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ELECTRONICALLY TRANSMITTED

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Nathaniel Schenker, Past President
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Re: Request for (a) Recusal of ASA President David Morganstein From Any Role in Considering Issues Raised in My October 8, 2015 Letter to ASA Leadership and (b) Designation of Another ASA Officer to Oversee ASA Consideration of Those Issues

Dear President Morganstein and other Members of the Leadership of the American Statistical Association:

This is a formal request that American Statistical Association (ASA) President David Morganstein be recused from any role in considering issues raised in my [letter](#) to ASA leadership dated October 8, 2015,¹ and that another officer be designated to oversee ASA's consideration of the issues raised in the letter. One basis for this request relates to an item titled "On Measuring Health Disparities: Don't Be Misled by Scanlan's Rule" that a Westat senior statistician, presumably a subordinate of Mr. Morganstein in Mr. Morganstein's role as Director of the Westat Statistical Staff, submitted to *Society Magazine* on October 13, 2015, as a purported reply to my article "[Race and Mortality Revisited](#)," *Society* (July/Aug. 2014). A second basis, which

¹ The earlier letter failed to include Past President Nathaniel Schenker among the addressees. A copy of the letter was emailed to Dr. Schenker on October 12, 2015. The earlier letter was also sent to the Chairs of the Committee on Law and Justice Statistics and the Scientific and Public Affairs Advisory Committee. Because of the nature of this letter, recipients are limited to member of the ASA Board.

is closely related to a matter the October 8, 2015 letter suggested as a basis for recusal of President Morganstein, involves the range of statistical analyses of Westat conducted under the supervision of Mr. Morganstein that points made in my letter suggest are unsound.

Section A discusses background relevant to the request for recusal. Section B discusses implications of the October 13, 2015 submission to *Society Magazine* by a Westat statistician with respect to the request for recusal. Section C discusses factors warranting President Morganstein's recusal that would exist irrespective of the October 13, 2015 submission. Section D discusses the need for ASA leadership to appoint an officer to oversee ASA consideration of the issues raised in my October 8, 2015 letter.

A. Background

In the October 8, 2015 letter to ASA leadership, I recommended that ASA do two things. First, I recommended that ASA form a committee to explore the ways analyses by statisticians and others of demographic and other differences in outcome rates are fatally undermined as a result of the failure to recognize patterns by which standard measures of differences between outcome rates tend to be systematically affected by the frequency of an outcome. Second, I recommended that ASA formally advise arms of the United States government that a statistical belief underlying important civil rights law enforcement policies – that reducing the frequency of an adverse outcome will tend to (a) reduce relative demographic differences in rates of experiencing the outcome and (b) reduce the proportions groups most susceptible to the outcome make up of persons experiencing the outcome – is the opposite of reality. For instant purposes, it suffices to note that the recommendation was based on views I had expressed, among other places since 1987, in three ASA publications – a Statistician's View column titled "[Misunderstanding of Statistics Leads to Misguided Law Enforcement Policies](#)" in the December 2012 issue of *Amstat News*, a guest editorial titled "[Can We Actually Measure Health Disparities?](#)" in the Spring 2006 issue of *Chance*, and an article titled "[Divining Difference](#)" in the Fall 1994 issue of *Chance* – and in the above-mentioned "Race and Mortality Revisited." The letter principally relied on the last item, which it referenced 22 times.

In the earlier letter (at 3-4), I suggested that ASA President David Morganstein consider recusing himself from any role in considering the issues raised in the letter for reasons relating to his role as Vice President and Director of the statistical consulting firm Westat and the attention the letter gives to, and the actions I have taken respecting, technical assistance guides produced by Westat through the IDEA Data Center pursuant to grants of the Department of Education's Office of Special Education Programs. In the letter (at 4 n.5), I mentioned in particular the IDEA Data Center technical assistance guide titled "[Methods for Assessing Racial/Ethnic Disproportionality in Special Education](#)," that I had criticized in a number of places, including in materials I brought to the attention of Department of Education personnel overseeing the pertinent grants.

Actions I had taken in raising problems with the guide included creation, on August 11, 2014, of the [IDEA Data Center Disproportionality Guide](#) subpage of the [Discipline Disparities](#)

page of jpscanlan.com discussing those problems; sending an August 11, 2014 [letter](#) to IDEA Data Center leadership bringing that subpage to their attention; mention of subpage in letters to government agencies and other entities and in a brief in the United States Supreme Court; and treatment of the guide in a January 20, 2015 University of California, Irvine [workshop](#) (slides 55-59), an October 18, 2014 George Mason University [workshop](#) (slides 98-108), an October 10, 2014 University of Maryland [workshop \(slides 98-108\)](#), and a September 5, 2014 University of Minnesota [workshop](#) (slides 62-63). Those actions also included my bringing the University of Minnesota workshop to the attention of the IDEA Data Center leadership and Mr. Morganstein while inviting IDEA Data Center and Westat personnel to attend the University of Maryland workshop.²

The criticisms of the IDEA Data Center disproportionality guide rely heavily on "Race and Mortality Revisited," which I first brought to the attention of the lead author of the guide by email of August 4, 2014, and to the attention of Mr. Morganstein by email of November 30, 2014. The severity of my criticism of the guide is reflected in the August 24, 2015 [letter](#) to the Department of Health and Human Services and Department of Education mentioned repeatedly in the October 8, 2015 letter to ASA leadership (at 4, 5, 7, 9, 15, 16, 19, 30, 31, 37, 38, 39), which discusses the disproportionality guide in the following terms (at 10-11):

I therefore encourage the DOE to have its staff carefully review the Educational Disparities and Discipline Disparities pages and all their subpages, as well as the articles and workshop materials mentioned above. I especially encourage that review with regard to the agency's ongoing consideration of the Government Accountability Office's recommendation that the agency provide states more specific guidance on determinations of significant disproportionality in special education, the subject on which the DOE issued a [Request for Information](#) in June 2014. The IDEA Data Center guide mentioned above, though not leading observers erroneously to believe that reducing the frequency of a putative adverse outcome will tend to reduce relative differences in rates of experiencing the outcome, nevertheless shows no awareness of the way the frequency of the outcome tends to affect each of the measures the guide recommends.³ As with other guides that fail to reflect such awareness, the guide cannot provide useful instruction on the appraisal of the strength of the forces causing outcome rates of advantaged and

² Subsequent to the October 8, 2015 letter to ASA leadership, I have given one methods workshop addressing issues raised in the letter. See "[The Mismeasure of Health Disparities in Massachusetts and Less Affluent Places](#)," Quantitative Methods Seminar, Department of Quantitative Health Sciences, University of Massachusetts Medical School (Nov. 18, 2015). Because of the focus of the presentation, I did not address the IDEA Data Center disproportionality guide. Typically, however, I would discuss the guide in such workshops, as in the four preceding workshops. Since the earlier letter, I have also written two letters of the type listed on pages 4-5 of the October 8 letter. See letters to [House Judiciary Committee](#) (Oct. 19, 2015) and [Boston Lawyers' Committee for Civil Rights and Economic Justice](#) (Nov. 12, 2015). Both letters reference the web page on the IDEA Data Center disproportionality guide.

³ Footnote 18 to the letter the Departments of Health and Human Services and Education reads: "The guide also shows no awareness of other problems with certain of those measures, which problems were alluded to several paragraphs above."

disadvantaged groups to differ, and necessarily will commonly lead users to believe things that are not true. See the above-mentioned October 26, 2012 letter to Harvard Medical School and Massachusetts General Hospital (and others) regarding their jointly produced *Commissioned Paper: Health Care Disparities Measurement* and the discussion of that document in "Race and Mortality Revisited" at 344-345.

