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Can social assistance reduce violent conflict and civil unrest?

Evidence from a large-scale public works programme in Ethiopia

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Abstract: We assess how one of the largest public works programmes in the world—Ethiopia's Productive Safety Net Programme (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 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 ruling party, making PSNP households less likely to engage in demonstrations.

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1   Introduction

Violent conflict and civil unrest have increased dramatically around the world in the last two decades (OECD 2021; Rustad 2024). These upward trends are most pronounced in Africa—a region that hosts 60% of the world's poor (World Bank 2023)—where the number of state-based armed conflict events nearly tripled between 2007 and 2023 (Rustad 2024), and the number of protests more than quadrupled between 2007 and 2019 (OECD 2021). During the same period, poverty reduction in Africa has been slowest in its fragile and conflict-affected areas (Beegle et al. 2018a), likely linked to the enormous economic losses associated with violent conflict and civil unrest (Braithwaite et al. 2014; Rohner and Thoenig 2021).

Although the causal relationship between poverty and conflict remains a topic of debate (Djankov and Reynal-Querol 2010; McGuirk and Burke 2020; Miguel et al. 2004), many political scientists and economists posit that poverty and inequality are significant contributors to violent conflict and civil unrest in low- and middle-income countries (Collier and Hoeffler 1998, 2004; Fearon and Laitin 2003; Justino 2025; World Bank 2018a). This raises a critical question: can social assistance or other anti-poverty programmes effectively promote peace and break the cycle of poverty?

We revisit this question in the context of Ethiopia's Productive Safety Net Programme (PSNP), implemented by the government of Ethiopia. Launched in 2005 and reaching eight million people, the PSNP is the largest public works programme outside of India,[1] and one of the longest running public works programmes in Africa (Beegle et al. 2018b). PSNP households receive cash or food transfers in return for labour-intensive public works focusing on environmental and infrastructure rehabilitation. Earlier quasi-experimental evidence shows that the programme has improved the food security and resilience of participating households (Berhane et al. 2014; Hoddinott et al. 2024; Knippenberg and Hoddinott 2017) and increased tree cover in the localities in which it operates (Hirvonen et al. 2022).

Our study focuses on the highland regions of Ethiopia and covers the first 15 years of the PSNP (2005–19), a period marked by ongoing sporadic armed insurgencies and a major anti-government protest movement in Ethiopia's two largest regions between 2014 and 2018. We combine geocoded data on conflict events with digitized PSNP administrative data to construct annual district-level (i.e. *woreda*, the third-level administrative unit in Ethiopia's government structure) panel data spanning the period 1997–2019. Then, applying difference-in-differences methods,[2] we find that the PSNP did not significantly alter the risk of violent events (defined as battles, explosions, or violence against civilians). However, it did reduce the likelihood of civil unrest (defined as protests or riots)[3] by 2.6 percentage points (95% CI: 1.2; 4.0), or by 47% when compared to mean probability of civil unrest in non-PSNP districts. The reduction of the likelihood of civil unrest is more pronounced in districts with a higher proportion of PSNP beneficiaries. We also find that the PSNP reduced the likelihood of a fatality occurring during civil unrest by 1 percentage point, or 52%. An event-study plot reveals that these effects originate from the period 2014–18, during which the widespread protests against the federal government took place.

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[1] The National Rural Employment Guarantee Act (NREGA) in India is the world's largest public works programme.

[2] Government-implemented social assistance programmes are often not randomized (Leight et al. 2024) and, consequently, most of the 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).

[3] We define 'civil unrest' as any event categorized by our conflict data source, the Armed Conflict Location and Event Data Project (ACLED) as a protest or riot. See Figure A1 in Appendix A for more details.

We find no pre-programme trend differences that could account for these findings and our results hold across a variety of robustness checks.[4]

Theoretically, social assistance and other redistributive programmes can reduce relative deprivation (Gurr 1971) and increase support for the incumbent government (De La O 2013; Manacorda et al. 2011; Zucco Jr 2013), making civil unrest less likely to occur (Justino and Martorano 2018). These programmes may also serve as an insurance mechanism that buffers the effects of income shocks, which can otherwise fuel violence and unrest. For example, Fetzer (2020) finds that India's NREGA, the world's largest public works programme, serves as an insurance mechanism that attenuates the relationship between negative rainfall shocks and lower incomes. The NREGA payments maintain the reservation wage of farmers at a higher level, making it more difficult for insurgent groups to recruit farmers to join the insurgency, therefore reducing violence. Public works programmes may also directly reduce unrest by keeping participants occupied, thereby limiting their availability to engage in protests and riots.

To understand how the PSNP reduced the likelihood of civil unrest, we explore these potential mechanisms. First, 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, building on Fetzer (2020), we examine if the PSNP serves as a mechanism to mitigate the relationship between civil unrest and negative income shocks, proxied by weather shocks. We do not find evidence that the PSNP weakens the weather shock–civil unrest link. 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 provides suggestive evidence that the programme may have reduced feelings of animosity towards the government, and thus the need to take part in anti-government protests.

Our study contributes to the empirical literature on conflict risk and social assistance programmes. The available empirical evidence on this topic is thin and focuses mainly on violent events showing mixed results: while unconditional cash transfers and public works can provoke short-term violence from insurgent groups seeking to disrupt government programmes (Khanna and Zimmermann 2017; Premand and Rohner 2024), other research suggests these programmes may reduce violence by providing income stability and limiting insurgent recruitment (Crost et al. 2016; Fetzer 2020).[5] In the context of Ethiopia, we cannot reject the null that the PSNP had no impact on the risk of violent events in the districts in which it operates.[6] Moreover, most of the available evidence on the topic of social assistance programmes and conflict risk comes from short- (Crost et al. 2016; Khanna and Zimmermann 2017) to medium-term (Fetzer 2020; Premand and Rohner 2024) assessments, ranging from nine months to six years after the launch of the programme. Our data cover the first 15 years of the PSNP, allowing us to assess both short- and long-term conflict dynamics.

To our knowledge, this study is the first to assess how social assistance programmes influence the likelihood of civil unrest. Our findings speak to the existing literature on the role of government-implemented programmes and service provision in strengthening the social contract between governments and citizens, helping to prevent civil unrest (Devarajan and Ianchovichina 2018; Justino 2025; Justino and Mar-

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[4] 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.

[5] Another branch in this literature focuses on humanitarian aid and its impact on conflict in recipient countries; see Nunn and Qian (2014) and Christian and Barrett (2024). For research evaluating the impact of development projects without transfer components on conflict, see Berman et al. (2011), Beath et al. (2013), and Crost et al. (2014).

[6] The estimated effect is negative but small and insignificant ($\beta = -0.0042$, or 0.4 percentage points; $p = 0.577$). However, this is not a precise null; the 95% confidence interval is relatively wide: $[-0.019; 0.011]$.

torano 2018, 2019). For instance, during the Syrian revolution in 2011–12, the risk of violence was lower in sub-districts with higher government provision of electricity (De Juan and Bank 2015). Similarly, in Latin America, beneficiaries of large-scale conditional cash transfer programmes have been found to be more likely to support the incumbent government (De La O 2013; Manacorda et al. 2011; Zucco Jr 2013). Moreover, one distinctive feature of the PSNP is that its public works are selected and planned by the communities themselves, with technical assistance from higher administrative levels (MoARD 2006; Wiseman et al. 2010). In other contexts, such community-driven development approaches have been shown to strengthen trust in the government and its legitimacy by delivering development projects that are responsive to community needs (Olken 2010; Parks et al. 2019).

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 livelihoods to approximately 80% 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 (De Waal 2017; Wiseman et al. 2010). Although the programme is primarily funded by international partners (World Bank 2018b), it is managed and implemented by the Ethiopian government. Approximately 80–85% of the households included in the PSNP receive food or cash payments in return for labour-intensive public works carried out over a six-month period outside of the main agricultural season; the other 15–20% of households with limited labour 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% of household consumption (Hirvonen and Hoddinott 2021).[7]

The PSNP combines geographic and community-level targeting. Districts were chosen for the programme based on how often they had requested and received emergency food aid before the programme's 2005 launch (MoARD 2006; World Bank 2020). 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 programme 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 2020).

When the programme began, it served 192 districts[8], 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 2020). Since its inception, the number of PSNP's beneficiaries has increased in the highland districts where it started, and the programme 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

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

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

(World Bank 2020). To date, none of the districts that entered the programme have exited from it (World Bank 2020).

Evaluations of the PSNP have found it to reduce food insecurity (Berhane et al. 2014; Gilligan et al. 2009), lessen the negative impacts of drought shocks (Hirvonen et al. 2023; Knippenberg and Hoddinott 2017; Sabates-Wheeler et al. 2022), and to have modest effects on agricultural productivity (Gazeaud and Stephane 2023; Hoddinott et al. 2012). 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 centres 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. Additionally, these highland regions experienced a simultaneous programme rollout (in early 2005). Second, despite the programme's expansion into other regions, the highland regions have continued to be a primary focus. In 2019, over 70% of all PSNP beneficiaries were from these four highland regions. Lastly, 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 (Lind et al. 2022; Sabates-Wheeler et al. 2013).

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.[9]

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% in 1995 to 27% in 2015 (World Bank 2024). The infant mortality rate decreased by 70% and child stunting fell from 68% in 1992 to 27% in 2019 (World Bank 2024). However, at the same time, political freedoms were severely limited. Assessing the degree of political rights and civil liberties, 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% of the World Press Freedom Index (Reporters without Borders 2024). Human Rights Watch reports serious human rights violations throughout our study period (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).

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[9] During our study period, the country was subdivided 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% of the total population in Ethiopia.

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 centred 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.[10]

Finally, between 2014 and 2018, a series of anti-government protests took place in the two most populous regions, Amhara and Oromia. While these protests were triggered by the 2014 announcement of expansion of the boundaries of Addis Ababa at the expense of farmers in the Oromia region (Human Rights Watch 2016), the underlying causes have been attributed to widespread feelings of economic and political marginalization (Abbink 2016; Dias and Yetena 2022; Human Rights Watch 2014; 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).[11] 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.[12]

3   Data

We generated an annual data series of PSNP beneficiary counts at the district level by digitizing the programme'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. We selected the year 2007 as a baseline because it aligns with the most recent Ethiopian census and its corresponding administrative boundaries.[13] The dataset used in our analysis comprises 617 districts, with 247 districts having received PSNP support since the programme's inception in 2005.

The conflict variable is calculated by totalling the number of events in each district per year, based on data from ACLED (Raleigh et al. 2010). ACLED categorizes conflict events into violent events and demonstrations (see Figure A1 in Appendix A). 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 that occurred in each event.[14]

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[10] Below we show that our results are robust to also including events involving foreign actors.

[11] On 2 October 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).

[12] Our study period does not include the civil war in 2020–22, primarily fought in the Tigray region and the subsequent widespread armed conflict in the Amhara region.

[13] 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.

