Comments of James P. Scanlan Regarding Selection of Monitor for Consent Decree
in *United States v. Police Department of Baltimore City et al.*, No. 17-cv-00099
(June 26, 2017)

Notes added July 22, 2017:
(a) In the heading of the final column of Table 6 on page 22, "Black" has been changed to "Male."
(b) The type of information regarding suspensions of preschool students in Tables 5 and 6 may be found for suspensions of K-12 students in Tables 2 and 3 of my July 17, 2017 letter to the Departments of Education, Health and Human Services, and Justice. That letter urges the agencies to explain to the public and school administrations that the agencies' prior guidance to the effect that lowering standards and otherwise reducing the frequency of adverse public school discipline standards tends to increase relative demographic differences in adverse discipline and the proportion more susceptible students make up of persons who experiences those outcome was not correct, and that reducing the frequency of adverse discipline outcomes tends to increase those measures. This is a matter addressed fairly succinctly in the "The Paradox of Lowering Standards," Baltimore Sun (Aug. 5, 2013).
(c) I discuss these comments in "The Government's Uncertain Path to Numeracy," Federalist Society Blog (July 21, 2017).
(d) These comments discuss that it is unlikely that the statistical experts identified in the Monitor proposals are aware that the central premise of the decree regarding the effects of reducing adverse outcomes on measures the Department of Justice commonly employs to quantify demographic differences is the opposite of reality. They recommend that the parties defer action on the proposals until they have explained this issue to the candidates. The parties did not follow the recommendation and proceeded to select, on July 21, 2017, six candidates to be interviewed. Those candidates are CNA Consulting, DLA Piper, Exiger, Powers Consulting Group, Susan Burke, Venable LLP.

These comments are submitted pursuant to the June 15, 2017 Amended Notice Regarding Comment on Monitor Selection in *United States v. Police Department of Baltimore City and Mayor and City Council of Baltimore*, No. 17-cv-00099.

Introduction

In earlier comments[1] regarding this case, I explained that a central premise of the proposed Consent Decree was the opposite of reality. I also urged the court to postpone action on the matter until the parties fully understood how the premise was mistaken. The Consent Decree was nevertheless entered without any party's addressing the mistaken premise. The purpose of the instant comments is to explain the pertinence of points made in the earlier comments to the selection of a Monitor and the activities of the Monitor and to suggest that the Monitor candidates be requested to provide supplemental submissions indicating whether they are capable of analyzing demographic differences in a statistically sound manner.

As explained in the earlier comments, consistent with a great many things the Department of Justice (DOJ) and other government agencies have done in recent decades, the Consent Decree is premised on the belief that relaxing standards or otherwise reducing the frequency of adverse outcomes will tend to reduce (a) relative (percentage) racial and other demographic differences in rates of experiencing those outcomes and (b) the proportions African Americans and other groups more susceptible to the outcomes make up of persons experiencing the outcomes.

In fact, relaxing standards or otherwise reducing the frequency of adverse outcomes tends to increase, not reduce, both (a) and (b). That is, restricting any adverse outcome to persons most susceptible to it, while tending to reduce relative differences in rates of experiencing the corresponding favorable outcome, tends to increase relative differences in the adverse outcome itself.[2] Similarly, such restricting of an adverse outcome, while tending to increase the proportions groups most susceptible to the outcome make up of persons experiencing the corresponding favorable outcome (hence, *reducing* all measures of difference between the proportions such groups make up of the relevant pool and the proportions they make up of persons experiencing the favorable outcome), also tends to increase the proportions the most susceptible groups make up of persons experiencing the adverse outcome itself (hence, *increasing* all measures of difference between the proportions such groups make up of the relevant pool and the proportion they make up of persons experiencing the adverse outcome).

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[1] To facilitate consideration of issues raised in documents such as this I include links to referenced materials in electronic copies of the documents, in some cases, for the reader's convenience, providing the links more than once. Such copies are available by means of the Measurement Letters page of jpscanlan.com. The online version of the document may be corrected or annotated, in which case such fact will be noted on the first page.

