Research Design

Purpose Statement of Research Design

A mixed methods research design will be used in this research in order to address the central research aim namely, whether there is any impact on stress and work self-efficacy as a consequence of university lecturers receiving EI coaching. An embedded mixed method design will be used, meaning that one data set provides a supportive secondary role in the study that is based primarily on the other data set. The primary purpose of this study will take an experimental quantitative approach to test whether EI coaching can positively influence work self-efficacy and reduce stress for lecturers at Technological University Dublin City Campus. A secondary purpose is to gather qualitative data that will explore the participants' experiences of the intervention. Secondary data will provide a more thorough analysis of the impact of the EI intervention.

Embedded Mixed Methods

This study will use a mixed methods approach. Campbell and Fisk (1959) were one of the first to use multiple methods to collect data and are inadvertently responsible for kick-starting the development of mixed methods research (Creswell & Creswell, 2018). One of the main benefits of a mixed methods approach is that it allows for a thorough analysis of the research questions in contexts in which there are a varying array of questions (Creswell & Creswell, 2018). Specifically, the primary rationale for the use of a mixed methods approach in this study is that it is the most suitable to deal with the differing variety of research questions that this study poses. The use of only quantitative or qualitative data would be insufficient to fully explore and understand the problem posed by the differing research questions, particularly within the paradigm of critical realism in which this research is set. In a broader sense the use of mixed methods will allow for a more holistic evaluation of the EI intervention on lecturers.

Specifically, an embedded mixed methods research strategy of inquiry will be applied. It gets its name because it mixes the different data sets at the design level as part of which one type of data is embedded within a methodology framed by the other data type (Creswell & Plano Clark, 2018). The embedded design is different in its intent from other forms of mixed methods design as it does not aim to converge two different data sets collected to answer the same question as is the case, for example, in a triangulation mixed methods design (Creswell & Plano Clark, 2018). Instead an embedded design allows researchers the opportunity to keep the two sets of results separately to answer different research questions, while allowing for an integration when interpreting the results overall.

There are different types of embedded approaches according to Creswell and Plano Clark (2018). This research will use an embedded experimental model, which means that qualitative data is embedded within a traditional quantitative experimental model. This places the emphasis on the quantitative methodology, with the qualitative aspect of the model playing a supporting role. This is suitable for this study as the overall research aim and questions in this study lend themselves more towards a quantitative approach, however, there are aspects of them, specifically research question three, that require a qualitative approach in order to be answered. For an overview of this study's embedded experimental research design see Figure 2. In general, using an embedded experimental design takes a predominantly post positivist standpoint but includes a constructivist aspect for the qualitative component (Creswell & Plano Clark, 2018). The selection and use of an embedded experimental model fits well with the paradigm in which this research is set, namely critical realism. This is because the critical realist paradigm does not believe in an all-out dichotomy between quantitative and qualitative methodology and rather places value on the reason for selecting such a methodology. The inclusion of a qualitative aspect in this study within the experimental design will allow for a deeper description and exploration, particularly in relation to the experiences of the participants during the intervention, mechanisms which arguably would go undiscovered had this aspect of the research not been conducted. Thus, this aspect of the study can be considered exploratory as opposed to determining cause. Creswell and Plano Clark (2018) point out one of the most common situations in which researchers use an embedded experimental model is when they aim to examine the process of an intervention or the relationship between variables. This highlights the appropriateness of the use of an embedded experimental model both within this study's paradigm and to achieve its aim and answer the research questions.

