From self-regulation to character: Digital well-being intervention effects on character strengths and subsequent distress

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Conflict of Interest Statement

L.I.C. reported a patent for Stress Toolbox issued (he is the lead author of the Stress Toolbox, which is the core content of the synchronous component of the Integrated Stress Toolbox for Healthcare Providers). L.I.C., L.B.V.A, and J.M.O.P. are employed by AtentaMente Consultores A.C. R.J.D. is the founder, president, and serves on the board of directors for the non-profit organization, Humin.

Abstract

Character strengths (CS) are theorized to be a core mechanism of human flourishing, yet causal evidence for their malleability and role in mental health remains limited. In a randomized controlled trial of a 13-week digital well-being intervention among 2,315 healthcare professionals in Mexico, we showed that the intervention increased a general CS factor relative to a waitlist control ($\beta = 0.515$, $p < .001$). Increases in CS mediated reductions in psychological distress at 37-week follow-up (indirect effect $= -0.013$, $p = .032$), accounting for 4.9% of the total effect. This mediation remained significant after controlling for social connection and insight into emotional processes, indicating that CS represent a distinct mechanism of change. CS did not mediate improvements in well-being, suggesting they primarily function as a resilience factor buffering distress. This study advances a causal science of character and identifies CS as a tractable target for intervention in high-stress occupational settings.

Keywords: character strengths, well-being intervention, distress, well-being, healthcare professionals

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Following Aristotle (2025), numerous scholars have argued that good character is foundational to human flourishing (Keyes et al., 2012; Peterson & Seligman, 2004; VanderWeele, 2021). Consistent with this view, empirical research indicates that character strengths (CS)—morally valued personality traits that function as mechanisms for enacting core moral virtues (Peterson & Seligman, 2004)—are associated with better mental health and higher well-being (Casali & Feraco, 2025), as well as better physical health (Weziak-Bialowolska et al., 2023). Experimental and quasi-experimental studies further suggest that interventions encouraging individuals to identify and apply their CS lead to reductions in depressive symptoms and improvements in various indicators of psychological functioning (Cebolla et al., 2025; Schutte & Malouff, 2019), as well as better workplace outcomes (Vásquez-Pailaqueo et al., 2025; Virga et al., 2023).

Interventions aimed at promoting virtuous behavior and emotional well-being may be especially consequential for healthcare professionals (HCPs). Despite their vital role in societal functioning, HCPs worldwide experience high levels of psychological distress and burnout, with particularly elevated rates in low- and middle-income countries (LMICs) (Robles et al., 2022; Rosales Vaca et al., 2022). In this context, fostering virtues within healthcare settings becomes essential, not only to uphold ethical and trustworthy patient care, but also to strengthen professional fulfillment, teamwork, and moral resilience, thereby buffering against the daily stressors inherent in clinical practice (Marcum, 2012; Pellegrino & Thomasma, 1993). Research evidence shows that greater endorsement of character strengths is positively associated with higher well-being and serves as a protective factor against burnout among HCPs (Heintz & Ruch, 2020; Huber, Strecker, Hausler, et al., 2020; Huber, Strecker, Kachel, et al., 2020; Kachel et al., 2021). Two experimental studies reported positive effects of a CS intervention on well-being among nursing students (Malik & Sinha, 2025) and rural community health workers in India (Bondre et al., 2025); however, these studies relied on relatively small samples (n = 108 and n = 61, respectively). Most of this research has been correlational, highlighting the need for well-powered experimental studies to establish the causal effects of character development on psychological functioning in healthcare contexts.

Present Study

The present study is a secondary analysis of data from a 2-arm randomized controlled trial (RCT) evaluating the Integrated Stress Toolbox for Healthcare Providers, a 13-week digital well-being intervention, conducted with a sample of HCPs (n = 2,315) who worked in 762 participating healthcare facilities across seven Mexican states: Campeche, Coahuila, Nuevo León, Oaxaca, Querétaro, Sonora, and Jalisco. The intervention combined didactic instruction with guided meditations and demonstrated effectiveness in reducing psychological distress and enhancing well-being (Hirshberg et al., 2025). Theory suggests that contemplative well-being interventions that do not explicitly target CS can nevertheless foster their development. In Aristotelian terms, meditation and other contemplative practices such as reflection and inquiry cultivate capacities such as attention regulation, emotion regulation, and acceptance, which in turn support practical wisdom (phronesis; Miller, 2019) by enhancing the ability to discern morally appropriate actions and regulate impulses—core mechanisms through which character matures. Meditation and contemplation are thought to engage moral and virtue-related processes by increasing awareness of personal values, cultivation of prosocial dispositions and empathy, and ethical behavior (Baer, 2015).

In the current analysis, we examined intervention effects on self-reported CS, using the widely adopted Values in Action (VIA) framework developed by Peterson and Seligman based on philosophical and cross-cultural analyses of moral traditions (McGrath, 2015a, 2024; Peterson & Seligman, 2004). The VIA framework includes 24 CS, such as curiosity, perseverance, kindness, prudence, and gratitude. The initial classification connected CS with six virtues: wisdom, courage, humanity, justice, temperance, and transcendence. More recent empirical work has converged on the presence of a strong general CS factor that robustly predicts mental health and well-being, while specific factors do not (Casali & Feraco, 2024; Cheng et al., 2022; Feraco et al., 2023; McGrath, 2015b; Ng et al., 2017). Consistent with this empirical evidence, we focused on the general CS factor, hypothesizing that intervention participants would show greater increases in CS than the waitlist control group (Hypothesis 1). We also predicted that the intervention would have a significant indirect effect through CS general factor on follow up distress and well-being (Hypothesis 2). As a sensitivity analysis, we tested whether CS represent a distinct mechanism of change, separable from other psychological mechanisms theorized to underlie well-being (Kral et al., 2024) and reported elsewhere for the current sample (Hirshberg et al., 2025). These included awareness of external cues and internal states, connection to others, insight into how psychological processes shape experience, and a sense of purpose. Ethical approval for the study was obtained from review boards overseeing each participating state, and from the University of Wisconsin–Madison Institutional Review Board. The parent trial was registered at ClinicalTrials.gov (NCT05767970). We report how we determined our sample size, all data exclusions, all manipulations, and all measures in the study. The data and analysis code are openly available in the Open Science Framework repository: https://osf.io/wtr5k.