[2] I cast the matter in terms of restricting the *adverse* outcome to those most susceptible to it in order to make the matter easier to understand. But the pertinent statistical pattern arises from restricting any outcome (whether adverse or favorable) to persons most susceptible to it. Doing so tends to reduce relative differences between rates of experiencing the corresponding opposite outcome and increase relative differences between rates of experiencing the outcome itself.

I have presented the key statistical points fairly succinctly in "Things DoJ doesn't know about racial disparities in Ferguson," *The Hill* (Feb. 22, 2016), "Misunderstanding of Statistics Leads to Misguided Law Enforcement Policies," *Amstat News* (Dec. 2012), and "The Paradox of Lowering Standards," *Baltimore Sun* (Aug. 5, 2013). I have explained them more fully, with a focus on the instant Consent Decree, in "Compliance Nightmare Looms for Baltimore Police Department," Federalist Society Blog (Feb. 8, 2017). See also discussion of DOJ actions regarding Baltimore police practices 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), "Will Trump Have the First Numerate Administration?" Federalist Society Blog (Jan. 4, 2017), and "Racial Impact Statement Laws in New Jersey and Elsewhere," Federalist Society Blog (Mar. 20, 2017). The February 8 and March 20, 2017 Federalist Society Blog posts both discuss that the process of finalizing the Baltimore Consent Decree, including the selection of a Monitor, could provide an opportunity for the DOJ to educate itself regarding a matter where its civil rights enforcement policies have long been based on a fundamental misunderstanding of statistics.

The pertinent patterns are also discussed at length in my comments for the Commission on Evidence-Based Policymaking (Nov. 14, 2016) (at 2-3, 24-29), while referencing DOJ actions in Baltimore, Maryland, and Ferguson, Missouri. The fifth through seventh recommendations to the Commission (at 46) are aimed at law enforcement and other government activities based on the mistaken understanding of the effects of reducing the frequency of adverse outcomes on measures of demographic difference regarding the outcomes.

A recent attempt to explain this matter to DOJ may be found in my April 13, 2017 letter to Attorney General Jeff Sessions and Acting Assistant Attorney General T. E. Wheeler, III (Sessions letter). My earlier attempts to explain this matter to the DOJ may be found in letters to the agency dated May 9, 2015, and April 23, 2012. I have also addressed this matter in emails to DOJ attorneys handling this case, among other things, stressing their obligation to explain to the court that a central premise of the Consent Decree is incorrect. But, as discussed in the Sessions letter, there are many areas where DOJ has led courts, policy makers, and the public to believe that actions will tend to reduce certain measures of racial and other disparities when in fact the actions will tend to increase those measures, and where the agency has an obligation to correct the misunderstandings it has caused or contributed to. For example, DOJ, in conjunction with other agencies, has led lenders and public schools to mistakenly believe that relaxing lending and school discipline standards will tend to reduce relative racial/ethnic differences in adverse borrower outcomes and adverse school discipline outcomes. DOJ has an obligation to inform such entities that the agency's views of such matters were mistaken.

Such obligations are among the reasons that DOJ should fully avail itself of the opportunity afforded by the Consent Decree Monitor selection process to educate itself on a matter that it has long misunderstood. Once DOJ fully understands the matter, it should immediately recognize an obligation to correct the misunderstandings its actions in this case have caused for the court, the defendants, and the citizens of Baltimore.

I illustrate the pertinent statistical principles with six tables below. Table 1 though 3, which are also Tables 1 through 3 of the Sessions letter, show that lowering test cutoffs or

income or credit score requirements will tend to increase relative differences in rates of failing the test and failing to meet the income or credit score requirements (while also showing, expressly or impliedly, that lowering the cutoff or requirements will tend to increase the proportion disadvantaged groups make up of persons failing to reach the test cutoff or failing to meet the income and credit score requirements). Table 4 shows how a dramatic reduction in searches by the U.S. Customs Service between 1998 and 2000 was accompanied by a dramatic increase in the proportion blacks made up of persons searched. Tables 5 and 6 show how modifying preschool discipline policies such as to give all students a reprimand rather than what would otherwise be their first suspensions would increase the proportion black children and male children make up of suspended preschool students.[3]

