

[This is a PDF version of an item posted on ASA Connect on October 14, 2016]

Dr. Norris:

I would suggest that before you write anything further on this matter, you read "Race and Mortality Revisited"<sup>1</sup> and do so with the following in mind.

In the article I seek to explain why it is impossible to analyze demographic differences in outcome rates in a useful way without understanding the way the measures employed in analyzing those differences tend to be affected by the prevalence of an outcome. And, as I have pointed out in various places, it does not matter whether the measures behave exactly in accordance with the patterns I describe. So long as the measures are in some manner affected by the prevalence of outcome, one cannot usefully appraise the differences in the circumstances of two groups reflected by their outcome rates – and whether, for example, such differences are increasing or decreasing or otherwise are larger in one setting than another – without attempting to take into account the way the measures used tend to be affected by the prevalence of an outcome.

With regard to law enforcement issues, it is especially important to understand that relaxing standards or otherwise reducing the frequency of adverse outcome tends to increase, not decrease, relative differences in rates of experiencing the outcomes. For example, the data in Table 1 of the 2006 Chance editorial<sup>2</sup> would seem to show conclusively that lowering an income requirement to secure some desired borrower outcome will tend to increase relative racial differences in failing to meet the requirement while reducing relative differences in meeting the requirement. The government, however, believes that lowering an income requirement will tend to reduce relative differences in failing to meet the requirement.

Thus, first things to consider are (1) whether anything you have been saying calls into question the essential validity of the points in "Race and Mortality Revisited" or raises issues about the importance of understanding the points it makes and (2) whether anything you are saying might suggest that the government is in fact correct as to the effects of generally reducing adverse outcome on relative differences in rates of experiencing those outcomes (or the proportion disadvantaged groups make up of persons experiencing the outcomes).

In "Race and Mortality Revisited" I also assert that there have been no statistically sound analyses of demographic differences in outcome rates, at least by persons employing either of the two relative differences or the absolute difference between rates to analyze such differences. For no such analyses have considered the implications of the patterns by which the measures

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<sup>1</sup> [http://jpscanlan.com/images/Race\\_and\\_Mortality\\_Revisited.pdf](http://jpscanlan.com/images/Race_and_Mortality_Revisited.pdf)

<sup>2</sup> [http://www.jpscanlan.com/images/Can\\_We\\_Actually\\_Measure\\_Health\\_Disparities.pdf](http://www.jpscanlan.com/images/Can_We_Actually_Measure_Health_Disparities.pdf)

employed tend to be affected by the prevalence of the outcome examined. And that holds for every institution in the world.

So a second thing to consider is whether anything you are saying calls the above proposition into question.

Further, and more concretely, in "Race and Mortality Revisited," Table 2 shows the ways reductions in poverty that remove from poverty the people who are just below the poverty line (as such reduction typically do) will tend to increase relative racial differences in poverty rates and reduce relative racial differences in rates of avoiding poverty. (I interject that no beliefs about the relationship of distributions can contradict what such data show.) Thus, I maintain that in times of declining poverty there can be no value whatever in studying why, for example, the relative racial difference in poverty increased without understanding the pattern reflected in the table (or why the white rate declined proportionately more than the black, which is a corollary to the increasing relative difference) without understanding the pattern reflected in the table. And that of course holds even if the pattern shown in the table merely provides a benchmark as to the effects on measures of a reduction in poverty. Same, of course, holds for the increases in poverty that will tend to reduce the relative differences in poverty rates. And the same holds for the other measures shown in the table.

Similarly, there is no value in opining (a) why having higher income tends to cause larger proportionate increases in black than white mortgage approval rates or (b) why having higher income tends to cause larger proportionate decreases in white than black mortgage rejection rates, without understanding the patterns discussed in "Race and Mortality Revisited." The list goes on and on with regard to essentially everything said about demographic differences where observers have relied on one measure or another without understanding how the prevalence of the outcome affects the measure. What value, for example, is there in opining about the mechanisms that cause British civil servants to show larger relative occupational differences in adverse health outcomes than the UK population at large without consideration of the fact that civil servants are a generally healthy group and without examining whether relative differences in the corresponding favorable outcomes are smaller among British civil servants than in the UK population at large? And is there any value in pondering why relative racial difference in infant mortality are greater among the well-educated than among the less-educated without understanding the implications of the fact that infant mortality rates tend to be lower among the well-educated or without attention to the fact that relative differences in infant survival tend to be lower among the well-educated than the less-educated.

