**Detailed response to: Letter to the Editor: *Problems with Conclusions in Army STARRS Publications***

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On behalf of the Army STARRS collaborators

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The current document provides a line-by-line response to a letter to the Editor of *JAMA Psychiatry* written by Charles W. Hoge, Christopher H. Warner, and Carl A. Castro entitled *Problems with Conclusions in Army STARRS Publications*.[1] The Hoge et al. letter was written in response to a series of three Army STARRS papers published in the March 2014 issue of *JAMA Psychiatry*.[2-4] We wrote a formal response to the letter for publication in *JAMA Psychiatry* that focused on the three main criticisms in the Hoge et al. letter,[1] but the word limits made it impossible for us to address the numerous secondary criticisms made by Hoge et al. We prepared the current document to provide a more detailed response. This response is organized by reproducing the entire Hoge et al. letter in italics and inserting our responses in the text as each criticism appears.

**1. Hoge et al. began as follows:** *The recent articles from Army STARRS conclude that soldiers have higher rates of mental disorders than civilians (especially intermittent explosive disorder—IED), that most conditions pre-existed military service, and that Army suicides are a direct result of deployments.*

**RESPONSE: The first two of these three statements are largely correct, although Hoge et al. omit some important details. We found that soldiers have higher rates of the 30-day mental disorders assessed in the Army STARRS All Soldier Survey (AAS) than "comparable" civilians. We also found that the majority of AAS respondents with 30-day mental disorders reported that they had a first onset of at least one of their 30-day disorders prior to enlistment. The third statement of Hoge et al., though, is incorrect. We did not say or imply that Army suicides are a "direct result" of deployments. In fact, we were quite clear in stating that causal interpretations of suicide trends could not be made from the naturalistic data we reported. We noted correctly that suicide risk overall was higher among currently deployed and previously deployed than never deployed active duty soldiers over the 2004-2009 time period we studied in our administrative data analysis. But we also noted that the rise in the suicide rate of active duty soldiers between 2004 and 2009 occurred not only among the currently and previously deployed, but also among the *never* deployed. We also reported in another Army STARRS paper published at about the same time as the three *JAMA Psychiatry* papers that the association between deployment history and suicide varied with rank and time in service and that the suicide rate of officers was actually higher among the never deployed than the currently or previously deployed.[5]**

**2. Hoge et al.:** *As the authors point out, these conclusions have important policy implications (such as how individuals are selected for service). It is critical to correctly interpret these articles within the context of other large "Framingham"-like initiatives, including the 67 year-long Millennium Cohort study involving >200,000 service members and veterans.*

**RESPONSE: The term "Framingham-like" initiative was first used to describe Army STARRS in the study RFP written by the National Institute of Mental Health. The term was intended to refer to a long-term naturalistic study that combined self-report information collected in surveys with health administrative data and various biological tests. The Army STARRS biological tests are based on blood samples obtained in**

**coordination with the surveys. The Millennium Cohort Study (MCS; [http://www.millenniumcohort.org](http://www.millenniumcohort.org)), in comparison, includes no biological tests. Nor is MCS a "67-year long" study. It is a longitudinal (mail-in or web based) survey of military personnel invited to participate in repeated self-report surveys that started data collection in 2001.**

**3. Hoge et al.:** *The conclusion that the mental health of soldiers is significantly worse than civilians is contingent on the civilian comparison group being representative. The National Comorbidity Study-Replication (NCS-R) subsample the authors chose for comparison is clearly not representative, as indicated by the uncharacteristically low prevalence of several disorders, for example depression <1%. National surveys from Centers for Disease Control (CDC) estimate the prevalence of depression in U.S. men and women ages 18-39 of 5% and 10%, respectively, entirely comparable to soldiers (4.8%).*

**RESPONSE: Soldiers do, in fact, have higher rates of mental disorders than *comparable* civilians. It is critical to be clear on the term "comparable," though, as there are many non-comparable samples. Hoge et al. compare the depression prevalence rate in the Army STARRS surveys with the rate in a CDC survey where roughly one-fourth of respondents had less than a high school equivalent education (making them ineligible for army service without a waiver), leading to a substantially inflated rate of depression compared to the rate that would have been obtained if the survey composition had been adjusted to be comparable to that of the Army. Our comparison sample, by contrast, was carefully constructed from the nationally representative National Comorbidity Survey Replication (NCS-R) to be identical to the active duty Army population on the joint distribution of age, gender, race-ethnicity, and education and to exclude people with serious health problems that are exclusions for Army service. Soldiers in the Army STARRS survey had substantially higher rates of current mental disorders than respondents in that representative comparison sample.**