[14] 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

After removing events categorized as strategic developments (88 observations),[15] events involving Eritrea and other foreign actors (94 observations), and those with the lowest geocoded precision (35 observations),[16] 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.[17] 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 Ethiopian highlands. More than 85% 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 dataset, the likelihood of observing a violent event, demonstration, or fatality is approximately 5% 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.[18] However, we later show that our results are robust to using the non-converted outcome variables.

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which must be the government—resulting in at least 25 battle-related deaths within a calendar year (Sundberg and Melander 2013). Additionally, 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% of district–year observations during the study period, and does not differentiate between event types, such as demonstrations, which restricts its applicability for analysing civil unrest.
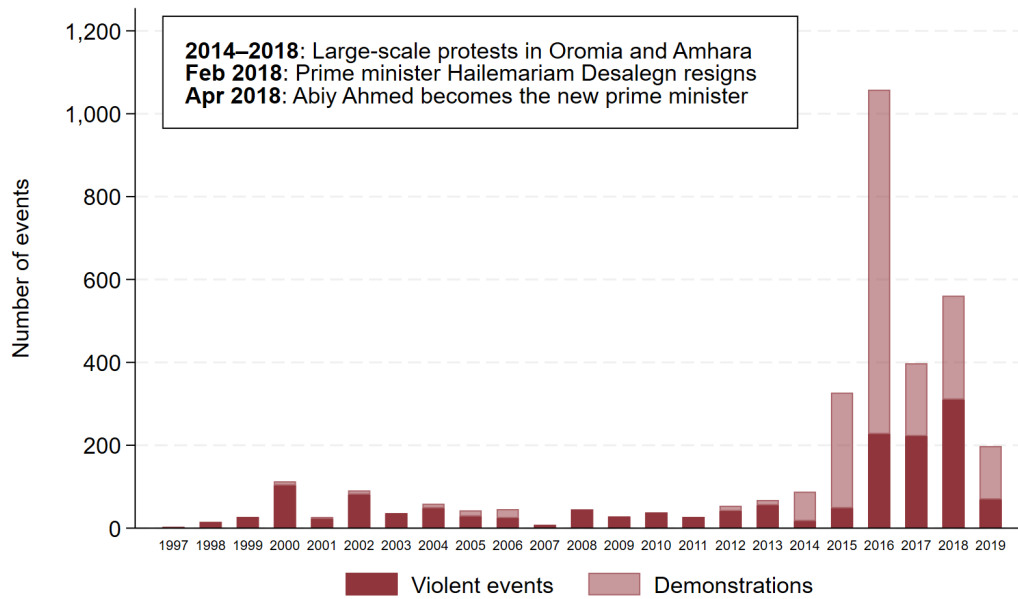
[15] According to documentation from ACLED (2023), 'strategic developments' represent crucial junctures in periods of political violence (e.g., recruitment drives, peace talks, high-level arrests) but are not collected and recorded in the same cross-comparable fashion as 'political violence' and 'demonstration' events. As such, the 'strategic developments' event type is primarily a tool for understanding particular contexts, and not necessarily a marker of conflict itself.

[16] Geo-precision is marked from 1 to 3 in the ACLED dataset, with 1 being the most precise and 3 the least precise. A conflict event is assigned code 3 if a large (non-specific) region is mentioned in the original conflict reporting (e.g., 'border area', 'forest', or 'sea').

[17] 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 (i.e. protests and riots) involved either the Ethiopian military force, the police, rioters, or demonstrators.

[18] 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 1: Conflict in highland Ethiopia, by year

(a) Conflict events in highland Ethiopia, by year

2014–2018: Large-scale protests in Oromia and Amhara
Feb 2018: Prime minister Hailemariam Desalegn resigns
Apr 2018: Abiy Ahmed becomes the new prime minister

Violent events   Demonstrations

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

1999-2002: Armed clashes between Ethiopian army and OLF
2014-2018: Large-scale protests in Oromia and Amhara
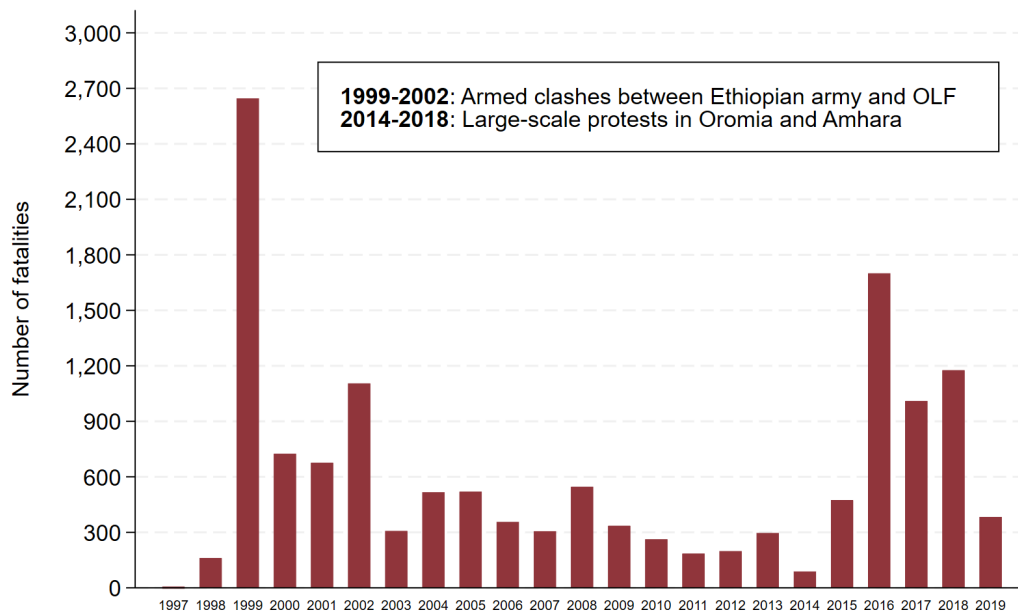
Source: authors' compilation.

Figure 2: Spatial distribution of conflict type by district (1997–2019)
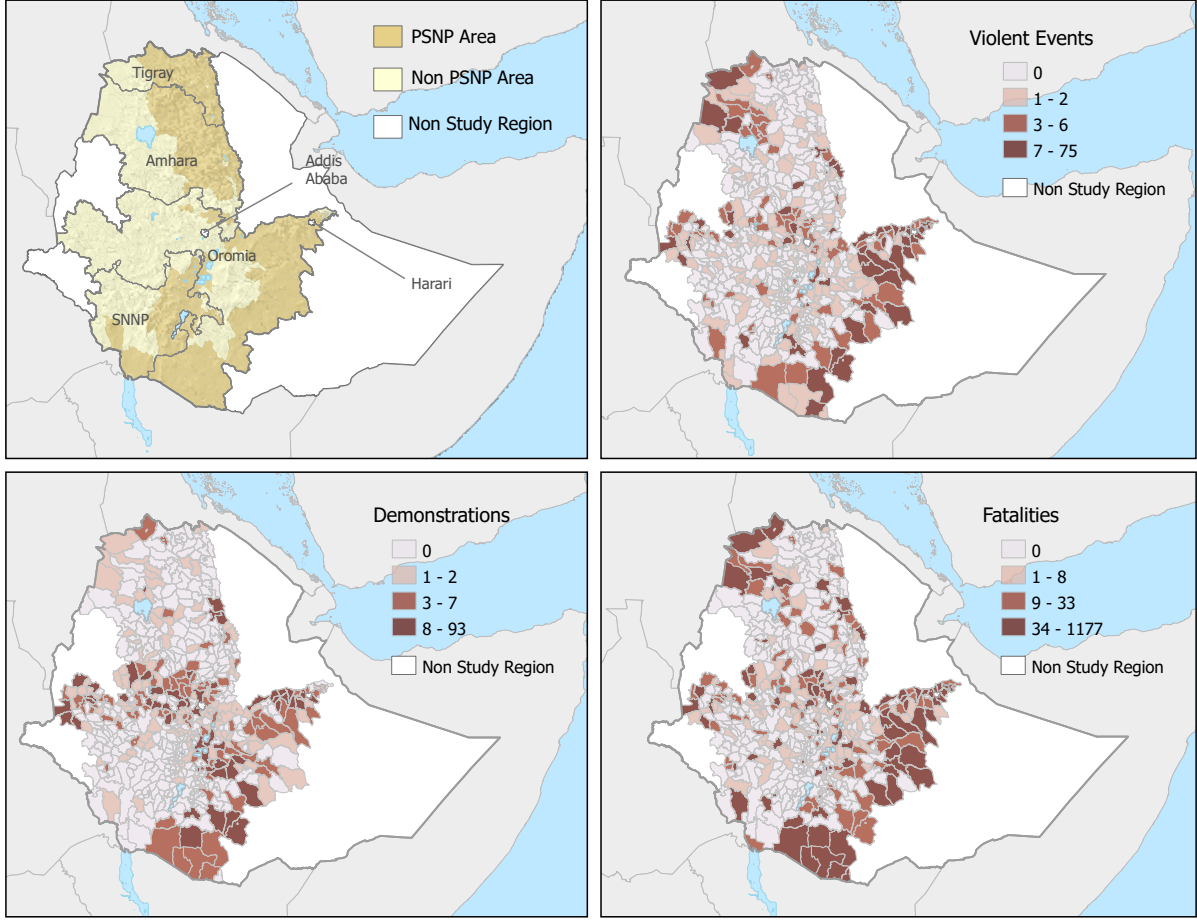
Note: a map of Ethiopia showing the highland study region and the spatial distribution of each type of conflict event. Top-left: the study region with PSNP districts (district boundaries not shown) in light brown and non-PSNP districts in beige. Top-right: violent events by district between 1997 and 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). The maps were created using ArcGIS Pro (version 3.2.0). GIS shapefiles were received from the Central Statistical Agency of Ethiopia.

In additional analyses we use the Ethiopian Rural Household Survey (ERHS) and several spatial data products (see Table A1). These datasets are introduced in the 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:

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

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 2005–19 and 0 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 2005–19 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:

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

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.[19] 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 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—that is, 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 Appendix B 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% 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 Appendix B, 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% ($p = 0.01$); however, it did not affect the likelihood that a fatality at a violent event occurred ($p = 0.973$). While this sub-analysis is indicative 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% 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 (2005–13) are small and not statistically different from zero. However, the treatment effects are all negative in the protest period (2014–18) and mostly statistically significant. The magnitudes of these effects are sizeable. For example, in 2014, the average control district had a 10.3% likelihood of

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

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%.

