

**Outline for Response to Request for Comments
for the Commission on Evidence-Based Policymaking
DRAFT
(Oct. 28, 2016)**

This is an outline of a planned response to Question 16 (“How can data, statistics, results of research, and findings from evaluation, be best used to improve policies and programs?”) of the September 14, 2016 Request for Comments for the Commission on Evidence-Based Policymaking. It will developed into a formal group of comments prior to submission, which must occur no later than November 14, 2016. The most recent version of this document as it evolves into the item to be submitted may be found at http://www.jpscanlan.com/images/Outline_of_Comments_to_CEBP.pdf.

The response will make the following six points:

1. It is not possible to soundly analyze demographic differences involving dichotomies (e.g., mortality/survival, receipt/non-receipt of healthcare, approval/rejection of a mortgage application) without taking into account the way the measures employed tend to be affected by the prevalence of an outcome.
2. Analyses of subgroup effects (also termed “interaction” or “effect heterogeneity”) cannot be based on an assumption that absent such effects one will observe a constant relative effect across different baseline rates for the outcome. Nor can one rely on such assumption to employ the results of a clinical trial to calculate the number-needed-to treat in a situation involving subjects with different baseline rates from those in the trial.
3. The rate ratio is an illogical measure of association that should not be used for any purpose.
4. It is impossible to soundly analyze demographic difference by comparing the proportion a group comprises of persons potentially experiencing an outcome and the proportion it comprises of persons actually experiencing the outcome.
5. Policies should never be based on the belief that relaxing a standard or otherwise reducing the frequency of an adverse outcome will tend to reduce relative differences in rates of experiencing the outcome and the proportions groups most susceptible to the outcome make up of persons experiencing it (as in the case of the belief underlying federal civil rights law enforcement policies involving lending, public school discipline, criminal justice, and employment). Exactly the opposite case.
6. Analyses of discrimination issues, whether involving binary or continuous variables, cannot be based on data pertaining solely to persons who accepted some outcome or situation.

Information that will be discussed with regard to each of the points follows:

1. It is not possible to soundly analyze demographic differences involving dichotomies (e.g., mortality/survival, receipt/non-receipt of healthcare, approval/rejection of a mortgage application) without taking into account the way the measures employed tend to be affected by the prevalence of an outcome.

This section will address problems in analyses of demographic differences that fail to address the effect of the prevalence of an outcome on the measure employed. To be clear, the key point is of the section is that analyses to date that have attempted to appraise changes in demographic differences over time or draw inferences about policies or practices have been almost invariably unsound and misleading as a result of failure to address this issue.

A particular focus of the section will be on studies of health and healthcare disparities which, according to the Department of Health and Human Services budget, consume almost 10% of the agency's budget. But the points made apply to all analyses of demographic differences, including things like analyses of changes in differences between poverty rates of advantaged and disadvantaged groups that fail to show any awareness, for example, that census data make clear that general reductions in poverty will tend to increase relative differences in poverty rates, reduce relative differences in rates of avoiding poverty, and reduce absolute differences between poverty rates. See discussion regarding Table 2 of "[Race and Mortality Revisited](#)," *Society* (July/Aug. 2014) (at 329, 343). See generally my "[Can We Actually Measure Health Disparities?](#)," *Chance* (Spring 2006).

Key references, apart from the two just mentioned, include my "[The Mismeasure of Health Disparities](#)," *Journal of Public Health Management and Practice* (July/Aug. 2016) (JPHMP commentary), "[Measuring Health and Healthcare Disparities](#)," Proceedings of the Federal Committee on Statistical Methodology 2013 Research Conference (March 2014)(FCSM paper), "[The Mismeasure of Discrimination](#)," Faculty Workshop, University of Kansas School of Law (Sept. 2013), as well as of my amicus curiae [brief](#) in *Texas Department of Housing and Community Development v. The Inclusive Communities Project, Inc.* (2014) (TDHCA brief) and my October 8, 2015 [letter](#) to the American Statistical Association (October 2015 ASA letter).