**4. Hoge et al.:** *The very low NCS-R PTSD prevalence (0.6%) is another example of an unreliable estimate (further complicated by the soldier survey including a non-standard 9-item measure, rather than the PTSD Checklist specified by the authors).*

**RESPONSE: The criticism of the PTSD prevalence estimate is, again, unfounded. The NCS-R 30-day prevalence estimate of PTSD is the best estimate available of prevalence in the segment of the US population most comparable to Army soldiers. Hoge et al., in comparison, provide no alternative population estimate. Nor do they justify their criticism that the NCS-R estimate is inaccurate.**

**The criticism of the Army STARRS measure of PTSD is also unfounded. As reported in a previous Army STARRS report that was cited in our paper but ignored in the Hoge et al. criticism,[6] diagnoses of PTSD based on the Army STARRS screening scale were validated against clinical diagnoses based on the Structured Clinical Interview for DSM-IV (SCID).[7] Good concordance was found between the two sets of diagnoses (AUC=0.75). Furthermore, the PTSD prevalence estimate based on our screening scale was unbiased in**

**comparison to the prevalence estimate based on the SCID ($\chi^2_1$=0.1, p=0.75). It is completely inaccurate, in light of this published evidence, to assert as Hoge et al. do that the Army STARRS measure of PTSD is unreliable. In fact, Army STARRS is perhaps the first, and certainly the largest, study of a military population to have used a direct interview-validated and calibrated measure of PTSD.**

**5. Hoge et al. then turned to a rather complex criticism of our results regarding Intermittent Explosive Disorder (IED). We disaggregate our response because of this complexity.** *Kessler, et. al.'s conclusion that IED is the most common mental disorder in U.S. soldiers (11%) is not supported. The IED diagnosis is very infrequently used in military and civilian settings (e.g., <0.1% of mental health diagnoses involving soldiers).*

**RESPONSE: The fact that the IED diagnosis is infrequently used by Army clinicians is irrelevant to whether the prevalence of the disorder is high, as we know from other research that people with anger problems seldom seek treatment. Moreover, IED is a relatively new diagnosis in DSM, and one with which clinicians have, arguably, not yet gained a lot of familiarity. It is hoped that our publication of these findings will draw more attention to IED by Army and civilian clinicians alike.**

**5a. Hoge et al. then called into question the validity of our diagnosis of IED as follows:** *Problems with the clinical validity of IED have led to extensive revision in DSM-5.*

**RESPONSE: Changes in criteria for DSM-5 are not relevant because we stated clearly that DSM-IV criteria were used in the Army STARRS surveys. (This was due to DSM-5 criteria not yet being published at the time the Army STARRS surveys were developed.) But if the implication of Hoge et al.'s criticism is that the DSM-IV criteria were flawed, then the criticism overlooks the fact that the primary change in the diagnosis of IED in DSM-5 was one that *broadened*, not restricted, the diagnosis, making the Army STARRS prevalence estimate of IED conservative. The APA's "Highlights of Changes from DSM-IV-TR to DSM-5" (http://www.dsm5.org/Documents/changes%20from%20dsm-iv-tr%20to%20dsm-5.pdf) states "(t)he primary change in DSM-5 intermittent explosive disorder is the type of aggressive outbursts that should be considered: physical aggression was required in DSM-IV, whereas verbal aggression and nondestructive/noninjurious physical aggression also meet criteria in DSM-5."**

**5b. Hoge et al.:** *IED-related questions on Army STARRS surveys likely reflect anger and aggression behaviors not specific to IED, i.e., "attacks of anger" (without good reason or unable to resist) that involve breaking or smashing something, hitting or trying to hurt someone, or threatening someone.*

**RESPONSE: We noted in our paper that all of the mental disorders assessed in the Army STARRS surveys "were assessed without DSM-IV diagnostic hierarchy rules." This means that some of the soldiers who met criteria for IED might not be given a *diagnosis* of IED by**

