The causal effect of flood experience on climate engagement: evidence from search requests for green electricity

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Abstract

It has been postulated that personal experience of climate change-related weather events may reduce the psychological distance to climate change and trigger engagement in climate protection measures. We use a novel longitudinal dataset on revealed household behavior and insured damage data to re-examine this relationship, which has mostly been studied by cross-sectional and self-reported data. Using a difference-in-differences estimator, we assess the causal effect of experiencing financial damage from the 2013 floods in Germany on the interest for renewable energy tariffs in online power portals, which we take as a proxy for engagement in climate protection. The results broadly confirm the expected positive effect of flood experience on climate engagement, but there are important non-linear effects. Most notably, the effect drops to zero if damage is very high meaning the causal effect of flood experience on interest in green energy holds only for moderately affected regions. One explanation for this inverted U-shaped effect is that high flood damage may constrain the available budget for costly climate protection, due to high recovery and reconstruction costs. We also suggest a number of psychological mechanisms that may play a role in explaining this non-linear effect, for example non-protective responses such as denial and fatalism if damage is high. When supporting private climate engagement, policymakers should not rely on a motivating effect of damage experience, but should acknowledge the economic and psychological limitations, especially of severely flood-affected households.

Keywords

Climate protection; Difference in differences; Floods; Green energy; Panel data

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1. Introduction

Effective mitigation of climate change depends crucially on the willingness of households to engage in climate-friendly behaviors, such as energy saving, usage of renewable energies, or voluntary off-setting of emissions. Psychological research has postulated that private engagement in these types of behavior is inversely related to the psychological distance to climate change. Hence, individuals who perceive climate change as a relatively certain and psychological close phenomenon (i.e. affecting people and places close to them) are more motivated to engage in climate action than people who are more psychologically distant to climate change (McDonald et al. 2015). Furthermore, direct personal experience of extreme climatic events and related damage has the potential to decrease the psychological distance to climate change. Consequently, it has been postulated that personal experience may trigger or intensify private engagement in climate change mitigation (Spence et al. 2011; Reser et al. 2014; Demski et al. 2017).

Accordingly, there is a growing literature of empirical studies on the question whether households' experience of climate-related weather events affects their willingness to engage in climate-friendly behaviour. Our review of studies to date (Table S1 in the Supplemental Material for a summary of the main features and findings of these studies) suggests that the empirical literature broadly supports the hypothesis of increased willingness to engage in climate action after the experience of a climate-change related event or damage. However, the summary also exhibits important limitations of existing studies, which we aim to address and build on in the current analysis.

First, most studies rely on cross-sectional data. With cross-sectional data, there is always the possibility that measured correlations are spurious, i.e. caused by unobserved factors. Hence, it is difficult to infer causality from the estimated relationships. While many authors acknowledge the fact that longitudinal (panel) data are actually better suited for their analyses, panel analyses have not been possible so far due to data constraints (Spence 2011, McDonald et al. 2015, Demski et al. 2017). Here, we present one of the first studies based on panel data. This enables us to assess the hypothesized relationship by estimation techniques that allow a causal interpretation of the effect, such as the difference-in-differences approach (DiD) explained in section 3.5.

Second, most studies rely on self-reported measures of both key variables (climate action and experience). Exemptions are the studies of Zahran et al. (2006), who use administrative data on natural hazard impacts as a proxy for personal experience, and real monetary donations used as a measure for climate action in the experimental analysis of Li et al. (2011). Although self-reported data are sometimes beneficial, e.g. due to their potential high level of detail, they may be biased by personal characteristics, measurement errors, socially desired responses, or strategic response behaviour (Chen et al. 2017; Osberghaus 2017). The extent of these biases vary with the particular formulation of questionnaire items, e.g. whether specific experiences or behaviours are elicited (Demski et al. 2017). We completely refrain from using self-reported information on experience or climate action measures. Instead, we use externally provided damage data reported by the insurance industry as an experience proxy and the revealed online search behaviour for electricity tariffs as an indicator for the interest in green energy. These measures should be unaffected by strategic or socially desired response behaviour. They are subject to little measurement error, and their reporting is independent of individual characteristics. Hence, we see this as a second major strength of our empirical strategy compared to the existing literature.

There are some studies that use both longitudinal data and behavioural (rather than self-report) variables. These studies have predominantly examined the relationship between local weather extremes and attention to climate change, measured by examining internet search or social media behaviour (Lang 2014). For example, Lang & Ryan (2016) find experiences with tropical cyclones are accompanied by an increase in Google search activity mentioning climate change two months after an event. Similarly, Sisco et al. (2017) find an increase in local Twitter messages focused on climate change after extreme cold and heavy snow in a U.S. sample. However, these studies do not distinguish between types of climate change attention. Indeed, Sisco et al. (2017) suspect that the increased attention on climate change just after extreme weather is likely to be dominated by sceptical attitudes. In addition, attention to climate change, in the form of internet searching or social media posting, is still relatively far removed from taking action to limit climate change. The current study addresses this limitation by examining a behavioural indicator related to climate change mitigation action (i.e. purchasing green energy).

Finally, existing studies have focused primarily on establishing whether a link exists between experience and climate action. They have not been able to examine the nature of this relationship in more detail. Here, we begin to do so by exploring the possibility of a non-linear relationship between experience and climate change engagement and to what extent this may differ as a function of regional differences. In financial terms, households face budget constraints. If households are motivated by the flood experience to do something, they may have to choose between costly climate action (e.g. paying a premium for renewable energy) and investments in flood protection measures such as insurance and structural measures.[1] A potential non-linear relationship may also relate to an uneven damage distribution on income levels. If low income regions are affected in an overproportioned manner (as suggested by an emerging strand of literature on the social aspects of vulnerability, see references in footnote 9), the severely affected regions may engage less with costly climate protection because of a lack of financial resources.

There are also psychological reasons to suspect the relationship between experience and climate engagement might not be linear, or hold for all subgroups. While experiences of extreme weather might hold the potential to the psychological distance of climate change,

[1] Indeed, the flood of 2013 in Germany caused households in affected districts to invest in flood protection measures (Osberghaus 2017).

experiencing particularly severe flood damage may also result in feelings of helplessness and fatalism (Hamilton-Webb et al. 2017). The psychological literature further suggests that people's responses to stress and increased perceptions of risk might not necessarily be one of taking protective or mitigating action. It could also lead to a number of non-protective responses, such as denial and risk minimisation (Whittle et al. 2014; Taylor et al. 2014; van der Linden 2015).

Therefore, we may expect those households or regions, which are severely affected, might have a different response to those only moderately affected by flooding. In particular we might expect them to be less inclined to act on climate change. While we are not able to examine these different mechanisms in detail using the current dataset, we do explore to what extent the relationship between experience and climate engagement is linear or non-linear. We also examine to what extent this relationship is different in specific subgroups (similar to Ogunbode et al. 2017), particularly focusing on comparing regions with different income levels and those that have had previous flooding experience.

2. The current study

We are able to add to the literature in the novel ways described in the previous section by focusing on a major flood that occurred in large parts of Germany in June 2013. Heavy rainfalls caused overflowing and dam failures at the rivers Danube and Elbe and some of their tributaries. The flood claimed at least 14 casualties, and affected around 600,000 people, of whom more than 80,000 had to be evacuated (Thieken et al. 2016). The total economic damage was higher than five billion €, of which 1.65 billion € were insured (GDV 2016). For businesses, the flood caused shocks in inter-regional supply chains and inoperability (Oosterhaven and Többen 2017, Schulte in den Bäumen et al. 2015). The flood insurance penetration amongst German households was at around 34% in 2013. This means a remarkable share of households did not have flood coverage. This was one reason why the federal government launched a multi-billion € relief fund in the direct aftermath of the flood (Neugart and Rode 2018 analyze effects of these relief payments on voting behavior). Being an extreme hydrological weather event, the flood drew the attention of society and media to the potential consequences of climate change. On the website google.de, the search term "climate change" peaked two times in 2013 – during the UNFCCC Conference of the Parties in late November, and during the onset of the floods in early June (Figure S1 in the Supplemental Material). The medial discussion on the possible link of the 2013 flood with global climate change was accompanied by publications of the German meteorological service, which emphasized the need to adapt to more severe flood events due to climate change (DWD 2013). Even before the flood, a large majority of household heads in Germany (88%) expected climate change to cause more flood events in Germany (survey data of 2012, Osberghaus et al. 2013). Hence, there is some potential that the specific event in June 2013 increased the public's general awareness of climate change and consequently, private engagement in climate action.