Methods

Participants and Procedure

Eligible participants were adults (≥18 years) working either as patient-facing HCPs or as regional or state-level healthcare administrators. Individuals were excluded if they had previously attended a training delivered by AtentaMente, the organization that delivered the intervention. Written informed consent was obtained from all participants. Participants could receive continuing education credits by their respective Ministry of Health for completing the intervention and study measures. Additional procedural details are reported in Hirshberg et al. (2025). A total of 2,315 participants completed baseline assessments and were randomized to the intervention (n = 1,157) or control (n = 1,158) condition. Sample size was determined for the parent trial. Participants were employed across 729 hospitals or clinics and 33 regional or state administrative offices. Demographic characteristics are shown in Table 1.

Table 1
Sample Demographic Characteristics

Characteristic	Intervention	Control
Age	34.51 (11.34)	34.82 (11.53)
Gender		
Woman	73.% (845)	73% (848)
Man	27% (308)	26% (306)
Prefer not to answer	<1% (4)	<1% (4)
Profession		
Nurse	35% (402)	32% (376)

Physician	33% (377)	31% (359)
Psychologist	8% (90)	8% (97)
Administrator	8% (96)	8%(87)
Dentist	6% (64)	8% (90)
Social work	3% (38)	3% (37)
Nutritionist	3% (33)	3% (31)
Laboratory technician	2% (25)	2% (30)
Physical therapist	1% (15)	3% (25)
Health promotor	1% (14)	1% (15)
Other	<1% (3)	<1% (5)

Note. Race and ethnicity were not collected, as these variables are not commonly assessed in Mexico and study partners raised concerns about their inclusion.

Participants completed eight assessments: baseline prior to randomization and assessments at 1, 3, 5, 8, 13, 25, and 37 weeks post-randomization. The outcomes examined here were measured at baseline, week 13 (post-intervention), and week 37 (follow-up). At the post-intervention, data were available for 73.7% of participants (1,705 of 2,315), including 70.8% of the intervention group (819 of 1,157) and 76.5% of the control group (886 of 1,158). At follow-up, 63.1% of participants provided data (1,460 of 2,315), comprising 59.9% of the intervention group (693 of 1,157) and 66.2% of the control group (767 of 1,158).

Intervention

The Integrated Stress Toolbox for Healthcare Providers is a 13-week well-being intervention that includes eight weekly 2-hour group sessions delivered via videoconference, a final session at week 13, and 13 weeks of companion content delivered through the Healthy Minds Program app (Healthy Minds Innovations, 2019). All sessions were recorded and available for asynchronous viewing. The intervention framed well-being as a set of trainable skills and combined psychoeducation (delivered through live lectures and podcast-style app content) with cognitive-behavioral and meditation-based practices. Although the intervention did not explicitly target CS, it included practices such as values clarification, compassion, and self-inquiry that may have indirectly engaged them. Most intervention participants attended or viewed the majority of sessions, with 63.4% (733 of 1,157) engaging in at least 7 of 9 sessions. App engagement was lower: 28.6% of participants (331 of 1,157) used the app for at least the recommended 300 minutes.

Measures

CS were measured with the 24-item Signature Strengths Inventory (SSI) (McGrath, 2015b). The measure provides respondents with a brief description of each strength (e.g., prudence is described as "You are wisely cautious; you are planful and conscientious; you are careful to not take undue risks or do things you might later regret"). Participants then rated how much they agree with the sentence "It is natural and effortless it is to express my … strength," from 1 = *very strongly disagree* to 7 = *very strongly agree*. Sample Cronbach's α and McDonald's ω were .96 both.

Psychological distress was the aggregate of z-scored 10-item Perceived Stress Scale (Cohen et al., 1983; sample α = , ω =) and Patient Reported Outcomes Measurement Information System (PROMIS) 8-item Adult Anxiety (sample α = , ω =) and Depression (sample α = , ω =) scales (Pilkonis et al., 2011). Well-being was assessed with the 5-item World Health Organization-5 Well-Being Scale (Bech, 2004; sample α = , ω =).

Alternative intervention mechanisms were measured with the Healthy Minds Index (Kral et al., 2024) that includes the 4-item Awareness (sample α = , ω =), 6-item

Connection (sample α = , ω =), 3-item Insight (sample α = , ω =), and 4-item Purpose (sample α = , ω =) subscales.

All measures were administered in Spanish. The measures battery was pilot tested on an independent sample, as reported in more detail in Hirshberg et al. (2025).

Statistical Analyses

Measurement Model

Building on recent evidence indicating the presence of a strong general factor in CS measures (Casali & Feraco, 2024; Cheng et al., 2022; Feraco et al., 2023; McGrath, 2015b; Ng et al., 2017), we modelled CS as a single latent construct with all indicators loading on a common factor using robust maximum likelihood (MLR) with full information maximum likelihood (FIML) to handle missing data. Model fit was evaluated using the robust Comparative Fit Index (CFI), robust Tucker–Lewis Index (TLI), Robust Root Mean Square Error of Approximation (RMSEA), and Standardized Root Mean Square Residual (SRMR). We started by fitting this structure to the baseline data. The strict, unidimensional model did not meet conventional thresholds for acceptable fit (West et al., 2012), with CFI = .893, Robust TLI = .882, Robust RMSEA = .080, 90% CI [.077, .083], SRMR = .044. To investigate whether misfit was due to model misspecification, we conducted exploratory factor analysis (EFA) and tested several different factor structures. Models with additional latent factors showed improved fit; however, consistent with prior research (Casali & Feraco, 2024; Cheng et al., 2022; Feraco et al., 2023; McGrath, 2015b; Ng et al., 2017), all solutions were dominated by a strong general factor (see Supplement 1 for more details). To our knowledge, no prior study examining the structure of CS measures has achieved conventional benchmarks for good model fit; reported fit indices in previous research have generally been comparable to, or poorer than, those observed in our study (Feraco et al., 2022). Additionally, the one-factor solution demonstrated excellent reliability. Thus, because the inclusion of specific factors did not alter the interpretation or functional role of the general factor, we retained the more parsimonious one-factor model for further analyses.

Measurement Invariance

We tested measurement invariance across groups using a stepwise approach that began with a configural model, which assesses whether the same general factor structure holds across groups (Meredith, 1993). After establishing configural invariance, we tested metric invariance by constraining factor loadings to be equal across groups. Finally, we examined scalar invariance by additionally constraining item intercepts. Results confirmed that the factor structure, loadings, and intercepts did not significantly differ between groups, supporting between-group comparison of CS trajectories (ps > .198; see more details in Supplement 2).

Hypothesis 1

We estimated a latent growth curve model to estimate change in the general CS factor across three time points (baseline, post-intervention, and 37-week follow-up). Intervention effects were tested by regressing intercept and slope on group. A detailed model specification is provided in Supplement 3.

