PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Discontinuing antidepressant medication after Mindfulness-Based Cognitive Therapy: A mixed-methods study exploring predictors and outcomes of different discontinuation trajectories, and its facilitators and barriers.
AUTHORS	Huijbers, Marloes; Wentink, Carolien; Simons, Esther; Spijker, Jan; Speckens, Anne

VERSION 1 - REVIEW

REVIEWER	Dr Ceri Wilson Anglia Ruskin University, UK
REVIEW RETURNED	05-May-2020

GENERAL COMMENTS	Thank-you for the opportunity to read this interesting and well-written manuscript. I have a few comments and suggestions below. Introduction There are a few minor typos in the introduction – just needs a quick proof read. Relevant literature is cited, I didn't think there was any essential literature missing. Methods Not much detail is given about the RCT, however as this has been detailed elsewhere and this is clearly signposted I think this is fine. I think there is some confusion about the difference between PPI (patient involvement) and patient participation under the heading 'Public and Patient Involvement' – the authors refer to participants in the qualitative element of the study as PPI however this is participation as opposed to involvement. See https://www.invo.org.uk/posttypefaq/what-is-public-involvement-in-research/ I found the methods section a little confusing in terms of distinguishing between the qualitative and quantitative methods – I suggest in the 'procedure' section clearly indicating the quantitative element, and stating briefly what kind of follow-up assessments you are referring to. See my suggested additions to the text in CAPITAL LETTERS below:

	"Eligible patients were randomly assigned to MBCT followed by guided discontinuation of ADM or to MBCT with continuation of ADM. Follow-up QUANTITATIVE assessments (MEASURES OF DEPRESSION, RELAPSE/RECURRENCE, ADM USAGE) took place 3, 6, 9, 12, and 15 months after baseline." I also think the authors should consider having separate subheadings 'quantitative analysis' and 'qualitative analysis'. The authors have stated that they received ethical approval but have not discussed the ethical considerations of the research as recommended in the reporting checklist e.g. process of obtaining informed consent from participants. No rationale is provided for why constant comparison was chosen as the best analysis approach. There is also no mention of reflexivity. Online supplement 1 I think it would be really worthwhile to add quotes in for all subthemes, as opposed to just under some of the subthemes. One other query about the supplement - the authors state that a pre-existing belief that ADM is harmful is a facilitator to discontinuation, but provide a quote from someone who believes ADM is harmful but has not discontinued. Did the participant describe that belief as a facilitator, even though they hadn't discontinued their medication – or have the authors classed it as a facilitator without the participants indicating that it is a facilitator? Results Given that the authors state that there was a lot of crossover between the patient and clinicians' reported facilitators and barriers, would it be worth presenting the patient and clinician findings together? Particularly as the authors don't currently present the clinician findings under theme and subtheme headings – rather a short paragraph summarising the clinician findings. It would be useful to have some quotes in the actual text – was this omitted because of word count restrictions? It is really interesting reading the quotes in the online supplement and it would be a shame not to have at least a few quotes in the main text. All the best with your revisions.

REVIEWER	Richard Morriss University of Nottingham, Nottingham, UK
REVIEW RETURNED	06-May-2020

GENERAL COMMENTS	The strength of this paper is the combination of quantitative data collected in a large randomised controlled trial with qualitative data of a maximum variance of participating patients and some prescribers. The quantitative data contrasts with previous studies but is consistent with my own experience of clinical practice as a psychiatrist working in a secondary care setting. The qualitative data is line with previous findings.

	There are however some important limitations of the design that are not acknowledged. There is no control group of patients withdrawing from antidepressant medication who do not receive mindfulness CBT. Predictors of discontinuation of antidepressants with MCBT might be predictors of take up of MCBT overall rather than discontinuation of ADM and this cannot be determined without a control group not receiving MCBT. The same issue may apply to the discontinuation outcomes. These issues are conflated by differences in attendance at MCBT sessions. The authors should report if these differences are statistically different (and also at attendance at medication discontinuation as well) These possibilities should be discussed as limitations of the method. There is no description of the measures collected at baseline and those that are presented in Table 1 do not reflect all the factors that a prescribing clinician would consider before recommending discontinuation. For instance there are mo measures of patient's subjective readiness to discontinue, mental and physical morbidity, other baseline medication which might lead to adverse effects on the patient, life situation, and past history of hospitalisation, self-harm, persistence of clinically important symptoms, baseline severity of anxiety. This is a limitation of predictors of discontinuation and may confound those factors that have been identified as well as outcome.These factors are also important to understand why there might be differences in outcome with MCBT on ADM discontinuation across studies. Again these issues should be covered in the discussion. Another limitation is that it is unclear whether the definitions of full, partial or no discontinuation were determined before the study started. during the study but before analysis or post-hoc. If post-hoc, then the definition might compromise the independence of both quantitative and qualitative analysis; it would be important to outline in the method any steps to reduce such bias and to discuss this matter as a limitation or strength if determined before the start of the study.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
Reviewer Name: Dr Ceri Wilson
Institution and Country: Anglia Ruskin University, UK