To examine if this indeed occurred we use data on the revealed interest in green electricity tariffs in Germany in the years 2012 to 2014. Electricity delivered under green tariffs is generated exclusively by renewable energy sources such as wind, water, or solar power. There is obviously a large variety of ways households may contribute to combatting climate change. In the German case we suggest that interest in renewable energies for private homes is a good approximation for the general willingness to engage in climate action. This is because a recent large-scale consumer survey has shown that Germans (in some contrast to US-Americans) perceive the usage of renewable energies as the most effective means of climate protection on the household level – i.e. as more effective in mitigating climate change than saving energy, buying energy-efficient appliances, reducing car use, and other climate-friendly activities (Lange et al. 2017).[2] We measure the interest in renewable energy based on user behavior at online electricity portals. At such portals, consumers search, compare, and eventually sign up for electricity tariffs for private homes. Importantly, we observe whether users filtered their search results for green electricity tariffs or not, which location of residence they indicated, and the time of the search.

Based on longitudinal data, we compare the time trend of interest in green energy in severely flood-affected districts with the corresponding trend in less-affected districts using a difference-in-differences estimation, including an analysis of the trends before the flood event. Thus, we are able to examine the causal effect of regional flood experience. For measuring regional flood intensity variance, we use flood damage data reported by the German insurance industry. Based on previous literature we expect a significant relationship between experience and climate action. However, we do not necessarily expect this relationship to be linear, as previously discussed. Finally, we examine whether the effect varies as a function of specific subgroups of the sample. Particularly if economic constraints play a role, we would expect to see small effects in economically deprived regions. We also examine whether affected districts had experienced a previous flood event in 2002, which was of comparable size and severity to the event in 2013. Due to this previous experience, households in these regions may be less susceptible to the impact of flooding in 2013 on climate engagement.

3. Data and Empirical Strategy

3.1. Panel structure

The main dataset for this analysis contains information on online search requests for electricity tariffs in Germany between January 2012 and December 2014 (from 17 months

[2] Purchasing green energy has been suggested to be one of the most impactful climate actions individual can take (e.g. Wynes and Nicholas 2017), although the effect may be more marginal in countries like Germany where emissions from power supply are covered by the EU Emissions Trading Scheme. Nonetheless, existing research also shows that the actual effectiveness of a particular action is only one of many factors that motivates individuals to engage in climate action. For a discussion of these factors, please refer to van der Linden et al. (2015) and Whitmarsh et al. (2013).

before to 18 months after the flood event). Although the search data allow an analysis on a more disaggregated spatial level, the flood damage data are only available on the district level. Thus, we collapse all data on the district-month level, resulting in a strongly balanced panel dataset (402 districts, 36 months; N=14,472).

3.2. Dependent variable: Interest for green electricity

We measure households' revealed interest in climate action by their search activities in German online electricity portals. The raw data were obtained from ene't GmbH, a provider of datasets for the energy sector. The dataset is fed i.a. by the portals "TopTarif", "Strom- und Gastipp", "Energieverbraucherportal", and "Mut-zum-Wechseln".[3] The aggregate of these portals captured more than 31 million user requests in the period from 2012 to 2014. After cleaning the raw data from users who are most probably automatic web crawlers, the number of "real" user requests is still at around 22 million.[4] Many users conduct several requests with different search parameters and filters within one session. We therefore combine the requests to 10.2 million search sessions using three different variables capturing the interest in green electricity: The variable *green_first* refers to sessions where the green electricity filter was set in the first search request. The variable *green_last* indicates that the user was interested in green tariffs in the final search request. Finally, *green_one* is a measure for those sessions where the filter was set at least for one search request. These data are collapsed on the district-month level due to the spatial resolution of the flood variable. For the rest of the analysis, we will focus on the dependent variable *green_last*, because this variable is arguably closest to the final decision whether a green tariff is actually chosen or not. The other variables are used in robustness checks. The descriptive statistics of the interest in green electricity are presented in Table 1, and a correlation matrix is provided in Table S2 in the Supplemental Material. Its temporal and spatial distributions are depicted in Figures S2 and S3 in the Supplemental Material, respectively.

3.3. Treatment variable: Insured flood damage

As a proxy for damage caused by the 2013 floods, we rely on data reported by the German insurance association (GDV). For each of the 402 German districts, the dataset provides the average pay-out triggered by the June 2013 floods per insurance policy.[5] The raw data are

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[3] The literal translation of these portal names are "top tariff", "electricity and gas tip", "energy consumer portal", and "courage to switch", respectively.

[4] Users were assumed to be web crawlers and excluded if the number of search requests exceeds twenty for one day, or if indicated user zip-codes of residence differ for different requests on the same day, or if a user conducted multiple requests within one second. Moreover, the requests of 60 zip-code-month-combinations were excluded which contained unrealistically high numbers of requests (more than 0.5 per inhabitant).

[5] The flood insurance penetration in Germany (i.e. the share of households who are flood-insured) varies strongly between federal states, and presumably also between districts. This could be a problem for the accuracy of our flood variable if penetration was correlated to flood risk. However, household-level analyses of flood insurance coverage in Germany find no evidence of such a correlation (Andor et al. 2017, Hudson et al. 2017). This may be due to risk-based insurance pricing. Furthermore, the flood intensity measure could be biased if flood insurance was not available (or only at prohibitive costs) in high flood risk zones. While there is no independent source for this information, GDV reports that for more than 99% of the

grouped into nine categories, which we combine into three groups: Districts with no or little damage (less than 47 € average pay-out, the lowest 66.7%), moderate damage (between 47 and 283 €, between 66.7% and 90% percentiles), and high damage (higher than 283 € per policy, the highest 10%). Hence, we use two dummies as treatment variables (indicators for moderate and high damage, respectively), with low damage districts as the reference category. The spatial distribution of the flood damage is depicted in Figure 1, descriptive statistics of the treatment variables are presented in Table 1.

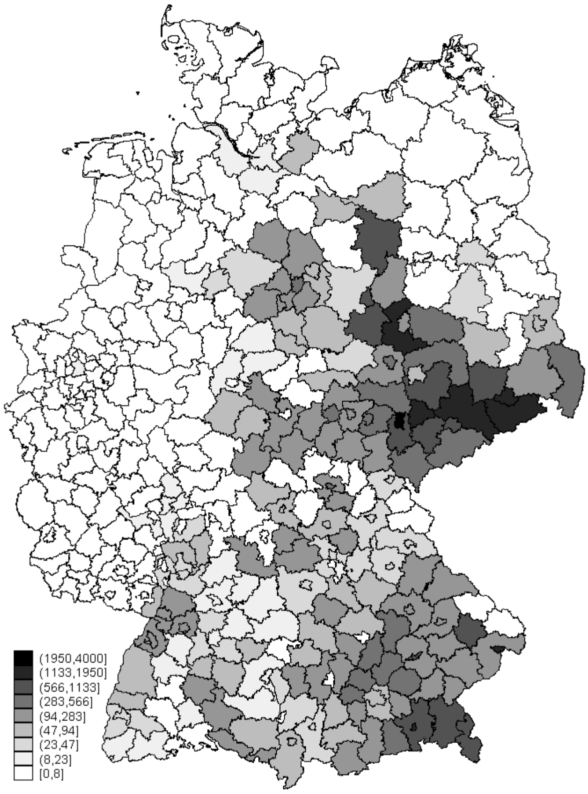

Figure 1: Average payout in € per insurance policy due to the 2013 flood event. Map based on GK3 projection, © GeoBasis-DE / BKG 2019.