Or, to take a matter utterly misunderstood when I addressed it 29 years ago<sup>3</sup> and that remains equally misunderstood today,<sup>4</sup> what possible value could there be in opining about why, during a particular period when poverty rates are generally changing, poverty is becoming more or less feminized without recognizing that – as is patently reflected in data in exactly the form the Census Bureau maintains it – general decreases in poverty will tend to feminize poverty and general increases in poverty will tend to defeminize poverty?.

These are questions that one ought to be able to answer in usefully addressing my work. The answers, however, ought all to be pretty obvious.

Further, it would be useful for you to know that every scholar who has considered the matter – and the National Center for Health Statistics – has essentially agreed with my claim about the ways relative differences tend to change as the prevalence of an outcome changes, starting in the UK in 2005.<sup>5,6</sup> Thus, for example, more than a decade ago NCHS statisticians recognized that because of the patterns I describe, determinations of whether health and healthcare disparities are increasing or decreasing would commonly turn on whether one examines relative differences in favorable outcomes health and healthcare or relative differences in the corresponding adverse health and healthcare outcomes. See "The Mismeasure of Health Disparities," Journal of Public Health Management and Practice (July/Aug. 2016)<sup>7</sup> [7] as well as "Race and Mortality Revisited" on this score. In this context, one must also ponder the value of a health disparities research regime where the CDC and AHRQ are unaware NCHS ever reached this conclusion and today are unaware that NCHS's recent hardly-noticed reversal of guidance repudiated a decade of research based on its earlier guidance including all the National Healthcare Disparities Reports.

It also would be useful for you to know that, in point fact, as the prevalence of outcomes has changed – or as is shown in things like life tables or NHANES data on systolic blood pressure and folate level – the observed patterns are pretty much in accord with the patterns I describe (even though there are factors at work, in the case of relative differences, mitigating the standard pattern as to one outcome and enhancing it as to other). That is, for example, if there occurs a lessening in the difference between the circumstances of two groups during a period of

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<sup>3</sup> "The 'feminization of poverty' is misunderstood," The Plain Dealer (Nov 11, 1987) (reprinted in *Current 1988;302*(May):16-18 and *Annual Editions: Social Problems 1988/89*. Dushkin1988.

[http://www.jpscanlan.com/images/Poverty\\_and\\_Women.pdf](http://www.jpscanlan.com/images/Poverty_and_Women.pdf)

<sup>4</sup> <http://jpscanlan.com/feminizationofpoverty.html>

<sup>5</sup> Carr-Hill R, Chalmers-Dixon P. The Public Health Observatory Handbook of Health Inequalities Measurement. Oxford: SEPHO; 2005: [http://www.sepho.org.uk/extras/rch\\_handbook.aspx](http://www.sepho.org.uk/extras/rch_handbook.aspx)

<sup>6</sup> <http://jpscanlan.com/scanlansrule/consensus.html>

<sup>7</sup> [http://www.jpscanlan.com/images/The\\_Mismeasure\\_of\\_Health\\_Disparities\\_JPHMP\\_2016\\_.pdf](http://www.jpscanlan.com/images/The_Mismeasure_of_Health_Disparities_JPHMP_2016_.pdf)

general declines in such outcome (or, say, as the population ages), such lessening will tend to cause the relative difference in the adverse outcome to increase less than it otherwise would while causing the relative difference in the corresponding favorable outcome to decrease more than it otherwise would. Of course, typically no one will know whether there has been such a lessening because they are unaware of the implications of the prevalence of an outcome on the standard measures employed to divine whether the differences between those circumstances are increasing or decreasing.