The JPHMP commentary is the shortest of the above treatments. A recent, more succinct treatment may be found in my [Comment on Mackenbach BMJ 2016](#). The most recent collection of extensive graphical and tabular illustrations may be found in my November 2016 University of Massachusetts Medical School seminar "[The Mismeasure of Health Disparities in Massachusetts and Less Affluent Places](#)," (UMMS seminar) ([abstract](#)).¹

Patterns by which measures of differences between outcome rates tend to be affected by the prevalence of an outcome include that whereby the rarer an outcome the greater tends to be the

¹ Illustrations in other workshops are similar to those in the UMMS seminar, though they vary somewhat in focus. And the other workshops do not give special attention to Massachusetts except with regard to the anomalies in the Massachusetts Medicaid pay-for-performance program. See [UC Irvine](#) (2015), [George Mason U](#) (2014), [U Maryland](#) (2014), [U Minnesota](#) (2014), [Harvard](#) (2012), [American U](#) (2012).

relative difference in experiencing it and the smaller tends to be the relative difference in avoiding it. Thus, to use examples that one in fact commonly observes when examining data closely, as mortality declines relative differences in mortality rates tend to increase while relative differences in survival rates tend to decrease; as healthcare improves relative differences in rates of receipt of appropriate care tend to decrease while relative differences in rates of non-receipt of appropriate care tend to increase. Absolute differences and odds ratios also tend to be affected by the prevalence of an outcome, though in a more complicated way than the two relative differences.

Roughly, as an outcome goes from being rare to being common, absolute differences tend to increase; as an outcome goes from being common to being very common, absolute differences tend to decrease. As the frequency of an outcome changes, the absolute difference tends to change in the same direction as the smaller relative difference. Since persons relying on relative differences to appraise demographic differences typically examine the larger of the two 'relative differences' such persons tend to systematically reach opposite conclusions about directions of changes over time from persons relying on absolute differences.

As the prevalence of an outcome changes, the difference measured by the odds ratio tends to change in the opposite direction of the absolute difference

Graphical illustrations of these patterns based on test score data and income data, on single pages for each, may be found in slides 45 and 88 of the UMMS [seminar](#).

All measures may change in the same direction as the prevalence of an outcome changes. In that case, one may infer that there occurred a meaningful change in the strength of the forces causing the outcome rates of the advantaged and disadvantaged groups to differ. But anytime a relative difference and the absolute difference change in opposite directions, the other relative difference will necessarily have changed in the opposite direction of the first relative difference and the same direction as the absolute difference.

Certain examples to be highlighted in this section pertain to the recognition of the above-described pattern of relative differences by National Center for Health Statistics statisticians, beginning in 2004, and their belief that they could address the matter simply by choosing to rely on the relative difference in the adverse outcome rather than the relative difference in the favorable outcome. In the case of healthcare that led to the situation where improvements in healthcare tended to be associated with increased healthcare disparities. But in 2015-16 NCHS reversed that position and now will measure healthcare disparities in terms of relative differences in favorable outcomes and improvements in healthcare will tend to be associated with reduced healthcare disparities.

Examples of the anomalies arising from the NCHS position and its reversal may be found in Tables 3 and 7 of the FCSM paper. Table 3 involved a study that specifically relied on NCHS guidance to find that general increases in mammography rate were associated with very large increase in mammography disparities by income group (meaning relative differences in rates of failure to receive mammography). Table 7 involved a study where the authors, unaware of or ignoring NCHS guidance, measured vaccination disparities in terms of relative differences in

receipt of vaccination rather than non-receipt of vaccination. The authors found substantial increases in vaccination rates to be associated with large decreases in disparities (while NCHS would have reached an opposite conclusion). The recent reversal of position by NCHS causes the agency now to disagree with the study that relied on its earlier guidance and agree with the study that ignored that guidance. The reversal also constitutes a repudiation of a decade of National Healthcare Disparities Reports that relied on the earlier guidance. See "The Mismeasure of Health Disparities," mentioned above.

Another anomaly is discussed at page 4-5 of the letter to the [Stanford Center on Poverty and Inequality](#) (Mar. 8, 2016). A 2016 study by the Center relied on NCHS guidance to measure insurance disparities in terms of relative differences in uninsurance. It also drew inferences about underlying processes based on the comparative size of relative differences in rates of uninsurance in different settings, though the comparative size of relative differences in rates of insurance would commonly support opposite inferences. In relying on the NCHS recommendation to rely on relative differences in adverse healthcare outcomes, the authors were unaware that the NCHS had already reversed its position.