The discussion at pages 344-345 of "Race and Mortality Revisited" regarding Harvard Medical School and Massachusetts General Hospital pertains to the obligations of issuing entities to withdraw guides that give misleading advice and the way that, as a result of my exchanges with them, certain entities have more reason to understand the flaws in their analyses of demographic differences than others. As a result of the actions in bringing issues regarding the IDEA Data Center disproportionality guide to the attention of Westat and IDEA Data Center leadership, those entities also are in a much better position than most like entities to understand the scope of the problems with their guidance on analyses of group differences in outcome rates.

The letter also gave the recipient Department of Education much reason for caution in relying on Westat and IDEA Data Center guidance in considering how to address the Government Accountability Office's recommendation that the agency provide states more specific guidance on determinations of significant disproportionality in special education. I forwarded the letter to leadership of the IDEA Data Center by email of August 25, 2015, and the Westat statistician's submission to *Society Magazine* discussed in Section B would quote from it.

The October 8, 2015 letter gave passing mention to the [Disabilities – Public Law 104-446](#) subpage of the [Discipline Disparities](#) page of [jpscanlan.com](#), which involves the measurement of significant discrepancies in the suspension or expulsion of students with disabilities. I created that page in May 2012 pointing out that the remedies Congress mandated where such discrepancies are found would tend to increase discrepancies as they are typically measured. I then specifically treated the matter in "Race and Mortality Revisited" at 342. On learning that the Data Accountability Center (a predecessor to IDEA Data Center) had produced, in September 2011, a guide titled "[Measuring Significant Discrepancy: An Indicator B4 Technical Assistance Guide](#)," I added, on November 22, 2014, a prefatory note to the [Disabilities – Public Law 104-446](#) subpage referencing "Race and Mortality Revisited" and discussing problems in the guidance provided in the September 2011 Data Accountability Center guide.

Finally, by way of correction, clarification or expansion of points made in the October 8, 2015 letter to ASA leadership, I note that in the letter I identified Mr. Morganstein as a Director of Westat. But Mr. Morganstein's director position is that of Director of the Statistical Staff, not as a member of Westat's Board of Directors. The IDEA Data Center is a government-funded entity created to provide technical assistance to states regarding reporting and analyzing data maintained pursuant to the Individuals with Disabilities Education Act. Westat is the lead organization for the IDEA Data Center (working with seven partners). Currently Westat's involvement with the IDEA Data Center is based on a \$6.5 million Department of Education Office of Special Education Programs grant awarded in 2013.

Further, while the October 8 letter mentioned that points it made would potentially call into question products in which Westat has an interest beyond the IDEA Data Center guides mentioned in the letter, given the representative nature of such documents, it would have been fair to say that points in letter would likely call into question a great deal of Westat's statistical work. It would also have been fair to say that since August 2014 much of my work regarding statistics has used work of Westat as a key example of unsound analysis of demographic differences in outcome rates.

B. The Westat Senior Statistician's October 13, 2015 Submission to *Society Magazine* Titled "On Measuring Health Disparities: Don't Be Misled by Scanlan's Rule"

On October 13, 2015, a senior statistician of Westat, who is one of the authors of the 2011 and 2014 versions of the IDEA Data Center disproportionality guide discussed above, emailed to the editor of *Society Magazine* the above-mentioned submission titled "On Measuring Health Disparities: Don't Be Misled by Scanlan's Rule."⁴ The author's affiliation was listed as Westat. In places where a single author would use the word "I," the submission used "we." There is no statement that the views expressed in the submission do not reflect the views of Westat. I do not know whether the item was intended to be submitted on behalf of Westat, though I assume many readers would regard it as representing the views of Westat. Presumably the submission was funded by Westat, at least in the sense that the author or authors were compensated by Westat during the time spent to draft the item.

The submission was presented as a reply to my 2014 *Society* article "Race and Mortality Revisited" mentioned above. But the submission principally discussed a table from my 2006 guest editorial in *Chance*, which table the submission described as my "original example" of the patterns I have described by which relative differences tend to be affected by the frequency of an outcome.⁵ Most pertinent to the subject of this letter, the submission attributed to me a position, as supposedly articulated in the 2006 *Chance* editorial and the 2014 *Society* article and other places, to the effect that the relative difference between rates at which two groups experience an outcome will always be larger where the outcome is less prevalent than where the outcome is

⁴ The term "Scanlan's rule" was first used by scholars in the United Kingdom in 2008, alluding to the pattern I had termed "heuristic rule X" or "HRX" in the 2006 *Chance* editorial. See Bauld L, Day P, Judge K. Off target: A critical review of setting goals for reducing health inequalities in the United Kingdom. *Int J Health Serv* 2008;38(3):439-454. The submission by the Westat statistician erroneously attributes the naming of the pattern "Scanlan's rule" to me.

⁵ The 2006 *Chance* editorial was actually approximately my twelfth published description of the pattern whereby the rarer an outcome the greater tends to be the relative difference in experiencing it and the smaller tends to be the relative difference in avoiding it. See the [Bibliography](#) subpage of the [Scanlan's Rule](#) page of [jpscanlan.com](#). Earlier publications on the subject, which date to 1987, had used various types of data to illustrate the pattern. Like the 2006 *Chance* editorial, the 1994 *Chance* article "Divining Difference" had discussed the implications of reductions in poverty that would allow everyone with an income above 50% of the poverty line to escape poverty. But the 1994 article principally illustrated the pattern, in its Table 1 and Figure 1, with test score data. Thus, what the submission described as my "original example" used to illustrate the pertinent pattern was not even my first example used in *Chance*.

more prevalent. The submission then sought to refute that supposed position by demonstrating that it is possible for the relative difference in experiencing an outcome not to be larger where the outcome is less prevalent than where it is more prevalent, including by showing that it is possible for there to exist situations (a) where one group's rate of experiencing an outcome is 40% and another group's rate is 20% (a risk ratio of 2.0) and (b) where one group's rate is 20% and the other group's rate is 10% (also a risk ratio of 2.0).⁶ The submission also showed that it is theoretically possible for general declines in poverty to be accompanied by reduced relative differences between poverty rates of advantaged and disadvantaged groups. It also showed that the overall prevalence of an outcome is not used in the calculation of the risk ratio.

The editor of *Society*, with the author's permission, passed the submission on to me to consider providing a response. After receiving comments from me, *Society* decided not to publish the submission. The editor so informed the author by email of November 1, 2015, with a copy to me, advising the author that I would be interested in further dialog regarding the subject of the submission. By email of November 1, I informed the author that I would be sending certain comments on the submission. In the email, I also informed the author of my October 8, 2015 letter to ASA and of the suggestion in that letter that ASA President Morganstein recuse himself from consideration of issues raised in the letter.

On November 11, 2015, I emailed a letter to the author. In the letter, among other things,⁷ I advised the author that the submission to *Society* had mischaracterized my work in a

⁶ As it happens, those are the same figures I used in "Race and Mortality Revisited" (at 339) to illustrate that it is impossible for a factor to have equal proportionate effects on different baseline rates of experiencing an outcome while at the same time having equal proportionate effects on rates of experiencing the opposite outcome. The figures similarly illustrate the illogic of the rate ratio as a measure of association. See the [Illogical Premises](#) and [Illogical Premises II](#) subpages of the [Scanlan's Rule](#) page of [jpscanlan.com](#). (In the October 8, 2015 letter to ASA leadership (at 12-13), I also use a risk ratio of 2.0 to make this point (but with pairs of rates of (a) 20% and 10% and (b) 10% and 5%, rather than pairs of rates of (a) 40% and 20% and (b) 20% and 10%). While the submission by the Westat statistician would regard the fact that the rate ratio is the same in both cases as indicating that the strength of association is the same in both cases, the submission does not address that the ratio for the opposite outcome is different in both cases. One remarkable aspects of the submission is that, while many would regard the most significant aspect of my work to be its pointing out that the two relative differences tend to change in opposite direction as the frequency of an outcome changes, the submission never acknowledges such argument or even acknowledge the fact of a second relative difference.

