Author’s response to reviews

Title: Compliance with referrals to medical specialist care: patient and general practice determinants: a cross-sectional study

Authors:
Christel van Dijk (christelvdijk@hotmail.com)
Judith de Jong (j.dejong@nivel.nl)
Robert Verheij (r.verheij@nivel.nl)
Tessa Jansen (t.jansen@nivel.nl)
Joke Korevaar (j.korevaar@nivel.nl)
Dinny de Bakker (d.debakker@nivel.nl)

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Response to the reviewers

Reviewer 2

We thank the reviewer for the throughout review of our manuscript, and agree that our manuscript concerns an important issue in health care.

1. The research question?

This a clear cut paper with a narrow clear research question. This question concerns the important question about compliance after referral. It has not been researched properly in The Netherlands in such a large study and the 13% non-compliance is an important finding. It is a benchmark for other countries health care functioning of referral.

The fact that in the US similar studies gave only slightly lower rates could indicate that referral non-compliance is hardly influenced by organisational or GP-actors, but other factors. The lower compliance in people with SES and the important role of patient factors is no surprise. The other finding in relation to compliance are a bit more unexpected (distance, differences between specialisms), but their importance is limited. So yes the subject is original, but I miss qualitative
research in the non-compliance group and in compliance to referrals, that would truly assess the problem.

These data give some crude information but require a lot of interpretation and guessing about the true causes of (non)-compliance.

So the conclusion is a bit narrow, yet clear and new, but not surprising. We are still left with lots of question about referral compliance.

The introduction is a proper description of the state of the art on this aspect of referrals.

Response: We agree with the reviewer that this research is just one step in the process to unravel the causes of (non)-compliance. And that qualitative research is an important next step to gain insight in the true causes. Using routinely recorded administrative health data, has the advantage of having large quantities of data available at low costs, but has limitations as well. One of these was that it was not possible to contact the compliance and non-compliance group within the scope of this study and add more qualitative analyses. However, think that the results of the present study does give important clues to focus more qualitative studies in the future. In addition, we think the fact that this is one of the first papers on this subject in Europe, is important in itself. The reviewer agrees on this point in his fifth comment.

One of the important findings of this study is that it is not necessarily lower SES, but lower SES in an urban neighbourhood that negatively affects the referral compliance. Future research should focus on important factors in urban deprived neighbourhoods, including for example focus group meetings with the non-compliance group in the urban deprived neighbourhoods.

We have included the suggestions for further research in the discussion section of the manuscript: “Further qualitative research is needed to gain more insight in the true causes of non-compliance in this group”

2. The data

One could be suspicious about these data from such large databases, which have few possibilities to check the data. The database Vektis however is owned by the insurers and is known to be accurate (needed for accountancy) and the authors seem to have checked and cleaned the practice data. The data are sound and their reliability is confirmed by the results.

Response: No further comments.
3. Discussion and conclusion

The interpretation is unbiased and the interpretation of the data is not speculative and based on assumption known from previous research. They do not mention an exhaustive literature search, but the relevant literature seems to be properly consulted. References are OK.

A weakness is that they have only literature from the US and nothing from the UK or any other European country. It may not have been researched there and it is good to mention more upfront that the subject is a bit of an orphan subject.

They start the conclusion stating that compliance to the referral is vital. Yet, they do not know how harmful non-compliance is in The Netherlands. In the US with non-insured patients it may be different. That is their reference. In the Netherlands often patients are referred for diagnostic reasons with no underlying disease. They may decide that referral is not worst while after all. Others may go abroad (often in Holland) or try alternative medicine. A more qualitative approach could have shed light on what is really going on. So I would start the discussion with the main results and be a bit less suggestive of the possible damage by non-compliance.

Response: We were surprised ourselves that no study from other European countries and especially the UK were found, as the UK has a gatekeeping system as well. We have even contacted researchers on this subject in the UK to confirm this. We agree with the reviewer that we have no indication of the harmfulness of non-compliance and that the reference on this subject is from the US, which may differ. We included the following sentence in the discussion:

“The consequences of non-compliance are unknown, but a study from the United States shows that non-compliance might result in delayed diagnosis and treatment, and poorer health outcomes [4]. As the level of health insurance coverage is lower and level of out-of-pocket payments are higher in the United Stated, consequences of non-compliance might be different [7].”