Apart from NCHS, no federal agency involved in health and healthcare research or any other matter has yet recognized that it is even possible for relative differences in an adverse outcome and relative differences in the corresponding favorable outcome to change in opposite direction much less that NCHS statisticians more than a decade ago recognized that this tends to occur systematically.² It is unlikely that Agency for Healthcare Research and Quality is yet aware that the NCHS has reversed its position regarding healthcare disparities and repudiated the first decade of National Healthcare Disparities Reports.

Similar anomalies exist with regard to reliance on the absolute difference between rates without regard to the way such measure tends to be affected by the prevalence of an outcome. See especially "Race and Mortality Revisited" at 337-339 regarding the way Massachusetts was led to believe that it was necessary to include a healthcare disparities element in its Medicaid pay-for-performance program and measured such disparities in a way that would tend to increase health and healthcare disparities regardless of how such disparities are measured.

Example of other striking anomalies may be found on my [AHRQ's Vanderbilt Study and Spurious Contradictions](#) pages, as well as in my letter to [Agency for Healthcare Research and Quality](#) (July 1, 2015). The last item involves the fact that confusion over how it was measuring disparities caused the National Healthcare Disparities Report to highlight as some of the largest reductions in healthcare disparities over a certain period situations where AHRQ would also find much larger disparities at the end of the period than at the beginning of the period.

² In addressing school discipline disparities the Department of Health and Human Services has indicated a view as to the affects of the prevalence of an outcome on relative differences in adverse outcomes that is the opposite of that recognized by NCHS. See letter to [Department of Health and Human Services and Department of Education](#) (Aug. 24, 2015). That matter is the subject of Point 5.

These anomalies, however, merely highlight confusion among persons and institutions conducting health and healthcare disparities research. The fundamental problem is that, whatever measures the research employs, the research makes no effort to distinguish changes in measures that are solely functions of changes in the prevalence of an outcome from changes that may be functions of policies or reflective of something meaningful about underlying processes.

As reflected in "Race and Mortality Revisited" and the October 2015 ASA letter, these same problems exist with regard all analyses of demographic differences regardless of the outcome. I note, however, that the above materials give only limited attention to disparities in educational outcomes. Observers analyzing those disparities employing relative differences have relied on either relative differences in the favorable outcome or relative differences in the adverse outcome while evidencing no understanding that improvement in educational outcomes will tend to reduce the former and increase the latter. See my letter [New York City Center for Innovation through Data Intelligence](#) (June 6, 2016) and my [Harvard CRP NCLB Study](#) and the [McKinsey Achievement Gap Study](#) subpages of the [Educational Disparities](#) page of [jpscanlan.com](#).

More often, however, disparities in rates of achieving various levels of proficiency are analyzed in terms of absolute differences between rates. Invariably, however, such analyses have been conducted without recognizing that improving achievement levels will tend to increase absolute differences between rates where outcome rates are fairly low (e.g., difficult subjects, reaching advanced proficiency levels) while reducing absolute differences where outcome rates are fairly high (e.g., easier subjects, reaching the basic proficiency level). See the Educational Disparities page and its [Disparities by Subject](#), [New York Proficiency Rate Disparities](#), [Education Trust High Achiever Study](#), [Education Trust Glass Ceiling Study](#), [Education Trust High Achiever Study](#), [Education Trust Glass Ceiling Study](#), [Annie E. Casey 2014 Proficiency Disparities Study](#) subpages.