Hypothesis 2

We tested whether change in the general CS factor from baseline to post-intervention mediated intervention effects on follow-up distress using a latent change score model. Change in CS was modeled by regressing general factor at post-intervention on the general factor at baseline, with residuals capturing the latent change. Intervention effects were specified by regressing the latent change factor on group (a-path). The follow-up distress was regressed on latent change (b-path), group (c-path), and baseline distress, enabling estimation of indirect (a × b), direct (c'), and total effects of the intervention via change in CS. The same model was used to estimate the effects on well-being. A detailed model specification is provided in Supplement 4.

Sensitivity Analysis

Given prior evidence for the central role of the CS general factor, we used a simplified specification based on observed scores. CS items were averaged at each time point, and change was computed as the difference between baseline and post-intervention means of CS items. Change scores for HMI subscales (awareness, connection, insight, and purpose) were computed analogously. We then estimated four parallel mediation models in which CS change and one alternative mechanism (each HMI subscale change, tested separately) were included as correlated mediators. A detailed model specification is provided in Supplement 5.

Results

To test intervention effects on the general CS factor trajectories (Hypothesis 1), we estimated a longitudinal growth curve model with intercept and slope predicted by group. As Table 2 shows, the intervention group exhibited greater positive linear change over time than the control group, as indicated by a positive and statistically significant effect of group on the latent slope. Model fit was as follows: robust CFI = .893, robust

TLI = .888, robust RMSEA = .050, 90% CI [.048, .051], SRMR = .037.

Table 2

Hypothesis 1 Model Parameter Estimates

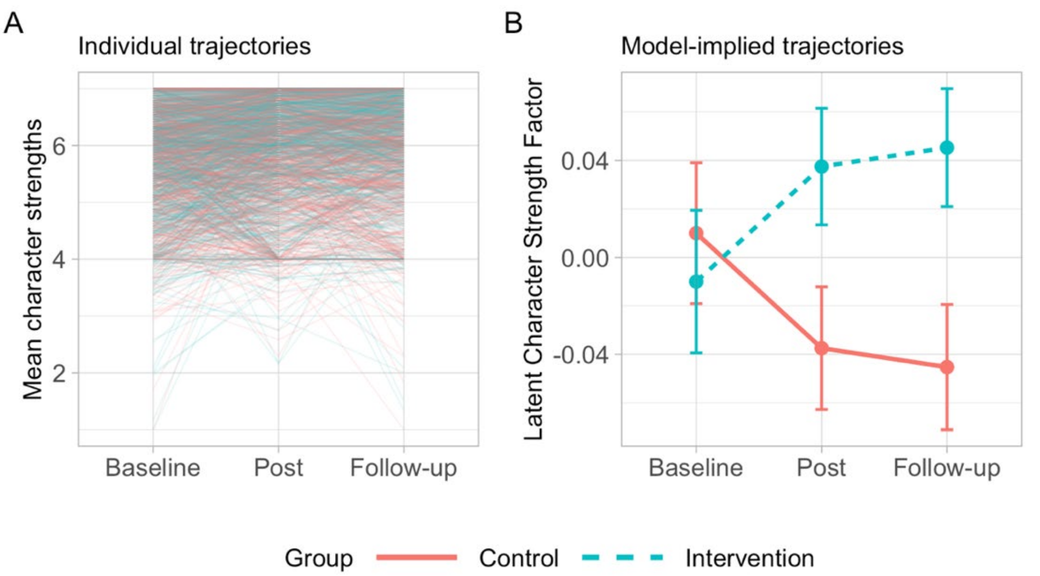
Parameter	*b*	SE	95% CI	*z*	*p*	β
Intercept variance	0.592	0.066	0.462, 0.722	8.919	<.001	1.000
Slope variance	0.005	0.037	-0.067, 0.077	0.140	.889	0.735
Intercept-slope covariance	0.012	0.041	-0.069, 0.092	0.284	.777	0.212
Group difference in intercept	-0.008	0.041	-0.089, 0.073	-0.195	.845	-0.005
Group difference in slope	0.086	0.023	0.042, 0.130	3.802	<.001	0.515

Note. b = unstandardized regression coefficient; SE = standard error; CI = confidence interval; β = standardized regression coefficient

Individual participant trajectories and mean general CS factor scores at each time point extracted from the measurement model are depicted in Figure 1.

Figure 1

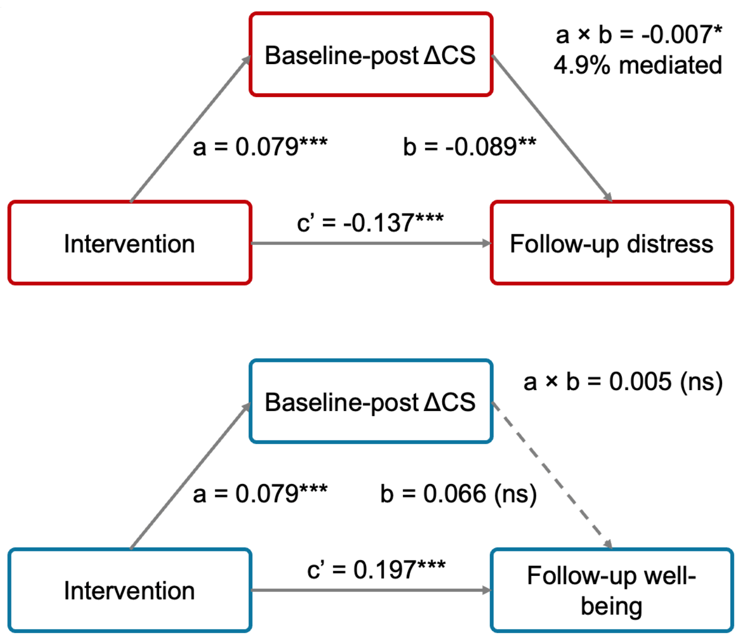
Individual Character Strengths Trajectories and Latent Factor Scores



Next, we examined whether change in the general CS factor from baseline to post-intervention mediated the effects of the intervention on distress and well-being at the 37-week follow-up, controlling for baseline levels of each outcome. As shown in Figure 2 the indirect effect was observed for distress, but not for well-being.

