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Online Process-Based Training for Perfectionism: A Randomized Trial

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Abstract

Process-based therapy (PBT) is model of psychotherapy designed to improve people's ability to use a variety of skills from evidence-based treatments to match environmental needs and personal goals in the moment. This randomized trial tested the effect of an online self-help intervention modeled after PBT principles for participants with perfectionism ($N = 77$). The intervention comprised two four-session trainings teaching skills from different evidence-based treatments (e.g., cognitive-behavioral therapy, acceptance and commitment therapy) and targeting cognitive and motivational processes: (1) cognitive training and (2) motivational training respectively. Participants completed 17 assessments throughout the intervention and at 3- and 6-month follow-up. Results indicated that the full intervention led to improvement in perfectionism, self-compassion, psychological distress, and cognitive skills targeted by the cognitive intervention (e.g., cognitive defusion; absolute β s = 0.02 to 0.66). In addition, the second four-session training (i.e., training after first four-session training) was associated with improvements in perfectionism, self-compassion, quality of life, and psychological distress (absolute β s = .09 to 2.90), suggesting it had incremental benefit. Whereas the cognitive training appeared to specifically impact cognitive processes, the motivational training increased both cognitive and motivational processes. These findings provide initial support for the feasibility and efficacy of a process-based approach, because they show that participants can benefit from learning skills from different orientations and applying them with reference to their goals. However, specific aspects of the PBT model, including whether interventions can precisely improve targeted skills, still need to be empirically tested in larger and more diverse clinical samples.

Keywords: perfectionism, process-based therapy, self-help, telehealth

Online Process-Based Training for Perfectionism: A Randomized Trial

Process-based therapy (PBT) is a modern, evolution science-informed approach to psychotherapy that organizes intervention based on theoretically and empirically supported core processes of change and treatment procedures from various traditions (Hofmann & Hayes, 2019; Ong et al., 2020). It explicitly moves away from disorder-specific manualized protocols and treatment packages (e.g., cognitive behavior therapy, acceptance and commitment therapy, dialectical behavior therapy), and instead toward personalized care centered on common therapeutic processes. In theory, shifting the focus of psychotherapy to processes over syndromes has the potential to streamline our conceptualization of psychological suffering, increasing the scope and precision of treatment strategies. That is, a smaller set of intervention tools may apply to a broader range of topographically diverse presentations while simultaneously targeting well-defined aspects of those maladaptive presentations. Although much has been written about PBT (e.g., Barnes-Holmes et al., 2020; Hayes & Hofmann, 2017; McCracken, 2021), the specifics of the theory have yet to be realized or empirically evaluated.

To live up to its potential, real-world tests of the application of PBT are needed to examine whether the model can meaningfully inform treatment and enhance wellbeing as hypothesized. A key aspect of PBT is identifying change processes linked to therapeutic procedures and personally meaningful outcomes (Hofmann & Hayes, 2019). From a functional analytic lens, a change process that cannot be directly manipulated or does not produce meaningful improvement is an unhelpful research or treatment target. Thus, we need to test specific processes of change to determine whether they can be altered with precision, and whether changes in these processes affect outcomes of interest.

PBT provides a conceptual foundation that is transdiagnostic without need for traditional diagnostic descriptors or criteria (Hofmann & Hayes, 2018). In fact, it may be more accurately understood as *transcending* or doing away with diagnoses altogether rather than spanning or relating to multiple diagnoses. Thus, the PBT model is better suited—even intentionally designed—to target processes and related problems independent of diagnostic labels (e.g., rumination, self-efficacy). Perfectionism is one such maladaptive process that can manifest as anxiety, depression, or disordered eating (Egan et al., 2011; Limburg et al., 2017). Whereas a protocol-for-syndromes approach might prescribe different treatments for the different clinical topographies, a process-based approach would instead focus on the underlying maintaining process: perfectionism. Thus, perfectionism is an excellent presentation on which to test PBT.

Perfectionism is defined as having exceedingly high personal standards accompanied with self-criticism when standards are not met, leading to significant distress and/or dysfunction (Shafran & Mansell, 2001). It is relevant to many diagnostic categories, including obsessive-compulsive disorder, obsessive-compulsive personality disorder, generalized anxiety disorder, and eating disorders (Egan et al., 2011). Moreover, it has been found to be a risk and maintenance factor for psychopathology and is associated with worse treatment outcomes (Egan et al., 2011), suggesting that it is a worthwhile treatment target. Self-compassion is commonly studied alongside perfectionism due to its relevance to the evaluative and self-critical aspects of perfectionism. Self-compassion has been found to moderate the impact of perfectionism on quality of life and impairment due to anxiety and depression in those with moderate to high levels of perfectionism (Ong et al., 2021). Furthermore, self-compassion appears to be related to treatment outcomes, such that increased self-compassion might facilitate reductions in

perfectionism and other mental health symptoms (Barnett & Sharp, 2016; Mehr & Adams, 2016; Ong et al., 2018).

Functionally, perfectionism is characterized by well-defined processes rather than specific behaviors (e.g., procrastination) or symptoms (e.g., anxiety) and can be conceptualized using the PBT extended evolutionary meta-model of change processes (EEMM; rhymes with “team”). The EEMM is a pragmatic framework that organizes an individual’s presenting concern along psychological dimensions (i.e., affective, cognitive, self, motivational, overt behavioral, attentional) within evolutionary systems (i.e., variation, selection, retention, and context; Hayes et al., 2020). Broadly defined, perfectionism could be described by all the EEMM change processes; however, for the purpose of the current study, we focused on two that appear to be particularly salient to perfectionism: cognitive variation and motivational variation.

Cognitive variation refers to the ability to interact flexibly with cognitive stimuli, like thoughts, rules, judgments, and memories, to allow for more adaptive behavioral responding (Hayes et al., 2020). In contrast, cognitive rigidity can lead to rule-governed behavior that ultimately is unhelpful or unworkable with respect to the person’s goals. In perfectionism, cognitive rigidity might manifest as laboring over a straightforward task like writing an email because of rigid adherence to a rule that dictates, “you must do things perfectly,” leaving the task incomplete with added self-criticism and distress. Cognitive variation training would therefore teach skills that expand the cognitive repertoire, including decentering, nonjudgmental observation, and reappraisal. Cognitive variation training gives people more options with respect to how they relate to thoughts in a given context to foster cognitive flexibility and goal attainment. With respect to the email example, being cognitively flexible may look like noticing

the rule that “you must do things perfectly,” recognizing that the email needs to be sent, and choosing to send the email anyway.

Motivational variation refers to the ability to contact reinforcement from various sources; that is, having multiple reasons for doing things (Hayes et al., 2020). Whereas, motivational rigidity may involve few sources of positive reinforcement, predominant negative reinforcement, or reinforcement regulated by external sources outside one’s control. For example, a basketball player who only feels positively about a game when they outscore their opponent and are explicitly praised by their coach likely lacks motivational variation in this context. Conversely, someone with motivational flexibility might play for a variety of reasons, including the intrinsic joy of competition, exercise, socialization, teamwork, etc. and could still find meaning and fulfillment from a game in which they did not score many points or receive external validation. Motivational variation training would therefore teach skills related to values exploration and identifying and prioritizing activities to increase contact with various sources of positive reinforcement.

Because PBT emphasizes targeting specific psychological processes—in this case, cognitive variation and motivational variation—rather than adhering to any particular therapeutic orientation, it draws from a range of evidence-based methods from different protocols. For example, cognitive variation training may include components of cognitive therapy such as thought records and reappraisal as well as components from dialectical behavior therapy and acceptance and commitment therapy such as acceptance and defusion. The PBT perspective makes room for eclectic and theoretically diverse techniques from any empirically based tradition, so long as they effectively target relevant processes and produce meaningful behavior change.

The current study is a proof of concept and preliminary test of the feasibility and efficacy of treating two specific features of perfectionism with two online self-help trainings based on PBT principles: a four-session cognitive training and a four-session motivational training (order in which trainings were delivered was randomized). Given the exploratory nature of this investigation, we recruited a convenience sample of participants who self-reported elevated levels of perfectionism. Initial key tests of the premises of PBT include determining whether improving these processes improves outcomes (procedural utility) and whether distinct procedures have distinct functions in targeting relevant processes (procedural precision).