Introduction

There are a few minor typos in the introduction – just needs a quick proof read.

We checked the introduction for typos and corrected them.

Methods

I think there is some confusion about the difference between PPI (patient involvement) and patient participation under the heading ‘Public and Patient Involvement’ – the authors refer to participants in the qualitative element of the study as PPI however this is participation as opposed to involvement. See https://www.invo.org.uk/posttypefaq/what-is-public-involvement-in-research/

We would like to thank the reviewer for pointing this out and agree with her that including patients in the qualitive study is participation rather than involvement. We adapted this paragraph as follows:

“At the time of the study, public or patients were unfortunately not yet involved in working with the research funder to prioritise research or offering advice as members of our own project steering group. However, ….“ (p.6)

I found the methods section a little confusing in terms of distinguishing between the qualitative and quantitative methods – I suggest in the ‘procedure’ section clearly indicating the quantitative element, and stating briefly what kind of follow-up assessments you are referring to. See my suggested additions to the text in CAPITAL LETTERS below:

We adapted this paragraph in accordance with the reviewer’s suggestions:

“Eligible patients were randomly assigned to MBCT followed by guided discontinuation of ADM or to MBCT with continuation of ADM. Follow-up QUANTITATIVE assessments (MEASURES OF DEPRESSION, RELAPSE/RECURRENCE, ADM USAGE) took place 3, 6, 9, 12, and 15 months after baseline.” (p.7)

I also think the authors should consider having separate subheadings ‘quantitative analysis’ and ‘qualitative analysis’.

We adapted the Statistical Analysis section accordingly (p.8 and 9).

The authors have stated that they received ethical approval but have not discussed the ethical considerations of the research as recommended in the reporting checklist e.g. process of obtaining informed consent from participants.

For reasons of word count restrictions, we refer the readers for further information about the process of obtaining informed consent from participants to the protocol paper of the study. We clarified this in the Methods section on p. 6:

“For further information on the specific in- and exclusion criteria and process of obtaining informed consent from participants we refer to the publication of the trial itself.6” (p. 6)

No rationale is provided for why constant comparison was chosen as the best analysis approach. There is also no mention of reflexivity.

In response to this reflection of the reviewer, we added some information to the paragraph on the qualitative analysis to further clarify this:

“The constant comparative method was used to develop a theory that was grounded in the data, namely by categorizing, coding, delineating categories and connecting them.22 (…) Subsequently codes were modified and categorized as various facilitators and barriers by the full research team, also consisting of a professor of psychiatry and mindfulness teacher with prior experience with qualitative research (AS) and a psychologist and post-doc researcher (MH). The cycle of comparison and reflection on “old” and “new” themes was repeated several times. Eventually, characteristic quotes were used to illustrate the final themes and subthemes.” (p. 9)

Online supplement 1

I think it would be really worthwhile to add quotes in for all subthemes, as opposed to just under some of the subthemes.

In accordance with the reviewer's suggestions, we added quotes for all subthemes in the table in Online Supplement 1.

One other query about the supplement - the authors state that a pre-existing belief that ADM is harmful is a facilitator to discontinuation, but provide a quote from someone who believes ADM is harmful but has not discontinued. Did the participant describe that belief as a facilitator, even though they hadn't discontinued their medication – or have the authors classed it as a facilitator without the participants indicating that it is a facilitator?

We understand this inconsistency might be confusing to the readers, but sometimes patients mentioning facilitators did not succeed in discontinuing their ADM, and sometimes those mentioning barriers did. In fact, most patients mentioned both facilitators and barriers. In case facilitators or barriers were clearly related to previous experiences, we added this in the supplement. In this case, the belief that ADM are harmful acted as an initial motivator, but was not enough to manage to discontinue in the end.