The patterns I describe will not always be observed, of course, as I have said again and again and again – and shown many examples of – for the last 25 years. But it is not possible to interpret the meaning of observed patterns of changes in a measure (or the comparative size of a measure in different settings) without knowing the usual effects of the prevalence of an outcome on the measure.

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I will eventually address further whether, when you developed a program either to illustrate or refute the pattern termed Scanlan's Rule (SR), you actually understood the pattern that I had been describing for close to three decades, as, say, illustrated in Figure 1 (at 13) of the October 2015 ASA letter,<sup>8</sup> including whether you then knew that I maintain that it is a pattern that exists across the entire distribution and have described it that way for over 25 years. (That is, that serially lowering a test cutoff from the point where almost every fails to a point where almost every pass, will tend to increase the relative difference in the decreasing outcome and decrease the relative difference in the increasing outcome (test passage)).

But you should look carefully at note 14 on page 10 of the letter and consider whether, in light of that note, one can possibly divide SR into trivial and non-trivial parts. Whether or not one recognizes that there really is only one part viewed from two perspectives, if one part is trivial, certainly the other part is trivial as well. I suggest that the division you created was not an interpretation of SR but something you developed at a time when you failed to understand SR as I envision it.

In July 2016 you contacted me saying that you had briefly scanned "Race and Mortality Revisited" and the July 2016 ASA letter<sup>9</sup> and thought you understood the key point, believing it to be sound, and wanted to suggest that I consider illustrating it graphically to assist the public and journalists in understanding it. Neither of the items you looked at had graphical illustrations and you were apparently unaware of my numerous graphical illustrations, including in over 30 conference presentations or university methods workshops in North America and Europe. I

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<sup>8</sup> [http://jpscanlan.com/images/Letter\\_to\\_American\\_Statistical\\_Association\\_Oct\\_8\\_2015.pdf](http://jpscanlan.com/images/Letter_to_American_Statistical_Association_Oct_8_2015.pdf)

<sup>9</sup> [http://jpscanlan.com/images/Letter\\_to\\_American\\_Statistical\\_Association\\_Oct\\_8\\_2015.pdf](http://jpscanlan.com/images/Letter_to_American_Statistical_Association_Oct_8_2015.pdf)

don't think you then looked at the 2015 University of Massachusetts Medical School seminar<sup>10</sup> to which I provided a link or at the Lambert & Subramanian 2014 Madras School of Economics paper (a later development of the article brought to your attention by Hoben Thomas) to which I provided you a link.<sup>11</sup> Nor was there any reason why you should have, since at that point you were simply doing me a favor.

But I gather that your only subsequent review of pertinent materials when you started posting on this thread involved review of my article posted at the beginning of this thread.

A further exchange following your initial comment led to my request of October 6, which was quite specific:

It may be that there are better ways, either as to tone or substance, of articulating my points than found, say, in my October 8, 2015, letter to ASA (or the letters to other entities collected in reference 10 or the workshops collected in reference 11). I always welcome suggestions. And I have received some useful ones (including from Dr. Norris). But it is best when the[y] are focused on particular illustrations that I do use.

I was eliciting view on how, for example, Figure 1 of the October 2015 ASA letter would fail to effectively convey its key points to persons of average intelligence. Be mindful I am working from the experience of over 30 conferences and workshop, with responses from audiences and other presenters, as well as interactions with editors who approved or suggested some of the illustrations (including that in "Divining Difference," Chance (Fall 1994)<sup>12</sup> as well as the 2006 Chance editorial). But it seems to me that you did not refer to any of those materials to see, for the first time, my graphical illustrations or the many tables I use to illustrate the patterns (including Table 5 of "Race and Mortality Revisited, which is also Table 5 of the October 2015 ASA letter, and which is key element in my efforts to show that value judgments have no place in efforts to appraise the difference in the circumstances of two groups reflected by their rates (or the forces causing the rates to differ).