Figure 2

Mediation Results with Standardized Effects



In the model for distress, as shown in Table 3, the proportional change path was significant and negative, indicating that participants with higher baseline CS showed smaller subsequent increases in CS. Intervention condition significantly predicted CS change, such that participants in the intervention group showed greater increases in CS from baseline to post-intervention. CS change, in turn, significantly predicted follow-up distress: greater increases in CS were associated with lower distress at follow-up, controlling for intervention condition and baseline distress. Intervention condition also retained a significant direct effect on follow-up distress, indicating lower follow-up distress in the intervention group after accounting for CS change. The indirect effect of intervention condition on follow-up distress through CS change was significant. The total effect of intervention condition on follow-up distress was also significant. The proportion mediated was significant and indicated that 4.9% of the total intervention effect on follow-up distress was accounted for by latent CS change. Model fit was as follows: robust CFI = .901, robust TLI = .895, robust RMSEA = .055, 90% CI [.053, .056], SRMR = .092.

Table 3

Hypothesis 2 Model Parameter Estimates (Distress)

Path	Description	Estimate	SE	95% CI	*z*	*p*	Std. estimate
Autoregressive (fixed)	Baseline CS → post-intervention CS	1.000					
	Baseline CS variance	0.989	0.064	0.864, 1.113	15.565	<.001	1.000
	CS change variance	0.516	0.038	0.442, 0.590	13.723	<.001	0.771
Proportional change	Baseline CS → CS change	-0.388	0.040	-0.466, -0.311	-9.801	<.001	-0.472
a	Group → CS change	0.129	0.037	0.057, 0.201	3.519	<.001	0.079
b	CS change → follow-up distress	-0.100	0.035	-0.169, -0.031	-2.825	.005	-0.089
c’	Group → follow-up distress	-0.250	0.038	-0.324, -0.176	-6.631	<.001	-0.137
Controlling for baseline outcome	Baseline distress → follow-up distress	0.603	0.023	0.558, 0.647	26.509	<.001	0.605
a × b	Indirect effect	-0.013	0.006	-0.025, -0.001	-2.143	.032	-0.007
c’ + (a × b)	Total effect	-0.263	0.038	-0.337, -0.189	-6.969	<.001	-0.144
(a × b) / total	Proportion mediated	.049					

Note. CS = character strengths.

Sensitivity analyses showed that change in CS remained a significant mediator in models that included change in connection and insight, but not in models that included awareness and purpose as alternative mechanisms (see Table 4).

Table 4

Sensitivity Analysis Indirect Effects (Distress)

Effect	**Estimate**	**SE**	**95% CI**	***z***	***p***	**Std. estimate**
Awareness						
Indirect effect through Awareness change	-0.046	0.011	-0.067, -0.026	-4.358	<.001	-0.025
Indirect effect through CS change	-0.008	0.005	-0.016, 0.001	-1.663	.096	-0.004
Connection						
Indirect effect through Connection change	-0.018	0.007	-0.031, -0.005	-2.655	.008	-0.010
Indirect effect through CS change	-0.011	0.005	-0.021, -0.000	-2.055	.040	-0.006
Insight						
Indirect effect through Insight change	-0.023	0.007	-0.036, -0.010	-3.428	<.001	-0.013
Indirect effect through CS change	-0.011	0.005	-0.021, -0.001	-2.159	.031	-0.006
Purpose						
Indirect effect through Purpose change	-0.023	0.009	-0.040, -0.006	-2.640	.008	-0.012
Indirect effect through CS change	-0.008	0.005	-0.017, 0.001	-1.716	.086	-0.004

As shown in Table 5, CS change did not significantly predict follow-up well-being after controlling for intervention condition and baseline well-being. Intervention condition retained a significant direct effect on follow-up well-being, indicating higher follow-up well-being in the intervention group after accounting for CS change. The indirect effect of intervention condition on follow-up well-being through CS change was not significant, however the total effect of intervention condition on follow-up well-being was significant. Model fit was as follows: robust CFI = .901, robust TLI = .895, robust RMSEA = .055, 90% CI [.053, .056], SRMR = .087.

Table 5
Hypothesis 2 Model Parameter Estimates (Well-Being)

Path	Description	Estimate	SE	95% CI	*z*	*p*	Std. estimate
Autoregressive (fixed)	Baseline CS → post-intervention CS	1.000					
	Baseline CS variance	0.998	0.064	0.872, 1.124	15.531	< .001	1.000
	CS change variance	0.512	0.038	0.437, 0.587	13.348	< .001	0.764
Proportional change	Baseline CS → CS change	-0.393	0.040	-0.471, -0.315	-9.891	< .001	-0.480
a	Group → CS change	0.130	0.037	0.058, 0.201	3.542	< .001	0.079
b	CS change → follow-up well-being	0.389	0.203	-0.008, 0.787	1.921	.055	0.066
c'	Group → follow-up well-being	1.900	0.212	1.484, 2.316	8.945	< .001	0.197
Controlling for baseline outcome	Baseline well-being → follow-up well-being	0.493	0.025	0.444, 0.541	19.751	< .001	0.504
a × b	Indirect effect	0.050	0.030	-0.008, 0.109	1.682	.093	0.005
c' + (a × b)	Total effect	1.951	0.212	1.536, 2.365	9.220	< .001	0.202
(a × b) / total	Proportion mediated	.026					

Note. CS = character strengths.

In sum, the intervention produced significantly greater increases in a general SC factor than the waitlist control, and increases in CS mediated reductions in psychological distress at 37-week follow-up. This mediation remained significant after controlling for connection and insight, but not awareness or purpose. CS change did not significantly mediate improvements in well-being, despite a significant overall intervention effect on well-being.

Discussion

Scholarly interest in the empirical study of virtue has expanded substantially over the past two decades, driven by longstanding theoretical accounts, both ancient and contemporary, that posit virtue-consistent traits such as CS as central to human flourishing (Aristotle, 2025; Jankowski et al., 2020; Keyes et al., 2012; Peterson & Seligman, 2004; VanderWeele, 2021). Empirical research has largely conceptualized CS as relatively stable dispositions and has focused primarily on their correlational associations with mental health and well-being (Gander et al., 2020). Despite evidence that CS-based interventions improve psychological functioning, whether CS themselves can be altered through intervention remains largely unexplored (Ruch et al., 2020), with (Gander et al., 2024) representing a notable exception.

In this study, we address this gap by providing longitudinal evidence from a large-scale RCT demonstrating that CS are malleable, can be systematically strengthened through digital well-being training, and improvements in CS are consequential for psychological functioning. Specifically, we show that a general CS factor demonstrates systematic growth following a 13-week well-being intervention and that this growth partially mediates reductions in psychological distress (but not well-being) observed at a 37-week follow-up. Although change in CS accounted for 4.9% of the intervention effect on distress, it remained a significant mediator when previously reported mediators (connection and insight, but not awareness and purpose) (Hirshberg et al., 2025) were included. This positions CS as a distinct psychological mechanism, separable from other mechanisms of action of well-being interventions. These findings extend existing virtue research by demonstrating that CS are not only associated with emotional functioning but can also serve as a mechanism through which well-being interventions exert lasting mental health effects.