⁷ In the letter to the author, I also explained that the author's submission to *Society* made certain statements evidencing a recognition that increasingly restricting an adverse outcome to segments of the overall population that are most susceptible to the outcome will tend to increase (a) relative demographic differences in rates of experiencing the outcome and (b) the proportions groups most susceptible to the outcome make up of persons experiencing it. I attached a list of eight questions as Attachment A to the letter (Attachment A hereto) and suggested that, consistent with Westat's recognition of the referenced pattern, Westat's answers would have to be "yes" to the first seven questions and "no" to the eighth question. And I suggested that such recognition imposed on Westat an obligation to inform all clients to whose activities the pattern is pertinent that such pattern exists and to advise those clients of the ways the pattern bears on their activities or guidance Westat or IDEA Data Center has provided them. But I noted that, in light of the failure of persons at Westat and IDEA Data Center to whose attention I brought "Race and Mortality Revisited" more than year earlier to take the actions I think are compelled by the recognition of the above-described pattern, I would, time permitting, likely address the same issues with the Westat Board of Directors. I urged the author to alert the Board that I am likely to do that.

number of significant respects, including by attributing of me a position that reductions in the frequency of an outcome will always be accompanied by increased relative differences in rates of experiencing the outcome and that relative differences between rates will always be larger where the outcome is less common. I also provided reasons that the author or authors must have known that the submission had misstated my position. Such reasons included that, apart from repeated usage or terms like “tend(s)” or “tendency” – 100 times in “Race and Mortality Revisited” and 31 times in the 2006 *Chance* editorial – I explicitly make the point that the pattern will not always be observed, in both “Race and Mortality Revisited”⁸ and in the 2006 *Chance* editorial⁹

⁸ See “Race and Mortality Revisited” at 330-331:

As explained in “Race and Mortality,” the pattern by which the two relative differences tend to change in opposite direction as the prevalence of an outcome changes will not be found in every situation where one examines the sizes of relative differences at different points in time or in settings differentiated other than temporally. Observed patterns are also influenced by the comparative sizes of the differences between the risk distributions of advantaged and disadvantaged groups in the settings being examined. We might also characterize that factor as (a) the difference in the circumstance of the groups reflected by their outcome rates, (b) the strength of the forces causing the groups’ outcome rates to differ, or (c) the strength of the association between group membership and the outcome.” The purpose of examining differing outcome rates of advantaged and disadvantaged groups is to understand this aspect of the matter in order, for example, to determine whether forces causing outcome rates to differ have increased or decreased over time and what factors contribute to such increases or decreases. But measures of differences between outcome rates that change solely because there occur overall changes in the prevalence of an outcome akin to that effected by lowering a test cutoff cannot provide useful information on such issues unless examined with an understanding of the way the measures tend to change solely because of changes in the prevalence of the outcome.

See *id.* at 343:

That one may observe departure [*sic*] from these patterns does not alter the situation. Departures from the patterns in fact are the principal, if not only, things worth exploring.

⁹ See the 2006 *Chance* editorial at 49:

To be sure, one will be able to find many departures from HRX, some of which might be due to true changes in the relative well-being of the groups being examined. But HRX is nevertheless pervasive enough that one cannot meaningfully interpret changes in group differences in susceptibilities to an outcome without taking HRX into account.

See *id.* at 51:

Based simply on HRX, we might, in some circumstances, draw inferences about the true nature of changes in the relative well-being of two groups during times of change in the prevalence of an outcome. When the rate of experiencing an adverse outcome is increasing for one group and declining for the other group, it would seem to reflect a true change in the relative well-being of the two groups. But such situations are likely to be rare in any case and even rarer when the increases or decreases are substantial. In theory, one might interpret any clear departure from expected patterns of changes in relative or absolute differences in times of overall increases or decreases in prevalence of an outcome to reflect some true change in the relative wellbeing of two groups.

and have similarly made that point clear in numerous other places over the last quarter century,¹⁰ including in the University of Minnesota [workshop](#) that I brought to the attention of Westat and IDEA Data Center when inviting them to send personnel to my similar [workshop](#) at the University of Maryland.¹¹

Such reasons also included the fact that a claim that such a pattern would always be observed would be idiotic.

The last point is important. For persons fully grasping the meaning of the arguments in the Westat statistician's submission, while assuming that a Westat statistician would not seriously mischaracterize the position of the author of work to which the Westat statistician was purportedly responding, would be inclined to regard the author of the antecedent work as manifestly incompetent with regard to anything involving the analysis of demographic

¹⁰ See "[Bias Data Can Make the Good Look Bad](#)," *American Banker* (Apr. 27, 1992):

Does this always happen? Of course not. Other factors, including such discrimination as a particular institution actually may engage in, often outweigh the mathematical tendencies. But the mathematical tendencies are essential parts of the picture. Without understanding them, no one can make heads or tails out of the data.

See also, among other places in "[Race and Mortality](#)," *Society* (Jan./Feb. 2000) (at 6 of the version on [jpscanlan.com](#)):

To be sure, the described tendencies may not predominate in every comparison of demographic disparities. Irregularities in the distributions of various factors among different populations, as well as irregularities in small data sets, may sometimes cause the tendency not to be observed at all. The size of one average difference underlying success and failure disparities can be sufficiently larger than another average difference to counteract entirely the statistical tendency on one side of the equation as it amplifies the tendency on the other side. And there certainly occur changes in the relative susceptibility of two groups to some condition that are not solely a function of the change in the prevalence of the condition. For example, that illegitimacy rates appear to be declining more among blacks than among whites, being contrary to the usual tendency (and not involving an irreducible minimum), suggests a true change in the relative susceptibility of blacks and whites. Nevertheless, invariably the tendencies described here constitute a crucial part of the picture and, unless that part of the picture is understood, it is impossible to draw meaningful conclusions about data on group differences. It is also impossible to intelligently direct resources to moderating or eliminating those differences

¹¹ Slide 7 of the Minnesota workshop states as "Caveat One":

Do not be distracted by the fact that one commonly finds departures from the patterns described here. Observed patterns are invariably functions of
(a) the strength of the forces causing rates to differ (differences in the circumstances of the groups being compared) and
(b) the prevalence-related/distributionally-driven forces described here.
Society's interest is in (a).
Only with a mastery of (b) can one understand (a).

The same language appears in slide 13 of the Maryland workshop.

differences in outcome rates. Such persons would also be inclined to doubt the editorial competence of the publications publishing such work, including the ASA publications.

In my letter to the author, I also explained that, even though *Society* would not publish the submission, the submission itself, in consequence of an assumption of *Society*'s editors that Westat or a Westat statistician would not submit a reply to "Race and Mortality Revisited" that is entirely lacking in merit or that would seriously mischaracterize my work, may diminish my credibility in the eyes of one or more members of the editorship of a journal with which I have a longstanding relationship.¹² I therefore requested that the author write to the editor of *Society* explaining the ways that the submission had mischaracterized my work and explaining the bearing of my work on activities of Westat. I provided a draft of such letter, which addressed twelve matters as to which the submission had been inaccurate or misleading.¹³ The draft letter also explained that the author of the submission to *Society* was a co-author of the IDEA Data Center disproportionality guide of which I had been so critical and that a substantial proportion of Westat's yearly revenue involves activities that commonly employ methods that I have criticized in "Race and Mortality Revisited" and elsewhere. The draft letter stated that Westat's interest in matters to which the reasoning of "Race and Mortality Revisited" pertains should have been disclosed in the transmittal of the October 13, 2015 submission.

