"Yellow Vests, Carbon Tax Aversion, and Biased Beliefs" is a very interesting paper on a clearly important policy questions.

The authors lay out two main contributions of their paper.

The first contribution is policy in nature; they seek to bolster our understanding of "the political economy of climate policies." In particular, they argue that they provide better methodology to answer this question than prior work by noting that "We contribute to the literature by providing robust evidence for causal effects where past studies essentially show correlations, often relying on proxies such as fuel consumption to proxy self-interest (e.g. Thalmann, 2004; Kallbekken & Sælen, 2011; Anderson et al., 2019). In contrast to such shortcuts, we do not assume that people are fully rational nor have perfect information. Thus, our methodology offers a novel look at the political economy of climate policies, as it allows one to disentangle erroneous beliefs from pure effects of preferences..." In terms of making a better methodology argument, however, it is worth noting that their survey results are, as far as I can tell, non-incentivized (although this should be made clear, as the discussion of WTP in Appendix F as well as other discussions in the paper give the opposite impression). How confident can we be in participants' stated preferences or beliefs? I appreciate their effort to have a representative sample, but it does seem to come a cost. That the authors do not focus on the exact policy proposed by the government (i.e., they "focus on a “Tax & Dividend” carbon tax with uniform lump sum compensation, which allows one to specify clearly the distributive effects of the policy, in contrast to the policy abandoned by the government") also is one argument in favor of why they should look at this question in a more controlled environment.

The second contribution is related to the literature on motivated and biased beliefs. The contribution to this literature seems to be context-related (rather than mechanism-related) though. The authors note: "An important stream of literature in economics has studied the endogenous formation of beliefs when they directly enter utility (e.g. Bénabou & Tirole, 2002), leading to a selective processing of new information with for example more weight being given to positive rather than negative news (Eil & Rao, 2011; Mobius et al., 2011; Sharot et al., 2011). This literature has, to a large extent, focused on beliefs with respect to oneself or to specific events, but we show that similar effects also occur with political attitudes." On this latter note, I'd recommend reviewing the working paper by Michael Thaler entitled "The "Fake News" Effect: An Experiment on Motivated Reasoning and Trust in News." That said, I think one of the contributions of this paper could be viewed as examining the potential relevance for evidence of motivated beliefs in settings that are closer to the "field." There is limited work that does that -- although I would also point out the working paper by Peter Schwardmann, Egon Tripodi, and Joël van der Weele entitled "Self-persuasion: Evidence from Field Experiments at Two International Debating Competitions."

Finally, in terms of framing their contribution, I think a clear discussion of what the reader learns above and beyond their companion paper that is forthcoming (T. Douenne & A. Fabre. French attitudes on climate change, carbon taxation and other climate policies. Ecological Economics, forthcoming) should occur in the Introduction.

DESIGN

Ultimately, I found the design to be quite difficult to evaluate because the survey was not fully explained. How questions are worded and displayed and the exact information the participants receive will certainly influence what results one would expect. It would be very useful to provide an Appendix with the full survey in English (rather than just a link to the full survey in French).

OTHER NOTES

- In the Abstract, the authors note that "Correcting these three biases would suffice to generate majority approval." It seems like this needs to be more appropriately caveated (i.e., under what assumptions etc) or removed.

- The authors note "We excluded the 4\% of respondents who spent less than 7 minutes on the full survey". Why was a 7 minute threshold chosen? More generally, given the various ways in which the authors exclude participants, can they show that their results are robust to relaxing/eliminating this exclusion criteria?

- When considering each unique treatment cell (defined according to *all* of the stages of randomization so that all participants in that cell effectively experienced the same set of treatment conditions), what is the sample size in each cell?