3.4. Covariates

To control for socio-economic conditions, we include the following covariates: *income* and *old* measure the log of average available household income, and the percentage of citizens aged 65 and older. This information is available on an annual basis and is provided by the INKAR regional database (BBSR 2017). *unemp* is available on a monthly basis and captures the district percentage of the unemployed in the total civilian work force (Bundesagentur für Arbeit 2017). Furthermore, we control for the district-month median of the electricity consumption reported by the users as part of their online request (*consumption*) and the absolute number of search sessions in a district-month (*number*). The descriptive statistics of the covariates are included in Table 1.

addresses flood insurance is readily available at reasonable cost. The remaining buildings may be insurable after site inspections or with additional clauses in the policies (GDV 2018).

Table 1: Descriptive statistics of key variables on district-month level.

Variable		Description	Min	Max	Mean	Std. dev.
Dependent variables (y_{it})	*green_first*	Share of sessions in which the first search included a request for green electricity	0	0.633	0.021	0.030
	green_last	Share of sessions in which the last search included a request for green electricity	0	0.635	0.035	0.032
	green_one	Share of sessions with at least one search included a request for green electricity	0	1	0.041	0.035
Treatment variables (D_i)	*dam_low* (dam<p67)	Average pay-out per insurance policy is in the indicated percentiles (*dam* = damage)	0	1	0.667	0.471
	dam_moderate (p67<dam<p90)		0	1	0.234	0.423
	dam_high (p90<dam)		0	1	0.100	0.299
Covariates (X_{it})	*income*	Logarithm of average available household income	7.17	8.15	7.44	0.12
	unemp	Unemployment rate	1.10	18.50	6.37	3.04
	old	Percentage of citizens aged 65 and older	15.20	29.50	21.40	2.43
	consumption	Median electricity consumption in kWh	1,500	12,000	3,482	377
	number	Number of requests	1	77,820	686	1,770

Number of observations: 14,472. Number of months: 36. Number of included districts: 402. Of these, 268 districts are classified as lightly damaged, 94 as moderately damaged and 40 as highly damaged.

3.5. Empirical strategy: Difference-in-differences estimation (DiD)

Interest in green electricity tariffs is measured before and after the 2013 floods, in severely hit regions as well as districts that were virtually not affected. Therefore, the data allows for a DiD estimation, which may be used to investigate the causal effect of an event or a policy change on a dependent variable (Greene 2012; Wooldridge 2009). The basic idea of DiD is as follows: There are two groups of individuals, states or districts which are broadly identical in terms of the dependent variable.[6] One of the two groups is subject to a treatment, which expectedly affects the dependent variable. The DiD estimator measures the trend of the dependent variable in the treatment group and compares it with the corresponding trend in the untreated control group. In a basic linear regression model the treatment effect is estimated as follows:

$$y_{it} = \beta_0 + \beta_1 T_t + \beta_2 D_i + \gamma T_t D_i + \varepsilon_{it} \qquad (1)$$

The dependent variable y_{it} depicts the share of sessions filtering for green electricity in district i and month t. y_{it} is regressed on two indicator variables (T_t equals zero before the flood and one afterwards; D_i equals zero for unaffected districts and one for treated

[6] More specifically, the time trend of the dependent variable before the treatment is assumed to be identical.

districts) and additionally on their interaction term $T_t D_i$. ε_{it} designates the error term; γ represents the estimate of the treatment effect and is the main parameter of interest.

We amend this basic DiD model along several dimensions: First, we add a vector of covariates (X_{it}) which improves the model fit.[7] Second, we include district- and month-fixed effects which absorb the effect of all (observed and unobserved) time-invariant district-specific factors (μ_i), and general month-specific effects, such as nation-wide variations of interest in green electricity (ϑ_t).[8] Third, we replace the single treatment variable D_i by the two binary variables D_i^{mod} and D_i^{high} which indicate districts with moderate and high flood damage, respectively. We omit the month of the flood event (June 2013, $t = 18$) as the districts were hit at different days in this month. We also omit the variables D_i^{mod}, D_i^{high} and T_t (but not their interaction variables) due to perfect multicollinearity with the fixed effects μ_i and ϑ_t, respectively. Hence, the final estimation model is the following:

$$y_{it} = \gamma_1 T_t D_i^{mod} + \gamma_2 T_t D_i^{high} + \delta X_{it} + \mu_i + \vartheta_t + \varepsilon_{it}, \qquad t = 1, \ldots, 17;\ 19, \ldots, 36 \qquad (2)$$

This baseline model will be estimated by OLS with standard errors clustered on the district level, since the impact of the flood differed strongly within a federal state, emergency relief was organized and flood alerts were issued at the district level. Several alternative specifications and subsamples are used in a number of robustness checks detailed in the Supplemental Material.

3.6. Pre-Treatment analysis

A crucial (but untestable) assumption of DiD is that prior to the treatment, and in absence of the treatment, the dependent variable followed the same trend in the different treatment and control groups. Although this assumption cannot be formally tested, its plausibility can be assessed graphically and by regressions. For the graphical analysis we split the districts into three groups, defined by low, moderate and high damage according to the thresholds given in Table 1. Figure 2 shows the time trend of *green_last* before and after the flood for all groups. Hence, from the graphical analysis one may conclude that the assumption of parallel pre-treatment trends is quite reasonable.

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[7] The "bad control" problem raised by Angrist and Pischke (2009, p.64) may occur if covariates depend on the treatment themselves Because an effect of the floods on the included covariates cannot safely be excluded, we run a regression without them as a robustness check.

[8] The test for over-identifying restrictions (*xtoverid* in STATA) suggests the use of the fixed effects model rather than the random effects model.

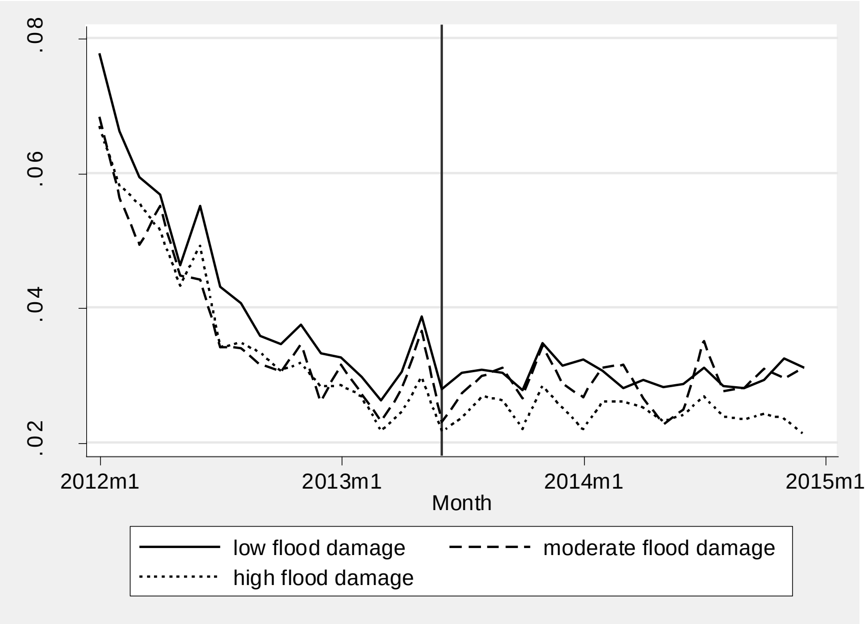

Figure 2: Time trend of *green_last* (the share of sessions in which the last search included a request for green electricity), for districts with low flood damage (damage below 67th percentile), moderate damage (damage between 67th and 90th percentile) and high damage (damage above 90th percentile). The vertical line marks the month of the flood event (June 2013). Figure S4 in the Supplemental Material depicts the same time trends after the fixed effects transformation.

