), after controlling for their response prior to the launch of the PSNP in 2004 and household level controls (Table E2). This corresponds to a 22 percent increase compared to the 2004 responses (Figure 8). In contrast, the corresponding PSNP estimate for the statement about the government's ability is smaller in magnitude and not statistically significant.[20] We cannot say definitively whether this increase is attributable to households' enrollment in the PSNP since the 2004 EHRS round or to other (unmeasured) time-varying factors correlated with their 2009 PSNP beneficiary status. The absence of pre-

[20]Re-estimating Eq. 3 without controls or excluding households for which the 2004 response is missing yields very similar results; see Panels B and C in Table E2.

Figure 8: Are PSNP households more likely to agree with statements about the government's intentions and ability?



Note: N = 1,543 households in the Ethiopian Rural Household Survey (EHRS). ANCOVA estimates. Capped lines represent 95%-confidence intervals.

2004 trust data prevents an assessment of pretreatment trends, which would help evaluate whether the parallel trend assumption holds in this context. Another limitation of this analysis is the lack of data during the 2014-2018 protest period.[21]

Despite these caveats, these estimates indicate that, between 2004 and 2009, trust in the government rose more among PSNP households compared to non-PSNP households. This suggests a mechanism by which PSNP recipient households were more satisfied with the ruling party and therefore less likely to participate in demonstrations (similar to mechanisms in the political economy literature linking participants of conditional cash transfer programs

[21] We are not aware of any Ethiopian longitudinal or repeated cross-sectional surveys that included questions about trust in the government and covered both the period before the launch of the PSNP and the 2014-2018 protest period. The Young Lives longitudinal survey, which currently spans from 2002 to 2021, asked similar questions in 2006 (Round 2) and 2009 (Round 3), but not in earlier or later rounds.

with higher levels of support for incumbent governments in Latin America (Manacorda et al., 2011; Zucco Jr, 2013; De La O, 2013)).

8 Conclusions

Over the past two decades, the prevalence of violent conflict and civil unrest has increased, particularly in Africa—a continent burdened with high levels of extreme poverty and slow progress in its fragile and conflict-affected areas. This situation prompts a crucial question: Can social assistance programs foster peace and break the cycle of poverty in these challenging settings?

We address this question by assessing the impact of Ethiopia's flagship safety net program, the Productive Safety Net Program (PSNP), on conflict dynamics within the country. While we cannot reject the null hypothesis that the PSNP has no effect on the likelihood of violent conflict, we find that it has reduced the likelihood of civil unrest during the widespread protest wave against the federal government in 2014-2018. We examine a handful of candidate mechanism explanations related to how the PSNP mitigates the likelihood of civil unrest, and find the most likely mechanism to be increased participant satisfaction with the government.

There are two potential channels through which the PSNP could foster greater trust in the government, though we cannot distinguish between them. First, government-led programs like the PSNP can reduce economic marginalization and contribute to a stronger social contract between the government and citizens. This could increase trust in the government as beneficiaries feel more supported and less economically insecure, a finding consistent with studies on cash transfer programs in Latin America (Manacorda et al., 2011; Zucco Jr, 2013; De La O, 2013). Second, the fact that the PSNP's public works are selected and planned by the communities themselves might also strengthen trust in the government. This community-driven approach makes development projects more responsive to local needs, which has been shown to increase satisfaction with the government and its legitimacy in other contexts (Olken, 2010; Parks et al., 2019).

Our findings contribute to the ongoing debate regarding the relationship between social assistance programs and conflict dynamics. Existing empirical literature on this topic has produced mixed results on the impact of social assistance on violent conflict – sometimes showing increases and other times reductions in violent conflict. Our study adds a null result to this body of literature and extends it by analyzing the impact of social assistance on civil unrest. Our findings suggest that government-led transfer programs can help alleviate economic marginalization and thereby contribute to long-term stability.

References

Abadie, A. (2005). Semiparametric difference-in-differences estimators. *The Review of Economic Studies*, 72(1):1–19.

Abadie, A., Athey, S., Imbens, G. W., and Wooldridge, J. M. (2023). When should you adjust standard errors for clustering? *The Quarterly Journal of Economics*, 138(1):1–35.

Abbink, J. (2016). Ethiopia's unrest sparked by unequal development record.

ACLED (2023). *Armed Conflict Location and Event Data Project (ACLED) Codebook*. ACLED.

Aguero, J. M. and Fasola, E. (2022). Distributional policies and social cohesion in a high-unemployment setting.

BBC News (2016). Oromo festival deaths: At least 50 killed in ethiopia stampede. Accessed: 2024-09-13.

Beath, A., Christia, F., and Enikolopov, R. (2013). Winning hearts and minds through development: Evidence from a field experiment in afghanistan. In *APSA 2013 Annual Meeting Paper, American Political Science Association 2013 Annual Meeting*.

Beegle, K., Christiaensen, L., Dabalen, A., and Gaddis, I. (2018). *Poverty in a Rising Africa*. World Bank, Washington, D.C.

Bellemare, M. F. (2015). Rising food prices, food price volatility, and social unrest. *American Journal of Agricultural Economics*, 97(1):1–21.

Berhane, G., Gilligan, D. O., Hoddinott, J., Kumar, N., and Taffesse, A. S. (2014). Can social protection work in Africa? The impact of Ethiopia's Productive Safety Net Programme. *Economic Development and Cultural Change*, 63(1):1–26.

Berhane, G., Golan, J., Hirvonen, K., Hoddinott, J. F., Kim, S. S., Taffesse, A. S., Abay, K., Assefa, T. W., Habte, Y., Abay, M. H., et al. (2020). *Evaluation of the nutrition-sensitive features of the fourth phase of Ethiopia's Productive Safety Net Programme*, volume 140. Intl Food Policy Res Inst.

Berman, E., Shapiro, J. N., and Felter, J. H. (2011). Can hearts and minds be bought? The economics of counterinsurgency in Iraq. *Journal of Political Economy*, 119(4):766–819.

Braithwaite, A., Kucik, J., and Maves, J. (2014). The costs of domestic political unrest. *International Studies Quarterly*, 58(3):489–500.

Cantoni, D., Kao, A., Yang, D. Y., and Yuchtman, N. (2024). Protests. *Annual Review of Economics*, 16.

Casey, K., Glennerster, R., Miguel, E., and Voors, M. (2023). Long-Run Effects of Aid: Forecasts and Evidence from Sierra Leone. *The Economic Journal*, 133(652):1348–1370.

Chabé-Ferret, S. (2017). Should we combine difference in differences with conditioning on pre-treatment outcomes? Working paper. Retrieved from https://www.tse-fr.eu/sites/default/files/TSE/documents/doc/wp/2017/wp_tse_824.pdf.

CIESIN (2016). Gridded population of the world, version 4 (GPWv4): Administrative unit center points with population estimates. Center for International Earth Science Information Network, Columbia University, Palisades, NY: NASA Socioeconomic Data and Applications Center (SEDAC). http://dx.doi.org/10.7927/H4F47M2C.

Clarke, D. and Tapia-Schythe, K. (2021). Implementing the panel event study. *Stata Journal*, 21(4):853–884.

Cochrane, L. and Tamiru, Y. (2016). Ethiopia's productive safety net program: power, politics and practice. *Journal of International Development*, 28(5):649–665.

Coll-Black, S., Gilligan, D. O., Hoddinott, J., Kumar, N., Taffesse, A. S., Wiseman, W., et al. (2011). Targeting Food Security Interventions When "Everyone is Poor": The Case of Ethiopia's Productive Safety Net Programme. *ESSP II Working*, 24.

Collier, P. and Hoeffler, A. (1998). On economic causes of civil war. *Oxford Economic Papers*, 50(4):563–573.

Conley, T. G. (1999). GMM estimation with cross sectional dependence. *Journal of Econometrics*, 92(1):1–45.

Crost, B., Felter, J., and Johnston, P. (2014). Aid under fire: Development projects and civil conflict. *American Economic Review*, 104(6):1833–1856.

Crost, B., Felter, J. H., and Johnston, P. B. (2016). Conditional cash transfers, civil conflict and insurgent influence: Experimental evidence from the Philippines. *Journal of Development Economics*, 118:171–182.

Crump, R. K., Hotz, V. J., Imbens, G. W., and Mitnik, O. A. (2009). Dealing with limited overlap in estimation of average treatment effects. *Biometrika*, 96(1):187–199.

De Juan, A. and Bank, A. (2015). The ba 'athist blackout? selective goods provision and political violence in the syrian civil war. *Journal of Peace Research*, 52(1):91–104.

De La O, A. L. (2013). Do conditional cash transfers affect electoral behavior? Evidence from a randomized experiment in Mexico. *American Journal of Political Science*, 57(1):1–14.

De Waal, A. (2017). *Mass starvation: The history and future of famine*. John Wiley & Sons.

Devarajan, S. and Ianchovichina, E. (2018). A broken social contract, not high inequality, led to the Arab Spring. *Review of Income and Wealth*, 64:S5–S25.

Dias, A. M. and Yetena, Y. D. (2022). Anatomies of protest and the trajectories of the actors at play: Ethiopia 2015–2018. In *Popular protest, political opportunities, and change in Africa*, pages 181–199. Routledge.

Didan, K. (2015). MOD13A3 MODIS/Terra vegetation Indices Monthly L3 Global 1km SIN Grid V006 Normalized Difference Vegetation Index (NDVI). NASA EOSDIS Land Processes DAAC. Accessed 2020-08-01 from `https://doi.org/10.5067/MODIS/MOD13A3.006`.

FAO (2022). The role of social protection in the recovery from covid-19 impacts in fragile and conflict-affected settings. Food and Agriculture Organization of the United Nations (FAO).

Fetzer, T. (2020). Can workfare programs moderate conflict? Evidence from India. *Journal of the European Economic Association*, 18(6):3337–3375.

Fiala, N., Rose, J., Aryemo, F., and Ankel-Peters, J. (2025). Timing matters: The (very) long-run impacts of cash grants during a crisis. *Journal of Development Economics*, page 103447.

Freedom House (2024). Country and territory ratings and statuses 1973-2024.

Funk, C., Peterson, P., Landsfeld, M., Pedreros, D., Verdin, J., Shukla, S., Husak, G., Rowland, J., Harrison, L., Hoell, A., and Michaelsen, J. (2015). The climate hazards infrared precipitation with stations—A new environmental record for monitoring extremes. *Scientific Data*, 2(1):150066.

Gazeaud, J. and Stephane, V. (2023). Productive Workfare? Evidence from Ethiopia's Productive Safety Net Program. *American Journal of Agricultural Economics*, 105(1):265–290.

