PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Prevention of Psychosocial Distress Consequences in Somatic Hospital Inpatients via a Stepped and Collaborative Care Model: Protocol of SomPsyNet, a Stepped-Wedge Cluster Randomised Trial
AUTHORS	Meinlschmidt, Gunther; Frick, Alexander; Baenteli, Iris; Karpf, Christina; Studer, Anja; Bachmann, Marco; Dörner, Andreas; Tschudin, Sibil; Trost, Sarah; Wyss, Kaspar; Fink, Günther; Schwenkglenks, Matthias; Caviezel, Seraina; Rocco, Tabea; Schaefert, Rainer; SomPsyNet, consortium

VERSION 1 – REVIEW

REVIEWER	Baliousis, Michael University of Lincoln
REVIEW RETURNED	24-Jun-2023

GENERAL COMMENTS	This seems to be an important study and a meticulously designed trial with robust methodology. There are some downsides to the narrative which introduce confusion, and which will need clarifying. My biggest question regarding suitability for publication as protocol arises from the indications that this study seems to have completed recruitment and the only thing that's outstanding is the long term follow up of health insurance data. This means that primary endpoints such as distress at the first follow up may have even been analysed. The use of past tense in many parts of the protocol adds to these questions. If the first part of the study has indeed been completed this would not be appropriate for publication as a protocol. It may then be appropriate to publish the outstanding parts of the study, i.e. the long term follow up on health insurance data, as a protocol for that research question, specifying this as an aim (without involving distress, etc.). These points require further clarification to determine the suitability of this ms for publication as protocol. Regarding the protocol itself, it develops a strong rationale for the study and its importance. However, a key limitation is the structure of the writing and the need for further editing throughout, with attention to grammar, syntax, and coherence. For example, the aims in the abstract need restructuring: what is being evaluated in relation to the screening procedure (does the implementation refer to screening?), what does “burden to population” mean, and what aspects of efficacy are being evaluated (are improvement of mental health and quality of life part of it)? There is a lot of confusion about the purpose or various aspects of the design, for

	example phase one is described as control but later it seems to function as a pilot whilst it's role as control returns in the supplements. Numerous statements require references. I have made additional specific comments in the uploaded in text (which often apply to other sections also). The statistical analysis requires further detail in the main body and in the supplement (apart from missing references in the main ms). Specifically, it is good practice to decide where to expend degrees of freedom a priori otherwise models will end up being overfitted and not representative with the study's far from huge sample (group of distressed participants in low hundreds) so the specification of the regression models needs some expansion in the protocol/supplements with consideration for power. I understand that sample size cannot change now so this is in the interest of transparency (especially as estimating power for multilevel models is not straightforward so that it is better to say so than provide an estimate which is for a different type of analysis and which has, therefore, questionable relevance). I have made specific comments in the uploaded annotated copy also. There is some inconsistency in the font formatting in the abstract.

REVIEWER	von der Warth, Rieka Albert-Ludwigs-Universitat Freiburg, Institute of Medical Biometry and Statistics
REVIEW RETURNED	26-Jun-2023

GENERAL COMMENTS	Dear Authors, thank you for having me review your paper and for conducting this important study. Even though I read your study protocol with interest, it is somewhat hard to follow as information are not where I would have expected it. I understand, that there is a conflict between describing those kind of complex studies and a precise manuscript, one should be careful that a manuscript is still understandable without reading all supplements (which in this case are extremely detailed). 1. There is a lack of clarity in your wording. The title of the study included "psychosocial distress", while you address "mental disorders" in your abstract and in the background. Psychosocial distress and mental disorders however, are two different concepts and should be differentiated. This is of special relevance, as you, as far as I understand; provide no treatment for mental disorders but counseling. Two further concepts you speak of, but not distinguish, are mental-somatic mutlimorbidity and mental-somatic comorbidity. Please, stick with one word or explain why both should be included in your protocol. 2. Within the abstract you speak of household costs? It gives the impression that you only assess costs on patient level. Please explain. 3. Introduction: Please consider re-writing, as you jump in the topics. Also, you concentrate on mental disorders in your introduction, which is confusing as you don´t assess mental disorders (no clinical interview). How do you define psychosocial distress? 4. Please update your references. I see, that you use inter alia Lehnert et al., 2011 and Hochlehner er al, 2011, which are highly relevant papers in this area, but there are studies that are more recent, e.g.:

	a. Jansen L, van Schijndel M, van Waarde J, van Busschbach J. Health-economic outcomes in hospital patients with medical-psychiatric comorbidity: A systematic review and meta-analysis. PLoS One. 2018 Mar 13;13(3):e0194029. doi: 10.1371/journal.pone.0194029. b. Wolff J, Heister T, Normann C, Kaier K. Hospital costs associated with psychiatric comorbidities: a retrospective study. BMC Health Serv Res. 2018 Jan 30;18(1):67. doi: 10.1186/s12913-018-2892-5. PMID: 29382387; PMCID: PMC5791176. c. von der Warth, R., Hehn, P., Wolff, J. et al. Hospital costs associated with post-traumatic stress disorder in somatic patients: a retrospective study. Health Econ Rev 10, 23 (2020). https://doi.org/10.1186/s13561-020-00281-0 5. current standard intervention options: In this section I would have expected an explanation of TAU in Switzerland at the moment. 6. Please check all abbreviations: I found some, that are used before the abbreviation is introduces (e.g. CL) 7. You state, that you expect SomPsyNet to benefit staff and stakeholders. How do you assess that information? I would have expected a process evaluation or similar. 8. Risk category and rationale: This whole paragraph is not understandable if you don´t live in Switzerland. Please elaborate, why this statement is important. 9. P.10, L. 54 – why are not all stakeholder translated (they are in the affiliations) 10. The overall phases of the study are not clear to me. I can see from Suppl. 3, that all 21 sites started at the same time with phase 0, why are some sites longer in phase 2 – are those the triplets you formed? Also, what is meant by step0-4 and t0-t4? You only speak of it in the Supplementary material, but how do they relate to the phases? 11. How where patient enrolled into the study? Where all patients asked to participate, except they met an exclusion criteria? How did you detect psychosocial distress? In Figure 1 you speak of a threshold – how was this threshold defined? In Supp. You stat that the cut-off values were pre-defined, but I can´t find them. Also, why was the agreement of the physician so important? 12. When were patient enrolled into the study? Right after hospital admission? Were the questionnaire self-administrated or as an interview? 13. In your exclusion criteria you state, that the study was tailored to other languages – which one? 14. Figure 1 is quite hard to read. Please consider at least changing font color to make the figure more accessible. 15. Why were the wards divided into up to three parts, when all sites were included into the study at the same time? 16. “The step-by-step implementation was also appreciated by the staff and stakeholders”? How do you know? Also, which stakeholders are meant? 17. P.13: You have two following sentences saying “According to the extension of the Consort statement”. Please re-phase. Overall, I would recommend a proofreading as some sentences and phrases sound incorrect. 18. Intervention: Was the personnel solely study personnel or the TAU personnel from the psychiatric ward? 19. Intervention: What are example contents of the intervention? (This information is somewhat given in German as part of the

	supplemental material, but I miss it as part of the main manuscript or at least in English). 20. I don´t understand why you need the focus sample; please explain further. (You sometimes call them focus groups, which made me expect a qualitative study part). 21. I understand that you tried to keep your study protocol short to make it readable, but that lead to missing information in the text, which are only given in the supplement. For instance, please elaborate a little more on the statistical analyses in the main text. 22. You have two sections called dissemination? 23. Just from the text it was hard to understand, why recruitment ended in 2022, the follow up is after six months but data collection will end in June 2026. Measurement points for all assessments including claims data should be stated in the text. Minor mistakes: • Within your manuscript you tend to switch fonts quite often. Please correct • P.5, L. 40: claims data • P-12, L. 6: please speak of gender affirming interventions or gender affirming care

REVIEWER	Demirer, Ibrahim University Hospital Cologne, Institute of Medical Sociology, Health Services Research, and Rehabilitation Science
REVIEW RETURNED	11-Jul-2023

GENERAL COMMENTS	Overall, I found your protocol intuitive to read and informative on the main subjects. You also provide rich information in the supplemental materials. My only concern lies in the quite large interval and in the sample size at phase 0 with 200-500. The assumed Hedge's G of 0.5 seems fairly high, given the heterogeneity. You also assume a drop-out rate of 25%, however, it is unclear to me, does these 25% also contain missing values and attrition? Furthermore, I would suggest adding more information on the confounding variables. E.g., what are the confounders relevant for the primary, and what are the confounders relevant for the secondary endpoints? Which confounders may remain unmeasured? What are potential mediators of the intervention that should/could be measured? - Such that the risk of bias can be assessed.

VERSION 1 – AUTHOR RESPONSE

Responses to comments from Reviewer 1 (Dr. Michael Baliousis, University of Lincoln)

Reviewer 1 comment 1:
This seems to be an important study and a meticulously designed trial with robust methodology. There are some downsides to the narrative which introduce confusion, and which will need clarifying.

Response:
Thank you for recognizing the importance and meticulous design of our study. We appreciate your feedback on the narrative, and we have taken steps to improve clarity throughout the manuscript to

address the confusion you pointed out.

Reviewer 1 comment 2:
My biggest question regarding suitability for publication as protocol arises from the indications that this study seems to have completed recruitment and the only thing that's outstanding is the long term follow up of health insurance data. This means that primary endpoints such as distress at the first follow up may have even been analysed. The use of past tense in many parts of the protocol adds to these questions. If the first part of the study has indeed been completed this would not be appropriate for publication as a protocol. It may then be appropriate to publish the outstanding parts of the study, i.e. the long term follow up on health insurance data, as a protocol for that research question, specifying this as an aim (without involving distress, etc.). These points require further clarification to determine the suitability of this ms for publication as protocol.

Response:
We apologize for any confusion caused. We understand your concerns regarding the state of recruitment and follow-ups in the study. We would like to reassure you that the trial is still ongoing, and none of the main endpoints has been analysed yet. The use of past tense may inadvertently have contributed to this confusion. We have revised the manuscript accordingly. Of note, the status of the study at time of submission (recruitment completed but data collection, including primary outcome data, ongoing) is in line with the criteria for suitability for publication as protocol in BMJ open.