Figure 3: Impact of PSNP on conflict risk, by conflict type
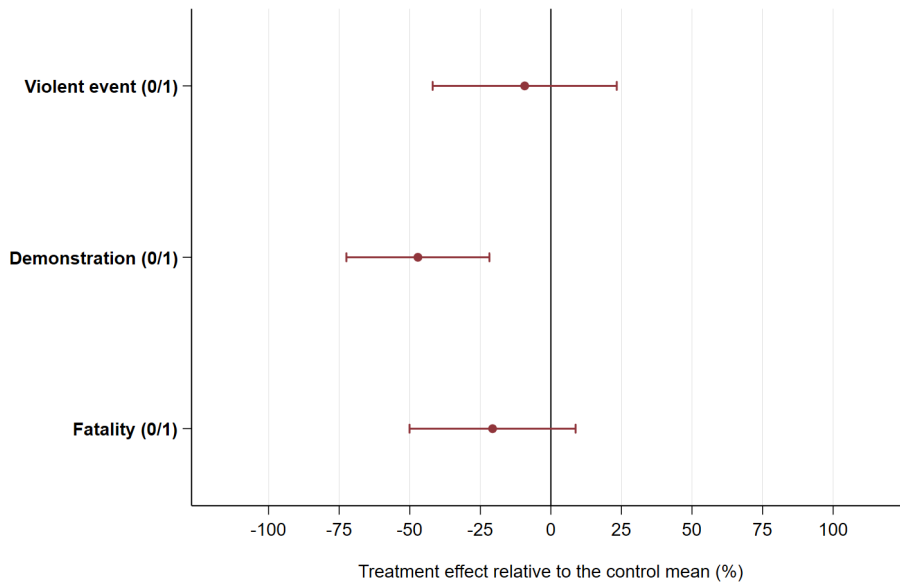
Note: 14,191 observations. Difference-in-differences estimates based on two-way fixed effects. The outcome variable is binary (0/1), obtaining value 1 if a violent event/demonstration/fatality occurred in the district in the given year. Solid dots mark the treatment effects relative to the control mean and capped bars are the corresponding 95% confidence intervals based on standard errors clustered at the district level.

Source: authors' compilation.

Figure 4: Impact of PSNP on likelihood of a demonstration, by year
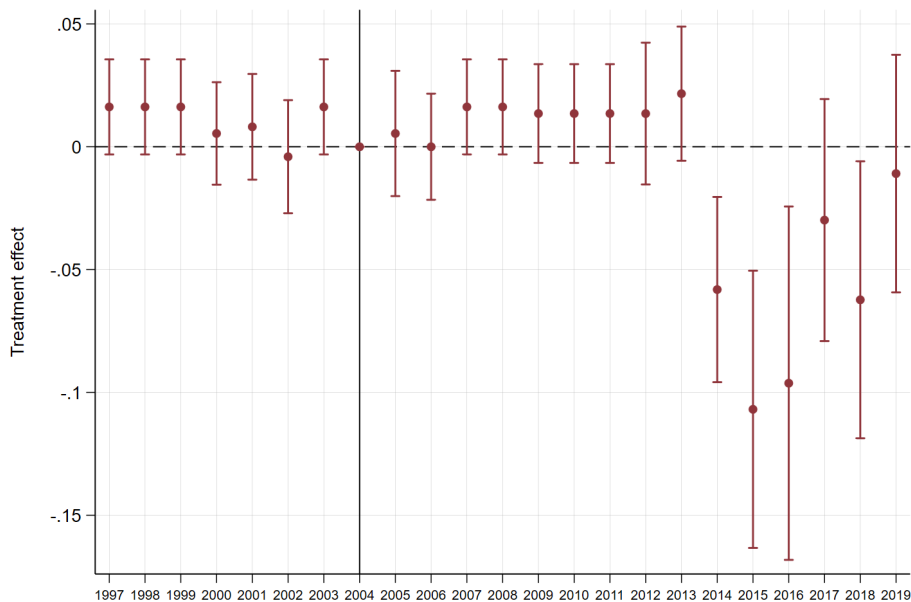
Note: 14,191 observations. Difference-in-differences event-study plot based on two-way fixed effects. PSNP was launched in 2005. The outcome variable is binary, obtaining value 1 if a demonstration occurred in the district in the given year. Solid dots mark the estimated coefficients and vertical capped bars are 95% confidence intervals based on standard errors clustered at the district level.

Source: authors' compilation.

Figure B2 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 B3 shows the plot of the event study for fatalities. Apart from 2017, the treatment effects are trending negative during the protest period but are mostly not statistically different from zero.

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 by 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.[20] 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-programme characteristics that predict districts' inclusion into the programme (Hirvonen et al. 2022; World Bank 2020) using several variables. These include the mean normalized difference vegetation index (NDVI) in 2000–04 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 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 IPTW to estimate Equation 1, we find that the results remain robust (Figure D1).

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. To this end, we used 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 used these data to construct an annual rainfall Z-score variable with zero mean and SD of 1. Negative Z-score values indicate that the annual rainfall was below the long-term mean in the district. Second, we used the 12-month lag SPEI (standardized precipitation evapotranspiration index) (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).

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[20] For more details about the matching approach, see Appendix C.

Third, we explore whether the programme's impact varied depending on the intensity of programme 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% of the caseload shares as cut-off thresholds. The results show treatment effects are generally stronger in districts with higher caseload intensity, particularly when using the top 25% threshold (Figure D2). However, as before, only the treatment effects on demonstrations are statistically different from zero. This finding strengthens our confidence that the programme itself, not some other unobserved factor, is driving the results, as the impact is more pronounced in districts with a greater intensity of programme participation.

Fourth, 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 km at 100 km intervals. Figure D3 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.

Fifth, the results are robust to various reconfigurations 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 D1), and including events involving foreign actors (Figure D1). However, 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 D4–D6) or one region at a time (Figure D7) from the dataset.

7   Potential mechanisms

The context we study in Ethiopia was different from other studies that examined the relationship between social protection programmes and violence (Crost et al. 2016; Fetzer 2020; Khanna and Zimmermann 2017; Premand and Rohner 2024). For example, in Ethiopia armed insurgencies 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 (Fetzer 2020; Khanna and Zimmermann 2017). In addition, there were no influential terrorist groups that attempted to sabotage seemingly successful social assistance programmes, such as was the case of Boko Haram sabotaging the government-led cash transfer programme in Niger (Premand and Rohner 2024). These contextual factors likely explain our null result regarding the impact of Ethiopia's PSNP on violent conflict.

However, differentiating our study from the previous literature, we find that the PSNP reduced the likelihood of demonstrations, particularly during the 2014–18 mass protest wave. Therefore, 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 seek to understand whether 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 estimated the following model:

$$Y_{d,t} = \beta(D_d \times T_t) + \eta(R_{d,t-1}) + \theta(R_{d,t-1} \times D_d) + \alpha_d + \delta_t + \varepsilon_{d,t} \tag{3}$$

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 1. Second, we used 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 created drought/rainfall shock variables by setting positive Z-score and SPEI values to zero, and used 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.

7.3 Are PSNP households more content with the government?

Our final testable hypothesis is whether the PSNP fostered greater sympathy and satisfaction towards the ruling party (the EPRDF), making PSNP households less likely to engage in demonstrations. To explore this hypothesis, we turn to the 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 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.[21] 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 than a response motivated by fear.

The respondents were given seven response options: strongly disagree; disagree; slightly disagree; neither agree nor disagree; slightly agree; agree; strongly agree. We create a binary indicator obtaining value 1 if the respondent agreed or strongly agreed with the statement and 0 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:

$$T_{i,t=1} = \pi(PSNP_{i,t=1}) + \zeta(T_{i,t=0}) + \gamma(X_{i,t=0,1}) + \varepsilon_{i,t=1} \tag{4}$$

where $PSNP_{i,t=1}$ obtains value 1 if the household participates in the PSNP in year 2009, and 0 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 Equation 4, 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 effect corresponds to a 22% increase compared to the 2004 responses (Figure 5). In contrast, the corresponding PSNP estimate for the statement about the government's ability is smaller in magnitude and not statistically significant.[22] We cannot say definitively whether this increase is attributable to households' enrolment in the PSNP since the 2004 EHRS round or to other

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[21] 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 (Cochrane and Tamiru 2016; Lavers 2021).

[22] Re-estimating Equation 4 without controls or excluding households for which the 2004 response is missing yields very similar results; see panels B and C in Table E2.

(unmeasured) time-varying factors correlated with their 2009 PSNP beneficiary status. The absence of pre-2004 trust data prevents an assessment of pre-treatment 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 2014–18 protest period.[23]

Figure 5: Are PSNP households more likely to agree with statements about the government's intentions and ability?
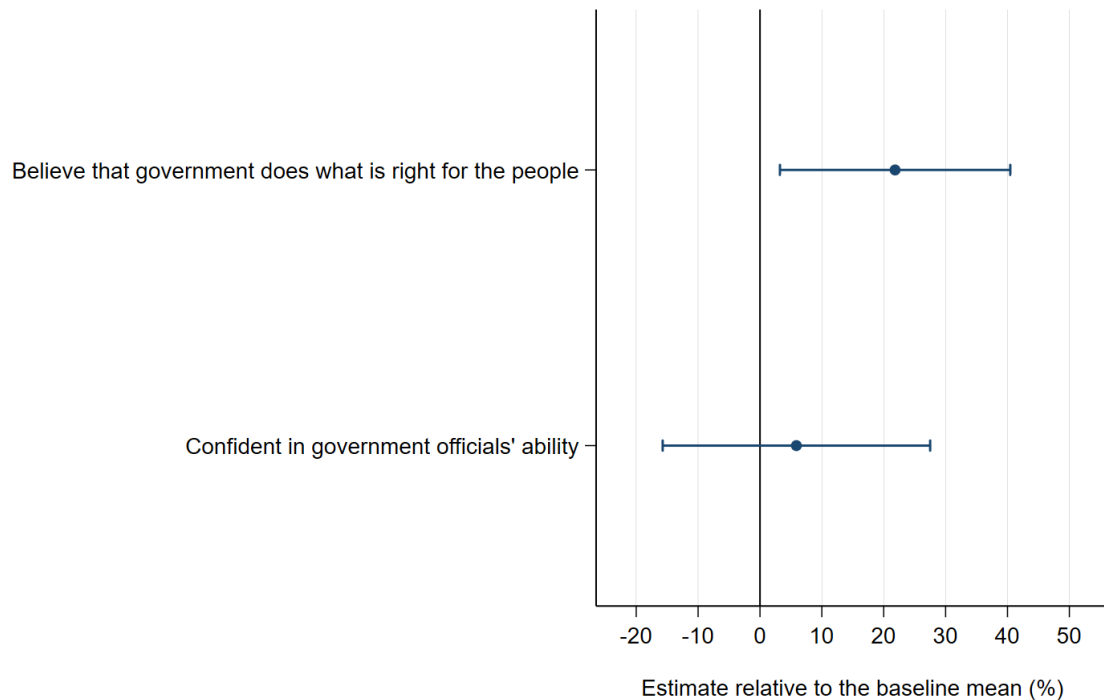
Note: *N* = 1,543 households in the ERHS. ANCOVA estimates. Capped lines represent 95% confidence intervals.
Source: authors' compilation.

