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**IN THE**

**Supreme Court of the United States**

**TEXAS DEPARTMENT OF HOUSING AND  
COMMUNITY AFFAIRS, ET AL.,**

*Petitioners,*

**v.**

**THE INCLUSIVE COMMUNITIES PROJECT, INC.,**

*Respondent.*

**On Writ of Certiorari to the  
United States Court of Appeals  
for the Fifth Circuit**

**BRIEF OF JAMES P. SCANLAN  
AS AMICUS CURIAE  
IN SUPPORT OF PETITIONERS**

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## **QUESTION PRESENTED**

**Whether disparate-impact claims are cognizable under the Fair Housing Act.**



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## **STATEMENT OF INTEREST OF AMICUS CURIAE<sup>1</sup>**

I am an attorney in Washington, D.C. who has dealt extensively with statistical analyses of discrimination issues. Since 1987, I have created a large body of work aimed at substantially reforming the analysis of group differences in the law and the social and medical sciences. Much of that work is devoted to explaining implications of the fact that reducing any outcome, whether the outcome be mortality, poverty, or rejection of a mortgage application, will tend to increase relative (percentage) differences between rates at which advantaged and disadvantaged groups experience the outcome, while reducing relative differences between their rates of experiencing the corresponding opposite outcome. The work is summarized in my recent article *Race and Mortality Revisited*, 51 *Society* 328 (July/Aug. 2014). That article also discusses the failure of agencies enforcing fair lending laws to recognize that relaxing loan standards, while tending to reduce relative differences in loan approval rates, will tend to increase the relative differences in loan denial rates on which enforcement agencies commonly focus.

To my knowledge, all scholarly articles recognizing that relative differences in a favorable outcome and relative differences in the corresponding adverse outcome tend to change in opposite directions as the frequency of an outcome changes have been responses

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<sup>1</sup> No counsel for a party authored any part of this brief and no counsel or party made a monetary contribution intended to fund the preparation or submission of the brief. Pursuant to this Court's Rule 37.3(a), all parties have consented to the filing of this brief. Letters evidencing such consent have been filed with the Clerk of the Court.

to my work. This includes the recognition by the National Center for Health Statistics that determinations of whether health and healthcare disparities have increased or decreased will commonly turn on whether one examines relative differences in favorable outcomes or relative differences in the corresponding adverse outcomes.

Consistent with my efforts to improve the analysis of demographic differences in every setting, I have a strong interest in ensuring that the Court's treatment of this case is informed by a sound understanding of pertinent statistical issues.

## **INTRODUCTION AND SUMMARY OF ARGUMENT**

Part I of the Argument section to this brief provides the statistical background to the legal issues addressed in Part II. Part I explains that virtually all efforts to appraise the strength of the forces causing favorable or adverse outcome rates of advantaged and disadvantaged groups to differ have been undermined by a failure to recognize the ways that standard measures of differences between outcome rates tend to be affected by the frequency of an outcome and by certain other failures of understanding concerning the measurement of group differences. Part I also describes the particular failure of understanding of federal regulators in applying the disparate impact doctrine to the Fair Housing Act (FHA), where for more than two decades they have encouraged lenders to relax lending standards while mistakenly believing that relaxing lending standards will tend to reduce relative differences in adverse borrower outcomes. Part I also explains that even when one fully understands the pertinent statistical issues, appraising the size of a disparate impact, and determining whether

one practice has a less discriminatory effect than another, are matters of great difficulty and considerable uncertainty.

Part II argues that in light of statistical issues described in Part I, a disparate impact provision in the FHA, as articulated by the Department of Housing and Urban Development (HUD) in its discriminatory effects rule, without sound guidance as to how to measure an impact, is unconstitutionally vague. For it puts covered entities in circumstances where actions that reduce the perceived impact of its practices according to one standard measure of impact commonly will cause the perceived impact to increase according to another standard measure. It does so, moreover, in a context where none of the standard measures provides a rational basis for appraising that impact. Part II also argues that even if the provision is not unconstitutionally vague, in light of HUD's longstanding failure to understand the statistical issues implicated in disparate impact analysis, the rule should not receive the deference the Court usually accords an enforcing agency's interpretation of a statute.

## **ARGUMENT**

### **I. STANDARD STATISTICAL ANALYSES OF DISCRIMINATION ISSUES ARE UNSOUND**

This part explains the unsoundness of standard statistical analyses of discrimination issues, whether characterized as disparate impact or disparate treatment, as a result of a near universal failure to understand certain fundamental concepts. Section A explains that standard measure of differences

between outcome rates of advantaged and disadvantaged groups cannot quantify the strength of forces causing those rates to differ because each measure tends to be systematically affected by the frequency of an outcome. It also describes a measure of the strength of those forces that is unaffected by the frequency of an outcome. Section B explains the fundamental unsoundness of analyses of discrimination issues based on comparisons of the proportion a group comprises of persons potentially experiencing an outcome and the proportion it comprises of persons experiencing the outcome. Specifically, information on the referenced proportions does not enable one to determine the underlying outcome rates that are essential elements to a sound appraisal of the strength of the forces causing those rates to differ. Section C explains the fundamental unsoundness of analyses of discrimination issues that examine information solely on persons who accepted an outcome. Specifically, it is not possible to determine whether a process causes a difference in outcomes without information on all persons subject to the process.

In discussing these subjects, I do not usually draw distinctions between disparate impact and disparate treatment. The measurement issues pertaining to both subjects involve determining the strength of an association between group membership and likelihood of experiencing some favorable or adverse outcome. Issues as to the strength of that association, which I will commonly refer to here as the strength of the forces causing the outcome rates to differ, are essentially the same whether disparate treatment or disparate impact is alleged.

