

**IN THE UNITED STATES DISTRICT COURT  
FOR THE WESTERN DISTRICT OF KENTUCKY  
LOUISVILLE DIVISION**

**Chelsey Nelson Photography LLC,  
and Chelsey Nelson,**

Plaintiffs,

v.

**Louisville/Jefferson County Metro  
Government; Louisville Metro  
Human Relations Commission–  
Enforcement; Louisville Metro  
Human Relations Commission–  
Advocacy; Verná Goatley, in her  
official capacity as Executive Director of  
the Louisville Metro Human Relations  
Commission–Enforcement; and Marie  
Dever, Kevin Delahanty, Charles  
Lanier, Sr., Leslie Faust, William  
Sutter, Ibrahim Syed, and Leonard  
Thomas, in their official capacities as  
members of the Louisville Metro Human  
Relations Commission–Enforcement,**

Defendants.

**Case No. 3:19-cv-00851-BJB-CHL**

**Plaintiffs' Reply in Support of  
Their Motion to Exclude  
Testimony of Netta Barak-Corren**

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## **Introduction**

Professor Netta Barak-Corren's conclusions are speculative, her methods are unreliable, and her findings are irrelevant. *See* Pls.' Mot. to Exclude Testimony of Netta Barak-Corren (MTE), ECF No. 90. Her testimony should be excluded.

More fundamentally, Barak-Corren's conclusions are troubling. According to her, courts should tolerate religious hostility and reject religious exemptions because the media *might* incorrectly describe court decisions, which in turn *might* cause creative professionals to exercise their constitutional rights (what Barak-Corren calls discrimination). Barak-Corren makes this conclusion with no evidence about what media creative professionals consume or how they react to which media and without using any reliable methodology. And then she applies her conclusion to this litigation with no evidence about Louisville, Louisville media, Louisville court decisions, or Louisville creative professionals—all the while overlooking the evidence *from her own study* that indicates that Louisville won't be affected by the so-called *Masterpiece* effect.

The unspoken assumption beneath Barak-Corren's testimony is that courts should consider how the media report on court decisions to avoid problematic results that may or may not occur because of those reports. But neither the media nor the masses dictate fundamental freedoms. The Bill of Rights "withdraw[s] certain subjects from the vicissitudes of political controversy, to place them beyond the reach of majorities and officials and to establish them as legal principles to be applied by the courts." *W. Va. State Bd. of Educ. v. Barnette*, 319 U.S. 624, 638 (1943). Barak-Corren's testimony should be excluded.

## **Argument**

Barak-Corren's testimony should be excluded because it is (I) unreliable and (II) irrelevant.

**I. Barak-Corren’s testimony should be excluded as unreliable.**

Barak-Corren’s testimony should be excluded because she does not use “reliable principles and methods,” has insufficient “facts or data,” and does not “reliably appl[y] the principles and methods to” this case. Fed. R. Evid. 702(b)–(d).

Contrary to Louisville’s claim, Chelsey does not challenge Barak-Corren’s testimony because of imperfect or weak methodology. Defs.’ Resp. to Pls.’ Mot. to Exclude Testimony of Netta Barak-Corren (RMTE) 6–7, ECF No. 99. Her testimony is unreliable. That’s Rule 702’s purpose—to weed out unreliable testimony. *See Tamraz v. Lincoln Elec. Co.*, 620 F.3d 665, 675 (6th Cir. 2010) (experts much reach conclusions “via a sound methodology.”); Fed. R. Evid. 702 advisory committee’s note, 2000 amend. (“[A]ny step that renders the analysis unreliable ... renders the expert’s testimony inadmissible.”).

Though Louisville disagrees, this purpose applies to “a bench trial.” RMTE 6. Rule 702 has no bench-trial exception. Louisville’s cases don’t say otherwise. *Cf. Deal v. Hamilton Cnty. Bd. of Educ.*, 392 F.3d 840, 851 (6th Cir. 2004) (opposers “provide[d] no legal arguments” for to exclude expert); *Douglas v. United States*, 2011 WL 2633612, at \*6 (E.D. Ky. July 5, 2011) (“[E]xperts must pass *Daubert* scrutiny.”). And courts nix unreliable experts at summary-judgment. *See, e.g., Newell Rubbermaid, Inc. v. Raymond Corp.*, 676 F.3d 521, 526–29 (6th Cir. 2012).

