

A Systematic Review of Volitional Personality Change Research

Corresponding Author: Dr Peter Haehner

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Version 0:

Decision Letter:

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Dear Dr Haehner,

Thank you for your patience during the peer-review process. Your manuscript titled "A Systematic Review of Volitional Personality Change Research" has now been seen by 3 reviewers, and I include their comments at the end of this message. They find your work of interest but raised some important points. We are interested in the possibility of publishing your study in Communications Psychology, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response to the reviewers. Please highlight all changes in the manuscript text file.

Editorially, we consider it important that the revised manuscript does not overstate the tentative relationship between personality change and health. Please address the methodological concerns raised by the reviewers. We ask that you retain your reporting on the deviations from your preregistration, as well as making it explicitly clear where analyses were planned as well as where they deviate or are exploratory. Please follow the PRISMA checklist including PRISMA abstract guidelines.

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that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, provided it still presents a significant contribution to the literature at that stage.

We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning.

We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Jennifer Bellingtier

Jennifer Bellingtier, PhD
Senior Editor
Communications Psychology

REVIEWER EXPERTISE:

Reviewer #1 personality

Reviewer #2 personality

Reviewer #2 personality, systematic reviews and meta-analysis

REVIEWER REPORTS:

Reviewer #1 (Remarks to the Author):

I read the manuscript "A Systematic Review of Volitional Personality Change Research" with much interest. I liked how the authors summarized the different theoretical models on personality change research and that they conducted a meta-analysis on the efficacy of volitional personality change with and without supportive interventions. The writing was clear and the analyses were relatively straightforward to follow.

I was not convinced by the opening argument that VPC is a critical lever with which to target public health. Studying personality change is interesting (perhaps in its own right) and VPC is related to all sorts of outcomes, but I think the focus on health as an outcome needs to be supported with more research and I was curious why the authors framed their introduction (and the paper generally) this way? In the results section it seemed that the strongest effects for VPC interventions were for traits like extraversion, and not for personality traits more saliently related to unhealthy behavior (e.g., neuroticism). If anything, that might be an argument for VPC to be a tool to counter the "loneliness pandemic". Thus, the focus in the introduction on health behaviour change as the ultimate outcome or rationale to care about VPC was hard for me to buy into.

On page 11 I would include an additional interpretation for why there seems to be mixed evidence for VPC. Moore et al. (2021), a study you cite, showed that individuals made more change on traits they felt autonomously motivated to change, rather than controlled motivation to change. In other words, those individuals who reported feeling agentic, choiceful, and self-concordant about changing a trait - those who "wanted to change" (autonomous reasons) made more progress than those who felt pressured or coerced by others to change - those that felt like the "had to" change (controlled reasons). Much work by Ken Sheldon, Andy Elliot and Richard Koestner supports the fact that goal progress (across domains) is improved when the motivation for pursuing the goal is autonomous rather than controlled. The distinction between autonomous and controlled reasons for trait change was not considered in all the studies you included in the meta-analysis, but provides a possible explanation for the variability in findings and could be discussed here.

On pages 18-19 you return to the issue of whether VPC can be used for health promotion. Again the evidence presented for this seemed weak and unconvincing. The "positive spillover" studies you described did not demonstrate a spill-over from personality trait change to health behaviour change. Rather you describe spill-over effects to increased life satisfaction, domain satisfaction, and self-esteem... these are all interesting and worthy outcomes, but do not relate to health promotion. Thus I am skeptical of the claim about VPC and health promotion (prevention)- both terms are used interchangeably throughout the manuscript...

Reviewer #2 (Remarks to the Author):

This article presents a systematic review of the existing volitional personality change literature. I greatly enjoyed reading this manuscript. It is well-written and adds much to the field of volitional personality change. Below, I list several suggestions for improvement (though not necessarily in order of importance).

1. The emphasis on health in the introduction and outlook sections feels a bit out of place and tacked on. While I understand the logic (personality predicts increased health interventions to increase personality traits lead to increased health), the review itself does not focus on health so a bookended emphasis on health doesn't make a lot of sense to me. It makes more sense to me to provide a solid mention in the 'practical implications' section and then focus on presenting a review that culminates in an increased understanding of whether people can or cannot accomplish their personality change goals – which is impactful in and of itself. Alternatively, you could include a discussion on why volitional personality change interventions (relative to personality change interventions that don't harness the power of intention) are particularly important in helping people improve their health. There is a vast health intervention literature that focuses on changing health behaviors and/or conscientiousness (see Magidson et al., 2014). What does VPC add? Moreover, a more explicit integration of health outcomes (psychological or physical) into your review would be helpful. You could add a research question that summarizes studies that include well-being as an outcome, for instance (see Hudson & Fraley, 2016).
2. Relatedly, with your current focus on health, how do you reconcile some review findings that indicate VPC may make it harder to improve health (i.e., that VPC to increase C leads to lower C)?
3. How does your review account for the bias of a large number of studies coming from one lab (i.e., Hudson et al.)?
4. I think this review misses several consistently asked questions in the VPC literature and thus misses an opportunity to provide the field with a more comprehensive review. Additional questions could include (but aren't limited to): what is the prevalence of personality change goals across studies (are there methodological, sample, etc. moderators)? What traits do people want to change? What are the relations between trait goals and corresponding current trait levels? Providing answers to these questions in this review will help the field move forward and answer other, novel questions.

Reviewer #3 (Remarks to the Author):

I really like the idea of the manuscript because research on volitional personality change (VPC) is promising for understanding personality change. And I think it is good to review and meta-analytically integrate the existing evidence. Here are some further strengths and potential issues that I noticed:

1. At several places in the manuscript the authors make the implicit or explicit argument that personality traits cause health. For example, in the first sentence of the abstract, they write "Personality traits are of critical importance for individual and public health". (If personality traits would only predict but not cause health, they would not be "critically important") Or in the first sentence of the conclusion, they write "Personality traits have important implications for public and individual health". The same argument is also made in the Practical Implications section. As far as I know and I saw from a look at the publications the authors cited, there are many studies that found links between personality traits and health, but there is no solid causally informative research on the effects of personality on health. It does, in my opinion, not make a big difference whether personality traits or changes in personality traits predict health-related outcomes, both kinds of associations can be confounded by third variables (e.g., genetics, changes in income or other psychological traits such as personal values). Even a time lag does not provide solid evidence for a causal effect because the effect of the confounder on health might come with a bigger time lag than the effect of the confounder on personality. Furthermore, most of these previous studies did not explicitly and systematically attempt to estimate causal effects of personality on health and thus their design and analysis methods were not tailored towards addressing causal research questions (e.g., Grosz et al., 2020; Loh & Ren, 2023; Lundberg et al., 2021; Mulder et al., 2024). I think the authors of the current manuscript should make it explicit that their argument rests on the assumption that personality causes health. But at the same time, I think they should also point out that it is not clear yet whether personality actually causes health.
2. Relatedly, the authors wrote on page 2 "Personality traits are stable albeit malleable across the lifespan and are theorized to have lasting influences on important life outcomes (8,9)" the references 8 and 9 (Bleidorn et al., 2022; Roberts et al., 2017) are meta-analyses on personality change. They do not propose theories about the effects of personality on important life outcomes. Hence, I would replace references 8 and 9 with papers about the impact of personality on life outcomes such as health (e.g., Friedman et al., 2014; Shanahan et al., 2014).
3. I think it would be good to include the number of VPC intervention studies in the abstract to provide a clearer overview.
4. The summary of theoretical perspectives on pages 4 and 5 is well-written and insightful.
5. I would remove "health" and the causal effect of volitional personality change on health from Figure 1 because these elements of the figure are not central features of the three outlined theories on VPC.
6. Relatedly, I do not agree with the following statement "According to VPC theories, people should be able to intentionally change their personality traits in a desired direction and these changes should have positive consequences for individual and public health (13,25)." I checked reference 13 and 25 (Hennecke et al., 2014; Allemand & Flückiger, 2017). The VPC theories they proposed do not focus on the effects on individual and public health.
7. "Hence, the goal of this systematic review was to summarize and integrate the emerging VPC literature and discuss directions future research." I think the word "for" is missing before "future research"
8. I am in favor of preregistrations. However, the authors did not preregister any hypotheses. So, it is unclear to me what the purpose of the preregistration was. Preregistrations help us to do confirmatory research by preregistering hypotheses and the way we test them (e.g., Nosek & Lindsay, 2018). But if we do not have any hypotheses, then the research is exploratory rather than confirmatory and we do not need to preregister anything. It is also not necessary to point out deviations from the preregistration because the findings are exploratory regardless of whether the authors deviated from the preregistration or not.
9. Literature search: I really liked that the authors did a forward search for each publication identified via other search strategies. In my experience, the forward search is particularly helpful in finding additional relevant publications.
10. Literature search: The authors only screened the reference of two review articles. It might be good if the authors

additionally screened all the reference sections of publications identified via other search strategies because there might be some older studies that investigated VPC but were not found with the other search strategies.

11. Page 6: When I first read the fifth inclusion criterion, I thought studies were only included if they were interventions. Only later, I realized that they meant the study needs to assess personality before and after the intervention if it was an intervention study. I think it would be good if the authors would revise how they describe their fifth inclusion criterion.

12. Given that the authors only included 29 studies, I think it would have been good that each article gets coded (Step 3) by two coders. The overall average of 94% was high but I think it would be good to make sure that all the information is extracted correctly.

13. The descriptive results are interesting because they highlight gaps in the research, such as the need for more VPC studies involving older populations.

14. I like the illustration of the typical research designs in Figures 3A and 3B. However, the research questions stated in the graphs could be better aligned with the theoretical and practical goals of the VPC literature. I would recommend the research question “Do personality change goals cause personality changes?” over the question “Do personality change goals predict personality changes?”. If change goals would only predict but not cause personality change, then change goals would not be very theoretically and practically relevant because some third variable (e.g., sex, genetics, income, personal values) would cause change goals and personality change. If change goals would only predict personality change, then theories should be about these third variables rather than about change goals. I think the authors ended up with the term “predict” because they aligned the research question to the question that can be readily answered with the designs and statistical analyses methods that are commonly used in VPC research. However, design and analysis methods should not determine the research question. The theory should determine the research question, and the research question should determine the design and analysis methods (e.g., Lundberg et al., 2021). Of course, once you explicitly state the actual research question of interest (“Do personality change goals cause personality changes?”), you need to discuss the assumptions that need to hold so that the statistical estimand (e.g., regression coefficient) can inform the theoretical estimand (research question). Similarly, for Figure 3b, I would recommend the research question “Do personality change intervention causes personality changes?” over “Do personality traits differ before and after the intervention?”. Testing whether personality traits differ before and after the intervention is one attempt to investigate the causal research question rather than the research question itself.

15. At the beginning of the subchapter “Findings on How Change Goals are Related to Volitional Personality Change”, I would clarify whether the review is narrative or includes quantitative aggregation (e.g., meta-analysis). Additionally, I would include a graph showing the average effect size for each Big Five trait separately.

16. “As illustrated in Figure 4, changes in personality traits over three months were almost three-times stronger for people who wanted to change their personality traits ($d = 0.14$) compared to normative personality changes over the same time interval ($d = 0.05$).” I was not sure how the authors came to these effect sizes ($d = 0.14$; $d = 0.05$) and whether they are comparable to the pre-post difference d (dz; e.g., Lakens, 2013) that is reported for VPC interventions. It would be great if the authors would clarify these points.

17. I like the classification of the interventions according to the taxonomy distinguishing relevant intervention strategies, levels, and aspects of intervention targets.

18. When the authors calculated the effect size pre-post difference d (dz), they used the bias correction factor c . It would be great if the authors could explain why they used this correction and cite a methods paper about it.

19. Figure 5 and the first funnel plot in the supplemental material suggest that one effect (probably the agreeableness effect in Stieger et al., 2021) is an outlier. I would double-check whether this effect size is coded and computed correctly.

20. Currently, the authors only present the meta-analytic effect sizes for extraversion and conscientiousness in the main text. The meta-analytic effect sizes for the other traits and the confidence intervals are only presented in Section 3 of the supplemental material. I think it would be good to present the meta-analytic effect sizes for all five Big Five traits and their confidence intervals in the main document.

21. “These studies provide initial evidence that changing personality traits may indeed be beneficial for other life domains.” I found it a bit odd to write that changing personality traits may benefit other life domains because personality traits are NOT a life domain such as health, relationships, or career. This is also an issue in the following sentence of the summary “Furthermore, the effects seem to be lasting and generalize to other life domains.”

22. I think it is inappropriate that the practical implications section is about the effects of VPC and VPC interventions on health even though the effects on health were not the focus of the current review. Relatedly, I think it is problematic that the authors state that “VPC could be used for health promotion in the general public” because it is unclear whether personality traits have a causal effect on health and whether VPC interventions have a causal effect on health. Even if VPC interventions affect health, VPC interventions might have a causal effect on health via other pathways than through personality change because such an intervention might change not only the targeted personality trait but also other psychological and environmental factors. By the way, it would be important to do more research on the potential risks and negative effects of VPC interventions. Moreover, as the authors point out, we do not yet understand what intervention characteristics and strategies make VPC interventions effective. Finally, it is unclear how generalizable and context dependent the efficacy of VPC intervention is. Hence, we need more research on VPC interventions before recommending them as health promotion in the general public.