Reviewer 1 comment 3:
Regarding the protocol itself, it develops a strong rationale for the study and its importance. However, a key limitation is the structure of the writing and the need for further editing throughout, with attention to grammar, syntax, and coherence. For example, the aims in the abstract need restructuring: what is being evaluated in relation to the screening procedure (does the implementation refer to screening?), what does „burden to population" mean, and what aspects of efficacy are being evaluated (are improvement of mental health and quality of life part of it)? There is a lot of confusion about the purpose or various aspects of the design, for example phase one is described as control but later it seems to function as a pilot whilst its role as control returns in the supplements. Numerous statements require references.

Response:
Thank you for your detailed feedback on the structure and content. We have undertaken a comprehensive review of the manuscript, with particular emphasis on the sections you pointed out:
- The aims in the abstract have been restructured for clarity.
- We have clarified the role and purpose of phase 1 and phase 0 in the study, ensuring consistent descriptions throughout the manuscript.
- Necessary references have been added where statements lacked them.
↳ See abstract and manuscript throughout.

Reviewer 1 comment 4:
The statistical analysis requires further detail in the main body and in the supplement (apart from missing references in the main ms). Specifically, it is good practice to decide where to expend degrees of freedom a priori otherwise models will end up being overfitted and not representative with the study's far from huge sample (group of distressed participants in low hundreds) so the specification of the regression models needs some expansion in the protocol/supplements with consideration for power. I understand that sample size cannot change now so this is in the interest of

transparency (especially as estimating power for multilevel models is not straightforward so that it is better to say so than provide an estimate which is for a different type of analysis and which has, therefore, questionable relevance).

Response:
We value your insights on the statistical analysis. We have expanded the section to provide:
- A detailed specification of the regression models to be used.
⌊ See page 20: “To estimate intervention effects, we will primarily conduct generalised linear mixed models of primary, secondary, and other outcome parameters adjusted for the clusters as random effects and for study conditions, calendar time, and potential confounders (e. g., gender, age categories, socioeconomic status) as fixed effects.”

- An acknowledgement of the complexities related to estimating power for multilevel models, with a rationale for our methodological choices. In the revised Methods section, we write:
⌊ See page 20: “Sample size calculations were undertaken, focusing on the primary endpoint, which was the change from baseline of the Mental Health Component Summary score, as gauged by the SF-36 questionnaire. Assuming an effect size of 0.5 SD and an additional 55% of patients received mental health support in the intervention arm, 208 patients were needed in each treatment condition. To allow for attrition as well as clustering of outcomes, we aimed for a sample of 300 distressed patients in each arm. Power calculations were originally made using basic two-arm clustered comparisons and verified using power simulations implemented in Stata.”

- Additional references to support our analytical decisions are provided in the Methods section as well as in the Supplemental material 9.
⌊ See Methods section and Supplemental material 9.

Reviewer 1 comment 5:
There is some inconsistency in the font formatting in the abstract.

Response:
Thank you for pointing this out. We have reviewed the abstract and standardised the font formatting for consistency.

Responses to comments from Reviewer 2 (Ms. Rieka von der Warth, Albert-Ludwigs-Universitat Freiburg)

Reviewer 2 general comment:
Dear Authors, thank you for having me review your paper and for conducting this important study. Even though I read your study protocol with interest, it is somewhat hard to follow as information are not where I would have expected it. I understand, that there is a conflict between describing those kind of complex studies and a precise manuscript, one should be careful that a manuscript is still understandable without reading all supplements (which in this case are extremely detailed).

Response:
Thank you for your thoughtful feedback and for acknowledging the intricacies of presenting complex studies in a precise manuscript format. We recognize the challenges you highlighted regarding the

positioning of information and the heavy reliance on Supplementary materials. Your input has underscored the need for us to strike a better balance between comprehensiveness and clarity. Considering your feedback, we revisited the structure of the manuscript to ensure that essential information is readily accessible within the main text. This should make it more user-friendly, even for those who may not delve into the supplemental sections. Your constructive critique is highly valuable to enhancing the accessibility and overall quality of our manuscript.
\ See manuscript throughout.

Reviewer 2 comment 1:
There is a lack of clarity in your wording. The title of the study included „psychosocial distress", while you address „mental disorders" in your abstract and in the background. Psychosocial distress and mental disorders however, are two different concepts and should be differentiated. This is of special relevance, as you, as far as I understand; provide no treatment for mental disorders but counseling. Two further concepts you speak of, but not distinguish, are mental-somatic mutlimorbidity and mental-somatic comorbidity. Please, stick with one word or explain why both should be included in your protocol.