In addition to estimating overall effect on outcomes of interest, we tested (a) whether the second training (i.e., four-session training completed after first four-session training) improved well-being and perfectionism to evaluate incremental effects above and beyond the first training. Consistent with a PBT approach, we examined (b) whether increases in cognitive and motivational processes would predict increases in well-being and decreases in perfectionism (procedural utility) and (c) whether a self-help process-based training would influence specific change processes (procedural precision; i.e., cognitive variation training increases cognitive variation and motivational variation training increases motivational variation) .

Method

Procedure

The study was approved by a university institutional review board and participants signed an informed consent document prior to participation. Data collection spanned from April 2020 to January 2021, during the COVID-19 pandemic. Participants were recruited using online postings to the researchers' lab Facebook page and Reddit. Postings were made to the following subreddits (i.e., separate topic discussion forums): perfectionism, selfhelp, anxietyhelp,

psychology, anxiety, sample size, depression, stress, self-improvement, OCD, overthink, time management, habits, and deciding to be better. To increase demographic diversity, we used “push out” advertising strategies that targeted online users who were not primarily looking to participate in research (Antoun et al., 2016).

Participants who met eligibility criteria ($N = 77$) were randomized into one of two groups using block randomization to ensure approximately equal numbers in each group. The first author generated the random allocation sequence with a random number table. Participants self-enrolled in the study; they were provided with a link to the informed consent document after completing the screener if they met eligibility criteria. Predetermined branching logic embedded in the informed consent process was used to implement the random allocation sequence (i.e., once participants completed the consent form, they were directed to a link that initiated one of the two study conditions).

Inclusion criteria for the study included: (a) reliable Internet and smartphone access; (b) being at least 18 years old; (c) scoring ≥ 29 (i.e., one standard deviation above the nonclinical mean) on the Concern Over Mistakes subscale of the original version of the Frost Multidimensional Perfectionism Scale (this cut-off was used in a previous telehealth treatment trial; Rozental et al., 2017); and (d) English fluency. Exclusion criteria included: (a) self-reported suicidality and (b) current engagement in psychotherapy. All screening was self-reported by participants during the self-enrollment process.

We employed a randomized crossover design wherein all participants completed the same two trainings but in different order, depending on their condition. Participants were encouraged to complete assessments throughout the study. They were compensated \$10 per assessment at pre-, mid-, and post-intervention, and 3-month and 6-month follow-up (up to \$50),

and \$5 for each assessment during the intervention phase (up to \$20). In total, 17 measurement points were included: (a) measurement of outcomes and target skills at baseline (1), during each week of the four-week training (4), mid-intervention/after the first training (1), post-intervention/after the second training (1), and at three- and six-month follow-up (2) and (b) measurement of cognitive and motivational variation following each of the eight intervention sessions (these eight were embedded in the trainings and uncompensated). Figure 1 presents a flowchart illustrating the study timeline and assessment schedule.

Intervention

Background

The format for the online training—including use of the Qualtrics platform, approach to delivering content, use of interactive exercises, and visual design, among other features—was grounded in an approach we have used in over 20 clinical trials to develop and evaluate online psychological interventions. In these studies, we have consistently found Qualtrics to be an engaging platform to deliver psychological interventions, to be acceptable to users, and to effectively target relevant processes of change and mental health outcomes (e.g., Arch et al., 2020; Fauth et al., 2021; Levin et al., 2018; Levin et al., 2017; Levin et al., 2020).

We followed a similar process in creating the intervention sessions for this study, as we have used in many prior projects over the past decade where we have successfully translated therapy protocols into self-guided online formats (e.g., Arch et al., 2020; Fauth et al., 2021; Levin et al., 2018; Levin et al., 2017; Levin et al., 2020; Levin et al., 2014). This process included a combination of (1) translating existing evidence-based therapy protocols and skills targeting specific processes of change to an online self-guided format using text, interactive exercises, tailoring, and graphics, (2) adapting prior online intervention content from our library

of previously tested programs (Levin et al., 2018; Levin et al., 2020), and (3) iteratively developing, reviewing, and refining sessions with a team of content experts. Thus, although the intervention sessions were created for this study, they were based on previously validated self-guided intervention modules and therapy protocols and followed a development approach we have found successful in creating effective, engaging programs in over 20 clinical trials.

Description

The current study intervention, *A Good Enough Training*, comprised two distinct but parallel online self-guided trainings: a cognitive variation training (CVT) and a motivational variation training (MVT). Trainings were hosted on Qualtrics, a secure data collection platform. Each training consisted of four ~15-minute sessions at a prescribed pace of two sessions per week, such that each training would be completed over two weeks and the full intervention would be completed in four weeks. Participants engaged in sessions independently, without external coaching or guidance.

The structure of the trainings was designed to be as similar as possible, with corresponding sessions following the same outline (refer to Table 1 for a description of session content). Every session was prefaced by a “Big Picture” reminder that aimed to anchor each training in two broad concepts: (1) function of stimuli relative to goals (e.g., “Is this thought moving me toward my goals?”) and (2) function of stimuli in context (e.g., “Is this thought helpful in this situation?”). The main content of each session included: (1) a Learning section where a skill was explained and generic examples were provided to illustrate the concept, (2) a Practice section where participants were guided to use the skill with personally relevant content, and (3) a Skill Building section that assigned homework between sessions. For example, in session 2 of the cognitive training, participants first learned to categorize and evaluate thoughts

in example scenarios, like a person who is about to bake a cake for a party and has the thought, “I’m a terrible baker.” After going through the example, participants were then prompted to identify a thought that had been bothering them in the past week and apply the skill they had just learned to the thought. The homework for session 2 was to complete a thought record and note the effects of practicing cognitive reappraisal. All sessions ended with a one-page summary of the concepts covered in the session.

With respect to the outline of the trainings, session 1 introduced the aims of the training and provided basic psychoeducation: thoughts in the cognitive training and sources of motivation in the motivational training. In addition, session 1 explained the overarching concepts of variation and selection, instructing participants to try out different strategies and to select strategies based on their goals and context. We specifically stated that no strategy is helpful in all situations and learning how to select strategies appropriately is key to goals achievement. The first session for each training also had an additional section on how to set SMART goals. Sessions 2 to 4 each targeted a skill, building on session 1. The cognitive training targeted cognitive reappraisal, cognitive defusion, and self-kindness in the context of self-critical thoughts, whereas the motivational training targeted weighing pros and cons (decisional balance), values, and stimulus control/contingency management. The last session had an additional section that provided resources related to the training (e.g., worksheets, links to previous sessions). All sessions except the first session opened with a Reflection and Review section that reviewed skills learned in the previous session.

Measures

Outcomes. We used the following measures to track outcomes and treatment response.

Frost Multidimensional Perfectionism Scale–Brief (FMPS-Brief; Burgess et al., 2016).

The FMPS-Brief is an eight-item self-report measure of perfectionism. It was used instead of the original FMPS due to its shorter length while still showing empirically supported construct validity (Burgess et al., 2016). It has two subscales: evaluative concerns (FMPS-E; e.g., “If I fail at work/school, I am a failure as a person”) and striving (FMPS-S; e.g., “I have extremely high goals”). Items are rated on a five-point Likert scale (1 = *strongly disagree* to 5 = *strongly agree*) and summed into a total score with higher scores indicating more perfectionism. Participant scores on the FMPS-Brief subscales demonstrated fair internal consistency in the present study ($\alpha = .78$ and $.70$ for evaluative concerns and striving respectively).

Self-Compassion Questionnaire–Short Form (SCS-SF; Raes et al., 2011). The SCS-SF is a 12-item self-report measure of self-compassion with six subscales: mindfulness, self-kindness, common humanity, over-identification, self-judgment, and isolation. Items on the SCS-SF are rated on a five-point Likert scale (1 = *almost never* to 5 = *almost always*) and summed into a total score with higher scores indicating more self-compassion. Participant scores on the SCS-SF demonstrated fair internal consistency in the present study ($\alpha = .79$).

Quality of Life Scale (QOLS; Burckhardt & Anderson, 2003). The revised 16-item self-report version of the QOLS assesses overall quality of life. Items are rated on a seven-point Likert scale (1 = *terrible* to 7 = *delighted*) and summed into a total score with higher scores indicating higher quality of life. Participant scores on the QOLS demonstrated excellent internal consistency in the present study ($\alpha = .92$).