We also check pre-treatment trends by regressing y_{it} on a series of interactions of month dummies with the treatment variables (Autor 2003; Pischke 2005). The results, presented in the first section of the Supplemental Material, support the conclusion of parallel trends of interest in green electricity before the 2013 floods.

4. Results

4.1. Baseline results

Table 2 summarizes the results of the DiD estimations of equation (2). The results show a significant positive relationship of the flood intensity in June 2013 and the interest in green electricity after the flood event in moderately affected districts, while households in highly affected districts do not change their search behavior. Hence, we can speak of an inverse u-shaped effect. Under the assumption of parallel pre-treatment trends of differently flood-affected districts, this relationship may be interpreted as causal.

Table 2: Regression results of DiD estimations. Dependent variable: Share of sessions with last request for green electricity. Standard errors are clusters on district level.

	Variable	Coefficient	Standard error	p-value
Treatment effects	Moderate damage ($D_i^{mod} * T_t$)	0.0045***	0.0013	0.001
	High damage ($D_i^{high} * T_t$)	0.0004	0.0019	0.822
Covariates (X_{it})	income	0.1186	0.0787	0.133
	unemp	-0.0007	0.0005	0.191
	old	0.0047	0.0043	0.269
	consumption	$-2.15e^{-6}$	1.64^{-6}	0.192
	number	$-1.87e^{-6}$	$1.25e^{-6}$	0.134
	constant	-0.8921*	0.5375	0.098
Fixed effects	for 35 months	included		
	for 402 districts	included		
Number of observations		14,070		
R^2		0.626		

*, **, *** denote significance levels of 10, 5, and 1%, respectively. Number of included districts: 402. Number of months: 35. The difference between the estimated treatment effects for high damage and moderate damage is significant ($p<0.05$). Following the estimation routine of STATA, the constant is the average value of the fixed effects.

In interpreting the results, one should keep in mind that time-invariant, district-specific effects (such as location-specific effects, time-persistent differences in income and political attitudes) as well as nationwide month-specific effects (such as effects of public debates) are fully captured in the district- and month-fixed effects. This might explain the non-significance of the covariates in the baseline results. We perform several placebo tests and robustness checks and show that the main result of a non-linear causal effect of flood experience on green electricity searches is stable over alternative model specifications, estimation samples, and definitions of outcome and treatment variables (details see Supplementary Material). We are therefore confident in saying that the floods of June 2013 triggered interest in green electricity, but only in moderately affected regions. The size of the effect, although statistically significant, is relatively small. The percentage of search sessions filtering for green electricity in the last requests increased by 0.45 percentage points in the moderately affected districts. This is well below the standard deviation of the dependent variable (which amounts to 3.2 percentage points). If we use these values to calculate the absolute number of additional search requests due to the flood up to December 2014, we obtain an estimate of around 2,700 additional green searches (compared to an aggregate of around 850,000 searches in the moderately and severely affected districts in the post-flood period).

4.2. Treatment heterogeneities

The effect of flood experience on climate engagement may vary between different groups of individuals. For example, flood affected households with a high income may be more willing or capable to engage in climate action than their equally affected, but poorer counterparts. In this section, we exploit observable differences of German districts to analyze possible treatment heterogeneities between the districts. We test all covariates mentioned in Table 1 as potential sources of heterogeneity between districts but focusing particularly on income. That is, we examine to what extent the non-linear relationship is evident in districts with varying levels of income. We also examine the effect of flood experience prior to the floods of 2013 (approximated by the average insurance pay-out per insurance policy issuance after a major flood in August 2002).

We use two strategies to identify heterogeneities: First, we estimate the baseline regression for two subsamples, separated by the median value of the variable, which may cause heterogeneity (H_i, e.g., income), measured before the flood (average value of 2009-2012). We also assure that all observations of a given district are sorted in the same subsample. Substantially different estimates of the treatment effect indicate possible treatment heterogeneities. Second, we estimate a specification with the DiD variables interacted by a continuous measure of H_i in the full sample. If both empirical strategies suggest treatment heterogeneities regarding H_i, this serves as a strong indication that the flood experience effect on climate engagement varies with the potential source of heterogeneity.

The results for income, summarized in Table 3, suggest that treatment effects indeed vary by financial resources of the households: The flood effect on interest in green electricity is higher in richer districts (as visible in columns 1 and 2). Interestingly, in richer districts moderate *and* high damage exhibit a positive effect on interest in green electricity. This suggests that the non-linear effect in the full sample is associated with low-income districts. The coefficients of interaction terms presented in column 3 confirm these results, particularly for severely damaged districts.

Table 3: Treatment heterogeneities regarding household income.

Source of heterogeneity: Household income in 2009-2012 ($inc0_i$)		**Split samples**		**Treatment effect interactions**
		$inc0_i$ **below median**	$inc0_i$ **above median**	
Treatment effects	$D_i^{mod}T_t$	0.0030	0.0063***	-0.1168
	$D_i^{high}T_t$	-0.0030	0.0073**	-0.2345**
	$inc0_iD_i^{mod}T_t$	-	-	0.0164
	$inc0_iD_i^{high}T_t$	-	-	0.0320**
	$inc0_iT_t$	-	-	-0.0132
Covariates (X_{it})	income	0.0677	0.1541	0.1189
	unemp	-0.0011*	-0.0014	-0.0006
	old	0.0038	0.0072	0.0046
	consumption	-2.71e^{-6}	-1.01e^{-6}	-2.07e^{-6}
	number	-5.97e^{-7}***	-4.89e^{-6}***	-1.89e^{-6}
	constant	-0.4923	-1.2088	-0.8929
Fixed effects	for 35 months	Yes	Yes	Yes
	for 402 districts	Yes	Yes	Yes
Number of observations		7,035	7,035	14,070
Number of districts		201	201	402

*, **, *** denote significance levels of 10, 5, and 1%, respectively. The heterogeneity variable $inc0_i$ is the logarithm of average monthly available household income in 2009-2012. As we use a continuous variable in column 3, the coefficients of the interaction terms do not directly correspond to the differences between

column 1 and 2. Following the estimation routine of STATA, the constant is the average value of the fixed effects.

Regarding prior experience, the estimated treatment effects seem to depend on flood experience before the floods of 2013 (Table S5). In 2002, another major riverine flood affected some of the districts that were flooded in 2013. Both empirical strategies suggest that the inverted U-shaped effect only exists in districts that had already suffered from flood damage in 2002. In districts unaffected by the 2002 flood, we find a positive effect for districts with moderate *and* high flood damage in 2013. We interpret these findings in the next section.

5. Discussion and Implications

This study is one of the first to present analysis of personal experience effects on climate engagement based on longitudinal data (instead of cross-sections) and on revealed actions and externally reported experience (instead of self-reported data). In addition, it is the first to examine non-linear effects. Using a difference-in-differences approach, we suggest that experiencing a major flood in 2013 had a positive causal effect on the interest in green electricity in Germany. However, the effect is relatively small and mainly observable for households living in moderately affected districts. In districts with very high flood damage (above the 90th percentile) the effect drops to zero. Furthermore, the positive effect appears to be particularly pronounced for households in districts with higher income and without previous flood experience. In these regions we observed increased search requests for green electricity after the flooding independent of whether they experienced moderate or severe damage.

There are of course limitations to this analysis. For example we were only able to examine one specific form of climate engagement (interest in green electricity), which has to be kept in mind when considering the overall size of the effect. Nonetheless it is one which has the highest perceived efficacy in terms of protecting the global climate (Lange et al., 2017). Moreover, we can only observe interest in green electricity, and not whether a green tariff was actually contracted. However, the searching and comparing of different tariffs is typically an important step before contracting.

