Expert Report of Stuart Grassian, M.D.
in Reply to April 18, 2017 Expert Report of Paul Gendreau

***British Columbia Civil Liberties Association and the John Howard Society of Canada v. Attorney General of Canada,* SCBC Vancouver Registry No. S-150415**

1. I have been asked to provide an expert report in relation to the above action. I certify that I am aware of my duty under Rule 11-2 of the British Columbia Supreme Court Civil Rules to assist the Court and not to be an advocate for any party. I have given this report in conformity with that duty. If I am called on to give further testimony it will also be in conformity with that duty.

A. Overview

2. I am a Board-certified psychiatrist with extensive experience in evaluating the psychiatric effects of solitary confinement. On November 25, 2016, I submitted an expert report in this matter, and on April 18, 2017, Dr. Paul Gendreau submitted a report disputing my opinions. I am in this report responding to Dr. Gendreau's report.

3. Dr. Gendreau's report is lengthy and expresses a large number of opinions. For the sake of clarity, I will in the body of this report respond to what I take to be the major arguments he expresses, and in doing so will attempt to cite to the relevant paragraphs of the Gendreau report. In an effort to avoid making this report unreadable, I will use footnotes to add more detail and will not respond to some of the more minor or tangential statements in the Gendreau report.

B. The Nature of Scientific-Medical-Psychological Knowledge

4. In his report, Dr. Gendreau makes some sweeping assertions as though they were unassailably true, when in fact they are not, for reasons to be explained below. The major arguments he puts forward in attacking my November report are:

a. that the conclusions published in my 1983 American Journal of Psychiatry article, and the extensive literature review published in my 2006 Washington University article are the product of a shoddy, unreliable methodology, and

b. that the Morgan study that I criticize in my November 2016 report should be seen as the gold standard of research in this area.

i. Dr. Gendreau Cites Two Means of Knowledge Acquisition

5. In ¶33 of his report, Dr. Gendreau makes a sweeping assertion as to the manner in which scientific (in this case, psychological) knowledge has been acquired. He states that such knowledge is acquired in two ways:

a. The first method he describes as information rooted in testimonials, anecdotes, and personal intuition - which he characterizes as essentially "common sense" declarations. He disparages this type of evidence as being very unreliable.

401 Beacon Street • Chestnut Hill, MA 02467
Tel: (617) 244-3315 Fax: (617) 244-2792
Email: stgrassian@gmail.com

b. The second is data that is quantifiable. Dr. Gendreau describes a "paradigm shift" that has in his opinion created the gold standard for psychological research - namely, the meta-analysis of multiple studies with quantifiable data. My November 2016 report was a response to an expert report of Dr. Jeremy Mills, and largely focused upon studies included in a meta-analysis in which he and Dr. Gendreau were co-authors.[1]

ii. A Third Means of Knowledge Acquisition

6. By implication, Dr. Gendreau puts my American Journal of Psychiatry ("AJP") article in the first, disparaged category, as though it were basically anecdotal in nature (he makes this assertion in part because my article was based upon observation of a small number of individuals - 14 in total). However, his sweeping assertion ignores another major source of medical and psychiatric knowledge - the description of a novel, or distinct, syndrome. This generally involves the description of unusual symptoms, or the particular constellation of a group of symptoms, and factors that seem to have a causative relationship to the syndrome or factors that seem to ameliorate or relieve the syndrome.

7. It is not uncommon for such knowledge to be acquired by observation of only a small number of individuals. In my report, I cited the discovery of AIDS as a typical example of medical knowledge acquisition of this sort. As I stated there:

> Consider for example how AIDS was identified. It only took a handful of patients who had developed a very unique set of symptoms (e.g. Kaposi's Sarcomas, Pneumocystis Carinii Pneumonias, a very low T-cell count, HIV virus in their blood, etc.) and who all apparently shared causal factors (primarily - the sharing of bodily fluids through transfusion or sexual contact) in order to decisively identify a new disease.