**a clinician when hierarchy exclusions are imposed. That is, the presence of another disorder may override assignment of IED as a primary diagnosis. However, IED measured in this stand-alone fashion is nonetheless the most prevalent mental disorder assessed in the AAS and this is an important finding.**

**5c. Hoge et al. then returned to the theme of clinical significance as follows:** *The low number of attacks considered sufficient to meet the definition in Army STARRS fails to approach current DSM-5 criteria.*

**RESPONSE: We would again note that DSM-IV criteria, not DSM-5 criteria, were used in the Army STARRS survey. We would also note that the Army STARRS clinical reappraisal study validated diagnoses of IED against clinical diagnoses based on the SCID and found good concordance (AUC=0.79). The clinical reappraisal study also found that the prevalence estimate of IED based on our screening measure was unbiased in comparison to the prevalence estimate based on the SCID ($\chi^2_1$=0.1, p=0.72). This last result demonstrates that Hoge et al. were incorrect in suggesting that we used too low a diagnostic threshold in our assessment of IED.**

**5d Hoge et al.:** *These behaviors are prevalent in the general population, and it is a mistake to conclude that soldiers have greater propensity toward them. For example, the CDC Youth Risk Behavior Survey found that 34% of male and 19% of female 12$^{th}$ graders, the population from which the Army recruits, have been in a physical fight in the past 12 months.*

**RESPONSE: Hoge et al. claim that "these behaviors," presumably referring to anger attacks, are not more prevalent in the Army than in the general population, but they have no real evidence on which to base such a claim because they have no data on the distribution of IED in the AAS or in the general population. We do, though, and we showed clearly in our paper that the prevalence of IED is dramatically higher in the Army STARRS survey (AAS) than in the calibrated general population comparison survey (NCS-R).**

**Hoge et al. make no comment about this dramatic difference. Instead, they present seriously flawed "evidence" to support their claim that anger attacks are not more common in the Army than in the general population. Their data are flawed in two ways. First, the behavior described by Hoge et al. is an inappropriate comparison with our measure of IED. Getting into a fight at school is quite a different matter than having a persistent pattern of uncontrollable and explosive anger attacks out of proportion to any precipitating event that leads to significant injury or property loss. Second, the sample used by Hoge et al. is another inappropriate comparison group. This sample, the CDC Youth Risk Behavior Survey, focused on 17 year-old school children, whereas the Army STARRS AAS survey considered soldiers with a median age of 20-21 and an age range that extended into the 40s.**

**5e. Hoge et al.:** *In addition to the definitional problems, the authors did not consider the context of certain aggressive behaviors (e.g. combat environment or combatives training), the* *well-known associations with deployment-related mental health problems, or the fact that aggression is a frequent clinical focus during treatment of PTSD, bipolar, attention deficit hyperactivity disorder, and many disorders not assessed in these ArmySTARRS publications (e.g, adjustment disorders, sleep disorders, personality disorders, alcohol or substance withdrawal, traumatic brain injuries).*

**RESPONSE: It's useful to parse this sentence into the three component criticisms:**

**i. The first is that we did not consider "*the context of certain aggressive behaviors (e.g. combat environment or combatives training);*" We must admit to not understanding this clause. Is the idea that a pattern of persistent explosive out-of-proportion anger attacks leading to injury or property loss should not be a source of concern if it occurs in the context of a "combative environment"? If so, we disagree.**

**ii. The second is that we did not consider "*the well-known associations with deployment-related mental health problems;*" Again, we must admit to not understanding the clause. We're not sure what association Hoge et al. are saying is well-known. The association of IED with deployment-related mental health problems? The association of anger attacks with deployment-related mental health problems?**

**iii. The third is that we did not consider "*the fact that aggression is a frequent clinical focus during treatment of PTSD, bipolar, attention deficit hyperactivity disorder, and many disorders not assessed in these ArmySTARRS publications (e.g, adjustment disorders, sleep disorders, personality disorders, alcohol or substance withdrawal, traumatic brain injuries).*" We assume that the implied criticism here is the same one as in Point #5b above: that our characterization of the IED diagnosis is incorrect. See our response above to that point.**

