

A Tale of Two *Dauberts*: Discriminatory Effects of Scientific Reliability Screening

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I. INTRODUCTION

Rare is the day that passes that the issue of race in criminal justice is not headline news.¹ Police shooting cases and allegations of racial bias raise

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¹ See, e.g., Laurel Eckhouse, *Big Data May Be Reinforcing Racial Bias in the Criminal Justice System*, WASH. POST (Feb. 10, 2017), https://www.washingtonpost.com/opinions/big-data-may-be-reinforcing-racial-bias-in-the-criminal-justice-system/2017/02/10/d63de518-ee3a-11e6-9973-c5efb7ccfb0d_story.html [https://perma.cc/LG3E-JE43]; Max

important questions about the legitimacy of law enforcement and, more broadly, about our nation's commitment to principles of equal protection.² While criminal justice reform has appropriately been a high-profile concern, fixing criminal justice alone will not and cannot solve America's problem with discrimination.³

Ehrenfreund, *Poor White Kids Are Less Likely to Go to Prison Than Rich Black Kids*, WASH. POST (Mar. 23, 2016), <https://www.washingtonpost.com/news/wonk/wp/2016/03/23/poor-white-kids-are-less-likely-to-go-to-prison-than-rich-black-kids/> [<https://perma.cc/2WG6-W4NX>]; Nicole Gonzalez Van Cleve, *Chicago's Racist Cops and Racist Courts*, N.Y. TIMES (Apr. 14, 2016), <https://www.nytimes.com/2016/04/15/opinion/chicagos-racist-cops-and-racist-courts.html> [on file with *Ohio State Law Journal*]; Lester Graham & Sandra Svoboda, *Poor and Black More Likely to Be Wrongfully Convicted*, MICH. RADIO (Nov. 3, 2016), <http://michiganradio.org/post/poor-and-black-more-likely-be-wrongfully-convicted> [<https://perma.cc/U6SP-BSF3>]; Andrew Kahn & Chris Kirk, *What It's Like to Be Black in the Criminal Justice System*, SLATE (Aug. 9, 2015), http://www.slate.com/articles/news_and_politics/crime/2015/08/racial_disparities_in_the_criminal_justice_system_eight_charts_illustrating.html [<https://perma.cc/MZ5L-FTLW>]; Adam Liptak, *Citing Racist Testimony, Justices Call for New Sentencing in Texas Death Penalty Case*, N.Y. TIMES (Feb. 22, 2017), <https://www.nytimes.com/2017/02/22/us/politics/duane-buck-texas-death-penalty-case-supreme-court.html> [on file with *Ohio State Law Journal*].

² See, e.g., Emily Bazelon, *Department of Justification*, N.Y. TIMES (Feb. 28, 2017), <https://www.nytimes.com/2017/02/28/magazine/jeff-sessions-stephen-bannon-justice-department.html> [on file with *Ohio State Law Journal*] (“The next time a disturbing video of a police shooting surfaces, it’s easy to imagine Trump and Sessions supporting the police without question and directing a crackdown on protesters, especially if they’re mostly black.”); Megann Horstead, *Experts Discuss Changing Police Perceptions During Naperville Forum*, NAPERVILLE SUN (Feb. 23, 2017), www.chicagotribune.com/suburbs/naperville-sun/ct-nvs-league-women-voters-policing-forum-st-0224-20170223-story.html [<https://perma.cc/8L6F-RNWA>] (“Community trust in local police is a valuable commodity that can easily be eroded by police shootings of African-Americans and abuses elsewhere but also can be strengthened by re-examining procedures and forming connections with residents, experts said Wednesday.”); John Sharp, *Mobile to Consider Forming a Citizen Police Advisory Council in Aftermath of Police Shooting*, AL.COM (July 8, 2016), http://www.al.com/news/mobile/index.ssf/2016/07/mobile_to_consider_forming_a_c.html [<https://perma.cc/N29D-E8UM>] (noting that police advisory council goals include “[s]trengthen[ing] bonds between the Police Department and the community, thereby [ensuring] equal protection and service to all”); Tim Sheehan, *Brand Proposes Citizen Board to Review Police Incidents, Improve Trust in Department*, FRESNO BEE (Mar. 9, 2017), <http://www.fresnobee.com/news/local/article137455133.html> [<https://perma.cc/7X6A-CH33>] (statement from Fresno Mayor Lee Brand) (“[The citizen board] will increase public confidence in the Police Department and work to strengthen and ensure the application of equal protection under the law for all citizens in the city of Fresno.”).

³ Certainly, the issue of discrimination in the United States is broader than criminal justice, or even justice system, concerns. In recent years, discrimination in voting rights, LGBTQ status, equal pay for equal work, and higher education have all garnered significant scholarly attention. See Mario L. Barnes et al., *Judging Opportunity Lost: Assessing the Viability of Race-Based Affirmative Action After Fisher v. University of Texas*, 62 UCLA L. REV. 272, 277 (2015) (suggesting a continued role for race-conscious affirmative action in mitigating long term racial discrimination in the United States); Anthony J. Gaughan, *Has the South Changed? Shelby County and the Expansion of the Voter ID Battlefield*, 19 TEX.

Even while criminal justice reform has been a front-page news item, discrimination within the civil justice system has been, at best, an underdeveloped area of research. Commentators like Jonathan Cardi, Alexander Tabarrok, and Eric Helland all lament the lack of rigorous empirical research in the field,⁴ and a brief review of available studies suggests it is an area ripe for further exploration.⁵

We therefore decided to empirically evaluate whether changes in the civil justice system have differing impacts on different communities and whether we could quantify the difference. Building on our prior research on scientific evidence,⁶ we decided to evaluate the effect of the adoption of the *Daubert* standard on filing rates within different communities, particularly communities of color. Once we created an extensive database and evaluated it with a fixed effects regression analysis, we confirmed that *Daubert* does have a disparate impact on communities of color, leading to their disproportionate exclusion from federal court.⁷

We found that when the federal system adopted the stricter standard of *Daubert* in 1993, there was a disproportionate and negative impact on filings from African-American plaintiffs along with a corresponding rise in filings from white plaintiffs.⁸ Yet that is not all we found. In prior work, we found that when a state adopted *Daubert* after 1993, there was a “return to federal court” effect

J. C.L. & C.R. 109, 122–25 (2013) (discussing the impact of voter identification laws and racial distrust in the United States); Russell K. Robinson, *Unequal Protection*, 68 STAN. L. REV. 151, 155, 158–63 (2016) (discussing recent cases and issues in equal protection law with LGBT rights); Nantiya Ruan & Nancy Reichman, *Hours Equity is the New Pay Equity*, 59 VILL. L. REV. 35, 37 (2014) (suggesting pay equity must also contain a component of scheduling equity, as a way to combat discrimination against female low wage workers in retail employment).

⁴W. Jonathan Cardi, *The Search for Racial Justice in Tort Law*, in CRITICAL RACE REALISM: INTERSECTIONS OF PSYCHOLOGY, RACE, AND LAW 115, 120 (Gregory S. Parks et al. eds., 2008) (noting that existing scholarship in the area of civil justice and race “only scratches the surface of this important topic”); Eric Helland & Alexander Tabarrok, *Race, Poverty, and American Tort Awards: Evidence from Three Data Sets*, 32 J. LEGAL STUD. 27, 28 (2003) (“It is odd that so little attention has been paid to the role of race and poverty in the American tort system because the influence of race on criminal trials has been extensively studied . . .”).

⁵Cardi, *supra* note 4, at 120 (suggesting, after reviewing prior empirical work, that “[l]imitations inherent in all of the foregoing studies’ methodology and geographical restrictions and their growing datedness indicate the need for further study of racial disparities in tort verdicts and settlements”). See also *infra* Part II.B.

⁶See generally Andrew Jurs & Scott DeVito, *Et Tu, Plaintiffs? An Empirical Analysis of Daubert’s Effect on Plaintiffs, and Why Gatekeeping Standards Matter (a Lot)*, 66 ARK. L. REV. 975 (2013) (expanding on prior research “by applying the fixed-effect statistical model to a new and different dataset”); Andrew Jurs & Scott DeVito, *The Stricter Standard: An Empirical Assessment of Daubert’s Effect on Civil Defendants*, 62 CATH. U. L. REV. 675 (2013) (answering the question of whether *Daubert* had a measurable effect on expert admissibility).

⁷See *infra* Part III.

⁸See *infra* Part III.D.

where filings rebound to pre-1993 patterns.⁹ Yet our analysis reveals that after state adoption of *Daubert*, there is no rebound for African-American plaintiffs; instead, the filing rates for black plaintiffs remain depressed.¹⁰

Our research shows that, in response to *Daubert*, black plaintiffs were less likely to file in federal court, and once they were pushed out of the civil justice system, they remained out. In essence, the *Daubert* admissibility standard impacts filings exactly like a method of tort reform, but only for claimants of color.¹¹ This finding is new in the literature and has profoundly troubling implications for our national ideal of equal justice under law. Yet troubling as it may be, our hope is that once this and other discriminatory effects are exposed, it will lead to civil justice reform to enhance the legitimacy of the justice system overall. In that regard, our research blends seamlessly with reform efforts in the criminal justice area.¹²

To assess these issues, we begin in Part II with a review of studies in civil justice, exploring both theoretical and empirical work with a focus on perceptions of civil justice by communities of color and women claimants. In Part III, we explain our research model, which measures the impact of changes in civil justice rules—specifically the gatekeeping standard for scientific evidence—on different communities. We demonstrate how we designed the research, created a database which incorporates U.S. Census demographic information, and how the results demonstrate that changes to civil justice rules in general, and to scientific admissibility standards in particular, can have a disproportionate and negative effect on communities of color. In fact, the changes suggest that *Daubert* can and does act on these communities as a type of tort reform measure, restricting access to civil justice and stoking the crisis of the legitimacy for civil justice within those communities. We finish in Part IV with a discussion of the implications of our findings and preliminary suggestions about policy prescriptions in response to them.

By empirically testing the effect of the *Daubert* standard on communities of color, we measured the disproportionate impact it has on those communities,

⁹Jurs & DeVito, ARK. L. REV., *supra* note 6, at 1002; Jurs & DeVito, CATH. U. L. REV., *supra* note 6, at 701–02.

¹⁰See *infra* Part III.D.

¹¹For a more detailed discussion of the empirical assessment of tort reform, see *infra* Part IV.A; see also Scott DeVito & Andrew Jurs, *An Overreaction to a Nonexistent Problem: Empirical Analysis of Tort Reform from the 1980s to 2000s*, 3 STAN. J. COMPLEX LITIG. 62, 103–09 (2015) (finding that after a tort reform damages cap is removed, there is not a rebound effect and filings continue to decline).

¹²Regarding criminal justice reforms and their intent to enhance legitimacy of the system, see, for example, STEPHANOS BIBAS, *THE MACHINERY OF CRIMINAL JUSTICE* 144–50 (2012) (advocating for reforms to enhance legitimacy of law enforcement); Anthony A. Braga, *Better Policing Can Improve Legitimacy and Reduce Mass Incarceration*, 129 HARV. L. REV. F. 233, 233 (2016); Tom R. Tyler, *Enhancing Police Legitimacy*, 593 ANNALS AM. ACAD. POL. & SOC. SCI. 84, 90–91 (2004) (noting that “[l]egitimacy-based policing has clear advantages for the police and the community” and suggesting reforms to enhance police legitimacy).

which underscores a long-standing crisis of legitimacy in state courts but also suggests more broadly that changes in civil justice should be reviewed for disparate impacts prior to their adoption.

II. PRIOR RESEARCH ON CIVIL JUSTICE AND RACE

Civil justice remains an underexplored area of research and analysis, but it is not entirely undeveloped. Before discussing our statistical analysis, we will first discuss prior work on issues of civil justice and race, starting with commentary from a theoretical perspective. Afterwards, we will examine prior empirical research assessing civil justice and race, along with a brief review of statistical research in the *Daubert* field. Ultimately, we agree with Professors Eric Helland and Alexander Tabarrok's insight that "[i]t is odd that so little attention has been paid to the role of race and poverty in the American tort system."¹³

A. *Theoretical Scholarship on Race and Civil Justice—A Call to Arms*

The impact of race on civil litigation is a multifaceted issue, one that multiple commentators have evaluated, unpacked, and discussed in recent scholarship in the field. In these pieces, the commentators frequently lament the lack of more detailed empirical analysis of the effect of race on civil litigation.¹⁴ Yet even without a more solid empirical footing, the commentary has illuminated a serious concern of discrimination within the civil justice system.

Most recent commentary in the field discusses the current state of affairs with civil justice, but the historical trend also shows the issue is not a new one. In her study reviewing legal decisions from the first half of the twentieth century, Jennifer Wriggins found that tort claim values have been influenced by race in the past and continue to be affected by race even today.¹⁵ By comparing awards in wrongful death cases, Wriggins concluded that "financial contributions of black decedents, their pain and suffering, and the grief of black survivors, at times, were weighed more lightly than similar harms suffered by whites."¹⁶ She suggests the disparity can be partially explained by negative

¹³ Helland & Tabarrok, *supra* note 4, at 28.

¹⁴ Cardi, *supra* note 4, at 120; Frank M. McClellan, *The Dark Side of Tort Reform: Searching for Racial Justice*, 48 RUTGERS L. REV. 761, 773 (1996); Jennifer B. Wriggins, *Torts, Race, and the Value of Injury, 1900–1949*, 49 HOW. L.J. 99, 135 (2005). Empirical researchers echo this point. See text accompanying note 10.