C. Dr. Gendreau's Attempt to Dismiss the Import of My 1983 American Journal of Psychiatry Article

i. The Identification of a Particular, "Pathognomonic" Syndrome Associated with Solitary Confinement

8. Dr. Gendreau dismisses my identification of a particular syndrome associated with solitary confinement, claiming it cannot be compared with the identification of purely "medical" illnesses such as AIDS, that somehow the illness I described is qualitatively different than a purely "medical" illness, that it is somehow too subjective to be given much credit. In ¶50 of Gendreau's Report, he asserts that in comparison to the identification of AIDS, the effects of solitary confinement cannot be understood without recognizing that it: "...is a much more complex matter as it involves a myriad of individual differences in offenders and organizations...". This statement reflects a lack of understanding that while there may be "myriad differences" in the individuals and conditions of confinement, the result is the same,

[1] Morgan, R. et. al., (2016) *Quantitative syntheses of the effects of administrative segregation on inmates' well-being.* Psychology, Public Policy, and Law, Vol. 22(4), Nov 2016, 439-461, found at http://dx.doi.org/10.1037/law0000089

very particular neuropsychiatric syndrome. The very same thing is true for individuals who develop AIDS; they may vary in a whole host of ways, but they share two things: one is the exposure to a particular toxic event; the other is the development of a particular syndrome.

9. Dr. Gendreau is primarily an experimental psychologist, not a clinician. He apparently is unaware that the particular syndrome I described - delirium - is a very different syndrome than the pure psychiatric syndromes, such as seen in schizophrenia or bipolar mood disorder. Delirium is not a psychiatric syndrome and is not seen in ordinary psychiatric practice. It is actually a medical - neuropsychiatric - disorder and results from an impairment in consciousness - in the individual's capacity to maintain an adequate state of alertness and orientation to the environment. Mistakes in diagnosis between delirium and these other, more purely psychiatric conditions, should never occur. Indeed, where misdiagnosis does occur, it is typically that delirium - an acute neuropsychiatric illness requiring aggressive treatment - is mistaken for dementia, a chronic neuropsychiatric condition.

10. Delirium is most commonly caused by situations in which there is an internal derangement of the brain's reticular activating system - the brain function controlling wakefulness and alertness. These derangements include, for example, sepsis, respiratory failure, congestive heart failure, liver or kidney failure, and so forth. Delirium is often a major problem in Intensive Care Units, in the treatment of very ill patients.

11. However, delirium can also be caused by a wide variety of situations in which there is an inadequate level of external stimulation. A description of such situations is included in my Washington University article (Appendix A of that article, pp. 354-66); it includes various medical situations - including eye-patched patients following eye surgery, patients in prolonged traction or other situations of prolonged bed rest with minimal stimulation. In ¶68, Dr. Gendreau suggests that the other situations of reduced environmental stimulation that I describe are not relevant to what happens to prisoners in solitary confinement. He fails to understand that there is a particular syndrome associated with decreased environmental stimulation, whatever the situation in which it occurs. Similarly, in ¶113, he argues that the symptom constellation I described might have been experienced by the inmates even before their incarceration. He is entirely mistaken. This symptom constellation - the syndrome of delirium - would not have been experienced by these inmates in settings other than solitary confinement (or perhaps, if they had been close to their deathbed in an Intensive Care Unit).