The findings from the analysis pose a number of questions about the mechanisms underlying the effect of experience on climate engagement. The finding that there is indeed a causal effect of flood experience on engagement may be due to the reduced psychological distance of climate change, which has been discussed in the literature (e.g. McDonald et al. 2015). Flood victims who establish a link between the flood and climate change, may perceive climate change as more certain, and closer in temporal and spatial terms compared to before the event. While we are not able to confirm this with the current data, our findings are in line with this suggestion. In addition, survey data show that many Germans indeed see a link between flood events and climate change: In 2014, 79% of households expected climate change to increase flood damage in Germany (Osberghaus and Philippi 2015). Nonetheless, our findings also indicate that the explanations for the effect of flood experience on climate engagement may need to be revised to account for the non-linear relationship exposed in the current analysis. Specifically, we have shown that the positive effect of flood experience does not hold for households living in districts with very high flood damage. There may be multiple mechanisms that give rise to this non-linear relationship. Thus, we suggest that future research should examine both economic and psychological explanations.

With regards to economic explanations, there are at least three possible mechanisms which may contribute to the non-linear effect in the full sample. First, households severely affected by flood damage may prefer to invest scarce financial resources into recovery and reconstruction activities, protection or insurance. Hence there is less money available for costly climate mitigation measures such as paying a premium for green electricity. Our findings provide some initial support for this explanation. Specifically, the notion that severely flood damaged households lack financial resources for costly climate action is supported by the heterogeneity in flood response. Households in economically viable districts are more responsive to flood experience compared to households in more deprived districts, independent of the severity of damage. Hence, the financial capabilities seem to play a role in the decision to engage in climate change mitigation in the aftermath of a flood.

The second potential mechanism combines this finding with the distribution of flood damage across income levels: If severely damaged districts tend to be poorer than less damaged regions, and given that economically deprived units are more reluctant in terms of climate engagement, this will contribute to a non-linear effect in the full sample. Indeed, pre-flood income in the 40 severely damaged districts was significantly lower than income in the baseline districts ($p<0.01$ according to the Wilcoxon-Ranksum Test, see Table S6). This means the flood damage was unevenly distributed across income levels. Given that poor districts happen to be affected more severely and respond less in terms of climate engagement because of their financial restrictions, this results in a non-linear relationship.[9]

A third economic explanation of the non-linear effect is related to disaster relief payments: If mainly severely affected households were eligible for disaster relief, they could feel less affected after their financial losses have been compensated, as compared to moderately affected households without access to relief payments. We are aware that this economic explanation contradicts to the former, but because of data scarcity, we cannot rule out one or the other. However, we do note that flood experiences are about much more than just financial effects. We would therefore stipulate that receiving disaster relief is unlikely to

[9] Although we cannot rule out that these relationships are specific to our sample, we would like to highlight that a negative correlation of (a) natural disaster risk and related damages and (b) income levels are relatively common in the empirical literature (Bin et al. 2017; Hallegatte et al. 2016; Masozera et al. 2007; Miljkovic & Miljkovic 2014; Sayers et al. 2018). Hence, we argue that the proposed mechanism is not necessarily unique to our data, but may occur in other contexts as well.

compensate for the psychological and health effects many flood victims will have experienced (e.g. fear and distress, loss of irreplaceable personal belongings etc.).

In line with this, there may also be psychological mechanisms that could explain the non-linear effects of flood experience on climate engagement. Specifically the role of emotion in disaster-recovery and the potential for non-protective responses are important to consider (Moser 2014; Whittle et al. 2012; Taylor et al. 2014; Rogers 1975; Rogers 1983). Those individuals who experience extensive damage and disruption as a result of flooding may experience feelings of helplessness and may even engage in threat denial if their coping appraisals are low (Grothmann and Reusswig 2006; Bruegger et al. 2015). Unfortunately, the datasets do not enable an analysis of psychological mechanisms underlying these effects because such data was not collected as part of the electricity search requests. Nonetheless we do find that households in districts with prior experience of flooding (in 2002), and who were severely affected by the 2013 event, did not show increased climate engagement, as opposed to those only moderately affected. In previous research it has been found that previous flood experience (especially recurring experience) may be associated with increased fear and health-stress outcomes (Hannson et al. 1982). If this is also the case here, it could explain why those that were severely affected and had experience of prior flooding were less likely to engage in climate action.

Hence, we suggest several potential explanations for the non-linear effect of flood experience on climate engagement, which are consistent with the data used in our analysis: First, severely-affected households have less financial resources for climate engagement due to the suffered flood damage, or second, because they were relatively poor even before the flood. Third, these households may actually feel less affected because they received relief payments. Fourth, severely affected households may be subject to non-protective responses such as feelings of helplessness or threat denial– especially if they have prior flooding experiences. Future research should attempt to obtain data that can assess multiple mechanisms such as the ones suggested here.

Notwithstanding the limitations, the analysis has important implications for understanding people's experiences and responses to climate change. Climate-related severe weather events provide important windows of opportunity to communicate the importance of climate mitigation actions, but policy makers and communicators need to take into account the differential effects such an experience might have on different regions and individuals (Messling et al. 2015, Whittle et al., 2012; Clayton et al., 2014). Our analysis suggests that moderately affected households may be more willing to engage in private climate protection measures after experiencing an extreme weather event, although we acknowledge that this effect is relatively small in economic terms. Significantly, the analysis also suggests that as damage increases to relatively high levels, the motivating effect of flood experience may diminish. This may be due to financial constraints as a result of high damage and the flooding disproportionally affecting low income districts, and/or due to psychological responses.

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Supplemental Material

Pre-treatment analysis by regression and temporal persistence of the effect

The assumption of parallel trends before the treatment can be assessed by regressing y_{it} on a series of interactions of month dummies with the treatment variables (Autor 2003; Pischke 2005). Basically, the time dummy T_t which indicates pre- and post-treatment observations in equation 2 is split up in several month dummies. The resulting estimation model is the following:

$$y_{it} = \sum_{m=1}^{36} (\gamma_{1m} M_m D_i^{mod} + \gamma_{2m} M_m D_i^{high}) + \delta X_{it} + \mu_i + \vartheta_t + \varepsilon_{it} \quad t, m = 1, \dots, 17, 19, \dots, 36$$

The variable M_m is an indicator variables which equals one if $m = t$ and zero otherwise. Then the estimated coefficients γ_{1m} and γ_{2m} capture possible differences in the time trends for each month. Given fully identical pre-treatment trends, all 34 estimated treatment effects before June 2013 (γ_{1m} and γ_{2m} for $m = 1, \dots, 17$) should be insignificant. For *green_last*, there is one significant interaction term before the flood (see Table S4). In the cases of *green_first* and *green_one,* four out of 34 interaction terms are significant ($p<0.1$). Hence, the number of significant coefficients seems sufficiently small to conclude that they may be significant by pure chance. Note that due to multi-testing the probability that four out of 34 tests are significant on the 10% level is quite high, even if there is actually no true relationship. Moreover, the significant treatment effects before June 2013 concentrate on February, March and June 2012, and January/February 2013. Hence, there are no significant treatment effects shortly before the flood. From this exercise we can conclude that there is no significant hint for pre-treatment differences of y_{it}.

The same regression may be used to inspect the temporal persistence of the estimated treatment effect. While we have focused on the pre-flood months for the pre-treatment analysis, we may also consider the post-flood months for analyzing the temporal pattern of the estimated post-treatment effects. The results (included in Table S4) show that the monthly treatment effects are almost always positive, but not always significant. The effects start to be significant in month three after the event, which is broadly in line with a similar temporal pattern of the effect of cyclone experience on Google searches related to climate change (Lang and Ryder, 2016).

Placebo test and robustness checks

In this section, we present a series of robustness checks and a placebo test to assess the stability of the baseline regression result, i.e. a significant effect of moderate flood damage on green electricity interest, while high flood damage shows no effect. Summary results of the robustness checks, numbered from I to XXI, are accessible in Table S3.