**5f. Hoge et al. then go on criticize our finding that IED is a predictor of the subsequent onset of suicidality as follows:** *The finding by Nock, et. al. of the strong association of IED with suicide attempts also supports the conclusion that IED survey items actually measured non-specific anger-related behaviors inherent to numerous conditions.*

**RESPONSE: This criticism is not only incorrect, but it misses a potentially important aspect of the results regarding IED in the Nock et al. paper: that anger attacks and IED, far from being epiphenomenal, might represent a common pathway through which a number of comorbid mental disorders influence suicidality. The key point missed by Hoge et al. in their interpretation of the Nock et al. result is that IED was found to be a strong predictor of suicidality in a multivariate model that controlled for major depression, bipolar disorder, panic disorder, PTSD, OCD, phobia, ADHD, and substance use disorders. Furthermore, comparison of the association of pre-enlistment history of IED with suicidality in a bivariate model (i.e., the association before introducing controls for these other disorders) with the association in a multivariate model (i.e., the association in a model that controlled for these other disorders) shows the two coefficients to be virtually identical (3.9 elevated relative-odds in the bivariate model and 3.7 in the multivariate model), whereas the bivariate associations of most other pre-enlistment mental disorders were smaller in multivariate than bivariate models, raising the possibility that the gross associations of these other disorders with suicidality might be mediated through IED. We are unclear how Hoge et al. conclude from these results that "*IED survey items actually measured non-specific anger-related behaviors inherent to numerous conditions*." Were that**

**the case, the multivariate association of IED with suicidality would be much smaller than the bivariate association.**

**6. Hoge et al. then turn to another line of criticism, in this case concerning the age of onset of the mental disorders assessed in the Army STARRS surveys:** *Kessler's conclusion that over 75% of soldiers who met criteria for a mental disorder experienced onset prior to coming in the Army is also unreliable. Time of onset for IED, for example, was determined using a single question that asked soldiers who screened positive for IED how old they were the first time they had an anger attack (which was compared with service entry date). Similar approaches were used for the other conditions. Certainly this is not sufficient to conclude that a disorder pre-existed service, or that the level of impairment was sufficient to meet diagnostic criteria or prohibit military service.*

**RESPONSE: Most active duty soldiers with current mental disorders did, in fact, report that the first onset of their temporally primary mental disorders occurred before the age of enlistment. Hoge et al. base their criticism of this conclusion on our use of retrospective reports to define age-of-onset (AOO). This criticism ignores two important points. First, an extensive literature cited in our paper, but ignored by Hoge et al., documents that prospective data converge with retrospective data in finding early AOO distributions of mental disorders consistent with those found in our report.[8] Second, Army STARRS used the same assessment methods with approximately 57,000 Army recruits who had just begun Basic Training.[9] We found high rates of prior lifetime mental disorders in that survey consistent with those implied by the retrospective reports about high rates of pre-enlistment onsets in our representative sample of all non-deployed soldiers, providing strong support for our claim that the majority of soldiers with current mental disorders had first onsets before enlistment.**

**7. Hoge et al. then turn their attention to the Schoenbaum et al. paper on patterns of suicide in Army administrative data. This critique begins with the following sentence:** *Schoenbaum, et. al. suggests a direct association between deployment and suicides not found in the Millennium Cohort study, and hypothesizes that study differences stem from service differences, response biases, or "people with emotional problems" having lower rates of survey participation.*

**RESPONSE: The reader should note that in Point #1 Hoge et al. claimed that we said "*Army suicides are a direct <u>result</u> of deployments* (emphasis added)," whereas in this passage they change the statement to claim that we said there is a *"direct <u>association</u>* (emphasis added)" between deployment and suicides. These are two very different statements, the first implying causality ("result of") and the second not implying causation. As noted in our response to Point #1 above, we never made any suggestion of causation between deployment and suicide even though we did find that in the total population of soldiers on active duty at any time between January 2004 and 2009 the gross suicide rate (i.e., without any controls for other correlates) among never deployed soldiers (15.6/100,000 person-years) was lower than among currently deployed (20.0/100,000 person-years) or previously**