Response:
Thank you for highlighting this inconsistency. We revised the manuscript to ensure that the distinction between „psychosocial distress" and „mental disorders", and the respective terminologies are used accurately and consistently throughout the manuscript. We also provided a clear definition for both terms early in the manuscript.
\ See page 7: "Two key distinctions must be made at this juncture: I) 'Psychosocial distress': This refers to an individual's emotional and psychological reaction to adverse events, encompassing stress, anxiety, and depression that might not qualify as clinically diagnosable mental disorders. II) 'Mental disorders': These are diagnosable conditions that can significantly interfere with an individual's cognitive, emotional, or social abilities. They include major depressive and anxiety disorders, which are among the leading global disabilities[1]. Our study works with the concept of psychosocial distress, as we did not seek to formally diagnose any mental disorders."

As for the terms „mental-somatic multimorbidity" (two conditions of comparable focus) and „mental-somatic comorbidity" (with one condition being first/primarily in focus and the other(s) primarily co-occuring), we will stick with „mental-somatic comorbidity" throughout the text for consistency and to avoid confusion, as we here talk about patients that become hospitalised because of somatic conditions.
\ See manuscript and Supplemental material throughout.

Reviewer 2 comment 2:
Within the abstract you speak of household costs? It gives the impression that you only assess costs on patient level. Please explain.

Response:
We apologise for any ambiguity. The mention of „household" in this context is inappropriate and we hence deleted the term.
\ See abstract.

Reviewer 2 comment 3:

Introduction: Please consider re-writing, as you jump in the topics. Also, you concentrate on mental disorders in your introduction, which is confusing as you don´t assess mental disorders (no clinical interview). How do you define psychosocial distress?

Response:
Thank you for this feedback. We restructured the introduction for a smoother flow. Additionally, we now provide a clear definition for „psychosocial distress" early in the introduction to ensure clarity.
⌞ See introduction throughout; see also reply to Reviewer 2 comment 1.

Reviewer 2 comment 4:
Please update your references. I see, that you use inter alia Lehnert et al., 2011 and Hochlehner er al, 2011, which are highly relevant papers in this area, but there are studies that are more recent, e.g.:
a. Jansen L, van Schijndel M, van Waarde J, van Busschbach J. Health-economic outcomes in hospital patients with medical-psychiatric comorbidity: A systematic review and meta-analysis. PLoS One. 2018 Mar 13;13(3):e0194029. doi: 10.1371/journal.pone.0194029.
b. Wolff J, Heister T, Normann C, Kaier K. Hospital costs associated with psychiatric comorbidities: a retrospective study. BMC Health Serv Res. 2018 Jan 30;18(1):67. doi: 10.1186/s12913-018-2892-5. PMID: 29382387; PMCID: PMC5791176.
c. von der Warth, R., Hehn, P., Wolff, J. et al. Hospital costs associated with post-traumatic stress disorder in somatic patients: a retrospective study. Health Econ Rev 10, 23 (2020). https://doi.org/10.1186/s13561-020-00281-0

Response:
We appreciate your suggestion on updating the references. Recognizing the value of incorporating the latest findings in the field, we have added recent studies in this area, including those you mentioned.
⌞ See page 8: "These findings corroborate established evidence on mental-somatic comorbidity[3,4,10,12–18], accentuating the essentiality for hospitals to address these challenges comprehensively, ensuring superior care for affected patients." & references.

Reviewer 2 comment 5:
current standard intervention options: In this section I would have expected an explanation of TAU in Switzerland at the moment.

Response:
Thank you for pointing this out. We have now included a comprehensive overview of the Treatment As Usual (TAU) in Switzerland, which we hope will provide readers with a clearer context of the current standard local interventions.
⌞ See pages 9-10: "The treatment as usual (TAU) with regard to psychosomatic-psychiatric CL services for somatic hospital inpatients provided in this study is reflecting current common procedures in Switzerland[30,31]. These CL services bridge the interface between mental and physical care within somatic hospitals. Depending on local needs and circumstances, individual CL services vary widely; organizationally they are assigned to psychiatric, psychosomatic, or psychological departments. Usually, they have multidisciplinary staffing, including medical specialists in psychiatry, psychosomatic medicine, and psychotherapy, trained clinical psychologists, psychological psychotherapists, or in some instances, specialised Advanced Nurse Practitioners (ANPs). Based on a diagnostic assessment, the integrated psychosomatic-psychiatric interventions emphasise a holistic approach to patient care, combining biological and psychosocial perspectives and treatments.

Intervention methodologies encompass psychoeducation, coping strategies, relaxation techniques, psychotherapeutic interventions, resource activation, and psychopharmacotherapy. At the centre are medical/therapeutic dialogues, which are foundational to foster a trustworthy patient-therapist relationship. With regard to the structure of care, a distinction can be made between a consultation (service called upon an as-needed basis), and a liaison model (service fully integrated within a ward): Consultation pertains to direct clinical assessments and advisories provided to the main treatment team, whereas liaison emphasises continuous collaboration with the psychosocial liaison staff being part of the ward team; Of note, TAU in the form of CL services slightly varies across institutions, medical specialities, and wards participating in SomPsyNet, especially with regard to intensity and staffing, depending on the specific settings."

Reviewer 2 comment 6:
Please check all abbreviations: I found some, that are used before the abbreviation is introduced (e.g. CL).

