

**IN THE UNITED STATES DISTRICT COURT  
FOR THE DISTRICT OF MARYLAND**

COALITION FOR EQUITY AND \*  
EXCELLENCE IN MARYLAND \*  
HIGHER EDUCATION, *et al.*, \*  
Plaintiffs  
v. \*

MARYLAND HIGHER EDUCATION \*  
COMMISSION, *et al.*, \*  
Defendants. \*

Case No.: CCB-06-cv-2773

\* \* \* \* \*

**DEFENDANTS’ FEDERAL RULE OF EVIDENCE 702  
MOTION TO EXCLUDE OPINION TESTIMONY  
CONCERNING THE EFFECTS OF “PROGRAMMATIC NICHES,”  
“HIGH-DEMAND” PROGRAMS AND “UNIQUE” PROGRAMS  
ON HBI ENROLLMENT BY OTHER-RACE STUDENTS**

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## I. INTRODUCTION

Plaintiffs seek to offer expert opinion testimony that is unsupported by any social science methodology at all (with regard to the effect of “programmatically niches”) and by unsound and unreliable social science (with regard to the effects of “high-demand” or “unique” programs). That testimony is inadmissible under Fed. R. Evid. 702.

In their “Second Corrected Reply Expert Report” (Exh. 9)<sup>1</sup>, Drs. Clifton Conrad and Walter Allen (Conrad & Allen) offer the fourth version of their plan to remake Maryland public higher education.<sup>2</sup> That plan is based on speculation that a remedial strategy based on creating “programmatically niches” at the four Maryland HBIs by “transferring” programs from non-HBIs (closing them and opening new ones at HBIs) and establishing other new HBI programs “offers great potential” to increase other-race enrollment at the HBIs. *Id.* ¶ 157. These “programmatically niches,” they recommend, should “to the extent possible . . . be both unique and high-demand” (¶ 170), and, even if not “unique,” “should be high-demand insofar as possible” (¶ 171), and should be “unique” to the extent possible. ¶ 172. Yet they concede that they can present no “scientific analysis” to support the theory that establishing “programmatically niches” will

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<sup>1</sup> The version attached as Exh. 9 has been redacted to remove all references to the HBIs’ mediation submissions. The parties are continuing to work together to prepare an agreed-upon version of the Conrad & Allen report, without those references.

<sup>2</sup> Although titled “Second,” this report is actually Conrad & Allen’s fourth edition of their plan. They submitted the first version without any supporting analysis on May 5, 2015 (ECF 406-2). The second version (the “Reply Expert Report”) followed on April 29, 2016, with a third version (the “Corrected Expert Reply Report”) on May 2, 2016 (the May 2 version was filed as ECF 474-2). The fourth and most recent version arrived July 6, 2016 Conrad & Allen have testified that they believe their work is now complete. Exh. 1 (Allen Dep., at 174); Exh. 2 (Conrad Dep., at 8).

attract other-race students. ¶ 204. Indeed, they say “it remains impossible to find ‘textbook examples’ or to ‘scientifically test’ the desegregative impact of the Plaintiffs’ remedial proposal.” ¶ 328. Beyond that, they say the proposition “*cannot* be tested” until after it has been implemented—at extraordinary cost to the State, its taxpayers, and its students. ¶ 212 (emphasis added). In the face of the admitted failures of other remedial plans, Conrad & Allen nevertheless urge the Court to order massive changes first, and only then assess whether the plan is sound.

Not only is their proposal based on a “strategy which has never been adopted or implemented in any state” (¶ 204), it is unsupported by studies or objective evidence from the social sciences showing that students choose colleges on the basis of “programmatically niches.” This includes the qualitative (interview-based) study of college students performed by Dr. Conrad in 1994 while working for the Department of Justice. And Plaintiffs’ subsidiary assertions – set forth in *quantitative* terms in their 2016 Second Corrected Report – that “high-demand” and “unique” programs (as Dr. Conrad defines them) will attract disproportionate numbers of other-race students to HBIs, are based on methodologically-unsound manipulations of data that defy the most basic precepts of social science.<sup>3</sup>

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<sup>3</sup> The data and analyses presented or relied on concern solely the enrollment of *white* students at HBIs. However, Conrad & Allen acknowledge that “the HBIs have been unable to attract significant numbers of Latino, Native American, Asian and Pacific Islander students.” ¶ 44. Plaintiffs offer no contention or evidence that their remedial plan will attract non-white students who are not African-American. Accordingly, this memorandum will refer to white rather than other-race enrollment.

The most telling empirical evidence—that prior remedial plans featuring “high demand” and “unique” programs have failed to increase white enrollment at HBIs—contradicts that theory. Conrad & Allen acknowledge that past judicial remedies based on “unique” and “high demand” programs have failed, but seek to distinguish those ineffective remedial strategies as “random”—i.e., not based on the unverifiable strategy of “programmatically niches.” ¶ 209. Consequently, the viability of the remedial plan rests entirely on the untested (and supposedly untestable) proposition that organizing programs in “niches” will make the difference between the wasted effort and expense of remedies ordered by other courts in other states, and Plaintiffs’ belief that their proposed remedy will succeed.

“A reliable expert opinion must be based on scientific, technical or other specialized knowledge and not on belief or speculation, and inferences must be derived using scientific or other valid methods.” *McKerrow v. Buyers Prods. Co.*, 2016 WL 1110303, at \*3 (D. Md. March 22, 2016) (quoting *Ogelsby v. General Motors Corp.*, 190 F.3d 244, 250 (4th Cir. 1999)). Apparently distinguishing the task of developing a proposed remedial plan from the rigor that would be required for a “research study” of the likely effects of new programs on other-race enrollment (Exh. 1, at 99, 102 (Allen Dep.) (Exh. 2, at 244-46, 274-76, 391 (Conrad Dep.)) Conrad & Allen have dispensed with the methodological constraints of social science. Although Plaintiffs are entitled to *propose* that the Court gamble billions of taxpayer dollars, the education and livelihood of thousands of students and faculty, and the future of public higher education in the State



on “programmatically niches,” they are not entitled to cloak that gamble with the seeming authority of expert opinion.

## II. RULE 702’S GATEKEEPING STANDARDS

The Court is familiar with the standards governing the admissibility of expert testimony under Fed. R. Evid. 702, having addressed those standards repeatedly in recent opinions including *McKerrow*; *Baltimore Aircoil Co., Inc. v. SPX Cooling Tech., Inc.*, 2016 WL 4426681 (D. Md. Aug. 22, 2016); *Equal Rights Ctr. v. Equity Residential*, 2016 WL 1258418 (D. Md. March 31, 2016); *Sprint Nextel Corp. v. Simple Cell, Inc.*, 2016 WL 524279 (D. Md. Feb. 10, 2016); and *Searls v. Johns Hopkins Hosp.*, 158 F. Supp. 3d 427, 441 (D. Md. 2016). Nonetheless, a few key points merit emphasis.

*First*, expert opinion testimony is admitted under an exception to the general rule that witnesses testify to facts (not opinions) based on their personal knowledge (not second-hand information). “The party seeking to introduce expert testimony has the burden of establishing its admissibility by a preponderance of the evidence.” *Heckman v. Ryder Truck Rental, Inc.*, 2014 WL 3405003, at \* 2 (D. Md. July 9, 2014) (citing *Daubert v. Merrell Dow Pharm. Inc.*, 509 U.S. 579, 592 n. 10 (1993)). *Accord McEwen v. Balt. Wash. Medical Ctr., Inc.*, 404 Fed. Appx. 789, 792 (4th Cir. 2010); Fed. R. Evid. 702, Advisory Committee Note to 2000 amendment (“the admissibility of all expert testimony is governed by the principles of Rule 104(a). Under that Rule, the proponent has the burden of establishing that the pertinent admissibility requirements are met by a preponderance of the evidence.”).

**Second**, in deciding admissibility, “the court acts as gatekeeper, only admitting expert testimony where the underlying methodology” is reliable and relevant. *Heckman*, 2014 WL 3405003, \*2. (citing *Daubert*, 509 U.S. at 589). The reason to bar the gate in this case is the unreliability of the methodology Plaintiffs’ experts deploy to support their opinions.

**Third**, although “conclusions and methodology are not entirely distinct,” *Heckman*, 2014 WL 3405003, \*2 (quoting *General Elec. Co. v. Joiner*, 522 U.S. 136, 146 (1997)), the Rule 702 question is not whether the experts’ conclusions (or the experts themselves) seem at first blush to be intuitively appealing or persuasive, but whether the experts’ *supporting methodology* is sound. “In applying *Daubert*, a court evaluates the methodology or reasoning that the proffered scientific or technical expert uses to reach his conclusion.” *Heckman, id.* (quoting *TFWS, Inc. v. Schaefer*, 325 F.3d 234, 240 (4th Cir. 2003)). The soundness of the methodology the expert uses to arrive at a particular opinion is distinct from the expert’s general qualification to testify about matters in a particular field. “Even where an expert is qualified to provide an opinion on a particular subject, . . . his testimony is not admissible if its underlying methodology does not satisfy Rule 702.” *McKerrow*, at \*3.

**Fourth**, in testing an expert’s methodology, courts look to the standards of the expert’s own field to assess the reliability of similar conclusions, because “the purpose of Rule 702’s gatekeeping function is to ‘make certain that an expert . . . employs in the courtroom the same level of intellectual rigor that characterizes the practice of an expert in the relevant field.’” *Cooper v. Smith & Nephew, Inc.*, 259 F.3d 194, 203 (4th Cir.

2001) (quoting *Kumho Tire Co., Ltd. v. Carmichael*, 526 U.S. 137, 152 (1999)). For example, when research in a field involves considering alternative explanations for an association between two variables before drawing causal inferences, an expert who “utterly fails to consider alternative causes or fails to offer an explanation for why the proffered alternative explanation was not the sole cause” should not be permitted to testify. *Cooper*, 259 F.3d at 202; *see also Roche v. Lincoln Property Co.*, 278 F. Supp. 2d 744, 749 (E.D. Va. 2003).<sup>4</sup> Likewise, when the methodology in the field for reaching an opinion such as the one the expert is offering “involves formulating a hypothesis to explain the world based upon what is already known and then subjecting that hypothesis to tests designed to falsify (or confirm) the hypothesis,” *McKerrow*, at \*6 (citations omitted), the expert must do the same to support the opinion testimony. *Daubert’s* gatekeeping rules require adherence to the methodological standards of the social sciences, just as they do to biological science or engineering. *Tyus v. Urban Search Mgmt.*, 102 F.3d 256, 263 (7th Cir. 1996).

