

**James D. Dunn, Alice Towler, Richard I. Kemp, and David White**  
**Author response to editor and reviewer's manuscript PONE-D-22-32244**  
**Selecting police super-recognisers**

**Author reply is denoted by \*\* and bold text. All page and line references refer to the tracked changes version.**

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**\*\*Manuscript formatting has been updated to adhere to style requirements.**

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**\*\*Ethics statement has been added under the Method section.**

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**\*\*Caption for supporting information file have been added to the end of the manuscript before the results section.**

4. We note that Figure 2 includes an image of a participant in the study. As per the PLOS ONE policy (<http://journals.plos.org/plosone/s/submission-guidelines#loc-human-subjects-research>) on papers that include identifying, or potentially identifying,

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**\*\*We confirm that consent has been given from the individual's pictures to use their image for publication. Note that we have switched the images in Fig 5 for images of a different person who has signed informed consent. The ethics statement now includes a statement about the faces in the figures: "The faces of the individuals depicted in the figures have given written informed consent (as outlined in PLOS consent form) to publish and use their faces for these purposes."\*\***

5. Please review your reference list to ensure that it is complete and correct. If you have cited papers that have been retracted, please include the rationale for doing so in the manuscript text, or remove these references and replace them with relevant current references. Any changes to the reference list should be mentioned in the rebuttal letter that accompanies your revised manuscript. If you need to cite a retracted article, indicate the article's retracted status in the References list and also include a citation and full reference for the retraction notice.

**\*\*Reference list has been reviewed and is correct.**

## Comments to the Author

### Reviewer #1

This is a very interesting paper describing selection of super-recognisers from a large organisation. While most of the work in this area tends to be descriptive or small-group lab-based, the authors have offered a major innovation here. This has the potential to become a very influential paper, and I recommend publication. Its main contributions are (i) description of organisational-level SR-selection; and (ii) the very interesting finding that SR performance does not generalise well to other tasks that seem related.

I have only two main comments to make.

First, I am a little confused about the direct statistical comparison of SR-Strict and SR-Weak groups (for example line 295 et passim). Am I right to think that the Strict group is a subset of the Weak group? It is hard to work this out, because of the non-integer df values (presumably due to a statistical correction?). If one of the SR groups is a subset of the other, then it is not straightforward to see how these can be compared with a simple t-test. Of course, it is easy to see how each SR group can be compared with other groups (e.g. line 291 et passim), and the size of any difference can be discussed. Those are straightforward

between-subjects comparisons, but the comparison of overlapping groups is not like this. Perhaps I am mistaken here, and the authors can clarify the procedure.

**\*\* The reviewer is correct that statistical comparison of the strict V weak criteria SR groups is complicated by the fact that the SR-strict group is a subset of the SR-weak group. On the other hand, comparing the relative average accuracy of these groups is important practically, because it quantifies the accuracy that would be expected given two selection criteria. As a solution, we have removed direct t-test comparisons in favour of reporting the effect size of the differences in individual tests, and we refer the reader to the summary analysis at the end for a formal analysis (which we have now moved to the end following R1's next comment).**

**Regarding the non-integer df values, we used a Welch t-test for all the planned comparisons (this is now noted on pg 26 ln 715). Welch's t-test also known as unequal variances t-test is generally applied when there is a difference between the variance of two populations and also when their sample sizes are unequal. We now note this near line 933.**

Second, I was not convinced that the summary beginning on line 212 was very helpful. The problem is that the reader is presented with aggregate data before knowing what the constituent tests were. This would be fine if it were straightforward, but even the aggregation comes with qualifications (i.e. about excluded tests – line 220), and these qualifications do not really make sense until one has read the detailed material that follows. In fact, I also found presentation of Fig 1 in this section to be a bit unhelpful here. It means that the very first visual presentation of data encourages the reader to consider an association between memory and matching that is not at all central to the purpose of the paper. For me, figure 2 would be a much better place to start. So, I would suggest that the authors consider putting the material in the section starting line 212 towards the end of their results section, not at the beginning. Note that this will not introduce narrative tension, as the results have already been summarised in an earlier section (line 160), which is good. I emphasise that this is just a suggestion for readability – not a condition for acceptance.