Response:
We apologise for the oversight. We have verified the manuscript to ensure that all abbreviations are appropriately defined upon their initial use. The instance of „CL" has been appropriately addressed.
⎩ See manuscript and Supplemental material throughout.

Reviewer 2 comment 7:
You state, that you expect SomPsyNet to benefit staff and stakeholders. How do you assess that information? I would have expected a process evaluation or similar.

Response:
We have incorporated as information to the section "Rationale of the research project" that a process evaluation accompanies SomPsyNet. Results of this process evaluation are regularly shared with different stakeholders such as local and national health authorities.
⎩ See page 10: "We anticipate that SomPsyNet will benefit patients, staff, and stakeholders, potentially leading to decreased healthcare resource utilisation in the mid- and long-term, impacting healthcare budgets positively. Of note, assessment of benefit for staff and stakeholders is not part of this SomPsyNet study outlined here, but is assessed in the context of a process evaluation that accompanies SomPsyNet. Results of this process evaluation are shared with different stakeholders such as local and national health authorities and key results published in scientific journals[33,34]."

Reviewer 2 comment 8:
The „Risk category and rationale" paragraph is not understandable if you don't live in Switzerland. Please elaborate, why this statement is important.

Response:
We recognize that our initial description may not have been clear to an international readership. We have revised the section to provide a clearer context and justification for the risk categories, ensuring it is comprehensible regardless of the reader's familiarity with the Swiss healthcare system.
⎩ See page 10: "We anticipate that SomPsyNet will benefit patients, staff, and stakeholders, potentially leading to decreased healthcare resource utilisation in the mid- and long-term, impacting healthcare budgets positively. Of note, assessment of benefit for staff and stakeholders is not part of

this SomPsyNet study outlined here, but is assessed in the context of a process evaluation that accompanies SomPsyNet. Results of this process evaluation are shared with different stakeholders such as local and national health authorities and key results published in scientific journals[33,34]."

Reviewer 2 comment 9:
On P.10, L. 54, why are not all stakeholders translated (they are in the affiliations)?

Response:
Thank you for catching that inconsistency. We have revised the manuscript to ensure uniform translation of stakeholder names, aligning with the affiliations section.

Reviewer 2 comment 10:
The overall phases of the study are not clear to me. I can see from Suppl. 3, that all 21 sites started at the same time with phase 0, why are some sites longer in phase 2 – are those the triplets you formed? Also, what is meant by step0-4 and t0-t4? You only speak of it in the Supplementary material, but how do they relate to the phases?

Response:
Thank you for pointing out the need for clarity regarding the phases. Of note, we here follow by large the nomenclature of Stepped Wedge RCT outlined by Hemming et al. (2018). According to Hemming et al. (see Table 1 there), a step is "A planned point at which a cluster or group of clusters crosses from control to intervention." Our study comprises three "conditions", denoted "phase 0", "phase 1", and "phase 2". All clusters started at the same time (at step 0) in phase 0 and transitioned at the same time (at step 1) from phase 0 to phase 1. However, the timing of transition from phase 1 to phase 2 could occur at different times across clusters (either at step 2, step 3 or step 4). The exact timing of transition was randomised for each cluster: First, we built triplets of clusters of roughly the same size (in terms of numbers of patients) and with similar patient populations. Second, within each triplet, the order in which the clusters transitioned was randomly selected from all possible allocation sequences, so that one cluster out of the triplet transitioned from phase 1 to phase 2 at step 2, another cluster of the same triplet transitioned from phase 1 to phase 2 at step 3 and the remaining cluster of the same triplet transitioned from phase 1 to phase 2 at step 4. In the figure of supplement 1 (formerly supplement 3, now supplement 1), the phases are colour coded and the steps (step 0 to 4) marked. Additionally, we have noted the periods (T0 to T4), which – in line with Hemming et al. – are defined as time between two steps ("A grouping of observations by time of measurement."). We have realised that this was not explicitly explained in the footnotes as well as in the main text, and we have since expanded on these notations, integrating information from the Supplementary material into the main manuscript for better coherence.
\ See page 12 (and Supplemental material 2): "As depicted in Supplementary material 1: Figure – Schedule of SomPsyNet stepped-wedge cluster randomised trial, all clusters started at the same time in phase 0 and transitioned at the same time (at step 1) from phase 0 to phase 1. However, the timing of transitioning of a specific cluster from phase 1 to phase 2 could occur at different times (either at step 2, step 3 or step 4) and this timing of transitioning from phase 1 to phase 2 was randomised: Some clusters transitioned from phase 1 to phase 2 at step 2, other clusters transitioned from phase 1 to phase 2 at step 3, and further clusters transitioned from phase 1 to phase 2 at step 4."

Reviewer 2 comment 11:
How were patients enrolled into the study? Were all patients asked to participate, except they met an exclusion criterion? How did you detect psychosocial distress? In Figure 1, you speak of a threshold – how was this threshold defined? In Supp. You state that the cut-off values were pre-defined, but I

can't find them. Also, why was the agreement of the physician so important?

