Peer Review Report

Review Report on Peri-pandemic acceptance of influenza and COVID-19 vaccination by Swiss healthcare workers in primary care 2020/21: a cross-sectional study

Original Article, Int J Public Health

Reviewer: Michał Jacek Jędrzejek
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EVALUATION

Q 1 **Please summarize the main findings of the study.**

Influenza and COVID-19 vaccination, as a key component of control efforts to prevent nosocomial transmission, is recommended each year [influenza] or according to national programs [COVID-19] for all healthcare workers in many countries. The World Health Organization (WHO) and the US Advisory Committee on Vaccination Practices (ACIP) both recommend that healthcare workers (HCWs) should receive influenza vaccination annually, as vaccination of healthcare workers is considered an important strategy to reduce influenza transmission. Similar recommendations are applied to vaccination against COVID-19. Adherence to influenza vaccination is known to be low and vary over time as well as between regions and different types of healthcare professionals and therefore understanding the barriers to influenza [and COVID-19] vaccination is important and necessary to increase vaccination adherence among HCWs.

The presented article raises a very actual topic nowadays, when vaccinations are perceived differently by individuals, as well as by the medical staff themselves. The aim of the present study was to determine the percentage of influenza and COVID-19 vaccinations and to identify the reasons for accepting or refusing vaccinations among Swiss primary care workers in the 2020/21 season. The authors obtained a large sample of 1331 participating HCWs. The influenza vaccination rate in the 2020/21 season was 61.3% in total, and 91.8% of participants were vaccinated against COVID-19. The authors obtained similar results as in the literature, e.g. invariably, physicians are more often vaccinated than other medical professions. The discussion is led in the right direction, but in my opinion it does not exhaust the complexity of the topic. It is worth emphasizing that the results of this study have an application value, they can be useful to increase the effectiveness of strategy to prevent nosocomial infections and improve the level of influenza vaccination of specific professional groups of healthcare workers.

Unfortunately, there are many shortcomings and errors in the text, including major methodological errors, and the editing of the text seems sloppy. I direct the text to fundamental changes, but I hope that after major changes the article will be suitable for publication in IJPH.

Q 2 **Please highlight the limitations and strengths.**

The sample size in this study, i.e. 1331, is large enough to have high statistical power to identify small to moderate associations. The advantage is to include various professional groups, e.g. pharmacists, in the research group. As it turns out, it is a good idea to include variables "recommending vaccinations for patients" and "ability to vaccinate" in analyses.

The first major limitation of this study is the lack of compatibility with the general population of Swiss HCWs. Due to this fact, the study sample is not representative or, more accurately, we don't know if it is representative of Swiss HCWs, and thus the generalization of the results is very limited. In addition to my major comments about methodology, selection bias is possible and therefore the generalizability of the results of this study is limited again. As the authors emphasized, participation in the study was voluntary and it is possible that motivated (and vaccinated) healthcare professionals were more likely to participate and complete the survey than their unvaccinated colleagues. Self-report is another possible limitation of the study, as the authors emphasized. Influenza or COVID-19 vaccination status was reported by the respondents themselves, which was not subject to independent verification, therefore the accuracy of the responses depended only on each respondent’s willingness to admit they had (not) been vaccinated.

Q 3 **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.**

Dear Authors,
First of all, before proceeding to the main commentary, I would like to congratulate the authors on the choice of the research topic and the large sample size.
Considering the high quality of publications and scientific reports, I have serious doubts about a part of the research methodology used, but I am convinced that after major changes, i.e. focusing on the obtained direct results, the article will be suitable for publication in the IJPH.