Depression Anxiety and Stress Scale (DASS; Lovibond & Lovibond, 1995). The 21-item self-report version of the DASS was used as a measure of general distress. It has three subscales: depression (e.g., “I felt I wasn’t worth much as a person”), anxiety (e.g., “I felt I was close to

panic”), and stress (e.g., “I found it difficult to relax”). Items are rated on a four-point Likert scale (0 = *never* to 3 = *almost always*) and summed into a total score with higher scores indicating more distress. Participant scores on the DASS demonstrated excellent internal consistency in the present study ($\alpha = .94$).

Target skills. Questionnaires were used to track changes in skills associated with cognitive variation (i.e., cognitive fusion and reappraisal) and motivational variation (e.g., motivation and engagement).

Cognitive Fusion Questionnaire (CFQ; Gillanders et al., 2014). The CFQ is a seven-item self-report measure of cognitive fusion. The CFQ has shown moderate to large correlations with other measures related to cognitive variation skills such as ruminative response styles and mindfulness (Gillanders et al., 2014) and that it is distinct from simply measuring negative thoughts (Krafft & Levin, 2021). Items are rated on a seven-point Likert scale (1 = *never true* to 7 = *always true*) and summed into a total score with higher scores indicating more cognitive fusion. Participant scores on the CFQ demonstrated excellent internal consistency in the present study ($\alpha = .90$).

Thought Control Questionnaire—Reappraisal subscale (TCQ-R; Wells & Davies, 1994). The six-item reappraisal subscale of the TCQ was used to track frequency of reappraisal thought control strategies in the context of unpleasant or unwanted thoughts (e.g., “I analyze the thought rationally,” “I try to reinterpret the thought”). The reappraisal subscale of the TCQ has demonstrated discriminant validity as distinct from worry (Fehm & Hoyer, 2004). The original four-point Likert scale was changed to a seven-point scale (matching the CFQ scale) to facilitate combining variables for analysis (1 = *never* to 7 = *always*). Items are summed into a total score

with higher scores indicating more frequent use of reappraisal. Participant scores on the TCQ reappraisal subscale demonstrated good internal consistency in the present study ($\alpha = .88$).

Engaged Living Scale—Valued Living subscale (ELS-VL; Trompetter et al., 2013). The 10-item valued living subscale of the ELS was used to track changes in motivation and engagement. The valued living subscale of the ELS has demonstrated moderate correlations with measures of personal growth, autonomy, environmental mastery, and purpose in life (Trompetter et al., 2013). The ELS-VL measures values identification and committed action (e.g., “I know what motivates me,” “I believe that my values are really reflected in my behavior”). The original five-point Likert scale was changed to a seven-point scale (matching the CFQ scale) to facilitate combining variables for analysis (1 = *totally disagree* to 7 = *totally agree*). Items are summed into a total score with higher scores indicating higher motivation and engagement. Participant scores on the ELS-VL demonstrated excellent internal consistency in the present study ($\alpha = .92$).

Hypothesized processes of change. Four face-valid items were developed and used to track changes in cognitive variation and motivational variation every intervention session. Items were rated on a seven-point Likert scale (1 = *not at all* to 7 = *very much so*). Means were calculated for each scale with higher scores indicating greater variation.

The cognitive variation items included: (a) “How much did you challenge the accuracy of your thoughts?”; (b) “How much did you recognize that thoughts aren’t facts?”; (c) “How much did the strategy you used for thoughts fit the situation?”; and (d) “How much did thoughts get in the way of what you wanted to do?” (reverse coded). Internal consistency for these scores was fair ($\alpha = .78$).

The motivational variation items included: (a) “How much did you consider reasons for choosing your actions?”; (b) “How much did you follow through on your chosen goals?”; (c)

“How much did the strategy you used for choosing your actions fit the situation?”; and (d) “How much did you do something because you felt like you should, not because it was important to you?” (reverse coded). Internal consistency for these scores was fair ($\alpha = .71$).

Impact of COVID-19. One questionnaire was used to assess functional impairment related to the COVID-19 pandemic and two items were developed to assess how the pandemic affected study participation specifically. The questionnaire and additional items were administered at baseline, mid-intervention, post-intervention, and follow-up.

Work and Social Adjustment Scale (WSAS; Mundt et al., 2002). The WSAS is a five-item self-report measure of functional impairment related to a specific problem. The WSAS is designed for the scale administrator to relate each item to a specific problem. In this case, “the COVID-19 pandemic” was inserted for each item (e.g., “Because of the COVID-19 pandemic, my ability to work is impaired”). Items are rated on a nine-point Likert scale (0 = *no impairment at all* to 8 = *very severe impairment*) and summed into a total score with higher scores indicating more functional impairment due to the COVID-19 pandemic. Participant scores on the WSAS had good internal consistency in the present study ($\alpha = .84$).

Additional items. The first item, “The coronavirus/COVID-19 made it harder for me to fully participate in the study (due to distraction, distress, anxiety, loss of income, loss of childcare, etc.)” was rated on a seven-point Likert scale from “not at all” to “extremely.” The second open ended, free-response item was: “How did the coronavirus/COVID-19 make it easier or harder for you to participate in the study? For example, you had more time to engage in the sessions or the topics no longer applied to you due to a change in your work situation.”

Statistical Analyses

Analyses were run using R version 4.0.5 (R Core Team, 2021) with the following packages: simr (Green & MacLeod, 2016), lme4 (Bates et al., 2015), lmerTest (Kuznetsova et al., 2017), ppcor (Kim, 2015), tidyverse (Wickham, 2017), careless (Yentes & Wilhelm, 2018), psych (Revelle, 2018), naniar (Tierney et al., 2020), and furniture (Barrett & Brignone, 2017). The study was preregistered at <https://osf.io/ve74h>.

Power analysis. Monte Carlo simulation was used to determine an adequate sample size to detect a fixed time effect size (slope) of 0.2. To detect a slope of 0.2 following one intervention with at least 90% power (specifically, 93.50%, CI = 91.79 to 94.95), 50 participants total and four assessment points per intervention were needed.

Overall trajectory. Multilevel models were used to test overall intervention trajectories from baseline to post-intervention (for cognitive variation and motivational variation) or 6-month follow-up using an intent-to-treat sample. A series of nested models was specified for each dependent variable: evaluative concerns (FMPS-E), striving (FMPS-S), self-compassion (SCS-SF), quality of life (QOLS), distress (DASS-21), cognitive fusion (CFQ), cognitive reappraisal (TCQ-R), valued living (ELS-VL), cognitive variation, and motivational variation. Simpler models, starting with the null model, were compared to the next most complex model at each step using χ^2 tests on log-likelihood values at $p < .05$. If there was no significant difference between models, the simpler model was retained for parsimony.

Random intercepts were included in the null model to account for different scores among participants at baseline. In the second model, time measured in weeks was included as a linear fixed effect. In subsequent models, time was included as a quadratic and then polynomial fixed effect respectively to test if a non-linear trajectory better fit the data. The better-fitting time term and condition (CVT first followed by MVT or “CVT First”, MVT first followed by CVT or

“MVT First”) were included in the next model. The final model included a time \times condition interaction term. Final models were fit using restricted maximum likelihood (REML) and all models converged on the optimal REML criterion within 50 iterations. Missing data were handled using listwise deletion at the observation level. Only coefficient estimates from final models are reported; p -values were based on the Satterthwaite approximation to degrees of freedom.

For baseline to follow-up analyses wherein the null model fit best, we conducted post-hoc multilevel analyses that paralleled the original series of nested models (i.e., null, linear time, quadratic time, interaction) using data from baseline to post-intervention. These analyses were not pre-planned and were exploratory. Their purpose was to examine if there were effects at post-intervention that were not maintained at follow-up.

Incremental effect of second training. Multilevel models were used to test differences in outcomes over the course of the second training using data collected during and after the second training. Time was added as a fixed predictor. Intercepts were allowed to vary by participant. Coefficient estimates and associated p -values from the models were examined to determine if there was a significant effect of completing a second training (i.e., fixed time effect). The dependent variable at baseline was added as a covariate.