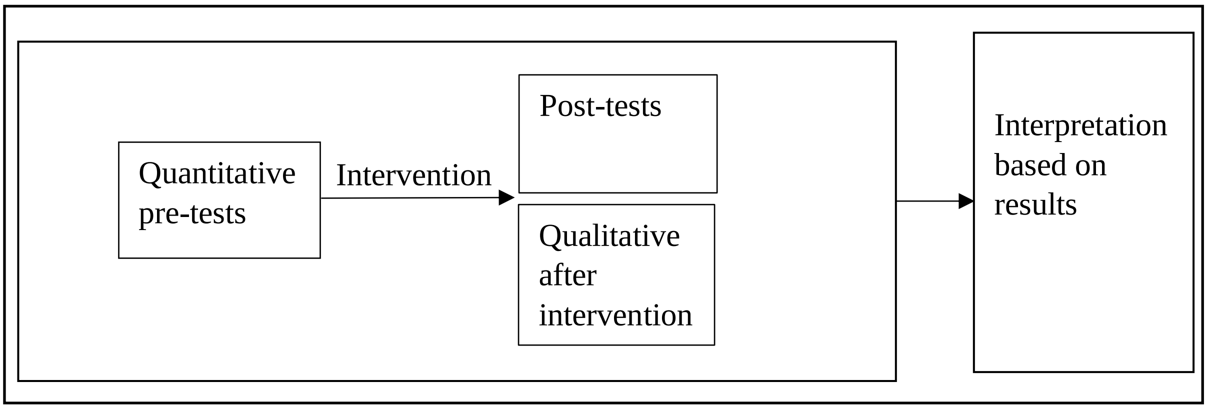

Figure 2.1 Overview of research design

Research Strategies

Porter et al. (2017) proposes three important methodological strategies that can be applied when investigating an intervention through the critical realist paradigm. These strategies are closely aligned with the present study design and strategies. The first strategy is a randomised controlled trial (RCT), which can test for efficacy of the intervention in a controlled environment (Porter et al., 2017). They support the use of a RCT alongside critical realism, suggesting that applying critical realism to RCTs could assist in accounting for the influence of social context and individual interpretation when it comes to understanding results. The second necessary strategy revolves around the development and testing of realist hypotheses that centre on the context of the intervention, allowing for theories to be built surrounding the processes of causation if relevant, as well as explaining what works, for whom, and in what circumstance. The third and final strategy is based on qualitative investigation that attempts to understand the human experience of the intervention, specifically to establish its beneficial and/or detrimental effects upon participants' lives (Porter et al., 2017). These three strategies are fundamental to the present research's design. The design and these strategies go some way to ensuring that results can be valid in the context in which they are conducted.

Quantitative

This study is primarily quantitative in nature and adopts the experimental method with its purpose being to attempt to "systematically manipulate theoretically relevant variables and to examine the effect of these manipulations on outcome variables" (Haslam & McGarty, 2003/2010, p.43). This aspect of the study aims to test several hypotheses discussed in detail as part of the data analysis plan that follows, but the overarching aim investigates the impact of the provision of EI coaching on stress and work self-efficacy for university level lecturers.

Before/After Design

The design can be considered a before/after design or a pre-post design. This contrasts with Hodzic et al. (2017) encouragement for future EI intervention research to include follow up measurements coinciding with the general acceptance that a before/after/follow-up is often a preferable design to that of before/after. While acknowledging this, in the case of the present study this was not possible. This is primarily due to restraints such as resources and time factors meaning it was not possible to include a longer-term follow-up measurement.

RCT

The design of the current study is a CONSORT compliant RCT, specifically the extension for social and psychological interventions (Montgomery et al., 2018). The study compares two parallel conditions: (1) an active EI coaching intervention group, and (2) a deferred intervention wait-list control group. The trial has been designed to test the superiority of receiving emotional intelligence coaching to decreasing stress and increasing work self-efficacy compared to a wait-list control group. This is in line with Hodzic et al. (2017) recommendation that future studies using an EI intervention be set up as an RCT. The use of an RCT can assist in randomising out some confounding variables. Equally as important is to make explicit the variables which cannot be fully controlled and therefore could potentially lead to bias. Both are explored in more detail in the sections below. As the name highlights the design has two critical aspects namely randomisation and control.

Randomisation. Randomisation is necessary in this study in order to control both the known and unknown factors that could affect the outcome variables between the intervention group and control group (Kim & Shin, 2014). Due to randomisation any differences in confounding variables between the groups should be through chance (Kendall, 2003). Random allocation is defined as splitting participants between the intervention and control group on a strictly unsystematic basis (Haslan & McGarty, 2003/2010, p.47). Random allocation means that there should be no selection bias in the study. Due to the comparatively small sample size of the study, as discussed in the power analysis section mentioned in the data analysis plan, simple randomisation will not be sufficient for this study as it is most effective in studies with a bigger sample size (Suresh, 2011). Instead, blocked randomisation with stratification will be used as it is a particularly useful technique for smaller trials (Kim & Shin, 2014). Blocked randomisation ensures an equal number of participants in the control and intervention group as in this study the allocation will be one to one between the two groups (Kim & Shin, 2014). Stratification will be used to also ensure a roughly equal split of the genders in both groups. This allows gender to be controlled for during the statistical analysis. Internet software will be used to generate the randomisation plan using blocks (https://www.studyrandomizer.com/).