Gilligan, D. O., Hoddinott, J., and Taffesse, A. S. (2009). The impact of Ethiopia's Productive Safety Net Programme and its linkages. *Journal of Development Studies*, 45(10):1684–1706.

Gurr, T. (1971). *Why men rebel*. Princeton University Press.

Hirvonen, K. and Hoddinott, J. (2021). Beneficiary views on cash and in-kind payments: Evidence from Ethiopia's Productive Safety Net Programme. *The World Bank Economic Review*, 35(2):398–413.

Hirvonen, K., Machado, E. A., Simons, A. M., and Taraz, V. (2022). More than a safety net: Ethiopia's flagship public works program increases tree cover. *Global Environmental Change*, 75:102549.

Hoddinott, J., Berhane, G., Gilligan, D. O., Hirvonen, K., Kumar, N., Lind, J., Sabates-Wheeler, R., and Taffesse, A. S. (2024). Securing food, building livelihoods? A 15-year appraisal of Ethiopia's Productive Safety Net Programme. Technical report, UNU-WIDER working paper.

Hoddinott, J., Berhane, G., Gilligan, D. O., Kumar, N., and Seyoum Taffesse, A. (2012). The impact of Ethiopia's Productive Safety Net Programme and related transfers on agricultural productivity. *Journal of African Economies*, 21(5):761–786.

Human Rights Watch (1998). *Human Rights Watch World Report 1997*, chapter Ethiopia. Human Rights Watch.

Human Rights Watch (2001). *Human Rights Watch World Report 2000*, chapter Ethiopia. Human Rights Watch.

Human Rights Watch (2014). Ethiopia: Brutal crackdown on protests. Accessed: 2024-08-26.

Human Rights Watch (2016). *Human Rights Watch World Report 2015*, chapter Ethiopia. Human Rights Watch.

Human Rights Watch (2017a). Fuel on the fire: The security force response to the 2016 irreecha cultural festival. Accessed: 2024-09-13.

Human Rights Watch (2017b). *Human Rights Watch World Report 2016*, chapter Ethiopia. Human Rights Watch.

IMF (2022). The imf strategy for fragile and conflict-affected states. International Monetary Fund (IMF).

Joffe, M. M., Ten Have, T. R., Feldman, H. I., and Kimmel, S. E. (2004). Model selection, confounder control, and marginal structural models: Review and new applications. *The American Statistician*, 58(4):272–279.

Justino, P. (2025). Revisiting the links between economic inequality and political violence: The role of social mobilization. *World Development*, 185:106820.

Justino, P. and Martorano, B. (2016). Redistribution, inequality and political participation.

Justino, P. and Martorano, B. (2018). Welfare spending and political conflict in Latin America, 1970–2010. *World Development*, 107:98–110.

Justino, P. and Martorano, B. (2019). Redistributive preferences and protests in Latin America. *Journal of Conflict Resolution*, 63(9):2128–2154.

Khanna, G. and Zimmermann, L. (2017). Guns and butter? Fighting violence with the promise of development. *Journal of Development Economics*, 124:120–141.

Knippenberg, E. and Hoddinott, J. (2017). Shocks, social protection, and resilience: Evidence from Ethiopia. Working paper. Retrieved from `https://ideas.repec.org/p/fpr/esspwp/109.html`.

Kroft, K. and Notowidigdo, M. J. (2016). Should unemployment insurance vary with the unemployment rate? theory and evidence. *The Review of Economic Studies*, 83(3):1092–1124.

Lavers, T. (2021). State infrastructural power and social transfers: the local politics of distribution and delivering 'progress' in Ethiopia. In *The politics of distributing social transfers in sub-Saharan Africa and South Asia*. Oxford University Press.

Leight, J., Hirvonen, K., and Zafar, S. (2024). The effectiveness of cash and cash plus interventions on livelihoods outcomes: Evidence from a systematic review and meta-analysis. Technical report, International Food Policy Research Institute Discussion Paper 02262.

Lind, J., Sabates-Wheeler, R., Hoddinott, J., and Taffesse, A. S. (2022). Targeting social transfers in Ethiopia's agro-pastoralist and pastoralist societies. *Development and Change*, 53(2):279–307.

Makahamadze, T. and Fikade, M. (2022). Popular protests in the amhara region and political reforms in ethiopia, 2016–2018. *Journal of Eastern African Studies*, 16(1):115–137.

Manacorda, M., Miguel, E., and Vigorito, A. (2011). Government transfers and political support. *American Economic Journal: Applied Economics*, 3(3):1–28.

McGuirk, E. and Burke, M. (2020). The economic origins of conflict in africa. *Journal of Political Economy*, 128(10):3940–3997.

McKenzie, D., Gibson, J., and Stillman, S. (2010). How important is selection? Experimental vs. non-experimental measures of the income gains from migration. *Journal of the European Economic Association*, 8(4):913–945.

Miller, D. L. (2023). An introductory guide to event study models. *Journal of Economic Perspectives*, 37(2):203–230.

MoARD (2006). Productive Safety Net Programme (PSNP) programme implementation manual (revised). Technical report, Addis Ababa: Ministry of Agriculture and Rural Development (MoARD) of Ethiopia.

OECD (2021). *Perspectives on Global Development 2021 From Protest to Progress?* OECD Publishing.

OECD (2022). States of fragility 2022. Organisation for Economic Co-operation and Development, Accessed: 2025-01-24.

Olken, B. A. (2010). Direct democracy and local public goods: Evidence from a field experiment in Indonesia. *American Political Science Review*, 104(2):243–267.

Parks, B., DiLorenzo, M., and Aboagye, D. (2019). Closing the broken feedback loop: Can responsive aid strengthen state legitimacy? Technical report, AidData Working Paper.

Ponticelli, J. and Voth, H.-J. (2020). Austerity and anarchy: Budget cuts and social unrest in Europe, 1919–2008. *Journal of Comparative Economics*, 48(1):1–19.

Premand, P. and Rohner, D. (2024). Cash and conflict: Large-scale experimental evidence from Niger. *American Economic Review: Insights*, 6(1):137–153.

Raleigh, C., Linke, R., Hegre, H., and Karlsen, J. (2010). Introducing ACLED: An armed conflict location and event dataset. *Journal of Peace Research*, 47(5):651–660.

Reporters Without Borders (2024). World press freedom index.

Rohner, D. and Thoenig, M. (2021). The elusive peace dividend of development policy: From war traps to macro complementarities. *Annual Review of Economics*, 13(1):111–131.

Rosenbaum, P. R. and Rubin, D. B. (1983). The central role of the propensity score in observational studies for causal effects. *Biometrika*, 70(1):41–55.

Roth, J., Sant'Anna, P. H., Bilinski, A., and Poe, J. (2023). What's trending in difference-in-differences? a synthesis of the recent econometrics literature. *Journal of Econometrics*, 235(2):2218–2244.

Rustad, S. A. (2024). *Conflict Trends: A Global Overview, 1946–2023*. Peace Research Institute Oslo (PRIO).

Sabates-Wheeler, R., Lind, J., and Hoddinott, J. (2013). Implementing social protection in agro-pastoralist and pastoralist areas: How local distribution structures moderate PSNP outcomes in Ethiopia. *World Development*, 50:1–12.

Simons, A. M. (2022). What is the optimal locus of control for social assistance programs? Evidence from the Productive Safety Net Program in Ethiopia. *Journal of Development Economics*, 158:102897.

Sundberg, R. and Melander, E. (2013). Introducing the ucdp georeferenced event dataset. *Journal of Peace Research*, 50(4):523–532.

USGS EROS (1996). Digital elevation - Global 30 arc-second elevation (GTOPO30). Retrieved from `https://doi.org/10.5066/F7DF6PQS`.

Vicente-Serrano, S. M., Beguería, S., and López-Moreno, J. I. (2010). A multiscalar drought index sensitive to global warming: the standardized precipitation evapotranspiration index. *Journal of Climate*, 23(7):1696–1718.

Wiseman, W., Van Domelen, J., and Coll-Black, S. (2010). Designing and implementing a rural safety net in a low income setting: Lessons learned from Ethiopia's Productive Safety Net Program 2005-2009. Report. Retrieved from `http://documents1.worldbank.org/curated/en/247601469672211732/pdf/701390ESW0P12100Net0in0a0Low0Income.pdf`, World Bank, Washington DC.

World Bank (2018). *The State of Social Safety Nets 2018*. The World Bank, Washington, D.C.

World Bank (2020a). Ethiopia poverty assessment: Harnessing continued growth for accelerated poverty reduction. Report. Retrieved from `https://doi.org/10.1596/33544`, The World Bank, Washington, D. C.

World Bank (2020b). World bank group strategy for fragility, conflict, and violence 2020–2025.

World Bank (2021). Ethiopia Country Profile. World Development Indicators Database. Retrieved from `https://data.worldbank.org/country/ethiopia?view=chart`, Accessed September 25, 2021.

World Bank (2023). *Global Economic Prospects, January 2023*. World Bank.

World Bank (2024). World Bank Open Data. Accessed: 2024-08-26.

Zucco Jr, C. (2013). When payouts pay off: Conditional cash transfers and voting behavior in Brazil 2002–10. *American Journal of Political Science*, 57(4):810–822.

Appendix

A Additional details about the data

A.1 ACLED Event Data and Spatial Data sets

Table A1: Armed Conflict Location and Event Data (ACLED) event types

General	Event Type	Sub-Event Type
Violent events:	Battles:	Armed clash
		Government regains territory
		Non-state actor overtakes territory
	Explosions/Remote violence:	Chemical weapon
		Air/drone strike
		Suicide bomb
		Shelling/artillery/missile attack
		Remote explosive/landmine/IED
		Grenade
	Violence against civilians:	Sexual violence
		Attack
		Abduction/forced disappearance
Demonstrations:	Protests:	Peaceful protest
		Protest with intervention
		Excessive force against protesters
	Riots:	Violent demonstration
		Mob violence
Non-violent actions:	Strategic developments:	Agreement
		Arrests
		Change to group/activity
		Disrupted weapons use
		Headquarters or base established
		Looting/property destruction
		Non-violent transfer of territory

Source: Raleigh et al. (2010).
Note: Non-violent actions are not considered in this study. We define 'civil unrest' as any event categorized as a protest or riot by ACLED.

