Do Natural Disasters Affect Household Saving? Evidence From the August 2002 Flood in Germany

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Abstract

Recently, there is a growing interest in understanding the effects of life experiences on financial decision making. An underexplored question is whether and how natural disasters affect household saving behavior. For this purpose, we exploit a natural experiment stemming from the European Flood of August 2002. Combining micro data with geo-coded flood maps allows us to analyze the causal impact of flood exposure on household savings within a difference-in-differences setting. We find that flood exposure depresses household saving behavior in the medium run. The most likely explanation is moral hazard induced by massive government support for affected households.

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1. Introduction

Over the last decade, a growing literature on the effects of prior life experiences on risk preferences and financial decision making evolved. This literature considered the effects of different types of experiences such as large economic crises (Malmendier and Nagel, 2011), violent conflicts (Voors et al., 2012), crime (Callen et al., 2014), the loss of a child (Bucciol and Zarri, 2015) or poor child health (Luik, 2017). Against the background of the ongoing process of global warming, which goes along with an increase of the frequency and severity of climate related natural disasters,[1] recently a number of studies has analyzed the effects of natural disasters on risk preferences and financial behavior. However, this strand of the literature is still in its infancy.

Only a few papers have yet studied the effects of natural disasters on professional financial decision making. Bernile et al. (2017) show that corporate risk-taking is related to early-life exposure to natural disasters of their CEOs. Moreover, there is evidence that managers of firms, located in the neighborhood of occurring hurricanes, temporarily increase corporate cash holdings although the objective hurricane risk did not change at all (Dessaint and Matray, 2017).

The literature on the effects of natural disasters on private financial decision making is only slightly larger. A number of studies has investigated whether natural disasters affect risk aversion and time preferences. These studies are typically based on experiments conducted with individuals from disaster-prone regions. Their results are mixed. As an example, Cassar et al. (2017) find that individuals, who were affected by the 2004 Tsunami in Thailand, were significantly more risk averse than comparable but unaffected individuals. A similar result is reported in Cameron and Shah (2015) for Indonesians exposed to earthquakes. However, Eckel et al. (2009) find evacuees of Hurricane Katrina from New Orleans to be more risk-seeking than unaffected individuals. A similar result is reported by Page et al. (2014) for homeowners living close to the 2011 Brisbane flood.

To the best of our knowledge, only two studies have up to now directly focused on private financial decisions. Interestingly enough, both papers study how the 2008 earthquake in the Chinese Sichuan province affected saving behavior. Somewhat surprisingly, both studies come to differing conclusions. The study by Filipski et al. (2019) concludes that individuals who lived close to the earthquake region but did not suffer damages themselves, reduced their savings and thus "lived as there's no tomorrow". Yao et al. (2019) find a decrease in saving only for relatively poor and less affected individuals while richer and more affected individuals increased post-disaster saving.

Our paper contributes to the up to now very scarce literature on the effects of natural disasters on saving. Our study differs from the ones by Filipski et al. (2019) and Yao et al. (2019) in two important dimensions: we study the impact of natural disasters in a highly developed country and we focus on a different disaster event. More precisely, we study the effects of the August 2002 flood

[1] This holds true for temperature extremes (Rummukainen, 2012; Zhang et al., 2011) and heat waves (Schär et al., 2004; Jones et al., 2008; Kyseli, 2009), droughts (Dai, 2011), precipitation extremes (Lehmann et al., 2015), floods (Hirabayashi et al., 2013) and even storms (Grinsted et al., 2012; Thomas, 2014).

in Germany, which occurred as a consequence of heavy precipitation in parts of Eastern Europe and caused serious destruction especially in Saxony (Rudolf and Rapp, 2003). We base our analysis on geo-coded panel-survey data from the German Socio-Economic Panel (SOEP) which we combine with flood maps of Saxony. We then study within a differences-in-differences (DiD) approach whether and how affected households adjusted their saving behavior in the aftermath of the flood in comparison to a control group consisting of similar, but unaffected households. While we find no significant effect on saving in the short-run (e.g. the year following the flood event), households significantly reduce their savings in subsequent years. We show that this (at first sight) surprising result most likely stems from a Samaritan's Dilemma, e.g. moral hazard behavior due to excessive governmental and private in-kind-support of affected households.

The paper is structured as follows. Section 2 summarizes the related literature on saving behavior in the aftermath of natural disasters. In Section 3, we briefly describe the flood event of August 2002 in Saxony, which is later used to define the treatment in the employed DiD approach. Section 4 introduces the employed data and delivers summary statistics. The estimation approach as well as the main empirical results are then presented in Section 5. In Section 6 we investigate the likely driving forces behind our results. Section 7 summarizes and concludes.

2. Related Literature

As we argued in the introduction, the literature on the effects of natural disasters on saving decisions is comparatively small and consists of only a few papers which directly touch upon this issue. To draw a reasonably complete picture of the existing knowledge, we also include papers from related strands of the literature in our subsequent literature review.

Whenever the likelihood of the occurrence and the consequences of natural disasters are common knowledge, the effect of factually occurring disasters on saving behavior are minimal. In this case, the disaster event reveals no new information on disaster risk and there is no reason to readjust the formerly chosen, optimal saving strategy.

However, the situation is different whenever individuals are Bayesian learners. In this more realistic case, the occurrence of natural disasters will be used to update expected probabilities of the occurrence or the severity of natural disasters (Cameron and Shah, 2015; Brown et al., 2018). As an example, Botzen et al. (2009) find that the perceived probability of floodings in the future is higher among individuals who have previously been evacuated due to flood events. Under Bayesian learning the occurrence of natural disasters might influence saving behavior in various ways, depending on the nature of the (perceived) risk and the primary motive for saving.[2]

[2]For a review of the motives of saving, see Browning and Lusardi (1996). As Horioka and Watanabe (1997) have shown empirically, households typically have various reasons for saving, most prominent among them life-cycle consumption smoothing, precautionary and bequest motives.

First, disasters might lead to a significant risk to life. Disasters with a significant death toll might be taken as an indication that total life risk has increased. Whenever individuals engage in saving as a means of consumption smoothing over the life cycle (see e.g. Modigliani and Brumberg, 1954), a decrease in life expectation should lead to less saving.[3] Filipski et al. (2019) deliver some supporting empirical evidence for this line of argument. In their microeconometric study of saving behavior before and after the 2008 Sichuan earthquake, the authors find a significant reduction of savings and thus "living as there's no tomorrow". Moreover, a number of experimental studies have engaged in testing this prediction within life-cycle saving experiments. Anderhub et al. (2000) introduce termination risk by sequentially revealing new information on the end of the repeated experiment. While the experimental participants typically deviated from the optimal saving decision (which is a common finding in the related experimental literature), they tended to react predictably to information on the termination horizon. In a slightly different experimental setting, Noussair and Matheny (2000) find that applying a random rather than a fixed ending rule in the experiment led to slightly more suboptimal cases of "binging", i.e. the suboptimal decision to reduce saving to zero in a certain period. Berlemann and Michailova (2019) study the effect of various levels of life risk in a field-experiment conducted over the internet and find that experimental participants reduced their savings only when life risk became excessive.

Second, whenever disasters destroy private capital goods, the occurrence of these disasters might affect individual risk preferences. While the assumption of exogenous preferences has a long tradition in economics (Stigler and Becker, 1977), more recently recently a growing body of literature allowing for endogenous preferences has evolved (Bowles, 1998). From a theoretical perspective, prospect theory (Kahneman and Tversky, 1979) delivers a reasoning for endogenous risk preferences. Prospect theory argues that individuals think in terms of expected utility relative to a reference point (e.g. current disaster risk) rather than absolute outcomes. As a consequence, individuals tend to be risk-seeking when considering losses related to a reference point (e.g. the status quo) while being risk averse when considering associated gains (Page et al., 2014). Various papers have studied the effects of natural disasters on risk preferences and perceptions. Mostly, these studies use field experiments in regions which have recently experienced a natural disaster. Eckel et al. (2009) conduct surveys and experiments with Hurricane Katrina evacuees. They find that a sample of individuals, which were recently evacuated from New Orleans, exhibits significantly more risk-seeking behavior than a sample of evacuees, which took part in the same experiments one year later, as well as a sample consisting of residents of Houston (i.e. the place of evacuation). The authors conclude that the risk preferences of survivors of natural disasters might be heavily disturbed by emotional stress in the short-run. Similarly, Page et al. (2014) conduct lottery experiments with homeowners affected by or living close to the 2011 Brisbane floods. They find that the flooded homeowners on average showed a significantly

[3] For a theoretical derivation of this result see e.g. Berlemann and Michailova (2019).

higher willingness to take risks. Cameron and Shah (2015) conduct surveys and experiments in rural East Java and study whether individuals living in an area hit throughout the previous three years by earthquakes or floods differ systematically from those living in unaffected areas. In contrast to the earlier reported studies, the authors find a significantly higher degree of risk-aversion among the affected households. Based on survey data collected soon after Cyclone Evan on Fiji, Brown et al. (2018) find an increase in individual risk perceptions among Indo-Fijians, while Indigenous Fijians remain unaffected. Yao et al. (2019) argue that disasters might affect risk preferences via cognitive self-control, which itself is positively correlated with saving. Whereas severely affected and poor households tend to report lower levels of self-control after the 2008 Sechuan earthquake, the authors find the opposite results for rich and less severely affected individuals.

Third, occurring disasters might affect current income as well as expectations on future income. Directly after a disaster, business activities of affected firms might be disrupted, thereby decreasing firm owners' income. The same could hold true for workers as their firms might be forced to dismiss them or reduce working time in consequence of disasters. As an example, Vigdor (2007) reports that individuals which were evacuated from New Orleans due to Hurricane Katrina on average lost three weeks of work. With a loss of 10 weeks, the effect was even more pronounced among evacuees who could not immediately return to their pre-Katrina addresses. In the case of Katrina, unemployment and income shocks could be traced well into the year 2006. Based on different data sources for the same disaster, Deryugina et al. (2018) report significant, although temporary income losses of the affected population. Whenever less income is available, saving will be reduced. Moreover, when it is expected that disasters occur more often or become more severe, expectations on long-run income will also be reduced. Again, less saving will be the consequence. In line with this argument, the macroeconometric study by Berlemann and Wenzel (2016) finds aggregate saving rates to decrease in the aftermath of drought events.

Fourth, the occurrence of disasters might induce individuals to believe that income uncertainty in general has increased. Theoretical work has shown that rising income uncertainty should lead to more saving and thus more capital accumulation (Leland, 1968). Skidmore (2001) shows, based on a life cycle expected utility model, that saving should increase as a result of rising expected future losses from natural disasters. This result does not hold when perfect insurance is available. However, Skidmore (2001) argues that even in highly developed countries disaster insurance is often unavailable due to the low likelihood of disaster occurrence and the immense damages to be covered in the case of disasters. Skidmore (2001) also presents some tentative empirical evidence from 15 highly developed countries, indicating that in fact countries which are more prone to natural disasters also exhibit higher saving rates.

Fifth, in the aftermath of disasters, saving might be reduced by enforced consumption. When the property of individuals is damaged or completely destroyed and they have only limited financial wealth, individuals might need to spend larger parts of their income on replacing durable consumption goods such as furniture or electronic devices or to finance maintenance costs of their properties. While the additional expenses could be partially financed via the reduction of unnecessary consumption, this might not suffice to save the same amount of money as before.

3. The August 2002 Flood in Saxony

During the first half of August 2002 Central Europe experienced record breaking rainfall amounts and intensities. These heavy precipitation events resulted in flash floods of small rivers in the Ore Mountains, the Bohemian Forest and in Lower Austria. From August 7-11, the first floods of larger rivers, fed from these areas, occurred and further intensified in the subsequent days.[4] In many of the affected regions, the water masses caused massive direct damage.

In Germany, the state of Saxony was hit most severely by the August 2002 flood. The majority of damages in Saxony were caused by the Elbe and the Mulde River; however severe damages were also caused by small tributaries such as the Weißeritz stream. Consequently, the flood affected many distinct parts of Saxony. In order to visualize the areas which were flooded in Saxony throughout the August 2002 flood, *Sächsisches Amt für Umwelt, Landwirtschaft und Geologie* and *Amt für Umweltschutz, Landeshauptstadt Dresden* constructed a consolidated flood map using information from various local organizations. The flood map was constructed using Geographic Information Systems (GIS), based on aerial photography, recorded flood-marks and hydraulic recalculations. The flood map contains information on the flooded areas in the form of polygons. As Figure 1 shows, the flood map can be combined with other geographic maps.[5]

In the upper part of Figure 1, we show a map of all watercourses in Saxony based on information available upon 2008.[6] The lower part of the figure displays a combination of a map of Saxony and the earlier described flood map. Here, we only show the flooded watercourses, among them the two most affected: the Elbe and Mulde River. The map also visualizes that, while large parts of Saxony were affected, no floods occurred in Eastern Saxony.

Figure 2 focuses on the flooded areas in and around Saxony's capital of Dresden. As the map indicates, Dresden suffered from the very high gauge stages of the Elbe River. However, the city center was flooded by a comparatively small subsidiary, the Weißeritz stream, which approaches Dresden from south-west. As the map uses a larger scale, the flood polygons are more easily visible. Note that the flood map can be used to check whether certain places, defined by their geographical coordinates (latitude, longitude), were flooded in August 2002. As we will explain in Section 4 in more detail, we take advantage of this option in our subsequent empirical analysis.

