1 Reviewer #1

International Attitudes Toward Global Policies

This study investigates the level and predictors of public support for the Global Climate Scheme which consists of introducing a global carbon pricing mechanism whose revenues would fund a universal basic income. The analysis of survey data from over 20 countries suggests that the policy finds supermajoritarian support in Europe and majority support in the United States. The main conclusion is that the Global Climate Scheme is preferred by a robust majority across countries and that "the expressed preference is sincere".

1. The results could be very context-dependent because existing conjoint-experimental research finds that this sensitivity is absent when voters can also select tax plans that would benefit the poor (see Ballard-Rosas et al. 2017 JOP) and because the impact of a policy on the richest individuals does not seem to drive preferences in other policy fields (Bechtel and Liesch 2020 POQ).

Because of attitudinal ambivalence (the fact that people hold conflicting values and exhibit different context-dependent social identities) and changes of opinion, it is well known that people's attitudes are context-dependent. We actually study in the paper the extent to which attitudes towards the Global Climate Scheme (GCS) would change followed a negative media campaign (subsection "Pros and Cons").

Yet, the claims that the support for the GCS contradicts the existing literature is unsubstantiated. First, we show in our literature review that people favor climate policies that are global and equitable, and that our results align well with the only previous survey that studied global carbon pricing (Carattini et al., 2019). Second, while the GCS benefits the poorest humans, our conjoint analyses already include policies benefiting poor fellow citizens (higher minimum wage, improved public services, higher welfare benefits) and we confirm that these policies are the most popular. Even though Ballard-Rosa et al. (2017) only compares different income tax schedules (and has thus nothing to say about the relative preference for global redistribution and national redistribution), our conjoint analyses allow to compare the relative preference for the GCS and a national redistribution scheme (Figure 8). In Spain and in the U.S., the GCS has no significant effect on the likelihood that a platform in preferred; while in France, Germany and the UK it has a positive effect, larger than the national redistribution scheme. Third, our results are perfectly compatible with those of Bechtel and Liesch (2020). This excellent conjoint analysis paper shows that American people care about the effect of a generic policy on the poorest fellow citizens about half as much as on their income, and that they care more about losses than gains. Extrapolating from these findings, we can infer that Americans would be indifferent between the status quo and a policy that costs them $1,000 per year while increasing the income of the poorest by $5,000. Or that a majority would support a policy that doubles the income of the poorest (Americans) and reduce their income by $1,000 per year. This is close to what we find in a global context, as the GCS would double the income of the poorest billion humans while costing about $1,000 per year to the typical American per-

son. Fourth, our results do not imply that the support for global redistributive policies is driven by their impacts on the richest individuals, contrary to what is claimed, as all the tax policies we test specify the revenue-use. Not mentioning that some previous papers have shown aversion for topmost inequality (e.g. Fisman, Kuziemko & Vannutelli, JEEA, 2020).

2. The authors highlight that there is no "significant policy proposal" on the table but that could also be a weakness of the study: did individuals know anything about this somewhat complex policy reform? Relatedly, how plausible is it from the perspective of both mass publics and experts that such a global policy would ever materialize?

First, several policies that we test are already ongoing or on the table: e.g. international transfers for climate loss and damages, $100 billion per year in international transfers to help low-income countries adapt to climate change, and increasing foreign aid. All these policies obtain a similar level of support than options not currently on the agenda like the GCS. Second, we informed respondents about the policies studied, and some of them are pretty straightforward to understand (e.g. taxing the wealth of millionaires to finance low-income countries). Third, while the most significant policy proposals (in terms of magnitude of international transfers) are currently not on the table, the implausibility from them ever materizalizing may precisely come from the fact that they are not on the table and that public support for such policies is not discussed. We could return the question: why global redistribution is still considered as implausible if majorities throughout the world support it? This unresolved question should pave the way for further research.

3. There is a significant disconnect between the claims about support for the global climate scheme and the actual survey item meant to elicit preferences over such a policy. That item merely asks "at which level(s) do you think public policies to tackle climate change need to be put in place?" (lines 80-81). But asking such a broad and amorphous question does not seem to be informative about the level of support for a rather specific, costly, and strongly redistributive global reform. It could be useful to focus more directly on the studies that are clearly focused on the actual policy question.