Response:
We amended the manuscript to provide a more detailed description of the enrolment procedure, including the information that all patients were asked to participate except they met an exclusion criterion. We also refined Figure 1 to increase clarity. We detected psychosocial distress through standardised self-reporting questionnaires and detailed this in the manuscripts. We have now included the specific values used as cut-off scores that were based on previously validated cut-off scores of the respective instruments under details in Supplementary material 5. The agreement of the physician was relevant, as interventions in the hospital need to be agreed upon by the responsible physician. We clarified this in the manuscript.
⌊ See page 13: “The study population are patients from selected wards in three somatic hospitals in Basel-Stadt: UHB, BESP, and UAFP. All patients who are hospitalised in a ward/cluster that participates in the study at the time of hospitalisation are assessed for eligibility according to the criteria below. Patients are enrolled on a daily basis at the day of or in the days following admission to a ward participating in the study, unless at least one of the following exclusion criteria applied”
⌊ See Supplemental material 5: “In phase 2, every patient with a positive result of “screening 2” (i.e., scoring above threshold, which was the case if at least one of the three instruments, Depressive Symptom Scale with 8 items from the PHQ (PHQ-8), Generalised Anxiety Disorder, questionnaire with 7 items (GAD-7), and Somatic Symptom Disorder, questionnaire with 12 items (SSD-12) scored equal or above the predefined values that were based on previously validated cut-off scores of the respective instruments: PHQ-8 >= 10; GAD-7 >= 10; SSD-12 >= 23) – if the physician in charge agreed – was offered the SomPsyNet intervention. The agreement of the physician was relevant, as interventions in the hospital need to be agreed upon by the responsible physician. This intervention was a stepped and collaborative care model (SCCM) centring around psychosomatic-psychiatric consultations.”

Reviewer 2 comment 12:
When were patients enrolled into the study? Right after hospital admission? Were the questionnaires self-administered or as an interview?

Response:
Patients were enrolled shortly after their admission to the ward participating in the study to ensure timely intervention. The patient journey up to that time-point varied, with admission from home, from other hospitals, from emergency department, ICU or other wards. We clarified this in the manuscript. The questionnaires were primarily self-administered, but assistance was provided for patients who requested it. We clarified this in the manuscript.
⌊ See page 13-14: “Patients are enrolled on a daily basis at the day of or in the days following admission to a ward participating in the study, unless at least one of the following exclusion criteria applied: (…) Of note, the patient journey until study inclusion varies, with admission to the ward participating in the SomPsyNet study either from home, from other hospitals, from the emergency department, from ICU, or from any other ward of the same hospital.”
⌊ See page 19: “Upon informed consent, patients completed a baseline questionnaire, predominantly via tablet-assisted software, with alternatives for paper-pencil or staff-guided questionnaires available. Hence, the questionnaires were primarily self-administered, but assistance was provided for patients who requested it.”

Reviewer 2 comment 13:
In your exclusion criteria, you state that the study was tailored to other languages – which ones?

Response:
We apologise for the ambiguity. We stated this option in the exclusion criteria, but ultimately no tailoring was implemented. Hence, even though originally considered, we did not tailor the study to any other language than German. Hence, regarding this exclusion criterion, we check whether there is any inability to understand and speak German. We clarified this in the manuscript.
⌊ See page 13: “Please note regarding the exclusion criterion “Inability to understand and speak German or any other language at which the study is tailored at that point in time” that even though originally considered, we did not tailor the study to any other language than German. Hence, regarding this exclusion criterion, we check whether there is any inability to understand and speak German.”

Reviewer 2 comment 14:
Figure 1 is quite hard to read. Please consider at least changing font color to make the figure more accessible.

Response:
Thank you for the feedback. We have revised Figure 1, enhancing its readability by adjusting amongst other the font colour.
⌊ See Figure 1

Reviewer 2 comment 15:
Why were the wards divided into up to three parts, when all sites were included in the study at the same time?

Response:
The division of wards into triplets was a methodological decision aimed at ensuring diverse representation and to control for variations within sites. Even though all sites began the study concurrently, the within-site divisions helped us to account for potential internal differences in patient care. The main idea here was to have the most balanced mix of patients possible in each phase of the trial. We thus tried to divide wards into smaller blocks (clusters) as much as this was logistically feasible. We have tried to make this clearer in the revised version of the protocol, where we now write:
⌊ See page 15 (see also Supplementary material 1-3): “According to the extension of the CONSORT 2010 statement[36]: “The SW-CRT involves randomization of clusters to different sequences that dictate the order (or timing) at which each cluster will switch to the intervention condition.” Thereby, 'clusters' refer to the specific sections of hospitals. As outlined in Supplementary material 3, we divided larger wards into 2-3 clusters, while smaller hospital wards were not divided, and thus constitute their own cluster. Because of the high heterogeneity between the wards, clusters were pre-grouped into triplets based on patient age, sex, and expected primary outcome as indicated by data from phase 0 that, with this regard, provided information similar to a pilot phase. Then clusters were randomised to different sequences.”

Reviewer 2 comment 16:
„The step-by-step implementation was also appreciated by the staff and stakeholders”? How do you know? Also, which stakeholders are meant?

