

Should the “Disorder” be dropped from “Post Traumatic Stress Disorder”?

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Introduction

The discussion about whether post-traumatic stress disorder, or PTSD, should be labeled as a disorder has been circulated for many years in the medical field, but arguably gained momentum by Former President George W. Bush as well as Former President Barak Obama, who always referred to PTSD as “post traumatic stress”, rather than its full title. The idea is that by avoiding calling PTSD a “disorder”, it might alleviate any burdens those who suffer might bear. There are two basic school of thought on the topic. On one hand, if the medical field refers to PTSD as a disorder, it might cause those with PTSD to lose motivation or drive for treating themselves, that once they believe they have a disorder, that it is nothing more than a mental disease they must learn to live with, such as Alzheimer’s or anything of the like. On the other hand, many argue that by calling PTSD a disorder, that it gives a bit of peace to those who have it, that they know they feel the way they do because of a disorder they have, and not simply because they are “weak” or “can’t handle it”. By labeling PTSD as a disorder, it also may allow people to believe they are not alone, as there is such a large community of people who have been diagnosed. In some cases though, there can be a certain shame in being diagnosed with PTSD, as will be discussed later with a study done by the Derner Institute of Psychology (Saraiya - Lopez-Castro, 2016). Perhaps, if society simply “dropped the ‘D’” in PTSD, there would be an improvement of symptoms and successful treatments all across the board, as will be discussed with a study performed by The Mental Health Service in Seattle, Washington (Blais RK, 2014). In another study performed by John Wiley and Sons Inc., we see that there may be a correlation between the way gender may affect successful treatment, as well as the “Loss of Diagnosis” for PTSD altogether (Pacella-Feeny, 2014). Springer Science & Business Media B.V. argued that

there also may be discrepancy about the way medical professionals diagnose PTSD versus the way they treat it, and that patients may be receiving treatment for the wrong reason

(Meltzer-Averbuch, 2012). Lastly, we will look at a study (also performed by John Wiley and Sons Inc.), where they concluded that for most domains of “quality of life”, treating a patient until the patient no longer meets diagnostic criteria is the goal, but that in some cases, further improvements may result by helping a patient achieve total remission (Schnurr-Lunney, 2016).

Literature Review

One factor that may affect the success of treating PTSD is the shame that often is cemented to the word “disorder”, specifically one so commonly known as PTSD. According to a study done by the Derner Institute of Psychology, there was a staggering percentage of individuals who remain symptomatic, even after receiving top-of-the line treatment and therapies for PTSD. This study, done by Tanya Saraiya and Teresa Lopez-Castro in 2016, aimed at revealing other factors besides fear, doubt, paranoia, and the like that thwart treatment. A growing body of research has implicated shame in PTSD's development and course, although to date no review of this specific literature exists (Saraiya - Lopez-Castro, 2016). This scoping review investigated the link between shame and PTSD and sought to identify research gaps (Saraiya - Lopez-Castro, 2016). What they found was that 47 studies met inclusion criteria, and stood as substantial support for an association between shame and PTSD. This review provides crucial synthesis of research to date, highlighting the prominence of shame in PTSD, and its likely relevance in successful treatment outcomes (Saraiya - Lopez-Castro, 2016).

Another point which begs the question as to whether including the “disorder” in PTSD is helpful or not includes a study done by The Mental Health Service in Seattle, Washington, concerning the importance of social support in seeking treatment. Social support is a robust predictor of resiliency and recovery from PTSD; however, barriers to seeking support are understudied (Blais RK, 2014). One aspect of this study shows how the results of the study are different with two different trauma related illnesses, PTSD and dysphoria. PTSD and anticipated enacted stigma from family and friends were explored as correlates of the likelihood of seeking support among 153 Iraq/Afghanistan U.S. service members (Blais RK, 2014). His results showed

that PTSD ($r = -.31, p < .001$) and anticipated enacted stigma ($r = -.22, p \leq .01$) were negatively associated with likelihood of seeking support (Blais RK, 2014). Now, correlation does not prove causation, but it is interesting to note that post hoc analyses showed that only dysphoria ($r = -.32, p < .001$) was significantly related to the likelihood of seeking support after accounting for anticipated enacted stigma and other PTSD clusters (Blais RK, 2014). Implications of these findings and ways to increase likelihood of seeking support are discussed in the study, as well as the effects of calling a mental illness a “disorder”, rather than just giving it a game like “dysphoria”.