In any case, in purporting to respond to my request, you ignored its specifics. Then, apparently unguided by an understanding of the patterns I described as reflected in my illustrations (or any other of my published work save for that just mentioned) and while still no more than briefly scanning "Race and Mortality Revisited"), you broke SR down into two parts. One you considered to be trivial and the other you considered to be non-trivial and addressed in your app. The non-trivial part was something never suggested by me and involved terms I did not even know (though probably I knew them when I took calculus 50 years ago).

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<sup>10</sup> [http://jpscanlan.com/images/Univ\\_Mass\\_Medical\\_School\\_Seminar\\_Nov\\_18,\\_2015\\_.pdf](http://jpscanlan.com/images/Univ_Mass_Medical_School_Seminar_Nov_18,_2015_.pdf)

<sup>11</sup> [http://www.jpscanlan.com/images/Letter\\_to\\_American\\_Statistical\\_Association\\_July\\_25,\\_2016\\_.pdf](http://www.jpscanlan.com/images/Letter_to_American_Statistical_Association_July_25,_2016_.pdf)

<sup>12</sup> [http://jpscanlan.com/images/Divining\\_Difference.pdf](http://jpscanlan.com/images/Divining_Difference.pdf)

The end result is something that by no means addresses whether one can measure demographic differences without consideration of patterns by which measures tend to be affected by the prevalence of an outcome and that does not at all enlighten me on the strengths or weaknesses of my illustrations of the patterns I want people to understand.

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In any case, please read "Race and Mortality Revisited" and ideally a few other things, say, like the 2013 Federal Committee on Statistical Methodology paper<sup>13</sup> and the 2016 Journal of Public Health Practice and Management commentary. Read the FCSM paper with special attention to its Table 3, at 17, keeping in mind that in the case where the authors found a very large increase in disparity in reliance on NCHS guidance, the NCHS would now say that it was very large decrease in disparity. And peruse a few of the 100 or so web pages I have discussing, among other things, nuances of the patterns as they appear in actual data. Such pages may usefully inform you as to things you might not have figured out on your own as well as to the scope of research that is undermined by the patterns I describe. Consider, for example, whether it is useful to know that researcher will examine in the same study such things as changes in (a) relative differences rates of receiving no immunization and (b) relative differences in rates of full immunization, while not understanding the reasons that general increases in immunization will tend to increase the former and reduce the latter.

Also, please examine my discussions of why what I term "irreducible minimums" and others call "minimum achievable levels" will tend to cause certain patterns I describe not to be observed as outcomes approach those minimums . See especially such discussion in "Race and Mortality," Society (Jan. Feb. 2000),<sup>14</sup> the 2006 Chance editorial, the 2006 British Society for Populations Studies paper,<sup>15</sup> "Race and Mortality Revisited," and my Irreducible Minimums<sup>16</sup> and Solutions web pages.<sup>17</sup> I do not believe they involve the same issue addressed in your app. But they are treatments of which you should be aware in treating the things addressed in the app. The issue is one of the many things I point to in order to explain why, merely being a tendency, SR will not always be observed.

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<sup>13</sup> Measuring Health and Healthcare Disparities. Federal Committee on Statistical Methodology 2013 Research Conference, Washington, DC , Nov. 4-7: Paper:

[http://jpscanlan.com/images/2013\\_Fed\\_Comm\\_on\\_Stat\\_Meth\\_paper.pdf](http://jpscanlan.com/images/2013_Fed_Comm_on_Stat_Meth_paper.pdf)

<sup>14</sup> [http://www.jpscanlan.com/images/Race\\_and\\_Mortality.pdf](http://www.jpscanlan.com/images/Race_and_Mortality.pdf)

<sup>15</sup> The Misinterpretation of Health Inequalities in the United Kingdom. British Society for Populations Studies Conference 2006, Southampton, England, Sept. 18-20, 2006:

[http://www.jpscanlan.com/images/BSPS\\_2006\\_Complete\\_Paper.pdf](http://www.jpscanlan.com/images/BSPS_2006_Complete_Paper.pdf)

<sup>16</sup> <http://www.jpscanlan.com/measuringhealthdisp/irreducibleminimums.html>

<sup>17</sup> <http://www.jpscanlan.com/measuringhealthdisp/solutions.html>

I suggest that it is also worthwhile to consider whether, as I claim, irrespective of SR (but for reasons suggested by SR), as discussed in "Race and Mortality Revisited" (at 339) and October 2015 ASA letter (at 12-13), the rate ratio is an illogical measure of association. The ultimate purpose of SR is not to predict patterns of relative differences. It is to show why relative differences are not useful measures.