In light of accumulating evidence for a strong general factor underlying self-reported CS measures (Feraco et al., 2023; Ng et al., 2017), we focused on this shared core rather than on individual CS. In the foundational work by Peterson and Seligman (Peterson & Seligman, 2004) and in much subsequent research, CS are conceptualized as tendencies toward virtuous behavior. From this perspective, a general CS factor can be understood as reflecting a broad disposition toward moral, value-consistent action. Accordingly, a positive slope in the intervention group suggests that participants may have engaged increasingly in value-congruent actions over time, rendering such behavior more habitual and less effortful.

Differential mediation suggests that growth in CS functions primarily as a resilience factor that protects against psychological distress rather than as a direct enhancer of well-being. Well-being effects may be driven more directly by affective and cognitive mechanisms of MBIs (Hirshberg et al., 2025), which may operate in parallel to character development. From the perspective of moral theory, CS are not static traits but moral and psychological excellences that are enacted in response to situational challenges and moral demands (Brady, 2018; Nussbaum, 2001; Peterson & Seligman, 2004). This functional role of CS may be especially salient in high-stress environments such as healthcare. Moreover, the data were collected at the end of the COVID-19 pandemic, an extremely stressful period for HCPs that called for moral resilience. Prior research has shown that CS predict resilience above and beyond sociodemographic characteristics and well-being indicators (Martínez-Martí & Ruch, 2017) and buffer the impact of job demands on absenteeism (van Woerkom et al., 2016). The sensitivity of the mediation effect for well-being via CS also argues against prior interpretations of the general CS factor as dispositional positivity (Feraco et al., 2023; Ng et al., 2017). If the general CS factor primarily reflects dispositional positivity, it should show substantial overlap with the well-being measure and therefore robustly mediate intervention effects on well-being.

The implications of these findings extend beyond individual mental health, particularly for healthcare systems in LMICs commonly operating under chronic resource scarcity, high patient loads, and limited institutional support. In such contexts, psychological distress is frequently intertwined with sustained ethical strain, as healthcare professionals must navigate difficult trade-offs under persistent constraints. CS may therefore support moral resilience—the ability to sustain value-consistent action under pressure. At the same time, strengthening individual resilience is necessary but insufficient for addressing distress and ethical behavior in LMIC healthcare systems, where structural factors such as underfunding, medical supply shortages, and understaffing play a central role (Herrera et al., 2025). Accordingly, interventions that indirectly foster character strengths should be viewed as one component of a broader, multi-level approach to ethical and psychological sustainability in healthcare, complementing rather than substituting for institutional support (Huber, Strecker, Hausler, et al., 2020).

Taken together, these findings lay the groundwork for a mechanistic science of character development by linking intervention-driven change in the general CS factor to downstream mental health outcomes. This study is not without limitations. First, although the behavioral interpretation is theoretically plausible and consistent with prevailing accounts of CS, the self-report measure used in this study does not allow us to determine with certainty whether changes in behavior occurred. One could argue that, rather than fostering actual character development, the intervention primarily altered participants' moral self-concept or promoted self-enhancement (Vonk & Visser, 2021). While changes in moral self-concept are plausible, social desirability or self-enhancement alone are unlikely to persist for several months after the intervention has ended, nor would they be expected to predict changes in distal outcomes. Future work could combine self-report with informant reports of CS (Buschor et al., 2013) to disentangle whether the observed mediation effects reflect changes in actual virtue-consistent behavior or shifts in self-perception. Second, although the RCT design allows us to establish a causal effect of the intervention on CS growth, it is limited in its ability to establish causal mediation mechanisms (Rohrer et al., 2022). Because the mediator is not randomized, inferences about the causal role of CS growth in reducing distress rely on strong assumptions, such as the absence of unmeasured confounders (Rohrer et al., 2022), which may not be feasible, even in longitudinal RCTs. Future studies could build on these findings by testing the causal role of CS in reducing distress using dismantling trial designs. In such designs, participants are assigned to intervention variants that differentially target the proposed mediator (e.g., MBI alone versus MBI combined with explicit CS practices), allowing for more direct tests of causal mechanisms. Finally, future studies could further clarify the nature of the general CS factor by examining its effects on moral self-concept and moral behavior.

Data Availability Statement

The data that support the findings of this study are openly available in the Open Science Framework repository: https://osf.io/wtr5k.

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Supplemental Materials

Supplement 1. Measurement Model Specification

Before testing the study hypotheses, we established a measurement model for the 24-item Signature Strengths Inventory (SSI) (McGrath, 2015). All models were estimated using robust maximum likelihood (MLR) with full information maximum likelihood (FIML) to handle missing data, unless otherwise specified. Goodness of fit was evaluated with the Robust Comparative Fit Index (CFI), Robust Tucker–Lewis Index (TLI), Robust Root Mean Square Error of Approximation (RMSEA), and Standardized Root Mean Square Residual (SRMR), applying conventional thresholds for acceptable fit (West et al., 2012).

Drawing on prior research that showed that character strengths (CS) measures include a strong general factor (Casali & Feraco, 2024; Cheng et al., 2022; Feraco et al., 2023; McGrath, 2015; Ng et al., 2017), we started with modelling CS as a single latent construct with all indicators loading on a common factor. All items loaded strongly on the general factor, with standardized loadings > .55 (see Table S1).

Table S1

Factor Loadings for the One-Factor Character Strengths Model

Item	Loading	SE	95% CI	Std. loading	*p*
Zest	1.39	0.05	1.29, 1.5	0.77	<.001
Persistence	1.29	0.05	1.2, 1.38	0.77	<.001
Hope	1.27	0.05	1.18, 1.37	0.76	<.001
Fairness	1.09	0.03	1.02, 1.15	0.75	<.001
Perspective	1.29	0.05	1.2, 1.39	0.75	<.001
Social intelligence	1.31	0.05	1.22, 1.41	0.75	<.001
Bravery	1.36	0.05	1.25, 1.46	0.74	<.001
Gratitude	1.07	0.03	1, 1.14	0.74	<.001
Kindness	1.10	0.03	1.04, 1.17	0.74	<.001
Love of learning	1.20	0.04	1.11, 1.28	0.74	<.001