I advised the author that if the author did not send the letter, I would request Westat to do so. But I also advised the author that the author's sending the letter would not necessarily affect any action on my part and that I might still request Westat to send a letter. I suggested, however, that, irrespective of any action I may take depending on whether the author sends the letter, the author and Westat have an ethical obligation to correct any significant misimpressions the submission had caused as to the nature of my work and related matters.

Finally, noting that items similar to that submitted to *Society* (which, as noted, principally discussed my 2006 *Chance* editorial) could have been submitted to other journals, including *Chance*, I requested that the author inform me as to whether the author or anyone else at Westat submitted any like item to another publication. And I requested that I be provided with a copy of any such item in order that I might determine whether I should request that a letter be sent to the publication's editors similar to the letter I requested regarding the *Society* submission.

I have not heard from the author. But in all likelihood I will be contacting the Board of Directors of Westat regarding this matter. And, given the nature of the submission to *Society* by the Westat statistician, there is some prospect that I will eventually pursue this matter in litigation, particularly if the author or authors are able to publish a similar item in a journal that

¹² See "[The Curious Case of Affirmative Action for Women](#)," *Society* (Jan/Feb 1992) (reprinted in *Current* (June 1992)); "[Race and Mortality](#)," *Society* (Jan./Feb. 2000) (reprinted in *Current* (Feb. 2000)).

¹³ The draft letter, which was Attachment B to the letter to the Westat statistician, is Attachment B to this letter.

does not afford me the opportunity to comment in advance of publication.¹⁴ In the event that I do pursue the matter in litigation, the issues raised would be of a nature as to which ASA should have an interest in expressing its views to the court in which the case is brought.

Regardless of whether I bring an action, however, I will likely create a web page pertaining to the submission. Such page would address the light the submission sheds on the accuracy of my descriptions of the ways measure tends to be affected by the frequency of an outcome as well as the soundness and candor of putatively scientific discourse, both generally and regarding matters in which a person or organization has a pecuniary interest. It would also use the submission to illustrate that seemingly scientific points can be essentially fatuous or misleading.¹⁵ Such page would also address the obligation of consulting entities to advise clients of ways in which prior work of the consulting entities may be incorrect or called into question by other work and the obligations of such entities to be absolutely candid in doing so.

And a work in progress will be addressing the shortcomings of statistical science generally and my efforts to correct those shortcomings including the obstacles encountered in doing so. Both the letter to ASA and the Westat statistician's submission to *Society* will have significant roles in such work.

Irrespective of matters arising from the nature of the submission,¹⁶ however, the fact of the submission by a person who reports to Mr. Morganstein should itself disqualify Mr.

¹⁴ Most works addressing my work have been in published in journals other than those in which my works were published and where the editors did not see a need to forward the submission to me before publishing it. See the [Consensus](#) subpage of the Scanlan's Rule page of [jpscanlan.com](#).

¹⁵ Using illustrations of a type that naïve observers could regard as quite scientific, the submission makes three key points in refuting my supposed work. First, using a formula, the submission shows that the overall prevalence of an outcome does not figure into the calculation of the risk ratio and maintains that such fact shows that the prevalence of an outcome is irrelevant to the risk ratio. The point based on the illustration is the same as an argument that the fact that the unemployment rate is not part of the calculation of the poverty rate refutes a claim that the level of unemployment influences the poverty rate. Second, using what it describes as an experiment, the submission shows that it is possible for there to exist situations (a) where one groups rate of experiencing an outcome is 40% and another group's rate is 20% (a risk ratio of 2.0) and (b) where one group's rate is 20% and the other group's rate is 10% (also a risk ratio of 2.0). The illustration would effectively refute a claim that such thing is impossible and thus contributes to the false impression that I make such a claim. But it has no bearing on anything actually said in my work. As pointed out in note 6 *supra*, I have used the same figures for a different purpose. Third, the submission uses a simulation to show that increases in black income without increases in white income could result in a situation where overall declines in poverty are accompanied by a narrowing of the ratio of the black poverty rate to the white poverty rate. Again, while contributing to the false impression that I maintain that declines in poverty must always be associated with increased relative difference in poverty rates, the illustration has no bearing whatever on anything actually said in my work.

¹⁶ The appropriateness of the recusal of President Morganstein based solely on the issues discussed in the earlier letter would seem self evident and rendered more so by the points in Section C *infra* and the mere fact that a Westat statistician is submitting a reply to "Race and Mortality Revisited" while my October 8 letter is being considered by ASA leadership. But if ASA leadership should deem it necessary actually to consider the nature of the submission

Morganstein from any role in considering the issues in my letter. And that would hold (a) regardless of the merit of the submission and (b) regardless of whether Mr. Morganstein had any advance knowledge of, or role in drafting or approving, the submission to *Society* and whether the author or authors, persons supervising the author or authors, or Westat itself regarded the item to be submitted on behalf of Westat.

C. Matters Apart From the October 13, 2015 Submission to *Society Magazine* Warranting Recusal of President Morganstein From Any Role in Considering Issues Raised in My Letter of October 8, 2015

The matters discussed at pages 3 to 4 of my October 8, 2015 letter to ASA leadership should alone provide ample basis for recusal of President Morganstein from consideration of the issues raised in my letter. I apologize for failing to note the matter in my transmittal or on the first page of the letter.

Further, my treatments of the problems in the IDEA Data Center disproportionality guide would be equally pertinent to a great deal of Westat work, both generally and with respect to the second recommendation in my letter. Westat's government [clients](#) include, in addition to the Department of Education (already mentioned), the Departments of Housing and Urban Development, Justice, Health and Human Services, and Treasury. Each of these entities has been the subject of specific criticism in my work for the failure to understand that reducing the frequency of an adverse outcome tends to increase, not decrease, relative differences in experiencing it.

As discussed in note 7 *supra*, in my recent letter to the Westat statistician who authored the submission to *Society* I explained that the submission made certain statements evidencing a recognition that increasingly restricting an adverse outcome to segments of the overall population that are most susceptible to the outcome will tend to increase relative demographic differences in rates of experiencing the outcome and the proportions groups most susceptible to the outcome make up of persons experiencing it. I also suggested that such recognition imposed on Westat obligations to inform all clients to whose activities the pattern is pertinent that such pattern exists and to advise those clients of the ways the pattern bears on their activities or guidance Westat or IDEA Data Center has provided them. At a minimum, that obligation would entail advising the above-mentioned federal agencies that their view that reducing the frequency of an outcome will tend to reduce relative differences in rates of experiencing it is mistaken.

If ASA were to accept the recommendation of Section B of my October 8, 2015 letter that the organization advise government agencies of their mistaken view as to the implications of reducing the frequency of an outcome, the question will arise as to why Westat did not earlier inform its clients of such matter. More general acceptance of the reasoning of my letter by ASA would presumably raise similar issues as a wide range of Westat clients.

to *Society* in order to resolve the recusal issue, I will provide ASA leadership a copy of the submission, along with my November 11, 2015 letter to the author.