¹⁵ Wriggins, *supra* note 14, at 130 (measuring effect of race on tort claims using a variety of methodological approaches). Wriggins expanded upon this analysis in a 2007 essay in *The Review of Litigation*. Jennifer B. Wriggins, *Damages in Tort Litigation: Thoughts on Race and Remedies, 1865–2007*, 27 REV. LITIG. 37, 44–46 (2007) (adding factors of access to attorneys and use of race-based compensation tables to earlier assessment of race and damages in tort).

¹⁶ Wriggins, *supra* note 14, at 130.

racial/racist generalizations,¹⁷ but also by assumptions about family structure¹⁸ that disproportionately affect black claimants and by the use of segregated precedents devaluing claims based on past practices.¹⁹ While the cases reviewed involved claimants before 1950, Wriggins makes clear that devaluation of injury to people of color is not only a historical fact, but is also something we must remain vigilant to identify and correct.²⁰

Just as racialized precedents in law could reduce damage awards to people of color in the past, other commentators have suggested that precedents for damages—specifically damage valuation tables—have resulted in disparate awards for people of color and women in modern litigation. In 2005, Professor Martha Chamallas described how her work on task forces addressing discrimination in the justice system led to her discovery that experts calculating damage awards using race- and gender-based tables is “commonplace.”²¹ Calculation of damages by use of these gendered or racialized tables has the effect of reducing recoveries by providing different baseline assumptions on work/life expectancy and also on annual income.²² By use of these tables, Professor Chamallas illustrates this concern with the case of *United States v. Bedonie*, where a civil damage award in a wrongful death case was explicitly reduced from a baseline of \$744,000 to \$433,000 based solely on the plaintiff’s status as a Native American man (who, on average, earn only 58% of the earnings of white men).²³ In response, Chamallas argues in favor of damages calculations based on “blended tables” created without regard to race or gender,²⁴ while arguing downward adjustments on damages violate equal protection.²⁵

Chamallas and Wriggins joined forces to write a detailed examination at gender and racial discrimination throughout tort law in their monograph, *The Measure of Injury*.²⁶ Building upon Wriggins’ work on historical damages and

¹⁷ *Id.* at 127–29.

¹⁸ *Id.* at 115–17.

¹⁹ *Id.* at 124–26.

²⁰ *Id.* at 138.

²¹ Martha Chamallas, *Civil Rights in Ordinary Tort Cases: Race, Gender, and the Calculation of Economic Loss*, 38 LOY. L.A. L. REV. 1435, 1438 (2005).

²² *Id.* at 1438–39; see also Michael I. Meyerson & William Meyerson, *Significant Statistics: The Unwitting Policy Making of Mathematically Ignorant Judges*, 37 PEPP. L. REV. 771, 801–10 (2010) (starting with review of Chamallas’s work on race- and gender-based tables, but providing additional examples of misuse of these statistics and noting that “using those tables reinforces the harm caused by wrongful discrimination”).

²³ Chamallas, *supra* note 21, at 1439–40. Chamallas notes a similar damages devaluation in the same case, reducing damages for wrongful death of a Native American woman from \$308,000 to \$171,000. *Id.*

²⁴ *Id.* at 1441–43 (reviewing how race and gender tables violate equal protection law).

²⁵ *Id.* at 1450 (advocating for the use of blended tables).

²⁶ See generally MARTHA CHAMALLAS & JENNIFER B. WRIGGINS, *THE MEASURE OF INJURY: RACE, GENDER, AND TORT LAW* (2010) (showing how gender and racial hierarchy affect tort law).

Chamallas's work on gender- and race-based tables for modern damages calculations, they conclude that all aspects of tort law have been affected by discrimination: "The mapping of gender onto the debate over physical versus mental harm, the operation of white racial privilege, and the devaluation of claims brought by racial minorities are dynamics that have shaped the law of intentional torts, negligence, causation, and damages."²⁷ In response, they suggest a broad incorporation of civil rights principles into tort law in order to "weave gender and race equality into basic tort law principles,"²⁸ while also expanding compensable harms to include reproductive, sexual and other gender-associated concerns currently undervalued or excluded by historical legacy.²⁹

Broadening the inquiry further, Frank McClellan argues that in addition to the direct harm to injured parties undercompensated due to race or gender discrimination identified by Wriggins and Chamallas, there is also a public harm component to discriminatory practices in tort law.³⁰ He believes that race has an impact "on every aspect of a tort claim, adversely affecting lawyers, clients, and the *public conception of justice*."³¹ So while it is clear that the individual tort claimant does worse in a discriminatory civil justice system—a harm that merits attention and must be systematically eliminated—there is also a public harm when we as a society allow private bias to control the public system of civil justice.³² In essence, permitting those biases to affect results in court unequivocally reduces the perception of legitimacy of the justice system.³³

Independent of the individual and public harms of a discriminatory tort regime, W. Jonathan Cardi also suggests that the harm also undermines the theoretical underpinnings of tort law, from either a corrective justice or law-and-economics approach.³⁴ If discrimination affects damages awards, then the overall effect must be that tort-related social policy will be suboptimal in light of the goals of tort to compensate those wronged and deter wrongdoers from other harms.³⁵ Cardi also comprehensively reviews the state of empirical research in the field of torts and notes, "Limitations inherent in all of the foregoing studies' methodology and geographical restrictions and their growing datedness indicate the need for further study of racial disparities in tort verdicts and settlements. Indeed, existing scholarship only scratches the surface of this important topic."³⁶

²⁷ *Id.* at 61.

²⁸ *Id.* at 188.

²⁹ *Id.* at 190 (supporting expansion of compensable harms to include claims of particular importance to women and people of color).

³⁰ See McClellan, *supra* note 14, at 772.

³¹ *Id.* (emphasis added).

³² See *id.*

³³ See *id.*

³⁴ Cardi, *supra* note 4, at 116.

³⁵ *Id.*

³⁶ *Id.* at 120.

The most recent analysis of discriminatory damages calculations, by Ronen Avraham in 2015, expands upon Cardi's discussion of the theoretical underpinnings of tort law.³⁷ Just as with Chamallas's work a decade earlier, Avraham identifies the use of race- and gender-based tables for damages calculation as a source of individual discrimination, but also as a source of negative systematic effects.³⁸ For example, he suggests that a rational manager of a bus transportation company could limit their liability by assigning their riskier drivers to work routes with more women and people of color, since the law currently makes it "cheaper for school bus companies to have accidents involving black girls than accidents involving white boys."³⁹ In response to this shocking situation, Avraham suggests that when one considers the theoretical underpinnings of tort law—corrective justice, distributive justice, and efficiency—the use of blended tables better supports the goals of the law.⁴⁰ This is true regardless of whether the tables currently used are accurate, although Avraham suggests that there are many reasons to think they are not.⁴¹ In response, courts should require blended tables in the determination of tort damages,⁴² and he notes a single example of a federal district judge—Judge Weinstein of the Southern District of New York—who has done so.⁴³ In addition to his analysis and proposed solution, Avraham also—as Cardi and others have before him—lamented the lack of attention race and gender discrimination in tort law (and civil justice in general) have received, calling it a "great embarrassment"⁴⁴ and a "problem [that] can no longer be ignored."⁴⁵

B. *Empirical Research on Race and Civil Justice—A Tale of Two Systems*

While Cardi and Avraham may be right that empirical investigation into discrimination in the civil justice system is sorely needed, those studies that are available have led to significant insights into the state of affairs in courtrooms today.⁴⁶ Dividing the existing research on race and civil justice into coherent pieces, broadly speaking, there are several nationally-focused empirical research studies and then a collection of state-level inquiries authored mainly

³⁷ RONEN AVRAHAM, IS RACE- AND SEX-BASED TARGETING EFFICIENT? A CLOSER LOOK AT TORT LAW'S DISCRIMINATORY DAMAGE AWARDS 8, 11 (2015), https://www-cdn.law.stanford.edu/wp-content/uploads/2015/09/Ronen-Avraham_Is-Race-and-Sex-Based-Targeting-Efficient-A-Closer-Look-at-Tort-Law%E2%80%99s-Discriminatory-Damage-Awards.pdf [on file with *Ohio State Law Journal*].

³⁸ *Id.*

³⁹ *Id.* at 5.

⁴⁰ *Id.* at 8–9, 33–55.

⁴¹ *Id.* at 9–10.

⁴² *See id.* at 10, 65.

⁴³ *See* AVRAHAM, *supra* note 37, at 20.

⁴⁴ *Id.* at 2.

⁴⁵ *Id.* at 11.

⁴⁶ *See infra* Part II.B.1.

by committees on race and justice appointed by individual state supreme courts.⁴⁷ In examining this research, we can see that significant evidence already exists of discriminatory effects in the civil justice system.

1. *National Studies*

Many of the national studies review the issue of civil justice as one part of an overall investigation into perceptions of the entire justice system. In explicitly evaluating the issue of race and civil justice, only two different research groups—relying on a single unified dataset—evaluate the concern: Eric Helland and Alexander Tabarrok, with their 2003 article, *Race, Poverty, and American Tort Awards*,⁴⁸ and Theodore Eisenberg and Martin Wells, with a 2002 article, *Trial Outcomes and Demographics: Is There a Bronx Effect?*⁴⁹ In both studies, the researchers relied on the same massive database created from combining census data from 1980 and 1990 with secondary sources reporting tort awards from both state and federal courts between 1979 and 2000.⁵⁰ Once they analyzed the data, though, only Helland and Tabarrok could find statistically significant differences in damage awards based on race.⁵¹

Helland and Tabarrok began with the dataset on federal and state jury verdicts and then analyzed it using a regression analysis to measure the effect of race and income in the jury pool on damage awards.⁵² They found that when people of color as a percentage of the jury pool increased, and when the percentage of people of color in poverty increased in that jury pool, there was a statistically significant increase in tort damage awards.⁵³ For each percentage increase in the jury pool of people who are both black and in poverty, as defined by census data, Helland and Tabarrok found a 3%–10% increase in tort damage awards.⁵⁴ The increase in awards may be as high as 7% for Hispanic jury pools with high poverty rates, based on census data, but the authors urge caution in that analysis due to weaknesses in the data.⁵⁵ In contrast, an increase in the percentage of the jury pool who is white and in poverty, as defined by census data, results in a 2%–3% decrease in tort damage awards.⁵⁶ While the researchers suggest that these data represent significant changes in jury behavior due to demographics, they caution that they are unable to report on the actual

⁴⁷ See *infra* Part II.B.2.

⁴⁸ Helland & Tabarrok, *supra* note 4, at 27–28.

⁴⁹ Theodore Eisenberg & Martin T. Wells, *Trial Outcomes and Demographics: Is There a Bronx Effect?*, 80 TEX. L. REV. 1839–40 (2002).

⁵⁰ *Id.* at 1843–46; Helland & Tabarrok, *supra* note 4, at 29–33.

⁵¹ Eisenberg & Wells, *supra* note 49, at 1839–40, 1869; Helland & Tabarrok, *supra* note 4, at 46–47. See *infra* text accompanying notes 55–57, 59–64.

⁵² Helland & Tabarrok, *supra* note 4, at 37, 40 tbl.6.

⁵³ *Id.* at 43–45.

⁵⁴ *Id.* at 46.

⁵⁵ *Id.* at 47.

⁵⁶ *Id.* at 46.

composition of juries (instead of the jury pool), nor can they separate out effects for race independent of poverty and race due to inherent data weaknesses.⁵⁷ Nonetheless, the study is an important assessment of race and civil justice and stands nearly alone in explicit analysis of solely that issue.

The only other study to assess the issue directly, by Eisenberg and Wells, did not find statistically significant effects based on race.⁵⁸ Using the same dataset as Helland and Tabbarok,⁵⁹ Eisenberg and Wells performed multiple regression analyses to test the folk wisdom that people of color “favor injured plaintiffs and give them inflated awards.”⁶⁰ Yet in almost every analysis they ran, the researchers could not find a statistically significant effect due to race. They could find no effect of race on win rates or damages awards, in either state or federal court.⁶¹ When they assessed the data using a more focused multivariable analysis, they again found no statistically significant effect of race on damages or win rates, in either state or federal court.⁶² In fact, the only statistically significant effect Eisenberg and Wells found related to the win rate for federal jury trials when the cause of action was limited to torts and the sample was limited to urban populations.⁶³ Within the context of so many findings that failed to meet significance, Eisenberg and Wells then spend several pages suggesting reasons for the finding,⁶⁴ before conceding that they “find little robust evidence that a trial locale’s population demographics help explain jury trial outcomes.”⁶⁵

Other national-level research examining race and civil justice tends to treat the issue as one subset of the overall assessment of the broader impact of race on overall justice. As such, it can be of limited applicability to the civil justice context, and yet the research shows a stark contrast between experiences in the justice system of whites and people of color.

The National Center for State Courts (NCSC) 1999 national survey demonstrates this concern in stark terms.⁶⁶ When the NCSC surveyed over 1,800 Americans of all backgrounds about the justice system, many respondents believed that people of color are treated worse by the system than others, although the percentage varies widely between respondents of different backgrounds.⁶⁷ Among black respondents, the percentage who believed they are

⁵⁷ *Id.* at 41, 52 (discussing difficulty in establishing distinction between race effects on damages and race-and-poverty effects, due to the limitations on the data).

⁵⁸ Eisenberg & Wells, *supra* note 49, at 1839–40, 1869.

⁵⁹ See *supra* note 48 and accompanying text.

⁶⁰ See Eisenberg & Wells, *supra* note 49, at 1839, 1843–46.

⁶¹ *Id.* at 1852 tbl.1.

⁶² *Id.* at 1856 tbl.2, 1858 tbl.3.

⁶³ *Id.* at 1861, 1862 tbl.4.

