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The effect of weather index insurance on social capital: Evidence from rural Ethiopia

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ABSTRACT

We study the effect of weather index insurance (WII) uptake on social capital. We measure individual social capital using experimental and survey-based measures and relate it to the actual purchase of WII. We use propensity score matching (PSM) and an instrumental variable (IV) to address endogeneity concerns. Our descriptive and PSM estimates show that WII uptake negatively and significantly affects social capital. We find that insured households contribute less to the public good than uninsured households. We also find qualitatively and quantitatively similar results using our IV approach, yet insufficient power renders most of our IV estimates insignificant. We however report robust significant effects of instrumented WII uptake on sociopsychological outcomes—WII uptake increases perceptions of self-sufficiency and free-riding behavior: these are potential channels through which a negative effect on social capital comes about. Although far from conclusive, our paper provides several pieces of evidence that suggest WII uptake negatively affects social capital.

KEYWORDS Ethiopia, free-riding, public-good game, self-sufficiency, social capital, weather index insurance

JEL CLASSIFICATION C93, G22, H41, O17

INTRODUCTION

Weather risks are ubiquitous in developing countries and a constant welfare threat to especially poor farmers. Weather risks have long remained uninsured because most households lack access to traditional crop insurance due to concerns of adverse selection, moral hazard as well as high transaction costs. In the absence of formal insurance markets, households rely on alternative informal insurance mechanisms to overcome a wide variety of risks. However, informal insurance mechanisms are best suited to overcome idiosyncratic rather than covariate risks, such as drought occurring at the village level or beyond (Morduch, 1999;Townsend,1994). The relatively recent introduction of index insurance in developing countries aims to fill this gap. Index insurance relies on the premise that it deals with classical asymmetric information problems as payouts are made based on a meteorological index, which is exogenously determined for the insured, and directly observed by the insurer (Leblois & Quirion, 2013).

Index insurance indemnifies covariate risks such as drought which are hardly covered by any pre-existing informal risk management strategies. A growing literature focuses on uncovering the drivers of demand for index insurance and, once adopted, its impact on agricultural investment and welfare. Previous studies show that despite heavy promotion, demand for index insurance is still very low due to both price and nonprice factors. Index insurance take-up decreases with price (Collier, 2019;Karlan et al., 2014; Mobarak & Rosenzweig, 2012), risk aversion (Hill et al., 2013), liquidity constraints (Belissa et al., 2019;Ginéetal.,2008), basis risk1 (Clarke, 2016;Collier,2019; Giné et al., 2008; Karlan et al., 2014), high wages in local labor markets and wages less sensitive to rainfall shocks (Mukherjee et al., 2021), lack of trust on the insurer (Belissa et al., 2019; Cole et al., 2013; Karlan et al., 2014), households' poor financial literacy (Awel & Azomahou, 2015) and enrollment in government social protection programs (Duru, 2016), but increases with subsidy, high fertilizer use (McIntosh et al., 2013) and wealth (Giné et al., 2008;Hilletal.,2013). In spite of the low average take-up, there is some evidence of its impact on agricultural investment and welfare. For example, Karlan et al. (2014), de Nicola (2015), and Cole et al. (2017) show that index insurance induces farmers to adopt riskier but more profitable technologies, and thus, enhances agricultural investments. The introduction of index insurance also substantially reduces poverty (Janzen et al., 2021).

Moreover, index insurance has impacts on ex post risk coping strategies, such as consumption smoothing and livestock protection (Bertram-Huemmer & Kraehnert, 2017; de Nicola, 2015;Janzen&Carter,2018) and subjective well-being (Tafere et al., 2018). We however do not know the effect of weather index insurance (WII) on less tangible outcomes, like, social capital. Given that high levels of social capital are associated with various aspects of economic development, it is important to understand if formal WII impacts social capital in any way. We define social capital here broadly following Putnam (1992)andColeman(1990) as a set of connections between individuals (networks) from which moral obligations and norms, trust and trustworthiness arise. We also follow (Glaeser et al., 2002) in that we look at individual decisions, and refer to this as individual social capital. This research fits into a broader literature investigating the effect of different types of insurance markets on social capital, including welfare programs (Attanasio & RıosRull, 2000), public transfers (Dercon & Krishnan, 2003), health insurance (Cecchi et al., 2016;Strupat&Klohn,2018), and context-free (experimental) insurance (Landmann et al., 2012;Linetal.,2014).

These studies document a crowding-out effect of social capital although a recent study by Takahashi et al. (2018) finds no effect of WII on what may be considered one of various elements of social capital: informal risk sharing—dabare.2

Our study contributes to this emerging field and empirically investigates the effect of WII on individual social capital as defined above. An important difference between our study and the ones listed above is that we focus on a specific insurance product that covers covariate (e.g., droughts) rather than idiosyncratic risks (e.g., illness). Also, our research differs from the study by Takahashi et al. (2018) in that we are interested in measures that capture multiple elements of social capital including cooperation in social dilemmas, trust and trustworthiness, and informal risk sharing, using outcomes of a one-shot linear publicgood game (PGG) and survey-based measures of social capital. Further, we focus on the effect of WII, which is a form of crop insurance, whereas Takahashi et al. (2018)investigate the effect of livestock index insurance. Our study is set in Ethiopia, home to the largest unsubsidized multifaceted WII scheme in Africa with adoption rates ranging between 18% and 32%. Welook at medium-term impacts related to insurance uptake some 6–8yearsafter its introduction. Since uptake is likely to be nonrandom we use propensity score matching (PSM) and an instrumental variable (IV) approach to address the endogenous uptake of WII. We exploit the low mobility of farmers in rural Ethiopia to instrument WII purchase with the self-reported distance of a household's residence to the location of the insurance foreman, the person who is in charge of advertising and selling the insurance product. Our empirical results are as follows.

Our descriptive and PSM estimates show that WII uptake negatively and significantly affects social capital. Specifically, the results show that insured households made fewer contributions to the public-good experiment as well as lower informal transfers to fellow villagers and financial contributions to community projects. Our findings from the IV approach show quantitatively and qualitatively similar results, in that WII uptake negatively affects social capital, yet coefficients become now largely insignificant due to insufficient power as indicated by an ex post power analysis (for more details, see Section 6.1.3). Finally, we find robust evidence that instrumented WII uptake affects individual sociopsychological attributes—insured households are more likely to believe that othervillagerstakeadvantageofthemifgiventhechanceandaremorelikelytoagreewith the statement that they can be successful on their own. These could be plausible channels through which a negative effect on social capital comes about, even though this is not fully borne out by our analyses. The rest of this paper is organized as follows. Section 3 provides the conceptual framework linking WII and social capital. Section 4 briefly discusses the evolution of WII in Ethiopia, the data sources and sampling procedure, our measurements of social capital, and descriptive statistics. Section 5 presents the estimation strategy. In Section 6, we present our key findings and a number of robustness checks. Section 7 concludes.

2 | MEASURING SOCIAL CAPITAL

Measuring social capital using quantitative methods is challenging as social capital has been defined and measured in several ways. Using a broad concept of individual social capital that contains elements of trust, trustworthiness, norms, social networks, and moral obligations we operationalize this concept through both experimental and survey-based measures. The use of an experimental PGG is commonly used in empirical economic studies to measure individual social capital in an incentive-compatible way. It is also used as an outcome to assess the impact of experimental field interventions, see, for example, Attanasio et al. (2015) and Cecchi et al. (2016) for the effect on social capital of conditional cash transfer and health insurance, respectively. Next to our experimental measure, we use two survey-based questions related to (i) whether the household made a private transfer to a fellow villager in the past 12 months and (ii) whether the household contributed to community projects in the past 12months. Finally, we use two questions related to beliefs about other villagers being fair, and perceived self-sufficiency to explore potential other sociopsychological effects that may plausibly underlie a causal relation from WII uptake to individual social capital measures.

Our experimental setting is a one-shot linear PGG with four participants. We chose a one shot version as to circumvent learning and reputation effects in multiround PGGs (Cecchi et al., 2016). Moreover, a one-shot PGG has an advantage over multiround games to avoid subject's fatigue. An important caveat of a one-shot public game is that it might not measure the outcome of real interest if subjects do not understand the game. In this study, much care has been taken to ensure that subjects understood the game. They participated in several trial rounds and were given questions of comprehension which should be correctly answered before they played the real game.

This is how the PGG was played. At the beginning of the experiment participants are given an initial endowment and asked how much of their endowment they want to contribute to the community project and how much to keep for themselves. The sum of the contributions to the community project is doubled by the experimenter and shared equally by all four members of the group regardless of their contribution to the community project. The share of the endowment that is kept privately is only beneficial to the individual who is keeping it. The payoff function for individual i from the public-good experiment is given as follows:

$$\pi_{(i)} = (E_i - C_i) + \alpha\left(\sum_{n=1}^{4} C_h\right), \tag{1}$$

where $\pi(i)$ denotes the payoff of individual i, Ei is the initial endowment, $C\ (i)$ is the contribution of individual i to the public good and α is marginal per capita return (MPCR) from the public good.

In this experiment, MPCR is equal to 0.5, meaning that each participant receives 0.5 Birr for each token contributed to the public good. At the beginning of the experiment, each participant receives an endowment, *Ei*, of 20 tokens (which is equivalent to 20 Ethiopian Birr) with one 10, one 5, and five 1Birr notes. The public-good experiment was conducted at local public schools and we invited 48 individuals (the household head) per tabia. Upon arrival, participants were randomly given an identification number and randomly allocated to a PGG group. Note that the experimenter knew to which group each individual belonged but the individual participant did not. Participants only knew that they would play the game with three other people in the room but had no idea who exactly, nor were they informed about the insurance status or any other characteristics of their fellow group members. Before the experiment began, we obtained informed consent from all participants. The experimenters provided oral instructions on the experiment as the majority of the participants were unable to read or write. After the instructions, we provided them with several examples to ensure they all understood the game and the calculation of payoffs. We checked for comprehension through several exercises and only once all the exercises had been completed and we were sure all respondents understood the procedures, we started the actual experiment. Participants had the opportunity to ask questions privately but were not allowed to talk with other participants during the entire experiment. In total, all 384 households' heads that were invited also participated in the experiment. Sessions took up to 1 h. Including a show-up fee, on average, participants earned 80 Ethiopian Birr.3

After completing the public-good experiment, respondents participated in a short socioeconomic survey. Participation rate was near 100%. Only one person did not participate in the survey for reasons unrelated to the research.4 Payments were made privately and in cash, immediately after the survey had been completed.

3 | CONCEPTUAL FRAMEWORK

How may WII affect social capital? The theoretical relationship between WII and social capital is ambiguous and WII could affect social capital in either direction. On the one hand, WII may crowd out social capital in at least three ways. First, WII purchase may encourage insured households to adopt or experiment with very risky technologies and thereby impose external costs (a moral hazard problem) on informal insurance groups, specifically, on the uninsured households. To get rid of these external costs, uninsured households may reduce their investment in social capital (Boucher & Delpierre, 2014). Second, in societies where there exist strong norms of redistribution, insurance payouts in the aftermath of common shocks may be expected to be shared, which implies WII purchase by an individual creates a positive externality on others (uninsured ones) and in turn induces free-riding behavior (de Janvry et al., 2014). Insured households are aware of this free-riding problem before they decide to purchase

WII. Therefore, to protect themselves from free-riders (uninsured households), insured households may decide to invest less in social capital. This argument is related to the evidence on adverse effects of social norms of redistribution. In developing countries,particularly in Sub-Saharan Africa (SSA), norms of redistribution of income are common (Collier & Garg, 1999) and Ethiopia is no exception to this. Accordingly, the adverse effects of coercive norms of redistribution are that people adopt costly strategies to hide their income to escape the pressure of the redistributive norms (Baland et al., 2011; Beekman et al., 2015; Boltz et al., 2019; Di Falco & Bulte, 2011; Di Falco et al., 2018; Grimm et al., 2017; Jakiela & Ozier, 2015). Third, as WII covers covariate risks such as drought in which the adverse effects span several years, insured households may perceive less of a need to depend on others and develop some form of individualistic or self-sufficient behavior. As a consequence, insured households may decide to invest less in social capital. In support of this hypothesis, growing studies in the economics and psychology literature have documented that market-based mechanisms induce people to be more selfish, individualistic, and materialistic. People exposed to such mechanisms also exhibit less fear of social exclusion, prefer solitary activities, and reduce interaction with others (Bowles, 2008; Di Tella et al., 2007; Vohs et al., 2006, 2008; Zhou et al., 2009).

On the other hand as WII and social capital insure two different types of risks, the introduction of WII may crowd in social capital. WII covers covariate risks, whereas social capital covers idiosyncratic risks, including basis risk. Dercon et al. (2014)andBergetal. (2017) show that the presence of an idiosyncratic basis risk in index insurance makes the two insurance mechanisms complement each other. However, they look at the opposite perspective in that whether informal insurance arrangements increase index insurance uptake. Second, WII may crowd in social capital as the introduction of WII often is part of a larger multifaceted risk management program and in any case always comes with some form of (group) training provided by the insurance company and other partners on, for example, payouts, liability, and soil fertility management (Oxfam America, 2013) which promotes villagers meeting and interacting with each other, thereby increasing social capital. This hypothesis is consistent with economic theory and empirical evidence that repeated interactions lead to higher trust and social capital (e.g., Cochard et al., 2004; Engle-Warnick & Slonim, 2004, 2006; Fudenberg & Maskin, 1986;Mehrotraetal.,2021). Third, insured households may share very important information in risk reduction activities and other information disseminated by the insurance companies to uninsured households. If insured households share relevant information to uninsured ones, the latter may respond by rewarding such action, for example, by strengthening their network with the former and provide help in times of need. This can be explained by the theory of reciprocity where people reward positive actions and punish negative ones (e.g., Falk & Fischbacher, 2006;Fehr&Gächter,2000;Rabin,1993).5

In sum, WII uptake may affect social capital positively, negatively, or both, and ultimately the net effect will be an empirical question. We now turn to such an example.

4 | CONTEXT, DATA, AND EXPERIMENTAL DESIGN

In this section, we discuss the evolution of WII, the data type and source, and the sampling strategy. We also present the experimental design and procedure, descriptive statistics, and balance test results across the treatment and control groups.

4.1 | WII scheme

As in many agrarian economies, Ethiopian farmers are constantly threatened by weather related shocks. The absence of formal insurance markets exacerbates adverse effects implied by these shocks. Since 2006 there has been experimentation of formal insurance with the World Bank implementing a pilot project with 28 farmers on WII in the Alaba district, southern Ethiopia. WII offers an annual payout when the rainfall amount is below a predetermined threshold level, for example, if the total amount of rainfall at key growth stages of the crops falls below 80–100mm, depending on the crop type. Payouts will take place once per year at the end of the harvest season. Unlike the traditional indemnity crop insurance, insurance payout in WII does not require plot level inspection and is based on the amount of rainfall measured by rain gauges in the villages or satellites. As a consequence, the insurance payout in WII is entirely exogenous to the insurance policyholder. The results from the pilot project revealed key issues that required considerable attention to scale up and sustain the project in the country. For more detailed information on the evolution of microinsurance in Ethiopia, see Amha et al. (2013).

The WII scheme was introduced in 2009 to strengthen farmers' food and income security in the region (Madajewicz et al., 2017). Adiha tabia in the Kolla Temben district was the first to receive the WII scheme. In 2010, the WII scheme scaled up to three districts—Kolla Temben, Raya Azebo, and Saesie Tsaedaemba. The WII scheme further scaled up to 11 districts and more than 80 tabias between 2010 and 2013.6 Since 2014, the focus was mainly on increasing the number of beneficiaries within treated villages rather than treating new villages, specifically in the Tigray region. Consequently, in 2017, the total number of insured households increased to about 27,136 farmers, of which about 23,567 farmers in the region of Tigray and 2322 farmers in the region of Amhara (Oxfam America, 2017) making it the largest WII scheme in Africa.

The WII scheme we study is part of the R4 Rural Resilience Initiative: an integrated risk management program combining four risk management strategies (asset creation, index insurance, diversification, and savings) for vulnerable rural households to better manage climate-related shocks. The program has been rolled out in 10 countries mostly in SSA, and is currently expanding into Latin America and the Carribean (WFP, 2022). In Ethiopia, districts are eligible to receive the R4 program if they are vulnerable to frequent droughts. The WII element is one of the four strategies and the only one for which tabias within R4-receiving districts need to fulfill an additional criterion: there needs to be a close match between satellite images on rainfall and farmers' self-reports on rainfall for WII to be introduced in the tabia. Thus, while the other parts of the R4 program were made available to all tabias in our sample, WII was not.

Districts have been selected to receive the R4 scheme based on their vulnerability to frequent droughts. Subsequently, tabias within selected districts were selected to receive access to WII based on the aforementioned “close match” criterion. Various tabias in our study region did not meet this criterion: there was no close enough match between satellite climate data and farmers' self-reports of rainfall. This criterion is important for insurance companies because the mismatch may indicate the presence of basis risk and they are less likely (and willing) to sell WII in areas with high basis risk. There are many possible reasons for such a mismatch to occur, some of them plausibly related to the difficulty of accurately measuring rainfall through satellite images in some areas. Studies by Kölle et al. (2021) and IRI (2013) report that satellite rainfall estimates work by taking images of clouds and inferring rainfall amounts from them. However, the rainfall estimated from satellites—derived from the detection and measurement of clouds—might not be accurate for a single pixel on a specific day. Excess cloud covers often complicate rainfall inference from clouds. Satellite images may also yield inaccurate drought estimates due to dust, solar angle, and satellite angle. Another challenge when using satellite images to measure rainfall anomalies is also that it may often generate large inaccuracies when used in different areas. While our sample comprises two neighboring districts, the area covers about 4000km2 which still allows for considerable variation in, for example, cloud cover. Alternatively, the mismatch may stem from inaccurate farmers' self-reports rather than problems with satellite images. While we have no detailed information on what exactly is causing the mismatch at the tabia level, we have little reason to believe that this is systematically related to individual-level variation in social capital. We also show that for most variables that are arguably time-invariant, there is no systematic difference between households with and those without access to WII. We return to these issues in more detail later.

The insurance product insures different types of crops grown (wheat, maize, teff, and sorghum). Insured farmers receive a payout once per year, at the end of the harvest season using satellite rainfall estimates. Farmers can choose between two insurance premiums, (I) the basic (minimum) premium where farmers pay a premium of about 160Birr with a corresponding liability of about 800 Birr ($36), or (ii) the maximum of about 500Birr premium with a corresponding liability of 3000 Birr ($136). Farmers, on average, pay a premium of about 20% of the insurance liability. Although farmers were provided with the option to select an insurance premium of 500Birr, they often preferred to pay the basic (minimum) premium. Nevertheless, even the basic policy has significant economic implications for poor farmers whose livelihood predominantly depends on rain-fed agriculture. Farmers can cover the cost of their primary farm inputs with minimum liability. In the rural areas of the Tigray region, the maximum liability of 3000Birr is nearly equivalent to 69% and 25% of annual per capita food and total (food and nonfood) consumption expenditure, respectively (National Planning Commission, 2017).

