Reviewer’s report

Title: Interdisciplinary and multiprofessional outpatient secondary individual prevention of work-related skin diseases in the metalworking industry: 1-year follow-up of a patient cohort

Version: 0 Date: 01 Aug 2018

Reviewer: Daloha Rodriguez-Molina

Reviewer’s report:

In this manuscript, the authors aimed at presenting the results of an observational, longitudinal, pre/post-test study of a secondary individual prevention program of occupational skin diseases among metalworkers. The main outcomes were 1) the proportion of participants remaining in their occupation despite skin diseases and 2) severity of skin disease, both at 1-year follow-up.

The importance of this manuscript is two-fold: 1) it describes the results of a standard prevention program by the Social Accident Insurance Institution for the woodworking and metalworking industries in Dortmund, Germany, and 2) it explores self-assessed alternatives to estimating the severity of skin diseases in comparison to a dermatological gold-standard. I believe that this manuscript contains information of high value to readers of the Journal, and also that the following suggestions will help to substantially improve the manuscript for publication.

General comments

Study design: The authors have provided several definitions of the study design used, but only the one included in the Discussion section seems to fit the methodology: "longitudinal, pre/post-test design." The use of "observational" is also accurate. In a cohort study, an unexposed group is expected. The use of "uncontrolled" is also discouraged because an observational study is by definition uncontrolled (there's no experimental manipulation of the exposure variable). The use of "cohort" to refer to the group of participants seems to be acceptable.

Lack of unexposed group: The reasons that the authors have given for a lack of unexposed group seem reasonable, but it's still difficult to put results for "remaining in their occupation" in perspective when there are no estimates to compare it against had the intervention not be made. Possible solutions include checking if the German Social Accident Insurance (DGUV) or similar entity holds data on this topic in Lower Saxony or other German Federal states regarding metalworkers with skin diseases without attending such a program. Another option would be to check how was "remaining in their occupation" in a similar population before the program was introduced.

Specific comments for clarification purposes

Background
Page 2, lines 2 and 3: please provide a reference to "occupational disease no. 5101), if any.

Methods

Participants fill-out written questionnaires at T1-T4, but it is not clear if these are all similar questionnaires. I'd suggest to clarify.

Page 4, line 25: I guess that "appropriate samples" refers to patients receiving pharmaceutical samples and not providing skin samples. I'd suggest to fix for clarification.

About the scores used: I understand that the target audience of the journal is mainly dermatologists who might be familiar with the scores described, but I could foresee some interest from other fields of expertise (e.g. educational sciences, social workers, other health-care specialties). I'd therefore recommend to briefly describe the meaning of the scores (the highest the score, the more severe the disease?). Also include cut-offs points for these scores if there are any.

About data input: I am assuming that most (if not all) questionnaires were in paper form. What kind of procedures were used to ensure quality of the data input? Was the data inputted directly in SPSS, or was there another data management software used?

About the statistical tests: part of the description of the tests used state that they were used to "calculate the [statistical] significance". Please note that calculating statistical significance is not the main aim of hypothesis testing. I'd recommend to rewrite these parts, and just stating the purpose of each test (e.g. t-tests for comparing the means of paired samples, etc.).

Please state that you used Pearson's chi-square test OR Fisher's exact test instead of AND.

For Fisher's test, please shortly explain the phrase "expected value < 5".

About the central limit theorem: Besides sample size > 30, there are several other assumptions to comply with before assuming normal distribution. For example, scores usually follow a Poisson distribution [0, +inf), as opposed to most continuous variables whose limits are (-inf, +inf). An alternative is to log-transform the scores and perform parametric tests on these transformed variables.

I'd also suggest to revise the possibility of using either mixed-effects modeling or (better yet) latent growth modeling for changes in self-assessed disease severity over time. The use of ANOVA is not completely accurate, as ANOVA doesn't take the directionality of time into account.

Use of complete cases was mentioned in the Discussion section but not in the Methods section. Please mention in the Methods section how were missing values handled.
Results

I'm confused about the use of the term "drop-outs" in the first paragraph of the Results section. If the word "drop-outs" refers to the 36 subjects who decided not to participate from the beginning (214 - 178 = 36), I’d recommend to change the term. The term is used for participants leaving the study after they have enrolled, and they can't be drop-outs if they haven't agreed to participate in the study in the first place. Drawing a flow-chart of the inclusion process is highly recommended, and will clarify especially this point.

The authors report participation rate at T1 (n = 178/214, 83.2%), and response rate at T3 (n = 149/178, 83.7%) and T4 (n = 128/178, 71.9%). Could you please also report response rate at T2?

In addition to the test statistic, I’d suggest to include the proportion of patients with contact dermatitis both in the study group and the drop-outs.

Please consider including a table describing socio-demographic and work-related characteristics.

The sub-section "Dermatological examination and atopy score" could benefit from re-structuring: I’d recommend to start with all results from T1 and then describe the differences between T1 and T2.

Please specify in the text at which time point is the mean atopy score of 6.9 (table 1 shows it's at T1, but it's not stated in the text), and provide a measure of dispersion.

The authors mention a trend for higher mean OHSI scores in smokers compared to non-smokers, although not statistically significant. Could you please provide test estimates, and/or (preferably) 95% confidence intervals?

It is not clear to me why was atopy not assessed at T2 (Table 2). Could you please explain?

I think that the use of the "school grade" system for self-assessment is a good idea. I'd suggest however to briefly explain it in the Methods section, especially for readers that are unfamiliar with the German grading system. It'd be a plus to include your motivation to use this system along the others for self-assessment of skin severity in this population.

Page 9: OHE has not been defined.

Although the results from the ANOVA are significant, it is problematic to say that there was a significant decrease in the self-assessed disease severity over time because ANOVA doesn't take the directionality of time into account. ANOVA will yield a significant result as long as there are significant differences in between at least one of the timepoint pairs, but can't specify in between which specific pairs or if this is a maintained trend over time (ANOVA is good for point estimates but not for time-varying estimates, so final conclusions about time trends are not accurate).
Discussion

I disagree with the statement that there is no selection bias based on the variable "prevalence of irritant contact dermatitis". It seems natural to me that patients with contact dermatitis might be highly interested in following a program specifically designed to diagnose, treat, educate about and keep an eye on skin diseases, especially when contact dermatitis is so prevalent in this population (81%-89% prevalence according to Table 2). Please revise your statement regarding this topic.

Page 15. The authors state that correlation coefficients of r = 0.536 and r = 0.547 imply a "large" correlation. I disagree. These numbers correspond to a "moderate" correlation. A "large" correlation might be accepted in values > 0.7.

In the sub-section "strengths and limitations": I understand the authors' idea of comparing this study to other study designs, but I'd recommend to avoid comparing it to an RCT. It's preferable to compare against other similar observational designs (e.g. difference in differences, time-series analysis, etc.).

Rewrite for clarification:

"Instead of 16.9% (n=14) of patients at T1 44.6% (n=37) of patients at T4 denied to use steroids: the patients who did not require steroids for 12 months more than doubled."

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

Yes

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

No

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

Yes

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