Participants will be randomly assigned to their prospective group when they have successfully enrolled in the study and completed the pre-tests. Group allocation will be revealed to the experimenters and the participant upon the participants' completion of the pre-tests. This means that allocation concealment can be achieved as neither the experimenters nor the participants can foresee group allocation before or during enrolment. This helps to alleviate selection bias and means that both group's performance in pre-tests will not be impacted by potential expectations caused by group allocation.

Although allocation concealment can be achieved in this study, due to the nature of the study, once participants are assigned to either the experimental or WLC condition, it will not be possible for blinding to occur. This is primarily because both the experimenter and participant will be aware whether said participant is in the intervention or control group. This opens up a possibility of bias in this study that it will not be possible to control for as it would require external coaches be used that are not directly involved in the research as well as using a matched active control intervention. This was beyond the scope of the current study as it would require more resources as well as a bigger sample size. However some masking will be achieved as the trial hypotheses will not explicitly be stated to participants. Participant expectations will also be assessed. This is in line with Grant et al. (2018) suggestions when blinding is not possible in social and psychological intervention trials. Participant expectations is something that will be discussed in more detail below as part of the control section.

Control. This study will use a wait-list control (WLC) group to measure the manipulation of the independent variable on the outcome variables with the WLC group acting as a benchmark of comparison for the intervention group. The use of a WLC is in line with Kotsou's et al. (2018) recommendation in their systematic review on EI interventions that an active control or WLC be used as a basis of comparison instead of an inactive control, in which the control group do not receive any intervention. Utilising a WLC in this study means that no participant who gives their consent to participate in the study will be denied the opportunity to access the intervention, thus providing a clear ethical advantage over a no-contact control. Another advantage of the WLC is that the opportunity to avail of the intervention may act as a key incentive for participants in agreeing to participate in the study from the outset. This is done by removing participants' potential reluctance to participate if there was a chance that they would not receive anything in return for their time if they were designated to the control group.

It would be disingenuous not to highlight that Kotsou et al. (2018) recommend an active control such as a relaxation group or an active drama improvisation group in EI intervention studies. However, a pertinent factor in the selection of university level lecturers as the study population was that this is an occupation typically with a high emotional labour and high work-related stress. As a group with high time commitments it was decided that it would be less attractive to take up their time to participate only in an active control. The inclusion of an active control would require more of a time commitment from participants and thus, run the risk of a higher dropout rate than a WLC group. This risk was not deemed sufficient in the case of this study. The resources were also not available to facilitate an active control group. Therefore, a WLC was a more applicable choice in the case of this study than an active control. As an active control will not be used it means that certain factors such as social interaction will be difficult to control in this study.

Kotsou et al. (2018) recommend a WLC because it allows motivation biases to be controlled for to some degree as “participants in the intervention group may have a greater motivation to change than participants of a no-waiting list control group, which could impact the outcome” (p.9). Randomisation also assists in this regard. However, Boot et al. (2013) suggests even the use of an active control does not rule out expectation or placebo effects and by extension neither does a WLC. Instead it is necessary to control for expectations as not to do so “is a fundamental design flaw that potentially undermines any causal inference” (Boot el al., 2013, p.445). The authors concede that research may not always be able to eliminate differences in expectations between the intervention and control, but it should at least try to recognise their impact by “explicitly assessing expectations” (Boot et al., 2013, p.449). Therefore, as part of the demographic form that participants will fill out before the intervention, a question will be asked in relation to their expectations and motivations regarding their participation in the study. While not being a formal control for baseline motivation and expectation, it will go some way in assisting the researcher in understanding the expectations participants bring with them regarding the study.

There are several possible disadvantages related to a WLC that need to be acknowledged. One ethical disadvantage of using a WLC occurs in situations whereby withholding an intervention from one group is associated with increased risk. This was not the case in this study as it was a positive intervention with a non-clinical group. Ultimately, it has been deemed ethically acceptable to include a WLC as withholding the intervention posed no known risks to participants while also giving them the opportunity to receive the intervention once post-tests are complete.