4.2 | Data

Our study is set in the Tigray region, northern Ethiopia, where about 80% of the population lives in rural areas, with their livelihoods mainly dependent on rain-fed agriculture. The region is characterized by low soil quality, sparse and erratic rainfall and frequent droughts (Hagos et al., 1999). More than half of the regional highlands are highly degraded and the average yield of cereal crops in the region is less than 1ton/ha, which is less than the national average of about 1.5 ton/ha. The average landholding per household in the region is about 1ha, which is also less than the national average of 1.37ha/household. Regional mean annual rainfall has been estimated at about 650mm ranging from 300 to over 1200mm (Pender & Gebremedhin, 2007). Tigray is one of the most drought-prone regions in the country with more than 25 severe drought periods in the last millennium (Di Falco et al., 2007). Tigray together with Wollo, in the northern part of the Amhara region, was the most severely affected area during the 1984 drought (Dercon & Porter, 2014).

Our target population comprises households in districts with frequent droughts eligible to receive the R4 program. In our sampling procedure, we did not include tabias that were not eligible to receive the R4 program. That is, tabias with less vulnerability to frequent droughts were excluded from the sampling frame. We use a multistage sampling procedure. First, we selected two randomly selected districts (Raya Azebo and Alamata). Both districts are vulnerable to frequent drought-related shocks. In the past 30years, drought occurs on average once every 3years. To address the adverse effects of frequent droughts, WII has been introduced in 2010 in Raya Azebo and in 2011 in Alamata districts. In total, 16 tabias have access to the WII scheme, of which 10 tabias are in Raya Azebo district and the remaining 6 tabias are in Alamata district. Of the 16 tabias with access to WII, we randomly selected five tabias: three from Raya Azebo district and two from Alamata district. We also randomly selected three tabias (two from Raya Azebo and one from Alamata district) without access to the WII scheme as follows. From all tabias exposed to frequent droughts we selected the no-access tabias that were eligible but left untreated (no access to WII) because of the mismatch between satellite and farmers' selfreported climate data.

We selected a total of 384 household heads using a two-stage stratified random sampling procedure. We stratified households based on their access to WII, and randomly sampled 240 with, and 144 households without, access to WII, respectively. Households within access tabias were subsequently stratified by their insurance status, that is, households that purchased WII and those that did not purchase WII. Of the 240 households with access to WII, 120 of them were insured households (i.e., those with a valid [not lapsed] contract during the data collection period) and the remaining 120 were uninsured households (i.e., those that did have access to, but have never purchased WII, in other words, the noncompliers in our treatment group). We used two sampling frames. Within each tabia we obtained a list of all insured households from the person responsible for selling the insurance (the insurance foremen) and randomly selected 120 insurance purchasers from this list. Our second sampling frame comprised a list of all households living in the tabia obtained from the village leaders. After earmarking all households that took up insurance, we randomly selected 120 households among those that were not insured. Name lists were cross-checked with the insurance foremen.

The tabias in our sample are similar in population size (each tabia constitutes 18%–22% of the total number of households in our sample) (Table 1). For this reason, we keep the sample size constant and randomly selected the same number of observations from each tabia. We also oversampled the insured households to obtain a sufficiently large number of observations from this group for the estimation and used appropriate sampling weights in our regression analysis. Data were collected on tablets during the slack period of March–May 2017.7 We invited 384 household heads that all agreed to participate in a public-good experiment that we use as our experimental measure of social capital. After the experiment, the participants were asked to participate in a short socioeconomic survey. The survey included questions on household assets and wealth, details on insurance access, insurance purchase, household

TABLE1 Weather index insurance (WII) adoption rate in treated villages.

District	Village	Total households	Insured households	WII adoption rate (%)
Raya Azebo	Hadealga	1952	625	32
	Hawelti	2275	398	18
	Mechare	1874	368	20
Alamata	Gerjale	2045	425	21
	Laelay Dayu	1818	357	20

Source: REST office in Raya Azebo and Alamata districts.

demographics, and proxies for social capital including informal transfers and financial contributions to community projects.

4.3 | Descriptive statistics

Table A1 in the appendix shows the summary statistics of our sample households. The total sample size is 383 household heads of which 76% are male-headed households. The average age of a household head is about 41years and the mean active number of household members in the sample is 2.65, which is slightly higher than the regional average. Nearly, 5% of the household heads have secondary levels of education, 22% have primary levels of education, 13% of the respondents have no formal education but can read and write, while the remainder 60%are illiterate. Themean landholding of households in the sample is 4.616 tsimad (approximately 1.15 ha),8 which is slightly higher than the regional average of only 1ha/household (Pender & Gebremedhin, 2007). Mean livestock holdings are 4.42 Tropical Livestock Units (TLU).9 Nearly 46% of the households in the sample own corrugated iron sheet roof houses and the remaining 54% own houses with a thatched roof. About 33% and 20% of the household heads declare that they watch TV and listen radio at least once a week. On average, participants contributed about 45% of their endowment to the public good, which is consistent with earlier evidence on the contribution in the PGG game.10

Table A2 in the appendix presents the balance test of sample households disaggregated by their purchase of WII. The results in Table A2 show that while insured and uninsured households are similar in most of the variables tested, we still find significant differences in four variables across the two groups. Uninsured households have more active members, have more livestock and farming land, and are more likely to own houses with a corrugated iron sheet roof than insured households. Table A3 in the appendix also presents the balance test of sample households disaggregated by access to WII. Table A3 shows that for most of the variables that are arguably time invariant, there is no systematic difference between households with and without access to WII, except that households with access to WII have more livestock.

5 | ESTIMATION STRATEGY

We analyze the effect of WII uptake by estimating the following regression model:

$$Sc_i = \alpha_1 + \beta_1 I_i + X_i\gamma_1 + \varepsilon_i, \qquad (2)$$

where Sci, the number of tokens shared in the PGG by household *i*, is our measure of individual social capital, *Ii* is a dummy variable that takes value of 1 if household *i* has purchased WII, 0 otherwise, *Xi* is a vector of controls, *εi* is the random error term, and *β* measures the effect of WII on social capital. We expect β to be negative (positive) if WII crowds out (in) social capital. WII uptake in our social capital equation (2), however, is endogenous as a household may self-select into WII purchase on the basis of unobservables, like, innate ability, that simultaneously drive the decision to purchase WII and investment in social capital. Also, reverse causality may be at play with a relation running from social capital to WII purchase. To attenuate these endogeneity concerns we employ an IV approach. To estimate the causal impact of an endogenous WII purchase on social capital we estimate the following equations:

$$Sc_i = \kappa_0 + \kappa_1 I_i + X_i\lambda + \varepsilon_i, \tag{3}$$

$$I_i^* = \pi_0 + \pi_1 z_i + X_i\psi + \upsilon_i, \tag{4}$$

$$I_i = \begin{cases} 1 & \text{if } \; I_i^* > 0, \\ 0 & \text{if } \; I_i^* \leq 0. \end{cases}$$

In Equations (3) and (4), εi and υi are correlated, that is, Cov(εi,υi)$\neq 0$. To obtain consistent estimates of $\kappa 1$ while the Cov(εi,υi)$\neq 0$, we need to have an IV, zi, which is correlated with I and uncorrelated with εi. The choice of our IV is guided by the literature. Geographical factors such as distance have been identified as exogenous key determinants of various socioeconomic variables. As a consequence, several studies used distance as IV. Dee (2004) used distance to college as an instrument for education attainment, and Neal (1997) used geographical proximity to Catholic schools as a source of exogenous variation in school attendance to measure its effect on test scores. More recently, Melesse and Cecchi (2017) used distance from markets as an instrument for farm-households market exposure. In this study, we use being geographically close to the insurance foreman (distance from farm-household's residence to the insurance foreman's residence) as an instrument for WII uptake.

WII is not an easy concept that farmers with low literacy levels understand instantly. Hence, for farmers to make an informed decision, they need to have adequate knowledge of the insurance product through various training sessions. The insurance company therefore trains local villagers, the so-called “insurance foremen,” who liaise between the farmers and insurance companies, and play a vital role in providing training and in turn improving farmers’ understanding of index insurance, marketing, and sales of the insurance product (Madajewicz et al., 2017). As the foreman has detailed knowledge about the insurance policy, he is the only person easily reachable to answer farmers' questions about the insurance policy. Accordingly, we expect that farmers living closer to the insurance foreman have the advantage of getting adequate information about the insurance policy and are more inclined to purchase the insurance product.

We argue that living at a closer distance to the insurance foreman's home is plausibly exogenous. Mobility of rural farmers is very low, and hence they are unlikely to self-select into being geographically closer to the insurance foreman. Of course one may worry that foremen are selected from (centrally located) areas with particular characteristics that also affect social capital, like, wealth, social network density, or education levels. During the data collection we observed no systematic pattern in the spatial location of the foreman. Ideally, we would have had GPS locations of all of our households and the insurance foreman, to substantiate our claim that there is no systematic pattern in where the foreman is located. We however do not have this information and instead provide two additional arguments why the location of the foreman is plausibly exogenous.

First, any farmer who can read or write and does not hold other positions in the village like village head or agricultural extension officer is eligible to become an insurance foreman. The insurance team is in charge of selecting the insurance foreman. The insurance team advertises the vacancy and then selects the insurance foreman based on his/her literacy level.11 This appointment is thus essentially a higher-level administrative process and it is unlikely that the insurance team is able to a priori predict who might be really good at selling insurance—for example, those with largest and/or most dense social networks which would otherwise compromise the exogeneity argument. Second, the fact that insurance foremen are not paid on a commission basis but receive a monthly fixed salary of 900 Ethiopian Birr (approximately $40) supports the idea that distance matters—also for the insurance foreman, as it is easier for the foreman to visit farmers that live close by than households that live further away. We also provide a series of tests to provide additional support for our claim to exogeneity. Among other tests, we perform falsification tests and check whether the instrument has only an indirect effect on the outcome variable through WII uptake (see Section 6.1.3 for more details).

6 | RESULTS

6.1 | Effect of WII on social capital

6.1.1 | Descriptive analysis

Westart our analysis with simple descriptive statistics of our experimental and survey outcome measures for insured and uninsured households within the access tabias. Figure 1 shows the unconditional means of contribution to the public good across the two groups. We see that uninsured and insured households contribute, on average, about 9.4 and 8 tokens, respectively. Likewise, Figure 2 presents the plot of the cumulative distribution of contributions to the public good and reports qualitatively similar results. Further, we look at differences in distributions— again between the insured and uninsured in the access tabias.

Table 2 (columns 1 and 2) presents the two-sample Kolmogorov–Smirnov test and shows that the contribution to the public-good experiment for uninsured households contains larger values than for insured households, and the largest difference between the distribution functions is 0.158. The approximate asymptotic p value for that difference is 0.049 for the one-sided test and 0.099 for the combined test. Figure 3 presents the unconditional mean of making a private transfer and

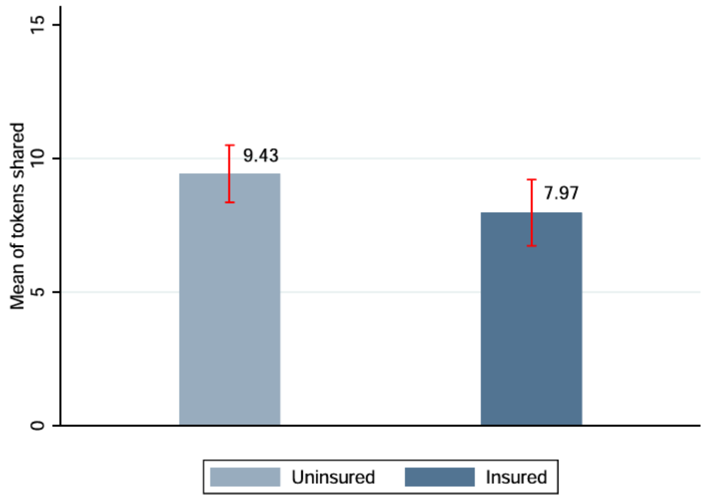

FIGURE1 Contribution to the public-good game, 95% confidence interval.

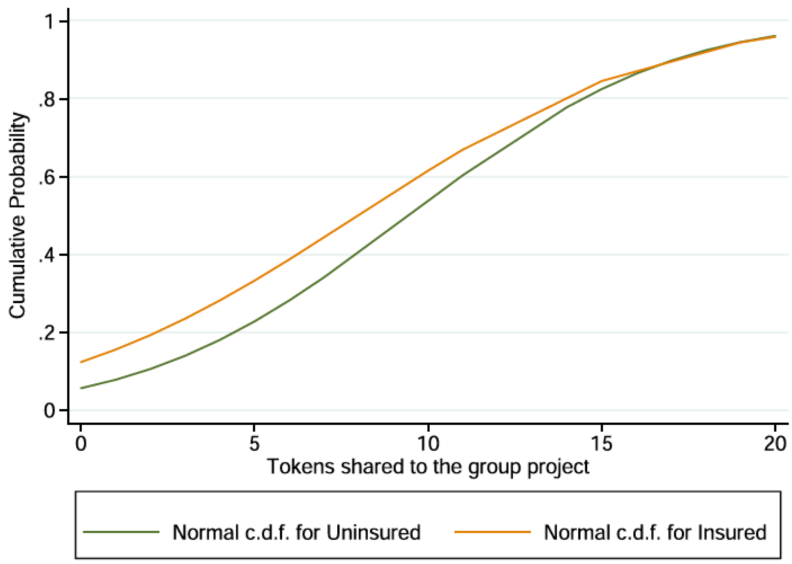

FIGURE2 CDFplot distribution of contributions to the public good. CDF, cumulative distribution function.

TABLE2 Two-sample Kolmogorov–Smirnov (K-S) test—WII uptake.

	Contribution in PGG		Private transfer		Financial contribution	
	(1)	(2)	(3)	(4)	(5)	(6)
	D	*p* Value	*D*	*p* Value	*D*	*p* Value
Uninsured	0.067	0.587	0.000	1.000	0.000	1.000
Insured	−0.158	0.049	−0.108	0.245	−0.158	0.049
Combined K-S	0.158	0.099	0.108	0.482	0.158	0.099

Abbreviation: PGG, public-good game; WII, weather index insurance.

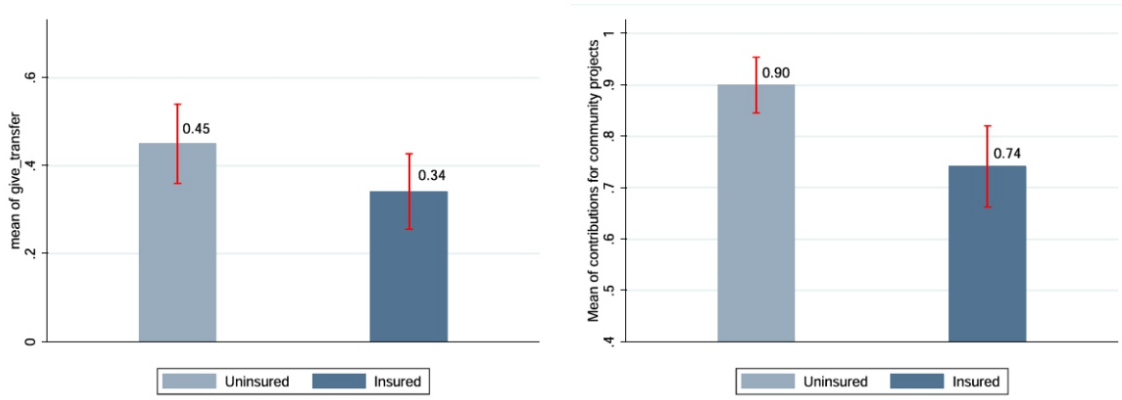

FIGURE3 Private transfers and financial contribution across insured and uninsured groups, 95% confidence interval.

financial contribution to community projects. It shows that 34% and 45% of insured and uninsured households, respectively, made private transfers to fellow farmers. On the other hand, 74% and 90% of insured and uninsured households, respectively, made financial contributions to community projects within a village. Columns (3)–(6) of Table 2 also show no difference for private transfers and a marginally significant difference for financial contributions to community projects for the insured versus the uninsured. The results from the descriptive analyses are indicative of insured households having somewhat lower levels of social capital, depending on which measure of social capital we use. We now turn to our formal regression analyses to further explore this correlation.

6.1.2 | Propensity score matching

We use a PSM approach to get rid of some of the imbalances we have in our covariates. The credibility of our matching results depends on whether the two underlying assumptions— conditional independence assumption (CIA) and common support condition—are satisfied. To check whether the CIA is met, we conduct a two-sample t test to test for the difference in observable means for insured and uninsured households before and after matching.

We also conduct a chi-square test for the joint significance of variables used in the propensity score estimation before and after matching. Table A4 in the appendix shows that although there were significant differences in some of the covariates before matching, these differences disappear after matching, and matching has substantially reduced the difference in mean bias between insured and uninsured households. Table 3 presents overall statistics about the quality of the matching. The chi-square (p χ 2 > =0.990) test indicates that the covariates are not jointly significant after matching. Also the low pseudo-R2 (pseudo-R =0.011 2) indicates no systematic difference in the distribution of covariates between insured and uninsured households after matching. Moreover, Table 3 presents the mean difference before and after matching. It shows that the mean standardized bias has reduced from 20.2% before matching to 4.4% after matching. This implies that matching has reduced total mean bias by about 78%.

We then proceed by checking the common support condition after estimating the propensity scores for insured and uninsured households. The predicted propensity scores for insured and uninsured households range from 0.125 to 0.957, and 0.053 to 0.829, respectively. The common

TABLE3 Matching quality test (WII uptake).

	(1) Pseudo-R^2	(2) $p > \chi^2$	(3) Mean bias
Before matching	0.130	0.000	20.2
After matching	0.011	0.990	4.4

Abbreviation: WII, weather index insurance.

TABLE4 Impact of WII on social capital—Matching results.

	Insured	Uninsured	Difference
Unmatched	7.975	9.433	−1.458*
			(0.831)
Matched (ATT)	8.026	11.076	−3.051***
			(1.066)

Note: Standard errors are in parentheses. Abbreviation: WII, weather index insurance.
***p< 0.01; **p< 0.05; *p<0.1.

support condition is thus satisfied in the region of 0.125–0.829, which indicates a substantial overlap between insured and uninsured groups, as also shown in the propensity score distributions and density distribution graphs presented in Figures A1 and A2 in the appendix.

Table 4 presents the treatment effect of WII on social capital using our PSM approach. We used Kernel matching with the default kernel type and bandwidth. Table 4 presents a negative and statistically significant effect of WII on social capital. However, since PSM is unable to control for unobservables, we proceed to the IV estimation that accounts for potential unobservables.

6.1.3 | IV estimation results

Before running the IV regression model, we check the validity of our instrument (distance to foreman) in several ways. First, to probe whether the IV is robustly correlated with the endogenous explanatory variable (WII uptake), we run a regression with WII uptake as dependent and the IV as the key explanatory variable plus a set of household covariates and village fixed effects in Table 5. Our regression results show a negative and statistically significant correlation between the distance to the insurance foreman's residence and the purchase of WII (columns 1 and 2). This indicates that the first criterion for a valid instrument—the instrument should be significantly correlated with the endogenous explanatory variable (WII uptake)—is effectively met. Second, we probe the validity of our instrument by performing a falsification test as in Di Falco et al. (2011). According to this test, a variable is a valid instrument if it affects the endogenous variable but not the outcome variable of households that did not receive the treatment. In our case, the distance to the insurance foreman's residence is a valid instrument if it significantly affects the insurance uptake variable, but not the investment in social capital by the uninsured households. Columns (3) and (4) show that among

TABLE5 Tests on the validity of the selected instrument.