Our dataset only included reimbursed medical specialist care. This included care in Belgium or Germany, if reimbursed by the health insurer. According the Dutch health insurance act, a patient can directly visit other EU countries for any treatment if they have a referral from a general practitioner of medical specialist. In practice, a lot of health insurance companies work with preferred providers, probably excluding health care providers abroad. We have included the following sentence in the strengths and limitation section:

“In addition, patients may have visited health care providers abroad. These services are only included in the Vektis data if the service is claimed and reimbursed by the health insurer. This is dependent on the health insurance policy, and especially the possible preferred provider policy”

And the following sentences in the conclusions and implication section:

“Or patients may visit alternative health care providers instead.”
“In addition, patients in the Netherlands are often referred for diagnostic reasons without a known underlying disease.”

4. Methods

The methods is straightforward and the logistic regression is adequately done. The only hesitation is that the introduction and research question would be better addressed with a more profound method including qualitative data. That would have yielded a deeper insight in the problem. The statistical analyses are sound and do not need additional scrutiny.

Response: The need for more qualitative research is more stressed in the discussion section now. See also reaction to the first comment.

5. Strengths and weaknesses of the methods

The weakness is the opportunistic use of the available data to give insight in a problem, that with a different study design could have been addressed more adequately. Yet, the authors don't pretend more than what they have found and this is important enough to be published.

Response: Indeed, we have made opportunistic use of routinely recorded administrative data and this approach has weaknesses. We agree with the reviewer, that we don’t pretend more than we have found and that the results of our study are important enough to be published.

6. Writing, organization and tables

The English is pleasant to read and seems OK to me. Page 12, 5 link should be linked.

The paper reads easily and the structure is clear and adequate. The same is true for the tables.

Response: We changed the word ‘link’ op page 12, line 5 into ‘linked’

Reviewer 3

We thank the reviewer for the complements on our manuscript.

1. This article is an analysis of database and deals on the reason of non-compliance of patients to the referral of the family physicians.
This is an interesting study, which is coming from a country where primary care is believed to be one of the best developed in Europe.

The question on non-compliance to referral recommendation might be an effort to improve quality of referral services.

This observational study covers a sample from the period between the years 2008-2010. This study is a subsequent study from a larger trial. Even the data selection process has been described thoroughly, the calculation of the general sample size is missing.

The paper is well-written, well-structured and presents sound evidence.

Response: We included the number of general practices in the period 2008-2010 of the general sample size in the manuscript.

Reviewer 4

We thank the reviewer for the useful comments on our manuscript.

1. Methods

For readers not familiar with the Dutch health care system the authors should clarify the usage of the data bases. Who is participating in the NIVEL data base? Which percentage of GPs? Is there a difference between NIVEL-participants and non-participants? Who is using Vektis? Are there other possibilities for specialists to claim their services?

Response: We understand that our manuscript needs some clarification with regard to the Dutch health care system. We made several changes in the manuscript:

“In these years combined, 140 general practices (3.4% of general practices in the Netherlands) were included in the database. General practices are selected based on the quality of their EHR and representative of the Dutch GP population. Overall, GPs that participate in NIVEL PCD are representative of the Dutch GP population with respect to age, gender, period of settlement, region and urbanization. No differences in medical treatment are found between GPs with varying degrees of EHR use [10].”

“Vektis collects, among others, DRGs claimed to all health insurers in the Netherlands.”

“In the Netherlands, medical specialists can claim their services to the patient’s health insurer. Only in exceptional cases, the care is paid by the patient itself.”
2. Practices or "practice years" were excluded from the study depending on data quality. How was this assessed? Which criteria were applied? In which case was "data on care episodes" or "data on contact" or "data on prescriptions" etc. judged to be "incomplete"? What do the numbers behind these exclusion criteria mean (±25%, ±30%, ±30%, ±50%)? What does the "±" mean? Why don't the numbers add up to 100%? Why don't the authors give the exact figures?

Response: Since we checked the data on more than ten data quality indicators, we did not include this information. The numbers do not add up to 100%, since general practices could have failed on various checks. We included some examples of the checks in the manuscript.

3. How can the data on capitation fees be used to judge whether a patient comes from a deprived urban area?

Response: In the Netherlands, GPs receive higher capitation fees for patients living in an deprived urban area. The GPs claim the capitation to the patient’s health insurer, with different claim codes for patients in deprived urban areas.

4. At the end of the "study design and population" section the authors claim that the included practices were representative of Dutch general practices with respect to urbanisation and region. How was this measured/assured? Practice characteristics regarding urban/suburban/rural/deprives/region are not shown in table 1. The practice characteristics of all "Dutch general practices" should be shown in comparison. Equally the patients' characteristics of all patients or at least all patients in the NIVEL data base could be shown.