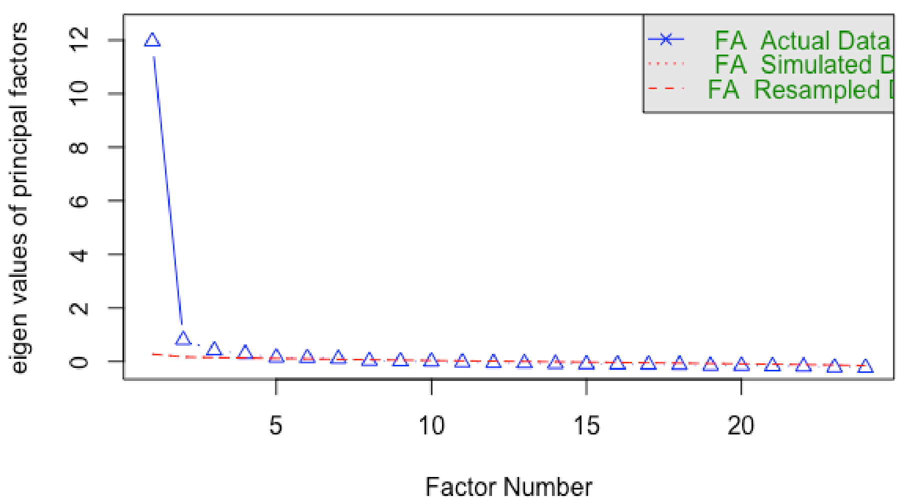
Item	Loading	SE	95% CI	Std. loading	*p*
Citizenship	1.26	0.04	1.18, 1.34	0.73	<.001
Spirituality	1.27	0.05	1.17, 1.37	0.72	<.001
Leadership	1.36	0.05	1.26, 1.47	0.71	<.001
Appreciation of beauty	1.16	0.05	1.07, 1.25	0.70	<.001
Humility	1.09	0.04	1.02, 1.17	0.70	<.001
Self-control	1.25	0.05	1.14, 1.35	0.69	<.001
Integrity*	1.00	0.00	1, 1	0.69	–
Curiosity	1.14	0.05	1.05, 1.23	0.66	<.001
Love	1.29	0.05	1.19, 1.39	0.66	<.001
Prudence	1.10	0.04	1.02, 1.19	0.66	<.001
Creativity	1.28	0.05	1.18, 1.39	0.64	<.001
Judgment	1.13	0.05	1.04, 1.22	0.64	<.001
Humor	1.12	0.04	1.03, 1.21	0.64	<.001
Forgiveness	1.11	0.05	1.01, 1.21	0.55	<.001

Note. *Integrity does not have a *p*-value because its unstandardized loading was fixed to 1.00 to set the metric of the latent factor; therefore, no standard error or significance test is estimated for this loading.

The strict, unidimensional model did not meet conventional thresholds for acceptable fit (West et al., 2012), with CFI = .893, Robust TLI = .882, Robust RMSEA = .080, 90% CI [.077, .083], SRMR = .044. To investigate whether misfit was due to model misspecification, we tested a series of alternative models incorporating additional latent factors. We started with parallel analysis (see Figure S1 for the scree plot) and exploratory factor analysis (EFA) specifying two factors with maximum likelihood (ML) estimation and oblimin rotation. One factor primarily reflected interpersonal/temperance-related strengths, with its strongest loadings on kindness, humility, gratitude, hope, prudence, fairness, citizenship, and related strengths. The second factor primarily reflected intellectual/wisdom-related strengths, with strongest loadings on creativity, curiosity, judgment, love of learning, perspective, and bravery. Several strengths, however, showed notable cross-loadings, particularly zest, social intelligence, leadership, persistence, and humor. The factors were strongly correlated (r = .78). Model fit for the two-factor EFA was as follows: TLI = .923, RMSEA = .065, RMSR = .03, and the two factors together accounted for 54% of the total item variance.

Figure S1

Parallel Analysis Scree Plot



Based on these results and prior research on the structure of character strengths measures (Casali & Feraco, 2024; Cheng et al., 2022; Feraco et al., 2023; McGrath, 2015; Ng et al., 2017), we tested four alternative confirmatory factor analysis (CFA) models and compared their fit with the baseline one-factor model. First, we estimated a two-factor model comprising correlated Interpersonal/Temperance and Intellectual/Wisdom factors. The Interpersonal/Temperance factor included integrity, zest, love, kindness, citizenship, fairness, forgiveness, humility, prudence, self-control, appreciation of beauty, gratitude, hope, humor, and spirituality. The Intellectual/Wisdom factor included creativity, curiosity, judgment, love of learning, perspective, bravery, persistence, social intelligence, and leadership. This model demonstrated improved fit relative to the baseline one-factor model (robust CFI = .925, robust TLI = .918, robust RMSEA = .067, SRMR = .037). The two factors were highly correlated (r = .769).

To evaluate the strength of a potential general factor, we conducted an exploratory bifactor analysis using Schmid–Leiman transformation, specifying five group factors and estimating the model with maximum likelihood extraction on the Pearson correlation matrix. All 24 items loaded strongly on the general factor (loadings = .52–.76), whereas loadings on the five group-specific factors were comparatively smaller and less consistent. The general factor accounted for 73% of the common variance, and omega hierarchical indicated that 83% of reliable variance in total scores was attributable to the general factor. In contrast, the specific factors accounted for relatively little unique reliable variance beyond the general factor (omega group = .09–.25).

Based on this evidence, as well as evidence of a strong general factor in CS measures in prior research (Casali & Feraco, 2024; Cheng et al., 2022; Feraco et al., 2023; McGrath, 2015; Ng et al., 2017), we tested a range of bifactor models where all factors were orthogonal. First, we tested a more parsimonious model in which all items loaded on a general factor, while creativity, curiosity, judgment, love of learning, perspective, bravery, persistence, social intelligence, and leadership additionally loaded on an orthogonal Intellectual/Wisdom specific factor. This bifactor model demonstrated slightly improved fit relative to the correlated two-factor model (robust CFI = .933, robust TLI = .924, robust RMSEA = .065, SRMR = .033).

Next, we estimated a bifactor model in which all items loaded on a general character strengths factor and additionally on five orthogonal specific factors representing Intellectual, Interpersonal, Emotional, Temperance, and Transcendence domains. This model provided the best fit of the tested structures and met conventional thresholds for adequate fit (robust CFI = .954, robust TLI = .944, robust RMSEA = .055, SRMR = .028). Inspection of parameter estimates indicated that several specific factors were weakly defined, with small, unstable, or non-significant loadings for multiple indicators, suggesting overparameterization. We therefore estimated a more parsimonious bifactor model retaining only the better-defined residual group factors. This model specified a general factor on which all items loaded, along with Interpersonal and Transcendence specific factors. Fit was slightly poorer than that of the five-specific-factor bifactor model (robust CFI = .937, robust TLI = .927, robust RMSEA = .063, SRMR = .033).