In another study performed by John Wiley and Sons Inc. in 2014, researchers decided to examine how cortisol levels change in patients after receiving successful PTSD treatment by examining the impact of successful treatment on the cortisol awakening response (CAR) (Pacella-Feeny, 2014). The study method begins with 29 adults participating in a treatment trial for chronic PTSD, providing saliva samples (when they wake up, and 30, 45, and 60 minutes after waking) before and after receiving either prolonged exposure therapy or sertraline (Pacella-Feeny, 2014). PTSD responder status (such as the loss of a PTSD diagnosis) served as the predictor variable. What they found was that their results revealed no significant main effects of PTSD responder status for any of the CAR outcomes (Pacella-Feeny, 2014). Interestingly, however, a significant gender-by-treatment response interaction for cortisol reactivity revealed that female treatment non-responders had higher cortisol reactivity following treatment than female responders, whereas cortisol reactivity did not change pre- to posttreatment for male responders (Pacella-Feeny, 2014). Furthermore, these findings stood firm even after controlling for age, trauma history, baseline medication status, baseline PTSD, and baseline depressive

symptoms (Pacella-Feeny, 2014). What they concluded was that a loss of a PTSD diagnosis may contribute to decreased cortisol reactivity in females. This now begs the question, how does PTSD affect males and females differently? More relevant, though, how does a change in the status of one's diagnosis with "Post-traumatic Stress Disorder" affect the success of their treatment?

In another study performed by Springer Science & Business Media B.V. in 2012, they argued that there is discrepancy about the way medical professionals diagnose PTSD versus the way they treat it, and that patients may be receiving treatment for the wrong reason (Meltzer-Averbuch, 2012). In their abstract, they claim that patients in Primary Care who have PTSD are often not diagnosed, and performed a cross-sectional study to determine how this interacts with other factors that argue a different approach to treating PTSD. The study assessed 592 adult patients for PTSD, and were granted access to each patient's Electronic Medical Record review of the past 12 months assessed Mental Health diagnoses and Mental Health treatments [Selective Serotonin Reuptake inhibitor and/or ≥ 1 visit with a Mental Health professional] (Meltzer-Averbuch, 2012). Of 133 adults with PTSD, 49% had received a Selective Serotonin Reuptake Inhibitor SRI (18%), a visit with Mental Health professional (14%), or both (17%) (Meltzer-Averbuch, 2012). Of those treated, 88% had an Electronic Medical Record - Mental Health diagnosis, the majority being depression (71%) and PTSD being the minority (17%) (Meltzer-Averbuch, 2012). The odds of receiving Mental Health treatment were increased 8.2 times for patients with an Electronic Medical Record - Mental Health diagnosis, and nearly 50% of patients with PTSD received Mental Health treatment, yet few had this diagnosis

documented (Meltzer-Averbuch, 2012). With these findings, they concluded that treatment was likely due to overlap in the management of PTSD and other mental illnesses.