This part draws heavily on my following papers that contain illustrations of its more complex statistical

points: *Race and Mortality Revisited*, 51 *Society* 328 (July/Aug. 2014); *Measuring Health and Healthcare Disparities*, Proceedings of the Federal Committee on Statistical Methodology 2013 Research Conference (2014) (FCSM paper); *The Mismeasure of Discrimination*, Faculty Workshop, University of Kansas School of Law (Sept. 20, 2013) (Kansas Law paper); and *Can We Actually Measure Health Disparities?*, 19(2) *Chance* 47 (Spring 2006) (Chance editorial). Subsequent references to these materials will generally be abbreviated.<sup>2</sup>

### **A. The Unsoundness of Standard Measures of Differences Between Outcome Rates as Measures of Association**

In the law as well as the social and medical sciences efforts to appraise the difference in the circumstances of advantaged and disadvantaged groups on the basis of rates at which the groups experience favorable or adverse outcomes have been almost universally undermined by the failure to recognize patterns by which standard measures of differences between outcome rates tend to be affected by the frequency of an outcome. The pattern most pertinent to the application of the disparate impact doctrine to the FHA is that whereby the rarer an outcome the greater tends to be the relative (percentage) difference between rates of experiencing the outcome and the smaller tends to be the relative difference between rates of avoiding it. The scope of the failure to understand this pattern, and of the implications of the

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<sup>2</sup> Further with respect to references, since most referenced materials have been authored by me, citations to such works do not include author name (though complete information is provided in the table of authorities).

failure, is exemplified by the incongruous situation that has existed in fair lending enforcement for more than two decades.

Concerned about the size of relative racial/ethnic differences in adverse borrower outcomes like rejection of home mortgage loan applications, and believing that an important contributing factor to these differences was the greater difficulty minority loan applicants had in meeting standard lending criteria, federal agencies responsible for the enforcement of the FHA and other fair lending laws have, since at least 1994, encouraged lenders to relax lending standards and otherwise to reduce the frequency of adverse borrower outcomes.<sup>3</sup> Such actions, while tending to reduce relative differences between rates at which whites and minorities have their loans approved or experience other favorable borrower outcomes, tend to increase relative differences between the rates at which such groups experience the corresponding adverse outcomes. Unaware that reducing the frequency of adverse outcomes tends to increase relative differences between rates of experiencing those outcomes—indeed, believing just the opposite—regulators have generally monitored the fairness of lender practices on the basis of relative differences in adverse outcomes.<sup>4</sup>

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<sup>3</sup> See *Interagency Policy Statement on Fair Lending*, Department of Housing and Urban Development, *et al.* (1994). See generally Stan Liebowitz, *Anatomy of a Train Wreck: Causes of the Mortgage Meltdown*, The Independent Institute (2008).

<sup>4</sup> Federal guidelines refer to “[s]ubstantial disparities in approval/denial rates.” *Interagency Fair Lending Examination Procedures* 9, Office of the Comptroller of the Currency *et al.* (August 2009). But because relative differences in adverse outcome rates tend to be much larger than relative differences in favorable outcome rates, fair lending analyses have usually

Thus, by responding to regulator encouragements or pressures to reduce the frequency of adverse borrower outcomes, lenders increased the chances that the federal government would sue them for discrimination.

Subsection 1 explains the patterns by which standard measures of differences between outcome rates tend to be systematically affected by the frequency of an outcome and why the existence of those patterns undermines the measures for quantifying the strength of the forces causing outcome rates to differ. Subsection 2 explains a method for quantifying the strength of those forces that is unaffected by the frequency of an outcome. With the background provided by Subsections 1 and 2, Subsection 3 explains the difficulties of determining whether relaxing a standard increases or decreases the impact of a policy even when the pertinent statistical issues are fully understood.

### **1. Patterns by Which Standard Measures of Differences Between Outcome Rates Tend to be Affected by the Frequency of an Outcome and Implications of those Patterns with Respect to the Utility of the Measures for Quantifying a Disparate Impact**

There are four standard measures of differences between outcome rates used to quantify group

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focused on the former. See materials collected on the Lending Disparities page and subpages of [jpscanlan.com](http://jpscanlan.com). As discussed in Part II, however, that regulators might sometimes focus on favorable outcomes increases the compliance problems faced by covered entities.

differences in the law and the social and medical sciences: relative (percentage) differences between rates at which two groups experience some favorable outcome; relative differences between rates at which the groups experience the corresponding adverse outcome; absolute (percentage point) differences between favorable or adverse outcome rates; and odds ratios. None of these measures can effectively quantify the strength of the forces causing outcome rates to differ because each measure tends to be systematically affected by the frequency of an outcome.

#### **a. Relative Differences Between Favorable or Adverse Outcomes**

The pattern by which standard measures of differences between outcome rates tend to be affected by the frequency of an outcome that is most pertinent to the analysis of discrimination issues is that whereby the rarer an outcome, (a) the greater tends to be the relative difference in experiencing it and (b) the smaller tends to be the relative difference in avoiding it. The pattern can be illustrated with normally distributed test score data where an advantaged group (AG) and a disadvantaged group (DG) have different average scores.

Table 1, which replicates Table 1 of *Race and Mortality Revisited*, with certain columns reordered and an additional column added for the odds ratio, is based on a situation where the means test scores of AG and DG differ by half a standard deviation, a situation where approximately 31% of DG scores are above the mean for AG. The first four data columns show the pass and fail rates at two cutoffs set at points where pass rates are 80% and 95% for AG. At such points,

the pass rates for DG would be approximately 63% and 87%.