This Court should likewise exclude Barak-Corren’s unreliable testimony that (A) speculates about media; (B) reaches unreliable results with unreliable methods; (C) misapplies legal regimes; and (D) has no facts about Louisville.

**A. Barak-Corren’s testimony is unreliable because she depends on speculative assumptions about the media.**

The *Masterpiece* Study depends on what creative professionals understood about *Masterpiece* “as mediated through and filtered by the media.” RMTE Ex. 2 (Tr.) 189, ECF No. 99–2. But Barak-Corren’s testimony speculates about

professionals' exposure to media reports about the *Masterpiece* decision. That speculation makes her testimony unreliable.

Louisville admits "it was not possible to examine what media sources" professionals saw after the *Masterpiece* decision. RMTE 8. Barak-Corren cannot measure post-*Masterpiece* effects without knowing if professionals knew about *Masterpiece* in the first place. MTE 5–6 (making same point). So her conclusions without that knowledge are unreliable.

Louisville cites two studies to cover up this problem. RMTE 8.<sup>1</sup> But neither fill the holes Louisville needs. In the first study, Tankard and Paluck knew about participants' media consumption because they "reported consuming mass media news about 3 to 4 times per week, with a 57% majority reporting daily consumption, which suggests that few participants were unaware of the ruling." 28 Psych. Sci. at 1339. Barak-Corren relies on this study to conclude that professionals *could* learn of the decision without media *if* the decision "spread[s] in society and shape[s] social views." Tr. 93:7–9. This is speculative. Unlike Tankard and Paluck, she has no evidence about whether professionals followed media or whether society generally knew about *Masterpiece*.

Likewise, Linos and Twist measured survey subjects' "exposure" to news coverage by asking them "whether they had heard about a series of seven prominent news stories around the time of the decisions." 45 J. Legal Stud. at 240. Then they divided the subjects between "the treated group"—those with some level of exposure—from the untreated group. *Id.* Throughout, they stressed

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<sup>1</sup> Margaret E. Tankard & Elizabeth Levy Paluck, *The Effect of a Supreme Court Decision Regarding Gay Marriage on Social Norms and Personal Attitudes* (Tankard & Paluck), 28 Psych. Sci. 1334 (2017); Katerina Linos & Kimberly Twist, *The Supreme Court, the Media, and Public Opinion: Comparing Experimental and Observational Methods* (Linus & Twist), 45 J. Legal Stud. 223 (2016).

“distinguish[ing] people who heard and understood the decisions from those who did not.” *Id.* at 239. Barak-Corren did none of this.

Besides no evidence of media exposure, Barak-Corren also cannot measure what specific media professionals saw (if any). MTE 7–8. Louisville re-frames the argument as “media bias” and claims any media bias is “irrelevant” because the *Masterpiece* Study doesn’t “measur[e] the impact of any media bias but rather the effect of the decision itself.” RMTE 8. Louisville’s argument misses a few key steps.

Recall that Barak-Corren relies on the media to communicate about *Masterpiece*. MTE Ex. B (HCRCL) 24–27, 47–48 n.150, ECF No. 90–3. And professionals’ understanding of *Masterpiece* is filtered through the media. Tr. 189. So what the media says about *Masterpiece* to the public is critical. And what the media says about *Masterpiece* depends on whether the media source is mainstream, progressive, or conservative. “[M]ainstream” and “progressive” outlets classified *Masterpiece* narrowly or critically while “conservative” media supposedly had “less reservations about its scope.” HCRCL 25–27.

Barak-Corren’s failure to match professionals with media sources makes her testimony unreliable. If professionals saw only mainstream or progressive media, they would have understood the decision narrowly or critically. If professionals saw only conservative media, then their *Masterpiece* knowledge would have depended on what type of conservative media they saw (because those reports varied). MTE 7–8.