⁶⁴ *Id.* at 1865–68.

⁶⁵ *Id.* at 1869.

⁶⁶ NAT’L CTR. FOR STATE COURTS, HOW THE PUBLIC VIEWS THE STATE COURTS: A 1999 NATIONAL SURVEY 43 (1999), <http://cdm16501.contentdm.oclc.org/cdm/ref/collection/ctc/mm/id/17> [on file with *Ohio State Law Journal*].

⁶⁷ *Id.* at 37.

treated worse than others is over 68%, as compared to about 40% of whites who agree with that sentiment.⁶⁸ It is not hard to see why, when black respondents are more likely to believe judges and juries are not representative of the community,⁶⁹ and less likely to believe judges are honest and fair⁷⁰ or that court personnel are helpful and courteous.⁷¹ Based on the survey data, the NCSC report concludes that black Americans are “clearly estranged from the courts.”⁷²

A 2000 follow-up survey concurs with that sentiment.⁷³ For that research, the authors collected about 1,600 responses from a variety of demographic groups, asking about fairness of court procedures and court outcomes.⁷⁴ When asked about outcomes of cases, a majority (57%) of whites who had no contact with the court system agreed outcomes are always or usually fair compared to only 21% of black respondents who also had no contact with the court system. Furthermore, over thirty percent of black respondents believed outcomes are seldom or never fair.⁷⁵ Similarly, when asked about court procedures, 64% of whites without court contact believed procedures are always or usually fair, while only 27% of black respondents agreed (and 54% responded that courts are sometimes fair).⁷⁶ In addition to these contrasts, when asked directly if black litigants are treated worse than other litigants, only 23% of whites believed that this happened either always or often, while a majority of black respondents (52%) agreed.⁷⁷ As with the 1999 NCSC study, the researchers concluded ultimately that “African-Americans are estranged from the court system.”⁷⁸

A 2015 NCSC follow-up study indicated that this racial gap has yet to close.⁷⁹ Based on 1,000 survey responses, the study found that a majority still believe African-Americans are treated poorly in the judicial system.⁸⁰ African-American responses rated lower on issues of trust and fairness in the system, with only 52% believing in the fairness of their direct experience with the system compared to seventy percent of white respondents.⁸¹ Significantly to the

⁶⁸ *Id.* at 38.

⁶⁹ *See id.* at 29.

⁷⁰ *Id.* at 30.

⁷¹ *Id.* at 26.

⁷² NAT'L CTR. FOR STATE COURTS, *supra* note 66, at 43.

⁷³ DAVID B. ROTTMAN & RANDALL M. HANSEN, NAT'L CTR. FOR STATE COURTS, HOW RECENT COURT USERS VIEW THE STATE COURTS: PERCEPTIONS OF WHITES, AFRICAN-AMERICANS, AND LATINOS 18 (2001), <http://cdm16501.contentdm.oclc.org/cdm/ref/collect ion/ctcomm/id/18> [on file with *Ohio State Law Journal*].

⁷⁴ *Id.* at 3–4.

⁷⁵ *Id.* at 5 tbl.1.

⁷⁶ *Id.*

⁷⁷ *Id.* at 11 tbl.9.

⁷⁸ *Id.* at 18.

⁷⁹ NAT'L CTR. FOR STATE COURTS, ANALYSIS OF NATIONAL SURVEY OF REGISTERED VOTERS 1 (2015), https://www.ncsc.org/~media/Files/PDF/Topics/Public%20Trust%20and%20Confidence/SoSC_2015_Survey%20Analysis.ashx [<https://perma.cc/KM7M-JKHJ>].

⁸⁰ *Id.* at 1, 4.

⁸¹ *Id.* at 4.

research model we developed, the survey responses demonstrate that the perception of unfairness and mistrust that is prevalent in state courts does not exist for federal courts: “There is no racial gap on the U.S. Supreme Court or the federal court system.”⁸²

Research on the role of race on civil justice at the national level remains—perhaps as a result of the difficulty of research design in the field⁸³—largely undeveloped, with most studies tangentially addressing the issue in a broader assessment of overall justice system concerns.

2. State-Specific Studies

While the role of race on civil justice remains an underdeveloped area for national studies, states have filled the gap by appointing race and justice commissions to research and analyze the issue. Many of these studies support the stark racial gap in trust in the justice system seen in the National Center for State Courts (NCSC) data.⁸⁴

Unlike many of the later works, the first jurisdiction-specific study to address race and civil justice came from the RAND Corporation rather than a state commission.⁸⁵ Audrey Chin and Mark Peterson assessed the outcomes in over 9,000 civil cases in Cook County, Illinois, in their 1985 study *Deep Pockets, Empty Pockets: Who Wins in Cook County Jury Trials*.⁸⁶ They found evidence of systematic bias against people of color in all stages of litigation. Chin and Peterson found that women and African Americans are much less likely to be plaintiffs in a case in the first place, and even when they are, are less likely to claim severe or disabling injuries.⁸⁷ As litigants, black claimants lost more often than whites,⁸⁸ and those that won their claim received smaller damage awards, on average 74% less than white claimants.⁸⁹ Black defendants lost more too.⁹⁰ Based on these stark findings, Chin and Peterson concluded that

⁸² *Id.*

⁸³ Wriggins, *supra* note 14, at 135.

⁸⁴ *See supra* text accompanying notes 66–72, 79–82.

⁸⁵ *See generally* AUDREY CHIN & MARK A. PETERSON, RAND INST. FOR CIVIL JUSTICE, DEEP POCKETS, EMPTY POCKETS: WHO WINS IN COOK COUNTY JURY TRIALS (1985), <https://www.rand.org/content/dam/rand/pubs/reports/2007/R3249.pdf> [<https://perma.cc/J3EF-YZ3N>] (finding evidence of systematic bias against people of color at all stages of litigation).

⁸⁶ *Id.* at v.

⁸⁷ *Id.* at vi, 16.

⁸⁸ *See id.* at viii, 26.

⁸⁹ *See id.* at viii, 30.

⁹⁰ *Id.* at viii, 26 tbl.3.6. Counter-intuitively compared to the other data, the black defendants who do lose their claims pay less, on average, than white defendants. CHIN & PETERSON, *supra* note 85, at 29.

“[I]tligants’ race seemed to have a pervasive influence on the outcomes of civil jury trials in Cook County.”⁹¹

The RAND Corporation study appears to be the catalyst for state exploration of the same issue in the years following its publication. New Jersey may have been first, formally establishing its Task Force on Minority Concerns in 1985.⁹² By 1988, enough states had formed commissions on racial and ethnic bias in the justice system that a national conference on the issue was held to streamline future research efforts in those and in other states.⁹³ In general, a state supreme court would create a commission or committee to examine the issue of fairness in the judicial system, and then the committee would collect data through public hearings, individual interviews, and surveys.⁹⁴

A large number of commission reports would be published in the years immediately following that convention, as the issue garnered significant attention; the frequency of reports has slowed significantly in recent years.⁹⁵ As

⁹¹ *Id.* at viii. The researchers also suggested the issues raised by their work require additional empirical study. *Id.* at 62.

⁹² N.J. SUPREME COURT TASK FORCE ON MINORITY CONCERNS, FINAL REPORT 1 (June 1992).

⁹³ Arline S. Tyler & Steven Montano, *State Panels Document Racial, Ethnic Bias in the Courts*, 78 JUDICATURE 154, 154 (1994).

⁹⁴ *See, e.g.*, STATE OF IOWA EQUAL IN THE COURTS TASK FORCE, FINAL REPORT OF THE EQUALITY IN THE COURTS TASK FORCE 1 (1993) (discussing the appointment of the Task Force by the Iowa Supreme Court) [hereinafter IOWA TASK FORCE REPORT].

⁹⁵ N.D. COMM’N TO STUDY RACIAL & ETHNIC BIAS IN THE COURTS, N.D. SUPREME COURT, FINAL REPORT AND RECOMMENDATIONS (June 2012) [hereinafter NORTH DAKOTA COMMISSION REPORT]; S.D. EQUAL JUSTICE COMM’N, S.D. SUPREME COURT, FINAL REPORT AND RECOMMENDATIONS (2006); COMM’N ON RACIAL & ETHNIC FAIRNESS IN THE JUDICIAL PROCESS, MD. COURT OF APPEALS, REPORT OF THE COMMISSION ON RACIAL AND ETHNIC FAIRNESS IN THE JUDICIAL PROCESS (June 2004); NEB. MINORITY & JUSTICE TASK FORCE, STATE JUSTICE INST., FINAL REPORT (Jan. 2003); PA. SUPREME COURT COMM. ON RACIAL & GENDER BIAS IN THE JUSTICE SYS., FINAL REPORT OF THE PENNSYLVANIA SUPREME COURT COMMITTEE ON RACIAL AND GENDER BIAS IN THE JUSTICE SYSTEM (2003) [hereinafter PENNSYLVANIA COMMITTEE REPORT]; IND. SUPREME COURT COMM’N ON RACE & GENDER FAIRNESS, IND. SUPREME COURT, EXECUTIVE REPORT AND RECOMMENDATIONS (Dec. 2002); TASK FORCE ON RACIAL & ETHNIC FAIRNESS IN THE LEGAL SYS., UTAH JUDICIAL COUNCIL, RACIAL AND ETHNIC FAIRNESS: REPORT ON THE STATE OF THE CRIMINAL AND JUVENILE JUSTICE SYSTEM (Sept. 2000); OHIO COMM’N ON RACIAL FAIRNESS, OHIO STATE BAR ASS’N, THE REPORT OF THE OHIO COMMISSION ON RACIAL FAIRNESS (1999); COLO. SUPREME COURT MULTICULTURAL COMM’N, FINAL REPORT TO THE CHIEF JUSTICE (June 1998); ALASKA SUPREME COURT ADVISORY COMM. ON FAIRNESS & ACCESS, STATE JUSTICE INST., REPORT OF THE ALASKA SUPREME COURT ADVISORY COMMITTEE ON FAIRNESS AND ACCESS (Oct. 1997); TENN. SUPREME COURT COMM’N ON RACIAL & ETHNIC FAIRNESS IN THE COURT SYSTEM, FINAL REPORT OF THE TENNESSEE SUPREME COURT COMMISSION ON RACIAL AND ETHNIC FAIRNESS (Feb. 1997) [hereinafter TENNESSEE COMMISSION REPORT]; STATE OF CONN. JUDICIAL BRANCH TASK FORCE ON MINORITY FAIRNESS, FINAL REPORT (Apr. 1996) [hereinafter CONNECTICUT TASK FORCE REPORT]; CAL. JUDICIAL COUNCIL ADVISORY COMM. ON RACIAL & ETHNIC BIAS IN THE COURTS, FAIRNESS IN THE CALIFORNIA STATE COURTS: A SURVEY OF THE PUBLIC, ATTORNEYS AND COURT PERSONNEL (Dec. 1993)

with the NCSC's national research, these reports generally address issues of civil justice as one component of larger systematic problems in the courts.⁹⁶ Just as with the NCSC's research, the state commission reports collectively demonstrate significant concerns about the justice system providing equal justice under the law.⁹⁷

Without reviewing the specific findings of each state's reports, several broad conclusions can be reached about the collective findings of the state commissions as a whole. On the issue of discrimination in the judicial system, survey respondents in every state showed concern about fairness in their state. For example, when researchers asked citizens about fairness in the court system, nearly 47% of respondents in Florida disagreed with the sentiment that courts treat whites and people of color the same (with 39% agreeing),⁹⁸ while in California 72% of black respondents believe the courts were unfair.⁹⁹ The concern with fairness applies to court professionals as well. In the State of Washington, clear majorities of both judges (55%) and attorneys (62%) said that people of color see themselves as disadvantaged in civil cases,¹⁰⁰ while the number of attorneys of color who believe a similar sentiment rises to nearly 68% in New York¹⁰¹ and 70% in Iowa.¹⁰² These numbers are both shocking, to see that disparities of treatment are widespread, and typical, in that every state court commission on fairness found some similar evidence in their analysis.

[hereinafter CALIFORNIA COMMITTEE REPORT]; IOWA TASK FORCE REPORT, *supra* note 94; MINN. SUPREME COURT TASK FORCE ON RACIAL BIAS IN THE JUSTICE SYSTEM, FINAL REPORT (May 1993) [hereinafter MINNESOTA TASK FORCE REPORT]; THE FAIRNESS & EQUAL. COMM. OF THE SUPREME COURT OF IDAHO, REPORT OF THE FAIRNESS AND EQUALITY COMMITTEE OF THE SUPREME COURT OF IDAHO (1992); N.Y. STATE JUDICIAL COMMISSION ON MINORITIES, N.Y. COURT OF APPEALS, REPORT OF THE NEW YORK STATE JUDICIAL COMMISSION ON MINORITIES (Apr. 1991) [hereinafter NEW YORK COMMISSION REPORT]; WASH. STATE MINORITY & JUSTICE TASK FORCE, WASH. STATE SUPREME COURT, FINAL REPORT (Dec. 1990) [hereinafter WASHINGTON TASK FORCE REPORT]; N.J. SUPREME COURT COMM. ON MINORITY CONCERNS, INTERIM REPORT (1989) [hereinafter NEW JERSEY COMMITTEE INTERIM REPORT]; TASK FORCE ON RACIAL/ETHNIC ISSUES IN THE COURTS, MICH. SUPREME COURT, FINAL REPORT OF THE MICHIGAN SUPREME COURT TASK FORCE ON RACIAL/ETHNIC ISSUES IN THE COURTS (Dec. 1989); Georgia Supreme Court Commission on Racial & Ethnic Bias in the Court System, *Let Justice Be Done: Equally, Fairly, and Impartially*, 12 GA. ST. L. REV. 687, 694-97 (1996); Oregon Supreme Court Task Force on Racial/Ethnic Issues in the Judicial System, *Report of the Oregon Supreme Court Task Force on Racial/Ethnic Issues in the Judicial System*, 73 OR. L. REV. 823, 830-34 (1994) [hereinafter Oregon Task Force Report]; Florida Supreme Court Racial & Ethnic Bias Study Commission, *Report and Recommendations of the Florida Supreme Court Racial and Ethnic Bias Study Commission*, 19 FLA. ST. U. L. REV. 591, 604-05 (1991) [hereinafter Florida Bias Commission Report].

