Author's response to reviews

Title: Narcissism in patients admitted to psychiatric acute wards: its relation to violence, suicidality and other psychopathology

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Author's response to reviews: see over
Dear Editor in Chief

RE: MS: 2086907454156348: Narcissism in patients admitted to psychiatric acute wards: its relation to violence, suicidality and other psychopathology

Thank you for the positive evaluation of our paper, and the helpful comments by the referees. We have extensively revised the manuscript and tried to respond convincingly to their comments.

In our thorough revision of the paper, we discovered that we had wrong Ns in the low and high narcissism groups. Therefore, the N of the low group has been changed from 86 to 88, and the N of the high group from 100 to 98.

We hope you will find our revision satisfying and that we have responded adequately to the issues raised by the reviewers.

With best regards on behalf of the authors

Marit F. Svindseth, MHSc
Responses to the reviewers’ reports

Title: Narcissism in patients admitted to psychiatric acute wards: its relation to violence, suicidality and other psychopathology

Version: 1  Date: 6 September 2007

Reviewer: Joel Paris

Reviewer’s report:

This submission is interesting in that it applies a scale of narcissism (an under-investigated personality trait) to psychiatric patients. However there are several problems with the methods.

1) The NPI was validated as a trait measure in the general population. But mental disorders have profound effects on the personality. There were many schizophrenics in this study, and there must also have been some with bipolar disorders. These are conditions that distort personality greatly. What exactly is the scale measuring in people with these conditions?

The reviewer raises a fundamental question here, and our response goes along several lines. Patients with manic or hypomanic disorders were excluded from the sample, and we are sorry that this important fact was omitted in our paper. It is now stated under Patient sampling.

We find the statement of the reviewer that: “mental disorders have profound effects on the personality” somewhat overstated. We rather read the literature so that some disorders change the personality, and such changes vary in content and duration. For example a patient with schizophrenia can have a personality change that increases his grandiosity for a short or longer while and such a trait change can be more or less amenable to treatment.
We also find that the reviewer’s statement has to be considered in the light of “trait” or “state” self-rating scales. The NPI-21 is a trait scale asking for what is characteristic for the patient over long time, in contrast to most “state scales” like the BPRS or the HADS, that ask for psychopathology with last 1-2 weeks.

A more fundamental question raised by the reviewer: “Can self-rating scales be rate in a valid way by patients with severe mental disorders?” In our view so is the case, but of course the trait changes induced by the psychopathology, are reflected in the scale scoring. For example a grandiose patient with schizophrenia will self-rate himself high on the grandiosity items of a scale. The criterion needed for a valid rating is really that the patient intellectually understands the questions posed to him. Ego-syntonic traits and motivations influence the self-rating of anyone, not just patients with severe mental disorders.

So by excluding patients with mania/hypomania and cognitive impairment, we consider that we have excluded those who have reduced intellectual capacity to understand the NPI-21 items. So we state that the included patients display valid expressions of narcissism by their NPI-21 scores.

2) It is also well known that acute mental disorders change personality. The authors have not shown that the scores on th NPI are stable over time, i.e, that they are a trait and not a state measure.

The NPI-21, the BPRS, the HADS and the RSES test were performed at admission and discharge. Among the 186 patients included at admission, 147 (79%) also had ratings at departure after a mean time of 20 days (range 2-197 days). The test-retest correlations were r=0.94 for NPI-21 total score, r=0.89 for Factor 1, r=0.89 for Factor 2, r=0.83 for Factor 3 and r=0.70 for Factor 4 (Table 2). The differences between the means at admission and discharge all showed effect sizes ≤0.15.

In contrast, HADS-A showed a correlation of 0.75 and an effects size of 0.80, and the corresponding findings for HADS-D were 0.79 and 0.67, respectively. For the BPRS total mean score and subscale mean scores the correlations ranged between 0.41 and 0.74, and except for the Thinking disturbance subscale, the effect sizes were all ≥0.55.
These results support our assumption that narcissism represents a trait and not a state as the effect sizes show that NPI scores are stable while scores on the state instruments (the interviewer scored BPRS and the self-rated HADS) show much lower test-retest correlations and higher effect sizes as mentioned in the paragraph above.