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 programmes with higher levels of support for incumbent governments in Latin America (De La O 2013; Manacorda et al. 2011; Zucco Jr 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 raises a crucial question: can social assistance programmes foster peace and break the cycle of poverty in these challenging settings?

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[23] 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 2014–18 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.

We address this question by assessing the impact of Ethiopia's flagship safety net programme, the 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 2014–18. 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 mechanisms through which the PSNP could foster greater trust in the government, though we cannot distinguish between them. First, government-led programmes 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 programmes in Latin America (De La O 2013; Manacorda et al. 2011; Zucco Jr 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 programmes 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 analysing the impact of social assistance on civil unrest. Our findings suggest that government-led transfer programmes can help alleviate economic marginalization and thereby contribute to long-term stability.

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Appendix

A   Additional details about the data

A.1   ACLED event data and spatial datasets

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

Event type	Sub-event type	Disorder type
Battles	Government regains territory	Political violence
	Non-state actor overtakes territory	
	Armed clash	
Protests	Excessive force against protesters	Political violence; Demonstrations
	Protest with intervention	Demonstrations
	Peaceful protest	
Riots	Violent demonstration	
	Mob violence	Political violence
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	
Strategic developments	Agreement	Strategic developments
	Arrests	
	Change to group/activity	
	Disrupted weapons use	
	Headquarters or base established	
	Looting/property destruction	
	Non-violent transfer of territory	
	Other	

Source: Table 2 in ACLED (2023: 11). © 2024 ACLED All rights reserved. Used with permission from ACLED.

Table A1: 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.

Source: authors' compilation.

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 AN-COVA 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 A2 presents summary statistics of the variables used in the analysis.

Table A2: 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.

Source: authors' compilation.

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 * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: authors' compilation based on data.

Figure B1: Impact of PSNP on likelihood of a fatality from all events, violent events, and demonstration
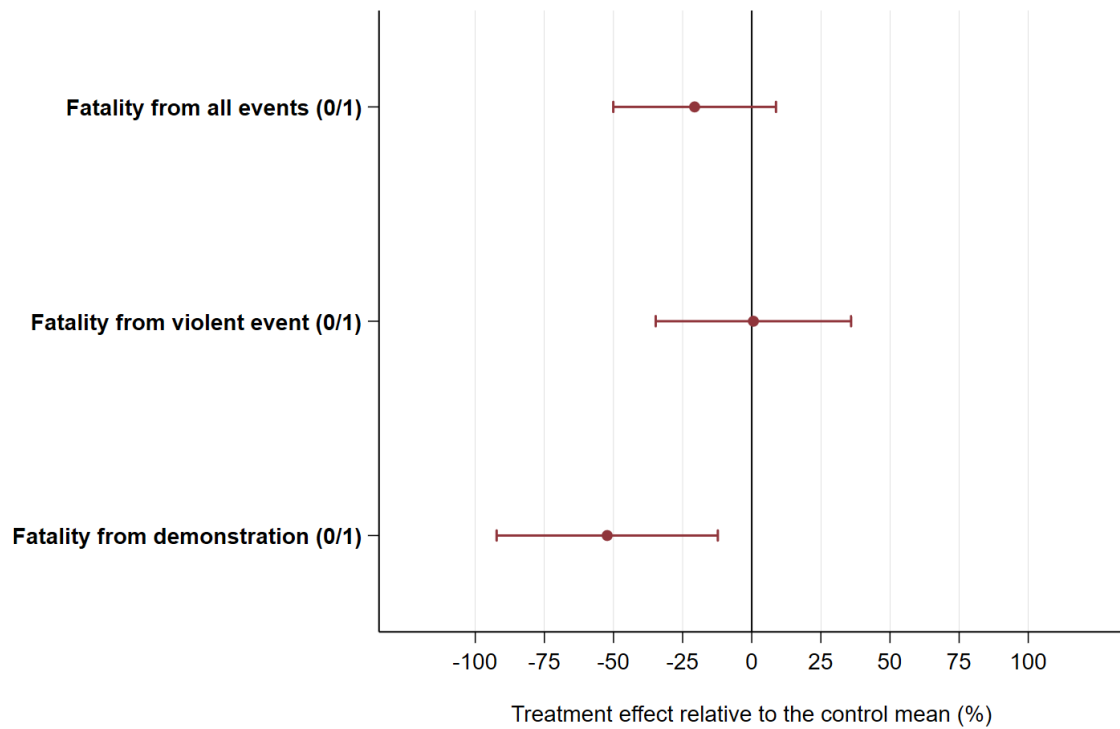
Note: the outcome variable is binary, obtaining value 1 if a fatality occurred in the district in the given year. Solid dots mark the estimated coefficients and vertical capped bars are 95% confidence intervals based on standard errors clustered at the district level.
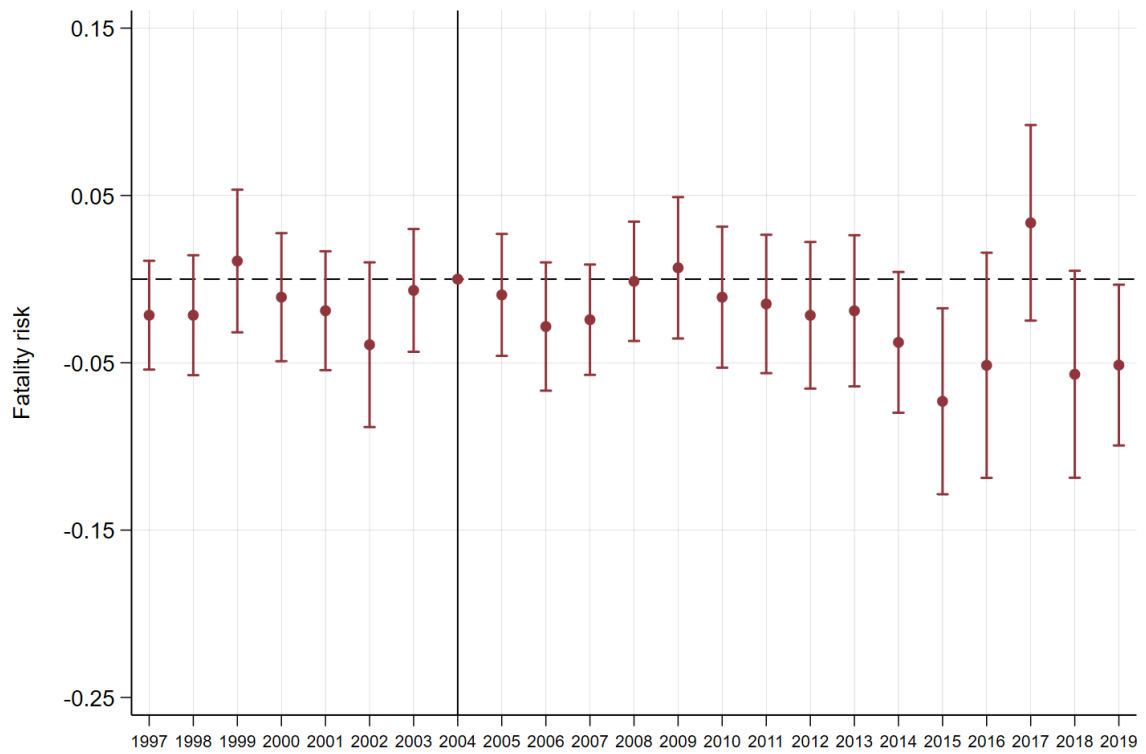Source: authors' compilation.

Figure B2: Impact of PSNP on likelihood of a violent event, by year
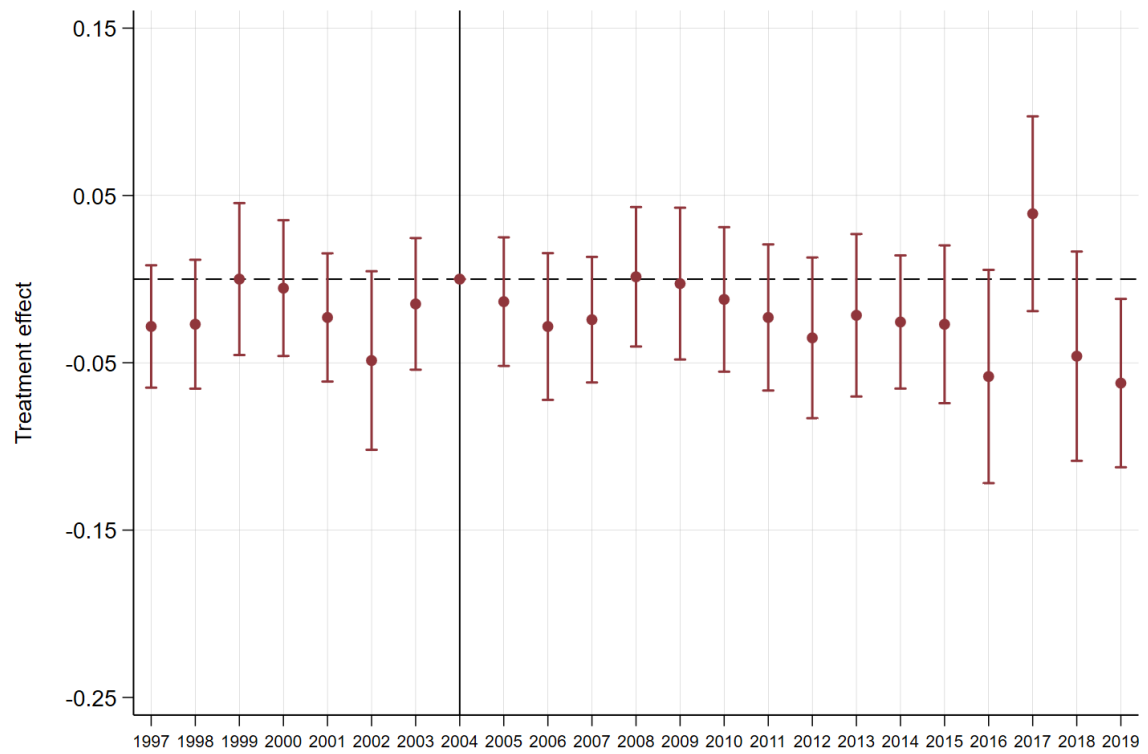
Note: event study plot. PSNP was launched in 2005. The outcome variable is binary, obtaining value 1 if a violent event occurred in the district in the given year. Solid dots mark the estimated coefficients and vertical capped bars are 95% confidence intervals based on standard errors clustered at the district level.

Source: authors' compilation based on data.

Figure B3: 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.

Source: authors' compilation based on data.

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 2020): 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.[24] 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 agroeco-logical 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 D1 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 IPTW estimate on fatality risk appears significant at the 10% level.

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.

Source: authors' compilation based on data.

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[24] 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; Ryan et al. 2019).