Response: NIVEL maintains a registry of all GPs working in the Netherlands: the practices they work and a number of other characteristics. We compared characteristics of practices participating in NIVEL PCD with all other practices in the Netherlands. We have included a reference of the registry of the GPs in the manuscript. In our opinion, an extra table is unnecessary.

5. Data in the text don't match with table 2 (text 92.1% eye problems; table ear problems). Are any of these differences statistically significant (using Chi²)? - I imagine, not. Even less so the difference between diagnosis and diseases (87.4% versus 86.0%). It should be stated that these differences are insignificant.

Response: We changed ‘eye problems’ into ‘ear problems’. The reported differences are statistically significant. Significant differences are now mentioned in the revised manuscript.
6. What does the second column of table 3 stand for? Most of the numbers are identical between the first and second column. Sometimes there are only numbers in the first, sometimes only in the second column. This needs to be explained.

Response: The difference between the models is the indicator for neighbourhood SES. In the first model ‘deprived urban area’ is included in the model, and in the second model ‘social status of the neighbourhood’ is included. In the revised manuscript, this is now explained in the method section.

7. The "significant" odds ratio of 1.01 for the distance to the nearest medical specialist needs some further explanation. "km" is a continuous variable. How was this variable categorized for the calculation of an odds ratio? Even though this variable may be "significantly" associated with referral compliance due to a hardly credible very small confidence interval, an odds ratio of 1.01 of course is nothing of any relevance. Please check this result. Please omit the sentence in the discussion that "those who lived further away from medical specialist care facility were more compliant". This result should also be deleted in the abstract, or the odds ratio of 1.01 must be reported there as well.

Response: We included the distance to the nearest medical specialist as continuous variable in the model. So, the odds ratio of distance to the nearest medical specialist shows the increased odds, when the distances increased with 1 km. We have checked the results from the analyses, and the odds ratio and confidence interval is correct. We agree with the reviewer that the association of 1.01 is not relevant. We have excluded the distance to the nearest medical specialist from the abstract and discussion.

8. Also, face-to-face contacts and guideline adherence of referrals were defined as continuous variables in the methods section. How were these variables categorized for the calculation of odds ratios?

Response: The number of face-to-face contacts and guideline adherence were included as continuous variable in the model. So, for example the odds ratio of face-to-face contacts shows the increased odds, when the average number of face-to-face contacts within the general practice increases with 1 contact.
9. Discussion

The limitation section should discuss the problem of multiple testing in an explorative study leading to significant results (e.g. odds ratios with 95% CIs completely below or above 1) just by chance.

Response: The reviewer is correct that analyses of 19 variables in the model could lead to significant results, in this case one by chance. We have checked all our significant results and all but one (75 years or older) are also significant at a stricter significance level of $p=0.01$. We included the significant level of $p=0.01$ in table 3.

10. In the discussion the authors refer to a higher compliance in patients with chronic conditions, but the data are not shown. Why? If this is really the case, it appears to be more important than the odds ratio of 1.01 for the distance to the nearest specialist which is reported and discussed extensively.

Response: We included the association (only OR, no 95%CI) of the chronic conditions in the discussion. We excluded the section on the distance to the nearest medical specialist.

11. The passage in the discussion about the relationship between referral compliance and the distance to a medical specialist should be reworded, as the odds ratio of 1.01 is really nothing - even though "significant" - and is - as a number - in concordance with the literature that there is no correlation between referral compliance and distance to medical facilities.

Response: We excluded the part of the distance to the nearest medical specialist from the discussion, as the difference does not seem to be relevant.

12. I would like to suggest for the discussion that each point in the discussion is started with a new paragraph (lines 15/16; 33/34; 54/55).

Response: We changed the manuscript according to the reviewer’s suggestion.

13. The fact that general practice characteristics were not associated with compliance does not allow the conclusion that this may also be true in all practices in the Netherlands. It might be just the other way around. Practices participating in NIVEL and practices with high quality documentation in the EMR (i.e. the practices included in the study) may very well have higher
referral compliance than the rest. In the present sample, referral compliance was very high. This may in part be due to the methodology applied and the selection of practices used.

Response: No differences in medical treatment are found between GPs with varying degrees of EHR use. So, we do not expect our differences to be the result of a selection of general practices. The methodology used, with a claimed DRG in a half year period after the referral by a specific medical specialty to whom the patient was referred to, would rather show an underestimation of the referral compliance than an overestimation. We believe that significant improvements are possible with the compliance rate of 86% that was found.