[4] A more detailed description of the flood event can be found in Appendix A.

[5] Originally, the employed flood map used the ETRS89 UTM 33N projection. In order to combine it with other geographic maps of Saxony we reprojected it to the EPSG:3857 Web Mercator projection.

[6] Data of watercourses are also provided by the *Sächsisches Amt für Umwelt, Landwirtschaft und Geologie* and *Amt für Umweltschutz, Landeshauptstadt Dresden.*

Altogether, the so-called "millennium flood" caused direct damages of € 9.068 billion in Germany (Bundesministerium der Justiz 2002). Slightly more than half of the damages (€ 4.892 billion, 54%) were to federal, state or municipal infrastructure. Direct damages of private companies and agriculture amounted to € 1.629 billion and thus 18% of total damages. The remaining direct damages of € 2.547 billion (28%) belonged to private households. The German state of Saxony was hit hardest by far. The damages which occurred within in the Saxonian borders amounted to € 6.084 billion (Bundesministerium der Justiz 2002) and thus two thirds of the total damage.

4. Data

The micro data used in this study comes from the German Socio-Economic Panel (SOEP).[7] The SOEP is a representative annual household survey which started in 1984 in West Germany and has included the regions that formerly constituted the German Democratic Republic since German Reunification in 1990. All household members above the age of 17 are personally interviewed. Moreover, each household head answers an additional questionnaire which is directed towards the household as a whole rather than individuals in the household. The core survey contains roughly 150 questions that allow researchers to extract information on the socio-economic infrastructure of the included households. Among others, the survey includes data on income, employment and the health status of household members.

As Saxony was the German state most heavily affected by the August 2002 flood, we concentrate our analysis on SOEP respondents living in Saxony when the August 2002 flood occurred. In 2002, the SOEP contained 30,033 people living in 12,692 households, of which 864 were in Saxony. In our empirical analysis, we are interested in comparing saving behavior before and after the flood occurred. We therefore exclude all respondents who were interviewed in 2002 after the flood. As the SOEP questionnaire is primarily carried out in the first half of the year, very few observations were excluded for this reason. The 797 households interviewed in 2002 before August compose our pre-disaster sample.

Our saving measure is based on answers to the following questions posed to each household head: "Do you usually have an amount of money left over at the end of the month that you can save for larger purchases, emergency expenses or to acquire wealth? If yes, how much?" This measure of savings has been previously used among others by Fuchs-Schündeln et al. (2019). As we are interested in real rather than nominal savings, we deflate the reported savings by the German consumer price index.[8] The corresponding variable S_{it} measures household savings of household i and time t and can be interpreted as a censored variable since negative saving is not possible given the question asked.

[7] The Socio-Economic Panel (SOEP), data for years 1984-2017, version 34, SOEP, 2017, DOI:10.5684/soep.v34. For a detailed description of the SOEP data, see Wagner et al. (2007).

[8] We make use of the consumer price index (code 61111-0001) published by the German Statistical Office. Savings are expressed in € values as of year 2000.

To study saving behavior for the propensity to save, we construct a dummy variable, S_{it}^{E}, taking the value of 1 whenever a household saves, zero otherwise. As an additional measure which reclassifies small saving amounts, we create $S_{it}^{E'}$ coded as 1 if a household saves more than 50 € a month, zero otherwise.

In addition to the described saving variables, we use various socio-economic control variables which have shown to be decisive in explaining saving behavior, such as age, gender, skills, marital status or the presence of children.[9] The exact definition of the control variables is provided in Appendix B.

A crucial issue in our empirical analysis is the identification of Saxonian SOEP households which were affected by the flood. Not surprisingly, the SOEP data-set does not contain a variable or question that pertains to this issue. To identify households within or nearby the flood area, we made use of anonymized regional information on the residences of SOEP respondents. In particular, we used the exact geocoordinates of each household. Since this data is highly sensitive, it is subject to data protection regulations and is only available at the SOEP Research Data Center in Berlin. We matched this data with the earlier described geo-coded flood map of Saxony. We then calculated the nearest distance to the flooded area for each household. This procedure is illustrated in Figure 3, which displays the flooded area (marked in red) of the city center of Dresden. The red and the blue markers display two exemplary household observations and their distance to the flooded area. The household with the red marker lives approximately 700 meters away (indicated by the black line), whereas the household with the blue marker lives within the flooded area and thus has a distance of 0 meters. Based on this information, we constructed various measures of flood exposure which will be explained in detail in the following section.

5. Empirical Analysis of Household Saving Behavior

The aim of our empirical analysis is to study whether and how Saxonian households exposed to the August 2002 flood adjusted their saving behavior in the aftermath of the flood event. A simple comparison of post-disaster saving in the group of flood-affected (treatment group) and non-affected individuals (control group) is insufficient for this purpose, as the choice of place of living can hardly be assumed to be random. Thus, we cannot rule out that flood-affected individuals (who often live close to watercourses) differed systematically in their saving behavior from those living in less flood-prone areas well before the flood. In order to overcome this problem, we apply a differences-in-differences (DiD) approach in our subsequent empirical analysis (see e.g. Angrist and Pischke 2009). The DiD approach allows us to control for potential pre-level differences between treated and non-treated households, provided that the treatment and the control group exhibit parallel trends in absence of the treatment. In the following, we first define our treatment and control group and deliver descriptive

[9] Some variables refer to the household head, while others are measured at the household level. For an extensive overview of the micro-level evidence on saving behavior, see Browning and Lusardi (1996).

statistics on their properties. We then turn to our main empirical analysis of saving volumes before and after the flood event. To shed more light on saving decisions, we then complement our analysis by studying the extensive dimension of the saving decision.

5.1. Definition of Treatment and Control Group

As it is our aim to study how the flood event of August 2002 affected saving behavior of flood-affected households, it is of paramount importance to assign the Saxonian SOEP households adequately to the treatment and the control group. The treatment group should consist of all flood-affected households, whereas the control group should comprise (at best comparable) households which were not affected by the flood event. Due to the unavailability of direct information on households' flood-affectedness, we base the decision to assign households to the treatment or the control group (or neither of the two groups) on the distance of a household's place of living to the nearest flooded area.

Naturally, households living within one of the flooded areas (defined by the earlier described flood map) should be assigned to the treatment group. These households were most likely directly affected by the flood event, e.g. by being evacuated for at least some time and/or having suffered an extent of the property damages mentioned in Section 3. However, it seems reasonable to assume that even those households living in close proximity to the flooded areas were affected by the flood event. As there was considerable uncertainty about the maximum gauge levels throughout the flood, many households living close to the flooded areas were also considerably at risk of being flooded themselves. Moreover, it should be taken into account that many households live nearby to their relatives and friends or have close relations with their neighbors. Households living close to the flooded areas are often engaged in neighborhood assistance throughout and directly after the flood. This local interaction of households most likely increased the flow of information about the magnitude of destruction and financial compensations.[10]

Furthermore, the psychological literature suggests that individuals often employ an "availability heuristic" when considering risks (Kahneman and Tversky 1979). Thus, the likelihood that individuals will use the occurrence of a natural disaster to update their own assessments of risks in a Bayesian sense increases in terms of the ease with which they can bring an instance to mind. Naturally, disasters individuals observe directly and in which relatives, friends and neighbors are involved will have the highest degrees of salience and most likely influence their own behavior (see Botzen et al. 2009).

In light of these arguments, it seems reasonable to assign households which lived very close to the flooded areas to the treatment group. Our main treatment, which we refer to as "broad treatment" in the following, therefore includes all households which lived within a distance of 75 meters from the flooded area in August 2002.[11] As a more conservative variant, we additionally define a "narrow

[10] Government compensation policies will be discussed in detail in Section 6.

[11] The definition of a reasonable threshold level is not trivial. On the one hand, an increase of the considered threshold

treatment" that includes only those households in the treatment group which lived within the flooded area in August 2002.

The control group must consist of households which have been neither directly or indirectly affected by the flood. These households should not have suffered directly from the flood catastrophe nor should they belong to the group of direct witnesses or be relatives or close friends of the flood victims. The likelihood that these conditions are fulfilled increases with the distance of a household to the next flooded area. We therefore drop all households from the control group which live closer than 500 meters to a flooded area. As the comparability of households tends to decrease over distance, we also drop all households from the control group which live more than 3,000 meters away from a flooded area.[12]

We should also mention that our time perspective is somewhat limited as parts of Saxony experienced another, yet less severe flood, in spring 2006. As this would ultimately threaten our identification strategy, we have restricted our post-disaster analysis to the time period of 2003-2005. Doing so, leaves us with a maximum treatment group of 61 households (broad treatment) and a control group of 300 observations.

The central identifying assumption of the DiD approach is that treated and non-treated households exhibit parallel pre-trends with regard to saving behavior in absence of the treatment. We test the assumption of parallel trends by conducting multiple placebo tests. None of the tests suggests that pre-trends were not homogeneous. The corresponding results and details are presented in Appendix C.[13]

Table 1 presents summary statistics for all variables in the pre-disaster year 2002 before August, conditional on being a member of the broad treatment or the control group.[14] Furthermore, both groups were assessed according to their balancing property by conducting simple t-tests for all dependent variables as well as for all covariates. With respect to our savings measure, we do not find any statistically significant differences between treatment and control group in 2002 before the flood occurred. Most control variables also do not differ systematically, with the exception of employment and non-working status. The differences with respect to employment and non-working status might be related to minor age differences, as the treated group is on average 3.9 years younger. However, our DiD approach will control for these differences by including them as covariates.

increases the treatment group and thus allows for an estimation of the effect based on a larger sample. On the other hand, a rising threshold level increases the likelihood of including unaffected households in the treatment group. We based the choice of our threshold on a grid search and increased the threshold in steps of 25 meters until a sample size of at least 50 observations for the treatment group was reached.

[12] In addition, we conduct a sensitivity analysis with two different upper thresholds of 2000 and 4000 meters. The results are shown in Tables D.13 to D.12 in Appendix D.

[13] Whenever non-linear estimation methods are applied, the identifying assumptions differ to some extent. See Appendix C for a discussion of this aspect.

[14] Summary statistics for the narrow treatment group are shown in Appendix B in Table B.8.

5.2. The Impact on the Saving Volume

In the first step of our empirical analysis, we study to the flood's effect on households' saving volume. Our empirical analysis of the flood's impact on the amount of saving, S_{it}, is based on the DiD regression outlined in Equation (1), which considers multiple time periods as discussed in Angrist and Pischke (2009). S_{it} is our saving measure of household i observed at time t, with $t \in \{2002, 2003, 2004, 2005\}$. The variable $TREAT_i$ is a (time-constant) dummy variable, indicating the treatment status of household i. Since we have observations for three post-disaster years, we include a set of time-dummies $YEAR_{j,t}$ taking the value of 1 whenever $j = t$, with $j \in \{2003, 2004, 2005\}$. For example, $YEAR_{2003,t}$ takes the value 1 for observations in 2003 and zero otherwise. The parameters of interest are given by δ_j, which correspond to the interaction terms $YEAR_{j,t} * TREAT_i$. Doing so allows us to identify the impact on the saving volume in 2003, 2004 and 2005. The vector $\bar{X}_{i,t}$ collects all control variables that may be relevant for the saving decision and $\epsilon_{i,t}$ captures the unexplained variation of our model.[15]

$$\begin{aligned} S_{i,t} &= \alpha + \lambda * TREAT_i + \sum_{j=2003}^{2005} \gamma_j * YEAR_{j,t} \\ &+ \sum_{j=2003}^{2005} \delta_j * YEAR_{j,t} * TREAT_i + \bar{X}'_{i,t}\beta + \epsilon_{i,t} \end{aligned} \tag{1}$$

As described in Section 4, the corresponding variable S_{it} has by definition no negative values. In principle, saving could be negative, but these cases are not recorded given the wording of the underlying question. To take this left-censoring into account, we follow Fuchs-Schündeln et al. (2019) and estimate a Tobit model using the basic framework outlined in Equation (1).[16] Standard errors are clustered at the level of households.

Table 2 presents the corresponding results in which we report the effects on the uncensored, unobserved latent variable. Thus, the estimated coefficients reflect the predicted change in desired saving levels. To ease interpretation, we also report marginal effects conditional on being uncensored.[17] We find that the flood depressed desired saving two and three years succeeding the disaster, irrespective of whether we define the treatment in a narrow or a broad way. With respect to the latter, we find savings to be reduced through flood exposure by € 46 in 2004, which translates into a saving reduction of 10 percent. In 2005 saving was reduced by approximately € 53 (-12 percent). When applying the narrow treatment definition, estimates suggest that the flooding reduced saving by € 91 in 2004 (-21 percent) and € 82 in 2005 (-19 percent).

[15]The vector $\bar{X}$ includes control variables for gender, age, education status, employment status, relationship status, number of children in household, homeownership and living area.

[16]In Appendix C we discuss the DiD model in the non-linear context with latent variables.

[17]Coefficients reported are marginal effects evaluated at the median of individual-level control variables.