(!) Formatted text and tables – see .pdf file

Major comment:

I. The authors state that one of the objectives of the study was to compare the influenza vaccination of Swiss primary care workers during the COVID-19 pandemic in the season 2020/21 with the pre-pandemic period in the 2015/16 season. First, an important point needs to be clarified. In my opinion, for this comparison to be possible and methodologically correct, it would have to be exactly the same population as previously studied (2015/16), or a population with the same statistical structure at least in terms of gender, age and profession, as previously studied (2015/16). Is it exactly the same population (same participants), or was the structure of the previously studied and the currently studied population statistically compared? There is no mention of this in the method description, so I'm guessing not. The use of the same questionnaire and designation of a similar (or the same) target group and recruitment method absolutely does not authorize a direct comparison of the results of these studies! The structure of the surveyed population (2020/21) may be completely different than in the 2016 study, and then the obtained results are incomparable. We assume that the two groups are different and not comparable without prior statistical comparison test. A lot can change in the population of Swiss healthcare professionals in just a few seasons, especially in the face of the COVID-19 pandemic. Of course, you can refer to the results of the previous study in the Discussion section, but with some caution, i.e. you can refer to previous research as an example or background, not a direct comparison in this situation, not as part of the methodology of the present study. A direct comparison of two groups is only possible if there are no statistical differences between them and the results can be generalized only if the surveyed group is representative, e.g. of the national or local population of health professionals (condicio sine qua non: known structure of the general population – national or local population of health professionals; furthermore, random or at least stratified or quota sample are used). This may be the future methodological direction of the present study.
Please see the data in Table 1 – comparison of the distribution of occupational groups from two studies – 2015/16 and 2020/21 (excluding the "other" category): the chi-square statistic is 199.217, and the p-value <0.00001, so we conclude that the observed differences in the structure of the groups are not random, thus a priori these groups are different at least in one term. Due to the different distribution of participation of individual occupational groups, regardless of the influence of external factors, different vaccination results can be obtained (even 61.3 vs. 40.2%). Note that there are more doctors (who are generally vaccinated) and fewer nurses (who are generally not vaccinated) in the study group – don't these differences skew the view?
Similarly, data from Table 2 – there are statistical differences in the gender structure between the 2020/21 and 2015/16 group (1111 and 214 vs. 793 and 264, respectively): the chi-square statistic is 28.5501, and the p-value is <0.00001, so we come back to the conclusion that the differences in group structure are not random, so a priori these groups differ again. So, these two groups differ in at least two categories, and therefore the results obtained from them are also incomparable.
Therefore, I propose that the last paragraph in the Introduction section be worded as follows: “The main objectives of the present study were to determine the rate of influenza and COVID-19 vaccination and to identify reasons for receiving or declining vaccine among Swiss primary care healthcare workers in the 2020/21 season.”. In this article, please focus only on the analysis of data for the 2020/21 season in the Methods and Results section, and move the remaining issues to the Discussion section, if appropriate.

II. I have serious doubts about the value of the stats (!!!). For example, in Table 2, the row "influenza 2020/21" – the "vaccinated" and "recommend" categories have very similar numbers listed by language region, but the authors reported very different p-values, i.e. 0.875 and 0.061. According to my calculations, the p-values for these groups are 0.875 and 0.790 respectively – see the table below. This is a huge difference, additionally considering that the result of p-value =.06, reported by the authors, is on the verge of statistical significance. Please verify the test values or explain how the authors calculated these statistics.

Another example from Table 2: the row “COVID-19 2020/21”, category “recommend” divided by language too (counts: 691, 288, 31 [obtained from Table] vs. 210, 106, 5 [calculated]), according to stats: the chi-square statistic is 4.0523 with the p-value=0.131844 (the result is not significant at p<0.05), so p-value is not 0.001 as calculated by the authors (!!!). Interestingly, in the corresponding place of the text (lines 187-190), the authors state that “There was no difference between sex and the language of the HCWs concerning the prospect of recommending both vaccinations (…)”, without providing the p-value, which directly suggests that there is no statistical significance. Or the failure to provide the p-value (theoretically statistically significant according to Table 2) is due to some sloppiness. It's bad either way. Please verify these calculations or explain how the authors calculated these statistics.
What is also disturbing, based on the data in the gender section of Table 2 again, I also obtained different p-values than the authors in most cases, although the results that were statistically significant remained so. What’s more, based on the given percentage (33.3) and sum (816), I conclude unequivocally that in the cell, in the row “influenza 2020/21”, “vaccinated”, and in the column “sex”, “d”, there should be a value of "2". Such minor shortcomings and huge discrepancies in statistical tests make the entire table unreliable, which in turn discredits all calculations in the publication and gives the impression of messy editing of the text of the publication. Be sure to verify all calculations!
Below I recreated part of the data table based on the data in Table 2 to compare the calculations. Of the 8 control statistical tests performed, only 3 were consistent (but without the given value of chi2, I cannot verify the correctness of the calculations with the results of p<0.05), and one disproved the statistical significance shown so far!
Table - see .pdf file