As a placebo test, we estimate a series of regressions with the month of the treatment varying from February 2012 to November 2014. The estimated treatment effects should be most significant for estimations with treatments defined around June 2013, the month of the real flood event. The joint significance of the estimated treatment effects indeed reaches a maximum if the event is defined at August 2013. This makes us confident that the event triggering the measured effect happened in mid-2013 and had an impact lasting for some months.

As mentioned in section 3.2, we focus on the dependent variable *green_last*. Re-running the baseline-regression with *green_first* (robustness check no. I) and *green_one* (II) as dependent variables yields similar results in terms of direction and significance of the treatment effects. In terms of overall model fit, the alternative models perform even slightly better. The dependent variable may also be defined as the number of searches for green electricity per citizen in the district (III). The main results stay robust.

In the baseline, the treatment was defined by two dummy variables indicating districts with moderate and high damage intensities, with low damage as the reference. As the data originally capture flood intensity in nine categories, we may introduce alternative group thresholds and a higher number of categories in the regression. In Table S3, we present regressions based upon an alternative threshold for separating moderately and severely affected districts (IV) and including all nine damage categories (V), confirming the baseline results. We also take the midpoints of each category and treat these data as continuous (VI). However, the resulting distribution of the flood intensity variable is highly right-skewed, and the treatment effect estimates become non-significant. Using log-normalised values of the midpoints reduces the skewness of the distribution, and the baseline results are confirmed (VII).

As shown in Table 1, some of the variables used in the regression are subject to large variation and contain relatively extreme values (such as *number* or *consumption*). Therefore, we re-estimate the baseline regression excluding outlier observations (VIII). Outliers are defined by bottom and top percentiles of *green_last*, *consumption*, *number* per citizen in district, and bottom percentile of *number*. In another specification, we exclude the observations from the district "Hamburg" which contain some seemingly erratic fluctuations in the dependent variables and number of requests (IX). Furthermore, we exclude all districts from the six federal states where no single district suffered moderate flood damage, in order to keep only relatively similar districts in the sample (X). The main results stay robust.

Some of the covariates could be affected by the treatment themselves. In this case, the causal interpretation of the estimated treatment effect is no longer warranted (bad control problem raised by Angrist and Pischke 2009). Although we do not expect that our covariates are significantly affected by flood intensity, we re-estimate the baseline regression without them (XI). As expected, the overall model fit decreases slightly, but the main results (a significant effect for moderately-affected districts and no effect for severely-affected districts) stay qualitatively identical.

To assess the role of unobserved heterogeneity, we re-estimate the baseline regression including controls, but without district-fixed effects (XII). The treatment effects become non-significant, which signals the importance of unobserved district heterogeneity for the baseline regression. Fixed effects may also be defined on smaller spatial units, such as communities or zip-code-levels. A model with fixed effects for 11,981 zip-code-community combinations confirms the results of the baseline model (XIII).

Our dependent variable condenses information from a number of search requests in a district-month to one mean value. The higher the absolute number of search requests in a district-month (i.e. *number*), the more reliable this mean value should be. This gives rise to regressions weighted by *number* (XIV), or by the population size in the districts (XV). Both weighting procedures yield similar results as the baseline. Another procedure to circumvent possible effects of the aggregation process is the usage of the original request data, without aggregating them on any spatial unit (XVI). The drawbacks of this approach are that (a) panel estimation techniques are no longer possible (there is no unique identifier on the household level), and (b) we have to assume that the flood damage was equally distributed on all inhabitants within a district. Nevertheless, a DiD estimation, augmented by dummy variables for zip-code areas and time indicators, confirms the previous results.

Regarding the clustering of standard errors, there are good arguments for using clusters at the district level (as done in the baseline). Flood emergency measures and flood alerts are organized by this administrative unit. However, larger-scale flood protection measures and financial disaster relief are normally organized by the 16 federal states. We therefore include a regression with standard errors clustered at the state level (XVII). The flood effect on moderately affected districts remains highly significant.

In the baseline, we assess the effect of the major riverine floods in Eastern and Southern Germany, which triggered high medial and political attention. However, in the same month a heavy rain event (named "Norbert") caused lower financial damage mainly in Western Germany (GDV 2016). In the baseline, we do not control for the damage induced by "Norbert" to show the marginal effect of the high-profile riverine flood event. If we include the damage of "Norbert" into the flood intensity variable (XVIII) or exclude the districts that were affected by "Norbert" from the analysis (XIX), the results stay robust.

Next, we test for the possibility that the standard errors of the estimated treatment effects may underestimate the true standard deviation due to autocorrelation (Bertrand et al. 2004). The suggested correction of collapsing the available time periods to one pre- and one post-period yields similar results as the baseline regression (XX). Finally, we take account of the fact that one month of the pre-treatment period showed a significant treatment effect (see section on pre-treatment analysis in the appendix) and exclude the first six months from the pre-treatment period (XXI). Again, the main results stay robust.

Tables

Table S1: Household surveys analysing experience effects on individual climate action and their main results.

Study	Location and sample size	Experience measure	Climate action measure	Main result
Zahran et al. 2006	USA, N=511	Injuries and fatalities from natural hazards in county of residence	Stated support for various climate policies	Positive relation in OLS regression ($p<0.1$)
Whitmarsh 2008	Two flood-affected cities in England, N=589	Reported experience of flooding in the last 5 years	Stated "action out of concern for climate change"	No significant correlation in logistic regression
Li et al. 2011	USA, N=251	Perceived temperature deviation at current day	Revealed donation to climate charity	Positive relation in OLS regression ($p<0.1$)
Spence et al. 2011	Great Britain, N=1,822	Reported personal experience of flooding in local area	Stated preparedness to "greatly reduce my energy use to help tackle climate change"	Positive indirect effect in mediation model ($p<0.05$)
Bichard & Kazmierczak 2012	Flood-prone areas in England and Wales, N=826	Reported personal experience of flooding	Reported current implementation of energy-saving measures and stated interest to install them in future	No significant correlations in Mann-Whitney test
Haden et al. 2012	162 farmers in Yolo county, California, US	Perceived changes in local water availability and summer temperature	Stated likeliness to adopt renewable energies or measures to reduce energy usage	No effect of perceived temperature changes, positive indirect effect of perceived water availability changes in mediation model ($p<0.05$)

Table S1 (continued)

Study	Location and sample size	Experience measure	Climate action measure	Main result
Broomell at al. 2015	24 countries, most of them OECD, N=11,614	Reported personal experience of global warming	Stated endorsement of general climate action and of three specific actions	Positive relation in OLS regression (p<0.01)
Demski et al. 2017	UK, nationwide and flood-affected regions, N=1,137	Reported experience of flooding in winter 2013/2014	Stated likeliness to engage in individual climate action and policy support	Positive indirect effect in mediation model (p<0.01)
Hamilton-Webb et al. 2017	200 farmers in Gloucestershire, UK	Reported personal experience of flooding	Stated current and future adoption of common climate change mitigation practices	No significant correlation between type of experience and mitigation response
Ogunbode et al. (2017)	Great Britain, N=1,048, same sample as Spence et al. (2011)	Reported personal experience of flooding in local area	Stated preparedness to "greatly reduce my energy use to help tackle climate change"	Positive indirect effect only for left-leaning voters (p<0.05)
Ung et al. (2018.)	Coastal Combodia, N=1,823	Reported experience with climate hazards (floods, storms, droughts)	Stated reduction of household energy consumption	Positive relation in OLS regression

Table S2: Spearman correlation coefficients of dependent variables, treatment variables and covariates. N=14,472.