This failure also undermines Barak-Corren’s conclusion that *Masterpiece* led to “[c]hanges in social norm perceptions.” HCRCL 48. This theory assumes “that an individual’s behavior depends ... on what actions she believes others will approve or disapprove.” Richard H. McAdams, *An Attitudinal Theory of Expressive Law*, 79 Or. L. Rev. 339, 340 (2000). HCRCL 47 n. 149 (citing same). Professionals who saw only mainstream media—where the decision was explained as “narrow” and as “not resolv[ing] the big constitutional questions at issue”—or only progressive media—



who reported on the case as a “license to discriminate”—would have no reason to think that *Masterpiece* ushered in a new social norm. HCRCL 25–27. Even those who viewed only “conservative” media would not have reached that conclusion because “conservative” reports on *Masterpiece* varied. MTE 8.

And professionals who saw nuanced conservative coverage or a mix of mainstream, progressive, or conservative coverage saw “two-sided coverage,”—i.e. coverage with both “supportive and critical information”—which makes it unlikely they would change their opinion about providing services for same-sex weddings. Linos & Twist, 45 J. Legal Stud. at 225–26. That’s because two-sided coverage “reduce[s] the impact of the Court decision on opinion change.” *Id.* Louisville’s contrary argument about two-sided coverage misstates the study.<sup>2</sup> RMTE 8.

Louisville claims that these “nuances are irrelevant because the *Masterpiece* decision was broadly reported and understood as a victory for a baker.” RMTE 9. That claim points up the problem. There’s no evidence about what professionals “understood” about the decision through media because Barak-Corren never measured that. And the media that Barak-Corren cites—mainstream, progressive, and conservative—demonstrate that the decision was not “broadly reported” as a victory. Indeed, most often, media called the decision narrow or criticized it. HCRCL 25–27. MTE Ex. E (GY Report) ¶¶ 30–31, ECF No. 90–6.

Finally, Louisville says that “[t]he fact that Professor Barak-Corren still found a significant *Masterpiece* effect” even with narrow and critical media reports “demonstrates the significant impact of the ruling.” RMTE 9. But that assumes,

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<sup>2</sup> See, e.g., Linos & Twist, 45 J. Legal Stud. at 226 (“Two-sided coverage, ... reduce[d] the impact of the Court decision on opinion change.”); *id.* at 230 (“[I]ndividuals who receive two-sided, competing frames are more likely to retain their original views.”); *id.* at 232 (“[P]eople who receive two-sided information should respond less positively; indeed, the net effect might be zero or even negative, depending on the relative strength of the competing frames.”); *id.* at 242 (“[O]ne-sided coverage produces larger effects than does two-sided coverage.”).

without evidence, the conclusion—that publicity about *Masterpiece* caused professionals to change behaviors. Assumptions without facts are unreliable. MTE 9–10 (collecting Sixth Circuit cases).

Even beyond that, Louisville never addresses the many other problems with Barak-Corren’s dependence on media. For example, her study cannot be replicated and Barak-Corren goes against the grain of the generally accepted practice of studying media, the Supreme Court, and public opinion. *Id.* at 8–9. These problems independently justify excluding Barak-Corren’s testimony.

**B. Barak-Corren’s methodological mistakes lead to unreliable conclusions.**

The *Masterpiece* Study’s methodological errors make it unreliable. For starters, Barak-Corren cannot measure pre-*Masterpiece* discrimination because of attrition rates in responses between Waves 1 and 2. MTE 12. Louisville argues Barak-Corren solved this problem because she “tested her conclusion against different data sets”: (1) a control group of professionals; (2) businesses that indicated willingness to serve both types of couples pre-*Masterpiece*; (3) businesses that indicated willingness to serve same-sex couples pre-*Masterpiece*; and (4) within business transitions before and after *Masterpiece*. RMTE 12. Louisville is incorrect.

