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|  |  |  | Tal Moran  E Tal.MoranYorovich@UGent.be  T 0032 (0)9 264 64 62  Faculty of Psychology and Educational Sciences  Henri Dunantlaan 2  B-9000 Ghent  Belgium  www.ugent.be |
| D. Stephen Lindsay, PhD, Editor  Psychological Science | |  |
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Dear Professor Lindsay,

Please find attached our Stage 2 Registered Report manuscript titled “Incidental Attitude Formation via the Surveillance Task: A Pre-Registered Replication of Olson and Fazio (2001)” (MS number PSCI-19-0402, [RR-PDR]), which was previously granted Stage 1 acceptance.

To briefly recap, the surveillance task (Olson & Fazio, 2001), originally published in *Psychological Science*, is an Evaluative Conditioning (EC) paradigm that has been used to support the idea that attitudes can be formed in the absence of a person’s awareness. It is one of the most highly cited EC procedures and papers (the original article has over 700 citations in Google Scholar) and has inspired conceptual, theoretical, and applied developments throughout the discipline.

Yet empirical evidence generated by this paradigm (and thus for unaware attitude formation) is exceptionally weak and subject to publication bias. With this in mind we contacted the original authors and asked for their assistance in designing a pre-registered replication of their original (2001) procedure. In order to collect the necessary sample size, we recruited twelve labs in Europe and the USA to help with the replication effort, including the lab of one of the original authors (total *N* = 1478).

In our preregistered *primary* analyses we set out to investigate if a surveillance task effect would emerge when the original Olson and Fazio (2001) awareness exclusion criterion was applied. Our preregistered *secondary* analyses then examined if this effect was robust to using three alternative exclusion criteria.

Results revealed a small but significant EC effect on the surveillance task using the primary exclusion criterion. We therefore replicated the original authors' effect, in the sense that significant results were found in both their study and ours. Critically, however, no EC effect emerged when any of the other three alternative exclusion criteria were applied. Moreover, effects did not differ significantly between the original authors criterion and the other three, undermining any strong conclusion we can make about the effect itself.

It is also worth noting that even the largest meta-analytic effect size we observed among the four exclusion criteria (*g* = 0.12 using the Olson & Fazio, 2001 exclusion criterion) was approximately half that observed in the meta-analysis of published literature (*g* = 0.20) and less than half of that observed in the original study (*g* = 0.27). Observed power in the published literature is therefore extremely low (median power = 0.14). Taken together, these two points suggest the published literature on the surveillance task reports significant results at a rate far above what one should expect in the absence of publication bias or selective reporting.

In short, although we replicated the surveillance task effect, we urge caution when using such an effect to make strong claims about ‘unaware EC’, especially when those claims are being used to justify *new* theory and interventions. We also encourage more careful reflection on existing theory and interventions that have already been founded on this effect. Strong claims necessitate strong evidence; evidence that we are currently lacking.

This replication was conducted in accordance with the ethical standards of APA. The preregistration for the project contains the protocol, all hypotheses, and all code for materials, data processing and analyses ([osf.io/3hjpf](https://osf.io/3hjpf)), and all finalized code and collected data are also freely available ([osf.io/hs32y](https://osf.io/hs32y/)). This manuscript has not been submitted to any other journal and its publication is approved by all contributing authors.

We have made several minor changes to the Stage 1 accepted manuscript in the service of communication and clarity. None of these represent changes to the accepted and preregistered protocol, but only to the clarity of communication of our methods and findings. Critically, we have also attached a separate document detailing all such deviations and a rationale for those changes.

Finally, the current manuscript represents the consensus opinion of its various co-authors. Yet throughout the project, and especially during preparation of the manuscript, many co-authors expressed individual opinions and disagreements on conceptual, theoretical, methodological, and analytic grounds. Given the limited word count and nature of the RRR we were unable to accommodate many of these distinct opinions. One solution we would like to suggest is to invite interested co-authors to write short (1000 word) commentaries that speak to these issues. Doing so would retain the integrity of the preregistered report while allowing individual researchers a forum to address issues, concerns, new directions, and ideas that the replication report stimulated. This would also provide the wider readership with an opportunity to experience this debate for themselves and offer them a more nuanced perspective on this study and behavioral effect than the main manuscript does alone.

We are aware that having researchers write commentary pieces on articles they are themselves authors of is somewhat unconventional. However, the RRR format and large multi-authorship papers are relatively recent evolutions with psychology. We feel that there is a tension between the need to create a consensus manuscript and yet the utility in advancing the science through hearing out minority opinions. We hope that *Psychological Science* shares this spirit of innovation. We are eager to hear if you would be willing to receive such submissions, and if so, what deadline would work for the journal.

We look forward to your reply.

Best Regards,

Coordinating Team:

Tal Moran ([Tal.MoranYorovich@ugent.be](mailto:Tal.MoranYorovich@ugent.be))

Sean Hughes ([sean.hughes@ugent.be](mailto:sean.hughes@ugent.be))

Ian Hussey ([ian.hussey@ugent.be](mailto:ian.hussey@ugent.be))

Jan De Houwer ([Jan.DeHouwer@ugent.be](mailto:Jan.DeHouwer@ugent.be))