**Last**, courts do not rely on an expert’s own assurances of reliability. “Nothing in either *Daubert* or the Federal Rules of Evidence requires a district court to admit opinion evidence that is connected to existing data only by the *ipse dixit* of the expert.” *Gen.*

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<sup>4</sup> Other courts agree that the failure to account for alternative explanations is fatal. *E.g.*, *Bickerstaff v. Vassar College*, 196 F.3d 435, 450 (2d Cir. 1999); *Raskin v. Wyatt Co.*, 125 F.3d 55, 67-68 (2d Cir. 1997); *In re Mirena IUD Prods Liability Litig.*, 2016 WL 890251, \*41 (S.D.N.Y. March 8, 2016). The Rules Advisory Committee Note to the 2000 amendment to Rule 702 includes this as a factor to consider. For qualitative as well as quantitative social science methods, the “ability to rule out alternative explanations or ‘rival hypotheses’ . . . is widely seen as the fundamental characteristic of scientific inquiry in general.” Exh. 25, at 243, 250 (Joseph A. Maxwell, *Using Qualitative Methods for Causal Explanation*, 16 *Field Methods* (2004)).

*Elec.*, 522 U.S. at 146. To defer to an expert's judgment about the soundness of his own methods would be to abandon Rule 702's gatekeeping function.

**III. THE ADMISSION OF TESTIMONY FROM CONRAD & ALLEN IN THE LIABILITY TRIAL HAS NO BEARING ON THE ADMISSIBILITY OF THE OPINION TESTIMONY CHALLENGED IN THIS MOTION.**

Although Drs. Conrad & Allen have candidly admitted they prepared their Reply Report on the assumption that they did not have to meet the methodological standards of social science in the remedial phase, the admissibility of expert opinion testimony about remedy is subject to *Daubert's* gatekeeping requirements. *People Who Care v. Rockford Bd. of Educ.*, 111 F.3d 528 (7th Cir. 1997).<sup>5</sup>

Plaintiffs may argue that no *Daubert* inquiry is required, or perhaps that the Court should apply a more lenient standard, because Conrad & Allen previously testified as expert witnesses in the liability phase of this case. Conrad & Allen qualified as expert witnesses in the liability phase on the basis of their experience and credentials as social science researchers in the field of higher education. *See* ECF 311, at 11 (Conrad permitted to offer opinion testimony “in the field of higher education, but particularly focused on academic programming and mission and desegregation”); ECF 314, at 37-38 (Allen permitted to offer opinion testimony “on the sound educational justification, student choice, and remedies”). Plaintiffs are now offering avowedly speculative

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<sup>5</sup> Fashioning an equitable remedy is no different (for purposes of evidentiary requirements) from calculating damages as a remedy. No one would argue that an expert could testify about damages without satisfying Rule 702. *See MyGallons LLC v. U.S. Bancorp*, 521 Fed. Appx. 297, \*9 (4th Cir. 2013) (applying *Daubert* to damages expert); *Baltimore Aircoil Co. v. SPX Cooling Tech., Inc.*, 2016 WL 4426681, \* 22 (D. Md. Aug. 22, 2016) (same); *Pulse Med. Instruments v. DIDS, LLC*, 858 F. Supp. 2d 505, 512 (D. Md. 2012) (same).

testimony that is presumptively inadmissible under *Daubert*. Even if Conrad & Allen’s general qualifications in the liability trial carry over to the remedy phase, the Court must *also* scrutinize their proffered remedy phase testimony to decide whether that new testimony is properly grounded in the social science research methods used, as in this case, to predict future outcomes based on causal inferences.

Plainly, the opinion testimony Conrad & Allen seek to offer in the remedy-phase trial is different in scope and content from their testimony in the liability phase. As this Court recognized in its February 2, 2016 order, “an evidentiary hearing will be necessary to inform the court on the complex question of what remedies are educationally sound, justified by the scope of the violation found, and best targeted to remedy that violation while enhancing rather than harming Maryland’s system of higher education.” ECF 460. Conrad & Allen explain that “[w]hile the liability analysis informed our proposal, it is our opinion that a remedial strategy differs in significant respects.” Exh. 9, ¶ 84 (Second Corrected Reply Expert Report). Their joint remedy-phase report is not a mirror image of their respective reports in the liability phase; rather, it addresses what they call “a different set of questions: what interventions or programs are most likely to desegregate the HBIs in Maryland, and why?” *Id.* The admissibility of Conrad & Allen’s answers to that “different set of questions” pertaining to remedy is not governed by the admission of their liability phase testimony.

And the burden of proof is also different, as Plaintiffs admit. ECF 367, at ¶ 350 (noting the difference between plaintiffs’ burden of proving an effect on student choice as

to remedy and defendants' liability-phase burden).<sup>6</sup> More specifically, their liability phase testimony did not include the various selective numerical analyses of Maryland HBI enrollment data offered in their Second Corrected Reply Report. And although Dr. Conrad did refer briefly to the study he conducted as a DOJ-hired expert during his liability phase testimony (ECF 311, at 41-43), plaintiffs strikingly chose not to rely on or even refer to that work in the section of their Proposed Findings of Fact and Conclusions of Law addressing the probable remedial efficacy of high-demand and unique programs. ECF 355, at ¶¶ 190-92 ("Programmatic Remedies are Consistent with Scholarship on Desegregation"). Ducking testimony critical of Dr. Conrad's methodology (Trial Day 17, pm 38-41, ECF 304; *see* ECF 448-5, at 56-59), Plaintiffs instead referred to other articles which had cited Dr. Conrad's paper without having performed any independent research or any evaluation of his work. *Id.* And, as well, this Court's 2013 liability opinion did not cite, let alone rely upon, Dr. Conrad's article. *See* ECF 382.

Plaintiffs may also argue against *Daubert* scrutiny because this Court referred in its liability ruling to Dr. Conrad as "the nation's preeminent scholar" on program duplication and described the Supreme Court as having "adopted" his definition of "unnecessary program duplication" in *United States v. Fordice*, 505 U.S. 717 (1992).

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<sup>6</sup> By the same token, this Court's determination that the State had failed to prove that "unnecessary program duplication" did not have segregative effects in the past does not mean that Plaintiffs have already proven that increasing unduplicated programs at HBIs would increase white enrollment in the future, in light of all of the factors affecting such decisions. Indeed, plaintiffs do not contend that unduplicated programs — *i.e.*, merely unique programs — would have such an effect, and the experience of other remedial plans is to the contrary.

ECF 382, at 45. But as noted above, the subject of the remedy-phase testimony is different from Dr. Conrad's testimony in the liability-phase, as is the supporting methodology set forth in a lengthy new expert report. It would be error to insulate the proposed remedy-phase testimony from rigorous gatekeeping scrutiny because of the Court's acceptance of Dr. Conrad's liability-phase testimony.

Moreover, the Supreme Court's "adoption" of Dr. Conrad's analysis in *Fordice* as a description of the district court's ruling was not an approval of his methodology as an expert witness in that case.<sup>7</sup> To begin with, the *Fordice* trial in 1987<sup>8</sup> predated the Supreme Court's 1993 *Daubert* decision, and no issues about the soundness of Dr. Conrad's methodology as a predicate for its admissibility were raised in the Supreme Court, the Fifth Circuit, or (so far as we can tell) in the district court. More fundamentally, the Supreme Court's explication of "unnecessary program duplication" was purely descriptive of the factual record as it came to the Court, along the way to deciding a dispute about the legal consequences of those (and many other) facts about

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<sup>7</sup> Indeed, the district court flatly rejected Dr. Conrad's methodology for assessing "unnecessary program duplication" in *Knight*, finding that Dr. Conrad's central concept of "core" programs was unrelated to the purposes and practice of public higher education. *Knight v. Alabama*, 787 F. Supp. 1030, 1317-20 (N.D. Ala. 1991). In *United States v. Louisiana*, 9 F.3d 1159, 1168-70 & n. 9 (5<sup>th</sup> Cir. 1993), the court remanded to resolve a disputed question of fact about whether program duplication existed in Louisiana in light of criticisms of Dr. Conrad's methodology—something the court could not have done if *Fordice* has actually cast that methodology in doctrinal stone. And the district court in the *Fordice* case itself expressed doubt in its opinion after remand that Dr. Conrad's analysis "actually yields an answer to the threshold question he himself poses: '[h]as this formally *de jure* curriculum system been dismantled?'" *Ayers v. Fordice*, 879 F. Supp. 1419, 1445 (N.D. Miss. Mar. 7, 1995). Again, that is not something that the court could have done if the Supreme Court had adopted Dr. Conrad's methodology in that very case.

<sup>8</sup> *Ayers v. Allain*, 674 F. Supp. 1523, 1526 (N. D. Miss. 1987) (trial commenced April 27, 1987).

public higher education in Mississippi. *See Ayers v. Allain*, 674 F. Supp. at 1540 (factual finding regarding program duplication). The question before the Court was whether a state’s “adoption and implementation of race-neutral policies alone suffice to demonstrate that the State has completely abandoned its prior dual system.” *Fordice*, 505 U.S. at 729. The Supreme Court reversed the Fifth Circuit on that legal question, without delving into the strength of, or the evidentiary foundation for, the district court’s “undisturbed factual findings,” *id.* at 733, including its assessment of the persistence and extent of program duplication “under the District Court’s definition.” *Id.* at 738.<sup>9</sup> The Supreme Court’s decision in *Fordice* has no bearing on the admissibility of different expert testimony concerning the *remedial effectiveness* of “programmatically niches” including “high-demand” and “unique” programs based on the methodology reflected in the Reply Report.