Figure C1: Maps of study region with spatial variables used in propensity scores
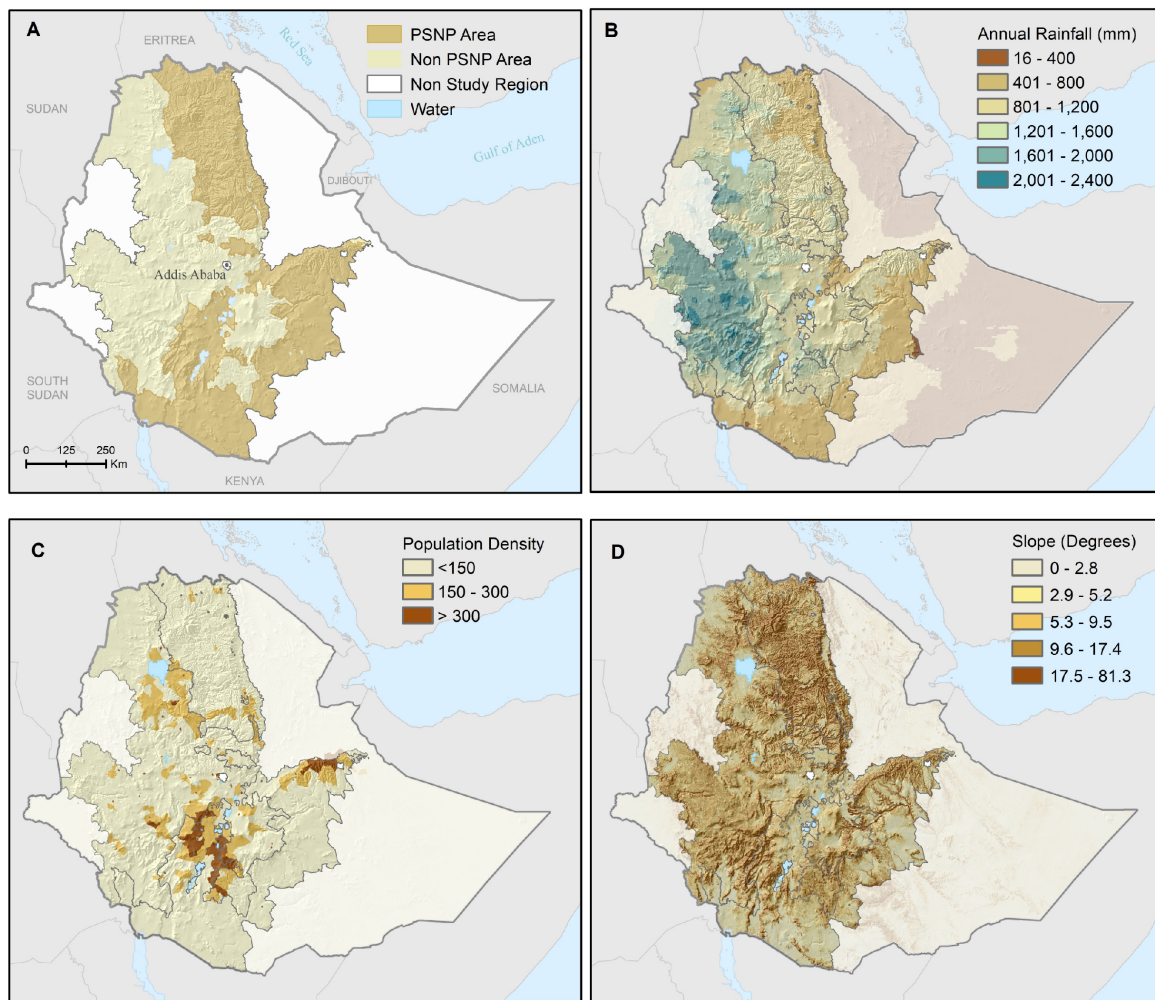
Note: map of Ethiopia showing the highland study region and spatial variables over a hill-shaded terrain. The area of the non study region has a light transparency effect applied for added context. A: Study region with the PSNP districts (boundaries not shown) in light brown and the non-PSNP districts 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.

Source: authors' compilation based on data listed in Table A1. The maps were created using ArcGIS Pro (version 3.2.0). GIS shapefiles were received from the Central Statistical Agency of Ethiopia.

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	-0.033***	-0.018*
	(0.011)	(0.011)	(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 * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
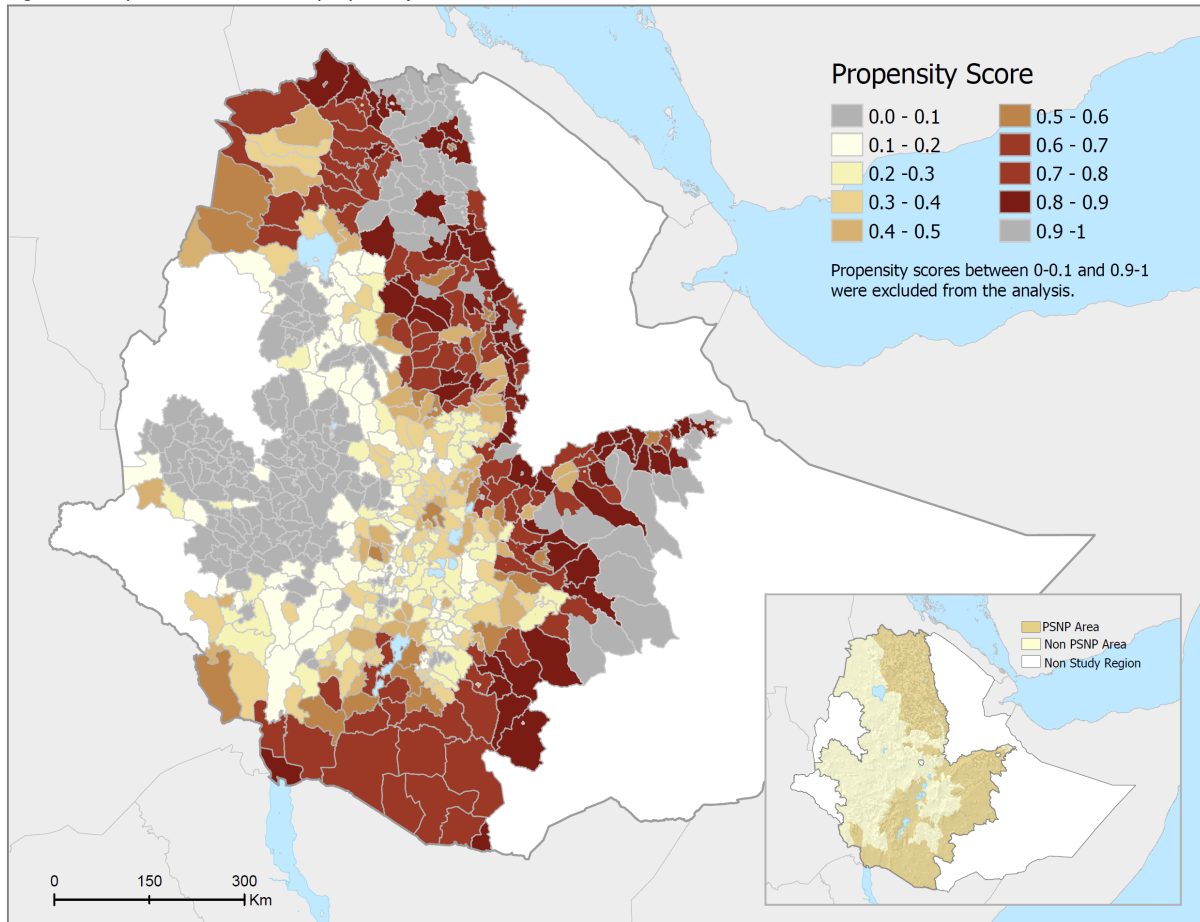
Source: authors' compilation.

Figure C2: The estimated propensity scores for all districts
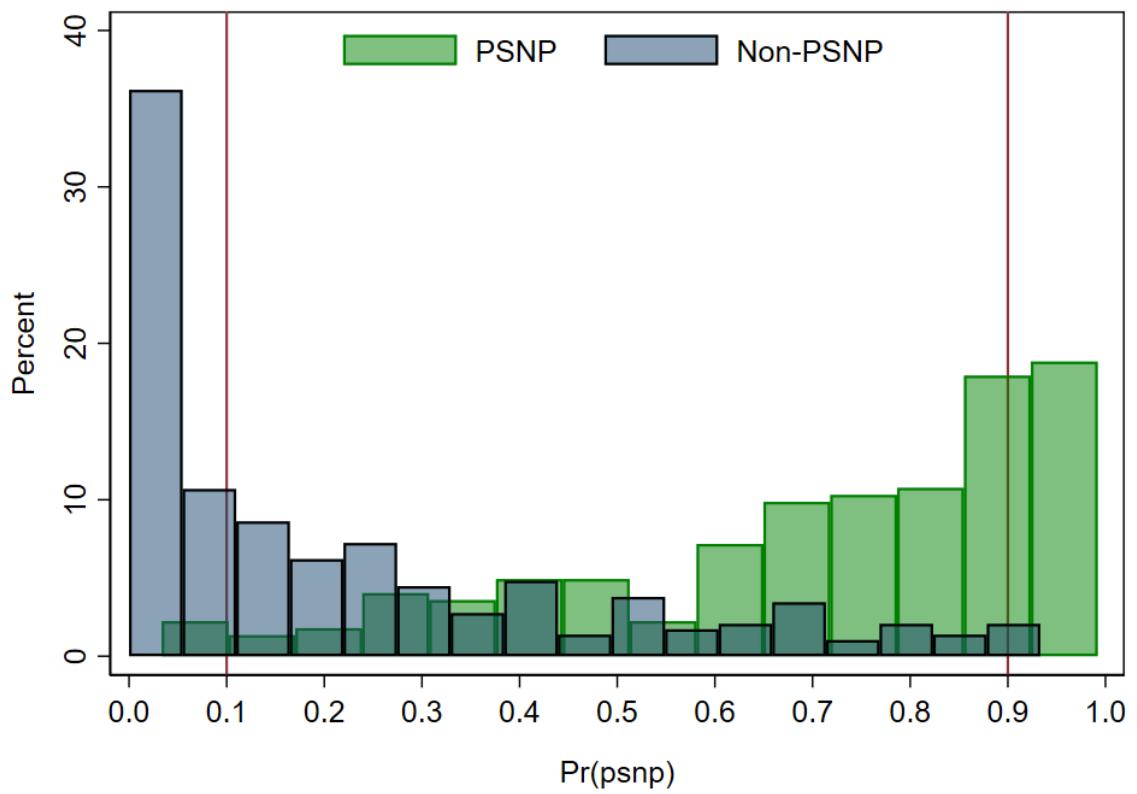
Note: N = 617 districts. The area between the vertical red lines marks the area of common support as defined by Crump et al. (2009).

Source: authors' compilation.