A simple back-of-the-envelope calculation is helpful to put the size of these effects into context. When assuming that the share of total direct damages to be born by private households is the same in Germany and in Saxony, Saxonian households' direct losses add up to € 1.7 billion. According to the EM-DAT database,[18] in Germany a total number of 330,000 individuals were affected by the August 2002 flood. Assuming that two thirds of these individuals lived in Saxony (in line with the share of damages which occurred in Saxony), the number of affected individuals amounts to roughly 220,000. Thus, the average damage per affected individual in Saxony can be estimated to be € 7,700. As a household on average consists of two individuals, the average direct damage per household amounts to roughly € 15,000. Our Tobit estimates suggest that the marginal flood effect was about € 85 per month for households in the narrow treatment group. Thus, the cumulative reduction of savings adds up to the average damage per household of € 15,000 after roughly 15 years, even without taking into account any return on savings. We thus detect a reduction of savings of considerable size.

Our results survive several robustness checks, including changes in the sample and the geographical thresholds used to define our treatment and control groups. Moreover, we controlled for potential elevation differences within the broad treatment group. Some households within this group might have actually not been exposed to potential flood risks since they lived at a safe height (e.g. on a hill). To address this, we calculated the elevation difference between the nearest flood border for each household in the broad treatment group. We then excluded those households which lived above a threshold of 5.83 meters.[19] Correcting for height differences does not affect the results for the broad treatment group.[20]

Against the background that the narrow treatment consists of a comparatively low number of observations, concerns about the consistency of our estimation results for this treatment group might be raised. While we obviously cannot raise the number of households living in the flooded area, we can at least rule out that our results depend on a single observation. In order to do so, we gradually excluded each treated observation from the sample and reestimated the Tobit regression. In none of the regressions the effect for 2004 or 2005 was rendered insignificant by this procedure. Moreover, we ran 300 additional Tobit-regressions where we randomly excluded 2 observations from the narrow treatment group. Again, the results turned out to be highly stable. None of the 300 estimated coefficients for 2004 and only one of the coefficients for 2005 became insignificant.

5.3. The Impact on the Propensity to Save

Our previous empirical analysis led to the conclusion that affected individuals reduced their savings significantly after the flood event. An obvious follow-up question is whether the decision to save at all, i.e. the extensive margin of the decision to save, is also influenced by the disaster event. Our

[18]See: https://www.emdat.be
[19]We explain the choice of the threshold in Appendix D.
[20]These robustness tests are presented in Appendix D.

subsequent empirical analysis of the flood's impact on the propensity to save, S^E, is again based on the DiD regression outlined in Equation (1). In contrast to before, we now employ the binary saving measure $S^E_{i,t}$ as dependent variable. We estimate a linear probability model (LPM), which allows for a simple interpretation of the estimated parameters.[21] Standard errors are again clustered at the level of households. We also estimate Equation (1) using our alternative binary saving measure, $S^{E'}_{it}$, which takes the value coded as 1 if a household saves more than 50 €.

Table 3 provides the results for both binary saving measures given the broad and the narrow treatment definition. Applying the first definition, we find a strong and significant negative effect on saving in 2004 when using 50 € as threshold for saving. The magnitude of the impact is remarkable in size. Our estimates suggest that the flood reduced the likelihood to save in 2004 among exposed households by approximately 17 percentage points, which corresponds to a reduction of 25 percent. In the other cases, coefficients are negative, but insignificant. The pattern becomes more pronounced when we focus on those households which lived within the flooded areas; we find a strong and significant negative effect on saving in both 2004 and 2005, irrespective of which threshold we use for defining whether a household saves or not. When considering only savings above 50 €, the estimated effect is (with a reduction of approximately 27 percentage points in 2005) more pronounced than when applying our broad treatment definition, although the coefficients are statistically not different from each other.

The previously described patterns are robust to changes in the geographical thresholds, estimation approach and sample.[22] Even excluding individuals living at a safe height do not influence the results. It is also worth mentioning that we do not find any evidence for time-constant differences in savings between treatment and control groups in any of our specifications (see Table 3). This is in line with the descriptive findings presented earlier.

Overall, the estimates in Table 3 suggest that the flood had a strong impact on the propensity to save for households within the flooded area. Households living close to the flooded area seem to have been less responsive to the flood and its consequences in terms of deciding whether to save or not.

6. Why Did Affected Households Reduce their Savings?

Against the background of our major empirical finding of a significant reduction in savings of the flood-affected households, it is an intriguing question as to the likely driving force behind the observed saving patterns. As outlined in Section 2, in principle saving might be affected via various channels. In the following, we discuss which channels might explain the detected saving pattern. To support our line of argument, we deliver complementary empirical evidence.

[21]In addition, we provide estimates based on a Probit model in Table D.15 in Appendix D, which do not differ qualitatively from our results below.

[22]The corresponding results are provided and discussed in Appendix D.

6.1. *Life Expectancy Channel*

The first channel, which we discussed in the review of the related literature, is the life expectancy channel. Occurring disasters might induce households to update their expectations on life expectancies. Whenever households expect to have a lower life expectation, the necessity to smooth consumption over the life cycle decreases and thus saving should be reduced. Individuals will reduce their expectations on life expectancy only if a disaster results in a significant death toll. Although the August 2002 flood was one of the worst flood-loss-events in Europe and Germany ever (Mechler and Weichselgartner 2003), its death toll was considerably small and amounted to 21 individuals in Saxony.[23] Given that the average number of deaths caused by floods in Germany over the period of 1980 to 2002 amounts to 6, the Saxonian flood of 2002 in fact caused above-average fatalities. However, this number is still very low as compared to the average figures in less developed countries such as Venezuela (2.016), China (328), India (292), Nepal (220), or Bangladesh (211).[24] Thus, although the August 2002 flood was an exceptional disaster event in terms of physical damages, the involved life risk turned out to be very low. As Kahn (2005) reveals, the death toll caused by flood events is strongly related to the level of development and the quality of institutions. As Germany is one of the richest countries of the world, households can afford high levels of protective measures. At the same time, high-quality institutions such as strict building codes or the availability of efficient emergency networks and early warning systems also contribute to a very low risk of death due to a flood event in Germany. Against this background, we consider it as highly unlikely that the observed reduction of savings in the aftermath of the flood event is due to updates of expectations on life expectancies.

6.2. *Risk Preference Channel*

The second channel through which the August 2002 flood might have affected saving behavior is a change in general risk preferences. While according to prospect theory individuals should become more risk seeking when experiencing a disaster event and, in consequence, reduce their precautionary savings, the earlier summarized empirical and experimental evidence is ambiguous. While the newer waves of the SOEP survey also include a question pertaining to a self-evaluation of the willingness to take risk on a 10-point scale, this item is not available before 2004 and thus cannot be used to study whether risk preferences have changed due to the flood treatments. However, there are two questions which are at least closely connected to risk preferences. The first question asks whether households heads are "worried about the general economic development". Although the term "general economic development" is not explicitly defined, for most individuals it is linked to a bundle of issues such as income, employment status and wealth. The answering categories are "very concerned" (0), "somewhat concerned" (1) and "not concerned at all" (2). The second question is similar, but asks for concerns about one's own economic situation. It also has the same answering categories. We

[23] Altogether, the flood caused 27 deaths in Germany, Austria and the Czech Republic.

[24] The reported average figures were extracted from Kahn (2005), Table 1.

can expect that a significant change in risk preferences should result in a higher probability to be concerned about the general economic development and one's own economic situation. In order to study this issue we use the same DiD setting as for the saving variables. Table 4 reports the results of the corresponding OLS estimations.[25] None of the interactions turns out to be significantly different from zero. We take this as a strong indication that risk preferences remained stable before and after the flood event.

6.3. Income Channel

A third possible channel is the income channel. The observed reduction in savings might result from a decrease of actual or expected future income. Macroeconomic studies investigating the flood's short-term impact on the Saxonian economy conclude that the effect was rather moderate (see, e.g., Müller and Thieken 2005; Berlemann and Vogt 2008) and short-lived. Müller and Thieken (2005) report that affected businesses interrupted production for only two to four days after the flood. However, as we cannot rule out that the individuals in our treatment group nevertheless suffered from systematic income losses, we study this issue explicitly based on SOEP data. Again, we make use of the DiD design and study the flood effect on labor income. Labor income includes the income of self-employed persons, but excludes non-regular elements such as e.g. Christmas allowances. Table 5 reports the estimation results of the corresponding OLS estimations. We do not find a significant income effect in any of the three years.[26] We also fail to find a systematic effect of the flood treatment on the likelihood to be registered as unemployed. In Table 5, we show the results of the corresponding linear probability models.

6.4. Income Uncertainty Channel

The fourth possible channel, which has been discussed in the literature review, is a possible increase in income uncertainty. However, as discussed in Section 2, an increase in income uncertainty should lead to an increase in capital accumulation and thus precisely the opposite of what we observe in the data. Moreover, the previously presented evidence points to the notion that income remained broadly unaffected in the years after the flood event occurred. It is thus highly implausible that the income uncertainty channel is relevant in explaining our estimation results.

6.5. Enforced Consumption Channel

Fifth, saving might be reduced due to increased expenditures in the aftermath of the flood event. Households which were severely affected by the flood might have used their available income to cope

[25] As the left-hand variable is factually an ordered variable, we repeated the estimations within an Ordered Probit estimation approach. The qualitative results remain unaffected by this procedure. The results are reported in Appendix D in Table D.17.

[26] In line with this finding we find significant reductions of the household saving rate. We report the corresponding OLS and Tobit estimation results in Appendix D in Table D.18.

with the consequences of the disaster, e.g. to pay for hotel costs, replace damaged furniture or clothes or finance maintenance costs of their properties. Understandably, this effect can only occur in our narrow treatment which consists exclusively of households living in the flooded area.

Provided the enforced consumption channel would be relevant, we should observe not only a decrease in savings, but also that households report increased expenses for maintenance costs of their properties. Moreover, we should expect that the affected households should more often report exhausted financial reserves. Finally, we should expect that the affected households reduce unnecessary consumption expenses. As an example, most households will be able to reduce their expenses for food to an extent in which they have more funds available to finance urgently necessary replacement purchases.

As the SOEP data includes information on maintenance costs (€), financial reserves (binary, available biannually) and expenses for food consumption (€, available biannually), we can check these hypotheses empirically. Table 6 reports the estimation results for OLS regressions explaining maintenance costs, financial reserves and expenses for food consumption. Again, we use the earlier described DiD estimation; however, as financial reserves and expenses for food consumption are only available biannually, the pre-treatment year here is 2001 and the post-treatment years are 2003 and 2005. We do not find a significant effect of the flood event on maintenance costs in any of the post-treatment years. Interestingly enough, we find the flood event to have a positive impact on the likelihood to hold financial reserves in the post-disaster year 2003, whereas there is no effect for the year 2005. Consumption expenses remain unaffected by the flood treatment.

One might argue that the enforced consumption channel is far more important for homeowners as the damages to real estate are typically much higher than those to personal belongings. As the SOEP also contains information on homeownership, we again can test whether the subgroup of homeowners is constrained by enforced consumption. In order to allow for different effects for homeowners and renters, we use a triple DiD setting here. The results,[27] indicate no significant treatment effect of homeownership on the likelihood to hold financial reserves in both post-treatment years 2003 and 2005. We find slightly reduced food consumption among the treated homeowners in the year 2003; however, in this year we did not detect any negative effect on saving. In 2005, when saving was significantly reduced, food consumption remained unaffected.

Altogether, the reported empirical results do not deliver any support for the empirical relevance of the enforced consumption channel. Moreover, two additional objections can be raised against the enforced saving reduction argument. First, quickly after the flood occurred, both the Saxonian and the Federal Government raised several emergency relief funds which were paid out only a few weeks after the flood event. In Saxony, nearly all requested emergency relief funds were paid out to affected

[27]No triple DiD could be estimated for maintenance costs. The estimation results are documented in Appendix D in Table D.19.

households by the end of January 2003.[28] Second, the time pattern we find in our estimation results does not support the enforced consumption argument. A large share of disaster-related expenses naturally occurred directly after the disaster. If in fact disaster-related expenses would have caused decreased savings, the savings effect should occur quickly after the flood event. However, our empirical results indicate that saving decreased not significantly before 2004.

6.6. Moral Hazard Channel

None of the considered channels can plausibly explain the significant reduction of savings of the flood-affected households after the flood event of August 2002 in Saxony. In the following, we argue that the most likely explanation of the observed saving pattern is moral hazard behavior due to the highly generous compensation policy applied by the German government in the aftermath of the flood event. The SOEP data does not include any micro information on compensations, households received after the flood event. However, detailed information on the aggregate compensation flows is available.

As discussed in Section 3, the total direct damages caused by the August 2002 flood in Germany amounted to € 9.1 billion. Parts of the damages were insured. In Germany, property insurance does not cover damages caused by floods. In order to insure against flood damages, it is necessary to procure a natural hazard insurance (Elementarschadenversicherung). Natural hazard insurance coverage varies greatly in between German federal states, partly due to historic reasons. While the average natural hazard insurance penetration rate was 10 percent in Western Germany, penetration in East Germany (and especially in Saxony) was significantly higher and ranged from 30-40 percent for private households.[29] Thus the average net damage (i.e. the uninsured part of direct losses) per household amounted to roughly € 10,000. On average, 25 percent of all affected firms were insured against natural hazards (Mechler and Weichselgartner 2003). Altogether, insurance indemnity payments to German insurants amounted to € 1.8 billion (Munich Re 2003) and thus covered roughly 20 percent of the direct flood damages.