Results

Given that the authors state that there was a lot of crossover between the patient and clinicians' reported facilitators and barriers, would it be worth presenting the patient and clinician findings together? Particularly as the authors don't currently present the clinician findings under theme and subtheme headings – rather a short paragraph summarising the clinician findings.

We thank the reviewer for this suggestion. Word count restrictions have been an important reason for us to present the results as they are. As the findings from the interviews with the clinicians to a great extent confirmed those of the patients, we thought the quotes of the patients might be most relevant to present. We did however, as mentioned in the paper, pay specific attention to the themes emerging from the clinicians which had not been mentioned by the patients.

It would be useful to have some quotes in the actual text – was this omitted because of word count restrictions? It is really interesting reading the quotes in the online supplement and it would be a shame not to have at least a few quotes in the main text.

We are grateful to the reviewer for this suggestion and indeed included a couple of interesting quotes in the main text:

One patient said, for example: "I was using Citalopram and I believe I had to taper within 2 weeks, but I was suffering quite a lot from withdrawal symptoms, so therefore, after consultation, tapered somewhat more slowly." (p.14).
"He [the psychiatrist] said: "I would not do it with your history and family matters". But I wanted to taper (..) and so I did." (p. 15)

Reviewer: 2
Reviewer Name: Richard Morriss
Institution and Country: University of Nottingham, Nottingham, UK

There is no control group of patients withdrawing from antidepressant medication who do not receive mindfulness CBT. Predictors of discontinuation of antidepressants with MCBT might be predictors of

take up of MCBT overall rather than discontinuation of ADM and this cannot be determined without a control group not receiving MCBT. The same issue may apply to the discontinuation outcomes. These issues are conflated by differences in attendance at MCBT sessions. The authors should report if these differences are statistically different (and also at attendance at medication discontinuation as well) These possibilities should be discussed as limitations of the method.

As a response to the reviewer, we reported the statistical difference of the differences in attendance of both MBCT sessions and medical consultations:

“Adherence to MBCT sessions differed significantly between those with a full discontinuation profile M=7.2 ± 1.5 versus the no discontinuation group, M=6.4 ± 2.0 (p = .003). Those who partially discontinued were in between, with an attendance of M=7.0 ± 1.4. The number of medication consultations also differed significantly among the groups, being highest for those with a full discontinuation profile (M=3.0 ± 2.0; range 0-13) versus partial discontinuation (M=2.3 ± 1.4; range 1-6) (p = .03) versus no discontinuation group (M=1.6 ± 1.5; range 0-11) (p < .00). The partial and full discontinuation group did not differ significantly (p = .06).” (p. 10)

We also discussed the lack of control group of patients who do not receive MBCT more fully in the Discussion section of the paper:

“Another limitation is that due to the set-up of the current trial, there is no control group of patients withdrawing from antidepressant medication who do not receive MBCT. Consequently, predictors of discontinuation of antidepressants with MCBT might be predictors of the take up of MCBT rather than discontinuation of ADM. The same issue might may apply to the discontinuation outcomes. These issues might be conflated by the differences found in attendance at MCBT sessions.” (p. 19)

There is no description of the measures collected at baseline and those that are presented in Table 1 do not reflect all the factors that a prescribing clinician would consider before recommending discontinuation. For instance there are no measures of patient's subjective readiness to discontinue, mental and physical morbidity, other baseline medication which might lead to adverse effects on the patient, life situation, and past history of hospitalisation, self-harm, persistence of clinically important symptoms, baseline severity of anxiety. This is a limitation of predictors of discontinuation and may confound those factors that have been identified as well as outcome. These factors are also important to understand why there might be differences in outcome with MCBT on ADM discontinuation across studies. Again these issues should be covered in the discussion.

Table 1 summarizes the main baseline demographic and clinical characteristics we considered relevant with regard to the chance of relapse/recurrence and hence collected during the trial. These do include measures of life situation, such as marital state and employment, and clinical characteristics like number of previous episodes, persistence of depressive symptoms and past history of suicide attempts. Unfortunately, they don’t include other measures like the patient’s subjective readiness to discontinue, somatic or psychiatric comorbidity and other medication.

