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Do Flood and Heatwave Experiences Shape Climate Opinion? Causal Evidence from Flooding and Heatwaves in England and Wales

Paul M. Lohmann[1,2] · **Andreas Kontoleon**[2,3]

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Abstract

Understanding how personal experience of extreme weather events raises awareness and concern about climate change has important policy implications. It has repeatedly been argued that proximising climate change through extreme weather events holds a promising strategy to increase engagement with the issue and encourage climate change action. In this paper, we exploit geo-referenced panel data on climate change attitudes as well as natural variation in flood and heatwave exposure in England and Wales to estimate the causal effect of extreme weather events on climate change attitudes and environmental behaviours using a difference-in-differences matching approach. Our findings suggest that personal experience with both flooding and heatwaves significantly increases risk perception towards climate change impacts but has no effect on climate change concern or pro-environmental behaviour, on average. Moreover, the findings indicate that the effect of flooding on risk perception is highly localised and diminishes at greater distances. For heatwaves, we find that the effect on risk perception is driven by the recent salient summer heatwaves of 2018 and 2019. Having experienced both events also significantly increases climate change concern and pro-environmental behaviour, in addition to risk perception.

Keywords Causal inference · Climate change beliefs · Climate change impacts · Difference-in-differences estimation · Extreme weather events · Panel data · Pro-environmental behaviour · Propensity score matching · Public opinion

JEL Classification C23 · D04 · D81 · Q54

✉ Paul M. Lohmann
p.lohmann@jbs.cam.ac.uk

1 El-Erian Institute of Behavioural Economics and Policy, Judge Business School, University of Cambridge, Trumpington Street, Cambridge CB2 1AG, UK

2 Centre for Environment, Energy and Natural Resource Governance, Department of Land Economy, University of Cambridge, Cambridge, UK

3 Department of Land Economy, University of Cambridge, Cambridge, UK

1 Introduction

The UK has set itself ambitious climate targets and strives to be an international leader in climate change policy.[1] However, in order to reach these objectives, pervasive behavioural and societal changes as well as widespread public support for increasingly ambitious mitigation and adaptation policies will be required. Despite widespread belief in the existence of climate change and emerging climate activism, climate change remains for many people a psychologically distant issue (Steentjes et al. 2017). Psychological distance refers to the belief that climate change is occurring in geographically distant regions, happening further into the future, and affecting different social groups (Spence et al. 2012; Taylor et al. 2014b). A closer look at specific attitudes towards climate change shows that some scepticism and uncertainty remain amongst the UK population, especially with respect to potential direct and personal impacts (Hagen et al. 2016; Taylor et al. 2017). A lack of personal relevance and perceived risk due to psychological distance has been identified as a major threat to public engagement around the issue. However, it has been postulated that highlighting the proximal consequences of climate change may increase engagement and motivation to act upon climate change (Loy and Spence 2020; Demski et al. 2017; Leviston et al. 2014; Reser et al. 2014; Spence et al. 2011).

The appeal of proximising climate change as a policy tool to motivate action, engagement and buy-in has sparked interest into whether personal experience with extreme weather events is related to heightened awareness and concern around climate change. Following recent advances in attribution science, there is mounting evidence that anthropogenic warming is linked with increasing intensity, frequency and duration of extreme weather events around the globe (IPCC 2021). In the UK, future heatwaves and flooding pose a particular threat to individuals and the economy (Slingo 2021). Climate projections suggest that summer heatwaves could occur every other year by the mid-21st Century (Slingo 2021) and temperatures exceeding 40°C could be reached every three-and-a-half years by 2100 (Christidis et al. 2019), posing a significant threat to public health. Moreover, expected annual damages from flooding could nearly double by 2050, if warming follows a 4°C pathway (Sayers et al. 2020). However, processing abstract statistical information on the risks associated with future climate change impacts is cognitively demanding and requires substantial effort. In contrast, experiential learning is intuitive and involves rapidly occurring affective, associative and automatic processes (Ogunbode et al. 2020). Moreover, experiencing extreme weather events plausibly attributable to climate change can increase the saliency of negative consequences for a specific place that people care about, increasing personal relevance and perceived risk and eliciting a state of aversive arousal (Brügger et al. 2015).[2] This, in turn, should motivate private adaptation and mitigation behaviour as well as increased support for government policies.

This compelling argument, founded in psychological and economic theories, has inspired both empirical and experimental research into the proposed relationship between ‘Climate Change Proximity’ and beliefs and engagement.[3] However, the existing empirical evidence is mixed (Howe et al. 2019; Howe 2021; Sisco 2021). Many empirical papers

[1] https://www.gov.uk/government/news/uk-enshrines-new-target-in-law-to-slash-emissions-by-78-by-2035.

[2] Aversive arousal refers to an unpleasant emotional state arising from the outlook of negative impacts for a certain place, or people implicated by that place, as a result of climate change.

[3] See Howe et al. (2019) and Sisco (2021) for reviews of empirical evidence and Schuldt et al. (2018) for experimental research. See Brügger et al. (2021) for a review of psychological processes underlying the association between extreme events and beliefs.

looking at real-world climate proximity (i.e., directly experiencing extreme weather events) have suffered from methodological drawbacks and thus have not been able to establish a causal link (Howe 2021). The lack of regional disaggregation in climate change opinion data and the reliance on correlational research designs have been identified as some of the most common pitfalls of past studies (Howe 2021; Marquart-Pyatt et al. 2014; McCright et al. 2016). Another key issue relates to potential selection bias arising from residential sorting, which has been insufficiently addressed in the extant literature (Howe 2019).[4] While most of the previous work has focused on finding any detectable relationship between extreme weather events and climate change attitudes, only few studies are able to provide insights into when and how personal experience has an impact (Brügger et al. 2021). Moreover, the primary focus of the existing literature has been on climate change attitudes and beliefs, while less is known about how behavioural outcomes (such as pro-environmental behaviours) respond to extreme weather experiences. Ultimately, changes in individual pro-environmental behaviour will play an important role in tackling climate change (Steg 2018).

In this paper, we investigate whether individuals' climate change risk perceptions, beliefs and pro-environmental behaviour change after they have experienced extreme weather events, specifically, flooding and heatwave events which affected large parts of the UK between 2009 and 2020. We exploit the geographic variation in flood and heatwave exposure combined with a propensity score matching and differences-in-differences identification strategy to estimate the causal effect of extreme event experience on three important domains of climate change attitudes: (1) risk perceptions towards future climate change impacts, (2) climate change concern and (3) self-reported pro-environmental behaviour. We utilise climate change opinion data from the UK Household Longitudinal Survey (UKHLS), a large-scale UK household panel survey covering approximately 40,000 households. For this project, we were granted access to the 'secure access' version of the dataset (SN 6676), which provides geo-referenced location information for survey participants (University of Essex 2020). This allows us to spatially link individuals' exact household locations with high resolution flood outlines and temperature grids.

This paper aims to address several important gaps found in the relevant literature. First, we utilise a difference-in-differences (DID) identification strategy to provide causal evidence for the relationship between extreme weather events and climate change attitudes in the UK. We strengthen our causal identification by introducing a complementary propensity score matching approach to minimise selection bias from unobserved residential sorting. Second, we present one of the most spatially precise analyses to date, by drawing on geo-referenced individual-level climate change opinion data, allowing us to observe the exact household location of each survey respondent. We establish extreme event exposure by linking this data with high-quality spatial data of flood events and high-resolution temperature grids using GIS techniques. Moreover, the panel structure of our opinion data allows us to control for unobserved individual characteristics (e.g. personality traits), which may be important determinants of climate change perceptions. Third, we improve on previous research by exploring a nuanced set of questions spanning three important dimensions of climate change attitudes: (1) climate change risk perceptions, (2) climate change concern and (3) self-reported pro-environmental behaviour. Our spatially detailed analysis

[4] In this context, selection bias occurs if individuals self-select into or away from areas which are more likely to experience extreme events. If people that live within proximity to extreme weather events systematically differ from the comparison group (i.e., people living further away), causal inference from both cross-sectional and longitudinal designs can be limited.

allows us to provide some novel insights into how and under what circumstances personal experience can have an impact on these outcomes. Finally, our study focuses on the two types of extreme weather events most relevant in the UK context, namely flooding and heatwaves. Our findings thus offer interesting insights into how attitudes and behaviour might respond to increasingly frequent weather events in the UK and give rise to important policy implications.

We show that, on average, personal experience with both flooding and heatwave events increase climate change risk perceptions but have no robust effect on climate change concern and stated pro-environmental behaviour. Moreover, we document a proximity effect for flooding and a frequency effect for major heatwaves. The closer a flood occurs to a household, the more pronounced its effect on risk perceptions. Moreover, individuals who experienced both the 2018 *and* 2019 summer heatwaves reported elevated climate change concern and pro-environmental behaviour, which has important implications given the increasing frequency of extreme heat events in the UK.

2 Literature and Mechanisms

2.1 Related Literature

A growing body of social science literature is interested in the link between personal experience with climate change variations and attitudes towards climate change. Numerous studies have assessed the relationship by linking spatially disaggregated opinion data with objective weather data (Howe et al. 2019; Sisco 2021). Climate parameters under investigation have included long-term climatic patterns and trends (Shao 2017) as well as seasonal, monthly and daily temperature anomalies relative to a statistically constructed baseline (Bohr 2017; Deryugina 2013; Marlon et al. 2021; Shao 2016; Bergquist and Warshaw 2019). A related strand of literature has produced ample evidence for a link between climate change beliefs and short-run weather fluctuations, which has been termed the "local warming effect" (Joireman et al. 2010; Damsbo-Svendsen 2020; Zaval et al. 2014). The latter effect refers to the phenomenon that individuals are more likely to believe in the existence of global warming if interviewed on a hot day, in contrast to cold days. The majority of studies find that immediate and salient local weather conditions directly influence people's beliefs (Sugerman et al. 2021).

A further group of studies focuses specifically on how personal experience of extreme weather events relate to climate change beliefs, concerns and risk perceptions.[5] In contrast to long-term temperature trends (which are difficult to detect) and short-term temperature fluctuations (which do-not accurately represent a changing climate), extreme weather events are often perceived as embodying highly salient physical manifestations of anthropogenic climate change which may be more easily attributable to climate change. Past research has primarily focused on the US and largely exploits the exogenous variation in extreme events as a form of natural experiment. The majority of these studies find a positive yet moderate effect of extreme weather phenomena on beliefs and attitudes, which diminishes with time (Albright and Crow 2019; Carlton et al. 2016; Dai et al. 2015; Deng

[5] Extreme weather events are commonly defined as significant unusual weather phenomena that have sufficient intensity to cause damages and/or disruption (Konisky et al. 2016).

et al. 2017; Konisky et al. 2016; Ray et al. 2017; Sisco et al. 2017; Zanocco et al. 2019; Hazlett and Mildenberger 2020).

In the European context, research has primarily focused on heatwave exposure (Frondel et al. 2017; Larcom et al. 2019) and extreme flooding events (Demski et al. 2017; Frondel et al. 2017; Osberghaus and Fugger 2022; Spence et al. 2012; Whitmarsh 2008). Research assessing the link between flood experience and climate change beliefs in the UK has produced somewhat mixed results. Early studies in the UK found that flood experience did not significantly affect climate change belief (Whitmarsh 2008). Later work by Spence and colleagues 2012 found that flood experience was positively related to the willingness to save electricity. Relatedly, flood experience has been linked to higher flood risk perception (Frondel et al. 2017) as well as household mitigation and adaptation behaviour (Osberghaus 2017; Osberghaus and Demski 2019). In a case-study of the severe 2013/2014 UK winter floods, Demski et al. (2017) found further evidence for heightened climate change concern and agency amongst flood victims, using subjective flood experience data. More recently, two case-studies in Germany have found that flood experience leads to heightened climate change concern (Osberghaus and Fugger 2022) and may even encourage climate change engagement (Osberghaus and Demski 2019). In contrast, heatwave exposure has been shown to make climate change more salient (Frondel et al. 2017; Taylor et al. 2014a), but has no effect on pro-environmental behaviour (Larcom et al. 2019).

A recent working paper by Rüttenauer (2021) explores the effect of both flood and heatwave exposure on climate change belief and behaviour using data from the UK. The author concludes that experiencing extreme weather events is associated with an increase in climate change belief, but has no effect on pro-environmental behaviour. While this study utilises individual-level panel data linked with objective measures of extreme weather events, it does not systematically account for a range of potential endogeneity problems such as residential sorting both prior and during the study period. One challenge with estimating the causal effect of extreme event exposure on climate change beliefs is that they do not occur randomly across geographic locations. While this is an obvious limitation for cross-sectional designs, it may also be of concern in longitudinal (DID) designs. If unobserved residential sorting leads to systematic differences between treatment and control groups, this may potentially violate the assumption of parallel trends, crucial to empirical identification (Bakkensen and Ma 2020). Furthermore, Rüttenauer (2021) explores only a subset of climate change attitudes collected in the survey data it uses. The UKHLS provides a host of additional climate change perceptions questions, which we utilise in full to construct an index of climate change concern. Finally, Rüttenauer (2021) relies on population weighted centroids of small-area geographical units as a proxy for participants' household location when assigning individuals to treatment and control groups. As our analysis will show, the effect of flood exposure is highly sensitive to flood proximity, suggesting that inaccuracies in participants' locations may weaken internal validity. In contrast, our 'secure access' dataset allows us to observe individuals' exact geographic location, providing the most geographically accurate analysis to date.