III. When performing re-calculations, remember to exclude 93 respondents working in the hospital only, if appropriate (if it is actually the only workplace) - see commentary on section 4.4. from the Discussion section.

Minor comments:

I. All text:
1. When writing about the novel coronavirus, we prefer to use "COVID-19" instead of "Covid-19" or "covid-19". This is consistent with the use of this abbreviation by the World Health Organization and its use in scientific articles. Please note that both the Centers for Disease Control and Prevention (CDC) and the WHO capitalize the name (e.g. https://www.who.int/emergencies/diseases/novel-coronavirus-2019 or https://www.cdc.gov/coronavirus/2019-ncov/your-health/about-covid-19.html). Some publications style this abbreviation with a capital "C" only, but this is a scientific publication, not a newspaper publication, so please unify the “COVID-19” record and correct the “SARS-CoV-2” record: the first mention of “COVID-19” is correct (line 35), the next written as "Covid-19"; similarly, it should be written "SARS-CoV-2" instead of "SARS-CoV2" (line 36 and 38 in the Introduction part).
2. Please standardize the record of the turn of the years. Now there are two different forms in one paragraph (even in one line: "1998/1999" or "2020-21", line 34), so please choose one writing style, i.e. 1998-99 or 2020/21. Similarly, please standardize the notation of numerals (see paragraph 3 in the Introduction section, in one sentence there are two forms at the same time, i.e. "4'170'683" and "11.972", lines 42-45).

II. The Introduction section:
The first paragraph: 1. The second sentence is unclear to me and I did not find the information contained in this sentence in reference [2] (!). I suggest reformulating this sentence, using another WHO document as reference (e.g. https://apps.who.int/iris/bitstream/handle/10665/336951/9789240010154-eng). If this sentence is to remain, then who belongs to category A? What is category B then? I only found the target groups identified by the SHC in the report on vaccination against 2020-2021 seasonal influenza (https://www.famhp.be/en/news/influenza_vaccine_vaccination_of_at_risk_groups_completed_vaccine_can_be_delivered_to_people).
2. Penultimate sentence ("Despite this recommendation..."): I think a reference to the literature should be left here. An interesting work: To K.W., Lai A., Lee K.C.K., Koh D., Lee S.S.: Increasing the coverage of influenza vaccination in healthcare workers: review of challenges and solutions. J. Hosp. Infect. 2016;94:133-142, https://doi.org/10.1016/j.jhin.2016.07.003
The second paragraph: 1. The abbreviations EU and EFTA should be expanded.
2. Please verify the data and information, what chart did the authors use? I don't know if it's a mistake, but according to Johns Hopkins University, to which the authors refer, the cumulative number of confirmed cases of COVID-19 in the world has reached over 727 million (not 627 million), including 6.71 million deaths, but in the period 04 Jan 2020 - 30 Dec 2022 (https://ourworldindata.org/grapher/cumulative-deaths-and-cases-covid-19?time=earliest..2022-12-30), and in the period to December 30, 2021, the graph shows at least 285 million cases, including 5.47 million deaths (https://ourworldindata.org/grapher/cumulative-deaths-and-cases-covid-19?time=earliest..2021-12-30). The data provided by the authors (627 million cases, 6.58 million deaths) fits the date range from 04 Jan 2020 to 31 Oct 2022, so I understand that it is

about the date range "2020-21" given by the authors in the text (line 38)? (https://ourworldindata.org/grapher/cumulative-deaths-and-cases-covid-19?time=earliest..2022-10-31). The term "2020-21" is very imprecise and, as you can see above, too much of a shorthand. Please verify this data or specify the period for which the data is provided (date range), as it is written in the third paragraph, so that the reader can independently and clearly verify the data.
I suggest merging the second and third paragraphs into one.