(6) Falsification treated group	(7) Intention -to-treat effect	(8) Falsification (unconditional) test
		−3.185***
		(1.040)
0.076	0.049*	0.009
(0.059)	(0.029)	(0.032)
0.067	0.100**	0.108**
(0.068)	(0.045)	(0.044)
−1.597	−2.521*	−3.118**
(1.807)	(1.366)	(1.400)
0.665*	0.354	0.414*
(0.353)	(0.219)	(0.210)
2.360	1.085	1.090
(1.459)	(0.903)	(0.900)
−0.191	−0.181	−0.217
(0.276)	(0.141)	(0.140)
−0.146	0.077	0.046
(0.216)	(0.099)	(0.098)
1.822	−2.131**	−2.207**
(1.582)	(0.993)	(0.987)

	(1) First stage IV	(2) First stage IV	(3) Falsification control group	(4) Falsification control group	(5) Falsification treated group
WII uptake					
Distance to foreman	−0.013***	−0.013***	−0.023	0.015	0.046
	(0.002)	(0.002)	(0.039)	(0.039)	(0.055)
Age		0.003		0.120**	
		(0.003)		(0.056)	
Male		−0.258***		−3.152*	
		(0.098)		(1.883)	
Active number of people		0.012		0.461*	
		(0.012)		(0.261)	
Education		0.023		0.815	
		(0.045)		(1.105)	
Land size		−0.013*		−0.264	
		(0.007)		(0.162)	
Livestock		−0.005		0.065	
		(0.005)		(0.111)	
Corrugated iron sheet		−0.052		−2.989**	
		(0.042)		(1.180)	

TABLE5 (Continued)

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(1) First stage IV	(2) First stage IV	(3) Falsification control group	(4) Falsification control group	(5) Falsification treated group	(6) Falsification treated group	(7) Intention -to-treat effect	(8) Falsification (unconditional) test
	0.013		−3.026**		3.617*	−1.858	−1.652
	(0.060)		(1.431)		(1.916)	(1.195)	(1.212)
	0.161**		3.962***		−1.263	2.591**	3.007***
	(0.067)		(1.310)		(1.698)	(1.055)	(1.061)
	0.002		−0.026		−0.041	−0.034	−0.028
	(0.002)		(0.046)		(0.048)	(0.034)	(0.034)
	0.097***		−0.727		−5.441**	−1.965	−1.561
	(0.035)		(1.437)		(2.329)	(1.193)	(1.202)
	0.020**		0.508**		−0.095	0.386**	0.438**
	(0.009)		(0.223)		(0.319)	(0.178)	(0.182)
		10.580***	5.534*	7.218***	9.273**	5.566***	6.930***
		(1.455)	(3.153)	(1.295)	(4.017)	(2.530)	(2.537)
240	240	120	120	120	120	240	240
		0.003	0.256	0.005	0.146	0.148	0.180

Watch TV
Listen radio
Number of years lived in villag
Religion
Risk preference
Constant
Observations
R^2

Note:The dependent variable in columns (1) and (2) is WIIuptake. Whereas ,the dependent variable in columns (3and4)and(5and6)is the number of tokens shared in the public-good game (PGG) by both insured and uninsured households. Robust standard errors are in parentheses. Abbreviations: IV, instrumental variable; TV, television.

***p<0.01; **p<0.05; *p<0.1.

uninsured households there is no effect of the instrument on contributions to public good.12 Third, as the falsification test proposed by Di Falco et al. (2011) imposes some conditions in the sense that the instrument should not be correlated with the outcome variable only for the control group, we further probe the validity of the instrument by relaxing this condition. We extend the falsification test to insured households in columns (5) and (6). Interestingly, we find that among insured ho use holds the re is no effect of the instrumenton contributions to public good. Column (7) runs the test on the unconditional sample (both insured and uninsured sample) and shows a positive and (borderline) significant ITT effect of the instrument on social capital. As we have only one instrument for one endogenous variable, we cannot run an over identification test.

Although imperfect, we check whether the IV directly affects the outcome variable or only indirectly through the purchase of WII in column (8). Once we include our instrument together with the endogenous variable there is no direct effect anymore of the instrument on social capital. Fourth, we check the association between the IV and potential confounders. Table A5 in the appendix shows that all of the potential confounders except the number of active people in a household—a proxy for household size—are insignificantly correlated with the IV. A test of overall model significance also shows that the explanatory variables included in our regression model do not collectively have a statistically significant effect on the IV at a 5% level of significance (F[13, 226] =1.56 or p F > =0.099).

We next turn to running our IV model. Columns (1)–(3) of Table 6 present the IV results ranging from the parsimonious model column (1), where we only include WII uptake and village fixed effects as an explanatory variable, to our preferred specification in column (3) that includes a set of relevant covariates and village fixed effects. In all cases, referring to the Stock et al. (2002) critical values for the relevance test, the associated partial F statistic exceeds the minimum 10 threshold value, and we reject the null hypothesis of having a weak instrument. We find qualitatively similar results as in our PSM estimates in that WII uptake negatively affects social capital. However, we find that the negative effect of WII uptake on contributions to the PGG is not robust across all specifications. Column (2) of Table 6 shows that insured households contributed about 3.9 fewer tokens to the public good than uninsured households. While this effect of WII uptake on contributions to the public good is significant at a 10% significance level, this effect dissipates once we include the village fixed effects in column (3).

Next to our experimental PGG outcomes, we use two alternative survey-based measures of social capital: (i) making private transfers to fellow villagers and (ii) financial contributions to community projects in the past 12months. Our naive probit regression results in columns (1) and (3) of Table 7 show that insured households are less likely to make private transfers to fellow villagers and financial contributions to community projects than uninsured households, but results become insignificant once we account for unobserved heterogeneity using our IVs approach. These results are qualitatively similar to the experimental outcomes in Table 6 and may indicate that experimental and survey-based measures capture a similar underlying concept we call social capital. The results of the IV estimation indicate that although the point estimates lack statistical significance, they are economically meaningful. Consequently, it is difficult to rule out the impact of WII uptake on social capital solely based on the lack of statistical significance.

TABLE6 Iv estimates on the effect of WII on social capital (experimental measures).

	(1) **Tokens shared**	(2) **Tokens shared**	(3) **Tokens shared**
WII uptake	−1.790	−3.879*	−2.513
	(1.897)	(2.204)	(1.858)
Age		0.110**	0.049
		(0.044)	(0.042)
Male		−3.248**	−3.145***
		(1.359)	(1.153)
Active number of people		0.427**	0.235
		(0.214)	(0.196)

Education		1.090	0.486
		(0.874)	(0.767)
Land owned		−0.225*	−0.070
		(0.133)	(0.112)
Livestock		0.040	0.015
		(0.101)	(0.083)
Corrugated Iron roof		−2.224**	−0.584
		(0.963)	(0.851)
Watch TV		−1.607	0.075
		(1.165)	(0.983)
Listen radio		3.098***	1.886**
		(1.118)	(0.951)
Number of years lived in village		−0.027	−0.033
		(0.032)	(0.031)
Religion		−1.473	−0.735
		(1.132)	(1.086)
Risk preference		0.449**	0.160
		(0.181)	(0.155)
Constant	11.839***	7.227***	12.650***
	(0.752)	(2.200)	(2.295)
Village fixed effects	Yes	No	Yes
Observations	240	240	240
R^2	0.299	0.178	0.348
First-stage F test	75.21	62.46	58.80

(Continues)

TABLE6 (Continued)

	(1) Tokens shared	(2) Tokens shared	(3) Tokens shared
Distance to foreman	−0.012***	−0.012***	−0.013***
	(0.001)	(0.001)	(0.002)
Confidence Interval of WII uptake	[−5.508 1.929]	[−8.199 0.441]	[−6.154 1.129]

Note: The dependent variable is the number of tokens shared in the public-good game. Robust standard errors are in parentheses. Abbreviations: IV, instrumental variable; TV, television; WII, weather index insurance. ***p< 0.01; **p< 0.05; *p<0.1.

To understand whether the noisy but economically meaningful point estimates are a precision issue, we estimated the Minimum Detectable Effect (MDE) for our realized sample.13 Here we estimate the MDE for our experimental measure of social capital, that is, tokens shared in the public-good experiment. Given our realized sample size of 240 observations the estimated MDE would be 2.337. This means that effects bigger than the MDE we would be able to pick up with our realized sample. Yet these are relatively large effects considering the mean difference of 1.458 between insured and uninsured households.14

6.2 | Impact of access to WII on social capital

In this section, following Cecchi et al. (2016), we seek to estimate the effect of having access to WII on social capital, irrespective of whether individual households took up the treatment. This allows us to get some idea on the role of interaction/spillovers that may potentially have been at play when comparing insured and insured households within the same (access) tabias. To be able to compare access and no-access tabias we first need to credibly establish that tabia-level access is plausibly exogenous to the individual household. Naturally we cannot use our IV here since the no-access villages do not have an insurance foreman. We thus revert to two other strategies including preintervention data for access and no-access areas and, as above, a propensity score approach. As already noted, the WII scheme was introduced between 2010 and 2012. Thus, to explore whether access and no-access villages were comparable before the introduction of the insurance scheme, we are able to use the 2007 Ethiopian Population and Housing Census data. While there is no information on social capital in these data, we can still compare intervention and nonintervention villages on the basis of other relevant characteristics, some of which are plausibly related to social capital. Table 8 presents the balance test of farm households in access and no-access villages. It shows that households in access and no-access villages are not systematically different in most of the variables tested, except that no-access villages are dominated by male-headed households more than access villages, and that no-access villages are more religiously diversified than access villages.

TABLE7 Effect of WII on social capital (survey measures).

	Private transfer		Financial contributions	
	(1)	(2)	(3)	(4)
	Probit	IV-LPM	Probit	IV-LPM
WII uptake	−0.162**	−0.104	−0.168***	−0.167
	(0.071)	(0.171)	(0.052)	(0.126)

Age	0.009**	0.008**	0.004*	0.003
	(0.005)	(0.004)	(0.002)	(0.002)
Male	−0.110	−0.093	0.089	0.079
	(0.113)	(0.105)	(0.065)	(0.072)
Active number of people	−0.023	−0.014	0.013	0.019*
	(0.023)	(0.019)	(0.009)	(0.011)
Education	0.039	0.034	0.006	0.005
	(0.087)	(0.077)	(0.034)	(0.051)
Land owned	0.018	0.014	−0.009	−0.009
	(0.014)	(0.011)	(0.006)	(0.009)
Livestock	−0.013	−0.011	0.004	0.004
	(0.010)	(0.009)	(0.004)	(0.004)
Corrugated Iron roof	−0.012	−0.010	−0.056	−0.069
	(0.095)	(0.086)	(0.039)	(0.052)
Watch TV	0.121	0.096	0.050	0.056
	(0.120)	(0.100)	(0.034)	(0.059)
Listen radio	−0.127	−0.120	0.032	0.057
	(0.103)	(0.100)	(0.037)	(0.063)
Number of years lived in village	−0.003	−0.003	−0.002*	−0.003*
	(0.003)	(0.003)	(0.001)	(0.002)
Religion	−0.013	−0.018	−0.048	−0.065
	(0.122)	(0.100)	(0.029)	(0.048)
Risk preference	0.085***	0.074***	−0.028***	−0.030***
	(0.022)	(0.018)	(0.009)	(0.009)
Constant		−0.209		0.933***
		(0.242)		(0.129)
Village fixed effects	Yes	Yes	Yes	Yes
Observations	240	240	240	240
Pseudo-R^2	0.144		0.236	
R^2		0.178		0.162

(Continues)

TABLE7 (Continued)

	Private transfer		**Financial contributions**	
	(1)	**(2)**	**(3)**	**(4)**
	Probit	**IV-LPM**	**Probit**	**IV-LPM**

First-stage *F* test	58.80	58.80
Distance to foreman	−0.012***	−0.012***
	(0.002)	(0.002)

Note: The dependent variable in columns (1) and (2)is a dummy taking a value of 1 if household I made a private transfer to a fellow villager, 0 other wise. The dependent variable in columns (3) and(4) is also dummy taking a value of 1 if household imade a financial contribution to community projects, 0 otherwise. Columns (1) and (3)report marginal effects. Robust standard errors are in parentheses.

Abbreviations: IV, instrumental variable; LPM, linear probability model; TV, television; WII, weather index insurance.

***p<0.01; **p<0.05;*p<0.1.

TABLE8 Balance test by access to weather index insurance using the 2007 census data.

	(1) No access	(2) Access	(3) Difference	(4) *p* Value
Age of the household head	43.720	43.769	−0.049	0.949
Male headed	0.732	0.670	0.062	0.005
Marital status of the household head	2.523	2.567	−0.044	0.722
Literacy level of the household head	43.720	43.769	−0.049	0.949
Currently attending school	0.038	0.041	−0.003	0.782
Employment status of the household head	0.166	0.155	0.011	0.532
Religion of the household head	0.627	0.815	−0.188	0.000
Observations				1935

Source: Central Statistical Authority, Ethiopia.

Since women are believed to contribute more than men to public goods and more religious diversityisassociatedwithlowerpublic-goodcontributionswewouldexpectno-accesstabiasto have lower levels of social capital while—albeit insignificant—almostallofourfindingspoint in theoppositedirection,suggestingthattheseimbalancesareunlikelytodriveourresults.

To enhance the comparability between access and no -access tabias, we use the following strategies. First, we run a probit model with WII uptakeas the dependent variable and the controls we used throughout our analyses. On the basis of these estimates, we predict theprobability of purchasing WII across households with and without access to WII. We furthermatch access and no - access households using the probability of purchasing WII and a set ofcontrols.15 Finally, we run the OLS estimation method only on the matched sample. The estimation results in Table 9 show that, though insured households are less likely to contribute

TABLE9 Impact of access to WII on social capital.

	Insured versus no access			Uninsured versus no access		
	(1)	(2)	(3)	(4)	(5)	(6)
Access to WII	−1.347	−1.327	−0.742	−0.034	−0.036	0.439
	(0.859)	(0.986)	(0.857)	(0.802)	(0.855)	(0.725)
Age		0.115**	0.136***		0.130***	0.136***
		(0.051)	(0.044)		(0.049)	(0.043)
Male		0.901	0.847		0.421	0.251
		(1.244)	(1.108)		(1.202)	(1.043)
Active number of people		−0.068	−0.277		−0.487*	−0.703**
		(0.285)	(0.266)		(0.283)	(0.291)
Education		1.844**	1.604*		1.128	0.787
		(0.931)	(0.831)		(0.826)	(0.725)
Land owned		−0.107	−0.065		−0.220*	−0.217**
		(0.142)	(0.135)		(0.122)	(0.109)
Livestock		0.069	0.018		0.127	0.064
		(0.147)	(0.131)		(0.120)	(0.095)
Corrugated iron sheet		−0.245	−0.290		−1.494*	−1.122
		(0.934)	(0.829)		(0.820)	(0.711)
Watch TV		1.193	1.439		−2.476**	−1.923*
		(1.278)	(1.187)		(1.146)	(1.070)
Listen radio		−1.209	−2.363**		1.503	0.290
		(1.167)	(1.062)		(1.039)	(0.937)
Number of years lived		−0.029	−0.025		−0.012	−0.008
		(0.036)	(0.030)		(0.033)	(0.030)
Religion		0.277	0.894		1.466	1.487*
		(1.220)	(1.022)		(1.003)	(0.871)
Risk preference		−0.260**	−0.007		−0.037	0.101
		(0.114)	(0.120)		(0.162)	(0.149)
Belief about others contribution			0.664***			0.601***
			(0.075)			(0.070)
Constant	9.338***	6.366***	−2.876	9.338***	5.369***	−1.359
	(0.567)	(1.915)	(2.038)	(0.567)	(1.864)	(1.899)
Observations	256	256	256	254	254	254
R^2	0.010	0.066	0.285	0.000	0.094	0.297

Note: The dependent variable is the number of tokens shared in the public - good game. Robust standard errors are in parentheses. Abbreviations: TV, television; WII, weather index insurance.
***p<0.01; **p<0.05;*p<0.1.

to the public good than those households in the nonaccess tabias, the estimation results are not statistically significant. Likewise, we find no significant difference in contribution to the public good between uninsured households in the access tabias compared to those households in the nonaccess tabias, suggesting spillovers are not that relevant in this context.

6.3 | Sociopsychological effects of WII

In this section, we look at other impacts that may plausibly be affected by WII uptake. Specifically, we look at three variables that could arguably serve as channels through which WII uptake affects social capital or be affected directly by WII uptake. First, we scrutinize the argument put forward by (Boucher & Delpierre, 2014) that the introduction of WII may crowd out informal risk - sharing arrangements if the informal risk - sharing groups suffer from a moral hazard problem. That is, WII may encourage insured households to adopt or experiment very risky technologies and impose external costs on the informal risk - sharing groups. The uninsured could then respond by lowering their investment in social capital. For moral hazard to be an underlying mechanism in this context, we should observe insured households to significantly adopt more risky technologies suchas chemical fertilizer and improved seed than uninsured households. Also, we should observe lower contribution among uninsured households, driving the crowding- out effect. However, we do not find a significant difference in technology adoption between insured and uninsured households.16 Moreover, we find no difference in contributions between insured and uninsured households. Taken together these results suggest that WII uptake does not necessarily alter households' investment decisions, nor does it support the idea that uninsured households anticipate insured households to behave more risky thereby imposing negative externalities on them.

Second, one may think that WII creates positive externalities thereby inducing free-riding behavior among the uninsured. If insured households anticipate this behavior, they would lower their contribution in anticipation (de Janvry et al., 2014). Third, the effect may be due to what we call self-sufficiency behavior. Yet, as both the second and third effects would go in the same direction (i.e., reduced contributions among the insured), it is hard to empirically disentangle between the two. Although far from perfect, our data go some way in examining such effects. In our survey we included questions about households' self- sufficiency behavior and their perception about fairness among their fellow villagers.

More precisely, to measure households' self-sufficiency behavior following Di Tella et al. (2007) we asked households: “Do you believe that it is possible to be successful on your own, or a large group that supports each other is necessary?” Similarly, to measure free-riding problems, we asked households the question in the World Value Survey questionnaire: “Do you think most people in this village would try to take advantage of you if they got a chance, or would they try to be fair?”

Our regression results in Table 10 show a statistically significant correlation between WII uptake and an individuals' perception about other villagers taking advantage of him (her) and self-sufficiency behavior, both in the probit and IV (we again use the distance from household's

TABLE10 IV estimates on the sociopsychological effects of weather index insurance (WII).