The August 2002 flood event occurred in the final phase of the election campaign for the German general elections, which were held on September 22. Before the flood, the governing coalition of Social Democrats and the Green Party did not perform well in the polls. According to the Politbarometer Flash Poll 08/2002, which is based on interviews conducted in between August 5 and 8, the coalition government was supported by only 44 percent of the voters, whereas the competing coalition of conservatives and liberals was favored by a majority of 51 percent of voters. Thus, prior to the flood event, the reelection prospects for Chancellor Gerhard Schröder were comparatively bad (Bechtel and Hainmüller 2011). The unforeseen occurrence of the flood event in the final campaigning phase provided an unexpected opportunity for the incumbent to regain public support. It is well known

[28] This might explain the reported increased likelihood to hold financial reserves in 2003. More details on post-disaster government aid programs are delivered in Section 6.5.

[29] This is primarily due to the fact that in GDR times, property insurance included natural hazard insurance. After German Reunification, East German property insurance contracts were taken over by Allianz Insurance.

that "citizens expect governments to act vigorously when these contingencies occur, and to restore the status quo ante as much as possible" (Bytzek 2008). Governments' performance in managing crises in the aftermath of civil emergencies are therefore one of the most important criterions for voters' evaluation of politicians (Iyengar and Kinder 1987), as the media typically report intensively on disasters and post-crisis disaster management. As Brandström et al. (2008) argue, the speed of government response to a disaster event is also of crucial importance. Voters tend to appreciate early commitments to generous and unbureaucratic aid packages (Bytzek 2008).

Well aware of the opportunity and the necessity, Chancellor Schröder quickly took initiative. In Germany, operational crisis management in the case of disasters is assigned by law to the state where the crisis occurs (Dombrowsky and Ohlendieck 1998). However, the federal government has the option to offer military assistance and provide financial aid to disaster victims. On August 13, and thus 4 days before the Elbe river reached its peak level, Schröder declared that nobody should be worse off than before the flood (Mechler and Weichselgartner 2003) and announced a first emergency aid program of € 385 million. Large parts of this program were paid out very quickly (Rudolph and Kuhn 2017). The first payments were made only two days after the aid program was announced (Hogwood 2004). Moreover, the federal government started "the largest military disaster relief operation ever carried out by German forces since World War II" (Bechtel and Hainmüller 2011). On August 20, more than 45,000 soldiers were already serving in the affected regions to stabilize dams, evacuate people in danger and provide support to the affected households. On August 26, the government initiated the "Flutopfersolidaritätsgesetz" (Bill on Solidarity with the Flood Victims) which constituted the "Sonderfonds Aufbauhilfe" fund with a volume of € 7.1 billion, "the largest amount ever spent in the context of a natural disaster in German history" (Bechtel and Hainmüller 2011).[30]

Additional emergency financing was granted by the European Union. Soon after the flood event, the European Union created an EU solidarity fund of € 1 billion for emergencies. A total of € 444 million was transferred to Germany in order to support the reconstruction of the affected areas (Bundesministerium der Justiz 2002). Moreover, many people across Germany donated generously to help those affected. As an example, a fund raising gala by the public TV channel ARD in cooperation with the newspaper "Bild" on August 16 in Burg raised € 16 million, the largest donation amount ever collected in a German television gala. Altogether, donations of € 242.6 million were collected for the flood victims (Mechler and Weichselgartner 2003).

In total, the funds available for compensating affected households and firms and financing the reconstruction of damaged infrastructure amounted to € 9.6 billion and thus exceeded the total direct damages of € 9.1 billion. Against the background of the close general elections, due to the fact that East German regions were still underdeveloped at that time and the flood of August 2002 was considered as an extraordinarily severe disaster event, the German government opted for a highly

[30] The fund was financed by a one-year shift of a planned income tax reform and an increase of the corporate income tax of 1.5 percentage points for one year.

generous compensation policy. In addition to immediate payments of € 500 per affected person and € 5000 per damaged building, 80 percent of reconstruction costs were refunded by the government. Since more than enough funds were available, the calculation and refunding of reconstruction costs was based on replacement costs rather than current value lost. As the affected households also received insurance indemnity payments and profited from the mentioned private donations, almost all direct damages were ultimately compensated. Given that average compensation rates after comparable natural disaster events in other highly developed countries amounted to only 45 percent (Linnerooth-Bayer et al. 2001), the compensation policy applied in the aftermath of the August 2002 flood in Germany must be qualified as exceptional (Mechler and Weichselgartner 2003).[31]

Against this background, we argue that the most likely explanation behind our finding of reduced savings after the August 2002 flood event is a decrease in precautionary savings due to moral hazard behavior.[32] Precautionary savings are intended as insurance against unexpected expenditures (Lusardi 1998). Whenever individuals have to bear the financial costs of property damages resulting from natural disasters, they will, depending on their degree of risk aversion, save parts of their income or buy insurance, when available. We argued earlier that at least Bayesian updaters will expect disasters to occur with a higher probability in the future after a disaster has occurred. Thus, we should observe an increase in precautionary savings after a disaster. However, the reduction of precautionary savings might stem from decreased perceived disaster losses. At first glance, there is again little reason to believe that perceived disaster losses decrease as a result of the occurrence of a disaster. However, the extraordinarily generous compensations in the aftermath of the August 2002 flood might have induced the belief among the flood victims[33] that in general they do not have to bear the costs of future disasters on their own, even when they are not insured.[34] Under these circumstances, it is reasonable to reduce precautionary savings. Interestingly enough, this holds true even when at the same time the expected likelihood of the occurrence of disasters increases. This moral hazard phenomenon is also known as "Samaritan's Dilemma" (Buchanan 1975; Coate 1995) or "Charity Hazard" (Raschky

[31]Note that various studies attribute the narrow victory of Gerhard Schröder in the 2002 general elections to the highly generous and efficient compensation policy applied by the government in the weeks before the election. See e.g. Jung (2003).

[32]In a similar vein, Raschky and Weck-Hannemann (2007) argue that individuals anticipate governmental and private aid in the case of natural disasters and therefore often refrain from purchasing private disaster insurance. For a more detailed discussion, see Antwi-Boasiako (2014).

[33]A similar effect is likely to occur within the group of households which lives close to the flooded areas and are therefore included in our broad treatment group. As we argued earlier, these households are likely in close contact with households which were flooded and profited from the generous compensations. Households living farther away from the flooded areas (i.e. the households in our control group) can be assumed to be less well informed on the exact compensation rules. Moreover, they had less incentives to save for precautionary reasons as they are in general less prone to flood events.

[34]Not surprisingly, the SOEP data contains no information on the exact compensations, the affected households received after the flood event. We thus cannot formally prove that the households in our narrow treatment were in fact almost completely compensated. However, there is little reason to believe that our treated individuals were not compensated.

and Weck-Hannemann 2007; Dobes et al. 2014) in the theoretical literature. Interestingly enough, the time pattern of saving reductions we find in our empirical analysis fits quite well with the moral hazard interpretation. Recall that we find no significant reduction of savings in the year 2003 when compensation procedures were still processed; we find significant saving reductions for the years 2004 and 2005 when almost all compensations were paid out.

7. Conclusions

In recent years, the economic literature has been characterized by an increasing interest in analyzing the consequences of extreme weather events (e.g. Felbermayr and Gröschl, 2014; Gröger and Zylberberg, 2016; Deryugina et al., 2018). This development is spurred by the ongoing debate of global warming and the related increase in the frequency and severity of natural disasters such as hurricanes and floods. An underexplored question is the extent to which these extreme weather events affect household saving behavior. In this paper, we address this question by exploiting the August 2002 flood in Germany as a natural experiment in a DiD framework. Different from the small earlier literature we focus on behavioral changes in a highly developed country.

We find that the flood significantly reduced the probability to save and also depressed saving volumes of the affected households. The effect is more pronounced among those households which have been directly exposed by the flood but also holds for those who have lived close to the flood line. This pattern is robust to several changes in the empirical model and data used. We provide additional results that demonstrate that changes in life expectancy, income, income uncertainty or risk preferences were unlikely to be important driving forces behind our results. We also find no support for the hypothesis that our results stem from enforced consumption due to the occurred damages. Instead we show that the most likely explanation driving our empirical findings is moral hazard behavior enforced by the generous financial compensation of the flood victims by the German state, the European Union and private donations in the aftermath of the flood. Therefore, our results are in line with the Samaritan's Dilemma and highlight the trade-off between short-term disaster relief and moral hazard effects in financial behavior.

From a policy perspective, our paper underlines the necessity to consider incentive effects when designing disaster aid packages for those affected by natural disasters. This holds particularly in the context of highly developed countries which principally can afford generous state support and high compensation rates. As our results indicate, such policies will likely reduce incentives to accumulate financial reserves for precautionary reasons. Even worse, the ascribed policies can also decrease incentives to insure against damages from natural disasters and to take precautionary measures to keep the damages from disasters as low as possible.

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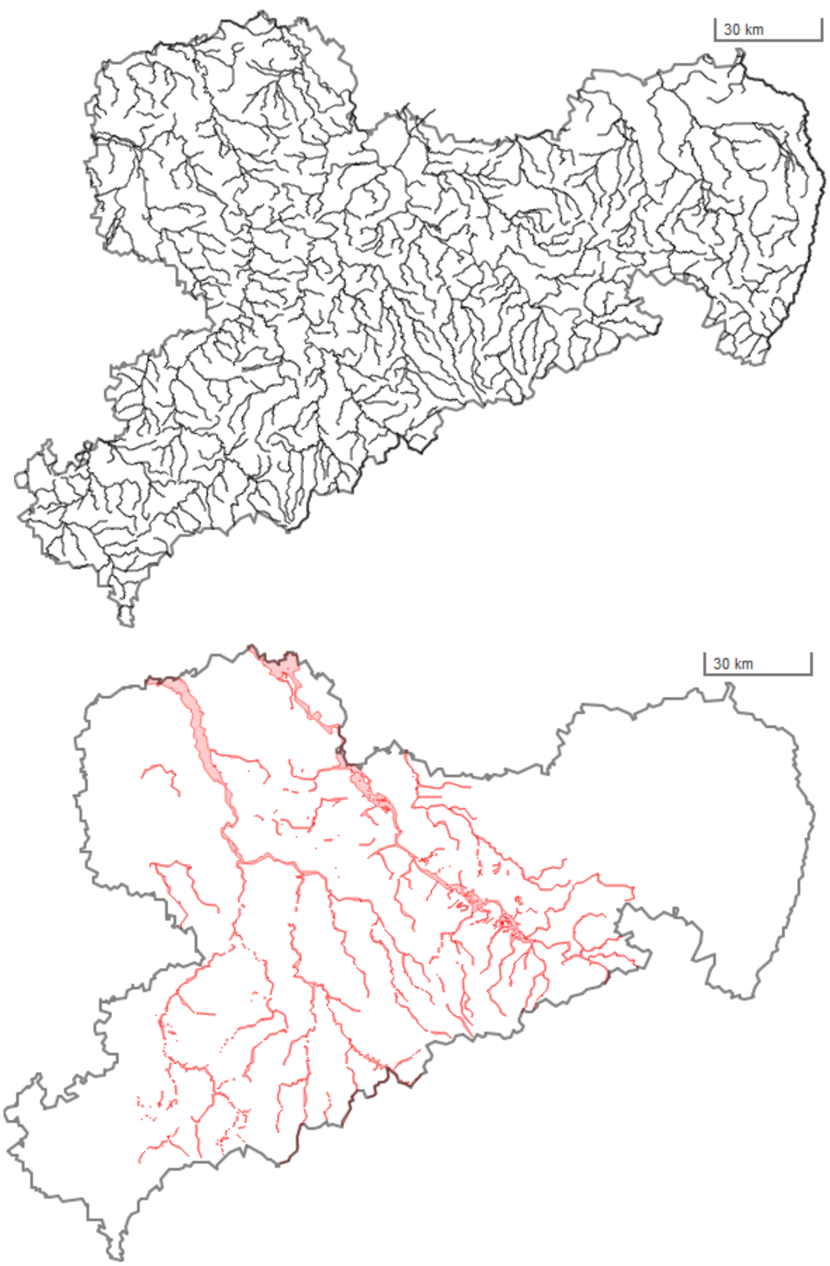
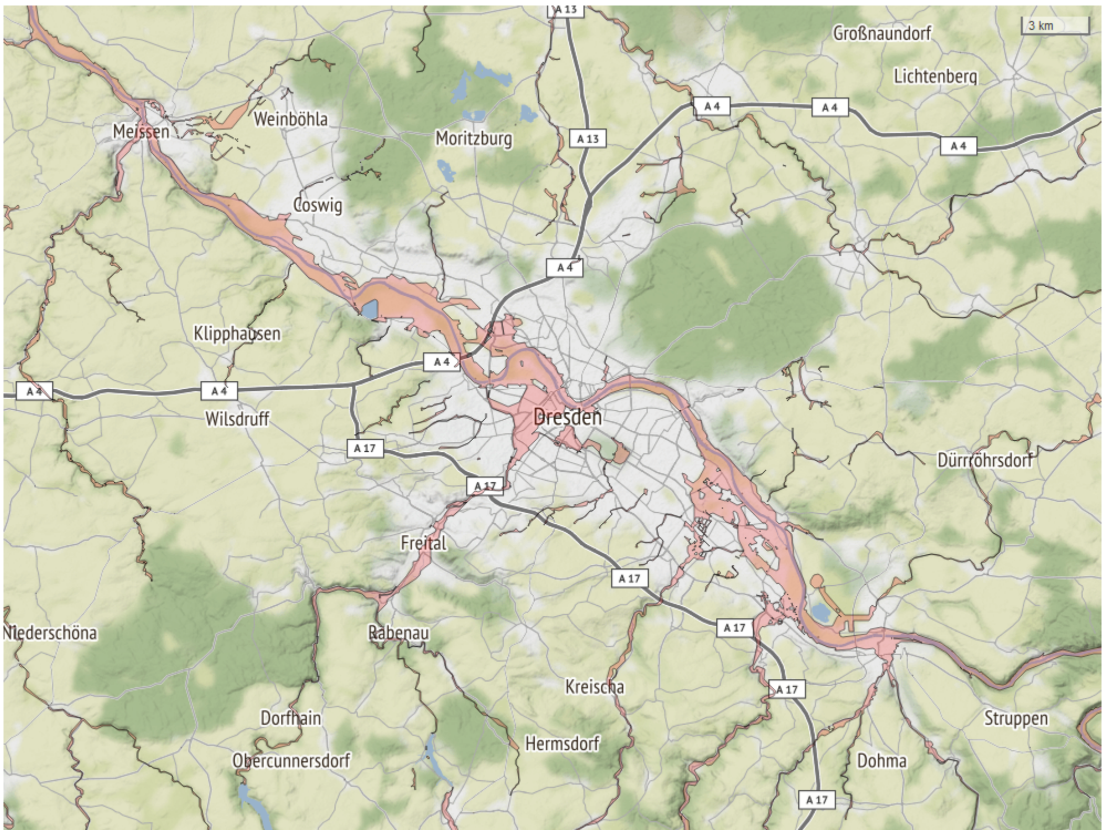
Figure 1: Watercourses and Flooded Areas in Saxony in August 2002