One notable development in the area is the work of Harvard Professor Andrew Ho and Stanford Professor Sean Reardon. Professor Ho independently recognized the patterns by which absolute differences between outcome rates tend to be affected by the prevalence of an outcome and the implications of those patterns regarding the appraisal of proficiency disparities. Figure 2 (at 353) of Professor Ho's 2008 paper³ is conceptually comparable to Table 5 (at 335) of "Race and Mortality Revisited" and Table 5 (at 22) of the October 2015 ASA letter and is essentially the same thing as Figure 2 (at 21) of the ASA letter with respect to demonstrating the effects of the prevalence of an outcome on measures of differences between outcome rates. See also Table 1 (at 21) and Figure 4 (at 24) of my 2006 British Society for Population Studies paper "[The Misinterpretation of Health Inequalities in the United Kingdom](#)." The Ho article and the illustration in its Figure 2 is focused solely on patterns by which absolute differences tend to be affected by the prevalence of an outcome. But the rates that underlie the table would also form the basis for the illustrations in Figures 1 and 3 in the ASA letter.⁴ See also Figure 3 (slide 45) the UMMS seminar.

³ Ho, Andrew D. 2008. "[The problem with 'proficiency': Limitations of statistics and policy under No Child Left Behind](#)," *Educational Researcher*, 37,351–360.

⁴ The pattern by which absolute differences tend to be affected by the prevalence of an outcome was also independently recognized in Houweling TAJ, Kunst AE, Huisman M, & Mackenbach JP. 2007. "[Using relative and](#)

Work of Professors Ho and Reardon regarding the measurement of proficiency disparities in ways unaffected by the prevalence of an outcome, seems to suggest approaches generally along the lines of the approach suggested in "Race and Mortality Revisited" and the October 2015 ASA letter.⁵ But such matter will be beyond the scope of these comments.

Otherwise, however, demographic differences are universally studied without regard to the way the measures employed tend to be affected by the prevalence of an outcome and hence are incapable of providing useful information on the effects of policies directed at addressing demographic differences or processes underlying the differences. Some indication of scope of unsound research may be found in the letters I have sent to organization and institutions about fundamental flaws in analyses that they conduct or that pertain to them, or that, in the case of legislative entities, involve matters they oversee.⁶ But the list should be examined with recognition that there is no research institution in the world that has yet shown an understanding of the patterns by which standard measures of demographic differences tend to be affected by

[absolute measures for monitoring health inequalities: experiences from cross-national analyses on maternal and child health](#)," *International Journal for Equity in Health* 6:15 (based on cross-national data on various healthcare outcomes). The article also recognized the pattern by which the two relative differences tend to change in opposite directions as the prevalence of an outcome changes.

⁵ Ho, Andrew D., and Reardon, Sean F. 2012. "[Estimating Achievement Gaps From Test Scores Reported in Ordinal 'Proficiency' Categories](#)," *Journal of Educational and Behavioral Statistics*, 37(4), 489–517.