The control group is a misnomer. Normally, control groups and experimental groups are the same “except that the experimental groups are subjected to treatments ... believed to have an effect on the outcome of interest while the control group is not.” Mary Earick Godby, *Control Group*, Britannica, <https://bit.ly/3nf2ACN>. Here, the “treatment” was supposedly exposure to the *Masterpiece* decision. But Barak-Corren does not examine the “control group’s” knowledge of *Masterpiece*. So the control group isn’t really controlling for anything. It’s just a group of professionals contacted after *Masterpiece*.

The final three data sets suffer from the regression fallacy because they depend on the unusually high positive response rate to same-sex requests in Wave 1. HCRCL 36 (70.8% Wave 1 response rate). MTE 13–14. Professionals who positively responded to same-sex wedding requests pre-*Masterpiece* will regress to their average same-sex responsiveness post-*Masterpiece*. *Id.* This logic applies to professionals who responded positively to same-sex *and* opposite-sex couples pre-*Masterpiece*. Those professionals’ responsiveness to same-sex requests will regress towards the mean. In both cases, Barak-Corren only subsets professionals who responded favorably to same-sex requests before *Masterpiece*. By definition, these response rates to same-sex inquiries after *Masterpiece* had nowhere to go but down.

Likewise, the regression fallacy applies to within business transitions. Barak-Corren measured the percent of professionals transitioning “from no/negative response pre-*Masterpiece* to positive response post-*Masterpiece*, and vice-versa, for same-sex and opposite-sex couples.” MTE Ex. C (JLS) 29–31, ECF No. 90–4. Barak-Corren posits that “same-sex couples were twice as likely to experience a negative transition, such that a previously willing business would decline to provide service post-*Masterpiece*.” *Id.* at 30. But again, Wave 1 professionals had atypically high positive responses to same-sex requests pre-*Masterpiece*. So any business transition from agreeing pre-*Masterpiece* to declining post-*Masterpiece* likely underwent regression. Statistics—not discrimination—accounts for the responsiveness change.<sup>3</sup>

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<sup>3</sup> Louisville also denies the regression fallacy because Barak-Corren compared responses for opposite-sex and same-sex couples in Waves 3 and 4. RMTE 13. According to Louisville, “[a]bsent the *Masterpiece* effect, the response rates would have been the same for both couple types.” *Id.* But there’s no evidence for that—Louisville just assumes that all professionals respond identically to all inquiries. Barak-Corren doesn’t even agree with that. MTE Ex. D (App.) 1–10, ECF No. 90–5 (explaining photographers are “pickier in general about their customers ...”).

Take a sports analogy. There's a theory that athletes decline between the first and second years of their careers—i.e., the “Sophomore Slump.” Debra Wetcher-Hendricks, *Does the Sophomore Slump Really Exist?*, Theory in Action 7(3) (2014) (attached as Ex. S). A professor tested this theory by analyzing baseball careers of Rookie of the Year (ROY) award recipients. *Id.* at 65–68. She found that after outstanding rookie seasons, these players' performance dipped in their second year and then remained consistent for the rest of their careers. *Id.* at 67. This led to two conclusions. First, ROY players regress to their average performance after their first-year high-level performance. *Id.* at 68. Next, “[s]econd-year performance ... is not unusually low” but “a Freshman Fluke, characterized by a comparatively good performance during the players' first years in the Major Leagues, exists.” *Id.*

The Freshman Fluke. That's Barak-Corren's Wave 1. Like the unusually successful rookie seasons for ROY players, Wave 1 contained an unusually high positive-response rate to same-sex requests relative to opposite-sex requests. All of Barak-Corren's post-*Masterpiece* analysis (and the so-called *Masterpiece* effect) depends on and is measured against Wave 1. But Barak-Corren never considers the possibility that any changes in post-*Masterpiece* responsiveness occurred because of a statistical correction. To continue the sports analogy, “a true regression to the mean situation suggests that the change in performance [after the first year] reflects statistical, rather than an athletic phenomenon.” MTE Ex. S at 68. Applied here, the responsiveness change could result from regression to the mean rather than discrimination. But Barak-Corren just assumes discrimination. MTE 14.