These comments principally focus on two matters. One involves ensuring that the selected Monitor understands that, contrary to the premise of the decree, relaxing standards or otherwise reducing the frequency of adverse criminal justice outcomes would tend to increase, not decrease, relative racial differences in such outcomes and the proportion African Americans and other disadvantaged groups make up of persons experiencing the outcomes, as well as ensuring that the Monitor also understands certain related issues concerning the measurement of demographic differences. A second matter involves ensuring fairness and efficiency in the selection of a Monitor, given that currently the statistical experts identified in the Monitor proposals, like most persons who analyze demographic differences, are likely to share the misunderstandings reflected in the aforementioned premise of the decree and are likely to fail to understand important related issues.

In particular regarding the latter matter, consistent with the beliefs long explicitly or impliedly reflected in government policies and largely unquestioned by the statistical and social science research communities, the statistical experts identified in the Monitor proposals probably all believe that relaxing a standard or otherwise reducing the frequency of an adverse outcome will tend to reduce relative differences in rates of experiencing the adverse outcome and the proportions groups most susceptible to the outcome make up of persons experiencing it. As discussed, and as will be illustrated below, the opposite is the case.

In the criminal justice context, to use an example based on a DOJ misunderstanding discussed in February 22, 2016 *Hill* item mentioned above and the Sessions letter (at 11), the experts identified in the Monitor proposals probably all believe that increasing the number of missed court appearances necessary to trigger issuance of an arrest warrant would tend to reduce the proportions groups with higher rates of missed court appearances make up of persons against whom warrants are issued for missed court appearances. To use an example implied in the February and March 2017 Federalist Society Blog posts mentioned above, the experts probably all believe that increasing the threshold for use of force by police would tend to reduce the proportions African Americans make up of persons against whom force is used.[4] Similarly, to

[3] Tables 1 though 3 also show the effects on the measures of difference as to the opposite (favorable) outcomes, while Table 4 though 6 do not.

[4] I often cast the matter in terms "relaxing a standard or otherwise reducing the frequency of an adverse outcome" because of a focus since 2012 on perceptions about relative differences in adverse borrower or adverse school discipline outcomes. In the case of police use of force the matter might be cast in terms of raising the standard for

use an example based on Table 4 below, the experts probably all believe that generally restricting searches in Baltimore would tend to reduce the proportions African Americans make up of persons searched. The experts probably also believe that the implementation of the alternatives to arrest and incarceration discussed in Paragraphs 218 and 219 of the Consent Decree would tend to reduce the proportions African Americans make up of young people arrested or incarcerated. Again, such actions are more likely to increase those proportions.

Further, Paragraph 423 of the Consent Decree requires that the Baltimore Police Department, with the aid of the Monitor, conduct an in-depth review of police hiring procedures to identify practices with a disparate impact on any demographic category and implement less discriminatory alternatives to such practices. As explained in the earlier comments in this case and the Sessions letter, while relaxing hiring standards will tend to reduce relative differences in rates of meeting the standards, it will tend to increase relative differences in failing to meet the standards. Thus, one will commonly reach opposite conclusions as to whether a modification to a practice has increased or decreased a disparate impact depending on which outcome one examines. Probably, however, all or most of the experts identified in the Monitor proposals are unaware that it is even possible for the relative difference in the favorable outcome and the relative difference in the corresponding adverse outcome to change in opposite directions as a standard is raised or lowered, much less that this will tend to occur systematically.

Effectively analyzing the data on demographic differences will be a crucial Monitor function. Currently, it is probable than none of the Monitor candidates' statistical experts has the knowledge and understanding to perform such function, though all may have the expertise to deal with pertinent issues once they have considered the points made in these comments and the Sessions letter and materials it references.[5] It would seem neither fair nor efficient to select candidates for interview from among entities that currently cannot perform a key monitoring function and then to educate the selected candidates or the ultimate selectee on performance of that function. It would be both fairer and more efficient for the Parties to request that the Monitor candidates provide supplemental submissions addressing the measurement issues discussed here and in referenced materials and explaining how they would deal with those issues. Then, after review of the supplemental submissions (and public comment thereon), the Parties

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the use of force. Raising the standard for imposition of the adverse outcome is the same thing as lowering the standard for experiencing the corresponding favorable outcome.