12. In ¶56, he describes my article as providing a "theoretical conceptualization" of the issue, and repeats this notion in ¶58. This is simply incorrect. There is nothing at all theoretical in the article; it is entirely empirical and observational. Stating otherwise reflects a lack of understanding of the nature of medical knowledge. Delirium has symptoms that are readily distinguishable from those of psychiatric disorders. These symptoms include:

a. Direct Manifestations of Decreased Alertness

i. Delirium is fundamentally an impairment of consciousness. Disturbances in thinking, concentration and memory are central to it; in more severe cases, the individual becomes entirely confused and disoriented. (Experimental studies with volunteers has demonstrated that this decrease

in consciousness is visible in the EEG within a few days as a generalized slowing of brain waves.)

ii. This impairment is fluctuating. It is especially severe in situations in which the individual experiences decreased environmental stimulation - for example, at night when there is less visual and auditory stimulation.[2]

b. A Wide Range of Perceptual Disturbances

i. In pure psychiatric disease, perceptual disturbances consist primarily of auditory hallucinations of voices, generally saying things that are meaningful and directed towards the individual. Visual hallucinations are relatively rare, and when they do occur, they are almost invariably of life-sized people uttering the voices. Other types of perceptual disturbance are virtually never described.

ii. On the other hand, in delirium the perceptual disturbances are far broader in scope and include distortions of perception (e.g. the blanket is misperceived as a moving body of water) and a lack of perceptual constancy (walls move in and out, objects become larger and smaller).

iii. In delirium, hallucinations are found in multiple modalities - for example, gustatory (tasting something when there is nothing in the mouth), olfactory (smelling something that is not there) or tactile (e.g. a feeling that worms are crawling inside the individual's skin).

iv. Hallucinations are more commonly initially not meaningful, but instead are "simple, elementary"- e.g. shadows, bugs, or even "pink elephants".

v. Individuals become "hyperresponsive" to external stimulation - jumpy, overly reactive. This problem may manifest as a heightened distractibility; the individual's attention is diverted by minor, irrelevant stimuli, a phenomenon that further impairs cognitive focus and concentration.

c. Psychomotor Activity

i. Psychomotor activity fluctuates between extremes of stupor and lethargy on the one hand, to extreme agitation ("psychomotor excitation") on the other. The latter is often associated with random, chaotic violence, often self-directed ("Motor excitement, often associated with sudden, violent destructive outbursts").

[2] In the prison setting, relief from solitary confinement typically produces a rapid abatement of the acute symptoms of delirium. This is not however to state that there are no lasting psychological burdens associated with solitary confinement - including Posttraumatic Stress Disorder.

ii. Its Publication in the American Journal of Psychiatry

13. It should be noted that this article was published in the AJP, the official research journal of the American Psychiatric Association and the preeminent peer-reviewed psychiatric journal in the United States. (Since its publication, I have described hundreds of other cases in which this syndrome developed while an individual was in solitary, and abated upon release. In my Washington University article, I provide a literature review describing a vast number of observations that are very parallel to my own. In the German literature cited therein, there are frequent references to the "particular stamp" of the psychiatric illness associated with solitary confinement.) Dr. Gendreau asserts that my AJP article would not be accepted by the journal if it were submitted now, but this assertion reflects a lack of understanding of the nature of the knowledge acquisition upon which the article is based.

iii. Dr. Gendreau's Attack on the Integrity of the AJP Article

14. In his attempt to disparage this article, Dr. Gendreau makes a number of assertions that are untrue:

a. In ¶90, he claims that although my November report described problems with paper and pencil tests, I myself relied upon them in my AJP article. There is no basis at all for this assertion, and it is simply not true.

b. In ¶98, he claims that there were inmates whom I interviewed who: "did not seem to be aware of the dire stress they were experiencing" and that I led them: "deliberately prompted to divulge [what I believed to be] the appropriate symptoms." This is untrue. I in no way led the inmates whom I interviewed. Indeed, I was unaware at the time that there was a specific syndrome associated with solitary confinement, and so even if I <u>wanted</u> to lead the inmate responses, I would have had no idea <u>where</u> to lead them. Moreover, as I stated in my publications, I was initially skeptical.

15. What I wrote in my 2006 Washington University Journal of Law & Policy article is this: "When I initially agreed to evaluate the Walpole prisoners I had not yet reviewed the literature on the psychiatric effects of solitary confinement and I was somewhat skeptical; I expected that inmates would feign illness and exaggerate whatever psychiatric symptomatology they suffered. I discovered, however, something very different."