The global survey not only asks this "broad and amorphous question", but also questions on the support for specific policies such as a global tax on greenhouse gases financing a global basic income. Contrary to what is claimed, the paper does not measure the support for the GCS with the question on the level at which climate policies are needed.

4. Similarly, it remains vague how preferences over dividing a carbon budget relates to the research question/claims. Also, one wonders whether these findings about burden-sharing resonate with existing research on this topic. The SI section A.1.2 is quite eclectic about how it portrays existing research on this topic and that review includes "staff discussion notes"papers instead of focusing on high-quality public opinion work published in expert journals. Think of the increasingly large literature on cost aversion and compensation (e.g., Gaikwad et al. 2022 APSR and the literature cited therein).

Attitudes towards burden-sharing are within the scope of attitudes towards

global climate or redistributive policies. Section A.1.2 provides a comprehensive review of attitudes on how to share the burden of climate mitigation between countries, and shows that our results align well with the existing literature. We thank the reviewer for pointing out the typo regarding the outlet of the IMF paper by Dabla-Norris et al. (2023), which failed to mention the institution. We disagree that this paper should not be cited: although it belongs to grey literature, it conducts representative surveys over 28,541 respondents on 28 countries and represents one the most large-scale evidence on the question at stake. On the contrary, the paper mentioned (Gaikwad et al., 2022) addresses the allocation of carbon tax revenues within a country and has nothing to do with burden-sharing between countries. Perhaps the reviewer was thinking about Gaikwad, Genovese & Tingley (unpublished, 2023)? In short, the latter paper abstracts from the concept of burden-sharing and shows that Americans prefer to decarbonize the U.S. rather than India, and that Indians prefer a slow domestic decarbonization rather than a rapid one led by a foreign entity. This conjoint analysis paper only reports the effects of some features of a (sometimes global) climate policy on the support for it but does not report the base level of support for the policy. Therefore, it is of little help to uncover the level of support towards burden-sharing principles.

5. A key question is whether the predicted levels of support are plausible, especially since the policy would entail considerable redistribution that would require exceptional and maybe even unprecedented levels of international administrative coordination with respect to both the extraction of resources and their re-allocation. I currently am missing a strategy to address the argument that the high levels of support reflect the fact that many respondents simply discount the reform as being purely speculative, having a close-to-zero probability of ever materializing, and lacking any clear (personal) costs above and beyond that any costs would fall on the very rich and respondents therefore think they would be net beneficiaries. The importance of (personal) costs has been documented in existing experimental global climate policy research and the findings suggest that a moderate increase in personal costs could lead to sizable shifts in public support even if there is a causally identified preference for allocating costs in line with fairness norms such as the ability-to-pay principle (see Bechtel and Scheve 2013 PNAS). This is in some sense consistent with the descriptive patterns report in Figure 2 and it would be helpful to make this explicit and think about possible explanations. Overall, however, it seems that any study intending to generate predicted levels of support for the types of global and unprecedented policies would have to carefully account for cost sensitivities.

That some of the policies we test have low chances of materializing might be a reason why people would overstate their support. Yet, it seems that with all the results we provide, the burden of proof for such a claim is now shifted to people (like the reviewer) who remain unconvinced that there is strong and genuine support for global redistribution. Indeed: First, the support is about as high for distant policies like the GCS as for existing ones like the $100 billion of international transfers for climate finance. Second, our results show that in some European countries like France, vote intention for a progressive candidate may

increase by 11 p.p. if they propose the GCS, which may be the most compelling evidence that the support is profound. Third, the reason why people could overstate their support in case the policy has few chances to occur is probably a social desirability bias. Yet, our list experiment shows no social desirability bias in favor of the GCS.