**IV. THE METHODOLOGY SUPPORTING THE PROFFERED OPINION TESTIMONY IS UNRELIABLE AND DOES NOT MEET ESTABLISHED STANDARDS IN THE SOCIAL SCIENCES FOR DRAWING SUCH CONCLUSIONS.**

Plaintiffs seek to call Drs. Conrad & Allen to testify to a prediction that creating “programmatically niches” based on “unique” or “high demand” programs will significantly increase white enrollment at Maryland’s HBIs. Such a prediction requires first drawing a causal inference that creating “niches” with “high-demand” and “unique” programs will

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<sup>9</sup> The Supreme Court faulted the district court for having wrongly assigned the burden of proving dismantlement of prior *de jure* policies to the plaintiffs, and without “mak[ing] it clear whether [the district court] had directed the parties to develop evidence on these points, and if so, what the evidence revealed.” *Id.* at 739. The district court erred by failing to “consider the combined effect of unnecessary program duplication with other policies.” *Id.*



lead more white students to enroll in HBIs. However, that inference is not based on any research or data at all, with regard to the causal effect of “programmatic niches” on white enrollment, and the data and analysis offered to support the subsidiary predictions that adding “unique” and “high-demand” programs will increase white enrollment do not meet the social science standards for drawing general causal inferences. Instead of rigorous analysis that explores alternative explanations, Conrad & Allen have recklessly extrapolated from a methodologically flawed 22-year-old case study and an analysis of Maryland data that uses invalid quantitative methods, ignoring the contradictory evidence of programs that are also (by their definition) “unique” or “high-demand” (or both) but have low white enrollments.

The evidence that Conrad & Allen’s testimony is based on invalid methods comes principally from their own words in the Reply Report and their depositions. Measured against social science standards for drawing causal inferences which they acknowledge, their work falls short. Treatises, textbooks, and articles confirm that the kinds of predictions Conrad & Allen seek to offer must be grounded in the rigorous analysis of data free from systematic bias. The declaration of Professor Allan J. Lichtman (Exh. 3 (Lichtman Dec.)) draws on his two remedy-phase expert reports to show that Conrad & Allen have used unreliable methods to reach erroneous conclusions. Finally, Professor Michael N. Bastedo, Director of the Center for Higher Education at the University of Michigan, offers additional confirmation in his declaration (Exh. 4 (Bastedo Dec.)) that the methodology used in Conrad & Allen’s Reply Report does not conform to the

standards applicable to qualitative research that seeks to make predictions based on causal inferences, as well as violating standards for quantitative analysis.

**A. THE SECOND CORRECTED REPLY EXPERT REPORT PRESENTS NO METHODOLOGY AT ALL IN SUPPORT OF THE OPINION THAT “PROGRAMMATIC NICHE” WILL BE EFFECTIVE.**

The Second Corrected Reply Expert Report candidly acknowledges that it does not rely on any “scientific analysis” for the conclusion that programmatic niches will be successful where other program-based remedies have failed. Exh. 9, at ¶ 204.<sup>10</sup> “In social science research, it is important not to make claims unsupported by evidence. Yet these kinds of conclusions and claims are made repeatedly throughout the Expert Report, even though the authors acknowledge that there is no evidence to support them.” Exh. 4, at ¶ 51 (Bastedo Dec.). Conrad & Allen do not present evidence “to support the assertion that programmatic niches built around distinctive institutional identities will have particularly desegregating effects.” *Id.* ¶ 50; Exh. 3, at ¶ 24 (Lichtman Dec.). Instead, Conrad & Allen invoke the vague authority of “our research and expertise.” *Id.* But that is the very kind of expert *ipse dixit* that this Court, echoing the Supreme Court, has recognized does not pass muster. *See, e.g., Baltimore Aircoil Co., Inc. v. SPX Cooling Tech. Inc.*, 2016 WL 4426681, \* 27 (D. Md. Aug. 22, 2016) (“This *ipse dixit* justification is the hallmark of an unreliable methodology,” citing *General Electric*). “The court

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<sup>10</sup> Plaintiffs have stated: “The remedial plans implemented in most of the other higher education desegregation cases have largely failed to desegregate the HBIs.” ECF 406-1 at 23; “By this measure [enrollment], the remedial plan in Alabama failed to produce meaningful white enrollment at two HBIs.” *Id.* at 26; “the *Ayers* litigation did little to achieve long-term desegregation at the state’s HBIs.” *Id.* at 29; “The HBIs in Louisiana remain racially identifiable.” *Id.* at 34.

cannot admit [Conrad & Allen’s] testimony based on [their] unsupported statement that [they are] relying on [their] ‘experience’ when [they have] not established what that experience is and how it would assist [them] to form [their] opinion.” *Nat’l Union Fire Ins. Co. v. Porter Hayden Co.*, 2014 WL 1323049, \* 2 (D. Md. March 31, 2014).

**B. THE METHODOLOGY SUPPORTING THE OPINIONS ABOUT “HIGH-DEMAND” AND “UNIQUE” PROGRAMS IS UNSOUND AND UNRELIABLE.**

Plaintiffs also present no reliable basis in social science research for testimony that creating or transferring “high-demand” or “unique” programs will increase white enrollment at the HBIs.<sup>11</sup> Conrad & Allen acknowledge that the “lesson learned” from experiences in other states that have implemented such remedial orders is that “the random addition of new programs—even if unique or high demand—are unlikely to lead to meaningful increases in white enrollment where those programs are *not* clustered within . . . programmatic niche[s] . . .” Exh. 9, at ¶ 209; *see also* ECF 406-1, at 23-36 (reviewing failures of other remedial plans); *id.* at 24-25 (table showing 2013 white enrollments at HBIs in Alabama, Louisiana and Mississippi ranging from a high of 6% to a low of 2.2%).<sup>12</sup> Thus, the data from real-world experimentation with “high-demand”

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<sup>11</sup> The term “high-demand” in this memorandum refers to both categories in the Reply Report, “high-demand” and “extremely high-demand,” because the methodological problems are identical.

<sup>12</sup> As Dr. Lichtman’s first report regarding remedy shows, the claim that the remedial strategy in Tennessee had greater success is not accurate. ECF 448-5, at 45-50. Drs. Conrad & Allen had the opportunity to respond to Dr. Lichtman’s analysis of the Tennessee data in their Second Corrected Report, but chose not to do so. *See* ¶¶ 289-335 (responding to Dr. Lichtman’s report). Likewise, Conrad & Allen do not respond to Dr. Lichtman’s criticism of Plaintiffs’ reliance on a set of 20 cherry-picked programs from other HBIs. ECF 448-5, at 50-55 (refuting ECF 406-2, at 28).

and “unique” programs do not support the prediction that putting more of them at HBIs will increase white enrollment. Dr. Conrad admitted in his deposition that he could not identify an HBI that had achieved what he considered to be a “distinctive identity” as a result of a court order: “It’s been a Sisyphean quest in many ways.” Exh. 2, at 380-81 (Conrad Dep.). In other words, by Plaintiffs’ own admission, it is only the purely speculative feature of creating “programmatically niches” that transmutes the dross of “unique” or “high-demand” programs into remedial gold.

The Second Corrected Reply Expert Report states that Dr. Conrad “has determined that high-demand programs are the single most important factor in terms of influencing student choice” from his “research and review of data from a number of states.” Exh. 9, at ¶ 177.<sup>13</sup> Likewise, it asserts that “Dr. Conrad’s research shows that unique programs are also very important.” ¶ 178. *See also* ¶¶ 47, 81. Those are claims about *causation*, which means that the social science methods underlying the research must be appropriate to support causal inferences of sufficient generality that they apply to Maryland HBIs in 2016 and for decades thereafter. Exh. 4, at ¶¶ 11-12 (Bastedo Dec.). However, the report identifies no such research or data; far less does it present a methodologically sound link between the data and the conclusion. The report later cites a recent book co-authored by Dr. Conrad addressing minority-serving institutions in general, but only as support for a list of other factors influencing student choice which are not addressed in Plaintiffs’

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<sup>13</sup> Paragraph 263 refers to analyses of desegregation plans in other states submitted in plaintiffs’ legal memorandum in support of the remedial proposal. *See also* ¶ 102. However, the report identifies nothing in those analyses that supports any conclusion in the report.

remedial proposal—not as support for the view that more “high-demand” or “unique” programs will increase white enrollment at HBIs. ¶ 180 & n.29.

In his deposition, Dr. Conrad identified three “foundations” for his opinions regarding “high-demand” and “unique” programs: (1) a 1994 study he conducted as an expert retained by the Department of Justice and later published; (2) an unspecified number of undocumented “non-random” conversations with people during visits at HBIs; and (3) the quantitative analysis presented in the 2016 Reply Report. Exh. 2, at 88-93 (Conrad Dep.). As discussed more fully below, neither (1) the 1994 study, nor (3) the 2016 quantitative analysis, uses valid and reliable social science methods to infer causation. No social scientist would rely on Dr. Conrad’s various undocumented conversations (2) as the basis for drawing reliable conclusions, and so this motion will not further address them. Exh. 3, at ¶ 34 (Lichtman Dec.).

### **1. The 1994 DOJ “Factors” Study**

Dr. Conrad’s 1997 article entitled “Factors Contributing to the Matriculation of White Students in Public HBCUs” (Exh. 6) is a shorter version of a report he prepared and submitted to the district court in *Knight v. Alabama*, in 1994, while serving as a retained expert witness for the Department of Justice. Exh. 5, (Clifton F. Conrad, Report in *Knight v. Alabama* (1994)). The report and article make general policy recommendations on the basis of causal inferences, and although they do not cite the 1994 study, Conrad & Allen apparently relied on it as one of the bases for drawing causal inferences and making policy recommendations about Maryland HBIs in 2016 in the

Reply Report. Accordingly, the applicable social science methodological standards are those for drawing such general causal inferences, whether the results of the study are primarily reported in numerical (quantitative) or narrative (qualitative) terms.