In reaction to the reviewer’s comments on this, we included the following paragraph in the Discussion Section of the paper:

“Although we did collect some baseline demographic and clinical characteristics that might influence discontinuation, this obviously does not include all the factors that a prescribing clinician would consider before recommending discontinuation. For instance there are no measures of patient's subjective readiness to discontinue, mental and physical comorbidity or other baseline medication which might lead to adverse effects on the patient. This is a limitation of predictors of discontinuation and may confound those factors that have been identified as well as outcome. These factors may also

be important to understand why there might be differences in outcome with MCBT on ADM discontinuation across studies." (p. 19-20)

Another limitation is that it is unclear whether the definitions of full, partial or no discontinuation were determined before the study started. during the study but before analysis or post-hoc. If post-hoc, then the definition might compromise the independence of both quantitative and qualitative analysis; it would be important to outline in the method any steps to reduce such bias and to discuss this matter as a limitation or strength if determined before the start of the study.

The definitions of full, partial and no discontinuation follow straight from those of full, partial and no adherence as defined in the protocol paper of the trial. As such, they have been determined before the study started. This is mentioned as a strength in the Discussion section of the paper:

"Rather than using opinions or hypothetical perspectives on tapering, we report data on the actual tapering process in an RCT, including clinical outcomes, in which full, partial and no discontinuation were defined before the study started 18." (p.19)

VERSION 2 – REVIEW

REVIEWER	Dr Ceri Wilson Anglia Ruskin University, UK
REVIEW RETURNED	10-Aug-2020

GENERAL COMMENTS	My previous comments have been addressed satisfactorily and I think this is an important contribution to the field.

REVIEWER	Richard Morriss University of Nottingham United Kingdom
REVIEW RETURNED	27-Jul-2020

GENERAL COMMENTS	I think the paper has clinically important and relevant new data to present that is of general interest to patients, primary care and secondary care mental health professionals. I am fully supportive of its publication ultimately. I also think there are important gaps in the information that is presented, and the trial's methods, results and discussion are not adequately described in relation to the trial from which this further analysis is defined. Also having set up a potentially interesting comparison with UK studies in the introduction the authors do not return to the topic in the discussion. There are important learning points that will help readers make sense of this literature if there was such a discussion. The main problem is that there is not an adequate description of the original trial in PICO format including the aim of the original trial and it s main results, including those of the primary outcome. This is hugely important since the whole sample of participants and clinicians are drawing their experience from the trial. It is difficult for the reader to interpret the study including comments on its strengths and limitations. A key issue is when the study was conceived. The data in this

	manuscript has taken 8 years to present for publication., However it seems qualitative data were collected closer to the trial and may have been planned before the trial started. Given the absence of key information on the quantitative analysis such as participant's views on antidepressant discontinuation, I find it difficult to tell if this was a pre-planned assessment with qualitative interviews designed to explore full, partial or no discontinuation or use of qualitative interviews that were never really conceived for this purpose. It is unclear when the qualitative interviews were conducted in relation to the other assessments in the study. If they were carried out during the trial then they may have influenced trial outcomes but if too late after the trial was completed then there is a danger that people do not remember what happened during the trial and are talking more form later experience out side the trial. The underlying methodology and philosophy for the qualitative interviews and analysis are not stated in the method. From the analysis this might be a grounded theory or thematic and pragmatic approach but it is not entirely clear since experts with possible firmly held views on the topic were involved in the process. It is unclear whether the focus of the interviews was on barriers and facilitators of antidepressant discontinuation or other aspects of trial outcome since we do not know what the aims of the trial were. In terms of the discussion, there appear to be quite contrasting results between the UK and Dutch trial results. The UK studies were conducted in primary care and Dutch trials in secondary care. In secondary care, patients are likely to be treatment resistant, have a longer and more complicated psychiatric and sometimes medical history, carry more risks of suicide and neglect. in UK primary care, they may not receive much explanation of the problems and been given a framework of understanding about their problems. They are much less likely to have an assessment that has considered their development as a person and factors associated with the evolution and maintenance of their depression. In secondary care they are likely to receive all of this. Therefore in primary care, patients might for the first time be given a clear framework about their depression and that would not involve descriptions of neurotransmitters so there is no competing frame work. For all these reasons, there might be considerable difference between UK primary care and Dutch secondary care results, and some of the data in this paper if contrasted with data from the UK suggest that at least some of these factors are likely to be operating. I think discussion of these issues would greatly enhance the understanding of the reader and help them understand what seem like contradictory results for UK and Dutch RCTs.