On the question of personal costs, our results are in line with the literature, which shows that support for a climate policy depends on the perceptions that it is fair, effective and in one's interest (e.g. Douenne & Fabre, 2022). Because it is global and corresponds to an equal right to emit for each human, the GCS is by construction fair and effective. On the other hand, it is costly to people in high-income countries. This explains why the policy is less supported in countries (like the U.S.) where costs are higher. This also explains why policies like the global tax on millionaires obtain more support: it involves no direct costs (only opportunity costs) to the respondents. Also, note that respondents support only *some degree* of global redistribution: if 90% of the respondents prefer to allocate some positive share of a global wealth tax revenues to low-income countries, the median preferred share for low-income countries is 30%, not 100%. Although this would represent an unprecedented international transfer (of about 0.7% of high-income countries' GDP), this would still be a low amount compared to the magnitude of public transfers that people consent to within a country. If the burden of a wealth tax is concentrated on the top 1%, the massive public support should not come as a surprise. Rather, the conundrum becomes why such a policy is not being implemented: as a frequent argument against a rich tax is that wealthy people would flee to another country, it is totally understandable that the support is high for a global wealth tax, which would address this concern.

6. This is related to the potential role of the list experiment as an attempt to address social desirability concerns. This problem would be particularly relevant if a policy is highly ethically sensitive or very politicized. But the main concern with the current study would be about the lack of a clear and somewhat attributable cost component. Also, Table 1 does not provide any information about what is being reported here and how the quantities have been estimated which is inconsistent with accepting transparent reporting standards. It is also somewhat misleading to claim that any differences here would indicate (through the wording selected for this table) that these reflect social desirability bias. There could be many alternatives explanations such as interaction effects between the proposed alternatives, e.g., beliefs about the political and economic consequences of pursuing multiple policies simultaneously that are unrelated to social desirability.

We do not agree that the GCS suffers from "the lack of a clear and somewhat attributable cost component". We make clear to the respondents that the policy would be costly to them (e.g. $85 per month for a typical American), and verify that people understand this cost with an incentivized comprehension question (after which we give the correct answer that the typical person in their country would lose).

We thought that Table 1 was self-explanatory in that it showed the regression

results of the number of supported policies on whether the list includes the GCS, plus some extra elements: the support at the simple stated question, the social desirability bias (which is the difference between the coefficient of the regression and the stated support) and the confidence interval for the social desirability bias. We thank the reviewer for pointing out that the Table deserves a explanatory note. We will add such a note to explain how to read the Table and report the method used (currently only in the section Methods).

List experiments are commonly used to measure social desirability bias (e.g. Kuklinski et al., JoP, 1997). The question does not specify whether the policies of the list would be implemented or not, let alone simultaneously. That the support for some policy in the list might spill over to the support for the GCS has no consequence on the result of the list experiment, as the other policies are present in the lists of both branches. Finally, the results from our first conjoint analyses show no interaction effects between the policies present in the list. In short, consistently with the literature, our list experiment is used to measure a potential social desirability bias.

7. It is not obvious how deviating from the standard conjoint design (lines 239-244) by adding the points component to assess a selection of previously shown profiles improves over existing work. To be clear, there could be a good reason for this design, but one would have to make that reason explicit. Moreover, these results again show that taxing the rich is the overriding concern. Since this finding conflicts with what we know about tax policy preferences (at least in the US, see Ballard et al. 2016 JOP; Scheve/Stasavage 2022 CPS), one would have to acknowledge this discrepancy which raises questions about the validity of the findings. One could begin exploring this issue by analyzing the results for the US separately although there are obvious limits to what can be realized within the current design framework due to comparability issues.

Our question on the prioritization of policies is *not* a conjoint analysis. The prioritization allows inferring individual-level preferences for one policy over another, while a conjoint analysis only allows inferring individual-level preferences for one platform over another or collective-level preferences for one policy over another. Also, by comparing platforms, conjoint analyses may be subject to interaction effects between policies of a platform (which can be seen as complementary, subsitute, or antagonistic) while the prioritization frames the policies as independent. That being said, we agree that both methods yield similar results. We used both to get more reliable results but we are ready to drop the prioritization from the main text if needed.