⁶ Recipients since 2009 include [Oklahoma City School District](#) (Sept. 20, 2016), [Antioch Unified School District](#) (Sept. 9, 2016), [American Statistical Association II](#) (July 25, 2016), [Federal Judicial Center](#) (July 7, 2016), [University of Oregon Institute on Violence and Destructive Behavior and University of Oregon Law School Center for Dispute Resolution II](#) (July 5, 2016), [University of Oregon Institute on Violence and Destructive Behavior and University of Oregon Law School Center for Dispute Resolution](#) (July 3, 2016), [New York City Center for Innovation through Data Intelligence](#) (June 6, 2016), [Consortium of Social Science Associations](#) (Apr. 6, 2016), [Population Association of America and Association of Population Centers](#) (Mar. 29, 2016), [Council of Economic Advisers](#) (Mar. 16, 2016), [City of Madison, Wisconsin](#) (Mar. 12, 2016), [Stanford Center on Poverty and Inequality](#) (Mar. 8, 2016), [City of Boulder, Colorado](#) (Mar. 5, 2016), [Houston Independent School District](#) (Jan. 5, 2016), [Boston Lawyers' Committee for Civil Rights and Economic Justice](#) (Nov. 12, 2015), [House Judiciary Committee](#) (Oct. 19, 2015), [American Statistical Association](#) (Oct. 8, 2015), [Chief Data Scientist of White House OSTP](#) (Sept. 8, 2015), [McKinney, Texas Independent School District](#) (Aug. 31, 2015), [Department of Health and Human Services and Department of Education](#) (Aug. 24, 2015), [Agency for Healthcare Research and Quality](#) (July 1, 2015), [City of Minneapolis, Minnesota](#) (June 8, 2015), [Texas Appleseed](#) (Apr. 7, 2015), [Senate Committee on Health, Education, Labor and Pensions](#) (Mar. 20, 2015), [United States Department of Justice and City of Ferguson, Missouri](#) (Mar. 9, 2015), [Vermont Senate Committee on Education](#) (Feb. 26, 2015), [Portland, Oregon Board of Education](#) (Feb. 25, 2015), [Wisconsin Council on Families and Children's Race to Equity Project](#) (Dec. 23, 2014), [Financial Markets and Community Investment Program, Government Accountability Office](#) (Sept. 9, 2014), [Education Law Center](#) (Aug. 14, 2014), [IDEA Data Center](#) (Aug. 11, 2014), [Institute of Medicine II](#) (May 28, 2014), [Annie E. Casey Foundation](#) (May 13, 2014), [Education Trust](#) (April 30, 2014), [Investigations and Oversight Subcommittee of House Finance Committee](#) (Dec. 4, 2013), [Mailman School of Public Health of Columbia University](#) (May 24, 2013), [Senate Committee on Health, Education, Labor and Pensions](#) (Apr. 1, 2013), [Federal Reserve Board](#) (March 4, 2013), [Harvard University et al.](#) (Oct. 26, 2012), [Harvard University](#) (Oct. 9, 2012), [United States Department of Justice](#) (Apr. 23, 2012), [United States Department of Education](#) (Apr. 18, 2012), [The Commonwealth Fund](#) (June 1, 2010), [Institute of Medicine](#) (June 1, 2010), [National Quality Forum](#) (Oct. 22, 2009), [Robert Wood Johnson Foundation](#) (Apr. 8, 2009).

prevalence of an outcome or the implications of those patterns with regard to interpretation of data on such differences.

In summary, many billions of dollars are wasted on research that does not even attempt to distinguish between the effects of changes in the prevalence of an outcome and effects of policies. More important, sound research is not conducted.

Thus, the Commission should form a committee specifically to address the flaws in analyses of demographic difference arising from the failure to consider the effects of the prevalence of an outcome on the measure employed. The October 2015 ASA letter, which recommended that the ASA form such a committee, may serve as a guide.⁷

And one thing the Commission should recommend that government agencies funding research should require the following:

All funding requests for research into demographic differences should include the following statements:

1. We are aware that there exist patterns by which measures commonly employed in this type of research tend to be affected by changes in the prevalence of the outcome examined irrespective of (a) actual changes in differences in the circumstances of advantaged and disadvantaged groups or (b) effects of policies aimed at mitigating those differences.
2. We intend to attempt to distinguish the effects of the patterns by which measures tend to be affected by the prevalence of an outcome from (a) and (b) in the following manner:

Studies themselves should include at their beginning the following statements:

1. This study has (or has not) attempted to distinguish between the effects of changes in the prevalence of an outcome on the measures employed and (a) actual changes in differences in the circumstances of advantaged and disadvantaged groups (b) effects of policies aimed at mitigating those differences.
2. Because of 1, this study may be (should not be) used to inform policy.

The language can be adjusted to address the situation of research aimed at appraising the effects of factors other than policies on demographic differences. It can also be adjusted to address situations where, rather than examining changes in differences over time, researchers compare, and opine about, the size of some difference within settings differentiated other than temporally (and where the settings differ in the overall prevalence of an outcome). Examples include comparing health disparities among British civil servants with those among the UK population at large or comparing demographic differences as to some outcome in countries like Norway and

⁷ On September 29, 2016, the ASA informed me that the organization's Science and Public Affairs Subcommittee believed that I was effectively highlighting the issues raised in my October 2015 letter and my other communications to the organizations and did not see an additional role ASA could play in the matter.

Sweden and states like Minnesota and Massachusetts with those in less affluent (or healthy) places. Such research has universally failed to recognize reasons to expect relative differences in adverse outcomes to be comparatively large, and relative differences in the corresponding favorable outcomes to be comparatively small, in settings where the adverse outcomes are comparatively uncommon. See "Race and Mortality Revisited" (at 34) regarding British civil servants. Issues regarding advantaged geographic areas are the subject of the UMMS seminar. See also the 2006 *Chance* editorial (at 50) and my "[It's easy to misunderstand gaps and mistake good fortune for a crisis](#)," *Minneapolis Star Tribune* (Feb. 8, 2014).