Response:

As this was only informal feedback we received from stakeholders – like Department heads, members of IT-services, and colleagues involved in the evaluation – and rather a side-note, we decided to delete this statement from the manuscript.

Reviewer 2 comment 17:
P.13: You have two following sentences saying „According to the extension of the Consort statement". Please re-phrase. Overall, I would recommend a proofreading as some sentences and phrases sound incorrect.

Response:
We apologise for the oversight. The repeated phrase has been corrected. We have also more generally revised the manuscript to rectify and refine unclear or grammatically incorrect sentences throughout.
⎩ See manuscript throughout

Reviewer 2 comment 18:
Intervention: Was the personnel solely study personnel or the TAU personnel from the psychiatric ward?

Response:
The personnel involved in the intervention is mainly study personnel and in some rare occasions TAU (Treatment as Usual) personnel from the psychosomatic wards, supplemented by the study team for training and support. We added this information to the manuscript.
⎩ See page 15: "The intervention is conducted by trained medical and psychological personnel, being mainly study personnel and in some rare occasions TAU personnel from the psychosomatic wards, supplemented by the study team for training and support."

Reviewer 2 comment 19:
Intervention: What are example contents of the intervention? (This information is somewhat given in German as part of the supplemental material, but I miss it as part of the main manuscript or at least in English).

Response:
Thank you for pointing this out. The primary components of the intervention included a range of support offers tailored to individual needs. We have added a paragraph providing respective example contents in the main manuscript in English.
⎩ See pages 15-16: "The intervention consists of consultations being a mix of in-person and telephone interactions, tailored to patients' needs, and oriented towards identifying individual psychosocial stressors and corresponding support options. Essential elements include pre/post consultation discussions, generation of support recommendations using a custom-built tool ('BAK-list') (see Supplementary material 4), coordinating support implementation, and providing a follow-up consultation after hospital discharge.
Utilising a comprehensive framework, the CL service evaluates each patient's distinct support requirements, suggesting appropriate intervention strategies at regional institutions offering respective services. These recommendations derive from a broad spectrum of specialised intervention avenues. For example: Expert institutions may offer tailored care for those with terminal illnesses, emphasising

comfort and comprehensive support. Recognized bodies may guide individuals through housing challenges, while other institutions may mediate tenant-landlord disputes, ensuring stable living conditions. Several organisations may cater to the diverse and multicultural populace, offering translation services, guidance, and tailored assistance for migrant families and seniors. There are dedicated centres that may provide transcultural addiction counselling, and specialised professionals who deal with specific mental health issues, including eating disorders. Specialised entities may ensure those with mobility challenges have access to essential transport facilities. Comprehensive care facilities are available for the ageing population, ensuring medical, social, and conflict-resolution needs are addressed. Additionally, there are platforms that specifically disseminate health information pertinent to this age group. For patients with distinct health challenges, there are associations focusing on a variety of conditions, from respiratory issues and allergies to rare diseases and cardiac concerns. Overall, the CL service acts as a bridge, connecting patients to these multifaceted support systems based on individual needs."

Reviewer 2 comment 20:
I don´t understand why you need the focus sample; please explain further. (You sometimes call them focus groups, which made me expect a qualitative study part).

Response:
We recognize the ambiguity. Indeed, both terms "focus group" as well as "focus sample" may be misleading, so we decided to replace both. Indeed, the follow-up assessments were only conducted in the subsample of patients distressed at baseline and in a subsample of non-distressed patients (those with baseline assessment later than 08-07-2022). This decision was made due to insufficient resources to assess the full sample at follow-up. We clarified this in the manuscript.
⌊ See manuscript throughout and Figure 1.

Reviewer 2 comment 21:
I understand that you tried to keep your study protocol short to make it readable, but that led to missing information in the text, which are only given in the supplement. For instance, please elaborate a little more on the statistical analyses in the main text.

Response:
You're right; the intention was to maintain brevity. However, based on your feedback, we have expanded upon the statistical analyses in the main text, providing key details on the methods used, while still referencing the supplement for readers seeking more comprehensive information.
⌊ See Method section in the main manuscript.

Reviewer 2 comment 22:
You have two sections called dissemination?

Response:
Thank you for catching that mistake. It appears to be an oversight during the editing process. We have since corrected this, subsumed the 'dissemination policy' section under "ETHICS AND DISSEMINATION" and ensured that each section in the manuscript has a distinct and relevant title.
⌊ See there and manuscript throughout

Reviewer 2 comment 23:
Just from the text, it was hard to understand why recruitment ended in 2022, the follow-up is after six months, but data collection will end in June 2026. Measurement points for all assessments, including claims data, should be stated in the text.

Response:
We appreciate the feedback on this aspect. To clarify, while the recruitment phase concluded mid December 2022, the six-month follow-up extends to June 2026, with health data collection going considerably beyond that time-period, thus extending data collection to June 2026. We have now provided a more detailed timeline in the manuscript, outlining the specific measurement points and reasons for the data collection period.
⌊ See pages 19-20: “Data collection started on 09-06-2020 and is anticipated to be completed on 30-06-2026 (estimated study completion date, including completion of collection of health claims data), with completion of the six months follow-up assessments in June 2023.”