Barak-Corren's testimony strikes out in other ways too. For example, Barak-Corren significantly altered the content of the emails she sent to professionals in each wave. MTE 14–16. Louisville cites a study by to remedy this problem.<sup>4</sup> RMTE

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<sup>4</sup> Marianne Bertrand & Sendhil Mullainathan, *Are Emily and Greg More Employable than Lakisha and Jamal? A Field Experiment on Labor Market*

MTE 14–15. But that study doesn’t help. That study measured racial bias in the labor market by submitting nearly identical resumes to employers while assigning half of the resumes “White-sounding names” and the other half “African-American-sounding names.” Bertrand & Mullainathan, 94 Am. Econ. Rev. at 992; *id.* at 1006 (conclusions based on “identical individuals” with different names). The study “randomly assigned” a race “to each resume” to “guarantee[] that any differences [in employer callbacks] ... are caused solely by the race manipulation.” *Id.* at 994. By contrast, Barak-Corren did not randomize her emails—i.e., she always sent one email from same-sex couples and a different email from opposite-sex couples. App. 1–10. In sum, Barak-Corren altered the content of the emails systematically (i.e., same-sex and opposite-sex requests) and did not randomize them, so the change in response rate could be because of the different content. GY Report ¶¶ 11–14.

Louisville doubles down by arguing these changes don’t alter the *Masterpiece* Study because Barak-Corren measured professionals’ responses “to identical inquiries from same- and opposite-sex couples after the *Masterpiece* decision.” RMTE 15. But this measurement is meaningless because Barak-Corren cannot know “the extent of discrimination towards same-sex couples ... before *Masterpiece*.” Tr. 147:12–21, 156:7–23. Without knowledge of pre-*Masterpiece* refusals, Barak-Corren cannot claim that refusals increased post-*Masterpiece*.<sup>5</sup> And any equal distribution “across couple types” or any differences in “non-responses” just confirms the regression to the mean analysis—i.e., that post-*Masterpiece* professionals responded based on their average responsiveness. RMTE 14–15.

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*Discrimination* (Bertrand & Mullainathan), 94 Am. Econ. Rev. 991 (2004) (attached as Exhibit R)).

<sup>5</sup> For these same reasons, Louisville is wrong to suggest that Barak-Corren’s conclusions do “not depend on any comparison to pre-*Masterpiece* responses.” RMTE 13. The study evaluates the effects of religious exemptions on professionals’ willingness to provide services for same-sex weddings as judged by their pre-and-post-*Masterpiece* responses to same-sex inquiries. *See, e.g.*, HCRCL 1, 24.

Another problem with Barak-Corren’s testimony is that she codes non-responses as negative responses. MTE 14–17. Louisville claims this doesn’t matter because Barak-Corren evaluated “systemic differences” between same-sex and opposite-sex inquiries “not isolated instances of non-responses.” RMTE 13. Even so, there were many variables that could have explained systematic differences in responses. MTE 14–17. Barak-Corren never addresses them.

**C. Barak-Corren’s testimony is unreliable because she misclassifies and misapplies legal regimes.**

Next, Barak-Corren’s testimony is unreliable because she tries to distinguish Louisville from Texas’s +RFRA/+AD jurisdictions based on a legal error and mischaracterizes other jurisdiction’s legal regimes.

In Texas, Barak-Corren found that +RFRA/+AD jurisdictions were immune from the *Masterpiece* Study. MTE 10– 11. She does not extend this conclusion to Louisville (even though it is also a +RFRA/+ AD jurisdiction) because she believes Kentucky’s Religious Freedom Restoration Act differs from Texas’s. *Id.* That was a mistake—the laws’ text is basically the same. *Compare* K.R.S. § 446.350 *with* Tex. Civ. Prac. & Rem. Code Ann. § 110.003(a). While Texas’s RFRA cannot be used as a defense “to a civil action ... under a federal or state civil rights law,” this exemption doesn’t apply to local civil rights laws. Tex. Civ. Prac. & Rem. Code § 110.011(a). So both RFRA’s provide defenses to local anti-discrimination laws. MTE 10–11.