⁹⁶ See *supra* text accompanying notes 66-72, 79-82.

⁹⁷ See *supra* text accompanying notes 66-72, 79-82.

⁹⁸ FLORIDA JUDICIAL MGMT. COUNCIL, COMM. ON COMM'N & PUB. OP., FLORIDA STATEWIDE PUBLIC OPINION SURVEY 12 (July 1996).

⁹⁹ CALIFORNIA COMMITTEE REPORT, *supra* note 95, at 4-27 fig.4-27.

¹⁰⁰ WASHINGTON TASK FORCE REPORT, *supra* note 95, at 45.

¹⁰¹ NEW YORK COMMISSION REPORT, *supra* note 95, at 188 tbl.II.4.2.

¹⁰² IOWA TASK FORCE REPORT, *supra* note 94, at 43.

Part of the problem may relate to the relative paucity of people of color as the fact finders within the judicial system, either as judges or jurors. In 1990, only 5.5% of judges in the State of Florida were people of color, and only two judges of color statewide served at an appellate level.¹⁰³ In California, a majority of people of color responding to the survey believed that minority defendants “seldom” receive a hearing before a jury of their peers,¹⁰⁴ while in Minnesota, the commission concluded that “jury pools rarely, if ever, are representative of the racial composition of our communities.”¹⁰⁵ One reason for this may be the discriminatory use of peremptory challenges.¹⁰⁶ In Tennessee, survey data suggested that attorneys commonly strike people of color from jury panels;¹⁰⁷ in Connecticut, 74% of attorneys of color believed that systematic use of peremptory challenges in this way, while an even higher percentage (83%) of people of color serving as employees of the court system agreed.¹⁰⁸

Since the survey responses and public hearings show a stark pattern of perceived discrimination against people of color in the justice system overall, the state commissions were unflinching in their criticism of the status quo. New York’s Commission concluded that “[r]educed to their essence, the numerous complaints, testimony and comments received by the Commission reflect the perception that minorities are stripped of their human dignity, their individuality, and their identity in their encounters with the court system.”¹⁰⁹ However degrading and discriminatory the treatment may be, the commissions did not find them to be based on overt racism. As the Oregon Commission found, “[w]hile overt, intended discrimination against minorities by nonminority judges, prosecutors, lawyers, and court staff is not common, strong evidence demonstrates that racial minorities are at a disadvantage in virtually

¹⁰³ Florida Bias Commission Report, *supra* note 95, at 613. These numbers improved by the time the Commission issued their follow-up report in December 2000, to 14.7% of judges of color overall, with two justices of color on the seven-member Florida Supreme Court. RACIAL & ETHNIC BIAS COMM’N, FLA. SUPREME COURT, “WHERE THE INJURED FLY FOR JUSTICE”: A TEN-YEAR RETROSPECT ON THE REPORT AND RECOMMENDATIONS OF THE FLORIDA SUPREME COURT RACIAL AND ETHNIC BIAS STUDY COMMISSION 3 (Dec. 2000).

¹⁰⁴ CALIFORNIA COMMITTEE REPORT, *supra* note 95, at 4-51 fig.4-59.

¹⁰⁵ MINNESOTA TASK FORCE REPORT, *supra* note 95, at S-13. A similar finding can be found in Pennsylvania’s report, where lack of diversity on juries led to a perceived lack of empathy for minority claimants and therefore lower damages awards. PENNSYLVANIA COMMITTEE REPORT, *supra* note 95, at 246.

¹⁰⁶ Of course, use of peremptory challenges in this manner violates equal protection under the Constitution. *Edmonson v. Leesville Concrete Co.*, 500 U.S. 614, 616 (1991) (holding that peremptory challenges in this manner violates equal protection in civil cases), and *Batson v. Kentucky*, 476 U.S. 79, 89 (1986) (holding that the use of peremptory challenges in this manner violates equal protection in criminal cases).

¹⁰⁷ TENNESSEE COMMISSION REPORT, *supra* note 95, at 42-43.

¹⁰⁸ CONNECTICUT TASK FORCE REPORT, *supra* note 95, at CIV-5.

¹⁰⁹ NEW YORK COMMISSION REPORT, *supra* note 95, at 10.

all aspects of the Oregon court system.”¹¹⁰ Tennessee’s Commission agreed, stating:

The findings and conclusion of the Commission show, in the main, that problems experienced by racial and ethnic minority persons in their interaction with the justice system rarely stem from overt acts of mistreatment or disrespect. Nor do explicit manifestations of racial bias abound. Rather, as the Commission has found, institutional bias is relentlessly at work.¹¹¹

Yet whether the source of the discriminatory treatment is overt racism or systematic bias compounding upon itself, it sends a clear message to litigants: people of color are not welcome in the system. This finding is pervasive in the state commission reports. New Jersey’s Committee explicitly found that the findings of disparate treatment led to a “reluctance on the part of at least some minority groups to seek justice in the civil courts.”¹¹² Washington’s Task Force found that “[m]inorities believe that bias pervades the entire legal system in general and hence, they do not trust the court system to resolve their disputes or administer justice even-handedly.”¹¹³ New York’s Commission concluded that perceptions of a racially biased court system “may deter minorities from affirmatively using the courts to seek legal redress.”¹¹⁴ Pennsylvania found that the effect of concerns regarding equal treatment has been “to deter plaintiffs from filing their cases in state court.”¹¹⁵ In the most recent report to date, North Dakota pronounced succinctly that “[i]f such mistrust holds for civil cases, minorities may simply refrain from use of the [state] civil court system.”¹¹⁶

The combined findings of the state commissions on fairness in the judicial system track closely with the suspicions first encountered in the NCSC’s national survey responses: people of color do not receive equal access to the judicial system in the United States.¹¹⁷ The survey data led many of the state commissions to find explicitly that the failure of the judicial system to give equal protection of the law has led people of color to foreswear use of state civil justice system altogether.¹¹⁸

C. *Empirical Research on Effect of Daubert in Civil Justice*

As we will discuss in Part III, our analysis focuses on one particular civil justice change—the adoption of the *Daubert* standard for expert admissibility—and measures how it affects different groups of claimants. Before we quantify

¹¹⁰ Oregon Task Force Report, *supra* note 95, at 828–29 (footnote omitted).

¹¹¹ TENNESSEE COMMISSION REPORT, *supra* note 95, at 5.

¹¹² NEW JERSEY COMMITTEE INTERIM REPORT, *supra* note 95, at 100–01.

¹¹³ WASHINGTON TASK FORCE REPORT, *supra* note 95, at xxi.

¹¹⁴ NEW YORK COMMISSION REPORT, *supra* note 95, at 12.

¹¹⁵ PENNSYLVANIA COMMITTEE REPORT, *supra* note 95, at 231.

¹¹⁶ NORTH DAKOTA COMMISSION REPORT, *supra* note 95, at 113.

¹¹⁷ See *supra* text accompanying notes 79–83.

¹¹⁸ See *supra* text accompanying notes 112–116.

that effect on different groups of claimants, we will first review prior empirical work measuring *Daubert*'s overall effect. These efforts include both statistical and survey research.

In a series of surveys from the 1990s, research groups led by Sophia Gatowski and Carol Krafka found that judges had no clear consensus on *Daubert*'s effect on evidence admissibility.¹¹⁹ Surveying state court judges, Gatowski *et al* asked judges whether *Daubert* raised, lowered, or didn't change the standard for evidence admissibility, as compared to the alternative *Frye* test.¹²⁰ The responses show no clear consensus.¹²¹ The largest response group of 36% said *Daubert* was not intended to raise or lower standards, while 23% said it lowered the bar for admission and 32% said it raised standards.¹²² While state court judges were unsure of *Daubert*'s true effect on evidence admissibility, Krafka's survey of federal judges found that one-third of the judges were more likely to exclude evidence after *Daubert*.¹²³ A more recent survey of state court judges by one of these authors found that, when asked directly whether *Frye* or *Daubert* is stricter, judges chose *Daubert* but not overwhelmingly.¹²⁴ The result is different when judges are split based on their home-state standard; *Frye* judges were evenly split on the question, but 87% of *Daubert* judges believed their home state is stricter.¹²⁵ Considering these responses collectively, surveys cannot yet definitively describe the effect of *Daubert*.

Statistical research in the decade after *Daubert* was similarly equivocal. In 2001, Lloyd Dixon and Brian Gill evaluated reported case decisions in Westlaw, to decide if *Daubert* had an effect on expert admissibility.¹²⁶ They concluded that judges' standards for reliability had tightened in civil cases after *Daubert* in 1993, stating: "[J]udges scrutinized reliability more carefully and applied stricter standards in deciding whether to admit expert evidence."¹²⁷ A study by Jennifer Groscup and her colleagues examined *Daubert*'s effect on criminal cases using a similar methodology, but concluded that "no change in the overall

¹¹⁹ Sophia I. Gatowski et al., *Asking the Gatekeepers: A National Survey of Judges on Judging Expert Evidence in a Post-Daubert World*, 25 L. & HUM. BEHAV. 433, 433 (2001); see Carol Krafka et al., *Judge and Attorney Experiences, Practices, and Concerns Regarding Expert Testimony in Federal Civil Trials*, 8 PSYCHOL. PUB. POL'Y & L. 309, 322–23 (2002).

¹²⁰ Gatowski et al., *supra* note 119, at 443.

¹²¹ *See id.*

¹²² *Id.*

¹²³ Krafka et al., *supra* note 119, at 329.

¹²⁴ *See* Andrew Jurs, *Gatekeeper with a Gavel: A Survey Evaluating Judicial Management of Challenges to Expert Reliability and Their Relationship to Summary Judgment*, 83 MISS. L.J. 325, 365–66 (2014).

¹²⁵ *Id.*

¹²⁶ LLOYD DIXON & BRIAN GILL, RAND INST. FOR CIVIL JUSTICE, CHANGES IN THE STANDARDS FOR ADMITTING EXPERT EVIDENCE IN FEDERAL CIVIL CASES SINCE THE DAUBERT DECISION 15–19 (2001).

¹²⁷ *Id.* at 61; *see also id.* at 28–29 (suggesting that standards for reliability became stricter in the years after *Daubert*).

rate of admission for all types of expert evidence was observed.”¹²⁸ One other study, by Albert Yoon and Edward Cheng in 2005, used a profoundly different methodology; they proposed to measure *Daubert*’s true effect by examining filings before and after the adoption of the standard, instead of by reviewing case reports as Dixon/Gill and Groscup had done.¹²⁹ When they did so, they concluded that the legal standard for admission of evidence had no effect on filings and therefore made no difference.¹³⁰

Having reviewed these studies, and frustrated with their equivocation, the authors of this paper also assessed the *Daubert* question using a method of quantitative case filing analysis similar to Cheng and Yoon.¹³¹ Those studies depended on a natural experiment presented by our two-tiered judicial system. When the federal system adopted *Daubert* in 1993, it did so independently of state court systems, thus we could measure changes in case filing rates in response to that initial shift.¹³² The experiment also contains a built-in quality control mechanism because we could also measure the changes in case filings for state adoption of a *Daubert* standard after 1993 when both systems would again have the same standard.¹³³

Using this natural experiment as our study design, we found that both civil plaintiffs and defendants act in ways that demonstrate that *Daubert* is perceived as a stricter standard than the *Frye* standard. When faced with a venue choice between a federal court using *Daubert* and a state court using *Frye*, plaintiffs will flee to state court to enjoy the friendlier *Frye* venue.¹³⁴ On the other hand, civil defendants are more likely to remove a case to a federal *Daubert* court than to leave it in a *Frye* state court.¹³⁵ We measured first these initial shifts in reaction to uneven scientific admissibility standard after *Daubert* in 1993, but ran our built-in quality control experiment by measuring litigants’ reactions to state adoption of *Daubert* after 1993.¹³⁶ We found that once a state adopts *Daubert*, so that scientific admissibility standards of state and federal venues

¹²⁸ Jennifer L. Groscup et al., *The Effects of Daubert on the Admissibility of Expert Testimony in State and Federal Criminal Cases*, 8 PSYCHOL. PUB. POL’Y & L. 339, 363 (2002).

¹²⁹ See Edward K. Cheng & Albert H. Yoon, *Does Frye or Daubert Matter? A Study of Scientific Admissibility Standards*, 91 VA. L. REV. 471, 482 (2005).

¹³⁰ *Id.* at 503 (“[A] state’s adoption of *Frye* or *Daubert* makes no difference in practice.”).

¹³¹ See Jurs & DeVito, ARK. L. REV., *supra* note 6, at 994–1000 (discussing the methodology of analysis for determining the effect of *Daubert* on litigant actions); Jurs & DeVito, CATH. U. L. REV., *supra* note 6, at 716–28 (discussing this same methodology).

¹³² See Jurs & DeVito, ARK. L. REV., *supra* note 6, at 1000–01; Jurs & DeVito, CATH. U. L. REV., *supra* note 6, at 727–28.