Hope that helps,
Michael P. Grosz

Publications not cited in the manuscript:

Friedman, H. S., Kern, M. L., Hampson, S. E., & Duckworth, A. L. (2014). A new life-span approach to conscientiousness and health: combining the pieces of the causal puzzle. *Developmental psychology*, 50(5), 1377.

Grosz, M. P., Rohrer, J. M., & Thoemmes, F. (2020). The taboo against explicit causal inference in nonexperimental psychology. *Perspectives on Psychological Science*, 15(5), 1243-1255.

Lakens, D. (2013). Calculating and reporting effect sizes to facilitate cumulative science: a practical primer for t-tests and

ANOVAs. *Frontiers in psychology*, 4, 863.

Loh, W. W., & Ren, D. (2023). A tutorial on causal inference in longitudinal data with time-varying confounding using g-estimation. *Advances in Methods and Practices in Psychological Science*, 6(3), 25152459231174029.

Lundberg, I., Johnson, R., & Stewart, B. M. (2021). What is your estimand? Defining the target quantity connects statistical evidence to theory. *American Sociological Review*, 86(3), 532-565.

Mulder, J. D., Usami, S., & Hamaker, E. L. (2024). Joint Effects in Cross-Lagged Panel Research Using Structural Nested Mean Models. *Structural Equation Modeling: A Multidisciplinary Journal*, 1-17.

Nosek, B. A., & Lindsay, D. S. (2018). Preregistration becoming the norm in psychological science. *APS observer*, 31.

Shanahan, M. J., Hill, P. L., Roberts, B. W., Eccles, J., & Friedman, H. S. (2014). Conscientiousness, health, and aging: the life course of personality model. *Developmental psychology*, 50(5), 1407.

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Version 1:

Decision Letter:

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We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers and a list of editorial requests. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Jennifer Bellington

Jennifer Bellintier, PhD
Senior Editor
Communications Psychology

REVIEWERS' EXPERTISE:

Reviewer #1 personality
Reviewer #2 personality

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

Thank you for the opportunity to review a revision of this manuscript.

Overall I was very satisfied with how the authors addressed my concerns and the thoughtful responses to my comments.

One small note with regards to my suggestion that motivation for change may account for some of the variability in outcomes in VPC and the authors' rebuttal that "However, there seems to be some contrary evidence in the domain of VPC, suggesting that autonomous goal motivation may not be a necessary condition for effective VPC (Hudson, 2021)."

I disagree with the rebuttal because it misunderstands the construct of autonomous motivation as reducible to "choice". In fact, autonomous motivation is conceptualized as finding an activity or goal enjoyable or interesting (intrinsic motivation), in line with important life values (identified motivation), and/or because one finds it to be important/valuable (identified motivation) (Ryan and Deci, 2017). Individuals often choose personal goals that do not align with their values or interests - they can choose goals because they think they "ought" to strive for something or feel pressured by others to want something (see Sheldon and Elliott's paper "Not all personal goals are personal"). Thus, choice is not an appropriate proxy for autonomous motivation.

I reviewed the Hudson (2021) study and I do not believe it provides "contrary evidence" of the importance of autonomous motivation in VPC. To my best understanding, in this study, students were randomly assigned to change trait conscientiousness or emotional stability, they did not choose which trait to change. However, as mentioned above, autonomous motivation is not the same thing as choice. It means that the person values this trait change or assigned behaviour, believes it to be somehow important or useful, and/or believes it to align with her other preferences or values. To my best understanding, the kinds of variables that capture autonomous motivation were not assessed in the study.

Given how valuable it is to be organized, hard-working, goal-directed (i.e., conscientious) for a whole host of achievement outcomes, it is likely that participants in the study felt highly autonomous about enhancing trait conscientiousness, even if they were randomly assigned to enhance this trait. Even if participants were not made consciously aware of changing the trait, they may have felt highly autonomous about engaging in tasks or prompts that helped them be more organized, hard-working and goal-directed because these seemed valuable or important tasks to them.

Thus, I see no contrary evidence that autonomous motivation is not an important predictor of VPC, and in fact, the existing evidence suggests it is an important mechanism.

Reviewer #2 (Remarks to the Author):

I thank the authors for their careful revision based on my and the other reviewer's suggestions. I believe the revised manuscript is much improved and have no further comments.

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Response Letter COMMSPSYCHOL-24-0319

REVIEWER EXPERTISE:

Reviewer #1 personality

Reviewer #2 personality

Reviewer #2 personality, systematic reviews and meta-analysis

REVIEWER REPORTS:

Reviewer #1 (Remarks to the Author):

I read the manuscript "A Systematic Review of Volitional Personality Change Research" with much interest. I liked how the authors summarized the different theoretical models on personality change research and that they conducted a meta-analysis on the efficacy of volitional personality change with and without supportive interventions. The writing was clear and the analyses were relatively straightforward to follow.

We thank the reviewer for the overall positive evaluation of our manuscript and for the constructive comments provided below.

Comment R1.1

I was not convinced by the opening argument that VPC is a critical lever with which to target public health. Studying personality change is interesting (perhaps in its own right) and VPC is related to all sorts of outcomes, but I think the focus on health as an outcome needs to be supported with more research and I was curious why the authors framed their introduction (and the paper generally) this way? In the results section it seemed that the strongest effects for VPC interventions were for traits like extraversion, and not for personality traits more saliently related to unhealthy behavior (e.g., neuroticism). If anything, that might be an argument for VPC to be a tool to counter the "loneliness pandemic". Thus, the focus in the introduction on health behaviour change as the ultimate outcome or rationale to care about VPC was hard for me to buy into.

We thank the reviewer for this comment. Our initial paper was framed around health, because personality traits predict a range of relevant health outcomes (Beck & Jackson, 2022; Friedman et al., 2014; Jokela et al., 2013, 2018; Kern & Friedman, 2011; Wright & Jackson, 2023), and because this paper was invited for a collection on behavioral change and health. However, based on this comment and the suggestions by the other reviewers, we comprehensively revised the framing of our manuscript.

First, we now mention that personality traits predict a range of relevant life outcomes, with health being one example. Furthermore, we included additional references to support the predictive effects of personality traits for these outcomes and refer to theoretical accounts that outline the causal assumptions underlying these effects.

Abstract (p. 1): "Personality traits predict a broad range of life outcomes, including relationship success, educational attainment, and health. As many people have the desire to change some aspects of their personality, volitional personality change (VPC) – self-directed trait changes in the direction of personal change goals – has recently gained increasing attention."

Introduction (p. 2): Personality traits – enduring patterns of thoughts, feelings, and behaviors – are powerful predictors of relevant outcomes across life domains^{1–8}. Traits such as neuroticism and conscientiousness have, for example, been linked to health behaviors, mental disorders, physical health problems, and mortality^{3,9,10}. Furthermore, personality traits predict relationship success, educational attainment, and well-being, with effect sizes comparable to those of other variables such as socio-economic status or cognitive abilities

^{4,11}.