Louisville tries to avoid this conclusion, first saying that Texas’s “carve-out” appears “on the face of the language in Texas[’s] ... RFRA.” RMTE 11. But Barak-Corren never explains how this limited carve-out for federal and state law distinguishes Texas’s RFRA’s from Kentucky’s. Next, Louisville says that neither caselaw nor Austin’s defense of its local antidiscrimination law show Texas’s RFRA protects against local antidiscrimination laws. RMTE 10 n.1. But the text of Texas’s RFRA speaks for itself. Tex. Civ. Prac. & Rem. Code § 110.011(a). Other authorities

agree. MTE 11. Louisville finally hedges her conclusions as “preliminary.” RMTE 11. But Barak-Corren unequivocally testified that “the report would have looked very differently” if Chelsey lived in Austin. Tr. 172:21–24. That’s decisive.

Louisville also never addresses how Barak-Corren’s misclassified legal regimes. MTE 11 n.8. Barak-Corren felt using varied legal regimes was “necessary” to uncover “real-world variation.” HCRCL 32. That Barak-Corren botched a pivotal part of her study shows her testimony rests on unreliable foundations.

**D. Barak-Corren’s testimony is unreliable because she has no facts about how her study would apply in Louisville.**

Barak-Corren’s testimony is also unreliable because she cannot compare Louisville to any of the studied states and she has no evidence about Louisville’s creative professionals, how her study would apply to a district court opinion, or how her study would apply today. MTE 17–22.

Start with comparing Louisville to the studied states. Barak-Corren tried to link *Louisville* to the studied states by comparing *Kentucky*’s demographics with the four studied states. MTE Ex. A (NBC Report) ¶¶ 17–23, ECF No. 90–2. Aside from religiosity, Barak-Corren made no attempt to compare Louisville’s demographics to the studied states. *Id.* So there’s no evidence that Louisville shares any similarities with any state in the *Masterpiece* Study. And there’s no basis for Barak-Corren to assume that Louisville parallels Kentucky. MTE 18–19 (noting different attitudes and political affiliations).

As for Louisville’s religiosity, Barak-Corren’s “evidence” was a Wikipedia page and three websites. NBC Report ¶ 20 nn. 2–3. Those are not reliable sources. MTE 19 (citing cases). And Louisville concedes Barak-Corren had no data on religious density in Louisville “as compared to Kentucky more generally.” RMTE 16. While Barak-Corren may have had information about religious density about Kentucky (*id.* at 16), Louisville is still not Kentucky. Without religiosity evidence,



Barak-Corren’s testimony is unreliable. Her conclusion depends on Louisville’s religiosity—she “expect[s] to observe the *Masterpiece* effect in Louisville” because of “the high degree of religiosity in the area.” NBC Report ¶ 23. Barak-Corren has no evidence about Louisville’s religious density. So that conclusion has no support.

Next, Barak-Corren never audited professionals in Louisville. Louisville does not dispute this. So Barak-Corren cannot know if professionals in Louisville would react the same to the *Masterpiece* Study as a florist in Floyd, Iowa, a photographer in Plano, Texas, or a baker in Bakersville, North Carolina.

Barak-Corren also has no evidence about how the *Masterpiece* Study would apply to a favorable ruling from this Court. Barak-Corren has no information or study on media and district court decisions and never claims district court and Supreme Court opinions attract the same media attention. MTE 16-17. Barak-Corren has no facts about how anyone would react to a district court decision.

Even so, Louisville cites several local articles and a press release to argue that media “should certainly be expected to publicize the Court’s ultimate decision.” RMTE 18. This response has two problems. First, Barak-Corren never mentioned or considered these articles. Louisville cannot add evidence to Barak-Corren’s testimony after-the-fact. Second, Barak-Corren had no methodology for choosing which media sources supposedly communicate to the public about judicial opinions. MTE 8–9 (making this point). So Louisville cannot even test its own theory. It is impossible to know if Barak-Corren would find the local articles to be relevant news sources, much less Louisville creatives. HCRCL 25–27.