Variables relevant to both trainings were included as outcomes in these analyses. Skills specific to a training were omitted as we would not expect any consistent incremental changes in these skills given that the second training could be either cognitive or motivational depending on study condition. For cognitive variation and motivational variation, pretest values were taken *before* the end of first training and posttest values were taken *before* the end of the second training because these variables were only measured during but not after the trainings; other

variables were measured *after* each training (see Figure 1 for assessment schedule). Although these analyses were run to answer one of our preregistered hypotheses, the analytic approach was not specified in our preregistration.

Relationship between processes of change and outcomes. Partial correlations were used to test if (1) changes in hypothesized processes of change (i.e., cognitive variation, motivational variation) from baseline to post-intervention were correlated with outcomes at (2) post-intervention, (3) 3-month follow-up, and (4) 6-month follow-up, partialing out baseline level of the outcome. Thus, six partial correlations were tested per outcome of interest: change in cognitive variation from pre- to post-intervention with outcome at post-intervention, 3-month follow-up, and 6-month follow-up (three subtotal) and change in motivational variation from pre- to post-intervention with outcome at post-intervention, 3-month follow-up, and 6-month follow-up (three subtotal). The rationale was to examine whether increases in processes of change were associated with better outcomes, which would support their role as an “active” treatment ingredient.

Outcomes related to overall well-being (e.g., perfectionism, self-compassion) were included in these analyses; skills specific to a training (e.g., cognitive defusion) were omitted because we were interested in how *variation* was associated with overall outcomes rather than with specific skills. For example, a person could be responding to their environment flexibly and adaptively (variation) to enhance well-being without necessarily relying on a specific skill. Bonferroni correction was used to reduce the probability of Type I error. Although these analyses were run to answer one of our preregistered hypotheses, the analytic approach was not specified in our preregistration.

Procedural precision. To evaluate the procedural precision of each training, we used multilevel models with variation score as the dependent variable. The predictor entered was a three-way interaction of time \times type of variation (cognitive, motivational) \times condition (CVT First, MVT First). Lower-order effects (e.g., time \times condition interaction) were automatically added to the model. Intercepts were allowed to vary by participant. The first model only included data collected during the cognitive training regardless of when it was completed, and the second model only included data from the motivational training regardless of when it was completed. In other words, data for each model were collected from Weeks 1 through 4 of each training (see Figure 1) and included participants from both conditions.

If a training precisely targeted its hypothesized process of change, we would expect the trajectory of variation score to depend on type of variation (i.e., a time \times type interaction). That is, cognitive variation but not motivational variation would increase in the cognitive training model. Condition was specified as a predictor to account for order effects. A significant three-way interaction would indicate that the effect of time on variation scores was moderated by type of variation and whether the training was completed first or second. For example, it could be that cognitive variation, but not motivational variation, increased in the cognitive training but only if it was delivered first. Coefficient estimates and associated *p*-values were used to identify significant effects. Although these analyses were run to answer one of our preregistered hypotheses, the analytic approach was not specified in our preregistration.

Results

Participants

In total, 382 people were assessed for eligibility to participate. Of these, 224 did not meet inclusion criteria, leaving 158 people enrolled in the study (see Figure 1 for reasons for

ineligibility). Prior to analysis, 81 participants were removed, including five participants who entered the same response ≥ 30 times on average on at least one assessment, seven participants who provided nonsensical responses to open-ended items, and 22 participants who demonstrated suspicious study behavior (e.g., similarly formatted emails from multiple participants with similarly formatted email addresses asking for compensation). We had originally planned to remove participants who entered the same response ≥ 15 times on average but increased this to ≥ 30 times following data collection. When examining the data, there appeared to be scenarios wherein a repeated response rate of 15 would have been plausible. Thus, we chose a more conservative cutoff to ensure that legitimate participants were not removed. In addition, we did not foresee problems related to suspicious and nonsensical study behavior and did not preregister a plan for handling these types of responses. Therefore, decisions to remove these participants were made post-data collection and not preregistered. Due to the excessive number of clearly suspicious responses, these post-hoc decisions seemed necessary to ensure integrity of the data to be analyzed. The remaining 47 participants did not participate in the intervention and did not provide any data post-enrollment. These participants signed up for the study within a short period of time after we placed an online ad, and inspection of screener responses during this period suggested almost all these new participants were spam or fake participants. Thus, we chose to remove these participants from our dataset.

Most of our sample ($N = 77$) identified as female (58%), White/European American (61%), straight (79%), having some college education (88%), and atheist (31%; see Table 2 for detailed breakdown). Throughout the study, participants on average endorsed moderately severe functional impairment due to COVID-19 (means > 20 ; norms from Mundt et al., 2002). Current findings should be interpreted within this context.

Session Use

Session means and standard deviations for time spent on training sessions and progress made are presented in Table 3. Among those who started a session, progress rates were high and ranged from 99.9 to 120.9% (values >100% indicate returning to the session after completing it previously). In contrast, the means of session progress in our intent-to-treat sample ranged from 33.8 to 75.4%, indicating that, on average, completion rate was low.

Overall Trajectory

Coefficient estimates and corresponding p -values for best-fitting models are reported in Table 4 (information on model comparison is presented in supplementary materials). The linear time model fit best for the FMPS-E, FMPS-S, SCS-SF, DASS-21, CFQ, TCQ-R, and cognitive variation. The null model fit best for the QOLS and ELS-VL. The interaction model fit best for motivational variation. A significant difference between conditions over time is reflected by the interaction model fitting better than the previously tested simpler model (i.e., null, time only), which suggests a significant interaction effect. Thus, for outcomes for which a simpler model was found to provide better fit than the more complex interaction model, we interpreted findings as supporting no difference between conditions over time. Results from these models are described in the following paragraphs. Means and standard deviations of self-report measures over time are presented in Table 5.

Outcomes.

Perfectionism. Time had a significant main effect on the FMPS-E and FMPS-S from baseline to follow-up, indicating that scores for evaluative concerns and striving decreased over time but that there was no difference in performance between conditions (see Table 4).

Self-compassion. Time had a significant main effect on SCS-SF from baseline to follow-up, indicating that scores decreased over time but that there was no difference between conditions (see Table 4).

Quality of life. The null model fit the data best, suggesting that there was no overall change in QOLS scores from baseline to follow-up. Post-hoc multilevel analyses showed that there was a significant change in scores from baseline to post-intervention (see Table 4).

Distress. Time had a significant main effect on DASS-21 from baseline to follow-up, indicating that scores decreased over the course of the study with no difference between conditions (see Table 4).

Target skills.

Cognitive fusion. Time had a significant main effect on CFQ from baseline to follow-up, indicating that scores decreased over time but that there was no difference between conditions (see Table 4).

Cognitive reappraisal. Time had a significant main effect on TCQ-R from baseline to follow-up, indicating that scores decreased over time with no difference between conditions (see Table 4).

Valued living. The null model fit the data best, suggesting that there was no change in ELS-VL from baseline to follow-up. Post-hoc multilevel analyses showed that, similar to quality of life, there was a significant change in scores from baseline to post-intervention (see Table 4).

Hypothesized processes of change.

Cognitive variation. Time had a significant main effect on cognitive variation from baseline to post-intervention, indicating that scores decreased over time but that there was no difference between conditions (see Table 4).

Motivational variation. The time \times condition interaction effect was significant, indicating that the trajectory of motivational variation scores from baseline to post-intervention differed between conditions (see Table 4). As illustrated in Figure 2, Panel B, it appears that on average the MVT First group experienced steeper improvement in motivational variation just before the end of the MVT sessions (around the two-week mark), which was maintained in the subsequent cognitive training.

Incremental Effect of Second Training

Evaluative concerns, striving, self-compassion, quality of life, distress, and cognitive variation significantly improved between the end of the first training and the end of the second training, suggesting incremental benefit from the second training. Motivational variation did not change from the first to second training. Coefficient estimates and p -values from these multilevel analyses are presented in Table 6.

Relationship Between Processes of Change and Outcomes

There were no significant correlations between processes of change and outcomes using a Bonferroni-corrected α ($.05/30 = .002$). Absolute r values ranged from .007 to .493. Pearson partial correlations and p -values are reported in supplementary materials.