¹³³ See Jurs & DeVito, ARK. L. REV., *supra* note 6, at 998–99; Jurs & DeVito, CATH. U. L. REV., *supra* note 6, at 700–01.

¹³⁴ Jurs & DeVito, ARK. L. REV., *supra* note 6, at 1000–01.

¹³⁵ See Jurs & DeVito, CATH. U. L. REV., *supra* note 6, at 699.

¹³⁶ See Jurs & DeVito, ARK. L. REV., *supra* note 6, at 1003; Jurs & DeVito, CATH. U. L. REV., *supra* note 6, at 731

are equal, the venue choices of civil plaintiffs and removal choices of civil defendants revert to the status quo.¹³⁷ These macro-level effects demonstrated that, in the aggregate and over millions of cases from 1990 to 2000, *Daubert* is perceived as stricter, and civil litigants on both sides act accordingly.

Yet even with the macro analysis we performed, we knew that these overall effects could mask distinctive effects on different types of claimants.

III. EMPIRICAL ANALYSIS

Considering that empirical research on the issue of race and civil justice has remained rare, and that our own research on *Daubert* measured overall effects on all litigants but could not tell us whether that was true for different communities, we decided to create a study to test and measure the effect of *Daubert* on communities of color.¹³⁸ Our study would have a similar model to the original *Daubert* work, measuring the reaction of litigants to *Daubert* by assessing changes in case filing data, but would also incorporate demographic data from the U.S. Census Bureau.

A. Summary of Results

Our analysis is intended to determine whether adoption of the *Daubert* evidentiary standard by the federal courts affected Black and White federal court filing rates differently. As we have shown previously, plaintiffs perceive the *Daubert* standard to be a stricter standard than the *Frye* standard it replaced in the federal and (many) state courts.¹³⁹ As a result, when federal courts adopt *Daubert* in states that continue to utilize the older *Frye* standard, federal filing rates decrease as plaintiffs file in state courts with perceived lower evidentiary standards.¹⁴⁰ Later, when states adopt *Daubert* as their evidentiary standard, we see a complete rebound in the federal filing rates as the disincentive to filing in federal court caused by the *Daubert* evidentiary barrier is removed.¹⁴¹ This effect can be seen in the following figure:

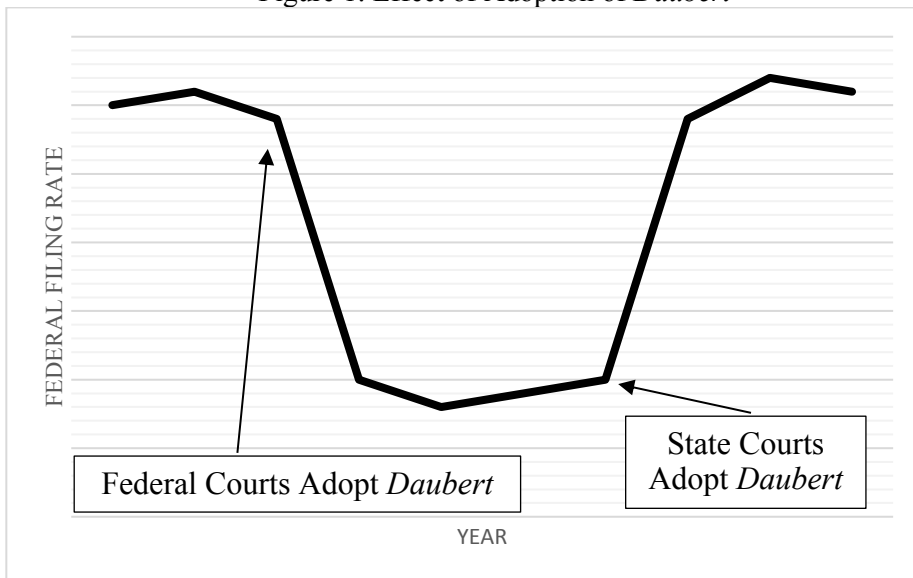
¹³⁷ See Jurs & DeVito, ARK. L. REV., *supra* note 6, at 1002; Jurs & DeVito, CATH. U. L. REV., *supra* note 6, at 701.

¹³⁸ Because the effects of race can be conflated with the effects of poverty, our analysis includes variables relating to poverty as well as race.

¹³⁹ Jurs & DeVito, ARK. L. REV., *supra* note 6, at 1003; *see also supra* text accompanying notes 131–136.

¹⁴⁰ See Jurs & DeVito, ARK. L. REV., *supra* note 6, at 1000 (noting a twenty-one percent decline in the probability of filing in federal court).

¹⁴¹ See *id.* at 1001 (noting a 22% increase in the probability of filing in federal court).

Figure 1: Effect of Adoption of *Daubert*

When examining the effect of *Daubert* through the lens of racial differences—how and if it affects Black and White plaintiffs differently—we expected to see a similar effect as for the population as a whole, but with a difference in the scale of impact for Black and White plaintiffs. We were correct that *Daubert* affected Blacks and Whites differently, but we were incorrect in assuming that the difference would simply be one of scale.

Whenever one seeks to analyze whether one variable is correlated with another, one must be sure that there are no unexamined variables that might explain the correlation in whole or in part. In terms of looking at the interaction of *Daubert* adoption and race, we were concerned that any correlation found might be caused instead by differences in poverty. Currently, and during the period under study, on average Blacks have lower average incomes and a higher poverty rate than Whites.¹⁴² Our concern was that any difference we might find in Black and White filing rates relative to *Daubert* might actually be caused by differences in poverty rates. In essence, poorer populations might be affected differently than richer populations. Since Blacks are, on average, more likely to be in poverty than Whites, on average, this difference could produce results that lined up along racial lines even though the difference was actually one of poverty, not race. To avoid the possibility of drawing a mistaken conclusion, we included poverty rate in our regression formulas.

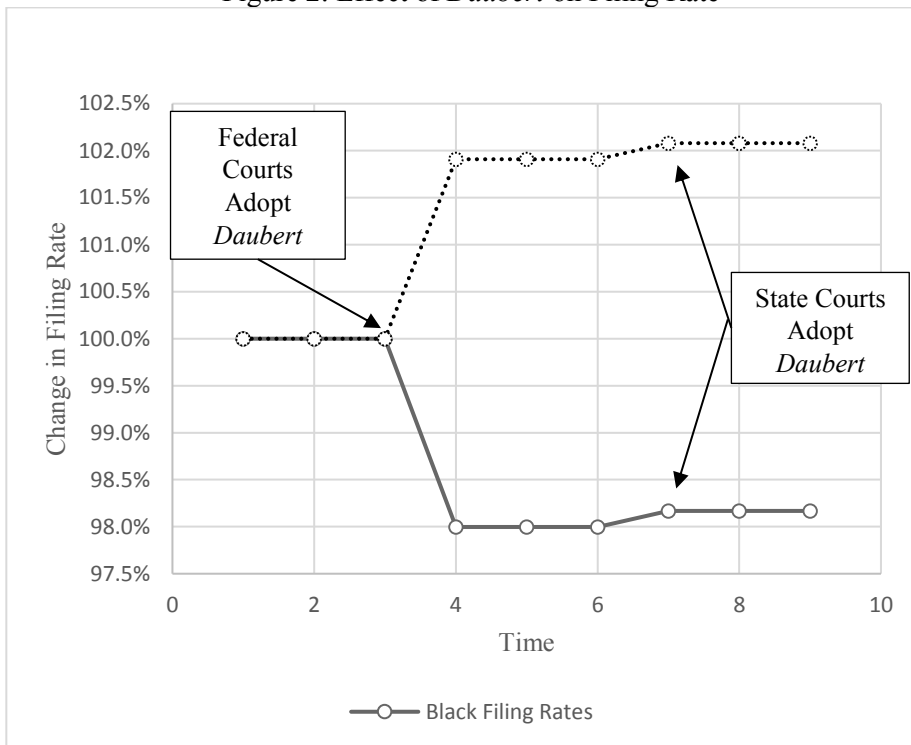
¹⁴² See *Historical Poverty Tables: People and Families - 1959 to 2016: Table 2. Poverty Status of People by Family Relationship, Race, and Hispanic Origin*, U.S. CENSUS BUREAU (Sept. 12, 2017), <https://www2.census.gov/programs-surveys/cps/tables/time-series/historical-poverty-people/hstpov2.xls> [on file with *Ohio State Law Journal*] (listing poverty rate by race from 1959 through 2016).

We then found that the effect of *Daubert* was not race-neutral; rather, when the federal courts adopt *Daubert*, filing rates permanently decrease as the percentage of Blacks in the county increases—even after the state courts adopt *Daubert*. In precise opposition to this result, filing rates permanently increase as the percentage of Whites in the county increases. At the same time, for Blacks and Whites, as the poverty rate increases, so does the filing rate; although the effect is larger when holding percentage of Whites in a county constant than when holding percentage of Blacks in the county constant. We can see this effect in Table 1, which notes the percentage change in filing rates per 1,000 people in a county as each variable increases one percentage point:

Table 1: Effect of Adoption of *Daubert* on Tort Filing Rate in Federal Court, by Race and Poverty Rate

	%Black	Poverty Rate	%White	Poverty Rate
<i>Daubert</i> Adopted By Federal Courts, % Change in Filing Rate	-2.003%	0.040%	1.909%	0.045%
<i>Daubert</i> Adopted By State Court, % Change in Filing Rate	-1.831%	0.072%	2.080%	0.080%

Figure 2, below, presents this information, relative to the percentage of Blacks and percentage of Whites in a population, in a chart form:

Figure 2: Effect of *Daubert* on Filing Rate

From these results, we conclude that *Daubert* is racially harmful in its effect because it permanently removes Black litigants from the federal court system and permanently increases access for White litigants.

B. *The Datasets*

Because we could find no existing dataset that combined census and federal filing data, we were required to create a dataset to perform our analysis. This dataset has, at its core, data from the publicly available court-statistic database created by the NCSC and the Federal Judicial Center (FJC), which contains data relating to every case filed in federal court.¹⁴³

The data we received from the FJC begins in 1960 and ends in 2012 and came in two separate files. The first file contained complete data from 1960 to 1985. It also contained part of the 1986 data. The second file contained complete data from 1987 to 2012. It also contained part of the 1986 data. We first merged these files to give us a dataset that would cover the entire period 1980 to 2012.

¹⁴³ See, e.g., FED. JUDICIAL CTR. INTER-UNIV. CONSORTIUM FOR POLITICAL & SOCIAL RESEARCH, ICPSR 3415, FEDERAL COURT CASES: INTEGRATED DATA BASE, 2001, Part 3: Civil Pending, 2001, 7 (2002) [hereinafter CIVIL TERMINATIONS] (defining the universe of cases as “[a]ll federal court cases in the United States in 2001”) (on file with authors).

To do so, we created a new file that merged the data for 1986 from both files (eliminating duplicates). We then merged the first and second files replacing these two files' data for the year 1986 with that from the third file we had created. This data was then limited to year of filing,¹⁴⁴ county,¹⁴⁵ nature of suit,¹⁴⁶ and origin.¹⁴⁷ Next, we limited the data to just those cases that were original proceedings in the court and over which the court had diversity jurisdiction. This gave us a "complete" database with filing information that we then merged with U.S. Census data to add variables for Black Population, White Population, and Total Population based upon the year of filing and associated County (FIPS) Code.¹⁴⁸ This produced a dataset representing 1,400,857 discrete filings in federal court from the period 1980 to 2012.

We next limited this dataset to torts filed in federal court using the nature of suit ("NOS") variable keeping just the torts:

240 TORTS TO LAND
245 TORT PRODUCT LIABILITY
310 AIRPLANE PERSONAL INJURY
315 AIRPLANE PRODUCT LIABILITY

¹⁴⁴The files do not have a variable for year; instead, they have a field called "filedate" which is the "date on which the case was filed in the district." CIVIL TERMINATIONS, *supra* note 143, at 15 (defining "FILEDATE" as "[t]he DATE on which the case was filed in the district") (emphasis in original). This field is in the format of day-month-year (e.g., "01 Jan 1985"); see *id.* at 11 (describing location of FILEDATE variable in the files). We created a filing year variable by pulling out the filing year from the filedate variable.

¹⁴⁵Counties are "[t]he code for the county of residence of the first listed plaintiff." *Id.* at 19. Counties are identified using their Federal Information Processing Standards ("FIPS") code—a unique code that the federal government assigns to identify counties in the United States. See, e.g., *2010 FIPS Codes for Counties and County Equivalent Entities*, U.S. CENSUS BUREAU, <http://www.census.gov/geo/reference/codes/cou.html> [<https://perma.cc/667K-5RYW>]. A list of FIPS codes is available from the U.S. Census Bureau. See *id.* Filings with a county code of zero were eliminated because they could not be identified to a given county. Filings with an 88888 county code were also eliminated because they indicate that the case relates to a tract of land located within the U.S. but outside the home state. Finally, filings with a county code of 99999 were eliminated because they indicate that the case relates to a tract of land located outside of the United States. See CIVIL TERMINATIONS, *supra* note 143, at 19.

¹⁴⁶The nature of suit variable is "[a] 3 digit statistical code representing the nature of the action filed." CIVIL TERMINATIONS, *supra* note 143, at 16. This includes foreclosure, torts to land, medical malpractice, etc. *Id.* at 16–17.

¹⁴⁷Origin is "[a] single digit code describing the manner in which the case was filed in the district." *Id.* at 15. This includes whether the case was an original proceeding, removed, remanded, etc. *Id.*

¹⁴⁸This was more complicated than it seems at first glance as the publicly available census data is in different formats at different times. As a result, we had to create separate programs for data from 1980 to 1989, 1990 to 1991, 2000 to 2010, and 2011 to 2012. Because of the complexity of this exercise, we not only carefully reviewed the programs that produced the data, but we also verified the data by rewriting all programs to create the data twice from scratch. This ensured the accuracy of the data we used.

