Overview

Let’s Get Moving (LGM) is a Systematic Approach to integrating physical activity promotion into the primary care setting. LGM combines a number of recommended strategies to support behavior change that can be incorporated into a brief intervention, such as motivational interviewing (MI), goal setting, written resources, and follow-up support. The aim of the study was to evaluate the process and feasibility of implementing LGM in UK general practices of primary care. More specifically, the aims were to assess two patient recruitment methods (opportunistic” vs. disease register), to track patients through the intervention and assess delivery of each component, to assess the time requirements of delivery, and to collect qualitative feedback from practitioners on their experiences in implementation.

Major Compulsory Revisions

1. The authors provide a good rationale for using Primary Care as a public health intervention to boost physical activity in adults. However, they cite several RCT’s that have examined this issue (i.e Elley, et al., 2003; Hardcastle et al., 2008; Hillsdon et al., 2002; Smith et al., 2000) but do not report the results of these studies. Based on the title alone of the Hillsdale et al 2002 study, counseling people to become more physically active in primary care was not successful. Brief intervention in primary care using an RCT design with valid outcome measures was also not found to be effective in reducing body mass index in overweight and obese children (Wake et al., BMJ 2009; 339:b3308; McCallum et al. J Paediatr Child Health, 2005; 41, 488-94), and the authors conclude that Primary Care is a method of public health intervention that does not change lifestyle behaviours or BMI and therefore may not be worth the high cost of investment. In this context, it is imperative that authors report the results of these RCT’s to determine if further brief intervention of PA in primary care is even warranted.

2. The authors make 2 broad-based conclusions in abstract and discussion. The first being that the information gleaned from the pilot should inform a revised update of the :LGM protocols before wider implementation is considered. Immediately following this conclusion is “that a robust assessment of effectiveness involving an experimental design and behaviour change measures is also warranted”. I highly agree with the first conclusion (LGM needs more
revision before further testing) given all the limitations that this feasibility study has, many of which the authors do a good job of noting. These limitations include a lack of quality control to assess how effective the lifestyle change training (including MI) was for health care practitioners, low recruitment rate, heavy attrition (almost 70%) at 3-month follow-up, misclassifying active subjects as sedentary and vice versa, trying to modify inclusion criteria to classify walking as exercise, including active subjects when the intent was to focus on inactive subjects based on the GPPAQ, which had no reported psychometrics to support its use, concerns by physicians that leisure facility programme and timetables may not be current. Another limitation that was not acknowledged was that authors report that 75% of subjects at follow-up reported an increase in physical activity. While I believe it is important for feasibility studies to collect preliminary outcome data, no mention of how that statistic was measured – verbal report or questionnaire or accelerometry? Given the lack of control group, high attrition, and a strong social desirability effect (i.e. telling the researchers what they want to hear) probably has biased and overestimated the true percentage of those who increased physical activity. Lastly, it appears that subjects of Asian or British Asian decent were over represented compared to the overall British population, and this may have influenced interest and conduct of brief intervention in primary care, another limitation that authors did not acknowledge.

3. The authors state that they wanted to target only inactive subjects based on their GPPAQ questionnaire that was modified to include walking. However, based on data in Table 1, only 198 subjects were classified as “inactive”. Thus, how do you explain the n=367 of those who were eligible and interested? This needs to be reconciled.

4. Since they collected some outcome data by asking subjects if they increased their physical activity, this could be added as an aim of the study as it could inform sample size of a larger RCT. However, this would involve description of the measure of physical activity in subjects and providing some reliability and validity to support its use. If this cannot be achieved, I recommend dropping the reporting of the statistic that 75% of subjects increased their physical activity due to reasons mentioned above (response bias, etc.)

5. A 3-month follow-up from a brief intervention is a good time sampling point, and authors suggest the low response rate (high attrition) was due to a brief follow-up, but typically response rate and retention gets worse over time, thus I don’t agree their response would have been better at 6-months.

6. The percent reported at follow-up in Figure 2 is 19% (101/526) but I think it should really be based on the percent who attended follow-up who were eligible and interested in the trial – n=367, thus response rate at follow-up would be 27.5%. Relatedly, an argument could be made that attendance at follow-up should be compared to those given the brief intervention, which would be 314, making the response rate at FU of 32%. Irrespective of the methods used to calculate response rate at 3-month follow-up, all are considered quite low, and if individual level outcome data were collected, the results would be virtually uninterpretable and meaningless due to the high attrition.
7. Page 14. –first sentence in middle paragraph – ‘In addition, this study focused on assessing feasibility of implementing LGM and did not include the collection of patient level behavior change data. Technically, this is incorrect since authors state that of the people who attended the 3-month follow-up, 75% self-reported an increase in physical activity. Thus, either drop that statistic for reasons mentioned above, or modify this sentence and discussion accordingly.

8. No statements in the methods were made regarding the study being granted research Ethics Board Approval or whether subjects were required to sign an informed consent form. This needs to be added.

MINOR ESSENTIAL REVISIONS

None Noted

DISCRETIONARY REVISIONS

1. On page 7, the description of the qualitative data that involved focus groups that were transcribed and analyzed using inductive content analysis, then results being grouped by theme area, reflecting key components of the intervention is consistent with validated qualitative methodology. It would be helpful for the reader to reference the qualitative approach to assessment, such as the Dillman method etc.

2. Although Figure 2 is a nice flow diagram, it would also be helpful if the number of subjects who were assessed for participating in the study – n=526 be added to abstract and methods.

3. In the results – page 8 – state that response rate in many categories (except follow-up) was better for the disease-state recruited subjects compared to the “opportunistically” recruited subjects, as discussed in the discussion. Because differences between the recruitment methods are discussed, state whether these differences are statistically significant (from chi-square tests) or clinically significant, or both.

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