2. Analyses of subgroup effects (also termed "interaction" or "effect heterogeneity") cannot be based on an assumption that absent such effects one will observe a constant relative effect across different baseline rates for the outcome. Nor can one rely on such assumption to employ the results of a clinical trial to calculate the number-needed-to treat in a situation involving subjects with different baseline rates from those in the trial.

I first explained the reasons to expect that a factor affecting an outcome rate will tend to cause a larger proportionate change in an outcome for the group with the lower baseline rate while causing a larger proportionate change in the opposite outcome rate for the other group in "[Divining Difference](#)," *Chance* (Fall 1994). Such pattern is simply a corollary to 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. Thus, in the many situations where in reality one finds the described pattern of relative differences one necessarily will also find the described pattern or proportionate changes.

That is, for example, data showing that relative differences in mortality decrease with age while relative differences in survival increase with age (as shown the [Life Tables Illustrations](#) page, the [Life Table Information Document](#), and [Table 6](#) of the [2008 Nordic Demographic Symposium presentation](#)) necessarily also show that age increases mortality proportionately more for the advantaged group while reducing survival proportionately more for the disadvantaged group. See also my [Interactions by Age](#) page. Data showing that relative racial differences in mortgage rejection rates are greater, while relative differences in mortgage approval rates are smaller, among higher-income than lower-income applicants necessarily also show that having high income reduces rejection rates proportionately more for whites than blacks but increases mortgage approval rates proportionately more for blacks than whites. See "Race and Mortality Revisited" (at 340-41). Data showing that relative racial differences in dichotomized poor self-rated health are greater, but relative differences in dichotomized good self-rated health are smaller, among higher-income than lower-income groups (see [Table 5](#) (at 66) of the UMMS seminar) necessarily also show that having high income decreases poor health proportionately more among whites than blacks while increasing good health proportionately more among blacks than whites.

For specific example where observers believe they have found a subgroup effect while focusing solely on the situation where a factor caused a larger proportionate change in a rate for the group with the lower baseline rate (while the factor was in fact also causing a larger proportionate change in the opposite outcome rate for the other group), see [Table 7](#) (at 341) of "Race and

Mortality Revisited, the tables in [JSM 2009 Presentation](#), and the [Comment on Berrington de Gonzalez NEJM 2010](#).

See the [Subgroup Effects – Nonclinical](#) page regarding the effect of family situation on black and white rates of being poor or non-poor and the corresponding effects of race on the rates at which female-headed and married-couple families are poor or non-poor. The page also discusses a study finding that proportionate increase in loan denial rates as a result of being in a particular location was greater for whites than for blacks while the effects of location on home ownership was proportionately smaller for whites than blacks. That is, while focusing on either the adverse or favorable outcome, the study found a larger proportionate effect on the group with the lower baseline rates (though the study did not make available the data that would typically show the opposite interactive effect in examining the opposite outcome).

See the [Subgroup Effects](#) page and [Comment on Chatellier BMJ 2011](#) regarding the problems in employing the relative effect (either as to the favorable outcome or the adverse outcome) in a clinical trial to estimate the absolute effect (and associated number-needed-to-treat) in a setting involving a different baseline rates from that in the trial.

To my knowledge, no studies of subgroup effect have considered these issues.

More recent treatments of this subject have discussed that, irrespective of the described pattern of proportionate changes, it is illogical to expect a factor to cause equal proportionate changes in different baseline rates for an outcome, since it is impossible for a factor to cause equal proportionate changes in rates of experiencing an outcome for groups with different baseline rates for the outcome while at the same time causing equal proportionate changes in the opposite outcome rates. See "Race and Mortality Revisited" (at 339), October 2015 ASA letter (at 12-13) and my, [Illogical Premises](#), [Illogical Premises II](#), and [Inevitability of Interaction](#) pages, as well as [Comment on FDA Proposed Subgroup Regulations](#) (May 16, 2014) and [Comment on European Medicines Agency Subgroup Guidelines](#) (July 31, 2014). A fairly succinct treatment of this point may be found in my [Comment on Hingorani BMJ 2013](#).