320 ASSAULT, LIBEL, AND SLANDER
330 FEDERAL EMPLOYERS' LIABILITY
340 MARINE PERSONAL INJURY
345 MARINE - PRODUCT LIABILITY
350 MOTOR VEHICLE PERSONAL INJURY
355 MOTOR VEHICLE PRODUCT LIABILITY
360 OTHER PERSONAL INJURY
362 MEDICAL MALPRACTICE
365 PERSONAL INJURY -PRODUCT LIABILITY
368 ASBESTOS PERSONAL INJURY - PROD.LIAB.
370 OTHER FRAUD
371 TRUTH IN LENDING
380 OTHER PERSONAL PROPERTY DAMAGE
385 PROPERTY DAMAGE -PRODUCT LIABILITY¹⁴⁹

This produced a dataset with 321,825 individual entries. We then took the individual filings file and created summary filings file for each year and FIPS code that contained the rate of federal filing per 1,000 people in the county (“RFT/1,000”), the percent of the population that was Black (“%Black”), and the percent of the population that was White (“%White”). This produced a file with 52,698 individual entries covering 3,135 FIPS counties for the years 1980 to 2012.

We then supplemented this dataset with poverty rates for each FIPS county from 1980 to 2012 using the following publicly available U.S. Census Bureau datasets:

Poverty rates by county 1960 to 2010;

1989 Poverty rates;
1993 Poverty rates;
1994 Poverty rates;
1997 Poverty rates;
1998 Poverty rates;
1999 Poverty rates;
2000 Poverty rates;
2001 Poverty rates;
2002 Poverty rates;
2003 Poverty rates;
2004 Poverty rates;
2005 Poverty rates;
2006 Poverty rates;
2007 Poverty rates;
2008 Poverty rates;
2009 Poverty rates;

¹⁴⁹ See, e.g., CIVIL TERMINATIONS, *supra* note 143, at 16–18.

2010 Poverty rates;
 2011 Poverty rates; and
 2012 Poverty rates.¹⁵⁰

This enabled us to identify census data on poverty rate for every county in the United States for the years 1980, 1989, 1990, 1993, 1994, and 1997–2012. Because the Census Bureau does not provide poverty-related census data for the years 1981–88, 1991, 1992, and 1994,¹⁵¹ we extrapolated the data for those years.¹⁵² These census datasets were combined to form a dataset that included three variables: FIPS code, year, and poverty rate. We then merged this dataset with our *Daubert* dataset so that for each filing we associated the appropriate poverty rate based on the FIPS code for the filing and the year of the filing.

C. *The Fixed Effects Analysis*

In order to overcome the problem of heterogeneous populations,¹⁵³ we used a fixed effects analysis to identify the correlations between RFT/1,000 and adoption of *Daubert*, and %Black or %White, and poverty rate (“PovRate”). Heterogeneous populations make statistical analysis difficult because they can mask the true cause of changes to the dependent variable.¹⁵⁴ For example, a correlation may be caused by the treatment (adoption of *Daubert*) or by the differences in the populations (highly urban Manhattan, New York compared with highly rural Trail County, North Dakota).¹⁵⁵ Even if we believe we have properly selected homogeneous populations, it is always possible that the populations are heterogeneous due to some unknown variable.¹⁵⁶

¹⁵⁰ See *Poverty Data Tables*, U.S. CENSUS BUREAU, <https://www.census.gov/topics/income-poverty/poverty/data/tables.All.html> [<https://perma.cc/48S9-W7JF>] (providing links to poverty data).

¹⁵¹ One of us, DeVito, confirmed this through a conversation with a member of the Poverty Branch of the U.S. Census Bureau on Oct. 2, 2017. See also *Poverty Data Tables*, *supra* note 150.

¹⁵² For example, we had the data for 1990 and 1993. We calculated $\text{povertyrate}_{1991} = \text{povertyrate}_{1990} - ((\text{povertyrate}_{1990} - \text{povertyrate}_{1993})/3)$; we calculated $\text{povertyrate}_{1992} = \text{povertyrate}_{1990} + (2 * ((\text{povertyrate}_{1993} - \text{povertyrate}_{1990})/3))$.

¹⁵³ Populations that differ relative to one or more statistically relevant factors are called “heterogeneous.” See, e.g., RONALD A. FISHER, *THE DESIGN OF EXPERIMENTS* 32–33 (9th ed. 1971) (discussing the problem of heterogeneity in the context of pairing and grouping); DAMODAR GUJARATI, *ECONOMETRICS BY EXAMPLE 5* (2011) (discussing the problem of heterogeneity); MYOUNG-JAE LEE, *MICRO-ECONOMETRICS FOR POLICY, PROGRAM, AND TREATMENT EFFECTS* 9–10 (2005) (discussing causal inference as compared to statistical association).

¹⁵⁴ See, e.g., LEE, *supra* note 153, at 18.

¹⁵⁵ See, e.g., FISHER, *supra* note 153, at 32–33; GUJARATI, *supra* note 153, at 5; LEE, *supra* note 153, at 9–10 (discussing causal inference as compared to statistical association).

¹⁵⁶ See, e.g., LEE, *supra* note 153, at 18 (discussing the impossibility in any experiment of making the various test populations identical in all relevant respects).

One way to address heterogeneity is to use a randomized, controlled experiment.¹⁵⁷ For example, we would begin by identifying a set of states *A–G* and recording the values of all of the relevant variables relating to those states at time *t*. We would then randomly assign each state to either treatment (adopt *Daubert*) or control (retain current evidentiary standard), and record the values for the relevant variables each subsequent year until time *t'*.

As is obvious from our example, for many studies a controlled, randomized experiment is not feasible, physically possible, or ethically permissible. One way to avoid these difficulties, and limit the effect of heterogeneous populations, is to use a quasi-experimental study (a study that specifically lacks the element of random assignment to treatment or control).¹⁵⁸ We utilize a quasi-experimental fixed-effects analysis that combines before-after analysis, matching, and the use of dummy variables to minimize the potential for heterogeneity to produce misleading results.

In “before-after” analysis, we isolate the effect of treatment, within the same group, by measuring the dependent variable (filing rate) before and after treatment is given.¹⁵⁹ The problem with a before-after analysis is that there might be some unknown covariate *that changes over time* within the population that affects the dependent variable.¹⁶⁰

In “matching,” we compare two populations that we believe are homogeneous relative to the dependent variable. For example, if North Dakota and South Dakota had populations that were, in all relevant ways, identical, we would introduce the treatment (adoption of *Daubert*) in South Dakota but not North Dakota, and then compare the rate of filing between those two states.¹⁶¹ For matching to be effective we must have two comparable populations¹⁶² with the same covariate values,¹⁶³ and that is not always easy to know because there is a risk that there is some unknown variable that explains the result.¹⁶⁴

Combining before-after analysis with matching addresses many of the problems of each individual approach.¹⁶⁵ Nonetheless, we are left with the problem of finding comparable populations. To limit that problem, we can

¹⁵⁷ See, e.g., CHRISTOPHER H. ACHEN, *THE STATISTICAL ANALYSIS OF QUASI-EXPERIMENTS* 1–2 (1986) (discussing randomized, controlled experiments).

¹⁵⁸ See *id.* at 2–5.

¹⁵⁹ See LEE, *supra* note 153, at 64–65.

¹⁶⁰ See *id.* at 65, 79, 99.

¹⁶¹ See *id.* at 79, 99.

¹⁶² James J. Heckman et al., *Matching as an Econometric Estimator*, 65 REV. ECON. STUD. 261, 261 (1998). Two populations are “comparable” if both populations “would have experienced the same outcomes . . . had they participated in the programme.” *Id.* at 262. A central difficulty with matching is ensuring that the two groups are comparable. *Id.*

¹⁶³ See LEE, *supra* note 153, at 79.

¹⁶⁴ See *id.* at 88–90; FISHER, *supra* note 153, at 32.

¹⁶⁵ See LEE, *supra* note 153, at 65, 79, 99.

engage in a fixed-effects regression analysis using dummy variables¹⁶⁶ to account for unknown variables.¹⁶⁷

Our *Daubert* analysis is comprised of two sub-analyses. Each of these analyses required slightly different fixed-effects regression formulas. The first formula (“Flight to *Frye*”) was used to analyze the effect of federal adoption of *Daubert* in states that did not have a *Daubert* standard and took the form:

$$\begin{aligned} \text{FilingRate}_{i,t} = & \beta_0 + \beta_1 FD_t + \beta_2 SD_{i,t} + \beta_3 \text{PerRace}_{i,t} + \beta_4 \text{PovRate}_{i,t} \\ & + \eta_1 S_{1,i} + \eta_2 S_{2,i} + \dots + \eta_{50} S_{m,i} + \gamma_1 Y_{1,t} \\ & + \gamma_2 Y_{2,t} + \dots + \gamma_{20} Y_{20,t} \end{aligned}$$

where $\text{FilingRate}_{i,t}$ is the rate of filing per 1,000 people in FIPS county i and year t ;

β_0 is the y intercept;

FD_t is a binary variable set to 1 if the federal courts have adopted *Daubert* at time t ;

β_1 is the correlation coefficient for FD_t variable;

$SD_{i,t}$ is a binary variable set to 1 if the state courts in state i have adopted *Daubert* at time t ;

β_2 is the correlation coefficient for the $SD_{i,t}$ variable;

$\text{PerRace}_{i,t}$ is the percent of FIPS county i in year t that are Black (when regressing on Black) or White (when regressing on White);

β_3 is the correlation coefficient for the $\text{PerRace}_{i,t}$ variable;

$\text{PovRate}_{i,t}$ is the percent of FIPS county i in year t that falls below the Poverty Rate;

β_4 is the correlation coefficient for the $\text{PovRate}_{i,t}$ variable;

¹⁶⁶ Dummy variables are variables that have a value of 1 if a condition is met and a value of 0 otherwise. GUJARATI, *supra* note 153, at 47. In our case we created one dummy variable for each year and for each state. So we would have Dummy_Alabama, Dummy_Alaska, etc. and Dummy_1983, Dummy_1984, etc.

¹⁶⁷ See LEE, *supra* note 153, at 79 (noting that difference-in-differences analyses “can deal with unobserved confounders to some extent”); see also Jurs & DeVito, CATH. U. L. REV., *supra* note 6, at 716–23 (discussing that “difference-in-differences” models are a special case of fixed effect analysis).

$S_{m,i}$ is a set of binary dummy variables such that $S_{1,i}$ is set to 1 when i is 1 (the state is Alabama) and to 0 otherwise, $S_{2,i}$ is set to 1 when i is 2 (the state is Alaska) and to 0 otherwise, etc.;

η_j is the correlation coefficient for the $S_{m,i}$ variable;

$Y_{r,t}$ is a set of binary dummy variables such that $Y_{1,t}$ is set to 1 when $t = 1$ (the year is 1983) and to 0 otherwise, $Y_{2,t}$ is set to 1 when $t = 2$ (the year is 1984) and to 0 otherwise, etc.;

γ_i is the regression coefficient for the $Y_{r,t}$ variable.

The second *Daubert* analysis (“Return to Federal Court”) was used to analyze the effect of state adoption of *Daubert* in states that adopted a *Daubert* standard after the federal courts. The Return to Federal Court analysis used the same regression formula as the Flight to *Frye* analysis but did not contain the FD_i binary variable nor its correlation coefficient β_1 .

D. *The Results*—*Daubert*

Our analysis of the effect of *Daubert* on filing rates was broken into two interrelated pieces. First, we examined the relationship between RFT/1,000 and race, in the context of federal court adoption of *Daubert*, looking at states that had not adopted *Daubert* during the period when the federal courts had first adopted *Daubert*. This required taking the *Daubert* dataset and limiting it to a seven year period surrounding 1993 (the year *Daubert* was adopted by the federal courts). We then limited our analysis to thirteen states whose state courts either had a *Frye* standard for the entire duration¹⁶⁸ or that adopted *Daubert* after 1993.¹⁶⁹ Because the year that a court adopted *Daubert* was likely to be a confusing year, we eliminated the entire year 1993 and, for each state that adopted *Daubert* subsequent to 1993, we eliminated, for just that state, the year of adoption. Finally, for any state that adopted *Daubert* during the study period but after 1993, we eliminated the data for any year that state had a *Daubert* standard.

Our expectation was that in those states where the federal court adopted *Daubert* while the state did not, we would see a decrease in RFT/1,000 as people fled from federal court (and the stricter *Daubert* standard) to state court (with the less strict state standard). Our results, relative to the adoption of *Daubert*, were consistent with that expectation as the correlation coefficient for federal

¹⁶⁸ Arizona, Florida, Kansas, Michigan, Minnesota, Missouri, New York, and Washington. See Cheng & Yoon, *supra* note 129, at 493.

¹⁶⁹ Alaska (1999), Connecticut (1997), Indiana (1995), North Carolina (1995), and Tennessee (1997). *Id.*

adoption of *Daubert* was negatively correlated with RFT/1,000.¹⁷⁰ At the same time, we found that *Daubert* was not racially neutral in its effect. There was a statistically significant negative correlation between the RFT/1,000 and %Black, and a statistically significant positive correlation between RFT/1,000 and %White. In addition, there was a statistically significant positive correlation between RFT/1,000 and poverty rate regardless of race.