Table A2: Spatial data sets: data source, time period used in the analysis, and spatial resolution

Variable (units)	Data product	Data Source	Time period	Native spatial resolution
Conflict events	ACLED	Raleigh et al. (2010)	1997-2019	Point data
Elevation (m)	SRTM v.3	USGS EROS (1996)	N/A	1 arc-second (~30m at equator)
Slope (degrees)	SRTM v.3	USGS EROS (1996)	N/A	1 arc-second (~30m at equator)
Population	GWP4.11	CIESIN (2016)	2005	30 arc-second (~1km at equator)
Normalized Vegetation Index	NDVI	Didan (2015)	2000-2004	0.05 degrees (~5.5km at equator)
Annual rainfall (mm)	CHIRPS v.2	Funk et al. (2015)	1996-2019	0.05 degrees (~5.5km at equator)
Standardized Precipitation Evapotranspiration Index	SPEI	Vicente-Serrano et al. (2010)	1996-2019	0.5 degrees (~55km at equator)

Note: Time-period refers to the years used in the analysis.

A.2 Ethiopia Rural Household Survey (ERHS)

The Ethiopia Rural Household Survey (ERHS) is a longitudinal survey focused on the highland regions of Ethiopia. Launched in 1989 in a small number of villages in central and southern Ethiopia, the survey expanded by 1994 to cover all four highland regions, with subsequent rounds in 1995, 1997, 1999, 2004, and 2009. In the final round, nine additional villages were included, giving a sample of 1,577 households across 21 villages.

We use data from the 2004 round (prior to the launch of the PSNP) and the 2009 round (after the launch of the PSNP), both of which included statements designed to assess trust in government actions and confidence in government officials' competence:

- "I believe that the government does what is right for the people."
- "I am confident in the ability of government officials to do their job."

Respondents selected from these options:

1. Strongly disagree
2. Disagree
3. Slightly disagree
4. Neither agree nor disagree
5. Slightly agree
6. Agree
7. Strongly agree

The first statement focuses on trust in the government's actions and decisions, implying an evaluation of the government's morality or intent. In contrast, the second statement assess the respondents' confidence in the *competence* of government officials to execute their tasks. A respondent motivated by fear may feel compelled to respond positively to all bpth statements to avoid a potential backlash. However, if a respondent only agrees with the first statement—indicating that the government is acting in the best interest of the people—it suggests a more genuine belief in the government's intentions or moral actions, rather than a fear-driven response.

In households with both a primary male and female, these questions were asked twice—once to the head (typically male) and then to the spouse (typically female). In female-headed households, the questions were only asked once, to the female head.

We use the responses from the household head. Agreement with each statement is defined in binary terms, coded as "1" if the respondent answered "Agree," or "Strongly agree".

In 2009, we have responses to these statements from 1,543 households. Of these, we successfully merge data from 1,099 households with the 2004 round, leaving 444 households without a 2004 response, primarily from the nine villages added in the 2009 round. We create a binary variable to indicate households without a 2004 response and then set the binary agreement variable to zero.

To estimate the association between trust in government and participation in the PSNP, we use an ANCOVA estimator (see the main text). We prefer ANCOVA over difference-in-differences here because ANCOVA allows us to retain the 444 observations lacking 'baseline' (2004) values for the outcome variable. We later show that our results remain robust when discarding these observations.

Table A3 presents summary statistics of the variables used in the analysis.

Table A3: Ethiopian Rural Household Survey, summary statistics

	mean	sd
Dependent variables, measured in 2009:		
Believes that government does what is right for the people (0/1)	0.623	0.485
Confidence in the ability of government officials to do their job (0/1)	0.399	0.490
Independent variables, measured in 2009:		
PSNP household (0/1)	0.217	0.412
Respondent is female (0/1)	0.296	0.456
Respondent's age in years	52.62	14.83
Respondent has no formal education (0/1)	0.673	0.469
Independent variables, measured in 2004:		
Believes that government does what is right for the people (0/1)	0.398	0.490
Response to the above statement is missing (0/1)	0.288	0.453
Confidence in the ability of government officials to do their job (0/1)	0.350	0.477
Response to the above statement is missing (0/1)	0.287	0.453
(log) household consumption per capita	4.106	0.679
Household consumption per capita missing (0/1)	0.282	0.450
Household size	5.785	2.131
Household size missing (0/1)	0.282	0.450
Tigray region (0/1)	0.096	0.295
Amhara region (0/1)	0.267	0.443
Oromia region (0/1)	0.371	0.483
SNNP region (0/1)	0.266	0.442

Note: N = 1,543 households. sd = Standard deviation, 0/1 = Binary variable.

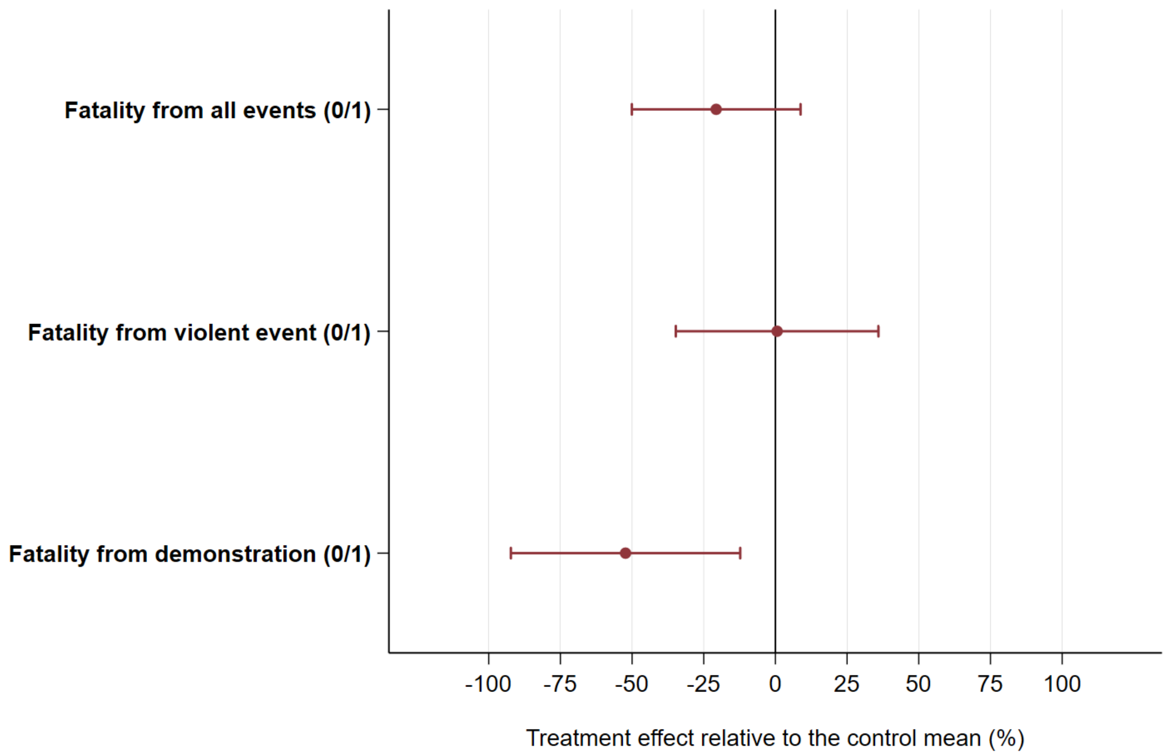
B Additional regression results

Table B1: Impact of PSNP on violent conflict, demonstrations, and fatality risk

	(1) Binary: Violent events	(2) Binary: Demonstrations	(3) Binary: Fatalities
Treatment (β)	-0.004	-0.026***	-0.010
	(0.008)	(0.007)	(0.008)
District fixed effects?	Yes	Yes	Yes
Year fixed effects?	Yes	Yes	Yes
Control mean:	0.046	0.055	0.051
Number of observations	14191	14191	14191

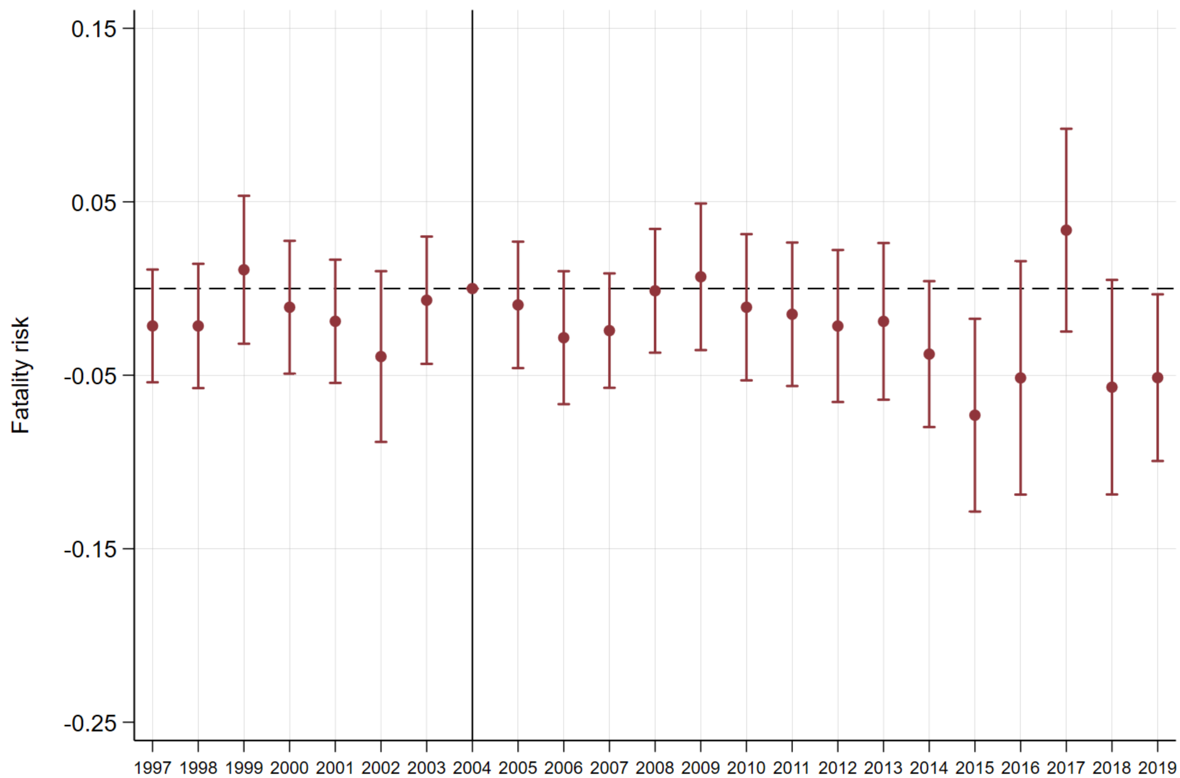
Note: OLS regression based on Equation (1). Standard errors (in parentheses) clustered at the district level. Statistical significance denoted with * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure B1: Impact of PSNP on likelihood of a fatality from all events, violent events, and demonstration



Note: The outcome variable is binary, obtaining value 1 if a fatality occurred in the district in the given year. Solid dots mark the estimated coefficients and vertical capped bars are 95% confidence intervals based on standard errors clustered at the district level.