**Table 1. Pass and fail rates of an advantaged group (AG) and a disadvantaged group (DG) at two cutoff points, with four measures of difference**

| Cutoff | AG Pass | DG Pass | AG Fail | DG Fail | AG/DG Pass Ratio | DG/AG Fail Ratio | Percentage Point Diff | Odds Ratio |
|--------|---------|---------|---------|---------|------------------|------------------|-----------------------|------------|
| High   | 80%     | 63%     | 20%     | 37%     | 1.27             | 1.85             | 17                    | 2.35       |
| Low    | 95%     | 87%     | 5%      | 13%     | 1.09             | 2.60             | 8                     | 2.84       |

The fifth data column is the ratio of AG's pass rate to DG's pass rate, which is used here as the indicator of the relative difference. The relative difference is the rate ratio minus 1 where the rate ratio is greater than 1 (*i.e.*, with the larger rate as the numerator in the ratio), which is my preferred way of presenting it.<sup>5</sup> At the higher cutoff, the ratio of AG's pass rate to DG's pass rate is 1.27, while at the lower cutoff the ratio is 1.09. This means that lowering the cutoff caused the relative difference in pass rates to decline from 27% to 9%. The fact that lowering cutoffs tends to reduce relative differences in pass rates is widely known and underlies the common belief that reducing cutoffs tends to reduce the disparate impact of employment tests as well as the common notion that employers must justify the cutoffs of employment tests—that is, justify that they are as high as they are.

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<sup>5</sup> See *Race and Mortality Revisited* at 329 n.1 regarding nuances of characterization and choice of numerator issues that do not affect the key points here.

The sixth data column, however, shows that lowering the cutoff caused the relative difference in failure rates to increase. The ratio of DG's failure rate to AG's failure rate increased from 1.85 to 2.60. Thus, whereas DG's failure rate was 1.85 times (85% greater than) AG's failure rate with the higher cutoff, it is 2.6 times (160% greater than) AG's failure rate with the lower cutoff.

The final two columns present the absolute differences between the pass (or fail) rates and the odds ratios, showing that lowering the cutoff reduced the former while increasing the latter. But I defer discussion of those measures until the next subsection.

With respect to the relative difference, which tends to be the primary measure of disparate impact or discrimination generally, observers who measure a test's disparate impact in terms of relative differences in pass rates would find that lowering the test cutoff reduced the disparate impact. Observers who measure the disparate impact in terms of relative differences in failure rates would find that lowering the cutoff increased the disparate impact.

The pattern whereby changing the frequency of an outcome tends to reduce the relative difference in one outcome while increasing the relative difference in the opposite outcome is not peculiar to test score data or the numbers I used to illustrate it. The pattern is a function of the shapes of all but highly irregular risk distributions and can be illustrated with virtually any data that allow one to examine rates at which advantaged and disadvantaged groups fall above or below various points on a continuum of factors associated with experiencing an outcome. To take examples or particular pertinence to the effects of relaxing lending standards on racial differences in

borrower outcomes, income and credit score data show that lowering an income or credit score requirement in order to secure some desirable borrower outcome will tend to reduce relative differences in meeting the requirement but increase relative differences in failing to meet it.<sup>6</sup>

The same holds for other approaches that reduce one outcome while increasing the opposite outcome without altering the strength of the forces causing the two groups' outcome rates to differ. For example, a program for reviewing the situation of persons that a decision-making process tentatively identifies to experience the adverse outcome or any action that generally reduces adverse outcome rates—whether the adverse outcome be rejection of a loan application, assignment to subprime rather than prime loan status, or termination from employment for misconduct or poor job performance—will tend to increase relative differences in the adverse outcome while reducing relative differences in the favorable outcome. Reductions in interest rates, by increasing all borrowers' chances of securing loans, will tend to increase relative differences in loan rejection rates while reducing relative differences in loan approval rates.<sup>7</sup>

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<sup>6</sup> Graphical illustrations of these patterns across the full range of outcomes using, respectively, test score data, income data, and credit score data may be found in the FCSM paper, the Chance editorial, and the Credit Score Illustrations subpage of the Scanlan's Rule page of [jpscanlan.com](http://jpscanlan.com). Other illustrations may be found by means of the Collected Illustrations subpage of the Scanlan's Rule page.

<sup>7</sup> See my *Getting it Straight When Statistics Can Lie*, *Legal Times* (June 28, 1993), regarding misunderstandings in varied contexts of the statistical implications of efforts to ensure that the

Similarly, granting extensions for mortgage payments or increasing the number of late or missed payments triggering foreclosure, or any other action that generally reduces foreclosure rates, while tending to reduce relative differences between rates at which advantaged and disadvantaged groups keep their homes, will tend to increase relative differences in foreclosure rates.

Even though the pattern whereby the two relative differences tend to change in opposite directions as the frequency of an outcome changes is evident in so many kinds of data, it remains largely unknown. So far as the published record reveals, the only government agency to recognize the pattern is the National Center for Health Statistics (NCHS), which concluded a decade ago that determinations of whether health and healthcare disparities were increasing or decreasing would commonly turn on whether one examined relative differences in the

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maximum number of persons experience favorable outcomes. See also *When Statistics Lie*, Legal Times (Jan. 1, 1996), regarding the tendency for actions a complaint suggested the defendant should have taken to cause approval of loans of named plaintiffs, if evenhandedly applied, would have tended to increase the relative difference in loan denial rates on which the putative class action was based. Many other examples of misunderstandings in legal setting may be found in *Race and Mortality Revisited* and the Kansas Law paper, as well as my *An Issue of Numbers*, National Law Journal (Mar. 5, 1990); *The Perils of Provocative Statistics*, 102 Public Interest 3 (Winter 1991); *Mired in Numbers*, Legal Times (Oct. 21, 1996); *Race and Mortality*, 37 Society 29 (Jan/Feb 2000); *Misunderstanding of Statistics Leads to Misguided Law Enforcement Policies*, Amstat News (Dec. 2012); *The Paradox of Lowering Standards*, Baltimore Sun (Aug. 5, 2013). Articles discussing these issues with a particular focus on fair lending are collected on the Lending Disparities page of [jpscanlan.com](http://jpscanlan.com)

favorable health or healthcare outcome or relative differences in the corresponding adverse outcome.<sup>8</sup> No other government agency has shown a recognition that it is possible for the two relative differences to change in opposite directions as the frequency of an outcome changes, much less that they tend to do so systematically.