Figure C3: Spatial distribution of propensity scores

Source: authors' illustration based on the predicted propensity scores calculated by the authors using the data listed in Table A1. The maps were created using ArcGIS Pro (version 3.2.0). GIS shapefiles were received from the Central Statistical Agency of Ethiopia.
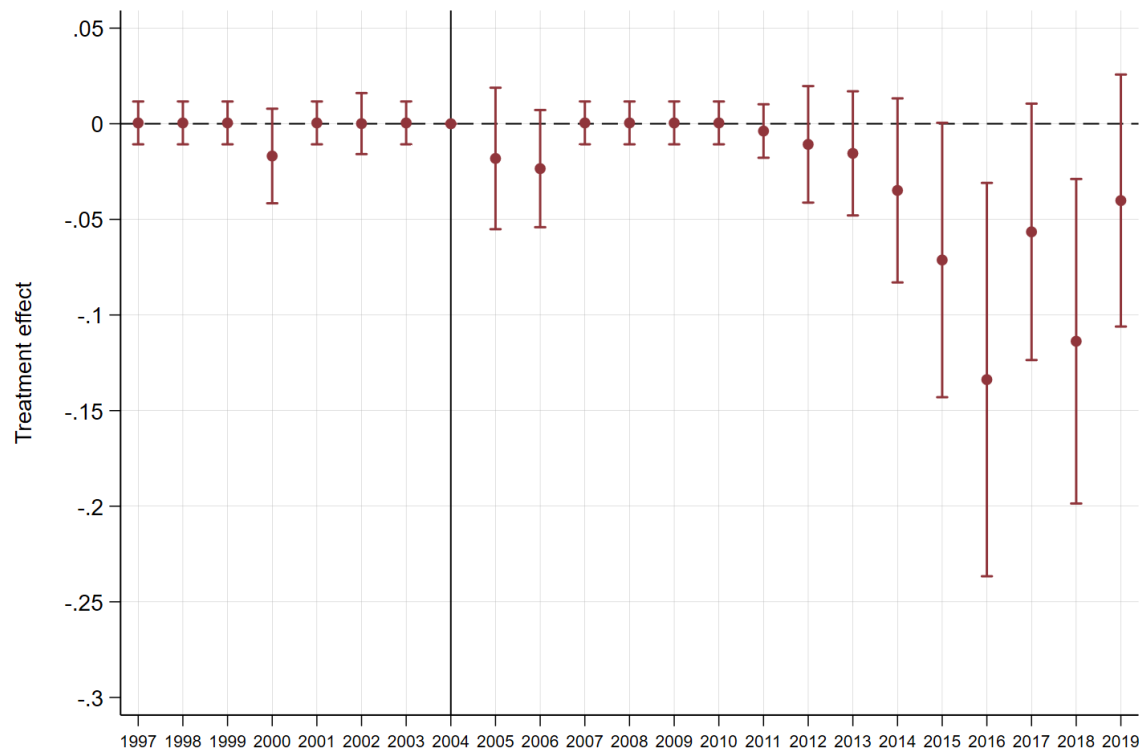
Figure C4: Impact of PSNP on likelihood of a violent event (IPTW estimates), by year
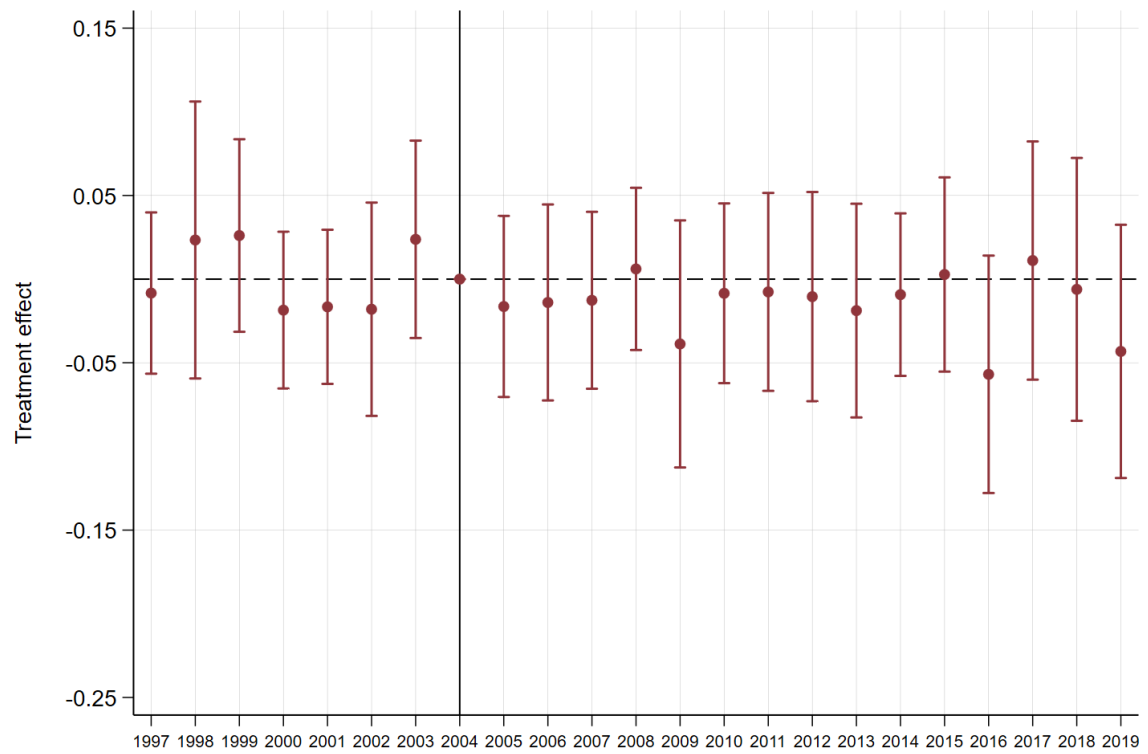
Note: event study plot based on IPTW. PSNP was launched in 2005. The outcome variable is binary, obtaining value 1 if a violent event occurred in the district in the given year. Solid dots mark the estimated coefficients and vertical capped bars are 95% confidence intervals based on standard errors clustered at the district level.
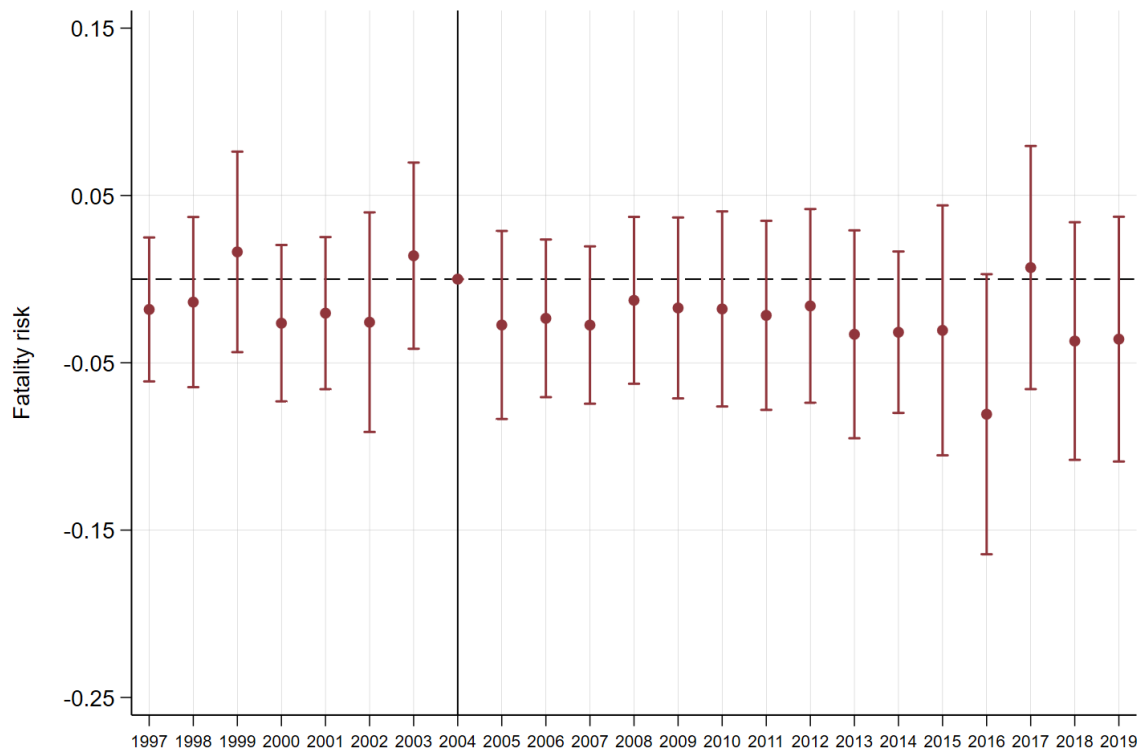
Source: authors' compilation.

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.

Source: authors' compilation.

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.

Source: authors' compilation.

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 (0.007)	-0.029*** (0.007)	-0.011 (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 * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Source: authors' compilation.

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 (0.007)	-0.025*** (0.007)	-0.010 (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 * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Source: authors' compilation.

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 (0.031)	-0.081** (0.035)	-0.863 (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 * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Source: authors' compilation based on data.

Figure D1: 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
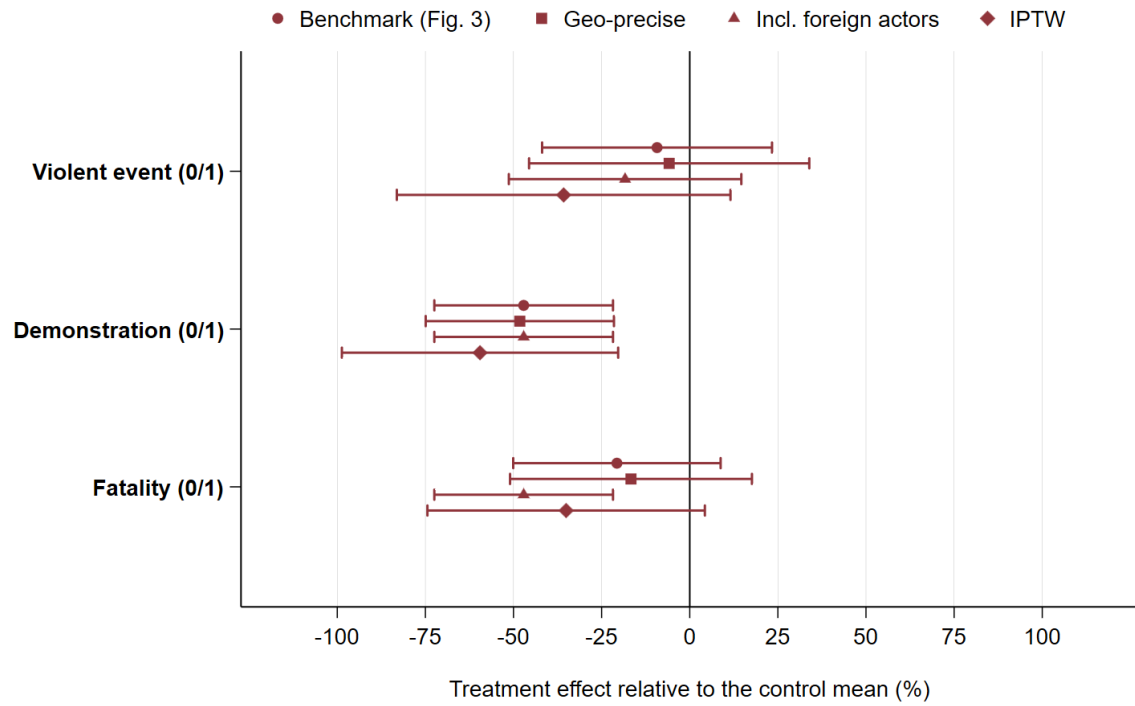
Note: N = 14,191 (23 years × 617 districts), except for IPTW, N = 7,199 (23 years × 313 districts), after restricting the data to common support. Solid dots mark the treatment effects relative to the control mean and capped bars are the corresponding 95%–confidence intervals based on standard errors clustered at the district level.

