Peer Review Report

Review Report on Adherence and association of digital proximity tracing app notifications with earlier time to quarantine: results from the Zurich SARS-CoV-2 Cohort Study

Original Article, Int J Public Health

Reviewer: Joanna Masel
Submitted on: 18 Apr 2021
Article DOI: 10.3389/ijph.2021.1603992

EVALUATION

Q 1 **Please provide your detailed review report to the authors. The editors prefer to receive your review structured in major and minor comments. Please consider in your review the methods (statistical methods valid and correctly applied (e.g. sample size, choice of test), is the study replicable based on the method description?), results, data interpretation and references. If there are any objective errors, or if the conclusions are not supported, you should detail your concerns.**

Major comments:

- The 8/43 central result I highlight in Q2 shows up as 8/42 in Supplementary 6. Similarly, 138 non-household non-app-notified in the main text is 141 in Supplementary 6. Supplementary 5 lists 52 non-household non-app-using contacts and 128 non-household app-using contacts (of who 42 or 43 received a notification). From this, I would guess (it was not explicit) that the 138 (or 141) control group for Figure 1 and the main result are the 52 non-app-users plus 128-43=85 app users who weren't notified, but that adds up to 127. Please reconcile numbers, and clarify. Note that I did not trace all numbers throughout the manuscript, just the ones that were key to what I interpret as the most significant findings. More quality control is presumably needed throughout. To reduce the extent of compliance bias, the control group in Figure 1 should use as its control only app-users who weren't notified, and not non app users.
- 8/43 is quite low, given that a major rationale for the app is its rapidity. While speed is nevertheless increased by having two notification systems, I would like to see more discussion on this point. Lines 316-324 allude to underlying problems – I would like to see them spelled out more explicitly, the better to be learned from. The result is what it is, and shouldn't be taken as a weakness re the decision to publish. But theoretically, the code was supposed to be provided to the index case at first contact. Did this not happen? Or was the first contact with contact tracers much delayed relative to the case learning their positive status directly from their test provider, giving the case the opportunity to notify their own contacts in the meantime? It's really important to learn from this low rate, and while it's good to hear about what has improved since the period evaluated in the manuscript, I'd also like to see more disclosure of the authors' assessment of what went wrong, i.e. exactly why these improvements were required.

Minor comments:

- The term "index case" to be inaccurate, especially re lines 188-189, where it is used to describe even those first identified as close contacts of another case. These are not index cases, they are just cases. "Index case" should be replaced by "case" throughout, there is no special patient-zero analysis in this work.
- Lines 217-218: I would like to see this number (fraction receiving notification) just for the non-household contacts, where it is most relevant. The current number is very high, but perhaps inflated by the fact that app usage is likely clustered by household, but within-household notifications are less important.
- Lines 304-306 state that it is "impossible" to assess app impact on contacts not traced by manual contact tracers. While it is outside the scope of the current study, it is not impossible. If cases with unknown source of infection are interviewed about their notification history, and ideally this is compared to matched controls, one obtains exactly this information, without violating any privacy-by-design within the app.
- Line 337. I'm not convinced this qualifies as a prospective study. The cohort is of people who had already got covid / been exposed and had already made their quarantine/app decisions. Recruitment was not prior to the events being surveyed.

Q 2 **Please summarize the main findings of the study.**

Out of 43 non-household close contacts who received a notification, 8 did so prior to being reached by manual contact tracers. These 43 notified non-household close contacts began self-quarantine about one day earlier than 138 non-app-

notified non-household close contacts, for reasons that are not causally clear – in addition to the 8 who were notified earlier, lines 282-283 speculate about reinforcement, but another option is that, if non app users are included in the 138 (see major concern in Q1), app users simply tend to higher compliance.

Q 3 **Please highlight the limitations and strengths.**

A strength of the study is that it is the first to demonstrate behavioral change in response to DPT.

A limitation is that the study population is likely unrepresentative of Swiss cases, in particular substantially enriched for those likely to have high compliance. This is most clear in Supplementary 3. Of those who contact tracers were able to reach (I would like to also have data on the dropout rate at this point, which is not currently given), 1312/3519 cases and 2231/6316 contacts did not agree to be recontacted. Of those who did and were, 393/793 cases and 271/640 contacts agreed to the study. German language requirements additionally excluded >10%. While the authors acknowledge this limitation on lines 310-312 (and the subsequent lack of causal inference on lines 330-331), its scope, as captured by the numbers in Supplementary 3, is not made sufficiently clear in the main text.

PLEASE COMMENT

Q 4 **Is the title appropriate, concise, attractive?**

The title makes a causal claim ("lead to") that the main text acknowledges was not proved.

Q 5 **Are the keywords appropriate?**

Yes

Q 6 **Is the English language of sufficient quality?**

Yes

Q 7 **Is the quality of the figures and tables satisfactory?**

Yes.

Q 8 **Does the reference list cover the relevant literature adequately and in an unbiased manner?)**

- Lines 54-55 give the impression that the DP-3T "blueprint" was the only influence worth mentioning on decentralized privacy-preserving proximity tracing. Citing the March 20 Covid Watch white paper https://blog.covidwatch.org/en/covid-watch-whitepaper-using-crowdsourced-data-to-slow-virus-spread would help balance this.
- Lines 67-68: the preprint by Wymant et al. https://github.com/BDI-pathogens/covid-19_instant_tracing/blob/master/Epidemiological_Impact_of_the_NHS_COVID_19_App_Public_Release_V1.pdf is a major omission re the current state of evidence on the impact of DPT on disease transmission
- Lines 84-86 seems to imply that no data is available on code usage rate by cases. This has been measured for all NHS-app-using cases by Wymant et al., for the Covid Watch Arizona app by https://doi.org/10.1101/2021.02.02.21251022, and is regularly reported for the Corona-Warn-App most recently at https://www.rki.de/DE/Content/InfAZ/N/Neuartiges_Coronavirus/WarnApp/Archiv_Kennzahlen/Kennzahlen_16042021.pdf?__blob=publicationFile. Importantly, these studies suffer either not at all or less from the bias of the current work towards the highly compliant (see reply to Q3). Indeed, the much higher rates of code usage described on lines 211-213 confirm for me that high-compliance bias in the study population is a major limitation of the current work.

QUALITY ASSESSMENT

Q 9 **Originality**

Q 10	Rigor	■■■□□
Q 11	Significance to the field	■■■■□
Q 12	Interest to a general audience	■■■■□
Q 13	Quality of the writing	■■■■■
Q 14	Overall scientific quality of the study	■■■■□

REVISION LEVEL

Q 15 Please take a decision based on your comments:

Major revisions.