### **b. Absolute Differences and Odd Ratios**

Appraisals of the strength of the forces causing outcome rates to differ in terms of absolute differences between rates or odds ratios are unaffected by which outcome one examines. In the case of the hypothetical test scores in Table 1, the absolute difference between rates—17 percentage points at the initial cutoff and 8 percentage points with the lower cutoff—is the same regardless of whether one examines pass or failure rates. Similarly, regardless of which outcome is examined, the odds ratio is 2.35 or its reciprocal (0.43) with the initial cutoff and 2.84 or its reciprocal (0.35) with the lower cutoff.<sup>9</sup>

But for a measure to effectively quantify the strength of the forces causing a pair of outcome rates to differ, the measure must remain constant when there occurs a general change in the frequency of the outcome akin to that effected by altering a test cutoff. And, like the two relative differences, the absolute difference and the difference measured by the odds

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<sup>8</sup> See, e.g., Kenneth G. Keppel & Jeffrey Percy, *Measuring Relative Disparities in Terms of Adverse Events*, 11 J Public Health Manag Pract 479 (2005). See *Race and Mortality Revisited* at 331-34 regarding the problematic nature of NCHS's response to this recognition.

<sup>9</sup> For discussion of some nuance of the odds ratio, see the Kansas Law paper at 8 n.5.

ratio tend to change systematically as the frequency of an outcome changes. They do so, however, in a more complicated way than the two relative differences.

Roughly, as uncommon outcomes become more common, absolute differences between rates tend to increase; as common outcomes become even more common, absolute differences tend to decrease. In general, as the prevalence of an outcome changes, the absolute difference tends to change in the same direction as the smaller relative difference.<sup>10</sup> As the frequency 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 the varying correlations of each of the four measures with the frequency of an outcome may be found in the references in note 6 *supra*.

### **c. The Four Measures Examined Together**

Table 2 illustrates the patterns described above in the context of hypothetical selection processes where one would draw varying conclusions about the comparative strength of the forces causing outcome rates of the advantaged (putatively favored) and disadvantaged (putatively disfavored) groups in four settings. The table, which is based on Table 5 of *Race and Mortality Revisited*, presents the favorable outcome rates for AG and DG (leaving the adverse outcome rates to be inferred), along with the four measures of differences between rates shown in Table 1. Like Table 1, the hypothetical is based on a

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<sup>10</sup> See the introductory section of the Scanlan's Rule page of [jpscanlan.com](http://jpscanlan.com) regarding nuances of the patterns by which absolute differences tend to be affected by the frequency of an outcome.

situation where mean scores of AG and DG differ by half a standard deviation.

**Table 2. Hypothetical favorable outcome rates of advantaged and disadvantaged groups, with four measures of difference**

| <b>Setting</b> | <b>AG<br/>Fav<br/>Rate</b> | <b>DG<br/>Fav<br/>Rate</b> | <b>AG/DG<br/>Fav<br/>Ratio</b> | <b>DG/AG<br/>Adverse<br/>Ratio</b> | <b>Percentage<br/>Point Diff</b> | <b>Odds<br/>Ratio</b> |
|----------------|----------------------------|----------------------------|--------------------------------|------------------------------------|----------------------------------|-----------------------|
| A              | 20.0%                      | 9.0%                       | 2.22 (1)                       | 1.14 (4)                           | 11.0 (4)                         | 2.53(1)               |
| B              | 40.0%                      | 22.6%                      | 1.77 (2)                       | 1.29 (3)                           | 17.4 (2)                         | 2.28 (3)              |
| C              | 70.0%                      | 51.0%                      | 1.37 (3)                       | 1.63 (2)                           | 19.0 (1)                         | 2.24 (4)              |
| D              | 80.0%                      | 63.4%                      | 1.26 (4)                       | 1.83 (1)                           | 16.6 (3)                         | 2.31 (2)              |

For instant purposes, the settings may be regarded as (a) selection processes at different entities, or (b) selection processes at the same entity at different points in time or as modified for such reasons as to improve accuracy or reduce a perceived discriminatory effect.

Within parentheses next to each measure are the rankings of the settings, from highest to lowest, according to the strength of the forces causing the outcome rates of AG and DG to differ. Those forces could be bias on the part of decision-makers or differences in the qualifications of the applicants from AG and DG that would explain observed results as other than a result of bias. The rankings could also be regarded as reflecting the comparative size of the disparate impact of the selection processes causing the outcome rates of AG and DG to differ.

There are four principal ways observers might rank the settings according to the strength of the forces causing the outcome rates to differ. Observers who rely on relative differences in favorable outcomes—as would commonly be done in an employment

discrimination case involving hiring or promotion—would rank them A,B,C,D.

Observers who measure disparities in terms of relative differences in adverse outcomes—as would commonly be done in a lending discrimination case or in an investigation of disparities in school discipline rates, and as might also be done in an employment discrimination case where the favorable outcome is retention and the adverse outcome is termination—would rank them D,C,B,A, the opposite of the ranking according to the first approach.

Observers who measure disparities in terms of absolute differences between rates—as has been done in studies of lending disparities by the Federal Reserve Board<sup>11</sup> and as is increasingly done in studies of public school proficiency disparities and healthcare disparities—would rank them C,B,D,A.

And observers who measure disparities in terms of odds ratios—as might be done in analyses in a variety of settings that attempt to adjust for differences in characteristics of the two groups by means of logistic regression—would rank them A,D,B,C, the opposite of the ranking based on absolute differences.

Given the specifications underlying the table, however, there exists no rational basis for distinguishing among the four settings. Any measure that does distinguish among them is a fundamentally unsound measure.