Procedural Precision

Cognitive training. The time \times type of variation ($B = -1.14$, $SE = 0.44$, $p = .010$) and time \times condition ($B = -0.89$, $SE = 0.45$, $p = .048$) interaction terms were significantly associated with variation scores, meaning that the trajectory of scores depended respectively on whether they were measuring cognitive or motivational variation and on the order in which trainings were administered. Figure 3, Panel A shows that cognitive variation increased more than motivational

variation over time during the cognitive training and more so when the cognitive training was completed first (i.e., before the motivational training).

Motivational training. Only time significantly predicted variation scores during the motivational training ($B = 1.05$, $SE = 0.36$, $p = .003$), meaning that cognitive and motivational variation both generally increased over time and there were no order effects. The time \times type \times condition interaction was not significant ($p = .073$). Figure 3, Panel B shows the trajectory of variation scores during the motivational training.

Discussion

The current study tested the effect of an online process-based self-help intervention for perfectionism. The intervention was developed to target two key features of perfectionism: cognitive and motivational variation. The broader objective was to provide a proof of concept for the PBT model, which does not discriminate among “brand name” treatment packages, but instead is primarily concerned with empirically based procedures and processes that meaningfully impact people’s lives (Hofmann & Hayes, 2019). As such, the current intervention drew from different therapeutic approaches and instructed participants to vary and select strategies given their goals and context.

Effect of Study Intervention

Consistent with study hypotheses, overall trajectories from baseline to 6-month follow-up reflected improvement in most outcomes, skills targeted in the cognitive training, and hypothesized processes of change. These included perfectionism (evaluative concerns and striving), self-compassion, distress, cognitive fusion, cognitive reappraisal, cognitive variation, and motivational variation. In addition, post-hoc analyses indicated significant change in valued living and quality of life from baseline to post-intervention. Contrary to our hypotheses, there

was no significant change in valued living and quality of life from baseline to follow-up, suggesting that gains made during the trainings were not maintained at follow-up. Secondly, there were almost no order effects on outcomes and target skills, meaning that participants generally performed similarly regardless of whether cognitive or motivational training was completed first. The only exception was that cognitive variation increased more so than motivational variation during the cognitive training when it was completed before the motivational training. We did not have specific hypotheses regarding order effects.

One of the tenets of PBT is to clarify which therapeutic procedures—in isolation or combination—are effective for a given person in their unique context. Identifying relevant change processes and related treatment procedures is critical for ensuring that care provided to clients is precise and useful while minimizing redundancy. Accordingly, a key question of the study is whether combining both cognitive and motivational training was more effective than only targeting one of these processes. Otherwise, it would have been inefficient to provide a longer training that included both procedures. To answer this question, we examined if the second training had an incremental effect on outcomes and variation. Perfectionism, self-compassion, quality of life, distress, and cognitive variation—but not motivational variation—improved during the second training, suggesting additive effects. As Figure 2 shows, it appears that gains in motivational variation were primarily gleaned from the first training (more so when the first training was the motivational training) and remained relatively constant during the second training. More fine-grained research may be helpful to determine if certain people hit a ceiling on gains for specific variables after the first training and to identify them, to facilitate appropriate treatment dosing.

These findings provide some support for the current process-based self-help intervention with respect to decreasing symptoms and distress, but the poor retention of gains in valued living and quality of life is concerning, given that these variables are critical to flourishing. That is, valued living and quality of life are associated with positive growth and life fulfillment, not just absence of illness (Keyes, 2005). Ironically, the process-based approach is designed to undermine the emphasis on syndromes and symptoms that has dominated much of psychotherapy (Hofmann & Hayes, 2019), so this and other similar process-based interventions need to consider methods that effectively enhance well-being.

At the same time, the current study was conducted during the COVID-19 global pandemic and participants on average reported at least moderately severe impairment due to COVID-19. This unique circumstance could have suppressed well-being scores, even though participants were seeing improvement in perfectionism and psychological skills. Furthermore, previous perfectionism trials reported less change in quality of life measures relative to symptom-focused measures at follow-up assessments (e.g., Egan et al., 2014; Ong et al., 2019), suggesting that well-being may be more difficult to change, especially over shorter periods of time (e.g., length of a research study), in this population.

Proof of Concept of the PBT Model

The PBT model requires processes of change to be *malleable* by available methods and *relevant* to personally meaningful outcomes. We were able to evaluate both these factors in this study. The increases in cognitive and motivational variation over time indicated that the intervention effectively targeted these processes and shifted them in the desired direction. However, we did not observe any significant relationships between gains in processes and outcomes at post-intervention and follow-up. That is, improvement in variation did not predict

better outcomes later, which means we did not find gains in processes to be relevant to subsequent functioning, contradicting our hypothesis. A potential explanation is that outcomes were measured too distally, so we were unable to detect more proximal associations between processes and outcomes. Denser longitudinal data and network analyses may be able to clarify how processes relate to outcomes on a more granular scale (e.g., Bringmann et al., 2015; Fisher & Bosley, 2020).

Another crucial ingredient of PBT is procedural precision: that treatment methods precisely affect target processes of change. This would mean, for example, that participants showed greater improvement in motivational variation than cognitive variation during the motivational training and vice versa. Establishing precise links between procedures and processes of change elucidates *how* to influence key processes hypothesized to improve well-being, facilitating matching of treatment kernels to target processes in the moment in context. Without this precision, deployed procedures may not have their intended effect on clients, such that a clinician could aim to move cognitive processes with a certain procedure but end up moving behavioral processes that are less central to the client's struggles. Consequently, the key premises and benefits of an idiographic approach to case conceptualization and treatment planning—such as targeting specific processes based on a person's unique presentation—become untenable.

Multilevel analyses supported precision of the cognitive training as cognitive variation increased more than motivational variation during the cognitive training. They also indicated an order effect, such that participants who completed the cognitive training first generally improved in cognitive variation more so than those who completed it after the motivational training. The lack of significant interaction effects in the motivational training model suggests that

motivational training did not specifically increase motivational variation. Rather, the motivational training appeared to increase both cognitive and motivational variation (see Figure 3). Hence, it appears that motivational processes may be more malleable and moved without being targeted by specific procedures, whereas cognitive processes may only respond to a targeted intervention. To the extent that even engaging in treatment (e.g., showing up for appointments, talking to another person)—regardless of the nature of the intervention—can be conceptualized as a form of behavioral activation, it is possible that just going through the self-help program was its own form of behavioral activation in our sample, leading to greater motivational variation.

Limitations

First, generalizability of our findings is unclear, given certain characteristics of our study. For example, the study was conducted during the COVID-19 pandemic. Based on qualitative feedback from participants, many found sessions easier to access due to being at home more often, while others reported being more distracted due to pandemic-related stressors. Other common themes were that usual stressors were gone (e.g., less social interaction, loss of employment), so it was difficult to apply skills to typical stressors. At the same time, psychological skills like those taught in the current intervention are intended to be flexibly applied, so our predictions about the effect of the trainings would have been the same regardless. Relatedly, our sample may represent a different population from people actively seeking treatment, and the latter could respond differently to the intervention. Thus, it would be more precise to assert that our findings most likely apply to those open to and/or interested in a self-directed intervention. Most of our sample also identified as White, straight, and cis-female, so it is possible that our results would not generalize to more diverse groups, contributing to an

endemic problem in psychology of poor representation in clinical trials (e.g., Mak et al., 2007; Polo et al., 2019). Second, the study did not include a waitlist condition to account for the effects of time, so it is ultimately unclear whether changes over time (or lack thereof) are indicative of benefits from both trainings, null effects, or even iatrogenic effects relative to how variables may have naturally changed over time without the trainings. Third, our assessment of variation variables could have been more robust. For example, cognitive and motivational variation were measured using newly developed measures, which had not been psychometrically validated. In addition, we did not measure cognitive and motivational variation at follow-up, so we do not know if gains in these critical processes of change were maintained in the longer term. Fourth, we used nomothetic analyses to interpret our data, which has limited utility in PBT due to its emphasis on idiographic assessment (Hofmann et al., 2020). While nomothetic analyses may be helpful for understanding aggregate effects, examining heterogeneity in trajectories using growth mixture modeling (e.g., to identify participants who benefited more from the trainings) or using network models to account for dynamic and bidirectional relationships between processes and outcomes would provide a fuller picture. Finally, we failed to calculate a priori power for analyses other than our primary outcomes, and it is possible that null findings, including interactive effects, are due to insufficient power. At the same time, several different tests were run in this study without controlling for Type I error. We chose not to do so due to the exploratory nature of the current study, which tests a novel application of a new model of psychotherapy (i.e., process-based therapy). However, we reported effect sizes and confidence intervals where relevant, so readers have a sense of the practical significance of our findings. Replication with larger samples is needed to verify our findings.