Taken together, the review of the recent literature reveals that there is increasing evidence for a link between personal experience and climate change attitudes. However, the wide variety of different research designs, differences in spatial and temporal scales, inconsistencies in measurement of climate change opinions, and the lack of methodological rigour limit the generalisability of the existing body of research (Howe et al. 2019; Howe 2021). Moreover, very few studies have been able to provide evidence about when and how experiences are likely to trigger different types of cognitive, emotional and behavioural responses (Brügger et al. 2021). This study addresses all the previously discussed methodological limitations, which allows us to provide more robust causal insights and additionally explore several potential mechanisms through which personal experience may affect climate change attitudes in the UK.

2.2 Mechanisms

There are several potential mechanisms through which personal experience of weather events could influence climate change perceptions, theoretically founded in both economics and cognitive science. In the first instance, people may update their prior beliefs and behaviour through a Bayesian updating process (Deryugina 2013; Druckman and Mcgrath 2019; Larcom et al. 2019). According to Bayes' Rule, climate change belief is a function of prior beliefs combined with new available information from an observed signal (Holt and Smith 2009). If extreme weather is interpreted as new evidence for climate warming, this should lead to a stable increase in climate change belief, which in turn should decrease uncertainty about climate sensitivity (Kelly and Tan 2015).[6] While Bayesian updating provides a plausible theoretical starting point, there are likely to be numerous complementary and alternative psychological processes that underlie the complex relationship between experience of extreme weather events and climate change beliefs and thereby influence the updating process (Brügger et al. 2021).[7] For instance, experiencing negative affective reactions associated with climate change may be a potential pathway through which personal experience interacts with climate change risk perceptions (Van Der Linden 2014). Moreover, the importance of extreme event experience depends in part on its location, intensity, duration, type and how it is interpreted (Marlon et al. 2018), as well as the degree of cognitive attribution (Ogunbode et al. 2019; Van Der Linden 2014). If no conscious link is drawn between the extreme event and climate change, Bayesian updating will not occur. In addition to this, individuals may engage in directional 'motivated reasoning', by which new evidence is interpreted in such that it maintains one's prior beliefs (Bayes and Druckman 2021; Druckman and Mcgrath 2019).

Moreover, numerous other heuristics and biases may be at work, leading to a departure from the Bayesian updating norm (Charness and Levin 2005; Charness et al. 2007). For instance, people may be subject to an "availability" heuristic, under which they give greater weight to recent salient events when computing the probability of an event to occur (Tversky and Kahneman 1973). Recent research finds support for this hypothesis, showing that short-lived changes in climate change beliefs during major heatwaves are likely to be explained by a salience effect rather than through a Bayesian process of updating (Bordalo et al. 2012; Deryugina 2013; Larcom et al. 2019).

Consistent with the theory of Bayesian Updating and the reviewed literature, we expect personal experiences of flooding and heatwave events to increase risk perceptions (i.e., the perceived likelihood of similar and related future events) and climate change concern. The closer an event occurs to the household, the more personally relevant and consequential its impacts might be (Brügger et al. 2021). To explore the average treatment effects on the treated (ATT), we define "personal experience" based on household location, following standard definitions implemented in the literature (detailed below).

[6] Prior literature suggests that learning to resolve climate uncertainty is a slow process, however, "tail learning" occurs relatively quickly (see Kelly and Tan 2015).

[7] Brügger et al. (2021) review the broader psychological literature and formulate a range of testable hypotheses about when and how experiences are likely to trigger different types of cognitive, emotional, and behavioural responses.

For flooding, we are able to incorporate information on the precise distance between the flood event and each household location, allowing us to explore potential distance decay effects. Despite having clear expectations regarding the direction of the treatment effects, we proceed conservatively by reporting two-sided significance levels throughout the analysis.

While average treatment effects on the treated provide a basis for comparison with the previous literature, we may reasonably expect that greater exposure (along various dimensions) will lead to a larger increase in risk perceptions and climate change perceptions, consistent with a Bayesian process of belief updating (Deryugina 2013). For heatwaves, we may expect that longer heatwave duration is associated with larger changes in climate change beliefs. Longer heatwaves are likely to be perceived as more unusual and hence are more salient than shorter heatwave spells. Moreover, longer heatwave spells may compromise physical well-being, especially for the elderly.

Similarly, more frequent exposure may amplify climate change concerns and risk perception, for which several potential psychological processes may account for. First, the more frequent a certain event, the more likely people are to be personally affected by it and hence notice and remember it. Furthermore, more frequent events may be perceived as more unusual and attribution to climate change may be facilitated by media coverage of the abnormality of recurring events. We, thus, can reasonably expect that greater frequency of extreme events is associated with larger changes in risk perceptions and climate change concern.

While we expect personal experience of extreme weather events to raise climate change concern and risk perception in the immediate aftermath of an event, previous research has suggested that such an effect may be short-lived (Larcom et al. 2019). Consistent with a 'salience effect' we may expect that the effect of flooding and heatwave exposure diminishes the greater the time between the event and the UKHLS interview date. The staggered timing of extreme events and UKHLS survey dates in our data allow us to explore this further.

An open question remains as to whether personal experience with extreme weather events would increase or decrease engagement with pro-environmental behaviour. Individuals who have directly experienced negative impacts (emotional or financial) may be reluctant to adopt effortful behaviours (Brügger et al. 2015). The same line of reasoning argues that intense emotional experiences may either mediate increased concern and action on climate change (Demski et al. 2017) or motivate people to deny and distance themselves from climate change to reduce unpleasant emotions such as anxiety or fear (Hamilton-Webb et al. 2017; McDonald et al. 2015). However, several important pre-conditions must be met for the formation of pro-environmental personal norms and ultimately individual climate actions and policy support, according to the Value-Belief-Norm framework (Stern et al. 1999). First, individuals must have a general awareness of consequences that their actions contribute towards climate change and potential impacts. Second, a mental link must be drawn between the awareness of consequences and the ascription of responsibility for the occurrence of climate change related impacts, or the ability to mitigate such threats through individual actions (Stern et al. 1999). Most recent evidence suggests that the summer heatwave of 2018 had no significant effect on pro-environmental behaviour in the UK (Larcom et al. 2019), whereas flooding in Germany significantly increased interest in green energy suppliers (Osberghaus and Demski 2019). Against this backdrop, our analysis will provide causal evidence on the relationship between extreme weather events and an index of stated pro-environmental behaviours.

3 Data Description and Empirical Approach

3.1 Data

3.1.1 Climate Change Perceptions and Pro-environmental behaviour

Data on climate change attitudes and environment related behaviour come from the UK Household Longitudinal Study (UKHLS). The UKHLS is a large annual household panel survey that follows the lives of approximately 40,000 households in yearly intervals since 2009. A feature of our analysis that differentiates it from other work mentioned in Sect. 2.1 is that we were granted access to the 'secure access' version of the UKHLS dataset (University of Essex 2020), which provides geo-referenced location information for each household. Households are assigned a grid reference (a location to the nearest metre) based on their postcode at the time of the UKHLS interview.

The first, fourth and tenth waves included an additional environmental behaviour questionnaire module which contains a rich set of questions on climate change attitudes, risk perception, as well as individual environmental behaviours.[8] Our empirical strategy does not arbitrarily select some of these questions, but instead utilises all the wealth of information contained in the data. We explore multiple dimensions of climate change attitudes by constructing three outcome variables. First, we measure climate change risk perception based on binary responses to the question: *Do you believe that people in the UK will be affected by climate change in the next 30 years*. Second, we construct an index for climate change concern based on responses to nine questions around environmental and climate change attitudes (e.g., *I don't believe my behaviour and everyday lifestyle contribute to climate change*). As all nine variables are highly correlated, we conducted a factor analysis to predict an underlying 'Climate Concern Factor' for each individual.[9] Finally, we construct an index of environmental behaviour based on self-reported environmental habits including household, consumption and travel behaviours (*e.g., walk or cycle for short journeys less than 2 or 3 miles; Switch off lights in the room that aren't being used*). Respondents indicated how frequently they engaged in each behaviour, ranging from 'never' (1) to 'always' (5), or "not applicable". Variables were recoded so that positive values reflect more pro-environmental behaviour, and the index was calculated by taking the sample average frequency for all behaviours applicable to the individual respondent. An overview of all questions that are used to construct the dependent variables for our empirical analysis are presented in Appendix Table 7.

[8] This three wave (unbalanced) panel allows us to observe individual beliefs and behaviour at three time points, but does not allow standard event study analysis with the full sample due to staggered treatment assignment (i.e. units are treated at varying times during the observation period - prior to the fourth or prior to the tenth wave). The identification challenges from staggered treatment assignment are discussed in Sect. 3.5.

[9] The response format to the environmental attitude questions was changed from a binary response format ('Yes'/'No') in Wave 1 to a 5-point Likert Scale ('strongly agree' to 'strongly disagree') in Waves 4 and 10. To ensure consistency in responses, we restrict our data to a two-period panel of Waves 4 and 10 when exploring changes in climate change concern.

3.1.2 Flooding and Heatwave Data

Flooding data for England and Wales comes from the 'Recorded Flood Outlines' database maintained by the UK Environment Agency[10] and the 'Recorded Flood Extents' dataset published by Natural Resources Wales.[11] Both geospatial datasets contain all records of historic flooding from rivers, the sea, ground and surface water for England and Wales, respectively, providing detailed information on each event, as well as their exact geographic extent. For our matching strategy (discussed in Sect. 3.3.2), we further utilise geospatial flood vulnerability indicators available via the Climate Just Tool (Sayers et al. 2017) and the Environment Agency's 'Risk of Flooding from Rivers and Seas' (ROFRAS).[12] The ROFRAS dataset provides a spatial classification (at 50 m resolution) of flood risk in four categories ('very low', 'low', 'medium' and 'high'), taking into account elevation, local water level and the condition of existing flood defences for a given area.

Temperature data was obtained from the 'HadUK-Grid' dataset maintained by the Met Office and made available via the Centre of Environmental Data Analysis (Hollis et al. 2019). The dataset contains gridded climate variables, interpolated from meteorological station data, at a resolution of 1 x 1 km for the entirety of the UK. To construct heatwave indicators for our analysis, we extracted daily maximum temperature records at the exact household location of each UKHLS participant at the time of the UKHLS interview.

3.2 Treatment Assignment

We use GIS software to identify individual level flood and heatwave exposure by linking the exact household location of each UKHLS participant recorded on the date of the interview with spatial flood and temperature maps. A household is allocated to the flood treatment group if at least one flood (as defined and recorded by the official sources mentioned above) occurred within a 1000-metre radius of its residence in a given 7-year interval prior to the Wave 4 and/or Wave 10 survey date. In our main analysis, a household remains in the treated group after they have experienced an event for the full observation period. The data were mapped and spatially joined using QGIS3.16.0. The spatial-join procedure is displayed in Fig. 1. If a flood outline intersects the 1-km radius surrounding each household location, then individuals of this household are assigned to the treatment group. Additionally, we calculate the smallest distance between the household location and the flood outline using the "Join attributes by nearest" tool. This spatial-join procedure is repeated for each flood outline within the vicinity of the household (e.g., floods that occurred in the same area but in different years). The outlined procedure allows us to identify a clear-cut flood treatment group (i.e., a binary classification into treated and untreated, required for our propensity score matching procedure), while incorporating information on the exact distance between the household location and the flood event.

We utilise all flood outlines recorded between 2007 and 2020 identified by a unique flood event code and their start and end date. When defining treatment assignment, two factors should be taken into account: physical proximity and temporal proximity to the

[10] Downloadable from: https://data.gov.uk/dataset/16e32c53-35a6-4d54-a111-ca09031eaaaf/recorded-flood-outlines.

[11] Downloadable from: https://datamap.gov.wales/layers/inspire-nrw:NRW_HISTORIC_FLOODMAP.

[12] Downloadable from: https://data.gov.uk/dataset/bad20199-6d39-4aad-8564-26a46778fd94/risk-of-flooding-from-rivers-and-sea.

event. For our main analysis, we consider a 1,000-metre radius as our primary treatment definition. For a sensitivity analysis of flood proximity, we expand the treatment radius to 2,000 metres and interact a binary treatment indicator for flood experience within that radius with a continuous variable for the minimum distance to the event. We explore sensitivity to temporal proximity by varying the number of years prior to a given survey date used to identify treatment. In our main analysis, we exclude individuals who had already experienced a flood event within seven-years prior to their first interview, as their treatment status provides no time-varying information for our within-individual analysis. Moreover, by excluding individuals with prior flood exposure, the focus of our analysis lies on individuals for whom floods are particularly novel and distinctive events (i.e. unusual and noticeable) (Brügger et al. 2021).[13] The degree of abnormality or unexpectedness has been found to be a significant predictor of attention to climate-related events (Sisco et al. 2017). It is important to note that only 11 individuals in our data had their household directly located within a flood zone (i.e. at zero metres distance to the flood). While we cannot ascertain the flooding-related damages suffered by the survey participants, it is reasonable to assume that the treatment group is primarily comprised of households which experienced a "near miss" from flooding. Excluding the individuals located directly in the flood zone from our analysis does not significantly alter the results.