In conclusion, although models incorporating additional latent factors demonstrated improved fit relative to the baseline one-factor model, consistent with prior research (Casali & Feraco, 2024; Cheng et al., 2022; Feraco et al., 2023; McGrath, 2015; Ng et al., 2017), all alternative solutions remained dominated by a strong general factor. The exploratory and confirmatory analyses indicated that multidimensional structure was present but comparatively weak: additional factors were highly correlated, several specific factors were poorly defined or unstable, and bifactor analyses showed that most reliable common variance was attributable to the general factor rather than domain-specific factors. Collectively, these findings suggest that the SSI primarily functions as an indicator of a broad overarching character strengths dimension, with narrower subdomains contributing relatively limited unique variance beyond that general factor.

Supplement 2. Measurement Invariance Results

Table S2

Measurement Invariance Test Results

Model	DF	X^2	AIC	BIC	CFI	RMSEA	ΔCFI	ΔRMSEA	ΔX^2	ΔDF	*p*
Configural	4866	17066.47	262437.06	265230.95	0.847	0.060	–	–	–	–	–
Loadings	4935	17145.23	262377.82	264810.70	0.847	0.060	< .001	< .001	78.76	69	.198
Intercepts	5004	17214.36	262308.95	264380.82	0.847	0.059	< .001	< .001	69.13	69	.473
Means	5007	17236.86	262325.45	264381.63	0.847	0.059	< .001	< .001	22.50	3	< .001

Note. DF = degrees of freedom; χ^2 = chi-square test statistic; AIC = Akaike information criterion; BIC = Bayesian information criterion; CFI = comparative fit index; RMSEA = root mean square error of approximation.

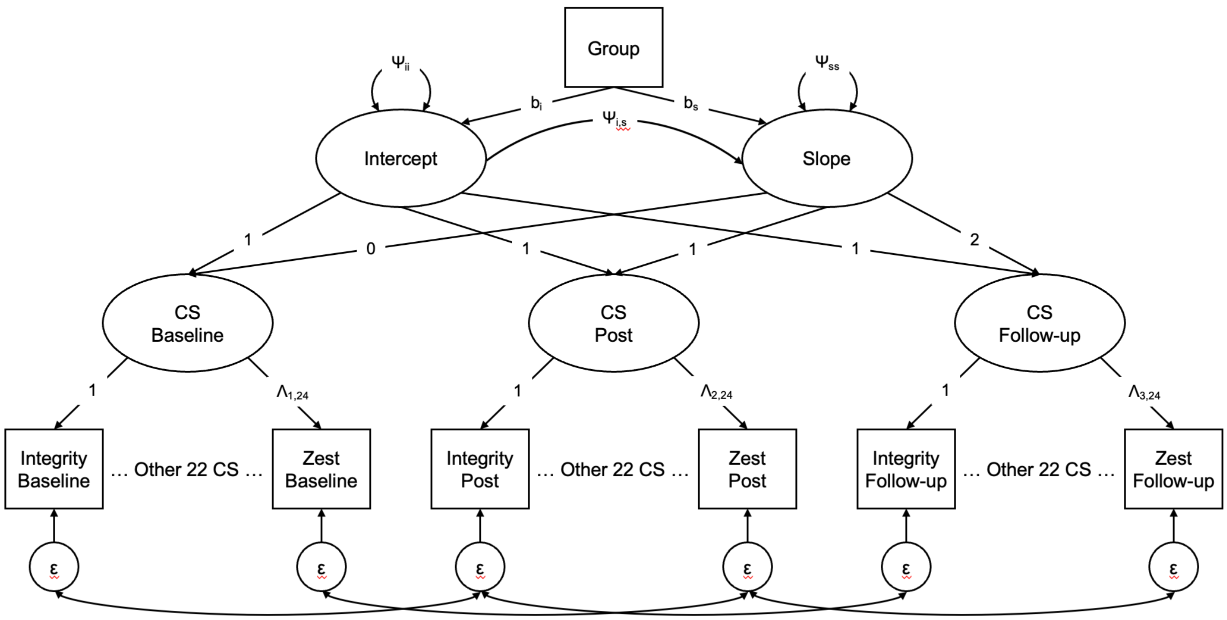
Supplement 3. Hypothesis 1 Model Details

The longitudinal growth model estimated change in the CS general factor across three measurement occasions: baseline, post-intervention, and follow-up. The model combined a longitudinal confirmatory factor model with a latent growth curve model, allowing change in character strengths to be estimated at the latent-variable level. At each time point, character strengths were modeled as a single latent general factor using the 24 CS items at each wave. To account for indicator-specific stability across adjacent waves, residual correlations were estimated between the same item measured at consecutive time points.

Change in the latent CS factor was modeled using latent intercept and slope factors. The intercept factor had fixed loadings of 1 on the latent CS factor at all three waves, representing time-invariant individual differences in overall CS level. The slope factor had fixed loadings of 0, 1, and 2 on the latent CS factors at baseline, post-intervention, and follow-up, respectively, representing linear change across the three waves. The variances of the latent intercept and slope and their covariance were freely estimated, allowing participants to differ in baseline CS, rates of change, and the association between baseline level and change. Group effects were tested by regressing the latent intercept and slope factors on intervention condition. In this regression-centered specification, the latent intercept and slope means were fixed to zero and the substantive parameters of interest were the regression coefficients from group to the intercept and slope factors. Figure S2 depicts the path diagram for this model.

Figure S2

Hypothesis 1 Model Path Diagram



The model was estimated with a mean structure using MLR estimation and FIML for missing data. In summary, this model tested whether the intervention changed the trajectory of the CS general factor over time while accounting for measurement error, repeated measurement of the same item content, individual differences in baseline CS, and individual differences in rates of change. Because unconditional growth factor means were fixed to zero in this specification, we report the estimated variances and covariance of the growth factors and on group differences in the latent intercept and slope.

In the resulting model, all CS loaded strongly and consistently on the general factor (see Table S3).