Figure B2: Impact of PSNP on likelihood of a fatality, by year



Note: Event study plot. PSNP was launched in 2005. The outcome variable is binary, obtaining value 1 if a conflict related fatality occurred in the district in the given year. Solid dots mark the estimated coefficients and vertical capped bars are 95% confidence intervals based on standard errors clustered at the district level.

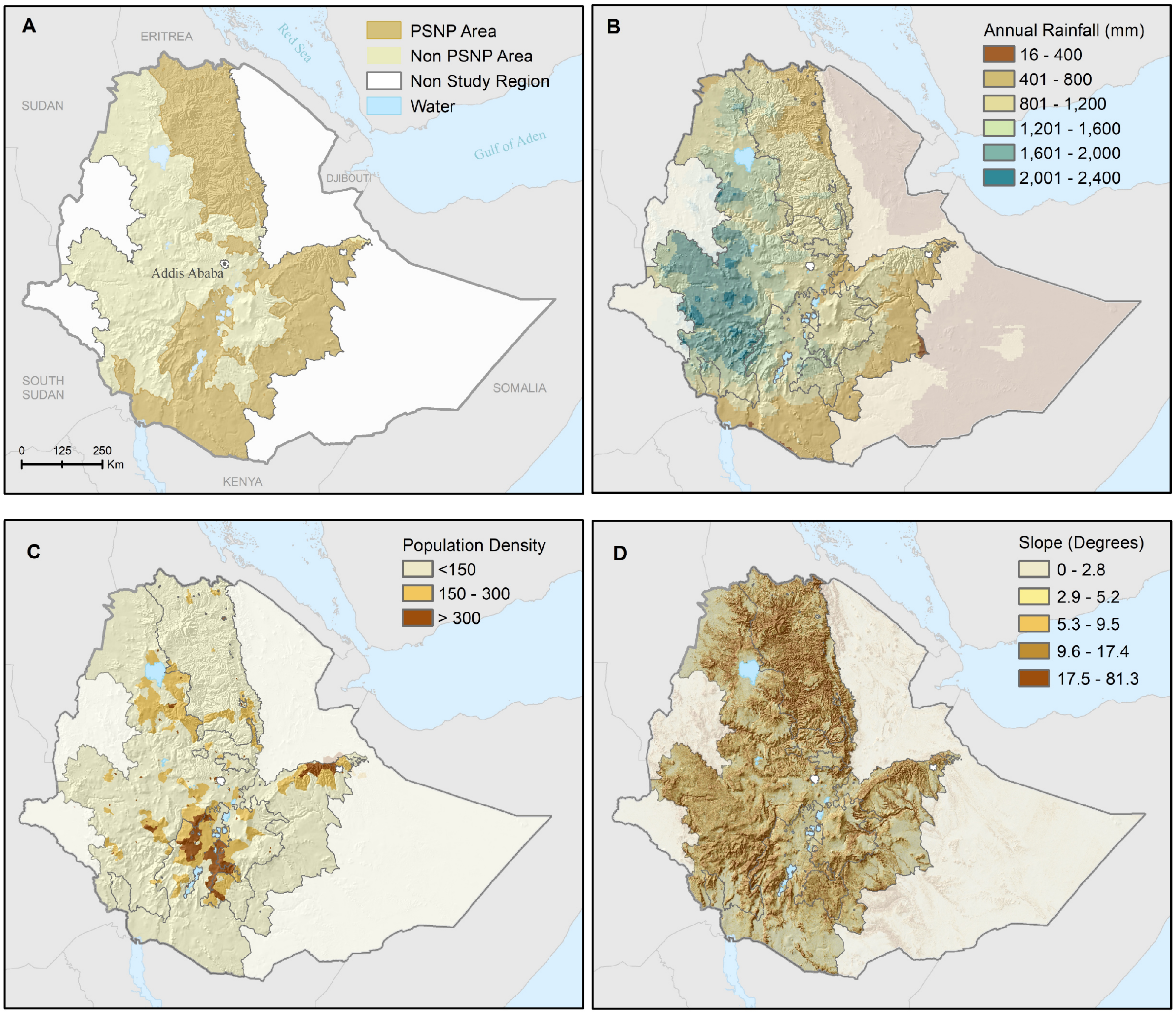
C Matching approach

We use a propensity score matching algorithm (Rosenbaum and Rubin, 1983) to match the PSNP and non-PSNP districts based on their pre-program characteristics that predict districts' inclusion into the program (Hirvonen et al., 2022; World Bank, 2020a): mean Normalized Difference Vegetation Index (NDVI) in 2000-2004 and its squared term, mean and standard deviation of annual rainfall in 1995-2004, mean elevation and slope, and mean population density in 2005.[22] Figure C1 shows maps of the spatial variables used in the propensity scores. Figure C2 shows the distribution of the propensity score for both PSNP and non-PSNP pixels. As expected, there are non-PSNP districts that received a very low score, indicating that they are very unlikely to be selected into the program based on their agro-ecological and other characteristics. Similarly, there are some PSNP districts for which the probability of selection was close to one. We defined the area of common support as pixels with the estimated propensity score within the interval [0.1; 0.9] (Crump et al., 2009). This meant discarding 304 districts. Figure C3 shows the spatial distribution of the estimated propensity scores, including the discarded and retained districts. We then used the estimated district-level propensity scores (PS) to calculate inverse probability treatment weights (IPTW) (Abadie, 2005; Joffe et al., 2004): 1/PS for the treated (PSNP) districts and 1/(1-PS) for the untreated (non- PSNP) districts. Restricting the data to the districts in the common support and applying the inverse probability treatment weights results in a balance of agroecological and other characteristics across the two groups (Table C1). Restricting the data to the districts in the common support and applying the inverse probability treatment weights in the estimation in Equation (1), we find that the results remain robust (Figure 7 and Table C2). Figures C4, C5, and C6 show the event study plots for the three outcomes. In all cases, we observe parallel pre-treatment trends. The treatment effects are highly significant for demonstration risk ($p < 0.01$), and only during the 2014-2018 period. The

[22] Matching approaches have been shown to perform well in reducing bias when combined with Difference in Differences in different contexts (Chabé-Ferret, 2017; McKenzie et al., 2010; **?**)

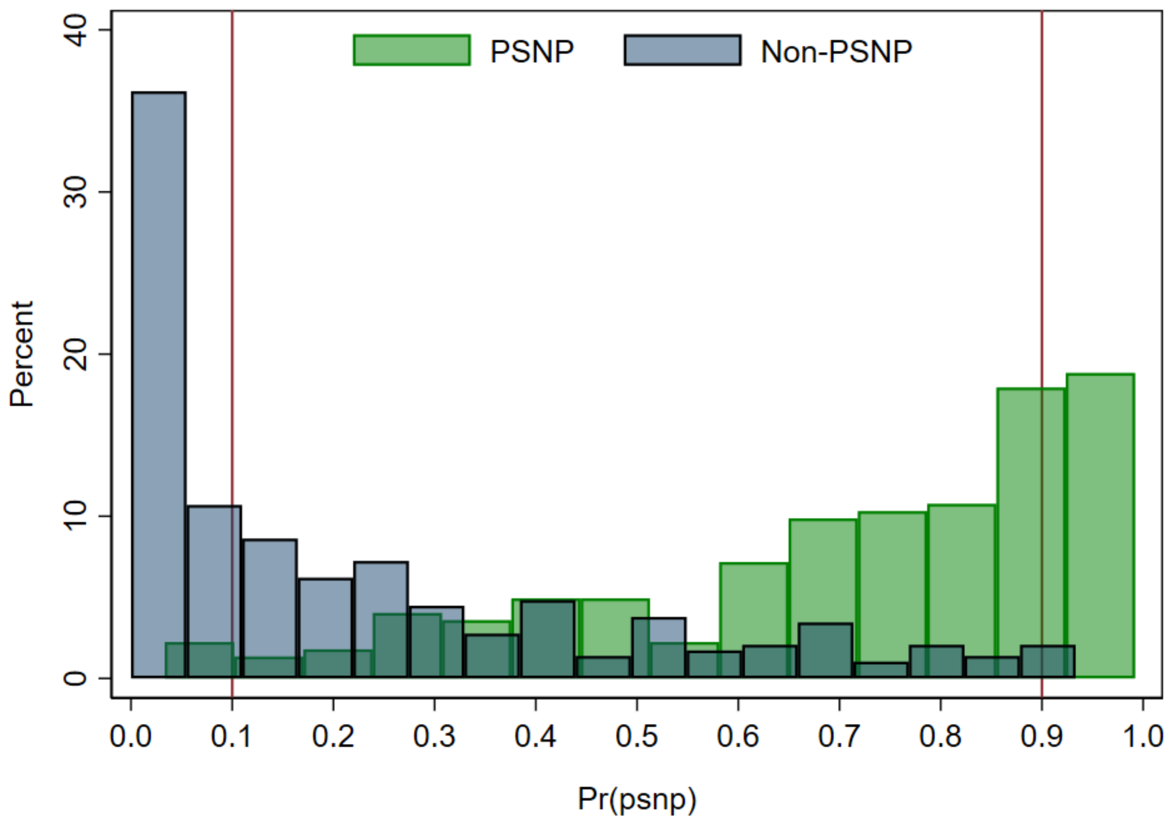
IPTW estimate on fatality risk appears significant at the 10% level.

Figure C1: Maps of study region with spatial variables used in propensity scores



Note: Map of Ethiopia showing the highland study region and spatial variables over a hill-shaded terrain. The area of the non study region has a light transparency effect applied for added context. A: Study region with the PSNP districts (boundaries not shown) in light brown and the non-PSNP districts in beige. B: Mean annual rainfall between 2005 and 2019. C: Population density, 2005. D: Terrain slope. Water bodies are only shown in the study region. See Table A2 for data sources.

Figure C2: The estimated propensity scores for all districts



Note: N = 617 districts. The area between the vertical red lines marks the area of common support as defined by Crump et al. (2009).