The use of correlation coefficients, while useful in building a model, is often not helpful in itself in understanding the impact of an independent variable (adoption of *Daubert*, race, poverty rate) on the dependent variable (federal filing rate). To clarify this impact, it is helpful to have a context. For our purposes, it will be instructive to focus on average RFT/1,000 for the counties and years under study so that we can “see” the size of the impact of our independent variables on federal tort filing rates. For this portion of the analysis (Flight to *Frye*), the average RFT/1,000 for the counties and years studied was 0.0710201. In essence, there were, on average, 0.0710201 federal court tort filings for each 1,000 people in those counties during the study period.

Our fixed effects analysis found a statistically significant¹⁷¹ negative correlation of -0.0014223 between %Black and RFT/1,000. This means for each 1% increase in the percentage of Blacks in a county, we have a 2% decrease in the filing rate. Poverty rate was also statistically significant¹⁷² with a positive correlation coefficient of 0.000028306. This means that, holding %Black constant, for every 1% increase in the poverty rate in a county we see a 0.04% increase in the federal filings per 1,000 people in the county.

The results for %White were the opposite to those for %Black. We found a statistically significant¹⁷³ positive correlation coefficient of 0.0013558 between %White and RFT/1,000. This means we have a 1.9% increase for each 1% increase in Whites in the county. Once again, poverty rate was statistically significant¹⁷⁴ with positive correlation coefficient of, in this instance, 0.000032009. Thus, holding %White constant, for every 1% increase in the poverty rate in a county we see a 0.05% increase in the federal filings per 1,000 people in the county.

On their face, these results are troubling. Federal adoption of *Daubert*, in states with a *Frye* standard, negatively impacts Black plaintiffs’ access to the federal courts, while positively impacting White plaintiffs’ access to the federal courts.

In the second part of our *Daubert* analysis, we examined the effects of states adopting *Daubert* after the federal courts. This required taking the *Daubert* dataset and limiting it to a nine-year period starting in 1994 (the year after *Daubert* was adopted by the federal courts) and ending in 2002. We then limited

¹⁷⁰ The correlation coefficient for federal adoption of *Daubert*, in this analysis, was -6.07x10⁻³.

¹⁷¹ The *p*-value was < 0.0005.

¹⁷² The *p*-value was < 0.0005.

¹⁷³ The *p*-value was < 0.0005.

¹⁷⁴ The *p*-value was < 0.0005.

to thirteen states whose state courts either had a *Frye* standard for the entire duration¹⁷⁵ or that adopted *Daubert* after 1993.¹⁷⁶

Our expectation was that there would be an increase in RFT/1,000 in those states that adopted *Daubert* as the state court evidentiary standard because the pressure to avoid federal court exerted by the stricter *Daubert* standard would be relieved as both courts now used the same standard. Our results relative to the state adoption of *Daubert* is consistent with this as state adoption is positively correlated with RFT/1,000.¹⁷⁷ But relative to %White, %Black, and poverty rate, we do not see the same kind of rebound that we had seen in our previous study.

It is, once again, useful to use the average federal filing rate per 1,000 people as a context for understanding our results. For this set of counties during the study period the average RFT/1,000 is 0.0701518.

Rather than a rebound in %Black, where the correlation coefficient would be positive or of a similar magnitude to the correlation coefficient in the Flight to *Frye* analysis, we found a statistically significant¹⁷⁸ negative correlation of -0.00128 between %Black and RFT/1,000. This corresponds to a 1.83% decrease in federal filing rates for each 1% increase in Blacks in the county. Poverty rate was, once again, statistically significant¹⁷⁹ with a positive correlation coefficient of 0.000050598. This means that, holding %Black constant, for every 1% increase in the poverty rate in a county we see a 0.07% increase in the federal filings per 1,000 people in the county.

This result is concerning because it entails that the suppression of Black federal filing rates correlated with the federal adoption of *Daubert*, noted above, does not go away with state adoption of *Daubert*. In our previous work (that did not look at race as a factor), we saw a return of federal tort filing rates to pre-*Daubert* levels once the state courts adopted *Daubert*.¹⁸⁰ Here this does not occur—in a race neutral world, we would expect all of that suppression to have been eliminated.

Things are markedly worse, for a race neutral world, when we examine the effect of state adoption of *Daubert* on %White where we found a statistically significant¹⁸¹ positive correlation coefficient of 0.001459058 between %White and RFT/1,000. Thus, for each 1% increase in Whites in a county, we have a 2.08% increase in the federal filing rates. Poverty rate was, once again,

¹⁷⁵ Arizona, Florida, Kansas, Michigan, Minnesota, Missouri, New York, and Washington. See Cheng & Yoon, *supra* note 129, at 493.

¹⁷⁶ Alaska (1999), Connecticut (1997), Indiana (1995), North Carolina (1995), and Tennessee (1997). *Id.*

¹⁷⁷ We used the binary variable “SD” to measure the effect of adoption of *Daubert* by the state courts, after the federal courts had adopted *Daubert*, on federal filing rates. Holding %Black constant, the correlation coefficient for SD was 0.026382. Holding %White constant, the correlation coefficient for SD was 0.026786.

¹⁷⁸ The *p*-value was < 0.0005.

¹⁷⁹ The *p*-value was 0.006.

¹⁸⁰ Jurs & DeVito, ARK. L. REV., *supra* note 6, at 977.

¹⁸¹ The *p*-value was 0.002.

statistically significant¹⁸² with a positive correlation coefficient of 0.000056235. This means that, holding %White constant, for every 1% increase in the poverty rate in a county we see a 0.09% increase in the federal filings per 1,000 people in the county. Once again, in a race neutral world, we would expect White federal tort filing rates to return to their pre-*Daubert* levels. The fact that the correlation coefficient increases (compared to the Flight to *Frye* states) is troubling, to say the least.

Table 2: Effect of the Adoption of *Daubert* on Tort Filing Rate in Federal and State Court, Controlling for Poverty Rate

	Base Filing Rate per 1,000 People	Effect of Change on White Population (% Change per 1% increase)	Effect of Change on Black Population (% Change per 1% increase)
<i>Daubert</i> Adopted by Federal Courts	0.0710201	0.001351089 (1.9% increase)	-0.0014223 (2.0% decrease)
<i>Daubert</i> Adopted by State Court	0.0701518	0.001459058 (2.08% increase)	-0.00128 (1.83% decrease)

From the foregoing analysis we conclude that adoption of *Daubert* has a negative impact on Black federal tort filing rates and a positive impact on White federal tort filing rates simpliciter. In essence, something about the *Daubert* standard pushes Blacks out of federal courts permanently and opens the door for more Whites in federal courts permanently.

E. Coherence of This Result with Previous Findings on *Daubert*

We have previously shown that federal court adoption of *Daubert* in states that had an older *Frye* (or other non-*Daubert*) standard resulted in an overall decrease in filings in federal courts.¹⁸³ In that analysis, we did not analyze the impact of race on filing rates. In our current work, we demonstrate that federal adoption of *Daubert* produces a permanent increase in filing rates for Whites and a permanent decrease in filing rates for Blacks. This appears contradictory—in essence, how can there be a permanent increase in White filing rates and a decrease in filing rates overall when Whites make up the largest portion of the population?

¹⁸² The *p*-value was 0.006.

¹⁸³ See Jurs & DeVito, ARK. L. REV., *supra* note 6, at 977.

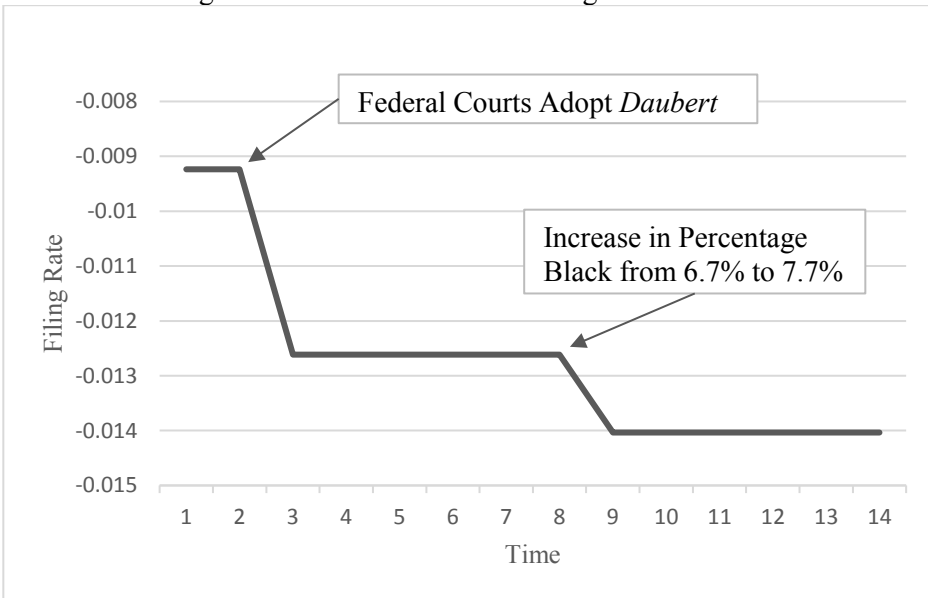
To explain this apparent contradiction, we must first return to our fixed effects regression formula:

$$\begin{aligned}
 \text{FilingRate}_{i,t} = & \beta_0 + \beta_1 FD_t + \beta_2 SD_{i,t} + \beta_3 \text{PerRace}_{i,t} + \beta_4 \text{PovRate}_{i,t} \\
 & + \eta_1 S_{1,i} + \eta_2 S_{2,i} + \dots + \eta_{50} S_{m,i} + \gamma_1 Y_{1,t} \\
 & + \gamma_2 Y_{2,t} + \dots + \gamma_{20} Y_{20,t}
 \end{aligned}$$

As we have previously shown, the correlation coefficient for FD (Federal adoption of *Daubert*) remains negative as plaintiffs flee to the less strict *Frye* standard and the correlation coefficient for SD (State adoption of *Daubert* after Federal adoption) is positive as plaintiffs return to federal court because the impediment of the stricter standard is removed.

At the same time, the correlation coefficient for %Blacks (PerRace looking at percentage reporting as Black) is negative. This means that as the percentage of Blacks increase, then that change will have a negative effect on the filing rate. (In our population, the mean percentage of Blacks is 6.7%.) Because the coefficients for %Black and FD are negative, that means those two variables can have an overall negative effect on the filing rate. For example, if we graph the filing rate starting with no federal adoption of *Daubert*, later federal adoption, then an increase from 6.7% of the population Black to 7.7% we see a negative effect on the filing rate because both FD and %Black have negative correlation coefficients.

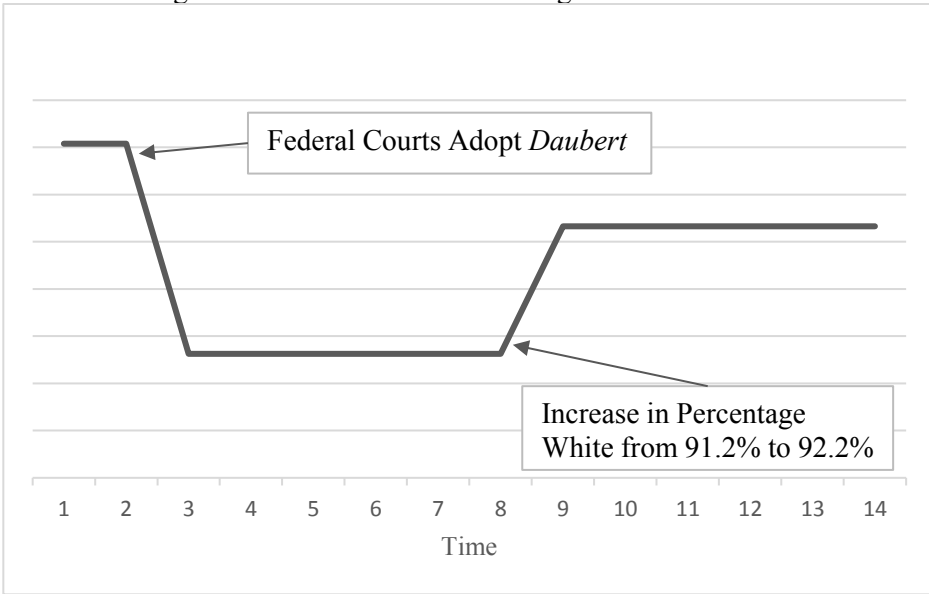
Figure 3: Increase of One Percentage Point in %Black



The correlation coefficient for %Whites (PerRace looking at percentage reporting as White) is positive. This means that, as the percentage of Whites in

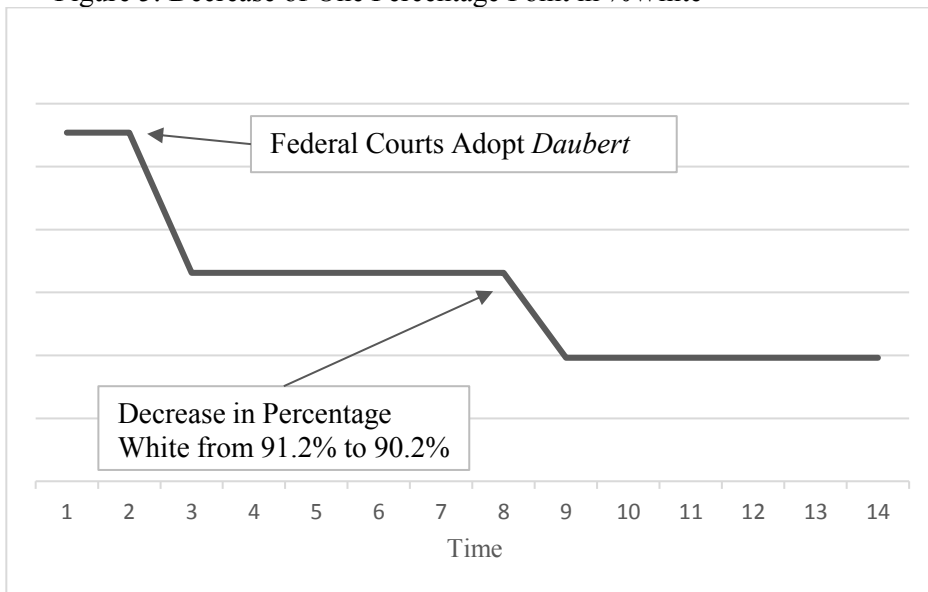
the population increases, there will be an increase in filing rates correlated to that increase. Because the effect of FD is negative, the combined effect of the two variables (ignoring the effects of any others in the regression equation) will be overall negative if there is decrease in %White and will be either positive or negative depending on how big an increase there is in %White. In Figure 4 below, we see that a one percentage point increase from the median (91.2%) percentage White in our population, results in an overall negative effect of FD and %White.