[5] It is, of course, possible that some of the experts in fact have an understanding of these issues since the patterns I describe are implied in many things most persons with statistical training do understand. Further, I have previously contacted at least two of the experts identified in Monitor proposals regarding measurement issues related to the issues addressed here. But, as discussed in my "Race and Mortality Revisited," Society (July/Aug. 2014), "The Mismeasure of Health Disparities," Journal of Public Health Management and Practice (July/Aug. 2016), Comments for the Commission on Evidence-Based Policymaking (Nov. 14, 2016), and Comments for the Commission on Evidence-Based Policymaking (Nov. 28, 2016), understandings of the ways measures tend to be affected by the frequency of an outcome is extremely rare even among persons whose principal activities involve the analyses of data on group differences. If any experts identified in the Monitor proposals have already demonstrated an understanding of the issues discussed here (including those in the concluding paragraphs), such fact should be an important consideration favoring the particular proposals.

can determine which applicants to interview from among candidates that appear capable of effectively analyzing demographic differences regarding the matters addressed in the Consent Decree.

There are substantial benefits of proceeding in this manner. First, the supplemental submissions would greatly aid the Parties in determining whether Monitor candidates are capable of addressing all relevant the statistical issues.

Second, given that the Consent Decree is premised on the mistaken understanding discussed above, Monitor candidates whose statistical experts now understand the pertinent issues (or whose statistical experts come to understand them as a result of review of this document or materials it references) may be reluctant to address those issues with the Parties out of concern that doing so will reduce the candidates' chances of selection. The request for supplemental submissions should materially assuage that concern, especially if, in requesting the supplemental submissions, the Parties acknowledge that the points made here are essentially correct.

Third, the request for supplemental submission can serve as means of informing the citizens of Baltimore of the reasons that the Consent Decree may not dramatically reduce the measures of disparity employed in the DOJ's underlying report and may in fact increase those measures. The Parties should, however, also take more formal and comprehensive actions to alert the citizens of Baltimore regarding mistaken impressions as to the consequences of the Consent Decree that DOJ actions here and elsewhere have promoted.

Fourth, once the candidates' statistical experts have been focused on the unsoundness of the statistical approaches long employed by the DOJ and other federal agencies, those experts, by means of the supplemental submissions and otherwise, may substantially assist DOJ in its own understanding of the pertinent statistical issues. That could be so with respect both to the Consent Decree itself and to the many other matters where the agency's failure of understanding has undermined its civil rights enforcement activities.

Description of pertinent statistical patterns

For reasons related to the shapes of underlying distributions of factors associated with experiencing an outcome or its opposite, all standard measures of differences between outcome rates (*i.e.*, the proportions of demographic groups experiencing a binary outcome) tend to be affected by the frequency of an outcome. The pattern most pertinent here is that 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 (*i.e.*, experiencing the opposite outcome). A corollary to this pattern is a pattern whereby the rarer an outcome, the greater tend to be the proportions groups most susceptible to the outcome make up of both persons who experience the outcome and persons who avoid the outcome.

The patterns can be easily illustrated with normally distributed test score data. Table 1 below shows the pass and fail rates of an advantaged group (AG) and a disadvantaged group (DG) at two cutoff points in a situation where the groups have normally distributed test scores

with means that differ by half a standard deviation (a situation where approximately 31 percent of DG's scores are above the AG mean) and both distributions have the same standard deviation. The table also shows (in columns 5 through 8) measures that might be used to appraise differences in test outcomes of AG and DG.

Column 5, which presents the ratio of AG's pass rate to DG's pass rate,[6] shows that at the higher cutoff, where pass rates are 80 percent for AG and 63 percent for DG, AG's pass rate is 1.27 times (27 percent greater than) DG's pass rate. If the cutoff is lowered to the point where AG's pass rate is 95 percent, DG's pass rate would be about 87 percent. At the lower cutoff, AG's pass rate is only 1.09 times (9 percent greater than) DG's pass rate.