	No free-riding			Self-sufficiency		
	(1)	(2)	(3)	(4)	(5)	(6)
	IV-LPM	IV-LPM	IV-LPM	IV-LPM	IV-LPM	IV-LPM
WII uptake	−0.430**	−0.387**	−0.411**	0.298*	0.362**	0.376**
	(0.179)	(0.177)	(0.180)	(0.175)	(0.180)	(0.171)
Age		−0.002	0.001		−0.007**	−0.006*
		(0.004)	(0.004)		(0.003)	(0.003)
Male		−0.042	−0.101		0.098	0.123
		(0.094)	(0.100)		(0.097)	(0.090)
Active number of people		−0.003	0.004		0.012	−0.011
		(0.014)	(0.016)		(0.016)	(0.018)
Education		−0.110	−0.093		0.189**	0.204***
		(0.070)	(0.068)		(0.075)	(0.072)
Land owned		−0.006	−0.014		0.016	0.001
		(0.011)	(0.011)		(0.013)	(0.012)
Livestock		0.011*	0.009		−0.022***	−0.019***
		(0.006)	(0.007)		(0.008)	(0.007)
Corrugated iron sheet		−0.122*	−0.119*		0.269***	0.153**
		(0.065)	(0.064)		(0.072)	(0.074)
Watch TV		−0.030	−0.058		−0.095	−0.144
		(0.078)	(0.082)		(0.101)	(0.095)
Listen radio		0.148*	0.211**		−0.094	−0.099
		(0.079)	(0.087)		(0.097)	(0.094)
Number of years lived		0.004	0.002		0.005**	0.004*
		(0.003)	(0.003)		(0.003)	(0.003)

Religion		0.177*	0.105		−0.006	−0.111
		(0.093)	(0.104)		(0.087)	(0.081)
Risk preference		−0.022	−0.018		0.011	0.018
		(0.017)	(0.016)		(0.018)	(0.018)
Constant	0.830***	0.842***	0.898***	0.287***	0.099	0.439*
	(0.048)	(0.210)	(0.231)	(0.055)	(0.207)	(0.226)
Village fixed effects	No	No	Yes	No	No	Yes
Observations	240	240	240	240	240	240
R^2	−0.096	0.022	0.057	−0.003	0.148	0.217
First-stage F test	75.40	62.46	58.80	75.40	62.46	58.80

(Continues)

TABLE10 (Continued)

	No free-riding			**Self-sufficiency**		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
	IV-LPM	**IV-LPM**	**IV-LPM**	**IV-LPM**	**IV-LPM**	**IV-LPM**
Distance to foreman	−0.012***	−0.013***	−0.012***	−0.012***	−0.013***	−0.012***
	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)
Mean (uninsured)	0.75			0.32		

Note: The dependent variable in columns (1)–(3) is a dummy variable taking a value of 1 if the respondent believes that most people in the village would try to be fair, 0 otherwise. The dependent variable in columns (4)–(6) is also a dummy variable taking a value of 1 if the respondent believes that he can be successful on his own, 0 otherwise. The table reports marginal effects probit in (1) and (3). Robust standard errors are in parentheses.

Abbreviations: IV, instrumental variable; LPM, linear probability model; TV, television.

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

residence to the insurance foreman's residence as an instrument) models. Thus, insured households are more likely to perceive that fellow villagers would take advantage of them if they got a chance, and are more likely to believe that they can be successful without the support of a large group, suggesting WII uptake may bring about sociopsychological impacts which could potentially serve as pathways through which a negative effect on social capital comes about.

7 | CONCLUSION

Index insurance is a promising innovation that helps small-holder farmers buffer against weather shocks, thereby possibly affecting other informal arrangements through a crowding-in or crowding-out effect. Theoretical predictions regarding the relationship between formal insurance and social capital are ambiguous, hence we need empirical research to better understand if and how the introduction of index insurance may crowd in or out social capital. This study provides the first empirical evidence on the medium-term impact of WII on a broad definition of individual social capital. We use a unique data set from a lab-in-the-field experiment and household surveys from Tigray region, northern Ethiopia where WII has been commercially traded to small-holder farmers since 2010. We use contributions in the PGG, and self-reported survey responses on private transfers to fellow villagers, and financial contributions to community projects as measures of social capital. As the WII uptake was voluntary, we use PSM and IVs approach to attenuate endogeneity concerns. We utilize the low mobility of farm households in the rural areas of Tigray region, to instrument WII uptake with the distance from farm-household's residence to the insurance foreman's location. The insurance foreman acts as a liaison between farmers and the insurance companies, and plays a paramount role in the promotion and marketing of the insurance policy.

Overall, our empirical findings consistently demonstrate a negative association between WII uptake and social capital. The results from the descriptive analysis, as well as naive OLS and Probit regression models, show a negative and significant association between WII uptake and social capital. To account for observed confounding factors, we employ PSM and find that WII uptake negatively and significantly affects social capital. To further account for unobserved confounding factors, we used an IV approach. The results of the IV approach are similar to those from the PSM and naive OLS/Probit regression models, reinforcing the negative association between WII uptake and social capital. Although we initially observe a negative and borderline significant effect of WII uptake on social capital in our IV models, this finding dissipates in our preferred specification where we include village-fixed effects to control for unobserved between-village variation. Interestingly, our experimental and survey-based measures of social capital show qualitatively similar results, suggesting they might be capturing the same underlying concept of social capital. Furthermore, we do find that WII uptake may bring about sociopsychological impacts, possibly related to social capital. We find that insured households are less likely to perceive other villagers to be fair (de Janvry et al., 2014) and more likely to believe they are able to be successful on their own. These sociopsychological impacts could serve as potential channels through which the effects on social capital come about.

To probe the robustness of our findings we also look at villages with and without access to WII. We find qualitatively similar results in that insured households contribute less to the public good than households in no-access villages. While we find a negative and borderline significant association between WII uptake and social capital in our most parsimonious specification, this effect dissipates after controlling for individual controls and village-fixed effects. On the other hand, we find no robust difference in social capital investment between uninsured households in access and households in no-access villages suggesting spillovers are not very relevant in this context. However, these results should be interpreted with caution as weare unable to make use of our IV approach in the no-access villages and thus have to confine ourselves to comparing preintervention data for access and no-access villages and a PSM approach to create a plausible counterfactual. All in all, we provide several pieces of evidence that suggest WII uptake may have negative effects on various elements of social capital. As indicated in Section 6.1.3, our current sample is, however, only sufficiently powered to detect significant effects that are about 60% larger than the unconditional mean difference between insured and uninsured households, which explains the largely nonsignificant effects in our IV estimates. We believe our results merit further research into the topic using larger samples and are ideally based on a randomized introduction of WII. Second, as WII and social capital indemnify aggregate and idiosyncratic risks, respectively, there is no evidence yet on the net effect of WII on welfare through increased technology adoption, productivity, and income. Finally, it would be interesting to evaluate different insurance product designs and marketing strategies, including, for example, offering WII to groups instead of individuals, and their impact on social capital. This is left for future work.

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CONFLICT OF INTERESTS STATEMENT

The authors declare no conflict of interest.

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Abandoning disaster relief and stimulating insurance demand through premium subsidies

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ABSTRACT

Premium subsidies can be used to address low demand for natural hazard insurance when it is partly caused by governmental disaster relief payments. We analyze how the introduction of ex ante premium subsidies affects the frost insurance demand of German winegrowers after the government changed insurance regimes to avoid ex post disaster relief payments. We find that the implementation of a premium subsidy in an immature market with low levels of participation, presumably caused by strong anticipation of disaster relief, is effective in increasing overall frost insurance demand. Receiving disaster relief payments 3 years before the introduction of the subsidy seems to make farmers more responsive toward the premium subsidy.

KEYWORDS
charity hazard, crop insurance, disaster relief, natural hazards, premium subsidies

INTRODUCTION

Accelerating climate change appears to make many extreme weather events more frequent and more severe (IPCC, 2012). Heatwaves in the Southwest of the United States in 2023, severe droughts in Western-Central Europe in 2022, and heavy rainfall and flooding in Western Europe in 2021 were all exacerbated by climate change (Schumacher et al., 2022; Tradowsky et al., 2023; Zachariah et al., 2023). In such a changing risk environment, adequate risk management becomes increasingly important (Collier et al., 2009). One valuable instrument to prepare for financial losses from extreme weather events is insurance. In addition to speeding up recovery from disasters through efficient claims handling, insurance coverage can provide information about risk exposure, which may also incentivize investments in prevention (Kousky, 2019).

A primary challenge in natural hazard insurance markets is low insurance demand when insurance is not mandated or subsidized (Glauber, 2013; Holzheu & Turner, 2018; Lamond & Penning-Rowsell, 2014; Meuwissen et al., 2018). For two of the largest natural hazard insurance markets–crop insurance and flood insurance–Swiss Re estimates that 60% of insurable crop losses and 83% of global economic losses from flooding are uninsured (Aggarwal & Xing, 2023; Bevere & Remondi, 2022). The most cited reasons for this protection gap are misperception of risk, often caused by a lack of awareness, and anticipation of governmental disaster relief payments (e.g., Kunreuther, 1996; Mulder, 2021). Anticipation of disaster relief payments is often referred to as charity hazard, a term introduced by Browne and Hoyt (2000) to express anticipation of receiving some sort of charity when a catastrophic loss occurs. Anticipating charity is argued to lower insurance demand and incentives for prevention.1 To overcome low demand caused by charity hazard, governments can introduce premium subsidies. Premium subsidies lower the price of insurance and they may reduce charity hazard as their introduction is often bound to the ostensibly credible condition that no further disaster relief payments will be made. In general, the more price elastic insurance demand and the more premium subsidies reduce charity hazard, the more effective premium subsidies are at overcoming low demand. By increasing insurance demand through premium subsidies, governments also benefit from shifting claims handling to insurance companies, as disaster relief payments usually impose significant bureaucratic burdens onto governments (Kousky, 2019). Insurance may also help to uphold creditworthiness after disasters occur enabling individuals to maintain their access to credit markets (Collier & Babich, 2019). Thus far, premium subsidies are most widely used on crop insurance markets. In particular, the U.S. has been actively shifting from ex post disaster relief toward ex ante premium subsidies on their crop insurance markets (Glauber, 2013; Kramer, 1983).

Westudy the German frost insurance market for winegrowers, which is part of the fragmented European agricultural insurance landscape. With warming climate, the buds of grapevine break earlier in the year leaving them more susceptible to spring frost in March or April.2 The insurance market for this frost peril was barely existent in Germany until 2020, with low levels of insurance participation even after two major frost events in 2011 and in 2017, both of which triggered large amounts of governmental disaster relief payments (dpa, 2017; Wissenschaftlicher Dienst des Deutschen Bundestages, 2018). In the state of Baden-Württemberg, the second largest wine producing state in Germany and the focus of this study, only 1.5% of vineyards were covered against frost risks in 2017 (Landtag Baden-Württemberg, 2017). Thereafter, insurance demand slightly increased, but remained low, with the share of vineyards insured at the largest insurance company not exceeding 5% of all vineyards in 2019 (based on the data introduced in Section 4). To put an end to disaster relief payments and help establish a private frost insurance market, the state government of Baden-Württemberg decided to subsidize premiums from 2020 onwards. The government explicitly communicated in a press release that the introduction of the subsidy implies a shift from ex post disaster relief payments to ex ante premium subsidies (Ministerium für Ländlichen Raum und Verbraucherschutz, 2019a).

This policy change provides us with an opportunity to analyze whether a shift towards ex ante subsidies can increase coverage levels in an immature insurance market with evident charity hazard. The frost risk of winegrowers serves as an example for a risk that has become increasingly relevant over recent years but is not covered by established insurance markets. Other climate-related risks that have become more salient recently but are not part of common insurance products (e.g., drought, wildfires, excessive rainfall or heat waves) may run into similar dynamics with governmental disaster relief payments and subsequent need for policy changes. We aim to provide a better understanding about policymaking in these contexts where climate change alters risk profiles. We study how the introduction of premium subsidies in Baden-Württemberg affects insurance demand and whether farmers who received recent disaster relief payments react differently to the subsidy compared to farmers who did not receive disaster relief payments. Our analysis is based on municipality-level data of winegrowers provided by the largest German crop insurance company. We examine changes in insurance demand at the extensive margin (insured acreage). Changes at the intensive margin (coverage level) are not part of the analysis as insurance products and especially deductibles are mostly standardized, and farmers have little freedom in adjusting their coverage levels.3 Typical contracts have coverage sums of around 10,000 € per hectare and deductibles of 20% of the coverage sum.

Wefocus on winegrowers, as no frost insurance markets for fruit growers, who are similarly affected by the frost risk, existed before the subsidy was introduced. Exploiting the federal structure of Germany, we use a difference-in-differences approach to analyze the subsidy introduction in the federal state of Baden-Württemberg, which was the first state to introduce the subsidy. The neighboring state of Rhineland-Palatinate serves as the control group. Rhineland-Palatinate and Baden-Württemberg are the two largest winegrowing states in Germany, accommodating approximately 63% and 26% of German vineyards, respectively (BMEL, 2021). In our research, we find the premium subsidy to be effective in increasing insurance takeup at the extensive margin. Many new farmers buy frost insurance after the premium subsidy is introduced. Additionally, we find that farmers who have recently received disaster relief payments show a much stronger immediate increase in insurance demand compared to farmers who have not recently received any disaster relief payments. Our results also suggest that receiving higher disaster relief payments in the past is linked to higher increases in insurance demand.

We add to the literature in two ways. We provide evidence of the effectiveness of premium subsidies in an immature insurance market with low participation levels. The premium subsidy marks the change from ex post disaster relief payments to ex ante premium subsidies, which, to our knowledge, has not been studied elsewhere. Other studies analyze subsidy rate changes inexisting subsidy programs to estimate their effectiveness (Garrido & Zilberman, 2008; O'Donoghue, 2014).

Their initial implementation has not yet been studied. We demonstrate that the introduction of a premium subsidy can be highly effective in markets with low participation rates and where regular disaster relief payments are common. Our work proposes that farmers are more responsive to the introduction of a premium subsidy if they have received recent disaster relief payments. We hypothesize that the effect is driven by loss experience, which has been shown to increase insurance demand (Cai & Song, 2017; Che et al., 2019; Gallagher, 2014; Kousky, 2017). In our context, we hypothesize charity hazard before the premium subsidy to be large enough to depress insurance demand so that most people remain uninsured. For the uninsured, the effect of loss experience cannot be observed through their insurance demands. Only when premium subsidies eliminate charity hazard does the effect of prior loss experience become observable, resulting in differing levels of insurance demand. It is predicted that farmers who experienced larger losses will show stronger increases in insurance demand. Overall, the paper contributes to policy-making on insurance markets covering losses from natural hazards that are not part of established insurance products. Understanding the shift from ex post disaster relief payments to ex ante premium subsidies provides insights into how premium subsidies function and guidance for similar future policies. Premium subsidies can be an effective instrument in early markets to boost insurance participation. Recent experience with disaster relief payments may create advantageous situations for policy introductions, as such an experience seems to increase responsiveness.

2 | FROST INSURANCE FOR WINEGROWERS

We analyze a premium subsidy that was introduced in Baden-Württemberg, a state of Germany, in 2020. It subsidizes insurance premiums against frost,4 storm, and heavy rainfall in viticulture and fruit growing by 50% (for contracts with at least 20% deductible) (Ministerium für Ländlichen Raum und Verbraucherschutz, 2019b). We focus on the risk of frost, as it is the most relevant of the three. The premium subsidy provides subsidization to almost all farmers. Only farmers covering less than 0.3 hectares of acreage and farmers choosing coverage sums above 30,000 € per hectare are not eligible for subsidization. These are exceptional cases such that only very few farmers do not meet the conditions of the subsidy. Before the introduction of the premium subsidy, the market for frost insurance contracts had been operating at low takeup levels. In 2017, only 1.5% of vineyards in Baden-Württemberg were covered against frost damage (Landtag Baden-Württemberg, 2017). In 2021, after the premium subsidy was implemented, overall insurance participation in Baden-Württemberg rose to 35% of vineyards (Fial, 2021).

As a severe frost event in 2011 already led to large losses and disaster relief payments in Baden-Württemberg, we expect that a lack of risk awareness cannot by itself explain low demand before the premium subsidy. The most plausible reasons for low demand before the premium subsidy are (high) prices of insurance coverage and charity hazard. From the insurer's data, which this study is based on and which is further described in Section 4, we observe that the loss ratios–the ratio of indemnity payments to premiums–in Baden-Württemberg were well above 1 in 2016, 2017 and 2019. Loss ratios equal to 1 imply that insured on average receive indemnity payments that are just as high as the premiums they pay. Loss ratios above 1 as in BadenWürttemberg either suggest high levels of adverse selection or insurance prices to be in favor of the insured. As frost risk exposure depends on the location and elevation of vineyards and the sort of grapevine grown, all of which insurers can observe, we argue that premium differentiation should be able to prevent large amounts of adverse selection. Given that substantial disaster relief payments were made in 2011 (~7 million €) and in 2017 (~50 million €), we argue that charity hazard is the most likely cause for low insurance demand (dpa, 2017; Wissenschaftlicher Dienst des Deutschen Bundestages, 2018).

The introduction of the subsidy arose from a debate that started in 2017 when the state of Baden-Württemberg made an ex post disaster relief payment to their fruit- and winegrowers after cold temperatures in April had caused major damage to many farmers' harvests. According to the Research Services of the national German parliament, approximately 8000 hectares, which make up around 30% of total vineyards in Baden-Württemberg, reported damages to 50% or more of their harvest in 2017 (Wissenschaftlicher Dienst des Deutschen Bundestages, 2018). The Research Services report that these losses to wine- and fruitgrowers led Baden-Württemberg to pay 49.44 million € in overall disaster relief, 14.91 million € of which to winegrowers. Disaster relief payments had to abide by EU regulations, thus payments were only made to farmers whose losses exceeded 30% of their average harvest and only 50% of losses were reimbursed (see article 39 in EU regulation no 1305/2013).5 Payments were also capped at 100,000 € per farmer.

Baden-Württemberg reportedly set limits on the total funds used for premium subsidies at 5 million € per year. From the government's perspective, the subsidy pays off financially when disaster relief payments that would be made if no premium subsidies were in place exceeded the funds used for subsidizing premiums. In our studied example, disaster relief payments to fruit- and winegrowers are just below 50 million € whereas the subsidization costs the government up to 5 million € per year. If there is one similar disaster as in 2017 within the next 10 years and the state does not make any disaster relief payments, the government's policy breaks even. If more than one of such disasters takes place within the next ten years, the government saves money by its premium subsidy policy.

Rhineland-Palatinate followed Baden-Württemberg with the introduction of a similar premium subsidy in 2021. In Rhineland-Palatinate, premiums against frost and hail, which are usually bought as bundled contracts, are subsidized by 50%, capped at 200 € per hectare in the first year (Ministerium für Wirtschaft, 2021).6 All farmers receive subsidies as long as premium subsidies are larger or equal to 200 €. Only very few farmers, who buy low levels of coverage and whose premiums do not exceed 400 € do not receive subsidies. In Rhineland-Palatinate, no disaster relief payments were made in 2017. The ministry rationalized their decision based on the availability of insurance products before the disaster had occurred.

Before the premium subsidy offered in Baden-Württemberg, subsidies on German crop insurance markets for winegrowers have played a negligible role in recent years. SaxonyAnhalt (0.7% of German vineyards) and Saxony (0.5% of German vineyards) subsidize premiums for all insurable risks by 50%. Rhineland-Palatinate subsidized insurance premiums against all insurable risks by 50% capped at 40 € per hectare until 2013, which is significantly less than the cap from 2021 at 200 € per hectare (BMEL, 2013). The subsidy was also aimed at insurance contracts for hail, as frost insurance contracts only started to be sold in 2013. Between 2014 and 2020, there were no premium subsidies in place in Rhineland-Palatinate. The analyzed premium subsidy in Baden-Württemberg is the first subsidy of its kind within Baden-Württemberg and the first explicitly aimed at the risk of frost within Germany.

There are two types of crop insurance contracts: yield insurance and revenue insurance. While yield insurance guarantees a predetermined amount of yield at a price that is agreed upon when signing the contract, revenue insurance guarantees a share of a predefined revenue. Yield insurance does not cover any price risks because the indemnity payment is independent of market prices. Revenue insurance includes coverage against fluctuations in price as the farmer receives indemnity when he falls short of his predefined revenue irrespective of whether he falls short because of low yield or because of low market prices. In Germany, yield contracts are the predominant form. Prices are determined by farmers in the form of coverage sums per hectare. When farmers purchase coverage, they state the number of hectares they would like to insure and the coverage sum per hectare (e.g., 10,000 €). Typical deductible levels are 20% of the coverage sum. Upon damage, the insurer reimburses farmers based on the percentage yield loss per hectare. If a farmer loses 50% of her yield on a hectare covered with 10,000 € and a 20% deductible (2000 €), she receives reimbursement of 3000 €. As price risks are not covered by the studied insurance contracts, they are not the subject of this study.