Table C1: Covariate balance after restricting the area to common support and applying inverse probability treatment weights

Variable	(1) Non-PSNP Mean/(SE)	(2) PSNP Mean/(SE)	(1)-(2) Pairwise t-test Mean difference
Mean annual NDVI, 2000-2004	0.489 (0.010)	0.498 (0.010)	-0.009
Mean rainfall (mm), 1995–2004	1027.73 (13.497)	1065.15 (31.934)	-37.42
Average population density (people per Sq. km)	170.97 (15.092)	179.07 (15.830)	-8.10
Average elevation (meters)	1952.74 (40.828)	1871.57 (47.507)	81.17
Average slope value (degrees)	10.929 (0.447)	11.483 (0.515)	-0.554
Number of observations (districts)	153	160	313

Note: NDVI = Normalized difference vegetation index. Mean values with standard errors in parentheses. The value displayed for t-tests are the differences in the means across the two groups. Observations are weighted using inverse probability treatment weights. Statistical significance of the t-test (last column) denoted at *** p<0.01, ** p<0.05, * p<0.10.

Table C2: Impact of PSNP on violent conflict, demonstrations, and fatality risk, estimates based on IPTW

	(1) Binary: Violent events	(2) Binary: Demonstrations	(3) Binary: Fatalities
Treatment (β)	-0.016 (0.011)	-0.033*** (0.011)	-0.018* (0.010)
Observations	7199	7199	7199

Note: Inverse probability of treatment weighting (IPTW) regression based on Equation (1). Standard errors (in parentheses) clustered at the district level. Statistical significance denoted with * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure C3: Spatial distribution of propensity scores

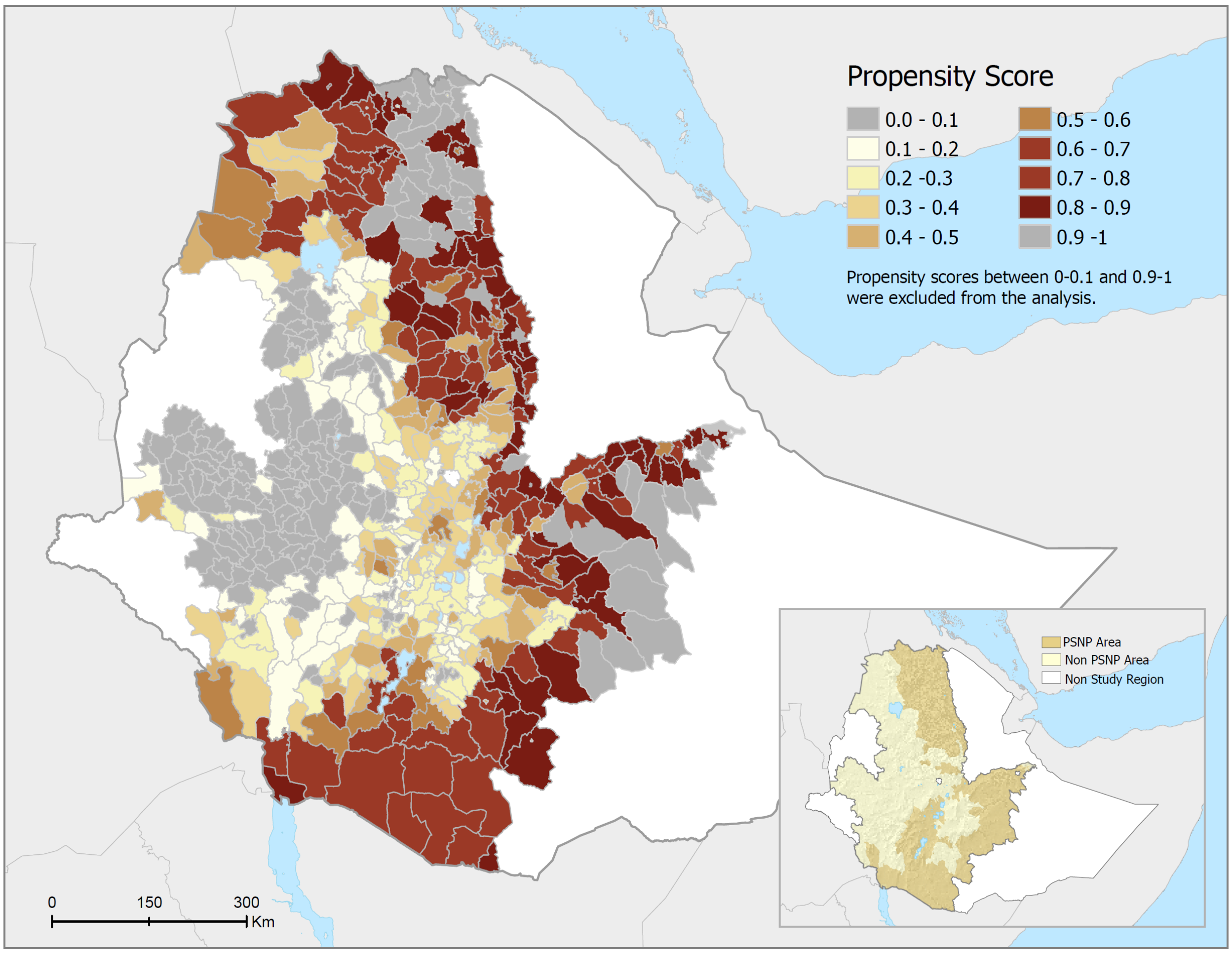
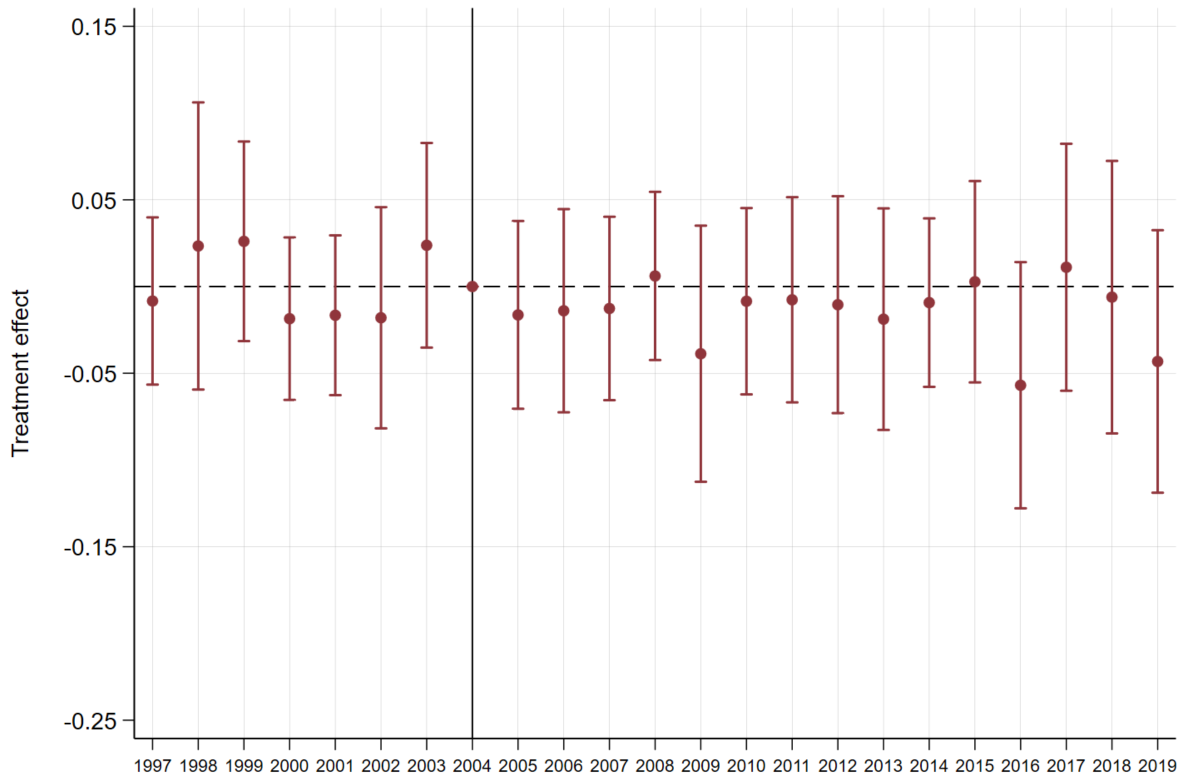
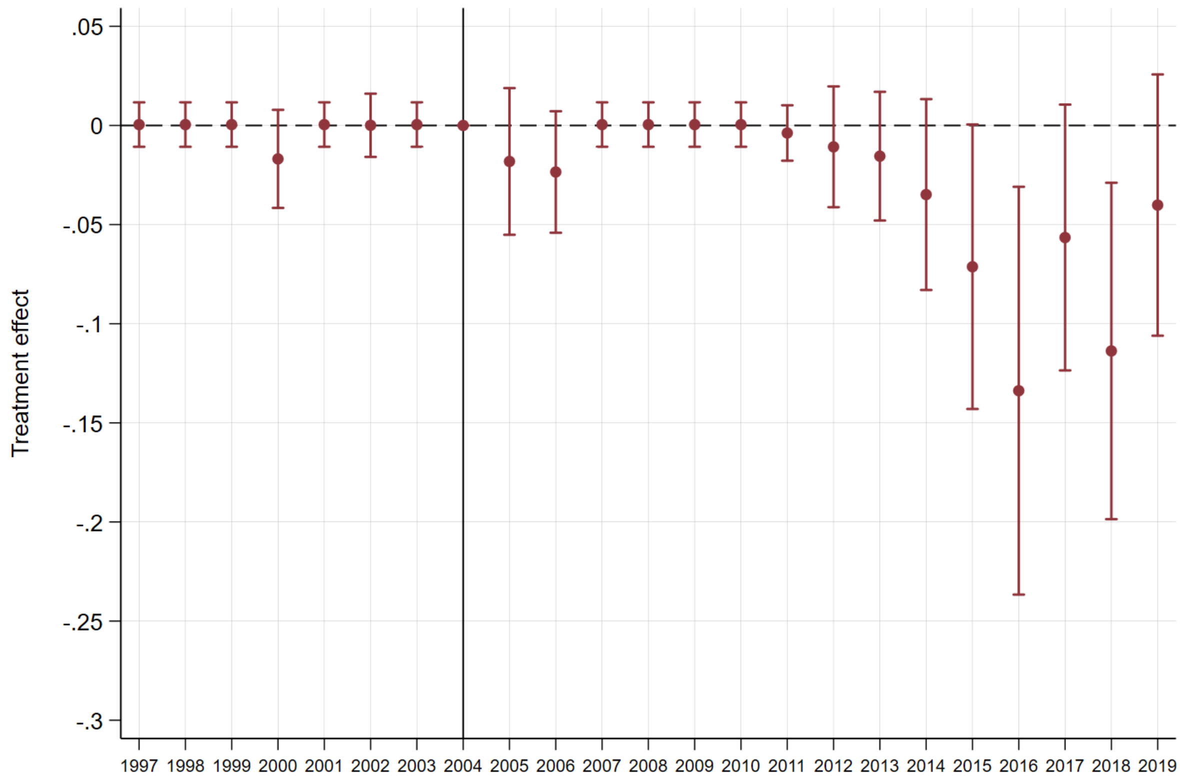