III. The Methods section:
The first paragraph: In the first sentence, please specify the duration of the study more precisely (i.e. “November 2021 – February 2022” instead of "winter"). According to the major comment, please change the last sentence (line 65) as follows: "To ensure comparability with..." to "To ensure coherence with...".
The second paragraph: Please expand the used abbreviation HMOs (line 70).
The third paragraph: According to the major comment, please rephrase the first sentence as follows: “The same questions regarding influenza vaccination were used as in the 2016 survey [19]” or remove this sentence. The authors mentioned that the original questionnaire was pre-tested “for clarity, comprehension and accuracy” (lines 77-79) – was there a (scientific) report on this testing? Is it possible to refer to the literature? If so, please include this in your references.
The fourth paragraph: Please add information (in the text) about the questionnaire that there were multiple-choice answers or a free text box or both when asked for reasons for accepting or refusing vaccination.
The last paragraph: According to the major comment, please remove the sentence: “2x2 continency tables were created to compare data from the 2016 survey [19] with the current survey” (line 102-103).

IV. The Results section:
Section 3.1.
The first paragraph: Please verify the percentage value given for “pharmacists” (line 112) – in the text the authors wrote “32.2%” vs. in Table 1 – “32.3%” and please unify the use of the word “practice” or “practise” – the authors used phrase "private practice" in the text (line 115) vs. "private practise" in Table 1. I think that the occupational category of 40.9% is missing in the last sentence, do you mean: “(…); while 40.9% of "others" worked as counsellors in a health league.”?
The second paragraph should be completely reworded. Referring a non-random selected occupational group to the country's general population is methodologically inappropriate. Regarding the discrepancy in the share of women in the study group and the general population – the true question is what is the percentage of women in the population of Swiss primary care workers. Does your 83.5% reflect this? Is the structure of Swiss healthcare professionals known? If so, then there should be a comparison with it. See comment on section 4.4. (Discussion section). Please neutrally (i.e. without comparisons) describe the results obtained during the surveyed season 2020/21, i.e. participant characteristics in terms of gender, age and language, as in the paragraph above.
Section 3.2.1. Similarly, please neutrally describe the results obtained during the surveyed season, without comparisons to the previous study or to the general population. For illustrative purposes, the first paragraph should read like: “Overall, 61.3% of participating HCWs were vaccinated against influenza during the 2020/21 season. Physicians showed the highest vaccination rate against influenza (87.3%), followed by pharmacists (73.7%) (Table 1). 64.5% of participating HCWs indicated that they also plan to get vaccinated against influenza in the upcoming 2021/22 season, more often among physicians and pharmacists, 85.4% and 68.6%, respectively.”. It would be interesting to count the potential dependencies for these results (chi-2) – see note below.
Similarly, please describe the results in differentiating by age and gender. The authors calculated the relationship (be sure to see the note on Table 2 – the major comment and below) of the level of vaccination between gender and age, and why not by occupation? Please count the statistics. If appropriate, I suggest merging some occupational groups for the chi-2 test. Please note that, by counting chi-2, we only show the existence or not of the relationship (please always provide the calculated chi-2 value if the result is statistically significant), but we do not know the direction of this relationship – please, if possible, confirm the direction of the relationship, if statistically significant, e.g. by correspondence analysis test. In the further part of the text, based on a multivariable logistic regression analysis (also calculated for gender and age, regardless of the calculated chi-2) the authors show that physicians were statistically more often vaccinated than nurses during the studied season, what is consistent with findings from other studies.
COVID-19 vaccination status section: should be 3.2.2 I guess? The last sentence of the first paragraph (lines 157-159) – has the described direction of dependence been confirmed by a statistical test (e.g. correspondence analysis)? If not, I suggest doing an analysis.
Section 3.3. Why are there different values in the text and in Supplementary Figure 1 (category "total") for reasons to get vaccinated against influenza and COVID-19, shouldn't these values be coherent? In the context of developing campaigns and activities aimed at increasing vaccination rates among HCWs, barriers declared by participants are also important, especially since the literature shows a large heterogeneity on this topic. In this paragraph, please provide the 3 most frequently indicated barriers to vaccination against influenza and COVID-19, and refer to the obtained result in the Discussion section.

