Reviewer's report

Title: Evaluation of the short message service reminders on patient's medication adherence results of a controlled study to assess the effects of and satisfaction with incoming messages

Version: 2 Date: 2 May 2013

Reviewer: Thyra E de Jongh

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Major Compulsory Revisions

Methods

1. It is unclear whether group assignment was random or not. Given that randomisation is not explicitly mentioned, either in the title or in the methods, one is inclined to assume that this was not done. If indeed there was no randomisation, it should be justified why this was not done. After all, given the controlled setting, this would have seemed entirely feasible and randomisation would have eliminated a potential source of allocation bias.

2. It is not clear on what the allocation ratio of 2:1 was based. Was this somehow informed by a power calculation? Is it related to the fact that there were 3 hospitals involved, i.e. was there cluster randomisation? This should be explained.

3. The outcome measures relating to adherence are insufficiently defined. For instance, what is meant by a “delayed dose”: i.e. at what point is a dose considered delayed and when is it considered missed completely?

Discussion and study limitations

4. Follow-up of only 7 days seems extremely short for an intervention that is supposed to aid in management of chronic disease. It is well known that interest in novel technologies such as SMS reminders wanes over time (E.g. Hanauer et al., Diabetes Technol Ther. 2009, 11(2):99-106) and that long-term adherence is much harder to foment than short-term adherence. This concern is especially relevant in light of the reported finding that the frequency of messages was already perceived as ‘too high’. Intervention fatigue is likely to be a limiting factor on the effectiveness of the intervention. Although this is recognised as a limitation in the discussion, it nonetheless raises questions about the relevance of this work in relation to management of chronic diseases. One could argue that the findings are more relevant to time-limited treatments, such as adherence to a course of antibiotics. This discussion needs to be worked out much better.

5. Given that the medication adherence outcome data are purely reliant on self-reporting, there is a high likelihood of reporting bias. Many studies have shown that self-reported adherence rates tend to be overestimated as compared
with findings from e.g. electronic pill-counting devices. This needs to be discussed and recognised as a limitation.

6. In the conclusion, it is mentioned that the PMP system may have sent more than one reminder to patients receiving several drugs from the clinical department at the time of recruitment. This implies that these patients would also have had to show adherence to several drug regimes. How has this been treated in the analysis? Is there potential for clustering of data? No explanation of this is given at all.

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

7. It is unclear what the data under the heading ‘demand for SMS’ in Table 5 refer to. The categories used here are identical to those used under ‘satisfaction with SMS’ in the same table. Is this correct? Please confirm.

8. The reporting of percentages throughout the text is somewhat inconsistent. Sometimes integers are used; other times one or even two digits. This needs to be made consistent.

Discretionary Revisions

Methods

9. It is stated that the SMS content depended on “the extent of patient involvement in the text message medication reminder service’. What does this mean? How is the extent of patient involvement defined and in what way did this influence the SMS content?

10. Are all patients, both in the intervention and control group, users of the PMP differing only in whether they receive the value-added SMS or is the PMP only used by those in the intervention group? If used by both groups, have there been any studies evaluating the effectiveness of the PMP without the SMS reminders already? If so, these should be discussed and referenced. If, however, the comparison is between ‘no PMP’ versus ‘PMP + SMS’, the availability of other adherence supporting features of the PMP should be discussed as a potential confounding factor.

11. Given that, according to Table 2, the denominator N is set to the number of participants in each group, rather than to the number of medication doses scheduled, I assume that the number of ‘delayed doses’ actually is the number of patients with a delayed dose (and similarly, the number of patients who missed a dose). This is in itself a justifiable choice, but it would also have been interesting to know if the number of delayed or missed doses per patient has decreased. In other words, even if a patient is still classified as ‘delayed/missed dose’, it is possible that his/her overall adherence has improved. Has this been considered as a possible outcome and if not, why not? After all, perfect adherence is often not required to achieve improved treatment effect.
12. The approach to analysis of the effectiveness data is somewhat unclear. It is stated that: “McNemar’s tests were conducted to understand the distinct levels of improvement in the incidence of delayed or missed medication doses among the patient groups.” Presumably, this was only carried out within the groups, i.e. comparison of pre-test vs. post-test data, as this test cannot be used for comparison between the intervention and control groups. However, it is not explained up front how the comparison between these two groups was done, whereas this is in fact the most relevant comparison. It appears that a simple odds ratio (OR) was used to describe the effect sizes. The methods section should mention this.

**Discussion**

13. The discussion into factors influencing adherence raises some questions. For instance, it is stated that “patients aged 65 years or older were significantly less likely to experience an improvement in the incidence of missed doses compared to the patients aged between 20 and 34 years”. The explanation given is interesting and grounded in literature, but it is striking that this same effect is not also seen for “delayed doses”. There the data show no significant effect at all, which sheds doubts on the explanation given. A similar lack of correlation between delayed and missed doses applies to the observation regarding military, civil servants, teachers and students.

14. There are quite a few studies that have used SMS reminders to impact medication adherence, e.g. for chronic disease management (e.g. asthma, diabetes, hypertension, HIV), adherence to oral contraceptives, or to chemoprophylaxis. This paper references a few of these studies [12, 13, 15, 16], though not necessarily the most appropriate. For instance, the reference by Da Costa TM 2010 [15] is a study centred primarily on appointment reminders rather than medication reminders. A better reference by the same authors would be: da Costa TM, et al. International Journal of Medical Informatics 81 (2012), 257-269. Similarly, the study by Kim et al. [16] uses SMS for supporting self-management of diabetes, but does not actually use medication reminders. The authors should ground their findings better in existing literature and, in doing so, distinguish between short-term and long-term effectiveness.

**General**

15. The English is a bit awkward at times and would probably benefit from some editing, but is generally acceptable.

**Level of interest:** An article of limited interest

**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