One final study performed by John Wiley and Sons Inc. argued that although research has shown that PTSD symptom change correlates with an improved quality of life, the question remains as to how much improvement in symptoms is necessary to result in “meaningful improvements” to the quality of life (Schnurr-Lunney, 2016). By using data from a randomized clinical trial of psychotherapy for PTSD in female military veterans and active duty personnel, researchers were able to examine the correspondence between certain benchmarks of improvement in PTSD symptoms as well as the changes in quality of life (Schnurr-Lunney, 2016). Participants were 235 female veterans and Army soldiers who were randomly assigned to 10 weekly sessions of Prolonged Exposure or to Present-Centered Therapy (Schnurr-Lunney, 2016). There, researchers operationalized PTSD symptom change in terms of four progressively stringent, mutually exclusive definitions, (No Response, Response, Loss of Diagnosis, and Remission) successively comparing each category to the prior one (Schnurr-Lunney, 2016). What they found was that the outcomes were clinically meaningful improvements, as well as good endpoints in the domains of clinician-rated and self-reported quality of life (Schnurr-Lunney, 2016). When looking at all received data, what they found was that “Loss of Diagnosis” was associated with improvement on all measures (except self-rated social functioning) and with achieving a good endpoint on all measures, while remission was associated with improvement in clinician-rated social impairment and a good endpoint in clinician-rated occupational impairment (Schnurr-Lunney, 2016). They concluded that for most domains of quality of life, treating a patient until the patient no longer meets diagnostic criteria (Loss of

Diagnosis) would be optimal, but that for some domains, further improvements may result by helping a patient achieve total remission (Schnurr-Lunney, 2016). The findings of this study correlate with the idea that perhaps if patients were told by their physician that they do not have (or no longer have) a “disorder”, that that might play a role in the entire treatment process altogether.

Critical Review

1. Saraiya-Lopez-Castro, 2016

Who funded this research?

The research reported in this article was supported by the following grant from the National Institute of Drug Abuse: 5R25DA035161-02 (PI: Hien).

What do the authors want you to accept as “fact”?

In the abstract, the authors state that “a large percentage of individuals remain symptomatic following gold-standard therapies,” but do not further the statement with any kind of evidence or backing. Additionally, they make the focus of their paper the role of shame in PTSD, but only introduce this idea by saying it is “a common emotional reaction after traumatic exposure” and has “extensive roots in both the theoretical and clinical PTSD literature”.

Did the authors appear to have a bias before conducting the study?

I could not personally detect any bias based on their backgrounds, introductions, or affiliations. Additionally, both authors declared no conflict of interest.

Did the authors provide the data they developed? If so, did they provide an analysis?

No. This study was one conducted by identifying published studies investigating the relationship of shame to PTSD. They used the databases PubMed, PsycInfo, Embase, Cochrane, and CINAHL.

Did they critique their own work?

Yes, and they also had two Academic Editors (Frances Kay Lambkin and Emma Barrett) peer-review their work. One thing they stated in review was that there was a “widespread use of

self-report measures” in the data they were working with. This skew towards explicit shame therefore had “significant implications for research”.

Did the authors use other research to support their findings? If so, did they critique the data or work(s) of others that they used?

Yes, they collected data from a total of 47 studies and a total of 6642 participants, and they critiqued the work they found as well. They stated that when it came to the “construct of shame”, they found “significant diversity” in its definition and operationalization across studies. Shame was examined as a trait, a product of traumatic exposure, and as state shame.

Are any of their findings “opinion”; or unsupported by their data and/or the work of others?

Actually, no. The authors did a very good job of remaining relevant, and had two different sections for analyzing their data; one for concrete evaluations, and one for implications, and all of the latter were supported by the gathered data.

What else could they do to improve the support for their findings?

There is very little they could have done to improve this study, but perhaps include some original data of their own to supplement the reviewed studies they gathered the information from. They also discussed that many of the studies they used had significant limitations, including “the use of convenience samples, non-validated measures, and the lack of clear operational definitions”. As a whole, this source was objectively comparing, analyzing, and synthesizing their information coherently and in a way that properly represents my own topic as discussed in the current literature.

2. Blais RK, 2014

Who funded this research?

This material is the result of work supported with resources and the use of facilities at VA Puget Sound, Seattle Division.

What do the authors want you to accept as “fact”?