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<sup>11</sup> See Marvin M. Smith & Christy Chung Hevener, *Subprime Lending Over Time: The Role of Race*, Federal Reserve Bank of Philadelphia (October 2010) and Robert B. Avery, Kenneth P. Brevoort & Glenn B. Canner, *The 2006 HMDA Data*, Federal Reserve Bulletin (December 2007).

Further, focusing solely on Setting A, the fact that the two relative differences would yield dramatically different interpretations respecting whether the forces causing the rates to differ are strong or weak illustrates that the measures are unsuitable even for making that appraisal.

In attempting to fully understand the implications of the described patterns with respect to appraisals of group differences, one will usefully consider the following points.

First, one will observe many departures from the patterns described above. But observed patterns are functions of (a) actual differences (changes) in the strength of the forces causing rates to differ in the settings being examined (which tend to drive all standard measures in the same direction) and (b) the distributionally-driven, frequency-related patterns described here. In legal settings, as elsewhere, the observer's interest in examining differing outcome rates is principally, if not solely, in order to understand (a). But only with a mastery of (b) can one understand (a).

Second, it does not matter whether the measures are affected by the frequency of an outcome in exactly the way described. The fact that a measure tends to be affected in any way by the frequency of an outcome undermines the measure for quantifying the strength of an association without consideration of the effects of the frequency of the outcome. Indeed, putting aside the fact that the two relative differences tend to change systematically in opposite directions as the frequency of an outcome changes, the simple possibility that they may do so (as is demonstrated in Tables 1 and 2 irrespective of any rationale underlying the creation of those tables) compels recognition that

the appraisal of whether one disparity is larger than another, or even whether a disparity should be deemed large or small, requires a new paradigm.<sup>12</sup>

## **2. A Theoretically Sound Measure of the Strength of the Forces Causing Outcome Rates of Advantaged and Disadvantaged Groups to Differ**

Implicit in the illustrations in Table 1 and 2 is that the only theoretically sound way to appraise the strength of the forces causing rates of advantaged groups and disadvantaged groups to differ is to derive from pairs of outcome rates the difference between the means (in terms of percentage of a standard deviation) of the underlying distributions of factors associated with experiencing the outcome at issue. That approach exists in the form of a statistical procedure known as the probit. I commonly call the value generated the EES, for estimated effect size. See *Race and Mortality Revisited* at 336-337.

For example, when favorable outcome rates for AG and DG are 30% and 10%, one can estimate that the difference between the means is .757 standard deviations; when those rates are 15% and 5% one can estimate that the differences between the means is .608 standard deviations. Perspective on the measure in the context of various pairs of rates that observers have attempted to interpret on the basis of standard

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<sup>12</sup> It may also be useful to recognize that, irrespective of the patterns described here, the rate ratio for either outcome is not only an unsound measures of association, but an illogical one as well. See *Race and Mortality Revisited* at 339. See also the Subgroup Effects, Illogical Premises, Illogical Premises II, and Inevitable Interaction subpages of the Scanlan's Rule page.

measures differences between outcome rates may be found in the tables in *Race and Mortality Revisited*, the Kansas Law paper, and the FCSM paper. See also the illustrations in the Four-Fifths Rule subpage of the Disparate Impact page of [jpscanlan.com](http://jpscanlan.com).

The EES/probit is not a perfect measure of the strength of the forces causing outcome rates to differ. It is based on the assumption that the underlying distributions are perfectly normal and rarely will one be certain that such is the case. Further, even when the underlying distributions are perfectly normal, there will be situations where the data examined are based on truncated portions of normal distributions, in which case the measure will not be sound.<sup>13</sup> And often it will be difficult to recognize whether one is dealing with a truncated population.

While these and other matters raise issues about the reliability of the EES, such issues do not provide a basis for relying on any of the standard measures of differences between outcome rates without consideration of the way such measures tend to be affected by the frequency of an outcome. That would hold even if the EES were not deemed a sufficiently reliable measure on which to base a legal decision.<sup>14</sup>

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<sup>13</sup> See the Truncation Issues and Credit Score Illustrations subpages of the Scanlan's Rule page of [jpscanlan.com](http://jpscanlan.com).

<sup>14</sup> The shortcoming of the EES do not undermine its utility for illustrating problems with standard measures, as in the many illustrations in *Race and Mortality Revisited*, the Kansas Law and FCSM papers, and elsewhere. See the LIHTC Approval Disparities subpage of the Scanlan's Rule page for an illustration of the way the .314 EES that can be derived from the 49.7% and 37.4% approval rates cited in the lower court opinions in this case can be used to show how changes in the number of units applied

### **3. Whether Relaxing a Requirement in Fact Reduces the Disparate Impact of the Requirement Within the Process of Which it Is a Part**

In many settings where a disparate impact is analyzed in terms of relative differences in adverse outcomes, what commonly seems an obvious less discriminatory alternative involves relaxing a criterion, which then will tend to increase that disparity as it is being measured. Fair lending regulators may eventually recognize that lowering standards and otherwise reducing the frequency of adverse borrowing outcomes tend to increase relative differences in experiencing the outcomes, and then cease to monitor the fairness of practices in terms of relative differences in adverse outcomes while encouraging actions that tend to increase those relative differences. But even if misunderstanding is eliminated from the situation—and with it reliance on standard measures of differences between outcome rates to quantify disparities or determine whether one practice has a smaller adverse impact than another—there would exist considerable difficulty in determining whether lowering a requirement to secure a favorable outcome reduces, or increases, the disparate impact of the requirement within the process of which it is a part. I explore these issues, somewhat uncertainly, in Section E of the Kansas Law paper.