There is no such paper as Ballard et al., Journal of Politics, 2016, and we do not really see what paper the reviewer refers to. As for Scheve & Stasavage (2022), this paper does not provide any survey evidence and simply highlights three explanations for why we do not observe more wealth redistribution: that other issues are more important to citizens, that citizens do not support wealth redisitribution, and that decisions are not taken democratically. The first and third explanations are compatible with our results: on the one hand, policies such as a higher minimum wage or improved public services are generally seen as more important as a wealth tax; on the other hand, taxing the rich may be

hindered by tax dodging and the lack of tax cooperation between countries, and given the electoral system, the majority may not have the final word in countries like France or the U.S. The second explanation does not seem to hold: previous surveys also find that majorities support redistribution, even in the U.S. For example, ISSP (2019) finds that in each of the 29 surveyed countries, a majority agrees that "It is the responsibility of the government to reduce the differences in income between people with high incomes and those with low incomes", with 70% agreement and 14% disagreement overall.

We note that the reader can already find the results for the U.S. in Supplementary Figure A29. We see that wealth taxes rank high in the prioritization of policies, right after funding affordable housing, student loan forgiveness and universal childcare (not mentioning that wealth taxes would be complementary to these policies).

8. There is also the issue of effectiveness and financing portion of such a global policy scheme. How plausible is it that taxing the richest would generate sufficient revenue? Moreover, one would want to know more about the mechanisms that explain mass preferences. For example, to what extent is it those who care strongly about addressing global warming who support drastic reforms?

Capping carbon emissions is widely seen as an effective and proven solution to reduce carbon emissions, as exemplified by the EU emissions trading system. The estimation of the global basic income that the GCS would finance is explained in Appendix E. The main hypothesis is a carbon price of \$90 per ton of CO_2 in 2030, in line with common model projections (Stern & Stiglitz, 2017).

We thank the reviewer for the suggestion to study the beliefs surrounding the support for the GCS. We can add a regression table analysing the attitudinal correlates of the support for the GCS.

9. The study connects a wide range of policy fields (tax policy, global climate action, novel decisionmaking bodies, foreign aid, . . .) and this renders the paper difficult to digest and the main contribution appears (unnecessarily?) somewhat superficial.

We realize that the wide coverage of the paper and its large number of results render it difficult to digest. Still, our view is that the overarching result that majorities support global redistribution is better proven if we present all results pertaining to these topics at once.

10. Is all the material presented sufficiently original given that Figure 1 is taken from an NBER working paper? Also, I am wondering whether the referencing is correct since NBER working paper 1714 is from 1985 and on "Does Deductibility Influence Local Taxation". Instead, Figure 1 seems to come from the "OECD Economics Department Working Paper, 1714." The figure has also been used in an 2022 op-ed, see: https://cepr.org/voxeu/columns/fighting-climate-change-international-attitudes-toward-climate-policies

We thank the reviewer for pointing out that the number 1714 mistakenly referred to another working paper series of that paper. We note that Figure 1 does not appear in the NBER nor in the OECD working papers. Contrary to what the reviewer says, it does not appear either in the VoxEU article (the reviewer has probably confused Figure 1 with the figure on *national* climate

policies presented on VoxEU). We acknowledge that Figure 1 appears in the appendix of the version of Dechezleprêtre et al. (2022) on the website of Adrien Fabre (and that figure is never commented in that paper). If it is important that this Figure be not reproduced, we can make sure that it is not present in the final publication (which should come out in the *American Economic Review*, given a favorable R&R decision).

11. Overall, the study and the survey design remains opaque at times. For example, the survey structure seems to have included so many conditions and skip-/assignment logics that it is hard to make sense of the results in terms of experimental conditions and potential order effects. It is also unclear how the Figure A8 results should be interpreted given that it includes a range of policy reforms across multiple issue areas.

The survey structure is presented with schemas in Appendix D.

We thank the reviewer for pointing out that the survey design remains opaque, and we will make our best to improve the description of the design. For example, we will make clear that the different treatments are independent from one another. We can also add some balance analyses and placebo tests of the effects of each treatment on unrelated outcomes.

Figure A8 is a conjoint analysis that mimick plausible choices between two platforms of progressive candidates (stemming from different left or center-left parties in Europe, or different candidates at a U.S. democratic party's primary). The goal of this conjoint design is precisely to assess how global redistribution compares to other issues like national climate or social policies. Its coverage of multiple issues is a feature, not a bug.