Figure 4: Increase of One Percentage Point in %White



In addition, as Figure 5 below shows, a one percentage point decrease from the median (91.2%) percentage White in our population, results in an overall negative effect of FD and %White.

Figure 5: Decrease of One Percentage Point in %White



In reality, the downward trajectory of the filing rate occurs at the same time as the original effect of *Daubert*. As a result, our data demonstrates that these changes based on %Black and %White occurred at the time of the original decision.

IV. DISCUSSION

Based on our analysis, *Daubert* gatekeeping does not act equally towards all claimants in the civil justice system, but instead acts disproportionately to limit access to justice for communities of color. In response to the federal adoption of *Daubert* in 1993, our previous work has shown a phenomenon we called “Flight to *Frye*,” as claimants leave a venue with a higher burden for evidence for an alternative venue with a lower burden.¹⁸⁴ Our data shows that in response to this change, filing rates among black claimants drop significantly while rates among white claimants actually rise.¹⁸⁵ How can we explain that result?

A. Explanations for the Disproportionate Impact on Claimants of Color

One major consideration clearly involves economic valuation of claims. When an attorney evaluates a potential case, the attorney must give a case a prospective value based on both the “likelihood of recovery” and also “damage

¹⁸⁴ See Jurs & DeVito, ARK. L. REV., *supra* note 6, at 1000–01.

¹⁸⁵ See *supra* Part III.D.

award after recovery.”¹⁸⁶ Should a legal change modify the likelihood of recovery, then the case becomes less desirable for the attorney. The same is true of a claim with exactly the same evidence but lower potential damages. When *Daubert* acts as a stricter standard, it reduces the likelihood of recovery for all claimants, and by so doing, will necessarily change an attorney’s valuation of any prospective case. We believe that this reduction in “likelihood of recovery” after *Daubert* changes some claims—those with lower economic damages—from “borderline yes” to “borderline no” cases for the prospective attorneys. When one considers that black Americans have a higher proportion of the population living in poverty and that the median income of the group is lower than white Americans, the change in case valuation by prospective attorneys—purely from an economic perspective—would disproportionately affect claimants of color.¹⁸⁷

We believe the effect cannot be solely related to income distribution, however. Survey data consistently shows black respondents are less likely to believe they are being treated fairly in the justice system, and all respondents believe people of color are treated worse.¹⁸⁸ It is not hard to see why. In their seminal study on civil justice in Cook County, Chin and Peterson found that claimants of color were less likely to win a recovery of any sort, and even if they won, they received 26% smaller damage awards than white claimants.¹⁸⁹ If these findings are consistent with the civil justice system today, attorneys evaluating a claim from a potential client of color may also be reducing the “likelihood of recovery,” independent of the income valuation component of the calculation. This can also push some cases from “borderline yes” to “borderline no,” again negatively affecting communities of color.

Whether it is based on crass calculations of “likelihood of success” based on perceptions of recovery within a flawed civil justice system, adoption of *Daubert* in federal court pushed people of color out of the civil justice system, disproportionately as compared to white claimants.

The disproportionate effect of *Daubert* may have been mitigated, after 1993, by adoption of the same standard in state court. Our previous work has shown that after 1993, when a state adopted *Daubert* for evaluation of evidence in state court, there was a “return to federal court” effect where claim levels returned to

¹⁸⁶ For a more detailed review of this analysis, see DeVito & Jurs, *supra* note 11, at 86–88.

¹⁸⁷ See, e.g., JOSEPH DALAKER, U.S. CENSUS BUREAU, U.S. DEP’T OF COMMERCE, CURRENT POPULATION REPORTS SER. NO. P60-214, POVERTY IN THE UNITED STATES: 2000, at 7 tbl. B (Sept. 2001), <https://www.census.gov/prod/2001pubs/p60-214.pdf> [<https://perma.cc/A2JW-E7L3>]; CARMEN DENAVAS-WALT ET AL., U.S. CENSUS BUREAU, CURRENT POPULATION REPORTS SER. NO. P60-226, INCOME, POVERTY, AND HEALTH INSURANCE COVERAGE IN THE UNITED STATES: 2003, at 12 tbl. 4 (Aug. 2004), <https://www.census.gov/prod/2004pubs/p60-226.pdf> [<https://perma.cc/CM5Q-RDPD>].

¹⁸⁸ For the national surveys that show this result, see *supra* notes 67–81 and accompanying text. For state survey results, see *supra* notes 99–102 and accompanying text.

¹⁸⁹ See CHIN & PETERSON, *supra* note 85, at viii, 30.

the status quo prior to 1993.¹⁹⁰ Yet, after analyzing these data, we found that even after a state adopts *Daubert*, claims by people of color remain significantly below their pre-1993 levels.¹⁹¹ We believe this demonstrates that once excluded, claimants of color are unlikely to return to the civil justice system.

We thought about why claims do not return to their status quo, and have two possible explanations. First, a prospective attorney's valuation of a particular claim will continue to incorporate the reduction in the "likelihood of success" prong of the calculation because although there is an equilibrium for the admissibility standard for the two venues, the equilibrium occurs with the stricter standard. This is different than a situation where a law is adopted, but later is overturned; in that situation, equilibrium is also reached but at the original standard.

Secondly, the reduction in filings may not respond to new changes in law because once prospective attorneys react to the initial change in law—in this case the adoption of the stricter *Daubert* standard—by modifying their practice to the "new normal," they may not be willing or able to return to the previous practice model even if the law changes again. We documented this effect in a prior paper discussing tort reform efforts.¹⁹² In that piece, we found that adoption of a noneconomic damages cap reduced the filing rate of claims from what it otherwise would have been without the cap.¹⁹³ One would expect that result. However, when we examined the effect of nullification of noneconomic damages caps, we were surprised to see that the nullification of the cap had no statistically-significant effect on filings; there was no "uptick" at all.¹⁹⁴ In fact, for medical malpractice claims, we found that after nullification of the damages cap, filing rates continued to decline even further.¹⁹⁵ We believe the *Daubert* admissibility standard has a similar effect on claimants of color, and in that regard has effects identical to tort reform.

B. Responses for Consideration

Whether these explanations play all, some, or none of the role in the disproportionate impact of the *Daubert* standard on communities of color, we believe the impact of the rule—combined with similar effects within the civil justice system—have led to a crisis of legitimacy within that system. It is not hard to imagine why state commission reports consistently show survey

¹⁹⁰ Jurs & DeVito, ARK. L. REV., *supra* note 6, at 1002; Jurs & DeVito, CATH. U. L. REV., *supra* note 6, at 701.

¹⁹¹ See *supra* Part III.D.

¹⁹² DeVito & Jurs, *supra* note 11, at 106–08.

¹⁹³ *Id.*

¹⁹⁴ *Id.* at 108.

¹⁹⁵ *Id.* at 109 fig.12.

responses from communities of color reporting mistrust, mistreatment, disrespect, and disadvantage in dealings with the law.¹⁹⁶

The issue of race is, of course, not an easy one, and any suggestion for reform will only provide some of the necessary response to mitigate historical wrongs. But we believe the best approach is the most straightforward one: when considering future changes to fundamental rules within the civil justice system—whether substantive, procedural, evidentiary, or otherwise—we must consciously and directly consider the impact of that change on communities of color prior to adoption. We would not expect the impact on communities of color to be the only major consideration in legal reform, but it certainly cannot be pushed aside either. Too long we have predicted that legal changes would affect all people similarly, but we believe that our study, along with other similar work, has shown these changes can disproportionately impact some groups. We should know whether that is true, before legal reform measures are adopted.

We have at least one example of efforts to expose disproportionate impacts leading to consideration of reform. In her work, Professor Chamallas showed how use of race or gender specific tables for economic damages calculations led to reductions in recoveries,¹⁹⁷ while Professor Avraham shows how these damage reductions can lead to perverse incentives.¹⁹⁸ In response, each suggested the same reform: use of blended or mixed tables for determination of tort damages.¹⁹⁹ Whether all judges follow this advice, certainly some judges now can see that a seemingly neutral rule—that damages should reflect the actual loss expected by the individual claimant—can have disproportionate and negative impacts on minority or female claimants. In response, Avraham notes that Judge Weinstein in New York now requires blended tables.²⁰⁰

We propose that instead of waiting until a disproportionate and negative effect can be measured, usually requiring waiting years for data to arrive, we explicitly make these considerations prior to major law reform.

C. *Suggestions for Future Research*

Helland and Tabarrok wrote in 2003 that “[i]t is odd that so little attention has been paid to the role of race and poverty in the American tort system”²⁰¹ while in 2008, Cardi noted that “existing scholarship only scratches the surface of this important topic.”²⁰² We agree with these and other authors who lament the paucity of research into race and the civil justice system. Clearly, our efforts

¹⁹⁶ See generally *supra* Part II.B (discussing state and national studies on discriminatory effects in the civil justice system).

¹⁹⁷ See Chamallas, *supra* note 21, at 1439.

¹⁹⁸ See AVRAHAM, *supra* note 37, at 5, 8–11.

¹⁹⁹ AVRAHAM, *supra* note 37, at 10, 65; Chamallas, *supra* note 21, at 1450.

²⁰⁰ AVRAHAM, *supra* note 37, at 20 (citing *McMillan v. City of New York*, 253 F.R.D. 247 (E.D.N.Y. 2008)).

²⁰¹ Helland & Tabarrok, *supra* note 4, at 28.

²⁰² Cardi, *supra* note 4, at 120.

on evaluating *Daubert* and its impact on communities of color only scratch the surface of the research needed for comprehensive evaluation of fairness within the civil justice system. Where to start?

Even within the area of scientific admissibility and *Daubert*, work remains to be done. In our research model, we combined case filing data and census data in order to evaluate the impact of *Daubert* on case filings within communities of color.²⁰³ However, the analysis relies on the following assumption, one that has also affected researchers like Helland and Tabarrok:²⁰⁴ when using aggregate data, one must assume that the racial makeup of claimants within a district generally reflects the racial composition of the district as a whole. As in any empirical research, more precise data could eliminate the necessity for this assumption, reasonable though it may be.

Beyond the *Daubert* arena, research into case filing rates could also evaluate a variety of other legal changes. We have, in prior work, evaluated the case filing effects of tort reform, finding that it acts as intended to reduce claims.²⁰⁵ Clearly, a researcher could use these tools to evaluate the effect of tort reform on communities of color or women, as some have already started to do,²⁰⁶ in order to see if theorists are correct that they bear disproportionate impacts of tort reform measures. Once the research method is clear, the same approach can be used to measure the impact of any past change to the law, even something as benign as the amount-in-controversy requirement for federal diversity jurisdiction.

We therefore believe that empirical research into legal changes in the civil justice system is sorely lacking, needed, and once used, can provide important insight into disproportionate impacts of seemingly neutral law reforms.

V. CONCLUSION

Civil justice reform efforts, even seemingly neutral or necessary reforms, can and sometimes do, hide nasty disproportionate effects on specific subcategories of claimants, particularly people of color. In an earlier series of papers, our empirical analysis of *Daubert*'s overall effect showed that both civil plaintiffs and civil defendants act in ways that demonstrate that *Daubert* is stricter. Yet this broader finding, we now have discovered, masked other effects; in fact, *Daubert* did not have one effect on all claimants, it disproportionately and negatively affects claimants of color. Our analysis shows that *Daubert*

²⁰³ Regarding the datasets and analysis we performed, see *supra* Part III.B, III.C.

²⁰⁴ Helland & Tabarrok, *supra* note 4, at 41.

²⁰⁵ See DeVito & Jurs, *supra* note 11, at 107.

²⁰⁶ See, e.g., NICHOLAS M. PACE ET AL., RAND INST. FOR CIVIL JUSTICE, CAPPING NON-ECONOMIC AWARDS IN MEDICAL MALPRACTICE TRIALS: CALIFORNIA JURY VERDICTS UNDER MICRA, at xxiii (2004), <http://bit.ly/N3oLZH> [<https://perma.cc/2HBH-V29C>]; Lucinda M. Finley, *The Hidden Victims of Tort Reform: Women, Children, and the Elderly*, 53 EMORY L.J. 1263, 1313 (2004).

resulted in fewer claims by black claimants, and that once the claimants are out of the system, they stay out.

Troubling as this finding may be, our hope is that exposing this and other discriminatory effects within the civil justice system will lead to efforts within the system to proactively evaluate disparate impacts prior to future reforms. Such efforts would enhance the fairness of the civil justice system, and act in concert with similarly minded criminal justice reform measures to improve the legitimacy of the judicial system overall. A nation committed to equal justice under law should expect no less.