As suggested in note 6 (at 5) of the October 8 letter to ASA, most entities like Westat could face similar inquiries from their clients. Most entities, however, would have the response that very few statisticians are aware of these issues. But as a result of the many actions I have taken (including communications to Westat and IDEA Data Center) respecting the IDEA Data Center disproportionality guide commencing in August 2014 – and as reflected by the fact that Westat gave enough attention to this subject to lead to the October 13, 2015 submission in purported response to "Race and Mortality Revisited" – the response that few statisticians are aware of this issue is far less justified for Westat than it would be for most similar entities. My confronting Westat about its obligations to its clients, already by means of the November 11, 2015 letter to the author of the *Society* submission and soon more directly, will further undermine any justification Westat might offer to those clients either as to why it failed to recognize issues of the type raised in "Race and Mortality Revisited" or as to why it failed to bring those issues to the attention of its clients.

Finally, it is only happenstance and my unfamiliarity with the prominence of Westat or the scope of its consulting activities, along with the time constraints that generally limit the number of such letters, that have caused Westat not yet to be a recipient of a letter of the type listed on pages 4-5 of my earlier letter (though the August 11, 2014 [letter](#) to IDEA Data Center was in effect a letter to Westat regarding a certain analysis issues). As discussed in note 7 *supra*, in my recent letter to the Westat statistician who authored the submission to *Society*, I explained that, in light of the failure of persons at Westat and IDEA Data Center to whose attention I brought "Race and Mortality Revisited" more than year earlier to take the actions I think are compelled by the recognition that increasingly restricting an adverse outcome to segments of the overall population that are most susceptible to the outcome will tend to increase relative demographic differences in rates of experiencing the outcome and the proportions groups most susceptible to the outcome make up of persons experiencing it, I would likely be contacting the Westat Board of Directors on such matter. In doing so, I will likely bring to the attention of the Board issues that exist regarding the IDEA Data Center disproportionality guide, as they bear on the guide itself and as they presumably would bear on other statistical analyses of Westat, irrespective of recognitions of statistical patterns reflected in the submission to *Society*. In doing so, I will necessarily be bringing to the attention of President Morganstein's employer what, according to the reasoning expressed in "Race and Mortality Revisited" and many other of my works, are fundamental problems in statistical analyses conducted by the staff directed by Mr. Morganstein, as well as the candor issues reflected in the matters that caused me to request that the Westat statistician send a letter to *Society* and that may cause me to bring an action against Westat.

Finally, there are other ways in which, consistent with my actions over some decades (but especially in recent years), I may be challenging the work of Westat by communications to entities with which Westat deals with respect to matters beyond the IDEA Data Center disproportionality guide. For example, on September 28, 2015, the Department of Health and Human Services and PCORI [awarded](#) two health disparities studies grants totaling \$23.5 million, while awarding Westat \$1.75 million to serve as the research coordinating unit providing

logistical and technical support for the two studies. The studies pertain to racial differences in uncontrolled high blood pressure. Both the awarding entities and the recipient entities are of a type that I might commonly be contacting, either with regard to broader issues or with regard to the particular awards. (I have already contacted PCORI on certain issues and have for some time planned a formal letter to its health disparities group.) But I would be especially likely to contact the relevant entities when an award addresses something I have specifically written about, as in the case of measurement of racial differences in hypertension and control of hypertension.¹⁷ So, time permitting, I will probably be contacting these entities about these particular awards and raising with them measurement issues that, according to the reasoning of my referenced letter to the Westat statistician, and the reasoning discussed generally above, Westat should itself be raising with them.

Virtually any action ASA takes that is in accord with the recognition of the patterns described in my October 8, 2015 letter by which measures tend to be affected by the frequency of an outcome will likely tend to validate my questioning of Westat's failure yet to acknowledge these patterns or to advise its clients of the pertinence of those patterns to the issues as to which they have sought guidance from Westat. It thus should be obvious that President Morganstein ought to have no ASA role in considering the issues raised in the letter.

While the above points in this section touch on implications of the October 13, 2015 submission to *Society* by a Westat statistician (and my responses thereto already taken or to be taken), key factors indicating the appropriateness of President Morganstein's recusal exist irrespective of the submission. Further, more than ample reason for President Morganstein's recusal existed solely in the points raised in my October 8, 2015 letter to ASA leadership.

D. Necessity of Appointment of an ASA Officer to Oversee Consideration of Issues Raised in My October 8, 2015 Letter

Shortly after I emailed my October 8, 2015 letter to ASA leadership, I received an email from President Morganstein advising that he had conferred with ASA leadership and that ASA Executive Director Ronald Wasserstein or ASA Director of Science Policy Stephen Pierson had indicated that one of them would be in contact with me shortly.

¹⁷ See "Race and Mortality Revisited" at 329-330; "[Can We Actually Measure Health Disparities?](#)," 7th International Conference on Health Policy Statistics, Philadelphia, PA (Jan. 17-18, 2008); [Comment](#) on Trivedi et al. JAMA 2006.

I have not yet heard from either Dr. Wasserstein or Dr. Pierson. But whether anyone from ASA has yet contacted me is a matter of little moment, inasmuch as the points I have to make are well articulated in my letter.¹⁸

It is important, however, that ASA leadership give expeditious attention to the issues in my letter, particularly the subject of Section B, which concerns the federal government's mistaken belief that reducing the frequency of an adverse outcome will tend to reduce relative differences in rates of experiencing the outcome and reduce the proportion disadvantaged groups make up of persons experiencing the outcome. That is a quite important subject affecting daily interactions between the federal government and governmental and nongovernmental entities covered by civil rights law, as well as the actions of state and local authorities who accept the government-promoted belief as to the statistical consequences of reducing adverse outcomes. The subject also involves a matter where, once forced to think carefully about the matter, virtually all persons with a statistical background would recognize both the error in the government's thinking and the implications of that error.

As discussed in note 7 *supra*, even the Westat's statistician's October 13, 2015 submission to *Society Magazine* recognizes that restricting an adverse outcome to the segments of the overall population most susceptible to it will tend to both increase relative differences in rates of experiencing the outcome and increase the proportion disadvantaged groups make up of persons experiencing the outcome. The fact that observers with a strong interest in challenging my work are forced to mischaracterize it in order to do so is further evidence of the essential soundness of its key elements.

In the usual course, it would be the responsibility of the ASA President to follow up with ASA senior staff regarding status of the consideration of issue raised in my letter. Simply recusing President Morganstein from a role in the consideration of those issues thus may eliminate an important element of the standard process. Therefore, upon recusal of President Morganstein, ASA leadership should appoint an officer to assume responsibility for oversight of the organization's consideration of issues raised in my letter.

Finally, the posting of the October 8, 2015 letter on ASA Connect elicited the interest of the House Judiciary Committee, which prompted me to send a [letter](#), dated October 19, 2015, to the leadership of the Committee. The letter (at 8) advised the Committee that, as a result of the October 8, 2015 letter to ASA leadership, ASA should shortly have a sufficient understanding of the matter to respond to Judiciary Committee inquiries regarding the effects of reducing the frequency of an outcome on relative demographic differences in experiencing the outcome. An ASA officer should have responsibility for ensuring that in the event of an inquiry from the House Judiciary Committee or any other entity, the organization is in a position to promptly respond.

¹⁸ ASA leadership should, however, keep me apprised of its actions, both generally and with respect to the recusal issue. Otherwise, among other things, I may be taking actions to prompt ASA to do things it has already done. Also, once President Morganstein is recused, my communications to ASA will no longer include him.

David R. Morganstein, President, *et al.*
American Statistical Association
December 2, 2015
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I will be providing a copy of this letter to the Westat statistician who submitted the purported reply to "Race and Mortality Revisited" to *Society Magazine*. That will obviate any issue as to the appropriateness of President Morganstein's doing so. That there may exist such an issue, however, is a further reflection of the problematic nature of President Morganstein's involvement with ASA's consideration of issues raised in my letter.