Given this consistent evidence for the predictive effects of personality traits on relevant life outcomes, the question arises whether personality traits can be actionable targets to promote the attainment of these outcomes. As personality traits are theorized to have lasting influences on health, relationships, work, and other life domains^{3,8,12–17}, they may be useful targets for interventions designed to improve people's quality of life across domains⁵. Moreover, many people are intrinsically motivated to change their personality^{18,19}, highlighting the possibility

of volitional personality change (VPC) in the general population.

Theoretical Background (p. 3): “The latter idea is supported by emerging evidence that not only the levels of personality traits but also changes in personality traits are linked to important outcomes across life domains. For example, Wright and Jackson (2023) found that both levels of neuroticism as well as increases in neuroticism predicted a worse health status and unemployment several years later. Similarly, increases in neuroticism and decreases in conscientiousness have been found to predict physical health problems and mortality ^{31–33}.”

Discussion (p. 29): “Personality traits could be actionable targets for public interventions because they have been theorized to impact health, well-being, and relationships and many people are intrinsically motivated to change aspects of their personality ^{2,5,8,11,16,17,70,71}.”

Second, as also suggested by Reviewer 3, we briefly discuss the causality of these effects and tuned down our language regarding the health implications of VPC:

Theoretical Background (p. 3): “Although most studies did not investigate the causal effects of personality traits on these outcomes, the robustness of findings across samples and study designs led many researchers to conclude that facilitating desired personality changes could be a way to promote well-being, health, and work-related success in the general public ^{3,5,34,35}.”

Conclusion (p. 32): “Personality traits are associated with a broad range of relevant life outcomes, including health, well-being, and relationship success ^{1–8,74}. As many people want to change their personality traits ^{18,19}, the question arises whether VPC can be used to promote personal growth and the attainment of desired outcomes in the general population. Initial results on VPC are promising. Although change goals were only weakly related to personality changes without interventions, interventions aimed at facilitating VPC proved to be successful in inducing personality trait changes in the desired direction. These findings support theoretical models that highlight the possibility of VPC ^{20,21,35}. However, future research is needed to better understand when VPC is (not) successful, which intervention strategies are most effective, whether VPC can be replicated in different age groups and cultures, and whether VPC can facilitate the attainment of desired life outcomes.”

Third, in line with this comment, we now state explicitly that VPC is an interesting topic in its own regard, because many people want to change their personality.

Theoretical background (pp. 2-3): “This novel evidence raised the question of whether personality traits can be changed volitionally. VPC could help people to come closer to their ideal self, increase their self-satisfaction, and promote the attainment of desired life outcomes ^{5,27–29}.”

Discussion (p. 29): “Worldwide, about 60% of individuals want to change at least some aspects of their personality ¹⁸. The results of the present review

provide important insights into whether and how desired personality change can be successful.”

Comment R1.2

On page 11 I would include an additional interpretation for why there seems to be mixed evidence for VPC. Moore et al. (2021), a study you cite, showed that individuals made more change on traits they felt autonomously motivated to change, rather than controlled motivation to change. In other words, those individuals who reported feeling agentic, choiceful, and self-concordant about changing a trait - those who "wanted to change" (autonomous reasons) made more progress than those who felt pressured or coerced by others to change - those that felt like the "had to" change (controlled reasons). Much work by Ken Sheldon, Andy Elliot and Richard Koestner supports the fact that goal progress (across domains) is improved when the motivation for pursuing the goal is autonomous rather than controlled. The distinction between autonomous and controlled reasons for trait change was not considered in all the studies you included in the meta-analysis, but provides a possible explanation for the variability in findings and could be discussed here.

We thank the reviewer for this interesting suggestion. We agree that individual differences in goal motivation are a possible explanation for differences in findings across studies. However, there seems to be some contrary evidence in the domain of VPC, suggesting that autonomous goal motivation may not be a necessary condition for effective VPC (Hudson, 2021). We therefore decided to discuss this idea in the broader context of individual differences in VPC. Specifically, examining individual-level moderators (e.g., goal motivation) and study-level moderators (e.g., study duration) is an important direction for future research to better understand why VPC differs across people and studies.

Results (p.12): “Second, differences in sample composition (college students vs. general public), assessment characteristics, and study length may also explain some of the heterogeneity in effect sizes^{19,21}. For example, studies that found significant associations between change goals and VPC tended to examine these effects over shorter time intervals and in college samples^{27,47} compared to studies that found no significant effects^{18,48,49}. Third, individual differences across participants may explain why findings on VPC differ across studies and people. For example, goal motivation, beliefs about the changeability of personality traits, and self-concept clarity could be relevant moderators of VPC that deserve more attention in future research^{22,50,51}.”

Comment R1.3

On pages 18-19 you return to the issue of whether VPC can be used for health promotion. Again the evidence presented for this seemed weak and unconvincing. The "positive spillover" studies you described did not demonstrate a spill-over from personality trait change to health behaviour change. Rather you describe spill-over effects to increased life satisfaction, domain satisfaction, and self-esteem... these are all interesting and worthy outcomes, but do not relate to health promotion. Thus I am skeptical of the claim about VPC and health promotion (prevention)- both terms are used interchangeably throughout the manuscript...

We thank the reviewer for this comment. They raised an important point that the existing VPC literature did not yet examine whether VPC interventions lead to

changes in health relevant behaviors. Even though there is growing evidence that within-person changes in personality trait predict within-person changes in health behaviors (Jokela et al., 2018), investigating whether VPC may do so as well is an important direction for future research. To address this comment, we revised our implication section in multiple regards.

First, we more broadly discuss the theoretical and practical relevance of VPC across life domains and used more cautious wording to describe the health implications of VPC:

Discussion (pp. 29-31): “Worldwide, about 60% of individuals want to change at least some aspects of their personality¹⁸. The results of the present review provide important insights into whether and how desired personality change can be successful. In line with theoretical models on VPC^{20,21,35}, having the goal to change one’s personality predicted personality changes in desired directions, and people who wanted to change showed stronger personality changes than people without this desire (Figure 4). However, change goals alone may not be sufficient for lasting personality changes. Contemporary VPC theories suggest that lasting changes in trait-relevant behaviors, habits, and personality states are needed to establish personality changes, which may be achieved by targeting common change factors such as discrepancy awareness and strengths orientation^{20,21,35}. Our findings support these theoretical predictions, because successful interventions targeted personality at the state, habit, and trait level and because effective intervention strategies corresponded to theorized change factors (e.g., resource identification as a strategy to facilitate strengths orientation). Overall, existing evidence for VPC supports contemporary VPC theories.