As Professor Bastedo explains in his declaration, some qualitative studies, including some “case studies” and studies applying “grounded theory,” are intended to describe a phenomenon of interest, including to portray the subjective perspective of participants. Exh. 4, at ¶ 10 (Bastedo Dec.). Social scientists do not draw general policy prescriptions from such a study, even if it qualifies as a methodologically-appropriate phenomenological or descriptive work, because such methods are not a foundation for causation-based predictions applicable to other “cases” in different places at different times. J. Douglas Toma explains in his chapter *Approaching Rigor in Applied Qualitative Research*, in *The SAGE Handbook for Research in Education*, edited by Dr. Conrad and Ronald C. Serlin (2d ed. 2011), that “[f]indings are *externally valid*—or generalizable—when they extend to certain individuals and settings beyond those immediately studied.” Exh. 29, at 263, 270.

Case studies like Dr. Conrad’s 1994 “Factors” study are not readily applied, or “generalized” to other settings. Exh. 4, at ¶ 22 (Bastedo Dec.); Exh. 3, at ¶ 29 (Lichtman Dec.). As an authority in the field – recognized by Dr. Conrad, Exh. 2, at 26 (Conrad Dep.) – put it, “the intent of this form of inquiry [qualitative research] is not to generalize findings to individuals, sites or places outside of those under study.” Exh. 16, at 203 (John W. Creswell, *Qualitative Methods, Research Design: Qualitative, Quantitative and Mixed Methods Approaches* (4th ed. 2014)) (*Research Design*); see also Exh. 25, at 243,

260 (Joseph A. Maxwell, *Using Qualitative Methods for Causal Explanation*, Field Methods (2004)); Exh. 28, at 353-54 (William R. Shadish, Thomas D. Cook, & William T. Campbell, *Experimental and Quasi-experimental Designs for Generalized Causal Inference* (Wadsworth Cengage Learning 2002)) (principles governing generalization of quantitative research); *id.* at 389-90 (qualitative research used to *explain* causation rather than to *establish* causation); *id.* at 501 (explaining that although in rare circumstances qualitative studies can reduce uncertainty about causation, “qualitative methods usually produce unclear knowledge about the counterfactual of greatest importance”).

Simply put, the difficulty in generalizing “is seen by the traditional canons as a weakness in the [qualitative study] approach.” Exh. 24, at 262 (Catherine Marshall & Gretchen B. Rossman, *Designing Qualitative Research* (6<sup>th</sup> ed. 2016)). Its strength is in the detailed characterization of particular instances, not generalization. Exh. 16, at 203-04 (*Research Design*).

As Professor Yin notes, the limited “generalizability” of case studies in social science parallels physical science: “in fact, generalizations in science are rarely based on single experiments; they are usually based on a multiple set of experiments that have replicated the same phenomenon under different conditions.” Exh. 30, at 20 (Robert K. Yin, *Case Study Research Design and Methods* (5<sup>th</sup> ed. 2014)) (*Case Study Research*).<sup>14</sup> Creswell agrees that the key to generalization is replication. Exh. 16, at 204 (*Research*

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<sup>14</sup> Yin also suggests that a case study may be generalized if it addresses an already recognized theory. *Id.* But Dr. Conrad admits that the applicable theory about college choice in general is not consistent with his study with regard to the importance of high-demand and unique programs.

*Design*); see also Exh. 19, at 272-73 (John Gerring, *Social Science Methodology* (2d ed. 2012)) (experiments can be generalized if standardized and replicated). By Dr. Conrad's own assessment, the 1994 study of 36 students and 44 HBI faculty and administrators cannot be "overgeneraliz[ed]," even as to southern HBIs in 1994, much less to make predictions about Maryland HBIs in 2016. Exh. 2, at 143 (Conrad Dep.). Although Dr. Conrad welcomed the idea of replication, no one has replicated his 1994 study in the intervening 22 years, and Plaintiffs have not asked him to do so in the eight years he has been involved in this case. Exh. 2, at 202-03 (Conrad Dep.).

Even if the 1994 study could otherwise be generalized to present-day Maryland, it is deeply flawed in design and execution and cannot serve as the basis for admissible expert testimony. In his November 2015 report, Dr. Lichtman identified five methodological flaws in Dr. Conrad's 1994 study: an inadequate sample, a non-random sample, sampling from the wrong population, interviewer bias, and sampling error in ranking factors. ECF 448-5, at 56-58. Those flaws can be distilled to two: a failure to address and counteract interviewer bias, and a flawed design producing systemic bias (failing to collect the right data). As Dr. Lichtman notes in his second report, Conrad & Allen's Reply Report offers no response to this earlier methodological critique. Attachment to Exh. 3, at 2 (Lichtman Dec. Attachment – Second Expert Report, dated August 30, 2016).

In brief, Dr. Conrad's 1994 study consisted of three phases: (1) preliminary telephone interviews with eight administrators (not students) from seven HBIs (overlapping with but not identical to the HBIs later used in on-site interviews) (Exh. 5,



at 3-4 (Clifton F. Conrad, Report in *Knight v. Alabama* (1994)); (2) interviews of administrators, faculty and students at two North Carolina HBIs (*Id.* at 6); (3) further interviews using a written survey instrument of administrators, faculty and students at three more HBIs (*Id.* at 6-7); Exh. 2, at 115-16 (Conrad Dep.)). In all, Conrad interviewed 80 people, only 36 of whom were students. (Exh. 5, at 2 (Clifton F. Conrad, Report in *Knight v. Alabama* (1994)). All but one of the students was a white student who had enrolled at an HBI. Dr. Conrad interviewed different numbers and proportions of administrators, faculty and students at different institutions. At least two-thirds of the students to whom Dr. Conrad spoke had been chosen by “institutional liaisons” from the HBI administration, and nearly half (44%) of the students were from a single school where Dr. Conrad had a pre-existing relationship with the institutional liaison, but which had been the least successful in attracting white students. Exh. 2, at 117, 157, 161, 163 (Conrad Dep.).

The proportion of graduate and undergraduate students interviewed is unknown, Exh. 2, at 164, 182 (Conrad Dep.), and although Dr. Conrad now asserts that he made an effort to speak to students enrolled in different programs, the study does not record what if anything he did in that regard. Exh. 2, at 176 (Conrad Dep.). He did not “weight” the number of students in proportion to the HBI’s overall enrollment or its white enrollment. (For example, he interviewed 9 people at North Carolina A&T, but 29 people at the smaller Southern University at New Orleans (SUNO)). Exh. 5, at 13-15 (Clifton F. Conrad, Report in *Knight v. Alabama* (1994)). Dr. Conrad interviewed no students who had chosen *not* to enroll at an HBI about their reasons or what might have influenced

them to attend. Exh. 2, at 129 (Conrad Dep.). Nor did he interview any high school or community college students facing a choice of college about what factors mattered to them in deciding whether to enroll at an HBI. Exh. 2, at 213 (Conrad Dep.) (discussing more recent study and describing such interviews as a strength).

Dr. Conrad explained to each of the interviewees that he was working for the Department of Justice on lawsuits for the purpose of desegregating the HBIs. Exh. 2, at 194-95 (Conrad Dep.); Exh. 5, at App B (Clifton F. Conrad, Report in *Knight v. Alabama* (1994)). To decide which factors were deemed important, Dr. Conrad created an arbitrary rule that he would count a “factor” (such as high-demand programs) in his report if it was mentioned by 60% of the students and 40% of the faculty and administrators he interviewed. Exh. 2, at 136 (Conrad Dep.) (no basis in academic literature). He handed “most interviewees” at the last three HBIs (27/36 students) a survey form that listed “high-demand” and “unique” programs as the first two factors he wanted to know about. Exh. 2, at 190, 197 (Conrad Dep.), Exh. 5, at App. C (Clifton F. Conrad, Report in *Knight v. Alabama* (1994)). He had some of the interviews transcribed, but for the most part relied on his own notes to classify the responses. Exh. 2, at 186-87 (Conrad Dep.). He listed “factors” in order of predominance, but did not report any numerical results. Nor did he break down the results by school, or between students and faculty or administrators.

In 1997, Dr. Conrad published a paper nearly identical to his expert report (but without the explanatory appendices) in a special issue of the *Journal for a Just and Caring Education* (“JJCE”). Exh. 6 (Clifton F. Conrad, *Journal for a Just and Caring*

Education, *Factors Contributing to the Matriculation of White Students in Public HBCUs* (1997)). He has no specific recollection whether the article was peer-reviewed. Exh. 2, at 120 (Conrad Dep.). Dr. Conrad testified that he “would call this a very preliminary set of observations study,” (Conrad Dep. 200) that one would have to be careful about generalizing from. Exh. 2, at 201 (Conrad Dep.). His own article described the study as “only a first step.” Exh. 6, at 42 (JJCE)).

**a. *The Recognized Problem of Experimenter/Interviewer Bias***

As Dr. Lichtman explains, social science research must account for and attempt to counteract the effects of unconscious as well as conscious bias. ECF 448-5 at 57; Exh. 23, at 71-94 (Geoffrey Marczyk, David DeMatteo & David Festinger, *Essentials of Research Design and Methodology* (John Wiley & Sons 2005)) (*Essentials*); *id.* at 149 (noting that case studies are “at greater risk” of experimenter bias). Bias toward a result that an investigator expects to find is known as “confirmation bias.” The different ways in which confirmation bias can influence research are described in Raymond Nickerson’s often-cited article, Exh. 26, at 175-220 (Raymond Nickerson, *Confirmation Bias: A Ubiquitous Phenomenon in Many Guises*, 2 Rev. of Gen. Psych. (1998)) (*Confirmation Bias*). Such bias can take the form of subtle influences on an interview subject that reinforce or prompt certain responses. ECF 448-5 at 57. In group interviews (or focus groups) conducted with students, subtle cues can also lead other participants to echo or endorse desired responses once given by a member of the group. *See id.* Bias can also influence the investigator’s coding or classification of interview responses. Exh. 26, at

181-82 (*Confirmation Bias*) (discussing tendency to perceive and recall the expected). Dr. Conrad agreed that “one of the single most important things” is to avoid confirmation bias—a researcher’s conscious or unconscious propensity to favor results that the researcher wants or expects to find. Exh. 2, at 41 (Conrad Dep.). He described it as “anti-method” to enter into an analysis with a predetermined view of how it should come out. *Id.* at 50.

