In this study the authors plan to investigate A. whether climate concern triggers mental health issues or reduces well-being, and B. whether individual climate action and efficacy believes moderate this effect. They plan to analyze longitudinal data from the ALSPAC survey, where participants’ well-being and mental health (among other variables) were measured two times: once at baseline (between ages 21 and 25) and once at age 31-32. Additionally, climate concern was measured between these two times, at age 30. The authors plan to analyze the data to answer their two research questions analyzing a large sample (N~1,000, imputable to ~5,000 ) via linear regressions. In the causal estimation of the effect of climate concern on mental health/well-being, the authors are thoughtfully planning to rule out multiple confounding variables in their analyses, such as baseline mental health / well-being, offspring sex, ethnicity, relationship status, various measures of socioeconomic position, personality traits, and parental measures of depression, anxiety and socioeconomic position.

I find the author’s research questions very sensible and urgent, given the ever-increasing psychological issues related to the climate crisis, especially among the youth. I particularly like the constructive approach of looking at climate action and efficacy beliefs as moderators of the more severe psychological reactions. I think the results of this study can greatly inform educational interventions aimed at helping young adults cope with their climate distress, via empowerment and constructive engagement in action.

The causal-effect estimations (rather than hypotheses) are well-stated and capable of answering the research questions. The protocol is well detailed, enabling reproducibility. Indicative power analyses were performed, although it is unclear which effect size metric was used (see below). Interpretations given different outcomes could be better elaborated in the final table. The authors provide an extensive introduction of the topic, with a satisfying overview of the prior literature and motivation for their study (i.e. the lack of longitudinal studies connecting climate concern to mental health via causal estimation). There are all reasons to believe the proposed research falls within established ethical norms, as data collection was part of a bigger survey project Based at the University of Bristol (ALSPAC).

I list some comments below, that I hope may help the authors improve their design.

* I would be cautious calling your measured construct “climate anxiety,” rather than “climate concern.” I appreciate your explanation for why you still decided to use the term “climate anxiety;” however, I believe the literature itself is quite confusing on the use of this term, which for this study is particularly problematic, for the following reasons. First, climate anxiety is often characterized by clinical symptoms that are well beyond “worrying”, such as impairment in concentration and sleep, mood and emotionality, changes in diet, social isolation (e.g. [Doherty 2015](https://academic.oup.com/book/339/chapter-abstract/134989462?redirectedFrom=fulltext&login=true); [Hogg 2021](https://www.sciencedirect.com/science/article/pii/S0959378021001709)). These symptoms are not captured (and should not be assumed) by the question “how concerned are you about the climate crisis?” In the real world (outside of a research study) these more severe symptoms may not be alleviated by mere engagement in climate action (rather, they may require clinical treatment). So, if for example you found that the effects of climate concern on mental health are moderated by climate action (because it somehow “alleviates” concern) it would not be accurate to believe that action alleviates *anxiety* the same way (somebody who suffers from severe climate anxiety may need to step away from climate action for a while? Or maybe would need therapy in combination with action). Second, the term “anxiety” generally comes attached to mental health issues (in fact, you are using “generalized anxiety” as one of your mental health outcomes). So, by using this term you are already associating your *concern* construct to mental health, when (from what I understand) the goal of this study is to find causal links between these two variables. Third, “climate concern” is likely a much more common emotional state across the population, than the more severe climate anxiety. So, using “climate concern” would make your results more widely generalizable. I am thinking especially about your second research question, on whether climate action moderates the degeneration of concern into mental health issues; if this was the case, your results would suggest that all those concerned about climate (rather than the smaller number of people affected by eco-anxiety), would benefit from engaging in climate action; this is quite a bigger impact result that should not be understated!
* Page 6 line 213: I wouldn’t call this “secondary analysis” – this is part of your main research questions (question 2); therefore, it is primary analysis, correct? I point this out because I know in RR it is quite important to distinguish primary and secondary analyses for Stage 2 follow-up on the Stage 1 analysis plan.
* Hypothesis 2:
  + You may want to specify that you are looking at *individual* pro-climate action (as shown in supplementary table 1). This is important e.g. for those who might want to build interventions based on your results, using action engagement as coping mechanism to climate concern. Plus, we already know that collective action helps ([Schwartz 2022](https://link.springer.com/article/10.1007/s12144-022-02735-6), which you also referenced), so in a way you are adding an extra piece here, which is worth mentioning. (I’m very curious to see if individual action also helps; I have a feeling people are less encouraged/relieved by it compared to collective action because it feels so small impact, but we will see form your results!).
  + The way the climate action survey question is framed worries me that it only captured occasional action (maybe done once), rather than recurring actions. This may confound your results: i.e. you may find no moderation effect of action for the people who did those actions only once and lightmindedly, while there may be a strong moderation effect for those who did the climate actions repeatedly and with strong change-making intentions (the type of action engagement that relieves people and prevents degeneration of concern into mental health). But mixing these participants up may water-down your moderation effect – maybe worth mentioning if you do not find one?
* Power analysis. It is unclear what effect size metric you are using in your power analysis. There is only the number but not the effect size symbol…is it Cohen’s d? Cohen’s f? eta^2? Partial eta^2? This information determines how large/small an effect size of 0.25 is, and therefore how big of an effect size your study is powered to detect.
* Another threat to causality that was not mentioned is that climate concern was not measured at age 25. If mental health was good at 25 (and climate concern, which was not measured, was low at 25), then climate concern high at 30, and mental health bad at 32, then you found your causal relationship (increase climate concern = worsen mental health). But if climate concern was already high at 25, and all the other variables kept the same, it might not be climate anxiety that changed mental health! In other words, for better causal inference you would need to see a change from low to high climate concern and a corresponding change from good to bad mental health. Now, the ALSPAC survey study was run, so there is not much to do about this (unless there was another time point at which climate concern was measured near the baseline period? But I imagine you already thought about this; if so maybe you could just mention this in your limitations?).
* One further question is about generalizability. The ALSPAC survey focuses only on Avon population, and it should be mentioned in the limitations that the results should not be assumed to necessary apply to other cultures or locations.

**More stylistic notes:**

* Page 3, line 79: APA citation style could be used to reference the news articles.
* Page 3, line 85: “as summarized by Clayton and Coffey (Clayton, 2020; Coffey et al., 2021)”
* Page 3, line 90: watch out for double parentheses, in the rest of manuscript as well
* Page 5: I found the description of the sample quite redundant (like the information about the mothers and about the offspring stage of the participants), but maybe this is costume in developmental psychology (which is not my field of expertise). Either way, I would start the paragraph with “The current research focuses on the ALSPAC offspring generation;” as it is now, it seems that the sample was recruited just for this study.
* It would help to state in the text the age window when the baseline variables were measured, at least for the well-being and mental health variables measured between age 21-25 (it took me a while to see that these were listed in Table 2, and having this info in the text would facilitate the immediate grasping of your cool longitudinal design!). Maybe you could add it in Fig. 1!

I hope these comments will be helpful, and looking forward to the authors’ reply!

Best,

Anna