16. In ¶¶98 and 99, Dr. Gendreau keeps pushing the idea that my questioning biased the inmates' answers. It is not clear whether he actually read what I wrote, or rather, decided that I was lying about it.

D. The Morgan Meta-Analysis: Dr. Gendreau's General Comments

17. As noted above, Dr. Gendreau asserts that studies generating quantifiable data are the best source of knowledge in psychology (and that meta-analyses of such studies is the gold standard of research in the area). In my November 2016 report, I note that a meta-analysis is only as useful as the data - the underlying studies - that go into it. The bulk of my November

2016 report is a critique of the studies underlying the *Morgan* meta-analysis. Dr. Gendreau argues against this critique.

i. Rating Scales and Psychological Tests; The Problem of Validity

18. Almost all of the studies underlying the *Morgan* article are based upon paper and pencil tests and rating scales that generate quantifiable data. In ¶90, Dr. Gendreau states that although I raise concerns about paper and pencil tests, I have used them myself. This is simply not true; I have no understanding why Dr. Gendreau makes this statement.

19. It should be understood that I do not decry the use of such tests, and make no such statement in my November report. I simply point out that such tools are subject to problems with validity, especially when administered to individuals who are in relatively unusual situations. There are intrinsic problems with research using quantifiable data such as scales and tests, namely, that relying upon paper and pencil tests as stand-ins for the actual clinical phenomena is only meaningful if those stand-ins are shown to be valid - to accurately reflect those clinical phenomena.

20. In my November report, I pointed out that the validity of a psychological test can only be determined by comparing the data generated by the test with information gained by direct clinical evaluation by a well-trained and seasoned clinician. In ¶54, Dr. Gendreau mischaracterizes this statement; he distorts it to read that I am asserting that only direct clinical observation can predict future behavior. No. I am only stating that the process of validation of psychological tests involves assuring that the results from such tests are consistent with those gained from direct clinical evaluation. (It is also odd that he limits the issue to the prediction of future behavior; my work involves the present, and the symptoms I describe are not limited to those manifest in particular behaviors.)

21. And even when a test is validated for a given population, it may not be valid for another population. For example, in ¶52, Dr. Gendreau states that The Beck Depression Inventory (a paper and pencil self-report of depressive symptoms) has been validated for a number of population groups. Such groups include individuals seeking psychiatric treatment, and students taking the Inventory as part of a psychology course. But this does not mean it is valid for this population - inmates who have nothing to gain, and possibly something to lose, if they reveal their problems.

22. As another example, on the MMPI[3] there is one question inquiring whether the person thinks he is being followed. For many populations, a positive answer would suggest either a serious mental illness (paranoia) or else lying ("faking bad"). But for example, plaintiffs claiming work disability are in a very different position; they may in fact be correct that they are being followed (by investigators for an insurance company, attempting to learn of their functional capacity).

23. Prison inmates are a subpopulation in their own unique situation. They have an intrinsic disincentive against revealing psychological problems or vulnerabilities. They naturally have

[3] The Minnesota Multiphasic Personality Inventory, a self-administered paper and pencil test.

little reason to trust that anything they reveal will be held in confidence, and they naturally fear that if they reveal vulnerability or problems, those revelations may be exploited, or for example to create doubt in administrative officials that the inmate is healthy enough to be paroled. Certainly there may well be some situations in which an inmate might feel safe to reveal psychological problems he is having, and might in fact do so in the hope of gaining some advantage (for example, mental health treatment) by doing so, but in none of the studies I critiqued in my November 2016 report was there any such incentive.