Figure 2 displays the GIS treatment assignment procedure for heatwave exposure. The procedure involves mapping the temperature grids at a resolution of 1 x 1 km for the entirety of the UK (left panel) and overlaying the exact household locations of the UKHLS participants (right panel). We use the "Sample Raster Values" tool in QGIS3.16.0 to extract daily minimum and maximum temperature values at each household location.

We define heatwave exposure as having experienced at least three consecutive days with day-time maximum temperatures exceeding 29°C, within three years from the survey date.[14] While no commonly accepted definition of a heatwave in the UK exists, our definition of heatwave experience has been applied in previous empirical research (Larcom et al. 2019). The World Meteorological Organisation defines heat waves as "unusual hot weather (Max, Min and daily average) over a region persisting at least two consecutive days during the hot period of the year based on local climatological conditions, with thermal conditions recorded above given thresholds"(WMO 2018). The definition suggests that heat waves are characterised by their magnitude (temperature or anomaly threshold surpassed), their duration (consecutive days) and their extent (geographical area affected). To explore the nuances of heatwave intensity, we construct an additional measure of heatwave duration, which counts the numbers of consecutive heatwave days experienced.

[13] In the absence of complete information on individuals' geographic location prior to the study period, our identification of pre-treatment is likely subject to some degree of measurement error. Although we risk falsely attributing some households as pre-treated (if they only recently moved to their current location), specifying a larger pre-treatment period is considered to be the more conservative approach. We therefore allow a 7-year pre-treatment interval, consistent with our treatment assignment approach. Our primary approach to treatment assignment is summarised in table 8. In our analysis of temporal proximity, we explore shorter and longer (pre-) treatment intervals, and additionally allow units to "switch back" to the control group if they were not treated again in the respective time-frame prior to the next survey date.

[14] As nearly the entire sample (78%) experienced a heatwave event in 2003 or 2006 (i.e., within 6-years of the first interview date), we specify a shorter (pre-) treatment window for heatwaves of three years.

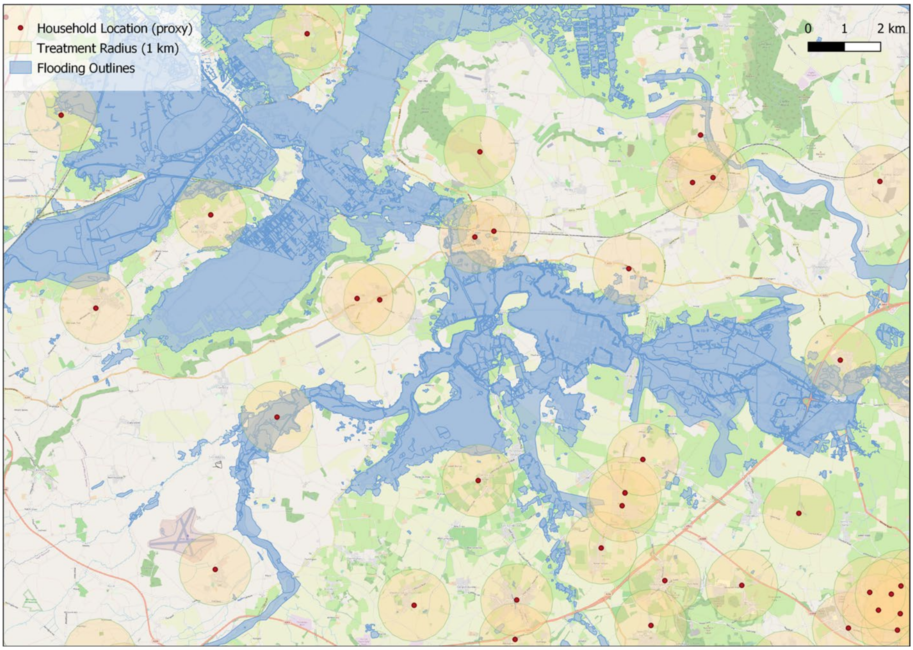

Fig. 1 Treatment assignment flooding - GIS spatial-join procedure. *Note:* Figure displays proxy household locations

3.3 Identification

To identify the causal effect of flood and heatwave experience on our three outcome variables, we rely on the assumption of parallel trends, which implies that the treatment and control groups would follow common trends in outcomes in the absence of treatment. In the absence of additional pre-treatment periods, we are unable to perform standard tests exploring the equality of trends prior to flood and heatwave exposure. Moreover, residential sorting over flood risk poses a challenge for empirical identification (Bakkensen and Ma 2020). Nevertheless, we can take several precautionary ex-ante measures to strengthen the plausibility of the parallel trends assumption in our data. In the following sections, we address several factors which may threaten the validity of our identification strategy and discuss our propensity score matching approach to mitigate concerns about diverging trends between treatment and control groups.

3.3.1 Residential Sorting

A major challenge to empirical identification in this context is residential sorting. Residential sorting happens when individuals self-select into or away from areas which are more likely to experience extreme events. If residential sorting is endogenous to event experience, the effect on climate change perceptions and attitudes is likely to be biased. A first concern relates to residential sorting that occurred during the observation period. For instance, experiencing an extreme event may induce people to move to a different area. To

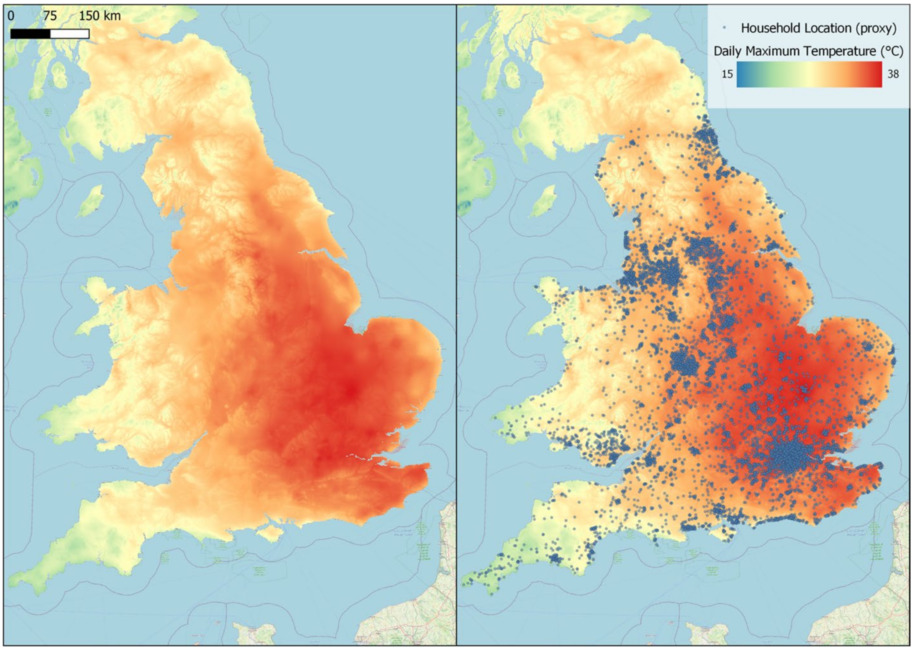

Fig. 2 Treatment assignment flooding - GIS spatial-join procedure. *Note:* Sample temperature gird of maximum temperatures recorded on 25th July 2019. Figure displays proxy household locations

mitigate the threat from residential sorting that occurred during the observation period, we exclude all residents (both treatment and control units) that moved during the observation period.[15]

A second concern relates to residential sorting that occurred prior to the observation period, which is not directly observable. The fact that treatment is not exogenously allocated across space and time may invalidate the assumption that allocation to treatment is independent of potential outcomes. For instance, flooding is much more likely to be experienced by households living near rivers and sea and especially likely for properties constructed on flood plains. If people sort over flood risk, it could be argued that those living in areas more susceptible to flooding are systematically different from people living elsewhere. In support of this argument, recent research shows that flood vulnerability is associated with a range of socio-spatial factors (Sayers et al. 2017) and differs between low and high-income households (Osberghaus 2021). In turn, people willing to live in flood-prone areas may *be* more risk-loving than people who choose to live in safe distance of flood plains. On the other hand, exposure to heatwaves is likely to also be associated with a range of socio-spatial factors. Heatwaves are much more likely to occur in southern regions of the UK and may be especially severe in cities (reinforced by the urban heat island effect) and

[15] To ensure accurate identification of household location for flood exposure, we exclude individuals who moved more than 1,000 m from their initial location during the observation period. We also apply the same approach for heatwave exposure for consistency. About 11.26% of the sample moved during the observation period. Although a more conservative approach may be warranted for flood exposure, households moving less than a kilometre away are considered to remain within the same community and experience flooding similarly.

more moderate close to the coast. While systematic differences in levels between treated and control units pose no immediate threat to the internal validity of our analysis, we may still be concerned that differences may be associated with diverging trends in outcomes for the two groups, which would violate our key identifying assumption. To mitigate this concern, we take several actions. First, we utilise a generalised DID approach with individual fixed effects which account for any individual differences which are constant over time. Second, we employ a propensity score matching approach to identify a subset of control units prior to analysis, which are more comparable to the treatment group and assess the balance of observable baseline characteristics between treated and matched control groups using statistical tests. The details of our matching strategy are discussed next.

3.3.2 Matching Strategy

In our case, we use Propensity Score Matching (PSM) to select a set of individuals from the control group who are 'comparable' to the treatment group, based on observable characteristics. The reasoning goes as follows: by selecting a control group with PSM we minimise any potential bias that may arise from selection into treatment. In the absence of pre-treatment data, we are unable to test for common trends in pre-treatment outcomes between treatment and control groups. However, a key advantage of the nearest-neighbour matching approach is that it narrows down the control group to units which are observationally similar to treated units and thus more likely to follow similar trends (Deryugina et al. 2020).

We construct the matching criteria for our primary definitions of flooding and heatwave exposure using data from multiple sources: First, we use a selection of small-area statistics drawn from the 2011 Census at the Lower Layer Super Output Area (LSOA) level,[16] including population density, unemployment rate, share of income deprived households and the share of elderly people aged over 75. Additionally, we use a set of indicators developed by Sayers et al. (2017) which use 2011 Census data to capture socio-spatial vulnerability across multiple dimensions at the LSOA level.[17] These small-area statistics provide important socio-economic background information at the macro-level, which are predictive of both flooding and heatwave exposure. Second, we use individual baseline characteristics from the UKHLS dataset, including net annual household income, highest attained qualification (education) and housing tenure. As our panel data is unbalanced, we include an additional matching variable which captures in which waves an individual completed the UKHLS questionnaire. This allows us to incorporate how many years and in which years each individual participated in the survey into the matching process. Including individual-level characteristics into the matching equation allows us to identify comparable control units with greater precision. Finally, we include a rural/urban indicator.

To estimate the propensity of flood exposure, we use additional flood-specific variables. To capture direct flood exposure at the macro (LSOA) level, we use information on the share of properties exposed to significant flood risk. To obtain an even more precise estimate of household-level flood risk, we utilise the 'Risk of Flooding from Rivers and Seas' (ROFRAS) dataset for England and Wales which provides a spatial representation of flood risk and classifies areas into very low, low, medium and high-risk areas. Using GIS, we

[16] LSOAs are geographic areas designed for reporting of small area statistics with an average area of 4 km^2 and a mean population of 1500.

[17] Dimensions include the population age, health and income profile, information use, local knowledge, housing tenure, housing characteristics, physical mobility, service availability, social networks, and crime. See Sayers et al. (2017), Appendix B for details.

identify the flood-risk of each UKHLS respondent based on a 500-metre radius. Finally, we include an index of neighbourhood flood vulnerability from the 'Climate Just Online Tool' to capture socio-spatial vulnerability (Sayers et al. 2017). The index captures neighbourhood flood vulnerability based on a pre-defined set of vulnerability criteria measured at the LSOA level.

Our matching approach follows a standard two-stage procedure (Imbens et al. 2009; Leuven and Sianesi 2003). We first predict the propensity score of being exposed to an extreme event using the variables outlined above in a probit model, where the dependent variable is an indicator for treatment assignment (See Sect. 3.2). We repeat this procedure separately for each flooding treatment radius as well as heatwave experience, excluding the flood-specific vulnerability and risk indicators for the latter. Subsequently, we use the estimated propensity score to identify the k nearest neighbours for each treated individual from the individuals that were not treated as per our treatment definitions.[18] Our most restrictive matching specification selects only one nearest neighbour for each treated unit ($k = 1$) without replacement. Additionally, we identify the two, three, four and five nearest neighbours ($k = 2$; $k = 3$; $k = 4$; $k = 5$) allowing for replacement (i.e., each control unit can serve as a match to more than one treated unit). For each matching specification, we iteratively assess baseline covariate balance between treatment and matched control groups. For the subsequent analysis, we select the matching specification which achieves the greatest balance on observable baseline characteristics as providing our main results. Robustness checks using the remaining matching specifications are provided in the Appendix. Using this approach, we find that $k = 3$ nearest neighbours achieves the greatest balance between treatment and control groups for flood exposure and $k = 1$ without replacement is most successful for heatwaves.