Dr. Conrad claimed (as he had in his 2010 deposition in this case) that he had no preconceptions and was surprised at his 1994 findings about “high-demand” and “unique” programs. Exh. 2, at 99 (Conrad Dep.). He testified that he did come into the 1994 study with a “predilection” based on prior studies of college choice that the institution’s *quality* mattered (*Id.* at 101, 102), but he specifically denied having a preconception that students would choose HBIs for “unique” and “high-demand” programs because that was contrary to the college choice literature. *Id.* at 97, 101. However, in 1990 – four years before beginning his research – Dr. Conrad had already “testified that the failure of [two Alabama HBIs] to have any unique high demand programs adversely impacted their ability to attract and enroll white students.” *Knight v. Alabama*, 787 F. Supp. 1030, 1316 (N.D. Ala. 1991). Indeed, Dr. Conrad in his 1990 expert report in *Knight* offered much the same assessment of the role of “unique” “high demand” programs in creating “meaningful distinctions” that will draw white students to HBIs as in the 2016 Reply Report in this case. Exh. 7 (Clifton F. Conrad, *Study of Academic Programs in Alabama’s Colleges and Universities: A report to the U.S. Department of Justice* (Oct. 1, 1990)). Meaningful distinctions, Dr. Conrad explained,

required “the conspicuous presence of a significant number of unique non-core, high-demand programs in both sets of institutions.” *Id.* at 41. Thus, Dr. Conrad had actually *testified* on behalf of DOJ in 1990 before he had done any research to the conclusion he claimed to have been investigating with an open mind in 1994 – thus violating the ‘single most important rule’ in his field. Exh. 2, at 50 (Conrad Dep.).

Not surprisingly, the initial phase of Dr. Conrad’s research—telephone interviews with HBI administrators—likely reinforced a predisposition consistent with his prior testimony. As Dr. Conrad acknowledged in his deposition, university administrators and faculty “are always searching to enrich and enlarge their mission and funding and program offerings,” and so tend to favor solutions that involve new programs. Exh. 2, at 168 (Conrad Dep.). He agreed that it was possible that administrators would have thought that telling DOJ’s emissary that the route to higher white enrollment was new programs would be a way to enrich their institutional offerings. (*Id.* at 194-95). Dr. Conrad testified in the *Fordice* remand that the HBI administrators he spoke to in his preliminary round of telephone interviews considered “the most important thing that those universities possessed [to be] high demand, unique programs,” which those administrators claimed were “drawing significant numbers of white students as a result.” Exh. 8, at 10438 (Fordice 7/11/1994 PM Trial Transcript); Exh. 5, at 8 (Clifton F. Conrad, Report in *Knight v. Alabama* (1994)) (describing preliminary survey of administrators: “the single most important factor influencing white student enrollment at their institution is the presence of program offerings in high demand fields”); *id.* at 10 (preliminary survey results concerning unique programs).

Aspects of Dr. Conrad's study design also actually increased rather than mitigated the risks of confirmation bias. For example, at least two-thirds of the students Conrad interviewed were not only volunteers, but also were selected for him by "institutional liaisons"—HBI administrators who were aware of the purpose of the study and (as Conrad acknowledged) may have had an institutional interest in new programs as a remedy. Exh. 2, at 168, 194-95 (Conrad Dep.). The institutional liaisons were "crucial." *Id.* at 163. Thus, the sample was not only non-random, but was chosen in a way that introduced a systemic bias in the direction of the importance of factors related to new programs. Another methodological flaw increasing the risk of confirmation bias was Dr. Conrad's use of a survey instrument handed to "most interviewees" at the three HBIs he visited after developing the instrument which listed "high-demand" and "unique" programs as the first and second factors, Exh. 5, at App. C (Clifton F. Conrad, Report in *Knight v. Alabama* (1994)), even though he acknowledged that primacy effects—the response of an interview subject to the first items on the survey form—was "something one needs to be mindful of." Exh. 2, at 190-91, 197 (Conrad Dep.). *See* Exh. 26, at 187 (*Confirmation Bias*) (discussing primacy effect). Combined with Dr. Conrad's explanation to interviewees that his interview's goal was to assist the Department of Justice in advancing desegregation through litigation, the survey instrument was a strong cue to college students (mostly selected by a school administrator) about the interviewer's desired or expected response. Thus, the survey instrument tended to prompt responses in line with Dr. Conrad's prior testimony and with his preliminary interviews with HBI administrators. Exh. 4, at ¶ 28-29 (Bastedo Dec.).

The effects of “confirmation bias” on social science research do not depend on a researcher’s intentionally skewing the data to support a favored or expected result. The key methodological issue is whether the researcher takes the steps required to control or combat the effects of confirmation bias, whether conscious or unconscious. Exh. 3, at ¶ 30 (Lichtman Dec.) (listing steps). In his deposition, Dr. Conrad agreed to a number of precautions good social science researchers use to counteract confirmation bias. However, Dr. Conrad did not use those techniques to guard against confirmation bias in his 1994 study.

**i. Dr. Conrad Failed to Search for “Negative Cases.”**

Probably the most important safeguard against confirmation bias is the consideration of “negative cases” that have the potential to falsify or contradict the association the investigator expects to find, revealing *alternative explanations*. Exh. 2, at 50-51 (Conrad Dep.); Exh. 16, at 202 (*Research Design*); Exh. 15, at 101 (Juliet Corbin & Anselm Strauss, *Basics of Qualitative Research: Techniques and Procedures for Developing Grounded Theory* (4th ed. 2015)); Exh. 21, at 73, 77 (Yvonna S. Lincoln & Egon G. Guba, *But Is It Rigorous? Trustworthiness and Authenticity in Naturalistic Evaluation*, *New Directions for Program Evaluation* (1986) (*But Is It Rigorous?*)); Exh. 30, at 203-04 (*Case Study Research*); Exh. 17, at 124, 127 (John W. Creswell & Dana L. Miller, *Determining Validity in Quantitative Inquiry*, *Theory Into Practice* (2000)) (*Determining Validity*). Looking for negative cases requires (and reflects) a willingness to collect data that may disprove or cast doubt on the result the investigator expects or

wants to find. For example, discovering that an HBI that is *unsuccessful* at attracting white students is offering “high-demand” and “unique” programs undercuts the inference that the existence of such programs at a “successful” HBI caused that institution’s success. A researcher who is aware of and seeking to counteract bias would look for such negative or “disconfirming” cases to test the association he or she expects to see. Exh. 4, at ¶ 15 (Bastedo Dec.); Exh. 3, at ¶ 33 (Lichtman Dec.). But Dr. Conrad utterly failed to do this. Exh. 4, at ¶ 20 (Bastedo Dec.) (“the authors did not provide any disconfirming or discrepant evidence that did not support their overarching themes and conclusions. Most important, the authors did not provide sufficient evidence to allow readers to consider rival theories or explanations.”).

Dr. Conrad’s paper stated that he chose the HBIs he visited for his 1994 study because they were all successful in attracting white students (Exh. 5, at 5 (Clifton F. Conrad, Report in *Knight v. Alabama* (1994)); Exh. 6, at 39 (JJCE), but in reality two of the institutions were much less successful, and one of those less successful institutions, SUNO, was the source of nearly half of his student interviews.<sup>15</sup> Exh. 5, at Table 3 of App. A (Clifton F. Conrad, Report in *Knight v. Alabama* (1994)) (SUNO – 4% white enrollment). However, instead of comparing SUNO with more successful institutions as a kind of negative case, Dr. Conrad reported the results from all of the HBIs together, as

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<sup>15</sup> Although he stated (Exh. 5, at 15 (Clifton F. Conrad, Report in *Knight v. Alabama* (1994)) that he interviewed 15 students at SUNO, Dr. Conrad also reported that “all 10 students I interviewed at [SUNO] said that offering attractive high-demand programs is the major factor in attracting whites to PBIs.” *Id.* at 21. SUNO students thus seemingly provided solid support for an association between enrollment and programs, despite low white enrollment at the school.



if all of the responses described successful strategies for attracting white students. Exh. 4, at ¶¶ 23-24 (Bastedo Dec.).

Dr. Conrad also acknowledged that he did not investigate the “negative case” of students who chose *not* to enroll in an HBI to ascertain what might have influenced those students to make a different decision—the question central to what would *increase* white enrollment. Exh. 2, at 129, 133-35, 141-42 (Conrad Dep.); Exh. 4, at ¶ 25 (Bastedo Dec.); Exh. 3, at ¶ 31 (Lichtman Dec.). Dr. Conrad acknowledged at the time that “[s]uch interviews may have yielded additional insights, and we encourage other researchers to interview high school seniors and others who are invested in choosing a college or university.” Exh. 6, at 42 (JJCE). Yet the Reply Report did not address the negative case of a study (cited by Professor Lichtman in his November 2015 report) that reached different conclusions from Dr. Conrad’s 1994 study on the basis of interviews of high school and community college students. Exh. 2, at 206-226 (Conrad Dep.).

**ii. *The Use of a Single Interviewer  
Increased The Likelihood of Bias***

Another way to counteract confirmation bias in data collection is to have more than one person who may have a particular bias conduct interviews. Exh. 2, at 44 (Conrad Dep.) (describing practice in prior study he conducted). That allows a researcher to compare responses to different interviewers to assess whether the interview process may be shaping the responses in some subtle way. However, Dr. Conrad chose himself to conduct all of the interviews.

*iii. Similarly, the Use of a Single Interview Coder Contributed to Bias*

Bias can be reflected not just at the interview stage, but when “coding” or classifying the transcripts of interview responses. Unlike a multiple choice survey instrument which calls for a selection among predetermined responses, the kind of interviewing Dr. Conrad conducted, requires the coding or classification of responses which may use different words to express similar ideas. Coding free-form interviews is necessarily highly subjective.