Finally, Barak-Corren has no evidence about how the *Masterpiece* Study might apply to present-day Louisville. MTE 19–20. Louisville counters that the *Masterpiece* effect harms “same-sex couples even if it is short in duration.” RMTE 19. That misses the point. The so-called *Masterpiece* effects are not just short-lived. The point is that there’s no evidence that any effect would happen in Louisville



today were the study re-run.<sup>6</sup> GY Report ¶ 21. Is the present-day social environment conducive to or immune from the *Masterpiece* Study? No one knows.

Louisville next argues that the lack of complaints “could be caused by numerous factors.” RMTE 18. Yes, present-day factors are relevant. And they’re relevant to the wedding industry and professionals too. But Barak-Corren does not account for any of these factors. For example, she does not “know what COVID did to weddings” and agrees “maybe the [weddings] field has changed.” Tr. 113:6–13. These contemporary nuances prove that Barak-Corren has no evidence about how the *Masterpiece* Study would apply today. That makes her conclusions unreliable.

Barak-Corren’s total lack-of-evidence distinguishes her testimony from the other cases Louisville cites where plaintiffs challenged less-than “perfect methodology,” critiqued research designs, or extrapolated from some data. RMTE 6–7, 15–16. Barak-Corren’s testimony has no data about Louisville and is unreliable.

## **II. Barak-Corren’s testimony should be excluded as irrelevant.**

Barak-Corren’s testimony should also be excluded because it is irrelevant to Chelsey’s free-speech claim and analyzing her claims under strict scrutiny.

As for Chelsey’s free-speech claim, Chelsey claims that the First Amendment protects her from creating photographs and writing blogs celebrating messages she opposes. Pls.’ Br. in Supp. of Their Summ. J. Mot. 6–15, ECF No. 92–1. And there’s no dispute that Chelsey’s photographs and blogs are speech. *Id.* at 6–7.

The *Masterpiece* Study is irrelevant to this claim because it only measured the effect of media reports on religious exemptions, as Louisville admits. Tr. 188:7–189:25; RMTE 20. Barak-Corren does not claim that her study applies to free-

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<sup>6</sup> Louisville claims that “the impact of judicial decisions is prolonged and significant.” RMTE 18–19. The cited studies are irrelevant because they either dealt with legislation or proved any effect only lasted up to 18 months. RMTE 19. And Barak-Corren admitted no study addressed effects on professionals. MTE 20.

speech exemptions. NBC Report ¶ 12 (granting Chelsey “a religious exemption from the application of Louisville’s” law could increase discrimination).

Louisville counters that Barak-Corren “*had no assumptions* about” her potential findings. RMTE 19. That may be. But it is irrelevant. The *conclusion*—that all negative or non-responses were discriminatory—*assumes* that professionals declined same-sex wedding requests post-*Masterpiece* due to discrimination rather than because of a legitimate, constitutionally protected, message-based objection to celebrating same-sex marriage. Those assumptions make her conclusions about religious exemptions irrelevant to Chelsey’s free-speech claim. MTE 23.

As for strict scrutiny, Louisville claims that Chelsey’s argument is “inconsisten[t]”—that she cannot say strict scrutiny demands evidence but argue that “empirical evidence” about exempting Chelsey is “irrelevant.” RMTE 20. But that’s the rub. Barak-Corren has no evidence—empirical or otherwise—about how the *Masterpiece* Study applies here. She never audited Louisville’s professionals, studied Louisville’s religiosity, tested the public’s interactions with media or district court opinions, or claims that religious-exemptions are the same as message-based objections. *See* MTE 20–21, 24–25. Barak-Corren also disclaims being an expert in current wedding markets. Tr. 113:1–13. And only .00051% (1/1,977) of professionals explicitly declined to celebrate same-sex weddings for conscience reasons. MTE 25.

Louisville counters that Chelsey puts “an undue emphasis on testimony from HRC witnesses” about the lack of complaints because their testimony “was not based on any rigorous or comprehensive analysis.” RMTE 18. But Louisville—not Chelsey—must show an actual problem exists. *Brown v. Ent. Merchants Ass’n*, 564 U.S. 786, 799–800 (2011) (state “bears risk of uncertainty,” not speakers). Barak-Corren’s testimony is irrelevant to that showing because she identifies no problem in Louisville and Louisville identifies no reason to doubt its own officials’ conclusion that the injunction has caused no issue in Louisville.