3. The rate ratio is an illogical measure of association that should not be used for any purpose.

This subject is encompassed in Point 2. It may be distinguishable in the way that the subject of the [Illogical Premises II](#) page is distinguishable from the subject of the [Illogical Premises](#) page. See also the October 2015 ASA letter (at 12-13). Whether it warrants separate treatment will be determined.

4. It is impossible to soundly analyze demographic difference by comparing the proportion a group comprises of persons potentially experiencing an outcome and the proportion it comprises of persons actually experiencing the outcome.

Demographic differences are often analyzed in terms of the difference between (a) the proportion a group comprises of persons potentially experiencing an outcome (the pool) and (b) the proportion it comprises of persons actually experiencing an outcome. Often this is done without

specifically quantifying the difference between (a) and (b). But a Department of Education-funded document titled “[Methods for Assessing Racial/Ethnic Disproportionality in Special Education](#)” which I discuss on the my [IDEA Data Center Disproportionality Guide](#) page, shows how to calculate both relative and absolute differences between (a) and (b) and those are the likely approaches of anyone who wants to quantify differences between (a) and (b).

The proportion a group comprises of persons experiencing an outcome is a function of the relative difference and therefore the appraisal of disparities based on any measure of the difference between (a) and (b) is unsound in the same way the relative difference is unsound when employed without regard to the manner in which the measure tends to be affected by the prevalence of an outcome. See October 2015 ASA letter (at 11-12). Further, however, as reflected in "Race and Mortality Revisited" and the October 2015 ASA letter, and as discussed more specifically in the Kansas Law paper (Section D, at 23-26), in order to appraise a disparity involving some outcome one must have the actual rates at which the groups being examined experience the outcome. That information cannot be derived from (a) and (b).

In addition, as discussed on the referenced IDEA Data Center Disproportionality Guide page, any measure of difference between (a) and (b) tends to be affected by the proportion the subject group makes up of the population. That is, suppose that the rates for an adverse outcome of an advantaged group (AG) and disadvantaged group (DG) are 10% and 20% respectively in two settings (e.g., different school districts). Putting aside how one might effectively appraise the difference in the circumstances of AG and DG reflected by the 10% and 20% outcome rates, there is no basis to say the difference is larger in one setting than the other. Yet, given those 10% and 20% rates, the relative and absolute differences between (a) and (b) will vary in a variety of ways depending on the proportion the disadvantaged group comprises of persons potentially experiencing the outcome. .

Thus, there are reasons why one should never attempt to appraise demographic differences based on comparisons of (a) and (b) irrespective of the reasons why one cannot appraise demographic differences based on other measures that are discussed in Section 1 without consideration of the effect of the prevalence of an outcome on the measure.

5. Policies should never be based on the belief that relaxing a standard or otherwise reducing the frequency of an adverse outcome will tend to reduce relative differences in rates of experiencing the outcome and the proportions groups most susceptible to the outcome make up of persons experiencing it (as in the case of the belief underlying federal civil rights law enforcement policies involving lending, public school discipline, criminal justice, and employment). Exactly the opposite case.

This matter is discussed with regard to lending and school discipline outcomes in my December 2012 Amstat News column “[Misunderstanding of Statistics Leads to Misguided Law Enforcement Policies](#),” *Amstat News* (Dec. 2012). It explains fairly succinctly why general reducing an outcome like rejection of a mortgage application, while tending to reduce relative differences in mortgage approval rates, tends to increase relative differences in mortgage rejection rates. The data referenced in the article regarding the effects of reducing income and credit score requirements on relative differences in meeting and failing to meet the requirements

may be found in Table 1 of the 2006 Chance editorial and Table 1 of my [Credit Score Illustrations](#) (with graphical illustration of the latter in Figure 1 (at 4) of the letter to the [Federal Reserve Board](#) (March 4, 2013)