		1	2	3	4	5	6	7	8	9
1	*green_first*	1.00								
2	*green_last*	0.81	1.00							
3	*green_one*	0.83	0.97	1.00						
4	*damage*	-0.01	-0.01	-0.01	1.00					
5	*income*	0.11	0.23	0.22	0.05	1.00				
6	*unemp*	-0.09	-0.15	-0.15	-0.22	-0.69	1.00			
7	*old*	-0.21	-0.30	-0.29	0.04	-0.35	0.51	1.00		
8	*consumption*	0.07	-0.01	0.00	-0.06	0.17	-0.36	-0.13	1.00	
9	*number*	0.25	0.21	0.21	0.01	0.04	0.04	-0.25	0.03	1.00

Table S3: Summary of robustness check results

No.	Description	Treatment effects [a]	Remarks
	Baseline	0.0045*** 0.0004	See section 4.1.
I	Dependent variable: *green_first*	0.0040*** -0.0007	The positive effects of *income* and *old* and the negative effect of *unemp* become significant ($p<0.1$).
II	Dependent variable: *green_one*	0.0050*** 0.0002	The negative effect of *consumption* becomes significant ($p<0.1$).
III	Dependent variable: Number of searches for green electricity per citizen in district	$9.42e^{-6}$ * $9.41e^{-6}$	The positive effect of *income* and the negative effect of *unemp* become significant, there are positive effects of *number* and district population ($p<0.05$).
IV	Treatment variable: Alternative threshold between moderately and severely affected districts	0.0046*** -0.0036	Nineteen districts are shifted from the highly affected to the moderately affected category. Hence, 268 districts are classified as lightly damaged, 113 as moderately damaged and 21 (5.2%) as highly damaged.
V	Treatment variable: Nine instead of three categories of flood intensities	0.0013 0.0029 0.0059*** 0.0045*** 0.0054*** -0.0035 -0.0034 0.0035**	Column 2 presents the estimated treatment effects if all nine damage categories of the raw data are used (reference: lowest category). The numbers of districts in each of the nine categories are: 211, 29, 28, 37, 57, 19, 13, 7, 1.
VI	Treatment variable: Midpoints of each category, quadratic regression to estimate non-linear effect	$4.02e^{-6}$ $-3.60e^{-9}$	Column 2 presents the estimated treatment effects for the simple and the quadratic value of the treatment variable. The treatment variable is extremely right-skewed (skewness: 4.21).
VII	Treatment variable: Log of the midpoints of each category, quadratic regression to estimate non-linear effect	0.0052*** -0.0006***	Column 2 presents the estimated treatment effects for the simple and the quadratic value of the treatment variable. The skewness of the treatment variable decreases to 0.71. The effect of *income* becomes significant ($p<0.1$).
VIII	Excluding outliers	0.0044*** 0.0011	N decreases to 13,515. The positive effect of *income* and the negative effect of *number* become significant ($p<0.05$).
IX	Excluding district “Hamburg”	0.0045*** 0.0004	N decreases to 14,035. The negative effect of *number* gets significant ($p<0.01$).
X	Excluding federal states without moderately affected districts	0.0038** -0.0002	N decreases to 11,585. The following federal states are dropped: Hamburg, Bremen, Nordrhein-Westfalen, Saarland, Berlin, Mecklenburg-Vorpommern.
XI	Without covariates	0.0041*** -0.0000	

Table S3 (continued)

XII	Without district-fixed effects (pooled OLS regression)	0.0017 -0.0008	In this specification, we can add time-invariant variables such as the share of green voters in the general elections in 2009 and the share of female citizens (both taken from BBSR 2017). The results show that both variables are positively associated with interest in green electricity ($p<0.01$). The variable *old* has the intuitive negative sign ($p<0.01$).
XIII	Fixed effects on the community- and zip-code-level instead of district-level	0.0023*** -0.0010	Fixed effects on the smallest available spatial unit (11,981 units). N increases to 268,559.
XIV	Observations weighted by *number*	0.0049* -0.0026	Analytic weighting by *number* as district-month observations with high *number* contain more reliable information. The negative effects of *unemp* and *consumption* become significant ($p<0.1$).
XV	Observations weighted by district population	0.0043** -0.0016	Analytic weighting by population as observations from large districts contribute more relevant information. The negative effect of *unemp* and the positive effect of *income* become significant ($p<0.1$).
XVI	No spatial aggregation	0.0037** -0.0004	Including dummy variables for 14,863 zip-code areas. N increases to 9,704,660.
XVII	Clustering standard errors at the federal state level	0.0045*** 0.0004	The positive effect of *income* becomes significant ($p<0.05$).
XVIII	Adding damage of heavy rain event "Norbert" to treatment variable	0.0034** 0.0015	Adding of "Norbert" damage shifts 43 districts from "low damage" to the "moderate damage" group. One district from the "low damage" and two districts from the "moderate damage" group are now classified as highly damaged. The new percentile thresholds are: 56; 89.
XIX	Excluding districts affected by heavy rain event "Norbert"	0.0041** 0.0012	113 of 402 districts are excluded. N decreases to 10,115. The negative effect of *number* and the positive effect of *old* become significant ($p<0.05$).
XX	Collapsing the panel data to one pre- and one post-treatment period	0.0043*** 0.0004	N decreases to 804. The negative effect of *number* becomes significant ($p<0.1$)
XXI	Shortening the pre-treatment time period to exclude months with different time trends	0.0036*** 0.0004	N decreases to 11,658. The number of months decreases to 29. The effect of *unemp* becomes positive ($p<0.01$).

a) Unless otherwise indicated, the first entry is the estimated treatment effect of moderate flood damage; the second refers to the estimated effect of high damage. The stars (*, **, ***) denote significance levels of 10, 5, and 1%, respectively.

Table S4: Monthly treatment effects (γ_{1m} and γ_{2m} for $m = 1, \ldots, 17, 19, \ldots, 36$)

Month (m)	Month before flood	Pre-treatment effects			Month (m)	Month after flood	Post-treatment effects	
		γ_{1m}	γ_{2m}				γ_{1m}	γ_{2m}
1	-17	-0.0047	-0.0048		19	1	0.0020	-0.0006
2	-16	-0.0051	-0.0023		20	2	0.0040	0.0018
3	-15	-0.0053	0.0018		21	3	0.0059**	0.0017
4	-14	0.0033	0.0004		22	4	0.0036	0.0002
5	-13	0.0036	0.0028		23	5	0.0044*	-0.0004
6	-12	-0.0063*	-0.0004		24	6	0.0023	-0.0001
7	-11	-0.0042	-0.0035		25	7	-0.0002	-0.0036
8	-10	-0.0020	-0.0009		26	8	0.0057**	0.0019
9	-9	0.0005	0.0029		27	9	0.0088**	0.0047
10	-8	0.0004	0.0013		28	10	0.0025	0.0023
11	-7	0.0005	-0.0004		29	11	-0.0003	0.0014
12	-6	0.0025	0.0007		30	12	0.0013	0.0018
13	-5	0.0039	0.0022		31	13	0.0092**	0.0021
14	-4	0.0025	0.0034		32	14	0.0045	0.0019
15	-3	0.0019	0.0022		33	15	0.0052*	0.0016
16	-2	0.0021	0.0004		34	16	0.0069*	0.0011
17	-1	0.0028	-0.0027		35	17	0.0023	-0.0025
					36	18	0.0054	-0.0033

The stars (*, **, ***) denote significance levels of 10, 5, and 1%, respectively. The left panel presents monthly treatment effects in the pre-treatment period, the right panel the temporal pattern of the treatment effects in the post-treatment period. Covariates and district-level and month-fixed effects are included but not reported. Number of observations: 14,472. Number of districts: 402. Number of months: 36.

Table S5: Treatment heterogeneities regarding prior flood experience.