Besides being irrelevant, consider some consequences of Barak-Corren's testimony. Pretend Barak-Corren performed the same study, but studied parade organizers instead. Call it the *Hurley* Study. Pretend further that the *Hurley* Study found parades more often excluded pro-LGBT messages from their parades after a Supreme Court decision than before. Would that justify compelling all parade organizers to include pro-LGBT messages? Of course not. See *Hurley v. Irish-Am. Gay, Lesbian & Bisexual Grp. of Bos.*, 515 U.S. 557, 572–73 (1995) (parade had First Amendment right to reject units that would “alter the expressive content of their parade”). Parades have a constitutional right to decide for themselves the messages they want to promote. Barak-Corren, though, relabels constitutional freedoms as discrimination and Louisville equates constitutional “message-based objections” with “discriminatory animus.” RMTE 20. Barak-Corren and Louisville are wrong.

Take another example. *Masterpiece* found that Colorado treated Jack Phillips with “a clear and impermissible hostility toward” his religious beliefs. *Masterpiece Cakeshop, Ltd. v. Colorado C.R. Comm'n*, 138 S. Ct. 1719, 1729 (2018). Under Barak-Corren's theory, the Supreme Court should have ruled against Jack Phillips—and tolerated Colorado's hostility—because of the speculative possibility that the media would misreport the case, the public would misunderstand the ruling, and professionals would discriminate more based on their incorrect understanding of a court opinion. GY Report ¶ 39. That cannot be right.

In the end, Barak-Corren's testimony is irrelevant to Chelsey's claims and to strict scrutiny. Her testimony should be excluded.

### **Conclusion**

Professor Netta Barak-Corren's testimony, including her report, her written articles, and any additional testimony she may provide should be excluded.

Respectfully submitted this 25th day of October, 2021.

By: s/ Bryan D. Neihart

Ryan Bangert  
TX Bar No. 24045446\*  
Jonathan A. Scruggs  
AZ Bar No. 030505\*  
Katherine L. Anderson  
AZ Bar No. 033104\*  
Bryan D. Neihart  
AZ Bar No. 035937\*  
**Alliance Defending Freedom**  
15100 N. 90th Street  
Scottsdale, AZ 85260  
Telephone: (480) 444-0020  
rbangert@adflegal.org  
jscruggs@adflegal.org  
kanderson@adflegal.org  
bneihart@adflegal.org

Hailey M. Vrdolyak  
IL Bar No. 6333515\*  
**Alliance Defending Freedom**  
440 First Street NW, Suite 600  
Washington, DC 20001  
Telephone: (202) 393-8690  
hvrldolyak@adflegal.org

David A. Cortman  
GA Bar No. 188810\*  
**Alliance Defending Freedom**  
1000 Hurricane Shoals Road NE  
Suite D-1100  
Lawrenceville, GA 30043  
Telephone: (770) 339-0774  
dcortman@adflegal.org

Joshua D. Hershberger  
KY Bar No. 94421  
**Hershberger Law Office**  
P.O. Box 233  
Hanover, IN 47243  
Telephone: (812) 274-0441  
josh@hlo.legal

*Attorneys for Plaintiffs*

\* Admission *Pro Hac Vice*

Attorneys for Plaintiffs

**CERTIFICATE OF SERVICE**

I hereby certify that on the 25th day of October, 2021, I electronically filed the foregoing document with the Clerk of Court using the ECF system which will send notification of such filing to all counsel of record who are registered users of the ECF system.

By: s/ Bryan D. Neihart

Bryan D. Neihart  
AZ Bar No. 035937\*  
Alliance Defending Freedom  
15100 N. 90th Street  
Scottsdale, AZ 85260  
Telephone: 480-444-0020  
bneihart@ADFlegal.org

*Attorney for Plaintiffs*  
\* Admitted *Pro Hac Vice*