The findings of the present review also support the idea that VPC may be practically relevant⁵. We found that interventions can facilitate lasting personality changes that are associated with changes in other life outcomes such as well-being and self-esteem^{23,29,65,66}. Given that personality changes predict a broad range of desired life outcomes, changes in personality traits could promote the attainment of these outcomes^{3,4,11,30}. For example, facilitating desired personality changes, such as increases in conscientiousness or decreases in neuroticism, could be a strategy to promote a range of favorable health outcomes like fewer health problems or lower mortality^{31–33}. The practical utility of intervention induced VPC is further supported by the fact that VPC seems to be possible with relatively mild and cost-effective interventions including digital smartphone applications²³. VPC interventions are thus less resource-intensive than other intervention strategies such as psychotherapy, underpinning that VPC could be used for health promotion in the general public.”

Second, we explicitly point to questions for future research that need to be addressed to better understand the practical relevance of VPC.

Abstract (p. 1): “Although future research is needed to draw definitive conclusions on the generalizability, mechanisms, and practical implications of VPC, the initial results on VPC are promising.”

Discussion (p. 30): “However, to fully understand the practical utility of VPC, future research is needed that provides a more direct test of whether VPC interventions can lead to measurable long-term effects in relevant life outcomes. Furthermore, change goals and VPC interventions were also sometimes associated with unwanted personality changes, underlining the necessity for future research on when and why VPC is successful and when it backfires.”

Conclusion (p. 32): “However, future research is needed to better understand when VPC is (not) successful, which intervention strategies are most effective, whether VPC can be replicated in different age groups and cultures, and whether VPC can facilitate the attainment of desired life outcomes.”

Reviewer #2 (Remarks to the Author):

This article presents a systematic review of the existing volitional personality change literature. I greatly enjoyed reading this manuscript. It is well-written and adds much to the field of volitional personality change. Below, I list several suggestions for improvement (though not necessarily in order of importance).

We thank the reviewer for taking the time to evaluate our manuscript and for the overall positive evaluation.

Comment R2.1

1. The emphasis on health in the introduction and outlook sections feels a bit out of place and tacked on. While I understand the logic (personality predicts increased health → interventions to increase personality traits lead to increased health), the review itself does not focus on health so a bookended emphasis on health doesn't make a lot of sense to me. It makes more sense to me to provide a solid mention in the 'practical implications' section and then focus on presenting a review that culminates in an increased understanding of whether people can or cannot accomplish their personality change goals – which is impactful in and of itself. Alternatively, you could include a discussion on why volitional personality change interventions (relative to personality change interventions that don't harness the power of intention) are particularly important in helping people improve their health. There is a vast health intervention literature that focuses on changing health behaviors and/or conscientiousness (see Magidson et al., 2014). What does VPC add? Moreover, a more explicit integration of health outcomes (psychological or physical) into your review would be helpful. You could add a research question that summarizes studies that include well-being as an outcome, for instance (see Hudson & Fraley, 2016).

We thank the reviewer for this comment. In line with the comments of the other reviewers, we reduced our overall focus on health and more broadly describe the predictive effects of personality traits. At the same time, we also used a more cautious wording when we describe the practical implications of VPC (please see Comment R1.1 for further details and examples of changes to the manuscript).

Furthermore, as suggested, we explicitly discuss the possible well-being implications of VPC.

Results (p. 21): “Finally, interventions aimed at facilitating VPC also seem to lead to positive spillover effects in other domains. For example, Allemand et al. (2024) and Olaru et al. (2023) found that the smartphone-app PEACH did not only lead to changes in personality traits but also to associated increases in life satisfaction, domain satisfaction, and self-esteem. These studies provide initial evidence that changing personality traits may indeed be beneficial for other life outcomes. However, future research should examine whether the effects of VPC interventions generalize to other outcomes such as physical or mental health.”

Discussion (p. 30): The findings of the present review also support the idea that VPC may be practically relevant⁵. We found that interventions can facilitate lasting personality changes that are associated with changes in other life outcomes such as well-being and self-esteem^{23,29,65,66}. Given that personality changes predict a broad range of desired life outcomes, changes in personality traits could promote the attainment of these outcomes^{3,4,11,30}.

Comment R2.2

2. Relatedly, with your current focus on health, how do you reconcile some review findings that indicate VPC may make it harder to improve health (i.e., that VPC to increase C leads to lower C)?

As we reduced our overall focus on health, this comment may not be as relevant to revised version anymore as it was to the original submission. We nonetheless briefly mention the possibility of unwanted personality changes when discussing the practical relevance of VPC.

Discussion (p. 30): “However, to fully understand the practical utility of VPC, future research is needed that provides a more direct test of whether VPC interventions can lead to measurable long-term effects in relevant life outcomes. Furthermore, change goals and VPC interventions were also sometimes associated with unwanted personality changes, underlining the necessity for future research on when and why VPC is successful and when it backfires.”

Comment R2.3

3. How does your review account for the bias of a large number of studies coming from one lab (i.e., Hudson et al.)?

We thank the reviewer for this comment. We handled the potential bias resulting from having a large number of studies coming from a single lab in multiple ways. First, in the narrative review, we explicitly paid attention to consider studies from different labs and for the Hudson-lab we focused on their mega-analytic aggregation that integrated findings across different primary studies. Second, in the *ShinyApp*, we included the sorting variable “similar design” which allows to gain a better overview of similarities and differences across studies from different labs. Third, in the meta-analysis, we ensured that we only included non-redundant samples of the Hudson-lab, and within studies we used the three-step procedure recommended by Pustejovsky and Tipton (2022) to deal with the dependency of effect sizes.

Comment R2.4

4. I think this review misses several consistently asked questions in the VPC literature and thus misses an opportunity to provide the field with a more comprehensive review. Additional questions could include (but aren't limited to): what is the prevalence of personality change goals across studies (are there methodological, sample, etc. moderators)? What traits do people want to change? What are the relations between trait goals and corresponding current trait levels? Providing answers to these questions in this review will help the field move forward and answer other, novel questions.

We thank the reviewer for this comment and for their ideas for additions to our review. As often the case with reviews, there are many interesting questions that one could address. However, from our perspective, the additions suggested by the reviewer do not fit the focus of the present review as they rather refer to cross-sectional questions related to the prevalence and correlates of personality change goals but not to longitudinal examinations of volitional personality change. From our point of view, our preregistered research questions do comprehensively cover the research questions asked in the literature on longitudinal examinations of VPC (e.g., is VPC possible, is VPC lasting, are there spill-over effects to other domains, how can interventions for VPC be designed).

Furthermore, we are aware that a working group of the Personality Intervention Network (<https://personalityinterventions.com/>) is currently preparing a systematic review on change goals that is intended to cover the questions that are mentioned in this comment. Thus, we would like to keep the scope of the present review as it is.

Reviewer #3 (Remarks to the Author):

I really like the idea of the manuscript because research on volitional personality change (VPC) is promising for understanding personality change. And I think it is good to review and meta-analytically integrate the existing evidence. Here are some further strengths and potential issues that I noticed:

We thank the reviewer for the overall positive evaluation of our manuscript and the detailed and constructive comments for further improvement.