Sincerely,

/s/ James P. Scanlan

James P. Scanlan

Attachments

ATTACHMENT A TO DECEMBER 2, 2015 LETTER TO ASA LEADERSHIP

(Attachment A to November 11, 2015 Letter to Westat Statistician Who Submitted
“On Measuring Health Disparities: Don’t be Misled by Scanlan’s Rule”
to *Society Magazine* on October 13, 2015)

1. Does Table 1 of “[Race and Mortality Revisited](#)” show that lowering test cutoffs will tend to increase relative differences between the failure rates of higher- and lower-scoring groups and reduce relative differences between the pass rates of such groups?
2. Do Table 2 of ““Race and Mortality Revisited,” and Table 1 of “[Can We Actually Measure Health Disparities?](#),” *Chance* (Spring 2006) (2006 *Chance* editorial) show that reducing poverty such as to enable everyone with incomes between the poverty line and 75% of the poverty line (in the former item), or 50% of the poverty line (in the latter item), will tend to increase relative differences between black and white poverty rates and reduce relative differences between black and white rates of avoiding poverty?
3. Does Table 1 of the 2006 *Chance* editorial show that lowering an income requirement to secure some desired borrower outcome will tend to increase relative differences between rates at which black and white loan applicants fail to meet the requirement and reduce relative differences between rates at which they meet the requirement?
4. Does Figure 1 of James Scanlan’s March 4, 2013 [letter](#) to the Federal Reserve Board (at 4) (for which the underlying data may be found in Table 1 of the [Credit Score Illustrations](#) subpage the [Scanlan’s Rule](#) page of [jpscanlan.com](#)) show that lowering a credit score requirement to secure some desired borrower outcome will tend to increase relative differences between rates at which black and white loan recipients fail to meet the requirement and reduce relative difference between rates at which they meet the requirement?
5. Will modifying criteria for assignment to special education programs in a manner to require stronger indication that assignment is appropriate before a student is assigned tend to increase relative differences between black and white assignment rates and reduce relative differences between black and white rates of avoiding assignment?
6. Does Figure 7 (slide 10) of James Scanlan’s 2008 International Conference on Health Policy Statistics [presentation](#) show that generally lowering systolic blood pressure in the US population will tend to increase relative differences between rates at which blacks and whites suffer from hypertension and reduce relative differences between rates at which blacks and whites avoid hypertension?
7. Does restricting an adverse outcome to the segments of the population most susceptible to the outcome tend to increase relative difference between rates at which advantaged and disadvantaged groups experience the outcome and reduce relative differences between rates at which such groups avoid the outcome?

8. Assume that following a general decrease in poverty, one observed that (a) the relative difference between the poverty rates of blacks and whites increased and (b) the relative difference between black and white rates of avoiding poverty decreased. In such circumstances, would it be possible to make a judgment as to the resources that would usefully be devoted to studying either why (a) occurred or why (b) occurred without understanding that general decreases in poverty tend to be accompanied by increases in relative demographic differences in poverty rates and decreases in relative demographic differences in rates of avoiding poverty?

ATTACHMENT B TO DECEMBER 2, 2015 LETTER TO ASA LEADERSHIP

(Attachment B to November 11, 2015 Letter to Westat Statistician Who Submitted
“On Measuring Health Disparities: Don’t be Misled by Scanlan’s Rule”
to *Society Magazine* on October 13, 2015)

[DRAFT LETTER TO SOCIETY MAGAZINE]

Dear Professor Imber:

This letter has two purposes. One purpose is to call to the attention of the editors of *Society Magazine* that in the item titled “On Measuring Health Disparities: Don’t be Misled by Scanlan’s Rule” that I submitted to *Society* on October 13, 2015, as a reply to James P. Scanlan’s article from the July/August 2014 issue of *Society* titled "Race and Mortality Revisited," a number of statements regarding Mr. Scanlan’s work were not accurate. A second purpose is to advise the editors of certain things about the relationship of Mr. Scanlan’s work to matters in which Westat has an interest that should have been disclosed with the submission.

A. Accuracy Issue

First, the submission casts the statistical pattern sometimes termed “Scanlan’s Rule,” and which Mr. Scanlan describes in the first sentence of "Race and Mortality Revisited" as “a statistical pattern, inherent in other than highly irregular risk distributions, whereby the rarer an outcome, the greater tends to be the relative (percentage) difference between the rates at which advantaged and disadvantaged groups experience the outcome and the smaller tends to be the relative difference between rates at which such groups avoid the outcome,” as a pattern that Mr. Scanlan maintains will always be observed. The submission then attempts to refute such putative claim by Mr. Scanlan by showing that it is theoretically possible for the pattern not to be observed.

In "Race and Mortality Revisited" and many other places, including the Spring 2006 *Chance* guest editorial titled “Can We Actually Measure Health Disparities?” that the October 13 submission principally discusses,¹ Mr. Scanlan has made it clear that the pattern is merely a tendency and that one will observe departures from it. He has also pointed out actual departures from it, discussing what one may learn from such departures. Thus, the submission should not have been crafted in a way to lead the reader to believe that Mr. Scanlan maintains that such pattern will always be observed. Further, a showing that it is theoretically possible for there to be departures from the pattern does nothing to refute Mr. Scanlan’s actual statements about the pattern.

Second, the submission states that Mr. Scanlan “terms [the referenced pattern] ‘Scanlan’s Rule,’” and gives the impression that such was the usage in the 2006 *Chance* editorial. In the 2006 *Chance* editorial, Mr. Scanlan used the terms “heuristic rule X” or “HRX” for the pattern. In conference presentation in 2006 and the following years, Mr. Scanlan typically referred to the

¹ A copy of the editorial is enclosed.

pattern as “interpretive rule 1” of “IR1” and commonly uses such term in methods workshops he has given in recent years. The first use of the term “Scanlan’s rule” to describe the pattern was in a 2008 article by scholars in the United Kingdom in the *International Journal of Health Services*,² which referenced the 2006 *Chance* editorial. Thus, the statement or implication in the submission that Mr. Scanlan named the pattern “Scanlan’s Rule” is not correct. Neither is the suggestion that such was his usage in the 2006 *Chance* editorial.

Third, the submission in several places states or implies that Mr. Scanlan’s objections to all standard measures of differences between outcome rates, including the absolute difference and the odds ratio, rest on the referenced pattern of relative differences.³ Mr. Scanlan has made clear in "Race and Mortality Revisited" and many other places that his objections to the absolute difference and odds ratio as measures of association are not based the described pattern of relative differences, but on different patterns.

Fourth, a key element of Mr. Scanlan’s work on measurement issues is the pointing out that relative differences in an adverse outcome and relative differences in the corresponding favorable outcome tend to change systematically in opposite directions as the prevalence of an outcome changes. As reflected in "Race and Mortality Revisited," the fact that the two relative differences commonly yield opposite conclusions about directions of changes in health disparities over time plays importantly into Mr. Scanlan’s questioning of the utility of either relative difference for appraising health and healthcare disparities or other types of disparities. And the fact that any time risk ratios are the same for different baseline rates of experiencing one outcome the risk ratios for the opposite outcome will necessarily be different plays importantly into Mr. Scanlan’s assertions that risk ratio (or associated relative difference) is an illogical measure of association.

The submission did not acknowledge either of these points and did not even acknowledge the existence of a relative difference (or associated risk ratio) other than that being discussed in the submission. The very astute reader might divine from the quotation on page 1 of the submission from the 2006 *Chance* editorial (or from Table 1 of the submission, which is based on Table 1 of the *Chance* editorial) that Mr. Scanlan has a point to make regarding the fact that the two relative difference tend to change in opposite direction as the prevalence of an outcome changes. But most readers, and in any event some readers, would regard any issues related to a second relative difference as irrelevant to the submission’s challenge to Mr. Scanlan’s work and would regard the submission as indicating that Mr. Scanlan makes no significant point, in either "Race and Mortality Revisited" or the 2006 *Chance* editorial, about the fact of a second relative difference.