In the introduction, the author states, very bluntly, that Post-Traumatic Stress Disorder is “multidimensional”. She goes on to say why this is so, however, it seems more opinion-based than actually fact, whether the majority thinks it is true or not.

Did the authors appear to have a bias before conducting the study?

Not only could I not detect a very high bias on either side of the discussions, but there does not appear to be any conflict of interest as far as where the funding for the project came, what the results imply, and/or where the article was published, nor by whom.

Did the authors provide the data they developed? If so, did they provide an analysis?

Yes, they did provide this data, and they also analyzed it during their review of the “current literature”. The analysis showed various ties and implications with PTSD and other mental illnesses such as dysphoria, and compared these findings using multiple graphs and equations.

Did they critique their own work?

Yes, they critiqued their work and pointed out some flaws they wished to improve upon in the future, such as gaining a larger sample than just 153 people from a same basic background, as PTSD can be a problem for many people who were never in the military.

Did the authors use other research to support their findings? If so, did they critique the data or work(s) of others that they used?

No, they did not use any data besides their own, which they gathered from a conducted experiment of 153 Iraq/Afghanistan service members.

Are any of their findings “opinion”; or unsupported by their data and/or the work of others?

As discussed above, RK Blais declares very early on in the article that PTSD is “multidimensional”. She goes on to say why this is so, but again it seems more opinion-based than actual fact, whether the majority thinks it is true or not.

What else could they do to improve the support for their findings?

One thing the author pointed out was the difficulty to gain a measurable idea for one concrete factor throughout the studies when it came to why PTSD in veterans may relate to lower likelihood of seeking support. For instance, they stated that another “possible barrier” to seeking support is anticipated enacted stigma, or AES, rather than simply veterans, like their study aimed to achieve. However, as a whole, this source was fairly objectively comparing, analyzing, and synthesizing their information coherently and in a way that properly represents my own topic as discussed in the current literature.

3. Pacella-Feeny, 2014

Who funded this research?

This article was funded by a contract grant sponsor from the NRSA, with contract grant numbers being T32 MH19985, R01 MH066347, and R01 MH066348.

What do the authors want you to accept as “fact”?

While there were many things the authors wanted the reader to accept as fact, I must say they were very good about citing their information and stating where it came from, as well as

complete References Page where all of the citations can take the reader. For instance, in the introduction, the author states that rates of posttraumatic stress disorder (PTSD) in the general population “range from 6.4 to 6.8%,[1, 2] but vary by gender”, giving clear and exact citations for the information claimed.

Did the authors appear to have a bias before conducting the study?

I did not see any signs of a bias, but rather, an extensive attempt to deter any kind of bias from all parties involved. PE therapists were Master's or PhD level and study psychiatrists were board certified and experienced in anxiety disorder treatment. Treatment sessions were video or audiotaped. PE supervision occurred weekly at each site including case discussion and tape review. The administration of sertraline was overseen by Medical Directors, and integrity ratings were based on published protocols.

Did the authors provide the data they developed? If so, did they provide an analysis?

Yes, they did provide this data, and they also analyzed it with “Statistics Package for the Social Sciences” Version 21, and an alpha level of .05 (two-tailed) was used to determine significance amid the numerical data which was found. They also discussed and analyzed the implications this data had for the diagnosing and treatment of PTSD.

Did they critique their own work?

Yes. The authors stated that their findings must be interpreted with caution in light of the following limitations: unequal gender distribution, small sample size and limited power, and single-day sampling protocols to assess the CAR.

Did the authors use other research to support their findings? If so, did they critique the data or work(s) of others that they used?

While they quoted other studies often, they never actually borrowed data from any articles or the like, but stuck to their own data for most, if not all, interpretation.

Are any of their findings “opinion”; or unsupported by their data and/or the work of others?

In reading the conclusion analysis, the authors do not appear to use subjective measures to finalize data, even when speaking of implications. They interpret the data with integrity, being careful not to stretch their findings to fit some aim.