A more relevant potential disadvantage of a WLC has been expressed by Cunningham et al. (2013) who’s exploratory study suggests that the use of a WLC may artificially inflate estimates of the intervention effect. The authors speculate that the extent to which inflation occurs may vary depending on factors related to the study population such as their readiness to change as well as factors linked to the nature of the intervention such as whether the intervention is clinical or non-clinical in its nature. Therefore, the authors suggest that this threat to valid inference must be considered before using a WLC. It is worth noting that the population of the present study are not in need of a treatment and the intervention is a non-clinical positive intervention. Similarly, Furukawa et al., (2014) suggest WLCs may cause a nocebo effect in psychotherapy trials, meaning being allocated to a WLC creates a negative expectation of "waiting for the desired active treatment" (p.189). Conversely, the opposite could also occur whereby being part of the WLC could deflate the estimates of effects as participants are left 'waiting for something' which alone could act as a beneficial intervention of sorts. The major takeaway from the two studies mentioned above in the context of the present research is the recognition that a WLC cannot be viewed as the same as that of an untreated group. Participants in the WLC group will become aware of the existence and content of the study through information they receive at both the recruitment stage and to allow for informed voluntary consent. The WLC group will also undergo the pre-testing which may impact them in some way, such as in this case, encouraging personal exploration of emotional intelligence in the interim of the post-tests, such as doing their own independent reading around the topic. To compound this, those in the WLC group will also be aware that they can have access to the intervention if they so wish, once post-tests are complete and this may impact how they act in the period between pre-tests and post-tests.

Ultimately, there are both advantages and disadvantages to the use of a WLC in this study and it is necessary to recognise these when considering all findings in this study. Plausibly, the issues surrounding a WLC paired with that of a small sample size may call into question the possibility for causal inference to be gleaned from this research. The data analysis plan will give a more specific outline on the inferential statistics that are planned to be used and as such the inferences that can be made from this research.

Quantitative Data Analysis Plan

The quantitative data analysis plan was primarily informed by the research questions. Therefore, it is necessary to outline the quantitative research questions and hypotheses in order to highlight how the data analysis plan will attempt to answer these questions and test the associated hypotheses.

Quantitative Research Questions. 1. How does the provision of EI coaching to university level lecturing staff impact their levels of stress?

a. What impact does EI coaching have on the perceived stress levels of lecturers?

Hypothesis 1a: If university level lecturers receive emotional intelligence coaching then their level of perceived stress will be reduced compared to those on a wait-list.

b. What impact does EI coaching have on the work-related stress levels of lecturers?

Hypothesis 1b: If university level lecturers receive emotional intelligence coaching then their level of work-related stress will be reduced compared to those on a wait-list.

2. What impact does EI coaching have on the work self-efficacy of lecturers?

Hypothesis 2. If university level lecturers receive emotional intelligence coaching, then their level of work self-efficacy will increase compared to those on a wait-list.

Main Analysis. The main analysis will consist of what will be referred to as doubly multivariate repeated measures design as it is referred to in Pituch & Stevens (2016, p.528) and is a form of a repeated measures MANOVA. This main analysis will be used to determine whether there are any differences between the independent variables on all the dependent variables.

Independent Variables. The repeated measures MANOVA will consist of one within groups variable as well as two between group variables all of which will be treated as categorical dichotomous variables. Time is the within groups variable and occurs at two times (pre and post intervention). Group is the first between groups categorical variable and occurs at two levels, intervention and WLC. Analysis will also include investigating the potential impact sex (male and female) has on the outcome of the intervention and therefore this will be included as the second between subjects independent variable. This means in total there are four groups: male intervention, female intervention, male control and female control.

Dependent Variables. In total there will be four dependent variables that will be included in the MANOVA, which are perceived stress, perceived work stress, perceived work self-efficacy and trait emotional intelligence. Perceived stress will be measured on the Perceived Stress Scale (Cohen et al., 1983) in order to assess hypothesis 1a, while perceived work stress will be measured through the Effort Reward Imbalance Questionnaire (Siegrist et al., 2004) in order to assess hypothesis 1b. Perceived work self-efficacy will be measured through a slightly adapted version of the Teachers Sense of Efficacy Scale (Tschannen-Moran & Hoy, 2001) to assess hypothesis 2. Finally, trait emotional intelligence as measured through the TEIQue (Petrides, 2001; Petrides & Furnham, 2003), will act as a manipulation check. A manipulation check is used in order to check that the manipulation of the independent variable was successful (Haslan & McGarty, 2003/2010, p.70). Therefore, in this study it ensures the coaching intervention was successful in improving the intervention groups emotional intelligence. The dependent variables will all be considered at the interval level as continuous variables. It is worth noting that there has been some debate as to whether Likert scales can be considered interval such as the case made by Jamieson (2004) that they should be recognised as ordinal. However, the position of this research is in line with the more prominent position in the literature for example that of Carifo & Perla (2008), that the individual items are collecting ordinal data but, the overall results garnered from the scale constitute interval data. Thus, where appropriate it is possible to use parametric techniques such as a MANOVA.