² See Bauld L, Day P, Judge K. Off target: A critical review of setting goals for reducing health inequalities in the United Kingdom. *Int J Health Serv* 2008;38(3):439-454.

³ The submission does not actually mention the absolute difference. But, as should be evident from the context, in the language “the risk ratio, odds ratio, and relative difference” in the second full paragraph on page 5 and in the language “odds ratio and relative difference” in note 2 on page 5, “relative difference” was supposed to be “absolute difference.”

Fifth, the submission describes its Table 1 (at 1) as a “reproduc[ti]on” of a table from the 2006 *Chance* editorial and mentions no modification to the original table. But Table 1 of the submission has two additional columns that were not in Table 1 of the 2006 *Chance* editorial.⁴ One column is that termed “EES” (the final column), the addition of which should have been noted. More important, however, a column was also added with the heading “Relative difference” (third last column). The table included in that column the relative difference between the rates at which the table shows blacks and whites to fall below each ratio of the poverty line. The submission does not include a column for the relative difference in falling above each point. This contributes to the incorrect impression that there is only one relative difference or that, in any case, issues concerning a second relative difference are irrelevant to Mr. Scanlan’s work and the submission’s challenge to that work.

Given that the table shows risk ratios for both falling below each point and falling above each point, if a column was added to show the relative difference associated with one risk ratio, a column should have been added to show the relative difference associated with the other risk ratio. And any relative difference column added should have been labeled to indicate which relative difference it contained. Regardless of whether the modification to the table was appropriate, the modification should have been noted.

Sixth, the submission characterizes Mr. Scanlan’s position to be that social progress, by reducing adverse outcomes, will necessarily increase demographic disparities. Apart from casting the matter in terms of a claim that something will always happen (the subject of the first point above), this characterization is incorrect in the following respect. Mr. Scanlan has made clear that his point is that when an adverse outcome declines, the relative difference in experiencing it will tend to increase, but the relative difference in avoiding it will tend to decrease, and that neither change necessarily indicates a meaningful change in the difference between the circumstances of advantaged and disadvantaged groups. Thus, the submission’s characterization of Mr. Scanlan’s position is not correct. Further, the submission’s failure to discuss the second relative difference would impede the reader’s ability to divine that the characterization is incorrect.

Seventh, the submission twice refers to the table in the 2006 *Chance* editorial as Mr. Scanlan’s “original example.” And, although the references listed with the submission include two articles by Mr. Scanlan from 2000, many readers of the submission might believe Mr. Scanlan’s first articulation of the referenced pattern of relative differences occurred in the *Chance* editorial.

In fact, however, the *Chance* editorial was approximately Mr. Scanlan’s twelfth published description of the pattern, and earlier descriptions date to 1987. The earlier descriptions use a variety of examples, including data involving situations where declines in the prevalence of an outcome were in fact accompanied by increases in relative differences in rates of experiencing the outcomes or where relative demographic differences in adverse outcomes were larger within more advantaged populations than within less advantaged populations. The income data example in the 2006 *Chance* editorial was not even the first example Mr. Scanlan used in *Chance* itself. In an article in the Fall 1994 issue of *Chance* titled “Divining Difference,”

⁴ The *Chance* editorial had only one table, which was designated “Table 1.”

Mr. Scanlan used a test score example similar to that used in Table 1 of "Race and Mortality Revisited." Thus, the statement that the table from the 2006 *Chance* editorial was Mr. Scanlan's original example is not correct.

Eighth, on page 1, referring to an "impressive number of publications" in which Mr. Scanlan described the referenced statistical pattern, the submission identifies 16 items by year between 2014 and 2000 (and letter within year). The submission then includes 18 items in its list of references (though without letter designations). Consistency issues aside, this would lead most readers to believe that 18 items listed among the references, half of which are under 900 words, comprised the entirety of Mr. Scanlan's work on this subject. In fact, prior to 2000, Mr. Scanlan published at least ten articles on the referenced pattern, all of which were substantially longer, and in more prominent forums, than more than half of the items listed. The earlier publications include the lead article in the Winter 1991 issue of *The Public Interest* ("The Perils of Provocative Statistics") and the article in the Fall 1994 issue of *Chance* mentioned above. Thus, the treatment of Mr. Scanlan's works in the submission substantially understated the scope of Mr. Scanlan's work on this matter.

Ninth, the submission states at pages 2-3 (original emphasis):

After deriving his rule by varying cut-points in a single population, Mr. Scanlan extended his rule to comparisons of different populations. As an example he cited large socio-economic differences in mortality rates in several Nordic countries (Lancet 1997), which Scanlan attributed to "the fact that mortality was relatively low in those countries" (Scanlan 2006). In referring to applications of Scanlan's rule, Mr. Scanlan mentioned considering "... disparities have increased or decreased over time or are otherwise larger in one setting than another." This statement suggests looking at *different* populations or changes over time.

This statement is inaccurate in several respects. Contrary to the assertion that the point about Nordic countries grew out of the varying cut point example in the 2006 *Chance* editorial, since 1987 Mr. Scanlan has been using actual data on outcome rates from differing populations to show that within populations (or subpopulations) where adverse outcomes are comparatively uncommon, relative demographic differences in adverse outcomes tend to be larger, while relative differences in the corresponding favorable outcomes tend to be smaller, than in populations where the outcomes are comparatively common. See "The 'Feminization of Poverty' is Misunderstood," *Plain Dealer* (Nov 11, 1987), "The Perils of Provocative Statistics," *Public Interest* (Winter 1991), and "Bias Data Can Make the Good Look Bad," *American Banker* (Apr. 27, 1992). He made the point with reference to several types of data in the 2000 *Society* article "Race and Mortality," some of which examples were repeated in the 2006 *Chance* editorial.

Rather than "attribut[ing]" large socio-economic differences in mortality in Nordic countries to "the fact that mortality was relatively low in those countries," Mr. Scanlan stated: "But the extent to which the size of the disparities in mortality rates was a function of the fact that mortality was relatively low in these countries has gone largely unnoticed." Thus, by failing to include certain words before and after the material it quoted, the submission overstated Mr.

Scanlan's point. Moreover, this overstatement occurred as a lead-in to the submission's effort to refute a putative claim that the relative difference will always be greater where an outcome is less prevalent than where it is more prevalent. As discussed above, Mr. Scanlan has made no such claim.

Tenth, the submission states at pages 1-2:

In his 2006 *Chance* editorial, "Can We Actually Measure Health Disparities?", Mr. Scanlan illustrated his rule by comparing poverty rates between Blacks and Whites based on the 2005 Census data. Specifically, he defined "poverty" as the percentage of White or Black families falling below a given cut-point. Table 1 reproduces a table from the 2006 *Chance* editorial. This table shows the percent of Black and White families falling below 600 percent of the US Federal poverty line, 500 percent of the poverty line, and so forth. For each definition of "poverty," Mr. Scanlan compared the percent of Blacks in poverty to the percentage of Whites in poverty. For example, if 600% of the poverty line is used to define poverty, then 91.9% of Blacks would be defined as being in poverty versus 79.5% of Whites, for a Black-White risk ratio of 1.16. As the cut-point used to define poverty decreases, the ratio of Blacks to Whites increases. For example, at 300% of the poverty line, the risk ratio increases to 1.44; at 100%, it increases to 2.28.