This kind of bias can be counteracted by having more than one person code or classify responses. Exh. 2, at 66-68 (Conrad Dep.); Exh. 16, at 203 (*Research Design*). Researchers often have more than one person code an interview to minimize subjectivity, as Dr. Allen did in a recent large educational study. Exh. 1, at 63-68, 95 (Allen Dep.). By contrast, Dr. Conrad did all of the coding himself. Moreover, he did not even have all of his interviews transcribed, so there was no way for anyone else to check his coding. (And the raw data is now long gone – he disposed of it in about 1998, while he was still working for DOJ as an expert witness. Exh. 2, at 139 (Conrad Dep.)). Unconscious confirmation bias would likely lead Dr. Conrad to pay greater attention to responses that met his expectation that unique and high-demand programs were important and to disproportionately record such responses in the “Cliff notes” (Exh. 2, at 188 (Conrad Dep.)) he chiefly relied on to record interviews in lieu of a transcript, and which he then used to report the relative importance of different factors. Exh. 4, at ¶ 29 (Bastedo Dec.).

Professor Bastedo found it “unnerving” that Dr. Conrad reported findings in his

report for DOJ before he had completed the transcription of the interviews (which apparently never happened). *Id.* at ¶ 34. The failure to transcribe all of the interviews “is not consistent with standards of rigor in qualitative research.” *Id.* ¶ 21. As Professor Seidman writes in his authoritative guide to interviewing, which advocates recording and *transcribing* interviews, “[t]o substitute the researchers’ paraphrasing or summaries of what the participants say is to substitute the researcher’s consciousness for that of the participant.” Exh. 27, at 114 (Irving Seidman, *Interviewing as Qualitative Research: A Guide for Researchers in Education and the Social Sciences* (3d ed. 2006)).

**iv. *The Failure to Use “Member Checking”***

Another way to guard against erroneous coding of responses is to “check” the researcher’s interpretation with the interview subject. Exh. 2, at 60 (Conrad Dep.). Dr. Allen agreed that it is an accepted method of guarding against bias. Exh. 1, at 75-76 (Allen Dep.) (referring to “informant checking”); Exh. 16, at 201 (*Research Design*); Exh. 21, at 77 (*But Is It Rigorous?*); Exh. 17, at 127 (*Determining Validity*); Exh. 4, at ¶¶ 17, 20 (Bastedo Dec.). Again, Dr. Conrad did not check his interpretations with his informants.

**v. *The Lack of an Independent Audit***

Another way to guard against confirmation bias is for a third party to review the data and protocols. Exh. 2, at 59-60 (Conrad Dep.); Exh. 16, at 202-03 (*Research Design*); Exh. 21, at 77 (*But Is It Rigorous?*); Exh. 17, at 128 (*Determining Validity*). Peer review is one ordinary means by which social scientists subject their work to

external audit. *Id.* at 124. Although the *Journal of a Just and Caring Education* generally called for peer review, Dr. Conrad's article was published in a special issue with outside editors. He had no specific recollection of peer review of the article, and Professor Bastedo considers it unlikely that the special issue submissions were peer-reviewed. Exh. 4, at ¶ 32 (Bastedo Dec.).

**vi. *The Failure to Seek Independent Sources, or to "Triangulate"***

As Dr. Allen explained in his deposition, triangulation refers to "relying on different methodological approaches and then reconciling, if you will, the information provided by the different approaches." Exh. 1, at 75 (Allen Dep.). The main purpose of triangulation is to increase the reliability of the study, rather than to correct directly for bias. *Id.* at 76; Exh. 16, at 201 (*Research Design*). But triangulation can also counteract unconscious bias that may have influenced the collection of data by adding independent contrary or confirmatory evidence through unbiased procedures. Dr. Conrad relied solely on interviews, and collected no documents (for example) shedding light on HBI marketing efforts, financial aid, demand for programs, the uniqueness of programs, or other facts that might have exposed differences between the factors subjectively identified as important by interviewees, and the actual objective strategies for recruiting white students employed at the various HBIs. Exh. 2, at 162 (Conrad Dep.). He also lumped together the results from all of the HBIs, precluding any limited "triangulation" in the form of comparisons of results among different "cases" by treating the data as a single case. Exh. 4, at ¶ 26 (Bastedo Dec.).

Although the study included interviews with faculty and administrators as well as students, the fact that most of the interview subjects were chosen by institutional liaisons familiar with the objectives of the study minimized both the independence of the groups of subjects, and the likelihood that this form of triangulation would remedy confirmation bias. *Id.* at ¶ 19 (noting problems with the selection of the interview subjects); Exh. 3, ¶ 32 (Lichtman Dec.). As Maxwell explains, “triangulation does not automatically increase validity,” for example when the data sources “may have the same biases and thus provide only a false sense of security.” Exh. 25, at 259 (Joseph A. Maxwell, *Using Qualitative Methods for Causal Explanation*, 16 *Field Methods* (2004)).

***b. The Design of the 1994 Study Was Also Inherently Flawed.***

As Dr. Lichtman explains, not only the conduct but the basic design of the 1994 study was systematically flawed. ECF 448-5 at 57. To begin with, it looked only at white students who *did* enroll in HBIs, not those who *didn't*. That is called “sampling from the wrong population,” or (in quantitative terms) “sampling on the dependent variable.”<sup>16</sup> *Id.* A social scientist may not properly draw an inference that certain factors caused the white students who did enroll to do so, without looking to see whether the same factors were at work for students who did not enroll. The outcome (enrollment in an HBI or not) is the dependent variable the study seeks to explain. Studying only students who did enroll violates the “basic and obvious rule: *selection should allow for*

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<sup>16</sup> When testing a hypothesis, the dependent variables represent the output or outcome whose variation is being studied (*e.g.*, white enrollment). The independent variables represent inputs or causes, *i.e.* potential reasons for variation (*e.g.*, high-demand programs).

*the possibility of at least some variation on the dependent variable.*” Exh. 20, at 129 (Gary King, Robert O. Keohane & Sidney Verba, *Designing Social Inquiry: Scientific Inference in Qualitative Research* (Princeton Univ. Press 1994)).<sup>17</sup> “When observations are selected on the basis of a particular value of the dependent variable, nothing whatsoever can be learned about the causes of the dependent variable without taking into account other instances when the dependent variable takes on other values.” *Id.* That is, even if a researcher wants to focus data collection on cases that illustrate a particular outcome within a range, a control group reflecting other outcomes is required to draw causal inferences. *Id.* at 134. Studying only cases where a particular outcome occurred (but not cases where the outcome did not occur) precludes a researcher from drawing the inference that certain factors caused the outcome. Exh. 4, at ¶¶ 24-25 (Bastedo Dec.).

Dr. Conrad acknowledged at his deposition that he has never interviewed students who chose not to attend HBIs about the factors influencing their decisions. Exh. 2, at 124, 129, 141 (Conrad Dep.). Nor, unlike the authors of a more recent study which

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<sup>17</sup> *Designing Social Inquiry* (DSI) has been described as “one of the most influential statements ever published on the relationship between quantitative and qualitative methods.” Exh. 11, at 5 (Henry E. Brady, David Collier & Jason Seawright, *Refocusing the Discussion of Methodology, Rethinking Social Inquiry: Diverse Tools, Shared Standards* (Henry E. Brady & David Collier eds. 2004)). DSI’s major conclusions, including the need to avoid selection bias in qualitative as well as quantitative studies of causation, are widely accepted. Exh. 12, at 39 (David Collier, Jason Seawright & Gerardo L. Munck, *The Quest for Standards; King, Keohane, and Verba’s Designing Social Inquiry* (Henry E. Brady & David Collier eds. 2004)); Exh. 14, at 85-102 (David Collier, James Mahoney & Jason Seawright, *Claiming Too Much: Warnings About Selection Bias, Rethinking Social Inquiry: Diverse Tools, Shared Standards* (Henry E. Brady & David Collier eds. 2004)); Exh. 19, at 240 (John Gerring, *Social Science Methodology: A Unified Framework* (2d ed. 2012)).

reached different conclusions, did he interview high school or community college students who were considering where to attend college, which Dr. Conrad agreed could be helpful. Exh. 2, at 130, 133 (Conrad Dep.). He agreed that studying only students who did enroll in HBIs would not “tell you about whether or not additional students from that no—N-O—group and how they might be influenced.” *Id.* at 134. Even if after-the-fact explanations of white students who enrolled at HBIs were a reliable indicator of causal influences on them, no conclusions can be drawn without considering whether those same influences were at work on students who made different decisions.<sup>18</sup> If so, a researcher would have to investigate alternative explanations.

A second design flaw is that the 1994 study did not compare “cases” with different results in terms of the key outcome—white enrollment. For example, Dr. Conrad could have studied the differences in white enrollment at different HBIs and tried to correlate those differences to the presence or absence of various factors. That would have allowed him to study the different levels of white enrollment at various HBIs as different values of the dependent variable, allowing for some degree of variation associated with the explanatory variables, or factors. But instead, he lumped together all of the responses from students, faculty and administrators at different schools, so that it is impossible to say whether (for example) the interviewees who most strongly reported the influence of “unique” and “high-demand” programs were predominantly from the schools with the lowest white enrollments, which would cast further doubt on the prediction that adding

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<sup>18</sup> Dr. Conrad acknowledged that he did not consider the known literature on the reliability of such retrospective explanations of choices already made. Exh. 2, at 166 (Conrad Dep.).

such programs would actually have the predicted effect.<sup>19</sup> Dr. Conrad treated all of the institutions as a single “case,” but one in which the interview subjects selected were not representative of the relevant populations of the case as a whole.

The study design was also flawed because the sample of students was unrepresentative. The number of interviews at each school had nothing to do with the relative overall or white enrollment at the school. Exh. 2, at 163 (Conrad Dep.). Dr. Conrad did not keep track of the proportion of graduate versus undergraduate students. *Id.* at 164. And although he said he made some effort to interview students from different programs, nothing in the study design would have prevented the institution-chosen students from disproportionately reflecting a small number, or even a single program. *Id.* at 173, 176. Dr. Conrad agreed that he was not claiming “I have a representative, much less a random sample.” *Id.* at 173.