Comment R3.1

1. At several places in the manuscript the authors make the implicit or explicit argument that personality traits cause health. For example, in the first sentence of the abstract, they write "Personality traits are of critical importance for individual and public health". (If personality traits would only predict but not cause health, they would not be "critically important") Or in the first sentence of the conclusion, they write "Personality traits have important implications for public and individual health". The same argument is also made in the Practical Implications section. As far as I know and I saw from a look at the publications the authors cited, there are many studies that found links between personality traits and health, but there is no solid causally informative research on the effects of personality on health. It does, in my opinion, not make a big difference whether personality traits or changes in personality traits predict health-related outcomes, both kinds of associations can be confounded by third variables (e.g., genetics, changes in income or other psychological traits such as personal values). Even a time lag does not provide solid evidence for a

causal effect because the effect of the confounder on health might come with a bigger time lag than the effect of the confounder on personality. Furthermore, most of these previous studies did not explicitly and systematically attempt to estimate causal effects of personality on health and thus their design and analysis methods were not tailored towards addressing causal research questions (e.g., Grosz et al., 2020; Loh & Ren, 2023; Lundberg et al., 2021; Mulder et al., 2024). I think the authors of the current manuscript should make it explicit that their argument rests on the assumption that personality causes health. But at the same time, I think they should also point out that it is not clear yet whether personality actually causes health.

We thank the reviewer for this important comment. In line with the suggestion of the other two reviewers, we comprehensively revised the framing of our introduction and other sections of the manuscript.

Specifically, we reduced the focus on health and embedded findings in the broader context of personality prediction (see our response to Comment R1.1 for details and changes to the manuscript).

Furthermore, as suggested by the reviewer, we briefly discuss the causal assumption underlying these effects and refer to relevant theoretical models. While an extended discussion of causality is beyond the scope of the manuscript, we think that these brief references should be sufficient to alert the readers of the assumptions that we make.

Theoretical background (p. 4): “This latter idea is supported by emerging evidence that not only the levels of personality traits but also changes in personality traits are linked to important outcomes across life domains. For example, Wright and Jackson (2023) found that both levels of neuroticism as well as increases in neuroticism predicted a worse health status and unemployment several years later. Similarly, increases in neuroticism and decreases in conscientiousness have been found to predict physical health problems and mortality^{31–33}. Although most studies did not investigate the causal effects of personality traits on these outcomes, the robustness of findings across samples and study designs led many researchers to conclude that facilitating desired personality changes could be a way to, either directly or indirectly, promote well-being, health, and work-related success in the general public^{3,5,34,35}.”

More generally, we revised our wording throughout the entire manuscript and stress the predictive effects of personality traits (which are well established) instead of more abstract claims on “the critical relevance” of personality traits.

Finally, as also suggested by Reviewer 1, we now introduce VPC as an interesting topic in its own regard because many people want to change their personality.

Theoretical background (pp. 2-3): “Over the past decades, however, it has become evident that personality traits possess both stable and malleable properties across the lifespan^{12,26}. This novel evidence raised the question of whether personality traits can be changed volitionally. VPC could help people to come closer to their ideal self, increase their self-satisfaction, and promote the attainment of desired life outcomes^{5,27–29}.”

Comment R3.2

2. Relatedly, the authors wrote on page 2 “Personality traits are stable albeit malleable across the lifespan and are theorized to have lasting influences on important life outcomes (8,9)” the references 8 and 9 (Bleidorn et al., 2022; Roberts et al., 2017) are meta-analyses on personality change. They do not propose theories about the effects of personality on important life outcomes. Hence, I would replace references 8 and 9 with papers about the impact of personality on life outcomes such as health (e.g., Friedman et al., 2014; Shanahan et al., 2014).

We thank the reviewer for paying attention to these details and for providing two helpful references. Our initial references were indeed only referring to the first part of the sentences that personality traits are both stable and malleable. We now added the suggested references and additional ones to support that personality traits are relevant for important life outcomes.

Comment R3.3

3. I think it would be good to include the number of VPC intervention studies in the abstract to provide a clearer overview.

We included the number of studies and effect sizes in the Abstract:

“However, VPC interventions were successful in promoting desired personality changes ($d = 0.22$, 95% CI = [0.005, 0.433], 7 studies, 26 effect sizes).”

Comment R3.4

4. The summary of theoretical perspectives on pages 4 and 5 is well-written and insightful.

We appreciate this positive feedback!

Comment R3.5

5. I would remove “health” and the causal effect of volitional personality change on health from Figure 1 because these elements of the figure are not central features of the three outlined theories on VPC.

We revised the figure as suggested.

Comment R3.6

6. Relatedly, I do not agree with the following statement “According to VPC theories, people should be able to intentionally change their personality traits in a desired direction and these changes should have positive consequences for individual and public health (13,25).” I checked reference 13 and 25 (Hennecke et al., 2014; Allemand & Flückiger, 2017). The VPC theories they proposed do not focus on the effects on individual and public health.

We thank the reviewer for this comment. Both theories state that VPC is important because personality traits predict important life outcomes, with maintaining or promoting health being one example (e.g., Hennecke et al., 2014, pp. 292-293: “In addition, coping with health declines become an important new goal in later life. Indeed, some of the traits that are higher in older adults than in younger adults,

namely conscientiousness and emotional stability, have positive effects on health-relevant behaviours”). However, the reviewer is right that these theories do not focus exclusively on the benefits of VPC for health. Hence, we revised the according sentences to describe the relevance of VPC more generally. Furthermore, we included Bleidorn et al. (2019) as additional reference because in that paper the public policy relevance of personality changes is described in detail.

“According to VPC theories, people should be able to intentionally change their personality traits in a desired direction and these changes could facilitate the attainment of important life outcomes 5,20,35”

Comment R3.7

7. “Hence, the goal of this systematic review was to summarize and integrate the emerging VPC literature and discuss directions future research.” I think the word “for” is missing before “future research”

Corrected.

Comment R3.8

8. I am in favor of preregistrations. However, the authors did not preregister any hypotheses. So, it is unclear to me what the purpose of the preregistration was. Preregistrations help us to do confirmatory research by preregistering hypotheses and the way we test them (e.g., Nosek & Lindsay, 2018). But if we do not have any hypotheses, then the research is exploratory rather than confirmatory and we do not need to preregister anything. It is also not necessary to point out deviations from the preregistration because the findings are exploratory regardless of whether the authors deviated from the preregistration or not.

We appreciate the reviewer’s thoughts on our preregistration. However, we disagree with this description of the purpose and necessity of preregistrations in exploratory research. Even if preregistrations are particularly important in context of hypothesis testing, they are nonetheless essential to endorse scientific rigor even without explicit hypotheses. For example, preregistrations prevent HARKing (hypothesizing after results are known) in both cases. As we preregistered our project, it is clear to the reader that we did not add any hypotheses about VPC after inspecting our findings. Furthermore, by preregistering a precise analysis plan, it is clear that we did not *p-hack* to come to our conclusions. That is, any findings we present are unbiased choices and not the result of exploiting researcher degrees of freedom. We thus kept the preregistration and transparently described where we deviated from the preregistered protocol.