The submission then purports to show that Mr. Scanlan misinterpreted his table by stating that "the outcome (poverty) is not becoming less frequent, it is being redefined."

In fact, the editorial said nothing about defining poverty at each point on the table. The editorial stated (at 47) that that table was being used to show patterns of different groups' rates of falling above and below various points that would be found "in any set of data reflecting more or less normal distributions of factors associated with whether one experiences or avoids some outcome." The only things the editorial said about black and white poverty rates were the points made at 47-48 concerning the effects on relative difference between black and white poverty rates, and on relative differences between black and white rates of avoiding poverty, of reducing poverty such as to enable everyone between the poverty line and 50% of the poverty line to escape poverty (or to enable all blacks but only 90% of whites between the poverty line and 50% of the poverty line to escape poverty). These are points similar to those Mr. Scanlan makes in "Race and Mortality Revisited" (at 329) with respect to its Table 2. We have nothing to say in contradiction of that point.

Further, we acknowledge that the table in the 2006 *Chance* editorial would support the point that Mr. Scanlan has made, as in "The Perverse Enforcement of Fair Lending Law," *Mortgage Banking* (May 2014) (at 91), that income data show that lowering an income requirement "will tend to reduce relative differences in meeting the requirement while increasing relative differences in failing to meet it." We also acknowledge that nothing said in the submission calls into question similar statements Mr. Scanlan makes at pages 329-330 of "Race and Mortality Revisited."⁵

⁵ See "Race and Mortality Revisited" at 329-330:

Eleventh, on page 5, at the beginning of the Discussion section, the submission states: “In [Mr. Scanlan’s] view, virtually the entire scientific community has failed to recognize Scanlan’s rule, that disparities increase as the frequency of an outcome decreases.”

The statement is inaccurate in two respects. First, it suggests that no one has agreed with Mr. Scanlan’s interpretation of the ways relative differences tend to be affected by the frequency of an outcome. As explained in "Race and Mortality Revisited," some very important scholars have agreed with Mr. Scanlan’s interpretations regarding relative differences, as has the National Center for Health Statistics.”

Second, the statement suggests that all Mr. Scanlan’s challenges to the scientific community’s analyses of demographic differences rest on the pattern of relative differences termed Scanlan’s Rule. As explained above, and as is made very clear in "Race and Mortality Revisited," many of Mr. Scanlan’s objections to the scientific community’s analyses of demographic differences involve other patterns.

Twelfth, at the beginning of the discussion section on page 5, the submission states: “Using examples like the one in Table 1, Mr. Scanlan has applied his rule to the fields of health, mortgage lending, education, and other settings.”

This statement suggests that Mr. Scanlan’s arguments about the ways measures tend to be affected by the frequency of an outcome are based entirely on theoretical illustrations. In fact, Mr. Scanlan’s work regarding measurement has used scores or hundreds of illustrations with actual data on rates at which demographic groups experience various types of outcomes. Some of these illustrations are in the tables of "Race and Mortality Revisited," and the article calls the reader’s attention to places where many other illustrations may be found. Page 7 of the August 24, 2015 letter to the Department of Health and Human Services and Department of Education referenced in the submission at pages 5-6 provides links to discussions of data on situations in fifteen jurisdictions where recent reductions in discipline rates were accompanied by increased relative difference in discipline rates.

The above matters do not necessarily cover every instance where the submission is in some manner inaccurate. But these are the matters that Mr. Scanlan specifically requested that I address.

National Health and Nutrition Survey data show that generally lowering blood pressure will tend to increase relative differences in hypertension while reducing relative differences in rates of avoiding hypertension and that generally improving folate levels will tend to increase relative differences in low folate while reducing relative differences in adequate folate; credit score data show that lowering a credit score requirement will tend to increase relative differences in failing to meet it while reducing relative differences in meeting it. Similarly, published life tables show that relative racial and gender differences in mortality are generally greater among the young than the old, while relative differences in survival are generally greater among the old than the young.

B. Disclosure Issues

I am a co-author of the 2011 and 2014 versions of a technical assistance guide titled “Methods for Assessing Racial/Ethnic Disproportionality in Special Education,” produced by the IDEA Data Center pursuant grants by the Office of Special Education Programs of the U.S. Department of Education. The IDEA Data Center is a government-funded entity created to provide technical assistance to states regarding reporting and analyzing data maintained pursuant to the Individuals with Disabilities Education Act. Westat is the lead organization for the IDEA Data Center (working with seven other partners). Currently Westat’s involvement with IDEA Data Center is based on a \$6.5 million dollar grant awarded in 2013.

Since August 2014, Mr. Scanlan has frequently raised issues about the utility of the guidance provided in the IDEA Data Center disproportionality guide. Mr. Scanlan’s activities regarding the disproportionality guide are somewhat summarized in note 5 at page 4 of Mr. Scanlan’s October 8, 2015 letter to the leadership of the American Statistical Association,⁶ and include creation of a web page on the guide and treatment of the guide in methods workshops at four universities. In addition to contacting leadership of IDEA Data Center and Westat about the guide, Mr. Scanlan has contacted the Department of Education employees responsible for overseeing the grant underlying the guide. The severity of Mr. Scanlan’s criticism of the guide is reflected at pages 10-11 of the August 24, 2015 letter to the Department of Health and Human Services and the Department of Education referenced at pages 5-6 of the October 13 submission to *Society*.⁷ Mr. Scanlan provided a copy of the letter to the leadership of the IDEA Data Center. In questioning the utility of the guide Mr. Scanlan commonly cites "Race and Mortality Revisited," which he has also brought to the attention of the leadership of the IDEA Data Center.

⁶ A link to the letter may be found in the third paragraph following the prefatory note on the home page of jpscanlan.com. On pages 3-4 of the letter, Mr. Scanlan seeks recusal of American Statistical Association President David Morganstein from consideration of issues raised in the letter because of Mr. Morganstein’s position as Vice-President and Director of Westat and (a) the relationship of issues raised in the letter to the IDEA Data Center disproportionality guide and (b) actions Mr. Scanlan has taken regarding the guide.

[Add here whatever is appropriate regarding you knowledge or lack of knowledge of the letter to American Statistical Association, or the recusal request therein, at the time of the submission to *Society*.]

⁷ The letter (a link to which may be found in the seventh paragraph following the prefatory note of the home page of jpscanlan.com) states:

The IDEA Data Center guide mentioned above, though not leading observers erroneously to believe that reducing the frequency of a putative adverse outcome will tend to reduce relative differences in rates of experiencing the outcome, nevertheless shows no awareness of the way the frequency of the outcome tends to affect each of the measures the guide recommends. As with other guides that fail to reflect such awareness, the guide cannot provide useful instruction on the appraisal of the strength of the forces causing outcome rates of advantaged and disadvantaged groups to differ, and necessarily will commonly lead users to believe things that are not true. See the above-mentioned October 26, 2012 letter to Harvard Medical School and Massachusetts General Hospital (and others) regarding their jointly produced *Commissioned Paper: Health Care Disparities Measurement* and the discussion of that document in "Race and Mortality Revisited" at 344-345.

Points Mr. Scanlan makes in "Race and Mortality Revisited" and elsewhere about reliance on risk ratios and other standard measures of differences between outcome rates to appraise demographic differences also pertain to many other activities of Westat. At least four Westat clients are government agencies whose analyses of demographic difference issues have been criticized by Mr. Scanlan. A substantial proportion of Westat's yearly revenue involves activities that commonly employ methods that Mr. Scanlan criticizes in "Race and Mortality Revisited" and elsewhere.

The above information regarding Westat's interest in matters to which the reasoning of "Race and Mortality Revisited" pertains should have been disclosed in the transmittal of the submission.