What else could they do to improve the support for their findings?

One thing the author pointed out was that though this research is suggestive of posttreatment cortisol changes, limitations exist regarding “the failure to account for diurnal cortisol variations”, and “to include both genders”. Furthermore, the extant literature has primarily focused upon “changes in cortisol levels across time”. Altogether still, this source was pretty good at objectively comparing, analyzing, and synthesizing their information coherently and in a way that properly represents my own topic as discussed in the current literature.

4. Meltzer-Averbuch, 2012

Who funded this research?

The work was supported by a Generalist Physician Faculty Scholar Award, RWJF #045452, from the Robert Wood Johnson Foundation, Princeton, NJ and by a career development award, K23 DA016665, from the National Institute on Drug Abuse, National Institutes of Health (J.M.L.). The authors would also like to thank Andrew J. Meltzer, M.D, for “providing valuable comments on the manuscript.”

What do the authors want you to accept as “fact”?

The very first sentence of the abstract is very blunt, and reads as follows: “In primary care (PC), patients with post-traumatic stress disorder (PTSD) are often undiagnosed.” I believe this is very dangerous ground because it is the opening sentence, and is not backed by any data that is being made known to the author. It almost acts to discredit them immediately.

Did the authors appear to have a bias before conducting the study?

The authors claimed to have no conflict of interest, and there were none apparent. However, there seemed to be many, many names in the Correspondence section, about 15 more names than usual. I do not know what to make of this, but it is cause for suspicion.

Did the authors provide the data they developed? If so, did they provide an analysis?

Yes, they provided the data they developed, as well as provide the proper analysis. They found that of 133 adults with PTSD, 66/133 received an SSRI (18%), a visit with MH professional (14%), or both (17%). Of those treated, (58/66 had an EMR MH diagnosis, the majority (71%; 47/66) depression and (18%; 12/66) PTSD. They then went on to describe the implications of this work analysis.

Did they critique their own work?

Yes. The authors stated that the study had limitations such as the fact that it was conducted at an urban, academic “safety net” hospital, with the majority of participants unemployed, or on disability, and earning less than \$20,000 annually. They said that “this may seem to make the results less generalizable to other practice settings”.

Did the authors use other research to support their findings? If so, did they critique the data or work(s) of others that they used?

No. The data discussed and analyzed was purely original, though references to other studies were made occasionally, and cited always.

Are any of their findings “opinion”; or unsupported by their data and/or the work of others?

While I would not say there were opinionated findings by the authors, at the end of their analysis, they do throw out a couple of hypotheses as to why the data looked the way it did, such as that “having more severe PTSD symptoms and/or having comorbid depression (by research interview) would correlate positively with participants receiving MH treatment”. This, however, is not the same as formulating a concrete conclusion off of speculation, and should actually be admired for going the extra step in analyzing data, eliminating bias, and critiquing their own work.

What else could they do to improve the support for their findings?

The cross-sectional study design does not allow for patients to be followed over time. This does limit the types of inferences one can make. For example, during the interview, patients were asked whether they have ever disclosed to a medical professional that they suffer from trauma-associated symptoms. For those patients who both answered affirmatively and also received MH treatment, one cannot assume this disclosure was made prior to receiving treatment. With everything that is being considered, this source excellently and objectively compared, analyzed, and synthesized their information coherently and in a way that properly represented my own topic as discussed in the current literature.

5. Schnurr-Lunney, 2016

Who funded this research?

This research was funded by the VA Cooperative Studies Program; Contract Grant Number: CSP #494. However, the author states that “views expressed in this article are those of the authors and do not necessarily represent the views of the Department of Veterans Affairs, the Department of Defense, or any US government agency.”

What do the authors want you to accept as “fact”?

The author states that “We” (she) agree[s]” with another researched named Kazdin, and regardless of the relevance to her own topic, I do not like when people use the term “agree” in science. She wants the audience to adopt “Kazdin’s” word as fact, but how can it be when she “agrees” with him? We agree on other opinions - not facts. Facts do not need a cosigner.

