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Dear Salvatore and Anna-Sophie:

Thank you for submitting “On the Prevalence of Condorcet’s Paradox” to the *Economic Journal*. I have consulted with three expert referees. Referee 1 is the most positive referee, recommending that the paper be accepted after minor revisions. Referee 2 argues that the current version of the paper should not be accepted, though they are open to redeveloping the paper in a way that would be suitable for publication. Referee 3 argues against publication. Based on the advice of the referees and my own read of the paper, I am going to offer you the opportunity to revise the paper for the *short paper* section at the *Economic Journal*. However, as will become clear, I cannot make any promises at this point. Rather, I am keeping the door open.

Whether there is or is not a Condorcet Paradox depends on the specific preferences of the agents. While theoretically possible, the question is whether such preferences are indeed prevalent. The paper argues that they are not.

I see the questions as fitting into three categories:

1. *How Novel is the Result?* Referee 2 effectively asks this question, in the context of asking for a comparison to older results in the literature. This is a question that I too had, in reading your introduction. Of course, you do talk about earlier work. But not in a way that the reader can appreciate the marginal contribution of this paper. ¹

That said, I think the question goes deeper: We know that there will be a Condorcet Winner if there exists a single ideological dimension for which agents’ preferences satisfy a single-crossing property relative to that dimension. (I believe this follows from Gans-Smart, but correct me if I am wrong.) I always thought that preferences that satisfy the single-crossing property are more “real world natural” than preferences that give rise to the Condorcet Paradox. In particular, I thought that the conventional wisdom was that single-peaked preferences have

¹I worry that you may just be showing a smaller magnitude relative to a set of papers that already showed the magnitude was smaller than expected?

a substantive appeal. Thus, I would have thought the result is less surprising than the paper makes it out to be. To be clear, it is definitely a contribution to document this—but perhaps not a general interest contribution.

2. *What Drives the Result?* This is a question raised by Referee 2. They ask what mechanism leads to the prevalence/absence of a Condorcet paradox. I worry about your ability to address this question with the current dataset. At the same time, I am currently unclear how necessary it is to address the question of the mechanism. To some extent, whether it is/is not depends on how novel your current result is or is not.

- If the result is not very novel, I think it is very important to address this question.
- If the result is very novel, then I am not sure you need to address this question for a short paper. Though, I would be sure to raise the question, as an avenue for future research.

Unfortunately, I cannot commit to what is or is not “very novel.” I will have to see your answer to the first question.

3. *Empirical Strategy* Referees 2-3 highlight that the paper lacks sufficient detail about the empirical strategy. I would add that, even when the paper is clear about what is done, the paper does not adequately convince the reader of the approach. Let me highlight some issues, but I want to emphasize that this list is not exhaustive:

- (a) As Referee 3 notes, the paper provides too little information about the polling data used. Who conducts these surveys? How many respondents are there? What is the sampling strategy? Etc.
- (b) Do you use all available elections? Or have you selected 221? Comment 6 of Referee 3 and Minor Comment 1 of Referee 2 make me worry that you may have chosen these 221 elections out of a larger pool. If you have, what determined the selection?
- (c) The paper assumes agents report truthfully. Why is this a good assumption in the context of this setting? As Referee 3 asks, might the institution influence whether subjects report truthfully? Might culture influence whether subjects report truthfully?
- (d) Even if we do not have to worry about strategic reporting: The paper appears to assume that preferences are measured accurately, i.e., there is no noise in reports or measurement error. This is far from a typical assumption. Why do we think it is a good assumption in this context?
- (e) As Referee 3 points out, it is not terribly helpful to point to what R package you use. Much more helpful would be a discussion of what is actually done.

I can well see that you may be able to address these questions about the empirical strategy. But, I can also see that Referees might have larger questions about the empirical strategy, after reading the new version of the paper.

Let me conclude with a couple of more modest comments:

1. As the referees note, the paper needs to do a better job of writing for a general interest audience.
2. I did not find the discussion of the IAC particularly helpful. You use it to contrast *actual* preference profiles from the fraction of potential preference profiles. I am not sure that the latter is what a researcher would jump to in 2024.

In addition, please address all comments raised by referees.

As you can see, I cannot make any commitments at this point. At the same time, I would like to give you the opportunity to convince both me and the referees of the importance and the empirical strategy. I look forward to reading the new submission!

Sincerely,

Amanda Y. Friedenberg