Assumptions. Evidently, the discussion above highlights that the data fulfils the assumption of a MANOVA in relation to the independent variables being categorical and the dependent variables being continuous. However, there are several other assumptions that need to be addressed before conducting the main analysis, some of which will need to be checked upon the completion of the data collection to ensure it meets the required assumptions of a repeated measures MANOVA.

One of the first assumptions of a MANOVA, just like other forms of ANOVA, is that of independence. This means independence of observations so that one participants result will not be dependent on that of another. This dependence can occur in situations when the intervention treatment consists of interaction among persons such as group counselling that results in observations influencing each other (Pituch & Stevens, 2016, p.221). In contrast independence can be maintained when the intervention is given individually (Pituch & Stevens, 2016, p.221). In the context of the present study it is evident that there is an element of dependence in that some of the intervention will be provided in a group setting, although some of it will also be on an individual basis. However, the dependence will likely be reduced in that there are no pre-defined groups to the coaching sessions and participants are free to interchange between the groups depending on their availability for that week. The amount at which participants do this will not be known until after the intervention. This means there will be no nesting of individuals in group and therefore rules out a common solution when independence is not present which is the use of multilevel modelling through nesting (Pituch & Stevens, 2016, p.578). Therefore, it was decided that despite some element of dependence in the study, the use of a MANOVA will still be the most appropriate available technique to analyse the data.

The other major assumption regarding the use of a repeated-measure MANOVA is that of multivariate normality, meaning that the residuals are assumed to have multivariate normality (Fields, 2017, p.754). Pituch and Stevens (2016) point out that to satisfy multivariate normality, "normality on each of the variables separately is a necessary, but not sufficient, condition" (p.225), but also recognise that "assessing univariate normality is often sufficient in practice to detect serious violations of the multivariate normality assumption" (p229). Therefore, in order to inspect the assumption of multivariate normality, once data is collected, multivariate normality of the residuals will be assessed. It is worth noting that a repeated-measures MANOVA is quite robust against departures from multivariate normality (Pituch & Stevens, 2016, p.480).

Post-hoc tests. Once the repeated measures MANOVA is conducted post-hoc tests will be conducted on the data. Field (2017, p.765) suggests discriminant analysis is superior as a follow-up analysis for MANOVA compared to a univariate ANOVA for each of the dependent variables. The latter results in inflation of Type 1 error and therefore suggests the use of a Bonferroni correction if this is applied. Pituch and Stevens (2016) explain that discriminant analysis is beneficial in cases that you are interested in learning "whether linear combination of dependent variables (instead of individual dependent variables) distinguish groups" (p.184). Discriminant analysis will therefore be the first option to use as follow-up. However, if it is the case in the current research that it will be more functional to assess whether the individual dependent variables distinguish groups then the traditional although somewhat flawed follow-on of using multiple univariate models with a Bonferroni correction will be used.

Power analysis/Sample size. Due to practical constraints it was not possible to dictate the sample size based on *a priori* power analysis. The main practical constraint revolved around limited resources in being able to deliver the intervention to enough participants as only one coach will be used. The expected sample size (N) in this study was set at roughly 60, as 30 was determined by the researcher to be the maximum number of participants that could be effectively put through the intervention with the time and resources available. However, if possible more than this will be recruited to account for attrition. In accordance with Grant et al. (2018) *a post hoc* power analysis calculation will not be completed. Instead of a priori or post hoc power analysis a *sensitivity* power analysis will be conducted to compute the effect size for the given N, alpha and beta/power.