Did the authors appear to have a bias before conducting the study?

Seeing as the funding for the research was coming directly from the Department of Defense, I was wary of any type of bias while reviewing the article, but was pleased to find little to none. I believe this is because proper treatment for PTSD is something the Department of Defense ought to be concerned about, given the circumstances.

Did the authors provide the data they developed? If so, did they provide an analysis?

Yes, the data used was all their own, and yes it was analyzed, but they also used data from outside sources. From thorough examination, they found that symptom improvement following PTSD treatment was associated with improved quality of life and is consistent with findings of prior studies in veteran and non-veteran populations.

Did they critique their own work?

Yes, though not very thoroughly. Personally, I am not convinced that their findings would still stand if they chose a larger and more diverse sample, particularly of non-veterans with less chronic PTSD, and that findings would differ if other treatments were studied.

Did the authors use other research to support their findings? If so, did they critique the data or work(s) of others that they used?

Yes, they used other research for their findings, and yes their sources were critiqued. They pulled data from the Tukey–Kramer adjustment for the comparisons of demographic and clinical characteristics among the four PTSD symptom change benchmark categories. They then examined continuous change in symptoms and in each quality of life measure by predicting posttreatment quality of life in least-squares regression models scores.

Are any of their findings “opinion”; or unsupported by their data and/or the work of others?

One thing that was stated that didn't quite sit well with me was towards the end of the conclusion, when the author states that “Kazdin” discusses “a variety of strategies for assessing clinical significance and states that multiple domains may be important”. The author then states that “We” (she) agree, and hope that this study encourages investigation of how indicators of clinical significance map onto meaningful change in quality of life”. I typically try to stay away from “agreeing” in the scientific community - if you need to “agree”, then there is something up for debate, and if something is up for debate, then it is not proven fact.

What else could they do to improve the support for their findings?

Their findings should be replicated using other treatments and populations. Generalizability may be limited because the sample did not include men or non-veterans, as well. Still, with everything

that is being said, I would say this source objectively compared, analyzed, and synthesized their information coherently and in a way that properly represented my own topic as discussed in the current literature.

Summary/Conclusion

Altogether, the data collected seems to indicate that the stigma behind Post-Traumatic Stress Disorder is in fact so strong, that it does matter whether the medical community labels it as a disorder or not. In the Saraiya-Lopez study, they found 47 studies which met inclusion criteria, and stood as substantial support for an association between shame and PTSD. This review provided crucial synthesis of research to date, highlighting the prominence of shame in PTSD, and its likely relevance in successful treatment outcomes. Similarly, in the Blais RK study, their post hoc analyses showed that a similar disorder, dysphoria, significantly related to the likelihood of seeking support after accounting for anticipated enacted stigma versus those of the other PTSD clusters.

Furthermore, the Pacella-Feeny findings stood firm even after controlling for age, trauma history, baseline medication status, baseline PTSD, and baseline depressive symptoms, and what they concluded was that a loss of a PTSD diagnosis may contribute to decreased cortisol reactivity in females. The Meltzer-Averbuch study showed that the odds of receiving Mental Health treatment were increased 8.2 times for patients with an EMR - MH diagnosis, and nearly 50% of patients with PTSD received Mental Health treatment, yet few had this diagnosis documented, and they concluded that treatment was likely due to overlap in the management of PTSD and other mental illnesses. Lastly, the findings of the Schnurr-Lunney study concluded that perhaps if patients were told by their physician that they do not have (or no longer have) a “disorder”, that that might play a role in the entire treatment process altogether, based on the data gathered. Something that would improve this literature would be if there were more studies done over a wider range of participants, as most of the participants were veterans, so maybe some

other ways of PTSD would be better such as PTSD from a near-drowning, car accidents, rape, etc.

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