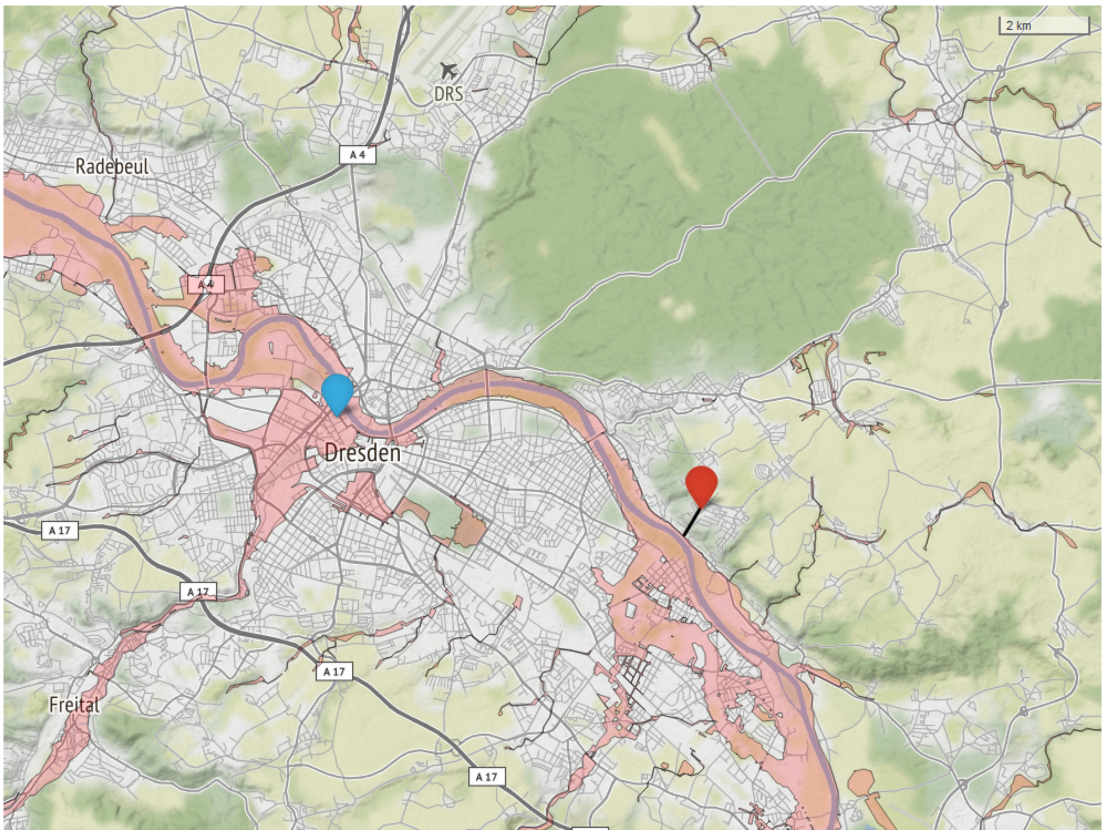
The upper part of the figure displays all Saxonian watercourses (in black, data from year 2008). The lower part of the figure shows the flooded areas (marked in red) in Saxony in August 2002. Both maps are based on data provided by *Sächsisches Amt für Umwelt, Landwirtschaft und Geologie* and *Amt für Umweltschutz, Landeshauptstadt Dresden.* The maps use the EPSG:3857 Web Mercator projection. Saxonian borders were extracted from the database of Global Administrative Areas (available under `https://gadm.org/data.html`). The maps were generated using the R-package *leaflet* (`https://cran.r-project.org/package=leaflet`) and the Java-script library *leaflet* (`https://leafletjs.com/`).

Figure 2: Flooded Areas in Dresden Region in August 2002



The figure displays the flooded areas (marked in red) in Dresden in August 2002, based on data provided by *Sächsisches Amt für Umwelt, Landwirtschaft und Geologie* and *Amt für Umweltschutz, Landeshauptstadt Dresden*. The map is based on the EPSG:3857 Web Mercator projection. The map was generated using the R-package *leaflet* (`https://cran.r-project.org/package=leaflet`) and the Java-script library *leaflet* (`https://leafletjs.com/`). Map tiles by Stamen Design (`https://stamen.com/`) under CC BY 3.0 (`https://creativecommons.org/licenses/by/3.0/`); Map Data © OpenStreetMap contributors (`https://www.openstreetmap.org/`) under ODbl (`https://www.openstreetmap.org/copyright`).

Figure 3: Flooded Areas in Dresden in August 2002



The figure displays the flooded areas (marked in red) in Dresden in August 2002, based on data provided by *Sächsisches Amt für Umwelt, Landwirtschaft und Geologie* and *Amt für Umweltschutz, Landeshauptstadt Dresden*. The map is based on the EPSG:3857 Web Mercator projection. The map was generated using the R-package *leaflet* (`https://cran.r-project.org/package=leaflet`) and the Java-script library *leaflet* (`https://leafletjs.com/`). Map tiles by Stamen Design (`https://stamen.com/`) under CC BY 3.0 (`https://creativecommons.org/licenses/by/3.0/`); Map Data © OpenStreetMap contributors (`https://www.openstreetmap.org/`) under ODbl (`https://www.openstreetmap.org/copyright`).

Table 1: Summary Statistics for Broad Treatment and Control Group (Pre-Treatment Levels of 2002)

	Broad Treatment Group					Control Group					T-Test
	N	Mean	Std.Dev.	Min	Max	N	Mean	Std. Dev.	Min	Max	Difference
Saving Volume (S)	61	272.024	417.133	0	2418.171	279	248.803	408.638	0	3869.074	-23.221
Binary Saving (S^E) (Yes = 1)	61	0.656	0.479	0	1	279	0.677	0.468	0	1	0.022
Binary Saving ($S^{E'}$) (Yes = 1)	61	0.656	0.479	0	1	279	0.627	0.484	0	1	-0.028
Controls (Household Head):											
Male	61	0.557	0.501	0	1	300	0.577	0.495	0	1	0.019
Age	61	48.049	13.414	27	82	300	51.937	15.861	21	87	3.887
Highest Education Primary	61	0.082	0.277	0	1	299	0.043	0.204	0	1	-0.038
Highest Education Secondary	61	0.623	0.489	0	1	299	0.585	0.493	0	1	-0.038
Highest Education Tertiary	61	0.295	0.460	0	1	299	0.371	0.484	0	1	0.076
Employed	61	0.656	0.479	0	1	300	0.503	0.501	0	1	-0.152*
Unemployed	61	0.115	0.321	0	1	300	0.090	0.287	0	1	-0.025
Non-Working	61	0.230	0.424	0	1	300	0.407	0.492	0	1	0.177**
Single	61	0.148	0.358	0	1	299	0.171	0.377	0	1	0.023
Married	61	0.705	0.460	0	1	299	0.582	0.494	0	1	-0.123
Other	61	0.148	0.358	0	1	299	0.247	0.432	0	1	0.100
No Children	61	0.623	0.489	0	1	300	0.717	0.451	0	1	0.094
1 Child	61	0.197	0.401	0	1	300	0.190	0.393	0	1	-0.007
2 Children	61	0.131	0.340	0	1	300	0.080	0.272	0	1	-0.051
3 or More Children	61	0.049	0.218	0	1	300	0.013	0.115	0	1	-0.036
Homeowner	61	0.410	0.496	0	1	300	0.347	0.477	0	1	-0.063
Living in Urban Area	61	0.459	0.502	0	1	300	0.410	0.493	0	1	-0.049
N	61					300					361

Note: Due to missing answers, observations can differ across variables. The statistics are based on SOEP answers in year 2002, before the flood occurred in August. A detailed description of the variables can be found in Table B.7 in Appendix B.
Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2: Effect of the Flood Event on Household Saving Volume (Tobit Model)

	Broad Treatment S (1)	Narrow Treatment S (2)
Year 2003	22.916	23.259
	(24.549)	(24.392)
Year 2004	-2.631	-2.386
	(33.546)	(33.245)
Year 2005	15.405	16.212
	(32.267)	(32.056)
Treated	4.921	-5.405
	(79.336)	(111.656)
Year 2003 × Treated	-76.830	-101.163
	(78.176)	(100.582)
Marginal Effect $[E(S\|S > 0)]$	-29.46	-37.93
	(30.22)	(38.16)
Percent Change	-6.607	-8.719
	(6.461)	(8.215)
Year 2004 × Treated	-128.327*	-276.523**
	(72.321)	(116.747)
Marginal Effect $[E(S\|S > 0)]$	-46.54*	-91.11**
	(26.37)	(39.91)
Percent Change	-10.44*	-20.95**
	(5.789)	(8.134)
Year 2005 × Treated	-145.770**	-240.784***
	(66.736)	(91.046)
Marginal Effect $[E(S\|S > 0)]$	-53.40**	-82.73***
	(24.44)	(31.55)
Percent Change	-11.98**	-19.02***
	(5.090)	(6.307)
Controls	yes	yes
Log Pseudolikelihood	-6868.148	-6105.021
Left Censored Obs.	447	389
Observations	1301	1150

Note: We report conditional marginal effects on factual saving. Marginal effects are computed for households with median characteristics. All regressions include the full set of control variables. Standard errors are clustered on the household level and reported in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Effect of the Flood Event on Households' Propensity to Save (Linear Probability Model)

	Broad Treatment		Narrow Treatment	
	S^{E}	$S^{E'}$	S^{E}	$S^{E'}$
	(1)	(2)	(3)	(4)
Year 2003	0.001	0.015	0.002	0.015
	(0.024)	(0.024	(0.024)	(0.024)
Year 2004	-0.027	-0.011	-0.026	-0.010
	(0.027)	(0.026)	(0.027)	(0.026)
Year 2005	-0.026	-0.031	-0.024	-0.031
	(0.029)	(0.030)	(0.029)	(0.030)
Treated	-0.014	0.039	0.050	0.100
	(0.062)	(0.062)	(0.104)	(0.103)
Year 2003 × Treated	0.000	-0.046	-0.026	-0.082
	(0.069)	(0.072)	(0.105)	(0.113)
Percent Change	0.0262	-6.811	-3.361	-11.03
	(9.951)	(10.24)	(13.49)	(14.46)
Year 2004 × Treated	-0.101	-0.169**	-0.209*	-0.271**
	(0.069)	(0.069)	(0.123)	(0.127)
Percent Change	-14.42	-24.87***	-27.32*	-36.45**
	(9.504)	(9.476)	(14.82)	(15.48)
Year 2005 × Treated	-0.079	-0.078	-0.178*	-0.174*
	(0.065)	(0.066)	(0.100)	(0.102)
Percent Change	-11.29	-11.42	-23.17*	-23.45*
	(9.100)	(9.484)	(12.40)	(13.06)
Controls	yes	yes	yes	yes
Adjusted R^2	0.122	0.136	0.114	0.126
Observations	1301	1301	1150	1150

Note: All regressions include the full set of control variables. Reported marginal effects refer to the change in the likelihood to save any money, caused by the occurrence of the flood event. Standard errors are clustered on the household level and reported in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Effect of the Flood Event on Economic Worries (OLS)