Even the sample of institutions is unrepresentative. Dr. Conrad’s report stated that he had chosen two of the five HBIs because “they have been under court order to desegregate—and as such could provide case studies that yielded valuable insight—and, secondarily because they have experienced some successes in attracting other race students.” He chose the other three schools “on the grounds that they have been relatively successful in attracting white students.” Exh. 5, at 5 (Clifton F. Conrad, Report in *Knight v. Alabama* (1994)). Although Dr. Conrad recently testified that he intentionally selected a range of schools, from those that had been successful at enrolling

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<sup>19</sup> The fact that a disproportionate number of the interviews (29/80) were conducted at SUNO, which had the lowest white enrollment (4%, as compared to 49% at Kentucky State University) of the five HBIs increases that probability.



white students to those that were not (Exh. 2, at 106 (Conrad Dep.)), that is not the explanation given in the study itself, and Dr. Conrad agreed that the investigator's written justification for the sampling process is important. Exh. 2, at 53 (Conrad Dep.). And, in any event, Dr. Conrad made no use of having selected for a range of enrollment numbers (if, in fact, he actually did so) because he failed to compare the results among the different HBIs. Exh. 4, at ¶ 26 (Bastedo Dec.).

Dr. Lichtman also explains that insofar as the study is actually quantitative in rank-ordering factors, it is statistically invalid because the number of interview subjects is too small in comparison to the number of factors to produce statistically meaningful results. ECF 448-5, at 58.

**2. The 2016 “Quantitative” Analysis of Programs Enrolling More than 10 White Students Was Equally Flawed, If Not More So.**

The Reply Report presents numerical data about white enrollment in Maryland HBIs that Dr. Conrad & Allen say are consistent with their remedial theory. However, both of them characterize the data presented as “descriptive”—Exh. 1, at 101-02 (Allen Dep.); Exh. 2, at 75, 274 (Conrad Dep.)—a social science methodological term that means it is not an appropriate basis for drawing causal (“if/then”) inferences. Exh. 3, at ¶¶ 5-6 (Lichtman Dec.).<sup>20</sup> And the descriptive analysis seems consistent with the remedial theory only because it selectively analyzes data, systematically excluding information that is not consistent with the theory.

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<sup>20</sup> “Descriptive” statistics are used to characterize a set of data. “Inferential” statistics are used to make predictions about other situations. Exh. 10, at 4 (Alan Agresti & Barbara Findlay, *Statistical Methods for the Social Sciences* (4th ed. 2009)).

Dr. Allen explained in his deposition that “it [the Reply Report] was not a research study, but really a task of designing a program that—designing an approach, I should say, that would achieve those ends.” Exh. 1, at 102 (Allen Dep.). Dr. Allen testified that they could not test a remedial plan that had not yet been established. But he did not explain why he and Dr. Conrad could not apply standard, methodologically sound statistical techniques (such as correlation or multiple regression analysis) to the entire body of data they had gathered. Dr. Conrad likewise testified he did not “want to draw causal inferences” from the data. Exh. 2, at 274 (Conrad Dep.). He described the quantitative analysis as “not doing a test per se” (278), and agreed they “didn’t directly test” a hypothesis about the causal relationship between white enrollment and “high-demand” and “unique” programs. Exh. 2, at 243-45 (Conrad Dep.). As he summed it up, “We weren’t testing per se the remedial theory.” *Id.* at 246.

Although it sprawls across 58 single-spaced pages with hundreds of pages of exhibits, Conrad & Allen’s Second Corrected Report actually presents only two quantitative analyses relating “high-demand” and “unique” programs to white enrollment. The *first* analysis looks at (non-“core”) programs at HBIs that enroll more than ten white students (and a subset of such programs enrolling more than fifteen white students) and then works backward to examine the extent to which such programs are “high-demand” or “unique.” Because of that enrollment cut-off, the analysis *does not* consider (and thereby excludes from the analysis) other “high-demand” or “unique” programs that have low white enrollments. The *second* analysis describes the “disproportion” between the number of white students at an HBI who are enrolled in “high-demand” programs and the

number of white students at the institution, but without regard to the *white student percentage* (of the total enrollment) in each program. Thus, that analysis finds a “disproportion” even if the percentage of white students in a “high-demand” program is actually lower than the percentage of white students in the HBI as a whole, lending no support to the theory that “high demand” programs will increase the percentage or proportion of white students at Maryland HBIs.

***a. The 2016 Study Suffers from Equally Deep-Seated Selection Bias.***

Conrad & Allen claim in their Second Corrected Report to have “systematically analyz[ed] which programs are enrolling or graduating meaningful numbers of white students” to “identify the major factors which influence white enrollment.” Exh. 9, at ¶ 85. But rather than use conventional statistical techniques such as multivariate regression, or even bivariate correlations, to measure the association between classifications such as “high demand” and white enrollment, they claim that “[t]his question does not easily lend itself to statistical or quantitative analysis.” *Id.* Instead, they present *ad hoc* collections of data assembled by identifying programs with certain specified levels of white enrollment and then looking at the characteristics of those selected programs. ¶¶ 214-222.

The Second Corrected Report’s assessment of high white enrollment programs at HBIs is a statistically invalid form of *quantitative* analysis based on the logical error of inferring that characteristics of programs with higher-than-average white enrollments *cause* those higher enrollments without having looked at whether programs with lower-

than-average white enrollments also have the same characteristics. (That is, whether similar values of the explanatory variables are associated with very different values of the dependent variable). Exh. 4, at ¶¶ 41-42 (Bastedo Dec.); Exh. 3, at ¶¶ 17-19 (Lichtman Dec.). “Necessarily any inference one draws about the *causes* of such a phenomenon will then be *invalid* because in ignoring cases in which the phenomenon didn’t occur one has omitted from one’s sample instances in which the putative cause might have been present, but *didn’t* generate the phenomenon of interest—an outcome that would falsify the conclusion.” Attachment to Exh. 3, at 49 (Lichtman Dec.) (quoting Dan Kahan, *Don’t Select on the Dependent Variable in Studying the Science Communication Problem*, available from the Yale Law School’s Cultural Cognition Project at <http://www.culturalcognition.net/blog/2013/11/19/dont-select-on-the-dependent-variable-in-studying-the-scienc.html>).<sup>21</sup>

As Barbara Geddes explains in her book, *Paradigms and Sand Castles*, “[t]he adverse effects of selecting cases only for study on the dependent variable stem from the logic of inference.” Exh. 18, at 91 (Barbara Geddes, *Paradigms and Sand Castles: Theory Building and Research Design in Comparative Politics* (Univ. of Mich. Press 2010)) (*Paradigms*). In her example, if we choose to study countries A and B because of some attribute they share:

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<sup>21</sup> As Dr. Lichtman explains, the analysis is also invalid because the number of white students in a program is correlated to the overall enrollment, which is correlated to the categories of “high demand” programs. Thus Conrad & Allen’s method of selecting which programs to analyze assures an association with the variables whose effects they purport to be trying to measure. Attachment to Exh. 3, at 49 n. 28 (Lichtman Dec.).

[i]f one studies only countries A and B, one can collect only part of the information needed, namely the extent of factors *X* through *Z* in countries A and B. Unless one also studies countries C through I (or a sample of them) to make sure they have less of *X* through *Z*, one cannot know whether the factors identified really vary with the outcome under investigation.

*Id.* By the same token, the existence of factors (“high-demand”) and (“unique”) in programs selected on the basis of a characteristic (white enrollment over a threshold value) cannot justify even a weak causal inference unless one also looks at whether those factors exist for programs when the selection characteristic is absent. As Dr. Lichtman demonstrates, there are many, many “high demand” and “unique” programs with low white enrollments. Attachment to Exh. 3, at 22-34 (Lichtman Dec.) (“high-demand”); 34-42 (“unique”); 42-46 (both). That is why only an analysis that takes into account the full range of data is methodologically appropriate. *See* Exh. 18, at 96 (*Paradigms*) (the “appropriate universe of observations on which to test a hypothesis depends on the domain implied by the hypothesis.”). Put in more rigorously mathematical terms, the flaw in limiting analysis to data selected on the dependent variable (the outcome thought to be causally linked to certain factors) “is that the selection mechanism is correlated with the error term in the underlying regression model. If such a correlation exists, causal inferences will be biased.” Exh. 13, at 56, 62 (David Collier & James Mahoney, *Insights and Pitfalls: Selection Bias in Qualitative Research*, World Politics, October (1996)).<sup>22</sup>

Although some researchers doing *qualitative* studies select which “cases” to look at on the basis of outcomes (e.g., was there a revolution in a given country?), the rationale

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<sup>22</sup> To illustrate the point, Dr. Lichtman performed a regression analysis of the full range of data. Attachment to Exh. 3, at 46-48 (Lichtman Dec.).

for making such a selection of the subject for inquiry does not extend to the kind of quantitative data Conrad & Allen present in lieu of an appropriate statistical methodology. *See* Exh. 22, at 227, 239 (James Mahoney & Gary Goertz, *A Tale of Two Cultures: Contrasting Quantitative and Qualitative Research*, Political Analysis (2006)). Mahoney and Goertz conclude that differences in objective account for the difference in methods. “If your goal is to estimate average causal effects for large populations of cases, it makes sense to avoid selecting on the dependent variable.” *Id.* at 240. “But if your goal is to explain outcomes in particular cases, it does not make sense to select cases without regard for their value on the outcome.” *Id.* The “cases” here would be the specific programs with high white enrollment, which Conrad & Allen seek to use to support causal inferences about other and future programs, not just to describe or explain those particular cases. Conrad & Allen have used a method (selecting what to study based on the dependent (outcome) variable) that is inappropriate for drawing general causal inferences.

Another methodological flaw is the failure to examine the association between “high-demand” programs and white enrollment systematically, rather than by picking examples that support their remedial thesis and discarding the rest. *See* ECF 448-5 at 15 (noting cherry-picking of programs). In his initial report, Dr. Lichtman showed that, for all “high-demand” programs, the percentage of white students enrolled was *lower* in those programs than the percentage in programs that were not “high demand” for both graduate and undergraduate programs. ECF 448-5 at 14 & Table 3 at 17; Tables 6 and 7 at 27-31 (enrollment data from all programs classified as high demand), 32-33

(discussing analysis of table data). Plaintiffs make the same kind of methodological error reflected in picking 20 graduate and professional programs from around the country to suggest an association between “high demand” and white enrollment, while ignoring, or failing to analyze, the data from all “high demand” programs (as plaintiffs’ experts define them) at HBIs. The appropriate methodology would be to look systematically at data from HBIs in other states. Exh. 4, at ¶¶ 46-47 (Bastedo Dec.). Such an analysis would have revealed a wealth of negative cases or disconfirming data that Conrad & Allen do not address. *Id.* ¶¶ 47-49 (discussing analysis of national IPEDS data from HBIs).