3.4 Summary Statistics

Table 1 shows the number of untreated and treated individuals based on our primary definitions of flood and heatwave exposure. Numbers displayed in columns (1) and (2) are obtained from the full sample, after excluding movers (as defined above) and those individuals who did not complete the full climate attitudes questionnaire, which we used to construct the outcome variables. Columns (3) and (4) display the untreated and treated units which are retained after implementing the preferred matching procedures for flooding and heatwave exposure, respectively.

Table 2 presents summary statistics for the full (unmatched) and matched samples based on our primary treatment definition for flood exposure. Panel A shows individual socio-demographic characteristics measured at baseline, Panel B presents the pre-treatment outcome variables, and Panel C shows the exposure (treatment) variable. Columns (1), (2), (4) and (5) present the means for control and treatment groups for the unmatched and matched samples, respectively. Columns (3) and (7) show the differences in means

[18] An important assumption for PSM is that there is sufficient common support across treatment and control group covariates to create reasonable propensity score matches. In the case of heatwave exposure, common support is not achieved for a subset of treatment units. To improve baseline covariate balance, we thus impose common support by systematically dropping the 15% of treated units for which the propensity score density of the control observations is the lowest (N = 747) (Angrist and Pischke 2009). Appendix Figs. 6 and 7 plot the propensity score density for the full sample and the matched sample. The kernel density plots show that our preferred matching procedures significantly improve overlap of treated and control propensity score distributions.

and corresponding p-values obtained from a two-sided comparison of means between treatment and control group. The comparison of means between unmatched and matched samples further illustrates the benefits of the nearest-neighbour matching approach. In the unmatched sample we observe statistically significant differences in education, tenure status, health, and rural household location. However, after selecting a more comparable subsample of control units for the matched sample, only the difference in health status between treatment and control groups remains statistically significant at a 5% level (column 7), suggesting that the matching procedure improved the balance on socio-demographic characteristics between the two groups.

Table 3 reports the summary statistics for the unmatched and matched samples based on our definition of heatwave exposure. The results of the two-sided t-test in Column (3), displayed as significance stars, imply that there are significant differences between treatment and control groups for nearly all socio-demographic characteristics and baseline climate change attitudes in the unmatched sample. Treated individuals have higher average income, education, are slightly younger, less likely to suffer from a chronic health condition and less likely to live in rural areas. Moreover, the treated group has a higher baseline level of climate change risk perception, concern and pro-environmental behaviour. As with flood exposure, our preferred k=1 nearest neighbour matching approach significantly reduces the differences in means between treatment and control group, and only age and health variables remain imbalanced in the matched sample. Importantly, our matching procedure ensures balance in baseline climate change attitudes, which strengthens the plausibility of the parallel trends assumption.

3.5 Empirical Strategy

To estimate the effect of extreme weather events on climate change beliefs and pro-environmental behaviour, we utilise the difference-in-differences (DID) estimator developed by Callaway and Sant'Anna (2021). A recent literature has shown the potential bias arising in generalised DID designs with staggered treatment timing and heterogeneous treatment effects (Baker et al. 2022; de Chaisemartin and D'Haultfoeuille 2022b; Goodman-Bacon 2021). In settings with heterogeneous treatment effects (i.e., differential treatment timings and varying treatment sizes), the conventional DID approach based on the two-way fixed effects (TWFE) estimation model can result in misleading and potentially invalid results due to the "forbidden comparisons" problem (see e.g. Baker et al. (2022) for an intuitive discussion).

Therefore, we use the approach developed by Callaway and Sant'Anna (2021) which is robust to heterogeneous and dynamic treatment effects in staggered designs. The approach first aggregates units into groups (or cohorts) that are first treated in the same period (i.e. prior to Wave 4 or Wave 10, in our setting). It is then possible to estimates the group-time average treatment effects - $\mathrm{ATT}_{(g,t)}$ - the average treatment effect on treated for a specific group g measured at time t. Effectively, the method estimates all possible combinations of canonical two-period DID designs that occur in the panel data. As we rely on imbalanced panel data, only observations that are balanced within a given two-period DID design are used for this estimation.[19]

$$ATT_{(g,t)} = [Y(g)_t - Y(NT)_t] - [Y(g)_{(g-1)} - Y(NT)_{(g-1)}] \tag{1}$$

[19] We use the user-contributed Stata command *csdid*: (Rios-Avila et al. 2023) to compute the estimators by Callaway and Sant'Anna (2021). Equations (1) and (2) are adopted from an accompanying online publication.

Table 1 Treated and Untreated Samples

	Full sample		Matched sample	
	(1)	(2)	(3)	(4)
	Untreated	Treated	Untreated	Treated
Flood exposure (1000 m)	16,756	1,221	2,625	1,220
Flood exposure (2000 m)	13,852	1,985	4,015	1,986
Heatwave exposure	12,336	4,981	4,205	4,263

Flood Exposure is defined as living within a 1,000(2,000)-metre radius from a recorded flood extent, respectively. Heatwave Exposure is defined as having experienced at least three consecutive days of daily maximum temperatures greater than 29 °C

Where ATT(g, t) represents the group-time average treatment effects. $Y(g)_t$ refers to the expected value of the outcome at time t for group g and $Y(NT)_t$ is the expected outcome for units that are 'never treated'. The same comparison is made for the period before the treatment occurred ($g - 1$). By using 'never treated' units as a comparison group we avoid the 'forbidden comparisons' problem and the corresponding risk of 'negative weights', if an earlier treated unit is used as control, which may potentially bias conventional TWFE estimates (Roth et al. 2023). Moreover, $\mathrm{ATT}_{(g,t)}$ cannot be computed for 'always treated units', excluding units that were treated prior to their first survey. To provide a meaningful interpretation of these estimates, we can aggregate the group-time average treatment effects to provide weighted average treatment effects for each outcome variable:

$$AGGTT = \frac{\sum (W_{(g,t)} * \mathrm{ATT}_{(g,t)})}{\sum W_{(g,t)}} \tag{2}$$

where $W_{g,t}$ is a weight corresponding to the size and precision of $ATT_{g,t}$, with larger and more precise estimators receiving more weight, while those derived from fewer observations are given less weight (Roth et al. 2023). Standard errors are computed using the delta method and are clustered at the individual level. Following equation (2), we provide two different aggregations of the group-time average treatment effects: First, we estimate a weighted average of all group-time average treatment effects with weights proportional to group size, to provide an overall ATT. Second, we compute cohort-specific average treatment effects, which produces an ATT for each treatment cohort. The units that were first treated before the Wave 4 survey are considered part of the Wave 4 treatment cohort, while those treated prior to the Wave 10 survey belong to the Wave 10 treatment cohort.[20]

To ascertain the robustness of our main results, we use a range of alternative estimation approaches, which provide unbiased treatment effects in settings with staggered treatment timing and heterogenous treatment effects (de Chaisemartin and D'Haultfoeuille 2022b; Roth et al. 2023). These include the doubly robust difference-in-differences estimator

[20] We estimate overall and cohort ATTs for our three primary outcomes described in section 3.1.1, which were specifically constructed to condense the UKHLS survey's wide range of climate change attitudes into three distinct variables. Prior to estimation, all outcomes were standardized (z-scored) on the mean to allow for a comparison of treatment effects in units of standard deviations across different outcomes. Group-time average treatment effects are adjusted for multiple hypothesis testing prior to aggregation (Callaway and Sant'Anna 2021). To limit the overall number of tests, we conduct additional sensitivity analysis only with those outcomes that display a significant overall ATT.

Table 2 Summary Statistics: Flood Exposure

	Full Sample				Matched Sample			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Untreated	Treated	Difference	Obs.	Untreated	Treated	Difference	Obs.
Outcome Variables								
Risk Perception (Yes = 1)	0.752	0.743	0.009	17,977	0.766	0.743	0.023	3,845
Concern Index	-0.160	-0.123	-0.037	16,979	-0.112	-0.122	0.010	3,581
Behaviour Index	2.981	2.994	-0.012	17,977	2.972	2.994	-0.022	3,845
Socio-demographic Indicators								
Household Income (£)	2907.349	2969.296	-61.948	17,977	2933.960	2970.174	-36.214	3,845
Education Level	3.754	3.977	-0.223***	17,977	3.902	3.979	-0.077	3,845
House Owned (Yes = 1)	0.773	0.848	-0.075***	17,977	0.829	0.847	-0.018	3,845
Age (Years)	48.391	48.839	-0.448	17,977	49.151	48.877	0.274	3,845
Chronic Health Condition (Yes = 1)	0.365	0.336	0.030**	17,977	0.369	0.336	0.033**	3,845
Rural (Yes = 1)	0.230	0.202	0.028**	17,977	0.206	0.202	0.004	3,845

Table displays the mean values of baseline outcome variables and socio-demographic characteristics, for untreated and treated groups based on our primary definition of flood exposure (1,000m) in the full and matched samples. Columns (3) and (7) report the difference between the mean values measured at baseline between untreated and treated groups and significance stars correspond to p-values obtained from a two-sided t-test for comparison of means. Obs. refers to the number of observations used to compute the means and conduct the t-test.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3 Summary Statistics: Heatwave Exposure

	Full Sample				Matched Sample			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Untreated	Treated	Difference	Obs.	Untreated	Treated	Difference	Obs.
Outcome Variables								
Risk Perception (Yes = 1)	0.747	0.759	-0.012*	17,317	0.766	0.755	0.011	8,468
Concern Index	-0.192	-0.085	-0.108***	16,406	-0.115	-0.111	-0.004	7,949
Behaviour Index	2.954	3.019	-0.065***	17,317	2.981	2.993	-0.012	8,468
Socio-demographic Indicators								
Household Income (£)	2746.294	3203.799	-457.505***	17,317	3080.253	3100.254	-20.001	8,468
Education Level	3.645	4.009	-0.364***	17,317	3.911	3.933	-0.022	8,468
House Owned (Yes = 1)	0.774	0.795	-0.021***	17,317	0.818	0.813	0.004	8,468
Age (Years)	48.625	48.412	0.213	17,317	47.943	48.816	-0.873**	8,468
Chronic Health Condition (Yes = 1)	0.381	0.325	0.055***	17317	0.360	0.333	0.027***	8,468
Rural (Yes = 1)	0.246	0.203	0.043***	17,317	0.239	0.232	0.007	8,468

Table displays the mean values of baseline outcome variables and socio-demographic characteristics, for untreated and treated groups based on our primary definition of heatwave exposure (1,000m) in the full and matched samples. Columns (3) and (7) report the difference between the mean values measured at baseline between untreated and treated groups and signficance stars correspond to p-values obtained from a two-sided t-test for comparison of means. Obs. refers to the number of observations used to compute the means and conduct the t-test.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

(DRDID) by Callaway and Sant'Anna (2021), the DID imputation estimator by Borusyak et al. (2022), the two-way Mundlak regression estimator by Wooldridge (2021)[21] and the DID estimator by de Chaisemartin and D'Haultfoeuille (2022a). For a detailed discussion of the above DID estimators, see Roth et al. (2023).

While our primary estimation approach relies on the assumption that once a unit is exposed to treatment it remains treated for the remainder of the observation period, we use the estimator by de Chaisemartin and D'Haultfoeuille (2022a) to provide additional sensitivity analysis and explore whether the ATT differs depending on the time interval between treatment and the survey date. This approach is more flexible than those previously discussed, in that it allows treatment to "switch on" and "switch off", and potential outcomes thus depend on the full path of past treatments (Roth et al. 2023).

To conduct additional analyses which require more flexible model specifications, we additionally use standard panel data regression techniques with individual and wave fixed effects (two-way fixed effects, or TWFE), which assumes that treatment effects are homogeneous across time, as specified in (3):

$$Y_{it} = \alpha_i + \lambda_t + \delta^{DD} Event_{it} + X_{it} + \epsilon_{it} \tag{3}$$

where Y_{it} is the measure of risk perception, climate change concern or pro-environmental behaviour of individual i at time t. Individual fixed effects are captured by α_i, which account for any unobserved time-invariant individual characteristics. λ_t are survey-wave fixed effects which account for common changes in climate-change beliefs over time. $Event_{it}$ is an indicator for a treated unit after a flood or heatwave event occurred within their vicinity, from which we obtain the difference-in-differences estimator (δ^{DD}), our coefficient of interest. X_{it} represents a set of time-varying socio-economic control variables which have been identified as important predictors of climate change attitudes, including income, education, age and housing tenure. ϵ_{it} is the random error term.