	Broad Treatment		Narrow Treatment	
	Overall Eco.	Own Eco. Sit.	Overall Eco.	Own Eco. Sit.
	(1)	(2)	(3)	(4)
Year 2003	0.202***	0.079	0.202***	0.080
	(0.045)	(0.052)	(0.046)	(0.052)
Year 2004	0.125***	0.155***	0.124***	0.157***
	(0.046)	(0.053)	(0.046)	(0.053)
Year 2005	0.237***	0.072	0.236***	0.072
	(0.047)	(0.054)	(0.047)	(0.054)
Treated	-0.002	-0.079	-0.081	-0.180
	(0.078)	(0.090)	(0.124)	(0.142)
Year 2003 × Treated	0.085	0.072	0.070	0.145
	(0.110)	(0.127)	(0.174)	(0.200)
Year 2004 × Treated	0.001	-0.101	0.169	0.087
	(0.111)	(0.128)	(0.176)	(0.202)
Year 2005 × Treated	0.063	0.129	0.160	0.335
	(0.112)	(0.129)	(0.179)	(0.205)
Controls	yes	yes	yes	yes
Adjusted R^2	0.053	0.093	0.057	0.108
Observations	1378	1376	1225	1223

Note: All regressions include the full set of control variables. Standard errors are clustered on the household level and reported in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Effect of the Flood Event on Labor Income and Employment Status (OLS)

	Broad Treatment		Narrow Treatment	
	Labor Income	Unemployed	Labor Income	Unemployed
	(1)	(2)	(3)	(4)
Year 2003	-123.364	0.016	-121.218	0.016
	(86.684)	(0.017)	(85.257)	(0.017)
Year 2004	-102.170	-0.001	-97.069	-0.001
	(85.336)	(0.016)	(85.656)	(0.016)
Year 2005	-182.869	0.005	-176.818	0.005
	(117.399)	(0.021)	(117.103)	(0.021)
Treated	-346.804*	0.008	-439.781	0.021
	(179.774)	(0.045)	(337.790)	(0.078)
Year 2003 × Treated	212.564	0.006	277.739	-0.015
	(170.025)	(0.024)	(304.666)	(0.020)
Year 2004 × Treated	114.157	-0.035	191.618	-0.035
	(119.261)	(0.032)	(195.322)	(0.048)
Year 2005 × Treated	273.590	-0.013	335.855	-0.131
	(188.898)	(0.046)	(339.740)	(0.081)
Controls	yes	yes	yes	yes
Adjusted R^2	0.148	0.043	0.153	0.033
Observations	662	1378	566	1225

Note: All regressions include the full set of control variables except the working status indicators. Standard errors are clustered on the household level and reported in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Effects of the Flood Event on Food Consumption, Financial Reserves and Maintenance Costs (OLS)

	Food Consumption (1)	Narrow Treatment Financial Reserves (2)	Maintenance Costs (3)
Year 2003	-1.365	-0.047**	-747.799
	(17.423)	(0.021)	(685.692)
Year 2004	-	-	-324.849
			(799.748)
Year 2005	14.386	-0.049*	-1449.417**
	(18.623)	(0.026)	(683.072)
Treated	-2.949	-0.133	5713.615
	(34.191)	(0.104)	(3605.200)
Year 2003 × Treated	6.063	0.186*	1266.551
	(34.349)	(0.103)	(3269.707)
Year 2004 × Treated	-	-	-5109.814
			(4763.675)
Year 2005 × Treated	-34.867	-0.040	-6035.113
	(51.458)	(0.120)	(3855.470)
Controls	yes	yes	yes
Adjusted R^2	0.160	0.142	0.046
Observations	422	905	406

Note: All regressions include the full set of control variables. Standard errors are clustered on the household level and reported in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix A. The August 2002 flood

In July and early August 2002, Central Europe experienced a relatively rare meteorological situation (Mudelsee et al. 2004). Normally, depressions evolving over the North Sea pass the British Islands, Scandinavia and then head towards Russia. However, as the air over Northern Europe was already comparatively cold at that time, depression "Ilse", which originated near Ireland, followed the so-called "Vb-track" and thus passed over the Western Mediterranean Sea and, then, moved northeastwards to reach Central Europe. On its way over the Western Mediterranean Sea, Ilse accumulated lots of moisture and then glided over colder and denser air from the northwest and was lifted orographically by the Bohemian Massif and the Ore Mountains. The warm and moist air masses caused prolonged and abundant precipitation in Central Europe. Several watercourses exhibited increased gauge stages and the soil was saturated with water in many parts of Saxony, Bavaria, the Czech Republic, and Austria (Löpmeier 2003). The first floods in these areas occurred between August 7 and 11, as water houses were only able to drain off above ground (GWS - German Weather Service 2007). In the early hours of August 12, Ilse crossed the Czech Republic and moved towards Saxony. The orographic conditions in Saxony caused extreme rain as Ilse completely unloaded its waters above Eastern Germany. In the Ore Mountains, which are close to the Czech boarder, official measures reported 312 liters of water per square meter within 24 hours (Rudolf and Rapp 2003). This all-time German record exceeded historical precipitation levels by a factor of four. In other central European regions, Ilse dropped between 80 and 167 liters of water per square meter in a 24-hour period. In many affected regions, the water masses caused massive direct damage.

In Saxony, the water masses caused destruction through various channels. First, small watercourses in the Ore Mountains flooded and caused destruction on their way down to the Elbe River. Much of the reported damage was caused by these tributaries that are normally rather small. Second, many of the water reservoirs located in the Ore Mountains already exhibited increased gauge stages. Traditionally the reservoirs had two functions, drinking water storage and flood prevention for the Elbe valley. In late July, many of the reservoirs had gauge stages close to maximum in order to provide ample fresh and drinking water for the summer season. When the somewhat unexpected heavy rain period started, emergency drainages became necessary in various reservoirs to prevent bursting dams. As a consequence of one of these emergency drainages, the Weißeritz stream, which is normally a small watercourse in the Ore Mountains, became a torrential river within a matter of minutes and caused massive destruction in several villages including the medium-sized city of Freital and Saxony's capital Dresden, where the water flooded the main station and substantial parts of the city center. Third, the specific orographic constellation of the region from Prague to Dresden makes the Elbe River the only significant drainage for increased water houses. Thus, the heavy rainfall at the beginning of August steadily increased the gauge level of the Elbe River. Finally, several flood waves from the Czech Republic made their way down the Elbe River and reached Eastern Germany after this heavy rain period of August 11 and 12. The already high gauge stages of the Elbe thus increased even further, thereby causing severe damage to many settlement areas close to the Elbe River.

Appendix B. Data and Variable Definitions

Table B.7: Variable Description

Dependent Variables	Description
S	Amount of household saving per month in € (in year 2000 prices).
S^E	Extensive margin of the saving decision (binary), set to 1 if $S > 0$ and zero otherwise.
$S^{E'}$	Extensive margin of the saving decision (binary), set to 1 if $S > 50$ and zero otherwise.
Control Variables	
Male	Dummy for gender of household head (female = 0, male = 1).
Age	Age of household head (in years).
Homeowner	Dummy for household heads living in their own dwelling (no = 0, yes = 1).
Living in Urban Area	Dummy for households living in urban area (no = 0, yes = 1).
Highest Education Primary	Dummy for primary education as highest education degree obtained by household head (no = 0, yes = 1). Corresponds to the reference category.
Highest Education Secondary	Dummy for secondary education as highest education degree obtained by household head (no = 0, yes = 1).
Highest Education Tertiary	Dummy for tertiary education as highest education degree obtained by household head (no = 0, yes = 1).
Single	Dummy for household head being single (no = 0, yes = 1). Corresponds to the reference category.
Married	Dummy for household head being married (no = 0, yes = 1).
Other	Dummy for household head being divorced, widowed or being married but living apart (no = 0, yes = 1).
Employed	Dummy for household head if working according to labor force status (no = 0, yes = 1). Corresponds to the reference category.

Unemployed	Dummy for household head if unemployed according to labor force status (no = 0, yes = 1).
Non − Working	Dummy for household head if non-working according to labor force status (no = 0, yes = 1).
No Child	Dummy for non children living in household (no = 0, yes = 1). Corresponds to the reference category.
1 *Child*	Dummy for one child living in household (no = 0, yes = 1).
2 *Children*	Dummy for two children living in household (no = 0, yes = 1).
3 *or More Children*	Dummy for more than two children living in household (no = 0, yes = 1).
Additional Variables	
SR	Monthly amount of household saving (S) divided by net household income.
Worries Overall Eco.	Concernedness of household head about the economy in general (0 = not concerned at all; 1 = somewhat concerned; 2 = not concerned at all).
Worries Own Eco. Sit	Concernedness of household head about own economic situation (0 = not concerned at all; 1 = somewhat concerned; 2 = not concerned at all).
Labor Income	Last month's net household work (including self-employment) income in € (in year 2000 prices).
Unemployment Status	Dummy for household head registered as unemployed (no = 0, yes = 1).
Food Consumption	Household's planned monthly grocery expenses in € (in year 2000 prices).
Financial Reserves	Dummy for household having financial reserves for emergencies available (no = 0, yes = 1).
Maintance Costs	Maintenance costs for the flat/house in € (in year 2000 prices).

Table B.8: Summary Statistics for Narrow Treatment and Control Group (Pre-Treatment Levels of 2002)

	Narrow Treatment Group					Control Group					T-Test
	N	Mean	Std.Dev.	Min	Max	N	Mean	Std.Dev	Min	Max	Difference
Monthly Saving Volume (S)	22	204.665	343.156	0	1450.903	279	248.803	408.638	0	3869.074	44.138
Household Saves Money (S^E)	22	0.682	0.477	0	1	279	0.677	0.468	0	1	-0.004
Household Saves Money ($S^{E'}$)	22	0.682	0.477	0	1	279	0.627	0.484	0	1	-0.055
Controls (Household Head):											
Male	22	0.364	0.492	0	1	300	0.577	0.495	0	1	0.213
Age	22	48.545	13.351	28	75	300	51.937	15.861	21	87	3.391
Highest Education Primary	22	0.091	0.294	0	1	299	0.043	0.204	0	1	-0.047
Highest Education Secondary	22	0.682	0.477	0	1	299	0.585	0.493	0	1	-0.097
Highest Education Tertiary	22	0.227	0.429	0	1	299	0.371	0.484	0	1	0.144
Employed	22	0.591	0.503	0	1	300	0.503	0.501	0	1	-0.088
Unemployed	22	0.136	0.351	0	1	300	0.090	0.287	0	1	-0.046
Non-Working	22	0.273	0.456	0	1	300	0.407	0.492	0	1	0.134
Single	22	0.091	0.294	0	1	299	0.171	0.377	0	1	0.080
Married	22	0.682	0.477	0	1	299	0.582	0.494	0	1	-0.100
Other	22	0.227	0.429	0	1	299	0.247	0.432	0	1	0.020
No Children	22	0.591	0.503	0	1	300	0.717	0.451	0	1	0.126
1 Child	22	0.227	0.429	0	1	300	0.190	0.393	0	1	-0.037
2 Children	22	0.091	0.294	0	1	300	0.080	0.272	0	1	-0.011
3 or More Children	22	0.091	0.294	0	1	300	0.013	0.115	0	1	-0.078**
Homeowner	22	0.545	0.510	0	1	300	0.347	0.477	0	1	-0.199
Living in Urban Area	22	0.591	0.503	0	1	300	0.410	0.493	0	1	-0.181
N	22					300					

Note: Due to missing answers, observations can differ across variables. The statistics are based on SOEP answers in year 2002, before the flood occurred in August. A detailed description of the variables can be found in Table B.7 in Appendix B.
Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix C. Identification Assumptions

A necessary precondition for the applicability of the DiD approach is that the so-called "common time trend assumption" holds. In our particular case, our identification strategy relies on the assumption that time trends in saving behavior in the treatment and control group are identical before the treatment. To check whether this assumption holds in our setting, we conducted several placebo tests.

In the first placebo test, we restrict our sample to the pre-treatment window January 2000 to July 2002 and assume that the flood happened on December 31, 2000. In the second placebo test, we use the same sample as our first placebo test and assume that the flood happened on December 31, 2001. The third placebo test is a modification of the second placebo test, in which we exclude year 2000 observations from our sample. In each placebo test, we compare saving behavior between our treatment and control group before and after the placebo flood event, using the DiD regression outlined in equation Equation (1).

The corresponding results are reported in Tables C.9 to C.11. Table C.9 shows the first, Table C.10 the second and Table C.11 the third placebo test. There is not a single case in which coefficients of our placebo treatments (i.e. the interaction effects) are significantly different from zero. [35] This is consistent with our assumption of equal trends in saving behavior before the August 2002 flood event.

[35] In all three placebo tests, we also used alternative dates to generate placebo floods (e.g. August 1). The results do not change and are available on request.