Figure C4: Impact of PSNP on likelihood of a violent event (IPTW estimates), by year



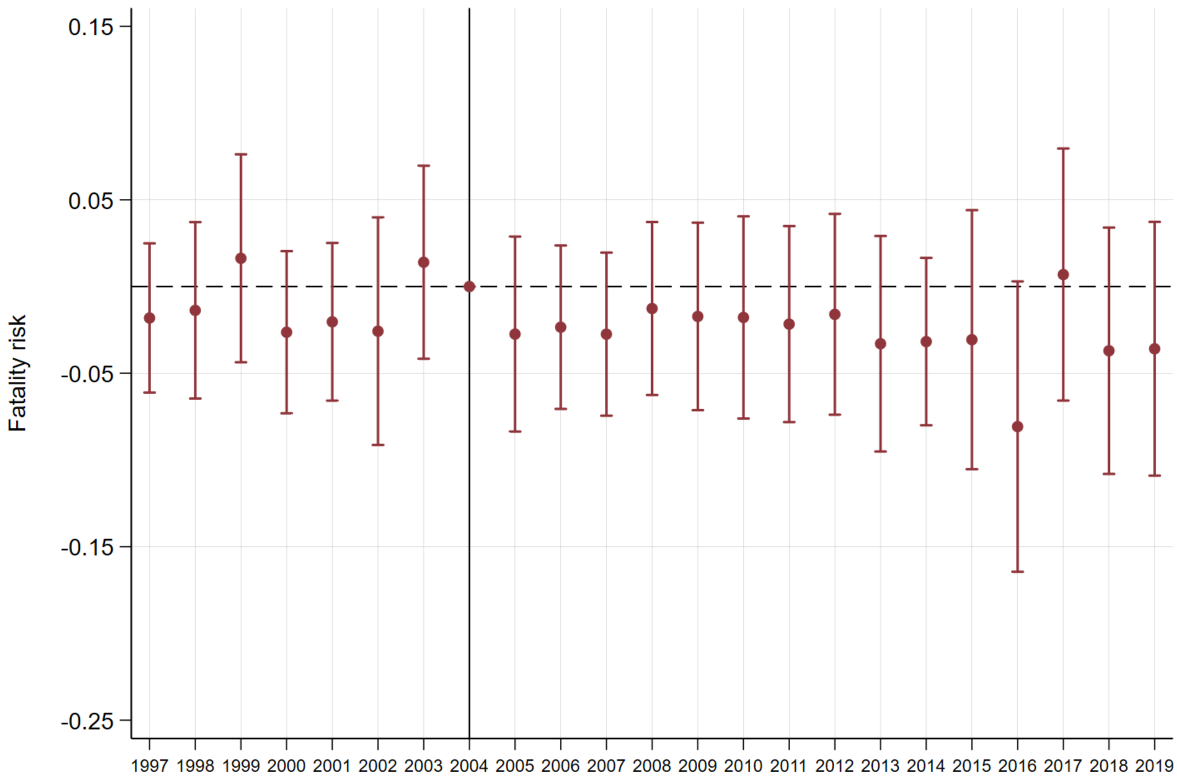
Note: Event study plot based on IPTW. PSNP was launched in 2005. The outcome variable is binary, obtaining value 1 if a violent event occurred in the district in the given year. Solid dots mark the estimated coefficients and vertical capped bars are 95% confidence intervals based on standard errors clustered at the district level.

Figure C5: Impact of PSNP on likelihood of a demonstration (IPTW estimates), by year



Note: Event study plot based on IPTW. PSNP was launched in 2005. The outcome variable is binary, obtaining value 1 if a violent event occurred in the district in the given year. Solid dots mark the estimated coefficients and vertical capped bars are 95% confidence intervals based on standard errors clustered at the district level.

Figure C6: Impact of PSNP on likelihood of a fatality (IPTW estimates), by year



Note: Event study plot based on IPTW. PSNP was launched in 2005. The outcome variable is binary, obtaining value 1 if a violent event occurred in the district in the given year. Solid dots mark the estimated coefficients and vertical capped bars are 95% confidence intervals based on standard errors clustered at the district level.

D Robustness checks

Table D1: Impact of PSNP on violent conflict, demonstration, and fatality risk, controlling for rainfall conditions

	(1) Binary: Violent events	(2) Binary: Demonstrations	(3) Binary: Fatalities
Treament (β)	-0.004	-0.029***	-0.011
	(0.007)	(0.007)	(0.007)
District fixed effects?	Yes	Yes	Yes
Year fixed effects?	Yes	Yes	Yes
Rainfall control?	Yes	Yes	Yes
Control mean:	0.046	0.055	0.051
Number of observations	14191	14191	14191

Note: OLS regression based on Equation (1). Rainfall controls include a contemporaneous and lagged rainfall z-score based on CHIRPS (Funk et al., 2015). Standard errors (in parentheses) clustered at the district level. Statistical significance denoted with * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D2: Impact of PSNP on violent conflict, demonstrations, and fatality risk, controlling for drought conditions

	(1) Binary: Violent events	(2) Binary: Demonstrations	(3) Binary: Fatalities
Treatment (β)	-0.004	-0.025***	-0.010
	(0.007)	(0.007)	(0.007)
District fixed effects?	Yes	Yes	Yes
Year fixed effects?	Yes	Yes	Yes
Drought control?	Yes	Yes	Yes
Control mean:	0.046	0.055	0.051
Number of observations	14191	14191	14191

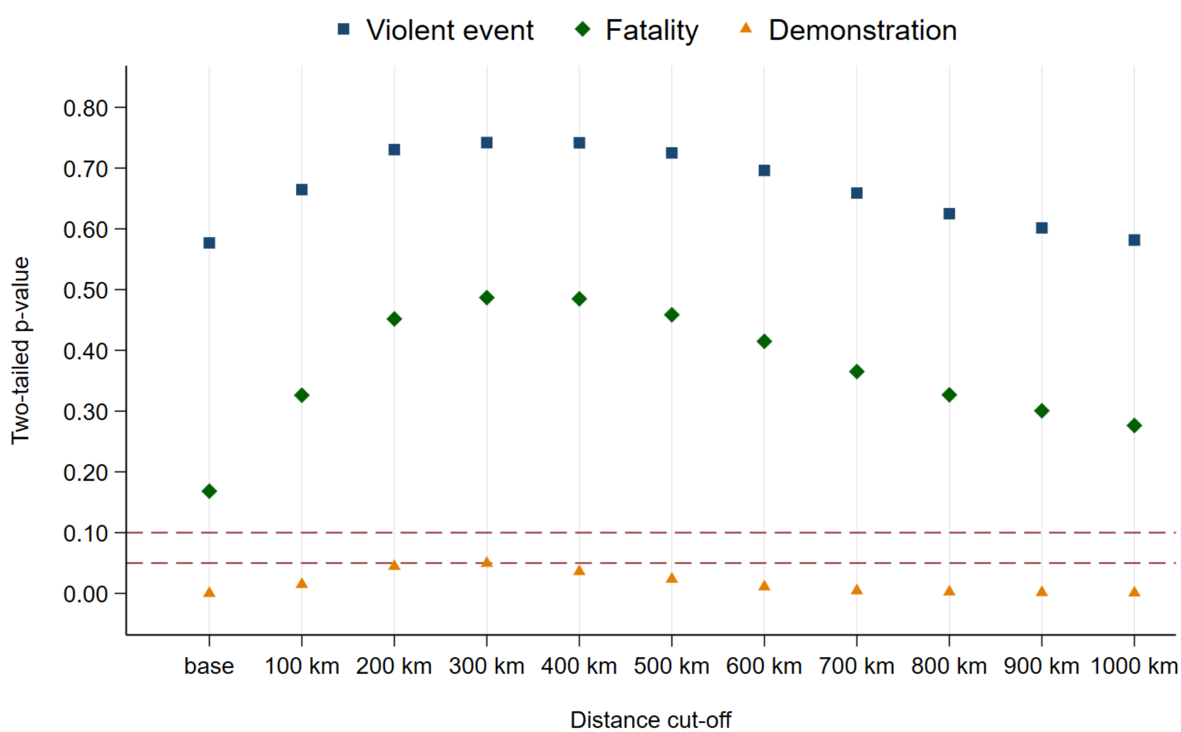
Note: OLS regression based on Equation (1). Drought controls include a contemporaneous and lagged Standardized Precipitation-Evapotranspiration Index, SPEI (Vicente-Serrano et al., 2010). Standard errors (in parentheses) clustered at the district level. Statistical significance denoted with * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D3: Impact of PSNP on the number of conflict events and fatalities

	(1) Number of violent events	(2) Number of demonstrations	(3) Number of fatalities
Treatment (β)	0.020	-0.081**	-0.863
	(0.031)	(0.035)	(0.688)
District fixed effects?	Yes	Yes	Yes
Year fixed effects?	Yes	Yes	Yes
Control mean:	0.087	0.153	0.766
Number of observations	14191	14191	14191