To explore the heterogeneity of the average treatment effect on the treated (ATT), we interact the conventional two-way fixed effects difference-in-differences estimator with additional variables of interest, capturing the intensity of treatment, as specified in equation (4). $Intensity_{it}$ serves as a placeholder for various measures of treatment intensity. First, we interact the DID estimator with an indicator ($ROFRAS_{it}$) equal to one if a household is located in an area that is at risk of flooding, which is objectively determined from the ROFRAS dataset (see Sect. 3.1.2). Second, we allow the treatment effect to vary by the distance to the flood event. We estimate the distance effect with a continuous variable ($MinDis_{it}$), which captures the minimum recorded distance to the flood event for treated individuals. For heatwaves, we are interested in whether treatment intensity is associated with heatwave duration. We construct a continuous measure ($MaxDur_{it}$) for the maximum number of consecutive days experienced during a heatwave episode. As we have no a priori assumptions about whether the treatment intensity has a linear or non-linear effect on climate change attitudes, we estimate both linear and quadratic functions of treatment intensity - $f(Intensity_{it})$ -, the latter shown in equation (4):

$$Y_{it} = \alpha_i + \lambda_t + \delta^{DD} Event_{it} + \theta(Event_{it} \times f(Intensity_{it})) + \epsilon_{it} \tag{4}$$

While our empirical strategy allows a causal interpretation of the results, it is important to note that our identification strategy follows an "intention to treat" approach. Respondents with flood and heatwave exposure were identified based on objective measures of flood

[21] Stata implementation by (Rios-Avila 2023).

and heatwave incidence alone. The actual individual subjective experience of the events remains unknown, and we are unable to ascertain that the respondents were physically present at the time of the weather event. Nonetheless, we argue that the household location is a good proxy for flood experience, whether direct (physically present at the time of the weather event) or indirect (via affected friends and family members). As previously discussed, it is reasonable to assume that our estimates capture the effect of experiencing a ”near miss” from flooding. Furthermore, the use of objective GIS data avoids potential biases commonly encountered with subjective measures of flood experience (Guiteras et al. 2015). For instance, self-reports of flood experience and damages may be subject to recall bias or influenced by other unobserved individual-level characteristics (Hassan 2006).

4 Results

4.1 Average Treatment Effects on the Treated

In the following section we present the average treatment effects on the treated (ATT) obtained from the two aggregation approaches of group-time average treatment effects (see equation (2)) following our primary definitions of flooding and heatwave exposure. As mentioned in Sect. 3.2, in our baseline specification, flood treatment is defined as living within a 1,000-metre radius from a flood extent and heatwave treatment is defined as having experienced at least three consecutive days of daily maximum temperatures greater than 29°C. Figure 3 provides a visual representation of the overall ATT of event experience on our three outcome variables. The full results are shown in Panel A of Table 4.

The results indicate that both flood and heatwave experience increase climate change risk perception but have limited effect on climate change concern and pro-environmental behaviour. Flood experience leads to an increase in risk perception by 0.076 standard deviation units (or 3.1 percentage points), whereas heatwave exposure causes a slightly smaller increase of 0.055 SD units (or 2.3 percentage points), both significant at the 5% level. In addition, we observe that flood experience also increases climate concern by 0.058 standard deviation units (5.2 percentage points). However, this estimate is only marginally significant at the 10% significance level and is not robust across alternative specifications, discussed in the next section. The estimates for pro-environmental behaviour are both close to zero and statistically indistinguishable from zero.

To further explore the composition of the overall ATT, we investigate whether the treatment effect differs depending on in which wave a unit was first treated (i.e. either prior to the Wave 4 or Wave 10 survey). These cohort-specific treatment effects are presented in Panel B of Table 4. Interestingly, we observe that the overall ATT appears to be primarily driven by individuals first exposed to treatment in the years preceding the most recent wave of data collection (Wave 10 Cohort).[22] These more recent extreme events received greater media attention and were more directly attributed to climate change.

In the Appendix we use a subsample of our data (flooding sample treatment group: Individuals = 140, N = 420; heatwave sample treatment group: Individuals = 299, N = 897) to investigate potential dynamic effects of treatment exposure over time. Specifically, units first treated in Wave 4 but observed in all three waves allow us to estimate the instantaneous effect of the event (T0 = treated in Wave 4) and the effect of this treatment one survey

[22] Data collection for Wave 10 took place between 2018 and 2020.

wave later (T1). These event-study estimates are visualised in Fig. 8, and corresponding regression output is presented in Table 9. The findings suggest that the effect of flooding on risk perceptions is short-lived, as only the estimate for T0 is statistically significant. In contrast, we find suggestive evidence for dynamic effects of heatwave exposure, with a significant effect at T0 which appears to increase further at T1. However, it is possible that some individuals in the heatwave sample were treated again prior to Wave 10, which may potentially be misinterpreted as a dynamic effect of previous treatment. We find support for this premise in section 4.5. In general, caution is advised when interpreting these findings due to the small sample of individuals for which we are able to estimate dynamic effects. Moreover, the cohort-specific estimates (Table 4, Panel B) indicate that the increase in risk perception is primarily driven by treatment in Wave 10 for which we are not able to observe dynamic effects.

Similarly, we are able to estimate pre-treatment differences (T-1) between treatment and control groups for units observed in all three waves that were first treated in Wave 10 (Flooding sample: Individuals = 488, N = 1464; Heatwave Sample: Individuals = 1784, N = 5352). We do not observe any significant differences in our primary outcome variables during the pre-treatment period. This further strengthens the validity of the parallel trends assumption. However, these results should be interpreted with caution as they do not represent the full matched sample, as discussed earlier.

Overall, the findings lend support to the risk perceptions hypothesis formulated in Brügger et al. (2021) that personal experiences of climate related events should increase the perceived likelihood of similar and related events in the future. Our results indicate that after having experienced flooding or heatwaves, people are significantly more likely to believe that the UK will be affected by climate change in the next 30 years. In contrast, extreme event exposure had little effect on climate change concerns and no statistically significant impact on pro-environmental behaviour.

4.2 Robustness

Table 5 demonstrates robustness of our main results when estimated using a range of alternative specifications and estimation procedures. In Panel A, we estimate the doubly robust DID estimator (Callaway and Sant'Anna 2021) based on inverse probability of tilting and weighted least squares using data from the matched sample. In Panel B we use the two-way Mundlak regression developed by Wooldridge (2021) with a 'never-treated' control group. In Panel C we estimate the dynamic DID estimator by de Chaisemartin and D'Haultfoeuille (2022a). In Panel D we estimate the imputation estimator by Borusyak et al. (2022). In Panel E we estimate the conventional TWFE estimator with additional time-varying controls which may affect climate change perceptions, including income, education level, age and housing tenure. We find that all five specifications produce estimates of similar magnitude and statistical significance to our primary estimates presented above, demonstrating the robustness of our main findings, with one notable exception: The conventional TWFE specification with time-varying controls is not statistically significant for heatwave exposure (Panel E).

The Appendix contains additional robustness checks and supplementary analysis. Table 10 shows the main treatment effects are robust to alternative matching regimes (i.e. 1:1, 1:2, 1:3, 1:4 and 1:5 nearest neighbour matching). Table 11 demonstrates that our main estimates produce similar p-values when clustering standard errors at a higher geographical level (i.e., Local Authority District level). Table 12 shows that the effect of flood

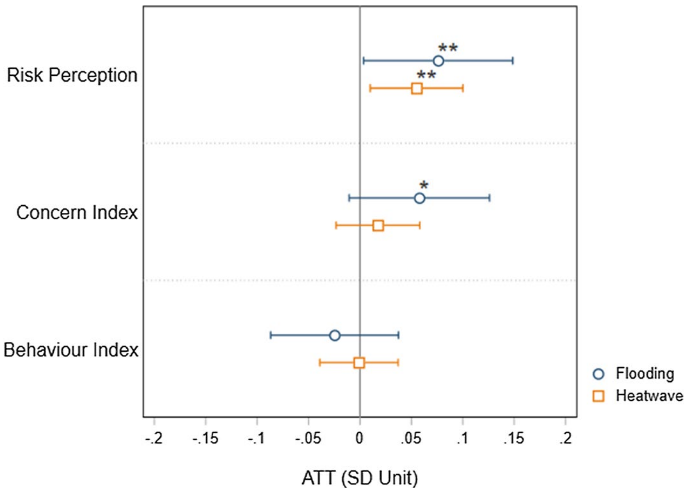

Fig. 3 ATT of Flooding and Heatwave Exposure. *Note:* Overall ATT of flooding and heatwave exposure estimated using the Callaway & Sant'Anna (2021) regression estimator. Dependent variables are standardised on the mean. Error bars indicate 95% confidence intervals. Corresponding standard errors and sample size for each estimate are presented in Table 4

Table 4 Main Results

	Flood Exposure			Heatwave Exposure		
	(1)	(2)	(3)	(4)	(5)	(6)
	Risk	Concern	Behaviour	Risk	Concern	Behaviour
	A: Overall ATT					
Overall ATT	0.076**	0.058*	−0.025	0.055**	0.017	−0.001
	(0.037)	(0.035)	(0.032)	(0.023)	(0.021)	(0.019)
	B: Cohort Estimates					
W4 Cohort	0.033		−0.079	0.018		0.112***
	(0.069)		(0.057)	(0.050)		(0.037)
W10 Cohort	0.096**	0.058*	0.001	0.066**	0.017	−0.035
	(0.044)	(0.035)	(0.038)	(0.026)	(0.021)	(0.023)
Individuals	3,761	2,972	3,761	8,198	6,804	8,198
Observations	9,443	5,944	9,443	20,496	13,608	20,496

Table reports different aggregations of the group-time average treatment effects of flood exposure (columns 1–3) and heatwave exposure (columns 4–6) estimated using the regression estimator by Callaway and Sant'Anna (2021) with the matched sample. Panel A reports the overall ATT. Panel B reports cohort-specific treatment effects. The dependent variable in columns (1) and (4) is a binary variable for climate change risk perception. The dependent variable in columns (2) and (5) is an index of climate change concern. The dependent variable in columns (3) and (6) is an index of pro-environmental behaviour. Dependent variables are standardised on the mean. Flood treatment is defined as living within a 1,000-metre radius from a flood extent and heatwave treatment is defined as having experienced at least three consecutive days of daily maximum temperatures greater than 29 °C. Standard errors clustered at the individual level in parentheses

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 5 Robustness: Alternative specifications - flood exposure

	Flood Exposure			Heatwave Exposure		
	(1)	(2)	(3)	(4)	(5)	(6)
	Risk	Concern	Behaviour	Risk	Concern	Behaviour
A. Callaway & Sant'Anna (2021)	0.082**	0.055	−0.028	0.053**	0.016	0.007
	(0.038)	(0.035)	(0.032)	(0.023)	(0.021)	(0.020)
B. Wooldridge (2021)	0.074**	0.058*	−0.027	0.050**	0.017	−0.004
	(0.037)	(0.035)	(0.031)	(0.023)	(0.021)	(0.019)
C. Chaisemartin & D'Haultfœuille (2022)	0.086**	0.058	−0.008	0.053**	0.017	−0.017
	(0.036)	(0.040)	(0.023)	(0.023)	(0.018)	(0.019)
D. Borusyak et al. (2022)	0.084**	0.058*	−0.021	0.045**	0.017	0.005
	(0.036)	(0.035)	(0.031)	(0.022)	(0.021)	(0.019)
E. TWFE + Controls	0.097***	0.046	0.001	0.032	0.007	0.001
	(0.035)	(0.033)	(0.030)	(0.022)	(0.020)	(0.018)
Individuals	3,845	3,112	3,845	8,468	7,103	8,468
Observations	9,611	6,224	9,611	21,036	14,206	21,036

Table reports ATT of flood exposure (columns 1–3) and heatwave exposure (columns 4–6) for a range of alterantive estimation approaches using the matched sample. The dependent variable in columns (1) and (4) is a binary variable for climate change risk perception. The dependent variable in columns (2) and (5) is an index of climate change concern. The dependent variable in columns (3) and (6) is an index of pro-environmental behaviour. Dependent variables are standardised on the mean. Flood treatment is defined as living within a 1,000-metre radius from a flood extent and heatwave treatment is defined as having experienced at least three consecutive days of daily maximum temperatures greater than 29 °C. Standard errors clustered at the individual level in parentheses

*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

exposure on risk perception persists when the treatment group is compared to those individuals who had previously experienced a flood in the past 10 years (i.e. an 'always treated control group'). While we may expect to find the treatment effect to decrease or entirely disappear using an 'always treated control group', this is not the case, suggesting that more recent salient flood events are likely driving the observed ATT.