To finance premium subsidies for crop insurance, German states can use their own tax funds as long as the subsidy is approved by the EU, which regulates agricultural markets through the Common Agricultural Policy (CAP). This is how Baden-Württemberg currentlyfinances its premium subsidy. Alternatively, states can use EU funds allocated through the CAP. Within the CAP, there are two programs that allow for premium subsidies. States can use funds targeted at the common organization of the market in wine (CMO), which is part of the so-called first pillar of CAP and was fundamentally reformed in 2008 (see EU Council Regulation No 479/2008). Rhineland-Palatinate, Saxony and Saxony-Anhalt finance their subsidies through these CMO funds. The second program within the CAP that allows for premium subsidies promotes risk management in agriculture and is part of the so-called second pillar of CAP (Article 37 of EU regulation No 1305/2013). To the authors' knowledge, Baden Württemberg plans to finance a share of their premium subsidy through the second pillar of CAP from 2024 onward.

3 | EFFECTS OF PREMIUM SUBSIDIES

To study the effects of premium subsidies, we first consider a model of insurance demand on the individual level. We use the model to understand the channels through which the premium subsidy affects insurance demand. To derive predictions for our empirical analysis on municipality level, we focus on the effects of a premium subsidy on the behavior of a representative farmer. We show that the general direction of the predictions is not driven by the exact specification of the farmer allowing us to infer hypotheses on the municipality level from a model of individual insurance demand.

We consider a model with two states of the world and a risk- averse individual (farmer) who assesses outcomes using a concave twice differentiable utility function u (.).The individual has initial wealth w and faces a loss of ∈ L (0,w)with probability ∈ p (0,1).7 Individuals can buy insurance, which provides an indemnity payment I αL = at a premium P αλpL = ,where ∈ α [0,1]denotes the coverage level and ≥ λ 1aproportional loading factor. The proportional loading factor captures transaction costs and profits of insurance companies. Under perfect competition and in absence of any transaction costs, the loading factor is 1 λ (=1) and premiums are actuarially fair. With actuarially fair premiums, individuals can transfer their risk to the insurance company at a premium equal to the expected loss of the risk. When insurance markets are not perfectly competitive, insurers can load their premiums λ (>1)and makeprofits.Whenλ<1, the insurer's indemnity is on average higher than the premium payment and the insurer makes losses. We assume that individuals anticipate disaster relief payments ∈ θ [0,1] 0 on their uninsured losses (Raschky &Weck- Hannemann, 2007).Weuse the terms anticipated disaster relief payment sand charity hazard synonymously from hereon. The final wealth in the no-loss stateis W w αλpL = − 1 ,and the final wealth in the loss state is W w αλpL θ αL = − −(1−)(1−) 2 0 . The expected utility of the individual to be maximized with respect to αis:

$$\max_{\alpha} E[u(\alpha)] = (1-p)u(w-P) + pu(w-P-(1-\theta_0)(1-\alpha)L). \qquad (1)$$

We can derive optimal insurance demand $\alpha^* \in [0,1]$ from Equation (1). It can be optimal to not buy any insurance ($\alpha^* = 0$) in which case $\frac{\partial E[u(\alpha)]}{\partial \alpha}\Big|_{\alpha=0} \leq 0$. Individuals buy full insurance ($\alpha^* = 1$) when $\frac{\partial E[u(\alpha)]}{\partial \alpha}\Big|_{\alpha=1} \geq 0$. Partial insurance ($0 < \alpha^* < 1$) is optimal when α^* solves the following first-order condition:

$$FOC : (1-p)u'(W_1)[-\lambda pL] + pu'(W_2)[-\lambda pL + (1-\theta_0)L] = 0. \tag{2}$$

We follow other analyses of insurance demand such as Mossin (1968) or Jaspersen et al. (2022) and derive an upper bound loading factor $\bar{\lambda}$ as well as a lower bound loading factor $\underline{\lambda}$ to study the effect of charity hazard on insurance demand. It holds that for any $\lambda \geq \bar{\lambda}$, individuals do not buy any insurance ($\alpha^* = 0$). Similarly, individuals buy full insurance ($\alpha^* = 1$) at any $\lambda \leq \underline{\lambda}$. Partial insurance is optimal for any λ between the upper bound and the lower bound, such that $\underline{\lambda} < \lambda < \bar{\lambda}$. Based on Equation (2) we derive $\bar{\lambda}$ and $\underline{\lambda}$ from $\frac{\partial E[u(\alpha)]}{\partial \alpha}\Big|_{\alpha=0} = 0$ and $\frac{\partial E[u(\alpha)]}{\partial \alpha}\Big|_{\alpha=1} = 0$:

$$\underline{\lambda} = 1 - \theta_0 \quad \bar{\lambda} = \frac{u'(w-(1-\theta_0)L)(1-\theta_0)}{(1-p)u'(w) + pu'(w-(1-\theta_0)L)}. \tag{3}$$

When $\lambda \geq 1$, which holds in most cases, $\underline{\lambda}$ shows that individuals never buy full insurance when charity hazard exists ($\theta_0 > 0$). We can further show that both $\underline{\lambda}$ and $\bar{\lambda}$ decrease when charity hazard increases (derivatives of $\underline{\lambda}$ and $\bar{\lambda}$ with respect to θ_0 are shown in Appendix A). A decrease of $\bar{\lambda}$ implies that individuals stop purchasing insurance at lower premium loadings. A lower $\underline{\lambda}$ implies that a lower premium loading factor is required for individuals to buy full insurance. Both effects represent a decrease in insurance demand. The effect is enhanced as the corridor between $\underline{\lambda}$ and $\bar{\lambda}$–the interval of λ, on which partial insurance is optimal–decreases in θ_0 (we show in Appendix A that $\frac{\partial \underline{\lambda}}{\partial \theta_0} \geq \frac{\partial \bar{\lambda}}{\partial \theta_0}$).

When partial insurance is optimal with $\underline{\lambda} < \lambda < \bar{\lambda}$, we derive the effect of θ_0 on α^* by applying the implicit function theorem:

$$\frac{\partial \alpha}{\partial \theta_0} = -\frac{\partial^2 E[u(\alpha)]/\partial \alpha \partial \theta_0}{\partial^2 E[u(\alpha)]/\partial \alpha^2} = -\frac{EU_{\alpha\theta_0}}{EU_{\alpha\alpha}}. \tag{4}$$

As $EU_{\alpha\alpha} < 0$, it follows that $\text{sgn}\left(\frac{\partial \alpha}{\partial \theta_0}\right) = \text{sgn}(EU_{\alpha\theta_0})$, which is given by:

$$EU_{\alpha\theta_0} = pu''(W_1)(1-\theta_0-\lambda p)(1-\alpha)L^2 - pLu'(W_2). \tag{5}$$

Partial insurance demand decreases in θ_0 when $EU_{\alpha\theta_0} < 0$, which holds when $1-\theta_0-\lambda p > 0$. We can show that $1-\theta_0-\lambda p < 0$ only holds when $\lambda > \bar{\lambda}$, in which case no inner solution exists (see Appendix A). Partial insurance can only be optimal when $1-\theta_0-\lambda p > 0$ and it then decreases in θ_0 with $\frac{\partial \alpha}{\partial \theta_0} < 0$.

Charity hazard crowds out insurance demand through three mechanisms. It lowers the boundary loading levels $\underline{\lambda}$ and $\bar{\lambda}$. It decreases the distance between $\underline{\lambda}$ and $\bar{\lambda}$, thereby reducing

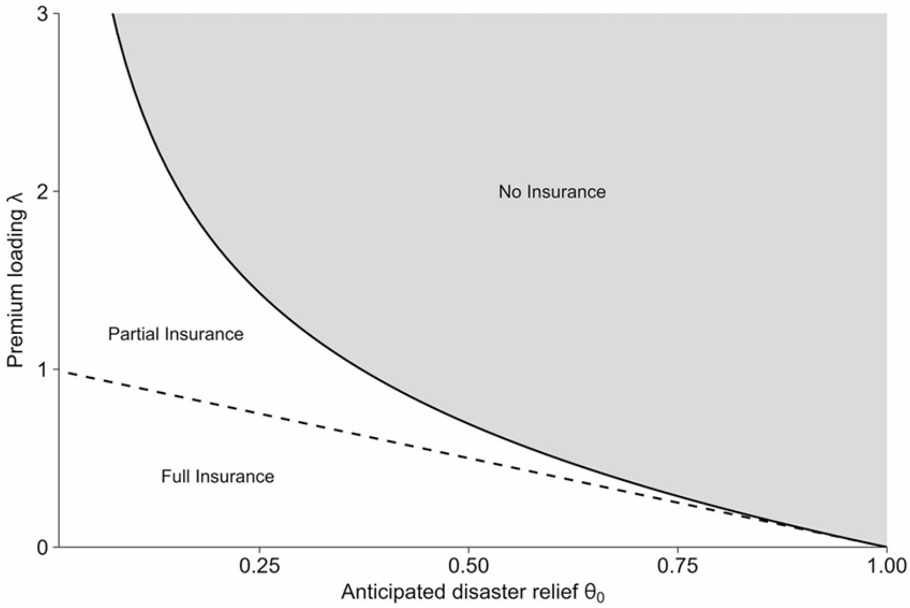

FIGURE1 Insurance demand under charity hazard

the range of λ, on which partial insurance is optimal. And charity hazard lowers partial insurance demand.

We provide an example to show how charity hazard crowds out insurance demand in Figure 1. We assume an iso-elastic utility function $u(W) = \frac{W^{1-\gamma}}{1-\gamma}$ with a constant relative risk aversion parameter of $\gamma = 0.5$, a loss probability p of 5% and a loss size L that is equal to initial wealth. Figure 1 shows how both $\underline{\lambda}$ and $\bar{\lambda}$ decrease with higher levels of charity hazard θ_0. The figure also demonstrates how the corridor between $\underline{\lambda}$ and $\bar{\lambda}$ decreases in θ_0. The area on which individuals buy insurance shrinks as charity hazard increases.

Given our setup of charity hazard, we study the effect of a premium subsidy. We assume that the government can subsidize a fraction $s \in [0,1]$ of the insurance premium. The subsidized insurance premium for an individual is $P^s = \alpha(1-s)\lambda pL$. When the government communicates that with the introduction of the premium subsidy, disaster relief payments will be held back in the future, we assume that the premium subsidy affects the anticipation of disaster relief payments. We model the reduction in charity hazard by the premium subsidy s with a function for anticipated disaster relief $\theta : s \to [0,1]$ with $\theta' \leq 0$ and $\theta(0) = \theta_0$. With premium subsidies s and disaster relief anticipation $\theta(s)$, the individual's objective function is:

$$\max_{\alpha} E[u(\alpha)] = (1-p)u(w-P^s) + pu(w-P^s-(1-\alpha)(1-\theta(s))L). \tag{6}$$

Equivalent to Equation (3), we can derive boundary loading levels. We now denote them as $\underline{\lambda}^s$ and $\bar{\lambda}^s$:

$$\underline{\lambda}^s = \frac{1-\theta(s)}{1-s} \quad \bar{\lambda}^s = \frac{u'(w-(1-\theta(s))L)(1-\theta(s))}{(1-s)((1-p)u'(w)+pu'(w-(1-\theta(s))L))}. \tag{7}$$

We can study the effect of a premium subsidy on both boundary loading factors $\underline{\lambda}^s$ and $\bar{\lambda}^s$ by determining the first-order derivatives with respect to s. We now denote final wealth in the no-loss state as $W_1^s = w - \alpha(1-s)\lambda pL$ and final wealth in the loss state as $W_2^s = w - \alpha(1-s)\lambda pL - (1-\theta(s))(1-\alpha)L$. With $k = (1-p)u'(w) + pu'(w-(1-\theta(s))L) > 0$, the derivatives are:

$$\frac{\partial \underline{\lambda}^s}{\partial s} = \frac{\overbrace{(1-\theta(\mathrm{s}))}^{(\mathrm{a})(+)} - \overbrace{\frac{\partial \theta(\mathrm{s})}{\partial s}(1-s)}^{(\mathrm{b})(+)}}{(1-s)^2} > 0. \tag{8}$$

$$\frac{\partial \bar{\lambda}^s}{\partial s} = \frac{\left(\begin{array}{l} \overbrace{u'\left(W_2^s\right)(1-\theta(s))k}^{(\mathrm{a})(+)} \\ \overbrace{-\frac{\partial \theta(s)}{\partial s}\left(u'\left(W_2^s\right)(1-s)k - u''\left(W_2^s\right)(1-\theta(s))(1-s)L\left((1-p)u'\left(W_1^s\right)\right)\right)}^{(\mathrm{b})(+)} \end{array}\right)}{((1-s)k)^2} > 0.$$

The premium subsidy increases the insurance demand by increasing both boundary loading factors $\underline{\lambda}^s$ and $\bar{\lambda}^s$ through two channels: (a) a price effect from reducing premiums and (b) a charity hazard effect from reducing anticipated disaster relief payments. Both effects are positive. The lower $\frac{\partial \theta(s)}{\partial s}$, that means, the more the premium subsidy lowers charity hazard, the more does the introduction of a premium subsidy increase $\underline{\lambda}^s$ and $\bar{\lambda}^s$.

The effect of premium subsidies on partial insurance demand is also characterized by a price effect and a reduction in charity hazard. Applying the implicit function theorem, we derive $\frac{\partial \alpha}{\partial s}$:

$$\frac{\partial \alpha}{\partial s} = \frac{\left(\begin{array}{l} \overbrace{\begin{array}{l} \lambda pL(1-p)\left(u''\left(W_1^s\right)(-\alpha(1-s)\lambda pL) + u'\left(W_1^s\right)\right) \\ +\lambda p^2 L\left(u''\left(W_2^s\right)\alpha L(1-\theta(s)-(1-s)\lambda p) + u'\left(W_2^s\right)\right) \end{array}}^{(\mathrm{a})(+/-)} \\ \overbrace{-\frac{\partial \theta(s)}{\partial s}\left(pL\left(u'\left(W_2^s\right) - u''\left(W_2^s\right)(1-\alpha)L(1-\theta(s)-(1-s)\lambda p)\right)\right)}^{(\mathrm{b})(+)} \end{array}\right)}{\underbrace{-\left((1-p)u''\left(W_1^s\right)(-\lambda(1-s)pL)^2 + pu''\left(W_2^s\right)((1-\theta(s))L-(1-s)\lambda pL)^2\right)}_{EU_{\alpha\alpha}<0}}. \tag{10}$$

The sign of $\frac{\partial \alpha}{\partial s}$ is determined by the sign of the numerator as the denominator is strictly positive. The first two terms in the numerator show the effect of the subsidy through lowering the premium. The sign of the price effect is ambiguous in Equation(10).The ambiguity of the sign of the premium effect(a)is caused by the possibility of insurance to be a Giffen good. The conditions under which insurance is a Giffen good are analyzed by Briys et al. (1989), Hau (2008) and Hoy and Robson (1981).Wes how in Appendix B that a highly unlikely combination of large losses, high loss probabilities would be necessary for insurance to turn into a Giffen good in our context. Hence, we expect the first term to be positive in most cases and insurance to be an ordinary good for which lower prices through subsidization lead to higher demand. The third term of the numerator shows how lowering charity hazard through the introduction of a premium subsidy increases insurance demand. We can show that $1 - \theta(s) - (1 - s)\lambda p < 0$ holds for all inner solutions (analogous to the proof in Appendix A, where we show that $1-\theta_0-\lambda p < 0$ for all inner solutions without premium subsidies).The size of the effect depends on $\frac{\partial\theta(s)}{\partial s}$. The more effective premium subsidies lower charity hazard, the more do they increase partial insurance demand.

We use the model to illustrate the effects of premium subsidies on German winegrowers. Based on the context of our study, we characterize a representative winegrower in Baden- Württemberg to form predictions about the effects of the subsidy. As discussed below in more detail, the main predictions are insensitive to changes in the winegrower' s characteristics. We argue that the derived hypothesis holds even if the parameter specification of winegrowers may vary across municipalities.We assume winegrowers to follow an iso-elastic utility function with constant relative risk aversion of 0.5.We consider a farmer facing the risk of losing 30%of his wealth, the average loss amongfarmers in Baden-Württemberg in2017in our sample. With a 30% loss, the farmer is barely eligiblefor disaster relief payments in our setting. The disaster relief payments reimburse him for 50%of his loss, which makesup15% of his initial wealth. As the government also made disaster reliefpayments in 2011,we assume that the farmer anticipates such disaster relief payments and set the initial level of charity hazard to θ 0=.5 0 .The premium subsidy amounts to 50%of the insurance premium. We additionally assume premium loadings to be weakly above1and loss probabilities to be weakly below25%.The data-providing insurance company reports loss ratios of 65.5% in 2022, 81% in 2021% and 60.6% in 2020 implying loading factors of 1.53, 1.23, and1.65 respectively (Vereinigte Hagel,2021,2022).Based on the sevalues, we use a loading factor of 1.5as reference point for the following discussion. We find the aggregated loss ratio in our sample to be above1and thereby the loading factor in our dataset to be lower than 1 in most years. We expect these low loading factors to be caused by large losses in recent years and little pricing experience by the insurance company. As we do not know the exact loading factor, we discuss the theoreticalpredictions for different loading factors. Among the insured winegrowers from our dataset, the probability of losing more than 30% of the coverage sum are 5.83%. The probability of losing more than 10% of the coverage sum are 15.06%. Reliable loss probabilities are difficult to obtain which is why we only make the weak assumption that the probability of losing 30% of the coverage sum lies below 25%. Table 1 summarizes.

Figure 2 shows the effects of the premium subsidy on insurance demand of the farmer specified in Table 1. We present other specifications in Appendix C showing that the model predictions are not driven by our assumption of risk aversion or loss size. Panel (a) shows insurance demandbefore the premium subsidy. Even at fair premiums λ (=1), the farmer would not buy insurance when he anticipates disaster relief payments of 50%, which is in line with low insurance demand in Baden-Württemberg before 2020. Panel (b) describes the situation in which the premium subsidy does not affect anticipation of disaster relief payments and charity hazard remains at θ(0.5) = 0.5. Without a reduction of charity hazard, the premium subsidy affects insurance demand only through its price effect. In our example, the price effect only matters when the premium loading factor λ is very low. For a loss probability of 1%, the individual does not buy any insurance when λ is larger than ~1.08. The individual only buys full insurance when ≤ λ 1. For loading factors between 1 and ~1.08, the individual buys partial coverage. Given the reference loading factor of 1.5 from above, it seems unlikely that the pure price effect would be able to increase insurance demand meaningfully. The size of the price effect crucially depends on the initial level of charity hazard θ0. The higher the level of initial charity hazard, the more depressed insurance demand without premium subsidies and the less visible the price effect.

Panel (c) shows the effect of the premium subsidy on insurance demand when the premium subsidy fully removes charity hazard. An example of a charity hazard function $\theta(s)$ which would completely eliminate charity hazard in the given example is a linearly decreasing function $\theta(s) = max\left\{\theta_0\text{-}s, 0\right\}$. The elimination of charity hazard has a strong impact on insurance demand. Full insurance is optimal for any loading factors below 2. Under the reference loading of 1.5, the representative farmer would buy full insurance. Figure 2 shows that in our specification the price effect appears small compared to the effect from lowering charity hazard. The effectiveness of the premium subsidy seems to mainly depend on how much charity hazard the premium subsidy can eliminate. Our model captures this ability of the premium subsidy by the charity hazard function $\theta(s)$. The exact form of this function depends on the risk and especially the political context. We

TABLE1 Parameter specification of a representative winegrower in Baden-Württemberg.