In summary, where a criterion entirely determines whether a person will experience the favorable outcome or the corresponding adverse outcome, there

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for can affect perceptions about whether any discriminatory effect of the selection procedure at issue has changed over time.

does not seem to be a rational basis to maintain that the stringency of the standard affects the size of the disparate impact at all. For example, where all who pass a test experience the favorable outcome and all who fail the test experience the corresponding adverse outcome, as would typically be the case with bar exams and other certification procedures as well as teacher competency and high school exit tests, the disparate impact of the test would seem unaffected by the cutoff. That seems also to hold with respect to things like standards for termination from a job for inadequate performance or misconduct and criteria for mortgage foreclosure.

Whether one meets a criterion such as a test score or credit score, however, often does not entirely dictate the outcome of the process save for those who fail to meet it. Those meeting the criterion still face other elements of the selection process in order to secure the desired outcome, and there may be reason to believe that the overall process will show a smaller disparity adverse to the disadvantaged group (as measured by the EES or some like measure that is unaffected by the frequency of an outcome) with a lower than a higher cutoff. But there can also be situations—as where all persons achieving a certain level on a criterion secure the favorable outcome while those failing to achieve it must compete further to secure the favorable outcome—where lowering the cutoff tends to increase the outcome disparity.

The above points apply most directly to situations where the ultimate outcome involves a dichotomy. The measurement issues addressed here do not directly affect continuous variables like loan rates. But continuous variables often are functions of dichotomies, sometimes strongly so, as where, for

example, achieving a credit score secures a loan applicant a lower rate. Lowering a cutoff for the favored rate from a very high point to a somewhat lower point tends to increase both relative and absolute differences between average interest rates of advantaged and disadvantaged groups up until certain points and then to decrease such differences (though those points may sometimes be different for the relative difference and the absolute difference between the averages rates). That means that sometimes lowering a cutoff will increase the perceived disparate impact and sometimes it will decrease the perceived disparate impact, and that sometimes lowering the cutoff would increase that impact according to one measure while decreasing it according to another measure.

The Addendum to the Kansas Law paper presents a simplified illustration of these patterns. Actual situations are more complex. But the greater complexity of actual situations only increases the difficulty of determining whether lowering a standard for achieving a desired outcome increases or decreases differences between the average loan rates that are influenced by the outcome.

As will be discussed in Section C, however, analyses of differences between the loan terms secured by advantaged and disadvantaged groups (and appraisals of differences in subprime rather than prime loan status) cannot prove whether there exists a disparity at all.

**B. The Fundamental Unsoundness of Analyses of Discrimination Issues Based on the Proportion a Group Comprises of Persons Potentially Experiencing an Outcome and the Proportion it Comprises of Persons Experiencing the Outcome**

Observers, including the Court,<sup>15</sup> sometimes analyze discrimination issues, whether or not characterized as disparate impact, in terms of differences between the proportion a group comprises of persons potentially experiencing a favorable or adverse outcome (the pool) and the proportion the group comprises of persons experiencing the outcome. Such information may establish that differences between outcome rates exist (causality issues aside). It even enables one to determine the relative difference between those outcome rates. For example, if DG comprises 40% of the pool but only 20% of persons experiencing some favorable outcome, and the pool is comprised entirely of AG and DG, one can arithmetically determine that AG's rate of experiencing that outcome is 2.67 times DG's rate.

But such information does not enable one to determine what those rates are. And, as shown in Section A, in order to appraise the strength of the forces causing the underlying outcome rates to differ, one must know the rates.<sup>16</sup>

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<sup>15</sup> See *Hazelwood School Dist. v. United States*, 433 U.S. 299 (1977); *Wards Cove Packing Co. v. Atonio*, 490 U.S. 642 (1989).

<sup>16</sup> See Table 6 of the Kansas Law paper for examples of varying advantaged group and disadvantaged group rate scenarios, and corresponding varying EES figures, for situations where

The last point should dispose of representational comparisons for proof of discrimination issues, if not in general, at least with respect to determining whether one disparity is larger than another. It may nevertheless be useful to note some of the ways appraisals of disparities in terms of differences between those proportions will be misleading.

Approaches to measuring those disparities include use of both relative and absolute differences between the proportion a group comprises of the pool and the proportion it comprises of persons experiencing the outcome the observer happens to be examining.<sup>17</sup> Both the relative difference and the absolute difference tend to change as the frequency of the outcome changes, in the following way.

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 is a pattern whereby reducing the frequency of an outcome tends to increase the proportion the group more susceptible to the outcome comprises both (a) of persons continuing to experience the outcome and (b) of persons not experiencing the outcome.<sup>18</sup> Thus, suppose an observer

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the disadvantaged group comprises 40% and 15% of the pool and, respectively, 20% and 5% of persons experiencing the favorable outcome.

<sup>17</sup> See *Methods for Assessing Racial / Ethnic Disproportionality in Special Education*, IDEA Data Center (May 2014)

<sup>18</sup> For example, in the case of the lowering of the test cutoff illustrated in Table 1, assuming equal-sized groups, the lowering of the cutoff would cause the proportion DG comprised of those failing the test to increase from 65% to 72% and the proportion DG comprised of those passing the test to increase from 44% to 48%. Table 1 of the Chance editorial shows how reducing poverty

is examining the difference between the proportion DG comprises of the pool and the proportion it comprises of persons experiencing the adverse outcome and that the adverse outcome decreases. Due to the increase in the proportion DG comprises of persons experiencing the adverse outcome, both the relative and absolute difference between the proportion DG comprises of the pool and the proportion it comprises of persons experiencing the adverse outcome would tend to increase. The opposite would tend to occur when the frequency of the outcome increased.

Suppose, however, that the observer instead examined the difference between the proportion DG comprised of the pool and the proportion it comprised of persons experiencing the corresponding favorable outcome. Since the decline in the adverse outcome caused DG to comprise a larger proportion of persons experiencing the favorable outcome as well as the adverse outcome, both relative and absolute differences between the proportion DG comprised of the pool and the proportion it comprised of persons experiencing the favorable outcome would decrease. Again, the opposite would tend to occur when the frequency of the adverse outcome increased.