Finally, in Appendix Table 13, we provide tentative evidence that the effect of flood exposure on risk preferences is primarily driven by people living in areas which have previously not been associated with any risk of flooding from rivers or seas (ROFRAS). We use the ROFRAS dataset (described in Sect. 3.1.2) to identify individuals whose household is located within an area (500 m radius) that is at risk of flooding (73% of the treatment group), which could serve as a proxy measure of flood risk awareness. The remaining 27% of the sample with no identifiable flood risk within their vicinity may be less aware of potential flooding threats and thus more likely to update their beliefs following an event. To explore this, we interact the flood risk indicator with the DID estimator, following equation (4). We find a (weakly) significant difference between individuals in flood-prone areas and those in non-flood-prone areas, suggesting that those individuals experiencing a "near miss" from flooding without prior awareness of flood risk are driving the observed treatment effect. However, these findings must be interpreted with caution as they are derived from the conventional TWFE estimator, which may produce biased estimates.

4.3 Treatment Intensity

Next, we explore multiple measures of treatment intensity, which may reveal more nuanced effects of extreme event exposure. We focus on risk perception as our primary outcome of interest, as this is the only outcome for which we find robust evidence for an ATT for both flooding and heatwaves. As discussed in Sect. 2.2, we expect greater spatial proximity to flood events to be associated with a larger increase in risk perceptions. The closer the event occurs to the household, the more personally relevant and consequential it is likely to be (Brügger et al. 2021). For instance, in the case of flooding, individuals may suffer direct damage to property or may be otherwise affected from damage to infrastructure and services.

To explore the effect of physical proximity to flooding, we construct a continuous variable capturing the minimum distance from the flood extent outline to the household location of people that experienced a flood within a radius of 2,000 metres. We allow the treatment radius to span 2,000 metres to evaluate whether a distance gradient exists at greater distances. Appendix Fig. 9 (left panel) shows the distribution of minimum distances at which flood events were experienced in our sample. As with our primary treatment definition (1,000 m), we identify a matched control group comparable to the treated units within a 2,000-metre radius using our nearest-neighbour matching strategy. Although we expect the effect of flood exposure to diminish with distance, we have no clear assumptions over the functional form and thus interact the treatment indicator with both the linear and quadratic term of distance to the flood event, respectively.

The left panel of Fig. 4 visualises the relationship by plotting the DID estimates for flood exposure at distances between 0 and 2,000 metres. As changes in risk perceptions can be directly interpreted in percentage points, we plot the non-standardised estimates. The results indicate a diminishing linear relationship between proximity and risk perceptions, significant at the 5% level (see Appendix Table 14 for the coefficient estimates of equation (4)).

The effect of flood exposure on risk perceptions is largest for people living closest to the flood extent and diminishes with distance. At distances greater than 1,000 metres from the event, the effect is no-longer statistically different from zero. Our findings suggest that the effect of flooding is highly localised and manifests that geo-referenced climate change belief data is of key importance for accurate identification of treated and untreated households.

Turning to heatwave treatment intensity, we hypothesised that the effect of heatwave experience may increase with the duration of the heatwave. We thus measure heatwave intensity as the maximum number of consecutive days with temperatures greater than 29°C experienced during the observation period. The distribution of heatwave duration in the treatment group is shown in the right panel of Appendix Fig. 9. We estimate equation (4) to explore both linear and quadratic interactions of heatwave duration. The right panel of Fig. 4 shows that while the effect of heatwave exposure increases with the number of days exposed, we find no evidence that this increase is statistically significant (see Appendix Table 14, right panel).

In sum, we observe that treatment intensity (measured as flood proximity and heatwave duration) is only somewhat associated with climate change risk perception. However, we find clear evidence that the effect of flooding on risk perceptions is highly localised, which provides support for the ’proximising’ strategy, previously discussed.

4.4 Temporal Proximity

In this section we explore the role of temporal proximity to the extreme event in shaping climate change attitudes. Based on salience theory and recent empirical research (Larcom et al. 2019) we hypothesised that the effect of extreme events may diminish the greater the temporal distance between the event and the survey date. To explore temporal proximity effects, we first identify treatment at different time intervals prior to each survey (e.g. within 2, 5 or 7 years prior to the survey). If a temporal decay effect is present, the treatment effect estimate should diminish at larger time intervals. To estimate the ATT, we use the DID estimator by de Chaisemartin and D’Haultfoeuille (2022a), which allows treatment to “turn on” and “turn off” at different times over an individual’s observation period.

The ATTs for different treatment time intervals are presented in Table 6. For flooding, we allow treatment to occur up to 10 years prior to the event. For heatwaves, we allow a maximum of 5 years, as the majority of the sample (78%) experienced either the 2003 or 2006 heatwave, thus providing insufficient time-varying information to estimate treatment effects at greater time intervals. Prior to estimation, we use our preferred matching procedures to determine a matched control group specific to each time-interval treatment group.

Several interesting results emerge from this analysis. For flood exposure, we find that the treatment effect is largest if we allow treatment to occur up to four years prior to the survey date. Similar estimates are produced at two and three years, however, there is no observable effect for individuals surveyed just one year prior to the survey. While this finding is finding may suggest that there is no effect if surveyed in the immediate aftermath of an event, the small sample size and the number of treated units (i.e. first time ‘switchers’) used to compute this effect is likely insufficient to provide reliable estimates. If we expand the (pre-) treatment interval to 10 years, we find a highly significant effect of flood exposure on risk perceptions. It is important to note that over longer time intervals, changes occur in the composition of the sample used to estimate treatment effects, as reflected in the sample size (N) and the number of switchers used for estimation. This is due to an increase in the number of ”always-treated” units, whose treatment status remains unchanged throughout the observation period and are thus excluded from the analysis.[23]

In relation to heatwave exposure, our findings indicate that the treatment effect is weakest with a one-year treatment interval and exhibits a slight increase as the treatment interval is extended. The effect is most pronounced and highly statistically significant when using a four-year treatment interval. However, as noted previously, the sample composition is substantially altered as a large number of ”always-treated” units are excluded from the analysis, which makes it difficult to draw clear conclusions on whether temporal effects exist.

Taken together, our analysis of temporal proximity provides only partial support for the premise that changes in climate change attitudes following extreme weather events are

[23] Also note that the estimator de Chaisemartin and D’Haultfoeuille (2022a) is computed by constructing a specific control group that avoids ”forbidden comparisons” as discussed in Sect. 3.5 and therefore differs from the sample size in our main analysis, which utilises all unit-wave observations (see de Chaisemartin and D’Haultfoeuille (2022b) for more details).

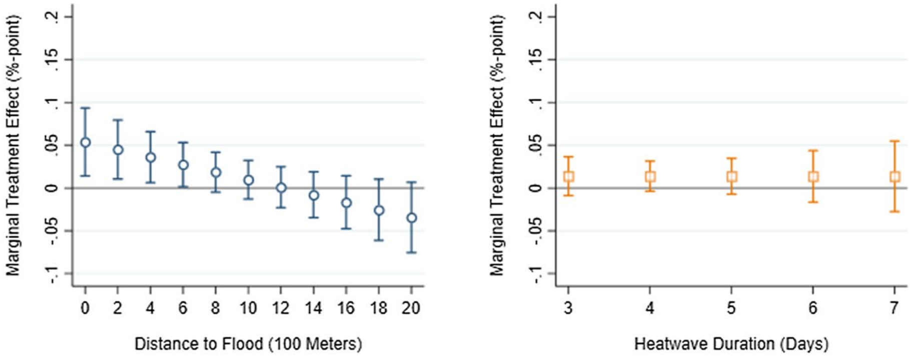

Fig. 4 Treatment Intensity. *Note:* The dependent variable is 'Risk perception' (non-standardised). The left panel displays the marginal treatment effect (%-point change) of flood exposure with increasing distance to the flood ($N = 15,076$). The right panel displays the marginal treatment effect (%-point change) of heatwave exposure with increasing heatwave duration ($N = 21,036$)

driven by a salience effect and are thus short-lived. Our analysis indicates that flood exposure has the most significant effect on people surveyed between two and four years after the event, with a slight temporal decay observed at greater time intervals. Conversely, for heatwave exposure, we observe the opposite pattern: the treatment effect estimate slightly increases at greater time intervals.

4.5 Major Heatwave Events

Finally, we explore the effect of major heatwave events on climate change beliefs and attitudes. While heatwaves corresponding to our definition were recorded in 2013 and 2017, the UK was subject to a highly abnormal prolonged period of extreme heat during the summer of 2018 and less than 12 months later experienced record-breaking temperatures in July 2019. Both events received substantial media coverage and the abnormal frequency and intensity of these successive events was widely attributed to climate change (Ma et al. 2020; McCarthy et al. 2019). We thus expect the major heatwaves of 2018 and 2019 to have more pronounced effects on climate change beliefs, due to their enhanced salience and attribution to climate change. Moreover, we might expect individuals who personally experienced both the 2018 and 2019 heatwaves (as opposed to only one) to be more likely to update their beliefs and behaviours.

To explore these questions, we modify equation (3) by replacing the binary treatment variable with a set of dummies that identify individuals who experienced (i) a heatwave prior to 2018, (ii) either the 2018 *or* 2019 heatwave or (iii) both the 2018 *and* 2019 heatwaves. The results are visualised in Fig. 5 and the corresponding estimates are presented in Table 15.

In line with our expectations, we find that being exposed to heatwaves in 2013 and 2017 had no effect on any of the outcome measures, while experience of one of the major

Table 6 Temporal Proximity

Years	Estimate	SE	LB CI	UB CI	N	Switchers
A: Flood Exposure						
1	– 0.068	0.079	– 0.223	0.087	1,443	277
2	0.124**	0.062	0.002	0.244	2,040	397
3	0.119**	0.052	0.017	0.220	3,310	655
4	0.133***	0.039	0.056	0.209	4,301	853
5	0.047	0.038	– 0.026	0.121	5,193	1,057
6	0.072**	0.035	0.002	0.141	5,565	1,179
7	0.086**	0.036	0.015	0.156	5,362	1,135
10	0.114***	0.040	0.035	0.192	4,211	859
B: Heatwave Exposure						
1	0.042*	0.023	– 0.002	0.086	11,664	3,813
2	0.047**	0.023	0.0013	0.091	13,472	4,389
3	0.053**	0.024	0.006	0.101	11,750	3,964
4	0.077***	0.029	0.021	0.134	6,163	2,477
5	0.071*	0.039	– 0.005	0.147	3,011	1,508

Table reports the ATT of flood exposure estimated using the Chaisemartin & d'Haultfoeuille's (2022) DID estimator for different treatment-time intervals (years) prior to the UKHLS survey date. The table presents the estimator, standard errors, lower bound and upper bound of the 95% confidence intervals, as well as the sample size and the number of switchers. The dependent variable is a (standarized) binary variable for climate change risk perception. Standard errors are computed using 100 bootstrap replications

*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

2018/2019 events significantly increased climate change risk perception (0.07 SD units, significant at the 5% level). The most pronounced effect on climate change attitudes (ranging between 0.11–0.17 SD units), across all three dimensions, is found for those individuals who experienced both the 2018 and 2019 heatwaves, with all three estimates significant at the 1% level. The findings from this analysis suggest that recent salient heatwave events, which received greater media coverage and for which a more direct link to climate change was established in the public domain, are driving the observed ATT on risk perception discussed in Sect. 4.1. Moreover, the interaction effect (2018 and 2019) corroborates our hypothesis that recurring extreme events may call attention to the abnormality of increasingly frequent hot and dry summers, especially in the UK where such events have been historically rare.

5 Discussion and Conclusion

In this paper, we link geo-referenced climate change opinion data with records of extreme weather events in England and Wales to explore whether people update their beliefs after experiencing extreme events. We find that personal experience with both flooding and

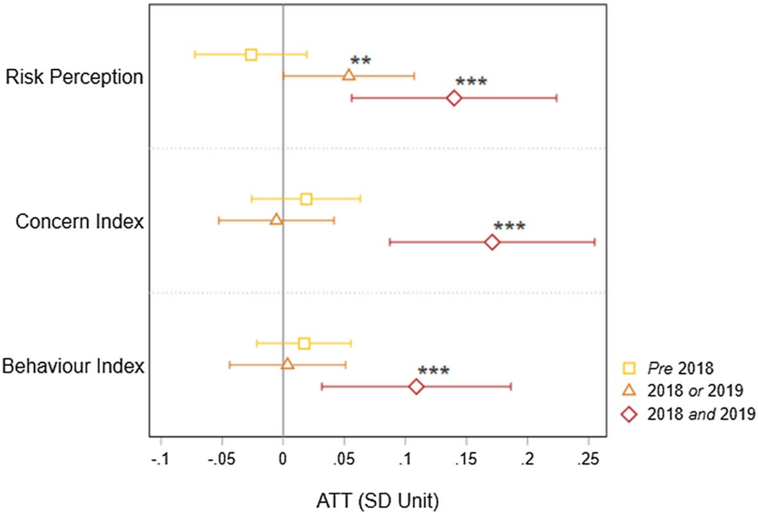

Fig. 5 ATT of Major Heatwave Events. *Note:* OLS estimates of equation (3) with three binary treatment variables for heatwave exposure (Pre 2018; 2018 or 2019; 2018 and 2019). Error bars indicate 95% confidence intervals. Dependent variables are standardised on the mean. Corresponding standard errors and sample size for each estimate are presented in Table 15

heatwaves increases climate change risk perception (belief that climate change will affect people in the UK within the next 30 years) but has no robust effect on climate change concern and self-reported pro-environmental behaviour, on average. However, we show that ATTs may mask some important nuanced effects of extreme event exposure, which provide interesting insights into when and how personal experience changes climate change attitudes.