VERSION 2 – AUTHOR RESPONSE

Reviewer: 1 Dr Ceri Wilson
Institution and Country: Anglia Ruskin University, UK Please state any competing interests or state ‘None declared’: None declared
My previous comments have been addressed satisfactorily and I think this is an important contribution to the field.

Thank you!

Reviewer: 2 Richard Morriss
Institution and Country: University of Nottingham, United Kingdom Please state any competing interests or state ‘None declared’: None declared

I think the paper has clinically important and relevant new data to present that is of general interest to patients, primary care and secondary care mental health professionals. I am fully supportive of its publication ultimately. I also think there are important gaps in the information that is presented, and the trial's methods, results and discussion are not adequately described in relation to the trial from which this further analysis is defined. Also having set up a potentially interesting comparison with UK studies in the introduction the authors do not return to the topic in the discussion. There are important learning points that will help readers make sense of this literature if there was such a discussion.

Thank you for this constructive feedback, we agree with your comments and will address them as best as we can.

The main problem is that there is not an adequate description of the original trial in PICO format including the aim of the original trial and its main results, including those of the primary outcome. This is hugely important since the whole sample of participants and clinicians are drawing their experience from the trial. It is difficult for the reader to interpret the study including comments on its strengths and limitations.

We agree that it will be helpful for the reader to have more background on the original trial, and to offer a short description of the trial rather than merely referring to the published paper. We have added such a summary in PICO format in the introduction:

“The current study describes secondary analyses of an RCT, in which 249 patients with recurrent depression in remission were randomly allocated to MBCT with continued use of ADM (n=121), or to MBCT followed by discontinuation (n=128). Results showed that discontinuing ADM after MBCT was associated with significantly higher relapse rates than continuing ADM after MBCT (intent-to-treat: 54% versus 39%, per-protocol: 69% versus 46%, respectively).6 In the original MBCT+discontinuation treatment arm, only 53% of the participants were able to fully discontinue within six months from baseline, 13% discontinued partially, 25% of the participants decided to continue their medication as it was despite the randomization, and for 9% it was unknown. Notably, discontinuation also occurred in patients asked to continue their medication: 12% discontinued fully and 14% partially. “ (p.5+6).

A key issue is when the study was conceived. The data in this manuscript has taken 8 years to present for publication., However it seems qualitative data were collected closer to the trial and may have been planned before the trial started. Given the absence of key information on the quantitative analysis such as participant's views on antidepressant discontinuation, I find it difficult to tell if this was a pre-planned assessment with qualitative interviews designed to explore full, partial or no discontinuation or use of qualitative interviews that were never really conceived for this purpose.

We agree that the timing and context of the qualitative part of the study deserves more explanation. In the Methods section under “Data”, we added the following:

“Quantitative data were collected between September 2009 and June 2013.” (p.6)

“These interviews were conducted after the trial (May and August 2013) as a follow-up study specifically focusing on the barriers and facilitators of ADM discontinuation.”(p.6).

It is unclear when the qualitative interviews were conducted in relation to the other assessments in the study. If they were carried out during the trial then they may have influenced trial outcomes but if too late after the trial was completed then there is a danger that people do not remember what happened during the trial and are talking more form later experience outside the trial.

There was indeed quite some time between ADM discontinuation and the qualitative interviews, which happened after the last follow-up. For further clarification, in the Results section > Qualitative results we added:

“The time elapsed between the final trial assessment and the qualitative interview was 0 – 23 months (Mn=11 months ± 6).” (p. 13).

The memory bias that may be associated with that delay is added to the discussion > strengths and weaknesses:

“In addition, given the delay between ADM discontinuation and the qualitative interview, memory bias may have occurred with regard to its barriers and facilitators . In addition, although we specifically asked about patients’ experiences within the trial, their responses appeared to also include previous and later experiences.” (p.20/21).

The underlying methodology and philosophy for the qualitative interviews and analysis are not stated in the method. From the analysis this might be a grounded theory or thematic and pragmatic approach but it is not entirely clear since experts with possible firmly held views on the topic were involved in the process. It is unclear whether the focus of the interviews was on barriers and facilitators of antidepressant discontinuation or other aspects of trial outcome since we do not know what the aims of the trial were.