**deployed (20.5/100,000 person-years) soldiers.**

**We made no causal interpretation of this pattern because there are many well-known differences between never deployed soldiers and ever deployed soldiers that could lead to an association between deployment and suicide in the absence of deployment causing an increase in suicides. Just to name a few: deployed soldiers differ from other soldiers in MOS, time in service, and in the fact that soldiers with recognized serious emotional problems are restricted from deployment. Furthermore, while currently and previously deployed soldiers might seem on a superficial level to be more similar in these regards, they differ in at least one very important way that has implications for suicide risk: that a much higher proportion of currently deployed than previously deployed soldiers are serving in their first term, when suicide risk is greatest. We know that soldiers with a history of treatment for mental disorders in their first term are significantly more likely than other soldiers not to reenlist for a second term, leading to non-comparability in selection factors among the currently and previously deployed. These kinds of complexities make it extremely hazardous to draw plausible causal inferences from associations between deployment status-history and suicide.**

**Returning to the above quoted sentence from the Hoge et al. letter: Hoge was the senior author of a recent paper published in *JAMA* from the Millennium Cohort Study (MCS)[10] that was mentioned by Hoge et al. in their criticism. Schoenbuam et al. noted in their discussion section that the gross association between deployment history and suicide found in the Army population data we analyzed was not found in the paper by Hoge and colleagues. Schoenbaum et al. then commented that this difference in pattern between the two studies could be due either to a difference in populations studied or to a response bias in the paper by Hoge and colleagues. In the current criticism, in addition to claiming inaccurately that Schoenbaum made a causal claim, Hoge et al. seem to be suggesting that we were in error in claiming that an association exists between deployment and suicide. A little bit of reflection on the Army STARRS and MCS designs makes it clear that this criticism is completely wrong, but in order to clarify this point for the reader we need to digress for a moment and describe the MCS.**

**As noted above in response to Point #2, MCS is a large longitudinal survey of three cohorts of military personnel across all services invited to participate in repeated self-report surveys designed, according to the recent paper by Hoge and colleagues in *JAMA*, "to study the health impact of serving in the military." (The cohorts include samples of personnel on military rosters in October 2000, October 2003, and October 2006.) The two key points about the MCS design for our purposes are as follows: (1) MCS is a <u>sample</u> while the Schoenbaum et al. paper is based on the entire <u>population;</u> and (2) MCS includes both active and deactivated personnel from all services while the Schoenbaum paper is based on active duty Army soldiers.**

**The Hoge and colleagues' paper in *JAMA* used many measures obtained in the MCS surveys to predict the 83 suicides completed by sample members as of the end of 2008. The paper did not report how many of those suicides were completed while respondents were still on active duty. Nor were suicides broken down by individual military service. As a result, data are not presented that can be compared directly to the population data we reported in our Army STARRS paper. It is important to note, though, that the data reported in our paper are <u>population</u> data – the actual suicide rates in the entire active duty Army over the study period – whereas the data reported in the paper by Hoge and**

**colleagues is based on an aggregated <u>sample</u> of military personnel across all services and including suicides completed both while on active duty and after leaving active duty. This means that there is no genuine inconsistency between the two sets of results. It could very well be that the suicide rates by deployment history among the subset of MCS respondents who are active duty Army soldiers are the same as the rates we reported for the population.**

**It is nonetheless noteworthy that to the extent to which the suicide rate among active duty Army personnel in the MCS might differ from the Army STARRS rate, the difference would have to be due to a bias in the MCS sample, as the population data reported in the STARRS report provide the true rates in the actual population. It is noteworthy in this regard that the MCS response rates are low (36% response rate in the first cohort, 21% in the second, and 22% in the third), which means that the suicide rate among military personnel in the MCS sample could very well differ from the true population rate, an observation made by others prior to publication of the Schoenbaum paper.[11-13]**

**The above observations support the claim in the Schoenbaum et al. paper that the failure of Hoge and colleagues to find a gross association between deployment status-history and suicide in the MCS <u>sample,</u> whereas we found such an association in the Army <u>population,</u> is due to some combination of (i) differences in sampling frames (i.e., the MCS sample including military personnel in all services both in and out of service while the Army STARRS sample focused on the active duty Army personnel) and (ii) bias (i.e., the possibility that 21-36% of originally sampled military personnel who agreed to be MCS respondents differed from the 64-79% who were non-respondents in rates of subsequent suicide).**