But whatever the direction of the changes in one outcome or the other, observers who measure disparities in terms of relative or absolute differences between the proportion a group comprises of the pool and the proportion it comprises of persons experiencing an outcome will tend to reach opposite conclusions about the directions of changes in

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would cause blacks to comprise a larger proportion of both the poor and the non-poor than they did previously.

the size of a disparity depending on whether they examine the favorable outcome or the corresponding adverse outcome. That is, as any outcome and its opposite change in overall frequency, observers who use these measures while focusing on the perceived overrepresentation of the disadvantaged groups among persons experiencing the adverse outcome will tend to reach opposite conclusions about directions of changes in disproportionality from those using these measures while focusing on the perceived underrepresentation of the disadvantaged group among persons experiencing the favorable outcome.

Given the outcome rates in Table 1, for example, regardless of the number of persons in each group, lowering the cutoff would decrease both relative and absolute differences between the proportion DG comprises of the pool and the proportion it comprises of those who pass, while increasing both relative and absolute differences between the proportion DG comprises of the pool and the proportion it comprises of those who fail.

Further, for any given pair of outcome rates, both relative and absolute differences between the proportion DG comprises of the pool and the proportion it comprises of persons experiencing the outcome the observer examines are affected by the proportion DG comprises of the pool, in complex and often contradictory ways. See the IDEA Data Center Disproportionality Guide subpage of the Discipline Disparities page of [jpscanlan.com](http://jpscanlan.com). But to my knowledge these factors have never been considered by those appraising a disparity in terms of the difference between the proportion a group comprises of the pool and the proportion it comprises of persons experiencing an outcome, just as they have never

considered that measures between those proportions tend to be systematically affected by the frequency of an outcome. As indicated at the outset of this section, however, there are no circumstances in which information on the proportion a group comprises of the pool and the proportion it comprises of persons experiencing an outcome can provide sound information on the strength of the forces causing the underlying outcome rates to differ (or the proportions to differ) or whether one situation involves a less discriminatory effect than another.

### **C. The Fundamental Unsoundness of Analyses of Discrimination Issues That Solely Examine Persons Who Accepted Some Outcome or Situation**

Many discrimination claims, including many of the more successful suits in terms of monetary recovery and many that are characterized as involving disparate impact, are based on analyses of persons who accepted some outcome or situation, and, where, among those accepting some outcome or situation, certain groups appear to be disfavored. In the employment context, such claims have involved allegations that among persons who are hired, members of disadvantaged groups are more often assigned to less desirable jobs or receive less compensation than similarly situated members of advantaged groups. In the lending context, they have involved allegations that among persons who receive loans, members of disadvantaged groups more often receive subprime rather than prime loans, more often are subjected to certain additional costs, or pay higher interest rates than members of advantaged groups, as in the much-publicized suits by the Department of Justice against Countrywide Financial Corporation

and Wells Fargo Bank that led to settlements totaling more than half a billion dollars. See *Race and Mortality Revisited* at 341-342.

In the case of claims concerning the disproportionate assignment of loans of disadvantaged groups to subprime status, relative differences in experiencing the favorable and the adverse outcome will tend to exhibit the patterns discussed in Section A. Thus, for example, anything that generally lowers the proportion of any given number of total loans assigned to subprime status would tend to increase relative differences in subprime assignment rates within the loan recipient population, while reducing relative differences in rates of receipt of prime loans within that population. Absolute differences would tend to behave, more or less, as they would within populations where outcome-related factors are normally distributed. Altering standards to receive particular terms would tend to cause the difficult to predict patterns of effects on relative and absolute differences in interest rates described in Section A.3. But the fact that the universe examined is comprised solely of persons who accepted loan terms would affect the patterns by which odds ratios tend to be affected by the frequency of an outcome in a manner akin to that observed in a truncated population. For the same reason the EES would not be an effective measure of differences between rates at which persons within the population of loan recipients experienced particular types of outcomes. See the Truncation Issues and Credit Score Illustrations subpages of the Scanlan's Rule page of [jpscanlan.com](http://jpscanlan.com).<sup>19</sup>

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<sup>19</sup> The patterns among a group of loan recipients would differ somewhat, and in an unknown way, from the patterns observed within a population that is truncated by the elimination of a

But analyses that seek to determine whether a process treats similarly situated persons of different demographic groups equally by examining only persons who accepted some outcome fail to satisfy the fundamental requirement of a sound statistical analysis—that it examine all persons subject to the process. See my *Illusions of Job Segregation*, 93 Public Interest 54 (Fall 1988), and *Are Bias Statistics Nonsense?*, Legal Times (Apr. 17, 1989). Yet the referenced analyses fail to examine persons who received no offer and persons who declined an offer because they found the offered terms unsatisfactory. Thus, such analyses cannot establish whether a difference between the outcomes of advantaged and disadvantaged groups exists at all, much less whether the forces causing the difference are larger in one setting than another.

In *Wards Cove Packing Co. v. Atonio*, 490 U.S. 642, 653-54 (1989), the Court appeared to foreclose selection discrimination claims based on analyses solely of persons who accepted some outcome by its finding concerning the fact that the comparison pool on which the plaintiffs sought to rely did not include persons not hired at all. Nevertheless, even after *Wards Cove*, employment discrimination suits that treated the incumbent employees as if they comprised the entire universe of persons seeking a particular type of job proved to be quite successful. See my *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).

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certain portion of the bottom part of the overall distribution. For, as explained immediately *infra*, the observed population also fails to include persons who would not accept the terms offered.

But, whether such claims were foreclosed by *Wards Cove*, they are fundamentally unsound. The same holds for recent lending claims that treat persons accepting some type of loan as if they comprised the entire universe or persons seeking the most desirable loans. These issues apply equally to analyses of claims challenging loan terms apart from assignment to subprime, as well as any other type of claim (including salary discrimination claims), that fail to consider persons who either decline an unsatisfactory offer in the first instance or, in the case of salary, leave an organization because of unsatisfactory salary progression or for any other reason. See Section F of the Kansas Law paper and my *The Perverse Enforcement of Fair Lending Laws*, Mortgage Banking 90 (May 2014).