To clarify the focus of the interviews, we updated the description of the interview questions in the Methods > Procedure section. We initially described only the 3 most essential questions for the sake of simplicity/brevity. However, to enable a more comprehensive understanding of the interviews and the data derived from them, we added the other questions as well:

“Patients and attending physicians were asked the following questions: 1) “How did the tapering go?”; 2) What expectations did you have about tapering ADM?”; 3) “What hampered your (the patients’) discontinuation process?”; 4) “What facilitated your (the patients’) discontinuation process?”; 5) “What was the role of mindfulness in your (the patients’) process of discontinuation?” and 6) “Do you have any suggestions for future guidance on tapering ADM?”” (p.6+7)

Although our initial intention was to use a grounded theory approach, the actual approach and data derived from the qualitative interviews may be more reflective of a thematic approach. Therefore, the section Statistical and qualitative analysis > Qualitative analysis was revised, describing the methodology as a thematic analysis:

“We used a thematic approach to analyze the data, with a focus on barriers and facilitators of the discontinuation process, including previous expectations and the possible role of mindfulness in that process. The underlying framework for the study can be described as a combination of a phenomenological and praxis-oriented approach, as we were both interested in participants’ views and in behavioural strategies that helped or hindered discontinuation.”(p.9+10)

In terms of background of the research team, it was added that one of the authors also worked as a mindfulness teacher:

“Subsequently codes […] and a psychologist and post-doc researcher who also worked as a mindfulness teacher (MH).” (p.10)

The description of the authors who conducted the interviews was described in slightly more detail:

“CW was a graduate student in Psychology during the interviews and data analysis, and a psychologist and PhD student researching ADM discontinuation in primary care at the time of writing.“ (p.8)

In terms of the discussion, there appear to be quite contrasting results between the UK and Dutch trial results. The UK studies were conducted in primary care and Dutch trials in secondary care. In secondary care, patients are likely to be treatment resistant, have a longer and more complicated psychiatric and sometimes medical history, carry more risks of suicide and neglect. in UK primary care, they may not receive much explanation of the problems and been given a framework of understanding about their problems. They are much less likely to have an assessment that has considered their development as a person and factors associated with the evolution and maintenance of their depression. In secondary care they are likely to receive all of this. Therefore in primary care, patients might for the first time be given a clear framework about their depression and that would not involve descriptions of neurotransmitters so there is no competing frame work. For all these reasons, there might be considerable difference between UK primary care and Dutch secondary care results, and some of the data in this paper if contrasted with data from the UK suggest that at least some of these factors are likely to be operating. I think discussion of these issues would greatly enhance the understanding of the reader and help them understand what seem like contradictory results for UK and Dutch RCTs.

Thank you for sharing these thoughts, which are very much appreciated. We agree that a discussion of these themes will improve the manuscript, so we have added a substantial paragraph to the discussion section under ‘Principal findings’ and an additional comparison with Kuyken 2008 in terms of predictors of discontinuation:

“However, two UK studies with a MBCT+discontinuation arm12 13 found much better discontinuation and relapse rates. Some possible explanations for these differences deserve attention. First, the UK studies were conducted in a primary care setting and ours in secondary care. Discontinuation of ADM in secondary care may be more challenging due to longer and more complicated psychiatric, and possibly medical, histories. In addition, the role of health care professionals might be different: GPs who guided discontinuation in the UK may have offered their patients a more psychologically-oriented framework for understanding depression whereas psychiatrists in the Netherlands may have offered a more biologically oriented one. In fact, some of them appeared to have actively advised against discontinuation. In our trial patients participated in mixed MBCT groups also including patients continuing their ADM, so guided tapering took place outside the MBCT context. In the UK-based studies, MBCT groups were homogeneous, allowing the tapering process to be a more integrated part of the training.
It is important to keep in mind that the analyses of the current study are not based on the original intervention and control conditions, but on the actual ADM continuation or discontinuation in the entire study population. In terms of predictors, full discontinuation occurred more frequently in women, and in those who were employed. Indeed, the qualitative data point to psychosocial stressors as possible barriers to discontinuation, and problems with finding or holding on to a suitable job might be one of them. Residual symptoms of depression may be a predictor of discontinuation too: levels of baseline depression were lower in the full versus no discontinuation group.” (p.18)

Aside from baseline depression levels, post hoc analyses by Kuyken et al 12 suggested that patients with a more severe psychiatric history (earlier age of onset and greater severity of the last episode)

were more likely to taper their ADM after MBCT. In the current study, age at onset was no predictor of successful discontinuation, neither was the number of previous episodes. The relation between clinical characteristics and discontinuation appears to be a complex interplay between several other factors. 25 (p.19).