Comment R3.9

9. Literature search: I really liked that the authors did a forward search for each publication identified via other search strategies. In my experience, the forward search is particularly helpful in finding additional relevant publications.

We indeed identified several additional studies in this forward search.

Comment R3.10

10. Literature search: The authors only screened the reference of two review articles. It might be good if the authors additionally screened all the reference sections of

publications identified via other search strategies because there might be some older studies that investigated VPC but were not found with the other search strategies.

We thank the reviewer for this suggestion. We now screened the reference lists of all eligible studies. Using this procedure we identified one additional relevant study (Hudson et al., 2021). However, this study largely overlaps with other studies from this lab – specifically the mega-analysis (Hudson et al., 2020) so that it did not alter any of our conclusions. We nonetheless included it in our review to be as comprehensive as possible.

Furthermore, we updated our database literature search to check whether other recent research fulfilled our inclusion criteria. However, this was not the case.

Comment R3.11

11. Page 6: When I first read the fifth inclusion criterion, I thought studies were only included if they were interventions. Only later, I realized that they meant the study needs to assess personality before and after the intervention if it was an intervention study. I think it would be good if the authors would revise how they describe their fifth inclusion criterion.

We revised the description of this exclusion criterion to make our exclusion processes clearer:

Methods (p. 7): “Studies fulfilling the following criteria were included in the systematic review: (1) empirical, quantitative study with human subjects, (2) written in English, (3) longitudinal personality assessment, (4) examination of VPC in line with the above presented definition. Additionally, if a study examined an intervention, the study had to have a prospective design (i.e., at least one personality assessment before and after assessment after the intervention).”

Comment R3.12

12. Given that the authors only included 29 studies, I think it would have been good that each article gets coded (Step 3) by two coders. The overall average of 94% was high but I think it would be good to make sure that all the information is extracted correctly.

We thank the reviewer for this suggestion. We agree that having all studies double-coded is a good idea to increase the trustworthiness of our findings. For the revision, the data extraction of all included studies was now done by two independent coders (i.e., the same coders involved in the initial double-coding process). This additional double-coding changed the agreement rate (increased slightly from 94% to 95%) and identified one coding error in our effect size extraction (see also Comment R3.19). Furthermore, the discussion of divergent codings resulted in some minor changes in the extracted data on intervention characteristics (see Table 3).

Comment R3.13

13. The descriptive results are interesting because they highlight gaps in the research, such as the need for more VPC studies involving older populations.

We agree!

Comment R3.14

14. I like the illustration of the typical research designs in Figures 3A and 3B. However, the research questions stated in the graphs could be better aligned with the theoretical and practical goals of the VPC literature. I would recommend the research question “Do personality change goals cause personality changes?” over the question “Do personality change goals predict personality changes?”. If change goals would only predict but not cause personality change, then change goals would not be very theoretically and practically relevant because some third variable (e.g., sex, genetics, income, personal values) would cause change goals and personality change. If change goals would only predict personality change, then theories should be about these third variables rather than about change goals. I think the authors ended up with the term “predict” because they aligned the research question to the question that can be readily answered with the designs and statistical analyses methods that are commonly used in VPC research. However, design and analysis methods should not determine the research question. The theory should determine the research question, and the research question should determine the design and analysis methods (e.g., Lundberg et al., 2021). Of course, once you explicitly state the actual research question of interest (“Do personality change goals cause personality changes?”), you need to discuss the assumptions that need to hold so that the statistical estimand (e.g., regression coefficient) can inform the theoretical estimand (research question). Similarly, for Figure 3b, I would recommend the research question “Do personality change intervention causes personality changes?” over “Do personality traits differ before and after the intervention?”. Testing whether personality traits differ before and after the intervention is one attempt to investigate the causal research question rather than the research question itself.

We followed the suggestion of the reviewer and revised the questions mentioned in the figures so that they better reflect the assumptions articulated in contemporary theoretical models (e.g., “Do change goals explain personality changes?” and “Do VPC interventions lead to personality changes?”).

Comment R3.15

15. At the beginning of the subchapter “Findings on How Change Goals are Related to Volitional Personality Change“, I would clarify whether the review is narrative or includes quantitative aggregation (e.g., meta-analysis). Additionally, I would include a graph showing the average effect size for each Big Five trait separately.

We thank the reviewer for this suggestion. We included a brief statement on how we summarized findings on VPC without intervention:

Results (p. 11): “We summarized findings on VPC without interventions in a narrative review as studies differed substantially in their way of quantifying these effects.”

Furthermore, as suggested, we included a graph illustrating the association between change goals and personality changes separately for the different Big Five traits (Figure 4).

Comment R3.16

16. “As illustrated in Figure 4, changes in personality traits over three months were

almost three-times stronger for people who wanted to change their personality traits ($d = 0.14$) compared to normative personality changes over the same time interval ($d = 0.05$).” I was not sure how the authors came to these effect sizes ($d = 0.14$; $d = 0.05$) and whether they are comparable to the pre-post difference d (dz ; e.g., Lakens, 2013) that is reported for VPC interventions. It would be great if the authors would clarify these points.

We thank the reviewer for this comment. We now provide more details on how we calculated these effect sizes in the figure caption and in the supplemental material (Section 6). Furthermore, we discuss the comparability of the different effect sizes in more detail. Generally, all effect sizes are standardized pre-post differences so that they should be comparable in this regard. However, they stem from different publications with different inclusion criteria and different methods to aggregate effect sizes, which should be kept in mind when comparing these effect sizes.

Figure 5 (p. 17): “This figure illustrates effect sizes of normative personality changes ¹² (effect size calculated based on Model 2 of Table 7), volitional personality change without intervention ⁴¹ (effect size calculated based on bottom half of Table 2), intervention-induced volitional personality change, and personality changes in context of psychotherapy ¹³ (effect size mentioned in abstract). More details on the calculation of these effect sizes can be found in Section 6 of the supplemental material. The blue distribution illustrates personality trait scores at the reference timepoint, whereas the red distribution illustrates scores for the second timepoint when change was measured. The peak of each distribution represents its mean value, with the x-axis in standard deviation units. As all effect sizes refer to pre-post comparisons, they are in principle comparable to each other. However, effect size estimates were derived from different publications relying on different inclusion criteria and different ways to aggregate effect sizes. VPC = volitional personality change.”

Furthermore, we corrected a typo in calculating the effect size of normative personality change, resulting in a slight change in the depicted effect size ($d = 0.04$ instead of $d = 0.05$).