ii. Conditions of Solitary Confinement

24. In ¶¶70-73, Dr. Gendreau makes some general observations about the need for a "precise definition" of solitary confinement conditions. His discussion is difficult to follow, but seems to suggest that inmates in solitary are exposed to significant amounts of perceptual stimulation, including artificial as well as natural light, doors slamming, staff talking, and so forth. In doing so, he appears to conflate two forms of stimulation - stimulation that is meaningful and anchoring, versus stimulation that is noxious and aversive. Aversive stimulation does not ameliorate the effects of inadequate environmental stimulation in solitary confinement; such stimulation actually worsens those effects. Lastly, not all "stimulation" will ameliorate the effects of perceptual deprivation. Only meaningful stimulation - stimulation that can help focus and maintain attention - is ameliorative. In the context of perceptual deprivation, the introduction of noxious stimulation is known to worsen the psychiatric toxicity of the situation. For example, in interrogating suspected IRA terrorists, the British employed high volume white noise as a means of worsening the effect of solitary confinement. At Guantanamo, the U.S. employed both temperature extremes and high decibel white noise to exacerbate the effects of solitary confinement.

25. In ¶74, Dr. Gendreau makes another statement putatively contradicting the conclusions in my AJP article, arguing that while general population provides more meaningful environmental stimulation than does solitary confinement, it has its own stressors, including that being in population can be interpersonally complicated and potentially dangerous. This is certainly true, but it is irrelevant. General population can cause psychological distress, but not the particular syndrome associated with solitary confinement.

iii. EEG Effects

26. In ¶¶105-09, Dr. Gendreau comments about his and others EEG studies that demonstrate EEG slowing, a finding consistent with stupor and delirium, after a number of days in solitary confinement. He notes that these EEG findings are associated with heightened EEG response to external stimulation, and speculates that this might in some fashion be a good thing. Dr. Gendreau has apparently not inquired in depth of inmates who have experienced such heightened response to external stimulation, because such inquiry would have revealed that the hyperresponsivity to external stimulation caused by solitary confinement is a painful, burdensome experience, one of the greatest continuing burdens after release from such confinement.

E. The Colorado Study

27. In responding to Dr. Gendreau's critique of my critique of the studies underlying the *Morgan* article, I am tempted to simply repeat my critiques, because for the most part he fails to address them. However, since that would be redundant, I will limit my response to the one study that both my November report and Dr. Gendreau's report discuss in substantial detail, and will attempt to respond only to statements he makes that I believe to be demonstrably incorrect.

i. Validity of Self-Report Scales

28. Dr. Gendreau's discussion of this study begins at ¶82. He begins by lauding the instruments used as having been well-validated. But in so doing, he ignores the concern raised in my November report that although these rating scales have been validated for some groups of individuals, they will not be valid if the test-taker is not disposed towards responding accurately to the instrument.

ii. "Natural" versus "Experimental" Studies

29. In ¶¶83-84, Dr. Gendreau lauds the Colorado study because it is a "natural" study, one that looks at real people in real situations. And since the researcher has no say in where a particular inmate is housed, they pose no ethical problems. He then expresses disdain towards those with a too academic mindset, researchers who "live in a utopian world where all experiments are perfectly controlled." He goes on to describe the chaos that would result if such perfect controls were instituted.

30. These arguments appear almost as though Dr. Gendreau were tilting at windmills. He states explicitly (¶2) that his report is a response to my November report. But my essential finding, reflected in my AJP article, is in fact based entirely on observations in a natural setting. Moreover, I have simply never argued in favor of perfectly controlled experiments.

iii. "Punitive Segregation"; "Instantaneous Effects"

31. Once again, in ¶85, Dr. Gendreau argues vehemently against propositions that I have never made. He starts by claiming that I asserted that exposing inmates to a few days of Punitive Segregation before transfer to Administrative Segregation (Ad Seg): "... must have had a devastating effect on the integrity of the study." I have no idea why he attributes this proposition to me, since I have never stated such a thing. And he also implies that I have asserted that Punitive Seg in Colorado entails conditions that are harsher than those in Ad Seg, which I have never done. Indeed, while I do not know, I actually doubt whether there is any substantial difference in the conditions in these two settings.