3) There were no differences from a general population sample on the scale. This is discussed as needing “further study”. But the failure to find differences from NORM might actually strengthen the study design, rather than as a problem. The explanation in the discussion, of factors cancelling each other out, is strained and unconvincing. And I don’t see what Peer Gynt has to do with this study, even if Ibsen comes to mind readily in Norway.

We agree with the observation of the reviewer. The fact that the NORM and the patients do not differ significantly on NPI-21 scores, can be interpreted in the way that the state of the patients that bring them to acute admission does not have a significant influence on their NPI-21 scores. Thus this finding supports our notion that the NPI-21 can be considered as a trait rather than a state scale.

We have followed the recommendations of the reviewer and deleted the “need for further study” and the explanation of “factors cancelling each other”. Sadly, we also have to agree that Ibsen’s Peer Gynt is not relevant for this paper, and that has been deleted.

4) The diagnoses among the inpatients are not well characterized (half are described as “other”).

We agree, and the diagnoses have now been spelled out in more detail.

5) Inpatients are more convenient but might it have been more useful to have conducted the study on outpatients? That is the clinical population in which interfaces between personality and diagnosis (other than psychosis) make a difference for treatment planning.
We agree with the reviewer, but that would have been another study. Our point of departure was to study “experiences of humiliation” associated with forced admissions.

6) I am not sure why the authors chose to do a median split on a scale that measures a continuous variable. Would the results have come out the same way with multiple regression?

A design comparing patients with high versus low scores on an instrument is quite common in psychiatric research. When the sample size is huge, the upper and lower quartiles frequently are compared. However, with a sample size of N=186, we found comparisons of the dichotomy more valid due statistical power.

7) The clinical meaning of the findings is not well explained. For example the observation that patients who are violent are more narcissistic is not really news – one of the factors on Hare’s PCL measures the same trait.

We agree that this finding is not news in a general way, but we considered it new in the setting of an unselected sample of patients admitted to an acute ward, and we consider this as one our findings with practical clinical consequences.

We have now interpreted the clinical meaning of the findings more fully in the Discussion.

8) I have never heard of a ”clinical observation” that people who consider suicide are more narcissistic. The negative correlation found in this study makes more sense.

We agree with the reviewer and the statement has been omitted in the revised manuscript.
Reviewer’s report

Title: Narcissism in patients admitted to psychiatric acute wards: its relation to violence, suicidality and other psychopathology

Version: 1 Date: 8 October 2007

Reviewer’s report: Tilman Steinert

General

The idea of the paper is good: Personality traits could represent a liability for violent and aggressive behaviour even in persons with acute psychiatric disorders. It is rather well-known that an association of personality traits and aggressive behaviour exists in personality disorders, where this kind of behaviour is part of diagnostic criteria in some disorders. But, as the authors state, there has not been much work on personality traits in people with e.g. psychotic disorders, and especially narcissism is a concept which has only poorly been recognized by psychiatric researchers. Some aspects of this paper are really well done. A good part of the study design is the comparison with a matched control group from the normal population.

However, I see many points of serious concern and therefore I cannot recommend a publication of the paper in the present form. None of them is addressed in the very small “limitations” section of the paper. This raises the question whether the authors were fully aware of all the methodological problems that are comprised in this study design.

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)
First of all, the major question that should be discussed here is the question of "trait" or "state". Most of the used scales such as the BPRS are measures of "states". These states are correlated here with an instrument that should measure a "trait", narcissism. The first question is, whether it makes sense to correlate state variables with a trait variable.

Most researchers in psychiatry find that this makes perfect sense. Just consider the huge literature on self-rated neuroticism (trait measure) and self-rated anxiety and depression (state measure).