Parameter specification of a representative winegrower		
Constant relative risk aversion	γ	0.5
Initial wealth and loss size	w, L	$w = 0.3L$
Loss probability	p	$p \leq 0.25$
Premium loading	λ	$\lambda \sim 1.5$
Charity hazard	θ_0	0.5
Premium subsidy	s	0.5

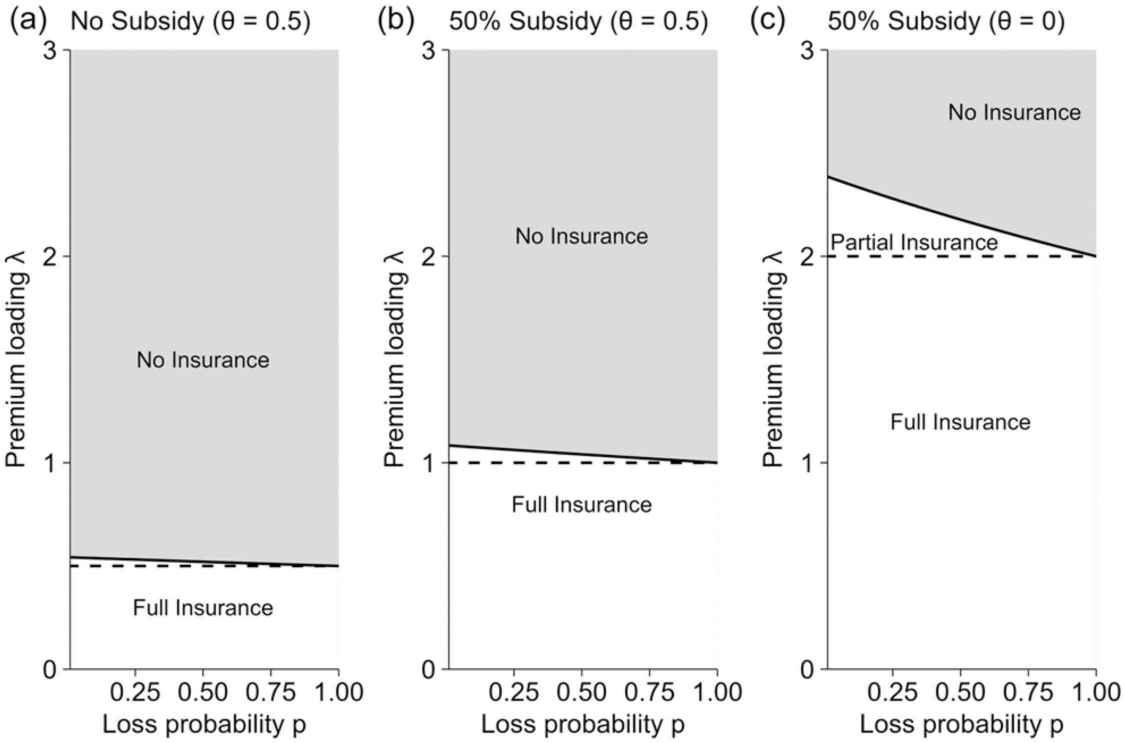

FIGURE2 Insurance demand of a representative winegrower.

would expect that the more credible a government communicates a reduction in future disaster relief payments alongside the introduction of a premium subsidy, the steeper θ s ( ).Otherfactors such as upcoming election years and the political power of the exposed population in demanding disaster relief payments may also define the slope and curvature of θ (s).

On the German frost insurance market for winegrowers, we expect that the active communication by the state of Baden-Württemberg about eliminating future disaster relief programs once the premium subsidy is implemented leads to a substantial decrease in charity hazard. In combination with the price effect of the subsidy, we hypothesize insurance demand to increase when premiums are subsidized:

Hypothesis 1. The introduction of a premium subsidy, which a policy maker uses to replace disaster relief payments, leads to an increase in insurance demand.

Besides intensifying charity hazard, disaster relief payments also imply severe loss experience for individuals. Gallagher (2014) uses the reception of disaster relief payments as a proxy for loss experience and shows that individuals buy more insurance after they experience a severe flood. Other studies show similar effects supporting the notion that experiencing severe losses leads to a subsequent increase in insurance demand (Cai & Song, 2017;Cheetal.,2019;Kousky,2017).

Gallagher (2014) suggests that the effect of loss experience can be explained by updated probability perception. Individuals follow a Bayesian learning model in estimating probabilities and their probability estimation goes up after experiencing a catastrophic loss. The size of the increase in probability estimation depends on the amount and quality of initial information and learning experience before the loss event (the strength of the prior in Bayesian language). He shows that the pattern in insurance demand that he observes can either be explained by discounting learning experiences over time or by lack of prior information. Jaspersen et al.

(2022) show that individuals who overestimate probabilities buy more insurance. Similarly, Collier et al. (2022) demonstrate how overestimation of small probabilities can explain unexpectedly high insurance demand. When individuals experience losses and update their probability estimation, loss experience leads to an increase in insurance demand. Alternatively, experiencing losses may affect insurance demand through changing risk preferences. Especially, when risk aversion decreases in wealth usually represented by decreasing absolute risk aversion (DARA), a loss of wealth makes farmers more risk averse (Guiso & Paiella, 2008; Haushofer & Fehr, 2014). Higher risk aversion increases insurance demand and may also explain why loss experience increases insurance demand.

As assumed in Figure 2 and the derivation of Hypothesis 1, we expect frost insurance demand of German winegrowers to be widely suppressed by charity hazard before the introduction of premium subsidies. The largest insurance company covers less than 5% of all vineyards just before the premium subsidy is introduced (based on the data introduced in Section 4). It follows that the effect of the catastrophic loss experience in 2017 on insurance demand is not visible because charity hazard depresses insurance demand. When the premium subsidy eliminates large parts of charity hazard and individuals buy more insurance, the effects from different loss experiences become visible. We expect individuals who experienced larger losses in 2017 to demand more insurance when the premium subsidy is introduced. Hypothesis 2 follows:

Hypothesis 2. The introduction of a premium subsidy, which aims to replace disaster relief payments, leads to a larger increase in insurance demand among individuals who have recently received disaster relief payments.

4 | DATA

The main data source for this study is a panel dataset provided by the largest German crop insurance company (“Vereinigte Hagel VVaG”). In 2017, it covered a market share of 57.3% of the German crop insurance market measured by the sum of premiums (BMEL, 2019). Based on information from the agricultural ministry of Baden-Württemberg, the company insured more than half of the total acreage covered by subsidized frost insurance contracts within BadenWürttemberg in 2022, and numbers have been relatively constant since the subsidy was introduced in 2020. The dataset includes information on frost insurance contracts that were taken out by winegrowers from Baden-Württemberg and Rhineland-Palatinate between 2013 (when frost insurance was first sold) and 2021.

We aggregate individual contract data at the municipality level because we analyze insurance demand at the extensive margin (insured acreage). We focus on the extensive margin as there is very little variation in contract specification within individuals, as they usually stay with their initially chosen contract. Individuals are also obliged to cover all their acreage such that there is no variation at the extensive margin within farmers over time. Farmers only appear in our sample once they buy insurance and we do not know their behavior before purchasing insurance at the data-providing insurer. There is no variation at the individual level of insurance purchasing behavior that we can use to infer the effect of the premium subsidy. By aggregating the data at the municipality level, we can retrieve an easily interpretable measure of how the premium subsidy affects insured acreage per municipality. We aggregate insurance demand per year by adding premiums, coverage sums, insured hectares and insured losses of all farmers within a municipality. If a farmer owns vineyards located in different municipalities, we attribute each vineyard to the municipality it lies in. For example, a farmer owns 10 hectares of vineyards of which 3 hectares lie in one municipality and 7 hectares in a different municipality, the coverage sum per hectare is 10,000 € and the resulting premium is 300 € per hectare. We would attribute 3 hectares of insured acreage, 900 € premiums and 30,000 € coverage sum to the first municipality and 7 hectares of insured acreage, 2100 € premiums and 70,000 € coverage sum to the second municipality. Our sample contains for a given year all municipalities in which the data-providing insurer covers vineyards.

There are 3557 individual-year observations from 1973 unique individuals in the original sample. We exclude 18 farmers (36 individual-year observations), who cover less than 0.3 hectares, and 5 farmers (16 individual-year observations), who choose coverage sums above 30,000 € per hectare in Baden-Württemberg after the premium subsidy is introduced. We also exclude 12 farmers (19 individual-year observations) in Rhineland-Palatinate. These are farmers, whose total premiums for combined hail and frost coverage is below 400 € in 2021 such that they do not receive premium subsidies as they do not reach the minimum subsidy of200 €. These farmers are excluded from the premium subsidy and do not receive treatment. As all of these farmers are spread out across different municipalities and all other farmers within these municipalities are eligible for premium subsidies, we cannot use these farmers as control group in our municipality-level analysis and exclude them from the sample. The final sample is based on 3486 individual-year observations from 1938 unique individuals and includes 2058 municipality-year observations from 598 unique municipalities. For regression analyses, 2021 is excluded for the reasons elaborated below. Excluding 2021, there are 1464 municipality-year observations and 412 unique municipalities in the dataset. Data on disaster relief payments from 2017 are provided by the agricultural ministry of Baden-Württemberg ("Ministerium für Ländlichen Raum und Verbraucherschutz BadenWürttemberg"). The information includes the absolute amounts of disaster relief payments that were transferred to each municipality and the number of farmers receiving disaster relief per municipality. Data about the total size of vineyards per municipality in Baden-Württemberg are downloaded from the statistical office of Baden-Württemberg ("Statistisches Landesamt BadenWürttemberg"). Information on vineyards per municipality in Rhineland-Palatinate is provided by the statistical office of Rhineland-Palatinate ("Statistisches Landesamt Rheinland-Pfalz"). Data for modeling spring frost risks per municipality include weather data, which are downloaded from the German meteorological service ("Deutscher Wetterdienst"), and phenological data on the budbreak of different types of grapevine, which are provided by a German public research institution on winegrowing ("Staatliche Lehr- und Versuchsanstalt für Wein- und Obstbau Weinsberg").

Based on the available data, we construct variables to empirically analyze the introduction of the premium subsidy. We measure insurance demand using relative insurance participation as the share of insured vineyards in hectares per municipality. The variable is calculated by dividing the size of vineyards in hectares covered by the data-providing insurer by the total size of vineyards within a municipality based on the numbers from the statistical offices. As the number of covered hectares is based on one insurer only, the variable only captures a fraction of the market. There are three municipalities in our sample for which the statistical offices do not record any vineyards. Within these municipalities, we assume the total size of vineyards to be equal to insured vineyards. Premium levels are defined as the premium per € coverage sum per municipality. As Goodwin (1993), Smith and Baquet (1996) and Feng et al. (2019) suggest, an important determinant of insurance demand is the relation of indemnities to premiums. Using the terminology of Goodwin (1993), we define the loss ratio per municipality as the sum of claims over the sum of premiums and lag the variable by one period. We lag the variable to ensure that we capture the effect of loss ratios on insurance demand. To capture diverse levels of frost insurance participation within municipalities before the introduction of the subsidy, we measure past insurance demand by lagging the dependent variable of insurance participation by 1 year.

Last, we construct a variable capturing spring frost risk exposure, which is the main risk that the premium subsidy aims at. The variable is based on temperature data of the closest weather station of each municipality. Temperature data are an average of the temperature measured at five centimeters over the ground and two meters over the ground. Spring frost occurs when the temperature drops below 0°C after the buds of plants have opened up (Chmielewski et al., 2010; Vitasse & Rebetez, 2018). Data from a local agricultural research institute ("Staatliche Lehr- und Versuchsanstalt für Wein- und Obstbau Weinsberg") provides dates for all years of the time series, at which budbreaks of several types of grapevine have taken place. Choosing the most conservative way to model spring frost risks, we cumulate temperatures below 0°C after the date at which the earliest budbreak among all documented sorts of grapevine has taken place. The more negative the temperature is after budbreak occurs, the higher the risk of spring frost within a municipality. The cumulative temperature is multiplied by minus one for ease of interpretation. As weather stations are not located in every municipality, frost risk measures are averaged across counties based on all municipalities in which insured farmers are located. We lag the variable as farmers buy insurance before the potential frost events in a given year.

The absolute amount of disaster relief payments per municipality (DR) indicates the amount of disaster relief payments in €. As absolute disaster relief payments are highly correlated to municipality size, we create two relative measures of disaster relief payments that are more robust to differences in municipality size. The first variable (DR/ha) measures the amount of disaster relief payments per municipality size by dividing disaster relief payments per municipality by the total amount of vineyards in hectares per municipality. The second variable (DR/farmer) measures the amount of disaster relief payments per farmer per municipality by dividing disaster relief payments per municipality by the number of farmers receiving disaster relief payments per municipality. Panel (a) of Table 2 provides descriptive statistics of the variables used in the regression split by Pre-treatment years 2013–2019 and Post-treatment year 2020. Frost risk in BadenWürttemberg appears to be higher than frost risk in Rhineland-Palatinate as shown by the frost risk variable and slightly higher premium rates in Baden-Württemberg. Within municipalities in which the data-providing insurer covers farmers between 2013 and 2019, it covers on average 11% of vineyards in Baden-Württemberg and 5.1% in Rhineland-Palatinate. The average insurance participation per municipality increases to 24% in Baden-Württemberg and to 9.4% in Rhineland-Palatinate in 2020. Pre-treatment, there are also more municipality-year observations (n) in Rhineland-Palatinate than in Baden-Württemberg. This difference in the number of observations can be explained by overall more municipalities in Rhineland-Palatinate that grow wine (493 municipalities) than in Baden-Württemberg (296 municipalities) as shown in Panel (b) of Table 2.

Panel (b) of Table 2 shows descriptive statistics of the disaster relief payments in Baden Württemberg in 2017. Of 296 wine-growing municipalities in Baden-Württemberg, 152 or 51.35% received disaster aid in 2017. These 152 municipalities cover 21,967.83 hectares of vineyards or 80.45% of the overall 27,295.69 hectares in Baden-Württemberg. There are 6,464

TABLE2 Summary statistics of municipality-year observations and disaster relief payments excluding2021.

Panel (a) – Descriptive statistics of regression variables (2013–2020)						
	Pre-treatment (2013–2019)			**Post-treatment (2020)**		
Variables	***n***	**Mean**	**SD**	***n***	**Mean**	**SD**
Baden-Württemberg (treatment group)						
Insurance participation	284	0.11	0.17	185	0.24	0.24
Premium per coverage sum (in €)	284	0.031	0.010	185	0.030	0.0079
Loss ratio (lagged)[a]	167	>1	7.11	117	>1	3.37
Frost risk (lagged)	167	7.5	10.03	117	5.99	4.48
Insurance participation (lagged)	167	0.11	0.17	117	0.12	0.18
Rhineland-Palatinate (control group)						
Insurance participation	788	0.051	0.092	207	0.094	0.16
Premium per coverage sum (in €)	788	0.023	0.0067	207	0.025	0.0051
Loss ratio (lagged)[a]	586	>1	4.47	182	<1	4.08
Frost risk (lagged)	586	3.94	5.73	182	2.46	2.44
Insurance participation (lagged)	586	0.040	0.053	182	0.086	0.16

Panel (b) – Descriptive statistics of disaster relief payments from 2017				
	All winegrowing municipalities		**Winegrowing municipalities receiving disaster relief**	
	Overall	**In-sample (2013–2020)**	**Overall**	**In-sampe (2013–2020)**
Baden-Württemberg				
# Municipalities	296	185	152	123
Size of vineyards	27,295.69 ha[b]	25,287.79 ha	21,967.83 ha[b]	21,266.05 ha
Rhineland-Palatinate				
# Municipalities	493	227	—	—
Size of vineyards	64,735.84 ha	49,400 ha	—	—
Specifics of disaster relief payments				
Total disaster relief to winegrowers (DR)			14,905,209 €	14,224,769 €
Number of applying farmers			947	899
Disaster relief per farmer (DR/farmer)			15,739.40 €	15,822.88 €
Disaster relief per hectare vineyard (DR/ha)			678.80 €	668.90 €

a Exact loss ratios are not reported to ensure confidentialityof the data.

b There are nine municipalities outside our sample, which are not recorded by the statistical offices of Baden-Württemberg. The

size of vineyards of municipalities within Baden-Württemberg may there fore be slightly underestimated.

winegrowers in Baden-Württemberg, of which 947 or 14.65% received disaster relief payments (Statistisches Bundesamt, 2020). The overall disaster relief payments to winegrowers in 2017 are 14,905,209 € of which 14,224,769 € went to municipalities within our sample. The municipalities within our sample that received disaster relief payments cover 21,266.05 hectares of vineyards and 899 of the farmers who applied for disaster relief payments. The resulting average disaster relief payment per appyling farmer within our sample is 15,822.88 € and the resulting average disaster relief payment per hectare of vineyard is 668.90 €.

5 | METHODOLOGY

To estimate the causal effect of the premium subsidy on insurance demand in Baden Württemberg, we follow a difference-in-differences approach at the municipality level, whereby Baden-Württemberg is the treatment group and Rhineland-Palatinate the control group. With this approach, we can exploit the quasi-natural experimental setting, which allows us to estimate the effect of the subsidy on insurance demand without the need for access to extensive firm-level data. For the introduction of the premium subsidy in Baden-Württemberg in 2020, Rhineland-Palatinate serves as the control group. As Rhineland-Palatinate introduces a subsidy on its own in 2021, it cannot serve as a control group for 2021, as it also receives a treatment. For this reason, we exclude data on 2021 from all regression analyses and only provide an estimate of the initial effect of the subsidy in 2020.