Conclusion

In this study, we examined the effects of a process-based self-help intervention for perfectionism and found some support of its efficacy for several outcomes, including perfectionism and self-compassion. However, it had limited impact on more global indices of well-being like quality of life at six-month follow-up. With respect to proof of concept of PBT, target processes of change were affected by the intervention but not consistently related to subsequent outcomes. Thus, there was support for manipulability but not relevance to meaningful outcomes. In addition, the cognitive training appeared to precisely target cognitive variation, whereas the motivational training improved both cognitive and motivational variation, suggesting that procedural precision is possible and that specifically aimed procedures may affect more processes than hypothesized. The current study represents a preliminary attempt to test the feasibility of the PBT model, providing initial data on the efficacy of a process-based intervention as well as its performance on several defining features of PBT.

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Table 1

Description of Primary Skill Targeted in Each Training Session

	Cognitive Variation Training	Motivational Variation Training
Session 1: Introduction and psychoeducation	Activating Event → Belief → Consequence <ul style="list-style-type: none"> Learn the relationships among external situations, thoughts, and behavioral consequences 	Clarifying Motivation <ul style="list-style-type: none"> Learn to identify functions of perfectionistic behaviors (e.g., to get social approval, to avoid being wrong)
Session 2: Skill 1	Categorizing and Evaluating Thoughts <ul style="list-style-type: none"> Identify different types of cognitive distortions (e.g., all-or-nothing thinking, fortune telling) Examine evidence behind thoughts Evaluate ability to handle worst-case scenario 	Pros and Cons <ul style="list-style-type: none"> Use decisional balance matrix to weigh pros and cons associated with choices
Session 3: Skill 2	Noticing Thoughts <ul style="list-style-type: none"> Externalize the source of thoughts (e.g., imagine a relative saying that to you) Add “I’m having the thought that…” before a thought Leaves on a stream exercise 	Values <ul style="list-style-type: none"> Definition of values Identify three values Use values in decision making
Session 4: Skill 3	Self-Kindness <ul style="list-style-type: none"> Perspective taking (e.g., how would you treat a friend in a similar situation?) Self-validation: recognizing that own emotional reactions are valid, appreciating behaviors in context 	Staying Motivated <ul style="list-style-type: none"> Stimulus control Public accountability Rewarding self for completing goals

Table 2

Demographic Description and Functional Impairment Due to COVID-19 (N = 77)

		Mean/Count (SD/%)
Age		27.9 (7.2)
Gender identity	Female	45 (58.4%)
	Male	30 (39%)
	Transgender	1 (1.3%)
	Not listed	1 (1.3%)
Ethnic identity	Asian/Asian American	13 (16.9%)
	Black/African American	4 (5.2%)
	Latinx	7 (9.1%)
	MENA	3 (3.9%)
	White/European American	47 (61%)
	Not listed	2 (2.6%)
	Prefer not to answer	1 (1.3%)
Sexual orientation	Asexual	1 (1.3%)
	Bisexual	6 (7.8%)
	Gay	3 (3.9%)
	Straight	61 (79.2%)
	Queer	1 (1.3%)
	Pansexual	1 (1.3%)
	Questioning	3 (3.9%)
Highest education level	High school diploma or equivalent	7 (9.1%)
	Vocational training	1 (1.3%)
	Some college	22 (28.6%)
	Associate degree	4 (5.2%)
	Bachelor's degree	12 (15.6%)
	Some post-undergraduate work	15 (19.5%)
	Master's degree	10 (13%)
	Specialist degree	1 (1.3%)
	Doctorate degree	4 (5.2%)
	Not listed	1 (1.3%)
Religion, spiritual practice, or existential worldview	Agnostic	14 (18.2%)
	Atheist	24 (31.2%)
	Buddhist	2 (2.6%)
	Christian	22 (28.6%)
	Hindu	1 (1.3%)

	Muslim	3 (3.9%)
	Other/Not listed	7 (9.1%)
Functional impairment due to COVID-19 (WSAS)	Baseline	22.4 (10.5)
	Mid-intervention	22.6 (8.5)
	Post-intervention	22.2 (9.5)
	3-month follow-up	22.3 (10.5)
	6-month follow-up	24.2 (9.3)

Table 3
Duration of Use (min) and Progress Made (%) Per Training Session

	Mean (SD)			
	Starters ¹		Intent-to-Treat	
	Duration	Progress	Duration	Progress
CVT				
Session 1	47.8 (226.1)	120.9 (53.1)	29.8 (179.3)	75.4 (72.2)
Session 2	20.0 (30.3)	101.4 (22.0)	8.3 (21.7)	42.1 (52.2)
Session 3	88.3 (379.5)	100.2 (0.5)	29.8 (221.7)	33.8 (47.7)
Session 4	69.4 (300.2)	100.0 (0.2)	23.4 (175.3)	33.8 (47.6)
MVT				
Session 1	14.6 (13.8)	115.1 (62.7)	5.5 (11.0)	43.4 (67.8)
Session 2	17.1 (39.2)	103.3 (17.4)	6.2 (24.8)	37.6 (51.1)
Session 3	21.2 (63.1)	100.0 (0.0)	7.4 (38.3)	35.1 (48.0)
Session 4	79.5 (364.4)	99.9 (0.3)	27.9 (216.5)	35.0 (48.0)

¹ Includes data from sessions that were started but not necessarily completed (i.e., progress = 100%).

Note. Duration counts time spent with browser window open and may not be an accurate depiction of time spent actively engaging with session material. Total progress may exceed 100% if participants return to a session after completing it previously.

Table 4

Coefficient Estimates from Multilevel Models for Overall Trajectories (Effect of Intervention on Outcomes)

	FMPS-E	FMPS-S	SCS	CFQ	TCQ: Reappraisal	ELS: VL (FU)	ELS: VL (Post)	QOLS (FU)	QOLS (Post)	DASS-21	Cog. Var.	Mot. Var.
Intercept	15.95* [15.32; 16.58] <i><.0001</i>	16.29* [15.64; 16.94] <i><.0001</i>	3.01*** [2.89; 3.13] <i><.0001</i>	35.96*** [34.51; 37.42] <i><.0001</i>	29.48*** [28.17; 30.79] <i><.0001</i>	48.81*** [46.48; 51.15] <i><.0001</i>	50.19*** [47.60; 52.77] <i><.0001</i>	76.12*** [72.00; 80.25] <i><.0001</i>	70.90*** [66.71; 75.08] <i><.0001</i>	27.66*** [24.39; 30.93] <i><.0001</i>	16.49*** [15.72; 17.27] <i><.0001</i>	17.83*** [16.78; 18.89] <i><.0001</i>
Time (Weeks)	-0.07* [- 0.10; -0.04] <i><.0001</i>	-0.08* [-0.11; -0.05] <i><.0001</i>	0.02*** [0.01; 0.03] <i><.0001</i>	-0.22*** [- 0.30; -0.14] <i><.0001</i>	-0.09** [- 0.15; -0.03] <i>.0039</i>		-0.57** [- 0.99; -0.15] <i>.0085</i>		2.51*** [1.88; 3.14] <i><.0001</i>	-0.16** [- 0.27; -0.05] <i>.0058</i>	0.66*** [0.46; 0.85] <i><.0001</i>	0.08 [-0.18; 0.34] <i>.5368</i>
Condition ¹												-0.09 [- 1.56; 1.37] <i>.9006</i>
Time × Condition ¹												0.41* [0.06; 0.77] <i>.0226</i>
AIC	2477.38	2415.72	826.41	3381.81	3173.06	3669.51	2959.93	3346.77	2722.33	3817.86	2191.99	2126.05
BIC	2494.38	2432.69	843.35	3398.77	3190.04	3682.25	2976.10	3358.96	2737.82	3834.80	2208.39	2150.64
Log Likelihood	-1234.69	-1203.86	-409.20	-1686.91	-1582.53	-1831.76	-1475.97	-1670.39	-1357.17	-1904.93	-1092.00	-1057.03
Num. obs.	518	514	511	513	516	516	421	430	355	511	445	445
Num. participants	76	76	75	75	75	75	75	63	63	75	68	68

****p* < 0.001. ***p* < 0.01. **p* < 0.05.

¹ MVT First is the reference level.