Source: authors' compilation.

Figure D2: Estimates by PSNP caseload intensity
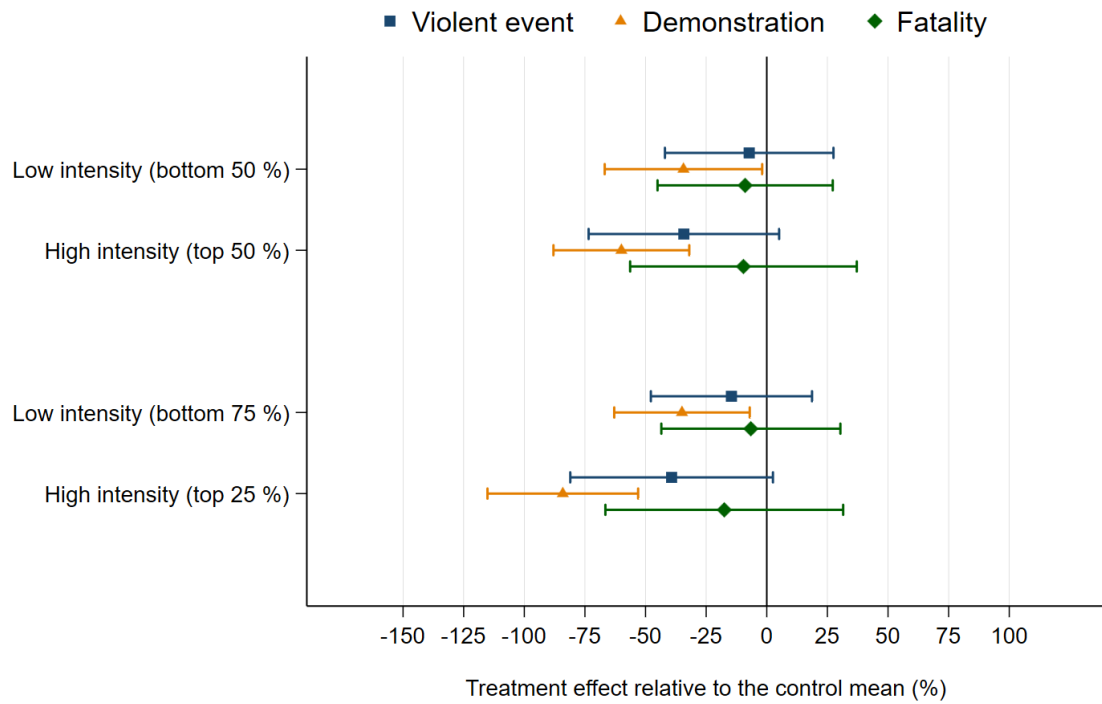
Note: N = 14,191 (23 years × 617 districts). Solid dots mark the treatment effects relative to the control mean and capped bars are the corresponding 95%–confidence intervals based on standard errors clustered at the district level. Caseload intensity is based on the share of PSNP beneficiaries in each district relative to the district's population in 2005.

Source: authors' compilation.

Figure D3: 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
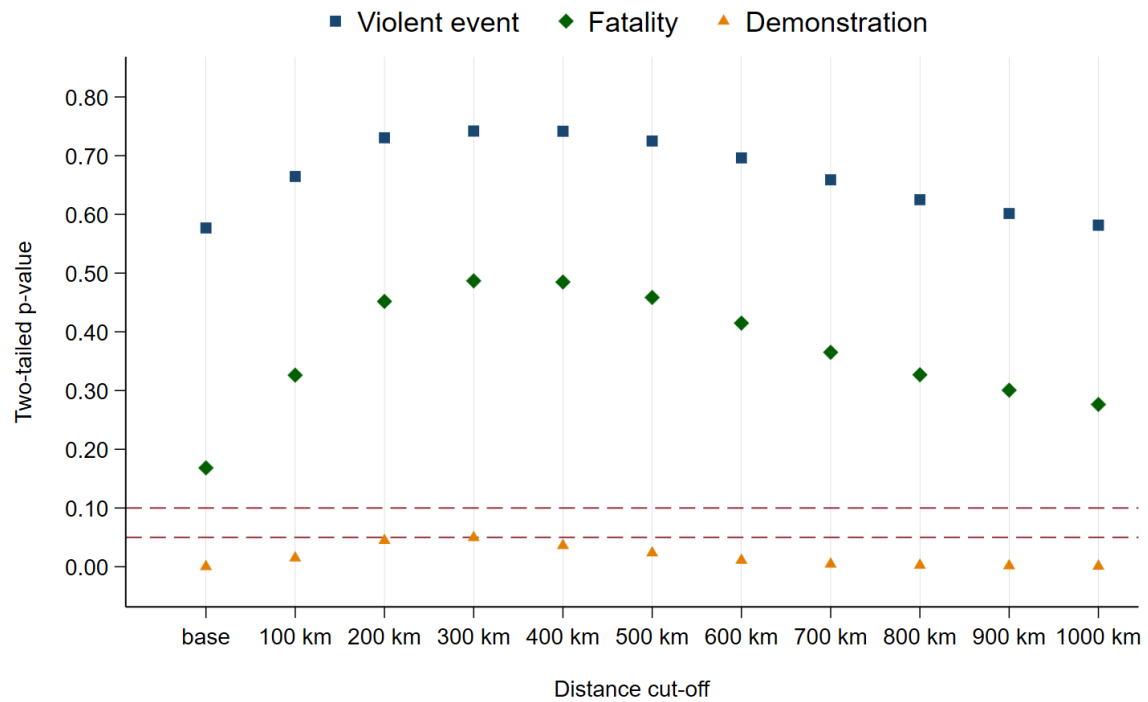
Note: 'base' refers to p-value based on standard errors clustered at the district level. The other p-values are based on Conley (1999) standard errors robust to spatial autocorrelation with different distance cut-offs. The horizontal dashed lines represent 5% and 10% critical values.
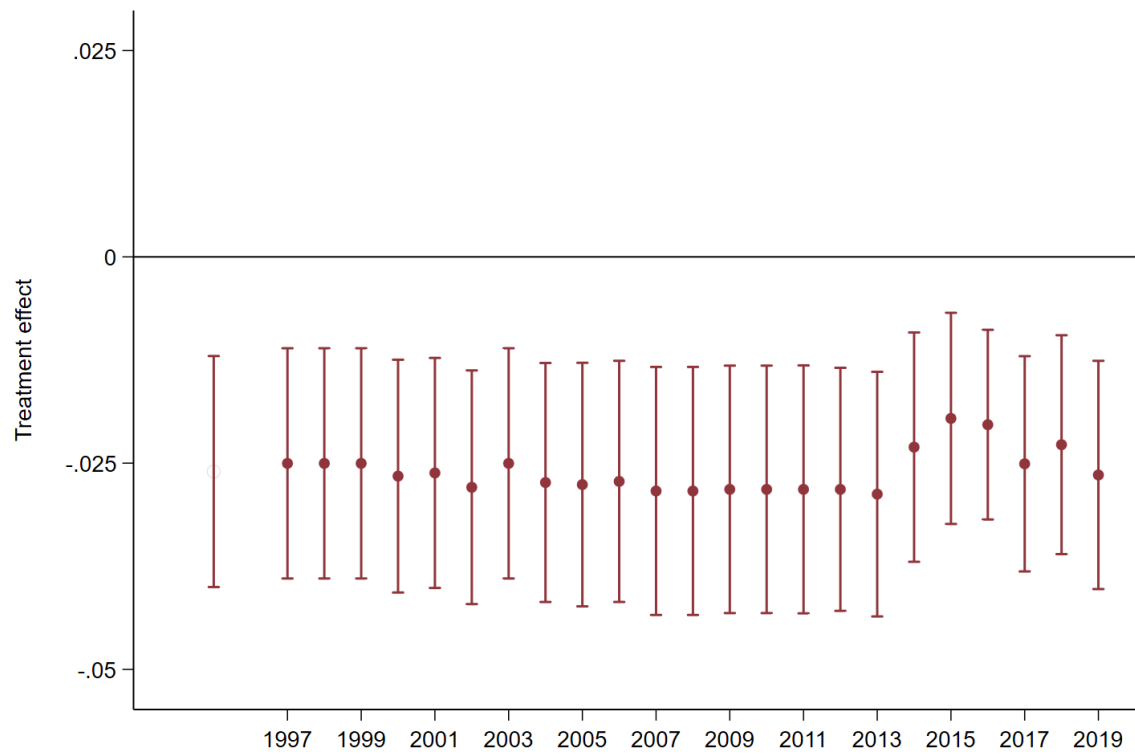
Source: authors' compilation.