Table 1. Illustration of effects of lowering a test cutoff on measures of differences in test outcomes

Row	(1) AG Pass Rate	(2) DG Pass Rate	(3) AG Fail Rate	(4) DG Fail Rate	(5) AG/DG Pass Ratio	(6) DG/AG Fail Ratio	(7) DG Prop of Pass	(8) DG Prop of Fail
1	80%	63%	20%	37%	1.27	1.85	44%	65%
2	95%	87%	5%	13%	1.09	2.60	48%	72%

That lowering a cutoff tends to reduce relative differences in pass rates is well understood and underlies the widespread view that lowering a cutoff tends to reduce the disparate impact of tests on which some groups outperform others.

But, whereas lowering a cutoff tends to reduce relative differences in pass rates, it tends to increase relative differences in failure rates. As shown in column 6, initially DG's failure rate was 1.85 times (85 percent greater than) AG's failure rate. With the lower cutoff, DG's failure rate is 2.6 times (160 percent greater than) AG's failure rate.

Columns 7 and 8 show the proportions DG makes up of persons who pass and fail the test at each cutoff in a situation where DG makes up 50 percent of persons taking the test. Column 7 shows that lowering the cutoff increases the proportion DG makes up of persons who pass from 44 percent to 48 percent (hence, *reducing* all measures of difference between the proportions DG makes up of persons who took the test and persons who passed the test). Column 8 shows that lowering the cutoff increases the proportion DG makes up persons who fail the test from 65

[6] While I commonly refer to patterns of relative differences in this letter, the table actually presents rate ratios (also termed risk ratios or relative risks). The relative difference is the rate ratio minus 1 where the rate ratio is above 1 and 1 minus the rate ratio where the rate ratio is below one. In the former case, the larger the rate ratio, the larger the relative difference; in the latter case, the smaller the rate ratio, the larger the relative difference. It is more common to employ the disadvantaged group's rate as the numerator for the favorable as well as the adverse outcome, which is the approach as to favorable outcomes of the "four-fifths" or "80 percent" rule for identifying disparate impact under the Uniform Guideline for Employee Selection Procedures. I have sometimes employed this approach, as in "Can We Actually Measure Health Disparities?," *Chance* (Spring 2006). More recently, however, I have usually used the larger figure as the numerator for both rate ratios, in which case, as to both favorable and adverse outcomes, the larger the ratio, the larger the relative difference. Choice of numerator in the rate ratio, however, has no bearing on the patterns by which as the frequency of an outcome changes, the two relative differences tend to change in opposite directions.

percent to 72 percent (hence, *increasing* all measures of difference between the proportions DG makes up of persons who took the test and persons who failed the test).

Inasmuch as the pattern by which the proportions more susceptible groups make up of persons experiencing and avoiding an outcome tend to be affected by the frequency of an outcome is a corollary to the pattern by which the two relative differences tend to be affected by the frequency of the outcome, in the discussion that follows I limit discussion of those proportions to situations where that matter is pertinent.

The aforementioned pattern by which the two relative differences tend to be affected by the frequency of an outcome is not peculiar to test score data or the numbers I chose to illustrate it. Rather, it can be found in virtually any setting where two groups have different, more or less normal, distributions of factors associated with experiencing some outcome. Income and credit score date, for example, show how lowering an income or credit score requirement, while tending to reduce relative racial differences in meeting the requirement, will tend to increase relative racial differences in failing to meet the requirement.