Table C.9: Placebo Estimates for Pseudo-Flood on 31th December 2000

	Broad Treatment			Narrow Treatment		
	S	S^E	$S^{E'}$	S	S^E	$S^{E'}$
	(1)	(2)	(3)	(4)	(5)	(6)
Year 2001	-12.109	-0.039	-0.032	-12.893	-0.040	-0.033
	(24.704)	(0.027)	(0.027)	(24.392)	(0.027)	(0.027)
Year 2002	-1.401	-0.041	-0.066**	-1.726	-0.042	-0.068**
	(27.089)	(0.029)	(0.029)	(26.595)	(0.029)	(0.029)
Treated	-19.790	-0.015	0.013	-77.398	-0.009	0.020
	(59.172)	(0.062)	(0.062)	(76.110)	(0.101)	(0.100)
Year 2001 × Treated	22.288	0.032	0.024	13.354	0.076	0.065
	(45.389)	(0.066)	(0.066)	(55.863)	(0.090)	(0.090)
Year 2002 × Treated	48.125	0.014	0.038	79.071	0.060	0.079
	(52.797)	(0.057)	(0.057)	(79.720)	(0.078)	(0.078)
Controls	yes	yes	yes	yes	yes	yes
Log Pseudolikelihood	-5220.784			-4627.767		
Adjusted R^2		0.128	0.139		0.119	0.139
Observations	989	989	989	876	876	876

Note: All regressions include the full set of control variables. Columns (1) and (4) are based on Tobit models. Columns (2),(3),(5) and (6) are based on linear probability models. Standard errors are clustered on the household level and reported in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.10: Placebo Estimates for Pseudo-Flood on 31th December 2001

	Broad Treatment			Narrow Treatment		
	S	S^E	$S^{E'}$	S	S^E	$S^{E'}$
	(1)	(2)	(3)	(4)	(5)	(6)
Year 2001	-8.166	-0.034	-0.028	-11.928	-0.035	-0.028
	(21.391)	(0.024)	(0.025)	(22.957)	(0.026)	(0.026)
Year 2002 (2002 = 1)	0.568	-0.038	-0.064**	-1.247	-0.039	-0.066**
	(26.350)	(0.028)	(0.028)	(26.249)	(0.029)	(0.028)
Treated	-8.631	0.001	0.025	-70.629	0.028	0.053
	(57.329)	(0.055)	(0.055)	(69.590)	(0.090)	(0.089)
year 2002 × Treated	36.954	-0.002	0.026	72.326	0.022	0.047
	(50.951)	(0.049)	(0.049)	(75.067)	(0.070)	(0.069)
Controls	yes	yes	yes	yes	yes	yes
Log Pseudolikelihood	-5220.813			-4627.772		
Adjusted R^2		0.129	0.139		0.119	0.133
Observations	989	989	989	876	876	876

Note: All regressions include the full set of control variables. Columns (1) and (4) are based on Tobit models. Columns (2),(3),(5) and (6) are based on linear probability models. Standard errors are clustered on the household level and reported in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.11: Placebo Estimates for Pseudo-Flood on 31th December 2001 (2000 Excluded)

	Broad Treatment			Narrow Treatment		
	S	S^E	SE'	S	S^E	SE'
	(1)	(2)	(3)	(4)	(5)	(6)
Year 2002	10.397	-0.001	-0.034	11.026	-0.002	-0.035
	(27.412)	(0.024)	(0.026)	(26.902)	(0.024)	(0.026)
Treated	2.959	0.015	0.038	-50.446	0.072	0.090
	(65.614)	(0.066)	(0.066)	(76.212)	(0.097)	(0.096)
year 2002 $\times$ Treated	26.709	-0.019	0.014	71.699	-0.016	0.014
	(59.339)	(0.060)	(0.061)	(82.583)	(0.088)	(0.088)
Controls	yes	yes	yes	yes	yes	yes
Log Pseudolikelihood	-3485.115			-3086.887		
Adjusted R^2		0.131	0.137		0.128	0.134
Observations	664	664	664	588	588	588

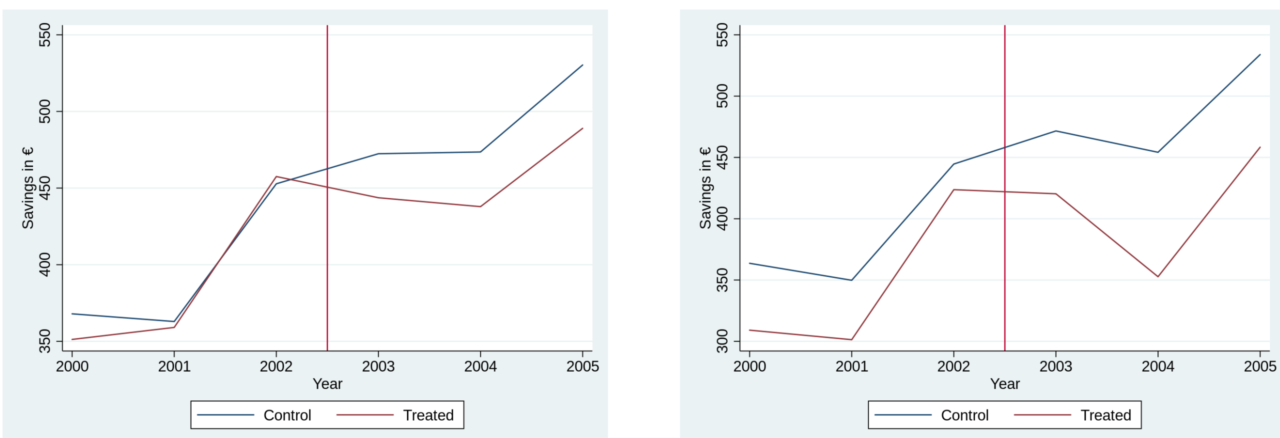
Note: All regressions include the full set of control variables. Columns (1) and (4) are based on Tobit models. Columns (2),(3),(5) and (6) are based on linear probability models. Standard errors are clustered on the household level and reported in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A graphical illustration of the trends in saving behavior provides further support in favor of the common trend assumption. Figures C.4 to C.6 show the predicted means of each saving outcome over time. Neither for the saving volume nor for the propensity to save, we do find any evidence for differences in trends of the control and treatment group before the flood occurred. Instead the figures suggest that trends were almost parallel until 2002. To clarify, the flood happened in August 2002, marked by the red line, and all respondents included in 2002 were interviewed before the flood.

Finally, it is necessary to discuss the identifying assumptions of the DiD approach in a non-linear context as we also estimate latent variable models (here a Tobit model). First, the sign of the treatment effect is equivalent to the sign of the interaction term parameter, given a strictly monotonic transformation function of a linear index (Puhani 2012). This allows for a simple qualitative interpretation of our non-linear Tobit estimates. Second, given a latent variable representation of the dependent variable, it is generally appropriate to assume and test the common trend assumption (Lechner 2011). For the case of a binary outcome variable and employing a Probit model, the common trend assumption does not hold without further modifications.[36] Therefore, our benchmark results on the propensity to save are based on a simple linear probability model (LPM) which does not require any additional parameter restrictions. In addition, we provide estimates of a Probit model (see Table D.15 in Appendix D), which qualitatively yields very similar results.

Figure C.4: Household Saving Volume

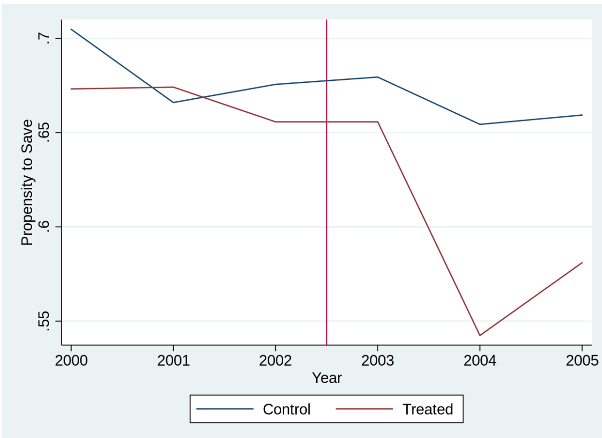
(a) Broad Treatment (b) Narrow Treatment



[36]Several parameter restrictions would be needed in this case. Alternatively, one could try to validate the common trend assumption for the non-linear transformation of the expected outcomes. See Lechner (2011) for a discussion.

Figure C.5: Households' Propensity to Save (S^E)

(a) Broad Treatment



(b) Narrow Treatment

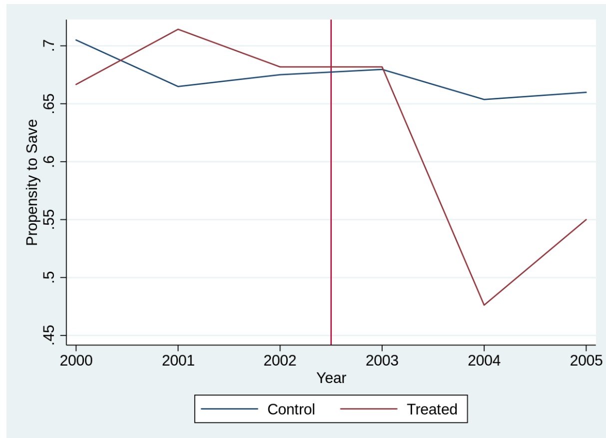
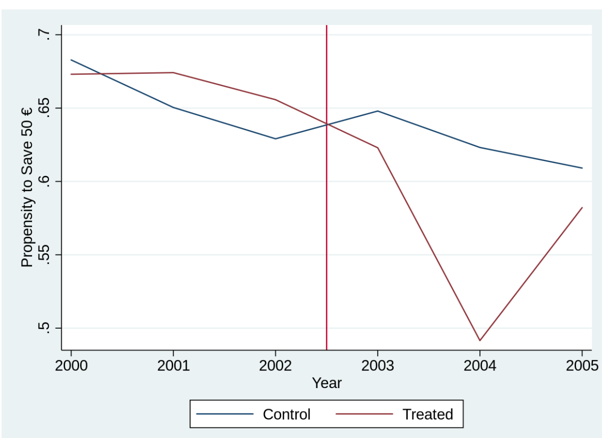
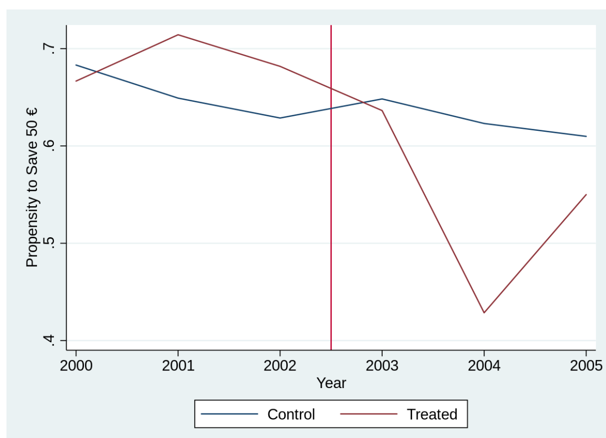


Figure C.6: Households' Propensity to Save ($S^{E'}$)

(a) Broad Treatment



(b) Narrow Treatment



Appendix D. Robustness Checks

This Appendix delivers estimation results for several alternative specifications and estimation approaches in order to evaluate the robustness of our results.

Appendix D.1. Use of Alternative Distance Thresholds for Control Group

One might be concerned that our results are driven by the choice of the distance threshold for the exclusion of observations from the control group. In order to check the sensitivity of our results for alternative threshold levels we repeated the main estimations for a threshold of 2,000 and 4,000 meters. As the results reported in Tables D.12 to D.14 show, the choice of the alternative thresholds are without material effect on the results.

Table D.12: Effect of the Flood Event on Saving Volume for Different Control Groups (Tobit Model)

	Broad Treatment		Narrow Treatment	
	2000 M. Thres.	4000 M. Thres.	2000 M. Thres.	4000 M. Thres.
	(1)	(2)	(3)	(4)
Year 2003	3.363	1.105	3.001	1.364
	(29.987)	(25.042)	(29.969)	(24.896)
Year 2004	0.631	-21.309	1.393	-20.958
	(41.250)	(32.793)	(41.077)	(32.510)
Year 2005	28.975	-8.694	30.584	-7.948
	(38.154)	(30.882)	(38.056)	(30.702)
Treated	-48.848	0.543	-58.651	-18.411
	(85.383)	(79.472)	(115.274)	(111.237)
Year 2003 × Treated	-65.161	-52.659	-98.942	-79.775
	(82.703)	(78.428)	(108.587)	(99.879)
Year 2004 × Treated	-135.848*	-102.413	-297.397**	-250.821**
	(79.072)	(71.882)	(126.659)	(115.234)
Year 2005 × Treated	-158.094**	-119.049*	-260.370**	-202.349**
	(73.926)	(65.873)	(102.283)	(87.820)
Controls	yes	yes	yes	yes
Log Pseudolikelihood	-5404.617	-7662.588	-4641.716	-6897.307
Left Censored Obs.	345	519	287	461
Observations	1014	1470	863	1319

Note: All regressions include the full set of control variables. Standard errors are clustered on the household level and reported in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D.13: Effect of the Flood Event on the Propensity to Save for Different Control Groups (Broad Treatment, Linear Probability Model)