Because analyses based solely on persons who accepted some outcome are fundamentally unsound, it is unnecessary to discuss plausible approaches to divining the strength of the forces causing the outcome rates (or rate levels) of advantaged and disadvantaged groups to differ or to determine whether one set of procedures has a less discriminatory effect than another. I note, however, that, as suggested in the second paragraph of this section, it is not clear that the strength of those forces can be effectively measured even if persons securing loans could properly be deemed the universe of persons seeking them.

## **II. THE DEPARTMENT OF HOUSING AND URBAN DEVELOPMENT'S DISCRIMINATORY EFFECTS RULE IS NOT ENTITLED TO CHEVRON DEFERENCE**

Respondents will maintain that, pursuant to *Chevron U.S.A., Inc. v. Natural Resources Defense Council, Inc.*, 467 U.S. 837 (1984), the Court should

defer to HUD's interpretation of the statute it enforces as reflected in the agency's discriminatory effects rule (24 C.F.R. § 100.500). Even apart from issues as to whether the statutory language of the FHA permits such interpretation, there are strong reasons why the Court should not accord *Chevron* deference to HUD's rule, including that the disparate impact provision is unconstitutionally vague.

Due process requires that a statute must apprise a person of reasonable intelligence of the nature of prohibited conduct. Otherwise it is unconstitutionally vague. *Federal Communications Commission, et al. v. Fox Televisions Stations, Inc., et al.*, \_\_\_ U.S. \_\_\_ (No. 10-1293). The void for vagueness doctrine, while principally applied to criminal statutes and those affecting speech, is not limited to those contexts. Even if the application in the civil context were to be limited to extraordinary situations, application would be appropriate here.

HUD's discriminatory effects rule interprets the FHA to impose liability on a covered entity where a practice "actually or predictably results in a disparate impact on a protected group" (24 C.F.R. § 100.500 (a)) and where, when there exists a legally sufficient justification for the practice, the entity's interests could be "served by another practice with a less discriminatory effect." *Id.* § 100.500 (c)(3). The rule does not state how to measure a disparate impact or determine whether one practice has a less discriminatory effect than another. The Supplementary Information provided with the rule contains discussion of a suggestion by a commenter that a disparate impact be defined as "a 20 percent difference between the relevant groups." But in explaining the decision not to provide guidance on the measurement

of a disparate impact, the agency failed to indicate whether it recognized that a 20 percent difference as to one outcome could mean something different from a 20 percent difference as to the opposite outcome. 78 Fed. Reg. 11468-11469.

HUD's failure to provide guidance on this issue occurs in the context of an incongruous fair lending law enforcement regime that has existed for at least two decades. As explained above, in applying the disparate doctrine to the FHA and other fair lending laws, the government has encouraged or pressured covered entities to do things that increase the chances that the government will sue them for discrimination. An element of that regime has involved the government's misleading covered entities to believe that relaxing standards would tend to reduce relative differences in adverse outcomes, something that is the exact opposite of reality. HUD's failure to address the matter contributes further to the pattern. That the misleading has occurred and continues to occur because of the government's lack of understanding makes the situation no less incongruous.

In the lending context, virtually every criterion of creditworthiness will disproportionately affect disadvantaged groups, and the two most standard measures of disparity commonly yield diametrically opposed interpretations as to which practice is the more discriminatory as well as to whether a particular disparity should be deemed large or small. Absence of sound guidance places lenders in an untenable situation with respect both to determining whether a practice causes a sufficient disparate impact to require justification for the practice and to fulfilling the obligation to adopt the justified practice with the least discriminatory effect. In making judgments on such

issues, an entity has no basis to know whether actions like relaxing or strengthening standards will be regarded as having increased or decreased the disparate impact, given that such modifications typically reduce one relative difference while increasing other. The entity faces this conundrum in circumstances where if it makes a wrong choice, in addition to facing the substantial civil penalties that HUD may impose for violation of the FHA, the entity will face potentially vast damage awards in civil actions, such as were secured by the Department of Justice in recent cases even without establishing liability at trial.

In such circumstances, due process requires that a provision imposing an obligation to implement the least discriminatory alternative provide guidance on how to identify that alternative. Moreover, such guidance would have to be sound—which is to say, cannot involve a measure that changes simply because the frequency of an outcome changes. Otherwise, among many comparable anomalies, a practice that in fact involves a less discriminatory effect than another can be deemed to have the greater discriminatory effect, and things like general changes in interest rates, with attendant changes in overall approval rates, could transform a compliant entity into a noncompliant entity though its practices have changed not at all. Yet the HUD rule provides no guidance on crucial measurement issues whatever.

Irrespective of the constitutional issue, however, this is manifestly a case where *Chevron* deference to an enforcing agency's presumptive expertise is unwarranted. For, during the course of HUD's more than twenty year involvement in efforts to apply the disparate impact doctrine to the FHA, the agency has

failed to show an awareness that it is even possible for the two relative differences to change in opposite direction, much less that they tend to do systematically, especially when standards are lowered in the manner that federal regulators have long encouraged.

Presumably, HUD did not address that issue or the other measurement issues discussed in Part I because it did not know that such issues exist. That few persons or entities are aware of such issues is of little moment. Responsible agency action entails learning about such issues and addressing them when agency action has important implications, and especially when, as here, the action can impose substantial liability on entities covered by the statute the agency enforces. Whether HUD's failure to do that is excusable or not, such failure should deny the agency action the deference commonly accorded to an enforcing agency's interpretations.

## CONCLUSION

For the reasons stated above, the decision of the court of appeals should be reversed.

Respectfully submitted,

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