32. Dr. Gendreau goes on to argue against my purported claim that: "Dr. Grassian is on record throughout his report that the effects of [solitary confinement] are instantaneous." He goes on to argue - apparently against another statement he believes I have made - that: "It assumes an extreme view of behavior that asserts that situations completely dictate behaviors and... that a person's traits, attitudes and values are of no consequence." This paragraph is bewildering. I have never argued that the pathological effects of solitary are instantaneous.

Moreover, I have never argued that an individual's traits, etc., are not relevant to understanding the individual's behavior.

33. And once again, it is odd that Dr. Gendreau only refers to "behavior", rather than to the full gamut of signs and symptoms of psychiatric disturbance associated with solitary confinement. In his report, Dr. Gendreau repeatedly emphasizes behavior (basically - negative behavior). In ¶75, he resorts to ridicule, claiming that I take a position that is patently absurd, he asserts that I believe that inmates are innocent creatures, not responsible for their misbehavior: "Also, forgotten in this matter is the responsibility of the offender. Dr. Grassian seems to suggest they are innocent bystanders which is incorrect." This statement bears no relationship to anything that I actually believe or have written.

iv. Pre-existing Problems and Their Prevalence Among Ad Seg versus General Population Inmates in the Study

34. In ¶¶86-87, Dr. Gendreau speculates that whatever psychological difficulties were observed among inmates in solitary confinement were probably present prior to their incarceration. It is an odd assertion, since it is nothing more than speculation. But more importantly, arguing about why inmates in solitary might have more psychological problems than those in general population is irrelevant to the Colorado study. Using the data generated from the self-report rating scales, the Colorado study concluded that those in solitary during the study year did not have more psychological problems than those housed in general population at the onset of the study, nor did they develop more psychological problems during the study year than those housed in general population.

35. My critique of the study includes my bringing forth evidence from the psychiatric crisis data that those in solitary did develop more psychological problems, a fact that demonstrates that the self-reports were not a valid measure of the psychiatric status of the inmates studied.

v. Ethical Concerns: The Failure to Provide Raw Data

36. In ¶89 and ¶94, Dr. Gendreau comments upon the discussion in my November report of the death, and likely suicide, of one of the inmates who had participated in the study, as well as my assertion that the authors' unwillingness to provide raw data raises serious ethical problems. He seems not to understand the fundamental issue I raised in my November report. In the case of that inmate, and in the cases of the large number of individuals who had psychiatric crises during the year of study, it was incumbent upon the authors to look at the last self-report by those inmates, in order to evaluate whether their self-reports prior to the incident were valid measures of the psychological distress that became very evident soon afterwards. The authors failed to make that analysis, and they refused to provide the raw data (the self-reports of those individuals) that would allow anyone else (including myself and Craig Haney) to do that analysis and evaluate the validity of the self-reports. Dr. Gendreau states that the authors gave him access to data he requested, but he does not explain exactly what data he requested, let alone what he discovered by viewing that data.

37. Another concern raised about the Colorado data in my November report (at ¶41) is that inmates did not necessarily remain in the same setting (general population versus solitary

confinement) during the course of the study year. Dr. Gendreau asserts at ¶91 that the numbers and amount of time so affected was small. But he cites no basis for this assertion. The Colorado study does not provide the relevant data, and he cites no other source of information for his assertion.

vi. "The Alyusha Effect"

38. At times, Dr. Gendreau's comments reflect a lack of careful reading of my report, and a predilection towards ridiculing my opinion. A graduate student, Alyusha Stucker, was the individual who actually interacted with the study inmates and administered the self-report scales. In ¶95, he claims that I am the author of the "Alyusha Law", and then chastises me for never having observed Alyusha administering the rating scales. Ms. Stucker is apparently a very attractive young lady. Dr. Gendreau attributes the following to me: "According to Dr. Grassian, Alyusha's physical appearance so discombobulated [the inmates]... that they felt compelled initially to give answers that did not jibe with Grassian's expectations."