***b. Construct Validity***

The 2016 analysis is also flawed by its reliance on arbitrary terms and numerical thresholds. “[C]onstruct validity is based on clearly stated and accurate operational definitions of a study’s variables.” Exh. 23, at 190 (*Essentials*). As Professor Lichtman put it: “analysis based on unreliable and invalid variables is meaningless.” Exh. 3, at ¶ 8 (Lichtman Dec.). There is no basis in social science methodology, for example, for focusing on programs with ten (or any other arbitrary number of) white students to assess a causal relationship between some factor and white enrollment. Conrad & Allen seem to have arrived at the particular threshold they used by eyeballing the data using other cut-off values. Exh. 1, at 135-38 (Allen Dep.). In other words, they developed their quantitative analysis by working backwards from what their initial assessment told them appeared to be “meaningful” results. Exh. 1, at 137-38 (Allen Dep.).

Because their quantitative analysis is backward-looking (that is, it begins with setting a threshold for “high” white enrollment, and then looks for causes in the attributes

of such programs), the validity of any association between programs with white enrollment above the threshold and attributes such as “high-demand” and “unique” also depends critically on how the key terms “high-demand” and “unique” are defined. But those terms have no external meaning and are defined in arbitrary and subjective ways. Different definitions of “high-demand” and “unique” would lead to different associations between high white enrollment and “uniqueness” and “high-demand,” even using Conrad & Allen’s flawed analysis.

There is no generally accepted definition of “high-demand” programs. Exh. 2, at 309-11 (Conrad Dep.). Even Dr. Conrad has used different definitions at different times. *Id.* at 319. That means, among other things, that the interviewees in Dr. Conrad’s 1994 study of white enrollment at HBIs that he coded as referring to “high-demand” programs may have contemplated something very different from the way Conrad & Allen use the term in their 2016 report. And even the numerical threshold Conrad & Allen use is malleable. As Dr. Lichtman pointed out in his initial report, although Conrad refers to certain numerical enrollment thresholds, those numbers are not his exclusive basis for classifying a program as “high-demand.” ECF 448-5 at 1 n.2. For example, Dr. Conrad included programs that had no graduates in the relevant year (2014). Exh. 2, at 313-17 (Conrad Dep.). However, Dr. Conrad has not explained or justified those subjective criteria. His classifications “cannot be replicated or tested.” ECF 448-5 at 1.<sup>23</sup>

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<sup>23</sup> Dr. Lichtman nonetheless found numerous errors in the classification of programs. Attachment to Exh. 3, at 6-9 (Lichtman Dec.). Some of those errors “overstat[ed] the relationship between high demand programs at the HBIs and the



Even if the definitions were purely and objectively numerical, however, they would still be arbitrary. The reasons for choosing certain numerical thresholds and the statistical consequences of using different thresholds are not explored. Attachment to Exh. 3, at 10 (Lichtman Dec.). Conrad & Allen also rely on a single year to classify programs as “high-demand,” even though program enrollments and degrees awarded vary considerably from year to year, such that a program considered “high-demand” in one year may not be so classified in the next.<sup>24</sup> Exh. 3, at ¶¶ 9-10 (Lichtman Dec.). That level of instability from year to year necessarily undercuts any conclusion that students chose which university to attend on the basis of having later chosen to major in a “high-demand” program which may not have been “high-demand” when they made the choice to enroll at the institution, and likewise make it impossible to say whether a program classified as “high-demand” in Conrad & Allen’s Second Corrected Report will remain “high-demand” by the time any remedial proposal was carried out, much less through the life of a remedial order.

Conrad & Allen’s definition of “unique” programs is also arbitrary. Conrad & Allen classify a program as “unique” if it is not offered at a non-HBI in the same region, but that definition disregards (a) duplication at a nearby HBI; and (b) duplication at a private university, so it does not reflect the existence or absence of real world competition. Exh. 3, at ¶ 14 (Lichtman Dec.); Attachment to Exh. 3, at 21 (Lichtman Dec.). Their definition has also changed. After separating the Baltimore and College

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percentages and numbers of white students” by excluding certain high demand programs with low white enrollment. *Id.* at 8.

<sup>24</sup> Attachment to Exh. 3, at 10, 12 (Lichtman Dec.) (Table 3 showing variability).

Park regions in their earlier report, Conrad & Allen have merged the Baltimore and College Park/D.C. metro areas for purposes of defining uniqueness, but have retained a category of “proximate uniqueness” for programs that are duplicated within the state, but not at Salisbury. Exh. 1, at 165 (Allen Dep.). That means many UMES programs are counted as “unique.” However, UMES is a “special case.” *Id.* at 144-45 (Allen Dep.). It is a statistical outlier among the Maryland HBIs. The effect of this classification scheme is to guarantee heavy representation of UMES programs with their higher-than-average-for-HBIs white enrollments among “unique” programs, creating a spurious statistical association between white enrollment and uniqueness that may actually reflect distinct characteristics of UMES.<sup>25</sup> Exh. 3, at ¶ 33 (Lichtman Dec.) (referring to UMES as a “confounding variable”); Attachment to Exh. 3, at 19 (Lichtman Dec.).

In addition, the definitions (and concomitantly any statistical relationship with white enrollment) of both “high-demand” and “unique” programs exclude, and therefore depend on, Dr. Conrad’s idiosyncratic definition of “core” programs. Exh. 2, at 284 (Conrad Dep.). There is no authoritative definition of the term. *Id.* at 297. Even Dr. Allen and Dr. Conrad do not agree about what the term means. *Compare* Exh. 1, at 143 (Allen Dep.) (core programs exist on every campus) *with* Exh. 2, at 284 (Conrad Dep.) (such programs are “not actually offered at all colleges”). Dr. Allen explained his

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<sup>25</sup> Conrad & Allen nowhere refer in their 2016 report to the cherry-picked enrollment data from supposedly unique programs at HBIs in other states presented in ECF 406-1 at 42-43. Even if they had relied on that material, such selective examples of a few programs that have high white enrollment cannot demonstrate in a rigorous or reliable way that creating or transferring “unique” programs will generally increase white enrollment. Conrad & Allen have presented no comprehensive analysis of other-race enrollment in “unique” programs in other states.

understanding that “core” refers to “general education courses, which are usually the first two years required courses, after which time students specialized.” Exh. 1, at 123 (Allen Dep.). Dr. Conrad’s understanding is based on an idealized notion of a traditional liberal education. Exh. 2, at 293-94 (Conrad Dep.); *id.* at 299 (referring to Alan Bloom’s *Closing of the American Mind* as a similar concept). However, the “core” can change over time. Exh. 2, at 292-94 (Conrad Dep.); Exh. 3, at ¶¶ 15-16 (Lichtman Dec.). For example, Dr. Conrad has added programs such as environmental studies (Exh. 2, at 302 (Conrad Dep.)), women’s and gender studies (*id.* at 301), and other inter-disciplinary programs on the basis of his own subjective judgments about how valuable the programs seem to be, but apparently without regard to educational or workforce needs of the state.

*c. The “Disproportion” Quantitative Analysis*

Conrad & Allen present another statistically invalid form of quantitative analysis that compares the percentage of white students at an HBI who are *enrolled* in programs they classify as “high-demand” or “unique” with the percentage of *programs* at the HBI. Exh. 9, at ¶¶ 223-226 (Second Corrected Reply Expert Report); Attachment to Exh. 3, at 56-63 (Lichtman Dec.). That comparison is logically irrelevant to whether adding or transferring “high-demand” or “unique” programs is likely to increase white enrollment, because it has nothing to do with the proportion of white students enrolled in such programs in relation to overall enrollment, *i.e.*, the percentage of white students in the program. Dr. Conrad testified that he had never seen a similar analysis in his 40 years in the field. Exh. 2, at 332 (Conrad Dep.). The methodological error here is “the failure to

provide a proper comparison.” Exh. 3, at ¶ 20 (Lichtman Dec.); *id.* at ¶ 21 (describing appropriate comparator).

By way of illustration, if “high-demand” programs attract proportionately more students than other programs (by definition), then one would expect proportionately more white students to be enrolled in such programs, even if the proportion of white students in each program was at or below the average for the university as a whole. At his deposition (Exh. 2, at 333-34), Dr. Conrad was asked about the following hypothetical:

- 100 programs at an HBI, of which 40 (40%) are high demand (HD).
- 60% of all white students are in HD programs, for a “disproportionality” of 1.5.
- However, 75% of *all* students are in HD programs, for a “disproportionality” of 1.875.

Dr. Conrad agreed that these facts would suggest “limited disproportionality, for sure” in the desegregative effect of high demand programs. *Id.* And yet he also admitted that neither he nor Dr. Allen had bothered to examine “the total proportion of all students.” *Id.* at 334.

As Professor Lichtman explains, “the proper methodology is to compare the percentage of white students in plaintiffs’ four program categories with the percentage of all students in these programs.” Attachment to Exh. 3, at 56 (Lichtman Dec.). When the proper analysis is performed, white enrollment percentages in the “high-demand” and “unique” programs are generally lower than white percentages are for the HBIs as a whole. That is, the percentage of white students enrolled in these programs is generally smaller than the percentage of *all* students enrolled in them. *Id.* at 59-62 (Tables 23-26).

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Other than the invalid 1994 study and the defective 2016 quantitative analyses, the rest of the Second Corrected Report relies solely on cherry-picked examples that cannot be used to draw causal inferences. Exh. 3, at ¶¶ 25-26 (Lichtman Dec.). Thus, none of the foundations on which Conrad & Allen rely to infer that more “high-demand” and “unique” programs will mean higher white enrollment survives review under the *Daubert* standard.

### **CONCLUSION**

The Court should grant the State’s motion and exclude all opinion testimony concerning the effects of “programmatically niches,” “high-demand” and “unique” programs on HBI enrollment by other-race students.

Respectfully submitted,

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