Section 3.4.1 and 3.4.2. Please refer to the notes to Tables 2, 3 and 5 (second major comment and below) for the correctness of the statistical calculations.

V. The Discussion section:
The first sentence absolutely needs to be rephrased. Generalizing the result of an unrepresentative group to the general population is a serious methodological error – phrase “(…) influenza vaccination uptake among HCWs in the primary care sector in Switzerland increased (…)” (lines 223–224). The authors have no grounds to treat the study group as representative and to generalize the results to the entire population of Swiss primary care workers!!! More appropriate would be: "Compared to the data available for Switzerland, our study showed a relatively high vaccination rate among the surveyed health professionals in the primary care sector. For example, a similar study found an influenza vaccination coverage ratio at 40% in the season 2015/16, compared to 61.3% in the present study. Similarly, our results were higher than those obtained by the FOPH for influenza seasons 2019–2022, which were each well below 30% (…).". The rest of the considerations in this paragraph are correct and interesting.
In my opinion, the first sentence of the second paragraph needs to be reformulated. Suggestion for the authors to conduct a chi-2 analysis of the declared willingness to be vaccinated against influenza next season in relation to the current vaccination status. Many studies, as well as the reviewer's own study, have shown that previous vaccination history was positively correlated with actual or future influenza vaccine intake.
Section 4.1. I cannot agree with the last sentence (lines 280–281). Why such a conclusion? Do the authors have literature data? Many pre-pandemic studies show a positive correlation with age over 40 in influenza vaccine intake among healthcare workers (but not all studies found that age was a significant predictor of vaccination at all).
Section 4.2. The first sentence, the phrase "awareness of a high risk of infection in the workplace" (line 284) – neither in the text nor in the additional materials do I find such data or such a variable for the study population. Please notice that rather “lack of awareness of the risk of getting sick” is a common barrier to influenza vaccination among healthcare professionals – it is actually the main barrier reported by participants in the present study. In addition, in the reviewer's own study, as in many others, the most common reason for receiving the influenza vaccine was the desire to protect oneself and family/friends, and then patients – in that order, so far it was a universal pattern, unlike in your study. Can the authors comment on this? Maybe there is an impact of the COVID-19 pandemic?
The last paragraph is written imprecisely and in a sloppy way, including the first sentence, which neither presents the results of the study (the most common barriers are different in the present study – see Supplementary Table 1, part A), nor does it refer the results to the literature, i.e. are the results consistent/inconsistent with other reports? Please rewrite this part of the text properly. Please note that many different barriers to vaccination have been identified in empirical studies, and the structure of the most commonly reported differed depending on the population studied. It would be interesting to develop this topic in the discussion – understanding the barriers to influenza and COVID-19 vaccination is important and necessary to increase vaccination adherence among HCWs.
Section 4.3. The considerations in this paragraph are very interesting. The distribution of declared barriers to influenza and COVID-19 vaccination only among nurses may be interesting.
Last sentence/paragraph of section 4.3. should be moved as the last paragraph of section 4.2 in my opinion. Please move it if the authors deem it appropriate.
Section 4.4. Limitations
The first major limitation of this study is the lack of compatibility with the general population of Swiss HCWs. Due to this fact, the study sample is not representative or, more accurately, we don't know if it is representative of Swiss HCWs, and thus the generalization of the results is very limited. Please include this fact in the text.
In addition to my major comments about methodology, this study has several limitations. Selection bias is possible and therefore the generalizability of the results of this study is limited. As the authors emphasized, participation in the study was voluntary and it is possible that motivated (and vaccinated) healthcare professionals were more likely to participate and complete the survey than their unvaccinated colleagues. Self-report is another possible limitation of the study, as the authors emphasized. Influenza or COVID-19 vaccination status was reported by the respondents themselves, which was not subject to independent verification, therefore the accuracy of the responses depended only on each respondent’s willingness to admit they had (not) been vaccinated. Did these 93 respondents indicate the hospital as the only workplace (line 333–334)? Why did the authors include them in this analysis? Analysing Table 1, I understand that a sample of 1331 participants contains these 93 hospital-based participants. If so, they should be excluded from the analysis (!!!). Calculate again in a reduced sample!
“As there tend to be more females employed in the health sector and they participate more often than men in health studies, participation in our study was heavily skewed towards females.” (lines 330–332) – how do the authors know this? Do the authors have literature data on this issue? Even if so, what impact does this have on the presented results? Is this a limitation since many studies show a positive correlation with male gender in influenza vaccine uptake among HCWs, as in the present study? The authors themselves say that the participation of women among the HCWs population is probably larger (but without support with the appropriate data), so it should be remembered that it is more appropriate to refer to the general group of (Swiss) HCWs, and