Comment R3.17

17. I like the classification of the interventions according to the taxonomy distinguishing relevant intervention strategies, levels, and aspects of intervention targets.

We appreciate this positive feedback!

Comment R3.18

18. When the authors calculated the effect size pre-post difference d (dz), they used the bias correction factor c . It would be great if the authors could explain why they used this correction and cite a methods paper about it.

We thank the reviewer for this comment. The bias correction factor is included to obtain an unbiased estimation of the population effect size (e.g., Hedge’s g instead of Cohen’s d ; Becker, 1988). This bias correction factor is routinely included in contemporary meta-analytic software (e.g., Viechtbauer et al., 2010) although the correction is in fact negligible for sample sizes of $N > 10$ (Morris, 2008). We briefly

mention our rationale for including the bias correction factor and cite according to references in the manuscript:

Results (p. 18): “Our effect size of interest was the standardized mean difference of the targeted personality trait before the intervention (M_{Pre}) and after the intervention (M_{Post}) with bias correction factor c to account for overestimation of the population effect size ^{58–60}”

Comment R3.19

19. Figure 5 and the first funnel plot in the supplemental material suggest that one effect (probably the agreeableness effect in Stieger et al., 2021) is an outlier. I would double-check whether this effect size is coded and computed correctly.

We thank the reviewer for this comment. Indeed, there was a coding error in the effect size extraction ($M = 3.48$ instead of $M = 3.84$), which we have missed in the initial quality checks of our data extraction. This coding error is now corrected.

Comment R3.20

20. Currently, the authors only present the meta-analytic effect sizes for extraversion and conscientiousness in the main text. The meta-analytic effect sizes for the other traits and the confidence intervals are only presented in Section 3 of the supplemental material. I think it would be good to present the meta-analytic effect sizes for all five Big Five traits and their confidence intervals in the main document.

We have added the requested information to the manuscript.

Comment R3.21

21. “These studies provide initial evidence that changing personality traits may indeed be beneficial for other life domains.” I found it a bit odd to write that changing personality traits may benefit other life domains because personality traits are NOT a life domain such as health, relationships, or career. This is also an issue in the following sentence of the summary “Furthermore, the effects seem to be lasting and generalize to other life domains.”

We have revised the sentences to make our writing clearer.

e.g., “Furthermore, the effects seem to be lasting and generalize to other constructs like well-being.”

Comment R3.22

22. I think it is inappropriate that the practical implications section is about the effects of VPC and VPC interventions on health even though the effects on health were not the focus of the current review. Relatedly, I think it is problematic that the authors state that “VPC could be used for health promotion in the general public” because it is unclear whether personality traits have a causal effect on health and whether VPC interventions have a causal effect on health. Even if VPC interventions affect health, VPC interventions might have a causal effect on health via other pathways than through personality change because such an intervention might change not only the targeted personality trait but also other psychological and environmental factors. By the way, it would be important to do more research on the potential risks and

negative effects of VPC interventions. Moreover, as the authors point out, we do not yet understand what intervention characteristics and strategies make VPC interventions effective. Finally, it is unclear how generalizable and context dependent the efficacy of VPC intervention is. Hence, we need more research on VPC interventions before recommending them as health promotion in the general public.

We thank the reviewer for raising this important point. We comprehensively revised the practical implication section. This section now includes a broader discussion of the theoretical and practical implications of our review. Furthermore, we are more cautious in our wording regarding possible health implications of VPC. Finally, we describe important directions for future research that need to be addressed to better understand the practical relevance of VPC.

Please see our responses to Comments R1.1 and Comment R1.3 for further details.

Hope that helps,
Michael P. Grosz

Publications not cited in the manuscript:

Friedman, H. S., Kern, M. L., Hampson, S. E., & Duckworth, A. L. (2014). A new life-span approach to conscientiousness and health: combining the pieces of the causal puzzle. *Developmental psychology*, 50(5), 1377.

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Reviewer #1 personality
Reviewer #2 personality

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

Thank you for the opportunity to review a revision of this manuscript.

Overall I was very satisfied with how the authors addressed my concerns and the thoughtful responses to my comments.

One small note with regards to my suggestion that motivation for change may account for some of the variability in outcomes in VPC and the authors' rebuttal that "However, there seems to be some contrary evidence in the domain of VPC, suggesting that autonomous goal motivation may not be a necessary condition for effective VPC (Hudson, 2021)."

I disagree with the rebuttal because it misunderstands the construct of autonomous motivation as reducible to "choice". In fact, autonomous motivation is conceptualized as finding an activity or goal enjoyable or interesting (intrinsic motivation), in line with important life values (identified motivation), and/or because one finds it to be important/valuable (identified motivation) (Ryan and Deci, 2017). Individuals often choose personal goals that do not align with their values or interests - they can choose goals because they think they "ought" to strive for something or feel pressured by others to want something (see Sheldon and Elliott's paper "Not all personal goals are personal"). Thus, choice is not an appropriate proxy for autonomous motivation.

I reviewed the Hudson (2021) study and I do not believe it provides "contrary evidence" of the importance of autonomous motivation in VPC. To my best understanding, in this study, students were randomly assigned to change trait conscientiousness or emotional stability, they did not choose which trait to change. However, as mentioned above, autonomous motivation is not the same thing as choice. It means that the person values this trait change or assigned behaviour, believes it to be somehow important or useful, and/or believes it to align with her other preferences or values. To my best understanding, the kinds of variables that capture autonomous motivation were not assessed in the study .

Given how valuable it is to be organized, hard-working, goal-directed (i.e., conscientious) for a whole host of achievement outcomes, it is likely that participants in the study felt highly autonomous about enhancing trait conscientiousness, even if they were randomly assigned to enhance this trait. Even if participants were not made consciously aware of changing the trait, they may have felt highly autonomous about engaging in tasks or prompts that helped them be more organized, hardworking and goal-directed because these seemed valuable or important tasks to them.

Thus, I see no contrary evidence that autonomous motivation is not an important

predictor of VPC, and in fact, the existing evidence suggests it is an important mechanism.

We thank the reviewer for their time in reviewing our work and the detailed thoughts about the difference between autonomous goal motivation and choice. We agree that the study by Hudson (2021) did not directly test autonomous goal motivation and thus provides only indirect evidence on the role of motivation in VPC. Based on the reviewer's suggestion, we revised the according sentence of our manuscript:

“Third, individual differences across participants may explain why findings on VPC differ across studies and people. For example, beliefs about the changeability of personality traits, goal motivation (e.g., autonomous vs. controlled), and self-concept clarity could be relevant moderators of VPC that deserve more attention in future research 22,50,51.”

Reviewer #2 (Remarks to the Author):

I thank the authors for their careful revision based on my and the other reviewer's suggestions. I believe the revised manuscript is much improved and have no further comments.

We thank the reviewer for their time and constructive comments in revising our work.