Source of heterogeneity: Flood damage in 2002 ($dam02_i$)		Split samples		Treatment effect interactions
		No flood damage in 2002	Flood damage in 2002	
Treatment effects	$D_i^{mod}T_t$	0.0042**	0.0054***	0.0036*
	$D_i^{high}T_t$	0.0058**	-0.0031	0.00581**
	$dam02_i D_i^{mod}T_t$	-	-	0.0004
	$dam02_i D_i^{high}T_t$	-	-	-0.0018**
	$dam02_i T_t$	-	-	-0.0000
Covariates (X_{it})	income	0.1609*	0.0634	0.1390*
	unemp	-0.0004	-0.0017*	-0.0008
	old	0.0066	-0.0015	0.0042
	consumption	-2.67e^{-6}	8.95e^{-7}	-1.98e^{-6}
	number	-1.38e^{-6}	-1.92e^{-6}	-1.90e^{-6}
	constant	-1.2469*	-0.3533	-1.0317*
Fixed effects	for 35 months	Yes	Yes	Yes
	for 402 districts	Yes	Yes	Yes
Number of observations		10,045	4,025	14,070
Number of districts		287	115	402

*, **, *** denote significance levels of 10, 5, and 1%, respectively. The heterogeneity variable $dam02_i$ is the logarithm of average flood damage per existing flood insurance policy in district *i* due to the flood event in August 2002. As we use a continuous variable in column 3, the coefficients of the interaction terms do not directly correspond to the differences between column 1 and 2. Following the estimation routine of STATA, the constant is the average value of the fixed effects.

Table S6: Comparison of pre-flood income levels in the three damage categories.

Damage category	Mean income in 2009-2012 (ln of available monthly income in €)	Difference to districts with low damage (baseline)[a]
Low damage (268 districts)	7.3826	---
Moderate damage (94 districts)	7.3989	+0.0163 (n.s.)
High damage (40 districts)	7.3325	-0.0501***

a) Significance levels of the differences are based on a Wilcoxon Ranksum Test of identical means. *, **, *** denote significance levels of 10, 5, and 1%, respectively.

Figures

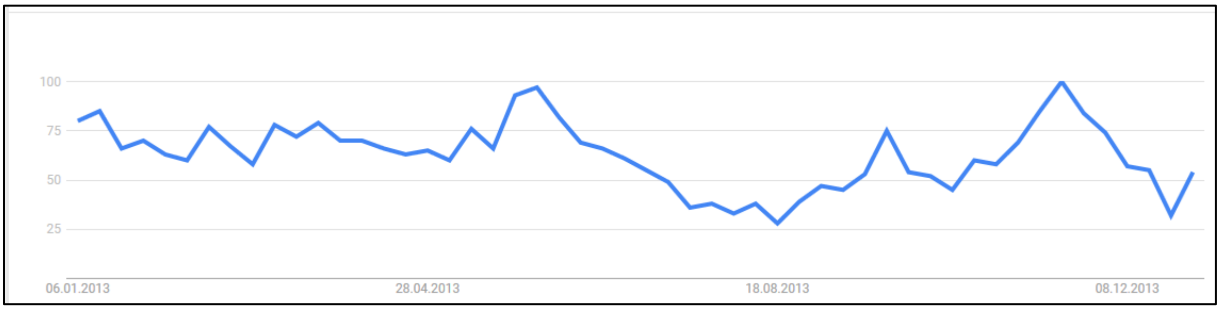

Figure S1: Interest in search term "Klimawandel" (climate change) on Google web search in Germany in the year 2013. The first peak in early June occurs at the same time as the onset of the flood event. The last peak in November depicts the final week of the UNFCCC conference of the parties in Warsaw. Source: Google trends, https://trends.google.de/trends/explore?date=2013-01-01%202013-12-31&geo=DE&q=klimawandel.

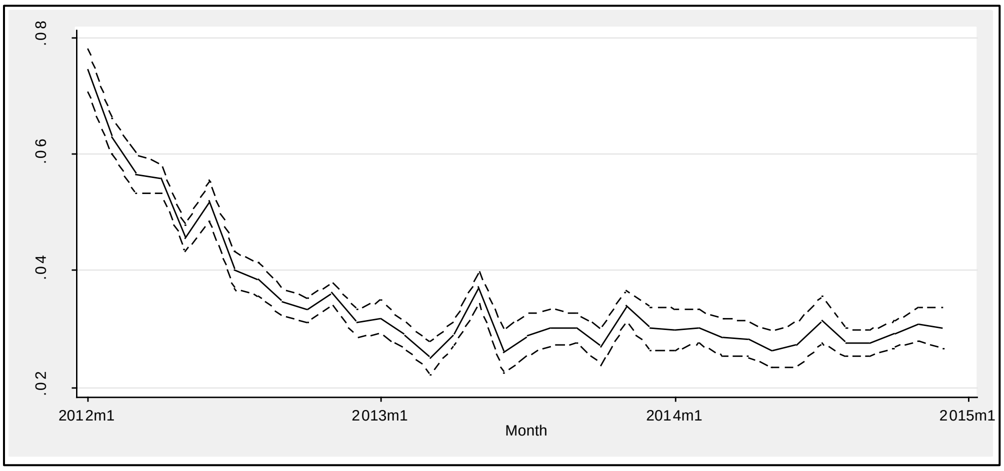

Figure S2: Share of online sessions with filter for green electricity offers in the last request (green_last), 36 months in 2012-2014. The solid line depicts the mean for all districts. The dashed lines depict the mean plus (minus) two standard deviations.

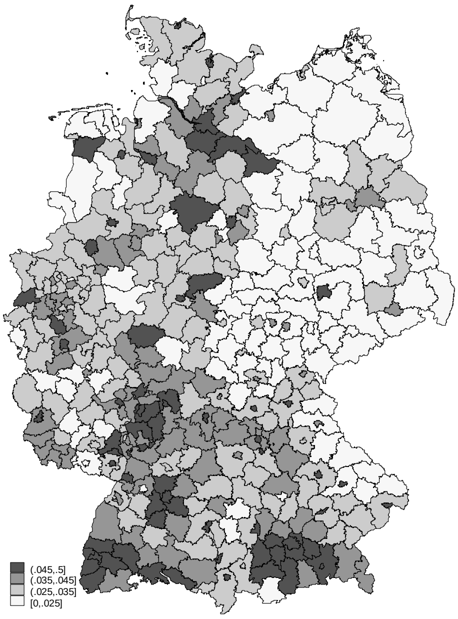

Figure S3: Share of online sessions with filter for green electricity offers in the last request (green_last) in the 402 districts, average for 2012-2014. Map based on GK3 projection, © GeoBasis-DE / BKG 2019.

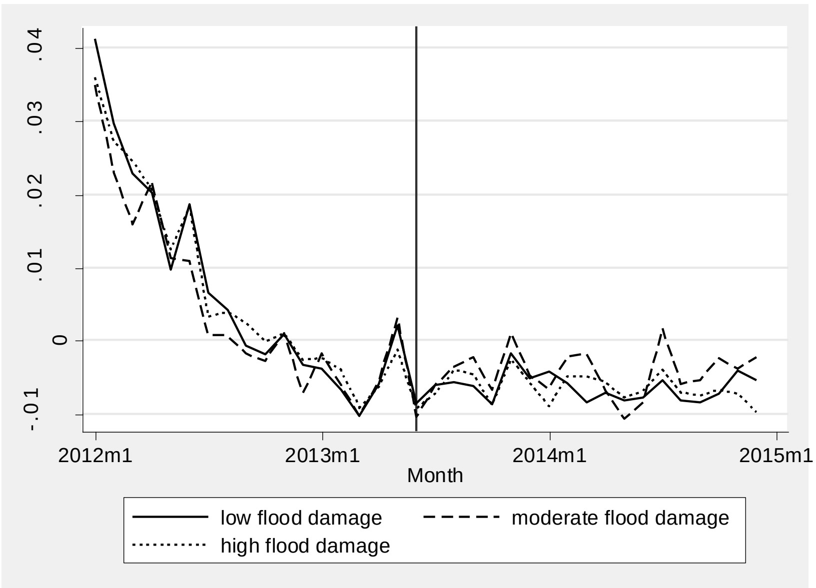

Figure S4: Time trend of the residuals of *green_last* (after the fixed effects transformation), for districts with low flood damage (damage below 67th percentile), moderate damage (damage between 67th and 90th percentile) and high damage (damage above 90th percentile). The vertical line marks the month of the flood event (June 2013).

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