12. Lastly, the title could be more informative about the content of the study. Also, it would be useful to convey the key finding or relationship that is investigated in the study in the title.

We thank the reviewer for this suggestion and are willing to consider changing the title to, e.g. "Global redistribution is genuinely supported by majorities worldwide". We thought that a more neutral title would be preferrable but are open on this question and will consult the editor.

2 Reviewer #2

The paper is in many ways a fun and interesting read, but my sense is that it doesn't reach the general interest bar of Nature Sustainability. A main reason behind me reaching that conclusion is that the policies discussed in the papers are generally formulated in very abstract terms. In the demand for redistribution literature one generally observes high agreement to general statements about the importance of things like increased equality and environmental sustainability. However, when policies become more concrete, and when it becomes clear who will actually be impacted, there is generally much less support. There are also often treatment effects from e.g. information present in the abstract, but not for concrete policies. For this reason, the generalizability of findings similar to the ones presented here are limited. This does, in itself, not preclude the paper from

being published, but it diminishes its external validity significantly.

We thank the reviewer for finding our paper interesting.

We disagree with the reviewer that the policies discussed are "formulated in very abstract terms". We refer to the questionnaires in Appendix C and D, where one can read the wording of each question. The policies we test are well-specified and we even provide an assessment of the individual cost of the Global Climate Scheme and the revenues generated by a wealth tax. For some reason, the reviewer seems to believe that the GCS is not a concrete policy (although a variant of it has been discussed in international climate negotiations as soon as 1990, see Appendix A.2.1) and would have liked that our treatments relate to a more concrete policy (presumably the global millionaire tax). The reason why we focused on the GCS is that we had good reasons to think that it would be less popular than a global millionaire tax, given that it would come with a financial cost for most respondents. Therefore, if the support for a costly policy like the GCS was found to be genuine using our experiments, this result could reasonably be extended to more consensual policies like the millionaire tax.

It may be true that support for global redistributive policies would be lower if they were becoming more concrete, but we cannot test that until they enter the public debate. We used all the methods available to us to test whether support for global redistributive policies is genuine. Our paper indicates that such policies may be supported once they enter the public debate, and this would be the first step to test whether attitudes will change once global redistribution is debated. To us, it is a result in itself that such policies are supported, even if this support might prove transitory. Preventing the publication of such a result on the ground that opinions may change means preventing the publication of any research on attitudes towards global redistributive policies, and more generally any policy that is not currently prominent in the public debate.

Finally, we have a different perspective on the question of the generalizability of the findings. By providing evidence on the support for various global redistributive policies in 20 countries and using a range of methods to test the sincerity of answers, we do think that our results are externally valid. For example, we expect strong majority support in a country like the Netherlands for a global tax on top incomes (say, over $1 million per year) to finance vaccination campaigns in low-income countries or UN agencies like the World Food Program, even though that specific country or this specific policy were not tested.

3 Reviewer #3

I'm not an expert in several of the disciplines relevant to this paper (namely, quantitative social science and political science involving surveys). I defer to other experts on whether the methods here are correct, including preregistration, etc.

I also partly defer re the paper's novelty, but if the work here has not already been done elsewhere, then it seems of sufficient novelty to warrant publication in Nature Sustainability.

I advocate for a revise and resubmit based on what I am knowledgeable about, as my impression is that this article provides useful and important new knowledge – with the proviso that the methods are approved by other reviewers, and that other reviewers identify no good reason to doubt it is above the minimum bar of novelty. From a political point of view, it is important to know how many people say they would support different policies, including the GCS policy that is one of the authors' focal points, and how robust those responses appear to be. From a wellbeing, equity, and poverty alleviation point of view, the authors' numbers check out regarding something on the order of $30/person/month in global carbon revenue would be generated with the magnitude of carbon pricing they consider, which could have important poverty alleviation implications if invested in pro-poverty alleviation ways.