**8. But Hoge et al. proposed an alternative interpretation:** *A more likely explanation not mentioned by Schoenbaum, et. al. is that his risk-factor analysis only presented unadjusted bivariate associations, whereas the Millennium Cohort study conducted multivariate analyses (linking DoD records with national death records) that controlled for important demographic differences, combat exposure, and mental disorders.*

**RESPONSE: This suggestion is incorrect, as the bivariate association between deployment status-history and suicide differs in the two studies. Adjustments for control variables are irrelevant in this regard. The basic descriptive association between deployment and suicide is different in the two studies. That difference would not exist if the two studies were examining the same population and the MCS sample was truly representative with respect to suicide risk.**

**9. Hoge et al. then go on to say:** *Nock, et. al. also failed to control for combat and underlying mental disorders in his analysis of deployment and suicide attempts.*

**RESPONSE: We are confused by this comment, but we will respond to what we think Hoge et al. are suggesting. The Nock et al. paper analyzed survey data for active duty soldiers who were either never deployed or previously deployed at the time of the survey. Currently deployed soldiers were not included in the sample. As a result, Nock et al. considered the association between number of deployments (0, 1, 2, 3+) and suicidality. The**

**results of a basic descriptive model were presented by Nock et al. before considering the associations of temporally primary mental disorders with the subsequent onset of suicidality. A dose-response relationship between number of deployments and subsequent onset of suicidality was found. As in the Schoenbaum paper, no causal interpretation was made of this association. It was merely a descriptive association that was found in the data and reported in our paper.**

**We assume that Hoge et al. are criticizing this analysis because they feel that we should have controlled for experiences that occurred during the deployment (e.g., combat, although this is not the only significant stressor that increases in prevalence during deployment) and mental disorders that might have been caused by deployment-related experiences. We take it that this criticism is related to the repeated reference of Hoge et al. in their letter as well as of Hoge and colleagues in their recent *JAMA* paper to "direct" effects of deployment, (see the statement by Hoge et al. in Point #1 above). The notion here seems to be that deployment *per se* does not cause suicide, but that the stresses and associated mental disorders caused by deployment cause suicide, resulting in deployment having no "direct" effect.**

**We have no quarrel with the suggestion of this possibility, but we object to Hoge et al. saying that we "failed" somehow in our analysis because we did not attempt to address the question of whether deployment is a cause of suicidality or the question whether this gross causal effect is mediated by deployment-related stressors. Had we wanted to address these questions, we would have had to develop a logic that allowed us to estimate what we considered to be a plausible estimate of the gross causal effect of deployment and then to introduce control variables (e.g., combat, mental disorders) that we could plausibly argue were consequences of deployment. None of this was attempted in the Nock et al. analysis. We were merely presenting descriptive associations. A serious effort along these lines would be much more complex than implied by Hoge etal. in their criticism.**

**10. Hoge et al. then turn back to the Schoenbuam et al. paper:** *Schoenbaum's own figures show Army suicides rising just as sharply during the study years in soldiers who never deployed as in currently or previously deployed soldiers. These data clearly show that deployments do not directly explain the sharp increase in suicide incidence. This is not to suggest that the extended conflicts in Iraq and Afghanistan have had no cumulative impact. War-zone deployment, particularly combat intensity, has contributed to a rise in mental disorder prevalence, but prevalence has also risen overall in the population; the presence of mental disorders is the strongest predictor of suicide. In other words, deploying to a war zone itself does not necessarily confer increased suicide risk, but increased risk does occur for individuals who develop mental disorders from deployment or non-deployment stressors.*

**RESPONSE: Hoge et al. are confusing two issues here: that the Army suicide rate rose over the time period 2004-2009 not only among those with a history of deployment but also among the never deployed; and that the suicide rate was consistently higher among those with a history of deployment than the never deployed. Both of these patterns were reported in the Schoenbaum paper. They are not inconsistent with each other. It is only when Hoge et al. incorrectly ascribed to us the claim that deployment "directly explains" suicide, a claim we never made, that the rising suicide rate among the never deployed takes on the**