Reviewer 2 comment 24:
Minor mistakes:
• Within your manuscript you tend to switch fonts quite often. Please correct
• P.5, L. 40: claims data
• P-12, L. 6: please speak of gender affirming interventions or gender affirming care

Response:
Thank you for pointing out these details. We addressed all of them to improve the clarity and consistency of our manuscript.
⌊ See respective places in the manuscript.

Responses to comments from Reviewer 3 (Mr. Ibrahim Demirer, University Hospital Cologne)

Reviewer 3 general comment:
Overall, I found your protocol intuitive to read and informative on the main subjects. You also provide rich information in the supplemental materials.

Response:
Thank you for your positive feedback. We are pleased to hear that you found our protocol intuitive and informative. Our goal was to offer a comprehensive overview while ensuring that the manuscript remains clear and coherent. Your acknowledgment of the rich supplemental materials further encourages us to maintain the detailed support for our readers. Your comments are greatly appreciated, and we strived to maintain this clarity in our revision.

Reviewer 3 comment 1:
My only concern lies in the quite large interval and in the sample size at phase 0 with 200-500. The assumed Hedge's G of 0.5 seems fairly high, given the heterogeneity.

Response:
We acknowledge your concerns regarding the assumed effect size. The Hedges' g of 0.5 was an

initial estimate based on previous studies in similar settings. Given the heterogeneity of our sample, we agree that one could also argue for a more conservative effect size, but we would like to abstain from post-hoc changes to this calculation. Regarding the rather large interval of the anticipated sample size of phase 0, this was due to some uncertainties regarding the length of the initial transition phase (i.e., duration of study initiation at all sites) and limited information on the number of patients eligible for study inclusion being admitted per month.
↳ See Supplementary material 9: “Of note, the rather large interval of the sample size of phase 0 was due to some uncertainties regarding the length of the initial transition phase and limited information on the number of patients eligible for study inclusion being admitted per month.”

Reviewer 3 comment 2:
You also assume a drop-out rate of 25%, however, it is unclear to me, does these 25% also contain missing values and attrition?

Response:
Thank you for bringing this to our attention. The estimated 25% drop-out rate does encompass both participants who choose to leave the study and those for whom we have missing outcome data points. We do not anticipate much missingness in terms of outcome data, but anticipate to lose about 25% due to the high morbidity rates in this population. We have provided further details on the rationale of these assumptions in Supplemental material 9. Missing covariate data will be imputed where meaningful using multiple imputations with chained equations (MICE). We clarified this in the manuscript.
↳ See Supplementary material 9: “Notably, the sample size needed to allow for an anticipated drop-out rate to reach sufficient power for both intent-to-treat and per-protocol analyses. Assuming a rate of drop-out and missing data due to other reasons of 25% (given that follow-up was conducted at baseline and 6 months and a rather morbid study sample), we (…)”.

Reviewer 3 comment 3:
Furthermore, I would suggest adding more information on the confounding variables. E.g., what are the confounders relevant for the primary, and what are the confounders relevant for the secondary endpoints? Which confounders may remain unmeasured? What are potential mediators of the intervention that should/could be measured? – Such that the risk of bias can be assessed.

Response:
Your suggestions are indeed valid. We expanded on the potential confounding variables and mediators in the manuscript. By providing this additional information, we hope to give readers a clearer understanding of the planned statistical analyses.
↳ See Supplementary material 9: “To estimate intervention effects, we intend to primarily conduct generalised linear mixed models of primary, secondary, and other outcome parameters adjusted for the clusters as random effects and for study conditions, calendar time and potential confounders (e. g., gender, age categories, socioeconomic status) as fixed effects. Of note, as the comparison of phase 2 versus phase 1 was randomised, we did not expect confounding ex ante for respective comparisons, as we do not expect that patient characteristics potentially predictive of the outcome are somehow associated with the timing of the rollout. Still, we intend to include covariates to improve fitting the model with greater precision. The exact choice of regression method will consider the distributional characteristics of the outcome parameters of interest. As mediators, we primarily intend to assess uptake of the CL-service offers and recommendations.”

VERSION 2 – REVIEW

REVIEWER	Baliousis, Michael University of Lincoln
REVIEW RETURNED	09-Oct-2023

GENERAL COMMENTS	The authors seem to have revised the manuscript thoroughly and to have addressed the reviewers' comments in a way which now does justice to this important study and project. The responses to each reviewer suggestion are very helpful. I do not have any further suggestions of consequence to make, although perhaps the authors might consider combining the opening paragraphs of section in P9 into a narrative leading to the remaining of that section. This is purely stylistic.

REVIEWER	von der Warth, Rieka Albert-Ludwigs-Universitat Freiburg, Institute of Medical Biometry and Statistics
REVIEW RETURNED	05-Oct-2023

GENERAL COMMENTS	Dear Authors, your paper improved significantly. Thank you for incooperating all of our comments! I only have one minor comment before I recommend acceptance: - please work on the introcudction part of the abstract. Stating the secondary outcome there is needless, as you define it in the methods part. I wish you all the best with the study and looking forward to read about the results!