Sensitivity Power Analysis. The given sample size (expected to be about 60) and an alpha set at .05 and power set at 0.80 will be used as the basis for a sensitivity power analysis to be conducted. Alpha and power were set at these numbers not only because it is the traditional cut-offs for these but also due to the nature of the study. A type I error (or a false positive) was more of a threat than a type II error (or a false negative). This is because in this case a false positive would mean that the results showed the intervention had sufficient evidence to assert an effect in reaching its aims when in reality there was not. This is a worse outcome than a false negative which would mean the results showed the intervention did not have sufficient evidence to assert an effect when in reality there was. This was judged to be the case due to more harm coming from a type I error as it would strengthen the evidence in favour of an intervention that doesn't work and may even be harmful and could lead to wasted resources. Whereas, in the case of a type II error, no harm would come from a false negative. Thus, due to the principle in research of minimising harm, a type I error would be more costly

Qualitative

The main purpose of including a qualitative aspect to this study is to act as a supporting role to the predominant quantitative experimental model and is therefore embedded within this framework. In this study the qualitative data will be primarily collected upon completion of the intervention in order to follow up on the experiences of participants during the intervention.[1] This is important as research question three aims to identify the experiences of participants pertaining to the EI intervention, data which can be collected most thoroughly through qualitative means. It is anticipated that the qualitative data in this study will be collected through a survey design using a questionnaire. However, if the responses are viewed as light in detail then purposefully sampled interviews may take place for some of the participants. This will be decided by the researcher based on the richness of the data the questionnaire provides.

Aligning with the overall mixed-methods approach used in this study, the questionnaire will consist of open- and close-ended questions. The open-ended questions provide a means of acquiring potentially rich qualitative data by allowing participants to use their own words to best describe their experiences of the intervention. Some close-ended questions will also be used in order to support or introduce the open-ended questions. Overall, the survey design will be used to gain insight and knowledge about the participants' experiences while acquiring responses to specific questions directed at their experiences of the EI intervention.

The primary rationale for using the survey design is to obtain information in relation to research question three that pertained to the experiences of the participants undergoing the intervention. A survey design was selected as the most applicable way of gathering this information as it offers the opportunity for all participants to express their experiences and

[1] Part of the demographic questionnaire participants fill out before the intervention will also have a small element of qualitative data collection.

provide data. This would not have been the case if interviews or focus groups were used as the primary qualitative data collection method as only a sub-sample would have been used to collect the qualitative data, due to the time-consuming nature of these methods of collection. Notwithstanding survey research in general is limited in that the findings may be limited to the group of people that are studied (Haslam & McGarty, 2003/2010). This mirrors the position of critical realism and interventions that as discussed previously advocates that the context of an intervention including the group of people included is important in stating whether an intervention works. Ultimately, a survey design means that the qualitative results in this study will be more representative of all participants than that which other methodologies would allow.

Another reason for the selection of a survey design is the dynamics at play in this study between researcher and participant. The researcher will also act as a coach while participants will be the coachees. This would have made it difficult in interviews or focus groups for the relationship to adequately return to that of researcher-participant and could exacerbate response bias. This bias could occur because of the possibility that a participant wished to be a good experimental subject by providing socially desirable responses or to give data which they think the researcher might want to receive. A survey design can mitigate some of this risk by not forcing participants to vocalise directly to the researcher their responses. By having the surveys instead of interviews there should be less to be gained for the participant to be a good experimental subject or to provide socially desirable responses and this should reduce response bias. Of course, response bias won’t be completely ruled out and must still be a consideration when analysing data collected from any survey design. Having said that if the questionnaires are not successful in obtaining rich data then the option may be taken to use purposefully sampled interview in which case these biases will be unavoidable but it will potentially allow for extra data to be collected if deemed necessary.

Qualitative Data Analysis Plan

Before outlining the qualitative data analysis plan it is first worth revisiting the qualitative research question and its associated sub-questions in order to understand what the analysis aims to achieve.

1. What are the experiences of university lecturing staff who receive EI coaching?

   a. How do lecturers believe their teaching practices were impacted by the EI coaching?

   b. What are the perceived benefits and issues for lecturers who receive EI coaching?

   c. How did the coaching fit into the context of the time in which it was conducted?

Thematic Analysis. The data from the open-ended questions will be analysed through thematic analysis, a qualitative analytic method most prominently outlined in Braun and Clarke (2006). Thematic analysis is a “flexible and useful research tool, which can potentially provide a rich and detailed, yet complex, account of data” (Braun & Clarke, 2006, p.78). Thematic analysis possesses a theoretical freedom making it a perfect fit for the theoretical framework in which this study is set and will allow for identifying, analysing and reporting patterns or themes within this study’s qualitative data set (Braun & Clarke, 2006). The thematic analysis in this research will be guided by the six phases to thematic analysis as outlined by Braun and Clarke (2006). Thematic analysis of the participant responses to the open-ended questions will gain meaningful understanding of the participants’ experiences of participating in the intervention.

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