Note. *p*-values are provided in italics below each coefficient estimate and its 95% confidence interval.

FMPS-E = Frost Multidimensional Perfectionism Scale-Brief Evaluative Concerns subscale; FMPS-S = Frost Multidimensional Perfectionism Scale-Brief Striving subscale; SCS = Self-Compassion Scale—Short Form; CFQ = Cognitive Fusion Questionnaire; TCQ = Thought Control Questionnaire; ELS: VL = Engaged Living Scale: Valued Living subscale; QOLS = Quality of Life Scale; DASS-21 = Depression Anxiety Stress Scales; Cog. Var. = cognitive variation; Mot. Var. = motivational variation.

Table 5

Means and Standard Deviations of Outcome Measures Over Time

	Baseline	Post	3MFU	6MFU	Cutoff [†]
FMPS-Evaluative	16.8 (2.9)	14.3 (3.9)	14.6 (3.6)	14.6 (3.9)	<14* (Burgess et al., 2016)
FMPS-Striving	17.3 (2.5)	14.8 (4.0)	14.6 (3.9)	15.0 (3.7)	<17* (Burgess et al., 2016)
SCS	2.8 (0.7)	3.3 (0.6)	3.3 (0.5)	3.4 (0.7)	>2.5 (Neff, n.d.)
QOLS	70.1 (17.8)	81.6 (17.9)	76.1 (22.0)	75.7 (19.3)	>71 (Langeland et al., 2007)
DASS	31.5 (14.9)	21.3 (14.6)	24.0 (15.1)	27.9 (15.8)	<16 (Chin et al., 2018)

[†] These reflect “healthy” cutoffs.

* Mean of nonclinical sample + 1 SD. We were unable to find empirically derived clinical cutoffs for the FMPS-Brief.

FMPS-E = Frost Multidimensional Perfectionism Scale-Brief Evaluative Concerns subscale; FMPS-S = Frost Multidimensional Perfectionism Scale-Brief Striving subscale; SCS = Self-Compassion Scale—Short Form; QOLS = Quality of Life Scale; DASS-21 = Depression Anxiety Stress Scales.

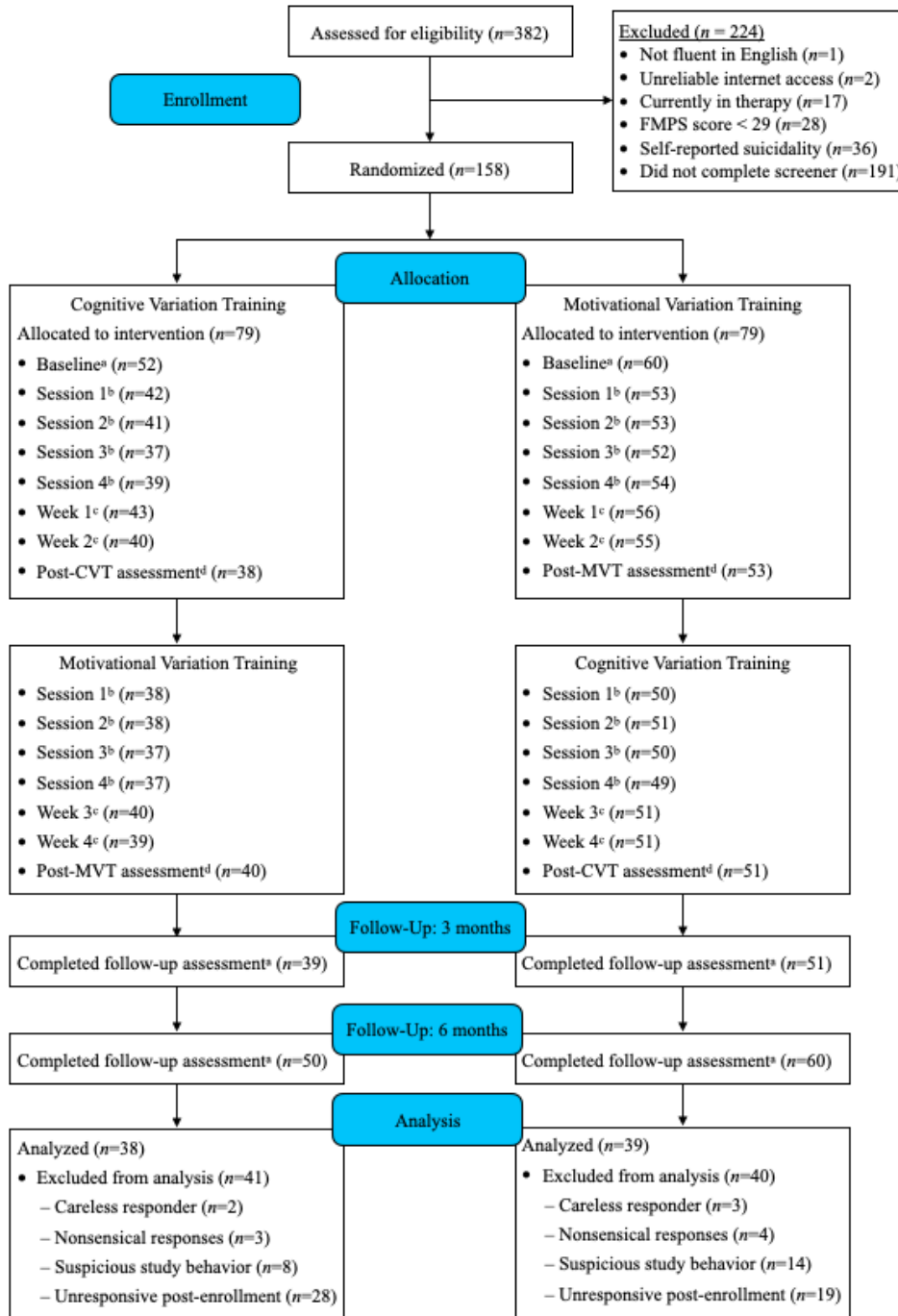
Table 6

Coefficient Estimates and Significance Values for Main Effect of Time from Post-VT1 to Post-VT2 (Incremental Effect of Second Training)

Outcome	<i>B</i>	SE	<i>t</i>	<i>p</i>
FMPS-E	-0.670	0.133	-5.035	<.001***
FMPS-S	-0.707	0.186	-3.806	<.001***
SCS	0.088	0.038	2.326	.024*
QOLS	2.518	0.626	4.022	<.001***
DASS-21	-2.904	0.583	-4.981	<.001***
Cognitive variation	0.466	0.215	2.171	.035*
Motivational variation	0.106	0.212	0.502	.618

*** $p < 0.001$. ** $p < 0.01$. * $p < 0.05$.

Note. VT1 refers to the first training received and VT2 refers to the second training received. FMPS-E = Frost Multidimensional Perfectionism Scale-Brief Evaluative Concerns subscale; FMPS-S = Frost Multidimensional Perfectionism Scale-Brief Striving subscale; SCS = Self-Compassion Scale—Short Form; CFQ = Cognitive Fusion Questionnaire; TCQ = Thought Control Questionnaire; ELS: VL = Engaged Living Scale: Valued Living subscale; QOLS = Quality of Life Scale; DASS-21 = Depression Anxiety Stress Scales.



The assessments given at each time point are denoted by the subscripts below:

^a FMPS, SCS, QOL, DASS CFQ, TCQ, ELS, WSAS, COVID-impact items

^b CVT and MVT processes of change items

^c FMPS, SCS, QOL, DASS CFQ, TCQ, ELS

^d FMPS, SCS, QOL, DASS CFQ, TCQ, ELS, COVID-impact items

Figure 1. Participant flowchart. CFQ = Cognitive Fusion Questionnaire; DASS-21 = Depression Anxiety Stress Scales; ELS = Engaged Living Scale; FMPS = Frost Multidimensional Perfectionism Scale-Brief; QOLS = Quality of Life Scale; SCS = Self-Compassion Scale—Short Form; TCQ = Thought Control Questionnaire; WSAS = Work and Social Adjustment Scale.