**appearance of being inconsistent with our interpretation.**

**11. Hoge et al. close as follows:** *With the conclusion of operations in Iraq, the drawdown in Afghanistan, and the projected Army downsizing, we expect to see gradual declines in prevalence of mental disorders as well as suicide incidence. This first set of publications from the $65 million Army STARRS epidemiological effort presents a misleading perspective of the mental health of U.S. soldiers, and has the potential to misinform public policy, health care strategies, and military accession standards. We hope future Army STARRS publications will better integrate findings with existing research and contribute to actionable recommendations.*

**RESPONSE: We need to parse this closing paragraph:**

**i. We agree with the first sentence that "*(w)ith the conclusion of operations in Iraq, the drawdown in Afghanistan, and the projected Army downsizing, we expect to see gradual declines in prevalence of mental disorders as well as suicide incidence*" to the extent that it applies to new soldiers. But it's unclear whether the same decline will occur among active duty soldiers or veterans who were deployed, experienced deployment-related traumatic events, and currently suffer from mental disorders related to those experiences. Will the mental health of those soldiers and veterans improve or worsen over time? It's almost certain that some soldiers will have one of these trajectories and other soldiers the other, although the proportions with each and the key factors influencing these trajectories are difficult to reckon based on currently available research. The examination of these trajectories and their predictors will be a central focus of our ongoing work with the Army STARRS cohort.**

**ii. We disagree with the second sentence that "*(t)his first set of publications from the $65 million ArmySTARRS epidemiological effort presents a misleading perspective of the mental health of U.S. soldiers, and has the potential to misinform public policy, health care strategies, and military accession standards.*" There was nothing misleading in our results. Indeed, as we showed above, none of the Hoge et al. criticisms has merit and all of the conclusions of our papers are supported. Soldiers do, in fact, have higher rates of current mental disorders than civilians. A majority of these soldiers had first onsets before entering the Army. These disorders are related to risk of suicidality. And 2004-2009 Army administrative data show clearly that suicide rates were substantially higher among currently and previously deployed than never deployed soldiers. While speculative causal interpretations can, of course, be made about the meanings of these results, it would be hazardous to do so based on such preliminary results. We reject the straw man causal interpretations that Hoge et al. inaccurately ascribed to us.**

**iii. The final sentence of the Hoge et al. critique calls for us to "*better integrate findings with existing research*" in our future publications. We're not exactly sure what that means, as we made every effort to begin with a firm understanding of previous research when we designed Army STARRS and we incorporated knowledge of previous findings into our analysis and interpretation of our results. In the three papers under discussion here we highlighted similarities and differences in our findings with other research. Indeed, that is precisely why the Schoenbaum et al. paper pointed out in its discussion section that "*(t)here is only one result reported here that would appear to differ from previous studies: our finding of elevated suicide risk among currently and previously deployed soldiers differs from*"**

***the finding of no association between deployment history and suicide in a recent report from the Millennium Cohort Study (MCS).”* We then went on to comment on the fact that the MCS sampling frame was different from the Army STARRS sampling frame (i.e., all services, including an unspecified proportion of respondents who were no longer on active duty versus the active duty Army) and that the MCS study was based on a sample with a very low response rate while the Army STARRS study was based on the true population data. We're not sure how Hoge et al. concluded from this that we failed to "integrate" our findings with existing research, but we feel quite confident that we did exactly that.**

**The final clause of the final sentence of the Hoge et al. letter calls on us to do more to "*contribute to actionable recommendations.*" We are doing our utmost to do just that. It would be premature to make actionable recommendations based on the preliminary results reported in the first three substantive Army STARRS papers under discussion here, but these papers were designed to provide a descriptive foundation on which to build future more in-depth analyses designed to produce actionable findings. The first Army STARRS paper to build on these three, which was published at virtually the same time as the *JAMA Psychiatry* papers, presented our first actionable recommendations. The challenges in producing such recommendations are great due to the complexity and rarity of the phenomenon under study and the difficulties in making plausible causal interpretations from data having a range of selection biases (most notable, the facts that risk factors for suicide might be related to volunteering for Army service, to selection out of deployment once in the Army, to exposure to a variety of experiences that are thought to be risk factors for suicide, and to early attrition from Army service). We are constantly aware of these complexities in planning our analyses and interpreting results. We welcome thoughtful ideas from external commentators to help us address these challenges.**

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