For flooding, our main results are in line with the more recent literature (Albright and Crow 2019; Demski et al. 2017; Frondel et al. 2017; Ogunbode et al. 2020; Osberghaus and Fugger 2022). We find that experiencing a flood within a 1,000-metre radius from the household increases risk perception, which is driven by individuals first-treated prior to the Wave 10 survey. However, we find no robust evidence that flood experience leads to changes in climate change concern or stated pro-environmental behaviour, on average. Moreover, we show that the effect of flooding on risk perceptions is highly localised and decreases rapidly as the distance to the flood increases. This finding highlights the importance of employing geo-referenced opinion and climate data and corroborates earlier evidence from Germany (Osberghaus and Fugger 2022). Although it remains challenging to draw conclusions on the underlying mechanisms, a plausible explanation for our findings is that the closer an event is to one's home, the more personally relevant and memorable it may be (Brügger et al. 2021). Moreover, flood victims may be more likely to establish a cognitive link between the flooding event and climate change and therefore perceive climate change as more certain, temporally close and personally relevant (Ogunbode et al.

2020). Furthermore, we find that increases in risk perceptions after flood experience are most pronounced amongst people surveyed in the first four years after the flood event and subsequently diminish slightly over time. This finding is in line with previous empirical evidence showing that flood risk discount in property markets as well as flood insurance take-up spike immediately after an event and subsequently return to baseline in the years after the event (Atreya et al. 2013; Gallagher 2014). Finally, we find suggestive evidence that the increase in climate change risk perceptions is driven by individuals who were living in areas not previously considered to be at risk of flooding, which aligns with the Bayesian updating paradigm.

With respect to heatwaves, our results suggest that experiencing a heatwave (at least 3 consecutive days of temperatures > 29°C) increases climate change risk perceptions but again has no effect on climate change concern and pro-environmental behaviour, on average. Our main result is consistent with previous studies which find that heatwave exposure is strongly correlated with subjective risk perceptions (Dai et al. 2015; Frondel et al. 2017) but has no effect on pro-environmental behaviour (Larcom et al. 2019). However, our results also provide suggestive evidence of more nuanced effects of heatwave exposure. Our analysis of temporal proximity finds no clear relationship between risk perception and the time interval (number of years from interview date) used to define heatwave exposure, although the interpretation of these findings is limited due to changing sample composition. Future research should thus seek to further explore the temporal dimension of belief updating following extreme events. However, we find suggestive evidence that exposure to both the 2018 and 2019 heatwaves caused a significant increase in risk perception, climate change concern and pro-environmental behaviour, whereas previous heatwaves in 2013 and 2017 had no effect. While the data utilized in this study do not allow us to precisely identify the exact underlying mechanism for this finding, one plausible explanation could be that the attention given to climate change in the media during the 2018 and 2019 heatwave events, could have facilitated the updating of beliefs. Moreover, the unusual consecutive occurrence of these heatwave summers could have further reinforced the association with climate change. This finding suggests that increasing frequency of severe heatwaves may aid autonomous adaptation to climate change in the future if heatwave exposure creates a positive feedback loop with engagement and behaviour.

Notwithstanding the numerous methodological innovations, our analysis is not without limitations. It is important to acknowledge that our analysis relies on unbalanced panel data, collected over three waves, which spans a period of up to 11 years, introducing challenges with respect to defining consistent (pre-) treatment periods. Changes in sample composition thus limit the interpretation of our findings from the temporal proximity analysis, which should be viewed cautiously. Moreover, given the short panel length (at most three observations per individual), we are unable to conduct standard event study analyses for the full sample, which generally entails estimating differences between treatment and control groups for multiple pre- and post-event periods. We are thus unable to provide a clear indication of how the treatment effect may evolve over time following event exposure. Future research should attempt to utilise objective climate perception data that is collected frequently and balanced within individuals over time, to provide insights into potential dynamic effects. While the use of objective indicators of extreme events minimises the risk of recall and reporting biases, we are unable to verify whether participants were physically present (i.e. at their household location recorded in the data) at the time of the events. Moreover, Van Der Linden (2014) points out the importance of cognitive attribution in order for an affective reaction to occur, which is supported by recent empirical evidence (Ogunbode et al. 2019). Although we provide suggestive evidence that recent salient heatwave events which the media largely attributed to climate change significantly affected beliefs and behaviour, we are unable to ascertain that respondents did in fact draw a cognitive link between the extreme weather event and climate change. Combining both objective indicators of extreme events with self-reports that incorporate measures of subjective attribution (see e.g., Ogunbode et al. 2020) may thus present an interesting avenue for future research. In addition, it is important to point out that the analysis relied entirely on self-reported (stated) climate change attitudes and pro-environmental behaviour. Especially with respect to the latter, stated behaviour does not necessarily allow us to draw firm conclusions about *actual* behaviour change. Future research should aim to explore observed behaviours (such as recently Osberghaus and Demski (2019)) to provide evidence for actual behaviour change.

The findings discussed above entail several policy implications. Although no single event can be directly attributed to climate change, the incidence of severe flooding and heatwave events could be harnessed to raise awareness towards future climate change risks, increasing not only the geographic relevance, but also the temporal proximity. While flood events do not appear to have a direct impact on climate change concern and pro-environmental behaviour, they may provide favourable conditions for climate change communication and engagement strategies in the months after the event. We recommend that risk communication campaigns in the wake of flood events should focus on the geographic proximity of events and highlight the link between the event and climate change to facilitate attribution. In turn, subjective belief updating brought about by well-targeted communication and engagement campaigns can facilitate the smooth capitalization of climate risk in property markets and encourage adaptive behaviours such as insurance choices and defensive investments, consequently improving market efficiency and bringing about substantial welfare gains (Gibson and Mullins 2020; Hino and Burke 2020). In this regard, several empirical studies have shown that severe flood events can trigger changes in property prices (Bin and Landry 2013; Kousky 2010) and flood insurance take-up (Gallagher 2014). Moreover, climate risk signals from extreme weather events may be harnessed to garner support among the public for more ambitious climate change mitigation and adaptation policies (Lawrence et al. 2014). While the determinants of public resistance to climate change policies are complex (Carattini et al. 2018), highlighting the proximal nature, personal relevance and urgency for mitigation may sway the public debate to consider more ambitious policy interventions.

A second key insight from our analysis is the potential of drawing attention to climate change by highlighting the unusual frequency of heatwave events. With intensity and frequency of heatwaves predicted to increase further with global warming (Christidis et al. 2019), our results suggest that this may prove a promising strategy to not only raise climate change concern but also encourage more sustainable behaviours. However, recent evidence from the US shows a rapid decline in the perceived remarkability of extreme temperatures among the general public (Moore et al. 2019). If heatwave events will soon be considered the "new normal" it may imply a limited window of opportunity to highlight the abnormality of increasingly frequent heatwave events. It remains to be seen whether social normalisation of extreme heat conditions will occur at a similar pace in the UK.

In sum, the findings of this paper suggest that it is reasonable to assume that experiencing the impacts of climate change will reduce the psychological distance to climate change for people in the UK, by increasing personal relevance and perceived risk. However, on average, extreme events will have little effect on the level of engagement and action for most people. Nonetheless, increasingly frequent heatwaves may have a somewhat 'self-correcting' effect on psychological distance to climate change and may even motivate behaviour changes. Highlighting the unusual frequency of extreme heatwaves in climate change communications and drawing attention to their anthropogenic cause appears to be a promising strategy to increase concern around climate change and in turn garner support for mitigation policies.

Appendix

See Figures 6, 7, 8 and 9.

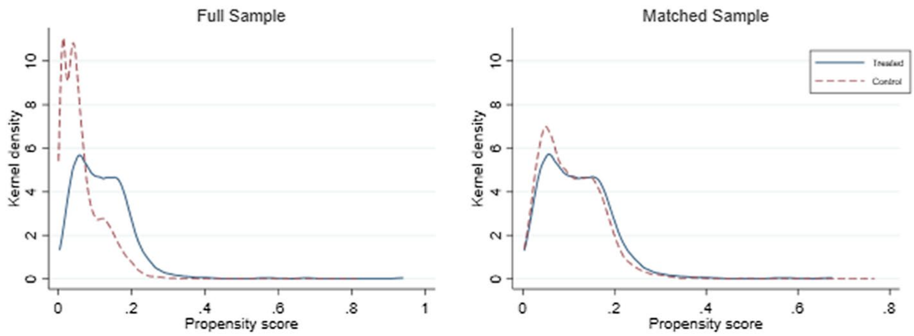

Fig. 6 Kernel-Density Plot - Flooding; Full sample: $N = 17,977$; Matched sample: $N = 3845$

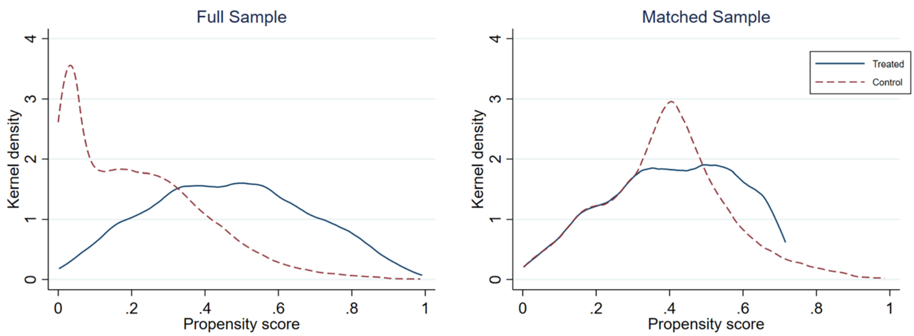

Fig. 7 Kernel-Density Plot - Heatwave; Full sample: $N = 17,317$; Matched sample: $N = 8468$

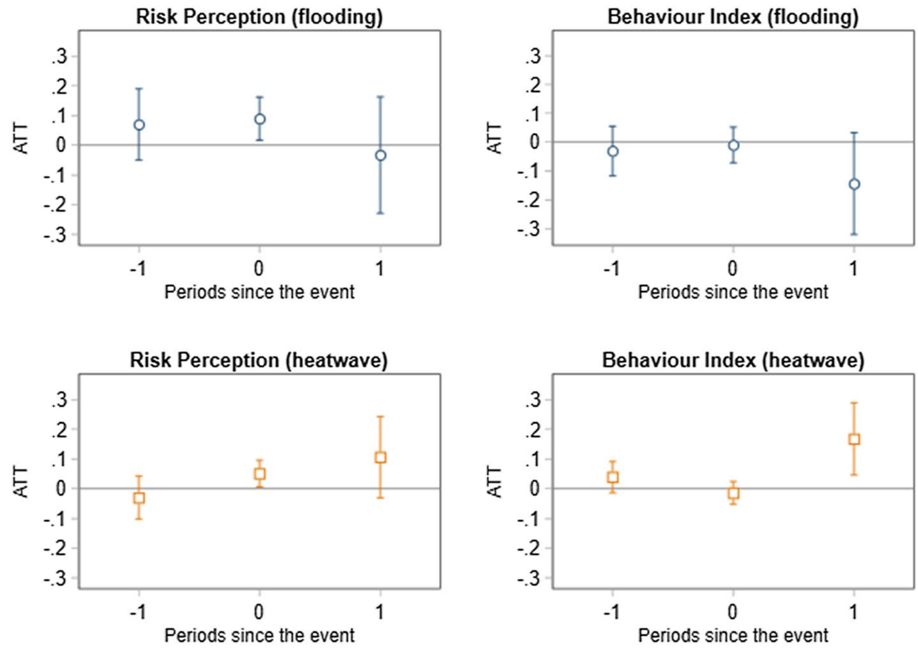

Fig. 8 Event study estimates. *Note:* Event study estimates using the Callaway & Sant'Anna (2021) Regression Estimator. Dependent variables are standardised on the mean. Error bars indicate 95% confidence intervals. Corresponding standard errors and sample size for each estimate are presented in Table 9

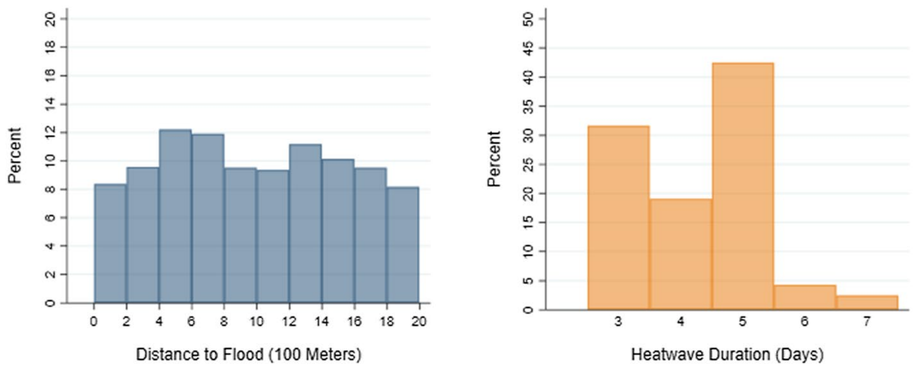

Fig. 9 Treatment Intensity. *Note:* Left panel displays sample distribution of distance to the flood extent for treated units within a radius of 2,000 m ($N = 1992$). Right panel displays sample distribution of heatwave duration for treated units ($N = 4226$)

See Tables 7, 8, 9, 10, 11, 12, 13, 14, 15.