Logically, the correlations should change when the state (psychopathological symptoms) will change. Or they do not (this was not examined here), and then NPI does not measure a trait but a state. This leads to the second question: Is the NPI stable even during acute, psychotic, depressed or manic states? If this question cannot be answered by "yes", the investigation makes little sense.

The NPI-21, the BPRS, the HADS and the RSES test were performed at admission and discharge. Among the 186 patients included at admission, 147 (79%) also had ratings at departure after a mean time of 20 days (range 2-197 days). The test-retest correlations were $r=0.94$ for NPI-21 total score, $r=0.89$ for Factor 1, $r=0.89$ for Factor 2, $r=0.83$ for Factor 3 and $r=0.70$ for Factor 4 (Table 2). The differences between the means at admission and discharge all showed effect sizes $\leq 0.15$.

In contrast, HADS-A showed a correlation of 0.75 and an effects size of 0.80, and the corresponding findings for HADS-D were 0.79 and 0.67, respectively. For the BPRS total mean score and subscale mean scores the correlations ranged between 0.41 and 0.74, and except for the Thinking disturbance subscale, the effect sizes were all $\geq 0.55$.

These results support our assumption that narcissism represents a trait and not a state as the effect sizes show that NPI scores are stable while scores on the state instruments (the interviewer scored BPRS and the self-rated HADS) show much lower test-retest correlations and higher effect sizes as mentioned in the paragraph above.

The authors report (interestingly, in the methods section, not within the result section, though it is one of the most important results) a high retest reliability of the NPI-21 after 20 days.
See the response to the former paragraph. These findings are now included in the Results.

However, the sample is only poorly characterized with about 50% ”other” diagnosis. I cannot imagine that narcissism as measured in this questionnaire should not be influenced by acute manic or depressive states.
See the response above.

*Patients with manic or hypomanic disorders were excluded from the sample, and we are sorry that this important fact was omitted in our paper. It is now stated under Patient sampling.*

*We have tested the correlations of NPI-21 total score at admission and discharge for the ICD-10 diagnostic groups and found that for substance abuse r 0.97, schizophrenia r 0.92, major depressive disorders r 0.94, neurotic disorders r 0.83 and personality disorders r 0.97.*

Obviously, it would be necessary to demonstrate that the NPI really measures personality traits independent from psychopathological symptoms in all the disorders included. Otherwise it has to be assumed that the results are seriously confounded by the acute psychopathological states in which the examination was done. The correlations reported here could indicate that there is such confounding, but don`t prove it without a re-correlation in the same individuals after stabilization.

*As suggested by the reviewer we have done such recorrelations at the time of discharge. Test-retest show strong correlations on the NPI-21 from admission to discharge. Table 2 shows these test-retest correlations, and they are considerable higher in the NPI-21 than in the BPRS and the HADS. These results thus support our assumption that narcissism represents a trait and not a state. The effect sizes of the NPI-21 measures are all <.15, while the lowest effects size for change BPRS and HADS measures is 0.41.*

There are several other serious points:
The whole paper is based on a self-developed questionnaire which has not yet been accepted for publication. It is not correct to quote it with the journal where it has been
submitted, correct would be “in preparation”, which is not a serious reference to base a paper on it. (Otherwise, we would read many papers with references such as “Science, submitted for publication…”). It would be wise to wait until the first paper has been accepted and only then submit further papers based on that work.

We are in some disagreement with the referee on this point. The NPI-21 is “not self-developed” in our view. We have tested the modified NPI-40 developed by Kansi (ref 7) and by structural equation modelling found that the NPI-21 version holds similar psychometric properties. The paper describing this development has twice been submitted to Journal of Personality Assessment where it finally was turned down on October 12, 2007. Considering the publication process of that paper we find the term “submitted” adequate, but in order with the recommendation we use “in preparation” in he revised paper.