	2000 Meters Threshold		4000 Meters Threshold	
	S^E	$S^{E'}$	S^E	$S^{E'}$
	(1)	(2)	(3)	(4)
Year 2003	-0.012	0.002	-0.004	0.011
	(0.026)	(0.029)	(0.022)	(0.022)
Year 2004	-0.024	-0.012	-0.027	-0.007
	(0.032)	(0.031)	(0.026)	(0.026)
Year 2005	-0.022	-0.033	-0.022	-0.023
	(0.032)	(0.034)	(0.027)	(0.028)
Treated	-0.031	0.017	0.004	0.055
	(0.064)	(0.064)	(0.061)	(0.061)
Year 2003 × Treated	0.010	-0.036	0.007	-0.041
	(0.071)	(0.074)	(0.069)	(0.071)
Year 2004 × Treated	-0.104	-0.170**	-0.097	-0.169**
	(0.072)	(0.071)	(0.069)	(0.069)
Year 2005 × Treated	-0.080	-0.075	-0.084	-0.086
	(0.068)	(0.068)	(0.064)	(0.065)
Controls	yes	yes	yes	yes
Adjusted R^2	0.117	0.128	0.115	0.127
Observations	1014	1014	1470	1470

Note: All regressions include the full set of control variables. Standard errors are clustered on the household level and reported in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D.14: Effect of the Flood Event on the Propensity to Save for Different Control Groups (Narrow Treatment, Linear Probability Model)

	2000 Meters Threshold		4000 Meters Threshold	
	S^E	$S^{E'}$	S^E	$S^{E'}$
	(1)	(2)	(3)	(4)
Year 2003	-0.012	0.002	-0.004	0.012
	(0.026)	(0.029)	(0.022)	(0.022)
Year 2004	-0.021	-0.010	-0.026	-0.006
	(0.032)	(0.031)	(0.026)	(0.026)
Year 2005	-0.018	-0.031	-0.021	-0.023
	(0.032)	(0.034)	(0.027)	(0.028)
Treated	0.031	0.072	0.063	0.113
	(0.105)	(0.105)	(0.103)	(0.112)
Year 2003 × Treated	-0.018	-0.074	-0.016	-0.075
	(0.108)	(0.117)	(0.103)	(0.112)
Year 2004 × Treated	-0.216*	-0.276**	-0.203*	-0.270**
	(0.125)	(0.130)	(0.122)	(0.126)
Year 2005 × Treated	-0.183*	-0.176*	-0.173*	-0.175*
	(0.103)	(0.104)	(0.100)	(0.101)
Controls	yes	yes	yes	yes
Adjusted R^2	0.105	0.113	0.110	0.120
Observations	863	863	1319	1319

Note: All regressions include the full set of control variables. Standard errors are clustered on the household level and reported in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix D.2. Use of Probit Models

Table D.15 shows the results for the propensity to save, based on a Probit rather than a linear probability model. The modeling approach does not affect the results qualitatively, as a comparison with Table 3 reveals.

Table D.15: Effect of the Flood Event on Households' Propensity to Save (Probit Model)

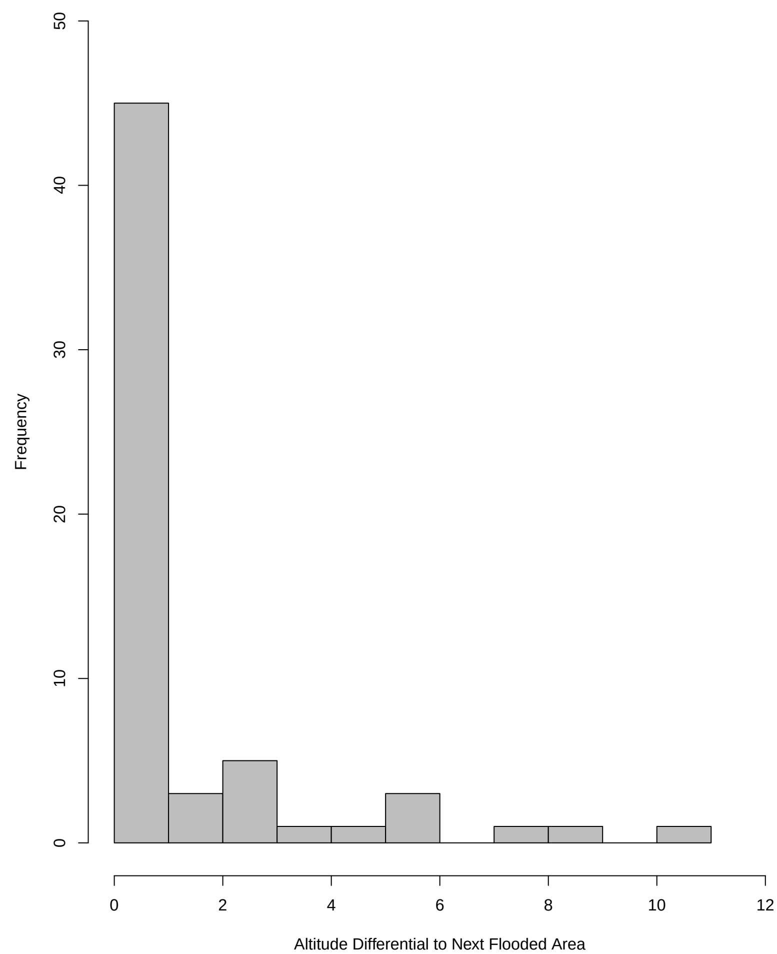
	Broad Treatment		Narrow Treatment	
	S_E	S_E'	S_E	S_E'
	(1)	(2)	(3)	(4)
Year 2003	-0.003	0.043	0.001	0.044
	(0.074)	(0.074)	(0.074)	(0.073)
Year 2004	-0.087	-0.032	-0.082	-0.030
	(0.085)	(0.079)	(0.084)	(0.078)
Year 2005	-0.082	-0.095	-0.078	-0.094
	(0.090)	(0.091)	(0.089)	(0.090)
Treated	-0.049	0.118	0.139	0.289
	(0.189)	(0.189)	(0.307)	(0.304)
Year 2003 × Treated	0.015	-0.126	-0.078	-0.245
	(0.213)	(0.219)	(0.316)	(0.332)
Year 2004 × Treated	-0.280	-0.490**	-0.571*	-0.742**
	(0.202)	(0.200)	(0.337)	(0.348)
Year 2005 × Treated	-0.229	-0.233	-0.493*	-0.486*
	(0.192)	(0.194)	(0.280)	(0.282)
Controls	yes	yes	yes	yes
Log Pseudolikelihood	-746.216	-761.855	-659.500	-676.984
N	1301	1301	1150	1150

Note: All regressions include the full set of control variables. Standard errors are clustered on the household level and reported in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix D.3. Exclusion of Extreme Altitude Differences from Broad Treatment

The broad treatment includes households which themselves were not flooded, but were exposed to significant flood risk as they lived less than 75 meters away from the flooded areas. The factual flood risk for these households not only depends on the distance to the next flooded area, but also on the topography of the living place. Households living in places with a significant altitude difference to the flooded area factually were not exposed to flood risk. One might therefore argue that they should be excluded from the broad treatment group.

Figure D.7: Histogram of Altitude Differentials to Nearest Flooded Area in Saxony, Broad Treatment Group



To identify households which should be excluded from the broad treatment group, we used elevation data from a digital elevation model (DGM 10) provided by *Staatsbetrieb Geobasisinformation und Vermessung*. The DGM 10 data consist of a raster data set with a resolution of 10 × 10 meters for entire Saxony. In the first step we identified the nearest point of each household to the flooded area, as visualized in Figure 3. In the second step we extracted the altitude above sea level for the household location and the nearest point to the flooded area to calculate the elevation difference. A histogram of the resulting altitude differentials is shown in Figure D.7. In the third step we excluded those households, associated with an elevation difference more than 5.83 meters, as these observations exceed the 95th percentile of the altitude differential and it was therefore quite unlikely that these areas were flooded. We then reestimated the models for the broad treatment group. The results, which are reported in Table D.16, turn out to be stable to this procedure.

Table D.16: Effect of the Flood Event on Saving for Different Control Groups, Altitude Exclusion (Tobit Model, Linear Probability Models)

	Broad Treatment		
	S	S^E	$S^{E'}$
	(1)	(2)	(3)
Year 2003	23.351	0.001	0.015
	(24.507)	(0.024)	(0.024)
Year 2004	-2.132	-0.026	-0.011
	(33.465)	(0.027)	(0.026)
Year 2005	16.214	-0.025	-0.030
	(32.238)	(0.029)	(0.030)
Treated	22.438	0.009	0.061
	(79.971)	(0.063)	(0.063)
Year 2003 × Treated	-79.763	-0.002	-0.050
	(79.814)	(0.073)	(0.075)
Year 2004 × Treated	-126.135*	-0.103	-0.173**
	(73.605)	(0.072)	(0.072)
Year 2005 × Treated	-150.685**	-0.086	-0.084
	(67.525)	(0.067)	(0.068)
Controls	yes	yes	yes
Log Pseudolikelihood	-6863.161		
Adjusted R^2		0.117	0.132
Observations	1290	1290	1290

Note: All regressions include the full set of control variables. Column (1) is based on a Tobit model. Columns (2),(3) are based on linear probability models. Standard errors are clustered on the household level and reported in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix D.4. Ordered Probit Model for Economic Worries

Table D.17 shows an alternative estimation of Table 4, based on an Ordered Probit model. No significant treatment effects can be observed, as before.

Table D.17: Effect of the Flood Event on Economic Worries (Ordered Probit Model)

	Broad Treatment		Narrow Treatment	
	Overall Eco.	Own Eco. Sit.	Overall Eco.	Own Eco. Sit.
	(1)	(2)	(3)	(4)
Year 2003	0.428***	0.141	0.426***	0.144
	(0.099)	(0.094)	(0.099)	(0.094)
Year 2004	0.256***	0.276***	0.253**	0.279***
	(0.099)	(0.095)	(0.099)	(0.096)
Year 2005	0.510***	0.127	0.508***	0.127
	(0.103)	(0.096)	(0.103)	(0.096)
Treated	-0.014	-0.142	-0.181	-0.322
	(0.165)	(0.160)	(0.258)	(0.255)
Year 2003 × Treated	0.226	0.124	0.185	0.260
	(0.245)	(0.226)	(0.381)	(0.358)
Year 2004 × Treated	0.009	-0.180	0.386	0.162
	(0.238)	(0.228)	(0.385)	(0.362)
Year 2005 × Treated	0.165	0.231	0.403	0.597
	(0.251)	(0.231)	(0.405)	(0.368)
Controls	yes	yes	yes	yes
Log Pseudolikelihood	-1067.421	-1291.649	-952.080	-1145.648
Observations	1378	1376	1225	1223

Note: All regressions include the full set of control variables. Standard errors are clustered on the household level and reported in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix D.5. Results for Saving Rate

Table D.18 displays the results for the saving rate. Irrespectively of the estimation approach, a negative and statistically significant effect in 2004 and 2005 can be observed for the narrow treatment definition.

Table D.18: Effect of the Flood Event on Households' Saving Rate

	Broad Treatment SR (OLS) (1)	Narrow Treatment SR (OLS) (2)	Broad Treatment SR (Tobit) (3)	Narrow Treatment SR (Tobit) (4)
Year 2003	0.001	0.001	0.001	0.001
	(0.006)	(0.007)	(0.009)	(0.009)
Year 2004	-0.003	-0.003	-0.006	-0.005
	(0.007)	(0.007)	(0.010)	(0.010)
Year 2005	0.004	0.004	0.002	0.002
	(0.008)	(0.008)	(0.011)	(0.011)
Treated	-0.007	-0.025	-0.009	-0.016
	(0.016)	(0.027)	(0.024)	(0.039)
Year 2003 × Treated	-0.015	0.002	-0.011	0.004
	(0.017)	(0.030)	(0.025)	(0.040)
Year 2004 × Treated	-0.029*	-0.044*	-0.048**	-0.084**
	(0.016)	(0.024)	(0.024)	(0.039)
Year 2005 × Treated	-0.023	-0.041**	-0.037	-0.075**
	(0.015)	(0.018)	(0.023)	(0.031)
Controls	yes	yes	yes	yes
Log Pseudolikelihood			7.128	9.602
Left censored Obs.			422	369
Adjusted R^2	0.132	0.144		
Observations	1276	1130	1276	1130

Note: All regressions include the full set of control variables. Standard errors are clustered on the household level and reported in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix D.6. Triple DiD for Food Consumption and Financial Reserves

Finally, table D.19 shows the results of the triple DiD for Food Consumption and Financial Reserves.

Table D.19: Triple DiD Effects for Homeownership on Food Expenses and Financial Reserves (OLS)

	Narrow Treatment	
	Food Expenses	Financial Reserves
	(1)	(2)
Year 2003	-18.137	-0.041
	(20.026)	(0.028)
Year 2005	28.626	-0.058*
	(21.930)	(0.033)
Treated	20.119	0.050
	(46.130)	(0.116)
Homeowner	43.661	0.051
	(33.403)	(0.050)
Treated × Homeowner	-52.054	-0.357*
	(56.525)	(0.194)
Year 2003 × Homeowner	46.447	-0.015
	(38.742)	(0.044)
Year 2005 × Homeowner	-35.506	0.024
	(43.041)	(0.053)
Year 2003 × Treated	75.223	0.150
	(55.617)	(0.101)
year3 × Treated × Homeowner	-119.077*	0.072
	(69.539)	(0.207)
Year 2005 × Treated	7.335	0.075
	(49.772)	(0.190)
Year 2005 × Treated × Homeowner	-13.224	-0.134
	(89.655)	(0.245)
Controls	yes	yes
Adjusted R^2	0.160	0.149
Observations	422	905

Note: All regressions include the full set of control variables. Standard errors are clustered on the household level and reported in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$