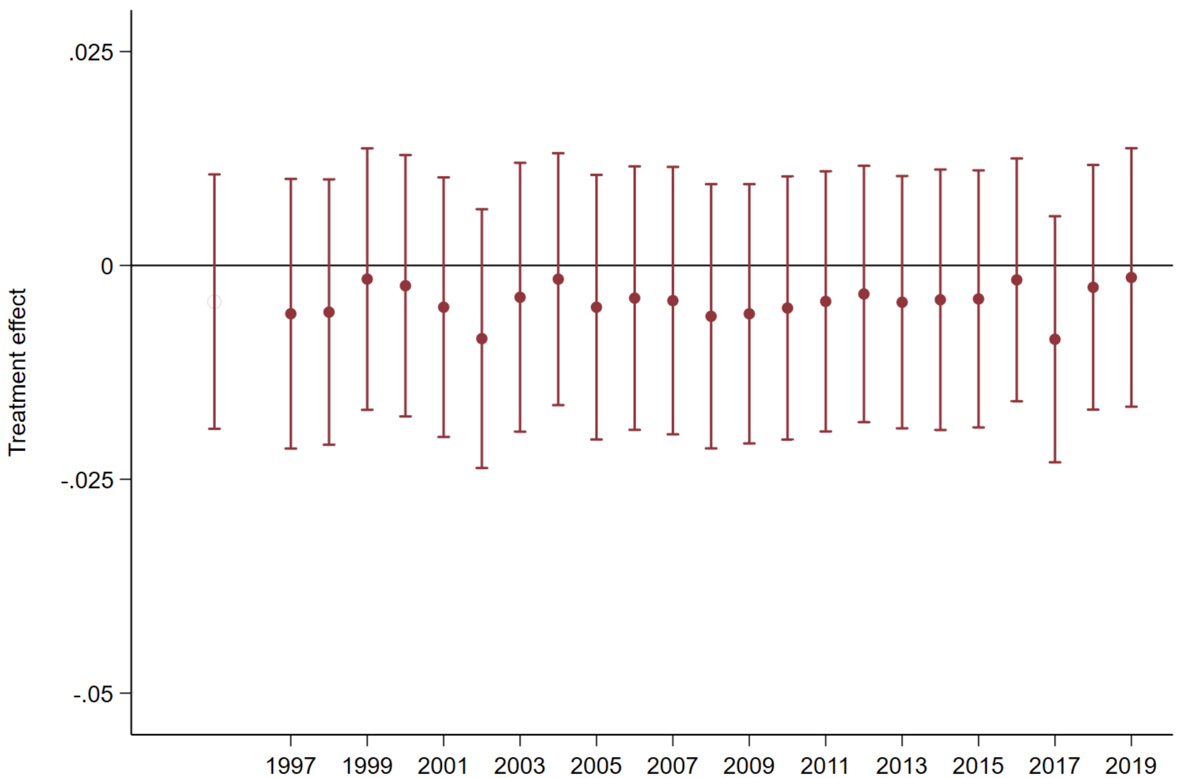
Note: OLS regression based on Equation (1). Standard errors (in parentheses) clustered at the district level. Statistical significance denoted with * p < 0.10, ** p < 0.05, *** p < 0.01.

Figure D1: Impact of PSNP on likelihood of a violent event, demonstration, or fatality: p-values based on Conley (1999) standard errors with different distance cut-offs



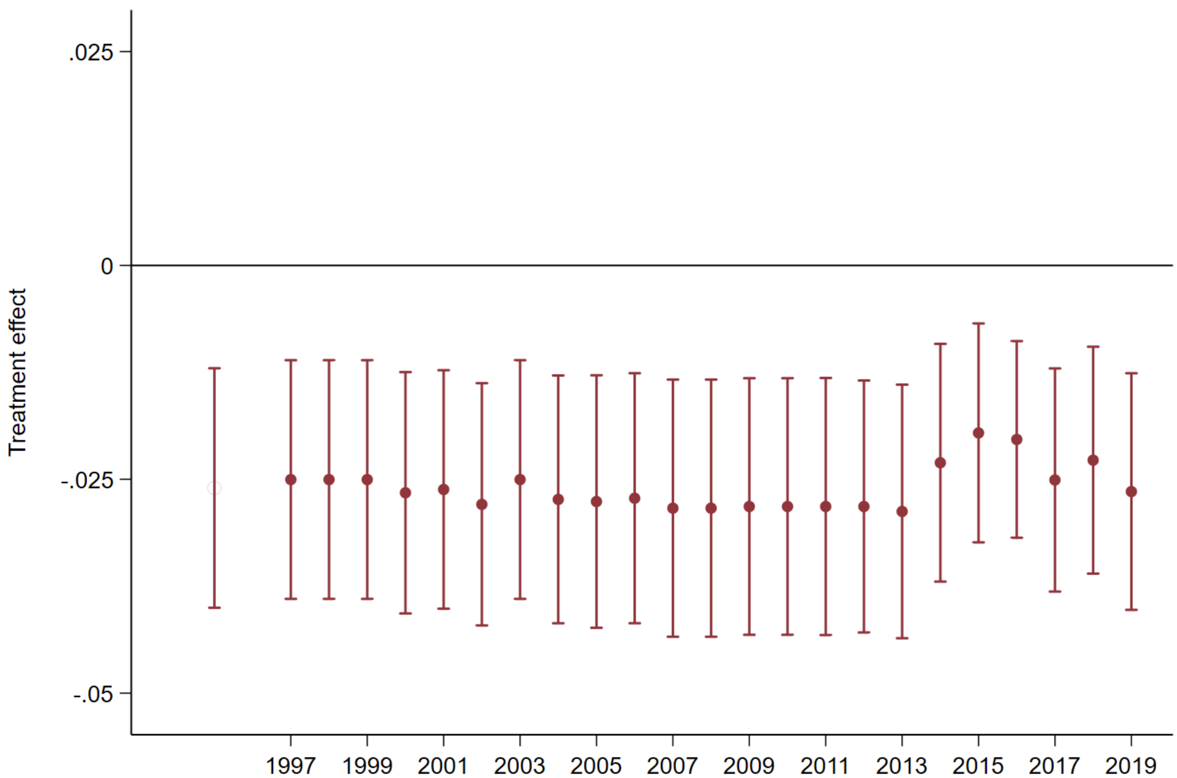
Note: 'Base' refers to p-value based on standard errors clustered at the district level. The other p-values are based on Conley (1999) standard errors robust to spatial autocorrelation with different distance cut-offs. The horizontal dashed lines represent 5% and 10% critical values.

Figure D2: Omit one calendar year at the time from the data set, Violent events



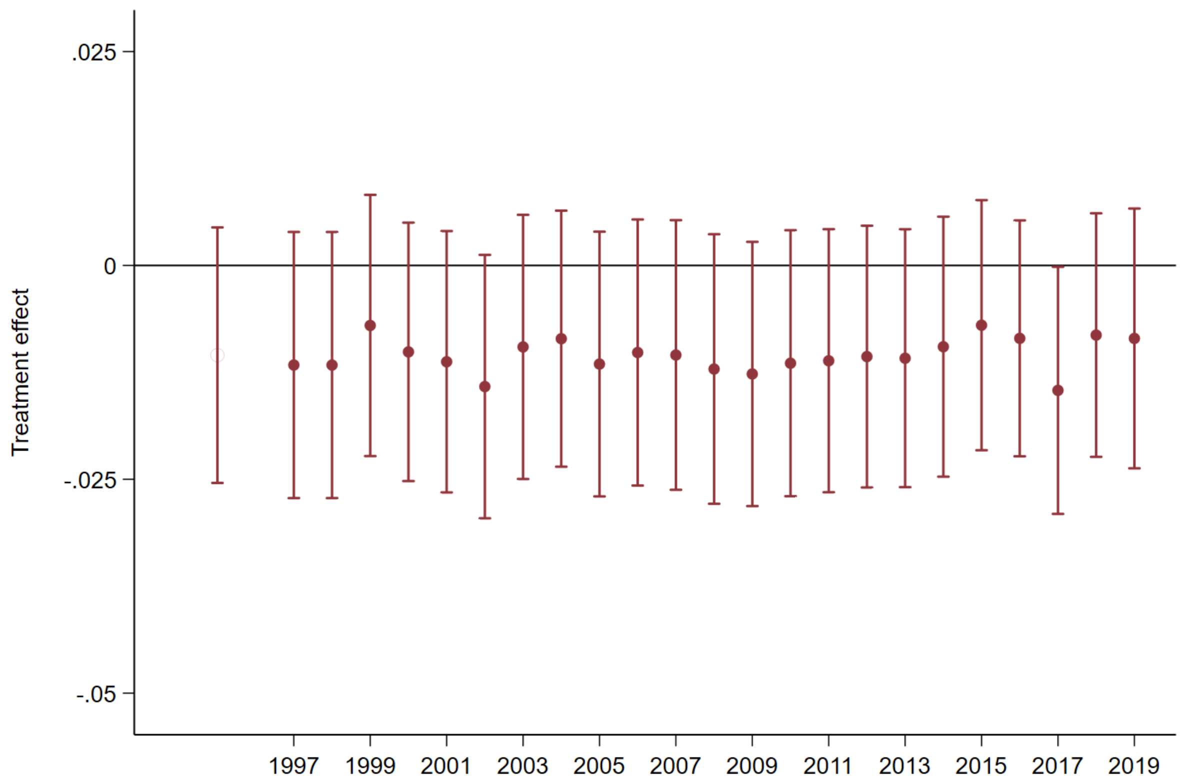
Note: The hollow circle marks the benchmark treatment effect estimate. The solid circles show the treatment effect estimate when a calendar year is omitted from the data set. The capped lines represent 95% confidence intervals based on standard errors clustered at the district level.

Figure D3: Omit one calendar year at the time from the data set, Demonstrations



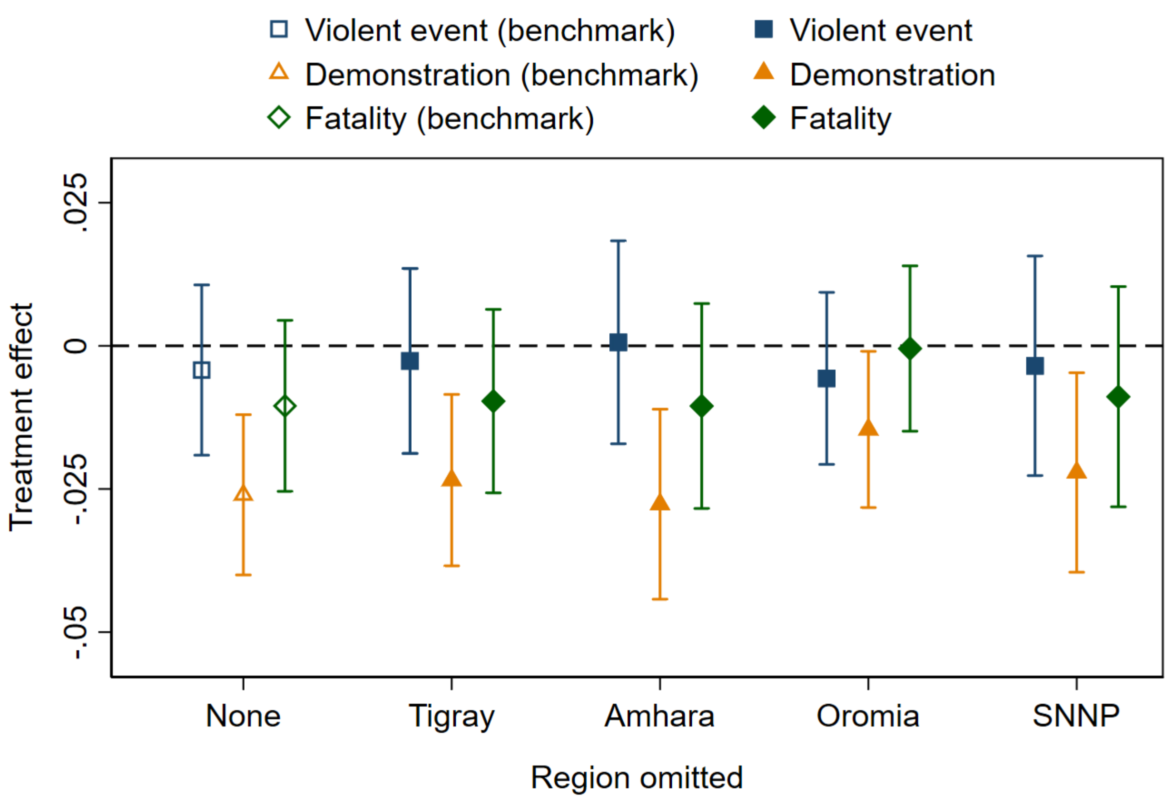
Note: The hollow circle marks the benchmark treatment effect estimate. The solid circles show the treatment effect estimate when a calendar year is omitted from the data set. The capped lines represent 95% confidence intervals based on standard errors clustered at the district level.