39. His statement, and his ridicule, are utterly groundless. Indeed, I have never even laid eyes on that young woman; I have no way of knowing whether she is either young or quite attractive. As described in my November report, at ¶42, the term "The Alyusha Effect" was coined, not by me, but by the authors of the Colorado study. It raised for those authors a real question as to whether inmates - young men starved for intimate female companionship - would be willing to reveal vulnerabilities and psychological problems to an attractive young woman. In other words, it was the authors who raised concern about whether "The Alyusha Effect" might undermine the validity of the self-reports.

vii. Self-Reports and Objective Data

40. Dr. Gendreau's report contains other major mischaracterizations of my report and of the Colorado study. For example, in ¶¶91-93, he argues that, supposedly opposed to my expectations, the Colorado data did not "generate huge increase in irreparably damaging symptoms that occur early on in [solitary confinement]." I am unaware of why Dr. Gendreau has come to believe that I argue for such a proposition; I do not. Indeed, I have often found that an inmate's ability to tolerate solitary confinement gradually erodes, and he becomes overtly symptomatic only after some period of time. Indeed, this was one of the hypotheses of the authors of the Colorado study (p.6/163) - that inmates would become more symptomatic over time.

41. The conclusion they reach from the self-report data was that this increasing psychiatric difficulty did not occur. However, as described in my November report, at ¶45, the psychiatric crisis data demonstrates that this deterioration over time did occur.

42. Dr. Gendreau, at ¶89, mischaracterizes both the Colorado study data and my own findings regarding the psychiatric crises that occurred during the study year. He states that:

> … during the study [year]… a few inmates in the study had major psychological crises [suicidal or psychotic symptomatology] (emphasis added)

In fact, as I wrote in my November report at ¶44, among the 59 psychiatrically vulnerable inmates assigned to solitary, there were 37 such episodes - almost 2 episodes for every 3 inmates. Yet among the 33 psychiatrically vulnerable inmates assigned to general population, there were only 3 episodes - 1 episode for every 11 inmates. (The chance that this difference could have been random is miniscule, $p<.0002$.)

43. Moreover, while according to the self-report rating scales there was no evidence of psychiatric deterioration over the course of the 12 months, the psychiatric crisis data demonstrated otherwise: Of the 37 episodes among the vulnerable group in solitary during the study year, only 12 happened in the first 6 months, whereas more than double that number (25 episodes) occurred in the second 6 months. (This difference is statistically significant at $p<.05$.)

44. Once again the objective data contradicted and invalidated the self-report rating scales. Dr. Gendreau's report reveals a lack of appreciation of this fact.

F. Conclusions

45. In his conclusions, Dr. Gendreau suggests that 60 days of continuous solitary confinement will not cause any undue psychiatric difficulty. He seems to have no appreciation of the import of the observations in my AJP article, which described serious psychiatric difficulties among inmates who had been confined for less than 15 days.

46. It is difficult to discern ways in which Dr. Gendreau's report strengthens the arguments put forward in Dr. Mills' report. His arguments are similar to those of Dr. Mills, but Dr. Gendreau's report contains a surprisingly large number of simple mistakes regarding the Colorado study data and mischaracterizations of my November report. His report also contains a number of *non sequiturs* - comments or assertions that bear no relationship to the matters at issue.

47. Indeed, the sheer number of questionable assertions in Dr. Gendreau's report created one of the challenges I faced in preparing the present report - how to create a readable, fairly linear narrative while not ignoring any of the more important misstatements in the Gendreau report.

48. In conclusion, Dr. Gendreau's report does not alter the arguments or conclusions I offered in my November 2016 report.

Stuart Grassian, M.D.

May 24, 2017