Reviewer's report

Title: A cost minimisation analysis alongside a clustered randomised trial in teledermatology

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Reviewer: Wilbert van den Hout

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IMPORTANT NEW REMARK

After looking at the trial paper, I have serious new doubts about the estimated preventability. If I understand correctly, preventability is assessed in a similar way by the dermatologist for both experimental and control patients. This raises a number of questions (possibly due to my misunderstanding):

1. The estimated percentages for experimental and control patients are estimated at 39% and 18.3%, which is significantly different. Shouldn’t these numbers be identical? Due to the randomization, experimental and control patients should be similar, so how can preventability be different? This is not explained in the trial paper.

2. You estimated preventability by the difference between 39% and 18.3%, that is 20.7%. However, if these numbers estimate the same parameter, should you not have used their average, that is 28.7%?

3. In fact, to go even a step further, since the experimental and control group are evidently different, should you not have used the most representative estimate, that is 39%?

These are fundamental questions about the key parameter in the model, so I hope that my questions are due to my misunderstanding. In that case, please explain clearly why I am wrong.

PREVIOUS REMARKS

Previous remark R2.1: This answer is unsatisfactory. For example, on page 13 of the new manuscript it is reported that for the GP time to complete the website forms a range of 1.8 to 6.3 is assumed. From reference 9 it is clear that this range represents the minimum and maximum of the reports by the GPs. This is not the range that should be used in PSA! In PSA the uncertainty about the average should be used. This uncertainty has an approximate normal distribution with standard deviation equal to the sample standard error. I suspect that this applies to more of the parameters, but I am unable to check this because insufficient detail is provided about how these ranges are determined.

Previous remark R2.2: The authors respond that the diagnostic and treatment costs for GP and dermatologist are in fact the same parameter (which is what I suspected). In that case table 1 should not include this parameter twice, because
mentioning it twice suggests that two independent parameters were used. Instead, an additional parameter should be included that models the cost difference between GP and dermatologist care: I am 100% sure that that care provided by GP and dermatologist is not equally expensive.

Previous remark R2.5: The authors now report that expert opinions were easily aggregated. This is not a correct answer to the question how expert opinions were aggregated.

Previous remark R2.7: The term significance is no longer used, but still “95% CI” intervals are reported. It should be explained what these intervals stand for in a (Bayesian) PSA context. Without warning, readers are likely to interpret them as confidence intervals, from which p-values can be concluded.

Previous remark R2.9: please also provide a reference for PSA in the medical literature.

Previous remark R2.10-12: The questions about the validity of the dermatologists assessment of preventability are treated as interesting suggestions. Instead, the paper should explicitly discuss the validity of this trial estimate in more detail. For the purpose of this analysis I think the assessment by the GP is the gold standard, assuming that for costs it is probably the referral that counts. GP assessment for referral is different from the dermatologists assessment based on the obtained live information (for example in case of spontaneous recovery).

Previous remark R2.13: the authors now explain that PSA was performed with all parameters that, according to one-way sensitivity analysis, influenced model outcomes. I think very few parameters do not influence outcome at all. What criterion was used to classify parameters as non-influential?

Previous remark R2.15: the authors reply that reliability was checked by presenting the model to others. This is not a valid check for reliability. Please rephrase as: “After checking the face-validity of our findings by presenting it at scientific meetings, we performed …”

Previous remark R2.17: In fact, the number of patients per GP is not mentioned at all as a parameter in the model. Yet, the investment costs for the digital camera should be distributed over these patients. Why is this parameter not in the model?

Previous remarks R2.22: It is stated that new figures are added, but axes in figures 3 to 5 are still truncated.

Previous remarks R2.24-25: I suggest to include all information in table 1.

Previous remarks R2.27: The reason why to totals in table 2 do not add up is not because they are calculated by the Monte Carlo simulation. The reason is that in the third cell of the last column you failed to report 1.6 investment costs (i.e. the difference between 1.6 in the third column and 0 in the fourth column).
OTHER NEW REMARKS (page and line numbers refer to the new version of the manuscript with track changes)

Page 3, line 1: “or” instead of “and” (in “… or when more consultations …”)

Pages 7 and 8: It is now reported that the average waiting time in the trial was six months. Then, about half of the patients must have had waiting times longer than six months. Then why not use a 12 month time horizon? What would have changed in the model?

Page 11, lines 1 and 2: “time spent by the patient” instead of “time costs of the patient”

Page 12, line 9: “other variables” instead of “other unit costs”?

Page 14, line 15: it is incorrectly stated that the difference in GP costs is only due to the difference in the first consultation costs: the differences in diagnosis/treatment and in follow-up visits are just as large.

Page 14, line 20: it is incorrectly stated that dermatologists’ costs stay the same: their 28.1 difference is much larger than than the 4.1 difference in out-of-pocket costs

Page 15, line 5: a break-even point is never CAUSED by variation

Page 15: in the scenario analyses, please compare the break-even points to the base case assumptions, to see how (un)likely they are.

Page 17, line 14: It is incorrectly stated that the prevented dermatology referrals DID NOT result in cheaper dermatology care. Table 2 reports a point estimate of -28.1 with range (-90.0 to 15.6). This shows that that dermatology costs ARE decreased (I think with at least 80% probability).

Page 18, line 5: “with 89% certainty” instead of “in 89% of the cases”

Page 18, line 19: “teledermatology was more likely to be cheaper than conventional care” instead of “the probability of teledermatology being a cheaper option is increasing”

Page 20, line 15: “hypothesis” instead of “assumption”

Figure 2: “DISTRIBUTION OF THE incremental costs of …”

Table 1: what does NA stand for?

Table 2: The point estimate for the teledermatology investment costs is 1.6, which is on the boundary of the 95% CI (1.6 to 2.1). How is that possible?

Level of interest: An article whose findings are important to those with closely related research interests
Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:
I declare that I have no competing interests