As the referee surely is well aware of a manuscript rejected by one journal may well be accepted by another, but this can be a somewhat time-consuming process. In order to save time during the three years research grant for Dr. Svindseth’s PhD-thesis, we find it defensable to submit the current manuscript. In order to help the readers we have included the NPI-21 questionnaire as an appendix.

The last paragraph of the introduction section does not tell what the aim of the study is but gives a summary of the methods used.

We have now given the aims of the study at the end of the Introduction.

Methods/study design. I do not believe that this study was originally designed to examine correlations of narcissism and aggression in psychiatric patients. If that would be the case, the authors certainly would have used better measures for their primary outcome variable, violent behaviour. Taking into account that for about two decades validated instruments are available which yield continuous score for violent behaviour such as the SOAS-R of the MOAS or the SDAS /and there are plenty of studies with these instruments), it is hard to understand why the authors use a simple self-made scale from which they received only three categories, and the category ”serious violence” was rare (absolute figures are not reported, which would be necessary). The categories used here
are not validated and have some obvious shortcomings: Violence against objects cannot be recorded, and so violence prior to admission. Suicidality is poorly defined, too.

We are sorry that we did not give the reference to the violence measure we have used. It is the Intensity subscale of the modified Scale for the prediction of aggression and dangerousness (See ref 18) "No violence" "Threats", "Mild violence", "Moderate violence" and "Severe violence". We recorded the violence from the first contact leading to admission to discharge. We decided to split the material in order to have emphasis on the most serious violence, heavy violence with intent to harm.

Suicidality was measured by a psychiatrist, on admission, and by BPRS interview with very experienced psychiatric nurses, and we have tightened up the definition as suggested by the reviewer. We have now specified “suicidality” as a score of “Moderate” ≥ score 4 on the 1 to 7 BPRS scale or its clinical equivalence.

Study design: It would deserve a further discussion why "narcissism" was chosen as personality trait. A related feature is the "psychopathy" concept of Hare, which comprises some characteristic narcissistic personality traits. There is really big number on publications on "psychopathy" as measured with Hare’s psychopathy checklist and the ability to predict violence in persons with personality disorders. This instrument was also used for psychotic in-patients. First of all, it would be necessary to examine the correlations of psychopathy and narcissism and to discuss the respective literature, if a new instrument should be introduced to examine relevant personality traits for violent behaviour.

Principal component analyses of Hare’s Psychopathy Checklist identify two separate factors: “narcissism” and “antisocial behaviour”, and only the former concept was of interest to us. Therefore we chose an instrument focusing only on narcissism. To examine the association between the NPI-21 score and level of violence was not an aim of the paper. However, since violence during hospitalization was recorded and an association had been suggested in the literature (ref 24-26), we found it clinically meaningful to explore the association between narcissism score and violence.
Results: The absolute values of violent and suicidal behaviour are missing. *We have included these values in the revised manuscript.*

Results: The authors seem to have correlated everything with everything, and they report and discuss every finding. Instead, they should have chosen: What makes sense and what gives relevant information? For example, table 3 tells us that NPI-scores and subscores were significantly higher in people with high narcissism than in those with low narcissism. This is not very surprising considering the fact that the groups had been separated by median-split. For the remainder of the results, a big number of significant correlations are reported and discussed. But it should be kept in mind, that, if you do 20 calculations, one of them will reach a .05 significance level by chance. There are several ways to compensate, mostly used is Bonferroni’s correction which means to divide the significance level by the number of calculations. For table 1, this would mean for example the significance level should be 0.05/31 = .0016. This makes the discussion much easier because there will not remain many significant correlations.

*We find the critique by the reviewer very pertinent here. We have made a critical selection of the results we present. We have also changes the level of significance to p<.01 due to multiple comparisons.*

Discussion: It was already mentioned that the discussion does not meet the serious points. One further point should be mentioned: P. 15, 3rd par.: "In this study patients with high levels of narcissism felt entitled to react aggressively upon what they sensed as threats….". This is a causal interpretation which certainly cannot be derived from the data.

*We have modified the Discussion according to the suggestions of the reviewer.*