Rhineland-Palatinate is a direct neighbor state to Baden-Württemberg with a common border along the River Rhine. Their special geography and climate provide favorable conditions for wine cultivation, which makes them the two largest wine producing states in Germany. The wine producing areas in the two states are shown dark gray in Figure 3. Most wine is grown along the Rhine. Rhineland-Palatinate grows large amounts of wine to the west of the Rhine, Baden-Württemberg to the east of the Rhine. Each state has an additional winegrowing region at tributaries of the Rhine. The Mosel region in Rhineland-Palatinate and the area around the Neckar in Baden-Württemberg are the second largest winegrowing regions in each state. The geographic proximity and similarity in growing wine along the Rhine makes the two states closely related. The two states also agreed on a close cooperation between their agricultural ministries in 2015 making their agricultural policy environments intertwined and comparable (Ministerium für Laendlichen Raum und Verbraucherschutz, 2015). The identifying assumptions for difference-in-differences analyses are parallel trends and no anticipatory effects (Roth, Sant'Anna, Bilinski, & Poe, 2023). The parallel trends assumption implies that insurance demand for frost insurance in Rhineland-Palatinate and BadenWürttemberg would have followed the same trends had Baden-Württemberg not introduced the premium subsidy. Assuming no anticipatory effects implies that farmers in BadenWürttemberg do not anticipate the introduction of the premium subsidy. They are assumed to behave, before the treatment, as if no premium subsidy was introduced the following year. The year of treatment is 2020, in which Baden-Württemberg started to subsidize premiums. In all specifications, insurance participation is the dependent variable. We include municipality fixed effects to control for time-invariant confounding factors such as municipality size or specific geography of a municipality and year fixed effects to control for municipality-invariant confounding factors such as inflation or other common economic shocks. To control for confounding factors that affect insurance demand, and that differ across Baden-Württemberg and Rhineland-Palatinate and vary over time, we use additional control

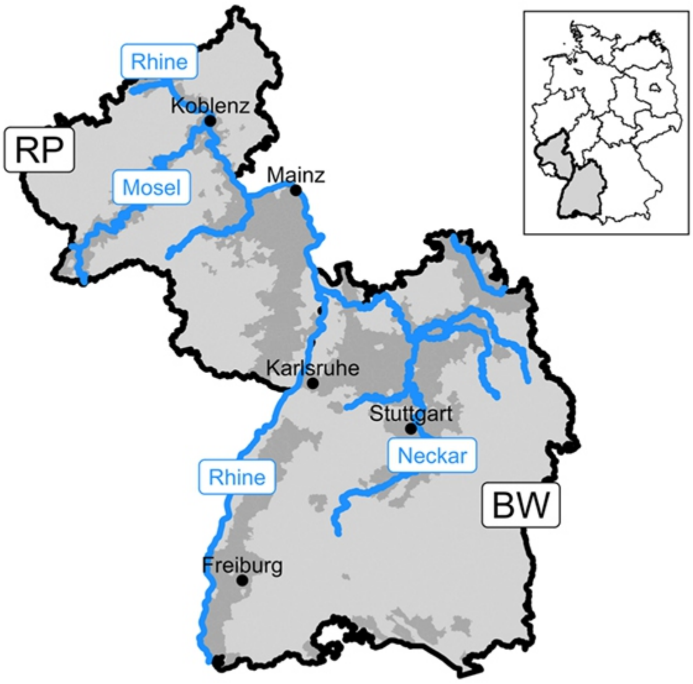

FIGURE3 Winegrowing areas in Baden-Württemberg (BW) and Rhineland-Palatinate (RP) (winegrowing municipalities in the dark gray).

variables. Our specification is a dynamic two-way fixed effects model by Roth et al. (2023) with added control variables. We add control variables based on the assumption from Meyer (1995) that they have the same effect on insurance participation in treatment and control group. The resulting specification adapts notation from Angrist and Pischke (2009, p. 237) and can be formalized as follows:

$$y_{it} = \mu_t + \omega_i + \sum_{\tau=-T_{Pre}}^{-2} \delta_\tau^{lead} * D_{it}^\tau + \sum_{\tau=0}^{T_{Post}} \delta_\tau^{lag} * D_{it}^\tau + X_{it}'\beta + \varepsilon_{it}, \quad (11)$$

where i and t denote municipalities and years with T_{Pre} denoting the total number of years before the year of treatment and T_{Post} the total number of years after the year of treatment. In our panel dataset from 2013 to 2020 with treatment taking place in 2020, there are seven potential pretreatment years ($T_{Pre} = 7$) and 0 posttreatment years ($T_{Post} = 0$) as we only observe the initial treatment year 2020 where $\tau = 0$. μ and ω denote year and municipality fixed effects, D_{it}^τ is a dummy variable that turns 1 when the treatment is τ periods away and municipality i is part of the treatment group. X_{it} denotes a vector of control variables that vary over time and municipality. The coefficients δ_τ^{lead} describe the effects of the dummy variable D_{it}^τ turning 1 when the treatment lies in the future, δ_τ^{lag} describe the effects of the dummy variable D_{it}^τ turning 1 when the treatment lies in the past. δ_{-1}^{lead} is excluded for interpretability. The coefficient δ_0^{lag} describes the difference between Baden-Württemberg and Rhineland-Palatinate between 2019 and 2020, which is the immediate treatment effect. δ_{-2}^{lead} describes the difference between Baden-Württemberg and Rhineland-Palatinate between 2019 and 2018 (δ_{-3}^{lead} the difference between Baden-Württemberg and Rhineland-Palatinate between 2019 and 2017 and so on). We can use the coefficients δ_τ^{lead} to test for parallel trends and anticipation of treatment. The coefficients δ_τ^{lag} are the treatment effects relative to the last year before the treatment, which is 2019 in our context.

Standard errors are clustered at the municipality level. Recent literature has questioned the assumption of independence across cross-sectional units (e.g., municipalities) when the treatment takes place at a higher level (e.g., state) and suggests clustering at the treatment level for reliable inference (e.g., MacKinnon et al., 2023). As our data limitations do not allow for clustering on the state level, we must assume that municipalities react independently to the state-level treatment. Bertrand et al. (2004) have shown that this may result in a significant underestimation of standard errors and over rejection of t-tests. Thus, we must interpret the inferential results with caution.

We add control variables that affect insurance demand but change over time and vary between Rhineland-Palatinate and Baden-Württemberg. We control for premiums per coverage sum to account for different prices of insurance. Woodard and Yi (2020) show that premiums are endogenous to coverage levels because premiums are linked to coverage levels by a rate curve. Measuring insurance demand as coverage level and using premiums as independent variable leads to biased results. Our measure of insurance demand is based on insured acreage and independent of coverage levels such that including premiums as control variable should not bias the results. To control for favorability of insurance contracts based on Goodwin (1993), we control for lagged loss ratios (indemnity payments divided by premium). To additionally control for different levels of risk exposure based on weather data, we control for lagged frost risk exposure. Last, we control for lagged insurance participation to capture potential serial correlation of insurance demand. Including lagged dependent variables as control variables can cause biases (Nickell, 1981). We present the effect of including the potentially biasing lagged dependent variable separately and provide further discussion in the results section (Section 6). We observe serial correlation of insurance demand in our data, but it seems that including a lagged dependent variable does not strongly affect the coefficients, which suggests that the resulting bias is small. The second part of the regression analysis evaluates whether treatment effects are heterogeneous among groups having received different amounts of disaster relief payments. To analyze the heterogeneity of the treatment, we split the treated sample into subsamples. We run separate regressions for each subsample of the treatment group excluding observations outside the subsample. First, the sample is split into two subsamples (“none”, “some”) separating municipalities that did not receive any disaster relief payments and municipalities that received some positive amount of disaster relief. A municipality in Baden-Württemberg does not receive any disaster relief payments (“none”), when no farmer within the municipality had uninsured losses above 30% of his harvest. In municipalities, which receive disaster aid (“some”), there is at least one farmer, whose uninsured losses are above 30% of his harvest, which makes him eligible for disaster relief. These specifications compare subsamples of different loss experiences in 2017 measured by disaster relief payments in Baden-Württemberg to the full sample of municipalities in Rhineland-Palatinate.

A more accurate control group would only include municipalities in Rhineland-Palatinate that had similar losses in 2017. The “none” group in Baden-Württemberg would be compared to municipalities in Rhineland-Palatinate, in which all farmers lost less than 30% of their uninsured harvest in 2017. The “some” group in Baden-Württemberg would be compared to municipalities in Rhineland-Palatinate where at least one farmer lost more than 30% of her uninsured harvest in 2017. We use the full sample of municipalities from Rhineland-Palatinate as control group because we do not observe uninsured losses in Rhineland-Palatinate in 2017. We provide results on a restricted sample in Rhineland-Palatinate in Appendix D. There, we use insured losses from 2017 as a proxy for uninsured losses and construct a control group, that may better match the loss experience of the treatment group from 2017. We further split up municipalities that received disaster relief payments into three subsamples (“low”, “medium”, “high”) according to the amount of disaster relief payments they received. As described in Section 4, absolute disaster relief payments (DR) are highly correlated with municipality size, which is why we construct the variables DR/ha and DR/ farmer. We set up all subsamples in such a way that the number of treated municipalities and treated municipality-year observations is nearly equal in all groups. Using a variety of variables to split up the sample can rule out the possibility that municipality size drives the results. Table 3 shows how the subsamples are constructed.

Political factors leading to the introduction of the treatment are a potential source of endogeneity that may bias the estimation. Besley and Case (2000) show that policy introductions may be endogenous to differences in the treatment and control groups even though parallel trends hold. The argument against an endogenous policy introduction in our setting is the fact that the control group (Rhineland-Palatinate) agrees on introducing a similar policy 1 year after the treatment group (Baden-Württemberg). Factors that lead to the introduction of a premium subsidy must therefore be comparable in both groups. As we only observe insurance demand from one insurance company, there may be an underlying selection bias within the data. When farmers who are insured at the Vereinigte Hagel VVaG are different from farmers represented by other insurance companies with respect to their insurance demand, the estimated treatment effects may be biased. As the Vereinigte Hagel VVaG is the largest player on the German crop insurance market, covering over 57% of overall premiums, we expect the sample to be generally representative and potential selection biases to be negligible (BMEL, 2019). The composition of our sample also seems to represent the overall German winegrowing population based on data of the national German statistical

TABLE3 Subsamples for analysis of heterogeneous treatment effects.

	low	medium	high
DR			
Intervals	[2,905.4; 34,216.41]	[34,216.41; 106,301.57]	[106,301.57; 942,507.19]
# Municipalities	45	39	39
# Municipality-year observations	102	111	112
DR/ha			
Intervals	[9.25; 428.85]	[428.85; 1,071.59]	[1,071.59; 18,234,21]
# Municipalities	44	40	39
# Municipality-year observations	102	110	113
DR/farmer			
Intervals	[2,905.4; 11,212.31]	[11,212.31; 15,683.52]	[15,683.52; 100,000]
# Municipalities	42	39	42
# Municipality-year observations	103	112	110

office (Statistisches Bundesamt, 2020). Table 4 presents how overall vineyards are distributed across winegrowers of different size. For example, winegrowers owning between 10 and 20 hectares of vineyards manage 29.89% of overall vineyards in Germany. In our sample 33.21% of vineyards are managed by winegrowers who own between 10 and 20 hectares of vineyards. Within our sample, large farmers seem to be overrepresented compared to the overalldistribution in Germany. Prior research suggests that smaller farmers are less likely to buy insurance (Coble et al., 1996; Enjolras et al., 2012;Wąs & Kobus, 2018). We expect that this holds for all insurance companies and is not specific to the Vereinigte Hagel VVaG making our sample representative of the population of insured farmers.

6 | RESULTS

Figure 4 presents the insurance demand of municipalities in Baden-Württemberg (BW)–split into the "none" and "some" group–and in Rhineland-Palatinate (RP). Before 2016, insurance participation is below 2% in all groups. In 2013, no farmer in Baden-Württemberg buys frost coverage. Around the loss event in 2017, participation slightly increases to around 3% in 2018. There appears to be a small descriptive effect of loss experience on insurance demand in 2018. It also appears that municipalities that did not receive disaster relief payments had higher insurance coverage in 2017 compared to municipalities that did receive disaster relief payments. Contracts are

TABLE4 Sample composition.

Size of winegrowers'vineyards (in ha)	Share of acreagein Germany	Share of acreagein sample
<5	16.26%	8.56%
5–10	17.88%	17.90%
10–20	29.89%	33.21%
>20	35.96%	40.61%

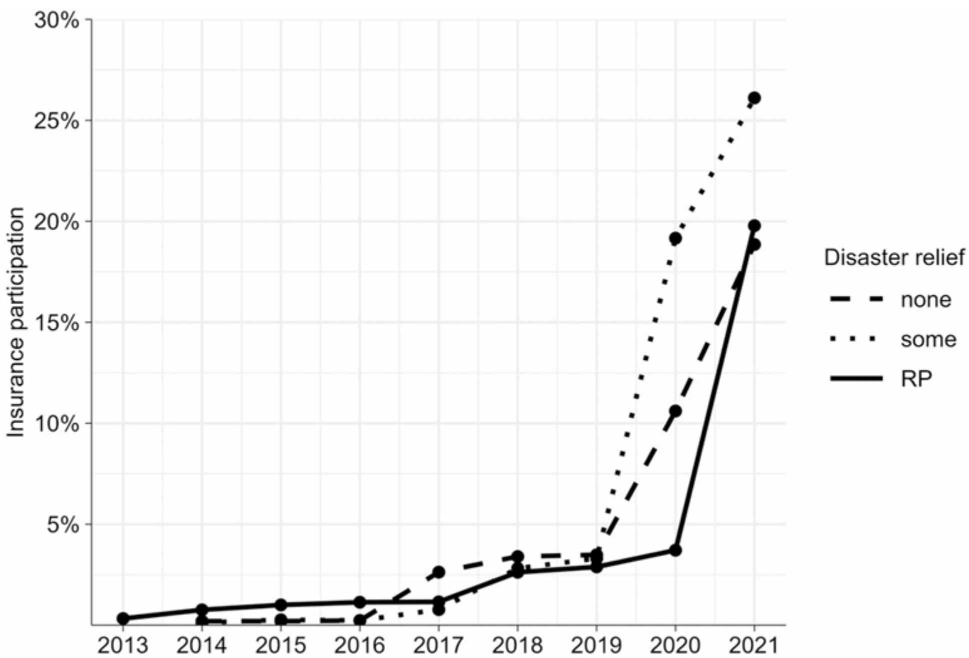

FIGURE4 Insurance participation by state and disaster relief group.

bought at the beginning of the year such that insurance participation in 2017 is independent of disaster relief payments later on in the year. The effect of disaster relief payments only shows from 2018 onwards. We use disaster relief payments as a measure for catastrophic loss size per municipality. When the amount of disaster relief payments is driven by insurance coverage before the loss event, disaster relief payments may not be an accurate measure of loss experience. In our data, insured losses per coverage sum are positively correlated at the municipality level with disaster relief payments in 2017 in Baden-Württemberg at 10% significance (Pearson's correlation coefficient: 0.2597, p: 0.066). Municipalities that do not receive disaster relief payments tend to have also had lower insured losses in 2017, suggesting that slightly higher insurance coverage in 2017 does not drive the size of disaster relief payments. The changes of insurance demand between 2016 and 2018 are relatively small changes compared to the effect sizes of the premium subsidies in 2020 and 2021.

Insurance participation strongly increases in the year in which the premium subsidy is introduced in either state. In Baden-Württemberg, insurance participation in the first year of subsidization increases by 14.15% points and an additional 7.20% points in the second year after introduction. The overall effects are averages of the subgroup effects weighted by the size of vineyards in either subgroup (21,967.83 ha in the “some” group and 5,327.86ha in the “none” group, see Panel (b) of Table 2). Insurance participation in the “some” group increases by 15.86% points and insurance participation in the “none” group by 7.12% points in the first year of subsidization. The descriptive effect of the subsidy on insurance participation in the first year after implementation is much stronger in those municipalities that receive disaster relief payments in 2017. In the second year after the introduction, the increases are 6.94% points in the “some” group and 8.24% points in the “none” group. It appears that in the second year after the introduction of the subsidy, the two groups no longer differ showing a similar increase from 2020 to 2021. According to our Hypothesis 2, the main difference between the two groups is their loss experience, which leads to different insurance coverage once the premium subsidy eliminates charity hazard. We expect that all other determinants of insurance demand affect both groups similarly such that once the effects of loss experience become visible, both groups follow similar trends again. In Rhineland-Palatinate, insurance participation increases by 16.07% points in 2021, the first year of subsidy introduction in the state.

To further analyze how the effectiveness of the premium subsidy is linked to the reception of disaster relief payments, we split the sample of treated observations into four groups according to the amount of disaster relief payments, measured by DR/ha and DR/farmer (see Table 3). Figure 5 shows the development of insurance participation for these groups. Further splitting the sample underlines that the importance of recent disaster relief reception is primarily visible in the immediate reaction toward the subsidy in 2020. The immediate increase in insurance demand seems to be stronger for those municipalities that receive higher amounts of disaster relief payments, whereby the municipalities that receive the highest amounts of disaster relief payments are most responsive when the subsidy is introduced. Within the second year after subsidy introduction, differences in the groups are not visible, and all groups develop in an approximate parallel manner. As above, we hypothesize that once the premium subsidy reduces charity hazard and the effects of loss experience become visible, the groups follow similar trends as their insurance demand is based on similar determinants.

We present regression results in Tables 5–7. We show our main analysis in the specifications (1), (2) and (3) of Table 5. Specifications (4)–(9) of Table 5 show subsample analyses based on the "some" and "none" group in Baden-Württemberg and Figure 4. Tables 6 and 7 show regression results on subsamples based on DR/farmer and DR/ha. Regression

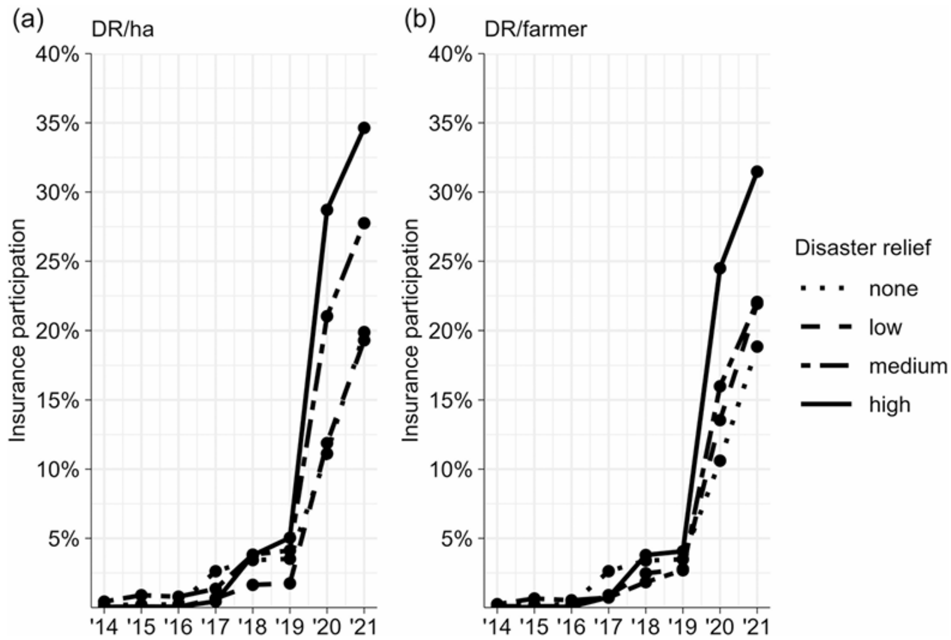

FIGURE5 Insurance participation in Baden-Württemberg by disaster relief group.

results on subsamples based on DR are shown in Appendix E. We show the effect of lagged insurance participation separately and discuss the potential bias from including the lagged dependent variable in the following.

From specifications (1)–(3) in Table 5, we see that the premium subsidy leads to an average increase of insurance demand per municipality of 13.30–13.85% points depending on the set of control variables. In the "none" group, shown in specifications (4)–(7) in Table 5 the premium subsidy increases insurance demand by on average 5.61–7.07% points. As in Figure 4, the premium subsidy has the strongest effect on the “some” group with an average increase per municipality of 16.18–17.02% points (see specifications (7)–(9) in Table 5). All treatment coefficients are in line with the descriptive Figure 4. The treatment effects are also robust across specifications suggesting that no single control variable drives the effects. Dividing farmers who have received disaster relief payments into three groups reveals that treatment effects tend to be higher when more disaster relief has been received. When the sample is split by DR/ha, treatment effects are 10.35%, 15.50% and 21.34% points for the low, medium and high levels of disaster relief payments (specifications (3), (6) and (9) in Table 6). Splitting the subsample by DR/farmer, treatment effects are 9.88%, 15.75% and 22.97% points respectively (specifications (3), (6) and (9) in Table 7). Treatment effects in Table 6 and Table 7 also only slightly vary by covariate specification. The results suggest that the size of the treatment effect is positively correlated with the amount of disaster relief payments received in 2017. In Figure 4 there is a small descriptive increase of insurance demand after the loss event in 2017. If the effect of loss experience was different between Baden-Württemberg and Rhineland-Palatinate, insurance demand in 2018 should differ between the states. The coefficients δlead −2 are however insignificant in the regression results with control variables in Table 5. Insurance demand in 2018 does not differ between Rhineland-Palatinate and Baden-Württemberg suggesting a similar effect of loss experience on both groups.