not to the general population of the country. Theoretically, it may turn out that the share of over 80% of women is a representative result. It should be remembered that the obtained correlations in the study (provided that the calculations are verified) are in line with literature, i.e. the impact of the profession, age and gender on the level of influenza vaccination among medical staff. Please remove this sentence or support it with relevant data.

VI. The Conclusion section:
The first sentence of the conclusion is methodologically incorrect and must be removed in the current form or at least reformulated. The second sentence is the result, not the conclusion. Since the study sample is not representative and there is no methodological basis for comparing it with the previous edition of the study, it cannot be unequivocally concluded that the higher influenza vaccination rate among surveyed healthcare professionals in the current study is solely due to the COVID-19 pandemic, it may well be the result of a reported selection error - see the first major comment.
What did the authors mean by "different settings" (line 355) in the last sentence?

TABLES
Tables 1, 2 and 3 must be merged into one, simultaneously with the absolute deletion of the rows for the 2015/16 survey (tables 1, 2 and 3) and data for the general population of Switzerland (tables 2 and 3).
Table 2: see the second major comment. Please also check the calculations in Table 3.
Table 3: The first relationship in table, according to my calculations, is statistically insignificant, completely different from what the authors state, i.e. p-value 0.08 vs. 0.005, respectively (!!!). Below is my table of numbers for calculations. Please verify the test values or explain how the authors calculated these statistics.
16-20 21-30 31-40 41-50 51-60 61+ total
Study 2020/21 19 133 277 379 407 116 1331
Vaccination training 11 98 204 249 267 74 903
Without training 8 35 73 130 140 42 428
x2=9.82552, p-value=0.08033
Be sure to verify all calculations in the table!
In Table 5, in the "total" row, in the "Flu vaccination 2021" category, the summed values are miscalculated (!), which should be: 1331, 42, 859, 131, 299, now it is: 1331, 46, 872, 133, 307. Moreover, I don't understand the category "No patients" - where did this come from?? In the survey for the question "Do you recommend seasonal influenza vaccination to your patients/clients?" there are only 3 possible answers "yes", "no", "in some cases". Please explain it. If there are 3 categories only, as in the questionnaire, the chi-2 statistic should be recalculated (!). In addition, in the sections "Recommend a flu/COVID-19 vaccine", the value "0" and small numbers, e.g. 1 or 4, appear in each section, so chi-2 statistic should not be counted (!!!). Merge categories first to increase the number of individual categories >5, i.e. the "No" and "No, but think about it" categories should be merged, as well as the "Yes" and "Currently in training" categories and then, taking into account the above note about the category "No patients" (???), the chi-2 statistic should be recalculated.
I checked Table 6 - the calculations are correct.

PLEASE COMMENT

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

Yes

Q 5 **Are the keywords appropriate?**

I suggest changing the keywords to: influenza; vaccine coverage; healthcare workers; COVID-19, primary care

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

Yes

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

No.

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

Yes

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 make a recommendation based on your comments:

Major revisions.