At the same time, there were a few questions I had that seemed important to clarify in the text – some important issues are:

1. Do the authors' poverty calculations tacitly assume that the global poor do not bear any of the costs from large carbon pricing schemes such as GCS, and somehow only receive benefits via the equal per capita distribution of the revenues? If so, this might appear to some readers to be a complication for their poverty calculations, because realistically the poor will bear some of the mitigation costs – instead, the correct focus for poverty calculations would be the net benefit, ie for a representative individual below the poverty line, we need to know that individual's net benefit = the carbon dividend they receive minus the additional mitigation cost they bear from the carbon pricing scheme (due to decreased economic growth affecting them, higher fuel and other costs of goods, etc). In other words, if the carbon dividend for those below the poverty line is $30 per person, we then need to subtract from that amount the increased costs for those individuals from higher priced energy, goods, foregone wages etc in order to know how much better off they are on balance under the carbon pricing scheme. Without anticipating this issue, a skeptical reader might worry that these costs turn out to be a high percentage of the benefits from the carbon dividend, thus undermining the reported poverty alleviation benefits.

If the authors have already accounted for mitigation cost in their poverty alleviation calculations, they should state this and briefly explain this in more detail in the text. Currently, the authors seem to say only that "The 700 million people with less than $2/day would be lifted out of extreme poverty, and fossil fuel price increases would cost the typical person in their country a specified amount (see Supplementary Section D)." However, supplementary Section D does not explain anything relevant to this issue (as far as I see), but rather simply explains how different survey respondents in rich countries are told that their own consumption will be decreased by additional mitigation cost in their own rich country.

If I've not misunderstood the situation, and the authors have in fact not accounted for the mitigation cost burden for the poor, perhaps a solution is to cite a paper that quantifies that the net benefits will be large for those at the bottom of the income distribution from a scheme like GCS, and thus that the net benefits are large as the benefits for the poorest greatly exceed the mitigation

costs. For one specific set of results that does this, in a paper the authors already cite, see Supplementary Figure 4 (especially panel C) of Budolfson et al "Climate action with revenue recycling has benefits for poverty, inequality and well-being".

The average carbon footprint of Sub-Saharan Africa is $0.75tCO_2$ per capita[1], and it is even lower for people living in extreme poverty, under one tenth of the world average of about $5tCO_2$ per capita (Chancel & Piketty, 2015). Under a GCS with a cash transfer of \$30 per person, if one's emission is one tenth of the world average, their net gain would be \$27 per month in nominal terms. In regions with extreme poverty like Sub-Saharan Africa (excluding high-income countries), the conversion factor from Market Exchange Rate to Purchasing Power Parity (PPP) is 2.4[2], so that the net gain is \$65 per month (or \$2.13 a day) in PPP. This is enough to lift out of extreme poverty anyone with less than \$2 a day in PPP (the poverty line is commonly given in PPP).

Admittedly, the net gain could be lower due to inflation or general equilibrium effects. However, increase in food prices woud probably be temporary (the time that supply adjusts to a higher demand) and increased investment capacity of the poorest humans would actualy foster growth in these regions rather than reduce GDP compared to a business as usual (BAU) scenario (note that there is no international transfers in a BAU scenario).

The method to compute the net gain and the PPP adjustment is described in Supplementary Appendix E (referred to in the sentence preceding the one cited by the reviewer). However, we did not report the net gain estimation for someone living in extreme poverty: we will add that to the Appendix.

We thank the reviewer for the reference of the Appendix of Budolfson et al. (2021). This paper finds a per capita dividend in the same order of magnitude as ours, at \$19 per month. Furthermore, once we account for the PPP adjustment, the effect we find on extreme poverty is in line with the calculations of Budolfson et al. (2021). As our computations already account for the cost of carbon pricing borne by poor people, we see no need to use the calculations of Budolfson et al. (2021) instead of ours.

2. A further issue is that the authors could be more clear about how many people they claim would be lifted out of poverty by their central GCS scenario. Here is a simple way of explaining one reason why this is important and involves a further complication: even if we ignore the issue in the previous comment and thus focus only on the per capita carbon dividend, by the authors' own lights the carbon dividend is only \$1/day (= \$30/month). So, if we want to know how many people will be lifted out of poverty, where that means living on more than \$2/day, it is not obvious how many people that would be if all we know is that every person will get an additional \$1/day? (It depends on how many of the poorest people are close enough to the \$2/day line that the extra amount puts them over that threshold. In addition, this illustrates the importance of the issue in the previous comment, since if the net benefit for each person in poverty is less than \$1/day, this will also effect how many people are raised for the first

[1] https://www.macrotrends.net/countries/SSF/sub-saharan-africa-/carbon-co2-emissions.