TABLE5 Regression results of main specification.

Disaster relief in 2017 > 0 (“some”)		
(7)	(8)	(9)
0.1618***	**0.1702*****	**0.1629*****
(0.014)	**(0.016)**	**(0.017)**
−0.01142*	−0.01895	−0.007386
(0.0049)	(0.011)	(0.013)
−0.04502***	−0.07952**	−0.04945*
(0.013)	(0.024)	(0.020)
−0.08036***	−0.07592*	−0.05195*
(0.018)	(0.029)	(0.024)
−0.07195*	−0.08465	−0.07037
(0.031)	(0.063)	(0.063)
−0.09086*	—	—
(0.040)		
—	0.7392	−0.1081
	(0.64)	(0.46)
—	0.001093	0.001410*
	(0.00071)	(0.00069)
—	0.00006025	0.0005526
	(0.00034)	(0.00035)
—	—	0.7399***
		(0.086)

Insurance participation						
	Full sample			Disaster relief in 2017 = 0 ("none")		
	(1)	(2)	(3)	(4)	(5)	(6)
δ_0^{lag}	**0.1376*** **	**0.1385*** **	**0.1330*** **	**0.07067** **	**0.06137** **	**0.05611** **
	(0.012)	**(0.014)**	**(0.014)**	**(0.022)**	**(0.021)**	**(0.020)**
δ_{-2}^{lead}	−0.007933	−0.003921	0.006853	0.0003634	0.01576	0.02405
	(0.0054)	(0.010)	(0.012)	(0.015)	(0.018)	(0.025)
δ_{-3}^{lead}	−0.03370**	−0.05978*	−0.03873	−0.02148	0.06542	0.06452*
	(0.013)	(0.030)	(0.026)	(0.026)	(0.040)	(0.029)
δ_{-4}^{lead}	−0.06210**	−0.05146	−0.03472	0.02670	0.05677	0.06281
	(0.022)	(0.036)	(0.033)	(0.038)	(0.051)	(0.051)
δ_{-5}^{lead}	−0.05721*	−0.05971	−0.04789	0.004744	0.03467	0.04084
	(0.028)	(0.049)	(0.049)	(0.024)	(0.036)	(0.036)
δ_{-6}^{lead}	−0.06262	—	—	0.004744	—	—
	(0.034)			(0.024)		
Premium per coverage sum	—	0.2968	−0.4479	—	−0.001926	−0.7236
		(0.66)	(0.53)		(0.36)	(0.49)
Loss ratio (lagged)	—	0.0007416	0.0009578	—	0.002463***	0.002455***
		(0.00067)	(0.00068)		(0.00072)	(0.00071)
Frost risk (lagged)	—	0.0002170	0.0006103	—	−0.0007222*	−0.00008034
		(0.00034)	(0.00034)		(0.00033)	(0.00028)
Insurance participation (lagged)	—	—	0.6268***	—	—	0.6764***
			(0.13)			(0.12)

TABLE5 (Continued)

Insurance participation									
	Full sample			**Disaster relief in 2017 = 0 ("none")**			**Disaster relief in 2017 > 0 ("some")**		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**
Two-way FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# Obs.	1,464	1,052	1,052	1,139	850	850	1,320	970	970
# Treated obs.	469	284	284	144	82	82	325	202	202
# Control obs.	995	768	768	995	768	768	995	768	768
R^2	0.36	0.34	0.46	0.062	0.071	0.37	0.43	0.43	0.57

Note: Significance code. Main coefficients of interest in bold and standard errors in parentheses. ***0.1%. **1%. *5%.

TABLE6 Regression results on subsamples (split by DR/ha).

Subsample analysis of insurance participation (split by DR/ha)

	low			medium			high		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
δ_0^{lag}	**0.1156*** **	**0.1169*** **	**0.1035*** **	**0.1528*** **	**0.1587*** **	**0.1550*** **	**0.2079*** **	**0.2226*** **	**0.2134*** **
	(0.019)	**(0.023)**	**(0.027)**	**(0.016)**	**(0.017)**	**(0.018)**	**(0.029)**	**(0.033)**	**(0.033)**
δ_{-2}^{lead}	−0.01084	−0.02579*	−0.01859	−0.006895	−0.02680	0.009022	−0.01558*	−0.02424	−0.03155
	(0.0092)	(0.013)	(0.018)	(0.0090)	(0.014)	(0.017)	(0.0076)	(0.023)	(0.024)
δ_{-3}^{lead}	−0.04746*	−0.09594***	−0.04553**	−0.07002**	−0.1064***	−0.05092**	−0.02253	−0.01788	−0.02294
	(0.020)	(0.015)	(0.017)	(0.027)	(0.023)	(0.016)	(0.018)	(0.053)	(0.050)
δ_{-4}^{lead}	−0.1004***	−0.09632***	−0.04550**	−0.1009***	−0.1237***	−0.06600***	−0.03879	−0.01829	−0.02835
	(0.0090)	(0.015)	(0.017)	(0.019)	(0.016)	(0.013)	(0.037)	(0.053)	(0.049)
δ_{-5}^{lead}	−0.1004***	—	—	−0.06990	−0.01967	0.01959	−0.05172	−0.1231***	−0.1313***
	(0.0090)			(0.062)	(0.11)	(0.091)	(0.042)	(0.031)	(0.035)
δ_{-6}^{lead}	—	—	—	−0.05663	—	—	−0.1048***	—	—
				(0.072)			(0.022)		
Premium per coverage sum	—	0.04915	−0.8047*	—	0.4508	−0.3630	—	0.7784	−0.05079
		(0.62)	(0.35)		(0.62)	(0.34)		(0.66)	(0.41)
Loss ratio (lagged)	—	0.002137**	0.002487***	—	0.002220**	0.002330***	—	0.001657*	0.001712*
		(0.00072)	(0.00070)		(0.00070)	(0.00068)		(0.00075)	(0.00075)
Frost risk (lagged)	—	−0.0004997	0.00003807	—	−0.0005339	0.0001906	—	−0.0001637	0.0006623
		(0.00033)	(0.00032)		(0.00030)	(0.00027)		(0.00034)	(0.00034)
Insurance participation (lagged)	—	—	0.7726***	—	—	0.8233***	—	—	0.8326***
			(0.081)			(0.063)			(0.062)

TABLE6 (Continued)

Subsample analysis of insurance participation (split by DR/ha)

	low			medium			high		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Two-way FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# Obs.	1,097	826	826	1,105	838	838	1,108	842	842
# Treated obs.	102	58	58	110	70	70	113	74	74
# Control obs.	995	768	768	995	768	768	995	768	768
R^2	0.16	0.17	0.50	0.28	0.29	0.60	0.35	0.36	0.59

Note: Significance code. Main coefficients of interest in bold and standard errors in parentheses.

***0.1%. **1%. *5%.

TABLE7 Regression results on subsamples(split byDR/farmer).

Subsample analysis of insurance participation (split by DR/farmer)	low			medium			high		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
δ_0^{lag}	**0.09954*** **	**0.1074*** **	**0.09877*** **	**0.1609*** **	**0.1697*** **	**0.1575*** **	**0.2270*** **	**0.2354*** **	**0.2297*** **
	(0.020)	**(0.025)**	**(0.028)**	**(0.018)**	**(0.022)**	**(0.023)**	**(0.026)**	**(0.030)**	**(0.030)**
δ_{-2}^{lead}	−0.009716	−0.02515	−0.005148	−0.01519	−0.02495	−0.01965	−0.005472	−0.007112	−0.003492
	(0.010)	(0.019)	(0.023)	(0.0084)	(0.015)	(0.017)	(0.0043)	(0.016)	(0.020)
δ_{-3}^{lead}	−0.06078	−0.2017***	−0.1336***	−0.03328*	−0.04227	−0.01591	−0.03663*	−0.1179***	−0.09219***
	(0.031)	(0.011)	(0.017)	(0.015)	(0.026)	(0.017)	(0.020)	(0.016)	(0.025)
δ_{-4}^{lead}	−0.1590***	−0.1818***	−0.1262***	−0.05021*	−0.03978	−0.02120	−0.1149***	−0.1336***	−0.1169***
	(0.011)	(0.013)	(0.016)	(0.021)	(0.034)	(0.024)	(0.0089)	(0.014)	(0.015)
δ_{-5}^{lead}	−0.1612***	−0.1847***	−0.1320***	−0.01676	0.1901***	0.1911***	−0.1325***	−0.1407***	−0.1377***
	(0.011)	(0.013)	(0.016)	(0.038)	(0.023)	(0.023)	(0.025)	(0.024)	(0.029)
δ_{-6}^{lead}	−0.1562***	—	—	0.05994**	—	—	−0.1316***	—	—
	(0.011)			(0.020)			(0.025)		
Premium per coverage sum	—	−0.02830	−0.8506*	—	0.4160	−0.4414	—	0.8308	−0.07305
		(0.62)	(0.37)		(0.60)	(0.32)		(0.66)	(0.38)
Loss ratio (lagged)	—	0.002428**	0.002511***	—	0.001782*	0.002007**	—	0.001884**	0.002122**
		(0.00076)	(0.00073)		(0.00070)	(0.00070)		(0.00070)	(0.00068)
Frost risk (lagged)	—	−0.0005936	0.00009574	—	−0.0003157	0.0003539	—	−0.0003855	0.0002999
		(0.00035)	(0.00035)		(0.00031)	(0.00029)		(0.00032)	(0.00030)
Insurance participation (lagged)	—		0.7720***	—	—	0.8324***	—	—	0.8075***
			(0.081)			(0.062)			(0.065)

TABLE7 (Continued)

Subsample analysis of insurance participation (split by DR/farmer)	low			medium			high		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Two-way FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# Obs.	1,098	829	829	1,107	841	841	1,105	836	836
# Treated obs.	103	61	61	112	73	73	110	68	68
# Control obs.	995	768	768	995	768	768	995	768	768
R^2	0.15	0.17	0.50	0.27	0.27	0.59	0.40	0.40	0.63

Note: Significance code. Main coefficients of interest in bold and standard errors in parentheses.
***0.1%.
**1%.
*5%.

To summarize the results, Figure 6 presents regression results in an event-study plot. Panel (a) refers to specification (3) of Table 5, Panel (b) to specifications (6) and (9) of Table 5 and Panel (c) to specifications (3), (6) and (9) of Table 6 with subsamples based on DR/ha. Panel (a) shows the overall treatment effect of 13.30 percentage points and Panel (b) shows treatment effects of 5.61% and 16.29% points. For the specifications shown in these two panels, none of the pre-trends is significantly different from 0 ensuring that the parallel trends assumption holds (at the 99% significance level). Panel (c) presents treatment effects of 10.35%, 15.50% and 21.34% points. While the treatment effects in Panel (c) are all significant, the results are to be taken with caution, as some of the pre-trends are significantly different from 0, violating the identifying assumption of parallel trends. The results merely provide an indication that receiving higher disaster relief payments may increase responsiveness to premium subsidies.

Figure 6 also shows that there are no visible anticipatory effects. The last trend before the policy implementation δ_{-2}^{lead} does not differ between the two states in any of the specifications. As Rhineland-Palatinate also introduces a similar policy 1 year after Baden-Württemberg, both states were similarly close to introducing premium subsidies in 2019, making it unlikely that farmers in Baden-Württemberg behaved differently just before the subsidy was introduced. Farmers also do not benefit from anticipating the introduction of premium subsidies. They can forgo purchasing insurance in 2019, but they do not benefit from purchasing less insurance before the introduction of premium subsidies. Based on insignificant pre-trends, similar policy situations in Baden-Württemberg and Rhineland-Palatinate and no benefit from anticipation, we do not expect any anticipatory behavior of farmers in Baden-Württemberg.

As shown in Figure 6 and discussed above, the parallel trends assumption is not violated in Panel (a) and Panel (b). For the "some" group–the subsample of municipalities that receive disaster relief payments–we can however only reject all pre-trends in specification (9) of Table 5. The trend δ_{-3}^{lead} is significantly different from 0 at 99% significance in specification (7) and (8). Specification (9) includes the lagged dependent variable as a control variable

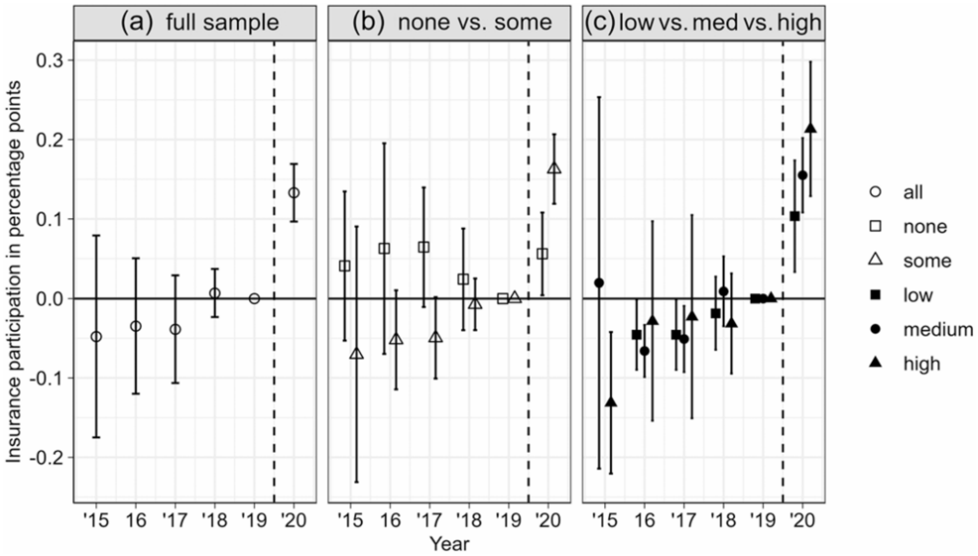

FIGURE6 Event-study plot of regression results shown in Table 5 and Table 6 (confidence intervals at 99% significance).

potentially biasing the coefficients. We argue that the lagged dependent variable does not severely bias the regression as all coefficients remain roughly the same as in specification (7) and (8). Following Nickell (1981), we can also analyze the direction of the bias. To identify the direction of the bias we regress lagged insurance participation on all independent variables from specification (9). The regression results of this auxiliary regression are shown in Table F1 in Appendix F. We find that the coefficient δ_{-3}^{lead}, which violates the parallel trends assumption in specification (8), is negative (δ_{-3}^{lead}: -0.04064) in this auxiliary regression. Based on Nickell (1981), that means that the coefficient δ_{-3}^{lead} is downward biased when including the lagged dependent variable in the regression. As the coefficient is negative in specification (9) and biased downwards, the unbiased coefficient is expected to be larger and closer to zero which makes it even more likely that the trend is not significantly different from zero. We do not expect the bias to be so large that the unbiased coefficient is positive and significant as all other treatment coefficients also only marginally change in specification (9) compared to specification (8). Overall, we argue that parallel trends hold in specification (9) and that the potential bias from including the lagged dependent variable is negligible as all treatment coefficients remain close to the values from specifications (7) and (8).

7 | DISCUSSION AND CONCLUSIONS

The descriptive analysis and the regression results show that the premium subsidy in Baden Württemberg has been an effective instrument in increasing overall insurance participation. Although the analysis focuses on Baden-Württemberg, descriptive results suggest that a similar increase takes place in Rhineland-Palatinate in 2021 after a similar subsidy is introduced. The size of the increase in Baden-Württemberg is highly relevant, as insurance participation increases by on average 13.30% points per municipality. The data-providing insurer covers approximately 30% of all winegrowers within the sampled area against hail damage. Given that the hail insurance market is usually referred to as a functioning market, 13.30% points is a sizeable increase. We find support for Hypothesis 1. Based on our hypothesis development in Section 3, the price effect of the subsidy by itself is unlikely to explain the entire increase of insurance demand. The size of the increase in insurance demand suggests that the premium subsidy is also able to lower the anticipation of future disaster relief payments. The additional decrease of charity hazard is also able to explain how our results are in contrast to a variety of crop insurance studies from the U.S. (e.g., O'Donoghue, 2014), which find inelastic demand among farmers and question premium subsidies. It may be that the price effect of the premium subsidy in our study is also small, which would be in line with inelastic demand, and that the results are mostly driven by a decrease in charity hazard. Garrido and Zilberman (2008) show that premium subsidies are an important driver of insurance demand on Spanish crop insurance markets, which our results can confirm for the German frost insurance market.

Analyzing the role of disaster relief payments shows that receiving recent disaster relief payments seems to be an important parameter in farmers' immediate response to the premium subsidy. Those municipalities that experienced catastrophic losses and received disaster relief payments are significantly more responsive toward premium subsidies than those municipalities that have not been subject to severe losses. There also seems to be a tendency that higher disaster relief payments make farmers even more responsive compared to lower disaster relief payments. We hypothesize in Section 3, that receiving large amounts of disaster relief payments implies catastrophic loss experience, which has been shown to increase insurance demand (Cai & Song, 2017; Che et al., 2019; Gallagher, 2014; Kousky, 2017). In our setting, the effect of loss experience is initially not visible because charity hazard depresses insurance demand. Once the premium subsidy reduces charity hazard, different levels of loss experience become visible, leading to higher insurance demand in municipalities that experienced losses and received disaster relief payments. We find the premium subsidy to be more effective among farmers who have received disaster relief payments, supporting Hypothesis 2.

Both findings suggest that the state eliminated or at least lowered charity hazard by introducing thepremiumsubsidy.Thestatewasabletousetheintroductionasacrediblecommitmentdevice to lower anticipation of future ex post disaster relief payments. We cannot identify what share of the overall increase in insurance demand is attributable to the lowering of charity hazard and thereduction of prices. The long-term effects of the premium subsidy are also unclear. Based on the data for this study and if trends from the first 2 years of the premium subsidy continue (~13% points increase in the first year and ~7% points in the second year), insurance demand could converge to a level of insurance participation around 30% in a couple of years. 30% insurance participation would be the same market share of the data-providing insurer as in the private hail insurance market.

Farmers would routinely add frost coverage to their hail insurance contracts. The state would be able to observe the costs of premium subsidies necessary to uphold frost insurance coverage and could assess how premium subsidies compare to disaster relief payments. Other climate-related risks such as drought, wildfires and heat waves have become more salient recently. Coverage against damages from these events is not always part of standard insurance products. When people are not aware of risks and do not buy insurance, states are often pressured into making disaster relief payments when losses are large. The resulting anticipation of future disaster relief payments depresses insurance demand. Premium subsidies as on the German frost insurance market are one potential policy change to eliminate charity hazard and to promote private insurance markets. Once a private market establishes, people experience payouts themselves, or see others receiving payouts. It has been shown that such experience makes them more likely to buy insurance, as they have a better understanding of the insurance product and its potential benefits (Cai et al., 2020; Cole et al., 2014;Karlanetal.,2014;Santeramo,2018). The findings of this study suggest that premium subsidies aimed at eliminating charity hazard work especially well when severe losses have recently been experienced and disaster relief payments have been received. Temporary premium subsidies or other monetary incentives may be used as an instrument to boost initial insurance demand and lower charity hazard after severe loss events have occurred. Once individuals' evaluation of insurance contracts has increased, monetary support may then be terminated.

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