Like explanations with regard to mortgage rejection issues date back to 1992, including "[Bias Data Can Make the Good Look Bad](#)," *American Banker* (Apr. 27, 1992). That item also explains, not simply why relative differences in mortgage rejection rates would be expected to be greater (though relative differences in mortgage approval rates would be expected to smaller) among higher-income than lower-income, but that, in point of fact, they were.⁸ I have most recently explained this issue in my July 25, 2016 letter to ASA leadership, and in "[Misunderstanding of Statistics Confounds Analyses of Criminal Justice Issues in Baltimore and Voter ID Issues in Texas and North Carolina](#)," Federalist Society Blog (Oct. 3, 2016) and "[Things the President Doesn't Know About Racial Disparities](#)," Federalist Society Blog (Aug. 5, 2016).

One should think that in a technologically advanced society, every person involved in the enforcement of civil rights laws and every person analyzing demographic differences, leave aside the compliance officers of lending institutions and school districts, would fully understand that generally reducing adverse outcome tends to increase (a) relative differences in rates of experiencing them and (b) the proportion disadvantaged groups make up of persons experiencing them (just as such persons would be expected to fully understand that lowering test cutoff tends to increase relative differences in failure rates). In fact, however, very few such people seem to know this and a great many, including every government agency but the National Center for Health Statistics, seem to believe the opposite. Notwithstanding the long involvement of the Departments of Justice and Education with issues concerning racial differences in testing outcomes it is doubtful that, in any institutional sense, either agency even understands that lowering a test cutoff tends to increase relative differences in failure rates.

In any case, government civil rights law enforcement policies are invariably based on beliefs about the effects of lowering standards and otherwise reducing adverse outcome rates on (a) relative differences in rates of experiencing those outcomes and (b) the proportions disadvantaged groups make up of persons experiencing those outcomes that are the opposite of reality.

Addressing this subject should be a high priority of the Commission and the committee mentioned in Section 1.

6. Analyses of discrimination issues, whether involving binary or continuous variables, cannot be based on data pertaining solely to persons who accepted some outcome or situation.

This matter was given only minor attention of in my October 2015 ASA letter (at 30).

⁸ See "Race and Mortality Revisited" (at 340-341) regarding the way observers have drawn inferences both on the basis of the larger relative difference in mortgage rejection rates and the smaller relative differences in mortgage approval rates among higher-income than lower-income applicants. That matter, however, is more germane to Section 1.

Almost all discrimination cases resulting in awards approaching or exceeding \$100 million (including settlement awards) have involved claims regarding discrimination against persons who accepted some outcome or situation. The essence of the allegations in such cases is that the persons received or ended up in a position or situation inferior to the one they would have received or ended up in absent discrimination. Such cases include the cases against Countrywide Financial Corporation (\$335 million) and Wells Fargo Bank (\$175 million), discussed in the December 2012 Amstat News column, which involved the alleged discriminatory assignment of loans to subprime rather than prime status and some other loan cost issues.

Claims like this are invariably based on analyses of data solely on persons who accepted some outcome or position and fail to consider the parts of the universe that were offered no situation or refused to accept the offered position or outcome (either by specifically rejecting an offer or by leaving a situation). I first addressed this issue with regard to so-called assignment discrimination claims in "[Illusions of Job Segregation](#)" Public Interest (Fall 1988). That was before such cases started to become extremely successful (and before the Supreme Court seemed to say that such cases were meritless). See "[Multimillion-Dollar Settlements May Cause Employers to Avoid Hiring Women and Minorities for Less Desirable Jobs to Improve the Statistical Picture](#)," *National Law Journal* (Mar. 27, 1995).

The same problems exist with regard to discriminatory lending claims (including those involving loan costs) and employment claims regarding pay differences. See "[The Perverse Enforcement of Fair Lending Laws](#)," *Mortgage Banking* (May 2014) (at 93), the Kansas Law paper (Section F, at 32-35), and the TDHCA brief (Section I.C, at 37-30).⁹

⁹ In order that no reader misinterprets the references to accepting a position or situation, I note that the unsoundness of these claims does not involve anyone's forfeiting a claim as a result of accepting a position or situation. The problem lies simply in the failure of the analyses to examine the entire universe at issue.