Figure D4: Omit one calendar year at the time from the data set, Fatalities



Note: The hollow circle marks the benchmark treatment effect estimate. The solid circles show the treatment effect estimate when a calendar year is omitted from the data set. The capped lines represent 95% confidence intervals based on standard errors clustered at the district level.

Figure D5: Omitting one region at the time from the data set

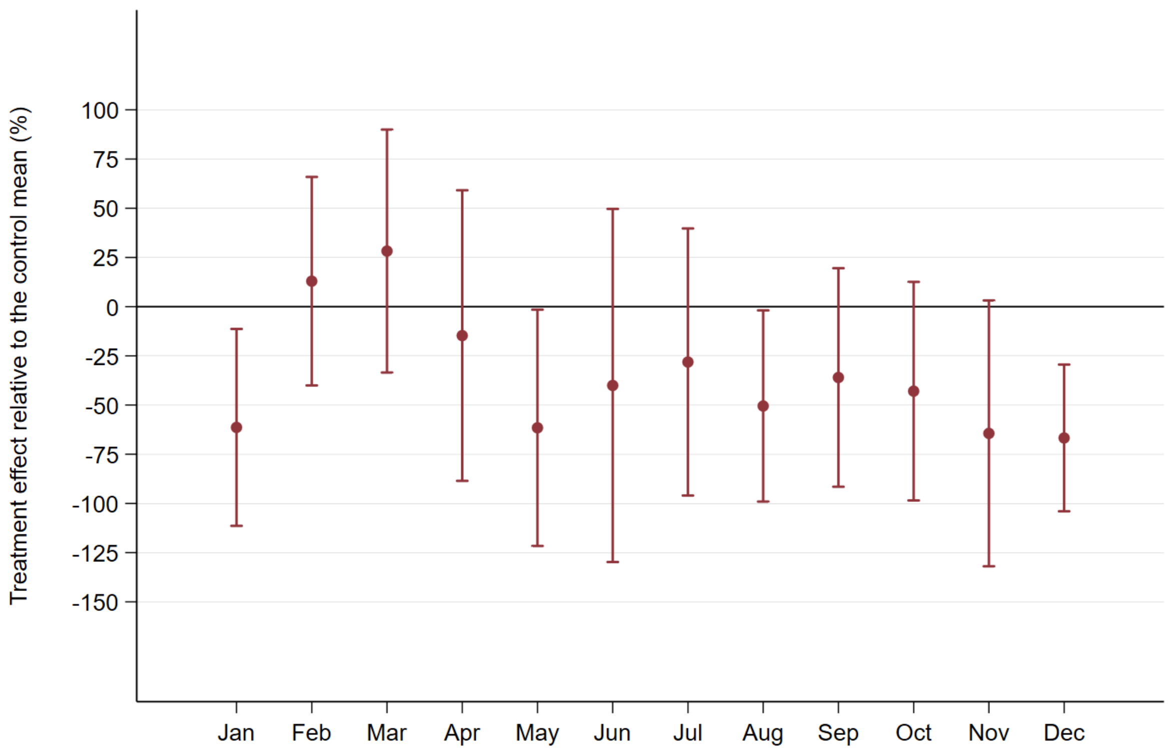


Note: The hollow symbols mark the benchmark treatment effect estimates. The solid symbols show the treatment effect estimate when a region is omitted from the data set. The capped lines represent 95% confidence intervals based on standard errors clustered at the district level.

E Analyses of the potential mechanisms

E.1 Are PSNP households too busy to take part in demonstrations?

Figure E1: Impact of PSNP on likelihood of a demonstration, by month



Note: N = 14,191 (23 years × 617 districts). Solid dots mark the treatment effects relative to the control mean and capped bars are the corresponding 95%–confidence intervals based on standard errors clustered at the district level.

E.2 Does PSNP weaken the weather-civil unrest link?

Building on Fetzer (2020), we seek to understand if civil unrest flares up during or after weather shocks and whether the PSNP attenuates this relationship. To explore this possibility in the context of the PSNP, we estimate the following model:

$$Y_{d,t} = \beta(D_d \times T_t) + \eta(R_{d,t-1}) + \theta(R_{d,t-1} \times D_d) + \alpha_d + \delta_t + \epsilon_{d,t}, \tag{4}$$

where $R_{d,t-1}$ captures the rainfall or drought conditions in the previous year in district d. A negative η coefficient would mean that negative deviations from the long-term mean increase the likelihood of demonstrations. Meanwhile, a positive θ would indicate this relationship is weaker in the PSNP districts.

We used different data sources and methods to construct our weather variables. First, as above, we used the CHIRPS (version 2) rainfall data (Funk et al., 2015) to construct an annual rainfall Z-score variable with zero mean and SD of one. Second, we used the the 12-month lag SPEI (SPEI-12) (Vicente-Serrano et al., 2010), a standardized indicator capturing climatic water balance from the long-run average during the whole calendar year. Third, to focus on drought events, we create drought/rainfall shock variables by setting positive Z-score and SPEI values to zero, and using these positive rectified variables in the regression analysis.

Table E1 shows the regression results. In the first column, the weather variable is rainfall Z-score based on the CHIRPS data. In the second column, the same variable has been positive rectified (i.e., the positive Z-score values have been set to zero) to place the emphasis only on negative deviations in annual rainfall. In the third column, the weather variable is the Z-score based on SPEI, while the fourth column uses the positive rectified version of this variable.

The first row shows the β estimates capturing the impact of the PSNP. They remain negative and statistically significant across all specifications, indicating that the PSNP reduces the likelihood of demonstrations. The η coefficients (second row) based on the rainfall Z-scores are all negative, indicating that negative rainfall deviations increase the risk of demonstrations in the non-PSNP districts. However, when we quantify weather fluctuations using the SPEI, all the estimated η coefficients are small in magnitude and not statistically different from zero. Together, these results indicate that the relationship between the likelihood of demonstrations and rainfall shocks or droughts is not robust.

The third row shows the θ coefficients: the differential impact of rainfall or droughts on the likelihood of demonstrations in the PSNP districts. Across all tables, the estimated θ are all close to zero and never statistically different from zero at the 5%-level, implying that the estimated impacts of rainfall and drought shocks on the likelihood of demonstrations are not meaningfully different between the PSNP and non-PSNP districts.

Table E1: The impact of weather shocks and the PSNP on demonstration risk

	(1) Rainfall z-score	(2) Rainfall z-score (positive rectified)	(3) SPEI	(4) (positive rectified)
Treament (β)	-0.0281**	-0.0274***	-0.0239**	-0.0280**
	(0.0118)	(0.0085)	(0.0111)	(0.0112)
Weather (η)	-0.0103**	-0.0195**	-0.0036	0.0006
	(0.0050)	(0.0087)	(0.0074)	(0.0103)
Weather X Treament (θ)	0.0012	0.0010	-0.0124*	-0.0096
	(0.0082)	(0.0148)	(0.0075)	(0.0148)
District fixed effects?	Yes	Yes	Yes	Yes
Year fixed effects?	Yes	Yes	Yes	Yes
Impact of 1 SD increase in weather variable in non-PSNP districts:	-0.013*	-0.023**	-0.004	0.001
Impact of 1 SD increase in weather variable in PSNP districts:	-0.010	-0.021	-0.016	-0.009
Control mean:	0.055	0.055	0.055	0.055
Number of observations	14191	14191	14191	14191

Note: OLS regression based on Equation (4). SPEI is the Standardized Precipitation-Evapotranspiration Index (Vicente-Serrano et al., 2010). The standard errors are reported in parentheses and are based on (Conley, 1999) with a distance cut-off of 500 km. Statistical significance is denoted with * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

E.3 Are PSNP households more content with the government?

Table E2: Are PSNP households more content with the government?

	(1)	(2)
Panel A: Preferred specification	Right for people	Government does job
PSNP household	0.087**	0.021
	(0.038)	(0.039)
Outcome variable at baseline?	Yes	Yes
HHs with missing baseline outcomes?	Yes	Yes
Controls?	Yes	Yes
Region fixed effects?	Yes	Yes
Baseline mean of the outcome variable	0.40	0.35
Number of observations	1543	1543
Panel B: No controls	(1)	(2)
PSNP household	0.090**	0.055
	(0.037)	(0.039)
Outcome variable at the baseline?	Yes	Yes
HHs with missing baseline outcomes?	Yes	Yes
Controls?	No	No
Region fixed effects?	Yes	Yes
Baseline mean of the outcome variable	0.40	0.35
Number of observations	1543	1543
Panel C: Drop HHs missing baseline trust	(1)	(2)
PSNP household	0.082**	0.011
	(0.040)	(0.040)
Outcome variable at the baseline?	Yes	Yes
HHs with missing baseline outcomes?	No	No
Controls?	Yes	Yes
Region fixed effects?	Yes	Yes
Baseline mean of the outcome variable	0.40	0.35
Number of observations	1099	1100

Note: OLS regression based on Equation (3). Unit of observation is household. Data source is Ethiopian Rural Household Survey (ERHS), 2009 and 2004 survey rounds. Column (1) are responses to the statement "I believe that the government does what is right for the people." Column (2) are responses to the statement "I am confident of the ability of government officials to do their job." Controls include log household per capita consumption in 2004, household size in 2004, and the household head's age, sex, and level of education in 2009. Unless otherwise stated, the specification includes a binary variable capturing households for which the 2004 value of the outcome value was missing (these missing values were set to zero). Robust standard errors (in parentheses). Statistical significance denoted with * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.