[2] See https://data.worldbank.org/indicator/NY.GDP.PCAP.PP.KD?locations=ZF&most_recent_value_desc=false and https://data.worldbank.org/indicator/NY.GDP.PCAP.KD?locations=ZF&most_recent_value_desc=false.

*time above the $2/day threshold.) The authors' seem to assert without further analysis that "The 700 million people with less than $2/day would be lifted out of extreme poverty". (see textbox – note that this quote from the authors most naturally is taken to be saying that *everyone in the world* would be lifted out of extreme poverty, but it is unclear why we should be convinced that is so for the reasons just given.)*

The data underlying the Budolfson et al paper referenced above would answer these questions, but for some reason their paper does not seem to have reported those specific numbers anywhere (ie it does not specifically report what % of those below $2/day would be lifted above $2/day in a GCS-like scenario). Perhaps these numbers are available from another published paper, or perhaps Budolfson et al would be willing to provide them if useful.

We confirm that everyone in the world would be lifted out of extreme poverty if the GCS is successfully implemented (i.e. if every human receives the basic income). The confusion stems from the PPP adjustment, that is only mentioned in a footnote in Supplementary Section E that the reviewer has probably missed. We can mention this PPP adjustment more prominently in the text.

3. As a side-note, perhaps worth somehow bringing out the subtlety of this a bit: if we change the world so that everyone who was previously living on less than $2/day is now living on $2.50/day, it is not clear that we have really ended extreme poverty (as the $2/day threshold does not mark a deeply precise and dramatic distinction in quality of life)?

This is a fair point. We used the common definition of extreme poverty by the World Bank, which is necessarily arbitrary. We acknowledge that the GCS would not eradicate poverty (only extreme poverty defined at the $2/day threshold). Still, we note that the GCS would benefit the global poor, defined as anyone with a carbon footprint lower than the (ex post) world average.

4. Within Fig 1 and Supplementary Information Fig S1, the authors say that the GCS scenario divides a 2deg global carbon budget into "tradable country shares". Can the authors explain explicitly how these "country shares" are determined and thus how this is equivalent to the GCS scenario described in the main text (since the text does not otherwise refer to "country shares" as far as I can see). Eg is each country's share of the total global permits simply that country's % of global population? Simply adding that information would be sufficient (if that is correct) – but this isn't completely clear from the text.

As shown in Supplementary Section C, the first question on the global emissions trading scheme did not specify how country shares would be allocated between countries. It was meant to be implicitly conveyed by the sentence "Following the support for the global quota, respondents are asked about their preferences for dividing the carbon budget among countries", but we acknowledge that this may not have been clear. We can add some words to clarify this above in the text.

5. The figures in the main text say that "The numbers represent the percentage of somewhat or strong support, after excluding indifferent answers." Is it a strong custom to report results in this way in this kind of context? It seemed odd to me, as it seemed to inflate the levels of support. (It is also a little opaque

to the reader given it is the only way of reporting things in the main text, given that an unknown and arbitrarily large % of the population could be indifferent – eg if 99% are indifferent, and the remaining 1% are all in favor, that would be reported as 100% in favor?).

I defer to the customary norms of how to report things if there is a strong customary norm in favor of reporting the data in the way the authors have. Otherwise I'd prefer the absolute % as reported in SI A3. (The authors could also consider reporting both numbers in the main text together, with the coloring based on one metric, but perhaps add the other in parentheses, eg a question that has a relative % of 84 and absolute % of 64 could be reported as "84 (64)".)

It seems to us that there is no customary norm on the matter. We share the opposite perspective that putting the emphasis on absolute levels would downplay the support, as an expedient reader might assume that all those who do not support actually oppose the policy. Displaying the absolute level of opposition may also be an interesting alternative. In any case, it is important to report both the absolute and relative levels of support in separate figures and to do it at least for one policy in the main text, so that the readers get a sense of the share of *indifferent* answers. This is already what we do with Figures A3 and A25, and as we write "the global quota obtains 64% absolute support and 84% relative support". We can add the absolute level of support for the support for loss and damages compensatio, the only other policy (asked with an option for indifference) for which we report the relative level of support.