I don't know how reading "Race and Mortality Revisited" or the other materials will affect your characterization of Scanlan's Rule. But I think that, however you characterize it, and even if you believe you have somehow disproved it, the fact will remain that it is not possible to analyze group differences without understanding SR as I have articulated it or knowing such more precise variation on it as may be divined in nature. And certainly one cannot draw sound inferences about processes on the basis of the comparative size of two relative differences without knowing that the relative difference as to the opposite outcome tends to (or in fact does) support an opposite inference. That holds in any case, but it holds especially in the circumstances where researchers talk about changes in relative differences in survival while in fact analyzing relative differences in mortality, and without having any idea that the two will often/commonly/sometimes (or did in fact) change in opposite directions – and without imagining that that is even possible.<sup>18</sup>

Similarly, the manner in which one characterizes Scanlan's Rule hardly obviates the need for the federal government to understand that its belief about the effects of reducing adverse outcomes on relative differences in rates of experiencing them is the opposite of reality. I add here that one cannot ignore that, in accordance with the patterns I describe, all across the country school districts that have been relaxing standards to reduce relative racial differences in suspensions and finding that such differences are increasing. Presumably, in many places where there is concern about the role of discrimination in these differences, persons are assuming that, since the reductions should have reduced the relative differences, the fact that they have increased must suggest worsening discrimination. That is a hardly an unreasonable inference given the standard belief about the way reductions should affect relative differences. But it is an unfounded inference given those effects as they actually tend to be.

The same holds for inferences that may be drawn by the President about racial discrimination in the criminal justice system if reforms that are mistakenly expected to reduce relative differences in adverse criminal justice outcomes are in fact accompanied by increases in those differences.

And, of course, it is a great mistake to talk about my work without understandings the patterns I describe by which absolute differences tends to be affected by the prevalence of an outcome – as discussed in "Race and Mortality Revisited" and scores of other places over the last decade. However one may characterize those patterns – and whatever may be said about the

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<sup>18</sup> [http://www.jpscanlan.com/images/Mortality\\_and\\_Survival.pdf](http://www.jpscanlan.com/images/Mortality_and_Survival.pdf)

accuracy of my descriptions – it ought to be clear that there can no value whatever (and much harm) from reliance on such measure without any consideration of the way it tends to be affected by the prevalence of an outcome. See especially the discussion of pay-for-performance in "Race and Mortality Revisited." But I am straying back to my first point of whether your analysis is calling into question the essential validity of the claim in "Race and Mortality Revisited" that one cannot rely on a measure without consideration of how it tends to be affected by the prevalence of an outcome.

Meanwhile, I will review your app for purpose of determining whether I understand it, agree or disagree with it, and whether, in either case, I think it has any important bearing on ideas I wish to covey in works like "Race and Mortality Revisited."

This is not to say there are no important things to be said in this area. There are many. "Race and Mortality Revisited" invites such things with its reference (at 337) to measures theoretically unaffected by the prevalence of an outcome “that might be better informed as to actual shapes of the underlying risk distributions.” And I certainly would like to see someone address how to deal with situations where we know that the distributions substantially depart from normal because they are truncated portions of larger distribution or the conundrum addressed in the Addendum to the Ferguson Arrest Disparities page.<sup>19</sup>[19] I am less interested in treatments of SR that do not call into question the need to know it.

Regards,

Jim Scanlan

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<sup>19</sup> <http://jpscanlan.com/disciplinedisparities/fergusonarrestdisp.html>