Such pattern is illustrated in Tables 2 and 3 below, which are abbreviated versions of Tables 1 and 2 of the Income and Credit Score Examples subpage of the Lending Disparities page of jpscanlan.com, which also explains the origins of the data. Tables 2 and 3 follow the format of Table 1 above (without the last two columns), while presenting, in place of the AG and DG pass and fail rates, the white and black rates of falling above and below various income levels or credit scores. Movement down the five rows of the tables illustrates the effects of lowering the income or credit score requirements on the two relative differences, revealing the patterns just described. That is, the lower the requirement, and thus the greater the overall rates of meeting the requirement and the smaller the overall rates of failing to meet the requirement, the smaller is the relative difference in meeting the requirement (column 5) and the larger is the relative difference in failing to meet the requirement (column 6). One will observe the same pattern for all 16 rows of Table 1 and all 14 rows of Table 2 on the referenced webpage.

Table 2. Illustration of effects of lowering an income requirement on relative differences in meeting the requirement and relative differences in failing to meet the requirement

Income	(1) Perc of Wh Abv	(2) Perc of Bl Abv	(3) Perc of Wh Bel	(4) Perc of Bl Bel	(5) Wh/Bl Abv Ratio	(6) Bl/Wh Bel Ratio
$100,000	27.0%	12.1%	73.0%	87.9%	2.23	1.20
$85,000	34.6%	17.3%	65.4%	82.7%	2.00	1.26
$75,000	41.1%	22.7%	58.9%	77.3%	1.81	1.31
$60,000	52.5%	31.3%	47.5%	68.7%	1.68	1.45
$50,000	61.0%	39.2%	39.0%	60.8%	1.56	1.56

Table 3. Illustration of effects of lowering a credit score requirement on relative differences in meeting the requirement and relative differences in failing to meet the requirement

Score	(1) Perc of Wh Abv	(2) Perc of Bl Abv	(3) Perc of Wh Bel	(4) Perc of Bl Bel	(5) W/B Abv Ratio	(6) B/W Bel Ratio
740	46.80%	19.50%	53.20%	80.50%	2.40	1.51
720	57.77%	27.01%	42.23%	72.99%	2.14	1.73
700	67.83%	35.67%	32.17%	64.33%	1.90	2.00
680	76.73%	45.42%	23.27%	54.58%	1.69	2.35
660	83.90%	55.70%	16.10%	44.30%	1.51	2.75

Table 4 is based on data from June 28, 2001 *Washington Post* article titled "New Policies Aim to Discourage Racial Profiling." The article discussed that between 1998 and 2000 the U.S. Customs Service implemented a number of reforms aimed at reducing racial disproportionality in searches by generally restricting the use of searches. Reforms included requiring supervisory approval for intrusive searches. The article discussed the program, which dramatically reduced the number of searches, as one that in fact was reducing racial disproportionality. Data in the article, however, showed that the proportion blacks made up of persons searched increased from 14.1% to 27.1%.

Table 4. Change in black proportion of persons searched by U.S. Customs Service between 1998 and 2000 (a period during which the Service enacted reforms restricting the use of searches, including by, inter alia, requiring supervisory approval for intrusive searches)

Year	Total Searches	Black Searches	Black Proportion of Persons Searched
1998	43,606	6,141	14.1%
2000	9,020	2,441	27.1%

The information in Tables 5 and 6 is drawn from a March 2014 Department of Education publication titled "Data Snapshot: School Discipline." The document provided information on the proportions demographic groups made up of preschool students suspended one time and suspended multiple times. From the information provided, one can then determine the proportions the groups made up of persons suspended one or more times and more than one time, which information is presented for black students in the two rows of the table.[7]

[7] Demographic differences in rates of experiencing things like single suspensions cannot be effectively analyzed, just as differences in rates of receiving grades of C or experiencing fair health cannot be effectively analyzed. See the Intermediate Outcomes subpage the Scanlan's Rule page of jpscanlan.com. It is possible the Department of Education has come to appreciate aspects of this issue. In the agency's 2016 publication on school discipline titled "2013-2014 Civil Rights Data Collection – A First Look," the agency included single suspensions within the category of "one or more suspensions."

Table 5 shows that if the standards were relaxed such that all persons suspended one or more times were given a reprimand instead of their first suspension, the proportion black children make up of suspended students would increase from 44% to 48%.