Figure D4: Omit one calendar year at the time from the data set, Violent events
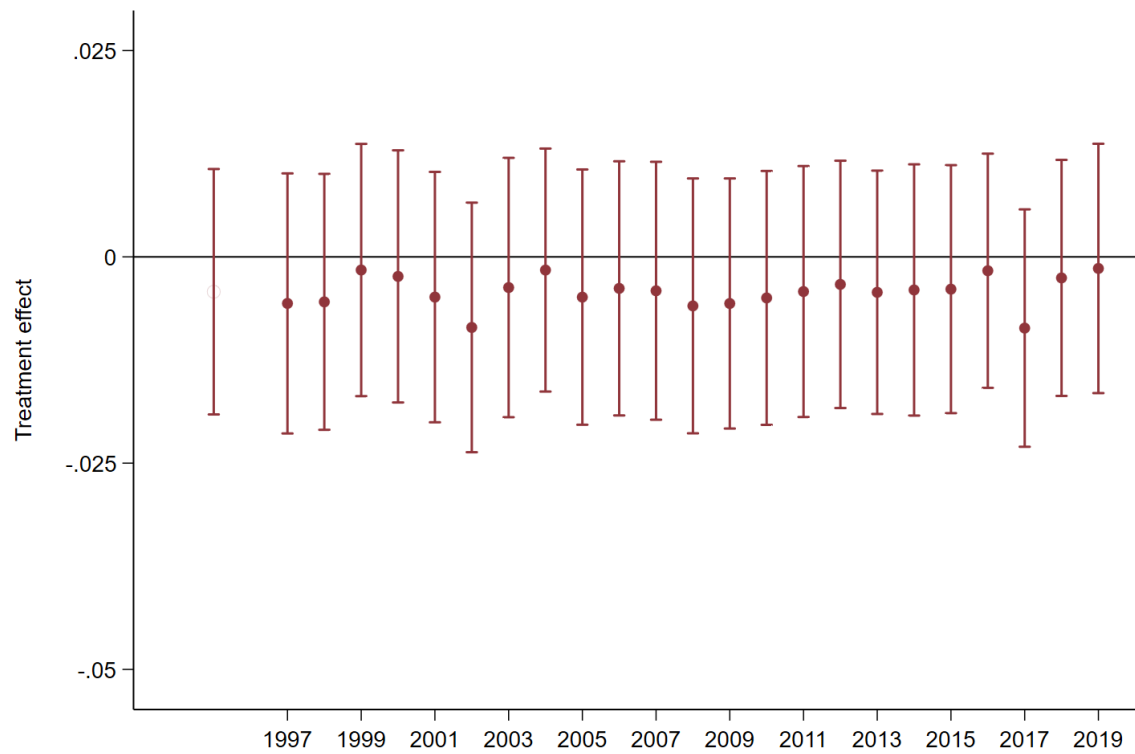
Note: the hollow circle marks the benchmark treatment effect estimate. The solid circles show the treatment effect estimate when a calendar year is omitted from the data set. The capped lines represent 95% confidence intervals based on standard errors clustered at the district level.
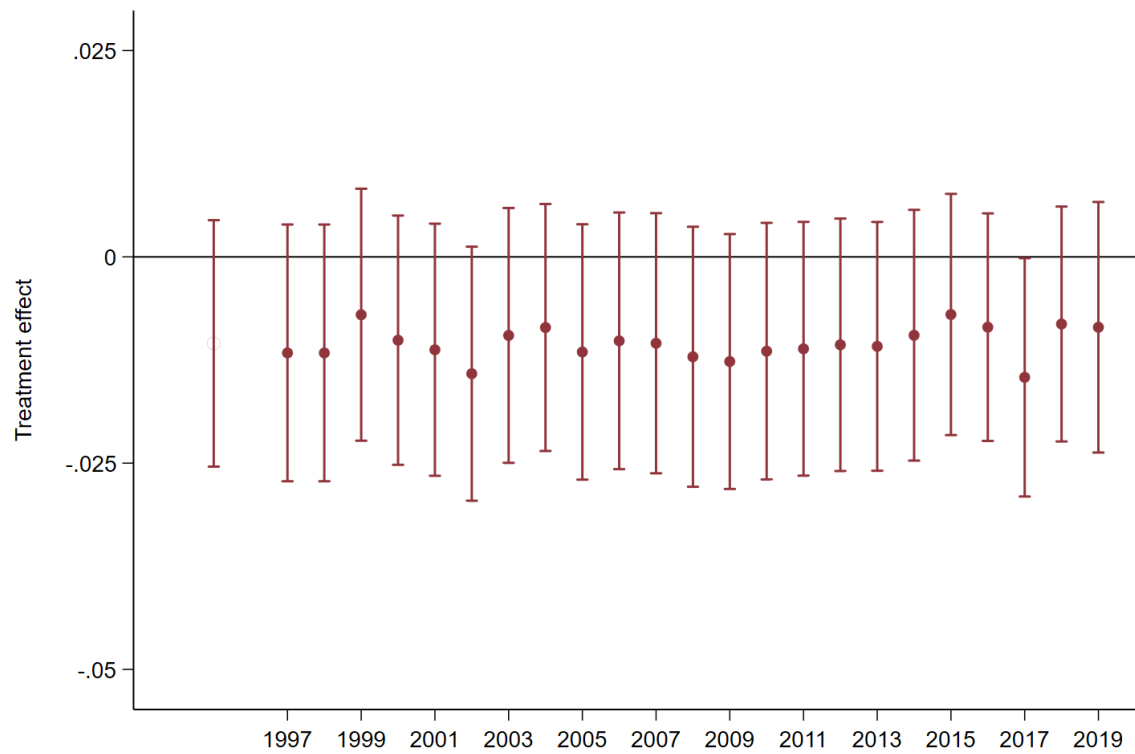
Source: authors' compilation.

Figure D5: 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.

Source: authors' compilation.

Figure D6: 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.

Source: authors' compilation.

Figure D7: Omitting one region at the time from the data set
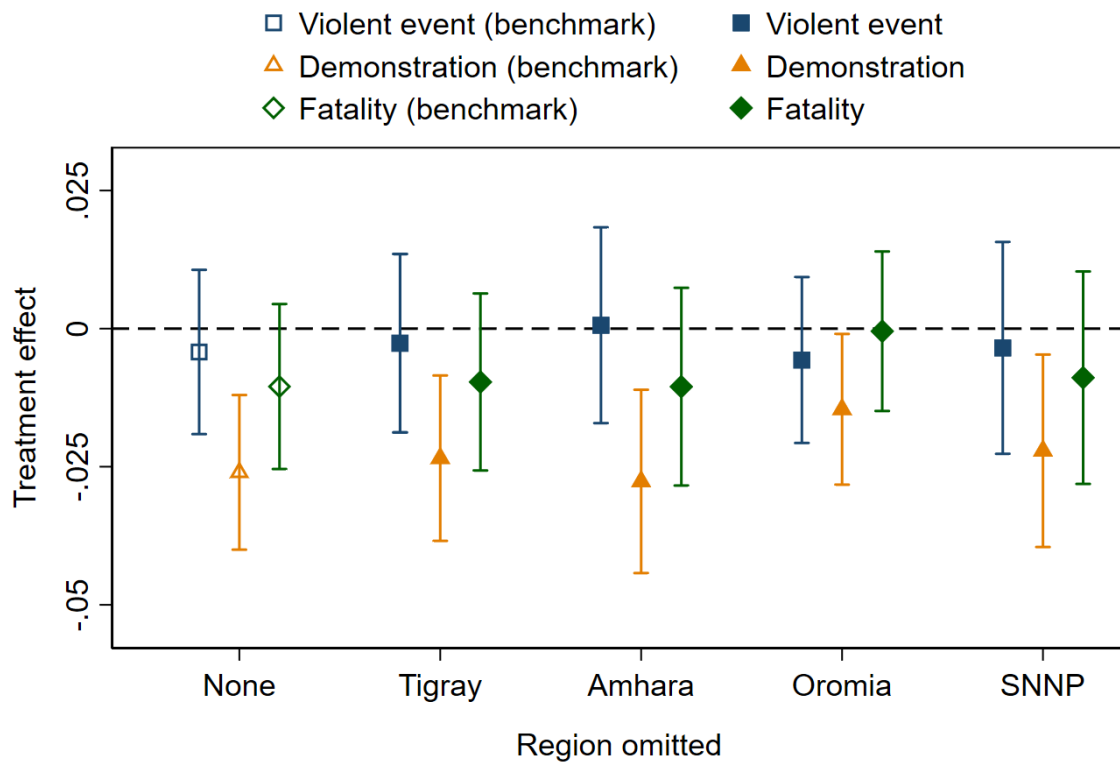
Note: the hollow symbols mark the benchmark treatment effect estimates. The solid symbols show the treatment effect estimate when a region is omitted from the data set. The capped lines represent 95% confidence intervals based on standard errors clustered at the district level.

Source: authors' compilation.

E   Analyses of the potential mechanisms

Figure E1: Impact of PSNP on likelihood of a demonstration, by month
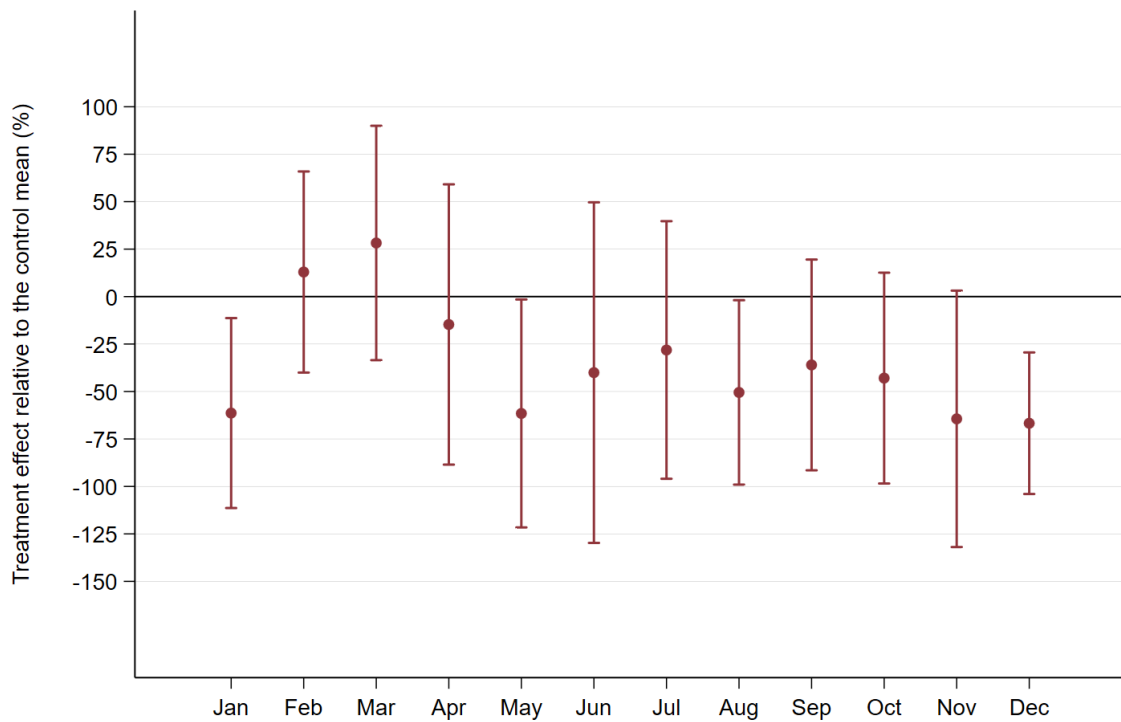
Note: N = 14,191 (23 years × 617 districts). Solid dots mark the treatment effects relative to the control mean and capped bars are the corresponding 95%–confidence intervals based on standard errors clustered at the district level.

Source: authors' compilation.

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) SPEI (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 (3). 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 * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Source: authors' compilation.

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 (4). 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 * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Source: authors' compilation.