**\*\* On reflection we agree with the reviewer and thank them for the suggestion, the summary analysis is now reported at the end of the Methods and Results section.**

Finally, I noticed a typo on line 168: “consistently higher” missing verb

**\*\*This error has been corrected.**

## **Reviewer #2**

This is a very interesting study in which the Authors exploited a dozen of validated paradigms to investigate facial perception and memory in a group of Australian police “super-recognisers” (SRs). The sample was divided into two subgroups: weak and strict SRs and their performance was compared to normative data or, when these data were not available, with students' performance. The study is well-conceived and the manuscript well-written. The set of paradigms explored an interesting set of different skills in this specific

population, and I am happy to endorse the publication of this work, after suggesting some minor hints which could help in further improve the clarity of the study:

1. The final sample is not so wide to allow the authors to include Sex as a between-subjects factor, but some more details on the gender of the stimuli would be useful for each paradigm. Is there a possible gender difference (both sample and stimuli) in such a domain? This topic should be at least named in the manuscript.

**\*\* All tests had roughly equal numbers of male and female faces (varying between 40:60 – 60:40 in ratio) except for the CFMT+ which only contains male faces. Below is a table showing the female:male ratio in each test which shows a consistently even ratio across tests.**

Test	% of female faces in test
GFMT	40:60
UNSW Face Test	50:50
Facial recognition candidate list test	50:50
Selfie-to-passport test	60:40
EFCT Upright	54:46
EFCT Inverted	61:39
Face and body matching test	40:60
Photoboard recognition memory test	50:50
Face-in-place recognition memory test	50:50

Gender differences in face identity processing individual difference tests tend to be very small (e.g. S1 Appendix in Dunn et al., 2020), and not consistently observed across studies either in the presence of a difference or the direction.

To test if there was any evidence that our selection process was biased towards selecting one gender over another, we compared the female:male ratio for the selected super-recognisers (39:61) to the female:male ratio of the NSW Police Force officers that completed all 3 the screening tests (40:60). This suggests that the selection process resulted in a gender balance that reflected the initial test cohort.

2. In the Introduction, brain and physiology difference in face recognition and face manipulation are named – I suggest to insert a further citation on hemispheric asymmetries for face recognition and spatial manipulation:

<https://www.ncbi.nlm.nih.gov/pmc/articles/PMC4671171/>

**\*\* The reference to physiology here was relating to associations between physiology and face identity processing ability. While we do agree that relationship between hemispheric asymmetries and face identity processing ability would be an interesting research topic, as the study cited here does not appear to directly test this association, we have opted not to cite in our introduction.**

3. To facilitate the reading of the manuscript, the set of paradigm could be numbered (from 1 to 12, including selection criteria).

**\*\*We considered this as it may have made cross-referencing easier, but after making the necessary edits we felt that it was not helpful and perhaps distracting. So we have retained the headings from the previous version of the ms.**

4. Page 12 starts with a significant 3-way ANOVA, but then two separate 2-way ANOVA are described. Since the 3-way interaction is significant, I strongly suggest reporting the overall interaction without splitting into two different analyses (which makes it impossible to see the real interaction among the three factors).

**\*\* The reviewer is correct that our ‘follow-up’ analysis did not address the nature of the 3-way interaction. However, we believe that the original presentation of the results somewhat mischaracterised our approach to analysis. In fact, we had intended a priori to analyse the effects of Study Duration and the Inversion Effect separately and this was not in fact a follow-up of the 3-way ANOVA.**

**We have revised this results section (from line 378) to make this clearer. We now report the full 3-factor ANOVA model in the Supplementary Materials with a Supplementary Figure to aid interpretation of the full ANOVA model. We believe the results of the 3-way ANOVA are consistent with the analysis reported in the main paper but that computing face inversion effects for individual participants aids interpretation of the pattern of results.**

5. Line 344: is “Exposure duration” the same factor named “Study duration” some lines above? Please, check for consistency in variable names.

**\*\*All instances of ‘Exposure duration’ have been corrected to Study duration.**

6. Line 353: the citation is not written according to the journal standard (numbers).

**\*\*This reference has been corrected.**

7. Page 14, line 413: the ANOVA interaction was significant: please, insert post-hoc comparisons before other (main effects) significant results.

**\*\*The tests that are reported in this section were already reporting simple main effects. Main effects are reported in supplementary materials as these were qualified by the sig interaction. We have added a reference to the supplementary materials here as it was missing.**