Table 5. Illustration of effect of giving all persons a reprimand instead of their first suspension on proportion black preschool students make up of persons experiencing one or more suspensions

Outcome	Black Proportion of Students Experiencing the Outcome
One or more suspensions	44%
Two or more suspensions	48%

Table 6 shows that if the standards were relaxed such that all persons suspended one or more times were given a reprimand instead of their first suspension, the proportion boys make up of suspended students would increase from 80 percent to 82 percent.

Table 6. Illustration of effect of giving all persons a reprimand instead of their first suspension on proportion male preschool students make up of persons experiencing one or more suspensions

Outcome	Male Proportion of Students Experiencing the Outcome
One or more suspensions	80%
Two or more suspensions	82%

If standards were further relaxed such that all persons were given reprimands for what would otherwise be their first two suspensions, the 44 percent and 82 percent figures would almost certainly rise still further. Rarely will one fail to observe such a pattern.

In the school discipline context, in point of fact, one observes that all across the country recent reductions in discipline rates have been accompanied by increased relative racial/ethnic differences in discipline rates. See the following web pages discussing such patterns with respect to the jurisdictions indicated in the page titles: California Disparities, Colorado Disparities, Connecticut Disparities, Florida Disparities, Maryland Disparities, Minnesota Disparities, Oregon Disparities, Rhode Island Disparities, Utah Disparities, Beaverton, OR Disparities, Denver Disparities, Henrico County, VA Disparities, Los Angeles SWPBS, Minneapolis Disparities, Montgomery County, MD Disparities, Portland, OR Disparities, St. Paul Disparities, South Bend Disparities. These patterns are occurring notwithstanding that school districts may well be doing many things beyond relaxing standards in attempting to reduce racial/ethnic differences in discipline rates.

In sum, as explained more fully in the Sessions letter and the materials it references, the patterns described above are hardly debatable. They will, of course, not be observed in every situation, since other factors also play a role. Given the way in which certain adverse outcomes disproportionality occur in Baltimore neighborhoods where African Americans comprise a very

high proportion of residents, it is hard to predict effects on overall measures of difference of general reductions in aggressiveness of enforcement (or police presence) in particular neighborhoods. A similar issue exists with respect to changes in approaches to different types of crimes. And to the extent that any observed differences in outcome rates are functions of biased policing, and aspects of the Consent Decree reduce that bias, all measures of racial difference should decrease.

But it is not possible for an entity to effectively monitor a decree like that entered in this case or any like matter without understanding the patterns, much less while entertaining beliefs about the effects of reducing adverse outcomes on measures of disparity that are the opposite of those reflected in the tables above.

The experts identified in the Monitor proposals probably also fail to understand issues concerning the analyses of demographic differences that go beyond the subject discussed above, though the issues are implicit in that discussion. For, example, as discussed in the Sessions letter, in a situation where the pass and fail rates of Table 1 are the favorable and adverse outcome rates resulting from subjective judgments of decision-makers, there is no rational basis for maintaining that one row of the table reflects a greater likelihood of biased decision-making than the other. As also discussed in the Sessions letter, in a situation where the pass and fails rates of Table 1 are the rates of failing to use force and using force of officers instructed to limit the use of force to extreme situations, other things being equal, the more officers attempt to follow those instructions the more their results will tend to look like those in the second row than those in the first row. It is unlikely that the identified experts currently understand such things.

Further, the above-discussed failures of understanding are but part of a larger failure of persons analyzing demographic differences to recognize patterns by which commonly employed measures tend to be affected by the frequency of an outcome and to consider those patterns in endeavoring to understand underlying processes. It is thus essential that the Monitor's statistical experts understand the range of issues discussed in "The Mismeasure of Discrimination," Faculty Workshop, University of Kansas School of Law (Sept. 20, 2013), "Race and Mortality Revisited," Society (July/Aug. 2014), and most parts of the Comments for the Commission on Evidence-Based Policymaking (Nov. 14, 2016). It is also essential that they understand the problematic issues discussed in the Addendum to the Ferguson, Missouri Arrest Disparities subpage of the Discipline Disparities page of jpscanlan.com.

Therefore, the Parties should request the Monitor candidates to address all such issues in their supplemental submissions.