6. A small issue: I thought GCS should not be described as a "Climate Club" (see textbox pg 8), as in the literature a climate club typically refers to a proper subset of nations that coordinate action together either to benefit themselves directly in some way, or to indirectly benefit themselves by getting other nations to deepen their mitigation efforts? (I don't think this is anything to worry about in the survey, just could come across as a bit of a distracting misnomer.)

To us, the GCS perfectly fits with the definition of a climate club: carbon pricing within the club and carbon tariffs at its borders. If there is a misnomer, it would rather be in the term "global" as the GCS may only be "international", i.e. not encompassing all countries. It is too late to change the name used in the questionnaire though.

7. Perhaps in the main text the authors should more explicitly situate their contribution relative to Carattini, S., Kallbekken, S. & Orlov, A. How to win public support for a global carbon tax. Nature 565, 289–291 (2019). This doesn't require any big reframing or revisions, but perhaps just a sentence or two?

In the literature review (Supplementary Section A.1.1), we write that "Carattini et al. (2019) test the support for six variants of a global carbon tax on samples in five countries, representative along gender and age. For a given variant, the sample size is about 167 respondents per country". Implicitly, our contribution relative to Carattini et al. (2019) is to have large-scale samples, representative along more dimensions than just gender and age (also income, diploma, etc.), and to test an emissions trading system (involving a cap on carbon emissions) rather than a tax. We can add these contributions more explicitly.

8. I thought for a broad audience it could be explained a little more explicitly what a "real-stake petition" is, and same for the other terms in this key sentence: "By employing a list experiment, a real-stake petition, and conjoint analyses, our study indicates genuine and robust support for the GCS among respondents. For example, the conjoint analyses provide evidence that political parties would not lose vote intention by endorsing the GCS." Explain and connect the dots more explicitly for a broad audience about what these things mean, and how they imply the conclusions the authors claim.

We thank the reviewer for pointing out passages in the text that are not clear to a broad audience. We will make our best effort to improve the wording while respecting the length constraint.

9. Regarding why politicians do not advocate GCS, the authors say "we hypothesized pluralistic ignorance, i.e. that the public and policymakers mistakenly perceive the GCS as unpopular". I thought a different approach might be for the authors in this article to be more agnostic about the explanation absent much deeper political science analysis, while noting that there are several plausible explanations, including this one? (It seems to require much more argument to advance the authors' explanation, which is not the focus of the paper here.) Eg a different explanation is that politicians are focused on marginal voters who might be indifferent in the majority, but in some cases would see support of GCS as a reason to veto the candidate whereas failing to support GCS as a main plank in their platform is not seen as a reason not to support the candidate. Other explanations are possible.

We should perhaps clarify in this passage of the paper that pluralistic ignorance was just one of the pre-registered hypotheses that may have explained the absence of global carbon pricing from the public debate.

We test other hypotheses throughout the survey (low stated support, lack of sincerity in stated support, conflicting core values), including the reviewer's hypothesis that the GCS would not bring electoral gain to a candidate. We reject the latter hypothesis with our conjoint analyses.

10. This doesn't necessarily require any changes, but wanted to note that the authors say: "A global quota with equal per capita emission rights produces the same distributional outcomes as a global carbon tax that funds a global basic income." In theory yes, but what about in practice? Some might think that these differ in attractiveness in reality, perhaps due to real-world complications of the sort that are the focus of political economy, administrative feasibility, etc. (Eg if experts were asked the same survey questions, they might differ in their support for each based on these considerations.)

The cited sentence is meant to introduce the two factors that explain the different levels of support between the global quota and the carbon tax funding a global basic income: framing and difference in the policy properties. Admittedly, we only cite the difference in effectiveness as this is the most prominent difference between a quota (where emissions are fixed) and a tax (where they are uncertain). We could also reword the sentence to acknowledge that other potential differences in the perceived features of both policies might play a role.

Thanks to the authors for this excellent contribution.