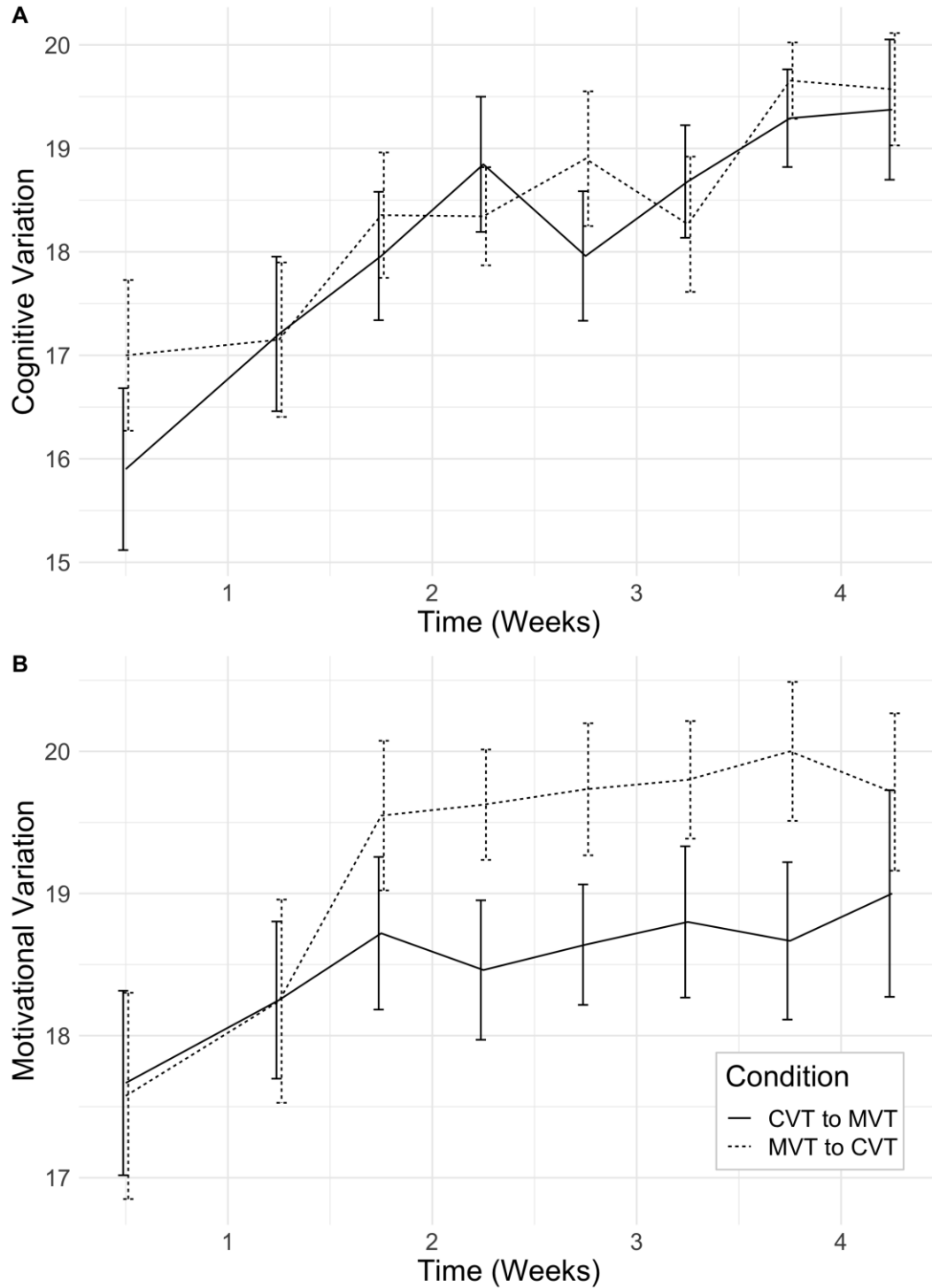


Figure 2. Changes in cognitive variation (Panel A) and motivational variation (Panel B) from baseline to post-intervention with standard error bars. There was a significant time effect on cognitive variation, with no differences between conditions, and a significant time×condition interaction effect on motivational variation.

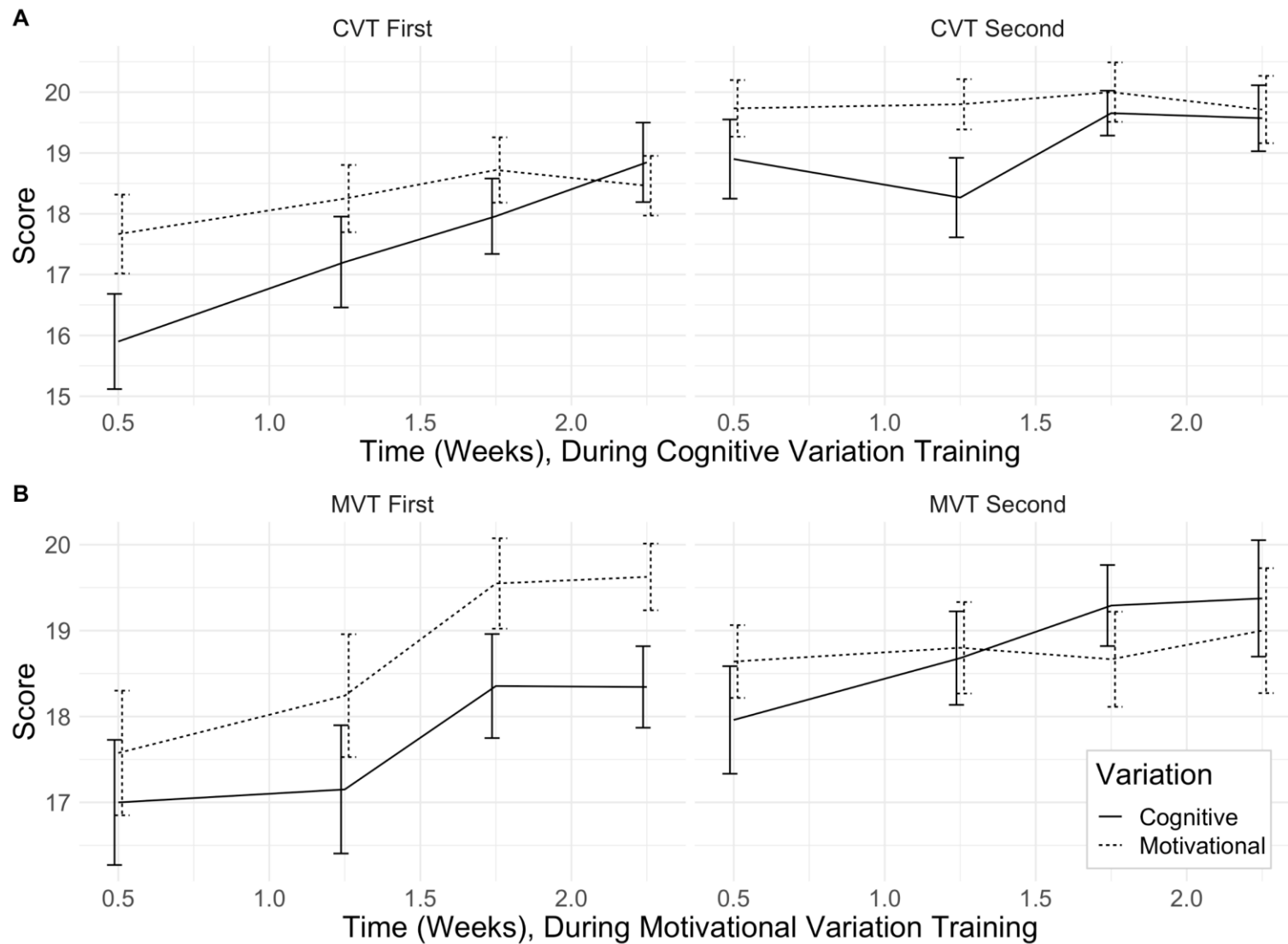


Figure 3. Cognitive and motivational variation scores with standard error bars during the cognitive training (Panel A) and motivational training (Panel B) respectively. There were significant interaction effects for time × type of variation and time × condition (order of training) during the cognitive training and only a significant time effect during the motivational training.