Table 7 UKHLS Variables: Climate Change Questionnaire

Dimension	UKHLS code	UKHLS wave	Response format	Description
Risk Perception	scopecl30	1, 4, 10	'Yes'/'No'	Do you believe that people in the UK will be affected by climate change in the next 30 years
Climate Change Concern	scenv_bccc	4, 10	5-point Likert: 'Strongly disagree' - 'Strongly agree'	The effects of climate change are too far in the future to really worry me
	scenv_pmep	4, 10		The so-called 'environmental crisis' facing humanity has been greatly exaggerated
	scenv_meds	4,10		Climate change is beyond control - it's too late to do anything about it
	scenv_crex	4,10		If things continue on their current course, we will soon experience a major environmental disaster
	scenv_tlat	4, 10		Any changes I make to help the environment need to fit in with my lifestyle
	scenv_nowo	4, 10		I don't believe my behaviour and everyday lifestyle contribute to climate change
	scenv_fitl	4, 10		I would be prepared to pay more for environmentally friendly products
	scenv_noot	4, 10		It's not worth me doing things to help the environment if others don't do the same
	scenv_canc	4, 10		It's not worth Britain trying to combat climate change, because other countries will just cancel out what we do
Environmental Behaviour	envhabit1	1, 4, 10	5-point Likert: 'Never' - 'Always'	environmental habits: tv
	envhabit2	1, 4, 10		Switch off lights in rooms that aren't being used
	envhabit3	1, 4, 10		Keep the tap running while you brush your teeth
	envhabit4	1, 4, 10		environmental habits: heating
	envhabit5	1, 4, 10		environmental habits: packaging
	envhabit6	1, 4, 10		environmental habits: recycled paper
	envhabit7	1,4,10		environmental habits: shopping bags
	envhabit8	1, 4, 10		Use public transport (e.g. bus, train) rather than travel by car
	envhabit9	1, 4, 10		Walk or cycle for short journeys less than 2 or 3 miles
	envhabit10	1, 4, 10		Car share with others who need to make a similar journey
	envhabit11	1, 4, 10		Take fewer flights when possible

This table provides an overview of the UKHLS variables used to construct the main dependent variables: risk perception, climate change concern and pro-environmental behaviour. UKHLS Wave refers to the survey waves in which the variables were measured. Description provides the question text used in the UKHLS self-completion questionnaire

Table 8 Treatment Assignment

	Control	Treatment	Excluded
Flooding Sample	"Never treated' - No flood at any time in the 7 years before any interview date	'Treated'- Flood in the 7 years before Wave 4 or Wave 10 interview date	'Always treated' - Flood in the 7 years before first interview
Heatwave Sample	"Never treated' - No heatwave at any time in the 3 years before any interview date	'Treated'- Heatwave in the 3 years before Wave 4 or Wave 10 interview date	'Always treated' - Heatwave in the 3 years before first interview

Table displays treatment assignment approach for the flooding and heatwave sample, respectively

Table 9 Event Study Estimates

	Flood Exposure			Heatwave Exposure		
	(1)	(2)	(3)	(4)	(5)	(6)
	Risk	Concern	Behaviour	Risk	Concern	Behaviour
Lead 1	0.070		−0.031	−0.029		0.040
	(0.061)		(0.044)	(0.037)		(0.027)
Event	0.089**	0.058*	−0.010	0.051**	0.017	−0.014
	(0.037)	(0.035)	(0.032)	(0.023)	(0.021)	(0.019)
Lag 1	−0.033		−0.144	0.107		0.168***
	(0.100)		(0.090)	(0.070)		(0.062)
Individuals	3,761	2,972	3,761	8,198	6,804	8,198
Observations	9,443	5,944	9,443	20,496	13,608	20,496

Table reports event study estimates of flood exposure (columns 1–3) and heatwave exposure (columns 4–6) estimated using the regression estimator by Callaway and Sant'Anna (2021) with the matched sample. Lead 1 refers is the pre-treatment difference between treatment and control groups. Event refers to the instantaneous treatment effect (treated in Wave 4) and 'Lag 1' is the lasting effect of treatment in Wave 4, observed in Wave 10. later The dependent variable in columns (1) and (4) is a binary variable for climate change risk perception. The dependent variable in columns (2) and (5) is an index of climate change concern. The dependent variable in columns (3) and (6) is an index of pro-environmental behaviour. Dependent variables are standardised on the mean. Flood treatment is defined as living within a 1,000-metre radius from a flood extent and heatwave treatment is defined as having experienced at least three consecutive days of daily maximum temperatures greater than 29 °C. Standard errors clustered at the individual level in parentheses

*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

Table 10 Robustness: Different number of neighbours

	Flood exposure			Heatwave exposure		
	(1)	(2)	(3)	(4)	(5)	(6)
	Risk	Concern	Behaviour	Risk	Concern	Behaviour
NN Matching 1:1	0.071*	0.044	−0.034	0.055**	0.017	−0.001
	(0.041)	(0.040)	(0.036)	(0.023)	(0.021)	(0.019)
Ind	2,354	1,864	2,354	8,198	6,804	8,198
Obs	5,946	3,728	5,946	20,496	13,608	20,496
NN Matching 1:2	0.090**	0.072*	−0.025	0.055**	0.030	−0.014
	(0.039)	(0.037)	(0.033)	(0.024)	(0.021)	(0.020)
Ind	2,974	2,350	2,974	7,992	6,486	7,992
Obs	7,479	4,700	7,479	19,872	12,972	19,872
NN Matching 1:3	0.076**	0.058*	−0.025	0.055**	0.012	0.005
	(0.037)	(0.035)	(0.032)	(0.026)	(0.023)	(0.022)
Ind	3,761	2,972	3,761	7,022	5,614	7,022
Obs	9,443	5,944	9,443	17,180	11,228	17,180
NN Matching 1:4	0.079**	0.050	−0.017	0.049**	0.034*	−0.007
	(0.036)	(0.034)	(0.031)	(0.022)	(0.020)	(0.019)
Ind	4,479	3,535	4,479	9,958	7,892	9,958
Obs	11,221	7,070	11,221	24,540	15,784	24,540
NN Matching 1:5	0.077**	0.048	−0.013	0.045**	0.030	−0.006
	(0.036)	(0.033)	(0.030)	(0.022)	(0.019)	(0.018)
Ind	5,111	4,035	5,111	10,625	8,345	10,625
Obs	12,780	8,070	12,780	26,101	16,690	26,101

Table reports ATT of flood exposure (columns 1–3) and heatwave exposure (columns 4–6) estimated using the Callaway & Sant'Anna (2021) regression estimator under different matching regimes. The dependent variable in columns (1) and (4) is a binary variable for climate change risk perception. The dependent variable in columns (2) and (5) is an index of climate change concern. The dependent variable in columns (3) and (6) is an index of pro-environmental behaviour. Dependent variables are standardised on the mean. Flood treatment is defined as living within a 1,000-metre radius from a flood extent and heatwave treatment is defined as having experienced at least three consecutive days of daily maximum temperatures greater than 29 °C. Standard errors clustered at the individual level in parentheses

*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

Table 11 Robustness: Clustering

	Flood exposure			Heatwave exposure		
	(1)	(2)	(3)	(4)	(5)	(6)
	Risk	Concern	Behaviour	Risk	Concern	Behaviour
Cluster Ind. (P-value)	0.040	0.097	0.435	0.017	0.401	0.957
Nr. Clusters	3,761	2,972	3,761	8,198	6,804	8,198
Observations	9,443	5,944	9,443	20,496	13,608	20,496
Cluster LAD (P-value)	0.049	0.088	0.500	0.018	0.394	0.959
Nr. Clusters	331	325	331	346	345	346
Observations	9,437	5,940	9,437	20,472	13,592	20,472

Table reports the p-values of our main ATT estimated using the Callaway & Sant'Anna (2021) regression estimator under different clustering regimes. 'Cluster Ind.' refers to clustering at the individual level, used in the main analysis. 'Cluster LAD' refers to clustering at the Local Authority District level. The dependent variable in columns (1) and (4) is a binary variable for climate change risk perception. The dependent variable in columns (2) and (5) is an index of climate change concern. The dependent variable in columns (3) and (6) is an index of pro-environmental behaviour. Dependent variables are standardised on the mean. Flood treatment is defined as living within a 1,000-metre radius from a flood extent and heatwave treatment is defined as having experienced at least three consecutive days of daily maximum temperatures greater than 29 °C

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 12 Robustness: Always treated control group

	Flood exposure		
	(1)	(2)	(3)
	Risk	Concern	Behaviour
Event	0.128***	0.062	−0.006
	(0.042)	(0.038)	(0.037)
Individuals	3,711	2,553	3,711
Observations	9,094	5,106	9,094

Table reports ATT of flood exposure estimated using the Callaway & Sant'Anna (2021) regression estimator where the control group consists only of individuals who had previously experienced a flood in the 10 years prior to the observation period. The dependent variable in column (1) is a binary variable for climate change risk perception. The dependent variable in column (2) is an index of climate change concern. The dependent variable in columns (3) is an index of pro-environmental behaviour. Dependent variables are standardised on the mean. Flood treatment is defined as living within a 1,000-metre radius from a flood extent. Standard errors clustered at the individual level in parentheses

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 13 Risk of Flooding

	(1) Risk	(2) Concern	(3) Behaviour
Event	0.181***	0.068	0.092*
	(0.061)	(0.062)	(0.050)
Event × ROFRAS	−0.113*	−0.024	−0.126**
	(0.068)	(0.067)	(0.057)
N	9,611	6,224	9,611

TWFE OLS estimates of equation (4) using the conventional TWFE estimator. Event is the DID estimator capturing the treatment effect of flood exposure within a radius of 1,000 m for individuals living in areas with no objectively determined flood risk. Event × ROFRAS interacted with the DID indicator (Event) captures the difference between zero objective risk households and households living in areas at risk of flooding from rivers and seas (objectively determined). All models include individual and wave fixed effects. Standard errors clustered at the individual level in parentheses

*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

Table 14 Treatment Intensity

	Flood Exposure		Heatwave Exposure	
	(1)	(2)	(3)	(4)
Event	0.132***	0.095	0.034	0.384
	(0.050)	(0.077)	(0.070)	(0.308)
Event x MinDis	−0.011**	−0.000		
	(0.004)	(0.017)		
Event x MinDis^2		−0.001		
		(0.001)		
Event x MaxDur			−0.000	−0.176
			(0.016)	(0.153)
Event x MaxDur^2				0.021
				(0.018)
R^2-Adjusted	0.325	0.325	0.305	0.305
Individuals	6,001	6,001	8,468	8,468
Observations	15,076	15,076	21,036	21,036

OLS estimates of equation (4) using the conventional TWFE estimator. The dependent variable is a standardised binary variable for climate change risk perception. Event is the DID estimator capturing the treatment effect of flood exposure within a radius of 2,000 m (columns 1 & 2) or heatwave exposure (columns 3 & 4). MinDis and MinDis^2 interacted with the DID indicator (Event) capture the linear and quadratic effect of a 100 m increase in the minimum recorded distance to the flood event for treated individuals, respectively. MaxDur and MaxDur^2 interacted with the DID indicator (Event) capture the linear and quadratic effect of a one day increase in the maximum number of heatwave days experienced by treated individuals, respectively. All models include individual and wave fixed effects. Standard errors clustered at the individual level in parentheses

*$p < 0.1$, **$p < 0.05$, ***$p < 0.01$

Table 15 Major Heatwave Events

	(1) Risk Perception	(2) Concern Index	(3) Behaviour Index
Pre 2018	−0.026	0.019	0.017
	(0.023)	(0.023)	(0.020)
2018 *or* 2019	0.054**	−0.006	0.004
	(0.027)	(0.024)	(0.024)
2018 *and* 2019	0.140***	0.171***	0.109***
	(0.043)	(0.043)	(0.039)
Individuals	8,468	7,103	8,468
Observations	21,036	14,206	21,036

OLS estimates of a modified version of equation (3) with the matched sample using the conventional TWFE estimator. The dependent variable in column (1) is a binary variable for climate change risk perception. The dependent variable in column (2) is an index of climate change concern. The dependent variable in column (3) is an index of pro-environmental behaviour. The explanatory variables are DID indicators for heatwave exposure to pre-2018 heatwaves, either the 2018 *or* the 2019 heatwave, or the 2018 *and* the 2019 heatwaves, respectively. All models include individual and wave fixed effects. Standard errors clustered at the individual level in parentheses

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

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Declarations

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