Table S3

Longitudinal Growth Model Factor Loadings

	Factor loading		
Item	**Baseline**	**Post**	**Follow-up**
Hope	0.76	0.81	0.83
Zest	0.77	0.80	0.79
Persistence	0.77	0.80	0.79
Fairness	0.75	0.81	0.78
Kindness	0.74	0.80	0.80
Citizenship	0.73	0.79	0.79
Gratitude	0.74	0.79	0.78
Love of learning	0.74	0.77	0.80
Bravery	0.74	0.75	0.78
Social intelligence	0.75	0.75	0.77
Perspective	0.75	0.74	0.76
Appreciation of beauty	0.70	0.76	0.78
Spirituality	0.72	0.75	0.77
Integrity	0.69	0.77	0.75
Humility	0.70	0.74	0.77
Self-control	0.68	0.73	0.74
Curiosity	0.66	0.75	0.73
Love	0.66	0.72	0.76
Leadership	0.70	0.68	0.74
Prudence	0.66	0.73	0.71
Humor	0.64	0.72	0.72
Judgment	0.64	0.68	0.72
Creativity	0.64	0.69	0.68
Forgiveness	0.55	0.64	0.66

Supplement 4. Hypothesis 2 Model Details

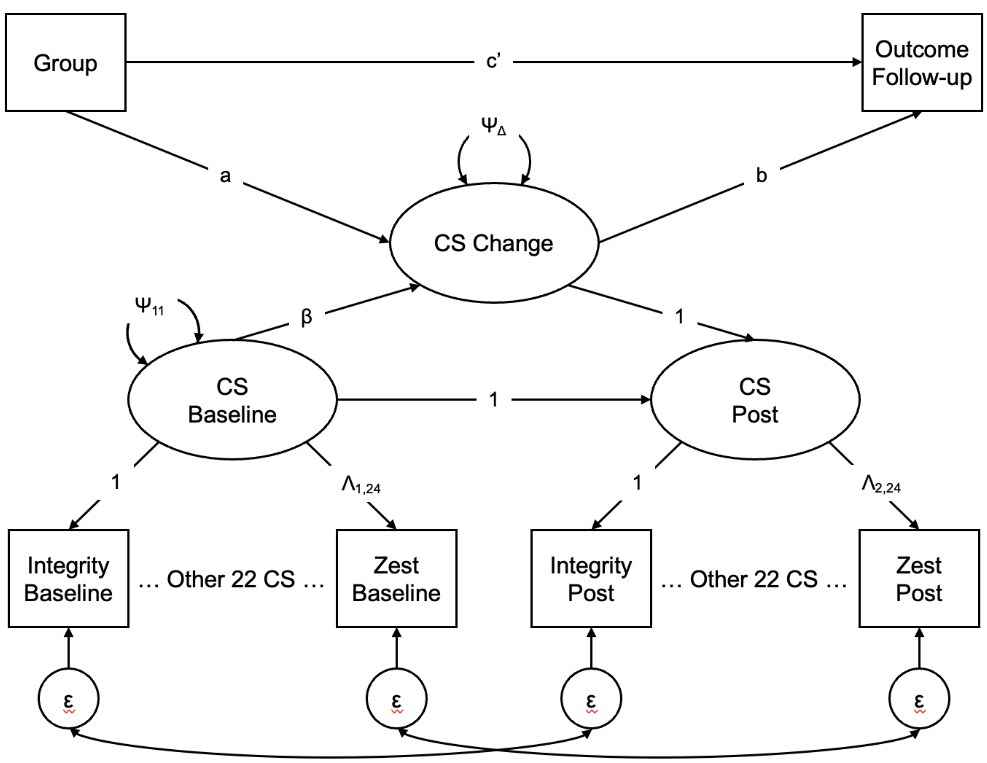
The latent proportional change score mediation model estimated whether change in the CS general factor from baseline to post-intervention mediated the effect of intervention condition on follow-up distress. The model combined a longitudinal confirmatory factor model with a latent change score model and a mediation model, allowing change in character strengths to be estimated at the latent-variable level while testing its association with subsequent distress. At each of the two CS measurement occasions, baseline and post-intervention, CS were modeled as a single latent general factor using the 24 CS items at each wave. Same-item residuals were allowed to correlate. As shown on Figure S3, a latent change factor was defined by fixing its loading on the post-intervention CS factor to 1. The post-intervention CS factor was regressed on the baseline CS factor with the autoregressive path fixed to 1, so that the post-intervention latent score was decomposed into baseline CS plus latent change. The intercept of the post-intervention CS factor was fixed to zero, consistent with the latent change score parameterization. The baseline CS variance and the variance of the latent change factor were freely estimated. CS change was regressed on baseline CS (β), capturing proportional change, or the extent to which initial CS level is associated with change over the intervention period.

The mediation portion of the model tested whether intervention condition predicted latent change in CS and whether latent change in CS predicted follow-up distress. As depicted on Figure S3, the path from group to latent CS change (a) estimated the intervention effect on change in the CS general factor from baseline to post-intervention. The path from latent CS change to follow-up distress (b) estimated whether

CS change predicted later distress, controlling for intervention condition and baseline distress. Follow-up distress was also regressed on group (c') to estimate the direct intervention effect after accounting for proportional CS change, controlling for baseline distress. The indirect effect of intervention condition on follow-up distress through latent CS change was defined as the product of the a and b paths. The total effect was defined as the sum of the direct effect and the indirect effect, and the proportion mediated was defined as the indirect effect divided by the total effect. Thus, the model tested whether intervention-related increases in the CS general factor accounted for part of the intervention effect on follow-up distress.

Figure S3

Hypothesis 2 Model Path Diagram



The model was estimated with a mean structure using MLR estimation and FIML to handle missing data. In summary, this model tested whether the intervention increased latent CS from baseline to post-intervention and whether this latent change predicted lower follow-up distress. An identical model was estimated for the well-being outcome.

Supplement 5. Sensitivity Analysis Model Details

Sensitivity analysis was conducted with parallel mediation models testing the change between baseline and post-intervention change in CS alongside the change in an alternative mediator (awareness, connection, insight, and purpose). Given prior evidence for the central role of the CS general factor, we used a simplified specification based on observed scores. CS items were averaged at each time point, and change was computed as the arithmetic difference between baseline and post-intervention means. Change scores for other mediators were computed analogously.

In each model, intervention condition predicted change in CS and change in one alternative mediator. Follow-up distress was then regressed on intervention condition, change in CS, change in the alternative mediator, and baseline distress. The paths from each mediator to follow-up distress estimated their unique associations with later distress while adjusting for the other mediator, intervention condition, and baseline distress. Change in CS and change in the alternative mediator were allowed to covary, and baseline distress was allowed to covary with both change scores. Specific indirect effects were defined as the product of the intervention-to-mediator path and the mediator-to-outcome path for each mediator. The total indirect effect was computed as the sum of the CS-specific indirect effect and the indirect effect through the alternative mediator. These models tested whether CS change remained a distinct mediator of intervention effects on follow-up distress when modeled in parallel with change in awareness, connection, insight, or purpose.

Supplemental Materials References

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